BIOMETRIKA

A JOURNAL FOR THE STATISTICAL STUDY OF BIOLOGICAL PROBLEMS

EDITED
IN CONSULTATION WITH FRANCIS GALTON
BY
W. F. R. WELDON KARL PEARSON
AND
C. B. DAVENPORT

VOLUME I
October 1901 to August 1902

CAMBRIDGE
AT THE UNIVERSITY PRESS

LONDON: C. J. CLAY AND SONS, AVE MARIA LANE
AND H. K. LEWIS, GOWER STREET
NEW YORK: THE MACMILLAN COMPANY
LEIPSIC: BROCKHAUS
BOMBAY AND CALCUTTA: MACMILLAN AND CO., LIMITED

CONTENTS

Memoirs.

Miscellanea.

Plates.

Subject and name indices will be issued every few years embracing several volumes.

IGNORAMUS, IN HOC SIGNO LABOREMUS.

VOLUME I · OCTOBER, 1901 · No. 1

BIOMETRIKA.

EDITORIAL.

(I.) *The Scope of Biometrika.*

It is intended that *Biometrika* shall serve as a means not only of collecting under one title biological data of a kind not systematically collected or published in any other periodical, but also of spreading a knowledge of such statistical theory as may be requisite for their scientific treatment.

A very few years ago, all those problems which depend for their solution on a study of the differences between individual members of a race or species, were neglected by most biologists. The complexity of organic structure is so great, and the number of distinguishable forms so enormous, that morphologists were obliged to simplify their conceptions by constructing for every species an ideal type, to which the individuals composing it conform with more or less exactness, and to neglect those deviations from the type which actually occur. Such simplification was not only justifiable, but absolutely necessary for many purposes; it has rendered enormous service to biology in the past, it does so still, and will continue to do so; nevertheless, there are many problems which cannot be dealt with by its aid.

The starting point of Darwin's theory of evolution is precisely the existence of those differences between individual members of a race or species which morphologists for the most part rightly neglect. The first condition necessary, in order that any process of Natural Selection may begin among a race, or species, is the existence of differences among its members; and the first step in an enquiry into the possible effect of a selective process upon any character of a race must be an estimate of the frequency with which individuals, exhibiting any given degree of abnormality with respect to that character, occur. The unit, with which such an enquiry must deal, is not an individual but a race, or a statistically representative sample of a race; and the result must take the form of a numerical statement, showing the relative frequency with which the various kinds of individuals composing the race occur.

As it is with the fundamental phenomenon of variation, so it is with heredity and with selection. The statement that certain characters are selectively eliminated from a race can be demonstrated only by showing statistically that the individuals which exhibit that character die earlier, or produce fewer offspring, than their fellows; while the phenomena of inheritance are only by slow degrees being rendered capable of expression in an intelligible form as numerical statements of the relation between parent and offspring, based upon statistical examination of large series of cases, are gradually accumulated.

These, and many other problems, involve the collection of statistical data on a large scale. That such data may be rendered intelligible to the mind, it is necessary to find some way of expressing them by a formula, the meaning of which can be readily understood, while its simplicity makes it easy to remember. The recent development of statistical theory, dealing with biological data on the lines suggested by Mr Francis Galton, has rendered it possible to deal with statistical data of very various kinds in a simple and intelligible way, and the results already achieved permit the hope that simple formulæ, capable of still wider application, may soon be found.

The number of biologists interested in these questions, and willing to undertake laborious statistical enquiries, is already considerable, and is increasing. It seems, therefore, that a useful purpose will be served by a journal especially devoted to the publication of statistical data, and of papers dealing with statistical theory. Many persons are deterred from the collection of biometric data, by the difficulty of finding such a means of publishing their results as this journal will afford, and those results which are published frequently lose much of their value because the data on which they are based are withheld, or because they are isolated in publications largely devoted to other forms of investigation. Further, *Biometrika* will endeavour to introduce a uniformity of statistical treatment, terminology, and notation, so that results obtained by different investigators on different types of life may be easily and effectively compared.

Biometrika will include (*a*) memoirs on variation, inheritance, and selection in Animals and Plants, based upon the examination of statistically large numbers of specimens (this will of course include statistical investigations in anthropometry); (*b*) those developments of statistical theory which are applicable to biological problems; (*c*) numerical tables and graphical solutions tending to reduce the labour of statistical arithmetic; (*d*) abstracts of memoirs, dealing with these subjects, which are published elsewhere; and (*e*) notes on current biometric work and unsolved problems. It is proposed to include memoirs written in English, German, French, or Italian.

(II.) *The Spirit of Biometrika.*

It is almost impossible to study any type of life without being impressed by the small importance of the individual. In most cases the number of individuals is enormous, they are spread over wide areas, and have existed through long periods. Evolution must depend upon substantial changes in considerable numbers and its theory therefore belongs to that class of phenomena which statisticians have grown accustomed to refer to as *mass-phenomena*. A single individual may have a variation which fits it to survive, but unless that variation appears in many individuals, or unless that individual increases and multiplies without loss of the useful variation up to comparatively great numbers—shortly, until the fit type of life becomes a mass-phenomenon, it cannot be an effective factor in evolution. The moment this point is grasped, then whether we hold variation to be continuous or discontinuous in magnitude, to be slow or sudden in time, we recognise that the problem of evolution is a problem in statistics, in the vital statistics of populations. Whatever views we hold on selection, inheritance, or fertility, we must ultimately turn to the mathematics of large numbers, to the theory of mass-phenomena, to interpret safely our observations. As we cannot follow the growth of nations without statistics of birth, death, duration of life, marriage and fertility, so it is impossible to follow the changes of any type of life without its vital statistics. The evolutionist has to become in the widest sense of the words a registrar-general for all forms of life. When he cannot observe and measure in Nature, then he must experiment on "populations" within the laboratory. But few biological laboratories have the space or the resources needed for dealing with the vital changes of populations, still less do the means at the disposal of individuals suffice for carrying out extensive experiments of this character. Much has been done and undoubtedly more will be done by the Marine Biological Laboratories for the study of mass-phenomena, but what is urgently needed is the establishment of a well-equipped Biometric Farm Laboratory, where breeding and survival experiments on large numbers could be carried out with ample room and care and, when necessary, for long periods. To this point we hope to return, and we shall not cease to urge its importance*.

But if we have thus to deal with a mass-phenomenon, may we not ask how it came about that the founder of our modern theory of descent made so little appeal to statistics? An illustration may aid us; the structure of our present theory of the moon is the creation of Newton, using his characteristic geometrical methods. But the practical astronomer in all lunar investigations to-day applies the analytical methods subsequently devised by the French mathematicians. The characteristic bent of Charles Darwin's mind led him to establish the theory of descent

---

* The failure of an attempt in this direction made a few years ago was, we believe, largely due to the fact that some of its supporters had not realised that the problem of evolution is a problem in the vital statistics of large numbers.

without mathematical conceptions; even so Faraday's mind worked in the case of electro-magnetism. But as every idea of Faraday allows of mathematical definition, and demands mathematical analysis in its modern statement, so every idea of Darwin—variation, natural selection, sexual selection, inheritance, prepotency, reversion—seems at once to fit itself to mathematical definition and to demand statistical analysis. Nor was the statistical conception itself entirely wanting in Darwin's work. *The Cross and Self-Fertilisation of Plants* forms a splendid collection of statistical observations and experiments which offers many points of departure for further statistical research*. That Darwin's mind did not work easily in mathematical lines is, perhaps, best evidenced in the passage of a letter of 1857 to Sir John Lubbock written when Darwin was dealing with the statistics of varieties in species as deduced from *Floras*:

> You have done me the greatest possible service in helping me to clarify my brains. If I am as muzzy on all subjects as I am on proportion and chance,—what a book I shall produce! (*Life*, II. p. 104).

But that he realised the importance of the statistical method for his investigations is evidenced not only by this very passage, but by several others. Thus considering the *variation* of our common species he writes in 1846:

> Andrew Smith once declared he would get some hundreds of specimens of larks and sparrows from all parts of Great Britain, and see whether, with finest measurements, he could detect any proportional variations in beaks or limbs, etc. This point interests me from having lately been skimming over the absurdly opposite conclusions of Gloger and Brehm (*Life*, II. p. 35).

Andrew Smith indeed missed the opportunity of being a veritable biometric pioneer!

Elsewhere Darwin recognises the importance of determining the variability of skeletons by measuring limbs (*Life*, II. p. 50). But, perhaps, the strongest evidence of his consciousness that biometry offers the only possible solution of problems of *inheritance* occurs in the words:

> I write now to say that I have been looking at some of our mongrel chickens, and I should say *one week old* would do very well. The chief point which I am, and have been for years, very curious about, is to ascertain whether the *young* of our domestic breeds differ as much from each other as do their parents, and I have no faith in anything short of actual measurement and the Rule of Three (*Life*, II. p. 51).

These words prove fully Darwin's consciousness not only of the need of measurements, but also of arithmetical work upon such data in the case of heredity. They may well serve as a motto for *Biometrika* and for all biometricians: *I have no faith in anything short of actual measurement and the Rule of Three.*

It is not a mere formal clothing of biological conceptions with mathematical symbols that is here indicated, or that we are considering, when we say that all

---

* See for example Mr G. U. Yule's use of Darwin's data in his recent memoir "On the Association of Attributes in Statistics," *Phil. Trans.* Vol. 194, A., 258.

Darwin's ideas fit themselves to algebraic definition. On the contrary—exactly as in the like case of the mathematical treatment of Faraday's conceptions of electromagnetism—the symbolic analysis widens our notions, it leads us at once to new points of view and it directly suggests—perhaps this is its most important advantage—fresh points for observation and novel directions for experimental research.

The danger will no doubt arise in this new branch of science that—exactly as in some branches of physics—mathematics may tend to diverge too widely from Nature. The biologist, the mathematician and the statistician have hitherto had widely differentiated fields of work. Each one of these fields is full of pitfalls, and when the worker amid living types wanders among symbolic forms, the mathematician by profession must give him a helping hand if he stumbles over a determinant or gets entangled in a differential. A like patience must be extended by the biologist to the mathematician when he makes blunders at which the morphological tyro would smile. As Mr Francis Galton said a few years ago, for these new problems we want a scientific firm with a biologist and a mathematician as acting partners and a logician as a consulting partner. Patient endeavour to understand each other's methods, and to bring them into harmony for united ends and common profit—this is the only method by which we can win for biometry a recognised place in the world of science and in the accepted academic curricula of the universities. The day will come—is, perhaps, already dawning with the younger workers—when we shall find mathematicians who are competent biologists, and biologists who are competent mathematicians, but our universities both as to teachers and laboratories are not yet adapted for the training of such men, and for some time to come we can in the main only hope for effective partnership and not for the all-round biometrician. We have a splendid, almost untraversed field to work in, and a great task to perform in winning not only full recognition from the scientific world, but public support for our work. If these conditions are kept in view the diverse degrees of mathematical and biological knowledge exhibited in our pages will not oppress our readers. We shall publish careful biometric observations, even if they be accompanied by only the most elementary statistical treatment; we shall look forward to our mathematical workers supplementing such fundamental observations by more elaborate statistical calculations. For this reason we shall not only print as copious observational and experimental data as possible, but endeavour to form a manuscript collection of such data available for further research. We hope that every number of *Biometrika* will present statistical material ready for the mathematician to calculate and to reason upon. All such investigations ancillary to data appearing in our pages we shall receive gladly and publish at the earliest opportunity.

On the other hand the biologist will find in our pages algebraic analysis which may repel him. We would still ask his attention for the general conclusions and for the formulæ reached by the mathematician. The biologist will find that they frequently suggest observations and experiments which he alone is in a position to undertake satisfactorily. We shall aid the more arithmetical part of his work by diagrams and numerical tables wherever it seems possible. In this manner we hope that *Biometrika* will provide material for both branches of science; that it will not only publish valuable biometric and statistical researches, but serve as a store-house of unsolved problems for both unemployed biologist and mathematician. We trust that bringing these men together may widen the activity of both.

Many of the problems of biometry can only be approached from the standpoint of the cooperative collection of material and reduction of statistics, and in this respect we shall strive to form a link between scattered biometric workers. *Biometrika* will ask for aid in cooperative work, and be at all times ready to publish requests for aid, and the forms and schedules which our contributors desire to be circulated or filled in. In this way it may be hoped that a guild of qualified collectors and workers may be gradually formed to whom appeal may be made for collecting, counting and observing. There are many men and women with the necessary training, scattered about this and other countries, who without having the opportunity for initiating original work are not only competent but glad to assist with collecting-box, camera or pencil. From such workers the Editors will be glad to hear and will endeavour to put them in touch with those desiring their aid.

Extensions, corrections, criticisms of the results published in our pages we shall heartily welcome whatever be their source. We expect to receive stalwart blows as well as to give them. All we shall demand in this respect is the chivalry which is needful in scientific controversy, which while combating error does not discourage honest endeavour. The most fertile men of science have made blunders, and their consciousness of such slips has been retribution enough; it is only their more sterile critics who delight to dwell too often and too long on such mistakes. In science, both in symbolic analysis and in our knowledge of Nature, we are very ignorant; we do not pretend that biometry will revolutionise our ideas of life. All we claim is that in certain aspects of biological research, biometry is an instrument which can aid us effectively in our gropings after truth. Only let the spirit in which it is used be that of the master-mind, the ideal so well and faithfully portrayed in the form and features on our frontispiece. *Ignoramus; in hoc signo laboremus!*

BIOMETRY.

By FRANCIS GALTON.

THIS Journal is especially intended for those who are interested in the application to biology of the modern methods of statistics. Those methods deal comprehensively with entire species, and with entire groups of influences, just as if they were single entities, and express the relations between them in an equally compendious manner. They commence by marshalling the values in order of magnitude from the smallest up to the largest, thereby converting a mob into an orderly array, which like a regiment thenceforth becomes a tactical unit. Those to whom these considerations are new, will grasp the results more easily by thinking of the array in its simplest, though not necessarily in its most convenient, form for mathematical treatment. Let them conceive each value to be represented by an extremely slender rod of proportionate length, and the rods to be erected side by side, touching one another, upon a horizontal base. The array of closely packed rods will then form a plane area, bounded by straight lines at its sides and along its base, but by a flowing curve above, which takes note of *every one* of the values on which it is founded, however immense their multitude may be. The shape of the curve is characteristic of the particular group of values to which it refers, but all arrays have a family resemblance due to similarity of origin; they all drop steeply at one end, rise steeply at the other, and have a sloping back. An array that has been drilled into some such formation as this, is the tactical unit of the new statistics. Its outline is expressed by a general formula whose constants are adapted to each particular case, and, being thus brought within the grip of mathematics, the internal relations of an array and their relations to those of any other array can be expressed in exact numerical forms. The new methods occupy an altogether higher plane than that in which ordinary statistics and simple averages move and have their being. Unfortunately the ideas of which they treat, and still more the many technical phrases employed in them, are as yet unfamiliar. The arithmetic they require is laborious, and the mathematical investigations on which the arithmetic rests are difficult reading even for experts; moreover they are voluminous in amount and still growing in bulk. Consequently this new departure in science makes its appearance under conditions that are unfavourable to its speedy recognition, and those who labour in it must abide for some time in patience before they can receive much sympathy from the outside world. It is astonishing to witness how long a time may elapse before new ideas are correctly established in the popular mind, however simple they may be in themselves. The slowness with which Darwin's fundamental idea of natural selection became assimilated by scientists generally, is a striking example of the density of human wits. Now that it is grown to be a familiar phrase, it seems impossible that difficulty should ever have been felt in taking in its meaning. But it was far otherwise, for misunderstandings and misrepresentations among writers of all classes abounded during many years, and even at the present day occasional survivals of the early stage of non-comprehension make an unexpected appearance. It is therefore important that the workers in this new field who are scattered widely through many countries, should close their ranks for the sake of mutual encouragement and support. They want an up-to-date knowledge of what has been done, and is doing, in it. They seek for opportunities of receiving judicious help from one another, sometimes in circulating questions, sometimes in discussing the preliminaries of new plans of campaign. Immense labour has too often been wasted in statistical research through a mistaken judgement of the value and real significance of the data employed. The fresh opinion of skilled onlookers is the safest test of the value of materials and affords a ready means of obtaining timely warning of the presence of vitiating conditions before an inquirer commits himself to any new statistical enterprise. Every investigator stands in need of expert criticism, for no pursuit runs between so many pitfalls and unseen traps as that of statistics.

This Journal, it is hoped, will justify its existence by supplying these requirements either directly or indirectly. I hope moreover that some means may be found, through its efforts, of forming a manuscript library of original data. Experience has shown the advantage of occasionally rediscussing statistical conclusions, by starting from the same documents as their author. I have begun to think that no one ought to publish biometric results, without lodging a well arranged and well bound manuscript copy of all his data, in some place where it should be accessible, under reasonable restrictions, to those who desire to verify his work. But this by the way. There remains another cogent reason of a very practical kind for the establishment of this Journal, namely that no periodical exists in which space could be allowed for the many biometric memoirs that call for publication. Biometry has indeed many points in common with Mathematics, Anthropology, Zoology, Botany, and Economic Statistics, but it falls only partially into each of these. An editor of any special journal may well shrink from the idea of displacing matter which he knows would interest his readers, in order to make room for communications that could only interest or even be understood by a very few of them. I am tempted to illustrate, or rather to over-illustrate, the coldness of welcome often afforded to a new departure in science, by an anecdote concerning the cause that really led to the foundation of the Geological Society of London. I have rarely related it in conversation, fearing to give pain to some one, and I have never done so in print; neither can I find that any version of it has been published by others. But now that nearly a century has slipped past since the event, there can be no harm in digging up and bringing to light a buried but amusing historical fact.

The story was told me long, long ago, in the 'forties, by Mr George Bellas Greenough, F.R.S. I was then an eager youth fresh from college, and he an elderly man; it was as follows. In 1806–7, when Geology was in its infancy and travellers were scarce owing to European wars, Mr Greenough and a few young friends compiled a list of questions with the view of ascertaining how far the facts of Nature might agree with the competing geological theories current in those days. Sir Joseph Banks was the President of the Royal Society at that time, an office which he exercised despotically for 43 years (1777–1820), becoming almost an autocrat over English scientific men. So it was to him that Mr Greenough and his young friends naturally went. They brought their questions and begged that copies of them might be circulated under official sanction among suitable persons, including foreign correspondents of the Royal Society. Sir Joseph was sometimes gracious in mood, frequently the reverse, and on this occasion he might be described as bearish. Not content with an emphatic "no," he dismissed them with words to the effect (in almost those very words, if my memory does not deceive me) that a few fools could ask more questions in half an hour than wise men might answer in years. The deputation departed, ready to burst with suppressed fury, and the moment they were quit of the house, agreed to circulate the questions on their own responsibility, which considering the persons and circumstances was an act of rare audacity. Out of this impromptu coalition, aided by a multitude of elsewhere recorded circumstances, the Geological Society was evolved, with Mr Greenough as its first President. (The official account of its origin is judiciously reticent, but not inconsistent with this little piece of history. It will be found in the preface to the first volume of its *Transactions*, published in 1811.) It is not in the least my intention to insinuate that Biometry might be served by any modern authority in so rough a fashion, but I offer the anecdote as forcible evidence that a new science cannot depend on a welcome from the followers of older ones, and to confirm the former conclusion that it is advisable to establish a special Journal for Biometry.

The primary object of Biometry is to afford material that shall be exact enough for the discovery of incipient changes in evolution which are too small to be otherwise apparent. The distribution of any given attribute, within any given species, at any given time, has to be determined, together with its relations to external influences. This affords a standard whence departures may be measured and the direction and rate of their progress ascertained. Evolutionary changes are exceedingly slow as a rule, but supposing that a thousand years or thirty generations of mankind, would suffice in some particular case for some conspicuous alteration in a species, exact measurements ought to discover its progress well within the limits of a human lifetime. Moreover the forms by which distribution is expressed in the new method are excellently fitted to bring to light any survivals of a less advanced type, which may serve as evidence of recent change. Also they quickly indicate incipient changes, through their power of isolating aberrant forms, and then of measuring the degree in which any of these may be favoured by natural selection. The organic world as a whole is a perpetual flux of changing types. It is the business of Biometry to catch partial and momentary glimpses of it, whether in a living or in a fossil condition, and to record what it sees in an enduring manner. It is an after-process to combine those glimpses into a continuously changing scene, much as some tumultuous procession is made to live and move again by means of a "biograph." Each biometric investigation may be compared to a solitary boring in a level plain, whose underlying geology has to be ascertained. A comparison of the cores brought up, will supply evidence of the depths of each of the buried strata and will justify many interpolations of unseen portions between the borings. For instance, it may not require many investigations to establish statistical laws of heredity on a secure basis, by ascertaining the limits within which those that have been already observed may hold good in a moderate number of widely different types of plant and animal life. Biology could soon be raised to the status of a more exact science than it can as yet claim to be, if each of many biometricians would thoroughly work out his own particular plot, although those plots may be very far indeed from occupying the whole of the area that admits of being directly explored.

VARIATIONSSTATISTISCHE PROBLEME UND MATERIALIEN.

VON PROF. DR F. LUDWIG.

I.

SCHON an anderem Ort* habe ich darauf hingewiesen, dass zwischen den zoologischen und botanischen Variationspolygonen ein wesentlicher Unterschied bestehen dürfte, insofern die letzteren vorwiegend komplex, polymorph, sind und nicht den einfachen Pearsonschen Typen monomorpher Variationspolygone entsprechen, wie sie von Anthropologen und Zoologen gefunden werden. Man kann das einmal mit dem bei der Pflanze *weniger begrenzten Wachsthum* erklären, bei dem *verschiedene* Entwicklungs-, bezüglich Wachsthumsstufen innerhalb der Species erreicht werden können (vgl. die Fibonaccikurven!); dann aber dürfte es zurückzuführen sein auf die bei den höheren *Pflanzen* im Gegensatz zu den höheren Tieren so *häufige asexuelle* und die ihr im Effect verwandte *autogame* sexuelle *Fortpflanzung.* Bei letzterer verhalten sich die Nachkommen wie Teile desselben Stockes, und sind nur im Besitz solcher Eigenschaften die das Mutterindividuum selbst kennzeichneten, die Variation führt zu den "petites espèces" deren Charaktere, wenn auch noch so minutiös, merkwürdig konstant gefunden werden. Die regelmässige sexuelle Fortpflanzung mit Xenogamie wird dagegen eine nivellierende Wirkung haben müssen, welche der Bildung "kleiner Arten" entgegensteht.

Diese Unterschiede in der Reihe der Fortpflanzungsarten, die beginnt mit den rein vegetativen Vermehrungsarten (bei dem europäischen *Kalmus,* bei *Ficaria verna, Trientalis europaea, Symphytum bulbosum, Apocynum hypericifolium, Lysimachia nummularia, Dielythra spectabilis,* der europäischen *Elodea canadensis* etc.†), mit der typisch parthenogenetischen Embryobildung (bei

---

* Ludwig; Een fondamenteel verschil in de veranderlijkheid bij het dier en de planten? *Kruidkundig Genootschap Dodonaea te Gent,* Elfde Jaargang, 1899, pp. 108—121.

† *Elodea* ist diöcisch, aber nur in dem einen Geschlecht in Europa verbreitet, die anderen genannten Arten sind selbststeril und bringen an Standorten, wo alle Individuen von demselben Stock abstammen, keine Samen. Vgl. F. Ludwig; On Self-sterility, *The Journal of the Royal Horticultural Society* (Hybrid Conference Report), Vol. XXIV. April 1900, pp. 214—217.

*Antennaria alpina**, *Alchemilla alpina, speciosa, alpestris,* u. a. *Alchemilla*arten†, die durch zahlreiche "petites espèces" ausgezeichnet sind), mit typischer Kleistogamie (bei *Plantago virginica* in Europa, *Collomia grandiflora* an vielen europäischen Standorten etc.), der Autogamie (wie bei *Erophila verna,* von der De Bary und Rosen zahlreiche "petites espèces" cultiviert haben), und endet mit der ausschliesslichen Xenogamie (mit Selbststerilität, ohne besondere asexuelle Vermehrung), sie müssen auch, wie ich meine, in den Variationspolygonen zum Ausdruck kommen.

Wo die sexuelle Fortpflanzung allein zur Geltung kommt, wird unter den von der Natur gegebenen Verhältnissen ein zwar komplexes aber *nahezu konstantes Variationspolygon* zu Stande kommen, wie ich es für *Chrysanthemum leucanthemum, Ch. inodorum, Ch. segetum* etc. nachgewiesen habe. Bei meinen "Hyperbinomialcurven" und verwandten Typen‡ haben die die abnorme Gipfelhöhe bedingenden nicht variierenden Exemplare wahrscheinlich vegetativen (asexuellen) oder autokarpen Ursprung. Da wo lokal die vegetative oder parthenogenetische (oder kleistogame) Fortpflanzung die ausschliessliche ist, werden die "petites espèces" ganz so wie in andern Fällen, wo die Individuen verschiedener Arten durch einander gezählt worden sind, *inkonstante Komplexcurven* (von zwar *gleicher Gipfellage, aber inkonstanter Frequenz*) ergeben, sofern nicht eine der kleinen Arten ganz beträchtlich überwiegt.

Das soeben erörterte scheint durch die folgenden Zählungen bei *Ficaria verna* Bestätigung zu finden. *Ficaria verna* (*Ranunculus Ficaria L.*) pflanzt sich sehr stark durch ober- und unterirdische Bulbillen fort und da sie selbststeril ist, so dass alle von demselben Individuum stammenden Bulbillen auch nach Generationen nur untereinander unfruchtbare Pflanzen ergeben, so bildet sie an vielen Orten überhaupt keine Früchte und pflanzt sich vorwiegend asexuell fort. Obwohl von den Systematikern nur wenig Varietäten beschrieben werden (vgl. Federico Delpino: *Dimorfismo del Ranunculus Ficaria L., Memoria letta alla R. Accademia delle Science dell' Istituto di Bologna* nella Sessione dell' 11 Aprile 1897, Bologna 1897, pp. 685—710), zeigen doch die folgenden statistischen Ergebnisse, dass dieselbe abgesehen von dem durch Delpino aufgefundenen Gynodimorphismus in manchen Gegenden (wo es kleinblütige ♀ und grossblütige ☿ gibt) in mehrere kleine Arten zu spalten ist.

Unsere Beobachtungen beziehen sich auf die Zahl der Blütenteile und zwar vorerst auf die relativen Anzahlen von Kelch- und Kronenblättern (erstere mit K, letztere mit C im Folgenden bezeichnet). (Ich teile hier das ganze Material

---

* *Cf.* H. O. Juel; *K. Svenska Vetenskaps-Academiens Handlingar,* Bd. XXXIII. No. 5, 59 pp. Stockholm 1900.

† Sv. Murbeck; *Lunds Univ. Arsskrift* XXXVI. 2, No. 7, 46 pp.; No. 9, 20 pp. Lund 1901.

‡ Vgl. Ludwig; Die pflanzlichen Variationskurven und die Gauss'sche Wahrscheinlichkeitskurve, *Bot. Centrbl.* Bd. 73, 1898.—G. Duncker; *Die Methode der Variationsstatistik,* Leipzig 1899, p. 127 ff.—G. Duncker; Variation und Asymmetrie bei *Pleuronectes flesus L., Wissensch. Meeresuntersuch.* N. F. Bd. 3 H. 2, p. 364.

mit, ob wohl dasselbe in dem vorliegenden Aufsatz nur nach einer Richtung hin zur Discussion kommt.)

Korrelative Zählungen der Kelch- und Kronblätter von *Ficaria verna.*

A. *Greiz* (*Lehrerseminar*) I^{te} *Tausend.*

Kronblätter

Kelchblätter	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	
0				3	5		1					9
1					1		1					2
2					1	1						4
3		8	16	83	**270**	65	26	9	3	1	1	**482**
4	1	3	21	64	97	59	29	5	4		1	284
5		1	10	30	52	44	33	21	19	1		211
6			1		1	2	2			2		8
	1	12	48	180	**427**	171	92	37	26	4	2	1000

B. *Greiz* (*Lehrerseminar*) II^{te} *Tausend.*

Kronblätter

Kelchblätter	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	
0				3	1	1	1					6
1			1	3								4
2		1	4	3	3							11
3	2	11	76	**270**	89	19	2	3	1			**473**
4		11	59	86	72	56	15	9	3		1	312
5	1	14	22	61	31	30	17	9	3			188
6				2	2	1	1					6
	3	37	162	**428**	198	107	36	21	7		1	1000

C. *Greiz (Seminar und Gymnasium) III^{te} Tausend.*

Kronblätter

Kelchblätter	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	
0						2								2
1					1									1
2				4	4	6	3	2		1	1			21
3	1	1	4	7	78	**262**	107	48	6					**514**
4		1	1	10	43	80	62	50	15	9	2		1	274
5			1	3	24	45	29	40	17	3				162
6					2	5	4	3	5	4	1			24
7						1		1						2
	1	2	6	24	152	**401**	205	144	43	17	4		1	1000

Letzteres C. setzt sich zusammen aus 300 Zählungen der Gymnasiasten von 1900, 300 solchen von 1901 und 400 Zählungen der Seminaristen von 1901 wie folgt:

D. 300 *Gymnasiastenzählungen.* 1900.

Kronblätter

Kelchblätter	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	
2			1		4	2	2		1	1			11
3			6	20	**86**	31	20	5					**168**
4	1		5	10	20	14	15	2	6			1	74
5			2	6	10	4	12	1	2				37
6				1	1	1	1	1	4	1			10
7													
	1		14	37	**121**	52	50	9	13	2		1	300

E. 300 *Gymnasiastenzählungen.* 1901.

Kronblätter

Kelchblätter	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	
2			2		1				3
3	1	1	1	30	**64**	31	14	1	**143**
4		1	1	12	29	21	17	12	93
5		1		12	13	5	11	7	49
6				1	3	2	1	3	10
7					1		1		2
	1	3	4	56	**111**	59	45	24	300

F. 400 *Seminaristenzählungen.* 1901.

Kronblätter

Kelchblätter	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	
0						2						2
1					1							1
2				1	4	1	1					7
3	1		3		28	**112**	45	14				**203**
4				4	21	31	27	18	1	3	2	107
5				1	6	22	20	17	9	1		76
6						1	1	1	1			4
	1		3	6	60	**169**	94	50	11	4	2	400

G. *Greiz (Lehrer Winkler) IV*^{te} *Tausend.*

Kronblätter

Kelchblätter	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	
2												
3			2	10	56	**530**	89	23	7	1		**718**
4				3	32	56	26	10	6	3	1	137
5			4	9	9	49	26	19	11	1	8	136
6					1	1	4	1	1			8
7							1					1
			6	22	98	**636**	146	53	25	5	9	1000

H. *Gera (Realgymnasium)* 1000.

Kronblätter

Kelchblätter	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	
0					3								3
1													
2		3	3	2	7	4	1	1					21
3		12	22	67	**460**	117	33	12	7	2		1	**733**
4		1	5	35	55	35	13	6	2				152
5			1	8	36	19	13	4	2	3			86
6					2		2	1					5
		16	31	112	**563**	175	62	29	11	5		1	1000

I. *Gera (Realgymnasium)* 675 *Zählungen.*

Kronblätter

Kelchblätter	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	
2						1					1
3		2	15	73	**347**	45	36	8			**526**
4		2	3	18	54	10	11	3			101
5		5	5	7	9	12	5				43
6				1			3				4
		9	23	99	**410**	68	55	11			675

K. *Gera (Lehrer Auerbach und einige Schülerzählungen)* 712 *Zählungen.*

Kronblätter

Kelchblätter	*7*	*8*	9	10	
2	4				4
3	5	**498**	93	15	**611**
4		14	11	3	28
5		37	8	24	69
	9	**549**	112	42	712

L. *Trogen* (*Kt. Appenzell, Schweiz*) 1900, *Kantonschule* (*A. Heyer*), 285 *Zählungen.*

Kronblätter

		5	*6*	*7*	*8*	*9*	*10*	*11*	
Kelchblätter	*3*	1	2	11	**133**	32	13	2	**194**
	4		1	18	42	10	2	1	74
	5		1	4	8	1	1		15
	6			1			1		2
		1	4	34	**183**	43	17	3	285

Die Einzelzählungen ergeben, und das kommt auch in den vorstehenden Gesammtergebnissen zum Ausdruck, dass in Deutschland (Greiz, Gera) und der Schweiz (Trogen) ganz bedeutend überwiegt *als Normalform* K_3C_8 (mit 3 Kelchblättern und 8 Kronblättern), dass aber *in verschiedener Präponderanz*—und das beweist eben ihre Bedeutung als "petites espèces"—verbreitet sind *namentlich die Formen* K_5C_8, K_5C_{10}, K_3C_{10}, K_3C_5, K_5C_5, K_3C_{13}, K_5C_{13}, und—selten K_0C_8.

Die Zählungen A—G sind von Schülern verschiedener höheren Lehranstalten in der Umgebung von Greiz an den verschiedensten Standorten gemacht worden und zwar "ohne Wahl." Bemerkenswert sind die unter A, B und F aufgeführten Zählungsergebnisse der Zöglinge des Greizer Lehrerseminars die von 41 Schülern gemacht wurden. Das erste Tausend (A) wurde von den Herren Seminaroberlehrern Collmann und Lotter, welche die Zählungen leiteten, zusammengestellt (von 18 Schülern), aus dem übrigen Material wählte ich beliebige 1000 Zählungen (sie stammten von 17 anderen Schülern) für B und den Rest von 400 Zählungen (von 6 Schülern) für F aus. Trotz der verschiedenen Zähler und des verschiedenen Ursprungs erwiesen sich die Resultate für K_3, C_8, K_3C_8 in hohem Grad übereinstimmend: für

	K_3,	C_8,	K_3C_8
bei A	48·2%	42·7%	27·0%
B	47·3%	42·8%	27·0%
(F	50·7%	42·3%	28·0%).

Die Zählungen waren im Anfang der Blütezeit gemacht; die von Herrn Lehrer Winkler (G) von gleichen Orten meist zu Ende derselben. Erfahrungsgemäss findet man zuletzt höhere Zahlen, dies spricht sich auch in den Ergebnissen bei G bezüglich der Blumenblätter aus. Andere Unterschiede ergeben sich daraus, dass gegen Ende der Blütezeit die Blätter leicht ausfallen und Herr W. nur vollkommene Blüten berücksichtigend wohl etwas über das Ziel hinaus kam.

Hier ist K_3C_8 bei 53% der Zählungen vorhanden, bei den Zählungen der Gymnasiasten ergeben sich 1901 etwas andere Zahlen als 1900 nämlich für

K_3C_8: 1900 28·7%; 1901 21·3%.

Die Resultate unter D, E, F ergeben zusammen unter C für

K_3,	C_8,	K_3C_8
51·1%	40·1%	26·2%

(mit Rücksicht auf die etwas verschiedene Zeit wieder gut mit A, B übereinstimmend).

Bei den Geraer Zählungen fehlt die Zeitangabe, doch ergibt sich hier die Procentzahl übereinstimmend höher als für Greiz: z. B. bei H für K_3C_8 46%; bei K (für das die Bemerkungen bei G zutreffen) sogar gegen 70%.

Nach der Frequenz ordnen sich die Hauptformen in den verschiedenen Beobachtungsreihen wie folgt:

Greiz
A K_3C_8, K_5C_8, K_5C_{10}, K_3C_{10}, K_3C_5, K_0C_8, K_5C_{13}, K_3C_{13}, K_5C_5
B K_3C_8, K_5C_8, K_5C_{10}, K_3C_{10}, K_5C_{13}, K_0C_8, K_3C_5, K_3C_{13}, K_5C_5
C K_3C_8, K_5C_8, K_5C_{10}, K_3C_5, K_3C_{10}, K_0C_8.

Gera
H K_3C_8, K_5C_8, K_3C_{10}, K_5C_{10}, K_3C_5, K_5C_{13}, K_3C_{13}
I K_3C_8, K_3C_{10}, K_5C_8, K_5C_{10}, K_5C_5, K_3C_5
K K_3C_8, K_5C_8, K_5C_{10}, K_3C_{10}.

Trogen K_3C_8, K_3C_{10}, K_5C_8, K_5C_{10}, K_3C_5.

Es findet sich mithin allenthalben (Greiz, Trogen, Gera) neben K_3C_8 als *Normalform* zunächst mit wechselnder Prävalenz K_5C_8, K_5C_{10}, K_3C_{10}, dann die Formen K_3C_5, K_0C_8, K_5C_{13}, K_3C_{13}, K_5C_5 in noch weiter wechselnder Prävalenz und geringster Frequenz. Die Bedeutung dieser Formen als "petites espèces" liesse sich nicht besser erweisen, als durch die Auffindung einer derselben als Normalform in anderer Gegend. Thatsächlich ist dies geglückt, bevor das vorliegende Material verarbeitet wurde. Herr A. Heyer, Institutslehrer in St. Gallen, erhielt 1900 zunächst 79 Schülerzählungen aus dem 900 m. hoch gelegenen Gais —in Trogen, seinem damaligen Wohnort war *Ficaria verna* bereits verblüht—die die grösste Frequenz für K_5C_8 ergaben. Herr Reallehrer Freund konstatierte sodann, dass dies die Normalform sei. Das Ergebniss war:

Kronblätter

Kelchblätter	6	7	8	9	10	11	12	
3		1	3	5				9
4		4	29	10	2			45
5	1	8	**56**	36	17	6	2	**126**
6		1	1		2			4
	1	14	**89**	51	21	6	2	184

Normalform K_5C_8
Nebenform K_5C_{10}
(und K_3C_8)

Im Jahr 1901 ergaben sich auf demselben Standort noch höhere Zahlen

Kronblätter

Kelchblätter	6	7	8	9	10	11	12	13	14	
2					1	1				2
3		2	25	25	26	10	3			91
4		11	46	98	94	31	3			283
5	2	19	68	140	**172**	132	63	18	2	**616**
6		2		1	3	1				7
7				1						1
	2	34	139	265	**296**	175	69	18	2	1000

also K_5C_{10} als Normalform; Nebenformen K_5C_8, K_3C_{10}, K_3C_8, K_5C_{13}.

Dass es sich bei der *Ficaria verna* von Gais um eine andere Rasse als die gewöhnliche handelt, zeigt auch der Vergleich der Korrelationsfelder von Staubgefässen und Stempeln (Zählungen von 1900).

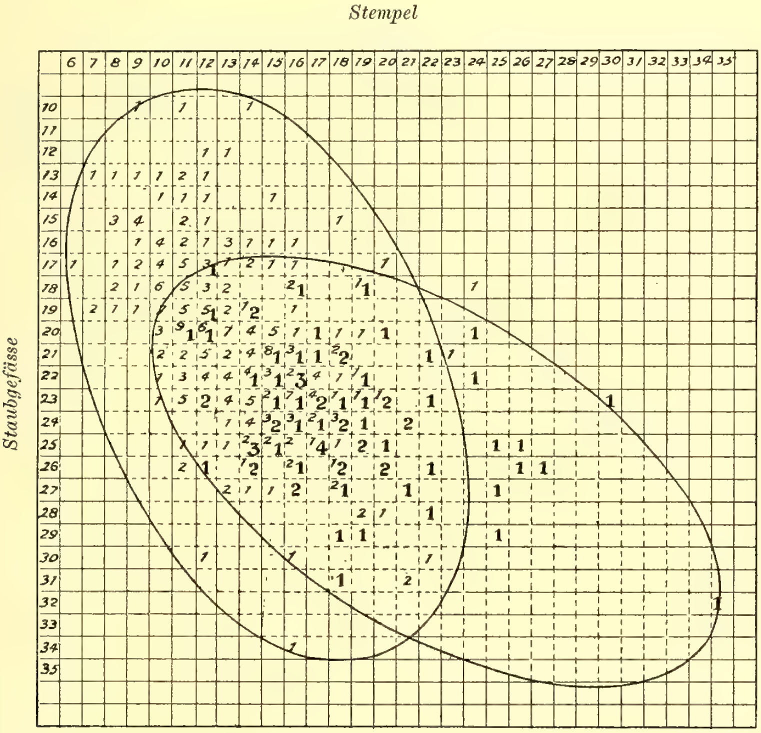


Fig. 1. Felder der korrelativen Variation der Staubgefässe und Stempel der gewöhnlichen Rasse von *Ficaria verna* und der Rasse von Gais (dicke Zahlen).

Zusammengehörige neuere Zählungen scheinen für die bei uns häufigste Form der *Ficaria* $K_3C_8S_{21}G_{13}$ zu ergeben (S Staubgefässe, G Stempel)*.

II.

Kontinuierliches oder rhythmisch pausierendes Wachsthum?

Wie die Variation in der Anzahl der Blätter etc. eines Stengels, der Blüten eines Blütenkopfes, *sprungweise* erfolgt und zwar nach der Reihe der Fibonaccizahlen und der von mir ermittelten Unterreihe derselben, so dass z. B. bei der Züchtung mehrblütiger Rassen von *Chrysanthemum segetum* (durch de Vries) nicht Rassen mit beliebigen Gipfelzahlen in den Variationscurven, sondern nur solche mit Gipfeln bei 13 16 21 26 34 42 47 etc. Randblüten entstehen konnten, so machen es gewisse Erscheinungen auch wahrscheinlich, dass das Wachsthum (der Stengel, Blätter etc.) gesetzmässig durch gewisse Hauptetappen hindurch läuft, bei denen es enden kann, nicht aber bei beliebigen anderen Werten seinen Abschluss erreicht. Bei den Zellfäden der *Melosira arenaria* (Moore) ist ein solches discontinuierliches rhythmisches Wachsthum allgemein bekannt; die Längen ungleichalosiger Fäden stehen genau in dem Verhältnis der Zahlen der Fibonaccireihe. Auch *bei höheren Pflanzen* deuten die Variationscurven für die Längen etc. der Stengel, Blätter etc. auf ein ähnliches Wachsthumsgesetz hin. So wurde für *Fagus silvatica* etc. nachgewiesen (*Bot. Jaarboek Dodonaea te Gent*, XI. 1899, p. 116) dass das Variationspolygon für die Blattlänge mehrgipfelig ist. Und zwar scheint die Länge des Blattes nicht durch alle Werte hindurch zu variieren, sondern sprungweise und zwar genau nach dem für die numerische Variation der Blütenstande etc. ermittelten Gesetz (cf. *Bot. Centralbl.* 1898 Bd. LXXV. p. 105). Für das Wachsthum der Halminternodien der Getreidearten ergibt sich ähnliches nach den Untersuchungen von de Bruyker (Over correlatieve variatie bij de Rogge en de Gerst, Gent 1898, pp. 42—56; besonders Fig. 4; vgl. meine Bemerkung im *Bot. Centralbl.* Bd. IX. Beihefte 6, 1900 p. 441). *Die bis jetzt vorliegenden Untersuchungen sind aber noch zu geringzählig um daraus weitgehende Schlüsse ziehen zu können. Ein eingehendes Studium von Komplexcurven bei Längenvariation etc. und ihrer*

---

* Bei der gleichzeitigen Zählung von Kelch-, Blumen-, und Staub-, oder Blumen-, Staub-, und Fruchtblättern bedient man sich zweckentsprechend eines parallelepipedischen Kastens mit parallelen gleicherweise gekästelten Schubfächern in die man Kugeln hineinlegt. Länge, Breite, Höhe entsprechen den 3 korrel. Elementen. Man erhält so einen Korrelationsraum bestimmter, der Species entsprechender Form und Lage—dem zweidimensionalen Korrelationsfeld bei 2 Varianten entsprechend. Eine geometrische Darstellung der Korrelation zwischen allen 4 Blütenteilen ist im dreidimensionalen Raum unmöglich.

Die aus den korrelativen Beziehungen zweier Merkmale abgeleiteten Häufigkeitsbeziehungen zwischen den "petites espèces" lassen sich auch geometrisch anschaulich machen wenn man auf den Quadraten des Korrelationsfeldes senkrechte Stäbe anbringt deren Längen den in ihnen stehenden Zahlen entsprechen, oder den Vertikal- und Horizontalreihen entsprechend zwei senkrecht einander durchkreuzende Systeme der zugehörigen Variationspolygone.

allmählichen Ausgestaltung wird hier allein zum Ziel führen. Ein Anfang wurde auf meine Veranlassung gemacht mit den Nadeln von *Pinus silvestris.* Mein Freund Herr A. Heyer, in St Gallen, hat von Februar bis April 1900 12000 frische Nadeln einer und derselben ca. 5—6 m. hohen Kiefer bei Trogen abgemessen und zwar, da die Länge der Nadeln von den unteren Ästen zum Gipfel zunimmt, je 4000 von den unteren, den mittleren und oberen Ästen der Krone. Die Gesammtergebnisse zeigt folgende Tabelle:

Pinus silvestris. *Nadellänge in Millimetern.*

	7	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	*21*	*22*	*23*	*24*	*25*	*26*	*27*	*28*	*29*
Frequenz	7	8	8	7	17	26	47	66	107	128	160	232	270	307	334	356	336	**362**	307	243	195	**203**	88
		3	12	6	11	7	25	16	21	39	72	70	84	110	127	196	207	**316**	270	290	311	**358**	326
	1	4	12	14	19	22	42	54	55	66	85	84	109	113	142	149	230	**244**	243	274	326	**360**	313
	8	15	32	27	46	55	114	136	183	233	317	386	463	530	603	701	773	**922**	820	807	832	**921**	727

mm.	*30*	*31*	*32*	*33*	*34*	*35*	*36*	*37*	*38*	*39*	*40*	*41*	*42*	*43*	*44*	*45*	*46*	*47*	*48*	*49*
4000 Nadeln von unteren Ästen	83	45	29	19	4	4	1	1												
4000 Nadeln von mittleren Ästen	276	194	**207**	131	96	83	60	35	26	11	3	1								
4000 Nadeln von oberen Ästen	243	230	154	113	89	53	41	33	26	16	15	7	4	5	1	2	3		2	2
12,000 Nadeln	602	469	390	263	189	140	102	69	52	27	18	8	4	5	1	2	3		2	2

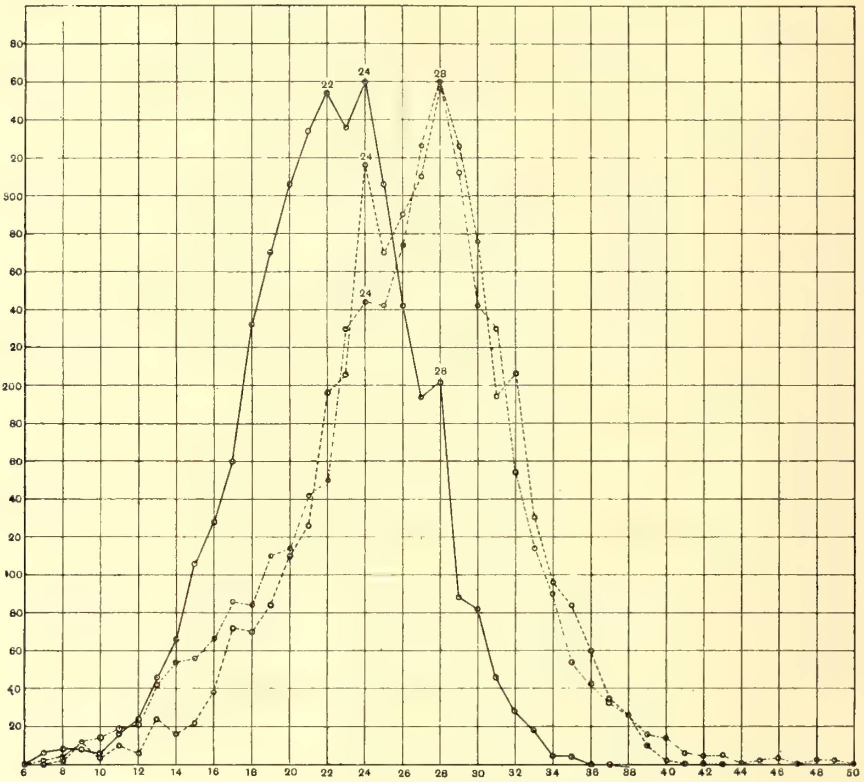


FIG. 2. *Pinus silvestris.* Nadellänge in Millimetern.

(1) ——— Polygon von 4000 Nadeln der unteren Äste.
(2) - - - - - - ,, ,, ,, mittleren ,,
(3) —•—•—• ,, ,, ,, oberen ,,

Vgl. die Polygone in der Abbildung.

Die Hauptcurve erscheint zusammengesetzt aus 2 polymorphen Curven mit den Hauptgipfeln bei 24 und 28 (ersterer teilt die Variationswerte von 7 bis 42 im Verhältnis 2 : 3, letzterer im Verhältnis 3 : 2), sie zeigt einen ganz ähnlichen Bau wie die oben erwähnte Curve von De Bruyker (dessen Fig. 4; nur dass hier die Verhältnisse 5 : 8 und 8 : 5 sind und die Gipfel ungleiche Frequenzen haben), noch mehr erinnern uns die Einzelkurven, die successive zu den obigen Resultaten geführt haben, in ihrem Verlauf an die Fibonaccikurven. [Es ist bemerkenswert dass Heyer schon bei seinen ersten Tausendzählungen immer mehr zu der Überzeugung kam, dass die Gipfel die Multipla einer gewissen "Einheitslänge" darstellten. So zeigte das erste Tausend eine Variation von 7—35 mm., die Gipfellagen bei 21 und 28. Als Einheitslänge betrachtete er 7 mm., $21 = 3 \cdot 7$, $28 = 4 \cdot 7$, $35 = 5 \cdot 7$ wären dann die Multipla—bei den eigentlichen Fibonaccikurven würden nur eben *bestimmte*, nicht *alle* Multipla auftreten wie dies auch in der *grossen Zahl* der Nadelzählungen thatsächlich der Fall ist.]

FIG. 3. *Pinus silvestris.* Nadellänge in Millimetern. Summationspolygon der 3 anderen Polygone 12000 Nadelmessungen eines Baumes enthaltend.

Auf die Erreichung *verschiedener, bestimmter Stufen*, bei wenig begrenztem Wachsthum scheinen auch solche Fälle vielgipfeliger Variationscurven bei einheitlicher Art zurückzuführen zu sein, wo die Gipfel von den ersten Zählungen an immer die gleichen bleiben so z. B. bei der Zahl der Ährchen von *Agropyrum repens* (vgl. auch die Blütenstände vieler Papilionaceen, Primulaceen etc.). Hier fand ich bei Schmalkalden und Greiz schon von den ersten Zählungen an überwiegen die Zahlen 11, 13, 17, 19 die auch in dem Gesammtergebnis meiner Zählungen (bei B), und von 348 Zählungen von A. Heyer bei Trogen (A) zu Tage treten:

Zahl der Ährchen bei *Agropyrum repens.*

	6	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	*21*	*22*	*23*	*24*	*25*	*26*	*27*
A		3	8	9	18	**28**	22	**35**	29	**32**	21	**22**	18	**32**	25	14	18	8	5			1
B	1	2	8	11	21	**41**	33	**46**	44	37	47	**52**	39	**45**	32	15	10	10	2			
Sa	1	5	16	20	39	**69**	55	**81**	73	69	68	**74**	57	**77**	57	29	28	18	7			1

Diese Zahlen sind auch bei anderen zweizeiligen Ähren mit Gipfelähren ($2n+1$, wo n wie im vorliegenden Fall 5 6 8 9 ist etc.) häufig (bei 4- bezw. 6zeiliger Gerste etc. wird man entsprechend Zahlen der Form $4n+1$, $6n+1$ erwarten). So bei dem polymorphen *Lolium perenne*, dessen Komplexcurve in der einen Gegend (z. B. Trogen 1899, 1900) 11 Ähren, in einer anderen (z. B. Basel 1900) 19 oder (Küsnacht 1900) 17 Ähren aufweist (die einzelnen "petites espèces" dürften aber hier begrenztes Wachsthum haben, da die einzelnen Curven eingipfelig sind); ferner bei dem wenig variablen *Brachypodium pinnatum*:

Zahl der Ährchen bei *Brachypodium pinnatum* (*Trogen*, *A. Heyer*).

	2	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*
Frequenz	2	5	68	291	584	**709**	697	522	294	172	57	16	3	2

Sa 3422 Expl. (vom Sommer 1900), wo also die 7 ($=2 . 3+1$) überwiegt. Bei dem verwandten *Brachypodium silvaticum* sind die *seitlichen* Ährchen meist unpaarig, denn das (einfache) Variationspolygon wird durch folgende Zahlen bestimmt:

Zahl der Ährchen bei *Brachypodium silvaticum* (*Trogen*, *A. Heyer* 1900).

	4	*5*	*6*	*7*	*8*	*9*	*10*	*11*	
Frequenz	1	11	81	289	**396**	193	43	6	(1020 Expl.)

III.

Einige weitere Beispiele von Fibonaccicurven.

Korrelation zwischen der Zahl der ♀ Blüten und ☿ Blüten im Blütenköpfchen von *Homogyne alpina*.

Unter den Kompositen liessen sich solche nachweisen bei welchen die Rand- und Scheibenblüten des Körbchens *gleichzeitig* nach höheren bezw. niedereren Zahlen hin variieren—bei ihnen ist der Korrelationscoeffizient positiv—und solche, bei denen beiderlei Blüten *entgegengesetzt* variieren, so dass eine Zunahme der einen Sorte eine Abnahme der anderen und umgekehrt bedingt, bei denen der Korrelationscoeffizient negativ wird. Im letzteren Fall bilden in der Regel erst die Summen der Zahlen der in Korrelation stehenden Glieder Zahlen der Fibonaccireihe etc., im ersteren Falle dagegen schon die Einzelzahlen. Diese Verhältnisse sollen hier nicht weiter erörtert werden, die Zählungen der Zwitterblüten und weiblichen Blüten im Köpfchen bei *Homogyne alpina* müssen noch fortgesetzt werden um den Korrelationscoeffizienten der Species und die

Weibliche Blüten im Köpfchen

	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	
0														1																												1
1																																										
2																																										
3																											1															1
4																	1																									1
5	1																															1										2
6					1			1					1							1							1															5
7												1		1		1	1			2		1																				7
8											1	1				1	2	1			1	2	2	4		2	1	1			1			1								**21**
9												1		1	1	2	2	1		1	1	1			1	3			1				2									18
10														1		1	3	2	1	1	1	1	2		1		1	1	1	1		2			1			1				**22**
11																		1	2		1	1		1			1								1	1				2		13
12															1	1		1		3	3	3	2	1	1	3	1	1	1	1				1	1				1	1		27
13								1										1			2	1		3	2	3	3	1	3	3	3	1	2	1		1					1	**32**
14														1									1	1		1		1									1					6
15																				1									1							1						3
16																				1																			1			2
17																												1														1
18																																										
19																																										
	1				1			2		1	1	3	2	5	2	6	9	7	3	10	9	10	7	10	5	**12**	9	6	7	5	4	4	4	3	3	3	1	1	2	3	1	162

Zwitterblüten

Variationspolygone für die Zahl der Zwitterblüten bezw. der weiblichen Blüten zuverlässig zu ermitteln. Die vorläufigen hier folgenden Zählungen von Heyer ergaben, dass letztere Fibonaccicurven zugehören und zwar für die weiblichen Blüten mit den Gipfeln bei 13 und 8, für die Zwitterblüten bei 34. (Nach einigen ergänzenden Zählungen von anderen Kollegen in der Schweiz haben die Zwitterblüten wahrscheinlich noch Gipfel bei 21, 29 und 42; die weiblichen bei 10. Die Lage der Korrelationsfelder im Coordinatennetz wird durch diese Zählungen nicht wesentlich geändert.)

Zählungen der Hüllblätter der Köpfchen ergeben gleichfalls Fibonaccicurven mit einem Hauptgipfel bei 13 und Secundärgipfeln bei 16, 10, 21.

Zahl der Hüllblätter	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	*21*	*22*	*23*	*24*
Häufigkeit	2	12	**18**	24	61	**153**	57	28	**30**	15	7	7	3	6		3	1

Meine Zählungen im Erzgebirge ergaben für *Homogyne* ähnliche Gipfellage wie für die Schweizer Exemplare.

Arnica montana.

H. de Vries hat gezeigt, dass durch künstliche Züchtwahl auch Rassen von Compositen erzielt werden können, deren Hauptgipfel auf eine der Nebenzahlen der Fibonaccireihe verschoben sind. In der Natur trifft man aber fast stets die Hauptgipfel bei den Hauptzahlen. Eine Ausnahme macht *Arnica montana*, deren Strahlencurve ihren Hauptgipfel bei einer Nebenzahl hat. Verschiedene Zählungen der Strahlenblüten, die ich 1896 am Oberhof in Thüringen, 1900 bei Greiz, Herr Lehrer Auerbach 1900 bei Gera machte, ergaben übereinstimmend den Hauptgipfel bei 16, Nebengipfel bei 13, 18, 21, 10. Das Gesammtresultat war:

Strahlenzahlen	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	*21*	*22*	*23*	*24*	*25*	*26*	*27*	*28*	*29*	*30*	*31*	*32*
Frequenz	2	1	3	8	15	19	48	76	85	71	**106**	69	73	50	36	27	7	3	2	1	2	1					1

Meine eigenen Zählungen bei Greiz ergaben (vgl. Fig. 4 untere Curve).

7	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	*21*	*22*
	2	4	8	8	15	**31**	22	18	**37**	23	**29**	13	12	**9**	

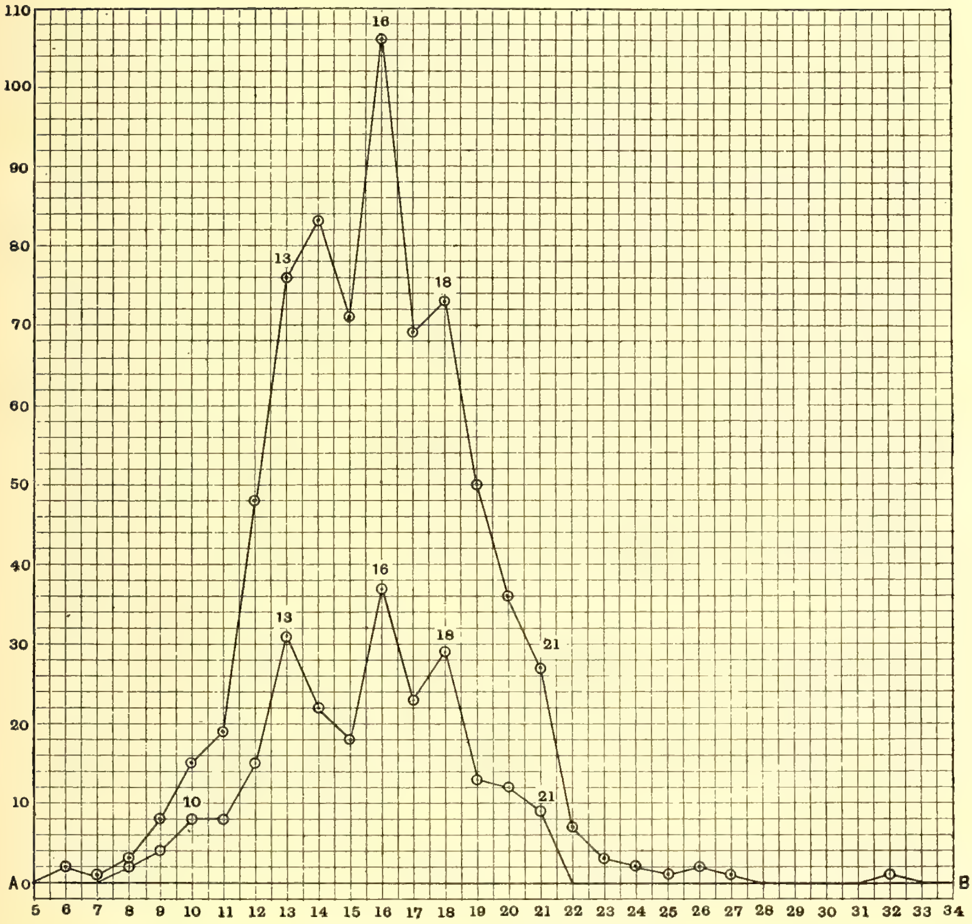


Fig. 4. *Arnica montana* (Strahlencurve).

A. Heyer sandte mir noch 1864 Zählungen von Strahlenblüten des *Bellidiastrum Michelli,* die bei der grossen Variationsweite der Species noch unzureichend zur Gewinnung des specifischen Variationspolygons, doch erkennen lassen, dass dasselbe einer Fibonaccicurve zugehört. Die Curve erstreckt sich von ca. 16 bis 89 und hat allem Anschein nach Gipfel bei 34, 26, 39, 55, 42. *Chrysanthemum Leucanthemum* hat nach seinen Zählungen (340 Exemplaren) eine ähnliche Curve bei Exemplaren aus der Schweiz wie bei den von mir in Deutschland gezahlten Exemplaren.

IV.

Blütendiagramm der Amygdaleen.

In meiner Abhandlung: Über Variationscurven und Variationsflächen der Pflanzen, *Bot. Centralbl.* 1895 Bd. LXIV. habe ich schon auf die Bedeutung der Variationsstatistik für die Ermittelung des Blütenaufbaues im Allgemeinen und der Blütenkreise von Rosaceen (Pomaceen: *Pirus communis,* Amygdaleen: *Crataegus oxyacantha, Crat. coccinea, Prunus spinosa*) im Besonderen hingewiesen. Von besonderem Interesse war der Nachweis des Vorhandenseins *acyclischer neben cyclischer Anordnung* in den Staubgefässkreisen. Das dort erörterte findet weitere Bestätigung durch das Polygon für die Staubgefässzahl von *Prunus spinosa,* wie sie von A. Heyer und mir festgestellt wurde (vgl. auch Fig. 5).

Zahl d. Staubgefässe	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	*21*	*22*	*23*	*24*	*25*	*26*	*27*	*28*	*29*	*30*	*31*	*32*	*33*	*34*	*35*
Frequenz	4	1	12	44	56	**77**	63	110	**163**	109	100	98	78	49	49	23	**36**	12	8	9	**14**	1	2	1		**1**

Die Staubgefässe treten in mehreren Kreisen auf, deren äusserer meist 10 St. enthält, der Innere enthält dann am *häufigsten* 8, (10 + 8 = 18), zuweilen aber nur 3 (10 + 3 = 13), oder 16 (10 + 16 = 26) oder es treten mehrere Kreise (10 + 10 + 10 = 30; 10 + 10 + 10 + 5 = 35) auf. Bei Leipzig fand Dietel nicht 18 sondern 21 als Gipfelzahl (10 + 8 + 3 oder dergl.). Für *Crataegus oxyacantha* ermittelte A. Heyer (Schweiz) übereinstimmend mit mir, ebensowie für *Cr. monogyna* den Gipfel bei 20 (= 10 + 5 + 5).

Zahl der Staubgef.	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	*21*	*22*	*23*	*24*	*25*	*26*	*27*	*28*	*29*	*30*	*31*	
Frequenz bei *Cr. oxyacantha*	1		3	2	8	24	46	60	131	174	**242**	102	54	37	12	4	6						908
Frequenz bei *Cr. monogyna*				2	4	21	49	83	174	242	**340**	121	37	12	6	1							1092

Die Einzelkreise für sich würden auch hier reine Fibonaccicurven geben.

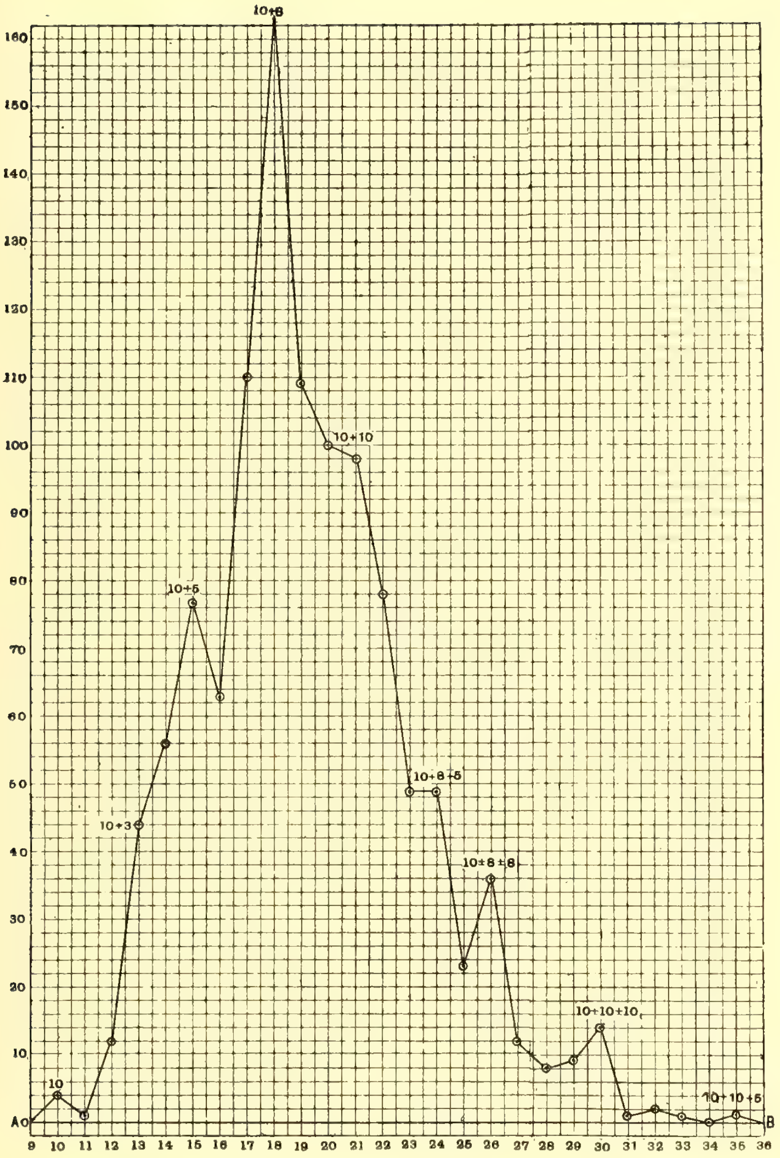


Fig. 5. Zahl der Staubgefässe von *Prunus spinosa*.

DATA FOR THE PROBLEM OF EVOLUTION IN MAN.

ANTHROPOMETRIC DATA FROM AUSTRALIA.

By A. O. POWYS*.

A. *Fertility of Man and Woman in relation to Age.*

To render the problem a definite one the influence of the number of earlier births is practically removed by considering only the *first* births, i.e. those within 12 months of marriage. Table I. gives such *first* births per 100 wives in quinquennial age-groups from the experience of New South Wales during the six years 1893–8. The second column shows the mean annual number of marriages contracted by women in the age-groups indicated in the first column. Kőrösi, the well-known statistician of Budapest, in a paper read before the Royal Society (*Phil. Trans.*, B, Vol. 186, Part II. pp. 792 *et seq.*), gave a somewhat similar table, in which, however, the probability of birth was shown to be higher the younger the wife. But it must be borne in mind that most of the marriages contracted under the age of 20 are compulsory, i.e. were contracted after conception had followed illicit intercourse. It is therefore only correct that the number of such ante-nuptial conceptions should be withdrawn from both the marriages and the resultant births. The third column shows the number of such ante-nuptial conceptions. Births occurring within 9 months of marriage have been so regarded. It is true that there are some legitimate 7 months children, but the births at that month show no inordinate dimensions, as the following figures indicate. Six years 1893–8: Under 2 months of marriage, 1334 cases; 2—3 months, 1204; 3—4 months, 1577; 4—5 months, 1657; 5—6 months, 1840; 6—7 months, 1942; 7—8 months, 1949; and 8 to 9 months, 1863.

---

* The following data—tables, calculated constants and diagrams—were freely placed at my disposal by Mr A. O. Powys of the Government Statist's Office, Melbourne. I have added but little to his own notes upon them. K. P.

TABLE I.

Births per 100 *Wives in Age Groups. Experience of New South Wales. Mean Annual Numbers for six years* 1893–8.

Age Group	Mean Annual Marriages	Ante-nuptial Conceptions	Unprejudiced Marriages	Mean Annual Births from 9 to 12 months after Marriage	Births per cent. of unprejudiced Marriages
Under 20	1400	568	832	208	25·0
20 and under 25	3719	1150	2569	995	38·7
25 „ 30	1869	366	1503	596	39·6
30 „ 35	622	98	524	160	30·5
35 „ 40	304	35	269	43	16·0
40 „ 45	133	7·5	125·5	5·2	4·1

The fourth column shows the difference between the second and third columns, leaving what we may term "unprejudiced" marriages. The fifth column shows the births following such unprejudiced marriages within the period 9 to 12 months after marriage, whilst the sixth column gives the percentage ratios between columns four and five. On reference to the diagrams it will be seen that the range extends from 14·5 years to about 45·8 years, with the maximum at about 24·5 years.

The actual curve determined by Mr Powys is

$$y = 40{\cdot}92\left(1 + \frac{x}{2{\cdot}0094}\right)^{\cdot 7746}\left(1 - \frac{x}{4{\cdot}2670}\right)^{1\cdot 6450},$$

with the mean at 27·00 years and mode at 24·49; the unit of x is five years.

As the five year unit of grouping is rather large Dr Alice Lee again worked out the constants of the curve, using Mr W. F. Sheppard's corrections* for the moments. The curve was now found to be

$$y = 41{\cdot}504\left(1 + \frac{x}{1{\cdot}6176}\right)^{\cdot 4893}\left(1 - \frac{x}{4{\cdot}1128}\right)^{1\cdot 2439},$$

with the mean at 27·048 years, the mode at 23·707, and the range from 15·62 to 44·27 years. It may be doubted whether this determination is as good as the previous one, for the curve rises *vertically* at the left-hand terminal of the range, and thus Mr W. F. Sheppard's corrections are shown by the resulting curve to be inapplicable to this case.

Table II. gives the like results for a special tracing from the Register of Victoria of the births resulting from the marriages contracted during the year 1897. On comparing the resulting curve with that for New South Wales it will be seen that the latter is higher in the earlier ages and lower in the later years than the former.

---

* *Lond. Math. Soc. Proc.*, Vol. XXIX., p. 369.

TABLE II.

Births per 100 *Wives in Age Groups. Experience of Victoria from Marriages of* 1897.

Age Group	Mean Annual Marriages	Ante-nuptial Conceptions	Unprejudiced Marriages	Mean Annual Births from 9 to 12 months after Marriage	Births per cent. of unprejudiced Marriages
Under 20	767	338	429	93	21·67
20 and under 25	3122	898	2224	732	32·91
25 „ 30	2235	437	1798	740	41·16
30 „ 35	775	144	631	238	37·72
35 „ 40	349	45	304	60	19·73
40 „ 45	130	16	114	9	7·89

Its equation, the unit of x being five years, is

$$y = 40{\cdot}73\left(1 + \frac{x}{2{\cdot}7140}\right)^{1{\cdot}1373}\left(1 - \frac{x}{3{\cdot}9254}\right)^{1{\cdot}6449},$$

with the mean at 28·26 years and the mode at 27·00 years. The range is from 13·43 to 46·63 years.

Dr Alice Lee, using Mr Sheppard's corrections, found

$$y = 41{\cdot}06\left(1 + \frac{x}{2{\cdot}4113}\right)^{{\cdot}8365}\left(1 - \frac{x}{3{\cdot}6969}\right)^{1{\cdot}2826},$$

with the mean at 28·264 years and the mode at 26·703 years. The range is from 14·65 to 45·19 years.

Here again the resulting form of the curve suggests that Sheppard's corrections are unsuitable, and probably the former equation is the better*.

Now whether Mr Powys' or Dr Lee's curves are used we see very clearly:

(*a*) that the fertility of women begins earlier in Victoria and ends later, but

(*b*) that the maximum fertility in New South Wales is reached almost three years earlier than it is in Victoria, say 24 years as compared with 27.

The more rapid development of women in New South Wales is undoubtedly due to its much warmer climate. Nearly one-half of the population of each colony is concentrated in their capital cities; the mean annual temperature of Sydney being 63° F., and that of Melbourne being 57° F.

We should expect the Victorian fertility to end later under these circumstances; that it also begins earlier is noteworthy. The year's advance on New South Wales may not really be a sensible difference; it may result from the large

---

* Clearly there is not in any of these curves really *high* contact at either end of the range, and the discovery of suitable corrections for the moments in such cases is one we would urge upon any unemployed mathematician. K. P.

number of ante-nuptial conceptions in the case of the younger brides which have been excluded, or from the difficulty we have referred to about finding the true value of the moments in such large groupings.

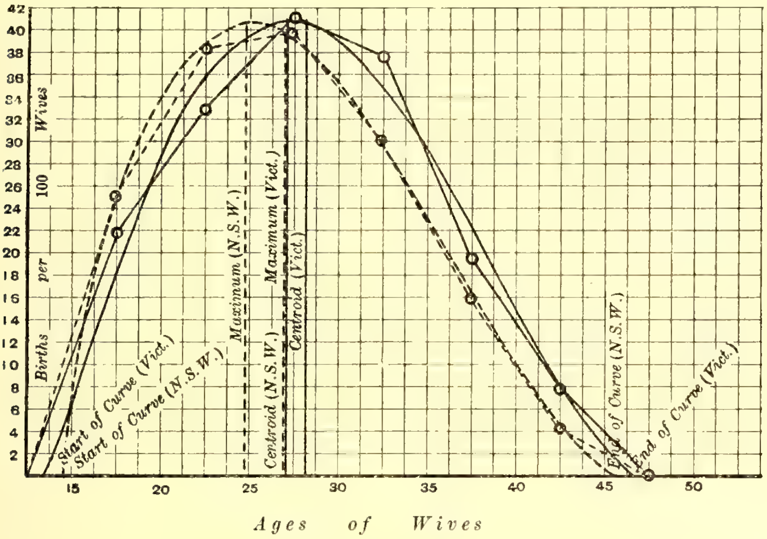


Fig. 1. *Births within* 12 *months of Marriage* (*Wives*). *Victorian Curve, firm lines:*

$$y = 40{\cdot}73\left(1+\frac{x}{2{\cdot}7140}\right)^{1{\cdot}1373}\left(1-\frac{x}{3{\cdot}9254}\right)^{1{\cdot}6449}.$$

Mean: 28·26 years. Mode (origin): 27·00 years. Unit of $x=5$ years.

New South Wales Curve, broken lines:

$$y = 40{\cdot}92\left(1+\frac{x}{2{\cdot}0094}\right)^{{\cdot}7746}\left(1-\frac{x}{4{\cdot}2670}\right)^{1{\cdot}6450}.$$

Mean: 27·00 years. Mode (origin) 24·49 years. Unit of $x=5$ years.

But the problem deserves further consideration. We are accustomed to consider that sexual ripeness is accelerated in warmer climates. Does this mean that the age of *initial* fertility is lowered or that the age of *maximum* fertility is lowered? The one is not unlikely to be confused with the other, if an appeal be made to 'general' observation, and not as in this case to actual statistics.

Table III. gives the like results for first births of the wives in the case of husbands based on the Victorian experience for 1897.

TABLE III.

Births per 100 *Husbands in Age Groups. Experience of Victoria* 1897.

Age Group	Mean Annual Marriages	Ante-nuptial Conceptions	Unprejudiced Marriages	Mean Annual Births from 9 to 12 months after Marriage	Births per cent. of unprejudiced Marriages
Under 20	55	29	26	2	7·70
20 and under 25	1708	591	1117	257	23·00
25 ,, 30	2751	702	2049	743	36·25
30 ,, 35	1637	334	1303	565	43·35
35 ,, 40	717	149	568	191	33·63
40 ,, 45	266	43	223	69	30·94
45 ,, 50	132	16	116	26	22·41
50 ,, 55	95	13	82	10	12·19
55 ,, 60	70	0	70	7	10·00
60 ,, 65	56	2	54	1	1·85
65 ,, 70	33	1	32	1	3·12
70 ,, 75	19	1	18	0	0·00

The equation to the curve, the unit of x being five years, is

$$y = 39\cdot75\left(1+\frac{x}{3\cdot6123}\right)^{1\cdot6321}\left(1-\frac{x}{11\cdot0331}\right)^{4\cdot3857},$$

with the mean at 36·71 years and the mode at 32·41. The range is from 14·34 to 87·57 years.

Unfortunately we have not the New South Wales experience for comparison, but we can hardly doubt that the age of maximum fertility would be somewhat younger.

An examination of Figures 1 and 2 will show how very satisfactorily these curves of Type I.* express the biometric data thus found.

They show quite clearly that at any rate for the Anglo-Saxon race the view of Kőrösi that the fertility is greater the younger the wife is not correct. The woman reaches her greatest reproductive vigour between 24·5 and 27 years, according to the climate, and the man at about 32 years. As we shall see under Section C these appear also to be the ages in the two sexes of maximum physique.

B. *On the Correlation between the Duration of Life and the Number of Offspring.*

The importance of this correlation as a fundamental factor in the quantitative theory of evolution has already been insisted upon†. Another determination of

---

* *Phil. Trans.*, Vol. 186 A, p. 367.

† Data for the Problem of Evolution in Man, V. On the correlation between the Duration of Life and the Number of Offspring. Beeton, Yule and Pearson, *R. S. Proc.* Vol. LXVII. p. 159.

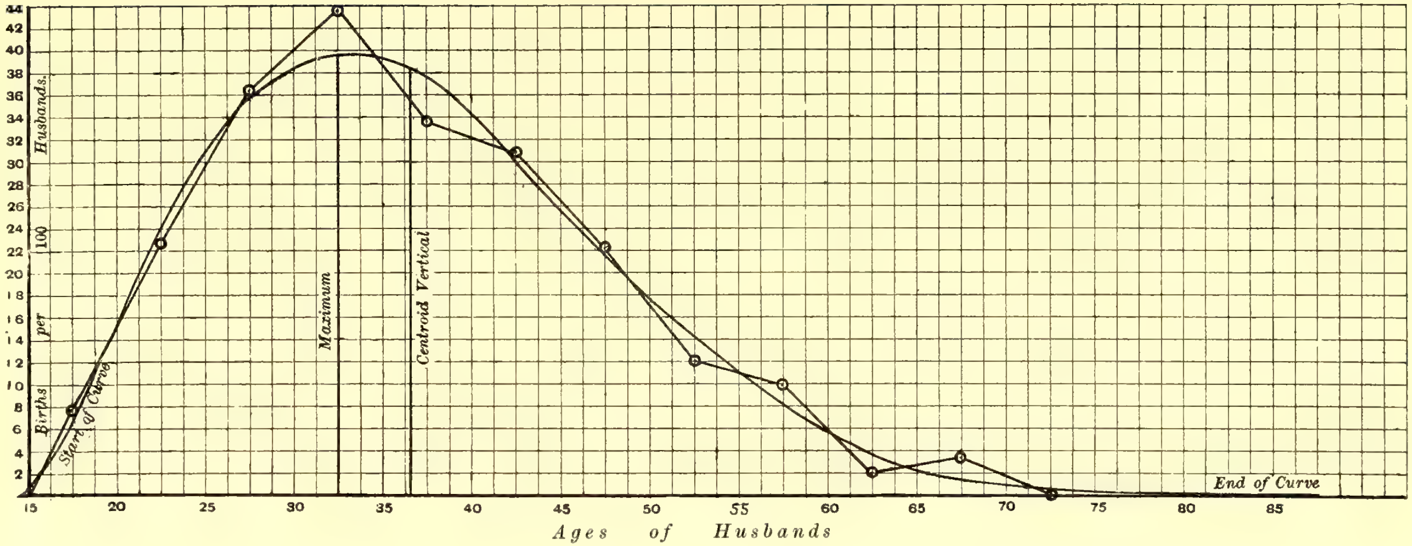


Fig. 2. *Births within 12 months of Marriage (Husbands). Victorian Curve.*

$$y = 39{\cdot}75\left(1+\frac{x}{3{\cdot}6123}\right)^{1{\cdot}6321}\left(1-\frac{x}{11{\cdot}0331}\right)^{4{\cdot}9857}.$$

Mean: 36·71 years. Mode (origin): 32·41 years. Unit of $x=5$ years.

it has thus considerable interest. Fig. 3 shows the general agreement between New South Wales experience and the statistics for English mothers of Beeton and Pearson[*]. The Australian statistics are given in Table IV. below.

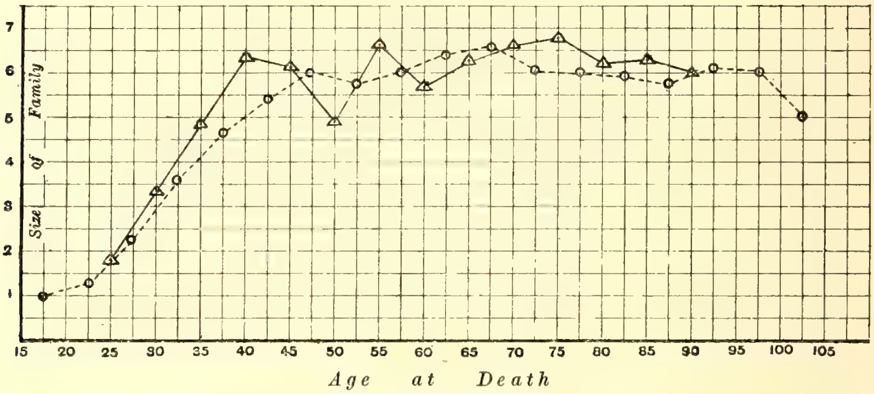


FIG. 3. *Comparison of English and New South Wales Experience as to Number of Offspring and Length of Life of Mother.*

Pearson, firm lines. Powys, broken lines.

TABLE IV.

Ages of Mothers at Death and Number of Children (Living and Dead). Experience of New South Wales 1898 *and* 1899.

Age	Deaths of Mothers			Number of Children			Average number of children to each mother
	1898	1899	Total	1898	1899	Total	
Under 20	28	23	51	26	24	50	0·98
20 and under 25	121	121	242	149	149	298	1·23
25 ,, 30	216	175	391	520	384	904	2·31
30 ,, 35	259	243	502	915	904	1819	3·62
35 ,, 40	259	270	529	1289	1265	2554	4·83
40 ,, 45	199	218	417	1094	1189	2283	5·48
45 ,, 50	201	200	401	1295	1124	2419	6·03
50 ,, 55	192	198	390	1077	1182	2259	5·79
55 ,, 60	229	242	471	1315	1536	2851	6·05
60 ,, 65	249	258	507	1659	1619	3278	6·47
65 ,, 70	268	281	549	1742	1883	3625	6·60
70 ,, 75	225	249	474	1339	1516	2855	6·02
75 ,, 80	258	243	501	1550	1462	3012	6·01
80 ,, 85	172	175	347	1109	966	2075	5·98
85 ,, 90	93	102	195	564	554	1118	5·73
90 ,, 95	25	24	49	160	142	302	6·16
95 ,, 100	12	10	22	60	72	132	6·00
100 and over	1	1	2	8	2	10	5·00
Totals:	3007	3033	6040	15,871	15,973	31,844	

---

[*] *R. S. Proc.* Vol. LXVII. p. 162 and diagram p. 178.

Mr Powys' statistics have been fitted with a cubical parabola in Figure 4[*]. Its equation is

$$y = 6\cdot165{,}263 + 0\cdot160{,}401x - 0\cdot047{,}676x^2 + 0\cdot002{,}788x^3,$$

the origin being at 57·5 years and the unit of x being five years. In fitting a range of 85 years, i.e. 15 to 100 was taken, omitting the data for 100—105 as too few to give a trustworthy average. The corresponding curve for English mothers is

$$y = 6\cdot4092 + 0\cdot079{,}120x - 0\cdot052{,}719x^2 + 0\cdot005{,}717x^3$$

with the same origin and unit for x.

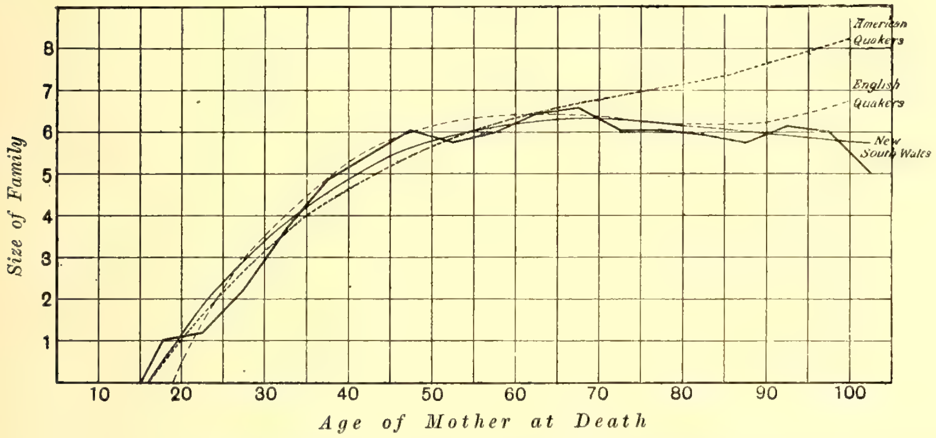


FIG. 4. *Theoretical Curves for Duration of Life and Number of Offspring. Great Britain, America, New South Wales. With New South Wales Experience.*

In Figure 4 the theoretical curves for English, American and Australian mothers are plotted on the New South Wales observations, and the comparison is of considerable interest. The English Quakers, it has been noted, show a tendency to have lessened offspring with parents living to great ages, and this tendency is even more marked in the case of New South Wales. Although the extreme tails, 90—100 years, of all three curves are of small value, yet there certainly appears to be a difference between the American on the one hand and the English and Australian mothers on the other. We are now more justified than we were in June 1900[†], in asserting that extreme longevity is not in all cases favourable to a maximum reproductivity. But our warning against rushing to hasty conclusions is confirmed, for a new community like New South Wales with plenty of food and occupation for all is seen to resemble an old community like England in this relationship, and not a new community like America. Under certain conditions we thus see that Weismann's views may be correct: women living to 60—70 may leave more offspring than either those who die younger or those who die older.

---

[*] I am responsible alone for this bit of work. K. P.

[†] *Loc. cit.* p. 166.

In other words, reproductive selection may tend to preserve the threescore years and ten set to the duration of man's life by the Psalmist. But the American statistics show that this tendency is not universal, and further, whether it exists or not, does not seem to depend on the relative density of population and the character of the environment.

C. *On the Stature of Man between the Ages of* 15 *and* 85.

Tables VI. and VII. give the heights of male and female criminals, with their ages based upon New South Wales experience. The criminals from 20 years upwards are for the years 1890–9, and under 20 years for the years 1895–9, each year's record being for *distinct* persons. Mr Powys writes: "I can imagine no solid reason why the heights of criminals should differ in any material way from those of the ordinary population, and the figures shown can therefore be regarded as typical of the latter."

In Australia probably a majority of the population live an outdoor life with plenty of food and exercise, and the differentiation of the criminal from other classes may be small. In Great Britain there is undoubtedly in strength and stature a considerable class differentiation. The upper middle classes are distinctly taller than the labouring classes, and in particular than the town handworkers[*]. The criminal here is probably of inferior physique, not because he is a criminal, but because in the bulk he is drawn from a differentiated class[†]. The mean stature of 1077 Cambridge undergraduates is 68″·9, and of 811 persons measured in Mr Galton's South Kensington Laboratory in 1884, 67″·9. The mean stature of the New South Wales male criminals is 67″·3, taking ages 25 to 30 for comparison. In the case of the women we have Cambridge 63″·8, South Kensington 63″·3, and New South Wales not exceeding 62″·8. It seems improbable that the general population of New South Wales is shorter than that of England; hence we shall be justified in believing that to some extent the New South Wales criminals are drawn from a class or classes less fully developed, owing to want of nourishment or to general environment, than the population at large[‡]. But looking at the curves for the *male* criminals grouped in either 5 or 10 year periods we see that the material is extremely homogeneous. In every case the curve is of Type IV.[§], in every case the mode is nearly identical with the mean, and in every case the fit is extremely good. For the males we should have got very good results by simply using the normal curve, and thus we have

---

[*] See Pearson: *The Chances of Death*, Vol. I. pp. 310—315.

[†] The school of Lombroso has jumped without a satisfactory *statistical* investigation at the existence of a definite 'criminal' type. Further light on the matter will be found in Dr W. R. Macdonell's paper on Criminal Anthropometry which will be shortly published in *Biometrika*. K. P.

[‡] For 1000 British middle-class from my own measurements (without boots) I find 68″·04 for males and 62″·95 for females, both sensibly taller than the New South Wales criminals. K. P.

[§] *Phil. Trans.*, Vol. 186 A, p. 376.

a confirmation of the result several times noted that the frequency of statures in a homogeneous group gives very nearly a normal distribution.

With the females the case is somewhat different; the observed distributions are much more irregular. This is a very common experience in statistics for this sex. It is probably to some extent due to greater difficulty in ascertaining the real age of women, and again to a very likely differentiation in stature between women who have borne and those who have not borne children. But admitting the irregularity, we see again that the mode and the mean are in sensible agreement. The distributions are closely normal. In all cases but one (ages 50 and under 60) the curves diverge from normality in the direction of the curve of Type IV. But in this one case of divergence, the curve, which is of Type II., is within the limits of the probable error of random selection a normal curve. It would seem reasonable to conclude that the stature distribution of these homogeneous groups is nearly normal, but that what divergence there is lies in the direction of curves of Type IV.

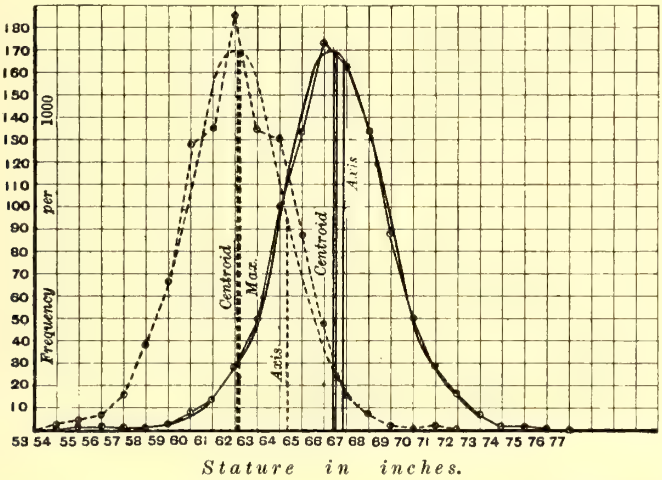


Fig. 5. *Statures per* 1000, *Male and Female, Ages* 20—25. *Male Curve, firm lines:*

$$y = \frac{166{\cdot}7100}{\left\{1+\left(\frac{x}{9{\cdot}9138}\right)^2\right\}^{9{\cdot}5378}} e^{-{\cdot}6617 \tan^{-1}\frac{x}{9{\cdot}9138}}.$$

Mean at 66″·95.
Mode at 66″·99.
Unit of $x=1''$.

Origin at 67″·33.
Modal Frequency =168·6.

Female Curve, broken lines:

$$y = \frac{96{\cdot}9970}{\left\{1+\left(\frac{x}{21{\cdot}5568}\right)^2\right\}^{43{\cdot}5885}} e^{-9{\cdot}9124 \tan^{-1}\frac{x}{21{\cdot}5568}}.$$

Mean at 62″·60.
Mode at 62″·66.
Unit of $x=1''$.

Origin at 65″·11.
Modal Frequency =170·07.

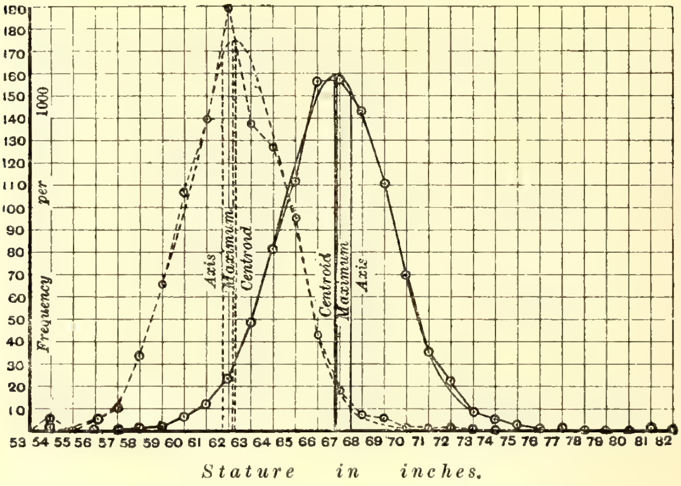


FIG. 6. *Statures per* 1000, *Male and Female, Ages* 25—30. *Male Curve, firm lines:*

$$y = \frac{152\cdot4356}{\left\{1+\left(\frac{x}{10\cdot5901}\right)^2\right\}^{10\cdot1018}} e^{-1\cdot5405\tan^{-1}\frac{x}{10\cdot5901}}.$$

Mean at 67″·30.	Origin at 68″·21.
Mode at 67″·40.	Modal Frequency $=161\cdot5$.
Unit of $x=1''$.	

Female Curve, broken lines:

$$y = \frac{168\cdot71}{\left\{1+\left(\frac{x}{8\cdot4961}\right)^2\right\}^{7\cdot6360}} e^{\cdot9670\tan^{-1}\frac{x}{8\cdot4961}}.$$

Mean at 62″·76.	Origin at 62″·14.
Mode at 62″·68.	Modal Frequency $=173\cdot8$.
Unit of $x=1''$.	

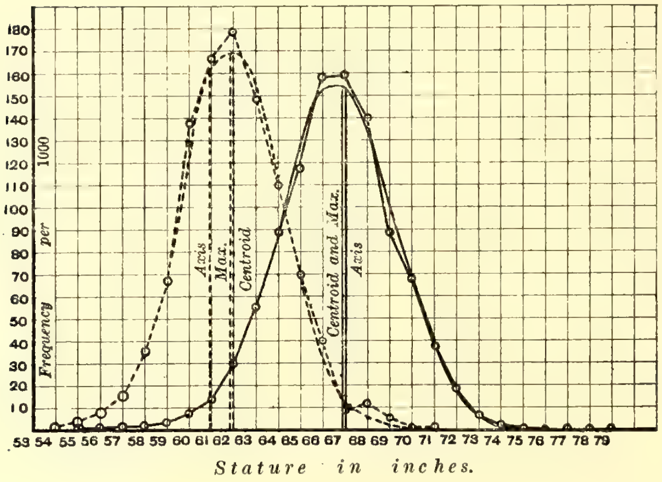


FIG. 7. *Statures per* 1000, *Male and Female, Ages* 30—40. *Male Curve, firm lines:*

$$y = \frac{154\cdot8557}{\left\{1+\left(\frac{x}{18\cdot6561}\right)^2\right\}^{27\cdot5221}}\, e^{-1\cdot0610\tan^{-1}\frac{x}{18\cdot6561}}.$$

Mean at 67″·15.	Origin at 67″·52.
Mode at 67″·16.	Modal Frequency = 156·4.
Unit of $x = 1''$.	

Female Curve, broken lines:

$$y = \frac{161\cdot15}{\left\{1+\left(\frac{x}{10\cdot9776}\right)^2\right\}^{12\cdot9717}}\, e^{2\cdot3324\tan^{-1}\frac{x}{10\cdot9776}}.$$

Mean at 62″·44.	Origin at 61″·37.
Mode at 62″·36.	Modal Frequency = 178·9.
Unit of $x = 1''$.	

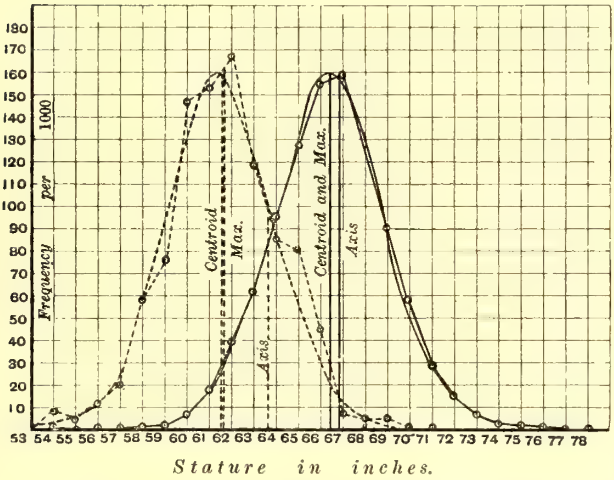


FIG. 8. *Statures per* 1000, *Male and Female, Ages* 40—50. *Male Curve, firm lines:*

$$y = \frac{157\cdot20}{\left\{1+\left(\frac{x}{10\cdot4170}\right)^2\right\}^{9\cdot4312}}\, e^{-\cdot7357\tan^{-1}\frac{x}{10\cdot4170}}.$$

Mean at 66″·91.	Origin at 67″·37.
Mode at 66″·96.	Modal Frequency = 159·16.
Unit of $x = 1''$.	

Female Curve, broken lines:

$$y = \frac{113\cdot67}{\left\{1+\left(\frac{x}{15\cdot7508}\right)^2\right\}^{20\cdot8386}}\, e^{-5\cdot3107\tan^{-1}\frac{x}{15\cdot7508}}.$$

Mean at 62″·12.	Origin at 64″·23.
Mode at 62″·22.	Modal Frequency = 159·29.
Unit of $x = 1''$.	

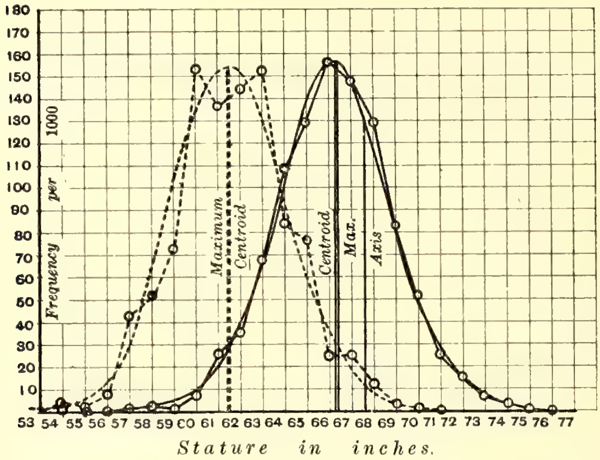


FIG. 9. *Statures per* 1000, *Male and Female, Ages* 50—60. *Male Curve, firm lines:*

$$y=\frac{127\cdot74}{\left\{1+\left(\frac{x}{16\cdot4710}\right)^2\right\}^{21\cdot2579}}e^{-4\cdot0223\tan^{-1}\frac{x}{16\cdot4710}}.$$

Mean at 66″·74. Origin at 68″·38.
Mode at 66″·82. Modal Frequency = 154·36.
Unit of $x=1''$.

Female Curve, broken lines:

$$y=153\cdot32\left(1+\frac{x}{19\cdot8943}\right)^{38\cdot6436}\left(1-\frac{x}{40\cdot2965}\right)^{78\cdot2814}.$$

Mean at 62″·22. Origin at 62″·05.
Mode at 62″·05. Modal Frequency = 153·32.
Unit of $x=1''$.

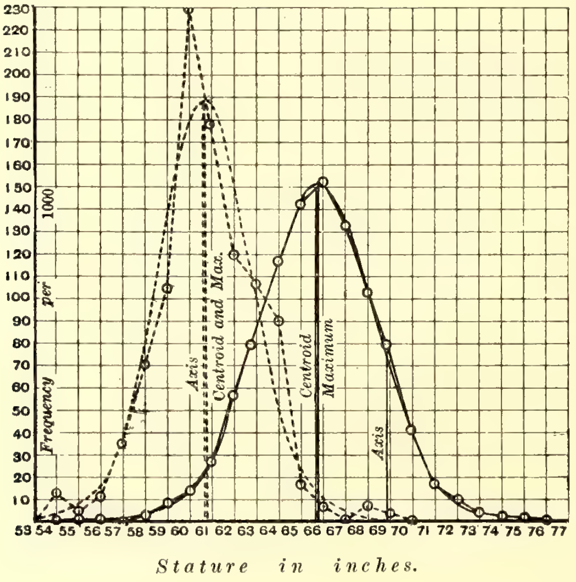


FIG. 10. *Statures per* 1000, *Male and Female, Age* 60 *and over. Male Curve, firm lines:*

$$y = \frac{71\cdot00}{\left\{1+\left(\frac{x}{18\cdot2028}\right)^2\right\}^{25\cdot5423}}\, e^{-8\cdot8304 \tan^{-1} \frac{x}{18\cdot2028}}.$$

Mean at 66″·26. Origin at 69″·54.
Mode at 66″·39. Modal Frequency $=151\cdot86$.
Unit of $x=1''$.

Female Curve, broken lines:

$$y = \frac{188\cdot43}{\left\{1+\left(\frac{x}{6\cdot6357}\right)^2\right\}^{5\cdot6618}}\, e^{\cdot1179 \tan^{-1} \frac{x}{6\cdot6357}}.$$

Mean at 61″·31. Origin at 61″·23.
Mode at 61″·30. Modal Frequency $=188\cdot57$.
Unit of $x=1''$.

The following Table gives the means, modes, standard deviations, and skewnesses of the various age groups*.

TABLE V.

Age Group	MEAN		MODE		STANDARD DEVIATION		SKEWNESS	
	Male	Female	Male	Female	Male	Female	Male	Female
20—25	66″·95	62″·60	66″·99	62″·66	2″·4745	2″·3654	+·0163	+·0243
25—30	67″·30	62″·76	67″·40	62″·68	2″·5624	2″·4317	+·0346	−·0333
30—40	67″·15	62″·44	67″·16	62″·36	2″·5866	2″·3027	+·0052	−·0358
40—50	66″·91	62″·12	66″·96	62″·22	2″·6181	2″·5552	+·0184	+·0396
50—60	66″·74	62″·22	66″·82	62″·05	2″·6337	2″·5911	+·0292	−·0172
60 and over	66″·26	61″·31	66″·39	61″·30	2″·6818	2″·3001	+·0478	−·0644

It will be seen that in every group the mean male stature is *less* than the modal stature, while in every female group, except two, the mean stature is *greater* than the modal stature. In the case of these two exceptions, the first is the youngest group, and the second, Ages 40—50, contains a very anomalous set of 14 dwarfs. The skewness of the distribution is in all cases small, and a normal curve would be, as we have indicated, a fair fit. The standard deviations are interesting; in the case of the man they increase with age, or it would appear that old men form a more variable group than young men. This is possibly due to the

---

* I am responsible for some of the calculations involved in the determination of the constants of this Table, and for the introduction of the following table. K. P.

rate of decadence largely varying with the individual. At any rate, whatever be its source, it is very important in its bearing on all attempts to estimate the intensity of selection, even in the case of other animals than man. We must be careful that there is not an age or growth factor at work, counterbalancing a reduction in variability due to selection. The female groups are more irregular in their results than the male; in particular the last group for women over 60 contains only 282 entries. Still the general tendency is the same—increased variability with old age.

Judging by standard deviations man would appear more variable than woman in stature. If we consider all the groups of equal weight and take the mean we have 66″·885 for the mean stature of men, and 62″·242 for women, the standard deviations being respectively 2″·5928 and 2″·4244. Judged by percentage variation we have for the coefficient of variation in man 3·88, and in woman 3·89, results sensibly equal.

The following Table gives relative experience, the agreement in variability is wonderfully close.

COUNTRY	GREAT BRITAIN				NEW SOUTH WALES	
Class	Upper Class *		Middle Class †		Criminal Classes	
Sex	Male	Female	Male	Female	Male	Female
Mean	68″·93	63″·82	67″·90	63″·30	66″·88	62″·24
Standard Deviation ...	2″·51	2″·42	2″·55	2″·40	2″·59	2″·42
Coefficient of Variation	3·6	3·8	3·7	3·8	3·9	3·9

---

* Cambridge Undergraduates: see Pearson, *The Chances of Death*, Vol. I. p. 313.

† Visitors to International Exhibition in 1884 measured in Mr Galton's South Kensington Laboratory: see Galton, *Natural Inheritance*, p. 200, and Pearson, *loc. cit.* p. 311.

The British measurements practically include nobody over 51 years of age.

TABLE VI.

Heights and Ages of New South Wales Criminals. 7 *years,* 1893—9.
(*Under* 20 *years of age, experience of years* 1895—9.)

Heights	MALES										
	Ages										
	15–16	16–17	17–18	18–19	19–20	20–25	25–30	30–40	40–50	50–60	60 and over
under 4′ 7″	6	4	3	—	—	1	3	2	7	3	
4′ 7″—4′ 8″	1	1	1	—	—	—	3	1	—	—	2
4′ 8″—4′ 9″	6	—	2	—	—	2	1	2	2	—	2
4′ 9″—4′ 10″	5	5	3	2	1	3	2	3	1	2	1
4′ 10″—4′ 11″	15	9	4	3	2	—	9	6	12	7	3
4′ 11″—5′ 0″	13	13	10	3	2	12	25	27	16	8	25
5′ 0″—5′ 1″	19	34	18	14	8	42	70	99	52	35	44
5′ 1″—5′ 2″	13	32	22	14	14	75	164	209	156	117	80
5′ 2″—5′ 3″	21	43	45	51	32	158	323	462	363	157	164
5′ 3″—5′ 4″	20	48	74	81	66	275	633	852	567	301	230
5′ 4″—5′ 5″	10	53	98	113	112	551	1056	1335	867	481	338
5′ 5″—5′ 6″	13	44	88	99	102	725	1427	1793	1154	569	411
5′ 6″—5′ 7″	17	40	98	129	134	939	2001	2398	1391	682	441
5′ 7″—5′ 8″	6	22	52	100	132	882	2019	2406	1431	655	384
5′ 8″—5′ 9″	3	8	41	55	84	734	1826	2103	1153	568	297
5′ 9″—5′ 10″	1	7	18	30	45	482	1410	1351	807	363	228
5′ 10″—5′ 11″		4	5	18	22	281	876	1027	520	232	123
5′ 11″—6′ 0″		2	5	10	22	151	455	568	263	121	49
6′ 0″—6′ 1″		2	5	5	3	82	287	300	142	65	25
6′ 1″—6′ 2″		1	1	2	5	35	105	122	61	30	11
6′ 2″—6′ 3″					1	8	37	33	27	10	2
6′ 3″—6′ 4″					—	3	11	11	8	1	2
6′ 4″—6′ 5″					1	1	—	5	1		
6′ 5″—6′ 6″							2	1	2		
6′ 6″—6′ 7″							1	1			
6′ 7″—6′ 8″											
6′ 8″—6′ 9″							1				
Totals	169	372	593	729	788	5442	12,747	15,117	9003	4407	2862

TABLE VII.

Heights and Ages of New South Wales Criminals. 7 *years,* 1893—9.
(*Under* 20 *years of age, experience of years* 1895—9.)

Heights	FEMALES										
	Ages										
	15–16	16–17	17–18	18–19	19–20	20–25	25–30	30–40	40–50	50–60	60 and over
under 4′ 7″	1	—	1	—	—	2	6	4	14	2	4
4′ 7″—4′ 8″	—	1	—	1	—	4	1	4	9	1	1
4′ 8″—4′ 9″	—	—	—	1	1	6	10	19	23	5	3
4′ 9″—4′ 10″	—	—	2	—	3	19	18	41	35	28	10
4′ 10″—4′ 11″	2	4	5	4	11	47	57	93	99	35	20
4′ 11″—5′ 0″	2	4	3	13	13	85	113	180	130	49	30
5′ 0″—5′ 1″	—	4	4	19	19	163	187	366	250	101	65
5′ 1″—5′ 2″	1	4	8	19	25	173	239	442	259	90	50
5′ 2″—5′ 3″	1	5	18	20	25	235	326	476	282	96	34
5′ 3″—5′ 4″		3	9	16	24	173	236	399	201	101	30
5′ 4″—5′ 5″		3	2	14	11	165	217	298	143	56	25
5′ 5″—5′ 6″		—	1	5	7	112	165	185	136	51	5
5′ 6″—5′ 7″		—	1	2	2	59	74	102	75	16	2
5′ 7″—5′ 8″		—	3	—	2	18	33	22	14	16	—
5′ 8″—5′ 9″		1	—	—	—	9	12	27	7	9	2
5′ 9″—5′ 10″						3	10	8	7	2	1
5′ 10″—5′ 11″						—	4	3	1	1	
5′ 11″—6′ 0″						1	1				
6′ 0″—6′ 1″							1				
6′ 1″—6′ 2″							1				
Totals	7	29	57	114	143	1274	1711	2669	1685	659	282

D. *On the Alteration of Stature with Old Age.*

This is an exceedingly interesting point, as the influence of age in the adult has to be allowed for in so many anthropometric investigations. Table VIII. gives the criminal experience of New South Wales for the years 1895-9, and the resulting curves are plotted and smoothed in Fig. 11.

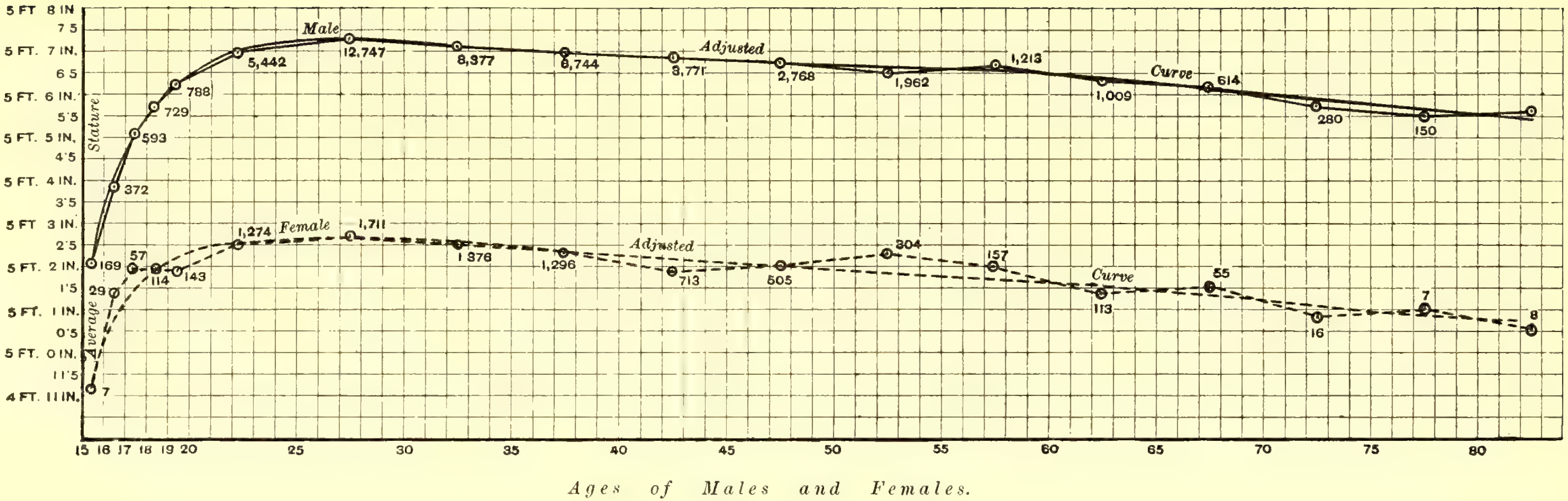


FIG. 11. *Change of Stature with Age.*

TABLE VIII.

Mean Height at various Ages of Males and Females. New South Wales Experience of 5 *years*, 1895—9.

Ages	Mean Height		Ages	Mean Height	
	Males	Females		Males	Females
15—16	5 ft. 2·12 in.	4 ft. 11·21 in.	50 and under 55	5 ft. 6·58 in.	5 ft. 2·32 in.
16—17	" 3·83 "	5 ft. 1·40 "	55 " 60	" 6·63 "	" 2·04 "
17—18	" 5·10 "	" 2·01 "	60 " 65	" 6·40 "	" 1·31 "
18—19	" 5·78 "	" 1·99 "	65 " 70	" 6·23 "	" 1·55 "
19—20	" 6·37 "	" 1·93 "	70 " 75	" 5·71 "	" 0·75 "
20 and under 25	" 6·95 "	" 2·60 "	75 " 80	" 5·49 "	" 1·07 "
25 " 30	" 7·30 "	" 2·76 "	80 and over	" 5·67 "	" 0·50 "
30 " 35	" 7·21 "	" 2·53 "			
35 " 40	" 7·07 "	" 2·33 "			
40 " 45	" 6·82 "	" 1·91 "			
45 " 50	" 6·74 "	" 2·05 "			

The observation curves are remarkably smooth, except in the points for women under 19; but for these youthful criminals there are very few cases. We see at once that the man reaches his maximum stature at about 28 years of age and the woman her maximum stature at about 25. The modal fertility of Victorian women is at 27 and of Victorian men at 32. For New South Wales women the modal fertility is 24·4, two to three years less than for Victoria. We have not the data for New South Wales men, but they would probably show a mode of about 29—30 instead of 32. Thus we see that the age of maximum fertility at any rate approaches, if it does not coincide with, the age of most fully developed stature. As Mr Powys remarks, this tendency of maximum stature age to coincide with that of maximum fertility can hardly be fortuitous. It seems probable that in man, as in other types of life, the age of maximum fertility is the age of most fully developed physique. It will be seen that we have here a definite law of fertility, which is *à priori* reasonable as compared with the result reached by Kőrösi (see p. 34 above)*.

If we examine Mr Powys' diagram, especially the adjusted curves, it would at first sight appear possible to fit them with algebraic expressions. The cubic parabolas determined by Dr Lee are:

---

* A similar conclusion is formed by M. V. Turquan: Fécondité comparée de l'homme et de la femme suivant l'âge. *Revue scientifique*, T. 5, p. 8, 1896. He makes a maximum fertility for both men and women at the earliest age; he however attributes it to the fact that, "à cet âge la prévoyance malthusienne...est peu connue ou du moins peu pratiquée chez les jeunes mariés de cet âge" (p. 12). His data exhibit, however, a secondary maximum for the father and mother at a later age and there is little doubt that Mr Powys' procedure,—to subtract antenuptial conception—is correct. K. P.

For men:

$$y = 66{\cdot}98419 - {\cdot}056{,}559x - {\cdot}001{,}458x^2 + {\cdot}000{,}0608x^3,$$

the origin being at 50 years of age, the unit of x a year and y being inches of stature.

For women:

$$y = 62{\cdot}17403 - {\cdot}046{,}309x - {\cdot}000{,}095x^2 + {\cdot}000{,}003x^3,$$

the origin being also at 50 years of age, the unit of x a year and y in inches of stature.

But these will be found to give rather poor fits. To see if anything better could be done by taking a higher parabola one of the fourth order was fitted to the male statistics*. Its equation is

$$y = 66{\cdot}521{,}011 - 1{\cdot}979{,}575\left(\frac{x}{35}\right) + 2{\cdot}814{,}332\left(\frac{x}{35}\right)^2 + 2{\cdot}608{,}370\left(\frac{x}{35}\right)^3 - 5{\cdot}352{,}748\left(\frac{x}{35}\right)^4,$$

with origin at 50 years, the unit of x a year and y in inches of stature.

This gave a fairly good fit from 27 to 85 years, because the curve has both points of inflexion in this part, but again fitted badly from 15 to 27 years. In fact from the period of maximum fertility in either sex to the end of life a straight line gives quite as good a fit as it is possible to find. Either the curves selected must therefore be such that they have an asymptote parallel to this line, or we must express by different algebraic expressions the periods of growth and of decadence. In neither case is a parabola of any order whatever a suitable analytical expression. Mr Powys' data being for criminals do not go back sufficiently into childhood to enable us to get a suitable curve for the growth period†.

Starting from the 25—30 group for both sexes we get the following best fitting straight lines for the period of decadence:

For males: $y = 68{\cdot}33938 - {\cdot}033{,}6664\,X$,
for females: $y = 63{\cdot}61104 - {\cdot}036{,}1345\,X$,

where y is the number of inches of stature at X years of age.

It will be found that these lines give excellent results for all ages from 27 to 85. We accordingly conclude that a man loses on the average ·34″ of stature and a woman ·36″ per ten years after 27 and 25 years of age respectively. Approximately we may take this $\frac{1}{3}$″ per ten years. Hence if the stature of a man or woman be measured in adult life we can ascertain by a simple calculation their probable stature in their prime. It is to this stature that we really ought to refer our measurements as a standard: for example, in comparing the stature of parents and children for the purpose of measuring the intensity of inheritance.

---

* I am responsible for the calculation of the constants of this curve and for those of the straight lines below. K. P.

† I have found fairly good results for the curve of growth of the average child from a logarithmic curve, but my data are at present not sufficient for a good determination. K. P.

ON THE INHERITANCE OF THE DURATION OF LIFE, AND ON THE INTENSITY OF NATURAL SELECTION IN MAN.

By MARY BEETON, Girton College, Cambridge, and KARL PEARSON.

CONTENTS.

(1) In a first study of the Inheritance of Longevity presented to the Royal Society in June, 1899*, Miss Beeton and I confined our attention to the material then available, namely, inheritance in the male line of direct descent and the correlation between the lengths of lives of brothers. In both cases we dealt only with adult lives. The limitation to the male line was enforced upon us partly by the practice of tracing pedigrees only through the male line, partly by the habitual

---

* *R. S. Proc.*, Vol. 65, p. 290.

reticence as to the age of women, even at death, observed by the compilers of peerages and family histories. The limitation as to adult lives arose also from the small interest exhibited by the same compilers as to the exact age at death of infants; indeed members of the family dying as infants often escaped any record at all. The difficulties thus referred to were finally overcome by working solely at the pedigree records of members of the Society of Friends. Through the kindness of Mr Isaac Sharpe, a great mass of material in the form of family histories and other records at the central offices of the Society, Devonshire House, E.C., was placed at Miss Beeton's disposal. Further material of a valuable nature was provided by the Secretary of the Friends' Provident Association, and we have cordially to thank both these gentlemen for their ready assistance and their invariable courtesy. Miss Beeton working solely at the material thus provided was enabled to form fairly extensive tables for the inheritance of longevity in the female line, and also with somewhat less extensive data for the deaths of infants* to form some appreciation of what we have elsewhere termed the inheritance of "brachybioty†." The whole of the material was extracted, tabled, and the constants calculated by Miss Beeton‡, and the work has been one demanding very considerable labour and continued caution. My function has been solely consultative in cases where difficulties in dealing with the raw material or in the calculation of the constants arose.

(2) With regard to the tabulation of the data we had found a five year unit of grouping sufficiently close in the case of adult lives. I do not think any serious error is introduced by grouping at the mid-point of five years all the deaths occurring in a five year period. But the matter is very different when we come to deal with individuals dying as minors; we cannot possibly group them together in five year periods, and if we were to do so, then we could not centre the group at the mid-point of the period. The average age at death of children, for example, dying in the first five years of life is certainly not 2·5 years, it is much less. It may be asked why we did not take the year as the unit of life. The answer is twofold: Had we done so we should, treating all lives together, have had to construct a correlation table with over 10,000 compartments! This is practically impossible on account of the labour involved. In the next place the mortality curve is not simple but compound, and the causes of infantile and adult mortality are largely different,—there is, as we have said, a tendency to brachybioty as well as one to longevity. Thus we have separated our tables in the case of those dying as adults and those dying as minors, using a five year period for the classification of the first and a one or a two year period for the classification of the second§. But here again a further difficulty arises. The actual days of birth and

---

* The whole material available was exhausted, and this must explain why in certain cases we have stopped at less than 1000 cases.

† *R. S. Proc.*, Vol. 65, p. 299.

‡ The whole of the calculations were afterwards corrected or verified by Dr Alice Lee, and we have to thank her most heartily for her aid.

§ Another reason for this division is also to be found in the fact that many infantile deaths are death are very frequently not stated. Either the individual is said to be n years of age at death, or the years of his birth and death are given. In the former case it would be quite reasonable for adults to assume that the average of such individuals was $n + \cdot 5$ years, for a man n years old at death may be of any age from exactly n to just under $n + 1$ years. In the latter case since the individual may be born any time and die any time between Jan. 1 and Dec. 31, his age may lie between $p - 1$ and $p + 1$ years where m is the given year of his birth and $m + p$ of his death, so that the mere subtraction of the birth and death years gives p years for his age, which corresponds to the probable average life of such individuals if adult. As the years of birth and death are almost always given even when the age is, we obtained the simple rule for classifying: subtract the year of birth from that of death and group on the mid-year of the five year period. Thus ages so found, 21, 22, 23, 24 and 25 are grouped at 23, which denotes exactly 23 years of life and not the mid-point of the 23rd year.

With regard to minors with one and two year periods our difficulties were more serious. p years' difference between the birth and death years still meant p years' life and therefore grouping together lives of p and $p + 1$ years, we could centre the group at $p + \cdot 5$ years, so long as we were not dealing with the first year or two of life and their very unequal mortality. It is these early years of life which present difficulties. If those we have recorded as dying in the same year as they are born, or under the heading 0, were just as likely to be born at any time of the year and to die at any later part of it, their mean life in years would be

$$\int_0^1 \int_x^1 (y - x)\, dx dy \bigg/ \int_0^1 \int_x^1 dx dy,$$

where x, y are the periods (in fractions of a year) from January 1 to the days of their birth and death. This gives a result $= \frac{1}{3}$, or the average life would be four months. Mr G. U. Yule kindly considered the problem from the actual statistics provided in the Report of the Registrar-General for 1891. This gives the mortality for each month for the first twelve after birth for the rural counties and three selected towns. Taking the former Mr Yule finds 2·34 months instead of four months for the mean duration of life. As he observes, 2·34 is probably somewhat too large for his material, as undoubtedly more children die towards the beginning than towards the end of the first month. On the other hand the infantile mortality among the Society of Friends is nothing like as great as in the general population. After some consideration we determined to centre the group dying in the first year of life at three months or ·25 years.

Now turning to the group clubbed together as 0—1 in some of our tables, we must ask where it is to be centred. The children recorded as 0 years at death we have seen are to be centred at ·25 years, and those placed under 1 year would be anything from 0 to 2, and accordingly have their mean at 1, if mortality did not

---

undoubtedly not recorded, and accordingly we have not got infant and adult deaths in their proper proportions. As many infants as possible were extracted, only about 1000 adults.

diminish so rapidly during the first two years of life. I find 917 children on the record under the heading 0, and 774 under the heading 1. Hence we have for the centre of the group $\frac{917 \times \cdot 25 + 774 \times 1}{917 + 774} = \cdot 59$. The real value must be something less than this owing to the rapid diminution of mortality in the first and second years of life. We have accordingly selected ·5 as the centre of the group recorded under 0—1, a value which makes the calculations simpler and cannot be very far from the truth. Important as some appreciation of the approximate value of the true centering is, the reader may note that a slight error does not sensibly alter the constants we are investigating. Thus, if we change the centre of the 0—1 group from ·5 to ·64 of a year this, for example in the cases of the correlation in lengths of life of mothers and minor sons, of fathers and infant daughters, only alters the value of the coefficients from ·0756 to ·0761, and from ·0520 to ·0523 respectively. In every case the change is far less than the probable error of the coefficient.

(3) Another point must be referred to here. In forming a correlation table for two perfectly homogeneous groups, e.g. adult sisters and adult sisters, the table was first formed by taking as one entry the elder sister (i.e. the one born first whether she died first or not) and as the other the younger sister. The table was then rendered symmetrical by adding corresponding rows and columns, so that we finally obtained a table giving the correlation between ages at death of a pair of sisters without reference to their relative position in the family. The correlation coefficient was found to be changed by this procedure by an amount less than the probable error. Thus the correlation between the ages at death of pairs of adult sisters in general is ·3322 ± ·0185, while that between elder sisters and younger sisters is ·3464 ± ·0183. In the case of brothers the coefficients are respectively ·2853 ± ·0194 and ·2990 ± ·0194.

But while the correlation remains unchanged by this procedure the means differ very widely. *The elder adult sister and adult brother live on an average four years longer than the younger adult sister or brother.* The actual values based on 1000 or more cases are as follows:

Average Life of Brothers and Sisters.

	All Adult	Elder Adult	Younger Adult
Sister	57·795	59·924	55·667
Brother	56·568	58·560	54·575

Here an elder brother or sister was one who might have been born one year or twenty before the younger brother or sister. This greater expectation of life on the part of the first born was so much beyond our anticipations, and as far as we are aware so little noticed hitherto, that we determined to make an independent investigation of this special point. Accordingly correlation tables were prepared in which one variable was the number of years' interval between the births of the elder and younger, and the second variable the difference in duration of life of elder and younger. Some remarks must be made on our method of procedure here. The greatest number of years' interval between births was found to be 29. The greatest difference of duration of life 98 years, and this might be either plus or minus; thus if the year was used as unit the correlation table would contain 5684 compartments, and would become very unmanageable. When the table was split into three—adults and adults, minors and minors, adults and minors, the last was still very unwieldy. It was thus necessary to use as unit a five year difference in the duration of life. Using one year difference as unit in the birth interval we found for adults and adults:

	Brothers	Sisters
Mean excess in life of elder ...	4·289 yrs.	4·542 yrs.
Standard deviation of excess...	22·0053 yrs.	22·1325 yrs.
Mean interval between births	6·462 yrs.	6·7503 yrs.
Standard deviation of interval	4·3530 yrs.	4·6856 yrs.
Correlation	·1062 ± ·0206	·1201 ± ·0246
Number of pairs	1051	733

The data are not quite the same as for our pairs of adult brothers and sisters given above, but they show much the same advantage, i.e. four years to the elder. They further demonstrate that longevity is correlated with position in the family. This fact is suggestive for the source of other variations in the characters of an array of brethren. It may be that variability within the array is not purely random but correlated, like variability in longevity, with the birth order. Our numbers show that on the whole the earlier born members of a family are the stronger, or at any rate fitted to survive the longer.

The tables of adults and minors proving very unwieldy we determined to draw up one table only for all brothers and all sisters whatever their age at death, and for this purpose Miss Beeton returned to the Friends' material and extracted the dates of birth and death of 1606 pairs of sisters and 2247 pairs of brothers. These were first classified according to 1, 2, 3 ... years' interval between their births, and then each of these classes subdivided into groups in which the character, life of elder less the life of younger, was nearest to 1, 2, 3 ... 90 or $-1, -2, -3 \ldots -90$ years. Cases in which the difference in duration of life was sensibly zero—very few numerically—were equally divided between the $+1$ and -1 groups. Finally each batch of five years was grouped under its mid-year, and thus Tables I. and II. were obtained. In the column A of these tables is given the number of cases in which for each year of interval between births the younger lives longer; in column B the number of cases in which the elder lives longer. It is only in the case of an interval of one or two years between births that A shows a majority over B. An examination of these columns demonstrates at once the great advantage of priority

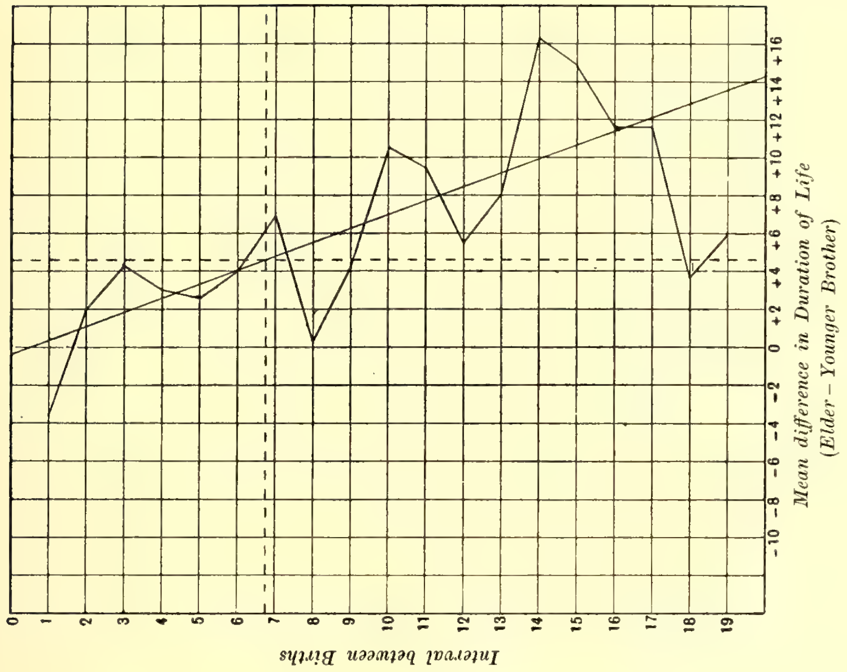

-10 -8 -6 -4 -2 0 +2 +4 +6 +8 +10 +12 +14 +16
Mean difference in Duration of Life
(Elder – Younger Brother)
0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19
Interval between Births

FIG. 2.

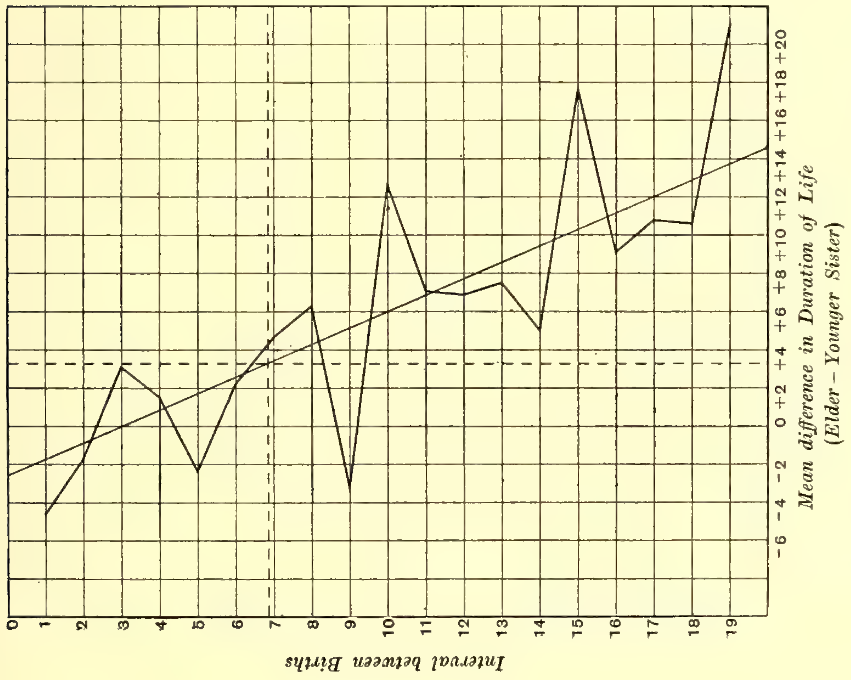

-6 -4 -2 0 +2 +4 +6 +8 +10 +12 +14 +16 +18 +20
Mean difference in Duration of Life
(Elder – Younger Sister)
0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19
Interval between Births

FIG. 1.

of birth as far as longevity is concerned. Out of 1606 pairs of sisters the elder lives longer in 922·5 cases; out of 2247 pairs of brothers the elder lives longer in 1303·5 cases. Or, in 57·4 per cent. of cases the elder sister, in 58·0 per cent. of cases the elder brother lives longer. Summing up our results as for the case of adults we have for all brethren of whatever age at death:

	Brothers	Sisters
Mean excess in life of elder ...	4·612 yrs.	3·232 yrs.
Standard deviation of excess...	30·2209 yrs.	29·7849 yrs.
Mean interval between births	6·717 yrs.	6·8400 yrs.
Correlation	·1096 ± ·0141	·1352 ± ·0165
Number of pairs	1606	2247

Now it will be seen that there is no sensible change in the correlation whether we take all pairs of brethren or only adult pairs, nor is there much difference between the mean intervals between births. But the elder brother has slightly gained and the elder sister distinctly lost in excess of life by taking brethren of all ages instead of merely adult pairs. I attribute this to the much larger correlation between the ages at death of infant sisters than we find in the case of infant brothers (see p. 60 below).

The following formulae enable us to predict the probable excess of life (e) of an elder brother or sister from a knowledge of the birth interval (i), both being taken in years:

$$\text{Elder brother: } e = \cdot 7282 i - \cdot 2795.$$

$$\text{Elder sister: } e = \cdot 8525 i - 2\cdot 5991.$$

Thus a brother born ten years before another brother has probably seven years' greater duration of life; a sister born ten years before another sister has probably about six years' greater duration of life. Theoretically the younger brother has the advantage of the elder until $i = \cdot 2795/\cdot 7282 = \cdot 38$ years about—an impossible difference. Theoretically the younger sister has the advantage of the elder sister until $i = 2\cdot 5991/\cdot 8525 = 3\cdot 05$ years about, or until the third year say of interval of birth. These results are apparent on Figs. 1 and 2, which give the regression lines, and are quite sufficient in practice for the determination of the probable difference in duration of life.

For the present we have contented ourselves by showing the importance of seniority in birth for the appreciation of longevity. But it will be clear to the reader that the correlations between longevity and actual standing in a family and between longevity and the ages of the parents at the birth are matters of much interest and deserve full investigation.

(4) In an earlier memoir* by one of the present authors some attempt has been made to resolve the mortality curve into components, of which the most important are those of extreme childhood and of old age. The mortality of youth and that of middle age are far less important. In keeping with this resolution we have mentioned in our first study† the existence of an inheritance of brachybioty as well as an inheritance of longevity. Not only for the purposes of manipulating the statistics, but also because we believe there is a real physical distinction, did we consider it desirable to separate brachybioty and longevity. But in order to avoid the immense labour of the cross-correlations if we divided the span of life into four or five sections, and considered how far death in one of those periods influenced duration of life of brethren dying in any one of them, we adopted only a twofold division, distinguishing only between minors and adults. Even this involved the preparation of no less than twelve fraternal correlation tables for duration of life, and in the case of cross-correlations, such as brother or sister minor with brother or sister adult, we were rarely able to collect more than four to five hundred pairs of cases.

Now a noteworthy result of our investigations is this: That while the correlation of adult and adult brethren and of minor and minor brethren are both quite sensible, in no one case is the correlation between the durations of life of an adult and a minor sensible within the limits of our probable error. The following table summarises our results:

Relationship	Correlation	Cases
Adult brother and minor brother	– ·0262 ± ·0246	753
Adult brother and minor sister	– ·0062 ± ·0349	374
Adult sister and minor sister	– ·0260 ± ·0291	537
Adult sister and minor brother	– ·0274 ± ·0328	421

Now although none of these correlations standing alone are sensible, it is noteworthy that they are all *negative;* an examination of the Tables XIV. XVIII. XXI. and XXII. will at once reveal the source of this. It is the effect of inheritance of the mortality of youth; minors dying from 16 to 20 are associated with adults dying from 21 to 25, i.e. minors dying late correspond to adults dying early. Diagrams of the regression lines‡ show this negative slope in the later years of minority. Here is the source of the negative sign. It might be thought, that if this effect of the mortality of youth were removed, we should get sensible positive correlations between the age at death of an adult brother or sister and of an infant brother or sister. But an examination of the regression lines as far as 10 or 12 years of age shows that there is really but little correlation between the dura-

---

* *Phil. Trans.*, Vol. 186 A, p. 407.

† *R. S. Proc.*, Vol. 65, p. 299, 1899.

‡ Not reproduced in this paper.

tions of life of adult and infant brethren. Without much more material than we have been able to accumulate I do not believe we could safely break up our data and correlate the duration of life of adults with that of infant brethren dying under 10 or 12 years of age. I think we must conclude: *That to have a brother or sister die as a minor considerably shortens the expectation of life, but whether the brother or sister dies at* 2 *or* 5 *or* 10 *or* 15 *does not make a difference which we can at present, without further data, consider as sensible.*

The general effect on the duration of life of an adult produced by the death of a brother or sister in their minority is shown in the following table:

Average Age at Death.

If there has died	Of adult brother	Of adult sister
Adult elder brother ...	54·575	57·209
Adult younger brother	58·560	
Minor brother	49·009	46·135
Adult elder sister ...	58·804	55·667
Adult younger sister ...		59·924
Minor sister	48·201	50·300

It is remarkable that the death of his sister as minor seems more important to a man than the death of his brother as minor, and *vice versâ* for a woman. I have dealt with this a little more closely by averaging the age at death of men and women who have lost brothers or sisters in infancy, i.e. before 11 years of age. I find:

Average Age at Death.

If there has died	Of adult brother	Of adult sister
Infant brother	50·116	46·027
Infant sister	47·830	50·236

The probable error is in each case under a year so that the results are significant. One would like to see the whole matter investigated with the ample material which could only be provided by large assurance offices. But our results suggest that some diseases to which a family may be prone are more liable to attack one sex in infancy and the other in the adult period.

The above averages suffice also to indicate that whether a person's brother or sister dies as an infant or between 10 and 21, there is no very large difference in that person's expectation of life. In the case of the man, indeed, it is an advantage for his brother to die as an infant rather than as an adolescent; this is, of course, due to the effect of the mortality of youth being sensible in the early years of adult life, and influencing the heritage. It appears to have much less influence in the case of sisters. One word here about the influence of environment. There may be some readers who will be inclined to consider that much of the correlation of duration of life between brethren is due to there being a likeness of their environment, and that thus each pair of brethren is linked together and differentiated from the general population. But it is difficult to believe that this really affects *adult* brothers or a father and his adult offspring. A man who dies between 40 and 80 can hardly be said to have an environment more like that of his brother or father, who died also at some such age, than like any other member of the general population. Of course two brothers have usually a like environment in infancy, and their ages at death, even if they die adults, may be influenced by their rearing. But if this be true, we ought to find a high correlation in ages at death of brethren who die as minors. As a matter of fact this correlation for minor and minor is 40 to 50 per cent. less than in the case of adult and adult. It would thus seem that identity of environment is not the principal factor in the correlation between ages of death, for this correlation is far less in youth than in old age.

(5) The following Table summarises our chief results:

TABLE A.

Synopsis of Numerical Results for Inheritance of Duration of Life.

See Table	Relatives		Mean Durations in years		Standard Deviations in years		Correlations	Regressions		No. of Pairs
	x	y	m_x	m_y	σ_x	σ_y	r_{xy}	R_{xy}	R_{yx}	n
	(i) DIRECT INHERITANCE.									
III	Father	Adult son ...	68·370 ± ·313	53·490 ± ·417	14·6974 ± ·2217	19·5706 ± ·2952	·1353 ± ·0209	·1016	·1802	1000
IV	Father	Minor son ...	63·847 ± ·333	5·541 ± ·140	15·1829 ± ·2358	6·3758 ± ·0990	·0865 ± ·0218	·2059	·0363	943
V	Father	Adult daughter	69·547 ± ·271	52·535 ± ·429	13·6649 ± ·1917	21·6121 ± ·3032	·1301 ± ·0195	·0822	·2057	1156
VI	Father	Minor daughter	63·573 ± ·366	6·289 ± ·157	15·8439 ± ·2587	6·8212 ± ·1114	·0520 ± ·0230	·1208	·0224	853
VII	Mother	Adult son ...	67·947 ± ·326	54·365 ± ·369	16·9033 ± ·2308	19·1254 ± ·2612	·1313 ± ·0190	·1161	·1486	1220
VIII	Mother	Minor son ...	58·597 ± ·434	5·046 ± ·151	18·1448 ± ·3069	6·3149 ± ·1068	·0756 ± ·0238	·2174	·0263	795
IX	Mother	Adult daughter	68·702 ± ·339	53·011 ± ·435	16·4167 ± ·2400	21·0441 ± ·3077	·1493 ± ·0202	·1166	·1914	1064
X	Mother	Minor daughter	57·656 ± ·446	5·763 ± ·159	18·4962 ± ·3155	6·5852 ± ·1122	·1377 ± ·0237	·3867	·0490	782
	(ii) COLLATERAL INHERITANCE.									
XI	Elder adult brother ...	Younger adult brother ...	58·560 ± ·412	54·575 ± ·412	19·3452 ± ·2918	19·3105 ± ·2918	·2290 ± ·0194	·2995	·2985	1000
XII	Adult brother	Adult brother	56·568 ± ·414	56·568 ± ·414	19·4302 ± ·2930	19·4302 ± ·2930	·2853 ± ·0196	·2853	·2853	1000
XIII	Minor brother	Minor brother	4·633 ± ·177	4·633 ± ·177	5·9695 ± ·1252	5·9695 ± ·1252	·1026 ± ·0294	·1026	·1026	517
XIV	Adult brother	Minor brother	49·009 ± ·456	5·532 ± ·159	18·5526 ± ·3225	6·4591 ± ·1123	− ·0262 ± ·0246	− ·0753	− ·0091	753
XV	Elder adult sister ...	Younger adult sister ...	59·924 ± ·424	55·667 ± ·443	20·3812 ± ·3000	21·2689 ± ·3130	·3464 ± ·0183	·3319	·3615	1050
XVI	Adult sister ...	Adult sister ...	57·795 ± ·436	57·795 ± ·436	20·9382 ± ·3082	20·9382 ± ·3082	·3322 ± ·0185	·3322	·3322	1050
XVII	Minor sister ...	Minor sister ...	4·908 ± ·186	4·908 ± ·186	5·8841 ± ·1317	5·8841 ± ·1317	·1748 ± ·0307	·1748	·1748	454
XVIII	Adult sister ...	Minor sister ...	50·300 ± ·602	5·740 ± ·187	20·6847 ± ·4257	6·4123 ± ·1320	− ·0260 ± ·0291	− ·0838	− ·0081	537
XIX	Adult brother	Adult sister ...	58·804 ± ·290	57·209 ± ·315	18·9472 ± ·2048	20·5869 ± ·2225	·2319 ± ·0145	·2134	·2520	1947
XX	Minor brother	Minor sister ...	4·580 ± ·149	4·528 ± ·137	5·8044 ± ·1053	5·3459 ± ·0970	·1435 ± ·0251	·1558	·1322	691
XXI	Adult brother	Minor sister ...	48·201 ± ·682	5·384 ± ·214	19·5545 ± ·4822	6·1350 ± ·1513	− ·0062 ± ·0349	− ·0196	− ·0019	374
XXII	Adult sister ...	Minor brother	46·135 ± ·673	4·857 ± ·202	20·4876 ± ·4762	6·1342 ± ·1426	− ·0274 ± ·0238	− ·0916	− ·0082	421

From this table the following equations, giving the probable age at death of male and female individuals when the ages at death of certain of the relatives are known, have been determined.

Prediction of Age at Death.

Let M = probable age at death of a man.
W = ,, ,, ,, ,, woman.
F = known age at death of father.
M = ,, ,, ,, ,, mother.
S_0 = ,, ,, ,, ,, son.
D = ,, ,, ,, ,, daughter.
S_0', D' = known ages at death of other sons and daughters.
B, B' = ,, ,, ,, brothers.
S_i, S_i' = ,, ,, ,, sisters.

All the above capital letters mark individuals who live to be 21 or more.

s_0 = known age at death of minor son.
d = ,, ,, ,, minor daughters.
s_0', d = known ages at death of other minors, sons and daughters.
m = probable age at death of a male minor.
w = ,, ,, ,, ,, female minor.
b, b' = known ages at death of minor brothers.
s_i, s_i' = ,, ,, ,, minor sisters.

All the above small letters mark individuals who die at or before 21 years.

Σ = standard deviation of the array of individuals having the probable age at death given by the equation.

TABLE B.

(1) $\mathrm{M} = 41{\cdot}170 + {\cdot}1802\, F \qquad \Sigma = 19{\cdot}3906$
(2) $\mathrm{M} = 44{\cdot}268 + {\cdot}1486\, M \qquad \Sigma = 18{\cdot}9598$
(3) $\mathrm{M} = 31{\cdot}500 + {\cdot}1802\, F + {\cdot}1486\, M \qquad \Sigma = 19{\cdot}0009$
(4) $\mathrm{W} = 38{\cdot}229 + {\cdot}2057\, F \qquad \Sigma = 21{\cdot}4284$
(5) $\mathrm{W} = 39{\cdot}861 + {\cdot}1914\, M \qquad \Sigma = 20{\cdot}8074$
(6) $\mathrm{W} = 25{\cdot}317 + {\cdot}2057\, F + {\cdot}1914\, M \qquad \Sigma = 20{\cdot}9056$
(7) $\mathrm{M} = 62{\cdot}935 + {\cdot}1016\, S_0 \qquad \Sigma = 14{\cdot}5622$
(8) $\mathrm{M} = 65{\cdot}229 + {\cdot}0822\, D \qquad \Sigma = 13{\cdot}5487$
(9) $\mathrm{M} = 59{\cdot}427 + {\cdot}0790\, (S_0 + S_0') \qquad \Sigma = 13{\cdot}8845$
(10) $\mathrm{M} = 61{\cdot}058 + {\cdot}0805\, S_0 + {\cdot}0684\, D \qquad \Sigma = 13{\cdot}5959$
(11) $\mathrm{M} = 62{\cdot}415 + {\cdot}0617\, (D + D_0') \qquad \Sigma = 12{\cdot}6697$
(12) $\mathrm{M} = 62{\cdot}706 + {\cdot}2059\, s_0 \qquad \Sigma = 15{\cdot}1260$
(13) $\mathrm{M} = 62{\cdot}813 + {\cdot}1208\, d \qquad \Sigma = 15{\cdot}8225$
(14) $\mathrm{M} = 62{\cdot}117 + {\cdot}1867\, (s_0 + s_0') \qquad \Sigma = 14{\cdot}9999$

(15) $\mathrm{M} = 62{\cdot}191 + {\cdot}2156\, s_0 + {\cdot}1174\, d$ $\Sigma = 15{\cdot}2830$
(16) $\mathrm{M} = 62{\cdot}564 + {\cdot}1028\, (d + d')$ $\Sigma = 15{\cdot}5640$
(17) $\mathrm{W} = 61{\cdot}635 + {\cdot}1161\, S_0$ $\Sigma = 16{\cdot}7569$
(18) $\mathrm{W} = 62{\cdot}521 + {\cdot}1166\, D$ $\Sigma = 16{\cdot}2320$
(19) $\mathrm{W} = 57{\cdot}728 + {\cdot}0903\, (S_0 + S_0')$ $\Sigma = 15{\cdot}9819$
(20) $\mathrm{W} = 57{\cdot}222 + {\cdot}0899\, S_0 + {\cdot}1017\, D$ $\Sigma = 15{\cdot}9425$
(21) $\mathrm{W} = 58{\cdot}585 + {\cdot}0875\, (D + D')$ $\Sigma = 15{\cdot}2221$
(22) $\mathrm{W} = 57{\cdot}500 + {\cdot}2174\, s_0$ $\Sigma = 18{\cdot}0929$
(23) $\mathrm{W} = 55{\cdot}427 + {\cdot}3867\, d$ $\Sigma = 18{\cdot}3199$
(24) $\mathrm{W} = 56{\cdot}770 + {\cdot}1972\, (s_0 + s_0')$ $\Sigma = 17{\cdot}9552$
(25) $\mathrm{W} = 55{\cdot}290 + {\cdot}1802\, s_0 + {\cdot}4440\, d$ $\Sigma = 17{\cdot}9290$
(26) $\mathrm{W} = 54{\cdot}807 + {\cdot}2903\, (d + d')$ $\Sigma = 17{\cdot}9169$
(27) $\mathrm{M} = 40{\cdot}429 + {\cdot}2853\, B$ $\Sigma = 18{\cdot}6227$
(28) $\mathrm{M} = 46{\cdot}596 + {\cdot}2134\, S_i$ $\Sigma = 18{\cdot}4307$
(29) $\mathrm{M} = 34{\cdot}233 + {\cdot}2447\, B + {\cdot}1633\, S_i$ $\Sigma = 17{\cdot}6049$
(30) $\mathrm{M} = 31{\cdot}455 + {\cdot}2220\, (B + B')$ $\Sigma = 17{\cdot}4034$
(31) $\mathrm{M} = 40{\cdot}476 + {\cdot}1602\, (S_i + S_i')$ $\Sigma = 17{\cdot}1345$
(32) $\mathrm{W} = 42{\cdot}390 + {\cdot}2520\, B$ $\Sigma = 20{\cdot}0257$
(33) $\mathrm{W} = 39{\cdot}259 + {\cdot}3222\, S_i$ $\Sigma = 19{\cdot}7491$
(34) $\mathrm{W} = 30{\cdot}037 + {\cdot}1793\, B + {\cdot}2784\, S_i$ $\Sigma = 18{\cdot}7762$
(35) $\mathrm{W} = 35{\cdot}027 + {\cdot}1961\, (B + B')$ $\Sigma = 18{\cdot}8877$
(36) $\mathrm{W} = 28{\cdot}971 + {\cdot}2494\, (S_i + S_i')$ $\Sigma = 18{\cdot}0391$
(37) $m = 4{\cdot}158 + {\cdot}1026\, b$ $\Sigma = 5{\cdot}9380$
(38) $m = 3{\cdot}875 + {\cdot}1558\, s_i$ $\Sigma = 5{\cdot}7443$
(39) $m = 3{\cdot}565 + {\cdot}0837\, b + {\cdot}1448\, s_i$ $\Sigma = 5{\cdot}7498$
(40) $m = 3{\cdot}771 + {\cdot}0931\, (b + b')$ $\Sigma = 5{\cdot}8843$
(41) $m = 3{\cdot}278 + {\cdot}1326\, (s_i + s_i')$ $\Sigma = 5{\cdot}6140$
(42) $w = 3{\cdot}923 + {\cdot}1322\, b$ $\Sigma = 5{\cdot}2906$
(43) $w = 4{\cdot}050 + {\cdot}1748\, s_i$ $\Sigma = 5{\cdot}7935$
(44) $w = 3{\cdot}442 + {\cdot}1163\, b + 1547\, s_i$ $\Sigma = 5{\cdot}4308$
(45) $w = 3{\cdot}417 + {\cdot}1199\, (b + b')$ $\Sigma = 5{\cdot}2214$
(46) $w = 3{\cdot}447 + {\cdot}1488\, (s_i + s_i')$ $\Sigma = 5{\cdot}6408$
(47) $m = 3{\cdot}223 + {\cdot}0363\, F$ $\Sigma = 6{\cdot}3519$
(48) $m = 3{\cdot}505 + {\cdot}0263\, M$ $\Sigma = 6{\cdot}2968$
(49) $m = 1{\cdot}434 + {\cdot}0363\, F + {\cdot}0263\, M$ $\Sigma = 6{\cdot}3033$
(50) $w = 4{\cdot}865 + {\cdot}0224\, F$ $\Sigma = 6{\cdot}8120$
(51) $w = 2{\cdot}938 + {\cdot}0490\, M$ $\Sigma = 6{\cdot}5224$
(52) $w = 1{\cdot}777 + {\cdot}0224\, F + {\cdot}0490\, M$ $\Sigma = 6{\cdot}6307$

Thus equation (2), for example, gives the probable age at death of a man when the ages at death of his father and mother are known. These equations have been determined by the usual regression formulae of the theory of correlation; but certain assumptions have had to be made in determining them. Thus from Table A we see that the variability for their age at death of adult sons of fathers is expressed by $\sigma_y = 19{\cdot}5706$ years, and of adult sons of mothers by $\sigma_y = 19{\cdot}1254$ years. Now to obtain our data we had to take such material as was available and we could not select only those sons whose parents' ages at death were *both* recorded. Accordingly our series of sons is not the same in the case of mothers as it is in that of fathers, and we have taken in our regression equation the mean σ_y as determined from both series. Again, in judging of the probable age at death of a man from those of his brother and sister, we find that the variability of age at death of a man with an adult brother is given by $\sigma_x = 19{\cdot}4302$ years, but of a man with an adult sister as 18·9472 years. Further, the mean age at death of a man with an adult brother is 56·568 years, but with an adult sister is 58·804 years. These are sensible differences, and there can hardly be a doubt that men with sisters live slightly but sensibly longer than those with brothers, and are slightly but sensibly less variable in their age at death. Now our data do not provide the constants for the specially differentiated class of men with *both* brothers and sisters. Accordingly we have been compelled to take the mean of the two classes —men with brothers and men with sisters—to represent both in age at death and variability the special class of men with both a brother and a sister. Other similar cases will occur to the reader, and there are some in which the assumptions made are less justifiable than the above. For example take the case of a minor having two minor brothers dying. We have only been able to use the mean age at death of a minor having at least one minor brother dying, but this is certainly greater than the mean age at death of a minor having at least two minor brothers dying. We were only able to obtain altogether 517 cases of minor brothers dying out of all the records of the Society of Friends accessible to us. It would have been idle to have attempted the differentiation of this small number into sub-classes of minors with one or with more than one brother dying as minor, or with one brother and with one sister dying as minor, and so on. Our equations do not pretend to give more than a rough appreciation, such as is compatible with the comparative paucity of our material, of the influence of the death of relatives on the probable age at death of any individual. Just because we consider the record of the deaths of minors to be very incomplete even in the case of Friends' family histories, while that of adults is fairly complete, we have purposely avoided the important problem which lies at the root of much of the practical use of equations (37) to (52), namely: What are the chances *in the material we are working on* that an individual will die as a minor or survive to be an adult? These chances can be determined for the general population from the Registrar-General's returns, but our material is considerably differentiated from the general population and we have felt bound to leave this problem unanswered.

(6) Before we proceed to illustrate these equations for the probable ages at death, it may be as well to compare, as far as is possible, our present data with what we gave in our first study of the inheritance of longevity*. We were then working solely with male inheritance and from different classes, those of the

---

* *R. S. Proc.*, Vol. 65, pp. 297, 299.

landed gentry and of the peerage. We have the following comparative results:

Constant	(1) Peerage	(2) Landed Gentry	Mean of (1) and (2)	Friends
M_F	65·835	65·963	65·899	68·370
M_S	58·775	60·915	59·845	53·490
M_B	60·971	—	60·971	56·568
σ_F	14·6382	14·4308	14·5345	14·6974
σ_S	17·0872	17·0986	17·0927	19·5706
σ_B	16·8354	—	16·8354	19·4302
r_{SF}	·1149	·1418	·1283	·1353
r_{BB}	·2602	—	·2602	·2853

Here M_F, M_S, M_B are the mean ages at death of fathers, sons, and brothers; σ_F, σ_S and σ_B are the standard deviations of the same classes respectively, and r_{SF}, r_{BB} are the correlations of ages at death of father and son and of pairs of brothers.

Now these classes are really widely differentiated. Many of the Friends belong to the farming and small trading, as well as to the greater commercial classes. We see at once that the fathers live longer and the sons live shorter lives than is the case with the peerage or landed gentry. The younger male Friends have also a greater variety in their ages at death. Yet notwithstanding these class differences the values of r_{SF} and r_{BB} are in substantially good agreement for the two investigations. The value of r_{SF} for the Friends lies between those for the peerage and the landed gentry; the value of r_{BB} differs from that for the peerage by less than the probable error of the difference.

If we compare the six diagrams of plotted regression polygons and their best fitting regression lines which accompany this paper with the three diagrams of the former paper we shall see that the two sets are in general agreement. We note that the straight line serves as well as any curve could do to give the general drift of the phenomena; the chief deviations from it are as a rule at early or late ages, where the data themselves are very sparse. We find also the same sensible divergence from the theoretical values for parental and fraternal inheritance marking the existence of a considerable non-selective death-rate. Fig. 3 for the regression of fathers on sons even suggests the same points as Fig. (i) of the earlier memoir*. In early manhood the inheritance of duration of life approaches much more nearly the slope of the theoretical line; in middle life the correlation is small, the non-selective death-rate having greater influence; while in old age the inheritance of longevity is again very manifest†.

With standard deviations for father and son of 14·6974 years and 19·5706 years, and a theoretical correlation of ·3 according to the Galtonian law, the slope

* *Loc. cit.* p. 296.

† *Loc. cit.* p. 299.

of the regression line of father or son ought to be ·2253; for adult sons it is only ·1016, but an examination of the diagram shows that it is much nearer ·2 than ·1 for early manhood. Further, our own results show that it is actually ·2059 for a father and a minor son. Thus our original statement* that in youth and infancy the regression line for parent and child would approach nearer its theoretical position seems to receive confirmation. The tendencies which cause an individual to have a short life, i.e. to die before the age of 35, are largely inherited tendencies, and they shorten also the life of the parent. The tendencies which lengthen the life of an individual beyond sixty years are also inherited, and they appear as lengthening the parental life. But between 35 and 60 the relationship of duration of life in parent and offspring is not very marked. We thus can distinguish an inheritance of brachybioty separated from an inheritance of longevity by a period of life in which the non-selective death-rate is predominant. The whole of

FIG. 3.

Regression Line: Fathers on Sons.

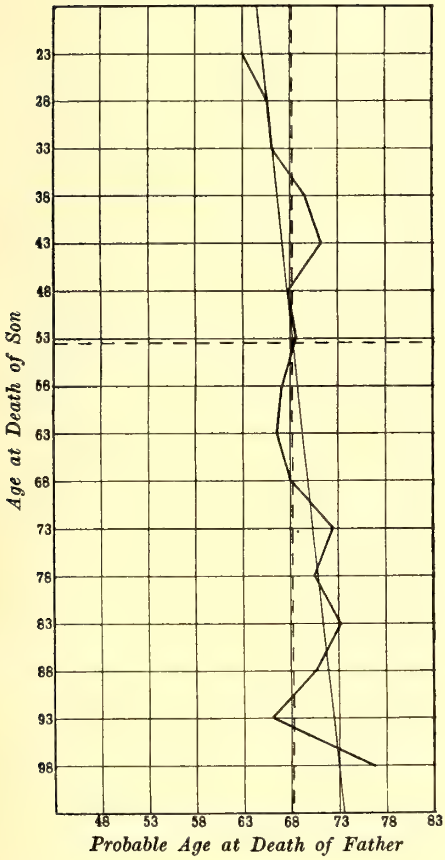
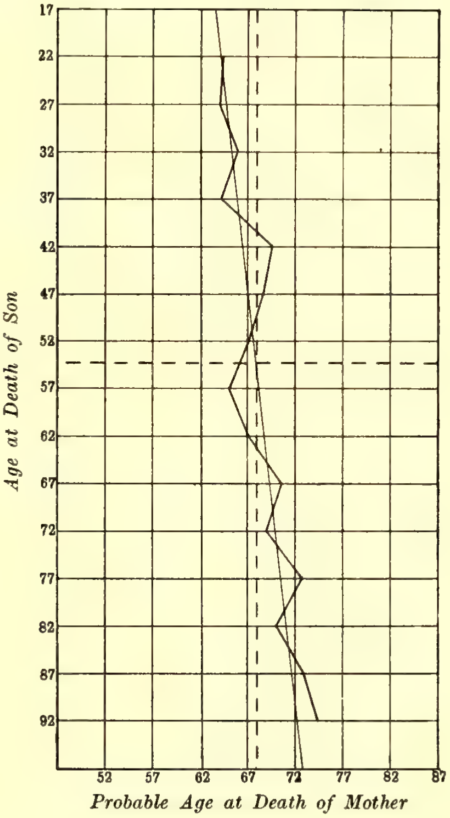


FIG. 4.

Regression Line: Mothers on Sons.



---

* The reader must disregard the results for ages 93 and 98, for they are based on very few cases.

FIG. 5.
Regression Line: Daughters on Fathers.

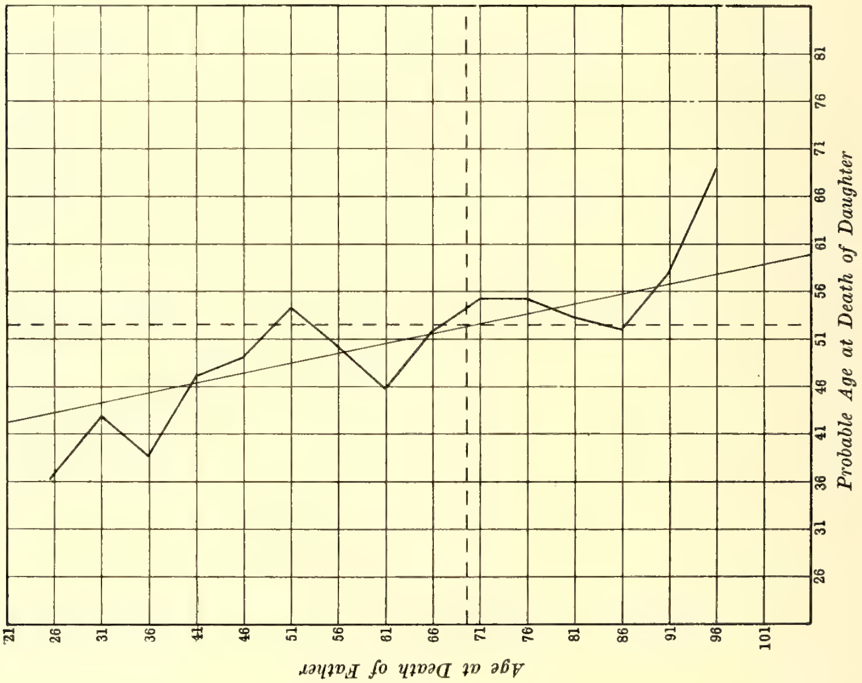


FIG. 6.
Regression Line: Daughters on Mothers.

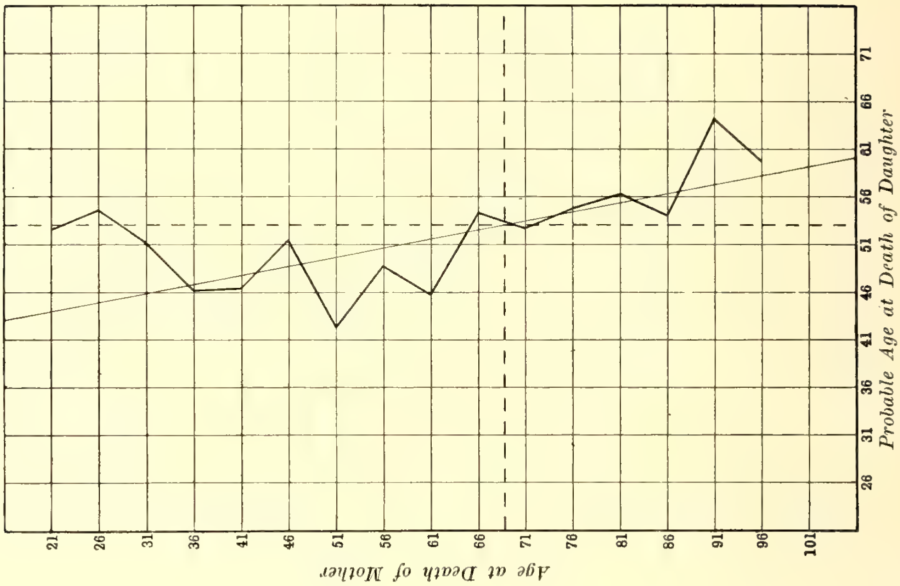


FIG. 8.
Regression Line: Sisters on Sisters.

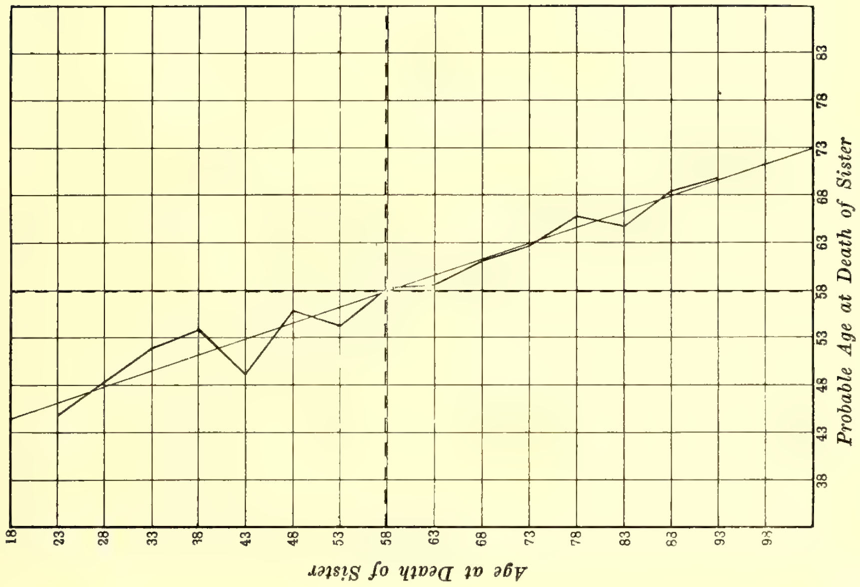
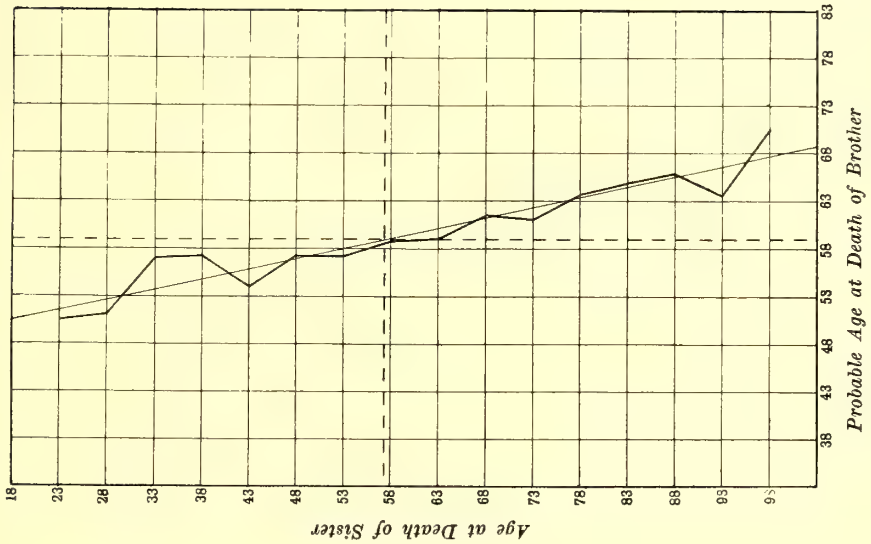


FIG. 7.
Regression Line: Brothers on Sisters.



this statement, however, must be confined to the parental relationship, for even in deaths in middle life we find a fairly continuous and unbroken slope for the regression line in the case of brothers or sisters: see for example Figs. 7 and 8. The problem is, of course, immensely complex and probably rendered especially so by a mixture of different classes of causes, i.e. inheritance of tendencies to develop one or more mortal diseases at definite ages in life, and inheritance of general physical weakness or physical robustness tending merely to shorten or lengthen life as a whole, and largely influenced by environment as far as the definite age at death is concerned. All we can do, in default of special statistics stating the cause of death for each individual, is to examine in broad outline the general influence on the duration of life of an individual of the ages at death of his or her nearest relatives.

(7) To illustrate the use of equations like (1) to (52) the following problems may be considered.

Illustration (i).—A's age is 44, his father and mother are alive at the ages of 79 and 74. His paternal grandfather died aged 69 and his paternal grandmother at 82; his maternal grandfather and grandmother lived to be 81 and 59 respectively. What are the expectations of life of his parents, and what is his own expectation of life based on theirs?

Now this is by no means so straightforward a problem as it might appear at first sight, and there is more than one way of looking at it which will give a fairly reasonable solution. We cannot apply equations (3) and (6) straight off, because the M and W there are a man and woman of the general population, but our man and woman belong to a select class, namely those who live to have adult sons. The average ages of such according to our Table A are: for father 68·370 years and for mother 67·947 years. Hence the probable age at death of A's father and mother would be given by the equations:

$$\begin{aligned} A\text{'s father's age} &= 68{\cdot}370 + {\cdot}1802\,(69 - 68{\cdot}370) + {\cdot}1486\,(82 - 67{\cdot}947) \\ &= 70{\cdot}571 \text{ years.} \end{aligned}$$

$$\begin{aligned} A\text{'s mother's age} &= 67{\cdot}947 + {\cdot}2057\,(81 - 69{\cdot}547) + {\cdot}1914\,(59 - 68{\cdot}702) \\ &= 68{\cdot}446 \text{ years.} \end{aligned}$$

Thus while A's father gains about 2·2 years by his ancestry, A's mother gains only ·5 years because her mother died comparatively early.

Again, we must not in calculating Σ for the selected class take the σ of sons in general, but rather the value 14·6974 years of fathers of adult sons, and similarly we must take 16·9033 years for the σ from which we calculate the Σ of A's mother's class. This leads us to the values 14·4339 and 16·5677. Or, A's father belongs in an array of men, whose mean age at death is 70·571 years, and whose standard deviation is 14·4339 years. Similarly, A's mother belongs to an array of women whose mean age at death is 68·446 years, and whose standard deviation is 16·5677.

Now A's father has already lived $79 - 70{\cdot}571$ years $= 8{\cdot}429$ years beyond the mean age, and A's mother $74 - 68{\cdot}446$ years $= 5{\cdot}554$ years beyond the mean age. We require their expectations of life. In default of better hypotheses we take the distribution of the array to be represented by the normal curve of errors. Then if X_1 be A's father's further expectation of life, and $x_1 = 8{\cdot}429$ years, $\sigma_1 = 14{\cdot}4339$ years,

$$8{\cdot}429 + X_1 = \frac{\frac{1}{\sqrt{2\pi}}\int_{x_1}^{\infty} xe^{-x^2/(2\sigma_1^2)}\,dx}{\frac{1}{\sqrt{2\pi}}\int_{x_1}^{\infty} e^{-x^2/(2\sigma_1^2)}\,dx},$$

$$= \sigma \times \frac{1}{\sqrt{2\pi}} e^{-\frac{1}{2}x_1'^2} \Big/ \frac{1}{\sqrt{2\pi}} \int_{x_1'}^{\infty} e^{-\frac{1}{2}x_1'^2}\,dx'.$$

These values are to be found from the usual tables, and lead to

$$X_1 = 17{\cdot}364 - 8{\cdot}429,$$
$$= 8{\cdot}935.$$

Thus A's father will probably live to be 87·935 years. Working out the case of A's mother in the same manner we find for her probable further length of life 11·388 years, or her probable age at death is 85·388 years.

If we turn to Ogle's expectation of life table we find the expectation of life of a man of 79 to be 5·07 years, and of a woman of 74 to be 7·25 years, or they will probably reach ages of 84·07 and 81·25 years respectively. In other words A's parents appear to have 4 years' greater duration of life than is deducible from Ogle's results. How is this to be accounted for? I think in the following way. The mean age of all our series of fathers is 66·334 years, but the mean age of fathers with an adult son is 68·370 years. Hence if we consider that the majority of men who live to be 79 are either fathers or in general quite equal to fathers in physique, it follows that A's father increases his expectation of life by 4 years over Ogle's estimate in two nearly equal parts, 2 years because he has an adult son and 2 years on account of the longevity of his ancestry. Treating the case of the mother in the same way we find the mean age at death of mothers of any kind to be 63·225 years, but the age of mothers with adult sons is 67·947 years; in other words about 4·7 years' difference in the duration of life. As A's mother only gains about ·5 years from ancestry, we might expect a total gain of about 5 years on Ogle's estimate. There is actually a gain of a little over four years*. Thus, allowing for the fact that we are selecting special classes, our results are not widely divergent from what we might expect from Ogle's general mortality table.

---

* The average age at death of our Friends series is 59·97 for adult males and 58·78 for adult females. Ogle's results give about 59·5 for adult males and 62 for adult females. So that our males are longer and not shorter lived than the females.

Turning now to the case of A himself, we shall use his parents' probable lives, 87·935 and 85·388 years, as if they were their actual ages at death*, but what class are we to suppose him to belong to? Does he belong to the mean population of adult sons? If so his class's mean age is 53·490 years. Actually he is a father, without having lost a child, or without any of his children reaching the age of 21. Hence all we can do is to class him under the general group of fathers with a mean age of 66·334 years. Thus we have

A's probable age at death

$$= 66{\cdot}334 + {\cdot}1802\,(87{\cdot}935 - 68{\cdot}370) + {\cdot}1486\,(85{\cdot}388 - 67{\cdot}947)$$
$$= 72{\cdot}748 \text{ years.}$$

Now the mean S.D. of all fathers = 14·8472, and therefore the S.D. of fathers with selected parents = 14·5810.

Thus A belongs to an array of men who die at the average age of 72·748 years with a standard deviation σ_1 of 14·5810 years.

---

* This assumption is not strictly legitimate for the reasons given in the next illustration. If the mean age of death of fathers be m_1 and of mothers m_2, and the father die at $m_1 + x$ and the mother at $m_2 + y$, the probable age at death of the son would be found from an equation of the form given above in Table B to be: $m_3 = c_1 + c_2 x + c_3 y$. Hence the expectation of life $E_{x,\,y}$ of a man aged $m_3 - a_3$ whose parents die at $m_1 + x$ and $m_2 + y$ would be

$$E_{x,\,y} = \frac{\Sigma^2}{\sqrt{2\pi}}\, e^{-\frac{(m_3 - a_3)^2}{2\Sigma^2}} \Bigg/ \frac{1}{\sqrt{2\pi}} \int_{-(m_3 - a_3)}^{\infty} e^{-\frac{X^2}{2\Sigma^2}}\, dX.$$

Now if the parents be alive at $m_1 + a_1$ and $m_2 + a_2$ years their chances of dying between $m_1 + x$ and $m_2 + y$ and $m_1 + x + \delta x$, $m_2 + y + \delta y$ will be

$$\frac{1}{\sqrt{2\pi}}\, e^{-\frac{x^2}{2\sigma_1^2}}\, dx \Bigg/ \frac{1}{\sqrt{2\pi}} \int_{a_1}^{\infty} e^{-\frac{x^2}{2\sigma_1^2}}\, dx \quad \text{and} \quad \frac{1}{\sqrt{2\pi}}\, e^{-\frac{y^2}{2\sigma_{22}}}\, dy \Bigg/ \frac{1}{\sqrt{2\pi}} \int_{a_2}^{\infty} e^{-\frac{y^2}{2\sigma_2^2}}\, dy$$

respectively.

Multiply the product of these chances by the expectation of life $E_{x,\,y}$; we have on integrating for x and y from a_1 to ∞ and from a_2 to ∞ respectively, the total value E of the man's expectation of life:

$$E = \frac{\dfrac{\Sigma^2}{\sqrt{2\pi}}\, \dfrac{1}{2\pi} \displaystyle\int_{a_1}^{\infty} \int_{a_2}^{\infty} \frac{e^{-\frac{(m_3 - a_3)^2}{2\Sigma^2}}\, e^{-\frac{x^2}{2\sigma_1^2}}\, e^{-\frac{y^2}{2\sigma_2^2}}}{\dfrac{1}{\sqrt{2\pi}} \displaystyle\int_{-(m_3 - a_3)}^{\infty} e^{-\frac{X^2}{2\Sigma^2}}\, dX}\, dx\, dy}{\dfrac{1}{\sqrt{2\pi}} \displaystyle\int_{a_1}^{\infty} e^{-\frac{x^2}{2\sigma_1^2}}\, dx \times \dfrac{1}{\sqrt{2\pi}} \displaystyle\int_{a_2}^{\infty} e^{-\frac{y^2}{2\sigma_1^2}}\, dy}.$$

Here m_3 is the function of x and y given above, Σ is the standard deviation of sons of selected parents, σ_1 and σ_2 the standard deviations of fathers and mothers of adult sons. Without using troublesome quadrature formulæ I do not at present see how to evaluate the integral in the numerator. It certainly deserves investigation.

Since A is 44 years of age, he is $x_1 = 28{\cdot}748$ years before the probable age at death of his class. His expectation of life is accordingly $28{\cdot}748 + X_1$ where

$$X_1 = \frac{\dfrac{1}{\sqrt{2\pi}}\displaystyle\int_{-x_1}^{\infty} xe^{-x^2/(2\sigma^2)}\,dx}{\dfrac{1}{\sqrt{2\pi}}\displaystyle\int_{-x_1}^{\infty} e^{-x^2/(2\sigma^2)}\,dx},$$

$$= \frac{\dfrac{1}{\sqrt{2\pi}}\displaystyle\int_{+x_1}^{\infty} xe^{-x^2/(2\sigma^2)}\,dx}{1-\dfrac{1}{\sqrt{2\pi}}\displaystyle\int_{+x_1}^{\infty} e^{-x^2/(2\sigma^2)}\,dx}.$$

Calculating this from the tables in the usual way we find:

$$X_1 = {\cdot}854 \text{ years.}$$

Thus while A gains 6·414 years from his ancestry he only gains ·854 years from the fact that he has already survived 44 years. His probable duration of life is 73·60 years. Ogle's table would give a man of 44, 22·7 years' expectation of life; from our data A has 29·6 years. The difference of 7 years is substantially due to A's good ancestry.

Illustration (ii).—A man of 50 and his wife of 44 years of age have had two children, one of whom, a daughter, died at 8, and the other, a son, at 12. The man wishes to provide an annuity for his wife, if she survives him. What is her expectation of life as a widow? By Equation (15) of Table B the man belongs to a group of men who die at the mean age of 65·717 years, with a standard deviation of 15·2830. By Equation (25) the woman belongs to a group of women who die at the mean age of 61·004 years, with a standard deviation of 17·9290. An erroneous solution of this problem might be obtained in the following manner: the man will most probably live 15·717 years, if he died at the most probable age of death; his widow if she lived to her most probable age of death would be 59·717 years old when he died and have 1·287 years still to live. This is very far indeed from her expectation of widowhood. Such a solution fails because the probable ages of death of certain arrays of men and women do not determine the expectations of life of men and women who have already lived to certain definite ages. Working out as in Illustration (i) the expectations of life of the man and woman, we find them to be 19·953 and 22·510 years respectively*. Would it be correct to say then that the expectation of widowhood is 2·557 years? This again is incorrect; the years of widowhood are in each case correlated with the age at which the husband dies, and we cannot find the mean value of these years from the mean ages of death of husband and wife.

The problem is considerably more complex and we must proceed as follows: Let x_1 be the number of years before or after 65·717 years at which the husband

---

* Ogle's life table gives 18·93 and 24·72 respectively. Our man and woman, however, do not represent the general population. They have been father and mother and have lost two children.

dies, then the wife, if alive, will be $x_2 = x_1 - 1{\cdot}287$ years from 61·004 years, the probable age at death of the array of such women. Hence the chance that the woman survives her husband if he dies at x_1:

$$= \frac{\dfrac{1}{\sqrt{2\pi}} \displaystyle\int_{x_2}^{\infty} e^{-x^2/(2\sigma_2^2)}\, dx}{\dfrac{1}{\sqrt{2\pi}} \displaystyle\int_{b}^{\infty} e^{-x^2/(2\sigma_2^2)}\, dx},$$

where $b = -17{\cdot}004$ and $\sigma_2 = 17{\cdot}9290$.

If she does survive him, her expectation of life

$$= \frac{\dfrac{1}{\sqrt{2\pi}} \displaystyle\int_{x_2}^{\infty} x e^{-x^2/(2\sigma_2^2)}\, dx}{\dfrac{1}{\sqrt{2\pi}} \displaystyle\int_{x_2}^{\infty} e^{-x^2/(2\sigma_2^2)}\, dx}.$$

Hence her expectation of widowhood, if the man dies at x_1 expressed in years, is the product of these expressions,

$$= \frac{\dfrac{1}{\sqrt{2\pi}} \sigma_2^2 e^{-x_2^2/(2\sigma_2^2)}}{\dfrac{1}{\sqrt{2\pi}} \displaystyle\int_{b}^{\infty} e^{-x^2/(2\sigma_2^2)}\, dx}.$$

Now the man's chance of dying between x_1 and $x_1 + dx_1$

$$= \frac{\dfrac{1}{\sqrt{2\pi}} e^{-x_1^2/(2\sigma_1^2)}\, dx_1}{\dfrac{1}{\sqrt{2\pi}} \displaystyle\int_{a}^{\infty} e^{-x^2/(2\sigma_1^2)}\, dx},$$

where $a = -15{\cdot}717$ and $\sigma_1 = 15{\cdot}2830$.

We now multiply the woman's expectation of widowhood if the man's death takes place at a particular age by the man's chance of dying at that age and integrate for all possible ages of the man at death. We thus find if E_w be the expectation of widowhood of the wife:

$$E_w = \frac{\dfrac{1}{\sqrt{2\pi}} \displaystyle\int_{a}^{\infty} e^{-x_1^2/(2\sigma_1^2)} \frac{1}{\sqrt{2\pi}} \sigma_2^2 e^{-x_2^2/(2\sigma_2^2)}\, dx_1}{\dfrac{1}{\sqrt{2\pi}} \displaystyle\int_{a}^{\infty} e^{-x^2/(2\sigma_1^2)}\, dx \times \dfrac{1}{\sqrt{2\pi}} \displaystyle\int_{b}^{\infty} e^{-x^2/(2\sigma_2^2)}\, dx}.$$

Here $x_2 = x_1 - h$, where $h = 1{\cdot}287$ years.

Now let us write the subject of integration in the numerator in the form

$$e^{-\frac{1}{2}\frac{1}{\sigma_0^2}(x_1 - c)^2 - \frac{1}{2}f^2}$$

or

$$\frac{1}{\sigma_0^2}(x_1 - c)^2 + f^2 = \frac{x_1^2}{\sigma_1^2} + \frac{x_1^2}{\sigma_2^2} - \frac{2hx_1}{\sigma_2^2} + \frac{h^2}{\sigma_2^2}.$$

We find

$$\sigma_0 = \frac{\sigma_1 \sigma_2}{\sqrt{\sigma_1^2 + \sigma_2^2}}, \qquad c = \frac{\sigma_1^2}{\sigma_1^2 + \sigma_2^2} h, \qquad f = \frac{h}{\sqrt{\sigma_1^2 + \sigma_2^2}}.$$

Thus if $X = x_1 - c$ we have

$$E_w = \frac{\sigma_2 \frac{\sigma_2}{\sqrt{2\pi}} e^{-\frac{1}{2}\frac{h^2}{\sigma_1^2+\sigma_2^2}} \times \frac{1}{\sqrt{2\pi}} \int_{a-c}^{\infty} e^{-\frac{1}{2}\frac{X^2}{\sigma_0^2}} \, dX}{\frac{1}{\sqrt{2\pi}} \int_a^{\infty} e^{-x^2/(2\sigma_1^2)} \, dx \times \frac{1}{\sqrt{2\pi}} \int_b^{\infty} e^{-x^2/(2\sigma_2^2)} \, dx}.$$

Put

$$a' = \frac{a}{\sigma_1}, \qquad b' = \frac{b}{\sigma_2}, \qquad c' = \frac{a-c}{\sigma_0},$$

and we have

$$E_w = \frac{\sigma_0 \frac{\sigma_2}{\sigma_1} \frac{1}{\sqrt{2\pi}} e^{-\frac{1}{2}\frac{h^2}{\sigma_1^2+\sigma_2^2}} \times \frac{1}{\sqrt{2\pi}} \int_{c'}^{\infty} e^{-\frac{1}{2}X'^2} \, dX'}{\frac{1}{\sqrt{2\pi}} \int_{a'}^{\infty} e^{-\frac{1}{2}x'^2} \, dx' \times \frac{1}{\sqrt{2\pi}} \int_{b'}^{\infty} e^{-\frac{1}{2}x'^2} \, dx'},$$

where

$$h/\sqrt{\sigma_1^2 + \sigma_2^2} = {\cdot}054{,}629, \qquad c' = -1{\cdot}397{,}888,$$
$$a' = -1{\cdot}028{,}396, \qquad b' = -\ {\cdot}948{,}408.$$

All parts of the above expression can now be found from tables of the ordinates and areas of the normal curve of errors. We find

$$E_w = 13{\cdot}6442 \, \frac{{\cdot}398{,}302 \times {\cdot}918{,}924}{{\cdot}848{,}113 \times {\cdot}828{,}538}$$
$$= 7{\cdot}107 \text{ years.}$$

Thus the wife has an expectation of a little more than seven years of widowhood, i.e. this is the mean period of widowhood of wives of her class. This is nearly six times the period found for the difference between the probable ages at death of husband and wife and three times the difference between their expectations of life. The whole problem of the expectation of widowhood seems deserving of treatment on general lines.

(8) We may now return to the bearing of our results on the problem of evolution. A very considerable series of investigations on a variety of organs and characters in man have given quite definite results as to both parental and fraternal inheritance. Whether we take mental or physical characters, the parental correlation lies between ·3 and ·5, some of our best results tending nearer to the latter limit; for fraternal correlation the limits appear to be closer, from ·4 to ·5. We may take as typical numbers ·4 for parents and ·45 for brothers. Now it is at once obvious that for duration of life treated as an inheritable character we have got results which fall far below these values. The direct explanation of this lies in the existence of a non-selective death-rate. Hence

the reduction in the correlation of heredity as given in this case will enable us to find approximately the proportions of the selective and non-selective death-rates. As we have already pointed out, the case of brethren is better suited than the case of parent and offspring to test the proportions of the selective and non-selective death-rates.

Now if we take parents and adult children, the average correlation from Table A for all combinations of sexes is ·1365. For adult brethren, including brothers and sisters, it is ·2831. If pN be the number of deaths in N cases which are non-selective, i.e. do not depend in any way on the constitution of the individual, then $(1-p)$ will represent the chance of any individual dying under the selective death-rate, and $(1-p)^2$ the chance that both the members of a pair so die. Hence the ratio of the selective death-rate cases to the whole number of such cases is $(1-p)^2N$ to N, or if r be the observed correlation, and r_0 the correlation to be expected from the laws of inheritance, we should expect $r/r_0 = (1-p)^2$*. We have therefore:

$$\cdot 1365 = (1-p)^2\, r_0,$$
$$\cdot 2831 = (1-p)^2\, r_1,$$

where r_0 and r_1 are the parental and fraternal correlations. The following table is based upon these equations; we have given a range of values to r_0 and r_1 covering the actually observed numbers for other characters.

TABLE C.

Selective Death-Rate in Man deduced from Ages at Death of Relatives.

Parental Correlation			Fraternal Correlation		
Value	Selective Death-Rate	Non-Selective Death-Rate	Value	Selective Death-Rate	Non-Selective Death-Rate
·3	67·5 p.c.	32·5 p.c.	·4	84·1 p.c.	15·9 p.c.
·4	58·4 p.c.	41·6 p.c.	·45	79·3 p.c.	20·7 p.c.
·45	55·1 p.c.	44·9 p.c.	·5	75·2 p.c.	24·8 p.c.

This table is in good agreement with the general results reached in the earlier paper, namely, that judging from parental inheritance the selective death-rate is 63 to 70 per cent., and from fraternal inheritance about 80 per cent. of the total death-rate †. The drop between the results derived from fraternal and parental correlation is due, as we have before remarked, to the environment changing much more from parent to offspring than it does from brother to brother. Even the environment of two sisters is usually much more alike than that of two

---

* *Phil. Trans.* A, Vol. 192, p. 277.

† *R. S. Proc.*, Vol. 65, p. 293.

brothers who frequently leave earlier the parental roof, often go much further and are more liable to accident, i.e. to non-selective death. Compare the correlation ·3322 of two sisters with the ·2853 of two brothers.

Look at the matter as we will it is impossible to place the selective death-rate at lower than 50 per cent., and in all probability with the same environment it is over 80 per cent.

(9) The following is, we believe, a fairly accurate statement of the general principle of evolution by natural selection combined with heredity:

(*a*) With a given environment individuals having certain constitutions are fitter to survive than others.

(*b*) They thus have a greater opportunity of reproducing themselves and rearing their offspring.

(*c*) Since the parents' characters are transmitted to their offspring the prevailing characters of the general population will in every case either be continuously modified or are only maintained stable owing to natural selection.

If (*a*) were not true the death-rate would be non-selective. We have shown in this paper that at least 50 and probably 80 per cent. of the death-rate in the case of man is certainly selective. Relatives—i.e. men with similar constitutions—have durations of life substantially correlated. In the case of civilised man the selection is largely due to the struggle with the physical environment, and not to the struggle of individual with individual. We should expect therefore the intensity of the selective death-rate in his case to be smaller than it is for many types of wild life.

In a paper published in the Royal Society's *Proceedings** we have shown that the number of offspring is directly related to the duration of life. We have thus quantitatively demonstrated the truth of (*b*).

Finally, in a fairly long series of papers only in part as yet published it has been proved that physical and intellectual characters are inherited from the parent; (*c*) is incontestably true.

I think, therefore, that we can no longer talk of natural selection as an hypothesis. It is in the case of man demonstrably at work either changing in a quantitatively definite manner his constitution as a whole or else necessary to keep that constitution stable. It is not now correct to say as Lord Salisbury said in 1894 of natural selection: "No man, so far as we know, has ever seen it at work." It is sensibly and visibly at work; a factor in 50 to 80 per cent. of the deaths in the case of man is not a slight perturbation, which we must seek with very refined analysis as the astronomer might seek for a small

---

* *Data for the Problem of Evolution in Man*, V. On the correlation between Duration of Life and Number of Offspring, Vol. 67, p. 159.

secular acceleration. It is something we run up against at once, almost as soon as we examine a mortality table. But if natural selection be at work and can be seen at work we are still far from the end of our researches. We have got beyond the stage of those who still persist in speaking of natural selection as a hypothetical source of change, but admitting once for all that it is continuously modifying races, we have still to investigate whether it can effectively differentiate races, i.e. be the source of the origin of species. This, whether we attack it from the standpoint of wild life or of laboratory experiment, is again a problem in statistics. Its solution depends on the relative fertility of different sections of a race with each other and with themselves. It cannot possibly be answered by observation of isolated cases, or by general reasoning. As we have said elsewhere: *sine numero nihil demonstrandum est.*

(10) We may note lastly one or two general points bearing on the laws of inheritance which arise from a consideration of Table A.

In the first place inheritance is stronger in members of the same sex, e.g. the correlation in the duration of life is greater between two brothers or two sisters, than between brother and sister; between father and son and mother and daughter, than between father and daughter or mother and son. The daughter seems to be more closely related in duration of life to her parents than the son. This is somewhat opposed to the result obtained for eye-colour*; but is I think quite explicable if we note how much more liable the male is to accidental death; thus the non-selective death-rate is intensified in his case. It is remarkable how much influence the death of a minor daughter has on the expectation of life of the mother as compared with that of the death of a minor son. One would have suspected that the higher mortality in childbed in the latter case would have made the reverse true.

Finally, I may draw attention to the fact that the women are sensibly more variable in their age at death than the men. This confirms the general conclusion reached elsewhere† as to the greater variability of women.

(11) *Summary of Results.*

(*a*) Material for the inheritance of duration of life ought to be collected on a much larger scale, e.g. by assurance offices, than is possible for two isolated workers. We only publish our data as the best available at present.

(*b*) Such material is of great interest, not only for actuarial but for biological problems.

(*c*) Of actuarial interest we may especially note the results:

(i) That the elder members of a family live sensibly longer than the younger.

---

* *Phil. Trans.*, Vol. 195, p. 117.

† Pearson, *Chances of Death*, Vol. I. p. 373.

(ii) That the expectation of life is seriously modified by either the ages of death of relatives or their present ages*, and can be quantitatively determined so soon as the latter are known.

(iii) The expectation of widowhood must be based not only on the present age of the husband, or on the age of relatives, but on the age at death of children.

(*d*) Of biological interest are:

(i) The possible correlation of a character or organ in a member of a family with his or her birth order.

(ii) The estimate of the intensity of the selective death-rate in man.

(*e*) Natural selection is manifestly at work in man, and is a factor in 50 to 80 per cent. of the deaths which occur.

We must place here a record of our great appreciation of the generous help we have received from Dr Alice Lee in the verification of the numerical work on the tables, and from Mr Karl Tressler in the preparation of the diagrams and the recalculation of the averages on which they are based.

---

* Thus that a man's father died at 80 is significant for his expectation of life, but it is also significant, if his father is alive at 75.

TABLE I. BROTHERS.

Difference in Duration of Lives in Years. Elder Brother—Younger Brother.

	Negative																		Positive																		No. of Cases		
Years' interval between births.	88	83	78	73	68	63	58	53	48	43	38	33	28	23	18	13	8	3	3	8	13	18	23	28	33	38	43	48	53	58	63	68	73	78	83	88	Totals	Younger lives longer	Elder lives longer
1					3	5	3	2	3	2	2	3	1	5	5	4	8	20	17	5	6	2	6	3	2	2	2	2		2	1		1				117	66	51
2			2	2	3	2	3	7	1	8	8	15	12	11	8	17	23	46·5	57·5	23	13	17	16	10	7	10	3	12	8	7	2	2	2		1	1	360	168·5	191·5
3		2		2	2	2	1	1	3	1	5	5	2	1	6	6	8	31	42	13	11	12	2	7	6	4	3	5	2	3	4	3		2			197	78	119
4		1			1	1	2	2	3	3	6	10	6	3	17	6	14	28·5	29·5	12	6	7	13	8	17	5	3	2	2	1	5	2	1		1		218	103·5	114·5
5				3	3	2	3	1	2	1	7	7	1	3	7	7	7	22·5	36·5	15	12	7	8	3	6	4	2	7	1	1	2	5	1	1			188	76·5	111·5
6				1		3	1	2	4	3	2	4	3	3	6	4	12	19·5	36·5	13	8	10	8	7	2	4	4	2	2	3	1	3	1	1			173	67·5	105·5
7				2	1		1	2	3	5	1	5	6	3	7	4	7	20·5	21·5	9	7	8	5	3	6	6	1	3	5	6	4	1	2	3			158	67·5	90·5
8		1		2	3	2	1	4	5		6	9	1	8	4	2	9	17·5	18·5	11	6	9	7	6	3	2	4	4	2	5	1	2	1	1	1		158	74·5	83·5
9	2			1			3	2	1	4	3	1	1	4	5	4	10	14	19	7	6	10	6	4	7	1	4	6	3		1	2		1			132	55	77
10			1		1	1		1		2		1	1	3	6	4	5	13·5	12·5	12	3	8	8	3	3	1	1	5	6	2	3	2			1		110	39·5	70·5
11			1		1	2		1	2		1		1	3	1	4	3	6·5	9·5	7	5	8	2	6	3	4	3	2	1	2	1	2	1				83	26·5	56·5
12				1		1		1	2		1		3	3	2	4	3	9·5	12·5	6	5	5	3	4	4	1	1	2	1	3	1						79	30·5	48·5
13				2	1		1		1	1	1	1	1	2	2	3	3	1·5	4·5	8	5	5	2	2		1	1	2	2	3	1	1	1		1		60	20·5	39·5
14										1	1		2		1	1	3	3	9	4	6	3	4		5	2		2	1	1	3		1	1			54	12	42
15					1								2		2	2	1	4·5	7·5	4	1	1	3	2	3	1	1	1	2	2	2	1					44	12·5	31·5
16								1				1	2	1	1		1	6	5	2	1	2	1			2	3	2		2			1				34	13	21
17		1												1	1	2		1·5	2·5	1	1	1		2	2	2	1		2								21	6·5	14·5
18									1		1	1		1		2	2	1	2	1				1	1			1		1				1			17	9	8
19				1					1	1			2			1	2	1	1	2	1	4	1				1				2			1			22	9	13
20				1	1												1	2					1	1							1	1					9	5	4
21																										1											1		1
22																		·5	·5		1			1	1	1	1			1							7	·5	6·5
23																									1												1		1
24														1							1											1					3	1	2
25																																					0		
26																		1																			1	1	
Totals	2	5	4	18	21	21	19	27	32	32	45	63	47	56	81	77	122	271·5	344·5	155	105	123	96	73	79	54	39	60	40	45	35	28	13	12	6	1	2247	943·5	1303·5

TABLE II. SISTERS.

Difference in Duration of Lives in Years. Elder Sister—Younger Sister.

Years' interval between births.	Negative																				Positive																				No. of Cases		
	98	*93*	*88*	*83*	*78*	*73*	*68*	*63*	*58*	*53*	*48*	*43*	*38*	*33*	*28*	*23*	*18*	*13*	*8*	*3*	*3*	*8*	*13*	*18*	*23*	*28*	*33*	*38*	*43*	*48*	*53*	*58*	*63*	*68*	*73*	*78*	*83*	*88*	*93*	Totals	Younger lives longer	Elder lives longer
1			2	1	1	2	2	1	2		1	2		2	3	1	3	2	7	9·5	11·5	3	6	5	2	3	3			1	2	1	1	1	1					82	41·5	40·5
2		1		1	1	5	2	2	2	5	2	8	2	4	3	8	11	10	21	30·5	33·5	15	12	6	10	13	2	2	6	4	2	6	1	1	2		1			235	118·5	116·5
3			1	1					3	1	2	3	2	1	2	5	4	7	9	27·5	27·5	15	12	7	8	6	4	4	2	5	1	3								163	68·5	94·5
4				1	1		1	3	2	2	1	1	5	3	3	5	7	6	9	20	22	9	9	4	3	4	3	3	3	1	2	1		2	1	2	2		1	142	70	72
5			3			1		1	3	4	2	1		7	4	2	4	5	9	21	25	14	5	12	6	4	2	3	3		3		1		1					146	67	79
6			1		1	1	2	1	2	4	3	1	1	2		5	5	6	11	18·5	23·5	10	10	8	2	5	3	3	6	2	3	2	2		1		1			146	64·5	81·5
7						1	1				2	4	2	1	5	4	9	6	5	8·5	5·5	6	11	10	3	7	4	4	1	2		3	1	2						108	48·5	59·5
8				1			1	1		1	2	2		1	3	2	1	3	4	11	12	10	8	5	10	2		3	2	1	3			3		1				93	33	60
9		1				1	3		2	1	2	1		3	1	2	5	6	4	8	12	12	2	3		4	3	1	1	2	1	1	1	1						84	40	44
10									1	1	1	1	1	1		2		3	3	2·5	10·5	8	7	4	3	4	1	3	1	2	1	1	4	1		1				68	16·5	51·5
11						1	1			1		2		1	4	2	2	2	3	10·5	10·5	1	7	7	1	5		1	1	1	1	2	2		1	2				72	29·5	42·5
12	1					1		1	1	1					3		2		3	6	6	2	6	3	1	3	2	4	3	4	1									54	19	35
13								1		1	1				3	2		1	6	6	5		5	6	4	2	2	1	1	1	1		1				1			51	21	30
14										1	3					1	1	5	2	5	5	3	2	2	2	1	2		2		2				1					40	18	22
15									1							1				1	3	3	2	2	3	1	2		1			1	2							23	3	20
16									1				1				1	1	2	2·5	4·5		2	2	2	2	1	2									1			25	8·5	16·5
17						1								1	1	2	1			·5	·5		2	2	1		2	1	2			1			1					19	6·5	12·5
18										1							1			2·5	3·5	1	1	2	1			1				2								16	4·5	11·5
19											1									2·5	1·5		1	1	1	1	1	2	1				1	1						15	3·5	11·5
20																					2	3		1				1			1									8		8
21																				1		1	2																	4	1	3
22																					1	1	1		1															4		4
23																				1	1												1							3	1	2
24																								1																1		1
25																					1	1																		2		2
26																												1												1		1
27																																										
28																																										
29																											1													1		1
Totals	1	2	7	5	4	14	13	11	20	24	23	26	14	27	35	44	57	63	98	195·5	227·5	118	113	93	64	67	38	40	36	26	24	24	18	12	9	6	6		1	1606	683·5	922·5

TABLE III.

Father and Adult Son.

Duration of Life of Father.

Duration of Life of Adult Son.	*23*	*28*	*33*	*38*	*43*	*48*	*53*	*58*	*63*	*68*	*73*	*78*	*83*	*88*	*93*	*98*	*103*	Totals
23	1	1	2	5	3	11	6	7	11	9	6	12	8	2		2		86
28				1	6	4	5	12	15	10	13	10	7		1	1		85
33			1	2	2	5	7	8	7	10	7	8	8	4	1			70
38		1	1	2		2	8	5	3	9	11	11	9	5	2	1		70
43		1			1	5	1	5	6	11	10	10	17	5				72
48			1	1	2	5	5	4	6	9	12	15	5	3				68
53		1		3		5	7	3	2	11	11	14	10	1	1	1		70
58			1	3		4	5	10	8	10	5	8	9	3		2		68
63		2	1	3	5	1	4	8	13	9	11	11	11	5				84
68			1	6	3	6	7	5	5	6	14	16	12	7	2			90
73		1		2	1	6	5	4	7	9	10	14	13	8	8	1	1	90
78			1	1	2	2	4	4	4	10	5	8	9	4		3		57
83				1	1	5	3	1	2	3	7	10	13	3	2	2		53
88		1				2	3		1	4	7	5	1	2		2		28
93						1				2	2							5
98						1						1		1	1			4
Totals	1	8	9	30	26	65	70	76	90	122	131	153	132	53	18	15	1	1000

TABLE IV.

Father and Minor Son.

Duration of Life of Father.

Duration of Life of Minor Son.	*26*	*31*	*36*	*41*	*46*	*51*	*56*	*61*	*66*	*71*	*76*	*81*	*86*	*91*	*96*	Totals
*0—1	4	12	17	20	24	32	32	48	56	42	43	39	16	6	3	394
2—3	1		9	10	10	11	10	21	17	21	17	14	10	2	1	154
4—5	1	2	2	4	2	5	4	12	9	11	11	5	3	2		73
6—7	1	1	1	2	2	2	7	5	8	6	7	4	4			50
8—9	2	1	1	1	1	1	3	4	3	5	3	7	4			36
10—11	1	1		3	3	1	5	2	6	3	7	1	1			34
12—13			3	2	1	2	4	3	4	5	8	2	5			39
14—15	1		1	4	3	1	4	1	3	6	9	6	2	1		42
16—17		1				3	2	9	3	1	7	4	3			33
18—19		2	2	1	2	6	4	8	9	7	9	3	3	2	1	59
20—21				2	2	2	2	3	2	3	1	7	4		1	29
Totals	11	20	36	49	50	66	77	116	120	110	122	92	55	13	6	943

* Centred at ·5 years.

TABLE V.

Father and Adult Daughter.

Duration of Life of Father.

Duration of Life of Adult Daughter.

	26	*31*	*36*	*41*	*46*	*51*	*56*	*61*	*66*	*71*	*76*	*81*	*86*	*91*	*96*	*101*	Totals
21	1	1	4	4	4	6	6	12	17	16	18	11	14	2			116
26	1		3	2	11	8	10	11	15	13	11	9	9	3			106
31	1	1	1	6	1	5	7	6	18	11	13	14	6	2			92
36	1		1	2	3	2	5	7	9	9	13	11	11	2	1	1	78
41		2	2	2	2	2	9	9	7	9	11	8	7	1	1		72
46		1		1	2		6	7	5	7	11	17	2	2	1		62
51		2	2		4	1	2	2	10	7	11	14	8	1			64
56			2		1	4	5	4	5	8	9	10	4	1			53
61		1		1	4	3	8	6	8	6	20	13	7	2	1		80
66			2		4	2	6	6	10	8	24	11	9	2			84
71	1			2	2	5	8	6	19	11	16	11	10	3	1	1	96
76				3	1	2	3	5	14	19	20	16	10	3			96
81					3	7	4	3	10	16	11	9	10	1	6		80
86				1	4	4	3		4	9	10	5	3	4	1		48
91				1		2	1		2	1	5	2	1	1		1	17
96				1		1		1	2	4		1		1		1	12
Totals	5	8	17	26	46	54	83	85	155	154	203	162	111	31	12	4	1156

TABLE VI.

Father and Minor Daughter.

Duration of Life of Father.

Duration of Life of Minor Daughter.

	26	*31*	*36*	*41*	*46*	*51*	*56*	*61*	*66*	*71*	*76*	*81*	*86*	*91*	*96*	Totals
**0—1*	7	10	16	24	12	22	18	38	38	40	45	39	19	4	2	334
2—3	1	4	6	7	11	9	2	12	20	10	16	7	11	1	1	118
4—5	1	2	4		7	6	7	11	10	4	6	10	1		1	70
6—7	1	3	2		2	6	4	3	11	3	4	5	4		1	49
8—9		1	3			6	5	6	6	1	6	3	2		1	40
10—11					3	3	1	3	5	3	8	2				28
12—13		1		1	3	2		2	4	4	4		3			24
14—15		1	1	5	3	4	2	5	6	2	6	3	3	2		43
16—17			3	1	3	2	6	4	4	3	8	4	5	1		44
18—19	1		4	2	4	7	2	9	11	6	8	3	4	1	1	63
20—21		1	2	2		3	1	3	3	8	6	1	9	1		40
Totals	11	23	41	42	48	70	48	96	118	84	117	77	61	10	7	853

* Centred at ·5 years.

TABLE VII.

Mother and Adult Son.

Duration of Life of Mother.

Duration of Life of Adult Son.	*22*	*27*	*32*	*37*	*42*	*47*	*52*	*57*	*62*	*67*	*72*	*77*	*82*	*87*	*92*	*97*	Totals
22		1	3	4	7	7	1	10	7	12	5	9	7	7	5	1	86
27	1	1	1	5	6	7	5	6	5	12	8	8	5	8	5		83
32		1	4	3	4	3	5	5	6	7	10	9	14	3	4		78
37		3	4	6	3	1	6	5	4	18	6	10	11	4	4		85
42		1	3	4	2	2	2	4	3	10	10	22	13	2	4	2	84
47		3	2	2	3	3	3	4	5	4	7	16	9	8	4	1	74
52	1		1	5	3	2	4	9	11	10	5	12	10	11	2		86
57			3	4	10	6	6	5	4	11	14	14	13	6	2		98
62		2	3	5	7	6	6	7	8	7	10	10	17	9	11		108
67		2	2	1	9	4	2	3	14	16	9	17	18	22	5	1	125
72	1	1	1	3	5	7	4	4	10	12	7	17	16	13	4	1	106
77	1			3		3	2	3	10	14	9	17	6	16	7	1	92
82		1	4	1	2	5	3	2	4	6	10	10	7	12	6	1	74
87		1				1	1	1	2	5	4	8	2	4	2	1	32
92									1		2	2		1			6
97										1			1				2
102											1						1
Totals	4	17	31	46	61	57	50	68	94	145	117	181	149	126	65	9	1220

TABLE VIII.

Mother and Minor Son.

Duration of Life of Mother.

Duration of Life of Minor Son.	*23*	*28*	*33*	*38*	*43*	*48*	*53*	*58*	*63*	*68*	*73*	*78*	*83*	*88*	*93*	*98*	*103*	Totals
**0—1*	9	13	34	30	20	29	20	27	39	27	34	49	19	16		1	1	368
2—3	4	7	8	23	8	5	12	9	8	21	12	8	7	2	1			135
4—5		2	6	6	7	4	4	7	3	3	4	5	2	2				55
6—7		1	4	2	3	3	4	5	2	2	1	2	4		1	1		35
8—9		1	2	2		2		2	2	2	4	9	2	2	1			31
10—11			2		1	2	1	2		4	4	2	4	1				23
12—13		1	1	2	2	1		2	2		2	2	6					21
14—15	1			1	3	5		2	4	2	3	8		1				30
16—17			1	2	1	1		4	2	2	1		4	1				19
18—19			5	3	5	4	4	1	4	6	5	7	3	2	1			50
20—21		1	2	3	3	3		6	1	1	2	3	1	1		1		28
Totals	14	26	65	74	53	59	45	67	67	70	72	95	52	28	4	3	1	795

* Centred at ·5 years.

TABLE IX.

Mother and Adult Daughter.

Duration of Life of Mother.

Duration of Life of Adult Daughter.

	21	*26*	*31*	*36*	*41*	*46*	*51*	*56*	*61*	*66*	*71*	*76*	*81*	*86*	*91*	*96*	*101*	Totals
21		1	2	5	7	2	7	5	9	11	9	10	11	8	2		1	90
26		1	3	6	5	6	4	2	10	8	5	13	10	12	2			87
31			3	4	1	4	2	6	3	8	17	12	8	10	3	1		82
36		1	2	5	4	7	4	6	8	4	16	8	13	13				91
41	1	1	2	1	4	2	2	4	7	6	12	12	8	7		1		70
46	1		1		3	1		3	3	6	8	8	11	8	2	1		56
51		1	1	3	2	8	3	2	4	8	6	7	4	4	4		1	58
56			1	2	1	4		2	7	4	6	8	5	4	3		1	48
61		1	1	3	2	4	2	1	4	12	6	12	18	6	4	1		77
66			2	1	2	4	2	5	5	7	14	16	14	7	5			84
71	1		1	2	1	4	3	3	4	13	8	13	14	13	12	3		95
76		3	2	1	3	2	1	3	5	5	12	12	20	13	3			85
81		1	1	3	2	3	1	2	1	11	10	10	14	9	7			75
86			2	2		1		2	1	5	6	6	4	10	3	1		43
91			1		1	2				1	3	4	2	1	2			17
96					1							2	2		1			6
Totals	3	10	25	38	39	54	31	46	71	109	138	153	158	125	53	8	3	1064

TABLE X.

Mother and Minor Daughter.

Duration of Life of Mother.

Duration of Life of Minor Daughter.

	21	*26*	*31*	*36*	*41*	*46*	*51*	*56*	*61*	*66*	*71*	*76*	*81*	*86*	*91*	*96*	*101*	Totals
*0—1	7	15	25	35	25	20	21	17	33	15	22	39	19	11	4			308
2—3	1	5	7	14	8	12	11	14	9	14	14	10	12	4	3	2		140
4—5		2	5	6	5	3	9	6	8	2	6	6	3		1		1	63
6—7			3	3	3	4	5	4	2	3	6	4	3	5		1		46
8—9		1	1	1	4	4	2	3	1	1	4	7	2	1				32
10—11		1	1	3	2	3	1	1	1		4		3	2	1			23
12—13				5	2	1	1	3	1		1	2	2	1				19
14—15				2	2	3		2	2	1	5	5	4	4	1			31
16—17		2	1	3	6	2	2	2	1	4	5	4	2	1	4			39
18—19		1	2	3	3	5			5	2	9	4	4	2	2			42
20—21		1		2	5	1	5	2	4	6	3	2	1	5	1		1	39
Totals	8	28	45	77	65	58	57	54	67	48	79	83	55	36	17	3	2	782

* Centred at ·5 years.

TABLE XI.

Adult Brothers (Asymmetrical).

Duration of Life of Elder Brother.

Duration of Life of Younger Brother.

	23	*28*	*33*	*38*	*43*	*48*	*53*	*58*	*63*	*68*	*73*	*78*	*83*	*88*	*93*	*98*	Totals
23	5	8	7	10	8	5	1	8	4	12	9	2	5	1			85
28	12	9	10	6	6	10	5	2	3	3	6		9	2			83
33	1	5	9	10	6	4	3	7	3	4	2	4	3	1		1	63
38	4		2	6	5	2	3	1	6	7	6		3				45
43	1	3	8	3	4	2	8	7	1	9	6	4	2	2			60
48	3	3	4	9	6	8	3	10	10	9	2	7	5	2			81
53	4	3	5	6	5	3	4	4	4	8	7	4	6	2		1	66
58	6	5	5	5	1	5	6	6	5	13	5	11	8	3	2		86
63		4	8	5	6	7	2	6	8	11	12	7	9	3			88
68	3	2	4	3	3	2	1	8	7	14	16	8	6	6	1		84
73	2		1	9		4	4	3	10	15	20	12	9	2	1	1	93
78	4	1	6	5	4	2	6	8	4	11	4	14	11	5	3	2	90
83	2		4	3	1	2	3	3	1	6	4	6	6	3	1		45
88	1		1	2			1			3	1	7	5	4		1	26
93										3			2				5
Totals	48	43	74	82	55	56	50	73	66	128	100	86	89	36	8	6	1000

TABLE XII.

Adult Brothers (Symmetrical).

Duration of Life of First Brother.

Duration of Life of Second Brother.

	23	*28*	*33*	*38*	*43*	*48*	*53*	*58*	*63*	*68*	*73*	*78*	*83*	*88*	*93*	*98*	Totals
23	10	20	8	14	9	8	5	14	4	15	11	6	7	2			133
28	20	18	15	6	9	13	8	7	7	5	6	1	9	2			126
33	8	15	18	12	14	8	8	12	11	8	3	10	7	2		1	137
38	14	6	12	12	8	11	9	6	11	10	15	5	6	2			127
43	9	9	14	8	8	8	13	8	7	12	6	8	3	2			115
48	8	13	8	11	8	16	6	15	17	11	6	9	7	2			137
53	5	8	8	9	13	6	8	10	6	9	11	10	9	3		1	116
58	14	7	12	6	8	15	10	12	11	21	8	19	11	3	2		159
63	4	7	11	11	7	17	6	11	16	18	22	11	10	3			154
68	15	5	8	10	12	11	9	21	18	28	31	19	12	9	4		212
73	11	6	3	15	6	6	11	8	22	31	40	16	13	3	1	1	193
78	6	1	10	5	8	9	10	19	11	19	16	28	17	12	3	2	176
83	7	9	7	6	3	7	9	11	10	12	13	17	12	8	3		134
88	2	2	2	2	2	2	3	3	3	9	3	12	8	8		1	62
93								2		4	1	3	3				13
98			1				1				1	2		1			6
Totals	133	126	137	127	115	137	116	159	154	212	193	176	134	62	13	6	2000

TABLE XIII.

Minor Brothers (Symmetrical).

Duration of Life of First Minor Brother.

Duration of Life of Second Minor Brother.	*0**	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	Totals
*0**	102	53	26	8	13	3	6	5	5	4	8	5	4	3	1	2	1	3	3	8	6	269
1	53	68	29	12	14	4	3	1	4	4	3	1	1	1	1	6	5	3	7	5	3	228
2	26	29	20	7	7	5	1	2	3		3	1	1	2	2	1		3	2	3	3	121
3	8	12	7	2	4	3			1	1	2	3		2	1	2	2		1	3	1	55
4	13	14	7	4	2	1	3	1	2		2	2	1				1		3	2	2	60
5	3	4	5	3	1			1				1						1	1		3	23
6	6	3	1		3				1				1			1		3	1	1		21
7	5	1	2		1	1			1	1	1				1			1		2	1	18
8	5	4	3	1	2		1	1					1					1				19
9	4	4		1				1				1	1	1								13
10	8	3	3	2	2			1						2	1	1				1		24
11	5	1	1	3	2	1				1		2		1					2	2		21
12	4	1	1		1		1		1	1						1		1				12
13	3	1	2	2						1	2	1						1		1		14
14	1	1	2	1				1			1											7
15	2	6	1	2			1				1		1					1		2		17
16	1	5		2	1														1			10
17	3	3	3			1	3	1	1				1	1		1					1	19
18	3	7	2	1	3	1	1					2					1				3	24
19	8	5	3	3	2		1	2			1	2		1		2				2	1	33
20	6	3	3	1	2	3		1										1	3	1	2	26
Totals	269	228	121	55	60	23	21	18	19	13	24	21	12	14	7	17	10	19	24	33	26	1034

* Centred at ·25 years.

TABLE XIV.

Adult Brother and Minor Brother.

Duration of Life of Adult Brother.

Duration of Life of Minor Brother.	*23*	*28*	*33*	*38*	*43*	*48*	*53*	*58*	*63*	*68*	*73*	*78*	*83*	*88*	*93*	*98*	Totals
*0**	21	14	16	17	15	21	17	10	13	16	15	5	6		1		187
1	16	13	19	12	6	9	11	4	13	12	8	4	4	1		1	133
2	6	5	7	4	6	11	5	8	7	6	6	8	5	1		1	86
3		4	5	5	2		4	1		1	2	1	1				26
4	3	4	3	6	2	2	4	2	6	2	1	1	1				37
5	2	2	2	1	1	3	4	1	1	1	1						19
6	5	4	2	2	2	2		2	1	3	1	2		2			28
7		3	1		6			1		3		2		2			18
8	1	5	3	5		1	2	3	1								21
9	4	2	1	1	3		1	1	3	2	2	1					21
10	1	1	2				1	1	2	5	1	2	1				17
11	1		2	3		3	1		2	1							13
12	1		1	1	1	1	1					1		1			8
13	2		1	2	2	1	2		1	1	1						13
14	2	1		1	1	1	1			3				1			11
15	1									3		2					6
16		3		2			2	1		2							10
17	3	1	1	1			2	1	1		3	1	2				16
18	1	5	2	2		2	1		2	1							16
19	7	4	2	4	4	2	1	2	2	3	3	6					40
20	4	5	2	1	5	2	1				2	3	2				27
Totals	81	76	72	70	56	61	61	38	55	65	46	39	22	8	1	2	753

* Centred at ·25 years.

TABLE XV.

Adult Sisters (Asymmetrical).

Duration of Life of Adult Elder Sister.

Duration of Life of Adult Younger Sister.	*23*	*28*	*33*	*38*	*43*	*48*	*53*	*58*	*63*	*68*	*73*	*78*	*83*	*88*	*93*	*98*	Totals
23	20	18	5	10	4	1	6	3	4	10	9	9	6	2			107
28	12	15	6	4	5	9	9	4	8	9	10	6	5	3	2		107
33	4	8	4	3	4	4	1	4	6	4	4	7	2				55
38	6	2	3	6	6	3	1	4	5	4	2	7	7				56
43	3	5	5	3	5	2	3	2	4	3	6	3	1	2			47
48	5	2	6	3	4	3	3	1	7	6	4	4	3	4	1		56
53	3	4	3	2	1		2	3	8	9	5	7	4	1			52
58	2	6	1	2	1	3	6	6	7	7	8	8	5	1	1		64
63	4	4	1	6	2	3	1	7	8	11	8	10	3	5	1		74
68	3	4	8	4	3	4	5	4	14	10	8	22	10	7	3		109
73	2	4	5	2		6	2	3	9	9	15	18	7	4	4		90
78	1	1	4	7	1	3	2	3	4	11	20	24	17	6			104
83	1	4	3	3	2	2		3	4	9	10	12	9	5	1	1	69
88		1		3	1	3			2	6	3	11	5	5	2		42
93						1	1	1	1	1	2	2	3	1	1		14
98				1					1			1		1			4
Totals	66	78	54	59	39	47	42	48	92	109	114	151	87	47	16	1	1050

TABLE XVI.

Adult Sisters (Symmetrical).

Duration of Life of First Adult Sister.

Duration of Life of Second Adult Sister.	*23*	*28*	*33*	*38*	*43*	*48*	*53*	*58*	*63*	*68*	*73*	*78*	*83*	*88*	*93*	*98*	Totals
23	40	30	9	16	7	6	9	5	8	13	11	10	7	2			173
28	30	30	14	6	10	11	13	10	12	13	14	7	9	4	2		185
33	9	14	8	6	9	10	4	5	7	12	9	11	5				109
38	16	6	6	12	9	6	3	6	11	8	4	14	10	3		1	115
43	7	10	9	9	10	6	4	3	6	6	6	4	3	3			86
48	6	11	10	6	6	6	3	4	10	10	10	7	5	7	2		103
53	9	13	4	3	4	3	4	9	9	14	7	9	4	1	1		94
58	5	10	5	6	3	4	9	12	14	11	11	11	8	1	2		112
63	8	12	7	11	6	10	9	14	16	25	17	14	7	7	2	1	166
68	13	13	12	8	6	10	14	11	25	20	17	33	19	13	4		118
73	11	14	9	4	6	10	7	11	17	17	30	38	17	7	6		204
78	10	7	11	14	4	7	9	11	14	33	38	48	29	17	2	1	255
83	7	9	5	10	3	5	4	8	7	19	17	29	18	10	4	1	156
88	2	4		3	3	7	1	1	7	13	7	17	10	10	3	1	89
93		2				2	1	2	2	4	6	2	4	3	2		30
98				1					1			1	1	1			5
Totals	173	185	109	115	86	103	94	112	166	218	204	255	156	89	30	5	2100

TABLE XVII.

Minor Sisters (Symmetrical).

Duration of Life of First Minor Sister.

Duration of Life of Second Minor Sister.	*0**	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	Totals
*0**	78	41	22	10	4	14	1	6	5	1		1	1	2	5	3	6	2	2	2	3	209
1	41	34	24	13	13	11	8	8	5	1	2	2	1	3	1	1	5	3	3	4	5	188
2	22	24	14	5	3	4	7	2	4	2	1		1	1	2	1	1	2	1	1	1	99
3	10	13	5	12	2	1		2	2	1	1	1				2	5		1	1	1	60
4	4	13	3	2	2		5		2	1	1				1		1	1	1	1	2	40
5	14	11	4	1				1	1						1				1	2	1	37
6	1	8	7		5			1				1	1					2			1	27
7	6	8	2	2		1	1		2	1	1		1			2		2	1	1	2	33
8	5	5	4	2	2	1		2		1	1				1	2	1	2	1			30
9	1	1	2	1	1			1	1			1							1			10
10		2	1	1	1			1	1			1				1			1	1		11
11	1	2		1			1			1	1								1		1	9
12	1	1	1				1	1								1				1		7
13	2	3	1													1						7
14	5	1	2		1	1			1							1	1	2		2		17
15	3	1	1	2				2	2		1		1	1	1	4		1	3	1		24
16	6	5	1	5	1				1						1		2	1			1	24
17	2	3	2		1		2	2	2						2	1	1			1		19
18	2	3	1	1	1	1		1	1	1	1	1				3			2	1		20
19	2	4	1	1	1	2		1			1		1		2	1		1	1			19
20	3	5	1	1	2	1	1	2				1					1					18
Totals	209	188	99	60	40	37	27	33	30	10	11	9	7	7	17	24	24	19	20	19	18	908

* Centred at ·25 years.

TABLE XVIII.

Adult Sister and Minor Sister.

Duration of Life of Adult Sister.

Duration of Life of Minor Sister.	*23*	*28*	*33*	*38*	*43*	*48*	*53*	*58*	*63*	*68*	*73*	*78*	*83*	*88*	*93*	*98*	Totals
*0**	16	16	7	5	6	7	8	10	14	13	6	13	7	4	1		133
1	16	5	12	8	10	3	6	5	7	7	3	4	5	3			94
2	4	7	9	3	2	3	3	2	1	3	6	8	1	1	1		54
3	2	1			1	3	2	1	3	2		2					17
4	4	2	1	1	3	3		2	1	1		1					19
5	4		2	1	2	1	1			1			1				13
6	9	2	1	1	2			2	3	1	5	2		1			29
7	5		1	1	1		1		2								11
8	1	4	1		1	1		2		3		3	1				17
9		1			1	2		1			1	1					7
10	2	1	1			1	1	1	1	2		1	1	1			13
11	1			1		1			1	1	1	2	1				9
12	2	3		1			1			1							8
13		2		4				1	1	1	2	3					14
14	2		2	1		1	1		1	2		1		2			13
15	1	2				1	1		2	1	2	3		1			14
16	2	2	1	2				1		1	3	1	1				14
17	2	3	2	1	1			1		1							11
18	1		2	2					3				2				10
19	4	2	1	4			2	3		4	1	2	2				25
20	5		2	1					2	2							12
Totals	83	53	45	37	30	27	27	32	42	47	30	47	22	13	2	0	537

* Centred at ·25 years.

TABLE XIX.

Adult Brother and Adult Sister.

Duration of Life of Adult Brother.

Duration of Life of Adult Sister.

	23	*28*	*33*	*38*	*43*	*48*	*53*	*58*	*63*	*68*	*73*	*78*	*83*	*88*	*93*	*98*	Totals
23	15	12	18	20	6	11	10	5	12	12	10	11	9	2			153
28	14	17	10	19	11	7	13	12	11	10	13	10	7	4			158
33	9	11	9	6	2	6	9	5	8	16	10	17	5	6			119
38	4	7	6	9	9	2	9	8	6	16	5	10	11	1	1		104
43	9	2	5	7	12	7	6	7	8	11	12	2	5	2			95
48	8	5	8	4	6	7	9	13	7	14	18	7	2	5		1	114
53	3	8	6	8	3	5	10	4	8	10	12	12	7				96
58	11	7	3	5	4	7	4	11	9	22	12	15	8	2	1		121
63	11	3	7	7	8	4	6	6	17	21	21	12	4	6			133
68	7	8	11	7	8	13	15	7	22	35	24	27	10	8	1	1	204
73	9	8	6	7	8	7	18	22	23	23	19	18	13	9		3	193
78	12	1	6	13	7	5	16	19	23	27	26	26	27	7	2	2	219
83	4	5	1	2	3	8	11	13	11	15	20	11	19	4	3		130
88		1	2	9	2	1	5	4	9	12	11	6	8	6	3		79
93		3	1		1		1	1	1	2	2	4	2	3			21
98				2						2			2		2		8
Totals	116	98	99	125	90	90	142	137	175	248	215	188	139	65	13	7	1947

TABLE XX.

Minor Brother and Minor Sister.

Duration of Life of Minor Brother.

Duration of Life of Minor Sister.

	*0**	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	Totals
*0**	50	41	15	8	3	4	6	1		2	6	1		1	1	1		5	2	3	1	151
1	30	41	24	16	5	3	4	2	2	2	4	1	1	5	1		1	2	2	3	4	153
2	15	19	8	4	5	6	3	1	1		2	2	1					2	2	2	2	75
3	7	10	8	2		2	3	2	3	3			2			1	1		1			45
4	7	5	5	1	1	3	2	1	1	1		1				1		2	2		4	37
5	6	9	7	6	1			1	1	2	1								1		1	36
6	3	2	3	4	1	1			1			1							1	2	2	21
7	7	1	1	1		1	3						1		1		1			1		18
8	2	6	3	4	1		1	1	1		1									3		23
9	1	3						1												1	1	7
10	5	5	3		2	1		1												1		18
11	1	2			1		1	1	1													7
12	1	3	2	1											1					1	3	12
13	4	3	1		2	1	2	1		1										2		17
14		1	1									2	1	1					1			7
15	3	1		1		1				1		1										8
16	4	2	2	1		1		2		1		1							1		1	16
17	1	4		1	1				1			1						1		1		11
18	2	1	3					1					1		1				1	1	2	13
19	2	4		1			1	1						1				1				11
20	1			1			1	1												1		5
Totals	152	163	86	52	23	24	27	18	12	13	14	11	7	8	5	3	3	13	14	22	21	691

* Centred at ·25 years.

TABLE XXI.

Adult Brother and Minor Sister.

Duration of Life of Adult Brother.

Duration of Life of Minor Sister.

	23	*28*	*33*	*38*	*43*	*48*	*53*	*58*	*63*	*68*	*73*	*78*	*83*	*88*	*93*	*98*	Totals
*0**	18	7	1	2	7	2	10	3	15	7	5	6	3	2	1	1	90
1	10	10	8	3	7	5	3	5	4	3	1	4	3				66
2	4	7	4	3	4	4	2	2	2	6	3	2	1				44
3	1			1	2	4		2	1			1					12
4	3	2		2	1					2	1						11
5	3	4	3		1	1			1	2	1		1	1			18
6	3	2	2	4	3	1	1	3			2	1					22
7	4	2	1	1					2	2							12
8	1	1			2		1				1	1					7
9				1							1						2
10	2				1		2	1	2	1		1					10
11	1				1		2										4
12	1			1	1							1					4
13	1				1		2	1		2	1	1		2			11
14	2				2			1		1		1					7
15	1		1	1				1		2	1						7
16	2		1		1	1	1	1		3		1	1				12
17		1	2	1	1			2	1								8
18	2	2	1	1				2		3		1	1				13
19	1	1				1											3
20	1	2	4	1						1	1	1					11
Totals	61	41	28	22	35	19	24	24	28	35	18	22	10	5	1	1	374

* Centred at ·25 years.

TABLE XXII.

Adult Sister and Minor Brother.

Duration of Life of Adult Sister.

Duration of Life of Minor Brother.

	23	*28*	*33*	*38*	*43*	*48*	*53*	*58*	*63*	*68*	*73*	*78*	*83*	*88*	*93*	*98*	Totals
*0**	24	13	11	8	11	7	9	1	5	10	9	10	6	4			128
1	8	14	8	10	9	2	3	3	7	1	5	2	2		1		75
2	6	4	5	5	2	1		1	2	2	3	4	3				38
3	3	3	1	3		1				1	4	3					19
4	6	5	2	2		2	2			3	2	1	2	1			28
5	2	1		2		1											6
6	6	3					1	1	1								12
7	1	2	1	2		1	1		2			3					13
8	4	3	2	2	1	2							1				15
9			1	2	1	1		1			1						7
10		1	1														2
11	1	1															2
12																	0
13	2	2		1	1				1			1					8
14	1	1		1							1						4
15	2		1	2				1	1	3		1	2				13
16	1								1	1	1	1					5
17	3	4		2		2					1	1	1				14
18	2	1	4	1		1	1	1					3				14
19	2	2				1	1			2	1		1				10
20	2	1		1		2		1	1								8
Totals	76	61	37	44	25	24	18	10	21	23	28	27	21	5	1	0	421

* Centred at ·25 years.

VARIATION IN AURELIA AURITA.

By EDWARD T. BROWNE.

Zoological Research Laboratory, University College, London.

In 1895 an article on the Variation of the Tentaculocysts of *Aurelia aurita* (Linn.), written by me, was published in the *Quarterly Journal of Microscopical Science.* The chief object of the research, suggested by Professor Weldon, was the finding out the variation in the number of tentaculocysts, and if a variation occurred among the Ephyrae to see how far it affected the adults. The material for that investigation was collected in Plymouth Sound by the officials of the Marine Biological Association in 1893 and 1894. The results showed that there was a considerable variation in the number of tentaculocysts. The number of tentaculocysts (marginal sense-organs) is definitely fixed in the larval stage, commonly known as an Ephyra. A normal Ephyra has eight marginal lobes, or arms, each with a terminal sense-organ (tentaculocyst), and a prolongation of the stomach, forming a canal, runs along the middle of each lobe to the sense-organ.

A variation in the number of lobes affects also the number of tentaculocysts and the number of radial canals. The three are distinctly correlated, except when a twinning of the tentaculocysts occurs. This is very rare and only occurred five times in 1000 adult specimens.

It is easy to detect cases of twinning in the adults but not in the larval Ephyrae. Occasionally an Ephyra is found having two tentaculocysts at the end of an arm and the canal bifurcated. In some specimens, no doubt, this is genuine twinning, but it is possible for the two tentaculocysts to become widely separated by the growth of the umbrella, and each to have its own canal system. I have, therefore, included twinning in the variation as it does not practically interfere with the results.

It may be taken for granted, that, whatever number of tentaculocysts an Ephyra starts life with, that number remains constant during the whole life of that individual and that no further increase in number takes place during development.

The specimens collected in 1893 belonged to the Ephyra stage and the examination of 359 individuals showed that 22·6 p.c. had either more or less than the normal number (eight) of tentaculocysts. (Table I.)

TABLE I.

Percentage Comparison of the Numerical Variation in the Tentaculocysts of the specimens taken in 1893, 1894, *and* 1898.

		1893	1894	1894	1898	1898	1898
		Ephyrae	Ephyrae	Adults	Series A Small Adults	Series B Large Adults	A and B combined
		359	*1116*	*383*	*2000*	*1000*	*3000*
Number of Tentaculocysts	*4*				0·05	0·2	0·1
	5		0·1		0·1	0·1	0·1
	6	1·1	0·5	0·5	0·6	0·8	0·7
	7	2·2	3·0	4·7	3·7	4·7	4·1
	8	77·4	79·1	77·3	79·8	77·1	78·9
	9	6·1	6·7	8·6	5·7	7·5	6·3
	10	5·0	5·5	4·2	4·5	5·2	4·8
	11	3·3	3·1	2·6	3·6	1·9	3·0
	12	3·9	1·5	1·8	1·2	1·8	1·4
	13	0·8	0·3		0·3	0·5	0·4
	14		0·1		0·1	0·1	0·1
	15			0·3	0·1	0·1	0·1

In the spring of 1894 a larger number of Ephyrae were taken and 1116 specimens examined. The tentaculocysts were found to vary in 20·9 p.c.

In the summer of 1894 large adult specimens were caught and in 383 individuals it was found that 22·8 p.c. showed variation in the number of tentaculocysts.

Race of Aurelia in the River Tamar.

During my visit to the Marine Laboratory at Plymouth in 1898 I was glad to hear from my friend Mr E. W. L. Holt that he had seen Aurelia in shoals at Saltash. This town is situated on the River Tamar and is about three miles from Plymouth Sound, into which the river flows. It was near the mouth of this river that the specimens were taken in 1893 and 1894, so it is very probable that they also belong to the Tamar race of Aurelia. The river is tidal for about 20 miles and its water is very muddy and polluted. How far above and below Saltash the Aurelia ascend and descend with the tide has not yet been definitely ascertained, but under ordinary conditions they do not come down with the tide into Plymouth Sound. The Aurelia were first discovered by Mr Holt at Saltash in 1897, and since then they have been seen yearly in the Tamar. I think that the Tamar Aurelia may be safely regarded as a local race, and that they are born in the waters of the Tamar and spend their whole lives in the river or its tributaries.

In Plymouth Sound Aurelia is not at all a common jelly-fish, and shoals are rarely seen.

2000 *Small Adults taken in April,* 1898. *The Range and Numerical Variation of their Tentaculocysts.* (*Series A.*)

On April 23rd, Mr Holt and I started operations to catch Aurelia and tried first Barnpool, a small bay at the mouth of the Tamar, but we only caught two specimens, one at the surface and another in a shrimp-trawl. We then proceeded up the Tamar and put out the trawl net near the mouth of the river Lynher, one of the tributaries of the Tamar, about half a mile below Saltash. With two hauls of the trawl nearly 400 specimens were taken. The muddy state of the water prevented an Aurelia from being seen at a depth of about two feet below the surface, and as a very few were visible the hand-nets were not often used. In the evening of the same day Mr Holt saw a shoal of Aurelia swimming with the tide, close to the surface, near the Railway Bridge at Saltash. The next evening in that locality we caught with hand-nets over 1000 specimens. On April 27th, a second visit was made and we captured about 800 specimens.

The specimens collected on these occasions ranged from large Ephyrae up to small immature adults not exceeding 40 mm. in diameter. Very few Ephyrae were present as it was about a month too late for them.

I examined 2000 specimens and measured the diameter of the umbrella. All the measurements were made after preservation and the umbrella was perfectly flat, resting on the bottom of an ordinary photographic dish.

The numerical variation of the tentaculocysts is shown in Table II. The range of variation is from 4 to 15 tentaculocysts, and 20·2 p.c. of specimens have either more or less than the normal number.

TABLE II.

The Numerical Variation of the Tentaculocysts of 2000 *Adults, collected in April,* 1898.

		Diameter of the Umbrella in Millimetres																				Total	Per-centage
Up to		*5*	*10*	*15*	*20*	*25*	*30*	*35*	*40*	*45*	*50*	*55*	*60*	*65*	*70*	*75*	*80*	*85*	*90*	*95*	*100*		
Number of Tentaculocysts	*4*			1																		1	0·05
	5		2																			2	0·1
	6		3	2	4	1	2															12	0·6
	7		13	20	29	8	5															75	3·75
	8		270	442	446	280	102	29	7	(normal)												1596	79·8
	9		17	30	38	17	8	4	1													115	5·75
	10		15	28	23	18	6	1														91	4·55
	11		17	19	17	12	5	1	1													72	3·6
	12		3	6	4	8	2	2														25	1·25
	13		3	2	1	1																7	0·35
	14		1				1															2	0·1
	15				1	1																2	0·1
Totals			344	550	583	346	131	37	9													2000	100 p.c.

1000 *Large Adults taken in June,* 1898. *The Range and Numerical Variation of their Tentaculocysts.* (*Series B.*)

About a month later, on May 27th, and June 2nd, we again visited Saltash for the purpose of collecting a supply of large adult specimens, sufficient to ensure the possession of 1000 specimens in good condition. We tried, at first, to catch them with a large trawl-net, which was kept expanded by the tide from the Laboratory steamboat "Busy Bee" at anchor. This method had to be abandoned as the Aurelia got crushed in the net and most of them were found badly torn and damaged. To obtain specimens in perfect condition we found that catching them, one by one, in a hand-net, as they floated past the steamer, was the best method. The specimens were taken within two feet of the surface; the river being too muddy to see anything at a greater depth. I feel much indebted to my friends, Mr E. W. L. Holt, Mr W. I. Beaumont, and Mr L. W. Byrne, for their kindness in catching nearly all the specimens; my share of the work was the preservation.

This series of specimens ranged from 20 mm. to 95 mm. in diameter, and most of the larger ones carried planulae in the pouches on the oral arms, showing that they had reached full maturity. The majority of the specimens were from 40 mm. to 65 mm. in diameter.

The numerical variation of the tentaculocysts is given in Table III. The range of variation is the same as that of the small adults collected in April, but the percentage of specimens showing a variation is greater, 22·9 p.c., being 2·7 p.c. more than in the small adults.

TABLE III.

The Numerical Variation of the Tentaculocysts of 1000 *Adults, collected in June,* 1898.

		Diameter of the Umbrella in Millimetres																				Total	Per-centage
	Up to	*5*	*10*	*15*	*20*	*25*	*30*	*35*	*40*	*45*	*50*	*55*	*60*	*65*	*70*	*75*	*80*	*85*	*90*	*95*	*100*		
Number of Tentaculocysts	*4*									1	1											2	0·2
	5										1											1	0·1
	6									1	3	1	1	1	1							8	0·8
	7						1	1	3	11	10	2	4	5	3	5	1	1				47	4·7
	8	(normal)				4	11	27	39	97	128	138	111	83	57	37	22	9	7	1		771	77·1
	9					1	3	2	4	13	12	12	8	4	6	6	1		2	1		75	7·5
	10								3	7	7	14	9	7	4		1					52	5·2
	11				1			1	1	1	4	2	3	5	1							19	1·9
	12						1		2	5	1	3	1	2	1	2						18	1·8
	13							2	1	1			1									5	0·5
	14												1									1	0·1
	15										1											1	0·1
Totals					1	5	16	33	53	137	168	172	139	107	73	50	25	10	9	2		1000	100 p.c.

The Combination of the Small and Large Adults.

The result of the combination of the small adults (Table II.) and the large adults (Table III.) is shown in Table IV. which is illustrated by a curve (1) showing the frequency of the tentaculocysts.

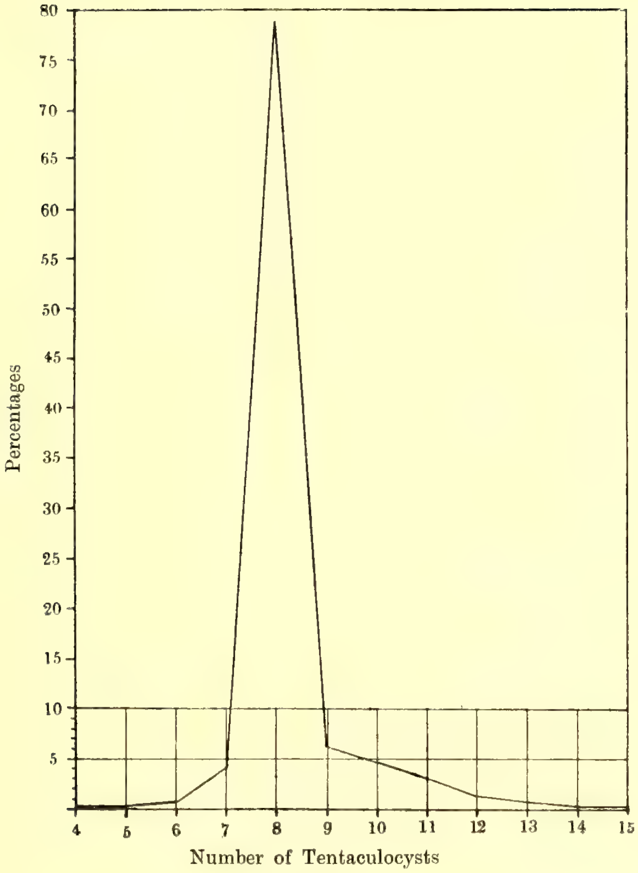
This combination gives a grand total of 3000 specimens belonging to the same race, and it shows that 21·1 p.c. have more or less than the normal number of tentaculocysts. I think that 21 p.c. may be regarded as the normal amount of variability of the Tamar race.

The table also shows that 4·9 p.c. of the specimens have less (4 to 7) than the normal number of tentaculocysts, and that 16·1 p.c. have an excess (9 to 15) of the normal number.

TABLE IV.

The Numerical Variation of the Tentaculocysts of 3000 *Adults.* (*Tables II. and III. combined.*)

		Diameter of the Umbrella in Millimetres																				Total	Per-centage
Up to		*5*	*10*	*15*	*20*	*25*	*30*	*35*	*40*	*45*	*50*	*55*	*60*	*65*	*70*	*75*	*80*	*85*	*90*	*95*	*100*		
Number of Tentaculocysts	*4*			1						1	1											3	0·1
	5		2								1											3	0·1
	6		3	2	4	1	2			1	3	1	1	1	1							20	0·67
	7		13	20	29	8	6	1	3	11	10	2	4	5	3	5	1	1				122	4·07
	8		270	442	466	284	113	56	46	97	128	138	111	83	57	37	22	9	7	1		2367	78·9
	9		17	30	38	18	11	6	5	13	12	12	8	4	6	6	1		2	1		190	6·33
	10		15	28	23	18	6	1	3	7	7	14	9	7	4		1					143	4·77
	11		17	19	18	12	5	2	2	1	4	2	3	5	1							91	3·03
	12		3	6	4	8	3	2	2	5	1	3	1	2	1	2						43	1·43
	13		3	2	1	1		2	1	1			1									12	0·4
	14		1				1						1									3	0·1
	15				1	1					1											3	0·1
Totals			344	550	584	351	147	70	62	137	168	172	139	107	73	50	25	10	9	2		3000	100



CURVE 1.

Percentage curve showing the numerical variation in the tentaculocysts of 3000 adults taken in 1898. (Table IV.)

Comparison between the Smallest and Largest Adults.

The measurement of the diameter of the umbrella of all the specimens was made in order that the variability of the smallest immature adults might be compared with that of the large mature ones.

The specimens have been separated into four groups for comparison (Table V.). The percentage of variability is practically the same in the smallest (5 to 10 mm.) and the largest (56—95 mm.) adults. Curve 2. The percentage of specimens with less than the normal number of tentaculocysts is almost the same in both groups and it therefore follows that that with the excess is also the same.

It appears, then, that neither a decrease nor an increase in the number of tentaculocysts is injurious to the race, and that no selection is taking place in the young and adult stages in the number of tentaculocysts.

TABLE V.

Comparison between the Smallest and Largest Adults taken in 1898.

Number of Tentaculocysts	5 to 15 mm. Umbrella-diameter		46 to 55 mm. Umbrella-diameter		5 to 10 mm. Umbrella-diameter		56 to 95 mm. Umbrella-diameter	
	Number of Specimens	Per-centage	Number of Specimens	Per-centage	Number of Specimens	Per-centage	Number of Specimens	Per-centage
4	1	0·11	1	0·29	0		0	
5	2	0·22	1	0·29	2	0·58	0	
6	5	0·56	4	1·18	3	0·87	3	0·72
7	33	3·69	12	3·53	13	3·78	19	4·58
8	712	79·65	266	78·24	270	78·49	327	78·79
9	47	5·26	24	7·06	17	4·94	28	6·75
10	43	4·81	21	6·18	15	4·36	21	5·06
11	36	4·03	6	1·76	17	4·94	9	2·17
12	9	1·00	4	1·18	3	0·87	6	1·45
13	5	0·56	0		3	0·87	1	0·24
14	1	0·11	0		1	0·29	1	0·24
15	0		1	0·29	0		0	
Totals	894	100	340	100	344	99·99	415	100

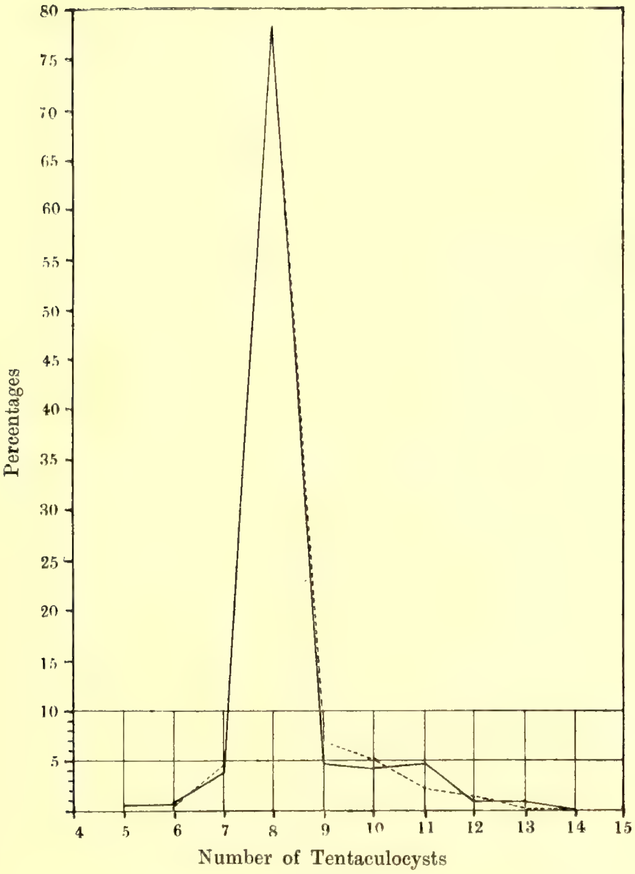
Combination of the Ephyrae and Adults taken in 1893, 1894, *and* 1898.

As evidence is in favour of the specimens taken in 1893 and 1894 belonging to the Tamar race of Aurelia, a Table (VI.) is given showing the results of combining all the specimens into one series, giving a total of 4858 specimens. These show that 21·29 p.c. have more or less than the normal number of tentaculocysts and the individual percentages compared with those of the 3000 taken in 1898 (Table III.) show a great similarity.

TABLE VI.

The Numerical Variation in the Tentaculocysts of the specimens taken in 1893, 1894, *and* 1898.

Year	Stage	Number of Specimens	Number of Tentaculocysts											
			4	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*
1893	Ephyra	359	0	0	4	8	278	22	18	12	14	3	0	0
1894	Ephyra	1116	0	1	6	34	883	75	61	35	17	3	1	0
1894	Adult	383	0	0	2	18	296	33	16	10	7	0	0	1
1898	Adult	3000	3	3	20	122	2367	190	143	91	43	12	3	3
Totals		4858	3	4	32	182	3824	320	238	148	81	18	4	4
Percentages			0·06	0·08	0·66	3·75	78·72	6·59	4·90	3·05	1·67	0·37	0·08	0·08



Curve 2.

Percentage curves of the smallest adults (5 to 10 mm. in diameter) and the largest adults (56—95 mm.) taken in 1898. (Table V.)

Continuous line for the smallest adults.

Dotted line for the largest adults.

Further Observations on the 1000 *Large Adults taken in June.* (*Series B.*) *A Correlation between the Radial Canals and the Tentaculocysts.*

The excellent condition of the large adults afforded a good opportunity of examining the radial canal system. The few figures given by Romanes and other observers show distinctly that there is a correlation between the radial canals and the tentaculocysts, and my observations confirm theirs.

In the 1000 adult specimens I found that every tentaculocyst (twinning excepted) had its own branched canal leading direct from the stomach, and always separated from the adjacent branched canals by a simple unbranched canal. By means of the canals the origin of the tentaculocyst could be traced home to either the perradial or interradial system.

An increase or decrease in the number of tentaculocysts usually leads to an asymmetrical arrangement of the canals, so that a tentaculocyst situated in a strictly adradial position may be traced by its canal to either the perradial or interradial system.

Three diagrams have been prepared to illustrate the correlation of the radial canal system and the tentaculocysts. Diagram I. shows the arrangement of the canals and tentaculocysts in a normal specimen. There are four branched perradial canals and four branched interradial canals which are separated by simple unbranched adradial canals. The branched canals are all of the same pattern, but show a considerable amount of variation.

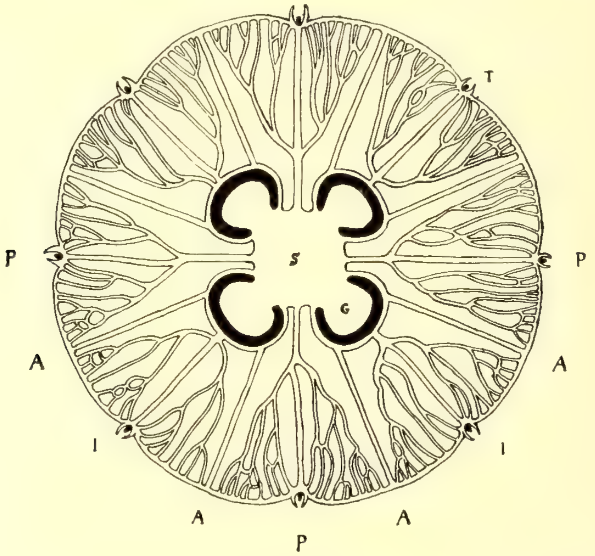


Diagram I.

The normal arrangement of the radial canal system in a normal specimen.

P, perradial. *I*, interradial. *A*, adradial. *G*, genital sac with gonad. *S*, stomach. *T*, tentaculocyst.

In the diagrams the oral arms are omitted (they are perradially situated) and also the tentacles which form a fringe round the margin of the umbrella.

The normal type is seen at the top of Diagram I. (perradial). There is a straight canal running direct from the stomach to the tentaculocyst, and it gives off two opposite branches, each of which is subdivided into many branches, all entering the ring canal. There are many modifications of this type and several are illustrated in the diagrams. The lateral branches are often not opposite each other, and in some specimens the main canal looks like a branch from one of the lateral canals.

The interradial branched canal is of the same pattern as the perradial. In some specimens it looks however quite different, but it is only a modification of the normal type and probably due in many cases to the outward growth of the genital sacs cutting off the base of the canal. Instead of one canal leaving the stomach there are three. The central one runs direct from the stomach to the tentaculocyst and the lateral ones are branched.

The adradial canals are always simple and unbranched and never run to a tentaculocyst.

Specimens which have less than the normal number of tentaculocysts have usually a corresponding decrease in the number of branched radial canals. If one perradial tentaculocyst be absent, the branched canal is also absent and its place taken by a simple unbranched canal. The loss of one or more branched canals does not produce a vacant space, the adjacent branched canals spread out and occupy a larger area.

A decrease in the number of tentaculocysts may occur through injury done to the umbrella. This can, as a rule, be detected by a careful examination of the margin with a lens. The presence of a branched canal without a tentaculocyst is suspicious, and if there were any traces of an old injury which had healed up, leaving a slight or an irregular margin to the umbrella, the specimen was rejected as a damaged one.

In a few specimens a branched canal without a tentaculocyst was found with the margin in a perfect condition and these were considered cases of genuine variation.

Anastomosing of the radial canals by cross branches forming a kind of network occurs in nearly all the large adults. It is rarely present in the small adults. The amount of anastomosing is very variable and not always regularly distributed amongst the radial canals. Often one or two quadrants in a specimen show considerable anastomosing, whilst the other quadrants are comparatively free from it. In two specimens, having the same sized umbrella, one may show a considerable amount of anastomosing, the other practically none at all. The adradial canals are often anastomosed to the branches of the perradial and interradial canals.

The Position of the Tentaculocysts in Specimens having a Decrease in Number.

In my previous publication on Aurelia there is a sentence which is rather misleading and requires an explanation. "An examination of the specimens (adults) does not show that any particular position on the margin of the umbrella is favoured either by an increase or decrease of the tentaculocysts." If the position of the tentaculocysts on the margin of the umbrella were marked down on skeleton diagrams, no attention being paid to the radial canal system, then the above statement would be correct. The material upon which that statement was based (adults of 1894) was not in first class condition and no attempt was made to examine the radial canal system, but only the number of tentaculocysts was marked on skeleton diagrams.

In the 1000 adults (Series B) there are 58 specimens (5·8 p.c.) with less than the normal number of tentaculocysts and the majority show that the absent tentaculocyst belongs to the perradial system.

The exact position of the absent tentaculocysts cannot be conveniently shown by the use of tables, but full details are given below. Specimens with less than the normal number of genital sacs have usually a decrease in the number of radial canals and tentaculocysts (Table VII.).

The Position of the Tentaculocysts in Specimens having an Increase in Number.

No less than 171 specimens (17·1 p.c.) show an increase in the number of tentaculocysts compared with 5·8 p.c. showing a decrease. This excess is one of the characteristic features of the variation and occurs in every series examined (Tables I. and VI.) and it clearly shows that the tendency is towards an increased number of sense-organs. It was to find out whether the perradial or the interradial series were more subject to variation that the radial canal system was examined, as it was the only reliable means of exactly locating the original source of the tentaculocysts. A tentaculocyst may be adradially, or nearly adradially, situated on the margin of the umbrella, but its canal may curve and join the perradial system or it may curve the opposite way and belong to the interradial system.

I have not called any tentaculocysts or branched canals adradial, though they may be in some specimens adradially situated. The increase in the perradial system is simply a case of doubling or trebling the number of canals and tentaculocysts; in the interradial system only doubling occurs. To call such canals and tentaculocysts adradial would imply that the interradial canal and tentaculocyst were absent, and this is not the case (Diagram III.).

The examination of the canal system shows that the increase is generally perradial and details are given on page 102.

Specimens with more than the normal number of genital sacs have usually an increase in the number of radial canals and tentaculocysts (Table VII.).

If a very slow and gradual change is taking place in the number of tentaculocysts then the tendency is towards the establishment of a race with ten tentaculocysts, due to an increase of two opposite perradial tentaculocysts arranged thus:—

Perradial	2 . 1 . 2 . 1.
Interradial	1 . 1 . 1 . 1.

If no check occurs and the variation still continues in the perradial system the ultimate result will probably be a race with twelve tentaculocysts arranged thus:—

Perradial	2 . 2 . 2 . 2,
Interradial	1 . 1 . 1 . 1,

the medusa having eight perradial tentaculocysts and canals, four interradial tentaculocysts and canals, and eight adradial unbranched canals.

A Detailed Account, giving the Position of the Tentaculocysts in the Specimens (Large Adults belonging to Series B) having more or less than the Normal Number.

Four Tentaculocysts. (Two specimens.)

Perradial	0 . 0 . 0 . 0.
Interradial	1 . 1 . 1 . 1.

One specimen with only four interradial tentaculocysts.

The interradial branched canals occupy the four quadrants. There are no adradial canals, but four simple unbranched canals, perradially situated. It is probable that in this specimen the perradial canal system is absent and the simple canals, perradially situated, are the representatives of the adradial system removed from their proper position.

Perradial	1 . 1 . 1.
Interradial	0 . 0 . 1.

One specimen with three genital sacs and three oral arms.

Five Tentaculocysts. (One specimen.)

Perradial	1 . 0 . 1.
Interradial	1 . 1 . 1.

This specimen has three genital sacs and three oral arms.

Six Tentaculocysts. (Eight specimens.)

Perradial	1 . 0 . 1 . 0.	1 . 1 . 0 . 0.
Interradial	1 . 1 . 1 . 1.	1 . 1 . 1 . 1.

Two specimens with two perradial tentaculocysts absent.

Perradial	1 . 0 . 1 . 1.
Interradial	1 . 1 . 0 . 1.

Two specimens with one perradial and one interradial tentaculocyst absent.

Perradial	2 . 0 . 2 . 0.
Interradial	0 . 1 . 0 . 1.

The above specimen is bilaterally symmetrical. In each half of the umbrella there are three branched canals with tentaculocysts.

Perradial	1 . 1 . 1.
Interradial	1 . 1 . 1.

Three specimens with three genital sacs and three oral arms.

Seven Tentaculocysts. (*Forty-seven specimens.*)

Perradial	0 . 1 . 1 . 1.
Interradial	1 . 1 . 1 . 1.

Thirty specimens with one perradial tentaculocyst absent.

Perradial	1 . 1 . 1 . 1.
Interradial	0 . 1 . 1 . 1.

Fourteen specimens with one interradial tentaculocyst absent.

When a perradial or an interradial tentaculocyst is absent the branched radial canal is usually absent and in its place is a simple unbranched canal.

Perradial	1 . 1 . 1.	1 . 2 . 1.
Interradial	1 . 2 . 1.	1 . 1 . 1.

Three specimens with three genital sacs and three oral arms.

Nine Tentaculocysts. (*Seventy-five specimens.*)

Perradial	2 . 1 . 1 . 1.
Interradial	1 . 1 . 1 . 1.

Sixty-three specimens have the extra tentaculocyst perradially situated.

In the place of a single branched perradial canal there are two perradial branched canals usually occupying about the same space as a normal single branched canal.

In two specimens the increase of tentaculocysts is due to twinning.

There is one normal branched canal with two tentaculocysts very close together, just separated by a few marginal tentacles.

Perradial	2 . 0 . 2 . 1.
Interradial	1 . 1 . 1 . 1.

One specimen with two extra opposite perradial tentaculocysts and one perradial tentaculocyst and branched canal absent.

Perradial	2 . 1 . 1 . 1.	2 . 1 . 1 . 1.
Interradial	2 . 1 . 1 . 0.	2 . 1 . 0 . 1.

Three specimens with an extra perradial tentaculocyst, and an extra interradial in one quadrant, but one absent in the adjacent or opposite quadrant.

Perradial	1 . 1 . 1 . 1.
Interradial	2 . 1 . 1 . 1.

Five specimens have an extra interradial tentaculocyst.

In one specimen the increase is due to twinning.

Three specimens with more than the normal number of genital sacs.

One with six genital sacs and six oral arms.

One with five genital sacs and five oral arms.

One with three genital sacs and three oral arms, and the tentaculocysts arranged thus:—

Perradial	3 . 2 . 2.
Interradial	0 . 2 . 0.

Ten Tentaculocysts. (*Fifty-two specimens.*)

Perradial	2 . 2 . 1 . 1.
Interradial	1 . 1 . 1 . 1.

Nineteen specimens have an extra tentaculocyst in adjacent quadrants.

Perradial	2 . 1 . 2 . 1.
Interradial	1 . 1 . 1 . 1.

Seventeen specimens have an extra tentaculocyst in opposite perradii. In one specimen the increase is due to twinning which occurs in the opposite perradii.

Perradial	3 . 1 . 1 . 1.
Interradial	1 . 1 . 1 . 1.

Three specimens have two extra perradial tentaculocysts in the same perradius.

Perradial	2 . 1 . 1 . 1.
Interradial	1 . 1 . 2 . 1.

Six specimens with an extra perradial and an extra interradial tentaculocyst.

Seven specimens have more than the normal number of genital sacs and oral arms.

Five with six genital sacs and six oral arms. Radial canals and tentaculocysts arranged thus:—

Perradial	1 . 0 . 1 . 0 . 1 . 1.	1 . 0 . 1 . 1 . 1 . 1.
Interradial	1 . 1 . 1 . 1 . 1 . 1.	1 . 1 . 1 . 0 . 1 . 1.

Two specimens with five genital sacs and five oral arms. Radial canals and tentaculocysts arranged thus:—

Perradial	1 . 1 . 1 . 1 . 1.
Interradial	1 . 1 . 1 . 1 . 1.

Eleven Tentaculocysts. (*Nineteen specimens.*)

Perradial	2 . 2 . 2 . 1.
Interradial	1 . 1 . 1 . 1.

Eleven specimens have three extra perradial tentaculocysts. In one of the specimens the increase of one tentaculocyst is due to twinning.

Perradial	2 . 1 . 2 . 1.	2 . 2 . 1 . 1.
Interradial	1 . 2 . 1 . 1.	1 . 2 . 1 . 1.

Two specimens have two extra perradial tentaculocysts and one extra interradial.

Perradial	1 . 2 . 1 . 1.
Interradial	1 . 2 . 1 . 2.

One specimen with one extra perradial and two extra interradial tentaculocysts.

Perradial	1 . 1 . 1 . 1.
Interradial	2 . 2 . 2 . 1.

One specimen with the normal number of perradial tentaculocysts, but with three extra adjacent interradial tentaculocysts.

Five specimens with more than the normal number of genital sacs.

One with ten genital sacs and ten oral arms.

Three with six genital sacs and six oral arms.

One specimen with five genital sacs and five oral arms.

Twelve Tentaculocysts. (*Eighteen specimens.*)

Perradial	2 . 2 . 2 . 2.
Interradial	1 . 1 . 1 . 1.

Six specimens have an extra tentaculocyst in each of the four perradii.

Perradial	3 . 2 . 1 . 2.	3 . 2 . 2 . 1.	3 . 3 . 1 . 1.
Interradial	1 . 1 . 1 . 1.	1 . 1 . 1 . 1.	1 . 1 . 1 . 1.

Three specimens have extra tentaculocysts all perradially situated, but only in two or three perradii as shown in the above formulae.

Perradial	2 . 2 . 2 . 1.	2 . 2 . 1 . 1.
Interradial	2 . 1 . 1 . 1.	1 . 2 . 1 . 2.

Three specimens have extra perradial and interradial tentaculocysts as shown in the above formulae.

Six specimens have more than the normal number of genital sacs.

Four with six genital sacs and six oral arms. Tentaculocysts arranged thus:—

Perradial	1 . 1 . 1 . 1 . 1 . 1.
Interradial	1 . 1 . 1 . 1 . 1 . 1.

Two with five genital sacs and five oral arms. Tentaculocysts arranged thus:—

Perradial	1 . 1 . 1 . 1 . 1.
Interradial	1 . 2 . 1 . 2 . 1.

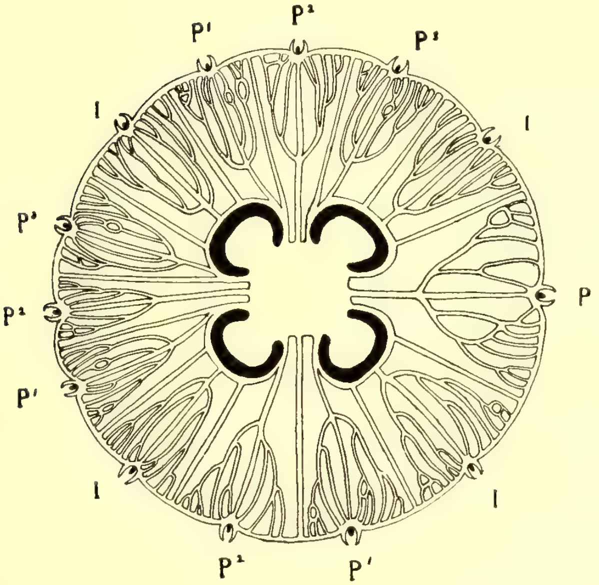


DIAGRAM II.

The arrangement of the radial canals in a specimen with thirteen tentaculocysts.

Perradial 3 . 3 . 1 . 2.
Interradial 1 . 1 . 1 . 1.

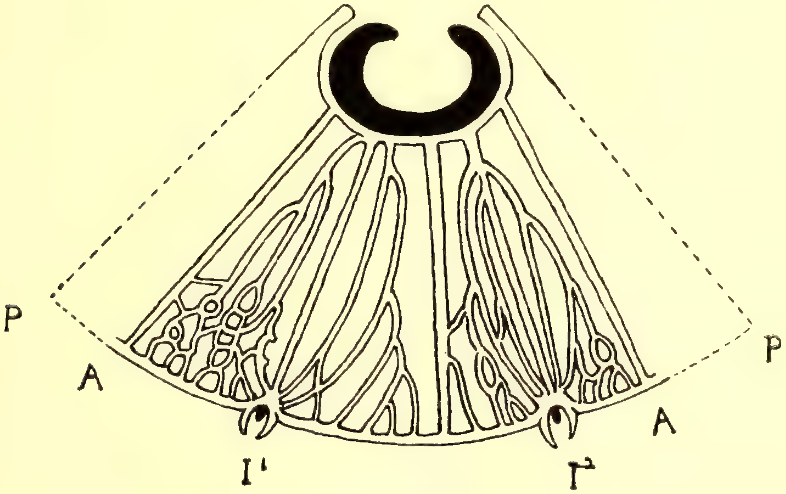


DIAGRAM III.

The arrangement of the interradial canals when an increase occurs in the number of interradial tentaculocysts.

Thirteen Tentaculocysts. (*Five specimens.*)

Perradial	3 . 2 . 2 . 2.	3 . 3 . 1 . 2.	
Interradial	1 . 1 . 1 . 1.	1 . 1 . 1 . 1.	(Diagram II.)

Two specimens with five extra perradial tentaculocysts.

Perradial	2 . 2 . 2 . 2.
Interradial	2 . 1 . 1 . 1.

One specimen with four extra perradial tentaculocysts and one extra interradial.

Two specimens with more than the normal number of genital sacs.

One with six genital sacs and six oral arms.

One with five genital sacs and five oral arms.

Fourteen Tentaculocysts. (*One specimen.*)

This specimen has eight genital sacs and eight oral arms.

Fifteen Tentaculocysts. (*One specimen.*)

Perradial	2 . 1 . 3 . 3.
Interradial	1 . 2 . 1 . 2.

There are five extra perradial and two extra interradial tentaculocysts.

Numerical Variation of the Genital Sacs of 3000 *Adults taken in* 1898. (*Series A and B.*)

Previous accounts by other writers on the Variation of Aurelia have been mostly confined to the variation of the genital sacs, and numerous illustrations have been published. (Ballowitz, 1899.)

In the Ephyra stage the genital sacs are not present, but their position and number are marked by the bundles of gastric filaments. Normally there are four interradial bundles of filaments. The gastric filaments are not figured in the diagrams of the adults. They are always situated along the inner side of the gonads.

In the small Ephyrae an increase or decrease in the number of gastric filaments indicates that the specimen will probably have an increase or decrease in the number of genital sacs, as the numbers of bundles and gonads are correlated. It is not however easy to count the number of bundles of gastric filaments in the small Ephyrae, especially when there are indications of an increase, as then owing to the closeness of the bundles they form occasionally an unbroken ring. When an Ephyra has developed into the adult form and the gonads begin to appear, and also the genital sacs, then any variation in the number of gonads or genital sacs is easily seen.

The 3000 specimens taken in 1898 show that 2·4 p.c. have more or less than the normal number (four) of genital sacs. The range of variation is from 2 to 10, and most of the specimens show an increase in number (Table VII.).

TABLE VII.

The Numerical Variation of the Genital Sacs of 3000 *specimens and its relation with the Numerical Variation of the Tentaculocysts.*

		Number of Genital Sacs									Abnormal Cases
		2	*3*	*4* Normal	*5*	*6*	*7*	*8*	*9*	*10*	
Number of Tentaculocysts	*4*	1	1	*(1)*							2
	5		2	*(1)*							2
	6		6	*(14)*							6
	7		5	*(117)*							5
	8		2	*(2363)*	1			1			4
	9		1	*(185)*	2	2					5
	10			*(131)*	4	8					12
	11			*(73)*	3	10	4			1	18
	12			*(30)*	3	10					13
	13			*(10)*	1	1					2
	14			*(2)*				1			1
	15			*(1)*			2				2
Totals		1	17	*(2928)*	14	31	6	2	0	1	72

There is a perfect correlation between the number of genital sacs and the number of oral arms, and I have not found any exceptions; a specimen with six genital sacs has always six oral arms.

There is not a perfect correlation between the number of genital sacs and the number of radial canals and tentaculocysts, though a decrease in the number of genital sacs is usually accompanied by a decrease in the number of radial canals and tentaculocysts, and an increase by an increase in the number of the radial canals and tentaculocysts.

In the 3000 adults 21·1 p.c. show a variation in the number of tentaculocysts and 2·2 p.c. of these have an abnormal number of genital sacs. But the variation in the number of genital sacs has a considerable effect upon the numerical variation of the tentaculocysts. Only four specimens out of 72 with an abnormal number of genital sacs have eight tentaculocysts.

The adults collected in 1894 (383 specimens) showed 2·08 p.c. with an abnormal number of genital sacs.

Method used for the Preservation of Aurelia.

The large adults were placed in buckets containing Formaline 5 p.c. solution in sea-water. (Formaldehyde 5 c.c. and sea-water 95 c.c.) They quickly died in a good state of expansion. When the bucket was full of specimens about 100 c.c. of strong Formaldehyde was added. The specimens were allowed to soak in this for six hours and occasionally stirred up. After this preliminary soaking in Formaline they were removed, one by one, and placed in a mixture of Formaline and Chromic Acid.

Formaline, 10 p.c. in freshwater	9 volumes.
Chromic Acid, 5 p.c. solution	1 volume.

They were left in this mixture for 24 hours; occasionally stirred up and a little strong Formaldehyde added. It is very important that the specimens should be thoroughly saturated with Formaline. The specimens were removed from the Chromic-Formaline solution and finally stored in bottles containing Formaline 10 p.c. in freshwater. I prefer storing in a 10 p.c. solution of Formaline, as there is then a fair margin against deterioration of strength.

These specimens are now three years old and are still in excellent condition. The addition of Chromic Acid greatly improves the specimens. The opaque whiteness, produced when Formaline only is used, is not so conspicuous and the jelly becomes more translucent. The specimens are also tougher and more pliable and can be handled without fear of damage. The yellowish-brown colour due to the Chromic Acid gradually disappears owing to oxidation and the specimens turn to a pale bluish-green. An ordinary black xylonite photographic dish is an excellent thing to examine the specimens in. The black background shows up the radial canal system.

REFERENCES.

Ballowitz, E., 1899. "Ueber Hypomerie und Hypermerie bei Aurelia aurita." *Archiv für Entwickelungsmechanik der Organismen*, Bd. VIII. pp. 239—252.

(This paper contains a good Bibliography and summary of the literature relating to the variation of Aurelia.)

Browne, E. T., 1895. "On the Variation of the Tentaculocysts of Aurelia aurita." *Quart. Journ. Micro. Sci.*, Vol. XXXVII. pp. 245—251.

A FIRST STUDY OF NATURAL SELECTION IN CLAUSILIA LAMINATA (MONTAGU).

By W. F. R. WELDON.

Among the terrestrial mollusca of Europe are many species which have certainly inhabited their present areas of distribution for a very long time*. The shells of some species are and have been exceedingly variable, while others are, and have apparently for long periods remained, almost invariable.

The problems presented by species belonging to either category are of great interest in connection with the theory of natural selection; and the characters which can be studied in recent and in fossil shells alike are often well fitted for numerical treatment. Such characters as the shape of the spire, the number of ridges and furrows on a given whorl, the size and shape of the aperture, can often be expressed in such a way as to admit of numerical comparison between individuals; these are the characters used in the discrimination of "species"; and in the present state of our knowledge they afford excellent examples of specific characters which appear to us "useless."

A typical example of a species which appears to have varied very little during a long period of time is *Clausilia laminata* (Montagu): and I wish now to record an attempt to determine the variability of one element of its spire.

If the shell of an adult *Clausilia laminata* be carefully ground upon a soft stone, it is possible to expose a section passing with sensible exactness through the plane which contains the axis of the spire. The columella is in many cases approximately straight, except near its lower end, where it bends abruptly and supports the clausilium. This bend in the columella is associated with a change in the character of the spiral, leading to the formation of the oblique aperture of the finished shell, but above the point where the bend occurs the columella may without serious error be treated as a conical tube, some ten or twelve millimetres long, and about half a millimetre in diameter at its lower (broader) end. If a shell be ground on a flat stone, until the middle of this long and narrow tube is

---

* For a discussion of the history of land shells in the Palaearctic Region, see Kobelt; *Studien zur Zoogeographie*, Wiesbaden 1897–98.

apparently reached through practically its whole length, the section exposed will be very near indeed to that containing the real axis of the spire. Such a section will have the appearance shown in Fig. 1; and from it we can ascertain the elements which determine the "pitch" and certain other characters of the spire.

The shell is of course essentially a tube, coiled round an axis so that the successive coils are in contact. The wall of the tube nearest to the axis forms the columella; that remote from the axis forms the outer surface of the shell. Where the successive whorls are in contact, a double septum is formed, running from the columella to the outer surface of the shell. The line of contact of successive whorls along the outside of the shell describes what we will call the "peripheral spiral"; the line of contact along the axial wall may be called the "columellar spiral*." If we know the pitch of these two spirals, and their distance apart at every point, we know the elements which determine all the important characters of the spire, except the "ventricosity," which is measured by the longitudinal curvature of the outer walls, for example by the convexity of the portion *BDE* in Fig. 1.

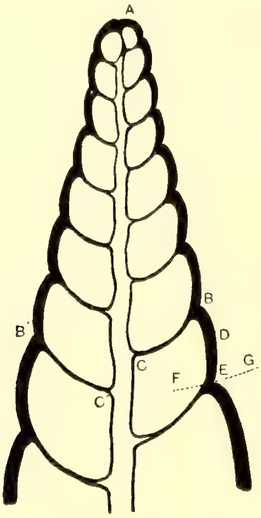


Fig. 1.

Now in a median longitudinal section through the shell, the relations of any point on the peripheral spiral, such as *B*, Fig. 1, to the corresponding point on the columellar spiral, *C*, and to the apex of the shell *A*, are fully determined if we know the lengths of the sides of the triangle formed by joining the three points *A*, *B* and *C*. If such a triangle be supposed to revolve round the columella, the length of each side changing as it revolves, while the angle *A* remains always

---

* I have deliberately used the word "spiral" to describe a curve in three-dimensional space, which mathematicians would call a "helix," in order to use a term which may be applied to pulmonates generally, without the inconvenience which would result from speaking for example of the "helix" of *Helix aspersa*.

at the apex of the shell, the angle *B* will describe a spiral which will be identical with the peripheral spiral of the shell if the law of change of the side *AB* is the same as that of the peripheral radius vector of the shell; and by finding an appropriate law of change for the sides *AC* and *BC*, the angle *C* may be made to describe the columellar spiral, while the side *BC* maintains the two spirals at their proper distance apart. We may therefore consider the two fundamental spirals, upon which the main characters of the shell depend, as generated by a triangle revolving round a long and narrow cone, so that one side always touches the cone, and one angle always lies at the apex of the cone, the length of each side changing as the triangle revolves.

In actual sections of shells, such as that drawn in Fig. 1, the peripheral and columellar spirals are each cut from 17 to 21 times; so that the elements of the fundamental triangle can be determined a great number of times. Further, since the section is approximately flat, the interval between any position *ABC* and the next position *AB′C′* in which the triangle can be measured, corresponds to a revolution through 180°. From such a number of data it should be possible to determine the law of change of the triangle with considerable accuracy. The measurements required are somewhat laborious, and I have not at present determined the rate of change of every side of this fundamental triangle, but only that of the side *AB*, the radius vector of the peripheral spiral. Such a determination leaves the distance between the columellar and peripheral spirals uncertain: it only determines that the radius of the peripheral spiral at any point lies somewhere on the arc of such a circle as that shown at *FEG*, Fig. 1. The results of this determination seem to me of some interest, so that I venture to publish them before the other measures are completed. A reason for considering these measures separately from the others is that they do not seem to be affected by a curvature of the upper part of the columella, which is often sensible, and sometimes considerable. The shells measured were chosen at random, and include some in which the columella is very sensibly bent.

The chief material used was collected during the summer of last year and during the present summer in the great beech-wood known as Der Holm at Gremsmühlen on the Dieck See, a lake on the eastern border of Holstein. In this wood *Clausilia laminata* was extremely abundant, occurring together with *C. biplicata*. Other species also occurred, but none were so common as *C. laminata*. The conspicuous species of *Helix* were *H. lapicida*, *H. nemoralis*, *H. arbustorum*, *H. hispida* and *H. rotundata*, all of which were exceedingly common. The forest slopes down to the very edge of the lake, and the moist nature of the ground where the *Clausilia* were collected is shown by the frequent presence of species of *Succinea*. The soil is the light sandy alluvium so common throughout Holstein, and the whole wood is probably less than 150 feet above the sea level.

The peripheral radii were measured in sections of 100 adult shells from this wood. The measures were recorded to 0·01 millimetre, and I believe they are fairly trustworthy. They were made with an instrument designed for me by

Mr Horace Darwin, and constructed by the Cambridge Scientific Instrument Company, which consists essentially of a long and flat piece of plate glass capable of movement in a plane, parallel to itself, under two compound microscopes. The part of the glass plate which moves under one microscope carries a scale, divided into tenths of a millimetre, while the microscope over it has a vernier in the eye-piece; the part of the plate moving under the second microscope carries the object to be measured, the microscope over it having cross wires in the eye-piece. The object is adjusted so that the organ to be measured is parallel to the line of motion of the glass plate, and the scale is read when first one extremity of the organ measured, and then the other, lies under the intersection of the cross wires, the difference between the two readings of the scale giving the required length. The only serious source of error is a slight uncertainty which sometimes exists about the exact position of the apex; but this does not, I believe, involve an uncertainty of 0·01 mm. in any but the uppermost measures, which are unfortunately also the shortest. An inspection of Fig. 1 will show that a small uncertainty in the position of A (which can only affect its position in a direction perpendicular or nearly so to the axis of the shell) will not have an important effect upon the estimated length of any radius inclined at an angle of less than 45° to the columella. The error from obliquity in the plane of the sections measured is certainly too slight to affect measures involving only quantities of the order of 0·01 mm.

Before the measures of the radii of two shells can be compared, it is necessary to find some way of determining the plane in which the section of either shell cuts it. It is clear that an infinite number of planes can cut the shell so as to pass through the axis, and there is no obvious means of finding out which of these contains the actual section measured. There is therefore no means of knowing the angle through which the first radius measured in any section has revolved before meeting the plane of the section. Since the angular distance of the plane of the section from the origin of the spiral cannot be determined, it is necessary to choose some arbitrary plane to which each section may be referred; and after consultation with Professor Pearson, all sections were referred to a plane containing a radius of the columellar spiral 5 mm. in length.

The distance of such a plane from the plane of any actual section was found by interpolation between two adjacent columellar radii. For example in Fig. 1, if the columellar radius AC were exactly 5 mm. in length, the plane of the section would exactly coincide with the plane of reference; if however AC were (as in the first individual of the series) 4·68 mm. long, and the next columellar radius AC' were 5·27 mm. long, then the plane of the section would be inclined to the plane of reference at a certain angle. Assuming the columellar spiral to be sensibly equiangular through 180° (an assumption fairly in accord with observation) the angle between the plane of the section and the plane of reference may be taken to be

$$\frac{5-4{\cdot}68}{5{\cdot}27-4{\cdot}68}\times 180^\circ = 0{\cdot}5424\times 180^\circ;$$

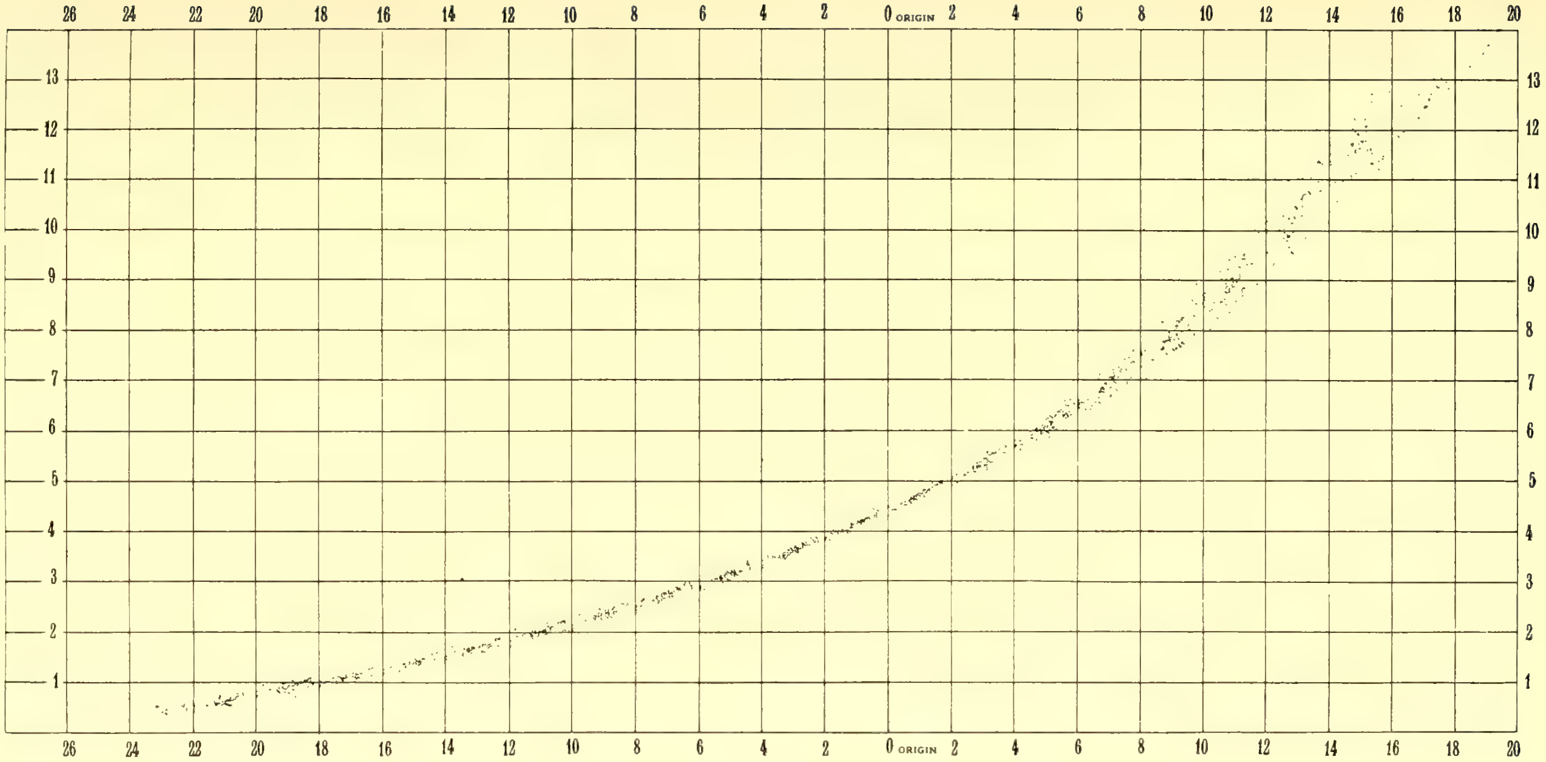


Fig. 2.

Diagram showing the length of every peripheral radius measured in 100 adult shells of *Clausilia laminata* from Gremsmühlen. The ordinate of each dot gives the length of a radius, the abscissa giving its angular distance from the plane of the standard columellar radius 5 mm. long. Each vertical unit of the scale denotes one millimetre of radial length; each horizontal division denotes two right angles of angular distance. Angular distances from the plane of the standard columellar length towards the apex of the shell are negative, those towards the mouth of the shell are positive.

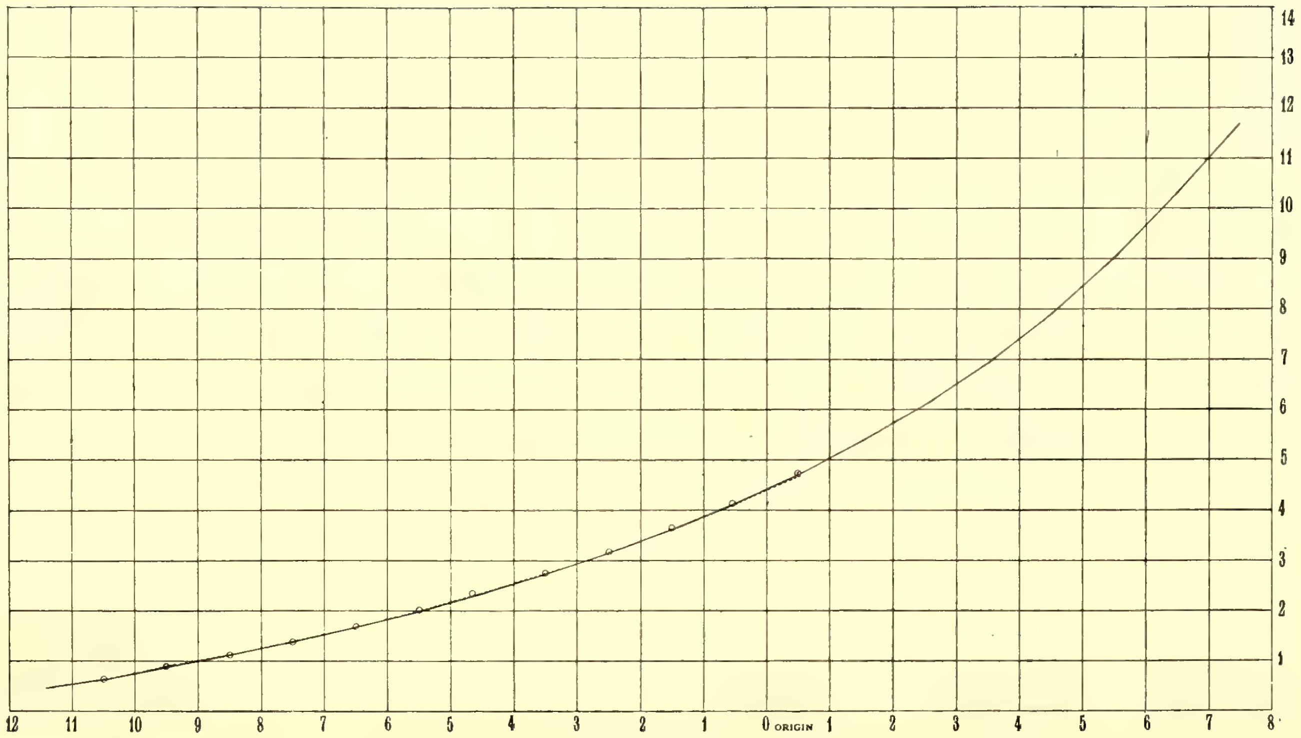


Fig. 3.

The continuous line shows the mean length of peripheral radii, at any angular distance from the standard columellar radius, in 100 adult shells from Gremsmühlen ; the dotted line shows the mean length of corresponding radii in 100 half-grown shells from the same place. The dots surrounded by circles show the mean length of peripheral radii in the upper whorls of 32 adult shells from Risborough.

Vertical scale as in Fig. 2. One unit of horizontal scale=180°.

and other sections may be treated in a similar way. Since all the measures in any section are known to be 180° apart, the position of one, relatively to the plane of reference, determines that of all the others.

By the means described, the position of every radius measured, with reference to the standard plane, was determined: and the length of each radius, in its proper position, is shown in Fig. 2, where the ordinate of every dot gives the length, and the abscissa the distance from the standard plane, of a single radius. The whole number of dots in the figure should be 1949, since this was the whole number of radii measured in the hundred shells: but many of the measures coincide in a diagram of this scale, so that the actual number of dots visible is smaller.

From these observations it was possible to determine a curve, representing a fairly close approximation to the law connecting change in the radius of the peripheral spiral with change in angular distance from the standard columellar radius. The determination was attempted in the following way. The observations were first sorted into groups, such that no two observations in any group differed by 180° in angular distance from the standard plane; the "centroid" of each group,—the point whose ordinate is the mean length of all the radii in the group, while its abscissa is the mean angular distance of all these radii from the standard plane—was determined, and this point was considered to lie upon the curve required. An inspection of Fig. 3 shows that the curve required is uniformly convex to the base line, so that the centroid of any segment lies in reality outside and above it. Small portions of the curve are however so nearly straight that the error introduced by the process adopted is not serious, at least in that portion of the curve which represents the upper part of the shell.

For reasons which will presently be pointed out, I have not determined the probable error of the mean radius length in every group, but only in six of them. The mean radius length of every group, together with the probable error of the six groups for which it was determined, is given in the following table:

TABLE I.

Mean Length of Peripheral Radius and Mean Angular Distance from the Columellar Plane, for each group of measures of 100 *adult Clausilia laminata.* (*Every mean value determined from* 100 *observations, unless otherwise stated.*)

(Angular distances *above* the plane of the standard columellar radius, towards the apex of the shell, are called negative.)

Angular Distance from Standard Columellar Radius	Mean Radial Length (mm.)
−11·4148 × 180°	0·4595 (37 observations)
−10·4906 × 180°	0·6254 (98 „)
− 9·50528 × 180°	0·8807
− 8·50528 × 180°	1·1237
− 7·50528 × 180°	1·3873
− 6·50528 × 180°	1·6813
− 5·50528 × 180°	1·9978
− 4·50528 × 180°	2·3443 ± 0·00815 mm.
− 3·50528 × 180°	2·7314 ± 0·00902 mm.
− 2·50528 × 180°	3·1504 ± 0·00905 mm.
− 1·50528 × 180°	3·6238 ± 0·00986 mm.
− 0·50528 × 180°	4·1366 ± 0·01054 mm.
+ 0·49472 × 180°	4·7206 ± 0·01147 mm.
+ 1·49472 × 180°	5·3599
+ 2·49472 × 180°	6·0885
+ 3·49472 × 180°	6·9119
+ 4·49472 × 180°	7·8879
+ 5·49472 × 180°	9·0029
+ 6·49472 × 180°	10·3057
+ 7·47936 × 180°	11·6877 (84 observations)
+ 8·56985 × 180°	12·4938 (26 „)
+ 9·66845 × 180°	13·5175 (4 „)

The regularity of the values obtained for the mean peripheral radius of the adult spiral made it seem that even so small a number of individuals might serve as a basis for comparison between young and mature individuals of the race. The way in which these shells are formed makes it less difficult to compare the mean magnitude and the variability of a character in young and in mature shells than it is in the case of most animal organs. For it is well known that the outer layers of the shell in *Clausilia* and other pulmonates are secreted by the growing edge of the mantle once for all; and that when the mantle edge has secreted these outer layers, the only subsequent change which can be effected is due to the deposition or absorption of the deepest layer, involving change in the thickness of the shell, but no change in its external shape. The only changes which the outer layers can undergo are those due to erosion by weather, or to such absorption of the supporting inner layer as may cause some portions of the shell to be broken away from the rest. Neither of these disturbing influences affects *Clausilia laminata* in Gremsmühlen to any sensible extent: and we may therefore conclude that the upper whorls of an adult shell represent the condition of the young shell, from which this adult was formed by the addition of new material, in a practically unaltered form. This conclusion may not justify us in comparing the very last, newly formed portion of a growing shell with the corresponding part of an adult, but except at this point, there is no doubt of its validity.

Such an unaltered record of the young condition of the shell, borne by every adult examined, is of great value as a means of estimating the incidence of natural selection: for by measuring the mean and the standard deviation of radii of the spiral in the upper part of a series of adult shells, we obtain a measure of the mean length and the variability of the radii exhibited by these shells when they were still young; and in this way we can estimate the condition of the spiral in a series of young individuals, of which we can say with certainty that every one was capable of attaining the adult condition. In a sufficiently large sample of young shells, we are sure that we have a number of individuals which will die before they attain maturity; and therefore a comparison between the mean character and variability of these young shells, and the mean character and variability of the upper whorls of adult shells, enables us to determine whether there is a correlation between the character of the spiral, and the death-rate, or not. For if the death-rate during growth affects individuals with all sorts of spirals to the same extent, then there will be no change in the mean character and variability of the spiral after the time of its first formation, whether we examine it in half-grown individuals, soon after it has been formed, or in adults. If, on the other hand, individuals with different kinds of spirals die at different rates during growth, then either the mean length or the standard deviation of radii, or both, will change: so that we shall certainly not get the same value for *both* these characters in young and in adults, if the character of the spiral is being acted on by any process of selective elimination.

In order to compare the radii of young shells with those of the upper whorls of adults, one hundred half-grown individuals from Gremsmühlen were ground and measured, the measures being treated in the manner already described. The smallest of these individuals was 5·3 mm. long, the majority being from 6 to 7 mm. long. The length of the largest was 8·8 mm., but only three were more than 8 mm., the greater number being less than 7 mm. long, the mean length of an adult shell being about 15 mm.

The mean radius-length, with its mean angular distance from the plane of the standard columellar radius, is given for every group in Table II. The probable error of the mean radius-length has been determined for the six lowest groups, which contain the last-formed radii. These groups are those for which the probable error is given in adults also (see Table I.).

TABLE II.

Mean Length of Peripheral Radius, and Mean Angular Distance, of each group of measures in 100 *half-grown Clausilia laminata.* (*Every mean value determined from* 100 *observations, unless otherwise stated.*)

Mean Angular Distance from Standard Columellar Radius	Mean Length of Peripheral Radius
−11·31214 × 180°	0·4808 (50 observations)
−10·45494 × 180°	0·6482 (97 „)
− 9·45872 × 180°	0·9132
− 8·45872 × 180°	1·1446
− 7·45872 × 180°	1·4154
− 6·45872 × 180°	1·7021
− 5·45872 × 180°	2·0367
− 4·45872 × 180°	2·3770 ± 0·00950
− 3·45872 × 180°	2·7683 ± 0·01035
− 2·45872 × 180°	3·1838 ± 0·01043
− 1·45872 × 180°	3·6489 ± 0·01137
− 0·49560 × 180°	4·1457 ± 0·01205 (91 observations)
+ 0·49048 × 180°	4·6952 ± 0·01489 (62 „)

It is difficult to compare the values of the mean radii directly in young and in adults, because the mean angular distance is not the same in corresponding groups of measures. The mean angular distance is however so close to $\frac{180}{2}^\circ$ in each case, that the value of the ordinate at that point, found by linear interpolation, will have a probable error only slightly greater than that of the entries in Tables I. and II. The necessary interpolations have been performed in Table III., the mean radius for the lowest (youngest) group of measures of the half-grown individuals being left unchanged, and the corresponding adult radius found by interpolation.

TABLE III.

Mean Peripheral Radius at Corresponding Points on the Spiral of 100 *Young and* 100 *Adult Clausilia.*

Angular Distance from Standard Columellar Radius in right angles	Mean Peripheral Radius	
	Adult	Young
−19	0·8820	0·9022
−17	1·1251	1·1350
−15	1·3889	1·4044
−13	1·6820	1·6903
−11	1·9996	2·0229
− 9	2·3463	2·3630
− 7	2·7336	2·7521
− 5	3·1529	3·1666
− 3	3·6265	3·6297
− 1	4·1397	4·1254
+0·9810	4·7181	4·6952

With probable errors of the order indicated by Tables I. and II., it is unlikely that any of these differences are significant. Even in the case of the last pair of entries the difference, although it is considerable (0·0229 mm.), is less than twice the probable error of the determination.

There is therefore no evidence of change in the mean character of the peripheral spiral during growth: on the contrary, the mean value of a peripheral radius is seen to be the same, within the limits of error of the determination, whether the radius be measured shortly after its formation, in young shells, or a long time afterwards, in the upper whorls of adult shells.

The individuals of the two samples measured may be considered, with sufficient accuracy, to represent samples of two successive generations; and the result obtained may be taken to show that the mean spiral of the young generation is sensibly identical with that of the parental generation, and is not altered during growth by any process of selective destruction. That is to say, individuals of *Clausilia laminata*, taken from a very old beech-wood, in a country which the species is known to have inhabited at all events since pre-glacial time, are in such equilibrium with their surroundings that the mean character of the shell spiral (so far as we have studied it) is the same from generation to generation, and is not changed during the course of a generation by any process of selective destruction.

If the view of inheritance, formulated by Mr Galton* and generalised by Professor Pearson†, be true, the mean character of a race can be fixed by natural selection in a far shorter time than is generally supposed; and we should be prepared to find local races in which the mean is sensibly stable through successive generations, in areas which have been inhabited even for a comparatively short period. It is, however, not necessary that the variability of such a race should be the same at all periods of growth. Professor Pearson has pointed out that if the law of inheritance referred to be approximately true, then a process of selection, so stringent that only individuals which accurately correspond to a particular type are allowed to breed, will fix this type as the mean of every generation after it has operated for a small number of generations; but even after such a selective process has operated for an indefinite time, the variability of young will be greater than that of the parental generation by an amount depending on the variability of the race at the time when selection began to operate. So that we may generally expect to find a reduction in the variability of a race during growth, long after selection has rendered the mean constant‡. Such selective destruction, which reduces the variability in every generation without changing its mean character, is what Professor Pearson calls "periodic selection."

In order to discover whether periodic selection is acting upon *Clausilia laminata* in Gremsmühlen or not, it is necessary to determine the variability of the

---

* Galton; *Roy. Soc. Proc.* Vol. 61, p. 402.

† Pearson; *Roy. Soc. Proc.* Vol. 62, p. 400.

‡ For a discussion of the residual variability here referred to, see Pearson; *Grammar of Science*, p. 384, and *Phil. Trans.* 1901 (in course of publication).

peripheral spiral at corresponding points of young and of adult shells. The proper measure of this variability is the standard deviation of the peripheral radii, in a number of individuals, measured at the same angular distance from the standard columellar radius, or the standard deviation of all the measures falling within a very narrow vertical strip of the diagram, Fig. 2. It would be impossible to find this standard deviation directly, without measuring many thousands of individuals; and the labour involved in preparing even a small series of measures is so great that a sufficiently large series for the purpose seems unattainable. Some indirect way of comparing the variability of young and of adults must therefore be found.

Before describing the method adopted in comparing the variability in young and in mature shells, a remark must be made. The variability of different parts of the peripheral spiral, as it appears from the distribution of the dots on Fig. 2, differs greatly in different regions. It is smallest in the immediate neighbourhood of the standard columellar radius, and increases as we pass away from this in either direction. The variability here indicated is not the variability of radii measured in different individuals at constant angular distance from the origin of the peripheral spiral: on the contrary, it is largely determined by the correlation between the different parts of the peripheral spiral and those radii of the columellar spiral which are nearly 5 mm. in length. The relative variability would be largely altered if we chose another columellar radius by which to determine the plane of reference. The artificial character of the variability which is here exhibited does not prevent us from using it to determine the probable error of the values obtained, or to establish a comparison between two sets of individuals which have been treated in exactly the same way.

If we consider any group of measures, such as those lying between two of the vertical divisions of Fig. 2, the straight line which gives the best linear approximation to their distribution is

$$y - y_0 = \frac{r_{xy}\sigma_y}{\sigma_x}(x - x_0),$$

where y_0 is the mean radial length in the group, σ_y the standard deviation of radial lengths; x_0 is the mean angular distance of the group from the standard columellar radius, σ_x the standard deviation of angular distances, and r_{xy} is Galton's function, measuring the correlation between radial length and angular distance from the standard radius*. The standard deviation of an "array" of radii within the group, whose angular distance is constant, is $\sigma_y\sqrt{1 - r^2_{xy}}$; and this will be taken as the measure of the variability of spiral radii at any point.

The values of r_{xy}, σ_y, and $\sigma_y\sqrt{1 - r^2_{xy}}$ for the six lowest groups of radii measured in the young shells, and for the corresponding groups of radii in adults, are given in Table IV. I wish here to express my gratitude to Miss Alice Lee, D.Sc., who has generously spent much time and labour in checking the whole

---

* Cf. Yule; *Roy. Soc. Proc.* Vol. 60, pp. 477—489.

of the computations connected with these Tables. Miss Lee has corrected several important arithmetical blunders of mine, and without her kind help I should certainly not have been able to publish this paper for some time. The results which follow from a study of this table seem to me of great importance; and such credit as may be due for obtaining them belongs far more to Miss Lee than to myself.

TABLE IV.

Values of r_{xy}, σ_y, *and* $\sigma_y\sqrt{1-r^2_{xy}}$ *for corresponding groups of Peripheral Radii in Young and in Adult-shells from Gremsmühlen.*

Angular Distance from Columellar Radius in right angles	r_{xy}		σ_y		$\sigma_y\sqrt{1-r^2_{xy}}$	
	Adult	Young	Adult	Young	Adult	Young
−8 to −10	0·7527 ±0·0292	0·7922 ±0·0251	0·120849	0·140872	0·07956 ±0·00379	0·08597 ±0·00412
−6 to −8	0·8068 ±0·0235	0·8374 ±0·0202	0·133731	0·153473	0·07901 ±0·00377	0·08388 ±0·00402
−4 to −6	0·8534 ±0·0183	0·8622 ±0·0173	0·134119	0·154627	0·06991 ±0·00333	0·07833 ±0·00375
−2 to −4	0·8726 ±0·0161	0·8782 ±0·0154	0·146252	0·168564	0·07143 ±0·00341	0·08062 ±0·00386
0 to −2	0·9290 ±0·0092	0·8800 ±0·0159	0·156270	0·170469	0·05784 ±0·00276	0·08096 ±0·00388
0 to 2	0·9415 ±0·0077	0·9367 ±0·0105	0·170046	0·173881	0·05732 ±0·00273	0·06087 ±0·00292

From Table IV. we see that the standard deviation of peripheral radii in the group from 8 to 10 right-angles from the standard columellar radius is nearly the same, both in young and in adult individuals—the small difference actually observed being hardly greater than the probable error of the determination. At every point, however, the variability of young shells is greater than that of adults; during the six right-angles of revolution immediately above the columellar radius the excess of variability in the young is always from about two to more than four times as great as the probable error of the determination.

The chances against an apparent excess of variability in a sample of young shells, so large as that recorded in the table, are very great, unless we admit that there is a real difference in variability between the newly-formed whorls of growing shells and the corresponding whorls of adults; and the necessary consequence of such an admission is that the variability of these newly-formed whorls is reduced after their formation by a process which destroys individuals with abnormal shells more rapidly than others, so that a process of "periodic selection" occurs.

If the result indicated by the observations we have discussed were true, without any qualification, then we might expect the variability of the very last-formed portion of a growing shell to be greater than that of any portion which precedes it; but the last line of Table IV. does not give the impression that this is the case. Of the young shells measured, 48 were so young that the last measurable part of their spire occurred above the point at which the columellar radius is 5 mm. long; but in 62 shells it was possible to measure a radius below this point: and the variability of these 62 radii appears, from the last line of Table IV., to be hardly, if at all, greater than that of the corresponding radii of adult shells.

The evidence of this small group of measures does not seem to me sufficient to outweigh that of the rest, because on the one hand the probable error of the determination is relatively large and on the other hand, an explanation of the apparent contradiction between the two sets of evidence may very possibly be found in some property of the last whorl of a growing shell. There are two obvious differences between the last-formed whorl of such a shell, and those which precede it. The last whorl is quite sensibly thinner than the others, because the inner, "nacreous" layer is not yet secreted in such quantity as in the higher parts of the shell; and the last whorl has nothing attached to its lower surface. Now, in the formation of a further whorl, the lower surface of the last-formed whorl will have a new layer of shell closely appressed against it: and my friend Professor Pearson has suggested to me that the strain on the shell, during this process, may cause a sensible increase in its variability. I think it very likely that some explanation of this kind will be found after further study of the last-formed whorl, which I hope shortly to undertake. In the meantime I publish the observations as they stand. They seem to me to show clearly that the mean character of the spiral radius is stable from generation to generation in Gremsmühlen, and that it is not being changed by selection during the growth of a young generation: and they seem to me to show further that the variability of young shells is diminished in every generation by a process of "periodic selection." Such selection is, of course, "indirect": that is to say, the life or death of the individual is determined in each case by the value of a (probably large) number of correlated characters, of which the length of the peripheral radius is only one.

I am aware that to many people it will seem absurd that the life or death of an individual should be supposed to depend upon differences of structural character so slight as those just described. The species of *Clausilia* show, however, if possible more clearly than other species, the way in which small and apparently unimportant differences of structure are associated with the difference between the survival and the total extinction of a race in a particular locality: and if so great a difference in death-rate as this is correlated with the apparently trifling differences which separate species, the hypothesis suggested by measurement of the spiral does not seem to me ridiculous.

At the risk of wearying the reader by repeating notorious facts, I wish to illustrate the relation between geographical distribution and specific characters by reference to a country which I know better than I know Holstein: and I therefore publish, in Table V., the mean lengths of the peripheral spiral radii in the upper whorls of a small series of adult *C. laminata* from a beech-wood in the neighbourhood of Monk's Risborough, in Buckinghamshire.

TABLE V.

Peripheral Radii of 32 *Adult Clausilia laminata from Risborough.*

Angular Distance from plane of Standard Columellar Radius in right angles	Mean Peripheral Radius
−19	0·8967
−17	1·1318
−15	1·4036
−13	1·6888
−11	2·0240
− 9	2·3686
− 7	2·7685
− 5	3·1741
− 3	3·6576
− 1	4·1395

The measurements of this race are not yet completed, and it is not worth while to discuss the probable errors of the small number of measures available; but a comparison between the results so far obtained and those recorded in Table III. (see also Fig. 3, p. 114) gives the impression that the races at Gremsmühlen and Risborough are sensibly identical in the mean character of their peripheral spiral.

The environmental elements common to the beech-wood near Monk's Risborough, where these shells were found, and the wood at Gremsmühlen, are those directly connected with the presence of beech-trees, and apparently few others. The wood (or copse) at Risborough is 800 feet above the sea level, on the steep side of a chalk down: it is of very small extent, and the beech-trees are tiny and young compared with the magnificent old trees at Gremsmühlen. The dryness of the ground is shown by the total absence of *Succinea*; while a difference of conditions important to terrestrial molluscs is shown by the abundance at Risborough of *Cyclostoma elegans*, a species which cannot live in the sandy plain of North Germany. The species of *Helix*, mentioned as common in Gremsmühlen, also occur in the wood at Risborough, but the prevalent colour varieties of *H. nemoralis* are different, and there are well-marked differences between the races of *H. arbustorum* in the two localities. In addition to these species, *H. aspersa*, which does not occur east of the Rhine, and *H. cantiana*, which occurs round Hamburg, are common at Risborough, but absent from Gremsmühlen. The small size of the Risborough wood, and the consequent absence of true forest conditions, permit the free entrance of *H. variabilis.* The only species of *Clausilia* which occur are *C. laminata* and *C. nigricans.*

The species of *Helix* mentioned, and *Clausilia nigricans,* are found in the hedges or meadows which surround the little wood at Risborough, but *Clausilia laminata* occurs only in the beech-wood. A similar relation between *Clausilia laminata* and *C. nigricans* can be demonstrated on many of the English chalk downs. In the places where beech-trees occur, the two species are found together; but *C. laminata* does not occur, or occurs but rarely, away from beech-trees, while *C. nigricans* is found in other places also.

These two races of *C. laminata* show *first* that races with a sensibly identical spiral exist in localities so widely separated that no crossing between their ancestors can have occurred for an immense period of time, and so different that the environmental conditions common to both are comparatively few; *secondly* that the existence of *C. laminata* is possible under conditions which permit the existence of *C. nigricans*; but that the latter species can live and multiply in places where the conditions necessary for the life of *C. laminata* do not occur. Now the differences between these two species are (1) a difference in size; (2) a difference in the pitch of the spiral; and (3) other slight differences in the shape of the shell and of various organs. All these differences seem to us, at present, of little physiological importance, and yet the sum of these differences is demonstrably associated with an enormous difference in susceptibility to certain environmental differences: and since this is the case we are justified in accepting evidence which points to a correlation between variation in death-rate and variation in a character within the limits of one species, although we are at present quite unable to imagine the process by which this correlation is brought about.

MISCELLANEA.

I. Change in Organic Correlation of *Ficaria ranunculoides* during the Flowering Season.

Among the numerous statistical studies we owe to Professor Julius MacLeod is one which deals with the change in correlation between the number of stamens and the number of pistils in *Ficaria ranunculoides* during a single flowering season [Over de Correlatie tusschen het aantal meeldraden en het aantal stampers bij het Speenkruid (*Ficaria ranunculoides*): *Botanisch Jaarboek*, Jaargang XI. 1899]. The number of stamens, and the number of pistils, were determined in each of a series of flowers borne by certain plants at the beginning of the flowering season, and again in a series of flowers from the same plants at the end of the season. The correlation between the number of stamens and the number of pistils differed widely in the two cases. The observations were made on the same plants (or their asexual offspring) during two successive seasons, and similar results were obtained on each occasion. The coefficient of correlation was in each case lower at the beginning of the flowering season, higher at the end; so that the same plants are shown to exhibit a rhythmical change in the amount of correlation between two important sets of organs. This fact is of very great interest in its bearing upon our knowledge of organic correlation in general: and it is to be regretted that Professor MacLeod should have expressed his valuable result in a manner which fails to exhibit its full importance, through his adoption of Verschaffelt's method of stating correlation.

Dr Verschaffelt has always failed, so far as I am acquainted with his writings, to distinguish between correlation and regression, so that he is unable to find a single constant, expressing the correlation between two variables, except in very special cases. It also follows that his estimate of the amount of correlation is generally erroneous.

The notation introduced by Mr Galton, when he first used the conceptions of correlation and regression in biological work, enables us to express the important result obtained by Professor MacLeod with great ease: and in order to show this, the Tables relating to the flowers observed in 1899 are here reprinted.

Table II. shows the frequency of flowers with a given number of stamens and pistils among the "late" flowers, which appeared from April 17 to 23, 1899. The column at the extreme right of the Table gives the whole number of flowers with a given number of stamens, the line at the foot gives the whole number with a given number of pistils. From these we find that the mean number of stamens (M_s) was 17·863271, the mean number of pistils (M_p) was 12·147453 *. The Standard Deviation or Error of Mean Square of the number of stamens, which we will call σ_s, was 3·29840; that of the number of pistils (σ_p) was 3·38776. Now if we call Δ_p a deviation from the mean number of pistils, and Δ_s a deviation from the mean number of stamens, the best value of the coefficient of correlation is

$$r=\frac{S(\Delta_p\Delta_s)}{n\sigma_p\sigma_s},$$

where n is the number of observations, and $S(\Delta_p\Delta_s)$ is the sum of the products of all associated deviations †. The value of this expression, for Table II., is

$$r=\frac{3121{\cdot}520053}{4167{\cdot}97287}=0{\cdot}7489.$$

---

* I have to thank Miss M. A. Lewenz, of University College, London, for going over the computations connected with these Tables.

† See Pearson; *Phil. Trans.* A. 1896; and Yule; *Roy. Soc. Proc.* LX.

TABLE I.

268 *Early Flowers.*

Number of Pistils

Number of Stamens	*2*	*3–6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	*21*	*22*	*23*	*24*	*25*	*26*	*27*	*28*	*29*	*30*	*31*	Totals
18												1																1
19						1			1	4																		6
20								3	1	3	1																	8
21							2	1	1		2	2		1														9
22								2	4	1	4		2			2	1											16
23								1	1	2	3		2	2	1													12
24								1	1	4	7	1	1	4	2	1												22
25						1	1	3	1	1	4	5	4	3		1		1					1					26
26								1		2	4	3	2	1	5	2	2	2	1	1								26
27								1		3	5	5	3	7	4	2	2	3	1	1			1					38
28									1	1	2	4	1	1	4													14
29			1		1				1	1	1	5	2	3	1	1		1	2	2							1	23
30												3	5	2	1	3	2	2	1			1						20
31											1	2	2	1	1	4	4	3	1		1							20
32											1		1		1	2	1	1	2		2	2						13
33														2	1	1	1	2										7
34																								1				1
35																			2				2					4
36																												0
37	1																											1
38																					1							1
Totals	1	0	1	0	1	2	3	13	12	22	35	31	25	27	21	19	13	15	10	4	4	3	4	1	0	0	1	268

TABLE II.

373 *Late Flowers.*

Number of Pistils

Number of Stamens	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	*21*	*22*	*23*	*24*	Totals
8							1														1
9																					0
10		1		1	2																4
11	1	1	2					1	1												6
12			4		1																5
13		1	4	3	2	1															11
14			2	10	3	3	2	2	1												23
15		3	4	14	7	4	5	4		1	1										43
16				2	10	6	8	4	4		1										35
17				1	2	5	6	3	7	2	2	2	1								31
18				3	7	13	6	5	13	8	5	3	3								66
19				1		4	7	7	9	7	4	0	3		1						43
20					1	1	5	6	6	12	2	5	2	1	1						42
21								3	2	4	1	3	4	2							19
22										2	3	5	1	2	2	1		1			17
23									1		1	1		2	1		1				7
24						1					1	1			1						4
25												2	2		1					1	6
26												1		3	1				1		6
27									1							1					2
28															1						1
29														1							1
Totals	1	6	16	35	35	38	40	35	45	36	21	23	16	11	9	2	1	1	1	1	373

This is what Mr Galton has called a *coefficient of correlation*, and it must be distinguished from a *coefficient of regression*. In the case before us, if we take an "array" of flowers, such that the number of stamens in every flower differs from the mean number by Δ_s, then the mean number of pistils in the array will be

$$M_p + r\frac{\sigma_p}{\sigma_s}\Delta_s,$$

where $r\frac{\sigma_p}{\sigma_s}$ is the "coefficient of regression." Now in this case the quantity which Dr Verschaffelt proposes as the measure of correlation is, if I understand him rightly, the mean for all arrays of

$$\frac{\dfrac{r\frac{\sigma_p}{\sigma_s}\Delta_s}{M_p}}{\dfrac{\Delta_s}{M_s}} = r\frac{\sigma_p}{\sigma_s}\cdot\frac{M_s}{M_p};$$

and by using this value as a measure of correlation, Professor MacLeod has been led to state his interesting results in a rather erroneous way. In the first place, since Dr Verschaffelt's measure of correlation is really in part a function of a measure of regression, it has one value if we determine the mean number of pistils associated with a given deviation from the mean number of stamens, another if we start with known deviation from the mean of pistils, and proceed to determine the mean associated deviation of stamens: for the mean number of stamens in an array of deviation Δ_p from the mean number of pistils is

$$M_s + r\frac{\sigma_s}{\sigma_p}\Delta_p,$$

and Dr Verschaffelt's measure of correlation in this case is $r\frac{\sigma_s}{\sigma_p}\frac{M_p}{M_s}$. But, apart from this, the introduction of the ratio between the means of the two variables has so much effect as to destroy the value of the quantity proposed as a measure either of regression or of correlation. We see this in Professor MacLeod's treatment of his results.

If we treat Table I., based upon 268 "early" flowers, which appeared between February 27 and March 17, as we treated the table of "late" flowers, we find

$$M_s = 26{\cdot}731343\,; \qquad \sigma_s = 3{\cdot}76088$$
$$M_p = 17{\cdot}447761\,; \qquad \sigma_p = 3{\cdot}89425$$
$$r = 0{\cdot}5065,$$

so that the correlation between number of stamens and number of pistils is very distinctly less in early than in late flowers. The probable error of the value of r for early flowers is 0·0306, and for late flowers it is 0·0153; so that the difference between the observed values is certainly significant.

In an array of early flowers, all of which have a number of stamens differing from the mean number by Δ_s, the mean number of pistils should, from the values obtained, be

$$17{\cdot}447761 + \frac{0{\cdot}5065 \times 3{\cdot}89425}{3{\cdot}76088}\Delta_s$$
$$= 17{\cdot}447761 + 0{\cdot}524\,\Delta_s,$$

or a given deviation from the mean number of stamens should be associated with a deviation of little more than half that amount from the mean number of pistils.

An examination of Table I. will show that this conclusion is as nearly in accord with observation as the numbers in the table would lead one to expect. It is certainly far more intelligible than that to which we are led by Dr Verschaffelt's method, which induces Professor MacLeod to assert that the correlation is in this case nearly perfect,—the difference being due to Dr Verschaffelt's use of the ratio between the means, so that the "measure of correlation" obtained by his method should be $0{\cdot}524\frac{M_s}{M_p} = 0{\cdot}803$.

In the case of the late flowers, Dr Verschaffelt's measure of correlation gives a value *greater* than unity; and Prof. MacLeod seems to have taken this as a sign of imperfect correlation: so that he describes the change in correlation as the reverse of that which actually occurs, and to speak of stamens and pistils as less perfectly correlated in late flowers than in early ones.

Dr Verschaffelt's method of attempting to measure correlation is so extensively used by botanists in his own country that it has seemed worth while to point out in some detail the divergence between his method and that based by Mr Galton on the theory of chance, and at the same time to indicate how it may lead to erroneous conclusions.

Professor MacLeod's investigations on the differences in mean, variability and correlation of the same plants during the flowering season provide a most valuable lesson as to the possible danger of asserting that such differences are significant of local races. They may be due solely to the local environment, or to the period at which the individuals were collected; the seasonal changes in these characters may be accelerated or delayed by local conditions.

W. F. R. WELDON.

II. **Statoblasts of** *Pectinatella Magnifica.*

In an interesting paper on the variation of the statoblasts of this freshwater Bryozoa in *The American Naturalist*, Vol. XXXIV. p. 964, Professor Davenport has determined (*a*) the standard deviation, σ, of the number of hooks of the whole population of 635 statoblasts. He finds it to be 1·326. He has also found (*b*) the average standard deviation, Σ, of eleven colonies of statoblasts. Each such colony is derived from a single statoblast. He finds the average value 1·197. We thus have:

$$\frac{\Sigma}{\sigma} = \frac{1{\cdot}197}{1{\cdot}326} = {\cdot}9027.$$

Now a statoblast colony is one the ancestry of which has been selected back indefinitely. Hence if we accept the "Law of Ancestral Heredity"* we should expect that $\Sigma/\sigma = {\cdot}8944$, a result in good accordance with the above. Thus heredity in this simple organism seems quite comparable with its value as found for man and the higher mammals, an exceedingly important result. We may hope that measurements like those of Professor Davenport will be carried out more extensively and on other species, for there is no more interesting problem than that involved in the diversity or constancy of heredity throughout living forms.

K. P.

---

* *R. S. Proc.* Vol. 62, p. 399, Mr Yule points out to me that a deduction of the fraternal correlation from this result—such as I have given in my paper "On the Principle of Homotyposis and its relation to Heredity" (*Phil Trans.* A. 1901, see Table XXXI.)—is open to question.

VOLUME I JANUARY, 1902 No. 2

VARIATION AND INHERITANCE IN THE PARTHENOGENETIC GENERATIONS OF THE APHIS "HYALOPTERUS TRIRHODUS" (WALKER).

BY ERNEST WARREN, D.Sc., University College, London.

[Received November 5, 1901.]

(1) *Material and Methods.*

Two years ago I made a preliminary investigation* on variation and inheritance among the parthenogenetic generations of *Daphnia magna*. In conformity with Weismann's views the individuals of the same parthenogenetic family ought to resemble one another very closely indeed, but this was found to be far from the case. The variability of the members of such a family was not very strikingly smaller than among sexually produced offspring. The intensity of inheritance, however, between offspring and parthenogenetic mother appeared very considerable; and with the data that were then available it seemed to be greater than with sexually produced young.

The object of the present investigation was to test the conclusions which were drawn from the daphnia results, but the supposed high value of the parental correlation had lost much of its apparent significance by a wider acquaintance with inheritance among sexually produced offspring.

Last July the great abundance of green-fly on some common columbines in a garden suggested that the *Aquilegia* might prove to be a suitable plant for breeding purposes. A dozen large plants in pots were obtained from a florist and placed on a table under an awning in a sheltered corner of a garden. One or two of these plants had plenty of *Hyalopterus*† on them while the remainder were very nearly clean. I placed a few aphides here and there over the clean plants and these individuals soon settled down and became quiescent.

---

* *R. S. Proc.* Vol. 65, 1899.

† *Monograph of the British Aphides*, G. B. Buckton, Vol. II. p. 114.

The weather at that time was very warm and dry but the air around the plants was kept moist by constantly drenching the table and the surrounding ground. On watching from day to day the widely scattered and perfectly quiescent individuals one could see the young being born, and when a little cluster of 6—12 young had been produced by a mature female the mother was carefully taken off and preserved in absolute alcohol. The brood on the leaf was then enclosed in a little cage of bolting-cloth which was firmly tied around the petiole. On previous occasions I have used cylinders of very thin glass with muslin tied over the top and bottom: in these cages, however, there was so much condensation of water that the animals were often drowned. Bolting-cloth cages answered exceedingly well, they stand away from the leaf, the observer can see through them, and there is no injurious condensation.

After about a dozen families had been caged in the manner described the plants had all become so infested that it was not safe to assume that any given cluster of young had all been produced by the adjacent mother. After this, a mature female was placed on a carefully cleaned leaf and enclosed in a cage. When the individual had produced a batch of young it was removed and preserved in alcohol and the brood was allowed to grow up. I believe the cultures were absolutely pure, for the number of young enclosed in the cages was always counted and registered, and afterwards compared with the number in the adult broods. When the brood had become mature, grandchildren of the original parent had been produced, and the adult individuals were preserved. Often by this time the leaf had become somewhat faded, and when such was the case the leaf with the grandchildren was cut off and placed on a fresh leaf and the whole enclosed in a cage. Frequently under such circumstances the whole brood died, for the young did not always seem capable of migrating to the adjacent leaf. Out of 60 broods I only obtained 30 sets of grandchildren.

The Measurements.

The dimensions selected for measurement were: (1) the distance between the eyes *AB* (Fig. 1) and (2) the length of the right antenna *CD*. Body-lengths such as *XY* or *AZ* were unreliable owing to the softness and consequent contractility of the hinder region of the body; sometimes the segments comprised in *ZY* were more or less telescoped inwards.

The specimen in a drop of spirit was placed in a well-slide and covered with a small piece of thin cover-slip and the dimensions were read off by an ocular micrometer under an A Zeiss objective. The magnification was such that the measurements were made to the accuracy of ·00221 of a millimetre*. Fortunately the shape of the animal is such that there was a strong tendency for

---

* Subsequently for the purpose of calculating the constants it was found that the variability was so considerable that the unit employed was ·0221 or ·0442 mm.

the line AB to lie horizontally. When AB and CD were not horizontal slight adjustment of the cover-slip would almost always render them so and this was effected not through pressure exerted on the body but by alteration in the position of the body as a whole, for the legs of the animal projecting out in various directions assured the necessary contact of the object both with the slide and the cover-slip. The antennae were as a rule very fairly straight; when curved the bend was generally at one of the joints so that the measurement could be made by adding together two lengths (see Fig. 1, $C'E' + E'D'$).

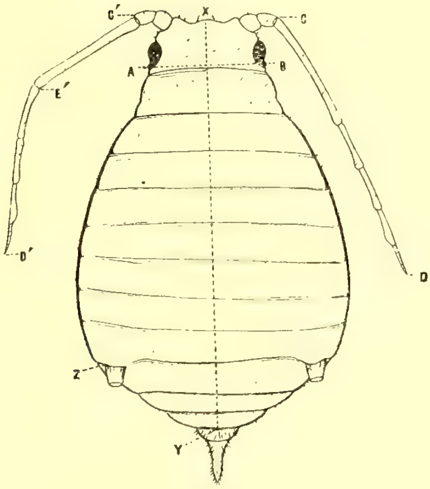


FIG. 1.

The absolute measurements AB (*Frontal Breadth*) and CD (*Length of R. Antenna*) and the ratio $\frac{CD}{AB}$ were all three dealt with in calculating the variability and the intensity of inheritance.

(2) *Growth.*

As the aphis grows the antennae become relatively longer, in other words the ratio $\frac{CD}{AB}$ rises. On this account it was essential, both for the ratio and the absolute dimensions, that all the parents and offspring should be measured as nearly as possible at the same stage of growth. In describing above how the experiment was conducted I used the words "mature female." Now, growth by no means ceases on the first production of young and I will define a "mature female" as one in which growth has become exceedingly slow. Growth at one period of life is very rapid, then it becomes slower and slower, but probably among many invertebrates it never entirely ceases during the whole life of the animal. A mature female then is one which has arrived at that stage of life at which the period of rapid growth has passed. Among *immature* females with a frontal breadth of 12—13 units the length of the antenna tended to be about five-thirds of the frontal breadth; among *mature* females, that had practically ceased to grow, with a frontal breadth of 16—17 units, the length of the antenna had risen to about seven-thirds of the frontal breadth. To insure that the individuals preserved were mature I generally left the families until one or more of the members began to die of old age. To emphasize the importance of this procedure it may be added that since the value of the ratio rises with the growth of the mother there could easily be introduced into the correlation between offspring and mothers a wholly spurious correlation, which has no reference to heredity, by inserting into the series a group of immature mothers with offspring at the same immature stage.

(3) *Influence of Environment.*

During the period when the mothers were selected the environment was very favourable, the temperature was high and the foliage of the plants was in good condition. Later, while the offspring were growing and especially when the grandchildren were developing the conditions were much less favourable. Much rain and a considerable fall of temperature occurred, also the foliage of the columbines was withering, as is always the case in late summer.

The unfavourable conditions of the environment progressively increasing had a marked effect on the absolute size attained by the *mature* individuals; this is well seen by comparing the third and fifth columns with the second in the accompanying table. In the second generation the absolute length of the antenna was not diminished in the same proportion as the breadth, and hence the mean of the ratios (23·469) was higher than in the parents. In the third generation we see the great effect of the environment, the means of the absolute dimensions of the *mature* individuals being very conspicuously lower than in the first and second generations.

TABLE I.

Means.

Dimension	Mothers (60) weighted with their fertility	Children (368)	Grandmothers (30) weighted with their fertility	Grandchildren (291)
Frontal Breadth (*AB*) Unit = ·01 mm.	37·646	35·898	37·562	33·932
Length of R. Antenna (*CD*) Unit = ·01 mm.	85·263	84·336	83·907	76·586
Ratio $\frac{CD}{AB} \times 10$	22·670	23·469	22·464	22·567

The normal duration of life of my aphides was about three weeks to a month. If during the period of active growth the environment is unfavourable the individuals become permanently small and will never attain to a considerable size. They apparently become mature and will live three weeks or more, but will ultimately die undersized however favourable the environment may afterwards become.

On account of this great sensitiveness of the organism to its environment it can be seen that to measure the correct correlation between offspring and parent (at least with respect to absolute dimensions) all the broods of the generation ought to experience an identical environment. The fact that the conditions of life were progressively less favourable, more especially with the grandchildren, would not necessarily alter the correlation if all the broods were affected alike. In practice it is not wholly possible to provide an identical environment for all the broods; temperature, humidity of the air, illumination, etc., were nearly the same for all the broods, but the leaves of a plant differ from one another very considerably in succulence, the amount of decomposition of the chlorophyll, etc. To eliminate as much as possible these differences I always endeavoured to select similar leaves for the broods to live on.

The differences which were observed among the individuals of the generation, when growth had practically ceased, must be ascribed to the laws of variation and inheritance, and it is to these differences that I shall return when discussing the variability and the intensity of inheritance in parthenogenetically produced offspring.

(4) *Fertility.*

The birth of young by a mature female is not a continuous process, a brood of about 6—12 offspring is produced in two or three days and then there is a pause before others are born. It is probable that the number of individuals in a brood bears a definite relation to the fertility of the mother which produced it. In the second row of Table II. is given the frequency of the number of individuals in the newly born families and in the third row the frequency of the adult broods. If

TABLE II.

No. in Brood	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	No. of Families	Total No. of Offspring
Frequency of newly born	—	2	8	15	7	7	9	2	6	1	2	1	60	522
Frequency of adults ...	3	7	8	19	7	5	5	1	2	1	2	—	60	455
No. of winged females ...	—	—	—	2	3	—	1	—	—	—	—	3	9 in all	

the frequency of the newly born broods be exhibited in a graphical form (Fig. 2) we see that the polygon conforms to the usual type of a fertility curve—there is marked skewness, the mean being considerably larger than the modal value.

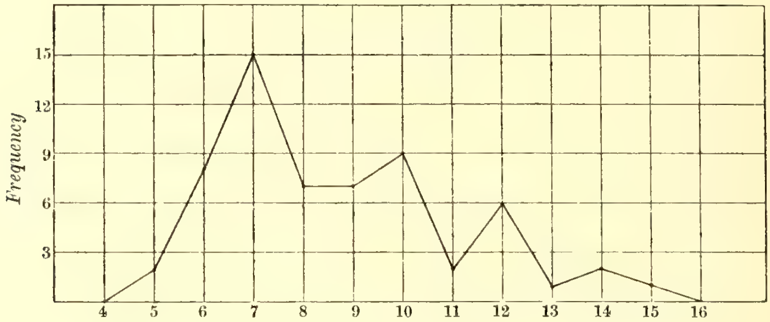


Fig. 2. Fertility Polygon. 60 Mothers, 522 Offspring.

In order to discover whether fertility was in any way related to the dimensions measured, correlation tables were prepared for the dimensions of the mother and the number in the brood (Table II A. Nos. 1, 2, 3). The coefficients of correlation are seen to be small, about ·12 ± ·09, but since the probable errors are so great we can only assert that the larger individuals appeared to be slightly more fertile than the smaller ones.

(5) *Death-Rate.*

The total number of offspring registered was 522. Out of this number 455 individuals grew up and were collected. In the population of 455 there were nine winged individuals and 78 large corpses. The winged females appeared in only four families. The large corpses were the bodies of individuals which had doubtless died of old age, for I purposely allowed the broods to live as long as possible so as to insure that the individuals collected were as mature as they would become. The remaining 368 animals were measured.

Those that died before growing up, 67 in all, probably succumbed from weakness in constitution, etc. This gives a death-rate of 12·8 per cent. for the second generation.

To ascertain whether there was any relation between the death-rate of the brood and the deviation of the mother the percentage death-rate for every family was calculated and correlation tables for this percentage and the dimensions of the parents were prepared (Table II A. Nos. 4, 5, 6). The coefficients of correlation for the absolute dimensions are about − ·20 ± ·084, that is the larger mothers tended to have healthier offspring.

It was also desirable to know whether deviation in adult families was associated with increased death-rate during their growth. The mean ratio of each family was calculated and a correlation table was made between it and the percentage death-rate, the coefficient is so small (no. 7) and the probable error so great that we can only say that perhaps there was a slight tendency for families with high ratio to have a small death-rate. We may assume that adult families which exhibited considerable divergence were parts of families, which if all the members had survived would have shown the same or greater divergence, hence we are justified in the assumption that the deaths which occurred were not at all, or very slightly selective with respect to the dimensions with which we are here concerned.

TABLE II A.

Number	x	y	r_{xy}
1	Length of Antenna of Mother	Number in newly born Brood ...	·120 ± ·086
2	Frontal Breadth of Mother ...	Number in adult Brood	·129 ± ·086
3	Ratio of Mother	„ „ „	·119 ± ·086
4	Length of Antenna of Mother	Percentage Death-Rate of Family	– ·201 ± ·084
5	Frontal Breadth of Mother ...	„ „ „ „	– ·184 ± ·084
6	Ratio of Mother	„ „ „ „	– ·070 ± ·087
7	Mean ratio of Family ...	„ „ „ „	– ·113 ± ·086
8	Number in newly born Brood	„ „ „ „	– ·188 ± ·084
9	Difference between the ratio of Mother and mean ratio of Family	„ „ „ „	– ·128 ± ·086
10	Difference between the ratio of Mother and mean ratio of Family	Number in newly born Brood ...	– ·007 ± ·087

Further, the fertility of the mother, measured by the number in the newly born brood, was apparently slightly correlated with the death-rate (no. 8), that is offspring of fertile mothers tended to be healthier than those of less fertile mothers.

Also, families in which the mean ratio diverged widely from the ratio of the parent tended to be healthier (no. 9) than those families which more nearly resembled the parent; but there was apparently no correlation between this resemblance and the fertility of the mother (no. 10).

(6) *Organic Correlations.*

For any consideration of the correlation of one organ of a parent with some other organ of the offspring it would be interesting to ascertain the inter-relations of the parts in the individual. The correlation table for frontal breadth (AB) and length of antenna (CD) of the second generation is given in Table III. The standard deviation of $AB = 1{\cdot}3415$ and of $CD = 2{\cdot}4744$; the coefficient of correlation is ·8023 ± ·0125. Hence the two dimensions are closely correlated.

TABLE III.

Frontal Breadth (AB) and Length of Antenna (CD), Second Generation.

Length of R. Antenna: Unit = 2 divisions, = ·0442 mm.

Frontal Breadth: Unit = 1 division of Ocular Micrometer = ·0221 mm.	*24·0–25·9*	*26·0–27·9*	*28·0–29·9*	*30·0–31·9*	*32·0–33·9*	*34·0–35·9*	*36·0–37·9*	*38·0–39·9*	*40·0–41·9*	*42·0–43·9*	*44·0–45·9*	*46·0–47·9*	*48·0–49·9*	*50·0–51·9*	Totals
12·0—12·9	—	—	—	1	—	—	—	—	—	—	—	—	—	—	1
13·0—13·9	1	0	4	4	2	2	—	—	—	—	—	—	—	—	13
14·0—14·9	—	2	7	13	9	9	7	1	—	—	—	—	—	—	48
15·0—15·9	—	—	1	12	20	24	18	17	5	—	—	—	—	—	97
16·0—16·9	—	—	—	1	2	7	22	25	22	7	2	2	—	—	90
17·0—17·9	—	—	—	—	—	—	8	13	16	23	17	6	2	—	85
18·0—18·9	—	—	—	—	—	—	—	—	7	3	9	4	3	1	27
19·0—19·9	—	—	—	—	—	—	—	—	1	0	3	3	—	—	7
Totals	1	2	12	31	33	42	55	56	51	33	31	15	5	1	368

The smaller individuals in the series, which, however, appeared mature in that they had very nearly ceased to grow, tended to have an antenna relatively shorter than in the larger animals. I took the frontal breadth as a criterion of the general size of the body and instituted a correlation table between it (AB) and the ratio $\left(\frac{CD}{AB}\right)$, Table IV. Since AB is the same number in both cases there should be a negative correlation if there be no organic correlation. As a matter of fact the coefficient of correlation is + ·3200 ± ·0316; the standard deviation of $AB = 1{\cdot}3415$ and of $\frac{CD}{AB} = 1{\cdot}8363$.

TABLE IV.

Frontal Breadth (AB) and Ratio $\left(\frac{CD}{AB}\right)$, *Second Generation.*

Ratio: Unit = 1 hundredth of Breadth.

Frontal Breadth: Unit = 1 division of Ocular Micrometer = ·0221 mm.	*18·0–18·9*	*19·0–19·9*	*20·0–20·9*	*21·0–21·9*	*22·0–22·9*	*23·0–23·9*	*24·0–24·9*	*25·0–25·9*	*26·0–26·9*	*27·0–27·9*	*28·0–28·9*	Totals
12·0—12·9	—	—	—	—	—	—	1	—	—	—	—	1
13·0—13·9	1	0	0	5	3	0	3	0	1	—	—	13
14·0—14·9	—	4	8	8	8	6	8	4	1	1	—	48
15·0—15·9	—	6	8	19	16	19	18	7	4	—	—	97
16·0—16·9	—	1	2	7	19	23	18	14	3	1	2	90
17·0—17·9	—	—	—	8	13	16	15	23	8	1	1	85
18·0—18·9	—	—	—	2	4	4	8	5	3	0	1	27
19·0—19·9	—	—	—	1	0	2	4	—	—	—	—	7
Totals	1	11	18	50	63	70	75	53	20	3	4	368

Professor Pearson* has introduced a formula for the purpose of extracting this "spurious correlation." If $\frac{x_1}{x_3}$, $\frac{x_2}{x_4}$ are two indices; v_1, v_2, v_3, v_4 coefficients of variation $\left(\frac{\text{standard deviation}}{\text{mean}} \times 100\right)$ of x_1, x_2, x_3, x_4 respectively; r_{12}, r_{23}, r_{34} coefficients of correlation between x_1 and x_2, x_2 and x_3, x_3 and x_4 respectively and ρ the coefficient of correlation between the indices, then

$$\rho = \frac{r_{12}v_1v_2 - r_{14}v_1v_4 - r_{23}v_2v_3 + r_{34}v_3v_4}{\sqrt{v_1^2 + v_3^2 - 2r_{13}v_1v_3}\ \sqrt{v_2^2 + v_4^2 - 2r_{24}v_2v_4}}.$$

Applying this to the present case

$$\rho = \frac{r_{AB,\,CD}\,v_{AB}\,v_{CD} - v_{AB}\,v_{AB}}{v_{AB}\sqrt{v^2_{CD} + v^2_{AB} - 2r_{CD,\,AB}\,v_{CD}\,v_{AB}}}.$$

Now, if the correlation $(r_{CD,\,AB})$ be supposed to disappear the expression nevertheless does not vanish but becomes

$$= -\frac{v_{AB}}{\sqrt{v^2_{CD} + v^2_{AB}}} = \rho_0,$$

$$v_{AB} = 8{\cdot}2587, \qquad v_{CD} = 12{\cdot}9682,$$

$$\therefore \rho_0 = -{\cdot}5372,$$

and $$\rho - \rho_0 = {\cdot}3200 + {\cdot}5372 = +{\cdot}8572.$$

Assuming that the expression $\rho - \rho_0$ has a definite meaning, we conclude that there is a strong correlation between the ratio and the size of the body in the adult animal.

(7) *The Variability of the Race.*

The coefficient of variation of any organ has been defined by Professor Pearson† as $\frac{\text{standard deviation}}{\text{mean}} \times 100$; it is thus a ratio-measure of variability while the standard deviation is an absolute measure of the same. In the accompanying table the standard deviations and coefficients of variation are given for parents and offspring.

The variability of the second generation (fourth and fifth columns, Table V.) is greater than that of the parents, and this we should expect since certain selective influences would doubtless be less active than among wild aphides. The fact that the mothers are weighted with their fertility will have no appreciable effect on the standard deviation, since as was mentioned above fertility is only slightly correlated with any of the dimensions.

---

* *Proc. Roy. Soc.* 1897.

† *Phil. Trans. of the Roy. Society*, Vol. 187 (1896) A, p. 277.

TABLE V.

Dimension	Mothers (60) weighted with their fertility		Children (368)		Grandmothers (30) weighted with their fertility		Grandchildren (291)	
	S. D.	Coefficient of Variation	S. D.	Coefficient of Variation	S. D.	Coefficient of Variation	S. D.	Coefficient of Variation
Frontal Breadth (AB) Unit = ·01 mm.	2·7682	7·3533	2·9647	8·2587	2·2815	6·0739	1·9099	5·6283
Length of R. Antenna Unit = ·01 mm.	9·2262	10·8205	10·9367	12·9682	7·3593	8·7707	7·9972	10·4422
Ratio $\frac{CD}{AB}\times 10$	1·4974	6·6054	1·8363	7·8244	1·2735	5·6692	1·6309	7·2270

We must compare the variability of the third generation (eighth and ninth columns) with the variability of the grandparents (sixth and seventh columns), the fewness of which render the values somewhat aberrant. The diminished variability of the frontal breadth (5·6283) of the third generation must be referred to the direct action of the environment, for we have already seen that the external conditions have an important influence on the ultimate size attained by the mature offspring, and apparently the standard deviation is also reduced in the case of our criterion of size, the frontal breadth.

It is interesting to notice that here as in man* the variability of children is greater than that of parents, the adult children being a population only partially selected. Similarly the variability of the grandchildren is greater than that of their grandparents (with the exception of the frontal breadth mentioned above). Also the variability of the grandmothers is considerably less than that of the mothers, for the grandmothers are a doubly selected population, being in fact only those individuals which are capable of producing *fertile* offspring.

These results prove that the variability of the race, measured by the coefficient of variation, is by no means smaller than among sexually reproduced forms. This coefficient varies considerably for different characters; to mention a few examples the following may be cited. Prof. Pearson and others have shown that for stature in man† the coefficient of variation varies from about 3—4, for the length of the long bones‡ from 4—6. Breadth of wing of 98 wild ♂ *Strenia clathrata*§ had a coefficient of 4·57.

---

* *Phil. Trans. Roy. Soc.* Vol. 187 (1896) A, p. 270.

† *Ibid.* p. 278.

‡ Lee and Pearson, *R. S. Proc.* 1897, Vol. 61, p. 348. Warren, *Phil. Trans. of the Roy. Soc.* 1898, Vol. 189, B, p. 140.

§ Not yet published.

(8) *Inheritance. Correlation between Parent and Offspring.*

The question now to be discussed is how far peculiarities in the parent are transmitted to the offspring and succeeding generation in parthenogenesis. In Tables VI. to XI. (placed at the end of the present paper) are given the correlation tables for the absolute measurements and the ratio.

The coefficients of *correlation* (r) were calculated from the formula $r = \frac{\Sigma(xy)}{n\sigma_1\sigma_2}$, where σ_1 and σ_2 are the standard deviations of parent and offspring respectively; and the coefficients of *regression* of offspring on mothers $= r\frac{\sigma_2}{\sigma_1}$. In the accompanying table the various results are tabulated together with their probable errors.

TABLE XII.

Genus	Dimension	Parents and Offspring (368)		Grandparents and Grandchildren (291)	
		Coefficient of Correlation	Coefficient of Regression	Coefficient of Correlation	Coefficient of Regression
Hyalopterus	Frontal Breadth (AB) ...	·3354 ±·0312	·3592 ±·0355	·3208 ±·0355	·2685 ±·0314
	Length of R. Antenna (CD)	·4273 ±·0287	·5065 ±·0377	·1766 ±·0383	·1919 ±·0244
	Ratio $\left(\frac{CD}{AB}\right)$	·4392 ±·0284	·5385 ±·0387	·2305 ±·0374	·2952 ±·0493
Daphnia	Ratio $\frac{\text{Length of Protopodite}}{\text{Length of body}}$	·466* ±·054	·619 ±·0809	[·27±·12†	·5±·2]
Mean of Coefficients		·41	·50	·24	·25

To this table are also added the results obtained from Daphnia. The means of the correlation between parents and the second and third generations are respectively ·41 and ·24; the grandparental correlation for Daphnia has been omitted, since the probable error is so very large.

Such correlation, and especially that of the grandparental, is distinctly too high for Galton's Law for sexual inheritance in its *simple* form, and the question arises whether inheritance is stronger in parthenogenetic than in sexual reproduc-

---

* 96 young, 23 mothers.

† 26 young, 7 grandparents.

tion. But a wide survey of the correlation of very various characters between parent and offspring of sexual forms lends no support to such a view. It should be noticed that the parental correlation for the ratio approximates more closely to the value usually found among sexual forms (·45—·50)* than do the coefficients of either of the absolute dimensions. It is very probable that these discrepancies must be referred to the effect of environment. This view is favoured by the following consideration. I selected the frontal breadth as a criterion of the general size of the body and it is the size of the animal that is most affected by the environment. Now, the coefficients for the frontal breadth are more aberrant than those for the length of the antenna, and so it is reasonable to assume that these deviations are to be ascribed to the varying conditions of life.

Figs. 3, 4 and 5 represent graphically the correspondence between the line of means of the arrays and the correlation line. Theoretically the mean of each array near the region of the general mean of the parents should be on the dotted line. Further away from the parental mean the means of the arrays are much less likely to approximate to the dotted line for the following reasons: (1) the unavoidable fewness of the more diverging individuals and the consequent impossibility of obtaining a satisfactory mean to these arrays, and (2) in the case of skew curves the correlation line should really be sigmoid and not straight.

In Fig. 3 the divergence of the uppermost point from the dotted line must be ascribed to the small number of individuals, there being only 7 members of this array (Table X.). In Fig. 4 the widely deviating point between divisions + 2 and + 3 (array 42·0—43·9, Table VIII.) is probably due to the circumstance that these families may have lived on particularly innutritious leaves, and this view is favoured by the fact that the adult individuals of these families tended to be small.

(9) *Cross-Inheritance.*

Although two organs *A* and *B* of an animal may be correlated yet it does not necessarily follow that there is a correlation between the organ *A* of the parent and *B* of the offspring. If, however, *A* and *B* are closely correlated in the individual it is probable that there will generally be found some cross-inheritance between *A* or *B* of the parent and *B* or *A* of the offspring.

If *A* (frontal breadth) of the offspring be directly acted upon by the environment and *A* and *B* (length of antenna) are dimensions closely correlated in the individual, then the irregular alteration of *A* in the offspring by different environments for different broods would probably affect the correlation between organ *B* of the parent and the same organ of the offspring. If the alteration be uniform, that is if all the offspring are acted upon by one and the same environment which

---

* Pearson, *R. S. Proc.* 1900, p. 157. Pearson and Lee, *Phil. Trans. of the Roy. Soc.*, Vol. 195 (A), 1900, p. 119.

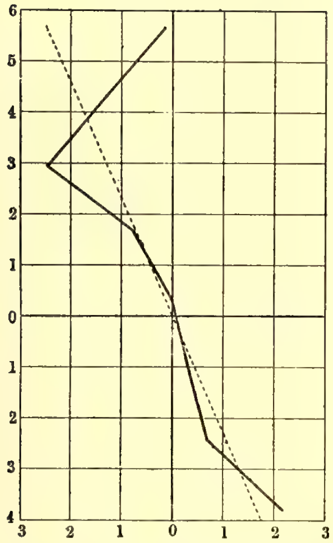


FIG. 3. Parents and Offspring. Ratio. 1 division $=\frac{1}{2}$ standard deviation.

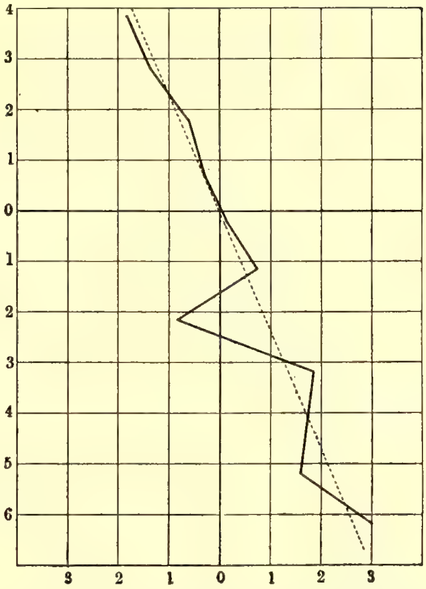


FIG. 4. Parents and Offspring. Absolute length of Antenna. 1 division $=\frac{1}{2}$ standard deviation.

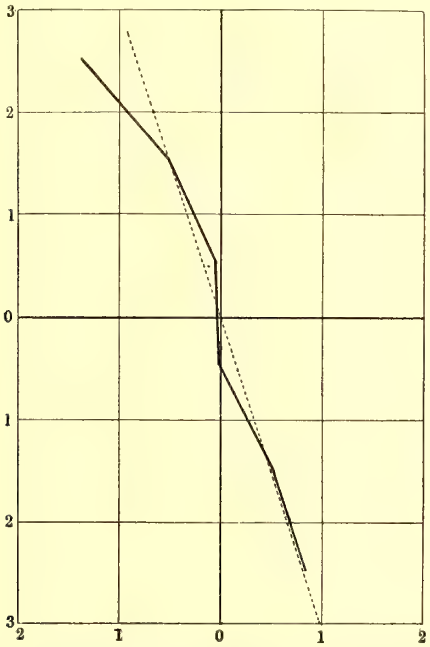


FIG. 5. Frontal Breadth: Grandparents and Grandchildren. 1 division = the standard deviation.

is different from that to which the parents were subjected, then the correlation of organ *B* between parent and offspring would not necessarily be changed.

We saw above that the correlation between frontal breadth and length of antenna *in the individual* was ·8023, and now we wish to know the correlation between the frontal breadth of the mother and the length of antenna of the offspring. The correlation table is given in Table XIII.; the coefficient of correlation is ·3004 ± ·0320.

TABLE XIII.

Frontal Breadth of Parents and Length of Antenna of Offspring.

Offspring: Length of Antenna. Unit = 2 divisions of Ocular Micrometer = ·0442 mm.

Parents: Frontal Breadth. Unit = 1 division of Ocular Micrometer = ·0221 mm.

	24·0–25·9	*26·0–27·9*	*28·0–29·9*	*30·0–31·9*	*32·0–33·9*	*34·0–35·9*	*36·0–37·9*	*38·0–39·9*	*40·0–41·9*	*42·0–43·9*	*44·0–45·9*	*46·0–47·9*	*48·0–49·9*	*50·0–51·9*	Weighted Mothers	Number of Families
14·0—14·9	—	—	2	3	1	—	—	—	—	—	—	—	—	—	6	1
15·0—15·9	—	2	4	7	6	8	6	8	8	2	3	5	1	1	61	9
16·0—16·9	1	0	5	9	13	15	28	27	20	9	7	2	2	—	138	25
17·0—17·9	—	—	1	9	7	8	8	9	9	8	8	1	—	—	68	11
18·0—18·9	—	—	—	3	6	11	13	11	13	10	5	2	1	—	75	11
19·0—19·9	—	—	—	—	—	—	—	1	1	2	3	3	—	—	10	2
20·0—20·9	—	—	—	—	—	—	—	—	—	2	5	2	1	—	10	1
Totals	1	2	12	31	33	42	55	56	51	33	31	15	5	1	368	60

Also we saw that the correlation between the frontal breadth (*AB*) and the ratio $\frac{CD}{AB}$ of the individual was + ·3200. In Table XIV. is shown the correlation distribution for the frontal breadth of mother and the ratio of offspring; the coefficient of correlation is ·1688 ± ·0342.

Now let us calculate the values of the cross-correlations by aid of Professor Pearson's theorem* that:

Cross-inheritance = mean of direct inheritances × organic correlation.

We find in the first case:

$$\text{Cross-correlation} = \tfrac{1}{2}(\cdot 3354 + \cdot 4273) \times \cdot 8023 = \cdot 3059,$$

and in the second case:

$$\text{Cross-correlation} = \tfrac{1}{2}(\cdot 3354 + \cdot 4392) \times \cdot 3200 = \cdot 1239.$$

The first result is in good agreement and the second as close as could be anticipated from the large probable errors of our results.

---

* *R. S. Proc.* Vol. 62, p. 411.

TABLE XIV.

Frontal Breadth of Parents and Ratio of Offspring.

Offspring : Ratio. Unit = 10 thousandths of Frontal Breadth.

Parents : Frontal Breadth. Unit = 1 division of Ocular Micrometer = ·0221 mm.	*18·0–18·9*	*19·0–19·9*	*20·0–20·9*	*21·0–21·9*	*22·0–22·9*	*23·0–23·9*	*24·0–24·9*	*25·0–25·9*	*26·0–26·9*	*27·0–27·9*	*28·0–28·9*	Weighted Mothers	Number of Families
14·0—14·9	—	—	—	3	2	0	1	—	—	—	—	6	1
15·0—15·9	—	3	8	5	4	10	13	7	7	0	4	61	9
16·0—16·9	1	5	6	25	27	33	23	13	4	1	—	138	25
17·0—17·9	—	2	2	10	18	8	12	11	4	1	—	68	11
18·0—18·9	—	1	2	7	12	18	17	14	4	—	—	75	11
19·0—19·9	—	—	—	—	—	1	2	6	1	—	—	10	2
20·0—20·9	—	—	—	—	—	—	7	2	0	1	—	10	1
Totals	1	11	18	50	63	70	75	53	20	3	4	368	60

(10) *The Variability among the Members of a Brood.*

We have already seen that the variability of the race is in no way smaller than in sexual animals. It is now necessary to discuss the variability in the individual family.

The standard deviation for all the families containing 7 or more members was calculated and the mean is given in the last column but one of the accompanying table (Table XV.).

TABLE XV.

Genus	Dimension	Variability	Parents (60) weighted with their fertility	Offspring (368)	Arrays $\sigma\sqrt{1-r^2}$	Mean of the S.D.'s of all families over and including 7	Total No. of families in which the S.D. was calculated
Hyalopterus	Breadth ... ... unit = ·01 mm.	S.D. ... Percentage	2·7682 57·4	2·9647 53·6	2·7930 56·9	1·5890 100	22 families and 190 individuals
	Length of Antenna unit = ·01 mm.	S.D. ... Percentage	9·2262 59·4	10·9367 50·1	9·8884 55·5	5·4839 100	
	Ratio ... ... unit = ·010 of Frontal Breadth	S.D. ... Percentage	1·4974 66·5	1·8363 54·2	1·6797 59·2	·9953 100	
Daphnia	Ratio ... ... unit = ·002 of Body Length	S.D. ... Percentage	2·2208 57·4	2·9503 43·2	2·6104 48·8	1·2747 100	22 families, 95 individuals
Mean of the percentages			60·2 °/。	50·3 °/。	55·1 °/。	100	

In the 6th column is given the average S.D. of an array calculated from the formula $\sigma\sqrt{1-r^2}$ and in 4th and 5th columns the S.D. of parents and offspring. The general standard deviations for arrays are of considerable magnitude, being in fact greater than the S.D.'s of the parents. The mean S.D.'s of the families are much less than those of arrays. We should expect the former to be smaller; for although all the parents which produce an array are alike in the particular dimension, yet they are unlike in their ancestry.

In this table is also given the ratio (expressed as a percentage) of the mean S.D. of the families to the standard deviation of parents, offspring, and arrays. To these results are also added those obtained from daphnia; and in the bottom line of the table the means of the percentages are given.

It must be realised that the families are too small to give a very reliable determination of the standard deviation, but nevertheless the mean results probably give a rough indication of actual fact. According to these results the variability of the family is seen to be 60 °/₀ of that of all the parents, or in other words if we bred from a single individual yet more than half of the racial variability would remain.

It is unfortunate that at present there are no data available from which family variability may be deduced for sexual forms and we must await further investigations before a comparison can be made with these results from parthenogenetic families. In the meantime it may be seen that the present results are antagonistic to the views on variation and inheritance expounded by A. Sedgwick *; for with a family variability of 60 °/₀ of the racial it is difficult to see "that genetic variations cannot occur in asexual reproduction, and that if any indefinite variability recalling genetic variability makes its appearance it must be part of the genetic variability and directly traceable to the zygote from which the asexual generations started."

(11) *The Correlation between Members of the same Brood.*

Let any member of a family be chosen, then we wish to find a number which will express its probable degree of similarity to any other member taken at random from the same family. Just as the mean standard deviation of all the families would be a measure of the dissimilarity of members of a family, so the correlation coefficient for all possible pairs of brethren would be a measure of their resemblance. If the S.D. of every family be very small or 0, that is if there be very little or no variation among brethren, the correlation would be perfect and equal to 1. If, on the other hand, the S.D. is large there is much dissimilarity among brethren, and consequently the correlation would be expressed by a number much less than 1. We have already seen that, contrary to a prevailing

---

* Presidential Address to Section D of the British Association, 1899. Reprinted in *Nature*, Vol. 60, p. 507.

opinion, the standard deviations of families are not inconsiderable, and hence the correlation between brethren will be much less than unity.

In Tables XVI.—XXI. (at the end of the present paper) are given the correlation tables for pairs of offspring. In Table XXII. the results both for aphis and daphnia are exhibited.

TABLE XXII.

Genus	Dimension	Pairs of Offspring 368 individuals 1114 pairs	Pairs of Grandchildren 291 individuals 1894 pairs
Hyalopterus	Frontal Breadth Length of Antenna Ratio	·6660 ± [·0112]* ·6785 ± [·0109] ·5890 ± [·0132]	·3890 ± [·0132] ·4132 ± [·0129] ·3382 ± [·0137]
Daphnia	Ratio	·6934 ± [·0270]†	——
Mean of Coefficients		·656	·380

In the fourth column are given the coefficients for grandchildren. From the nature of the experiment the collections of grandchildren consisted of a mixture of *sisters* and cousins‡ in unknown proportions. The coefficients for the offspring are high, but for the following reasons further investigation is needed before it can be concluded that there is greater similarity among brethren in parthenogenesis than in sexually produced offspring. The value for the ratio is distinctly lower than for the absolute measures; as in the case of comparing the S.D. of arrays with the mean of the standard deviations of the separate families, so in the case of the correlations the values for the ratio appeared the most normal.

Each brood or collection of grandchildren lived on one leaf, and consequently every member of a family was subjected to a practically identical environment. Those that had an innutritious leaf would all tend to be small when adult and those with a particularly wholesome leaf would be large.

We have seen, however, that there is considerable correlation between the ratio and the adult size of the animal, hence even in the parental correlation of

---

* These probable errors have been calculated from the usual formula $\cdot 67449 \frac{1-r^2}{\sqrt{n}}$. I have made $n=1114$ but it is doubtful whether this gives an accurate probable error.

† 23 families, 168 pairs.

‡ In a *rough* sort of way the correlation of true cousins might be found from this. If r = correlation of cousins and there be m pairs of sisters and m' of cousins, then $(m+m')\cdot 3801 = m\cdot 6445 + m'r$. The proportions of sister and true cousin pairs from the total individuals and total pairs given in Table XXII, could not be widely different from the ratio 3 to 10. Hence the cousin correlation would be as high as ·3, or nearly half the fraternal correlation.

the ratio the environment might be expected to have some effect, but the greatest effect would naturally be on the fraternal correlation. Theoretically the mothers and all the broods ought to have lived on exactly the same sort of leaf, or failing this the mothers could have lived on one kind of leaf and *all* the broods on some other kind. Unfortunately this is not wholly possible in practice.

(12) *Summary.*

The present investigation well illustrates the exceeding difficulty of breeding organisms for statistical purposes, and it emphasizes the necessity for the greatest caution in drawing conclusions from the results.

The reaction of the organism to its environment is so prompt that it is practically impossible to breed two generations under such conditions as to ensure the same mean and the same variability in the two generations. The differences which occur are due to two sets of causes: (1) the unavoidable removal of certain selective agencies which would act under purely natural conditions, (2) the direct effect of the environment both on the parents and on the offspring. The resultant effect of numerous influences under either or both of these headings might increase or diminish the apparent intensity of inheritance. I will now summarize in separate paragraphs the facts and conclusions deduced from the present investigation.

(1) The mean of the ratio rises very greatly with the growth of the individual.

(2) An individual was said to be "mature" or "adult" after the period of rapid growth had passed.

(3) Under unfavourable conditions the individual grows slowly, after a time offspring may be produced, and ultimately growth nearly ceases and the individual is "mature" but permanently small.

(4) The unfavourable conditions of the environment had a marked effect on the absolute dimensions of the adult third generation, but they had little effect on the mean of the ratio. These conditions did not induce any great formation of winged individuals.

(5) The number of offspring in a brood may be taken as a rough index of the fertility of the parent. There was but little correlation between fertility and the dimensions measured on the parents.

(6) In the second generation there was a death-rate of 12·8 per cent. among the immature animals. The larger individuals tended to have healthier families. Offspring of fertile mothers were somewhat stronger in constitution than those of less fertile mothers. But those families in which the majority of the adult members were widely divergent from the filial mean exhibited little difference in their death-rate.

(7) The frontal breadth and the length of antenna are closely correlated in the adult individual, the coefficient being ·80. The frontal breadth (taken

as a criterion of the general size) has a correlation with the ratio of + ·32, but if we accept the significance of the expression $\rho - \rho_0$, there is really a much larger organic relationship than is indicated by this number.

(8) The variability of the race, measured by the coefficients of variation, was large (6—10), and there is apparently no significant difference between them and the coefficients found among sexual forms.

(9) The mean of the coefficients, for aphis and daphnia, of the parental correlation was ·41, and of the grandparental ·24; thus here again there is no marked difference between parthenogenetic and sexual reproduction.

(10) Two examples of cross-inheritance were examined: (1) frontal breadth of parent and length of antenna of offspring $r = {\cdot}30$; and (2) frontal breadth of parent and the ratio of offspring $r = {\cdot}17$. These results are in good accord with Prof. Pearson's theorem for cross-inheritance.

(11) The variability of arrays of offspring is by no means significantly small: and the variability of the individuals of a brood, measured by the mean of the standard deviations of the individual families, amounts to as much as 60 °/ₒ of the racial variability. This may perhaps be rather less than in sexual reproduction, but no very satisfactory direct comparison is at present possible.

(12) A convenient measure of the similarity of brethren is afforded by the fraternal correlation. From numerous data Prof. Pearson* regards ·49 or ·50 as the average fraternal correlation among sexual forms, while the mean coefficient for aphis and daphnia is ·66; but it should be noticed that the ratio of aphis, on which the environment would have less effect, more nearly approaches the sexual value than do the absolute dimensions. The question as to whether we have here a real difference between parthenogenetic and sexual offspring can only be decided by further investigation both on aphis and other forms.

(13) *Concluding Remarks.*

The coefficient of correlation for a pair of organs must have a definite physical meaning, and if no spurious correlation be introduced, it should indicate the proportion of the manifold causes, affecting the two organs in question, which are common to each.

The causes of inheritance are probably alike in nature to the causes of the correlation of parts of an individual. Thus the right and left organs of a bilateral animal are usually strongly correlated, and this fact translated into a physical meaning must imply that the numerous causes which build up the organ on the two sides are nearly identical. Now, it seems probable that a long comparative series of observations of the correlation between different parts of the numerous organisms and the *cross-inheritance* between these various parts would throw a flood of light on the actual mechanism of both growth and inheritance.

---

* Pearson, *R. S. Proc.* 1900, p. 157. Pearson and Lee, *Phil. Trans. Roy. Soc.* Vol. 195 (A), p. 119.

We know from Driesch's experiments that the mere position of a cell in the blastosphere determines its fate, and that the blastomeres are at first, so to speak, merely plastic masses which are welded into an organism by the life of the structure as a whole. The failure of inheritance must be regarded as of the same nature as the failure in the growing organism to produce the normal relative proportions of the different parts of the body.

And finally, with regard to the special subject of this paper, inheritance in parthenogenesis. From the nature of the case, since there is only a female parent, the inheritance cannot be a blend of maternal and paternal characters. If it were "exclusive inheritance" without reversion the coefficients ought to be far higher than in sexual forms. But this is not the case. The inheritance could be either of the nature of a blend of ancestral characters, or the "exclusive" transmission of ancestral characters, that is reversion.

In the accompanying table in the second column I give the theoretical values deduced by Professor Pearson* for exclusive inheritance, without reversion, in the third column the values for blended inheritance or exclusive inheritance with reversion and $\gamma = 2{\cdot}35$ †. In the fourth and fifth columns are the values found for eye-colour in man and coat-colour in the horse, and in the sixth column the mean coefficients for aphis and daphnia. In the case of the parthenogenetic animals the parental correlation compared with the results from the sexual forms is low, and the fraternal correlation distinctly high. If we exclude the less reliable absolute dimensions and only consider the ratios, the parental correlation is raised (seventh column). The question to be decided by further investigation is whether these differences are real or are merely the effects of the reaction of the organism to its environment. So far as the present investigation goes there is no increase, rather a decrease is shown in parental inheritance as we pass from sexual to parthenogenetic forms. There is possibly an increase, but we cannot as yet assert it definitely, in fraternal correlation. On the whole the numbers for both sexual and parthenogenetic inheritance lie broadly within the same limits.

TABLE XXIII.

Relationship	Exclusive Inheritance, no reversion	Blended Inheritance or Exclusive Inheritance with reversion. $\gamma=2{\cdot}35$	Man. Eye-colour	Horse. Coat-colour	Aphis and Daphnia. Mean coefficients for all dimensions	Aphis and Daphnia. Mean coefficients for ratios only
Parental ...	·50	·40	·49	·52	·41	·45
Grandparental	·25	·20	·32	·33	·24	·25
Fraternal ...	·4 to 1·0	·66	·47	·63	·66	·64

---

* *Phil. Trans. Roy. Soc.* Vol. 195 (A), p. 119.

† *Proc. Roy. Soc.* Vol. 66, 1900, p. 149.

TABLE VI.

Parents and Offspring.

Offspring: Frontal Breadth. Unit = 1 division of Ocular Micrometer = ·0221 mm.

Parents: Frontal Breadth. Unit = 1 division of Ocular Micrometer = ·0221 mm.

	12·0–12·9	*13·0–13·9*	*14·0–14·9*	*15·0–15·9*	*16·0–16·9*	*17·0–17·9*	*18·0–18·9*	*19·0–19·9*	Weighted Mothers	Number of Families
14·0—14·9	—	5	1	—	—	—	—	—	6	1
15·0—15·9	—	2	18	16	14	9	2	—	61	9
16·0—16·9	—	4	13	36	46	29	7	3	138	25
17·0—17·9	1	1	12	19	12	13	9	1	68	11
18·0—18·9	—	1	4	26	17	22	4	1	75	11
19·0—19·9	—	—	—	—	1	8	1	—	10	2
20·0—20·9	—	—	—	—	—	4	4	2	10	1
Totals	1	13	48	97	90	85	27	7	368	60

TABLE VII.

Grandparents and Grandchildren.

Grandchildren: Frontal Breadth. Unit = 1 division = ·0221 mm.

Grandparents: Frontal Breadth. Unit = 1 division of Ocular Micrometer = ·0221 mm.

	13·0–13·9	*14·0–14·9*	*15·0–15·9*	*16·0–16·9*	*17·0–17·9*	Weighted Grand-parents	Actual Number of Grand-parents
14·0—14·9	3	4	1	—	—	8	1
15·0—15·9	—	12	5	2	—	19	4
16·0—16·9	3	46	50	30	2	131	14
17·0—17·9	2	23	44	18	—	87	7
18·0—18·9	—	8	8	12	3	31	3
19·0—19·9	—	—	8	5	2	15	1
Totals	8	93	116	67	7	291	30

TABLE VIII.

Parents and Offspring.

Offspring: Length of R. Antenna. Unit = 2 divisions of Ocular Micrometer = ·0442 mm.

Parents: Length of R. Antenna. Unit = 2 divisions of Ocular Micrometer = ·0442 mm.

	24·0–25·9	*26·0–27·9*	*28·0–29·9*	*30·0–31·9*	*32·0–33·9*	*34·0–35·9*	*36·0–37·9*	*38·0–39·9*	*40·0–41·9*	*42·0–43·9*	*44·0–45·9*	*46·0–47·9*	*48·0–49·9*	*50·0–51·9*	Weighted Mothers	Number of Families
30·0—31·9	—	—	2	3	1	2	4	1	—	—	—	—	—	—	13	2
32·0—33·9	—	—	1	7	1	2	6	2	1	1	—	—	—	—	21	4
34·0—35·9	1	2	7	4	11	10	13	18	11	4	4	2	—	—	87	14
36·0—37·9	—	—	1	5	4	5	3	11	6	1	2	0	1	—	39	8
38·0—39·9	—	—	1	5	7	13	17	9	13	10	7	2	1	—	85	14
40·0—41·9	—	—	—	4	4	5	7	10	13	7	6	5	1	1	63	9
42·0—43·9	—	—	—	2	5	4	3	3	1	—	—	—	—	—	21	4
44·0—45·9	—	—	—	1	0	1	2	1	3	6	6	4	1	—	25	3
46·0—47·9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0	0
48·0—49·9	—	—	—	—	—	—	—	1	1	1	1	—	—	—	4	1
50·0—51·9	—	—	—	—	—	—	—	—	—	2	5	2	1	—	10	1
Totals	1	2	12	31	33	42	55	56	51	33	31	15	5	1	368	60

TABLE IX.

Grandparents and Grandchildren.

Grandchildren: Length of R. Antenna. Unit = 2 divisions = ·0442 mm.

Grandparents: Length of R. Antenna. Unit = 2 divisions of Ocular Micrometer = ·0442 mm.

	24·0–25·9	*26·0–27·9*	*28·0–29·9*	*30·0–31·9*	*32·0–33·9*	*34·0–35·9*	*36·0–37·9*	*38·0–39·9*	*40·0–41·9*	*42·0–43·9*	Weighted Grand-parents	Actual Number of Grand-parents
30·0—31·9	—	—	—	3	1	4	—	—	—	—	8	1
32·0—33·9	—	—	—	2	4	8	8	4	3	—	29	2
34·0—35·9	—	1	6	15	10	3	11	7	2	—	55	7
36·0—37·9	—	4	7	10	9	7	4	2	—	—	43	7
38·0—39·9	—	—	4	5	9	10	17	10	5	—	60	6
40·0—41·9	1	0	3	11	25	15	10	7	4	2	78	5
42·0—43·9	—	—	—	—	2	0	0	1	—	—	3	1
44·0—45·9	—	—	—	—	—	3	2	3	4	3	15	1
Totals	1	5	20	46	60	50	52	34	18	5	291	30

TABLE X.

Parents and Offspring.

Offspring: Ratio. Unit = 10 thousandths of Frontal Breadth.

Parents: Ratio $\left(\frac{CD}{AB}\right)$. Unit = 1 hundredth of Frontal Breadth.

	18·0–18·9	*19·0–19·9*	*20·0–20·9*	*21·0–21·9*	*22·0–22·9*	*23·0–23·9*	*24·0–24·9*	*25·0–25·9*	*26·0–26·9*	*27·0–27·9*	*28·0–28·9*	Weighted Mothers	Number of Families
18·0—18·9	—	—	—	—	1	6	—	—	—	—	—	7	1
19·0—19·9	—	—	—	—	—	—	—	—	—	—	—	0	0
20·0—20·9	1	2	6	15	8	4	2	—	—	—	—	38	7
21·0—21·9	—	4	7	9	13	18	13	4	—	—	—	68	10
22·0—22·9	—	4	2	13	17	26	21	14	7	—	—	104	18
23·0—23·9	—	1	3	7	16	8	18	17	5	1	—	76	14
24·0—24·9	—	—	—	6	7	7	9	14	4	1	—	48	7
25·0—25·9	—	—	—	—	1	1	12	4	4	1	4	27	3
Totals	1	11	18	50	63	70	75	53	20	3	4	368	60

TABLE XI.

Grandparents and Grandchildren.

Grandchildren: Ratio. Unit = 10 thousandths of Frontal Breadth.

Grandparents: Ratio. Unit = 1 hundredth of Frontal Breadth.

	18·0–18·9	*19·0–19·9*	*20·0–20·9*	*21·0–21·9*	*22·0–22·9*	*23·0–23·9*	*24·0–24·9*	*25·0–25·9*	*26·0–26·9*	Weighted Grand-parents	Actual Number of Grand-parents
20·0—20·9	—	5	11	11	9	13	5	1	—	55	4
21·0—21·9	—	1	7	9	10	15	3	1	—	46	4
22·0—22·9	—	4	8	13	8	13	6	1	—	53	10
23·0—23·9	2	2	11	20	29	16	21	10	3	114	10
24·0—24·9	—	—	—	4	3	6	5	5	—	23	2
Totals	2	12	37	57	59	63	40	18	3	291	30

TABLE XVI.

Pairs of Brethren.

Frontal Breadth, 2nd generation (60 families, 368 individuals).

The 2nd Individual taken.

The 1st Individual taken.
Unit = 1 division of Ocular Micrometer = ·0221 mm.

	12·0–12·9	*13·0–13·9*	*14·0–14·9*	*15·0–15·9*	*16·0–16·9*	*17·0–17·9*	*18·0–18·9*	*19·0–19·9*	Number of individuals involved
12·0—12·9	—	—	4	1	—	—	—	—	5
13·0—13·9	—	26	21	8	1	1	—	—	57
14·0—14·9	4	21	114	101	20	10	—	—	270
15·0—15·9	1	8	101	210	149	56	6	1	532
16·0—16·9	—	1	20	149	160	146	31	8	515
17·0—17·9	—	1	10	56	146	266	98	20	597
18·0—18·9	—	—	—	6	31	98	50	17	202
19·0—19·9	—	—	—	1	8	20	17	4	50
Totals	5	57	270	532	515	597	202	50	2228

TABLE XVII.

Pairs of Grandchildren.

Frontal Breadth, 3rd generation (30 sets of families, 291 individuals).

The 2nd Individual taken.

The 1st Individual taken.
Unit = 1 division of Ocular Micrometer = ·0221 mm.

	13·0–13·9	*14·0–14·9*	*15·0–15·9*	*16·0–16·9*	*17·0–17·9*	Number of individuals involved
13·0—13·9	8	39	23	9	—	79
14·0—14·9	39	440	387	112	9	987
15·0—15·9	23	387	612	417	39	1478
16·0—16·9	9	112	417	518	66	1122
17·0—17·9	—	9	39	66	8	122
Totals	79	987	1478	1122	122	3788

TABLE XVIII.

Pairs of Brethren.

Length of R. Antenna, 2nd generation (60 families, 368 individuals).

The 2nd Individual taken.

The 1st Individual taken. Unit = 2 divisions of Ocular Micrometer = ·0442 mm.

	24·0–25·9	*26·0–27·9*	*28·0–29·9*	*30·0–31·9*	*32·0–33·9*	*34·0–35·9*	*36·0–37·9*	*38·0–39·9*	*40·0–41·9*	*42·0–43·9*	*44·0–45·9*	*46·0–47·9*	*48·0–49·9*	*50·0–51·9*	Number of individuals involved
24·0—25·9	—	—	1	1	1	—	—	—	—	—	—	—	—	—	3
26·0—27·9	—	2	6	4	2	—	—	—	—	—	—	—	—	—	14
28·0—29·9	1	6	8	23	14	4	5	5	2	1	1	1	—	—	71
30·0—31·9	1	4	23	50	35	13	13	4	3	1	1	—	—	—	148
32·0—33·9	1	2	14	35	38	36	22	17	12	3	1	1	—	—	182
34·0—35·9	—	—	4	13	36	56	54	31	16	3	1	—	—	—	214
36·0—37·9	—	—	5	13	22	54	66	59	44	24	15	10	2	1	315
38·0—39·9	—	—	5	4	17	31	59	64	76	19	20	14	3	2	314
40·0—41·9	—	—	2	3	12	16	44	76	78	43	30	14	5	1	324
42·0—43·9	—	—	1	1	3	3	24	19	43	46	56	25	9	1	231
44·0—45·9	—	—	1	1	1	1	15	20	30	56	48	35	15	2	225
46·0—47·9	—	—	1	0	1	0	10	14	14	25	35	20	8	4	132
48·0—49·9	—	—	—	—	—	—	2	3	5	9	15	8	0	1	43
50·0—51·9	—	—	—	—	—	—	1	2	1	1	2	4	1	—	12
Totals	3	14	71	148	182	214	315	314	324	231	225	132	43	12	2228

TABLE XIX.

Pairs of Grandchildren.

Length of R. Antenna, 3rd generation (30 sets of families, 291 individuals).

The 2nd Individual taken.

The 1st Individual taken. Unit = 2 divisions of Ocular Micrometer = ·0442 mm.

	24·0–25·9	*26·0–27·9*	*28·0–29·9*	*30·0–31·9*	*32·0–33·9*	*34·0–35·9*	*36·0–37·9*	*38·0–39·9*	*40·0–41·9*	*42·0–43·9*	Number of individuals involved
24·0—25·9	—	—	—	3	3	3	—	—	—	—	9
26·0—27·9	—	6	5	9	4	6	3	—	—	—	33
28·0—29·9	—	5	22	75	33	21	13	6	5	—	180
30·0—31·9	3	9	75	194	110	74	33	23	11	4	536
32·0—33·9	3	4	33	110	178	134	118	70	36	10	696
34·0—35·9	3	6	21	74	134	128	156	90	62	17	691
36·0—37·9	—	3	13	33	118	156	214	152	85	12	786
38·0—39·9	—	—	6	23	70	90	152	74	53	17	485
40·0—41·9	—	—	5	11	36	62	85	53	24	14	290
42·0—43·9	—	—	—	4	10	17	12	17	14	8	82
Totals	9	33	180	536	696	691	786	485	290	82	3788

TABLE XX.

Pairs of Brethren.

Ratio, 3rd generation (60 families, 368 individuals).

The 2nd Individual taken.

The 1st Individual taken.
Unit = 1 hundredth of Frontal Breadth.

	18·0–18·9	*19·0–19·9*	*20·0–20·9*	*21·0–21·9*	*22·0–22·9*	*23·0–23·9*	*24·0–24·9*	*25·0–25·9*	*26·0–26·9*	*27·0–27·9*	*28·0–28·9*	Number of individuals involved
18·0—18·9	—	—	2	1	—	—	—	—	—	—	—	3
19·0—19·9	—	4	19	18	7	6	5	1	2	—	—	62
20·0—20·9	2	19	30	32	10	1	5	—	—	—	—	99
21·0—21·9	1	18	32	62	71	30	21	5	3	—	—	243
22·0—22·9	—	7	10	71	122	83	41	14	16	0	4	368
23·0—23·9	—	6	1	30	83	136	91	57	27	1	—	432
24·0—24·9	—	5	5	21	41	91	130	94	39	10	12	448
25·0—25·9	—	1	0	5	14	57	94	118	41	7	4	341
26·0—26·9	—	2	0	3	16	27	39	41	18	2	16	164
27·0—27·9	—	—	—	—	—	1	10	7	2	—	—	20
28·0—28·9	—	—	—	—	4	0	12	4	16	0	12	48
Totals	3	62	99	243	368	432	448	341	164	20	48	2228

TABLE XXI.

Pairs of Grandchildren.

Ratio, 3rd generation (30 sets of families, 291 individuals).

The 2nd Individual taken.

The 1st Individual taken.
Unit = 1 hundredth of Frontal Breadth.

	18·0–18·9	*19·0–19·9*	*20·0–20·9*	*21·0–21·9*	*22·0–22·9*	*23·0–23·9*	*24·0–24·9*	*25·0–25·9*	*26·0–26·9*	Number of individuals involved
18·0—18·9	—	1	0	3	6	0	0	1	—	11
19·0—19·9	1	26	37	42	14	11	1	—	—	132
20·0—20·9	—	37	88	94	89	59	37	6	2	412
21·0—21·9	3	42	94	148	155	134	65	34	2	677
22·0—22·9	6	14	89	155	202	202	129	45	7	849
23·0—23·9	—	11	59	134	202	266	154	53	4	883
24·0—24·9	—	1	37	65	129	154	100	56	12	554
25·0—25·9	1	0	6	34	45	53	56	34	7	236
26·0—26·9	—	—	2	2	7	4	12	7	—	34
Totals	11	132	412	677	849	883	554	236	34	3788

TABLES FOR TESTING THE GOODNESS OF FIT OF THEORY TO OBSERVATION.

By W. PALIN ELDERTON, *Actuary.*

[Received October 18, 1901.]

On the Test for Random Sampling.

Any theoretical description by means of curve or series is *ceteris paribus* admissible as a graduation of a given set of frequency observations, provided the observed values do not differ from the values provided by this theory by more than the reasonable deviations due to random sampling. There may be utilitarian reasons (e.g. relative fewness of descriptive constants, or their easy calculation) or philosophical reasons (e.g. general theories as to the nature and distribution of causes producing frequency phenomena) why we should adopt one theoretical description rather than another, but apart from such reasons that theoretical description is best, which describes the observed frequencies with the "greatest probability." By "describing the observed frequencies with the greatest probability" we understand a good although conventional test of fitness. Suppose the theoretical description of the frequencies to be the actual distribution of the whole population; we ask in how many cases per 100 in a series of random samplings should we differ from the theoretical distribution by as wide a system of deviations as that observed, or by a still wider system? In other words we want to find out the probability P that in random sampling deviation-systems as great as or greater than that actually observed will arise. This point has been dealt with in a paper by Professor K. Pearson published in the *Philosophical Magazine*[*], and it is there shown that if there be $n' = n + 1$ frequency groups in the series, and m_r and m_r' be the theoretical and observed frequencies in any group, it is necessary to find

$$\chi^2 = S\left\{\frac{(m_r - m_r')^2}{m_r}\right\} = \text{sum}\left(\frac{\left\{\begin{array}{c}\text{squares of differences of theoretical}\\ \text{and observed frequencies}\end{array}\right\}}{\text{theoretical frequency}}\right),$$

---

[*] On the Criterion that a given System of Deviations from the Probable in the case of a Correlated System of Variables is such that it can be reasonably supposed to have arisen from Random Sampling, Vol. L. pp. 157—175, July, 1900.

and that P will then be calculated from:

$$P = \sqrt{\frac{2}{\pi}} \int_{\chi}^{\infty} e^{-\frac{1}{2}\chi^2} d\chi + \sqrt{\frac{2}{\pi}}\, e^{-\frac{1}{2}\chi^2} \left(\frac{\chi}{1} + \frac{\chi^3}{1.3} + \frac{\chi^5}{1.3.5} + \dots + \frac{\chi^{n'-3}}{1.3.5\dots(n'-3)}\right)$$

if n' be even, and from:

$$P = e^{-\frac{1}{2}\chi^2} \left(1 + \frac{\chi^2}{2} + \frac{\chi^4}{2.4} + \frac{\chi^6}{2.4.6} + \dots + \frac{\chi^{n'-3}}{2.4.6\dots(n'-3)}\right)$$

if n' be odd.

Now although χ^2 can be found quite easily without any special mathematical knowledge, the calculation of P from the above formulæ is very troublesome. But it is quite clear that some test of the above kind is absolutely needful in all biometric enquiries in which we wish to test theory against observation. In the paper referred to a small table for P in terms of n' and χ^2 was given, but this table beside being far from extensive enough for actual practice, was based in some entries on values of the probability integral which had not been calculated by the use of higher differences. The present Table I. is an attempt to provide a more extensive and accurate system of values for P. It gives the values of P for $n' = 3$ to 30 and from $\chi^2 = 1$ to 30 by units and from $\chi^2 = 30$ to 70 by tens.

Method of Calculating Tables.

In order to simplify the work of calculating P for values lying outside the range of this table, or in cases where interpolation would not give sufficiently accurate results a series of additional tables are given which were used in the calculation of Table I. Thus Table II. gives the values of $\log\left(\chi\sqrt{\frac{2}{\pi}}\, e^{-\frac{1}{2}\chi^2}\right)$ and $\log(e^{-\frac{1}{2}\chi^2})$ to eight figures. Table V. gives $\log e^{-\frac{1}{2}}$ and $\log\sqrt{\frac{2}{\pi}}$ to ten figures*. Table III. gives the cologarithms of $n(n-2)(n-4)\dots 1$ (or 2) needed for the coefficients of the powers of χ to eight figures. Table IV. gives the values of $\sqrt{\frac{2}{\pi}}\int_{\chi}^{\infty} e^{-\frac{1}{2}\chi^2} d\chi$ for $\chi^2 = 1$ to 30 to eight figures, i.e. as long as it is practically sensible. Further values of this integral may be deduced from the tables for $\frac{2}{\sqrt{\pi}}\int_0^t e^{-t^2} dt$, which are given for $t = 0$ to 4·80 to eleven places of decimals for the higher values in Czuber's *Theorie der Beobachtungsfehler*, Leipzig, 1891.

In calculating the tables Erskine Scott's 10-Figure Logarithms and Filipowski's 7-Figure Antilogarithms were used. The method of calculation was, briefly, as follows. Tables were first made of $\log\left(\sqrt{\frac{2}{\pi}}\, e^{-\frac{1}{2}\chi^2}\right)$ and $\log e^{-\frac{1}{2}\chi^2}$ by continuous

---

* Thus incidentally the ordinates of the normal probability curve, $y = \frac{1}{2}\sqrt{\frac{2}{\pi}}\, e^{-\frac{1}{2}x^2}$, are given for the squares of the abscissæ.

addition and after adding $\log \chi$ to the former, the resulting figures were reduced from ten to eight places of decimals so as to avoid the error that would arise from the accumulation of small differences in the eleventh place in the value of $\log e^{-\frac{1}{2}}$. These tables were carefully checked by addition and by examining every tenth value in the continuous work. The table of $\text{colog}\,[n] \left(= \log \frac{1}{n(n-2)(n-4)\ldots}\right)$ was originally calculated to ten places. The only other auxiliary table required was for $\sqrt{\frac{2}{\pi}} \int_{\chi}^{\infty} e^{-\frac{1}{2}\chi^2} d\chi$, and these values were calculated to seven places of decimals by second differences from a table of values of $\frac{2}{\sqrt{\pi}} \int_0^t e^{-t^2} dt$*. It was quite safe to omit third differences. The values of P were then calculated from formulæ (i) and (ii) given above. In making the table, to find χ^{2s}, a column of $s \log \chi^2$ was first set up, and then by means of four moveable slips of paper (two for n' even and two for n' odd) a second column calculated giving the sum of $s \log \chi^2$, $\text{colog}\,(2s+1)$ and $\log \left(\sqrt{\frac{2}{\pi}} \chi e^{-\frac{1}{2}\chi^2}\right)$. These figures were checked by addition. The use of slips with $\text{colog}\,(n)$ written on them saved a very large amount of copying. The antilogarithms of the items of the second column were then put in a third column and the values of $\sqrt{\frac{2}{\pi}} \int_{\chi}^{\infty} e^{-\frac{1}{2}\chi^2} d\chi$ having been written at the top of it, the figures given in Table I. were found by continuous addition. The values for n' even were calculated in like manner. The numbers obtained were tested when possible against those originally published in the *Phil. Mag.* and against a few additional values calculated by Miss M. A. Lewenz. The work was of course checked at every stage, but when the table was completed the second differences in each column were examined and found to run smoothly. The like method of differences was appealed to in the case of discrepancies between the short table and the present table, which were not due to the approximate value taken for the probability integral. It is hoped that the table as it now stands is substantially free from error.

In using the present method of testing goodness of fit it is essential to bear in mind a warning given in the paper in the *Phil. Mag.* referred to above (footnote, p. 164): "A theoretical probability curve without limited range will never at the extreme tails exactly fit observation. The difficulty is obvious where the observations go by units and the theory by fractions. We ought to take our final theoretical groups to cover as much of the tail area as amounts to at least a unit of frequency in such cases."

Further we ought to be careful to read the corresponding *areas* of the frequency curve and not merely the mid-ordinates, when we have not a great number of groups, or when, although the groups are numerous, the frequency is very skew.

---

* The Table in Galloway's *Treatise on Probabilities* was the one actually used.

Illustration of use of Tables.

In the table below we have the distribution of the cephalic index in 900 skulls of modern Bavarian peasants. The frequency is given in the second column. In the third column we have the distribution as indicated by the normal curve of errors. Is this a reasonable description of the series of measurements? In the fourth column are given the values of $m_r - m_r'$ and in the fifth those of $(m_r - m_r')^2/m_r$. The resulting value of χ^2 is 18·36 and $n' = 24$. Table I. gives us: $n' = 24$, $\chi^2 = 18$, $P = {\cdot}757489$, and $\chi^2 = 19$, $P = {\cdot}701224$. Hence the required probability is nearly ·737, or roughly in every three cases out of four a random sampling would lead to a system of deviations diverging more widely from theory. Thus the fit may be considered a good one.

Cephalic Index of Bavarian Skulls.

Index	Observed	Calculated*	$m_r - m_r'$	$\frac{(m_r - m_r')^2}{m_r}$†
Under 71·5	2	1	− 1	1
72	0	1	+ 1	1
73	2·5	1·5	− 1	·67
74	1·5	3·5	+ 2	1·14
75	3·5	7·5	+ 4	2·13
76	12·5	13·5	+ 1	·07
77	17	23	+ 6	1·57
78	37	35·5	− 1·5	·06
79	55	52·5	− 2·5	·12
80	71·5	69·5	− 2	·06
81	82	86	+ 4	·19
82	116	98·5	−17·5	3·11
83	98	103	+ 5	·24
84	107	99·5	− 7·5	·57
85	82	88·5	+ 6·5	·48
86	74	72	− 2	·06
87	58	54	− 4	·30
88	34·5	37·5	+ 3	·24
89	19	23·5	+ 4·5	·86
90	10	14	+ 4	1·14
91	8	7·5	− 0·5	·03
92	3	3·5	+ 0·5	·07
93	1·5	2	+ 0·5	·125
Over 93·5	4·5	2	− 2·5	3·125
Totals	900	900	0	$\chi^2 = 18{\cdot}36$

---

* The calculated values are given to the nearest half skull because the observed values only run to this unit.

† The numbers in the fifth column were obtained from the squares of those in the fourth by dividing them by the corresponding numbers in the third. The squaring is at once done from *Barlow's Tables* and the division to the accuracy required by *Crelle's Rechentafeln*. Both these books are indispensable to biometricians.

TABLE I.

χ^2	$n'=3$	$n'=4$	$n'=5$	$n'=6$	$n'=7$	$n'=8$	$n'=9$	$n'=10$	$n'=11$
1	·606531	·801253	·909796	·962566	·985612	·994829	·998249	·999438	·999828
2	·367879	·572407	·735759	·849146	·919699	·959840	·981012	·991468	·996340
3	·223130	·391625	·557825	·699986	·808847	·885002	·934357	·964295	·981424
4	·135335	·261464	·406006	·549416	·676676	·779778	·857123	·911413	·947347
5	·082085	·171797	·287298	·415880	·543813	·659963	757576	·834308	·891178
6	·049787	·111610	·199148	·306219	·423190	·539750	·647232	·739919	·815263
7	·030197	·071897	·135888	·220640	·320847	·428880	·536632	·637119	·725444
8	·018316	·046012	·091578	·156236	·238103	·332594	·433470	·534146	·628837
9	·011109	·029291	·061099	·109064	·173578	·252656	·342296	·437274	·532104
10	·006738	·018566	·040428	·075235	·124652	·188573	·265026	·350485	·440493
11	·004087	·011726	·026564	·051380	·088376	·138619	·201699	·275709	·357518
12	·002479	·007383	·017351	·034787	·061969	·100558	·151204	·213308	·285057
13	·001503	·004637	·011276	·023379	·043036	·072109	·111850	·162607	·223672
14	·000912	·002905	·007295	·015609	·029636	·051181	·081765	·122325	·172992
15	·000553	·001817	·004701	·010363	·020256	·036000	·059145	·090937	·132061
16	·000335	·001134	·003019	·006844	·013754	·025116	·042380	·066881	·099632
17	·000203	·000707	·001933	·004500	·009283	·017396	·030109	·048716	·074364
18	·000123	·000440	·001234	·002947	·006232	·011970	·021226	·035174	·054964
19	·000075	·000273	·000786	·001922	·004164	·008187	·014860	·025193	·040263
20	·000045	·000170	·000499	·001250	·002769	·005570	·010336	·017913	·029253
21	·000028	·000105	·000317	·000810	·001835	·003770	·007147	·012650	·021093
22	·000017	·000065	·000200	·000524	·001211	·002541	·004916	·008880	·015105
23	·000010	·000040	·000127	·000338	·000796	·001705	·003364	·006197	·010747
24	·000006	·000025	·000080	·000217	·000522	·001139	·002292	·004301	·007600
25	·000004	·000016	·000050	·000139	·000341	000759	·001554	·002971	·005345
26	·000002	·000010	·000032	·000090	·000223	·000504	·001050	·002043	·003740
27	·000001	·000006	·000020	·000057	·000145	·000333	·000707	·001399	·002604
28	·000001	·000004	·000012	·000037	·000094	·000220	·000474	·000954	·001805
29	·000001	·000002	·000008	·000023	·000061	·000145	·000317	·000648	·001246
30	·000000	·000001	·000005	·000015	·000039	·000095	·000211	·000439	·000857
40	·000000	·000000	·000000	·000000	·000001	·000001	·000003	·000008	·000017
50	·000000	·000000	·000000	·000000	·000000	·000000	·000000	·000000	·000000
60	·000000	·000000	·000000	·000000	·000000	·000000	·000000	·000000	·000000
70	·000000	·000000	·000000	·000000	·000000	·000000	·000000	·000000	·000000

TABLE I.—*continued.*

χ^2	$n'=12$	$n'=13$	$n'=14$	$n'=15$	$n'=16$	$n'=17$	$n'=18$	$n'=19$	$n'=20$
1	·999950	·999986	·999997	·999999	1·	1·	1·	1·	1·
2	·998496	·999406	·999774	·999917	·999970	·999990	·999997	·999999	1·
3	·990726	·995544	·997934	·999074	·999598	·999830	·999931	·999972	·999989
4	·969917	·983436	·991191	·995466	·997737	·998903	·999483	·999763	·999894
5	·931167	·957979	·975193	·985813	·992127	·995754	·997771	·998860	·999431
6	·873365	·916082	·946153	·966491	·979749	·988095	·993187	·996197	·997929
7	·799073	·857613	·902151	·934711	·957650	·973260	·983549	·990125	·994213
8	·713304	·785131	·843601	·889327	·923783	·948867	·966547	·978637	·986671
9	·621892	·702931	·772943	·831051	·877517	·913414	·940261	·959743	·973479
10	·530387	·615960	·693934	·762183	·819739	·866628	·903610	·931906	·952946
11	·443263	·528919	·610817	·686036	·752594	·809485	·856564	·894357	·923839
12	·362642	·445680	·527643	·606303	·679028	·743980	·800136	·847237	·885624
13	·293326	·369041	·447812	·526524	·602298	·672758	·736186	·791573	·838571
14	·232993	·300708	·373844	·449711	·525529	·598714	·667102	·729091	·783691
15	·182498	·241436	·307354	·378154	·451418	·524638	·595482	·661967	·722598
16	·141130	·191236	·249129	·313374	·382051	·452961	·523834	·592547	·657277
17	·107876	·149597	·199304	·256178	·318864	·385597	·454366	·523105	·589868
18	·081581	·115691	·157520	·206781	·262666	·323897	·388841	·455653	·522438
19	·061094	·088529	·123104	·164949	·213734	·268663	·328532	·391823	·456836
20	·045341	·067086	·095210	·130141	·171932	·220220	·274229	·332819	·394578
21	·033371	·050380	·072929	·101632	·136830	·178510	·226291	·279413	·336801
22	·024374	·037520	·055362	·078614	·107804	·143191	·184719	·231985	·284256
23	·017676	·027726	·041677	·060270	·084140	·113735	·149251	·190590	·237342
24	·012733	·020341	·031130	·045822	·065093	·089504	·119435	·155028	·196152
25	·009117	·014822	·023084	·034566	·049943	·069824	·094710	·124915	·160542
26	·006490	·010734	·017001	·025887	·038023	·054028	·074461	·099758	·130189
27	·004595	·007727	·012441	·019254	·028736	·041483	·058068	·078995	·104653
28	·003238	·005532	·009050	·014228	·021569	·031620	·044938	·062055	·083428
29	·002270	·003940	·006546	·010450	·016085	·023936	·034526	·048379	·065985
30	·001585	·002792	·004710	·007632	·011921	·018002	·026345	·037446	·051798
40	·000036	·000072	·000138	·000255	·000453	·000778	·001294	·002087	·003272
50	·000001	·000001	·000003	·000006	·000012	·000023	·000042	·000075	·000131
60	·000000	·000000	·000000	·000000	·000000	·000001	·000001	·000002	·000004
70	·000000	·000000	·000000	·000000	·000000	·000000	·000000	·000000	·000000

TABLE I.—*continued.*

χ^2	$n'=21$	$n'=22$	$n'=23$	$n'=24$	$n'=25$	$n'=26$	$n'=27$	$n'=28$	$n'=29$	$n'=30$
1	1·	1·	1·	1·	1·	1·	1·	1·	1·	1·
2	1·	1·	1·	1·	1·	1·	1·	1·	1·	1·
3	·999996	·999998	·999999	1·	1·	1·	1·	1·	1·	1·
4	·999954	·999980	·999992	·999997	·999999	1·	1·	1·	1·	1·
5	·999722	·999868	·999939	·999972	·999987	·999994	·999998	·999999	1·	1·
6	·998898	·999427	·999708	·999855	·999929	·999966	·999984	·999993	·999997	·999999
7	·996685	·998142	·998980	·999452	·999711	·999851	·999924	·999962	·999981	·999991
8	·991868	·995143	·997160	·998371	·999085	·999494	·999726	·999853	·999924	·999960
9	·982907	·989214	·993331	·995957	·997595	·998596	·999194	·999546	·999748	·999863
10	·968171	·978912	·986304	·991277	·994547	·996653	·997981	·998803	·999302	·999599
11	·946223	·962787	·974749	·983189	·989012	·992946	·995549	·997239	·998315	·998988
12	·916076	·939617	·957379	·970470	·979908	·986567	·991173	·994294	·996372	·997728
13	·877384	·908624	·933161	·951990	·966121	·976501	·983974	·989247	·992900	·995384
14	·830496	·869599	·901479	·926871	·946650	·961732	·973000	·981254	·987189	·991377
15	·776408	·822952	·862238	·894634	·920759	·941383	·957334	·969432	·978436	·985015
16	·716624	·769650	·815886	·855268	·888076	·914828	·936203	·952947	·965819	·975536
17	·652974	·711106	·763362	·809251	·848662	·881793	·909083	·931122	·948589	·962181
18	·587408	·649004	·705988	·757489	·803008	·842390	·875773	·903519	·926149	·944272
19	·521826	·585140	·645328	·701224	·751990	·797120	·836430	·870001	·898136	·921288
20	·457930	·521261	·583040	·641912	·696776	·746825	·791556	·830756	·864464	·892927
21	·397132	·458944	·520738	·581087	·638725	·692609	·741964	·786288	·825349	·859149
22	·340511	·399510	·459889	·520252	·579267	·635744	·688697	·737377	·781291	·820189
23	·288795	·343979	·401730	·460771	·519798	·577564	·632947	·685013	·733041	·776543
24	·242392	·293058	·347229	·403808	·461597	·519373	·575965	·630316	·681535	·728932
25	·201431	·247164	·297075	·350285	·405760	·462373	·518975	·574462	·627835	·678248
26	·165812	·206449	·251682	·300866	·353165	·407598	·463105	·518600	·573045	·625491
27	·135264	·170853	·211226	·255967	·304453	·355884	·409333	·463794	·518247	·571705
28	·109399	·140151	·175681	·215781	·260040	·307853	·358458	·410973	·464447	·517913
29	·087759	·114002	·144861	·180310	·220131	·263916	·311082	·360899	·412528	·465066
30	·069854	·091988	·118464	·149402	·184752	·224289	·267611	·314154	·363218	·414004
40	·004995	·007437	·010812	·015369	·021387	·029164	·039012	·051237	·066128	·083937
50	·000221	·000365	·000586	·000921	·001416	·002131	·003144	·004551	·006467	·009032
60	·000007	·000013	·000022	·000038	·000064	·000104	·000168	·000264	·000407	·000618
70	·000000	·000000	·000001	·000001	·000002	·000004	·000007	·000011	·000019	·000030

TABLE II.

χ^2	$\log\left\{\chi\sqrt{\frac{2}{\pi}}\,e^{-\frac{1}{2}\chi^2}\right\}$	$\log e^{-\frac{1}{2}\chi^2}$	χ^2	$\log\left\{\chi\sqrt{\frac{2}{\pi}}\,e^{-\frac{1}{2}\chi^2}\right\}$	$\log e^{-\frac{1}{2}\chi^2}$
1	$\bar{1}\cdot68479282$	$\bar{1}\cdot78285276$	*51*	$\overline{11}\cdot68121586$	$\overline{12}\cdot92549071$
2	$\bar{1}\cdot61816058$	$\bar{1}\cdot56570552$	*52*	$\overline{11}\cdot46828520$	$\overline{12}\cdot70834347$
3	$\bar{1}\cdot48905897$	$\bar{1}\cdot34855828$	*53*	$\overline{11}\cdot25527422$	$\overline{12}\cdot49119623$
4	$\bar{1}\cdot33438109$	$\bar{1}\cdot13141104$	*54*	$\overline{11}\cdot04218593$	$\overline{12}\cdot27404899$
5	$\bar{1}\cdot16568886$	$\bar{2}\cdot91426380$	*55*	$\overline{12}\cdot82902315$	$\overline{12}\cdot05690175$
6	$\bar{2}\cdot98813224$	$\bar{2}\cdot69711655$	*56*	$\overline{12}\cdot61578858$	$\overline{13}\cdot83975450$
7	$\bar{2}\cdot80445839$	$\bar{2}\cdot47996931$	*57*	$\overline{12}\cdot40248475$	$\overline{13}\cdot62260726$
8	$\bar{2}\cdot61630713$	$\bar{2}\cdot26282207$	*58*	$\overline{12}\cdot18911408$	$\overline{13}\cdot40546002$
9	$\bar{2}\cdot42473615$	$\bar{2}\cdot04567483$	*59*	$\overline{13}\cdot97567885$	$\overline{13}\cdot18831278$
10	$\bar{2}\cdot23046765$	$\bar{3}\cdot82852759$	*60*	$\overline{13}\cdot76218123$	$\overline{14}\cdot97116554$
11	$\bar{2}\cdot03401675$	$\bar{3}\cdot61138035$	*61*	$\overline{13}\cdot54862328$	$\overline{14}\cdot75401830$
12	$\bar{3}\cdot83576379$	$\bar{3}\cdot39423311$	*62*	$\overline{13}\cdot33500696$	$\overline{14}\cdot53687106$
13	$\bar{3}\cdot63599760$	$\bar{3}\cdot17708587$	*63*	$\overline{13}\cdot12133415$	$\overline{14}\cdot31972382$
14	$\bar{3}\cdot43494271$	$\bar{4}\cdot95993863$	*64*	$\overline{14}\cdot90760662$	$\overline{14}\cdot10257658$
15	$\bar{3}\cdot23277708$	$\bar{4}\cdot74279139$	*65*	$\overline{14}\cdot69382607$	$\overline{15}\cdot88542934$
16	$\bar{3}\cdot02964420$	$\bar{4}\cdot52564414$	*66*	$\overline{14}\cdot48099412$	$\overline{15}\cdot66828209$
17	$\bar{4}\cdot82566143$	$\bar{4}\cdot30849690$	*67*	$\overline{14}\cdot26611232$	$\overline{15}\cdot45113485$
18	$\bar{4}\cdot62092598$	$\bar{4}\cdot09134966$	*68*	$\overline{14}\cdot05218213$	$\overline{15}\cdot23398761$
19	$\bar{4}\cdot41551928$	$\bar{5}\cdot87420242$	*69*	$\overline{15}\cdot83820498$	$\overline{15}\cdot01684037$
20	$\bar{4}\cdot20951024$	$\bar{5}\cdot65705518$	*70*	$\overline{15}\cdot62418221$	$\overline{16}\cdot79969313$
21	$\bar{4}\cdot00295765$	$\bar{5}\cdot43990794$	*71*	$\overline{15}\cdot41011512$	$\overline{16}\cdot58254589$
22	$\bar{5}\cdot79591210$	$\bar{5}\cdot22276070$	*72*	$\overline{15}\cdot19600496$	$\overline{16}\cdot36539865$
23	$\bar{5}\cdot58841744$	$\bar{5}\cdot00561346$	*73*	$\overline{16}\cdot98185290$	$\overline{16}\cdot14825141$
24	$\bar{5}\cdot38051190$	$\bar{6}\cdot78846622$	*74*	$\overline{16}\cdot76766009$	$\overline{17}\cdot93110417$
25	$\bar{5}\cdot17222904$	$\bar{6}\cdot57131898$	*75*	$\overline{16}\cdot55342762$	$\overline{17}\cdot71395693$
26	$\bar{6}\cdot96359847$	$\bar{6}\cdot35417173$	*76*	$\overline{16}\cdot33915654$	$\overline{17}\cdot49680968$
27	$\bar{6}\cdot75464644$	$\bar{6}\cdot13702449$	*77*	$\overline{16}\cdot12484787$	$\overline{17}\cdot27966244$
28	$\bar{6}\cdot54539633$	$\bar{7}\cdot91987725$	*78*	$\overline{17}\cdot91050256$	$\overline{17}\cdot06251520$
29	$\bar{6}\cdot33586907$	$\bar{7}\cdot70273001$	*79*	$\overline{17}\cdot69612157$	$\overline{18}\cdot84536796$
30	$\bar{6}\cdot12608346$	$\bar{7}\cdot48558277$	*80*	$\overline{17}\cdot48170578$	$\overline{18}\cdot62822072$
31	$\bar{7}\cdot91605644$	$\bar{7}\cdot26843553$	*81*	$\overline{17}\cdot26725605$	$\overline{18}\cdot41107348$
32	$\bar{7}\cdot70580334$	$\bar{7}\cdot05128829$	*82*	$\overline{17}\cdot05277323$	$\overline{18}\cdot19392624$
33	$\bar{7}\cdot49533808$	$\bar{8}\cdot83414105$	*83*	$\overline{18}\cdot83825810$	$\overline{19}\cdot97677900$
34	$\bar{7}\cdot28467333$	$\bar{8}\cdot61699381$	*84*	$\overline{18}\cdot62371146$	$\overline{19}\cdot75963176$
35	$\bar{7}\cdot07382065$	$\bar{8}\cdot39984657$	*85*	$\overline{18}\cdot40913404$	$\overline{19}\cdot54248452$
36	$\bar{8}\cdot86279064$	$\bar{8}\cdot18269932$	*86*	$\overline{18}\cdot19452656$	$\overline{19}\cdot32533727$
37	$\bar{8}\cdot65159301$	$\bar{9}\cdot96555208$	*87*	$\overline{19}\cdot97988972$	$\overline{19}\cdot10819003$
38	$\bar{8}\cdot44023670$	$\bar{9}\cdot74840484$	*88*	$\overline{19}\cdot76522419$	$\overline{20}\cdot89104279$
39	$\bar{8}\cdot22872997$	$\bar{9}\cdot53125760$	*89*	$\overline{19}\cdot55053062$	$\overline{20}\cdot67389555$
40	$\bar{8}\cdot01708042$	$\bar{9}\cdot31411036$	*90*	$\overline{19}\cdot33580963$	$\overline{20}\cdot45674831$
41	$\bar{9}\cdot80529511$	$\bar{9}\cdot09696312$	*91*	$\overline{19}\cdot12106183$	$\overline{20}\cdot23960107$
42	$\bar{9}\cdot59338058$	$\overline{10}\cdot87981588$	*92*	$\overline{20}\cdot90628780$	$\overline{20}\cdot02245383$
43	$\bar{9}\cdot38134293$	$\overline{10}\cdot66266864$	*93*	$\overline{20}\cdot69148812$	$\overline{21}\cdot80530659$
44	$\bar{9}\cdot16918780$	$\overline{10}\cdot44552140$	*94*	$\overline{20}\cdot47666333$	$\overline{21}\cdot58815935$
45	$\overline{10}\cdot95692047$	$\overline{10}\cdot22837416$	*95*	$\overline{20}\cdot26181397$	$\overline{21}\cdot37101211$
46	$\overline{10}\cdot74454589$	$\overline{10}\cdot01122691$	*96*	$\overline{20}\cdot04694054$	$\overline{21}\cdot15386486$
47	$\overline{10}\cdot53206866$	$\overline{11}\cdot79407967$	*97*	$\overline{21}\cdot83204355$	$\overline{22}\cdot93671762$
48	$\overline{10}\cdot31949311$	$\overline{11}\cdot57693243$	*98*	$\overline{21}\cdot61712348$	$\overline{22}\cdot71957038$
49	$\overline{10}\cdot10682329$	$\overline{11}\cdot35978519$	*99*	$\overline{21}\cdot40218080$	$\overline{22}\cdot50242314$
50	$\overline{11}\cdot89406301$	$\overline{11}\cdot14263795$	*100*	$\overline{21}\cdot18721596$	$\overline{22}\cdot28527590$

TABLE III.

Table of colog $[n]$:—$[n] = n(n-2)(n-4)\ldots\ldots$

n odd nos.	colog $[n]$	n even nos.	colog $[n]$
1	·00000000	2	$\overline{1}$·69897000
3	$\overline{1}$·52287875	4	$\overline{1}$·09691001
5	$\overline{2}$·82390874	6	$\overline{2}$·31875876
7	$\overline{3}$·97881070	8	$\overline{3}$·41566878
9	$\overline{3}$·02456819	10	$\overline{4}$·41566878
11	$\overline{5}$·98317551	12	$\overline{5}$·33648753
13	$\overline{6}$·86923215	14	$\overline{6}$·19035949
15	$\overline{7}$·69314089	16	$\overline{8}$·98623951
17	$\overline{8}$·46269197	18	$\overline{9}$·73096701
19	$\overline{9}$·18393837	20	$\overline{10}$·42993701
21	$\overline{11}$·86171908	22	$\overline{11}$·08751433
23	$\overline{12}$·49999124	24	$\overline{13}$·70730309
25	$\overline{13}$·10205123	26	$\overline{14}$·29232974
27	$\overline{15}$·67068747	28	$\overline{16}$·84517171
29	$\overline{16}$·20828947	30	$\overline{17}$·36805045
31	$\overline{18}$·71692778	32	$\overline{19}$·86290048
33	$\overline{19}$·19841384	34	$\overline{20}$·33142156
35	$\overline{21}$·65434579	36	$\overline{22}$·77511906
37	$\overline{22}$·08614407	38	$\overline{23}$·19533546
39	$\overline{24}$·49507946	40	$\overline{25}$·59327547
41	$\overline{26}$·88229561	42	$\overline{27}$·97002618
43	$\overline{27}$·24882715	44	$\overline{28}$·32657350
45	$\overline{29}$·59561464	46	$\overline{30}$·66381567
47	$\overline{31}$·92351678	48	$\overline{32}$·98257443
49	$\overline{32}$·23332070	50	$\overline{33}$·28360443
51	$\overline{34}$·52575052	52	$\overline{35}$·56760109
53	$\overline{36}$·80147465	54	$\overline{37}$·83520733
55	$\overline{37}$·06111196	56	$\overline{38}$·08701930
57	$\overline{39}$·30523711	58	$\overline{40}$·32359131
59	$\overline{41}$·53438510	60	$\overline{42}$·54544006
61	$\overline{43}$·74905526	62	$\overline{44}$·75304837
63	$\overline{45}$·94971471	64	$\overline{46}$·94686839
65	$\overline{46}$·13680135	66	$\overline{47}$·12732446
67	$\overline{48}$·31072655	68	$\overline{49}$·29481554
69	$\overline{50}$·47187746	70	$\overline{51}$·44971750
71	$\overline{52}$·62061911	72	$\overline{53}$·59238501
73	$\overline{54}$·75729625	74	$\overline{55}$·72315329
75	$\overline{56}$·88223499	76	$\overline{57}$·84233970
77	$\overline{58}$·99574426	78	$\overline{59}$·95024509
79	$\overline{59}$·09811717	80	$\overline{60}$·04715511
81	$\overline{61}$·18963215	82	$\overline{62}$·13334125
83	$\overline{63}$·27055406	84	$\overline{64}$·20906197
85	$\overline{65}$·34113514	86	$\overline{66}$·27456352
87	$\overline{67}$·40161588	88	$\overline{68}$·33008084
89	$\overline{69}$·45222588	90	$\overline{70}$·37583833
91	$\overline{71}$·49318448	92	$\overline{72}$·41205051
93	$\overline{73}$·52470154	94	$\overline{74}$·43892265
95	$\overline{75}$·54697793	96	$\overline{76}$·45665142
97	$\overline{77}$·56020620	98	$\overline{78}$·46542534
99	$\overline{79}$·56457100	100	$\overline{80}$·46542534

TABLE IV.

χ^2	$\sqrt{\frac{2}{\pi}}\int_{\chi}^{\infty} e^{-\frac{1}{2}\chi^2}\,d\chi$
1	·3173106
2	·1572992
3	·0832646
4	·0455003
5	·0253474
6	·0143060
7	·0081506
8	·0046776
9	·0026998
10	·0015654
11	·0009112
12	·0005321
13	·0003115
14	·0001828
15	·0001076
16	·0000634
17	·0000374
18	·0000221
19	·0000132
20	·0000078
21	·0000046
22	·0000027
23	·0000016
24	·0000011
25	·0000007
26	·0000004
27	·0000003
28	·0000002
29	·0000001
30	·0000000

TABLE V.

Function	Log. Function
$e^{-\frac{1}{2}}$	$\overline{1}$·7828527590
$\sqrt{\frac{2}{\pi}}$	$\overline{1}$·9019400615

THE EGG OF CUCULUS CANORUS.

AN ENQUIRY INTO THE DIMENSIONS OF THE CUCKOO'S EGG AND THE RELATION OF THE VARIATIONS TO THE SIZE OF THE EGGS OF THE FOSTER-PARENT, WITH NOTES ON COLORATION, &c.

BY OSWALD H. LATTER.

[Received October 2, 1901.]

Introductory.

THE present paper is the outcome of an examination of 44 Cuckoo's eggs in the collections at the Charterhouse Museum. The results of this preliminary investigation were communicated to the Congress of South Eastern Natural History Associations, held in the summer of 1901 at Haslemere, under the auspices of the Haslemere Microscope and Natural History Society, of which I have the honour to be a member. Finding that 44 was far too small a number of eggs for my purpose I extended the series of measurements by including a large number obtained at the British Museum of Natural History, S. Kensington, and I may here take the opportunity of expressing my thanks to the Director, Prof. Ray Lankester, and Mr Ogilvie Grant for granting me permission to examine the fine series under their care, and also to Mr Baldwin Young of Sheffield, who kindly supplied measurements of six Cuckoo's eggs in his possession. The total number of Cuckoo's eggs measured and included in this enquiry is 243, of which 223 were known to have been deposited in the nests of 42 different species of birds, while the foster-parents of the remaining 20 were not ascertainable; these 20 have not been excluded from the calculations, for their effect upon the value of mean length and breadth is practically negligible. In dealing with coloration, a further 45 which were not accessible for purposes of measurement have been included, bringing the total to 288. All measurements were taken with sliding callipers reading to millimetres, the decimal parts of a millimetre being estimated by eye aided with a strong lens. The dimensions measured are greatest length and greatest breadth.

It has been established by many observers that the female Cuckoo lays her egg upon the ground and then taking it in her beak puts it into the nest of the foster-parents of her offspring. An explanation is needed of the success which attends this imposition. Are the foster-parents deceived either by similarity of colouring or of size into fancying the Cuckoo's egg to be one of their own? or are they indifferent to these qualities? or are some small birds more expert than others in detecting fraud?

The theory which finds more favour than others is that put forward by Prof. A. Newton (*Dictionary of Birds*, p. 123); who, after mentioning the history of speculation on the matter, writes as follows :—"Everyone who has sufficiently studied the habits of animals will admit the tendency of some of these habits to become hereditary. That there is a reasonable probability of each Cuckow most commonly putting her eggs in the nest of the same species of bird, and of this habit being transmitted to her posterity, does not seem to be a very violent supposition. Without attributing any wonderful sagacity to her, it does not seem unlikely that the Cuckow which had once successfully foisted her egg on a Reed-wren or a Titlark should again seek for another Reed-wren's or another Titlark's nest (as the case may be).........and that she should continue her practice from one season to another.......... Such a habit could hardly fail to become hereditary, so that the daughter of a Cuckow which always put her egg into a Reed-wren's, Titlark's, or Wagtail's nest, would do as did her mother......... and it can hardly be questioned that the eggs of the daughter would more or less resemble those of her mother. Hence the supposition may be fairly regarded that the habit of laying a particular style of egg is also likely to become hereditary .... The particular '*gens*' of Cuckow which inherited and transmitted the habit of depositing in the nest of any particular species of bird eggs having more or less resemblance to the eggs of that species would prosper most in those members of the '*gens*' where the likeness was strongest, and the other members would (*caeteris paribus*) in time be eliminated....... The operation of this kind of natural selection would be most needed in those cases where the species are not easily duped—that is in those cases which occur the least frequently. Here it is we find it, for observation shows that eggs of the Cuckow deposited in the nests of the Red-backed Shrike, of the Bunting, of the Red Start and of the Icterine Warbler approximate in their colouring to eggs of these species—species in whose nests the Cuckow rarely (in comparison with others) deposits eggs."

I must confess that I approached this investigation with decided scepticism as to the validity of Prof. Newton's theory. It is very doubtful whether the Cuckoo is aware that she has "successfully foisted her egg on a Reed-wren" or on any other bird: so far as is known she takes no further interest in the egg—it may escape detection, it may be ejected. Moreover, the theory seems to demand that male Cuckoos should mate with female Cuckoos reared by foster-parents of the same species, or else that the inherited habits and characters of every female Cuckoo should follow only the female line of descent. For suppose a Reed-wren-reared female Cuckoo to mate with a Robin-reared male Cuckoo, then their offspring might be reasonably expected to inherit some characters from each parent and to possess mixed tendencies, some urging them to lay in Robins' nests and others in those of Reed-wrens, and, unless inheritance run only in the female line (or mating taking place only between individuals of like foster-parentage), the tendencies would get further mingled in each succeeding generation. This criticism appears to me to apply with equal force to Reh's theory of the intuitive selection of the nests of the species by which the Cuckoos themselves were reared. Further, there is very little, if any, evidence in support of the operation of natural selection in eliminating eggs that do not match those of the clutch into which they have been introduced. I have not come across any record of such badly-matched Cuckoo's eggs having been found ejected from the nest. Lastly, it is very difficult to conceive how perfection of colour-matching can have arisen by natural selection with relatively few opportunities for the working of this force; and, if the case be as Prof. Newton states, I certainly should have expected to find a large number of Cuckoo's eggs in the nests of those birds whose eggs had at length been so admirably copied.

Size-Matching.

In spite of these criticisms I am compelled by the results of my investigations to admit that I now believe Professor Newton's theory to be in the main correct: I will return to this point later. It will be seen by the appended summary of results and tables of measurements that the mean length (22·40 mm.) and mean breadth (16·54 mm.) of 243 Cuckoo's eggs are respectively greater than the mean lengths and breadths of the eggs of any of the four species [viz. *Anthus pratensis*, the Meadow Pipit; *Anthus trivialis*, the Tree Pipit; *Accentor modularis*, the Hedge Sparrow, and *Erithacus rubecula*, the Robin], of whose eggs I was able to measure a reasonable number. The range of length extends from 19·1 mm. to 25·0 mm.; that of breadth from 14·0 mm. to 18·8 mm.: the standard deviation (σ) of length being 1·058, and the coefficient of variation (C. of V.) of length 4·72, those of breadth being respectively 0·6496 and 3·93. Of the four species which serve as foster-parents and are here dealt with, the Meadow Pipit alone exceeds the Cuckoo in degree of variation, but it must be remembered that the number of measured eggs of Robins and other species is very much less than in the case of the Cuckoo, so that it is quite possible that the Cuckoo does not differ greatly from other birds in this respect*. It is however interesting to note that in

---

* Further, the Cuckoo's eggs were probably laid by about 200 separate hens, while the 74 Meadow Pipit's eggs, for example, are formed by 20 clutches or due to 20 hens only.

each of the five species length of egg is a far more variable dimension than breadth: this is probably due to uniformity of diameter of the oviduct, and it may also be of importance to the comfort of the female bird during the period of incubation, for an egg projecting far above its fellows in consequence of greater breadth would probably inconvenience the sitter.

My enquiry has thus resolved itself chiefly into an attempt to ascertain (1) if the eggs of Cuckoos deposited in the nests of any one species stand out as a set apart from Cuckoo's eggs deposited elsewhere; (2) if the same eggs depart from the rest in such a direction as to approximate in size to the eggs of that particular species of foster-parent. The method employed is to find the mean (M) length or breadth, as the case may be, thence to compute the standard deviation (σ) by the formula $\sigma^2 = \frac{\text{sum}\,(M-x)^2}{n}$, where x = the measurement of any one egg and n = the number of eggs measured: and then to find $\frac{100\,\sigma}{M}$, the coefficient of variation. To test whether any deviation is significant, M_r is taken as the mean of the whole race of Cuckoos and M_s the mean of Cuckoo's eggs found in the nest of any one species of foster-parent: the standard deviation (σ_s) of such eggs is also ascertained. The value of $M_r - M_s$ is then compared with that of $0{\cdot}67449\sqrt{\frac{\sigma_r^2}{n_1} + \frac{\sigma_s^2}{n_2}}$, where n_1 = total number of Cuckoo's eggs and n_2 = the number of Cuckoo's eggs in the nests of the species in question, which is the probable error of $M_r - M_s$ due to random sampling. If the value of $M_r - M_s$ be not at least 1·5 to 3 times as great as the value of the other expression the difference of M_r and M_s is not definitely significant. Referring now to the tabulated summary below, it will be seen that in the matter of length, the eggs of Meadow-Pipit-Cuckoos (to coin a convenient phrase), of Wagtail-Cuckoos (this includes all species of Wagtails, for their eggs are very similar), and of Robin-Cuckoos, do not differ significantly from those of the whole race of Cuckoos, but those of Hedge-Sparrow-Cuckoos, Tree-Pipit-Cuckoos and of Wren-Cuckoos certainly do present differences marking them out as distinct sets. On the other hand in the matter of breadth the differences are significant in the cases of Meadow-Pipit-Cuckoos, Hedge-Sparrow-Cuckoos and Wren-Cuckoos. It therefore seems highly probable that there are certain "gentes" of Cuckoos whose members being closely related lay eggs of somewhat similar dimensions and in the main confine their attentions each to its own particular variety of foster-parent. Breadth, as I have already pointed out, seems more likely than length to be a disturbing factor in the nest of the foster-parent if it in any marked way depart from the normal, and it is very remarkable to note how very low are four of the values of σ_s in the breadth tables, viz. 2·28 (Meadow-Pipit-Cuckoo), 2·58 (Wagtail-Cuckoo), 2·65 (Tree-Pipit-Cuckoo), 1·92 (Wren-Cuckoo).

Next, as to whether these sets differ from the main body in the sense of the particular species of foster-parent. In the Wren-Cuckoos this is so beyond

Summary.

Species	Number of Eggs	Mean (M)	Standard Deviation (σ)	Coefficient of Variation	Significance Test (Ratio of difference to its probable error)
Length in millimetres					
Cuculus canorus	243	22·4	1·0585	4·72	
C. canorus-Anthus pratensis (Meadow-Pipit-Cuckoo)	45	22·3	0·8933	4·00	difference not significant (1·1)
Anthus pratensis	74	19·7	1·2504	6·37	
C. canorus-Accentor modularis (Hedge-Sparrow-Cuckoo)	14	23·1	1·0116	4·37	difference *significant* (3·71)
Accentor modularis	26	20·0	0·8096	4·02	
C. canorus-Erithacus rubecula (Robin-Cuckoo)	16	22·5	0·6628	2·50	difference not significant (1·25)
Erithacus rubecula	57	20·2	0·8565	4·24	
C. canorus-Motacilla, sp. 4 (Wagtail-Cuckoos)	26	22·6	0·8783	3·88	difference not significant (1·25)
Wagtails estimated	—	19·9			
C. canorus-Anthus trivialis (Tree-Pipit-Cuckoo)	15	23·1	0·8504	3·68	difference *significant* (4·5)
Anthus trivialis	27	20·0	0·6978	3·489	
C. canorus-Troglodytes parvulus	15	21·1	0·7558	3·58	difference *significant* (9·3)
Troglodytes parvulus	Estimated from W. J. Gordon	17·7			
Breadth in millimetres					
Cuculus canorus	243	16·5	0·6496	3·93	
C. canorus-Anthus pratensis (Meadow-Pipit-Cuckoo)	45	16·7	0·3815	2·28	difference *significant* (3·4)
Anthus pratensis	74	14·6	0·5611	3·84	
C. canorus-Accentor modularis (Hedge-Sparrow-Cuckoo)	14	16·8	0·5161	3·07	difference *significant* (2·4)
Accentor modularis	26	14·7	0·4146	2·81	
C. canorus-Erithacus rubecula (Robin-Cuckoo)	16	16·4	0·5326	3·24	difference not significant (·96)
Erithacus rubecula	57	15·4	0·4771	3·09	
C. canorus-Motacilla, sp. 4 (Wagtail-Cuckoos)	26	16·6	0·4389	2·58	difference not significant (·93)
Wagtails estimated	—	14·9			
C. canorus-Anthus trivialis (Tree-Pipit-Cuckoo)	15	16·6	0·4397	2·65	difference not significant (·75)
Anthus trivialis	27	15·1	0·4488	2·97	
C. canorus-Troglodytes parvulus	15	15·8	0·3042	1·92	difference *significant* (12·3)
Troglodytes parvulus	Estimated from W. J. Gordon's "Our Country's Birds"	12·7			

doubt. Unfortunately I had not material sufficient to determine trustworthy means of length and breadth of Wren's eggs, but I have estimated them from measurements given *in inches* by W. J. Gordon in *Our Country's Birds,* and feel confident that no error of any importance exists in his figures, for the measurements given by him of other species' eggs approximate very closely with the means obtained by myself in each case. It will be seen that the egg of the Wren is far smaller than that of any other species with which we are dealing, and that the lengths and breadths of Wren-Cuckoos' eggs are very much less than those of other Cuckoos. For the other species where the differences are significant this sense of the variation is not clear so far as it concerns breadth, though it appears to be so in the matter of length: the two subjoined tables give the comparison.

Mean Length			
Hedge-Sparrow-Cuckoo	23·1	Hedge-Sparrow	20·1
Tree-Pipit-Cuckoo	23·1	Tree-Pipit	20·0
Meadow-Pipit-Cuckoo	22·3	Meadow-Pipit	19·7
Wren-Cuckoo	21·1	Wren	17·7

Mean Breadth			
Hedge-Sparrow-Cuckoo	16·8	Tree-Pipit	15·1
Meadow-Pipit-Cuckoo	16·7	Hedge-Sparrow	14·7
Tree-Pipit-Cuckoo	16·6	Meadow-Pipit	14·6
Wren-Cuckoo	15·8	Wren	12·7

Colour-matching.

As already stated, 288 Cuckoo's eggs were examined in this connection and compared with the eggs among which they had been deposited. In 39 cases the matching was extremely close, and in a further 109 there was a fair approximation, rendering the Cuckoo's egg more or less similar in appearance to those of the foster-parent, the two categories giving a total of 148 eggs more or less closely matched to their several clutches, or at any rate within the limits of colour-variations exhibited by the species in question. Reh has observed this last same phenomenon in nests of the Red-backed Shrike (*Lanius collaris*), and eggs of this description possess an especial interest, for they seem to afford very strong support to Prof. Newton's theory. It is a fairly frequent occurrence to find in the nests of birds whose eggs exhibit considerable variation of colour, e.g. Meadow-Pipit, Tree-Pipit and Reed-Warbler, a Cuckoo's egg, not resembling the particular clutch in which it occurs but which would match eggs of another clutch of the same species. On the other hand, I found but three instances of Cuckoo's eggs with a close resemblance to eggs of any one species being deposited elsewhere than in the nests of that species, viz. egg No. 152 found in nest of Lesser Whitethroat is a good match to a Meadow-Pipit's eggs; No. 173 in nest of Chaffinch matches Wagtail's eggs; No. 187 in Nightingale's nest matches House-Sparrow's (very variable) eggs.

Colour-matching.

	Good
11	Meadow-Pipit
7	Wagtails
6	Orphean-Warbler
4	Garden-Warbler
3	Red-Start
1	Robin
1	Sedge-Warbler
1	Barred-Warbler
1	Aquatic-Warbler
1	Whitethroat
1	Wheatear { sp. Saxicola melanoleuca
1	Pied-Flycatcher
1	Hedge-Sparrow
Total 39	

	Fair, or within limits of colour-variation of egg of species
45	Meadow-Pipit
19	Wagtails
11	Tree-Pipit
7	Reed-Warbler
6	Robin
5	Blackcap
4	Whitethroat
2	Marsh-Warbler
2	Sedge-Warbler
2	Red-backed-Shrike
1	Lesser-Grey-Shrike
1	Lesser-Whitethroat
1	Tree-Sparrow
1	Common-Bunting
1	Sky-Lark
1	Crested-Lark
Total 109	

	Not matched nor within limits of colour-variation of egg
17	Wren
19	Hedge-Sparrow
11	Robin
12	Yellow-Ammer
13	Reed-Warbler
4	Garden-Warbler
6	Meadow-Pipit
4	Tree-Pipit
5	Willow-Warbler
5	Greenfinch
4	Chaffinch
4	Linnet
3	Pied-Wagtail
2	Blackcap
2	Red-Start
2	Lesser-Whitethroat
2	Nightingale
2	Wood-Warbler
2	Spotted-Flycatcher
2	Sedge-Warbler
2	Red-backed-Shrike
1	Woodchat-Shrike
1	Rock-Pipit
1	Sky-Lark
1	Crested-Lark
1	Goldfinch
1	House-Sparrow
1	Common-Bunting
1	Cirl-Bunting
1	Great-Reed-Warbler
1	Marsh-Warbler
1	Grasshopper-Warbler
1	Dartford-Warbler
1	Barred-Warbler
1	Wheatear
1	Orphean-Warbler
1	Chiffchaff
1	Fire-Crested-Wren
Total 140	

The appended table of colour-matching gives in a concise form the more important numerical facts. The total number of successes, complete or partial, in colour-matching is so great, nearly 51·4% of the whole series examined, that mere chance is impossible as an explanation. Moreover, the accuracy with which highly remarkable Cuckoo's eggs are deposited in appropriate clutches is so striking as to tempt one to dally with conscious selection and deliberate choice on the part of the female Cuckoo: for instance, six blue Cuckoo's eggs occur in the series, of these three were in Red Starts' nests (Nos. 155, 158, and one not accessible for measurement), one (No. 159) in nest of *Saxicola melanoleuca,* and two others (not accessible) in nests of Hedge-Sparrow and Pied-Flycatcher. All these birds lay blue eggs, and so far as my observations go, blue Cuckoo's eggs are not deposited elsewhere, though Howard Saunders's statement (*Manual of British Birds,* p. 288) that "these, (sc. blue eggs), have not been invariably located in nests of the Hedge-Sparrow and the Red Start" leads me to suppose that they may have been found in the nests of birds whose eggs are not blue. Again, the egg of the Orphean Warbler is of a very distinct type, and yet in six cases out of seven the Cuckoo's egg deposited in the nest of this species resembles this type to a nicety, nor is this particular variety of Cuckoo's egg to be found in any other nest.

	Matched more or less	Not matched	Total	Percentage matched
Meadow-Pipit ...	56	6	62	90·3 °/₀
Wagtails... ...	26	3	29	89·6 °/₀
Hedge-Sparrow ...	1	19	20	5·0 °/₀
Robin	7	11	18	38·8 °/₀
Reed-Warbler ...	7	13	20	35·5 °/₀
Tree-Pipit ...	11	4	15	73·3 °/₀
Wren	0	17	17	0·0 °/₀
Yellow-Ammer ...	0	12	12	0·0 °/₀
Garden-Warbler	4	4	8	50·0 °/₀
Orphean-Warbler	6	1	7	85·7 °/₀

Perhaps the most striking point in connection with colour-matching is its entire absence from the eggs of Wren-Cuckoos, which, though closely resembling one another, in no case match those of the Wren itself. The Wren is the only bird of those dealt with in this paper that constructs a nest of such a character as to render a view of the eggs impossible alike to the Wren and the Cuckoo; hence failure in colour-matching cannot possibly reveal the intruder to the lawful owner and discrepancy in size becomes of more importance. It is very remarkable that, in both length and breadth, as already pointed out, the eggs of Wren-Cuckoos show a far wider variation from the average Cuckoo and in the direction of the Wren's egg than is the case with any other species. It is too a well known fact that the Wren is peculiarly intolerant of interference with her nest—at any rate at the hands of human beings.

To sum up, we note that there are three cases of practically no colour-matching, the Hedge-Sparrow-Cuckoo, the Wren-Cuckoo and the Yellow-Ammer-Cuckoo; in the first two of these cases there is an attempt both as to length and breadth at size-matching. In the third case no significant size-difference is to be found from our data, but these are too scant to be really conclusive.

In spite then of the criticisms expressed above and of the absence of actual evidence of the ejection of Cuckoo's eggs by small birds, I feel compelled to admit that there is a selective process at work, tending, in many cases, to bring the Cuckoo's eggs into agreement with those of the host both in size and colour, and am inclined to suggest that perhaps there may be *local* "gentes" of Cuckoos which as a rule, but by no means exclusively, patronise the nests of particular species. It seems well established (1) that a Cuckoo returns every year to the same locality, and (2) according to Reh, lays its eggs only in the nests of that particular species which it, or its ancestors, happen to have adopted for that purpose, while the coloration of the egg of every female Cuckoo is peculiar to itself. The evidence that my material furnishes on these points is as follows:—the Cuckoo's eggs in the South Kensington Collection from any one locality frequently exhibit strong resemblances *inter se*, e.g. (*a*) Nos. 107, 108, 109 (Robin-Cuckoo), 180 (Greenfinch-Cuckoo), 189 (Spotted-Flycatcher-Cuckoo), and 61 (Meadow-Pipit-Cuckoo), all taken at Churt in the year 1860, are all so closely similar that they may well be from one and the same bird; (*b*) Nos. 25 and 26 (Meadow-Pipit-Cuckoo) from Lochend, but not dated; (*c*) Nos. 34 and 35 (both in same nest of Meadow-Pipit) from S.W. Lancashire, dated June 29, 1866, and 33 (Meadow-Pipit-Cuckoo) from N.W. Cheshire, dated 25 May, 1866, and all three collected by H. E. Smith; (*d*) No. 193 (Red-backed-Shrike-Cuckoo, June, 1863), 110 (Robin-Cuckoo, June, 1863), 195 (Skylark-Cuckoo, May, 1862), 181 (Greenfinch-Cuckoo, 1864), 243 (Linnet-Cuckoo, June, 1864), and 168 (Yellow-Ammer-Cuckoo, June, 1864), all coming from Churt, and (*e*) Nos. 225—236 (Wren-Cuckoo), Pomerania, 1879, 1880 and 1881, exhibit the same phenomenon in their several sets. But it is to be observed that eggs of the same coloration are not confined to the nests of any one species of foster-parent, except in (*c*) and (*e*). Again, eleven nests contained two Cuckoo's eggs apiece, viz. Nos. 28 and 29, 34 and 35, 36 and 37, 38 and 39, and two others not accessible for measurement, all in Meadow Pipits' nests, 139 and 140 in Orphean Warblers', 210 and 211 in Pied Wagtails', 214 and 215, 217 and 218 in Pied Wagtails', and 145 and 146, 147 and 148 in Reed Warblers', and in each case the two eggs are obviously laid by the same parent. Reh mentions the fact that in 1893 within two kilometres from Leipzig no less than 70 nests were found containing Cuckoo's eggs, and of these, 58 (83 °/₀), were in nests of Red-backed Shrike. It seems possible then that in any given locality a majority of the Cuckoos may favour some one particular species of foster-parent, and if this be so the chances of male and female Cuckoos of like rearing mating together are very largely increased, and the difficulty raised in a preceding passage is to some extent removed. It is however evident that the isolation of "gens" from "gens" is not perfect, and this may perhaps be accounted for by occasional matings between birds of unlike foster-parentage and the offspring inheriting mixed tendencies.

In conclusion I must acknowledge my great indebtedness to Professor Karl Pearson for the interest he has taken in this investigation and for much kind advice and assistance in the statistical portions.

APPENDIX I. *Cuckoo* (*C. canorus*). *Length of Egg in millimetres.*

(The names of species refer to the nest in which the eggs were found.)

Clutch Unknown

1. 22·5
2. 20·1
3. 23·3
4. 22·9
5. 23·1
6. 22·0
7. 22·3
8. 23·6
9. 24·7
10. 23·7
11. 24·0
12. 20·4
13. 21·3
14. 22·0
15. 24·2
16. 21·7
17. 21·0
18. 20·1
19. 21·9
20. 21·9

Meadow-Pipit

21. 21·7
22. 22·6
23. 20·9
24. 21·6
25. 22·2
26. 22·5
27. 22·2
28. 24·3
29. 22·3
30. 22·6
31. 20·1
32. 22·0
33. 22·8
34. 22·0
35. 22·4
36. 22·3
37. 20·6
38. 22·1
39. 21·9
40. 23·0
41. 22·0
42. 22·0
43. 22·1
44. 22·0
45. 19·6
46. 22·8
47. 22·0
48. 23·4
49. 23·8
50. 23·3
51. 22·5
52. 22·3
53. 21·9
54. 22·0
55. 21·7
56. 23·3
57. 22·2
58. 22·3
59. 22·8
60. 22·9
61. 23·7
62. 22·0
63. 21·9
64. 22·2
65. 24·4

Tree-Pipit

66. 22·7
67. 23·3
68. 24·0
69. 23·6
70. 22·1
71. 21·8
72. 21·1
73. 23·4
74. 23·8
75. 23·3
76. 24·0
77. 23·5
78. 23·2
79. 24·0
80. 22·4

Rock-Pipit

81. 23·9

Hedge-Sparrow

82. 22·0
83. 23·9
84. 20·9
85. 23·8
86. 25·0
87. 24·0
88. 21·7
89. 23·8
90. 22·8
91. 23·1
92. 23·1
93. 23·5
94. 23·0
95. 23·0

Robin

96. 21·8
97. 23·0
98. 23·3
99. 22·4
100. 22·4
101. 23·0
102. 23·0
103. 23·0
104. 23·9
105. 22·3
106. 22·0
107. 22·6
108. 22·0
109. 22·1
110. 21·1
111. 23·0

Blackcap

112. 21·3
113. 19·9
114. 22·9
115. 23·3
116. 22·1
117. 20·9

Whitethroat

118. 21·9
119. 22·9
120. 22·4
121. 19·1

Chiffchaff

122. 20·9

Sedge-Warbler

123. 23·0
124. 21·0
125. 22·0
126. 21·3

Wood-Warbler

127. 22·0
128. 22·0

Willow-Warbler

129. 21·3
130. 21·1
131. 23·1
132. 21·0

Garden-Warbler

133. 23·0
134. 23·1
135. 22·9
136. 23·2

Orphean-Warbler

137. 24·4
138. 21·5
139. 23·3
140. 22·3

Reed-Warbler

141. 23·2
142. 22·0
143. 22·2
144. 21·2
145. 21·6
146. 21·6
147. 21·9
148. 22·0
149. 22·9
150. 22·8

Marsh-Warbler

151. 22·7

Lesser-Whitethroat

152. 21·0
153. 22·5

Barred-Warbler

154. 21·9

Red-Start

155. 24·0
156. 23·2
157. 22·3
158. 23·0

Saxicola-Melanoleuca

159. 23·1

Reed-Bunting

160. 23·2
161. 20·9

Yellow-Ammer

162. 22·5
163. 21·9
164. 22·5
165. 23·3
166. 23·2
167. 23·0
168. 21·1
169. 22·9
170. 23·3

Corn-Bunting

171. 20·1
172. 22·9

Chaffinch

173. 23·3
174. 22·3
175. 22·9
176. 21·2

Goldfinch

177. 20·9

Greenfinch

178. 22·7
179. 22·8
180. 22·1
181. 23·4
182. 21·2
183. 22·5

House-Sparrow

184. 23·9

Tree-Sparrow

185. 24·0

Nightingale

186. 22·8
187. 23·2

Spotted-Flycatcher

188. 22·1
189. 22·4

Lesser-Grey-Shrike

190. 23·0

Woodchat-Shrike

191. 22·0

Red-backed-Shrike

192. 22·1
193. 20·8

Sky-Lark

194. 22·2
195. 21·2

Crested-Lark

196. 22·5
197. 21·1

Pied-Wagtail

198. 23·0
199. 23·4
200. 24·0
201. 23·3
202. 23·1
203. 22·4
204. 21·8
205. 21·8
206. 24·9
207. 24·0
208. 22·1
209. 21·0
210. 22·6
211. 21·9
212. 24·0

White-Wagtail

213. 22·3
214. 22·6
215. 22·0
216. 22·7
217. 22·3
218. 22·5

Blueheaded-Yellow-Wagtail

219. 21·2
220. 22·4

Yellow-Wagtail

221. 22·2
222. 22·2
223. 23·0

Wren

224. 19·8
225. 22·1
226. 21·5
227. 20·9
228. 22·0
229. 21·0
230. 22·3
231. 21·0
232. 20·3
233. 20·9
234. 22·0
235. 20·0
236. 20·8
237. 21·2
238. 21·0

Fire-Crested-Wren

239. 24·2

Wheatear

240. 22·8

Linnet

241. 24·7
242. 24·0
243. 22·9

APPENDIX II. *Cuckoo* (*C. canorus*). *Breadth of Egg in millimetres.*

Clutch Unknown	*1.*	17·0
	2.	14·9
	3.	16·0
	4.	17·4
	5.	17·4
	6.	16·5
	7.	17·2
	8.	17·2
	9.	18·0
	10.	17·8
	11.	18·0
	12.	15·0
	13.	16·0
	14.	16·5
	15.	17·3
	16.	16·9
	17.	16·1
	18.	15·8
	19.	15·9
	20.	16·2
Meadow-Pipit	*21.*	16·1
	22.	17·0
	23.	16·2
	24.	16·2
	25.	16·9
	26.	16·9
	27.	17·3
	28.	16·8
	29.	16·8
	30.	17·0
	31.	16·5
	32.	16·9
	33.	16·5
	34.	17·0
	35.	17·0
	36.	16·3
	37.	16·2
	38.	16·8
	39.	17·0
	40.	16·9
	41.	17·0
	42.	17·0
	43.	17·3
	44.	16·8
	45.	15·8
	46.	17·1
	47.	16·9
	48.	16·4
	49.	16·4
	50.	16·8
	51.	17·1
	52.	17·0
	53.	17·1
	54.	17·2
	55.	16·2
	56.	16·7
	57.	16·8
	58.	16·2
	59.	16·4
	60.	17·2
	61.	17·0
	62.	17·2
	63.	17·0
	64.	16·2
	65.	16·2
Tree-Pipit	*66.*	16·3
	67.	16·6
	68.	17·0
	69.	16·9
	70.	16·3
	71.	16·7
	72.	16·5
	73.	16·2
	74.	16·3
	75.	16·7
	76.	17·5
	77.	17·3
	78.	16·4
	79.	17·3
	80.	16·0
Rock-Pipit	*81.*	16·4
Hedge-Sparrow	*82.*	17·0
	83.	16·9
	84.	15·8
	85.	17·3
	86.	17·5
	87.	17·5
	88.	16·2
	89.	16·5
	90.	16·2
	91.	17·1
	92.	16·1
	93.	16·9
	94.	16·7
	95.	17·0
Robin	*96.*	16·0
	97.	15·9
	98.	17·1
	99.	16·6
	100.	16·9
	101.	16·1
	102.	17·2
	103.	16·2
	104.	16·9
	105.	15·2
	106.	16·3
	107.	17·0
	108.	16·0
	109.	16·4
	110.	16·4
	111.	17·0
Blackcap	*112.*	16·1
	113.	16·0
	114.	16·0
	115.	16·1
	116.	17·8
	117.	15·3
Whitethroat	*118.*	17·0
	119.	16·4
	120.	16·6
	121.	14·0
Chiffchaff	*122.*	15·3
Sedge-Warbler	*123.*	16·0
	124.	16·1
	125.	16·3
	126.	16·5
Wood-Warbler	*127.*	16·2
	128.	15·2
Willow-Warbler	*129.*	15·8
	130.	15·8
	131.	16·6
	132.	16·0
Garden-Warbler	*133.*	16·2
	134.	16·8
	135.	16·0
	136.	17·0
Orphean-Warbler	*137.*	17·9
	138.	16·0
	139.	16·4
	140.	16·3
Reed-Warbler	*141.*	16·9
	142.	17·1
	143.	17·0
	144.	16·1
	145.	16·5
	146.	16·5
	147.	16·1
	148.	16·5
	149.	17·9
	150.	16·5
Marsh-Warbler	*151.*	16·7
Lesser-Whitethroat	*152.*	16·0
	153.	16·2
Barred-Warbler	*154.*	16·8
Red-Start	*155.*	17·7
	156.	16·2
	157.	16·7
	158.	17·0
Saxicola-Melanoleuca	*159.*	17·1
Reed-Bunting	*160.*	16·9
	161.	15·9
Yellow-Ammer	*162.*	16·0
	163.	16·0
	164.	16·1
	165.	17·2
	166.	16·5
	167.	17·0
	168.	17·0
	169.	17·0
	170.	16·8
Corn-Bunting	*171.*	15·8
	172.	17·0
Chaffinch	*173.*	16·1
	174.	16·2
	175.	17·3
	176.	15·7
Goldfinch	*177.*	16·0
Greenfinch	*178.*	14·5
	179.	16·7
	180.	16·9
	181.	17·0
	182.	16·2
	183.	17·0
House-Sparrow	*184.*	17·7
Tree-Sparrow	*185.*	16·0
Nightingale	*186.*	17·2
	187.	16·0
Spotted-Flycatcher	*188.*	15·8
	189.	16·5
Lesser-Grey-Shrike	*190.*	16·8
Woodchat-Shrike	*191.*	17·0
Red-backed-Shrike	*192.*	17·1
	193.	15·7
Sky-Lark	*194.*	18·1
	195.	15·6
Crested-Lark	*196.*	16·4
	197.	15·9
Pied-Wagtail	*198.*	16·3
	199.	16·7
	200.	17·0
	201.	16·3
	202.	16·7
	203.	16·5
	204.	16·0
	205.	16·0
	206.	16·8
	207.	15·8
	208.	16·2
	209.	17·1
	210.	16·0
	211.	16·9
	212.	17·2
White-Wagtail	*213.*	16·8
	214.	17·0
	215.	17·0
	216.	16·9
	217.	17·3
	218.	16·9
Blueheaded-Yellow-Wagtail	*219.*	15·9
	220.	17·2
Yellow-Wagtail	*221.*	16·7
	222.	16·4
	223.	16·3
Wren	*224.*	15·0
	225.	16·0
	226.	16·2
	227.	15·7
	228.	16·2
	229.	15·5
	230.	16·0
	231.	15·9
	232.	15·5
	233.	15·9
	234.	16·0
	235.	15·7
	236.	15·9
	237.	16·0
	238.	16·0
Fire-Crested-Wren	*239.*	16·9
Wheatear	*240.*	16·6
Linnet	*241.*	16·3
	242.	18·8
	243.	17·1

APPENDIX III.

Frequency Distribution of Cuckoo's Eggs.

Length	Number	Breadth	Number
18·75—19·25	1	13·75—14·25	1
19·25—19·75	1	14·25—14·75	1
19·75—20·25	7	14·75—15·25	5
20·25—20·75	3	15·25—15·75	9
20·75—21·25	29	15·75—16·25	73
21·25—21·75	13	16·25—16·75	51
21·75—22·25	54	16·75—17·25	80
22·25—22·75	38	17·25—17·75	15
22·75—23·25	47	17·75—18·25	7
23·25—23·75	22	18·25—18·75	0
23·75—24·25	21	18·75—19·25	1
24·25—24·75	5		
24·75—25·25	2		
	243		243

These distributions are fitted with normal curves in the accompanying diagrams.

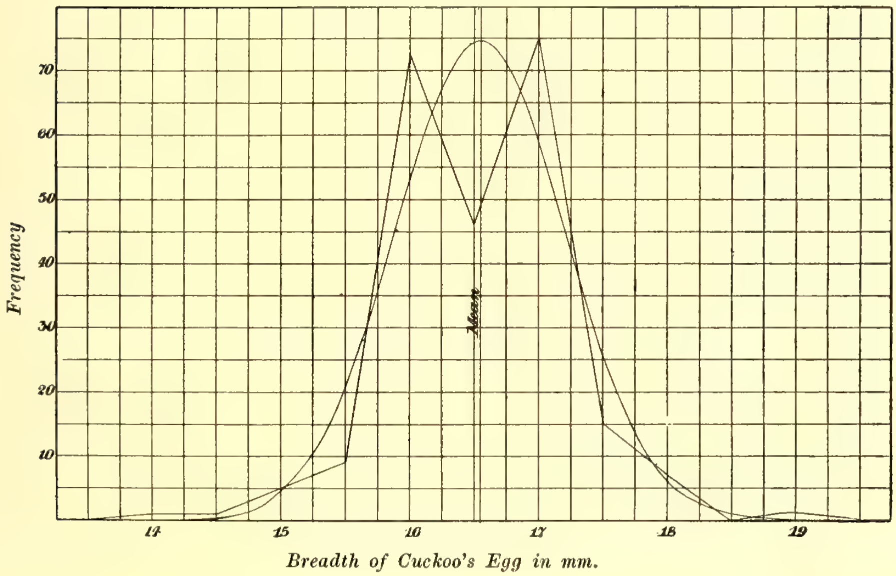
Length $$y = 45{\cdot}793\, e^{-\frac{1}{2}x^2/(2{\cdot}117)^2}.$$

Origin at 22·40.

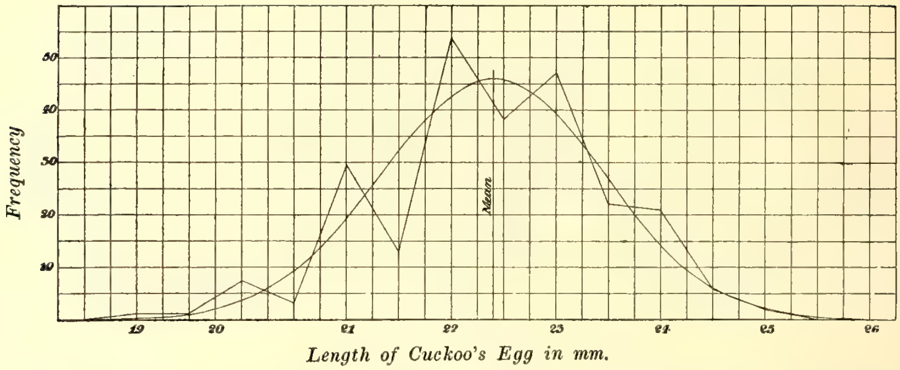
Breadth $$y = 74{\cdot}618\, e^{-\frac{1}{2}x^2/(1{\cdot}2992)^2}.$$

Origin at 16·54.

Breadth of Cuckoo's Egg.



Length of Cuckoo's Egg.



The curves give fairly reasonable graduated values, considering: (i) the paucity of data, and (ii) the possibility of class differences within the race indicated in this memoir.

APPENDIX IV.

Table of Egg Measurements.

The following is a summary of my measurements on the Cuckoo's and other birds' eggs. I have added the results of recent measurements on the eggs of House-Sparrow, Blackbird, Song-Thrush, Starling and Linnet made by Professor Pearson and some of his co-workers. The whole serves to illustrate the relative smallness of the Cuckoo's egg.

Bird	Approximate length in inches	No. of Cases	Length of Egg			Breadth of Egg		
			Mean	S. D.	C. of V.	Mean	S. D.	C. of V.
Cuckoo ...	14	243	22·40	1·0585	4·72	16·54	·6496	3·93
Blackbird ...	10	114	29·44	1·3568	4·61	21·73	·7874	3·62
Song-Thrush ...	9	151	27·44	·9988	3·64	20·69	·5162	2·50
Starling ...	8—8·5	27	29·78	1·0973	3·68	21·76	·4233	1·94
Wagtail* ...	7—8	16	20·75	1·4448	6·96	14·67	·3703	2·52
Yellow-Ammer	7	32	21·55	·6821	3·17	16·04	·4045	2·53
Tree-Pipit ...	6·5	27	20·01	·6978	3·49	15·09	·4488	2·97
Meadow-Pipit	6	74	19·72	1·2504	6·37	14·56	·5611	3·84
House-Sparrow	6	687	21·82	1·1946	5·47	15·51	·5245	3·38
Hedge-Sparrow	6	26	20·12	·8096	4·02	14·73	·4146	2·81
Robin	6	57	20·22	·8565	4·24	15·43	·4771	3·09
Linnet... ...	5·5—6	65	17·14	·5984	3·49	13·33	·3581	2·69

* This was a mixed series made up of 6 eggs of White-Wagtail, 7 of Pied-Wagtail and 3 of Blue-headed-Yellow-Wagtail. This accounts for the great variability in length. We see that the Cuckoo's egg is the most variable of the whole series in breadth and with the exception of the Meadow-Pipit's and House-Sparrow's the most variable also in length. The biggest of all the birds here dealt with, the Cuckoo has an egg hardly longer than the House-Sparrow's or broader than the Yellow-Ammer's.

ON CRIMINAL ANTHROPOMETRY AND THE IDENTIFICATION OF CRIMINALS.

By W. R. MACDONELL, M.A., LL.D.

[Received November 5, 1901.]

CONTENTS.

PART I.

Material and Methods.

PART II.

Application of Methods.

PART III.

Identification of Criminals.

PART I.

MATERIAL AND METHODS.

(1) The object of this memoir is threefold:

(i) To test to what extent the criminal classes diverge in physical characters from other classes of the community.

(ii) To consider how far the shorter methods recently proposed by Professor Karl Pearson for finding the variability and correlation of characters in the case of normal frequency may be applied to some of the chief anthropometric measurements now customarily made, and

(iii) To determine what is the best manner in which these measurements can be applied to the identification of criminals.

I shall first consider the material I have had at my disposal; I shall then indicate the methods I have used for the determination of its metrical constants, and finally apply my results to the consideration of the above three problems. In the course of my work I shall have to consider the important point of the homogeneity and normality of my material, and apply three separate tests:

(i) If broken up into groups, the statistical constants of a sub-group ought to remain, within the limits of random sampling, the same as the bulk of the observations.

(ii) Frequency distributions, if plotted and fitted with frequency curves, ought to give nearly normal distributions.

(iii) The regression lines in type cases ought to be closely represented by straight lines.

(2) *Nature of material used.* The data on which the memoir is based were obtained, through the kindness of Dr Garson, from the Central Metric Office, New Scotland Yard, where the register of habitual criminals is kept, and their identification effected. In an interesting paper published in the *Journal of the Anthropological Institute,* Vol. XXX. 1900, July—Dec., Dr Garson has explained in detail the metric system of identification in force in England, and the sort of prisoners whose metric description is registered at the Central Office, and whom we may call briefly "habitual" criminals.

But in addition to the official forms on which the descriptions of habitual criminals are recorded, the Central Office possesses a number of "practice" forms which record the metric description of a less pronounced type of criminal than the "habituals"—prisoners whose crimes and sentences are comparatively slight, and who may be called "non-habitual." These practice forms are filled up by warders on probation, and as their advancement in the Service depends on the accuracy with which they measure and describe the prisoners, they may be expected to do so with the utmost care. Indeed, I understand from Dr Garson that these "practice" forms may be accepted with as much confidence as the official forms for habitual criminals.

It is with these non-habitual criminals that we are concerned. Obviously, the larger the number of individuals we include, the better will be the results; on the other hand, the number must not be extremely large, otherwise the investigation becomes too laborious. Accordingly a group of 3000 was decided on as likely to give good results, while still keeping the work within manageable compass. Three thousand metric forms were thereupon obtained from Scotland Yard, containing particulars of 3000 male prisoners undergoing their sentences in the chief prisons of England and Wales. The majority of the prisoners were English and Welsh, many were Irish, and only a few Scotch; no foreigners or youths under 21 were included. The forms were drawn at random from the mass on the office shelves; we are therefore dealing with a random sampling.

The metric description recorded on the forms includes (*inter alia*) certain physical measurements, viz., those of the Head Length, Head Breadth, Face Breadth, Left Middle Finger, Left Cubit, Left Foot, and Height, of each individual. These measurements are taken, in the case of height, to the nearest $\frac{1}{8}$ of an inch, in the case of the other characters, to the nearest millimetre; thus, Head Length of 19·2 centimetres includes all head lengths from 19·15 cm. to 19·25 cm.; Height of 5 ft. $5\frac{1}{8}$ in. includes all heights from 5 ft. $5\frac{1}{16}$ in. to 5 ft. $5\frac{3}{16}$ in. These characters form the subject of the present memoir, and will be studied from the anthropometrical point of view in the earlier part of the paper; the problem of identification, with which Scotland Yard is concerned, will be discussed in the latter part.

(3) *Methods employed.* In dealing with this large mass of observations, I propose to apply the methods explained in Professor Karl Pearson's memoirs in the *Philosophical Transactions*, Vol. 195, A., pp. 1—47, 79—150, in order to calculate the Means, Standard Deviations, and Coefficients of Correlation, with their probable errors, for the above seven characters. These methods were devised by Professor Pearson to deal with characters not quantitatively measureable, but they are used here with the object of saving much of the labour involved in making these calculations in the ordinary way from the usual elaborate correlation tables. It will be seen later on that the saving of time and labour is very considerable.

To obtain the coefficients of correlation Professor Pearson shows (*loc. cit.* p. 2) that a 4-fold table is required; to find standard deviations and means, and probable errors and error correlations of all the quantities involved, a 9-fold table is required (*loc. cit.* pp. 82, 84). If a 16-fold table is formed, it will give the required 4-fold and 9-fold tables, as is shown in the following example, which is a frequency correlation table for the Head Length and Face Breadth of the 3000 criminals.

Head Length (in cms.)

Face Breadth (in cms.)

Class	*18·8* and under	*18·9* to *19·1*	*19·2* to *19·3*	*19·4* and over	Totals
13·4 and under	453	222	135	229	1039
13·5 to *13·6* ...	147	99	63	152	461
13·7 to *13·8* ...	112	99	74	176	461
13·9 and over	146	177	156	560	1039
Totals	858	597	428	1117	3000

From this the following 9-fold table is constructed:

Head Length.

Face Breadth.

Class	*18·8* and under	*18·9* to *19·3*	*19·4* and over	Totals
13·4 and under	453	357	229	1039
13·5 to *13·8* ...	259	335	328	922
13·9 and over	146	333	560	1039
Totals	858	1025	1117	3000

and finally a 4-fold table to find coefficient of correlation:

Head Length.

Face Breadth.

Class	*19·1* and under	*19·2* and over	Totals
13·6 and under	921	579	1500
13·7 and over	534	966	1500
Totals	1455	1545	3000

(It may be remarked that this latter table would also give the means, but we shall use the 9-fold table for this purpose.)

It seemed at the outset that the best results would be obtained by choosing the limits of divisions so that the totals of each column and of each row in the 4-fold and 9-fold tables should be as nearly as possible equal to a half and a third respectively of the whole 3000; but this principle was found in some cases to be inapplicable*.

(4) Before making use of these short tables we will first consider the results obtained in the case of our 3000 criminals from three frequency correlation tables (Tables I. II. and III.) made up in the usual way. The methods of preparing the tables and calculating the results, with their probable errors, are very fully explained

---

* In fixing the limits much help was obtained from Dr Garson's results.

in Mr G. Udny Yule's paper "On the Theory of Correlation" in the *Journal of the Royal Statistical Society*, Vol. LX., Part IV., December 1897. We have modified Mr Yule's formula for the square of the standard deviation by diminishing it by the quantity $\frac{1}{12}$, in accordance with Mr W. F. Sheppard's paper in the *Proceedings of the London Mathematical Society*, Vol. XXIX., Nos. 634/5.

The results are as follows:

TABLE 1.

3000 *Criminals.*

	Standard Deviation	Mean
Head Length (cm.) ...	·6046 ± ·0053	19·1663 ± ·0075
Head Breadth (cm.)...	·5014 ± ·0044	15·0442 ± ·0062
L. M. Finger (cm.) ...	·5479 ± ·0048	11·5474 ± ·0068
Height (ins.)	2·5410 ± ·0221	65·5355 ± ·0313

TABLE 2.

3000 *Criminals.*

	Coefficient of Correlation
Head Length and Head Breadth	·4016 ± ·0103
Head Breadth and Height ...	·1831 ± ·0119
L. M. Finger and Height ...	·6608 ± ·0069

(5) *Tests of Normality.* At the foot of Tables II. and III. (see pp. 215, 216) are shown the mean Finger Length and mean Head Breadth of each column array; these means are plotted on Figs. 1 and 2, and the lines are drawn which show the theoretical regression of Finger on Height, and Head Breadth on Height. The slope of these lines is calculated from the formula $\tan\theta = \frac{r\sigma_2}{\sigma_1}$, where r is the coefficient of correlation, and σ_1, σ_2, the standard deviations of the correlated organs. (See Yule, *loc. cit.*) The means of the column arrays at the extremities of the tables are not included, as they are based on such small frequencies.

It will be observed that the regression lines fit the observations very well, i.e., the regression is very closely linear.

Fig. 1. Line of Regression: Left Middle Finger on Height. 3000 Criminals.
Mean Height 65·5. Mean Finger 115·5. Tan $\theta = 1·425$.

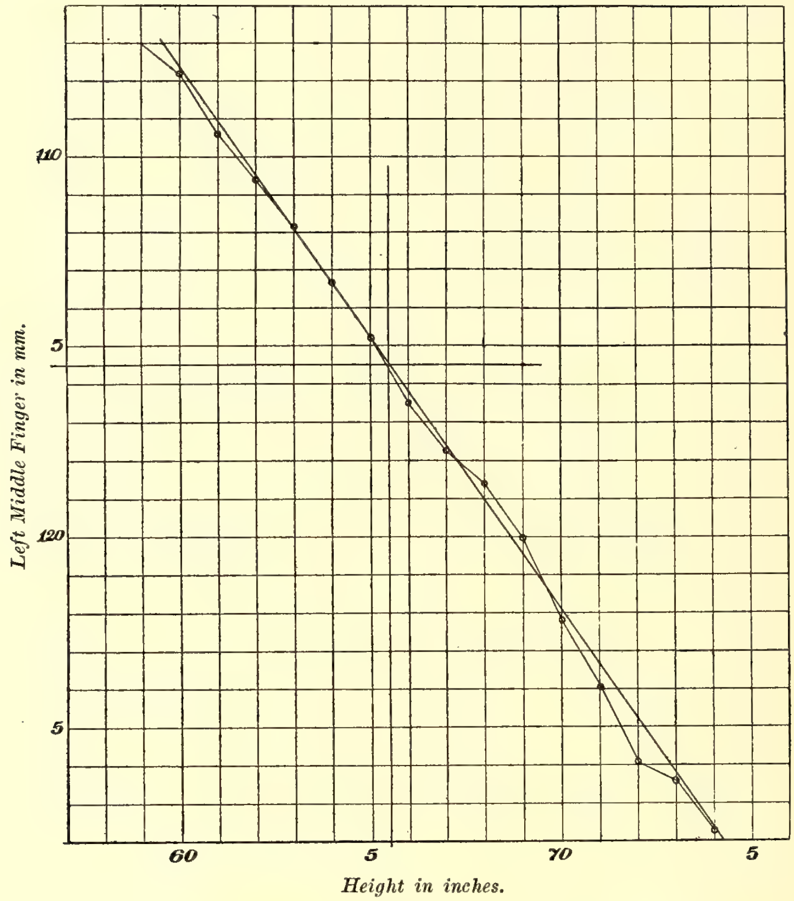
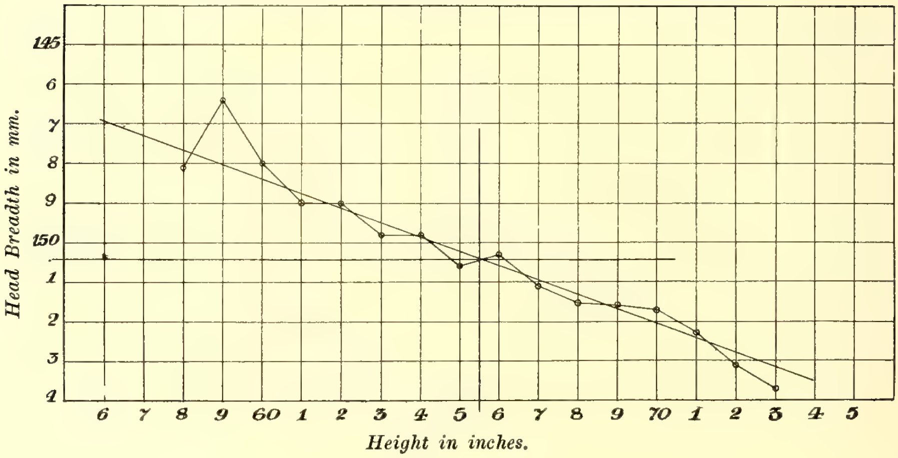


Fig. 2. Line of Regression: Head Breadth on Height. 3000 Criminals.
Mean Height 65·5. Mean Breadth 150·4. Tan $\theta = ·3612$.



We will next attempt to fit a curve to the observed frequencies of Head Breadth and Height in the case of the 3000 criminals in accordance with Professor Pearson's method (see *Phil. Trans.* Vol. 186, A. pp. 343—414).

(i) Head Breadth.

The moments are:

$$\mu_2 = 25{\cdot}14526 \qquad \beta_1 = {\cdot}010385$$
$$\mu_3 = 12{\cdot}84962 \qquad \beta_2 = 3{\cdot}032609$$
$$\mu_4 = 1917{\cdot}47151.$$

Therefore the criterion $2\beta_2 - 3\beta_1 - 6 = +{\cdot}03406$; the curve is therefore of Type IV., and the remaining constants are:

$$d = -\ {\cdot}253365 \qquad \text{Skewness} = -\ {\cdot}050526$$
$$r = 356{\cdot}203035 \qquad m = 179{\cdot}101518$$
$$\nu = -194{\cdot}9783 \qquad a = 82{\cdot}900557$$
$$\log y_0 = \overline{20}{\cdot}3543484.$$

The equation to the curve is:

$$x = 82{\cdot}900557 \tan\theta,$$
$$\log y = \overline{20}{\cdot}3543484 + 358{\cdot}203035 \log\cos\theta + 1{\cdot}477904\,\theta.$$

The maximum ordinate or mode is 239·39 and occurs at $x = 45{\cdot}124714$, corresponding to a head breadth of 15·0188; the mean or centroid occurs at $x = 45{\cdot}378079$, its value being 15·0442.

The curve obviously approximates very closely to the normal curve.

(ii) Height.

$$\mu_2 = 6{\cdot}45660 \qquad \beta_1 = {\cdot}002578$$
$$\mu_3 = {\cdot}83300 \qquad \beta_2 = 3{\cdot}175076$$
$$\mu_4 = 132{\cdot}36172.$$
$$2\beta_2 - 3\beta_1 - 6 = +{\cdot}342418.$$

The curve is again of Type IV., and the remaining constants are:

$$d = -\ {\cdot}058071 \qquad \text{Skewness} = -\ {\cdot}022853$$
$$r = 38{\cdot}067473 \qquad m = 20{\cdot}033736$$
$$\nu = -2{\cdot}870793 \qquad a = 15{\cdot}42649$$
$$y_0 = 433{\cdot}647.$$

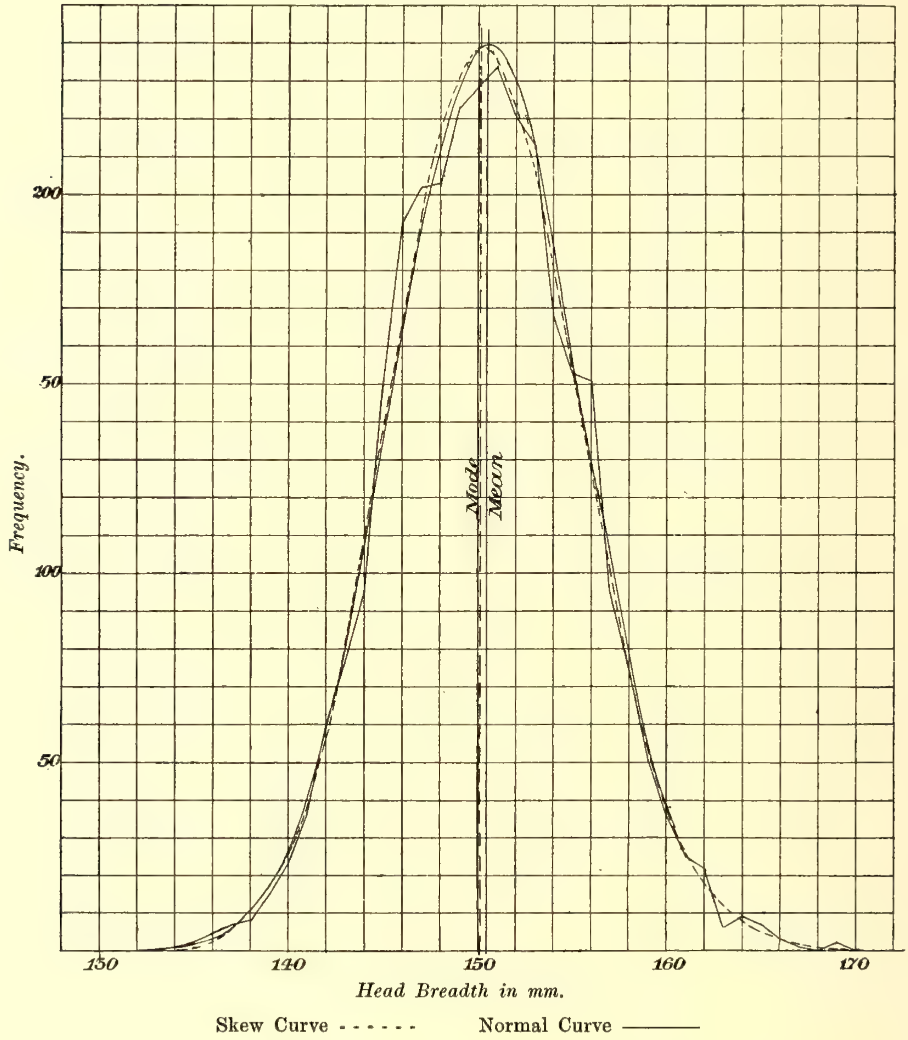
The equation to the curve is:

$$x = 15{\cdot}42649 \tan\theta,$$
$$y = 433{\cdot}647 (\cos\theta)^{40{\cdot}067473}\, e^{2{\cdot}870793\,\theta}.$$

The mode is 480·574 and occurs at $x = 1{\cdot}105291$, corresponding to a height of 65″·4774; the mean occurs at $x = 1{\cdot}163362$, its value being 65″·5355.

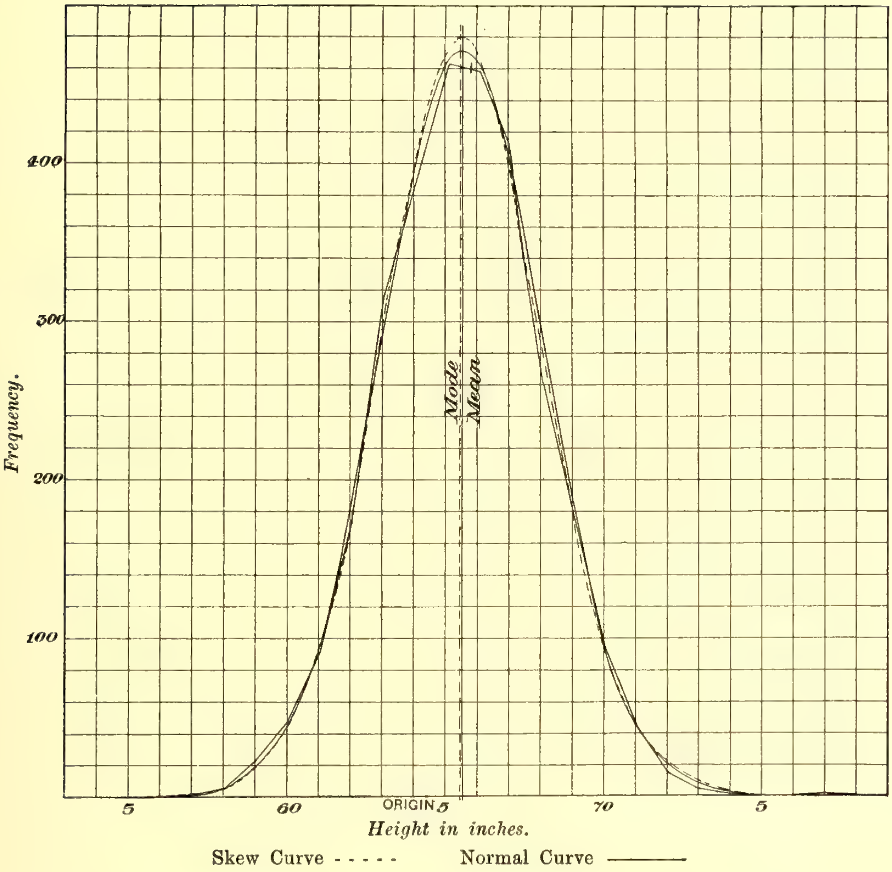
This curve also is very nearly normal. Both curves are drawn on Figs. 3 and 4, which bring out clearly the closeness of the two curves to each other.

FIG. 3. Head Breadth in Criminals.



Seeing that the regression lines are closely linear, and the frequency curves closely normal, we may assert that the distribution is at any rate very approximately normal. This is an important result, as it gives us confidence in applying Professor Pearson's abbreviated method, based as it is on the assumption of normal frequency.

FIG. 4. Height of Criminals.



(6) *Tests of Homogeneity.* In order to see whether or not we are dealing with a homogeneous class, three frequency correlation tables (Tables IV. V. and VI.; see pp. 217—219) were formed from the first batch of forms taken at random from Scotland Yard, and comprising particulars of 1306 out of our 3000 criminals. These tables give the following results, the method of calculation being the same as before:

TABLE 3.

1306 *Criminals.*

	Standard Deviation	Mean
Head Length (cm.) ...	·6002 ± ·0079	19·1438 ± ·0112
Head Breadth (cm.) ...	·5133 ± ·0068	15·0302 ± ·0096
L. M. Finger (cm.) ...	·5451 ± ·0072	11·5244 ± ·0102
Height (ins.)	2·5906 ± ·0342	65·4193 ± ·0407
Face Breadth (cm.) ...	·5022 ± ·0066	13·6350 ± ·0094

TABLE 4.

1306 *Criminals.*

	Coefficient of Correlation
Head Length and Head Breadth	·3958 ± ·0157
Head Breadth and Height ...	·1973 ± ·0179
L. M. Finger and Height ...	·6682 ± ·0103

To compare the results thus obtained for the two sets of 3000 and 1306, we will compare the absolute difference of their standard deviations, means, and coefficients of correlation with the square root of the sums of the squares of the probable errors. Let us call the difference D, the square root in question S, the coefficient of correlation r; and to show the form of the calculation we will take the standard deviation of Head Length as an example. For the sake of keeping as many significant figures as possible, we will use the millimetre as our unit.

$$\begin{array}{lr} \text{S.D. of 1306 criminals is} & 6{\cdot}00247 \pm {\cdot}07922 \\ \quad ,, \quad 3000 \quad ,, & 6{\cdot}04593 \pm {\cdot}05265 \\ \hline D = & {\cdot}04346 \\ \{({\cdot}07922)^2 + ({\cdot}05265)^2\}^{\frac{1}{2}} = S = & {\cdot}09512 \end{array}$$

Thus it appears that D is less than one-half of S.

Collecting all our results together, we have the following tables:

TABLE 5.

1306 *and* 3000 *Criminals.*

	Standard Deviation		Mean	
	D	*S*	*D*	*S*
Head Length... ...	·04346	·09512	·22535	·13451
Head Breadth ...	·11840	·08059	·13922	·11397
Height...	·04963	·04072	·11618	·05759
L. M. Finger	·02758	·08632	·22941	·12208

TABLE 6.

1306 and 3000 Criminals.

	Coefficient of Correlation	
	D	*S*
Head Length and Head Breadth	·00586	·01884
Head Breadth and Height ...	·01420	·02154
Finger and Height	·00735	·01245

From these figures it is apparent that there is no sensible difference between the 1306 and the 3000; *D* is not in any case, *even in the means,* twice to thrice *S*. We may therefore treat our material for practical purposes as *normal* and *homogeneous.*

Before leaving this group of 1306, I may add that having formed correlation tables for Head Length and Left Middle Finger, Head Length and Face Breadth, I find the coefficients of correlation to be as follows:

TABLE 7.

1306 Criminals.

	Coefficient of Correlation
Head Length and Finger ...	·2861 ± ·0171
Head Length and Face Breadth	·4074 ± ·0156

(7) *Comparison of classes from which criminals are drawn with the educated classes of the community.* It will now be interesting to compare our 3000 criminals with an entirely different social class of the population, and for this purpose I have prepared frequency correlation Tables VII., VIII. and IX. showing the correlation between Head Length and Head Breadth, Height and Head Breadth, Height and Head Length, in the case of 1000 male students at Cambridge. The figures are taken from cards in the possession of the Cambridge Anthropometrical Committee*, and are given in inches, but for Head Length and Head Breadth I have reduced the results to centimetres in order to facilitate comparison.

---

* Thanks are due to the Committee for their courtesy in allowing the use of their cards.

TABLE 8.

1000 Cambridge Men.

	S. D.	Mean
Head Length (cms.) ...	·6161 ± ·0093	19·3509 ± ·0131
Head Breadth (cms.)...	·5055 ± ·0076	15·3959 ± ·0108
Height (ins.)	2·5447 ± ·0384	68·8550 ± ·0543

TABLE 9.

1000 Cambridge Men.

	Coefficient of Correlation
Head Length and Head Breadth ...	·3448 ± ·0188
Height and Head Breadth	·1529 ± ·0208
Height and Head Length	·2816 ± ·0196

We will now calculate D and S for the 3000 criminals and 1000 Cambridge men:

TABLE 10.

1000 Cambridge Men and 3000 Criminals.

	S. D.		Mean	
	D	S	D	S
Head Length ...	·0115	·0107	·1846	·0151
Head Breadth ...	·0041	·0088	·3518	·0124
Height	·0037	·0443	3·3195	·0626

TABLE 11.

1000 Cambridge Men and 3000 Criminals.

	Coefficient of Correlation	
	D	S
Head Length and Head Breadth ...	·0568	·0214
Height and Head Breadth	·0301	·0240

We will next give the coefficient of variation, which is defined as the percentage variation in the mean, the standard deviation being treated as the total variation in the mean (*Phil. Trans.* Vol. 187, A., p. 277).

TABLE 12.

Coefficients of Variation. 1000 *Cambridge Men and* 3000 *Criminals.*

	1000 Cambridge Men	3000 Criminals
Head Length ...	3·1839 ± ·0481	3·1544 ± ·0275
Head Breadth ...	3·2836 ± ·0496	3·3332 ± ·0291
Height	3·6958 ± ·0558	3·8773 ± ·0338

The probable error of the coefficient of variation, v, is calculated from the formula:

$$\text{probable error} = \cdot 6745 \frac{v}{\sqrt{2n}} \left[1 + 2\left(\frac{v}{100}\right)^2\right]^{\frac{1}{2}},$$

where n is the number of measurements. (See Pearson, *Proceedings of the Royal Society*, Vol. 61, p. 345.)

From an examination of these results it appears that there is but little difference in variability or correlation between the criminal and the educated classes, but a most noteworthy difference in *means*. It is also to be noted that there is practically no difference in variability whether measured absolutely or by coefficients of variation.

Finally, we will compare the cephalic index in Cambridge men and criminals. In calculating the index it is to be noted (i) that 11 millimetres are deducted from the head measurements, in accordance with Dr Alice Lee's memoir "A First Study of the Correlation of the Human Skull" (*Phil. Trans.* Vol. 196, A., p. 252); and (ii) that we know only the mean Head Breadth and mean Head Length, and therefore in calculating the mean of the Cephalic Index, which is 100 times the ratio of Head Breadth and Head Length, we must apply the formulæ given in Professor Pearson's paper in the *Proceedings of the Royal Society*, Vol. 60, p. 492. He there shows that if x_1, x_2, be the absolute sizes of any two correlated organs, m_1, m_2, their means, σ_1, σ_2, their standard deviations, r_{12} their coefficient of correlation, i_{12} the mean value of $\frac{x_1}{x_2}$, Σ_{12} the standard deviation of $\frac{x_1}{x_2}$, then

$$i_{12} = \frac{m_1}{m_2}(1 + v_2^2 - r_{12}v_1v_2),$$

$$\Sigma_{12} = i_{12}(v_1^2 + v_2^2 - 2r_{12}v_1v_2)^{\frac{1}{2}},$$

where

$$v_1 = \sigma_1/m_1, \quad v_2 = \sigma_2/m_2.$$

Applying these formulæ we find:

TABLE 13.

Cephalic Index. 1000 *Cambridge Men and* 3000 *Criminals.*

	S. D.		Mean	
	Head Index	Skull Index	Head Index	Skull Index
1000 Cambridge Men ...	2·9478	2·9021	79·562	78·330
3000 Criminals	2·7900	2·7435	78·538	77·227

The great difference in cephalic index between the two classes is to be noted, and it is interesting to observe that there is a large difference in the skull capacity as measured by Dr Lee's formula (*Phil. Trans.* Vol. 196, A., p. 235),

$$C = 6{\cdot}752\,L + 11{\cdot}421\,B - 1434{\cdot}06,$$

where C = capacity in cubic centims, L and B the length and breadth of skull in millims.

For Criminals $L = 191{\cdot}663 - 11 = 180{\cdot}663$

$B = 150{\cdot}442 - 11 = 139{\cdot}442.$

For Cambridge Men $L = 193{\cdot}509 - 11 = 182{\cdot}509$

$B = 153{\cdot}959 - 11 = 142{\cdot}959.$

Calculating C by the formula, we find it is

For Criminals	1378·34
For Cambridge Men	1430·98

but in the absence of the measurement of the height of the skull, we do not lay much stress on these determinations.

Summing up the results of this part of the inquiry, I conclude that there is a substantial difference in stature, and in size and shape of head between the two classes; I do not assert that the source of the criminality is to be found in this difference, but only that criminals are drawn from a different section of the community. As bearing on this point it is worth noting that the mean height in Galton's middle-class measurements at the International Exhibition of 1884, viz. 67″·9, approaches our criminal mean more closely than does the Cambridge mean.

I should add that I believe the head measurements in the prisons and at Cambridge are made practically in the same way.

For further comparison I give the general position as to stature and cephalic index of our criminal classes.

TABLE 14.

Stature (Men).

	Number	Mean, cms.	Standard Deviation, cms.	Coefficient of Variation
Cambridge Students...	1000	174·88	6·46	3·70
English Sons* ...	1078	174·40	6·94	3·98
English Fathers* ...	1078	171·95	6·87	3·99
U.S.A. Recruits† ...	25878	170·94	6·56	3·84
N.S. Wales Criminals‡	2862	169·88	6·58	3·80
Frenchmen§	284	166·80	6·47	3·88
English Criminals ...	3000	166·46	6·45	3·88
Germans§	390	165·93	6·68	4·02

* Calculated from family measurement data, collected by Professor Pearson.

† Pearson: *Phil. Trans.*, Vol. 186, A., p. 386.

‡ Powys: *Biometrika*, Part I. Vol. I. p. 44.

§ Pearson: *The Chances of Death*, Vol. I. p. 295.

TABLE 15.

Cephalic Index.

	Number	Mean	Standard Deviation
Bavarian Peasants* ...	100	83·41	3·58
Baden Recruits* ...	6748	81·15	3·63
French Peasants* ...	56	79·79	3·84
Cambridge Students ...	1000	78·33	2·90
Criminals	3000	77·23	2·74
Brahmans of Bengal* ...	100	76·86	3·65
Mahomedans of Bengal*	100	75·77	3·37
Whitechapel English* ...	107	74·73	3·31

* Professor Pearson's *The Chances of Death*, Vol. I. p. 290. As his results are based either on skull measurements or on "corrected" head measurements, I have used the "corrected" figures of Table 13.

PART II.

APPLICATION OF METHODS.

(8) I will now proceed to apply the abbreviated method already described to the calculation of coefficients of correlation, standard deviations and means, and I will begin with Height and Head Breadth.

First, the following 16-fold table was formed (see Table II., p. 215).

3000 *Criminals.*

Height (feet and inches).

Head Breadth (cms.)	$5{\cdot}4\frac{9}{16}$ and under	$5{\cdot}4\frac{9}{16}$—$5{\cdot}5\frac{9}{16}$	$5{\cdot}5\frac{9}{16}$—$5{\cdot}6\frac{9}{16}$	over $5{\cdot}6\frac{9}{16}$	Totals
14·8 and under	455	163	160	299	1077
14·9—15 ...	158	71	71	151	451
15·1—15·2 ...	158	70	65	162	455
over *15·2* ...	283	158	162	414	1017
Totals	1054	462	458	1026	3000

Forming from this a 4-fold table on the principle stated in § 3, we have:—

3000 *Criminals.*

Height (feet and inches).

Head Breadth (cms.)	$5{\cdot}5\frac{9}{16}$ and under	over $5{\cdot}5\frac{9}{16}$	Totals
15 and under	847	681	1528
over *15* ...	669	803	1472
Totals	1516	1484	3000

Here $\alpha_1 = {\cdot}010667$ $\alpha_2 = {\cdot}018667$.

Hence from the probability integral tables:

$$h = {\cdot}013366 \qquad k = {\cdot}023400$$

$$\log HK = \bar{1}{\cdot}2016623, \qquad \epsilon = {\cdot}156822,$$

and the equation in θ, where r, the coefficient of correlation, $= \sin\theta$, is:

$${\cdot}156822 = \theta + \tfrac{1}{2}({\cdot}000313)\,\theta^2 - ({\cdot}000727)\,\frac{\theta^3}{6} + ({\cdot}000313)\,\frac{5\theta^4}{24}.$$

$\theta = {\cdot}156822 = 8^\circ\,59'\,7''$ gives a very close solution, and therefore $r = {\cdot}1562$, the probable error being $\pm\,{\cdot}0190$.

On comparing this with the result obtained by the ordinary method, viz. $r = \cdot 1831 \pm \cdot 0119$, we conclude that we might improve our result by taking other limits of division; we therefore re-form our table as follows:

Height (feet and inches).

Head Breadth (cms.)	*5·7*$\frac{9}{16}$ and under	over *5·7*$\frac{9}{16}$	Totals
15·3 and under	1799	397	2196
over *15·3* ...	587	217	804
Totals	2386	614	3000

Here $\alpha_1 = \cdot 590667$ $\alpha_2 = \cdot 464000$

$h = \cdot 82507$ $k = \cdot 61889$

$\log HK = \bar{2}\cdot 9708265$ $\epsilon = \cdot 186974$

and the equation in θ is:

$$\cdot 186974 = \theta + \cdot 255314\theta^2 - \cdot 133837\theta^3 + \cdot 044029\theta^4.$$

Approximating to the root by Newton's rule, we find:

$$\theta = \cdot 1794814 = 10^\circ\, 17'\, 1'' \text{ and } r = \cdot 1785,$$

a better result than before, but we will try yet another division, viz.

TABLE X.

Height (feet and inches).

Head Breadth (cms.)	*5·4*$\frac{9}{16}$ and under	over *5·4*$\frac{9}{16}$	Totals
14·8 and under	455	622	1077
over *14·8* ...	599	1324	1923
Totals	1054	1946	3000

Here $\alpha_1 = - \cdot 297333$ $\alpha_2 = - \cdot 282000$

$h = - \cdot 38173$ $k = - \cdot 36114$

$\log HK = \bar{1}\cdot 1420742$ $\epsilon = \cdot 184125.$

The equation in θ now becomes:

$$\cdot 184125 = \theta + \cdot 068929\theta^2 - \cdot 042689\theta^3 + \cdot 024088\theta^4,$$

a solution of which is $\theta = \cdot 1820710 = 10^\circ\, 25'\, 55''$,

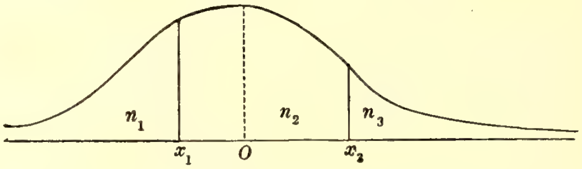
and $r = \cdot 1811$

with a probable error of ± ·0210.

As this result coincides very closely with that obtained by the usual method, we will rest satisfied with it, and in subsequent tables adopt the divisions of

Height and Head Breadth on which it is based. Comparing it with the worst value obtained above, ·1562, we see that even in that case the difference is only of the order ·02, or of the probable error of sampling.

(9) I have next to find the standard deviations and means of the two characters; this I shall do by applying the method explained in Professor Pearson and Dr Lee's memoir "On the inheritance of characters not capable of exact quantitative measurement" (*Phil. Trans.* Vol. 195, A., p. 82). Let us suppose our 16-fold table reduced to a 9-fold table, and let n_1, n_2, n_3, the three classes of one character which are thus formed, be represented by the areas of the normal curve in the accompanying diagram.



In all our investigations, the mean will fall within the area n_2. Let $Ox_1 = p_1$, $Ox_3 = p_3$, be the distances of the mean from the boundaries of n_2; then $p_1 + p_3$ in absolute measurement is known, $= \eta$, say, being the range of the class n_2.

Remembering that the equation to the normal curve is of the form

$$y = \frac{N}{\sigma\sqrt{2\pi}} e^{-\frac{1}{2}\frac{x^2}{\sigma^2}},$$

where N is the total frequency and σ the S.D., we have

$$\frac{n_2 + n_3 - n_1}{N} = \sqrt{\frac{2}{\pi}} \int_0^{h_1} e^{-\frac{1}{2}x^2}\, dx,$$

$$\frac{n_1 + n_2 - n_3}{N} = \sqrt{\frac{2}{\pi}} \int_0^{h_3} e^{-\frac{1}{2}x^2}\, dx,$$

where $$h_1 = \frac{p_1}{\sigma}, \quad h_3 = \frac{p_3}{\sigma}.$$

Now the left-hand side is known, and from a table of the areas of the normal curve h_1 and h_3 are found. Therefore as $\sigma = \frac{p_1 + p_3}{h_1 + h_3} = \frac{\eta}{h_1 + h_3}$, the standard deviation is known. Also, as $p_1 = h_1\sigma$, the distance of the mean from the left-hand boundary of n_2, and therefore the mean itself, are known.

To find the probable error of σ, we have

$$\sigma = \frac{\eta}{h_1 + h_3}, \quad \therefore \delta\sigma = -\frac{\eta(\delta h_1 + \delta h_3)}{(h_1 + h_3)^2}.$$

$$\therefore \Sigma^2_\sigma = \frac{\eta^2}{(h_1 + h_3)^4} (\Sigma^2_{h_1} + \Sigma^2_{h_3} + 2\Sigma_{h_1}\Sigma_{h_3}R_{h_1h_3}).$$

Now $$\frac{N-2n_1}{2N}=\frac{1}{\sqrt{2\pi}}\int_0^{h_1}e^{-\frac{1}{2}x^2}\,dx;$$

therefore, if $$H=\frac{1}{\sqrt{2\pi}}e^{-\frac{1}{2}h^2},$$

we have $$-\delta n_1=NH_1\delta h_1 \text{ and } \Sigma_{h_1}=\Sigma_{n_1}/(NH_1).$$

Similarly $$-\delta n_3=NH_3\delta h_3 \text{ and } \Sigma_{h_3}=\Sigma_{n_3}/(NH_3).$$

Also $\Sigma_{h_1}\Sigma_{h_3}R_{h_1h_3}=\Sigma_{n_1}\Sigma_{n_3}R_{n_1n_3}/N^2H_1H_3$, and it is shown in Professor Pearson's memoir "On the correlation of characters not quantitatively measurable" (*Phil. Trans.* Vol. 195, A., p. 11), that

$$\Sigma^2_{n_1}=\frac{n_1(N-n_1)}{N},\qquad \Sigma^2_{n_3}=\frac{n_3(N-n_3)}{N},$$

$$\Sigma_{n_1}\Sigma_{n_3}R_{n_1n_3}=-\frac{n_1n_3}{N},$$

therefore $$\Sigma^2_{h_1}=\frac{n_1(N-n_1)}{N^3H^2_1},\qquad \Sigma^2_{h_3}=\frac{n_3(N-n_3)}{N^3H^2_3},$$

$$2\Sigma_{h_1}\Sigma_{h_3}R_{h_1h_3}=-\frac{2n_1n_3}{N^3H_1H_3}.$$

The probable error of σ is accordingly $\cdot 67449\Sigma_\sigma$,

$$=\cdot 67449\frac{\eta}{(h_1+h_3)^2}\left\{\frac{n_1(N-n_1)}{N^3H^2_1}+\frac{n_3(N-n_3)}{N^3H^2_3}-\frac{2n_1n_3}{N^3H_1H_3}\right\}^{\frac{1}{2}}.$$

We have next to find the probable error in p_1, that is, the probable error of the mean.

$$p_1=h_1\sigma,$$

$$\delta p_1=h_1\delta\sigma+\sigma\delta h_1.$$

Therefore $$\Sigma^2_{p_1}=h^2_1\Sigma^2_\sigma+\sigma^2\Sigma^2_{h_1}+2h_1\sigma\Sigma_\sigma\Sigma_{h_1}R_{\sigma h_1};$$

also $$h_1+h_3=\frac{\eta}{\sigma},$$

$$\delta h_3=-\delta h_1-\eta\frac{\delta\sigma}{\sigma^2},$$

$$\Sigma^2_{h_3}=\Sigma^2_{h_1}+\frac{\eta^2}{\sigma^4}\Sigma^2_\sigma+\frac{2\eta}{\sigma^2}\Sigma_\sigma\Sigma_{h_1}R_{\sigma h_1}.$$

Substituting for $\Sigma_\sigma\Sigma_{h_1}R_{\sigma h_1}$ from the first equation, and remembering that $\eta=\sigma(h_1+h_3)$, we have finally, after a few reductions,

$$\Sigma^2_{p_1}=\frac{\sigma^2(h_1\Sigma^2_{h_3}+h_3\Sigma^2_{h_1})}{h_1+h_3}-\Sigma^2_\sigma h_1h_3,$$

and the probable error $=\cdot 67449\Sigma_{p_1}$.

(10) We will now apply these formulæ.

Height and Head Breadth. We first form the 9-fold Table XI. (p. 222).

Taking Height first, we have:

$$n_1 = 1054, \quad n_2 = 920, \quad n_3 = 1026.$$
$$h_1 = \cdot 38173, \quad h_3 = \cdot 40702, \quad \eta = 2.$$

Therefore
$$\sigma = \frac{2}{\cdot 78875} = 2\cdot 53566 \text{ inches},$$
$$p_1 = \sigma \times h_1 = \cdot 967937.$$

The mean is therefore
$$64\tfrac{9}{16} + \cdot 967937 = 65\cdot 5304 \text{ inches}.$$

For Head Breadth
$$n_1 = 1077, \quad n_2 = 906, \quad n_3 = 1017,$$
$$h_1 = \cdot 36114, \quad h_3 = \cdot 415198, \quad \eta = \cdot 4,$$
$$\sigma = \frac{\cdot 4}{\cdot 77634} = \cdot 515238 \text{ (cm.)},$$
$$p_1 = \sigma \times h_1 = \cdot 186073.$$

In finding the mean it is to be noted that the lower boundary of n_2 is 14·85 as the measurements are taken to the nearest millimetre; the mean is therefore

$$14\cdot 85 + \cdot 186073 = 15\cdot 0361 \text{ cm}.$$

The probable errors of the S.D. are

Height	± ·0495
Head Breadth	± ·01015.

Height and Left Middle Finger. Our 4-fold table is Table XII., p. 222 (see also Table III., p. 216).

$$h = -\cdot 38173, \quad k = \cdot 01169, \quad \epsilon = \cdot 714334.$$

The equation in θ is:

$$\cdot 714334 = \theta - \cdot 002232\theta^2 - \cdot 024306\theta^3 - \cdot 000849\theta^4 - \cdot 000919\theta^5,$$

whence
$$\theta = \cdot 7251964, \quad r = \cdot 6633.$$

The 9-fold table is Table XIII., p. 222 (see also Table III., p. 216).

For Finger
$$n_1 = 706, \quad n_2 = 1612, \quad n_3 = 682,$$
$$h_1 = \cdot 7214, \quad h_3 = \cdot 74769, \quad \eta = \cdot 8,$$
$$\sigma = \frac{\cdot 8}{1\cdot 46909} = \cdot 544555,$$
$$p_1 = h_1\sigma = \cdot 392842.$$

Therefore the mean is

$$11{\cdot}15 + {\cdot}392842 = 11{\cdot}542842,$$

and the probable error of $\sigma = {\cdot}0075$.

Head Length and Head Breadth. The 4-fold table is Table XIV., p. 222 (see also Table I., p. 214).

$$h = -{\cdot}03761\,;\ k = -{\cdot}36114\,;\ \epsilon = {\cdot}408616.$$

$${\cdot}408619 = \theta + {\cdot}006791\theta^2 - {\cdot}021942\theta^3 + {\cdot}002606\theta^4 - {\cdot}008325\theta^5,$$

whence $\theta = {\cdot}4090070, \quad r = {\cdot}3977.$

For S.D. of Head Length, we have from the 9-fold Table XV., p. 222,

$$n_1 = 463, \quad n_2 = 2043, \quad n_3 = 494,$$

$$h_1 = 1{\cdot}018019, \quad h_3 = {\cdot}975451, \quad \eta = 1{\cdot}2.$$

$$\sigma = \frac{1{\cdot}2}{1{\cdot}99347} = {\cdot}6019654,$$

and $p_1 = h_1\sigma = {\cdot}6128122,$

therefore $\text{Mean} = 18{\cdot}55 + {\cdot}6128122 = 19{\cdot}1628122,$

and the probable error of σ is ·0071.

(11) *Comparison of Results.* We can now compare the results obtained by the new method with those obtained from the full correlation tables:

TABLE 16.

Coefficient of Correlation.

	Old Method	New Method
Head Length and Head Breadth	·4016 ± ·0103	·3977 ± .0176
Head Breadth and Height ...	·1831 ± ·0119	·1811 ± ·0210
L. M. Finger and Height ...	·6608 ± ·0069	·6633 ± .0142

TABLE 17.

S.D. and Mean.

	Standard Deviation		Mean	
	Old Method	New Method	Old Method	New Method
Head Length (cm.)	·6046 ± ·0053	·6020 ± ·0071	19·1663 ± ·0075	19·1628 ± ·0086
Head Breadth (cm.)	·5014 ± ·0044	·5152 ± ·0101	15·0442 ± ·0062	15·0361 ± ·0072
Finger (cm.) ...	·5479 ± ·0048	·5446 ± ·0075	11·5474 ± ·0068	11·5428 ± ·0075
Height (ins.) ...	2·5410 ± ·0221	2·5357 ± ·0495	65·5355 ± ·0313	65·5304 ± ·0352

The extremely close agreement of the results thus obtained in two very different ways, especially the agreement in the means, is deserving of special attention and is a striking illustration of the value of the new method.

(12) *Application of Methods, continued.* I will now proceed to calculate the remaining standard deviations, means, and coefficients of correlation.

Head Length and Face Breadth. See Tables XVI. and XVII., p. 223.

$$h = -\cdot 03761\,; \quad k = 0\,; \quad \epsilon = \cdot 405552,$$

$$\cdot 405552 = \theta - \cdot 001415\,\frac{\theta^3}{6}\,;$$

whence $\theta = \cdot 4055676$ and $r = \cdot 3945 \pm \cdot 0172$.

For S.D. and Mean of Face Breadth:

$$n_1 = 1039, \quad n_2 = 922, \quad n_3 = 1039, \quad h_1 = h_3 = \cdot 39524\,;$$

therefore $\sigma = \cdot 5060216 \pm \cdot 009856$.

$$p_1 = h_1\sigma = \cdot 2000, \text{ therefore Mean} = 13\cdot 45 + \cdot 2 = 13\cdot 65.$$

Head Length and Cubit. Tables XVIII. and XIX., p. 223.

$$h = -\cdot 03761, \quad k = -\cdot 067734, \quad \epsilon = \cdot 310452,$$

$$\cdot 310452 = \theta + \cdot 001274\theta^2 - \cdot 001\theta^3 + \cdot 000529\theta^4\,;$$

whence $\theta = \cdot 3103547\,; \quad r = \cdot 3054$.

For S.D. and Mean of Cubit:

$$n_1 = 911, \quad n_2 = 1002, \quad n_3 = 1087, \quad h_1 = \cdot 51389, \quad h_3 = \cdot 35224,$$

whence $\sigma = 1\cdot 96275 \pm \cdot 0171$.

Also $p_1 = h_1\sigma = 1\cdot 00864$; therefore Mean $= 44\cdot 05 + p_1 = 45\cdot 05864$.

Head Length and Foot. Tables XX. and XXI., pp. 223, 224.

$$h = -\cdot 03761, \quad k = -\cdot 10295, \quad \epsilon = \cdot 345875,$$

$$\cdot 345875 = \theta + \cdot 001936\theta^2 - \cdot 002\theta^3 + \cdot 000801\theta^4\,;$$

whence $\theta = \cdot 3457144, \quad r = \cdot 3389$.

For S.D. and Mean of Foot:

$$n_1 = 883, \quad n_2 = 979, \quad n_3 = 1138, \quad h_1 = \cdot 540778, \quad h_3 = \cdot 307234,$$

whence $\sigma = 1\cdot 1792286 \pm \cdot 0103$.

Also $p_1 = h_1\sigma = \cdot 6377009$; therefore Mean $= 25\cdot 05 + p_1 = 25\cdot 6877$.

The divisions chosen for Foot are not quite satisfactory, the class "over 26" being too large.

Having now found the S.D. and Mean of all seven characters, we no longer require 9-fold tables, and we shall therefore now use only 4-fold tables in order to find r.

Head Length and Finger. Table XXII., p. 224.

$$h_1 = -\cdot 03761, \quad k = \cdot 01169, \quad \epsilon = \cdot 305411,$$

$$\cdot 305411 = \theta - \cdot 000220\theta^2 - \cdot 000259\theta^3 - \cdot 000091\theta^4;$$

whence $$\theta = \cdot 3054401, \qquad r = \cdot 3007 \pm \cdot 0181.$$

Head Length and Height. Table XXIII., p. 224.

$$h = -\cdot 38173, \quad k = -\cdot 03761, \quad \epsilon = \cdot 34673,$$

$$\cdot 34673 = \theta + \cdot 007178\theta^2 - \cdot 024479\theta^3 + \cdot 002727\theta^4;$$

whence $$\theta = \cdot 3468384, \qquad r = \cdot 3399 \pm \cdot 0207.$$

Head Breadth and Face Breadth. Table XXIV., p. 224.

$$h = -\cdot 36114, \quad k = 0, \quad \epsilon = \cdot 660597,$$

$$\cdot 660597 = \theta - \cdot 021737\theta^3 + \cdot 008269\theta^5;$$

whence $$\theta = \cdot 6659329, \qquad r = \cdot 6178.$$

If we calculate r from a division nearly median (Table XXIV.^{a}, p. 224) we have:

$$h = \cdot 0234, \quad k = 0, \quad \epsilon = \cdot 655725,$$

$$\cdot 655725 = \theta - \cdot 0000913\theta^3 - \cdot 0000365\theta^5;$$

whence $$\theta = \cdot 655755, \qquad r = \cdot 6098.$$

Head Breadth and Finger. Table XXV., p. 225.

$$h = -\cdot 36114, \quad k = \cdot 01169, \quad \epsilon = \cdot 15085,$$

$$\cdot 15085 = \theta - \cdot 002111\theta^2 - \cdot 021757\theta^3 - \cdot 000811\theta^4;$$

whence $$\theta = \cdot 1509732, \qquad r = \cdot 1504.$$

If we adopt a median division (Table XXV.^{a}, p. 225) we shall find $r = \cdot 1459$.

Head Breadth and Cubit. Table XXVI., p. 225.

$$h = -\cdot 36114, \quad k = -\cdot 06773, \quad \epsilon = \cdot 135737,$$

$$\cdot 135737 = \theta + \cdot 012230\theta^2 - \cdot 022402\theta^3 + \cdot 004684\theta^4;$$

whence $$\theta = \cdot 1355670, \quad r = \cdot 1352 \pm \cdot 0196.$$

Head Breadth and Foot. Table XXVII., p. 225.

$$h = -\cdot 10295, \quad k = -\cdot 36114, \quad \epsilon = \cdot 208238,$$

$$\cdot 208238 = \theta + \cdot 01859\theta^2 - \cdot 023273\theta^3 + \cdot 007092\theta^4;$$

whence $$\theta = \cdot 2076318, \qquad r = \cdot 2061.$$

Face Breadth and Finger. Table XXVIII., p. 225.

$$h = 0, \quad k = \cdot 01169, \quad \epsilon = \cdot 326748,$$

$$\cdot 326748 = \theta - \cdot 000023\theta^3,$$

whence $$\theta = \cdot 3267488, \qquad r = \cdot 3210.$$

Face Breadth and Cubit. Table XXIX., p. 226.

$$h=0, \quad k=-{\cdot}06773, \quad \epsilon={\cdot}292839,$$

$${\cdot}292839=\theta-{\cdot}0007647\theta^3,$$

whence

$$\theta={\cdot}2928580, \qquad r={\cdot}2887.$$

Face Breadth and Foot. Table XXX., p. 226.

$$h=0, \quad k=-{\cdot}102952, \quad \epsilon={\cdot}371625,$$

$${\cdot}371625=\theta-{\cdot}0017665\theta^3,$$

whence

$$\theta={\cdot}3717160, \qquad r={\cdot}3632.$$

Face Breadth and Height. Table XXXI., p. 226.

$$h=0, \quad k=-{\cdot}38173, \quad \epsilon={\cdot}351419,$$

$${\cdot}351419=\theta-{\cdot}024286\theta^3-{\cdot}009185\theta^5;$$

whence

$$\theta={\cdot}3525333, \qquad r={\cdot}3453.$$

Foot and Finger. Table XXXII., p. 226.

$$h=-{\cdot}10295, \quad k={\cdot}01169, \quad \epsilon={\cdot}859268,$$

$${\cdot}859268=\theta-{\cdot}000602\theta^2-{\cdot}001789\theta^3-{\cdot}000249\theta^4-{\cdot}000713\theta^5,$$

whence

$$\theta={\cdot}8613318, \qquad r={\cdot}7587.$$

Finger and Cubit. Table XXXIII., p. 226.

$$h={\cdot}01169, \quad k=-{\cdot}06773, \quad \epsilon=1{\cdot}007431,$$

$$1{\cdot}007431=\theta-{\cdot}000396\theta^2-{\cdot}000787\theta^3-{\cdot}000164\theta^4-{\cdot}000314\theta^5;$$

whence

$$\theta=1{\cdot}0091435, \qquad r={\cdot}8464\pm{\cdot}0079.$$

As the cubit includes the finger, their correlation may be considered "spurious." Let us define "forearm" as the cubit minus the finger; then we can find S.D. and mean of forearm, and correlation of forearm and finger, as follows:—

Let $a=$ forearm, $c=$ cubit, $f=$ finger.

$$c=a+f \text{ and } cf=af+f^2.$$

Therefore

$$S(cf)=S(af)+S(f^2),$$

or

$$r_{fc}\,\sigma_f\,\sigma_c=r_{af}\,\sigma_a\,\sigma_f+\sigma^2{}_f,$$

therefore

$$r_{af}=\frac{r_{fc}\sigma_c-\sigma_f}{\sigma_a},$$

which gives the correlation of forearm and finger. Substituting the known values of the symbols on the right-hand side we have:

$$r_{af}={\cdot}7297.$$

Also $a = c - f$, therefore $\sigma^2_a = \sigma^2_c + \sigma^2_f - 2\sigma_c\sigma_f r_{cf}$,
and substituting we find $\sigma_a = 1{\cdot}5297$,
and $\text{Mean}_a = \text{Mean}_c - \text{Mean}_f = 33{\cdot}5158$.

Left Foot and Left Cubit. Table XXXIV., p. 227.

$$h = -{\cdot}10295, \quad k = -{\cdot}06773, \quad \epsilon = {\cdot}923660,$$

$${\cdot}923660 = \theta + {\cdot}003486\theta^2 - {\cdot}002523\theta^3 + {\cdot}001439\theta^4 - {\cdot}0009986\theta^5;$$

whence $\theta = {\cdot}9222992$; $r = {\cdot}79699$.

Height and Left Cubit. Table XXXV., p. 227.

$$h = {\cdot}01337, \quad k = -{\cdot}06773, \quad \epsilon = {\cdot}925690,$$

$${\cdot}925690 = \theta - {\cdot}000453\theta^2 - {\cdot}000794\theta^3 - {\cdot}000188\theta^4 - {\cdot}000317\theta^5;$$

whence $\theta = {\cdot}9270676$; $r = {\cdot}7999$.

Here we have adopted the median division. If we work with the skew division, Table XXXV.^{a}, p. 227, we have:

$$h = -{\cdot}38173, \quad k = -{\cdot}06773, \quad \epsilon = {\cdot}910952,$$

$${\cdot}910952 = \theta + {\cdot}012927\theta^2 - {\cdot}024939\theta^3 + {\cdot}004901\theta^4 - {\cdot}009349\theta^5;$$

whence $\theta = {\cdot}9222068$; $r = {\cdot}7969$;

but it will be observed that in the second equation for θ the terms converge much more slowly than in the first; we will therefore retain the first value, especially as the difference between the two is only ·00292.

Height and Left Foot. Table XXXVI., p. 227.

$$h = -{\cdot}013366, \quad k = -{\cdot}102952, \quad \epsilon = {\cdot}82577,$$

$${\cdot}825770 = \theta - {\cdot}000688\theta^2 - {\cdot}001796\theta^3 - {\cdot}000285\theta^4 - {\cdot}0007153\theta^5;$$

whence $\theta = {\cdot}8276709$; $r = {\cdot}7364$.

Here again we have adopted the median division.

Adopting the skew division, Table XXXVI.^{a}, p. 227, we have:

$$h = -{\cdot}38173, \quad k = -{\cdot}10295, \quad \epsilon = {\cdot}881436,$$

$${\cdot}881436 = \theta + {\cdot}01965\theta^2 - {\cdot}025795\theta^3 + {\cdot}007422\theta^4 - {\cdot}009565\theta^5;$$

whence $\theta = {\cdot}8845487$; $r = {\cdot}7736$.

It will be noticed that when r is large, the terms involving the higher powers of θ do not always converge as rapidly as we should like in the equations for θ; the values of θ are therefore not to be relied on beyond the 4th figure or so. To go up to the 6th power of θ would considerably increase the labour of solving the equations; we have therefore not gone beyond the 5th power, the term involving which is:

$$+\left[h^4k^4 + (h^2+k^2)\{3(h^2+k^2) - 6h^2k^2 - 8\} + 20h^2k^2\right]\frac{\theta^5}{120}.$$

(13) For convenience of reference, we will now collect all the standard deviations, means, and coefficients of correlation as ascertained in the course of the preceding inquiry; also the coefficients of variation.

TABLE 18.

3000 Criminals.

	Standard Deviation	Coefficient of Variation	Mean	
Head Length (cms.) ...	·6046	3·1544	19·1663	Table 1
Head Breadth (cms.) ...	·5014	3·3332	15·0442	Table 1
Face Breadth (cms.) ...	·5060	3·7071	13·6500	§ 12
Left Middle Finger (cms.)	·5479	4·7445	11·5474	Table 1
Left Cubit (cms.) ...	1·9627	4·3560	45·0586	§ 12
Left Foot (cms.) ...	1·1792	4·5906	25·6877	§ 12
Height (ins.)	2·5410	3·8773	65·5355	Table 1
„ (cms.)	6·4540		166·4572	

TABLE 19.

Coefficients of Correlation. 3000 *Criminals.*

	Head Length	Head Breadth	Face Breadth	Finger	Cubit	Foot	Height
Head Length	1·	·40163	·39454	·30071	·30539	·33886	·33993
Head Breadth	·40163	1·	·61779	·15040	·13515	·20614	·18308
Face Breadth	·39454	·61779	1·	·32097	·28869	·36322	·34527
Finger ...	·30071	·15040	·32097	1·	·84638	·75871	·66084
Cubit ...	·30539	·13515	·28869	·84638	1·	·79699	·79986
Foot	·33886	·20614	·36322	·75871	·79699	1·	·73636
Height ...	·33993	·18308	·34527	·66084	·79986	·73636	1·

(14) It now becomes necessary to test the probable errors of some of these results.

TABLE 20.

Probable Errors of the Means in Criminals.

	By Full Correlation Table of 3000	By Full Correlation Table of 1306	By New Method	$h_1 + h_3$
Head Length ...	·0075	·0112	·0086	1·99347
Head Breadth ...	·0062	·0096	·0072	·77634
Face Breadth ...	—	·0094	·0070	·79048
Finger	·0067	·0102	·0075	1·46909
Height	·0313	·0484	·0352	·78875

TABLE 21.

Probable Errors of S.D. in Criminals.

	By Full Correlation Table, 3000	By Full Correlation Table, 1306	By New Method, 3000	$h_1 + h_3$
Head Length (cms.)	·0053	·0079	·0071	1·99347
Head Breadth (cms.)	·0044	·0068	·0101	·77634
Face Breadth (cms.)	—	·0066	·0099	·79048
Finger (cms.) ...	·0048	·0072	·0075	1·46909
Height (ins.) ...	·0221	·0342	·0495	·78875

TABLE 22.

Probable Errors of r in Criminals.

	By New Method	By Formula $\frac{\cdot 6745\,(1-r^2)}{\sqrt{1100}}$
Height and Head Breadth ($r = \cdot 1831$)	·0210	·0197
Head Length and Face Breadth ($r = \cdot 3945$)	·0172	·0172
Head Length and Finger ($r = \cdot 3007$)	·0181	·0185
Head Length and Height ($r = \cdot 3399$)	·0207	·0180
Head Breadth and Cubit ($r = \cdot 1352$)	·0196	·0200
Finger and Cubit ($r = \cdot 8464$)	·0079	·0058

It will be noted from the results for Head Length and Finger that the Probable Errors of the standard deviations by the new method for 3000 are very nearly equal to those by the usual method for 1306 when $h_1 + h_3$ is large (see § 9), that is, when we arrange our 9-fold table so that the middle division is large. A reference to § 11 will also show that by arranging our tables in this way we obtained remarkably good results for S.D. and mean of Head Length and Finger; we might therefore expect improved results if we were to arrange all our 9-fold tables throughout on this principle.

It is to be noted that the Probable Errors of the means by the new method are practically as small as those of the standard deviations.

Owing to the laborious character of the calculation for finding the probable error of r by the new method, I have worked out only eight of the 21 errors (see Tables 16 and 22), but these eight being fairly representative will give a good idea of the magnitudes involved.

In the 2nd column of Table 22, the probable error is calculated by the usual formula for normal frequency, supposing $n = 1100$, and on the whole the results correspond very closely with those obtained by the new method, except in the case of the last coefficient, where the difference is considerable, but as it happens the absolute amount of error is small in either case. We may therefore conclude that by applying the new method to 3000 cases we shall obtain the coefficients of correlation to the same degree of accuracy as if we calculated them from a full correlation table of 1100 cases.

As a result of the preceding investigation I conclude that the new method (i) saves a great deal of time and labour; (ii) gives excellent results for standard deviation and mean, especially if the middle division of the 9-fold table is made comparatively large; and (iii) gives less good results for r when r is high, owing to the slowness of convergence of the terms in the equation for θ, but on the whole it enables us to calculate r with an accuracy fairly comparable to that attained from an ordinary frequency table containing about one-third the number of frequencies.

(15) I will now state some results, based on Tables 18 and 19, which I think are sufficiently novel and important to be emphasised. I note that the determinations of Left Foot, so far as I can ascertain, are the first that have been made, and have therefore the interest of novelty.

The following table shows how the mean head measurements of criminals compare with those of the British Association and the University College series which are given by Dr Alice Lee on page 251 of her memoir, "A first study of the Correlation of the Human Skull" (*Phil. Trans.*, Vol. 196); the B.A. measurements are averages obtained by Dr Lee from the values given for several years in the "Reports of the B.A. Anthropometric Committee"; the University College measurements were taken by Professor Karl Pearson on twenty-five members of the College staff.

TABLE 23.

Mean Head Measurements.

	B. A.	U. C. Staff	Criminals
Length	198·1	196·38	191·7
Breadth	155·0	153·48	150·4

As regards variability, Dr Lee's memoir, p. 230, enables me to make the following comparison:

TABLE 24.

Coefficients of Variation.

	Head Length	Head Breadth
Aino Males ...	3·195	2·759
German Males ...	3·371	3·887
Criminals	3·154	3·333

And if we compare our coefficients of variation as a whole with those obtained for other parts of the body by various investigators, we shall find a general agreement in the results.

For instance, Miss Whiteley has shown that the coefficients of variation for the first joints of the fingers range from 4·695 to 5·361 (*Proceedings of the Royal Society*, Vol. 65, p. 129); and Professor Pearson has found that these coefficients for the long bones (Aino and French) range from 4·655 to 5·425.

From these figures it appears that 3 to 5·5 are representative values for variability in man, while in plants it may run to 40!

I may direct attention to some of the general results of the table of correlation (Table 19); e.g., the close relationship of finger and foot, of cubit and foot, which I think is novel and interesting, of height and finger, and cubit and finger. It will be noticed that the high correlations between finger, cubit, foot and height agree generally with the correlations between height and long bones as calculated by Professor Pearson (*Phil. Trans.* Vol. 192, A., p. 181, Table III.). For instance, he gives the coefficient of correlation between height and radius as ·6956, as compared with ·7999 the coefficient of correlation between height and cubit in criminals.

Turning now to the head measurements, Head Length, Head Breadth and Face Breadth, we observe that our results confirm the view that parts of the head are less highly correlated together than other parts of the body. It is also to be noticed that comparatively little correlation exists between the head and the other four characters, and that Head Length is more closely correlated with Cubit than is Head Breadth or Face Breadth, while Face Breadth is more nearly correlated with Foot than is Head Length or Head Breadth. Long head associated with long arm (i.e. cubit) rather suggests the gorilla type, but I can think of no type suggested by the association of long foot and broad face.

PART III.

APPLICATION OF THESE RESULTS TO CRIMINAL IDENTIFICATION.

(16) The conditions to be satisfied in selecting a series of organs for criminal identification are:

(i) Comparative ease and accuracy of measurement.

(ii) Small correlation between them, so that fairly few organs will provide a reliable index to the criminal population.

Head Length, Head Breadth and Face Breadth can be fairly accurately determined; Cubit and Finger with less accuracy; Height changes about $\frac{1}{3}''$ per 10 years from about 27 years of age*, and is not very reliable; Foot is a doubtful measurement.

---

* *Biometrika*, Vol. I. p. 49.

Now the organs selected by Scotland Yard are very far from being slightly correlated. We have seen that Face Breadth and Head Breadth are highly correlated, and the correlations between Finger, Cubit, Foot and Height are very high. It cannot therefore be said that these seven measurements form an ideal group, and I do not suppose they would have been chosen if their correlations had been actually known beforehand. My results at this point conflict with those of Dr Garson, who writes in his paper in the *Journal of the Anthropological Institute* to which I have already referred: "In a mixed population such as we have to deal with in England, the correlation between the different measurements used for the classification of criminals is slight."

Supposing, however, these organs have been selected, we may ask in the next place: What is the best order for entering an Index? This is the problem to which we shall next turn, and for its solution I propose to apply a general theorem in correlation which may be stated thus:

(17) Given n variables, then the most probable value A_1 of one of the variables, for given values $A_2, A_3, \dots A_n$, of the remaining $n-1$, is given by the equation

$$A_1 - m_1 = -\sigma_1 \left\{\frac{R_{12}}{R_{11}} \frac{(A_2 - m_2)}{\sigma_2} + \frac{R_{13}}{R_{11}} \frac{(A_3 - m_3)}{\sigma_3} + \dots + \frac{R_{1n}}{R_{11}} \frac{(A_n - m_n)}{\sigma_n}\right\},$$

and its variability $\bar{s}_1 = \sigma_1 \sqrt{\frac{\Delta}{R_{11}}}$, where σ_1 is the S.D. of the organ A_1, and σ_q of the organ A_q; m_1 the mean of A_1 and m_q the mean of A_q; r_{1q} the correlation coefficient of A_1 and A_q, and $r_{qq'}$ the correlation coefficient of A_q and $A_{q'}$; and finally Δ is the determinant

$$\begin{vmatrix} 1 & r_{12} & r_{13} & r_{14} & \dots & r_{1n} \\ r_{21} & 1 & r_{23} & . & & . \\ \vdots & . & . & . & & . \\ r_{n1} & r_{n2} & . & . & & 1 \end{vmatrix}$$

and R_{pq} the minor corresponding to r_{pq}.

When $A_2, A_3, \dots A_n$ are known for only one individual, the probable error of the determination A_1 is

$$\cdot 67449\, \sigma_1 \sqrt{\frac{\Delta}{R_{11}}};$$

when they are known for p individuals, the probable error of A_1, which is then the corresponding mean value, becomes

$$\cdot 67449\, \sigma_1 \sqrt{\frac{\Delta}{pR_{11}}}.$$

When $n = 2$, the reconstruction formula becomes

$$A_1 - m_1 = \frac{\sigma_1}{\sigma_2} r_{12} (A_2 - m_2),$$

and the probable error

$$\cdot 67449\, \sigma_1 \sqrt{\frac{1 - r^2_{12}}{p}}.$$

(See Pearson, *Phil. Trans.* Vol. 192, A., pp. 171–2.)

(18) In our case Δ is the determinant of the 7th order* :

$$\begin{vmatrix}
1\cdot & \cdot 40163 & \cdot 39454 & \cdot 30071 & \cdot 30539 & \cdot 33886 & \cdot 33993 \\
\cdot 40163 & 1\cdot & \cdot 61779 & \cdot 15040 & \cdot 13515 & \cdot 20614 & \cdot 18308 \\
\cdot 39454 & \cdot 61779 & 1\cdot & \cdot 32097 & \cdot 28869 & \cdot 36322 & \cdot 34527 \\
\cdot 30071 & \cdot 15040 & \cdot 32097 & 1\cdot & \cdot 84638 & \cdot 75871 & \cdot 66084 \\
\cdot 30539 & \cdot 13515 & \cdot 28869 & \cdot 84638 & 1\cdot & \cdot 79699 & \cdot 79986 \\
\cdot 33886 & \cdot 20614 & \cdot 36322 & \cdot 75871 & \cdot 79699 & 1\cdot & \cdot 73636 \\
\cdot 33993 & \cdot 18308 & \cdot 34527 & \cdot 66084 & \cdot 79986 & \cdot 73636 & 1\cdot
\end{vmatrix}$$

the value of which is found by the laborious process of reduction to be $= \cdot 012129$.

Also

$$\begin{array}{lll}
R_{11} = \cdot 016272 & \text{and} & \sqrt{\Delta/R_{11}} = \cdot 86336 \\
R_{22} = \cdot 020907 & & \sqrt{\Delta/R_{22}} = \cdot 76167 \\
R_{33} = \cdot 022556 & & \sqrt{\Delta/R_{33}} = \cdot 73330 \\
R_{44} = \cdot 047496 & & \sqrt{\Delta/R_{44}} = \cdot 50534 \\
R_{55} = \cdot 071864 & & \sqrt{\Delta/R_{55}} = \cdot 41083 \\
R_{66} = \cdot 040198 & & \sqrt{\Delta/R_{66}} = \cdot 54930 \\
R_{77} = \cdot 038347 & & \sqrt{\Delta/R_{77}} = \cdot 56240.
\end{array}$$

Now the best organ to leave to the last will be that the variability of which is least reduced by selecting the other six. For this means that the individuals from whom we have in the last instance to select our man, will be least crowded together and therefore least likely to be indistinguishable. As $\bar{s} = \sigma \sqrt{\frac{\Delta}{R}}$, the greater $\sqrt{\frac{\Delta}{R}}$, the less the reduction in variability; this function we have already calculated for $n = 7$, and we see that in that case it is greatest for Head Length. We will therefore keep Head Length to the last, and proceed to calculate $\sqrt{\frac{\Delta}{R}}$ for the six organs that remain after the elimination of Head Length. The new Δ is obviously $R_{11} = \cdot 016272$, and the new R's can be calculated without much trouble. On comparing the new $\sqrt{\frac{\Delta}{R}}$ series, we shall find that Head Breadth is the next character to be eliminated, and we proceed in like manner until we have dealt with all the characters.

---

* Values of the correlation coefficients are given to 5 places of figures in order to calculate the determinants more accurately.

The result of these operations is shown in the following table:

TABLE 25.

Showing the value of $\sqrt{\Delta/R}$ *for* 3, 4, 5, 6 *and* 7 *characters.*

	7	*6*	*5*	*4*	*3*
Head Length ...	·86336	—			
Head Breadth ...	·76167	·78408	—		
Face Breadth ...	·73330	·73763	·91541	—	
Finger	·50534	·50568	·50607	·50959	·51403
Cubit	·41083	·41083	·41089	·41452	·47660
Foot	·54930	·55016	·55060	·55671	·58296
Height	·56240	·56502	·56504	·57244	—

The order in which the various characters should be taken is therefore: 1st, Cubit; 2nd, Finger; 3rd, Foot; 4th, Height; 5th, Face Breadth; 6th, Head Breadth; 7th, Head Length, and is the same as that which is given by the first column, for $n = 7$. It is to be noted that the head measurements come last.

This result conflicts with the practice at Scotland Yard, where, I understand, the cabinet is entered in the order Head Length, Head Breadth, Face Breadth, Finger, Cubit, Foot and Height, the head measurements being taken first.

Should it be maintained that in fixing the order of the characters we should use $\bar{s}$, and not $\sqrt{\frac{\Delta}{R}}$, as the criterion, that is, that we should leave to the last the organ whose absolute variability is greatest, not the organ whose variability is least reduced, after selection of the other six, I have calculated the following table to show how this principle will work out:

TABLE 26.

Showing the value of $\bar{s} = \sigma\sqrt{\Delta/R}$ *for* 3, 4, 5, 6 *and* 7 *characters.*

		7	*6*	*5*	*4*	*3*
Head Length	$\sigma=$ ·6046	·5220	·5244	·5251	·5289	—
Head Breadth	$\sigma=$ ·5015	·3819	·3821	·3823	·3823	·3935
Face Breadth	$\sigma=$ ·5060	·3711	·3738	·3741	·3769	·3804
Finger	$\sigma=$ ·5479	·2769	·2801	·3543	·5051	·5178
Cubit	$\sigma=$1·9628	·8064	·9323	—	—	—
Foot	$\sigma=$1·1792	·6478	·6707	·7478	—	—
Height	$\sigma=$6·4540	3·6297	—	—	—	—

In the case of height the unit of record is $\frac{1}{8}$th inch = 3·175 mm., for the other characters the unit is 1 mm.; we therefore divide the 3·62971 of the table by

3·175 to obtain a true comparison for height, and find $\bar{s} = 1·1432$, which is still the greatest value of the 7 column.

On this principle, then, the order is 1st, Face Breadth; 2nd, Head Breadth; 3rd, Finger; 4th, Head Length; 5th, Foot; 6th, Cubit; and 7th, Height; it will be observed that this is the order of the standard deviations of the characters, but differs widely from the Scotland Yard order.

I think, however, that $\sqrt{\Delta/R}$ is the correct criterion. We cannot read height to the same accuracy as head length. $\bar{s}$ for height $= 3·6297$, and for head length ·5220, but head length is read to $\frac{1}{2}$ mm. and height to $\frac{1}{16}$ in. Now head length is hardly likely to be correct to more than 1 mm. or height to $\frac{1}{4}$ inch = 6 mm. say, or even to $\frac{1}{2}$ inch = 12 mm.; hence I doubt if stature with an $\bar{s}$ of 3·6 is really better than head length with a range of ·5. The right principle seems to be to suppose each organ on its own S.D. equally useful, and then judge its efficiency by the extent to which that S.D. is altered by selection of the other characters.

(19) Professor Pearson has pointed out to me that the ideal index characters would be given if we calculated the seven directions of uncorrelated variables, that is, the principal axes of the correlation "ellipsoid." Thus, given $x_1, x_2, \dots x_7$ correlated variables, the seven uncorrelated variables are:

$$X_1 = l_{11}x_1 + l_{12}x_2 + \dots + l_{17}x_7$$
$$X_2 = l_{21}x_1 + l_{22}x_2 + \dots + l_{27}x_7$$
$$\&c. \qquad \&c.$$

where the l's give the directions of the principal axes, and $X_1, X_2, \dots X_7$ would be the proper index functions to identify criminals by if we have nothing better than the present correlated characters to work with. Of course this necessitates a preliminary determination of 49 numerical multipliers, but if these were once calculated, the uncorrelated characters of a criminal could be easily found from the measured correlated characters, and his identity established from an X-cabinet, into which we might enter *in any order*. I propose to return in a later paper to this calculation.

(20) *Reconstruction of Height from a knowledge of Finger, Cubit, and Foot, for medico-legal purposes.*

I now propose to consider in greater detail the four characters which have very high correlation, viz., Finger, Cubit, Foot and Height. For these four,

$$\Delta = \begin{vmatrix} 1· & ·84638 & ·75871 & ·66084 \\ ·84638 & 1· & ·79699 & ·79986 \\ ·75871 & ·79699 & 1· & ·73636 \\ ·66084 & ·79986 & ·73636 & 1· \end{vmatrix} = ·031586,$$

and
$$\begin{array}{llll} R_{11} = \cdot121635, & \sqrt{\Delta/R_{11}} = \cdot509587, & R_{12} = & \cdot088885 \\ R_{22} = \cdot183825, & \sqrt{\Delta/R_{22}} = \cdot414520, & R_{13} = & -\cdot031910 \\ R_{33} = \cdot101914, & \sqrt{\Delta/R_{33}} = \cdot556710, & R_{14} = & -\cdot014211 \\ R_{44} = \cdot096392, & \sqrt{\Delta/R_{44}} = \cdot572436, & R_{23} = & \cdot030695 \\ & & R_{24} = & -\cdot065693 \\ & & R_{34} = & \cdot029405 \end{array}$$

Using the values of S.D. and Means given in Table 18 we can now reconstruct the various characters if one or more are given us; we shall choose Height (H) as being probably the most useful example.

(i) *Reconstruction of Height from Finger.*

$$H = 166\cdot45716 + \frac{6\cdot45397}{\cdot54786} \times \cdot66084\ (\text{Finger} - 11\cdot54737),$$
$$= 166\cdot45716 + 7\cdot7849\ (\text{Finger} - 11\cdot54737).$$

Probable error $= \cdot67449 \times 6\cdot45397\ \sqrt{1 - (\cdot66084)^2}/\sqrt{p}$,
$= 3\cdot2671/\sqrt{p}$.

(ii) *Reconstruction of Height from Cubit.*

$$H = 166\cdot45716 + 2\cdot6301\ (\text{Cubit} - 45\cdot05864).$$

Probable error $= 2\cdot6127/\sqrt{p}$.

(iii) *Reconstruction of Height from Foot.*

$$H = 166\cdot45716 + 4\cdot0301\ (\text{Foot} - 25\cdot68770).$$

Probable error $= 2\cdot9453/\sqrt{p}$.

(iv) *Reconstruction of Height from Finger and Cubit.*

In this case,

$$\Delta = \begin{vmatrix} 1\cdot & \cdot66084 & \cdot79986 \\ \cdot66084 & 1\cdot & \cdot84638 \\ \cdot79986 & \cdot84638 & 1\cdot \end{vmatrix} = \cdot101914,$$

$$\begin{array}{lll} R_{11} = \cdot28364, & R_{12} = \cdot01614, & R_{13} = -\cdot24054, \\ \sigma_2 = \cdot54786, & & \sigma_3 = 1\cdot96275, \\ m_2 = 11\cdot54737, & & m_3 = 45\cdot05864. \end{array}$$

Therefore equation for H is:

$$H = 166\cdot45716 - \cdot6703\ (\text{Finger} - 11\cdot54737) + 2\cdot7886\ (\text{Cubit} - 45\cdot05864)^*$$

and probable error $= \dfrac{\cdot67449 \times 6\cdot45397}{\sqrt{p}} \sqrt{\dfrac{\cdot101914}{\cdot28364}}$,
$= 2\cdot6094/\sqrt{p}$.

---

[*] This equation is of special interest, it shows that for persons of a given cubit the longer the finger, the less the height.

(v) *Reconstruction of Height from Finger and Foot.*

In this case,

$$\Delta = \begin{vmatrix} 1\cdot & \cdot66084 & \cdot73636 \\ \cdot66084 & 1\cdot & \cdot75871 \\ \cdot73636 & \cdot75871 & 1\cdot \end{vmatrix} = \cdot183825,$$

$$R_{11} = \cdot42436, \quad R_{12} = -\cdot10216, \quad R_{13} = -\cdot23497,$$

$$\sigma_2 = \cdot54786, \qquad \sigma_3 = 1\cdot17923,$$

$$m_2 = 11\cdot54737, \qquad m_3 = 25\cdot68770.$$

Therefore the equation becomes:

$$H = 166\cdot45716 + 2\cdot8360\ (\text{Finger} - 11\cdot54737) + 3\cdot0304\ (\text{Foot} - 25\cdot68770),$$

and probable error $= 2\cdot8651/\sqrt{p}$.

(vi) *Reconstruction of Height from Cubit and Foot.*

In this case,

$$\Delta = \begin{vmatrix} 1\cdot & \cdot79986 & \cdot73636 \\ \cdot79986 & 1\cdot & \cdot79699 \\ \cdot73636 & \cdot79699 & 1\cdot \end{vmatrix} = \cdot121635,$$

$$R_{11} = \cdot36481, \quad R_{12} = -\cdot21299, \quad R_{13} = -\cdot09888,$$

$$\sigma_2 = 1\cdot96275, \qquad \sigma_3 = 1\cdot17923,$$

$$m_2 = 45\cdot05864, \qquad m_3 = 25\cdot68770.$$

Therefore the equation is:

$$H = 166\cdot45716 + 1\cdot9198\ (\text{Cubit} - 45\cdot05864) + 1\cdot4834\ (\text{Foot} - 25\cdot6877),$$

and probable error $= 2\cdot5136/\sqrt{p}$.

(vii) *Reconstruction of Height from Finger, Cubit and Foot.*

The Δ and R's are already calculated, and from these we can form the equation in H. We will however calculate only the probable error, which is

$$\frac{\cdot67449 \times 6\cdot45397}{\sqrt{p}} \sqrt{\frac{\cdot031586}{\cdot096392}}$$
$$= 2\cdot4919/\sqrt{p}.$$

(viii) *Reconstruction of Height from all the other six organs* (see formula in § 17).

This formula has not been worked out numerically, but its probable error

$$= \frac{\cdot67449 \times 6\cdot45397}{\sqrt{p}} \sqrt{\frac{\cdot012129}{\cdot038347}}$$
$$= 2\cdot4482/\sqrt{p}.$$

We will now collect the probable errors which we have just calculated, taking $p = 1$, which is the case that most interests us.

TABLE 27.

Reconstruction of Height.

From	Probable Error
Finger	3·2671
Cubit	2·6127
Foot	2·9453
Finger and Cubit	2·6094
Finger and Foot	2·8651
Cubit and Foot	2·5136
Finger, Cubit and Foot	2·4919
Finger, Cubit, Foot, Head Length, Head Breadth and Face Breadth	2·4482

It will be noted that we do not improve much on the result obtained from Cubit and Foot by taking into account Finger also; nor on the result obtained from Cubit alone by taking into account Finger and Foot or even by the addition of all the remaining six characters.

As a practical problem, it is conceivable that a medico-legal case might arise where it would be important to discover the most probable height, given a foot or a cubit; the equations in (iii) and (ii) would enable us to reconstruct the height with a probable error of 2·9453 mm. and 2·6127 mm. respectively.

(21) *Examples of Reconstruction Formulæ.* I will illustrate the sort of errors that will be made by the use of these formulæ by applying them to the reconstruction of stature in ten cases selected at random from my material.

The Table 28 shows the difference between the actual and the calculated heights, *minus* denoting that the calculated stature is in defect, *plus* that it is in excess.

It will be noticed that the result of calculating from Finger and Cubit is practically the same as from Cubit alone.

As was to be expected, the reconstruction is only approximate, but it is quite as good as the results obtained for the reconstruction of stature from the long bones*. Given the Cubit, the Finger and Cubit or the Foot and Cubit, we can estimate the stature with a probable error of about one inch. We see that the actual ten cases give deviations, plus and minus, clustered round the probable error of each method of estimating in quite a reasonable manner. Occasionally our estimate might be wrong by upwards of two inches, but such cases are rare. It is of course the old story: No formula will give the stature with very great closeness from the measurement of one or two other organs. If it did, organic relationship would be perfect correlation, variation would have ceased, and with it of necessity the Darwinian theory of evolution. Allowing accordingly for this limitation, I believe the results obtained to be of use for purposes of criminal investigation. For example a finger, cubit or foot having been discovered, and

---

* Pearson: On the Reconstruction of the Stature of Prehistoric Races, *Phil. Trans.* Vol. 192, A., p. 188.

a crime being suspected, the police might from my formulæ give a very fair estimate of the stature of the supposed victim for the purpose of identification. The form of such an estimate from the cubit, for my first individual, for example, would be 62″ ± 1″.

TABLE 28.

Showing the difference in cms. between Actual and Calculated Height in 10 *Criminals selected at random.*

Actual Stature, cms.	STATURE CALCULATED FROM						Average
	Finger	Cubit	Foot	Finger and Cubit	Finger and Foot	Foot and Cubit	
160·3	+ ·3	−2·4	+ ·6	−2·4	− ·2	−2·1	−1·03
168·3	−1·4	−2·3	−6·2	−2·3	−5·	−3·8	−3·50
170·2	−2·6	−4·2	−3·3	−4·3	−3·	−3·9	−3·55
158·4	+5·4	+1·3	−2·	+1·2	− ·5	− ·5	+ ·82
161·9	+1·1	+3·1	+2·6	+3·3	+1·8	+2·8	+2·45
165·7	+ ·3	− ·4	+3·2	− ·5	+2·5	+ ·8	+ ·98
167·6	+1·6	+ ·5	+5·8	+ ·4	+5·	+2·6	+2·65
160·	+4·5	+1·	+3·7	+ ·9	+3·7	+1·5	+2·55
161·9	+8·1	+6·2	− ·6	+6·	+1·9	+3·9	+4·25
169·5	−5·	−1·1	−4·2	− ·8	−4·6	−2·	−2·95
Average	3·03	2·25	3·22	2·21	2·82	2·39	2·47
Prob. Error	±3·3	±2·6	±2·9	±2·6	±2·9	±2·5	±2·8

(22) *Summary.* I will conclude with a short summary of the results set forth in detail in this paper.

(i) The great value of the new method of calculating standard deviations, means, and coefficients of correlation is demonstrated; it economises labour, and leads to results closely approximating to those obtained by more laborious methods.

(ii) Then as regards the criminals; they are shown to be homogeneous, fairly normal in the distribution of the selected characters, markedly different from the educated classes in stature, and in size and shape of the head, but in variability they agree generally with other classes and races of mankind.

(iii) Finally as regards the problem of identification, it has been shown that certain characters at present in use are not very suitable for the purpose, owing to their high correlation, but assuming them to be employed, we have discovered the proper order in which they should be entered in an index cabinet—an order very different from that determined by Scotland Yard—and have indicated a method of calculating uncorrelated characters which would furnish an ideal system of identification.

TABLE I. 3000 *Criminals.*

Head Length (centimetres).

Head Breadth (centimetres).

	16·						17·										18·										19·										20·										21·				Totals
	4	5	6	7	8	9	0	1	2	3	4	5	6	7	8	9	0	1	2	3	4	5	6	7	8	9	0	1	2	3	4	5	6	7	8	9	0	1	2	3	4	5	6	7	8	9	0	1	2	3	
13·4	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1
5	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	2
6	–	–	–	–	–	–	–	–	–	–	1	–	–	–	1	1	–	–	–	–	–	–	–	–	–	–	–	1	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	5
7	–	–	–	–	–	–	–	–	–	1	1	–	–	–	–	1	–	1	–	–	–	–	–	1	–	–	1	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	7
8	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	3	–	–	–	1	–	–	–	–	1	2	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	8
9	–	–	–	–	–	–	–	1	–	–	–	–	1	1	1	1	–	–	–	–	1	–	2	1	–	–	1	1	–	1	1	–	–	–	–	–	1	1	–	–	–	–	–	–	–	–	–	–	–	–	15
14·0	–	–	–	–	–	–	–	–	–	–	–	2	–	1	1	1	–	–	1	3	1	1	4	2	–	1	–	1	1	1	–	1	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	23
1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	1	1	1	–	1	3	7	1	4	4	4	2	3	–	–	1	–	–	2	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	36
2	–	–	–	–	1	–	–	–	1	1	–	1	–	–	1	1	1	2	2	3	5	3	7	–	4	4	1	5	1	5	2	3	1	3	–	–	–	–	1	–	–	–	1	–	–	–	–	–	–	–	60
3	–	–	–	–	–	–	–	–	–	–	1	–	1	1	–	2	2	1	1	3	8	4	5	6	6	8	4	8	2	2	4	1	2	2	1	–	–	–	1	–	–	–	–	–	–	–	1	–	–	–	77
4	–	–	–	–	–	–	–	–	–	1	–	–	1	–	1	2	3	3	5	1	6	4	8	6	10	6	4	7	4	3	8	3	2	5	–	1	1	–	–	–	–	–	–	–	–	–	–	–	–	–	95
5	–	–	–	–	–	–	–	–	–	1	1	–	–	–	–	3	3	2	7	4	5	8	5	6	14	8	9	8	13	12	10	6	10	4	4	1	3	–	1	1	–	1	–	–	–	–	–	–	–	–	150
6	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	3	6	4	4	8	8	13	6	10	10	14	13	20	13	11	7	9	5	5	9	4	3	2	2	2	1	–	–	–	–	–	–	–	–	–	193
7	–	–	–	–	–	–	–	–	–	–	2	–	1	1	–	2	4	9	6	6	1	12	12	13	15	15	15	16	18	7	13	8	10	5	4	2	2	1	1	–	1	–	–	–	–	–	–	–	–	–	202
8	–	–	–	–	–	–	–	–	–	–	–	–	–	1	3	–	1	4	6	5	12	9	11	11	11	12	17	15	26	13	9	12	7	6	5	2	3	–	–	–	–	–	–	2	–	–	–	–	–	–	203
9	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	3	–	3	6	2	8	7	12	12	10	19	17	25	18	16	22	10	11	5	8	1	2	4	–	1	1	–	–	–	–	–	–	–	–	223
15·0	–	–	–	–	–	–	–	–	1	–	–	–	–	–	2	3	5	5	3	3	8	12	10	11	21	12	13	19	17	18	14	8	10	6	9	7	5	2	–	2	1	–	1	–	–	–	–	–	–	–	228
1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	3	–	3	2	5	11	8	11	5	17	13	27	26	18	14	21	9	2	10	8	5	4	7	1	–	2	–	2	–	–	–	–	–	–	234
2	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	3	2	7	12	6	10	9	16	18	16	16	19	18	8	14	11	7	5	6	4	2	1	4	3	–	2	–	1	–	–	–	–	221
3	1	–	–	–	–	1	–	–	–	–	–	–	1	1	–	2	2	–	3	4	6	8	2	8	10	19	16	14	19	10	12	19	10	14	6	7	3	3	–	6	3	3	–	–	–	–	–	–	–	–	213
4	–	–	–	–	–	–	–	–	–	–	–	–	–	1	1	–	–	–	1	4	4	3	3	1	4	5	13	10	12	16	12	8	7	14	9	10	11	8	5	1	2	2	–	–	1	–	–	–	–	–	168
5	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	2	–	2	3	3	4	4	7	10	13	12	10	7	5	7	4	9	8	6	7	8	6	7	3	3	1	1	–	–	1	–	–	–	153
6	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	1	3	3	5	6	5	12	5	8	5	11	9	16	18	9	6	9	6	7	1	2	1	1	–	–	1	–	–	–	–	151
7	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	1	1	–	1	2	2	2	2	5	5	11	5	9	6	6	7	7	3	1	1	5	3	6	–	1	1	–	1	–	–	–	–	95
8	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	1	2	1	1	3	1	3	3	8	4	8	11	4	1	4	3	3	3	4	1	3	2	–	–	–	–	–	–	–	–	75
9	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	1	1	–	3	–	4	3	–	2	1	1	3	4	3	4	2	9	2	–	2	1	3	–	1	–	–	–	–	–	–	51
16·0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	1	–	–	1	1	4	2	1	–	4	2	5	3	1	2	3	3	1	1	–	–	–	–	–	–	–	–	36
1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	2	–	–	–	4	2	1	1	2	1	–	1	2	4	2	1	–	–	1	–	–	–	–	–	–	–	–	25
2	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	1	–	2	–	3	–	2	3	1	3	2	–	–	1	1	–	1	–	1	–	–	–	–	–	–	22
3	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	1	1	2	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	6
4	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	2	–	–	–	–	–	1	1	2	2	–	–	–	–	–	–	–	–	–	–	–	9
5	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	1	–	–	1	1	1	–	–	–	1	–	1	–	–	–	–	–	–	–	–	–	7
6	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	3
7	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1
8	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	0
9	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	2
Totals	1	–	–	–	1	1	–	1	3	4	6	4	7	7	12	24	35	38	51	57	88	123	114	128	153	175	199	223	241	187	176	172	142	133	114	84	79	53	54	33	30	24	5	11	1	3	2	–	–	1	3000

TABLE II. 3000 *Criminals.* Height (feet and inches).

Head Breadth (millimetres).

	4′ 7″ $\frac{9}{16}$—8″ $\frac{9}{16}$	8″ $\frac{9}{16}$—9″ $\frac{9}{16}$	9″ $\frac{9}{16}$—10″ $\frac{9}{16}$	10″ $\frac{9}{16}$—11″ $\frac{9}{16}$	5′ 11″ $\frac{9}{16}$—0″ $\frac{9}{16}$	0″ $\frac{9}{16}$—1″ $\frac{9}{16}$	1″ $\frac{9}{16}$—2″ $\frac{9}{16}$	2″ $\frac{9}{16}$—3″ $\frac{9}{16}$	3″ $\frac{9}{16}$—4″ $\frac{9}{16}$	4″ $\frac{9}{16}$—5″ $\frac{9}{16}$	5″ $\frac{9}{16}$—6″ $\frac{9}{16}$	6″ $\frac{9}{16}$—7″ $\frac{9}{16}$	7″ $\frac{9}{16}$—8″ $\frac{9}{16}$	8″ $\frac{9}{16}$—9″ $\frac{9}{16}$	9″ $\frac{9}{16}$—10″ $\frac{9}{16}$	10″ $\frac{9}{16}$—11″ $\frac{9}{16}$	6′ 11″ $\frac{9}{16}$—0″ $\frac{9}{16}$	0″ $\frac{9}{16}$—1″ $\frac{9}{16}$	1″ $\frac{9}{16}$—2″ $\frac{9}{16}$	2″ $\frac{9}{16}$—3″ $\frac{9}{16}$	3″ $\frac{9}{16}$—4″ $\frac{9}{16}$	4″ $\frac{9}{16}$—5″ $\frac{9}{16}$	Totals
13·4	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
5	—	—	—	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2
6	—	—	—	1	—	—	—	—	3	—	—	—	—	1	—	—	—	—	—	—	—	—	5
7	—	—	—	—	—	—	1	1	3	1	1	—	—	—	—	—	—	—	—	—	—	—	7
8	—	—	—	1	1	2	—	—	—	2	—	1	1	—	—	—	—	—	—	—	—	—	8
9	—	—	—	1	—	—	1	5	1	3	4	—	—	—	—	—	—	—	—	—	—	—	15
14·0	—	—	—	—	1	3	—	5	5	1	2	3	3	—	—	—	—	—	—	—	—	—	23
1	—	—	—	1	—	1	1	2	7	5	9	9	1	—	—	—	—	—	—	—	—	—	36
2	1	—	—	1	1	2	9	6	6	7	10	6	5	4	1	1	—	—	—	—	—	—	60
3	—	—	—	2	4	4	8	6	13	5	11	12	7	—	1	2	1	1	—	—	—	—	77
4	—	—	—	1	4	7	7	11	17	7	19	7	5	6	3	1	—	—	—	—	—	—	95
5	—	—	1	—	2	6	15	22	21	29	23	15	6	8	2	—	—	—	—	—	—	—	150
6	—	—	1	2	5	5	17	24	27	33	26	18	15	11	7	2	—	—	—	—	—	—	193
7	—	—	—	1	2	6	15	23	25	34	31	28	19	8	7	2	—	—	1	—	—	—	202
8	—	—	2	4	2	6	15	19	27	36	24	20	23	14	6	4	1	—	—	—	—	—	203
9	—	—	—	1	6	10	10	25	20	38	37	39	13	14	7	2	—	—	—	—	—	1	223
15·0	—	—	1	1	3	4	10	31	36	33	34	29	17	12	10	3	3	1	—	—	—	—	228
1	—	—	—	—	1	5	10	26	39	33	30	45	15	19	7	2	2	—	—	—	—	—	234
2	—	1	1	—	1	4	12	28	30	37	35	28	17	14	8	3	1	—	1	—	—	—	221
3	—	—	—	3	4	6	12	18	26	34	47	18	26	6	7	5	1	—	—	—	—	—	213
4	—	—	—	—	4	8	10	12	19	25	30	25	13	8	4	5	2	2	1	—	—	—	168
5	—	—	—	—	—	1	8	15	16	27	13	33	22	10	6	1	1	—	—	—	—	—	153
6	—	—	—	—	3	5	5	14	17	26	23	16	17	14	6	3	2	—	—	—	—	—	151
7	—	—	—	—	—	1	2	9	16	11	12	19	8	5	3	5	1	2	1	—	—	—	95
8	—	—	—	1	1	—	3	4	5	13	16	11	10	7	2	1	1	—	—	—	—	—	75
9	—	—	—	—	1	1	—	4	6	7	9	6	7	3	5	2	—	—	—	—	—	—	51
16·0	—	—	—	1	—	1	1	2	2	7	4	12	1	3	2	—	—	—	—	—	—	—	36
1	—	—	—	—	—	1	—	3	2	3	5	4	4	2	—	—	—	1	—	—	—	—	25
2	—	—	—	—	—	—	3	—	—	2	3	4	6	3	1	—	—	—	—	—	—	—	22
3	—	—	—	—	—	—	—	—	3	—	—	—	1	2	—	—	—	—	—	—	—	—	6
4	—	—	—	—	—	—	—	1	—	2	—	3	—	1	1	—	1	—	—	—	—	—	9
5	—	—	—	—	—	—	—	1	1	1	—	1	2	—	—	1	—	—	—	—	—	—	7
6	—	—	—	—	—	1	—	—	—	—	—	—	—	1	—	1	—	—	—	—	—	—	3
7	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	1
8	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
9	—	—	—	—	—	—	—	—	—	—	—	1	—	—	1	—	—	—	—	—	—	—	2
Totals	1	1	6	23	48	90	175	317	393	462	458	413	264	177	97	46	17	7	4	—	—	1	3000
Means	142	152	148·2	146·4	148·1	149·0	149·0	149·8	149·8	150·6	150·4	151·1	151·5	151·6	151·7	152·3	153·1	153·7	152·5	—	—	149	

TABLE III. 3000 *Criminals.* Height (feet and inches).

Left Middle Finger (millimetres).	4′ 7″ $\frac{9}{16}$—8″ $\frac{9}{16}$	8″ $\frac{9}{16}$—9″ $\frac{9}{16}$	9″ $\frac{9}{16}$—10″ $\frac{9}{16}$	10″ $\frac{9}{16}$—11″ $\frac{9}{16}$	5′ 11″ $\frac{9}{16}$—0″ $\frac{9}{16}$	0″ $\frac{9}{16}$—1″ $\frac{9}{16}$	1″ $\frac{9}{16}$—2″ $\frac{9}{16}$	2″ $\frac{9}{16}$—3″ $\frac{9}{16}$	3″ $\frac{9}{16}$—4″ $\frac{9}{16}$	4″ $\frac{9}{16}$—5″ $\frac{9}{16}$	5″ $\frac{9}{16}$—6″ $\frac{9}{16}$	6″ $\frac{9}{16}$—7″ $\frac{9}{16}$	7″ $\frac{9}{16}$—8″ $\frac{9}{16}$	8″ $\frac{9}{16}$—9″ $\frac{9}{16}$	9″ $\frac{9}{16}$—10″ $\frac{9}{16}$	10″ $\frac{9}{16}$—11″ $\frac{9}{16}$	6′ 11″ $\frac{9}{16}$—0″ $\frac{9}{16}$	0″ $\frac{9}{16}$—1″ $\frac{9}{16}$	1″ $\frac{9}{16}$—2″ $\frac{9}{16}$	2″ $\frac{9}{16}$—3″ $\frac{9}{16}$	3″ $\frac{9}{16}$—4″ $\frac{9}{16}$	4″ $\frac{9}{16}$—5″ $\frac{9}{16}$	Totals
9·4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
5	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
6	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
8	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
9	—	—	1	—	1	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	3
10·0	1	—	—	1	2	—	2	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	7
1	—	—	—	1	3	1	—	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	7
2	—	—	2	2	2	1	—	2	—	1	—	—	—	—	—	—	—	—	—	—	—	—	10
3	—	1	1	3	2	2	3	5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	17
4	—	—	1	1	2	3	3	4	3	3	—	—	—	—	—	—	—	—	—	—	—	—	20
5	—	—	—	1	3	7	6	4	3	1	3	1	—	1	—	—	—	—	—	—	—	—	30
6	—	—	—	1	4	5	9	14	6	3	1	—	—	1	—	—	—	—	—	—	—	—	44
7	—	—	1	2	4	9	14	16	15	7	3	1	2	—	—	—	—	—	—	—	—	—	74
8	—	—	—	2	5	6	14	27	10	7	1	2	1	—	—	—	—	—	—	—	—	—	75
9	—	—	—	—	2	6	14	24	27	14	10	4	1	—	—	—	—	—	—	—	—	—	102
11·0	—	—	—	2	6	12	15	31	37	27	17	10	6	—	—	—	—	—	—	—	—	—	163
1	—	—	—	3	3	12	22	26	24	26	24	7	4	1	—	—	—	—	—	—	—	—	152
2	—	—	—	3	2	7	21	30	38	29	27	20	4	1	—	—	—	—	—	—	—	1	183
3	—	—	—	1	—	5	10	24	26	39	26	24	7	2	—	—	—	—	—	—	—	—	164
4	—	—	—	—	3	4	9	29	56	58	26	22	10	11	—	—	—	—	—	—	—	—	228
5	—	—	—	—	—	5	11	17	33	57	38	34	25	11	2	—	—	—	—	—	—	—	233
6	—	—	—	—	2	1	4	13	37	39	48	38	27	12	2	2	—	1	—	—	—	—	226
7	—	—	—	—	—	2	9	17	30	37	48	45	24	9	9	2	—	—	—	—	—	—	232
8	—	—	—	—	1	—	2	11	15	35	41	34	29	10	5	1	—	—	—	—	—	—	184
9	—	—	—	—	1	1	2	12	10	27	32	35	19	10	9	3	1	—	—	—	—	—	162
12·0	—	—	—	—	—	—	1	4	8	19	42	39	22	16	8	2	2	—	—	—	—	—	163
1	—	—	—	—	—	—	—	2	4	13	22	28	15	27	10	4	1	—	—	—	—	—	126
2	—	—	—	—	—	—	1	2	5	6	23	17	16	11	8	1	1	—	—	—	—	—	91
3	—	—	—	—	—	—	—	—	4	8	10	13	20	23	6	5	—	—	—	—	—	—	89
4	—	—	—	—	—	—	1	1	1	2	7	12	4	7	7	1	—	—	1	—	—	—	44
5	—	—	—	—	—	—	—	1	—	1	3	12	11	8	6	8	—	2	—	—	—	—	52
6	—	—	—	—	—	—	—	—	—	1	—	3	5	7	8	6	3	1	1	—	—	—	35
7	—	—	—	—	—	—	—	—	—	1	1	7	5	5	8	2	2	—	—	—	—	—	31
8	—	—	—	—	—	—	—	—	—	—	1	2	3	1	8	5	3	1	1	—	—	—	25
9	—	—	—	—	—	—	—	—	—	—	—	1	2	2	—	1	1	—	—	—	—	—	7
13·0	—	—	—	—	—	—	—	—	—	—	3	—	1	—	1	—	2	1	—	—	—	—	8
1	—	—	—	—	—	—	—	—	—	—	—	1	1	—	—	—	—	—	—	—	—	—	2
2	—	—	—	—	—	—	—	—	—	—	1	1	—	1	—	3	—	—	—	—	—	—	6
3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	1	—	—	—	2
4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	1
Totals	1	1	6	23	48	90	175	317	393	462	458	413	264	177	97	46	17	7	4	0	0	1	3000
Means	100	103	102·8	107·0	107·8	109·4	110·6	111·8	113·3	114·8	116·5	117·7	118·6	120·1	122·2	123·9	125·9	126·4	127·7	—	—	112	

TABLE IV. 1306 *Criminals.*

Head Length (millimetres).

Head Breadth (millimetres).

	17·									*18·*										*19·*										*20·*										*21·*	
	1	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*0*	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*0*	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*0*	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*0*	Totals
13·4	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1
5	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1
6	–	–	–	–	–	–	–	1	1	–	–	–	–	–	–	–	–	–	–	–	1	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	4
7	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	3
8	–	–	–	–	–	–	–	–	–	–	3	–	–	–	1	–	–	–	–	1	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	6
9	1	–	–	–	–	–	1	–	–	–	–	–	–	1	–	1	1	–	–	–	–	–	–	1	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	7
14·0	–	–	–	–	2	–	–	1	–	–	–	–	3	1	–	1	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	9
1	–	–	–	–	–	–	–	1	–	–	1	1	–	–	2	5	–	3	2	1	1	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	18
2	–	–	1	–	1	–	–	–	1	1	1	1	–	2	–	4	–	1	1	1	1	1	2	–	1	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	21
3	–	–	–	–	–	1	1	–	2	–	–	1	1	5	2	2	2	4	4	1	4	1	–	3	1	2	–	–	–	–	–	1	–	–	–	–	–	–	–	1	39
4	–	–	1	–	–	–	–	–	–	1	1	2	–	3	1	3	3	6	4	4	3	3	2	3	3	–	3	–	–	–	–	–	–	–	–	–	–	–	–	–	46
5	–	–	–	–	–	–	–	–	2	1	–	7	1	2	6	2	3	5	4	5	3	5	4	6	1	3	1	2	1	1	–	–	–	–	–	–	–	–	–	–	65
6	–	–	–	–	–	–	–	–	2	1	1	1	3	3	5	4	6	7	10	7	7	6	5	3	2	4	2	4	2	1	1	1	1	1	–	–	–	–	–	–	90
7	–	–	–	–	–	1	1	–	1	1	2	1	2	–	3	10	5	6	8	6	8	8	5	4	3	6	2	3	2	1	1	1	–	1	–	–	–	–	–	–	92
8	–	–	–	–	–	–	–	2	–	–	3	3	3	4	4	5	4	5	3	4	7	11	6	3	4	6	4	2	–	2	–	–	–	–	–	–	1	–	–	–	86
9	–	–	–	–	–	–	–	–	–	3	–	2	3	1	5	2	3	7	4	7	8	12	7	10	6	4	3	2	3	1	–	3	–	1	1	–	–	–	–	–	98
15·0	–	1	–	–	–	–	–	1	2	2	3	2	1	5	6	4	4	5	8	5	5	5	6	8	3	2	3	5	1	2	1	–	1	–	–	1	–	–	–	–	92
1	–	–	–	–	–	–	–	–	–	–	–	2	2	–	5	5	4	2	11	6	11	10	10	10	8	2	1	3	4	2	2	4	–	–	–	–	–	–	–	–	104
2	–	–	–	–	–	–	–	–	–	–	1	2	–	6	7	3	5	3	6	9	7	6	7	9	3	6	5	1	1	3	2	–	1	2	1	–	1	–	–	–	97
3	–	–	–	–	–	–	1	–	1	2	–	–	3	3	3	–	4	5	11	5	6	7	4	5	8	4	7	1	3	1	1	–	4	1	1	–	–	–	–	–	91
4	–	–	–	–	–	–	1	1	–	–	–	1	1	2	1	1	–	1	–	7	4	7	8	6	5	4	6	2	5	2	3	4	–	1	–	–	–	–	–	–	73
5	–	–	–	–	–	–	–	–	–	–	–	–	–	2	–	2	4	1	2	5	5	5	2	2	2	3	3	3	3	3	3	2	3	1	–	–	–	–	–	–	56
6	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	2	4	3	–	4	4	2	2	5	5	6	7	4	3	3	1	1	–	–	–	1	–	–	–	–	57
7	–	–	–	–	–	–	–	–	–	1	–	1	1	–	–	–	1	2	2	3	2	5	3	3	3	2	1	1	1	1	1	4	1	4	–	1	1	–	1	–	46
8	–	–	–	–	–	–	–	–	–	–	–	1	–	1	1	–	–	2	1	2	3	2	–	1	1	3	1	2	1	2	–	1	1	–	1	–	–	–	–	–	27
9	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	2	–	1	–	–	2	–	–	3	2	2	2	–	3	–	–	1	–	2	–	1	–	–	–	22
16·0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	1	1	2	1	1	–	3	–	3	–	1	1	–	1	1	1	–	–	–	–	–	18
1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	2	1	–	1	1	–	–	1	1	1	–	–	–	1	–	–	–	–	–	10
2	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	1	–	1	–	–	–	1	2	1	2	2	–	–	–	–	–	–	–	1	–	–	–	12
3	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	1	1	1	–	–	–	–	–	–	–	–	–	1	–	–	–	5
4	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	1	–	–	–	–	–	1	–	1	1	–	–	–	–	–	–	–	–	5
5	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	1	–	–	–	–	1	–	–	–	–	–	–	–	–	3
6	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	1
7	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	0
8	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	0
9	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1
Totals	1	1	2	0	3	3	5	7	12	13	17	28	24	43	53	57	55	68	83	85	96	102	79	83	66	66	56	43	35	30	20	24	14	13	8	3	6	0	1	1	1306

TABLE V. 1306 *Criminals.*

Height (feet and inches).

Head Breadth (millimetres).	4' 7"$\frac{9}{16}$—8"$\frac{9}{16}$	8"$\frac{9}{16}$—9"$\frac{9}{16}$	9"$\frac{9}{16}$—10"$\frac{9}{16}$	10"$\frac{9}{16}$—11"$\frac{9}{16}$	5' 11"$\frac{9}{16}$—0"$\frac{9}{16}$	0"$\frac{9}{16}$—1"$\frac{9}{16}$	1"$\frac{9}{16}$—2"$\frac{9}{16}$	2"$\frac{9}{16}$—3"$\frac{9}{16}$	3"$\frac{9}{16}$—4"$\frac{9}{16}$	4"$\frac{9}{16}$—5"$\frac{9}{16}$	5"$\frac{9}{16}$—6"$\frac{9}{16}$	6"$\frac{9}{16}$—7"$\frac{9}{16}$	7"$\frac{9}{16}$—8"$\frac{9}{16}$	8"$\frac{9}{16}$—9"$\frac{9}{16}$	9"$\frac{9}{16}$—10"$\frac{9}{16}$	10"$\frac{9}{16}$—11"$\frac{9}{16}$	6' 11"$\frac{9}{16}$—0"$\frac{9}{16}$	0"$\frac{9}{16}$—1"$\frac{9}{16}$	1"$\frac{9}{16}$—2"$\frac{9}{16}$	2"$\frac{9}{16}$—3"$\frac{9}{16}$	3"$\frac{9}{16}$—4"$\frac{9}{16}$	4"$\frac{9}{16}$—5"$\frac{9}{16}$	Totals
13·4	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
5	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
6	—	—	—	1	—	—	—	—	2	—	—	—	—	1	—	—	—	—	—	—	—	—	4
7	—	—	—	—	—	—	—	1	—	1	1	—	—	—	—	—	—	—	—	—	—	—	3
8	—	—	—	1	1	1	—	—	—	2	—	1	—	—	—	—	—	—	—	—	—	—	6
9	—	—	—	—	—	—	—	3	1	2	1	—	—	—	—	—	—	—	—	—	—	—	7
14·0	—	—	—	—	1	2	—	2	1	—	1	1	1	—	—	—	—	—	—	—	—	—	9
1	—	—	—	1	—	—	1	1	2	4	3	5	1	—	—	—	—	—	—	—	—	—	18
2	—	—	—	1	1	1	4	2	4	1	4	1	1	—	—	1	—	—	—	—	—	—	21
3	—	—	—	2	3	2	4	3	7	3	6	5	2	—	—	1	—	1	—	—	—	—	39
4	—	—	—	—	2	3	5	5	7	3	10	5	2	3	1	—	—	—	—	—	—	—	46
5	—	—	—	—	1	3	7	10	9	15	10	5	—	3	2	—	—	—	—	—	—	—	65
6	—	—	—	2	2	3	7	9	14	12	13	8	9	6	3	2	—	—	—	—	—	—	90
7	—	—	—	1	1	5	7	10	15	12	14	9	9	3	4	2	—	—	—	—	—	—	92
8	—	—	—	2	1	4	6	11	10	15	11	7	8	8	2	1	—	—	—	—	—	—	86
9	—	—	—	—	2	8	5	13	10	14	11	17	4	8	3	2	—	—	—	—	—	1	98
15·0	—	—	—	—	—	1	4	16	13	10	12	18	6	6	3	3	—	—	—	—	—	—	92
1	—	—	—	—	—	3	6	14	15	16	15	18	7	7	2	—	1	—	—	—	—	—	104
2	—	1	—	—	1	2	6	16	17	14	11	13	5	6	3	1	1	—	—	—	—	—	97
3	—	—	—	1	1	1	7	9	12	14	20	8	12	1	3	2	—	—	—	—	—	—	91
4	—	—	—	—	3	4	3	6	7	9	14	13	4	4	1	4	—	1	—	—	—	—	73
5	—	—	—	—	—	1	3	7	3	13	4	10	9	3	3	—	—	—	—	—	—	—	56
6	—	—	—	—	2	1	2	2	6	12	11	6	5	5	2	3	—	—	—	—	—	—	57
7	—	—	—	—	—	1	2	5	7	3	5	8	5	4	1	2	1	1	1	—	—	—	46
8	—	—	—	1	—	—	—	1	2	4	4	4	6	3	1	1	—	—	—	—	—	—	27
9	—	—	—	—	—	—	—	1	3	5	4	2	3	1	2	1	—	—	—	—	—	—	22
16·0	—	—	—	1	—	1	1	2	1	3	2	4	1	2	—	—	—	—	—	—	—	—	18
1	—	—	—	—	—	—	—	—	—	2	4	2	—	1	—	—	—	1	—	—	—	—	10
2	—	—	—	—	—	—	1	—	—	1	2	2	2	3	1	—	—	—	—	—	—	—	12
3	—	—	—	—	—	—	—	—	3	—	—	—	1	1	—	—	—	—	—	—	—	—	5
4	—	—	—	—	—	—	—	1	—	2	—	1	—	1	—	—	—	—	—	—	—	—	5
5	—	—	—	—	—	—	—	1	—	1	—	1	—	—	—	—	—	—	—	—	—	—	3
6	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
8	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
9	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	1
Totals	—	1	—	15	23	48	81	151	171	193	193	175	103	80	37	26	3	4	1	—	—	1	1306

TABLE VI. 1306 *Criminals*. Height (feet and inches).

Left Middle Finger (millimetres).	4′ 7″ 9/16—8″ 9/16	8″ 9/16—9″ 9/16	9″ 9/16—10″ 9/16	10″ 9/16—11″ 9/16	5′ 11″ 9/16—0″ 9/16	0″ 9/16—1″ 9/16	1″ 9/16—2″ 9/16	2″ 9/16—3″ 9/16	3″ 9/16—4″ 9/16	4″ 9/16—5″ 9/16	5″ 9/16—6″ 9/16	6″ 9/16—7″ 9/16	7″ 9/16—8″ 9/16	8″ 9/16—9″ 9/16	9″ 9/16—10″ 9/16	10″ 9/16—11″ 9/16	6′ 11″ 9/16—0″ 9/16	0″ 9/16—1″ 9/16	1″ 9/16—2″ 9/16	2″ 9/16—3″ 9/16	3″ 9/16—4″ 9/16	4″ 9/16—5″ 9/16	Totals
9·4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
5	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
6	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
8	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
9	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
10·0	—	—	—	1	1	—	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4
1	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
2	—	—	—	1	1	1	—	1	—	1	—	—	—	—	—	—	—	—	—	—	—	—	5
3	—	1	—	2	1	2	1	3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	10
4	—	—	—	1	1	2	2	2	1	1	—	—	—	—	—	—	—	—	—	—	—	—	10
5	—	—	—	1	2	4	1	2	1	—	1	1	—	—	—	—	—	—	—	—	—	—	13
6	—	—	—	1	2	—	3	9	3	1	1	—	—	1	—	—	—	—	—	—	—	—	21
7	—	—	—	2	3	4	7	7	6	2	1	—	—	—	—	—	—	—	—	—	—	—	32
8	—	—	—	2	2	3	7	16	3	—	—	—	—	—	—	—	—	—	—	—	—	—	33
9	—	—	—	—	2	5	5	12	12	9	6	—	—	—	—	—	—	—	—	—	—	—	51
11·0	—	—	—	1	4	5	11	12	16	12	6	4	3	—	—	—	—	—	—	—	—	—	74
1	—	—	—	1	—	7	10	8	11	10	7	3	2	1	—	—	—	—	—	—	—	—	60
2	—	—	—	1	1	4	9	13	20	18	11	10	—	—	—	—	—	—	—	—	—	1	88
3	—	—	—	—	—	3	2	12	12	13	15	11	3	2	—	—	—	—	—	—	—	—	73
4	—	—	—	—	—	2	5	17	21	22	9	10	3	5	—	—	—	—	—	—	—	—	94
5	—	—	—	—	—	2	7	12	18	21	17	14	7	6	—	—	—	—	—	—	—	—	104
6	—	—	—	—	2	—	—	—	16	20	21	14	13	8	1	2	—	1	—	—	—	—	98
7	—	—	—	—	—	2	4	8	16	17	25	15	10	6	—	1	—	—	—	—	—	—	104
8	—	—	—	—	—	—	1	6	5	10	18	17	9	5	2	1	—	—	—	—	—	—	74
9	—	—	—	—	1	1	—	6	6	14	14	17	8	5	4	1	—	—	—	—	—	—	77
12·0	—	—	—	—	—	—	—	3	4	9	19	22	10	7	4	1	—	—	—	—	—	—	79
1	—	—	—	—	—	—	—	—	—	3	4	8	10	11	6	2	—	—	—	—	—	—	44
2	—	—	—	—	—	—	1	1	—	2	8	3	6	3	3	1	—	—	—	—	—	—	28
3	—	—	—	—	—	—	—	—	—	6	4	5	8	12	3	3	—	—	—	—	—	—	41
4	—	—	—	—	—	—	1	—	—	2	3	8	2	2	3	—	—	—	1	—	—	—	22
5	—	—	—	—	—	—	—	1	—	—	1	5	4	3	2	5	—	—	—	—	—	—	21
6	—	—	—	—	—	—	—	—	—	—	—	1	1	2	4	2	1	1	—	—	—	—	12
7	—	—	—	—	—	—	—	—	—	—	—	4	3	1	2	2	—	—	—	—	—	—	12
8	—	—	—	—	—	—	—	—	—	—	—	1	1	—	3	2	—	—	—	—	—	—	7
9	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	1	1	—	—	—	—	—	3
13·0	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	1	1	—	—	—	—	4
1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
2	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	2	—	—	—	—	—	—	3
3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	1
Totals	—	1	—	15	23	48	81	151	171	193	193	175	103	80	37	26	3	4	1	—	—	1	1306

TABLE VII.

1000 *Cambridge Men.*

Head Length (ins.)

Head Breadth (ins.)	*6·8*	*6·9*	*7*	*7·1*	*7·2*	*7·3*	*7·4*	*7·5*	*7·6*	*7·7*	*7·8*	*7·9*	*8*	*8·1*	*8·2*	*8·3*	*8·4*	*8·5*	*8·6*	Totals
5·5	—	—	—	—	1	—	2	—	—	—	—	—	—	—	—	—	—	—	—	3
5·6	—	—	—	—	2	3	1	4	2	—	—	—	—	—	—	—	—	—	—	12
5·7	1	1	—	1	2	6	5	7	5	8	3	4	—	—	—	—	—	—	—	43
5·8	—	—	2	4	6	7	15	12	12	11	7	3	1	—	—	—	—	—	—	80
5·9	—	—	1	4	6	16	16	24	20	28	7	5	4	—	—	—	—	—	—	131
6	—	1	—	6	14	12	24	40	45	40	28	9	13	4	—	—	—	—	—	236
6·1	—	—	1	3	4	6	11	27	42	40	22	15	10	2	2	—	—	—	—	185
6·2	—	—	1	2	6	3	14	18	22	25	13	14	17	2	4	1	—	—	—	142
6·3	—	—	1	—	1	4	9	8	16	15	15	16	6	7	—	—	—	—	1	99
6·4	—	—	—	—	1	—	2	4	6	6	3	4	7	2	1	1	—	—	—	37
6·5	—	—	—	—	—	1	1	—	3	3	—	2	3	1	1	—	—	—	—	15
6·6	—	—	—	1	—	—	—	1	2	3	—	1	—	2	1	1	—	—	—	12
6·7	—	—	—	—	—	—	—	—	—	1	—	—	—	2	—	—	—	—	—	3
6·8	—	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	—	—	2
Totals	1	2	6	21	43	58	100	145	175	180	98	75	61	22	9	3	—	—	1	1000

TABLE VIII.

1000 *Cambridge Men.*

Height (feet and inches).

Head Breadth (ins.)	5′ *1½–2½*	*2½–3½*	*3½–4½*	*4½–5½*	*5½–6½*	*6½–7½*	*7½–8½*	*8½–9½*	*9½–10½*	*10½–11½*	*11½–6·0½*	6′ *0½–1½*	*1½–2½*	*2½–3½*	*3½–4½*	*4½–5½*	Totals
5·5	—	—	—	—	—	1	1	—	—	0·5	0·5	—	—	—	—	—	3
5·6	0·5	0·5	—	1	1	3	1·5	1·5	0·5	1	1·5	—	—	—	—	—	12
5·7	1	—	—	—	2	5·5	6·5	9	10	5	2·	1	—	—	1	—	43
5·8	—	3	4	6	11	13·5	11	12	9·5	5·5	3·5	—	—	—	1	—	80
5·9	2	4	3·5	4·5	14	16	19	20·5	22	8·5	12·5	2	1·5	1	—	—	131
6·	—	4	6	5·5	22·5	32·5	42·5	38	26	28·5	8·5	14	6	1	1	—	236
6·1	—	3	5	9	12	27	22·5	35	24·5	23·5	8·5	4·5	8	2·5	—	—	185
6·2	0·5	3·5	4	6·5	11·5	12·5	15·5	28	24·5	16	5·5	7	2	3	1·5	0·5	142
6·3	—	—	2	7	5	11·5	9·5	22	12·5	13·5	4·5	8·5	—	2·5	0·5	—	99
6·4	—	1	—	—	3	0·5	5	8	4	4	4·5	6	1	—	—	—	37
6·5	—	—	—	—	—	—	1	4	3	1	1	2·5	0·5	2	—	—	15
6·6	—	—	—	1	1·5	0·5	4	1	—	1	1	2	—	—	—	—	12
6·7	—	—	—	—	1	—	—	—	—	—	—	—	2	—	—	—	3
6·8	—	—	—	—	—	—	—	—	2	—	—	—	—	—	—	—	2
Totals	4	19	24·5	40·5	84·5	123·5	139	179	138·5	108	53·5	47·5	21	12	5	0·5	1000

TABLE IX.

1000 *Cambridge Men.*

Head Length (inches).

Height (feet and inches).	*6·8*	*6·9*	*7*	*7·1*	*7·2*	*7·3*	*7·4*	*7·5*	*7·6*	*7·7*	*7·8*	*7·9*	*8*	*8·1*	*8·2*	*8·3*	*8·4*	*8·5*	*8·6*	Totals
5′ *1½—2½*	—	1	—	—	0·5	—	—	2	0·5	—	—	—	—	—	—	—	—	—	—	4
2½—3½	—	—	1	2	2·5	2	2	4·5	2·	2	1	—	—	—	—	—	—	—	—	19
3½—4½	—	—	—	1	2	—	5·5	6	5·5	4·5	—	—	—	—	—	—	—	—	—	24·5
4½—5½	—	—	0·5	1	3	4	3	6·5	4·5	4·5	6·5	7	—	—	—	—	—	—	—	40·5
5½—6½	1	—	2·5	5·5	1	6	10·5	14	21	11	7	1·5	2	1	0·5	—	—	—	—	84·5
6½—7½	—	—	—	1·5	8·5	10	14·5	20	21·5	19·5	12	7·5	6	1	1·5	—	—	—	—	123·5
7½—8½	—	1	—	2·5	8·5	6·5	22·5	24·5	29	24	9·5	4	3	3	1	—	—	—	—	139
8½—9½	—	—	0·5	5·5	4	8·5	16·5	24·5	31·5	36·5	14	21·5	12	3	1	—	—	—	—	179
9½—10½	—	—	0·5	2	4	8·5	15	13·5	21·5	26	14·5	13·5	13	4	1·5	1	—	—	—	138·5
10½—11½	—	—	—	—	3	7·5	3·5	12	22·5	25·5	16	10	6·5	1	0·5	—	—	—	—	108
11½—6·0½	—	—	—	—	3	4	5	6·5	8·5	9	5·5	3	5	3	1	—	—	—	—	53·5
6′ *0½—1½*	—	—	1	—	2	1	2	6	2	8·5	7·5	5	5	3	1·5	2	—	—	1	47·5
1½—2½	—	—	—	—	—	—	—	3	2	4·5	2·5	1	5·5	2	0·5	—	—	—	—	21
2½—3½	—	—	—	—	1	—	—	—	2	4·5	1	—	3	0·5	—	—	—	—	—	12
3½—4½	—	—	—	—	—	—	—	1·5	1	—	1	1	—	0·5	—	—	—	—	—	5
4½—5½	—	—	—	—	—	—	—	0·5	—	—	—	—	—	—	—	—	—	—	—	0·5
Totals	1	2	6	21	43	58	100	145	175	180	98	75	61	22	9	3	—	—	1	1000

TABLE XI.

Height (ins.)

Head Breadth (cms.)	under *$64\frac{9}{16}$*	*$64\frac{9}{16}$* to *$66\frac{9}{16}$*	over *$66\frac{9}{16}$*	Totals
14·8 and under	455	323	299	1077
14·9 to *15·2* ...	316	277	313	906
over *15·2* ...	283	320	414	1017
Totals	1054	920	1026	3000

TABLE XII.

Height (ins.)

L. M. Finger (cms.)	under *$64\frac{9}{16}$*	over *$64\frac{9}{16}$*	Totals
11·5 and under	849	665	1514
over *11·5* ...	205	1281	1486
Totals	1054	1946	3000

TABLE XIII.

Height (ins.)

L. M. Finger (cms.)	under *$64\frac{9}{16}$*	*$64\frac{9}{16}$* to *$66\frac{9}{16}$*	over *$66\frac{9}{16}$*	Totals
11·1 and under	515	149	42	706
11·2 to *11·9* ...	504	607	501	1612
over *11·9* ...	35	164	483	682
Totals	1054	920	1026	3000

TABLE XIV.

Head Length (cms.)

Head Breadth (cms.)	*19·1* and under	over *19·1*	Totals
14·8 and under	705	372	1077
over *14·8* ...	750	1173	1923
Totals	1455	1545	3000

TABLE XV.

Head Length (cms.)

Head Breadth (cms.)	*18·5* and under	*18·6* to *19·7*	over *19·7*	Totals
14·8 and under	279	728	70	1077
14·9 to *15·2* ...	113	670	123	906
over *15·2* ...	71	645	301	1017
Totals	463	2043	494	3000

TABLE XVI.

Head Length (cms.)

Face Breadth (cms.)	*19·1* and under	over *19·1*	Totals
13·6 and under	921	579	1500
over *13·6* …	534	966	1500
Totals	1455	1545	3000

TABLE XVII.

Head Length (cms.)

Face Breadth (cms.)	*18·8* and under	*18·9* to *19·3*	over *19·3*	Totals
13·4 and under	453	357	229	1039
13·5 to *13·8* …	259	335	328	922
over *13·8* …	146	333	560	1039
Totals	858	1025	1117	3000

TABLE XVIII.

Head Length (cms.)

Left Cubit (cms.)	*19·1* and under	over *19·1*	Totals
44·9 and under	836	583	1419
over *44·9* …	619	962	1581
Totals	1455	1545	3000

TABLE XIX.

Head Length (cms.)

Left Cubit (cms.)	*18·8* and under	*18·9* to *19·3*	over *19·3*	Totals
44 and under	375	310	226	911
44·1 to *45·7*	264	365	373	1002
over *45·7* …	219	350	518	1087
Totals	858	1025	1117	3000

TABLE XX.

Head Length (cms.)

Left Foot (cms.)	*19·1* and under	over *19·1*	Totals
25·5 and under	832	545	1377
over *25·5* …	623	1000	1623
Totals	1455	1545	3000

TABLE XXI.

Head Length (cms.)

Left Foot (cms.)	*18·8* and under	*18·9* to *19·3*	over *19·3*	Totals
25 and under	378	300	205	883
25·1 to *26* ...	268	345	366	979
over *26* ...	212	380	546	1138
Totals	858	1025	1117	3000

TABLE XXII.

Head Length (cms.)

L. M. Finger (cms.)	*19·1* and under	over *19·1*	Totals
11·5 and under	880	634	1514
over *11·5* ...	575	911	1486
Totals	1455	1545	3000

TABLE XXIII.

Height (ins.)

Head Length (cms.)	$64\frac{9}{16}$ and under	over $64\frac{9}{16}$	Totals
19·1 and under	665	790	1455
over *19·1* ...	389	1156	1545
Totals	1054	1946	3000

TABLE XXIV.

Head Breadth (cms.)

Face Breadth (cms.)	*14·8* and under	over *14·8*	Totals
13·6 and under	834	666	1500
over *13·6* ...	243	1257	1500
Totals	1077	1923	3000

TABLE XXIV^a.

Head Breadth (cms.)

Face Breadth (cms.)	*15* and under	over *15*	Totals
13·6 and under	1077	423	1500
over *13·6* ...	451	1049	1500
Totals	1528	1472	3000

TABLE XXV.

Head Breadth (cms.)

L. M. Finger (cms.)	*14·8* and under	over *14·8*	Totals
11·5 and under	611	903	1514
over *11·5* ...	466	1020	1486
Totals	1077	1923	3000

TABLE XXV^a.

Head Breadth (cms.)

L. M. Finger (cms.)	*15* and under	over *15*	Totals
11·5 and under	841	673	1514
over *11·5* ...	687	799	1486
Totals	1528	1472	3000

TABLE XXVI.

Head Breadth (cms.)

Left Cubit (cms.)	*14·8* and under	over *14·8*	Totals
44·9 and under	570	849	1419
over *44·9* ...	507	1074	1581
Totals	1077	1923	3000

TABLE XXVII.

Left Foot (cms.)

Head Breadth (cms.)	*25·5* and under	over *25·5*	Totals
14·8 and under	587	490	1077
over *14·8* ...	790	1133	1923
Totals	1377	1623	3000

TABLE XXVIII.

Face Breadth (cms.)

L. M. Finger (cms.)	*13·6* and under	over *13·6*	Totals
11·5 and under	913	601	1514
over *11·5* ...	587	899	1486
Totals	1500	1500	3000

TABLE XXIX.

Face Breadth (cms.)

Left Cubit (cms.)	*13·6* and under	over *13·6*	Totals
44·9 and under	849	570	1419
over *44·9* ...	651	930	1581
Totals	1500	1500	3000

TABLE XXX.

Face Breadth (cms.)

Left Foot (cms.)	*13·6* and under	over *13·6*	Totals
25·5 and under	865	512	1377
over *25·5* ...	635	988	1623
Totals	1500	1500	3000

TABLE XXXI.

Face Breadth (cms.)

Height (ins.)	*13·6* and under	over *13·6*	Totals
$64\frac{9}{16}$ and under	683	371	1054
over $64\frac{9}{16}$ ...	817	1129	1946
Totals	1500	1500	3000

TABLE XXXII.

Left Foot (cms.)

L. M. Finger (cms.)	*25·5* and under	over *25·5*	Totals
11·5 and under	1103	411	1514
over *11·5* ...	274	1212	1486
Totals	1377	1623	3000

TABLE XXXIII.

L. M. Finger (cms.)

Left Cubit (cms.)	*11·5* and under	over *11·5*	Totals
44·9 and under	1196	223	1419
over *44·9* ...	318	1263	1581
Totals	1514	1486	3000

TABLE XXXIV.

Left Foot (cms.)

Left Cubit (cms.)	*25·5* and under	over *25·5*	Totals
44·9 and under	1089	330	1419
over *44·9* ...	288	1293	1581
Totals	1377	1623	3000

TABLE XXXV.

Height (ins.)

Left Cubit (cms.)	$65\frac{9}{16}$ and under	over $65\frac{9}{16}$	Totals
44·9 and under	1158	261	1419
over *44·9* ...	358	1223	1581
Totals	1516	1484	3000

TABLE XXXV^a.

Height (ins.)

Left Cubit (cms.)	$64\frac{9}{16}$ and under	over $64\frac{9}{16}$	Totals
44·9 and under	902	517	1419
over *44·9* ...	152	1429	1581
Totals	1054	1946	3000

TABLE XXXVI.

Height (ins.)

Left Foot (cms.)	$65\frac{9}{16}$ and under	over $65\frac{9}{16}$	Totals
25·5 and under	1088	289	1377
over *25·5* ...	428	1195	1623
Totals	1516	1484	3000

TABLE XXXVI^a.

Height (ins.)

Left Foot (cms.)	$64\frac{9}{16}$ and under	over $64\frac{9}{16}$	Totals
25·5 and under	873	504	1377
over *25·5* ...	181	1442	1623
Totals	1054	1946	3000

MENDEL'S LAWS OF ALTERNATIVE INHERITANCE IN PEAS.

BY W. F. R. WELDON.

[Received December 9, 1901.]

IF two plants, or two animals, of different characters be allowed to breed together, the parental characters may affect the offspring in any one of three ways. In the most usual case, the characters by which the parents differ may appear so intimately blended in the offspring that each young animal or plant appears intermediate in character between its parents, but it is not generally possible for us at present to resolve its body into separate elements, some of which resemble one parent, and some the other. In other cases, however, the body of the young is easily divisible into regions, in some of which the character of one parent is presented in a recognisable and often apparently unaltered state, while the rest of the body presents a similar resemblance to the other parent. In the cases of the third class the body of the offspring may entirely resemble that of one parent, the characters of the other being apparently unrepresented. While it is perfectly possible and indeed probable that the difference between these three forms of inheritance is only one of degree, it is still convenient to discuss them separately.

The work of Galton (No. 13) and Pearson (No. 24) has given us an expression for the effects of *blended* inheritance which seems likely to prove generally applicable, although the constants of the equations which express the relation between divergence from the mean in one generation and that in another may require modification in special cases. Our knowledge of *particulate* or mosaic inheritance, and of *alternative* inheritance, is however still rudimentary, and there is so much contradiction between the results obtained by different observers, that the evidence available is difficult to appreciate. It is the purpose of this essay to describe some cases of alternative inheritance, which have lately excited attention, and to discuss the contradictory evidence concerning them.

I.—*Mendel's Results with Peas.*

In 1865 Gregor Mendel (No. 21) described the results of crossing various races of the common Pea. He does not waste time in discussing the question whether all his races belong to one "species" or not, but describes the result of crossing any two of them as "hybrid." It is probable, however, that all his races belonged to what is now called *Pisum sativum,* to the exclusion of *Pisum arvense.*

The races used differed, no doubt, in many ways: but special attention is paid to seven sets of characters, with regard to each of which it was possible to separate the races into two categories. Thus the shape of the seeds might be round, with only slight and shallow wrinkles on the surface, or irregular and deeply wrinkled. The cotyledons of the seeds might be yellow or green in colour, and so on. The pairs of characters, recognised in this way for each organ or set of organs studied, are distinguished, according to their power of affecting hybrid offspring, into *dominant* and *recessive.* The characters presented by a race are not necessarily all "dominant," or all "recessive"; thus the character of roundness in seeds is dominant, that of wrinkled irregular shape recessive; yellow colour of cotyledons is dominant, and green recessive; but a race of peas may have smooth and rounded seeds with green cotyledons, or yellow cotyledons and wrinkled seeds.

The first general result obtained by Mendel may be stated as follows: *If peas of two races be crossed, the hybrid offspring will exhibit only the dominant characters of the parents; and it will exhibit these without (or almost without) alteration, the recessive characters being altogether absent, or present in so slight a degree that they escape notice.*

This may be called the Law of Dominance, and it at once explains the terms dominant and recessive.

The second result is that*: If the hybrids of the first generation, produced by crossing two races of peas which differ in certain characters, be allowed to fertilise themselves, all possible combinations of the ancestral race-characters will appear in the second generation with equal frequency, and these combinations will obey the Law of Dominance, so that characters intermediate between those of the ancestral races will not occur.*

From its consequences, this may be called the Law of Segregation.

The significance of these results may most easily be seen by considering an example. It has been said that yellowness of cotyledons is dominant over greenness of cotyledons. That is to say, if a plant from a race with yellow cotyledons and one from a race with green cotyledons be cross-fertilised, the resulting seeds will have yellow cotyledons, no matter which plant be used as the female parent. Mendel chose ten plants, some from a race with green and some from a race with yellow cotyledons, and upon these plants he made 58 crosses, so that the ♀ parent was sometimes of green-seeded, sometimes of yellow-seeded race. The resulting seeds had always yellow cotyledons. From these seeds

258 plants were grown, which produced 8023 seeds; and these seeds had the characters indicated by the law of segregation. The combinations of pure ancestral character, which are here possible, are four; since the gamete of either sex may conceivably be capable of transmitting either "yellowness" or "greenness" of cotyledon; Mendel's assumption is that every gamete inherits only one of these "alternative" characters, half the gametes of either sex inheriting each character, or the power of transmitting it, so that there may conceivably be the following combinations:

♀ yellow × yellow ♂
♀ yellow × green ♂
♀ green × yellow ♂
♀ green × green ♂.

But by the law of dominance, if unlike gametes unite, the resultant seed has the dominant character, while if similar gametes unite the resultant seed has naturally the character transmitted by both its constituent gametes. It follows that three of the above combinations give rise to seeds with yellow cotyledons, and only one to green seeds; and if each combination occurs equally often, the chance that a given hybrid seed of the second generation will be yellow is $\frac{3}{4}$, the chance that it will be green is $\frac{1}{4}$.

Now, of the 8023 seeds of Mendel's second generation, 6022 were yellow and 2001 green. Seeds of intermediate colour did not occur. The ratio between either of these numbers and the number of seeds observed is an excellent approximation to that required by Mendel's law of segregation*. The plants of the first hybrid generation invariably bore seeds of both colours, and as a rule seeds of both colours were associated in the same pod. Pods containing only yellow seeds did occur, pods with green seeds only did not. This is all in accordance with the law of segregation; for the number of peas per pod being from six to nine, the chance of getting a pod with yellow seeds only is from $(\frac{3}{4})^6$ for pods with six seeds to $(\frac{3}{4})^9$ for pods with nine seeds, or from 0·18 to 0·075; so that about 18 per cent. of smaller, and about 8 per cent. of larger pods, should contain only yellow seeds; but the chance of a pod containing even six green and no yellow seeds is only $(\frac{1}{4})^6 = \frac{1}{4096}$. Assuming that there were some 1500 pods on the plants, it is clear that the absence of pods without yellow seeds is in good accord with Mendel's law of dominance.

The third hybrid generation must, if the foregoing statements are true, be heterogeneous, for the hybrid seeds of the second generation are said to be of three kinds; those formed by the union of two gametes each transmitting only dominant characters, those formed from gametes each transmitting only recessive characters, and those formed by the union of dissimilar gametes. Therefore the offspring of these seeds should also be of three kinds; in the case before us, one kind, produced from yellow seeds, should give rise to yellow-seeded plants, like

---

* See below, Table I. p. 233.

those of their pure-bred yellow-seeded ancestors; one kind, produced from green seeds, should give rise to apparently pure-bred green-seeded plants; while the third kind, being yellow seeds produced by the union of one gamete with "dominant" and one with "recessive" properties, should give rise to plants identical with the hybrids of the first generation. This result was actually obtained. Those seeds of the second hybrid generation, which had green cotyledons, gave rise to plants which in turn produced only green seeds. From the yellow seeds of the second hybrid generation 519 plants were raised, of which 166 produced only yellow seeds, behaving therefore like pure-bred plants of yellow race, and 353 had both yellow and green seeds, in the proportion of three yellow to one green, as in the first hybrid generation. The observations were continued through six generations; and the descendants of those plants of the second generation which produced only one kind of seed remained throughout apparently pure-bred, producing each its proper kind of seed as regularly as its pure-bred ancestors; the hybrids of every generation behaved exactly like those of the first or second generation. The result of this process is that the percentage of hybrids diminishes in every generation: for suppose each plant in each generation to produce four seeds only, the plants of the first hybrid generation will produce one apparently pure-bred yellow-seeded and one apparently pure-bred green-seeded plant, with two hybrids; in the third generation there will be four apparently pure-bred yellow plants from the apparently pure yellow-seeded plant of the second generation, and two others, one from each hybrid; there will in the same way be six apparently pure green-seeded plants, and four hybrids. Assuming this process to go on, the numbers in successive generations will be:

Generation	Yellow	Hybrid	Green
1		1	
2	1	2	1
3	6	4	6
4	28	8	28
5	120	16	120
6	496	32	496
etc.			

So that the number of apparently pure-bred forms of each kind increases, and in the $(n+1)$th hybrid generation, the three classes of plants are in the proportions of $2^n - 1 : 2 : 2^n - 1$.

Results closely similar to these were obtained with seven sets of differential characters; namely

1. *The shape of the seed* (round and feebly wrinkled, *dominant*; irregular and deeply wrinkled, *recessive*).

2. *The colour of the cotyledons,* already dealt with.

3. *The colour of the seed-coat* (grey, grey-brown, or leather-coloured, with or without violet dots, associated with purple-violet flowers and red colouring round leaf-axils, *dominant*; white, associated with white flowers, *recessive*).

4. *The shape of the ripe pod* (not constricted, smooth, *dominant*; constricted between the seeds, *recessive*).

5. *The colour of the unripe pod* (green, associated with green stem, mid-ribs of leaves, and calyx, *dominant*; yellow, associated with similar colouring of stem, mid-ribs and calyx, *recessive*).

6. *The distribution of the flowers* (scattered along the axis, *dominant*; gathered into a short umbel-like cluster, at the extremity of the axis, *recessive*).

7. *The length of the main stem* (tall, *dominant*; short, *recessive*).

The only qualifications Mendel offers, in applying his general statements to these very varied characters, are (1) that the violet dots on the seed-coat are often more numerous and larger in hybrids than in pure-bred forms, and (2) the observation that the mere fact of hybridisation produces an increase in the size of the vegetative organs, so that hybrid plants are often taller than either of their parents, an observation made previously by Knight (No. 18) for peas, and by many later naturalists for peas and other plants (see the summary of the evidence by Darwin, No. 9, Vol. II. Chap. 17, and in addition to the authorities there cited, Naudin, No. 22. For extensive observations on Peas, see Tschermak, Nos. 27 and 28).

It is clearly important to test these remarkable statements by a careful study of the numerical results, and by the application of such tests as may be possible. It seems to me that by neglecting these precautions some writers have been led to overlook the wonderfully consistent way in which Mendel's results agree with his theory, saying that his numbers "are not large enough to give really smooth results," and while they thus unwillingly do rather less than justice to Mendel's own work, at the same time they accept results which seem really inconsistent with those obtained by Mendel as proof that his statements are applicable to a wider range of cases than those he actually observed.

Mendel's observations fall into two groups; the first group relates to a series of cross-fertilisations, in which each pair of races crossed differed in only one of the seven sets of characters dealt with; the second group contains observations on races which differed in two or more sets of characters.

The observations of the first group are more numerous than those of the second, and may be considered first.

The seven sets of observations, showing the dominance of one character in the first hybrid generation, must of course rest upon Mendel's statement, which I think no one who reads his paper will find the slightest difficulty in accepting. The behaviour of hybrids of the second generation can be tested from the numbers which are given. From what has been said concerning yellow and green cotyledons, it is evident that the chance of getting a dominant character in a plant of the second hybrid generation is the chance that one or other of three combinations, out of four which are all possible and equally probable, occurred during the fusion of the gametes which gave rise to the plant. The chance that a given plant of the second hybrid generation will present dominant characters is therefore $\frac{3}{4}$; and the records may be treated as attempts to verify this experimentally. Now if a series of n things be observed, and the chance that any one of them has a particular character is $\frac{3}{4}$, the "probable error" of the expectation that $\frac{3}{4}n$ of the things observed will show this character is well known to be $0{\cdot}67449\sqrt{n \times \frac{3}{4} \times \frac{1}{4}}$; so that if a number of such series are observed, in half of them the frequency with which the desired character occurs will lie between the limits $\frac{3}{4}n \pm 0{\cdot}67449\sqrt{n \times \frac{3}{4} \times \frac{1}{4}}$, and in half of them it will lie outside these limits. In each of Mendel's records of a second hybrid generation the probable error has been calculated, and the result is shown in the following table:

TABLE I.

Individuals with Dominant Characters in the Second Hybrid Generation.

Characters Crossed	Individuals of Second Hybrid Generation	Number of Dominant Individuals	Dominant Individuals on Mendel's Theory	Probable Error of Theory	Deviation of Observation from Theory
1. (Shape of Seeds) ...	7324	5474	5493	±24·995	−19
2. (Colour of Cotyledons)...	8023	6022	6017·25	±26·160	+ 4·75
3. (Colour of Seed Coats)...	929	705	696·75	± 8·902	+ 8·25
4. (Shape of Pod)	1181	882	885·75	±10·037	− 3·75
5. (Colour of Pod)... ...	580	428	435	± 7·034	− 7
6. (Distribution of Flowers)	858	651	643·5	± 8·555	+ 7·5
7. (Height of Plant) ...	1064	787	798	± 9·527	−11

Here are seven determinations of a frequency which is said to obey the law of Chance. Only one determination has a deviation from the hypothetical frequency greater than the probable error of the determination, and one has a deviation sensibly equal to the probable error; so that a discrepancy between the hypothesis and the observations which is equal to or greater than the probable error occurs twice out of seven times, and deviations much greater than the probable error do not occur at all. These results then accord so remarkably with Mendel's summary of them that if they were repeated a second time, under similar conditions and on a similar scale, the chance that the agreement between observation and hypothesis would be worse than that actually obtained is about 16 to 1.

The accuracy with which the theory fits the results obtained in the third hybrid generation may be tested in the same way. The plants of the second generation, which exhibit recessive characters, ought to produce offspring which also exhibit only recessive characters, and Mendel assures us that this was the case; of those which exhibit dominant characters, one-third should produce apparently pure-bred offspring of dominant character, two-thirds should produce "hybrid" offspring. The behaviour of plants of the third generation, the offspring of plants with dominant characters, is shown in Table II.

TABLE II.

Proportion of Plants with Dominant Characters, among Hybrids of the Second Generation, which transmitted only Dominant Characters to their offspring.

Characters Crossed	Number of Second Generation Hybrids observed	Number which transmitted only Dominant Characters	Theoretical number	Probable Error of Theoretical Expectation	Deviation of observed number
1. (Shape of Seeds) ...	565	193	188·3	±7·558	+4·7
2. (Colour of Cotyledons)...	519	166	173	±7·244	−7·0
3. (Colour of Seed-Coat) ...	100	36	33·3	±3·180	+2·7
4. (Shape of Pods)	100	29	33·3	±3·180	−4·3
5. (Colour of Pods)	100	40	33·3	±3·180	+6·7
6. (Distribution of Flowers)	100	33	33·3	±3·180	−0·3
7. (Height of Plant) ...	100	28	33·3	±3·180	−5·3

Three of these seven determinations give results which differ from that indicated by Mendel's hypothesis by less than their probable errors; the difference between the actual result and that postulated by the hypothesis is equal to the probable error in one case, and greater in three cases; but a discrepancy as great as twice the probable error only occurs once. Here again, therefore, Mendel's statement is admirably in accord with his experiment.

Mendel made several series of observations to test the validity of his statement in cases involving more than one pair of differential characters. The number of possible combinations quickly becomes too great to deal with experimentally, and the most complicated case recorded is that of hybrids between female parents of a race producing round smooth seeds with yellow cotyledons and grey-brown seed-coats, and male parents of a race with angular green seeds and white seed-coats. The original hybrids were 24 in number, and from these 639 hybrids of the second generation were grown and observed. Denoting the dominant characters, roundness, yellowness, and greyness of seed-coat, by A, B, C, and the corresponding recessive characters, angularity, greenness, and whiteness of seed-coat, by a, b, and c respectively, it is clear that the possible combinations of these characters in the hybrids of the second generation are the possible combinations of the three sets of characters

$$AA + aA + Aa + aa,$$
$$BB + bB + Bb + bb,$$
$$CC + Cc + cC + cc.$$

Since there are four possible combinations of each pair of characters, the number of possible combinations of the three pairs of characters is clearly $4^3 = 64$; and if we consider the union of male dominant with female recessive gametes to be the same as that of female dominant and male recessive, the number of different combinations is 27. Of these 27 different combinations, eight will each occur once, 12 will each occur twice, six will each occur four times, and one will occur eight times, in 64. The combinations actually deduced from the behaviour of the hybrids of the second generation and their offspring occurred with the following frequencies in the 639 plants:

TABLE III.

Frequency of the various possible combinations of Characters in Hybrids of the Second Generation from Races which differed in three Characters.

Combination	Frequency	Combination	Frequency	Combination	Frequency
AABBCC	8	*AABBCc*	22		
AABBcc	14	*AAbbCc*	17	*AABbCc*	45
AAbbCC	9	*aaBBCc*	25	*aaBbCc*	36
AAbbcc	11	*aabbCc*	20	*AaBBCc*	38
aaBBCC	8	*AABbCC*	15	*AabbCc*	40
aaBBcc	10	*AABbcc*	18	*AaBbCC*	49
aabbCC	10	*aaBbCC*	19	*AaBbcc*	48
aabbcc	7	*aaBbcc*	24		
		AaBBCC	14	*AaBbCc*	78
		AaBBcc	18		
		AabbCC	20		
		Aabbcc	16		

These numbers are all correlated, so that the system of results must be judged as a whole. Applying the method of Pearson (No. 25) the chance that a system will exhibit deviations as great as or greater than these from the result indicated by Mendel's hypothesis is about 0·95 (see Elderton, this Journal, *ante*, p. 161), or if the experiment were repeated a hundred times, we should expect to get a worse result about 95 times, or the odds against a result as good as this or better are 20 to 1.

Mendel's statements are based upon work extending over eight years. The remarkable results obtained are well worth even the great amount of labour they must have cost, and the question at once arises, how far the laws deduced from them are of general application. It is almost a matter of common knowledge that they do not hold for all characters, even in Peas, and Mendel does not suggest that they do. At the same time I see no escape from the conclusion that they do not hold universally for the characters of Peas which Mendel so carefully describes. In trying to summarise the evidence on which my opinion rests, I have no wish to belittle the importance of Mendel's achievement. I wish simply to call attention to a series of facts which seem to me to suggest fruitful lines of enquiry.

II.—*Other Evidence concerning Dominance in Peas.*

It is certain that an alternative inheritance, which may produce something like Mendel's phenomenon of segregation, occurs as a result of crossing races of animals and plants, when nothing comparable with dominance can be observed in the immediate offspring of the cross. The two phenomena must therefore be considered separately (cf. Correns, No. 5).

The evidence concerning dominance in the first hybrid generation of Peas relates chiefly to the colour of cotyledons and seed-coat, and to the shape of the hybrid seed. In judging it, we must be careful to realise what the statement, that a character is dominant, really means. Many races of Peas are exceedingly variable, both in colour and in shape. A race with "round smooth" seeds, for example, does not produce seeds which are exactly alike; on the contrary, many seeds of such a race as *Victoria*, used by Rimpau (No. 26) as a typically round and smooth-seeded Pea, or *Express* used by Tschermak (No. 27) in the same way, show very considerable irregularities; while in races such as *Prince of Wales* or *Telephone*, used by Tschermak and others as types of races with wrinkled seeds, hardly any two seeds are alike. So that both the category "round and smooth" and the category "wrinkled and irregular" include a considerable range of varieties. At the same time, the categories are undoubtedly often discontinuous, the most wrinkled seed of such a race as *Express* or *Victoria* being so much smoother and more rounded than the most regular seed of the typically "wrinkled" races, that no one who knows both races would hesitate for a moment in deciding which race a given seed resembled.

The statement that smoothness of seed is dominant over the production of wrinkles means therefore that if a parent, belonging to a variable race which falls into the category "smooth-seeded," be crossed with a parent belonging to a variable race of the category "with wrinkled seeds," the offspring will themselves be variable, but will always belong to the category "smooth-seeded": and as it is with shape, so it is with colour.

The first detailed account of the *colour* in hybrid Peas, which I have been able to find, is that of Goss, 1848 (No. 15), who fertilised the flower of a "blue-seeded" race with pollen of a race with yellowish white seeds. The hybrid seeds were all yellowish white; and the plants raised from them "produced some pods with all blue, some with all white, and many with blue and white seeds in the same pod"; or the result was probably that obtained by Mendel. In a note to this paper, the then Secretary of the Horticultural Society says that Mr Alexander Seton crossed the flowers of *Dwarf Imperial* "a well-known green variety of the Pea," with the pollen of "a white free-growing variety." Four hybrid seeds were obtained, "which did not differ in appearance from the others of the female parent." These seeds therefore did *not* obey the law of dominance, or if the statement be preferred, greenness became dominant in this case. The seeds were sown, and produced plants bearing "green" and "white" seeds side by side in the same pod. An excellent coloured figure of one of these pods is given (*loc. cit.* Plate 9, Fig. 1), and is the only figure I have found which illustrates segregation of colours in hybrid Peas of the second generation. In 1849 Gärtner (No. 12, pp. 81—85) described the result of crossing races of Peas; the yellow-seeded *Pariser Wachserbse* which he calls *Pisum sativum luteum*, was fertilised (*a*) with pollen of *P. sativum macrospermum* which had seeds of an impure greenish-yellow, and (*b*) with pollen of the green-seeded *P. sativum viride*. In the first case the hybrid seeds were all pure yellow; in the second case twelve seeds were produced in four hybrid pods; and these were all of a greenish-yellow colour, although the greenish tinge disappeared from some of them on drying. Another yellow-seeded Pea (*P. sativum nanum repens*) fertilised with the pollen of the green-seeded *P. sativum viride* gave five hybrid pods with seeds, of which one contained five dirty green seeds, a second had five seeds which were "not distinctly yellow, but yellowish green," although they lost their greenness after being dried for two months. The others were not yellow like the mother, but "dirty yellow." It is clear that the "greenness" of *P. sativum viride* did in these cases affect the colour of the seeds, when its pollen was used to fertilise plants of yellow-seeded race, though it is difficult to judge exactly how great the effect was. The disappearance of the green colour on drying may have been simply due to the development of an air-space between the seed-coat and the cotyledons, which as Tschermak (No. 27) has pointed out may obscure the colour of the cotyledons in the dry seed, or it may have been due to a real increase in yellowness. The flowers of *P. sativum viride* were fertilised with pollen from two pure yellow-seeded races (*P. sativum luteum* and *P. sativum nanum repens*), and in the first case the seeds produced were "not distinctly yellow, still less blue or green, but dirty yellow." In the second case the seeds produced were yellow; and when *P. sativum viride* was fertilised by *P. sativum macrospermum* the seeds produced were apparently yellower than those of the male parent.

The effect of the dominance of yellowness over greenness of cotyledons in hybrids of the first generation, if it were indeed universal, would be so striking that one can hardly conceive that the great founders of our modern races of Peas, such as Knight (No. 18) and Laxton (No. 19), should have failed to notice it, and yet neither of them, in describing experience gained from crossing great numbers of plants, says that it is of general occurrence.

The colour of seeds of the first hybrid generation has not, so far as I can discover, been carefully described between the time of Mendel and the year 1893, when Giltay (No. 14), who does not appear to have known Mendel's work, crossed several yellow-seeded peas with the green-seeded race *Reading Giant*, and found that the colour of the cotyledons was always yellow, showing that Mendel's law of dominance was completely valid in this case. (Giltay points out the necessity of removing the seed-coats before determining the colour of the cotyledons; this is also emphasised by Tschermak, and I have found it quite necessary even in seeds of pure race.) In 1900 general attention was directed to

Mendel's work, and the result of crossing Peas has lately been described by Correns (Nos. 3 and 4) and in greater detail by Tschermak (Nos. 27 and 28). Correns confirms Mendel's statements concerning the dominance of yellow cotyledons, but Tschermak makes a more detailed statement, which does not so fully agree with Mendel. It is not quite easy to follow Tschermak's account, because he does not describe all his very numerous and careful experiments in such a way that one can be sure how many hybrid peas he observed. He certainly crossed between 80 and 100 flowers, belonging to green or yellow-seeded races, with pollen from plants with seed of the opposite colour, obtaining between 300 and 400 hybrid seeds. Of these hybrid seeds about 40 were not distinctly yellow, so that 90 per cent. of the hybrids exhibited the dominant character. Of the seeds which did not exhibit this character, some were yellow with green patches, eight were green, four were "yellowish green," and five are described as showing "Grün und Uebergänge von Gelb zu Grün," so that while some 10 per cent. of the hybrids did not exhibit dominance of yellow, some 2 or 3 per cent. exhibited a close approximation to the character of the "recessive" parent. A further case is recorded by Tschermak, where a plant of the yellow-seeded variety *Buchsbaum*, growing in the open, produced a pod in which every pea except one was green, the exception showing a little yellow. One of the green seeds was sown, and the plant produced fifteen yellow and three green seeds, thus behaving like a hybrid. Tschermak considers that this is a case of accidental crossing, with dominance of green, and although some of the green peas, produced in his experiments, may have been due to accidental self-fertilisation, he regards some of them, at least, as hybrids (cf. No. 28, pp. 663—664). It should be said that some of the crosses referred to were made between the (yellow-seeded) *P. arvense*, var. *Graue Riesen*, and green-seeded varieties of *P. sativum*. In all such crosses the law of dominance of yellow held absolutely.

These results show clearly enough that the law of dominance is, as Tschermak says, not absolutely true of cotyledon colour, and as will presently be shown, the exceptions to the law, which he observed, form a very large percentage of the total result obtained when certain races were crossed.

The case of cotyledon colour has been considered first, because the evidence with regard to it is more favourable to Mendel than is the evidence touching other seed characters. The *shape* of seeds, whether smooth and rounded or irregular and wrinkled, is even more difficult to express in words than the colour of their cotyledons, and the varieties appear to be even less constant. The evidence against the universal validity of the law of dominance is here much stronger than in the case of cotyledon-colour. Two striking cases were observed by Rimpau (No. 26, pp. 36, 37), who crossed the smooth-seeded race *Victoria* with two wrinkled races, *Knight's Marrow* and *Telephone*. He made each cross in both the possible ways, and found the second hybrid generation dimorphic, as usual. From the cross *Victoria* ♀ × *Knight's Marrow* ♂ he obtained round and wrinkled seeds of the second generation, as Mendel's statements would lead us to expect. The wrinkled seeds were sown, and produced both round and wrinkled seeds, the wrinkled (recessive) character becoming "true" only in the fifth year. The wrinkled seeds of the second generation from the cross *Victoria* ♀ × *Telephone* ♂ also gave rise to plants with rounded and wrinkled seeds; but the descendants of these were not observed. If Mendel's law of segregation was here followed, his law of dominance was not; because if both are valid, a plant of the second hybrid generation which exhibits recessive characters must behave, so far as those characters are concerned, like a pure-bred individual. The most careful account of seeds of the first hybrid generation is that of Tschermak, in the papers already quoted. The variety *Telephone* especially, when crossed with smooth-seeded varieties, gave a large number of wrinkled seeds, or seeds of intermediate character. Thus the *Pois d'Auvergne* used by Tschermak had the ripe seeds "always round and smooth"; but out of 27 seeds recorded from seven crosses between this pea as the ♀ and *Telephone* as ♂ parent, nine were slightly wrinkled, and two out of ten resulting from the reciprocal cross were slightly wrinkled. These are difficult cases to judge; but the cross *Telephone* ♀ × the smooth-seeded *Plein le Panier* ♂ gave seeds which "differed only slightly from those of the mother," although the wrinkles were perhaps not so deep. Again, *Telephone* ♀ × the smooth *grünbleibende Folger* ♂ gave seeds which certainly belonged to the category "wrinkled," although they were more rounded than those of pure *Telephone*. After pointing out the exceptions to dominance of yellow cotyledon-colour already mentioned, Tschermak summarises his view of the dominance of wrinkles as follows: "Auch in Bezug auf die Form fehlt es nicht an Fällen, in denen sich das "sonst dominirende mit dem sonst recessiven Merkmale in einem gewissen "Verhältnisse combinirte. So ergiebt *im Allgemeinen* die Bestäubung einer "gerunzelten Markerbse mit Pollen einer glattsämigen Varietät abgerundete, "schwach gerunzelte Produkte, und zwar eher als die umgekehrte Verbindung "der Eltern."

This is a sufficient statement of Tschermak's result, so far as it concerns races of *P. sativum*. Correns (No. 3) found that the *grüne späte Erfurter Folger-Erbse* which has round smooth seeds, was not dominant in this respect over the *Pahl-Erbse mit purpurrothen Hülsen*.

The foregoing crosses show that the law of dominance does not always hold for the shape of seeds, when the races crossed belong to the species *P. sativum*. The race of *P. arvense* called *Graue Riesen* is held by some botanists to belong to a different "species," and may therefore be considered separately. Tschermak has crossed *Graue Riesen* with five races of *P. sativum*, and he finds that the form of the first hybrid seeds *follows the female parent*, so that if races of *P. sativum* with round smooth seeds be crossed with *Graue Riesen* (which has flattened, feebly wrinkled seeds), the hybrids will be round and smooth or flattened and wrinkled, as the *P. sativum* or the *Graue Riesen* is used as female parent. There is here a more complex phenomenon than at first sight appears; because if the flowers of the first hybrid generation are self-fertilised, the resulting seeds of the second generation invariably resemble those of *P. arvense* in shape, although in colour they follow Mendel's law of segregation!

The discussion of *seed-coat colour* is more difficult than that of the two characters already dealt with, because the recent writers (especially Tschermak) pay attention to a character, namely green pigment in the seed-coat, which Mendel does not mention. It is clearly unreasonable to expect that every character in the seed-coat should obey the same law, and therefore in trying to estimate the agreement between Mendel and later workers, many valuable data collected by Tschermak will be neglected. The most striking exception to the law of dominance is that observed by Correns. The *grüne späte Erfurter Folger-Erbse* has a nearly colourless seed-coat (recessive, Mendel); the *Purpurviolett-schottige Kneifel-Erbse* and the *Pahl-Erbse mit purpurrothen Hülsen* have the seed-coat uniformly orange, becoming brown with age. In hybrids of the first generation, between either of these last varieties and the *Erfurter Folger-Erbse*, the coats of seeds (often in the same pod) were sometimes nearly colourless, sometimes intensely orange-red, but generally more or less orange-red, and spotted more or less strongly with blackish violet. The seeds of extreme colour, those with orange or those with almost colourless seed-coats, gave plants of the second generation which again showed the same extremes of colour in the seed-coats, connected by transitional forms. This is clearly a case in which one of Mendel's characters obeys neither the law of dominance nor the law of segregation.

The appearance of blackish-violet dots on the seed-coats of similar hybrids was noticed by Mendel. In his crosses however one parent showed traces of such dots, which were only exaggerated in the offspring. In the plants used by Correns as parents, the seeds with brown coats are not said to have possessed these dots. An exaggeration of the purple dots, of a kind similar to that observed by Mendel, but even greater in degree, was found by Tschermak in crosses between *Graue Riesen* and varieties of *P. sativum* with colourless seed-coats.

Enough has been said to show the grave discrepancy between the evidence afforded by Mendel's own experiments, and that obtained by other observers, equally competent and trustworthy. It does not seem to me reasonable to doubt the substantial accuracy of any of the statements made by the observers quoted. I have deliberately refrained from collecting the numerous scattered records of odd crosses which exist in the various journals devoted to horticulture, because many of these are published either anonymously or by persons whose skill in performing the difficult operation of crossing in such a way as to be sure of the result may possibly be doubted. The evidence brought together rests upon the statements of men whose knowledge and skill are beyond question, and the only conclusion which can, I think, be fairly drawn from it is, that dominance of any of the characters mentioned is not an invariable attribute of the character, but that a cross between pairs of parents, such that the different members of each pair differ to the same extent in cotyledon colour or in similar characters, may in different cases lead to widely different results, and one great reason why this is so becomes evident on a little consideration.

Mendel treats such characters as yellowness of cotyledons and the like as if the condition of the character in two given parents determined its condition in all their subsequent offspring. Now it is well known to all breeders, and is clearly shown in a number of cases by Galton and Pearson, that the condition of an animal does not as a rule depend upon the condition of any one pair of ancestors alone, but in varying degrees upon the condition of all its ancestors in every past generation, the condition in each of the half-dozen nearest generations having a quite sensible effect. Mendel does not take the effect of difference of ancestry into account, but considers that any yellow-seeded Pea, crossed with any green-seeded Pea, will behave in a certain definite way, whatever the ancestry of the green and yellow peas may have been. [He does not say this in words, but his attempt to treat his results as generally true of the characters observed is unintelligible unless this hypothesis be assumed.] The experiments afford no evidence which can be held to justify this hypothesis. His observations on cotyledon colour, for example, are based upon 58 cross-fertilised flowers, all of which were borne upon ten plants; and we are not even told whether these ten plants included individuals from more than two races. The many thousands of individuals raised from these ten plants afford an admirable illustration of the effect produced by crossing a few pairs of plants of known ancestry; but while they show this perhaps better than any similar experiment, they do not afford the data necessary for a statement as to the behaviour of yellow-seeded peas in general, whatever their ancestry, when crossed with green-seeded peas of any ancestry.

When this is remembered, the importance of the exceptions to dominance of yellow cotyledon-colour, or of smooth and rounded shape of seeds, observed by Tschermak, is much increased; because although they form a small percentage of his whole result, they form a very large percentage of the results obtained with peas of certain races. The fact that *Telephone* behaved in crossing on the whole like a green-seeded race of exceptional dominance shows that something other than the mere character of the parental generation operated in this case. Thus in eight out of 27 seeds from the yellow *Pois d'Auvergne* ♀ × *Telephone* ♂ the cotyledons were yellow with green patches; the reciprocal cross gave two green and one yellow-and-green seed out of the whole ten obtained; and the cross *Telephone* ♀ × (yellow-seeded) *Buchsbaum* ♂ gave on one occasion two green and four yellow seeds.

So the cross *Couturier* (orange-yellow) ♀ × the green-seeded *Express* ♂ gave a number of seeds intermediate in colour. [It is not quite clear from Tschermak's paper whether *all* the seeds were of this colour, but certainly some of them were.] The green *Plein le Panier* ♀ × *Couturier* ♂ in three crosses always gave either seeds of colour intermediate between green and yellow, or some yellow and some green seeds in the same pod. The cross reciprocal to this was not made; but *Express* ♀ × *Couturier* ♂ gave 22 seeds, of which four were yellowish-green.

These facts show *first* that Mendel's law of dominance conspicuously fails for crosses between certain races, while it appears to hold for others; and *secondly* that the intensity of a character in one generation of a race is no trustworthy measure of its dominance in hybrids. The obvious suggestion is that the behaviour of an individual when crossed depends largely upon the characters of its ancestors. When it is remembered that Peas are normally self-fertilised, and that more than one named variety may be selected out of the seeds of a single hybrid pod, it is seen to be probable that Mendel worked in every case with a very definite combination of ancestral characters, and had no proper basis for generalisation about yellow and green peas of any ancestry.

Now in such a case of alternative inheritance as that of human eye-colour, it has been shown that a number of pairs of parents, one of whom has dark and the other blue eyes, will produce offspring of which nearly one half are dark-eyed, nearly one half are blue-eyed, a small but sensible percentage being children with mosaic eyes, the iris being a patchwork of lighter and darker portions. But the dark-eyed and light-eyed children are not equally distributed among all families ; and it would almost certainly be possible, by selecting cases of marriage between men and women of appropriate ancestry, to demonstrate for their families a law of dominance of dark over light eye-colour, or of light over dark. Such a law might be as valid for the families of selected ancestry as Mendel's laws are for his peas and for other peas of probably similar ancestral history, but it would fail when applied to dark and light-eyed parents in general,—that is, to parents of any ancestry who happen to possess eyes of given colour.

This neglect of ancestry, the tendency to regard offspring as resembling their parents rather than their race, accounts for much of the apparent inconsistency between the results obtained by different observers who have crossed plants or animals.

The writer who has most clearly recognised the importance of ancestry in connection with Mendel's work is Correns. In a recent summary of his views (No. 6) and in his fuller account of experiments in crossing races of Maize (No. 7) he says that between the complete equivalence of two characters and the complete dominance of one of them, all intermediate stages may exist, and that the dominance of a character varies (*a*) according to the individuality of different gametes from the same gonad; (*b*) according to the individuality of the different plants of the same race; (*c*) according to the race of the plant. He also points out that the offspring of pure-bred races differ in their power of transmitting "alternative" characters (which Correns, adopting Mendel's theory of the constitution of gametes in this case, calls schizogonic) from the offspring of a cross, although the apparent characters of both may be the same. Correns illustrates and justifies his statements by detailed accounts of experiments with Maize; and his figures of particoloured heads of seed, produced by fertilising a white-seeded female flower with pollen from a single male flower of blue-seeded race are most striking. They show that the result may be either a blue seed, or a white seed, or a piebald white and blue seed, or a seed of uniform but intermediate colour. The peculiar process of double fertilisation to which this change of colour in the extra-embryonic tissues of Maize is due does not seem to me to affect the relevance of the observations.

In order to emphasize the need that the ancestry of the parents, used in crossing, should be considered in discussing the results of a cross, it may be well to give one or two more examples of fundamental inconsistency between different competent observers.

Correns (No. 5) himself has crossed the Stock *Matthiola annua* = *M. incana* D.C., which has its green parts covered with grey hairs, and the smooth-leaved Stock, *M. glabra* D.C. The 111 hybrids obtained from reciprocal crosses all had hairy leaves. Trevor Clarke made a similar cross, using of course different races, and found that half his seedlings had smooth leaves, half had hairy leaves (No. 2).* Again, the petals of the *M. incana* used by Correns were violet, those of the *M. glabra* were yellowish white. The petals of the hybrid seedlings were invariably either violet of the shade of *M. incana*, or violet more or less obviously spotted with pale violet. On the other hand, Nobbe (No. 23) crossed a number of varieties of *M. annua* in which the flowers were white, violet, carmine-coloured, crimson, or dark blue. These were crossed in various ways, and before a cross was made the colour of each parent was matched by a mixture of dry powdered colours, which was preserved. In every case the hybrid flower was of an intermediate colour, which could be matched by mixing the powders which recorded the parental colours. The proportions in which the powders were mixed are not given in each case, but it is clear that the colours blended.

Again, de Vries (Nos. 30 and 31) whose evidence for most of his statements concerning the validity of the law of dominance has not yet been published, crossed *Datura tatula* with the smooth-fruited form of *D. stramonium*. *D. tatula* has blue or violet flowers, and dull purple stems; the flowers of *D. stramonium* are white and its stems bright green. De Vries found that in all his hybrid seedlings the flowers were blue, and he concludes that the colouring of *D. tatula* is dominant in Mendel's sense. Naudin (No. 22) crossed *D. tatula* with the variety of *D. stramonium* which has spiny fruit and white flowers. The flowers of his hybrids were violet, but paler than those of *D. tatula*, and their stems were flushed with purple, but to a less extent than those of *D. tatula*; so that in his crosses a blending of the parental colours occurred. Again, de Vries found that the fruit of his hybrids was spiny, and concludes that the production of spines is a dominant character. Naudin crossed the smooth-fruited and the spiny-fruited forms of *D. stramonium* (which he called distinct "species") and found that among the fruits of forty hybrids some were completely spiny; others were smaller than the normal fruits of the spiny form (in this resembling the smooth form), while their spines were shorter and weaker than in the pure spiny form; others again were a

---

* According to Bateson (*Journ. Roy. Hort. Soc.* Vol. XXIV. p. 64), Miss Saunders has obtained a similar result. Correns doubts whether Trevor Clarke's flowers were completely cross-fertilised.

mosaic of smooth and spiny segments. On the other hand, *D. stramonium* of the spiny form, crossed with *D. ceratocaula*, which has a smooth fruit of different character, gave hybrids whose fruits exactly resembled those of *D. stramonium*.

Examples might easily be multiplied, but as before, I have chosen rather to cite a few cases which rest on excellent authority, than to quote examples which may be doubted. I would only add one case among animals, in which the evidence concerning the inheritance of colour is affected by the ancestry of the varieties used. Many people have crossed the various white, piebald and wild-coloured varieties of the rat (*Mus decumanus*), the closely similar varieties of the mouse (*Mus musculus*) have also been frequently crossed. In both rats and mice von Fischer (Nos. 10 and 11) says that piebald rats crossed with albino varieties of their species, give piebald young if the father only is piebald, white young if the mother only is piebald. Crampe (No. 8) finds that in either case the offspring are a mixture of piebald forms and albinos. Results such as those which Crampe records in rats are commonly obtained when piebald and albino mice are paired; but both Haacke (No. 17) and von Guaita (No. 16) find that when the ordinary European albino mouse is paired with the piebald Japanese "dancing" mouse, the offspring are either like wild mice in colour, or almost completely black. Again, Crampe says that when white and wild-coloured mice are crossed the offspring are invariably coloured like wild mice. Colladon is reported by Prévost and Dumas (*Ann. Sci. Nat.* I., 1824) to have obtained both albino and wild-coloured individuals from similar crosses, but no piebald individuals.

These examples, chosen from many others which might have been cited, seem to me to show that it is not possible to regard dominance as a property of any character, from a simple knowledge of its presence in one of two individual parents. The degree to which a parental character affects offspring depends not only upon its development in the individual parent, but on its degree of development in the ancestors of that parent. A collection of cases which illustrate this point is given by Bateson (No. 1).

III.—*The Hybrid Peas of the Telephone Group.*

If Mendel's statements were universally valid, even among Peas, the characters of the seeds in the numerous hybrid races now existing should fall into one or other of a few definite categories, which should not be connected by intermediate forms. In attempting to follow the results obtained by Tschermak and others I have carefully examined the seed characters of some twenty named varieties, and the present condition of many I have studied seems to me quite incompatible with the general validity of Mendel's statements. The aberrant behaviour of the race called *Telephone* has already been observed by Rimpau and Tschermak, and I have therefore endeavoured to learn the past history and the present condition of this Pea and the races allied to it. In my attempt I have received very great help from Messrs Carter and Co., who originated *Telephone* and its allies, from Messrs Sutton and Sons, and from Messrs Vilmorin-Andrieux and Co., of Paris. All these gentlemen have supplied me with quantities of seeds of various races, and have spent much time and trouble in answering questions concerning them. I gladly take this opportunity of expressing my gratitude to all of them, and especially to Messrs Carter and Co., the value of whose help will be presently apparent.

In 1876 Culverwell introduced into the English market a Pea which he called *Telegraph*. This was a hybrid race, and he says that his stock was derived from a single cross-fertilised flower. The stock of *Telegraph* was ultimately bought by Carter and Co., who found it so variable that without further crossing they produced from it, by simple selection, the four races now known as *Telephone, Stratagem, Pride of the Market*, and *Duke of Albany*. These four races, together with *Telegraph*, are still cultivated, and I have examined them all. Their history is well known, and is authenticated not only by Carter's records, but by letters of Culverwell, and by Laxton (No. 20). Between 1880 and 1890 Carter and Co. crossed *Stratagem* with *Giant Marrow*, another of Culverwell's races; and the offspring of this cross was introduced into the market in 1892 as *Daisy*. A few years later the race *Daisy* was crossed with *Lightning*, a Pea which has round, nearly smooth seeds, and yellow cotyledons. The result of this cross is known as *Early Morn*. Culverwell's *Giant Marrow* is not now easy to obtain; but all the other races referred to are grown commercially, on a large scale, and I have examined the characters of their seeds.

In attempting to judge the results of other observers, including those of Mendel himself, I have constantly found it difficult to understand the statements made, because of the vagueness of the terms used to describe shape and colour. In order to make my own statements about colour as intelligible as may be, I selected from a sample of *Telephone* grown by Carter a series of 18 peas, which show, after removal of the seed-coats, a fairly gradual series of transitional colours from a deep green to an orange yellow. These seeds were arranged in groups of three, so that six colour-groups were formed; and these groups, numbered from one to six, the first being green and the sixth orange-yellow, form a scale in terms of which I shall express the colour of all the peas described. The groups are represented in Figs. 1—6 on Plate I. [Peas of all these colours are numerous, as are peas exhibiting the other characters shown on the plate: and I shall have much pleasure in sending a replica of the set from which the plate was photographed to any naturalist who may write to me before my stock is exhausted.]

In the same way, I have tried to make my statements about the shape of seeds intelligible by means of the photographs reproduced on Plate II.*

(1) *Telegraph* was at first very variable, both in shape and in colour, and *Telephone* did not become thoroughly differentiated from it for some years. In 1878 the Royal Horticultural Society gave a certificate to *Telephone* as a

---

* Plates I. and II. will be found facing p. 254.

new variety, and in 1879 a controversy arose as to the reality of the distinction between the two. The Editor of the *Gardener's Chronicle* (1879, p. 210) says, "It is clear that what Messrs Carter and Co. are sending out as *Telephone* is "the most wrinkled seed selected from *Telegraph*," and later in the same year (August 2nd, p. 146) the Editor describes seeds sown at his direction in order to test the identity of the two races. Among the seeds sown are three samples of *Telegraph*. In one the seeds are said to be "mixed, round and wrinkled," in the second and third they are "but slightly wrinkled." And since on the one hand the seeds of *Telephone* are admittedly "wrinkled," while on the other hand there is some doubt whether *Telegraph* and *Telephone* are really distinct, it is clear that the offspring of the hybrid *Telegraph* had not become sharply divided into a smooth-seeded and a wrinkled race in 1879, as they should have done on Mendel's hypothesis. The presence of forms intermediate between the categories "round" and "wrinkled" in 1879 seems proved by the fact that the controversy referred to arose. The *Telegraph* of to-day is more than 25 generations removed from the original cross, and it should, on Mendel's view, have split into two groups, one with seeds completely smooth and rounded, the other with wrinkled seeds. Seeds of intermediate type should not occur, and only one plant out of every $2^{25}-1$, or say one plant in every thirty-three millions, should bear seeds of both kinds.

As a matter of fact, the seeds of *Telegraph* as grown by Carter and Co. exhibit every conceivable condition between the perfectly smooth forms shown in Fig. 1, Plate II., and the forms with well-marked wrinkles shown in Fig. 4 on the same plate. The groups photographed in Figs. 1—4 were chosen from a single quart of *Telegraph* given to me by Messrs Carter; and a rough estimate of the relative frequency with which the characters of each group occurred in a small sample gave the following result:

Group 1	242
Group 2	228
Group 3	28
Group 4	2
	500

It is not suggested that the groups differ successively by approximately equal increments of wrinkles, so that the scale they form is certainly imperfect. The grouping of the peas is also very rough. But with all these defects the study of *Telegraph* seems to me to demonstrate that the offspring of a hybrid Pea, 25 generations at least after crossing, may contain a large percentage of individuals which on Mendel's view that roundness is dominant ought never to occur at all.

The distinction in colour between *Telegraph* and *Telephone* was also discussed twenty years ago. The seed-coats are often so opaque in all these races, that it is difficult to estimate cotyledon colour from descriptions of the pea with its seed-coat uninjured. A statement by Culverwell seems however worth quoting. In 1882 he writes of *Telegraph* and *Telephone*: "The two will always "come from one sort, more especially from the green variety." And a line or two later, describing the result of sowing what he thought a good sample of *Telegraph*, he says, "Strange to say, although the peas were taken from one lot, those sown "in January produced a great proportion of the light variety known as *Telephone*. "These were of every shade of light green up to white, and could have been "shown for either variety." (*Gardener's Chronicle*, July 1882, p. 150.)

This is only one of a number of statements, scattered through the *Gardener's Chronicle* from 1879 onwards, which show that neither *Telegraph* nor *Telephone* was recognised as sharply divided into green or blue and yellow or white races during the early years of their existence; Culverwell's statement that the green variety was especially variable in colour in 1882, shows more than this, however, because it shows that in his opinion a plant of a late generation, which exhibited a "recessive" colour, did not produce exclusively recessive offspring, as Mendel says it should. At the present day they are recognisably distinct, both in shape and in cotyledon colour; but the colour of the cotyledons is variable in both. Using the groups of seeds photographed on Plate I. as a scale, I found among 576 seeds of *Telegraph*, 512 seeds of fairly uniform colour, and 64 obviously piebald seeds. The self-coloured seeds fell into one or other of the colour groups from 1 to 6, with frequencies given in Table IV. (p. 250). The piebald remainder were generally half bright yellow and half bright green. There were generally only two patches of colour on each seed, and one cotyledon was often as yellow as Group 6, Plate I., while the other was as green as Group 1.

(2) *Pride of the Market*. If shape and colour be considered together, this is one of the most constant races selected from the offspring of *Telegraph*. Of 500 seeds whose shape was examined, there were

Like Fig. 1	260
" Fig. 2	218
" Fig. 3	20 or 21
" Fig. 4	2 or 1

while the colour, as shown in Table IV., is distinctly less variable than in *Telegraph*, and the percentage of piebalds perhaps significantly smaller.

(3) *Stratagem* is nearly as constant as *Pride of the Market* in colour, but it is more variable in shape. The seeds are larger than those of any other race belonging to this group, and are normally much flattened; for this reason it is difficult to compare the seeds of *Stratagem* with those of the races just described. Three groups of *Stratagem* seeds are photographed in Figs. 7, 8 and 9 (Plate II.) and they show the characteristic shapes and wrinkles fairly well. An inspection of Figs. 1—9 will show better than any words, *first* how completely the categories "smooth" and "wrinkled" pass into each other in these races;

and *secondly* how enormously the range of variation included under the two differs.

The colour of *Stratagem* is interesting because of its intermediate condition between the extremes observed in the group of races to which it belongs. As will be seen from Table IV., piebald seeds are rarer than in any of its allies, and the great majority of seeds fall into the colour-groups 2 and 3*, with a very small percentage of seeds so green as Group 1, or as yellow as Groups 4 and 5. The general impression produced by a mass of these seeds, when stripped of their coats, is certainly green; but it is a far yellower green than that of either *Telegraph* or *Pride of the Market*; and I think there can be no question that in *Stratagem* a blend of green and yellow has been inherited, and fixed by a process of selection.

(4) *Telephone*, in the samples given to me by Carter and Co. or by Sutton and Sons, is fairly uniform in shape, being practically always as wrinkled as Figs. 5 and 6. It appears however from Tschermak's account (No. 27, p. 480) that samples which he bought in Ghent and in Quedlinburg were more variable, and contained smoother, more oblong forms. The same thing is true of a sample which I bought in Oxford from Messrs McGreal and Co. The colour of all the samples I have seen (from Vilmorin-Andrieux, Carter, Sutton, and McGreal) is exceedingly variable. Every one of the six colour-groups is abundantly represented, and indeed the scale of colours, which was found suitable for the whole series of races, was selected from this race alone. There can be no question that self-coloured seeds of every shade intermediate between the darkest green of Fig. 1 and the most intense orange-yellow of Fig. 6 occur in every large sample of this race, while piebald peas, with every degree of difference between the colours of their patches, occur also. Groups of piebalds, illustrating their main characters, are shown in Figs. 13—18 on Plate I. The colour variation of this race, together with Tschermak's description of its behaviour when crossed as a green-seeded pea with exceptional dominance, led me to tabulate the colours in several samples. The results are given in Table IV.; and while the different samples differ considerably among themselves, they all show the same general characters. In all of them the range of colour is the whole range of the scale, and the groups of intermediate colour occur most frequently; in all of them there is a large percentage of piebald individuals. It is evident that in a very large percentage of these individuals the characters "green" and "yellow" are perfectly blended, while others present a mosaic or piebald character; but even in these piebald individuals the patches of colour cannot, in some cases, be spoken of as pure green or pure yellow; the pea may exhibit a blend of green and yellow on every part of its surface, but the proportions in which the two are mixed may differ in the two cotyledons, or in different parts of the same cotyledon. The suggestion that yellow and green are mutually exclusive alternatives, or that

---

* As will be seen from Plate I., Figs. 7—12, many seeds of this race are intermediate in colour between Figs. 2 and 3.

anything like Mendel's phenomena of dominance and segregation has occurred in the history of *Telephone*, is clearly absurd.

(5) *Duke of Albany* resembles *Telephone* so closely in the general characters of its seeds, that I have only examined a small sample, sent to me by Messrs Sutton and Sons. The frequency of colour varieties in this sample is given in Table IV.

(6) *The Hybrid Descendants of Stratagem.* It has already been pointed out that the average colour of *Stratagem* must be regarded as a blend of yellow and green. The yellow properties of this blend are well seen in *Daisy*, the result of the cross *Stratagem* × *Giant Marrow*. In shape, the seeds of *Daisy* are generally oval, with two opposite concave faces, the whole surface being wrinkled to a variable extent. Forms intermediate between this and the thinner flattened form of *Stratagem* also occur, as do more rounded forms. The chief shapes may be gathered from Figs. 9 and 10 (Plate II.). The average colour is a fairly uniform green; but as will be seen from Table IV., piebald individuals, and individuals of colour 4, occur.

Daisy and *Lightning* were crossed, as has been said, to produce the race *Early Morn*. In its present condition *Lightning* is a very round and smooth-seeded Pea, the shapes which occur being shown in Figs. 13 to 15 (Plate II.). The wrinkles of the group photographed in Fig. 15 occurred in about 16 out of 250 seeds examined. The colour varieties include a small percentage of green and piebald seeds; but the great majority of the seeds are of a deeper orange than that of Group 6. *Lightning* is what I imagine most of the writers on Mendel's laws would accept as a typical round, smooth, yellow Pea. The seeds of the second generation (produced by the first hybrid plants) were variable both in shape and colour. Messrs Carter and Co. have circulated reproductions of a photograph of one pod, containing peas of the second generation. The pod contains seven seeds, of which three are labelled "white," three "blue," and one "green." Two of the seeds seem smooth, while the rest are wrinkled in various degrees. By the kindness of Messrs Carter and Co. I have been able to examine a sample of *Early Morn* not as it is now, but as it was when they introduced it. The race was apparently selected from seeds of the second hybrid generation which were "recessive" both in colour and in shape; and although we must remember that the seed saved when making the stock from which a new race might be established was selected, and cannot be supposed to represent all the seed produced by the offspring of all the green and wrinkled hybrids, yet so far as the evidence available goes it favours the view that the offspring of wrinkled seeds were themselves wrinkled, for the sample given to me contains only wrinkled seeds of fairly uniform character. These seeds do not in the least resemble the seeds of *Daisy*. As will be seen from Figs. 16—18 (Plate II.), they are far more like those of *Telephone* in the character of their wrinkles than they are like either of their parents. This is another example of the danger incurred by using Mendel's categories without careful examination. The statement that the seeds of *Daisy* and *Early Morn* are wrinkled and irregular, while those of *Lightning* are fairly smooth and rounded, is perfectly true; but if made in this form, without further explanation, it suggests a likeness between *Daisy* and *Early Morn* which does not exist. So far as the shape of *Early Morn* exhibits a reversion to any of its known ancestors, the reversion is directly to the most wrinkled type of the original *Telegraph*. In sending me the sample I have examined, Messrs Carter wrote of *Early Morn*: "You will clearly see the trace of "the white Pea which was one of its parents." The analysis of the colour-variations, given in Table IV., abundantly confirms Messrs Carter's statement: the majority of seeds are green; but seeds of intermediate colour, and piebald seeds, do occur with quite sensible frequency.

TABLE IV.

Frequency of Colour Variation.

Race	Colour 1	Colour 2	Colour 3	Colour 4	Colour 5	Colour 6	Piebald	Total
Telegraph	354	95	47	10	4	2	64	576
Pride of the Market	447	76	19	2	2	1	55	602
Stratagem	200	367	154	16	5	0	40	602
Telephone (Carter)...	191	289	195	59	38	38	133	943
„ (Sutton)...	13	83	112	32	15	13	43	311
„ (Vilmorin)	29	69	69	23	3	2	5	200
Duke of Albany ...	26	70	121	53	11	20	27	328
Daisy...	78	175	27	7	0	0	17	304
Early Morn	267	239	81	2	1	1	9	600

IV.—*Mendel's Law of Segregation.*

The history of the Telephone group of Peas is clearly inconsistent with the universal validity of Mendel's laws of dominance and segregation. The hybrid *Telegraph* produced seeds of various colours at the time of its origin, and now, more than five-and-twenty years after its introduction, it does so still. From the variable offspring of this hybrid races have been produced by selection, which bear either yellow-green seeds of fairly constant character (*Stratagem*) or a mixture of self-coloured and conspicuously piebald seeds, the self-coloured seeds presenting every conceivable colour between deep green and intense orange-yellow (*Telephone*). These races are many generations removed from their common hybrid ancestor, and it may be suggested that phenomena such as those described by Mendel could have been observed in the earlier generations. No decisive answer to such a suggestion can now be obtained; but the hybrids *Daisy* and *Early Morn* are both little removed from their cross-fertilised ancestor; in their present selected form both exhibit characters which should, on Mendel's view, be recessive, and should therefore produce invariably recessive offspring, while in fact their offspring is variable.

The behaviour of these races is much more exactly in accord with the statements of Laxton. In 1866, the year in which Mendel's paper was published in printed form, Laxton published a short summary of the results gained during many years of work upon Peas, which shows that his experience was altogether different from that of Mendel. He says of the colour and character of the seed:

"The results of experiments in crossing the Pea tend to show that the colour "of the immediate offspring or second generation sometimes follows that of the "female parent, is sometimes intermediate between that and the male parent, and "is sometimes distinct from both; and although at times it partakes of the colour "of the male, it has not been ascertained by the experimenter ever to follow the "exact colour of the male parent. In shape, the seed frequently has an intermediate character, but as often follows that of either parent. In the second "generation, in a single pod, the result of a cross of Peas different in shape and "colour, the seeds are sometimes all intermediate, sometimes represent either or "both parents in shape or colour, and sometimes both colours and characters, with "their intermediates, appear. The results also seem to show that the third "generation, that is to say, seed produced from the second generation or the "immediate offspring of a cross, frequently varies from its parents in a limited "manner, usually in one direction only, but that the fourth generation produces "numerous and wider variations, the seed often reverting partly to the colour and "character of its ancestors of the first generation, partly partaking of the various "intermediate colours and characters, and partly sporting quite away from any of "its ancestry." (Laxton, No. 19, p. 156.)

In a review of his own work, nearly a quarter of a century later, Laxton says again: "By means of cross-fertilization alone, and unless it be followed by careful "and continuous selection, the labours of the cross-breeder, instead of benefiting "the gardener, may lead to utter confusion." (No. 20.)

These statements show that the phenomena of inheritance in cross-bred Peas, as Laxton observed them, were far more complex than those described by Mendel; but they do not preclude the possibility of a simple segregation, such as Mendel describes, in particular cases. Such cases of simple segregation have been described in Peas by both Correns and Tschermak in the papers already cited. The proportions of dominant and recessive individuals, found by Correns in his later generations, were in accord with Mendel's results; those found by Tschermak, as he himself points out, were extremely improbable, on Mendel's hypothesis.

Taking these results together with Laxton's statements, and with the evidence afforded by the Telephone group of hybrids, I think we can only conclude that segregation of seed-characters is not of universal occurrence among cross-bred Peas, and that when it does occur, it may or may not follow Mendel's law. The law of segregation, like the law of dominance, appears therefore to hold only for races of particular ancestry. In special cases, other formulae expressing segregation have been offered, especially by De Vries (32) and by Tschermak (29) for other plants, but these seem as little likely to prove generally valid as Mendel's formula itself.

The fundamental mistake which vitiates all work based upon Mendel's method is the neglect of ancestry, and the attempt to regard the whole effect upon offspring, produced by a particular parent, as due to the existence in the parent of particular structural characters; while the contradictory results obtained by those who have observed the offspring of parents apparently identical in certain characters show clearly enough that not only the parents themselves, but their race, that is their ancestry, must be taken into account before the result of pairing them can be predicted.

ALPHABETICAL LIST OF AUTHORS CITED.

1. BATESON, W. On Progress in the Study of Variation. Science Progress, N. S., Vol. I. No. 5 (1897) and Vol. II. No. 6 (1898).
2. CLARKE, TREVOR. Report of Proceedings: International Horticultural Exhibition and Botanical Congress, London, 1866, p. 143.
3. CORRENS, C. G. Mendel's Regeln über das Verhalten der Nachkommenschaft der Rassenbastarde. Berichte d. deutsch. botan. Gesellsch. XVIII (1900), pp. 158—168.
4. ——. Summary in Botanische Zeitschrift, Abth. II, 1900.
5. ——. Ueber Levkojenbastarde. Botan. Centralbl. LXXXIV (1900), pp. 97—113.
6. ——. Ueber Bastarde zwischen Rassen von *Zea Mays*. Ber. d. deutsch. botan. Gesellsch. XIX (1900), pp. 211—220.
7. ——. Bibliotheca Botanica, No. 53 (1901).
8. CRAMPE. Zuchtversuche mit zahmen Wanderratten. An elaborate series of papers in Landwirthschaftliche Jahrbücher, VI (1877), XII (1883), XIII (1884), XIV (1885). The chief results quoted in the text are in the paper of 1877; but the whole series should be referred to.
9. DARWIN, C. The Variation of Animals and Plants under Domestication (quoted from 2nd Edition, London, 1875).
10. FISCHER, VON. Iltis (*Mustela Putorius*) und Frett (*M. furo*). Der Zoologische Garten XIV (1873), pp. 108—112.
11. ——. Beobachtungen über Kreuzungen verschiedener Farbenspielarten innerhalb einer Species. Der Zoologische Garten XV (1874), pp. 361—374.
12. GÄRTNER, C. F. VON. Versuche und Beobachtungen über die Bastarderzeugung im Pflanzenreich. Stuttgart, 1849.
13. GALTON, F. The Average Contribution of each several Ancestor to the total Heritage of the Offspring. Roy. Soc. Proc. LXI (1897), pp. 401—413.
14. GILTAY, E. Ueb. d. directen Einfluss d. Pollens auf Frucht- und Samenbildung. Jahrb. f. wiss. Botanik XXV (1893).

15. GOSS, J. Transactions Horticultural Society V (1848), pp. 234—236.

16. GUAITA, G. V. Versuche mit Kreuzungen etc. der Hausmaus. Ber. d. Naturf. Gesellsch. Freiburg X (1898).

17. HAACKE, W. Biologisches Centralbl. XVI (1896).

18. KNIGHT, T. A. Phil. Trans. 1799, pp. 195—204.

19. LAXTON, T. Observations on the Varieties effected by Crossing in the Colour and Character of the Seed of Peas. Report of Proceedings: International Horticultural Exhibition and Botanical Congress: London, 1866, p. 156.

20. ——. Journ. Roy. Hort. Soc. XII (1890), pp. 29—40.

21. MENDEL, G. Versuche über Pflanzen-Hybriden. Abhandl. d. Naturf. Vereins in Brünn IV (1865), pp. 3—47;

Also (edited by E. Tschermak) in Ostwald's Klassiker der exakten Wissenschaften No. 121;

Further, English translation by Bateson in Journal Royal Horticultural Society XXVI (1901).

22. NAUDIN, C. Nouvelles Recherches sur l'Hybridité dans les Végétaux. Nouvelles Archives du Muséum I (1865), pp. 25—176.

23. NOBBE, F., and others. Untersuchungen über den Einfluss der Kreuzbefruchtung auf die Nachkommenschaft. Die landwirthschaftliche Versuchsstationen XXXIV (1888).

24. PEARSON, K. The Law of Ancestral Inheritance. Roy. Soc. Proc. LXII (1898), pp. 386—412.

25. ——. On the Criterion that a given System of Deviations from the Probable in the case of a Correlated System of Variables is such that it can be reasonably supposed to have arisen from Random Sampling. Phil. Mag. Vol. L (1900), pp. 157—175.

26. RIMPAU, W. Kreuzungsprodukte landwirthschaftlicher Kulturpflanzen. Landwirthschaftliche Jahrbücher, Jahrg. XX (1891).

27. TSCHERMAK, E. Ueber künstliche Kreuzung bei *Pisum sativum.* Zeitschr. f. d. landwirthschaftliche Versuchswesen in Oesterreich, Jahrg. III (1900), pp. 465—555.

28. ——. Weitere Beiträge über Verschiedenwerthigkeit der Merkmale bei Kreuzung von Erbsen und Bohnen. Zeitschr. f. d. landwirthsch. Versuchswesen, Jahrg. IV (1901), pp. 641—731.

29. ——. Ueber Züchtung neuer Getreiderassen, etc. Zeitschr. f. d. landwirthsch. Versuchswesen, Jahrg. IV (1901).

30. VRIES, H. DE. Sur la loi de disjonction des Hybrides. Comptes Rendus, CXXX (1900), pp. 845—847.

31. ——. Das Spaltungsgesetz der Bastarden. Ber. d. deutsch. botan. Gesellsch. XVIII (1900), pp. 83—90.

32. ——. Ueber erbungleiche Kreuzungen. Ber. d. deutsch. botan. Gesellsch. XVIII (1900), pp. 435—443.

EXPLANATION OF PLATES I. AND II.

PLATE I.

Figs. 1—6. Seeds of the hybrid Pea Telephone, the seed-coats removed, arranged so as to form a colour scale.

Figs. 7—12. Seeds of the hybrid Pea Stratagem, with the coat removed, forming a colour scale.

Figs. 13—18. Each figure shows a pair of cotyledons belonging to one seed of the Pea Telephone. The series shows the various degrees of difference in colour between the two cotyledons of the same seed.

Figs. 19—24. Peas in their seed-coats, showing the way in which the cotyledon colour is masked. 19—20, Telephone; 21, Telegraph; 22, Stratagem; 23, Pride of the Market; 24, Early Morn.

PLATE II.

PHOTOGRAPHS OF PEAS, SHOWING DEGREES IN THE DEVELOPMENT OF WRINKLES.

Figs. 1—4, degrees of wrinkling in Telegraph.

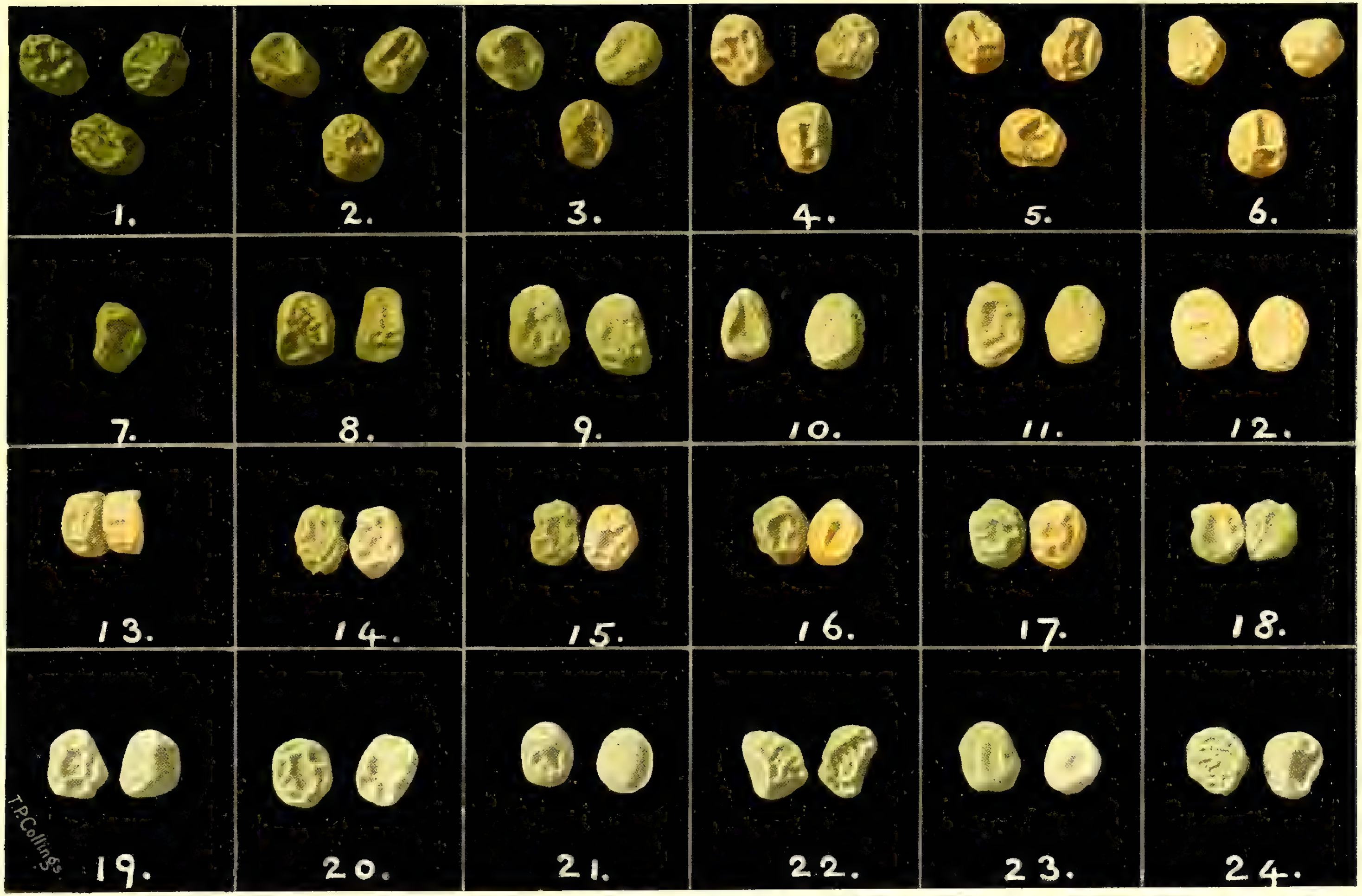
Figs. 5—6, seeds of Telephone.

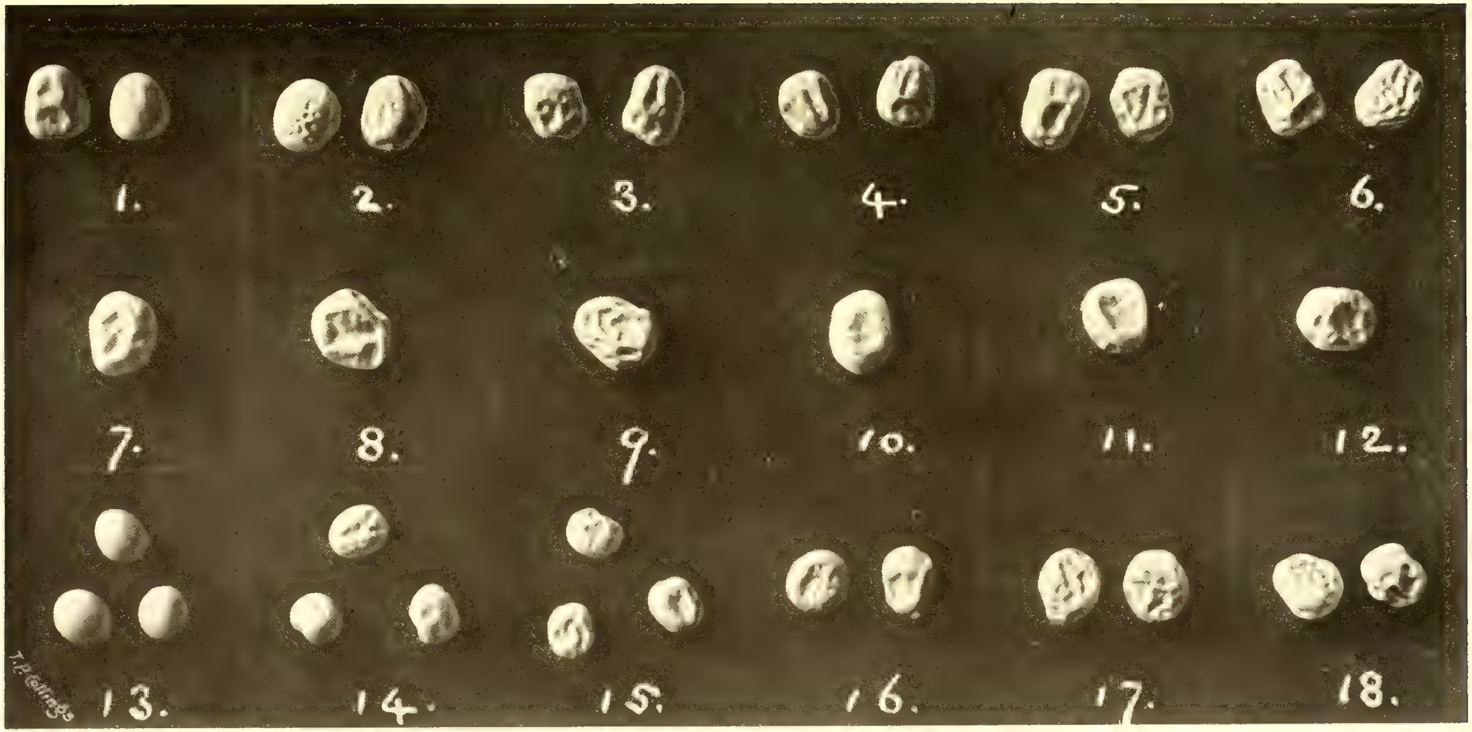
Figs. 7—9, seeds of Stratagem.

Figs. 10—12, seeds of Daisy (= Stratagem × Giant Marrow).

Figs. 13—15, seeds of Lightning.

Figs. 16—18, seeds of Early Morn (= Daisy × Lightning).


1.
2.
3.
4.
5.
6.
7.
8.
9.
10.
11.
12.
13.
14.
15.
16.
17.
18.
19.
20.
21.
22.
23.
24.
T.P.Collings


1.
2.
3.
4.
5.
6.
7.
8.
9.
10.
11.
12.
13.
14.
15.
16.
17.
18.
T. P. Collings

MISCELLANEA.

I. Variability, Symmetry and Fertility in an abnormal Species.

In the jelly-fishes of the group Hydromedusæ four may be said to be the *normal* number of radii. There are ordinarily *four* radial canals, lips and gonads 90° apart. *Pseudoclytia pentata* (Mayer) differs from all other Hydromedusæ in that it normally has 5 radial canals, 5 lips and 5 gonads, 72° apart. The species occurs in great numbers at the Tortugas, Florida. We may speak of it as an "abnormal" species. In the *Science Bulletin of the Brooklyn Museum*, Vol. I. No. 1, Dr A. G. Mayer has given an interesting account of the variation of this species. The frequency of the different numbers of radial canals and of lips is given as follows:

Frequency.

	1	*2*	*3*	*4*	*5*	*6*	*7*	*8*
Radial canals ...	—	1	8	56	860	64	6	1
Lips	2	5	18	123	798	49	1	—

From the correlation table the following results have been determined by Miss M. Lewenz of University College, London:

Organ	Mean	Standard Deviation	Correlation
Canals ...	5·004 ± ·009	·4413 ± ·0067	·3255 ± ·0191
Lips ...	4·868 ± ·012	·5559 ± ·0084	

The correlation between canals and lips is accordingly only moderate.

The frequency distribution of lips has been specially dealt with by Professor K. Pearson in a "Supplement to a memoir on Skew Variation," *Phil. Trans.* A, Vol. 197, p. 456. It forms an illustration of one of the curve-types omitted in his original memoir on that subject.

The individuals with 5 rays are nearly all radially symmetrical. The percentage of radially symmetrical individuals diminishes as we depart from this mode in either direction. In about two-thirds of the cases in which the canals have not a perfect *radial* symmetry they are nevertheless *bilaterally* symmetrical. There is a strong inherent tendency for some sort of symmetry.

Typically one gonad occurs on each radial canal, but one or more may fail to develop. Thus 730 medusæ with 5 canals had only 3080 gonads instead of the possible 3650.

The "coefficient of fertility" is $\frac{3080}{3650} = \cdot 8438$.

The coefficient of fertility differs according to the number of canals and to their regularity. Thus:

Number of canals	Coefficient of Symmetry	
	All individuals	Irregular individuals
3	·75	·555
4	·782	·69
5	·844	·63
6	·814	·69
7	·785	·57

It appears then that *the less typical an individual the less its fertility, and irregular individuals are more sterile than those having some sort of symmetry.* Thus the typical form and symmetry tend to be preserved.

It will be noted that this is a case of "reproductive selection," i.e. that there exists a correlation between fertility and the intensity of certain characters.

C. B. DAVENPORT.

II. Variation of the Egg of the Sparrow (*Passer domesticus*).

I HAD recently occasion for a special purpose to measure 180 clutches, containing 687 eggs of the house-sparrow. They were collected from Essex, Hertfordshire, Buckinghamshire, Berkshire, Surrey, Cambridgeshire, and even as far north as Nottinghamshire and Yorkshire. They were thus a fairly representative English series. The bulk of them were collected early in the year, and were undoubtedly first-nesting eggs. Having calculated out the constants for my series, it may not be without interest to compare them with constants obtained from the measurements of Professor Hermon C. Bumpus on both the American and English races of *Passer domesticus.* Professor Bumpus in 1896 measured 868 American and 868 English eggs and published* diagrams giving the frequency in length and index—breadth/length—of these series. He did not publish, as far as I am aware, the means and standard deviations of his frequency distributions, but concluded from his diagrams that the egg of the American sparrow is shorter and rounder than that of the English sparrow and further that it is more variable in length and shape. The greater variability of length and shape was judged not by the standard deviation (S. D.), or the coefficient of variability (C. of V.), but by the extent of the ranges. This method is, I believe, open to criticism†, and it seemed desirable to work out the numerical constants of Professor Bumpus' series. The results I have found are given in the table below, absolute lengths being in mms.

Sufficient data are not given for me to determine the S.D.'s of the breadths in Professor Bumpus' series, and as for the actual values of the breadths, I think, all we can say is that they must be very nearly equal and lie between 15·3 and 15·4. Now from these results we conclude:

(i) That the American egg is not more but *less* variable than the English in length.

---

* *Biological Lectures delivered at the Marine Biological Laboratory of Wood's Holl.* Summer Session, 1896. Boston, 1897.

† *The Chances of Death,* Vol. I. *Variation in Man and Woman,* p. 275.

(ii) That while the American egg is more variable in shape than Bumpus' English series, it is less variable than mine, although the difference between the two is really not significant.

(iii) That the American egg is sensibly shorter than the English egg, if judged by either English series.

(iv) That the American egg has probably slightly less breadth than the English egg.

(v) That having considerably less length and only slightly less breadth, the American egg appears rounder than the English egg.

	English Eggs		American Eggs
Character	Pearson (687)	Bumpus (868)*	Bumpus (868)
Mean Length ...	21·8229 ± ·0456	21·7368 ± ·0362	21·3065 ± ·0356
S. D. Length ...	1·1946 ± ·0322	1·0677 ± ·0256	1·0481 ± ·0252
C. of V. Length	5·47	4·91	4·92
Mean Breadth...	15·5052 ± ·0200	15·3683 ± [?]	15·3443 ± [?]
S. D. Breadth ...	·5245 ± ·0141	[?]	[?]
C. of V. Breadth	3·38	[?]	[?]
Index	71·2150 ± ·1433	70·8662 ± ·1092	72·1843 ± ·1250
S. D. Index	3·7560 ± ·1013	3·2155 ± ·0772	3·6831 ± ·0884

If we put aside Professor Bumpus' colour and shape appreciations[†], I think we must conclude from the actual measurements that there is no evidence that the American egg is more variable than the English egg. On the contrary it appears slightly less variable, as we might expect if the sparrow imported into America had been largely drawn from one district of the mother country. In the next place the American egg does appear to be ·5 mm. shorter than the English. This might be due to (*a*) direct selection in America, or to (*b*) absence of the periodic selection we may suppose to maintain the stability of the species in England. But it may equally likely be due to original import into America of an English local race, to good or bad seasons in England or America in the year the eggs were collected, or to a large preponderance of second nestings in the clutches measured by Professor Bumpus. All these points could, I think, be investigated and answered. Professor Bumpus has opened a most interesting field of inquiry, and for the next few years arrangements ought to be made for the annual measurement in both America and England of good series of sparrows' eggs, with due record of district, probable nesting and climate of previous winter and spring. We might then learn the effect of each of these factors in both America and England, and at least put on record valuable results for comparison with similar measurements 20 or 30 years hence. Only in this way shall we be able to ascertain whether there are persistent sources of differentiation at work.

K. PEARSON.

---

[*] I could find only the indices of 867 English eggs given.

[†] The extraordinary variety of mottling and tint on my eggs was commented on by all who saw them; I consider that it varied a good deal with the district. I found no method of quantitatively measuring it. It disappeared to some extent with time and I daresay something of the colour would be liable to fade if the eggs were preserved for any time and transported.

III. Note on Variation in Leaves of Mulberry Trees.

Attention has frequently been drawn to the difference in type of leaves growing on different parts of trees, and under the heading of *heterophylly* it is a topic familiar to botanists. It is only recently, however, that it has become a subject for statistical investigation. We may refer, for example, to the researches of De Brugher on "light" and "shade" leaves of the beech*, and to the measurements of A. Heyer on the needle of the pine embodied in Dr Ludwig's paper in the first number of *Biometrika*†. The present note deals with a similar case; it is hoped that it may serve as a preliminary study for a more extensive statistical investigation when further material is next year available. The importance of such investigations into a possible correlation between the character and situation of like organs is manifest if leaves are to be used as "homotypes" in the study of *homotyposis.*

The leaves of the common Mulberry appear to vary greatly according to their position on the tree, if I may judge from five trees which I have examined, and three of which information has been sent me.

Fig. I shows the usual, and greatly preponderating type of leaf, which on a large tree varies in size from about 3 to about 6 inches in length. In many there is a slight bulge or fulness at one place near the top, Fig. X, *A*, *B*, *C*: so that the leaf could not be pressed without some overlapping. Fig. III shows the greatest variation from the usual type that I have found: and Fig. II an intermediate form.

So far as I have observed, the deeply incised leaves (Fig. III) are found (*a*) largely on small twigs growing immediately from the main trunk and near its bottom, (*b*) occasionally on larger twigs or small boughs growing among the lower boughs: one has been found at the end of a low bough, but *never* high up on the tree.

The intermediate forms (Fig. II) are found (*a*) on small twigs along with No. III, (*b*) at the end of the lower boughs, and (*c*) apparently occasionally, but very rarely, at the end of higher boughs. The usual type is found in great preponderance over the whole tree: and the small twigs generally show this type too, though it is possible to find twigs with more or less incised leaves only.

With the assistance of Miss Eleanor Hobhouse the leaves of 8 mulberry trees of very different ages were examined. From these few trees it would appear that

(*a*) The age of the tree does not affect the matter: as the two youngest trees had very little variation, but the next in age had a very great deal.

(*b*) Well-grown upright trees with no ground or trunk shoots seem to have but little variation.

(*c*) Where the greatest number of cut leaves appears, the most deeply-cut leaves are also found.

(*d*) The lowest boughs and twigs have the greatest variation in respect of incision.

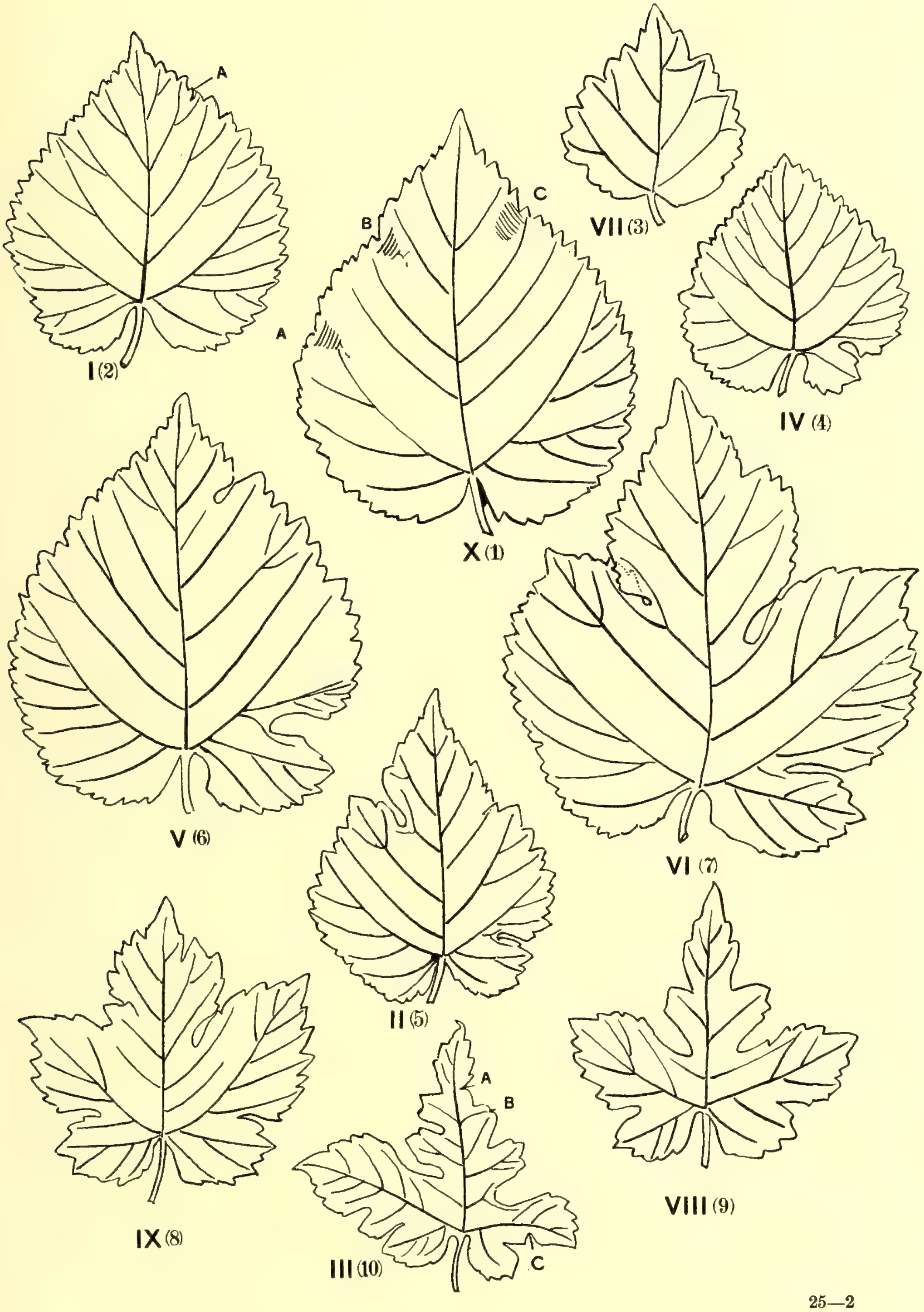
(*e*) The types melt into one another, and it would I think be easy to make a complete scale between Fig. X with its three bulges, and Fig. III with its emaciated form. In fact, the figures here given, if arranged according to the numbers in brackets, will form a very fairly continuous gradation from one extreme to the other; but I question whether any *one* of the trees examined would yield the complete scale.

AGNES FRY.

---

* *Bot. Jaarboek Dodonaea te Gent*, Vol. XI. p. 116, 1899.

† *Biometrika*, Vol. I. p. 20 *et seq.*


A
I(2)
B
C
A
X(1)
VII(3)
IV(4)
V(6)
VI(7)
II(5)
IX(8)
A
B
C
III(10)
VIII(9)

IV. On the Modal Value of an Organ or Character.

THE mode has been defined as that value of an organ or character for which the frequency per unit variation of the organ or character is a maximum. The mode is so important for many purposes that its accurate determination is essential. Unfortunately rough and often practically worthless determinations of its value have been given somewhat frequently in recent papers.

It has to be remembered that in nine cases out of ten the mode cannot be settled by inspection of the observations, and that its calculation involves processes far more elaborate than those necessary to find the mean, and unless such processes are used the probable error of the mode will be so large as to render its determination of no practical service*.

The method frequently adopted is the following: The whole range of the organ measured is, say, 6 to 9 mm. This is divided into elementary ranges of ·4 or ·5 mm., and 500 to 1000 individuals being measured they are grouped into these elementary ranges. One of these elements exhibiting a maximum frequency, the mid-point of this element is selected as the modal value of the organ in question.

Now such a frequency grouping as that referred to is ample for a good determination of the mean value of the organ or, with the use of the proper corrective term, for that of the standard deviation. It is generally idle for determining the mode by inspection. The mode will extremely rarely be at the mid-point of the element of observed maximum frequency, and in very many cases will not lie in that element at all. To be reasonably certain that it does, there must be a very large preponderance of frequency in that element. In order to recognise the truth of the first statement, i.e. that the mode does not bisect the element of maximum frequency, consider the three consecutive frequencies 52, 93 and 84 which actually occur in a certain system of 400 measurements†. The group 93 falls into a certain range of 8·15 to 8·55 mm. and the modal value is supposed on inspection to fall into this range. If we accept this, it will surely fall nearer to the 84 than to the 52 side of the range, and therefore it is clearly erroneous to place it in the middle at 8·35. We might allow for this bias of the mode by interpolating a curve through the tops of ordinates of 52, 93 and 84 and finding its maximum ordinate, or better still by taking a curve, the *areas* of which on the corresponding ranges had these values.

This might give fairly good results if 52, 93 and 84 were the true values of the frequencies of the given ranges for the whole population. But they are not; they are subject to really considerable errors of random sampling. Let us find these errors. If an individual falls 93 times in 400 trials into a certain group, its chance of falling into this group is $\frac{93}{400}$, and the chance that it will not is $\frac{307}{400}$. Hence the S.D. of the random sampling of this group is $\sqrt{400 \times \frac{93}{400} \times \frac{307}{400}} = 8{\cdot}45$. Similarly the S.D.'s of the 84 and 52 groups are 8·15 and 6·73 respectively. Multiplying by ·67449 to obtain the probable errors our system of frequencies reads:

$$52 \pm 4{\cdot}54, \quad 93 \pm 5{\cdot}70, \quad 84 \pm 5{\cdot}49.$$

Now it will be clear that three numbers subject to probable errors of this kind can hardly give a very accurate determination of the mode! Indeed the difference between 93 and 84 is 9, and the probable error of this difference is 8·97‡. Thus the difference is sensibly equal to the

---

* The whole subject is well discussed by G. U. Yule: "Notes on the History of Pauperism," *Journal R. Stat. Soc.* Vol. LIX., pp. 318—357. See especially the "Supplementary Note on the Determination of the Mode," with illustration of various approximate determinations (p. 398), and also the account of the discussion on this point at the meeting (p. 356).

† Left marginal length of left-handed fiddler crabs, Diagram No. 44, of Robert M. Yerkes' paper: "A Study of Variation in the Fiddler Crab, *Gelasimus Pugilator*, Latr.," *Proc. American Academy of Arts and Sciences*, Vol. XXXVI. p. 429.

‡ It must be remembered that the errors in 93 and 84 are negatively *correlated*, and that accordingly the probable error of the difference is not the square root of the sum of the squares of the probable errors of 93 and 84.

probable error of the difference, or the odds are only about 3 to 1 against the 84 group actually coming out larger than the 93 group. In other words in every four random samples of this material we should find on the average the mode in one case (if judged by inspection) to fall in the 84 and not the 93 element! Thus large frequencies about the modal value are subject to large absolute probable errors, and unless we have investigated the probable error of the differences of such frequencies, we shall have no security for even having found the elementary range within which the mode really lies. In fact, if we want to find the mode satisfactorily we must take into account the frequency in elements lying outside the groups 52 and 84. Five groups will be better than three, seven than five, and so on. But where are we to stop? Clearly it will be best to take all the component frequencies, or *the mode can only be found with the maximum of accuracy when we deduce it like the mean from the whole series of observations.* It will rarely if ever lie at the mid-point of the group of *apparent* maximum frequency of observation, and very often will lie outside the range of this group altogether. It is quite fallacious to suppose it a constant of the distribution easily determined by inspection. To discover it involves some theory of the nature of the distribution of the frequency or some interpolation hypothesis; it cannot be found until the errors of random sampling have been smoothed by some such process.

For practical purposes the *median* is one of the easiest quantities to determine, and this can be found in a very few minutes from inspection of the measurements, i.e. count half the observations from either end of the frequency distribution, and this will land the counter part-way, say n individuals, into some elementary frequency group. Look out the individuals in the observation-book falling into this group and arrange them in order of size, the nth individual from the proper end of the group either gives the median value of the organ (total number of individuals odd) or we place the median value (total number of individuals even) mid-way between the n and $n+1$th individuals. If the median has been found as well as the mean, then a quite good value of the mode may be deduced by remembering that the median lies between the mode and the mean and that the distance from the median to the mode is double the distance from the mean to the median; this is close enough for practical purposes in the majority of frequency distributions*. Unless the mode be determined in this manner or from a complete treatment of the frequency distribution the mere tabulation of modes by inspection seems of small value, and the reasoning upon modes so determined liable to lead to fallacious conclusions.

K. PEARSON.

V. On the Change in Expectation of Life in Man during a period of circa 2000 years.

It is well known that the expectation of life at each age has changed in England very sensibly during the last 50 years—Farr's table differs very considerably from Ogle's table. The same remark applies, if we compare the Registrar-General's life table for 1881–90 with that of J. P., F.R.S., based on the London bills of mortality for 1728 to 1757†. But an opportunity has occurred for comparing the expectation of life in man at an interval of nearly 2000 years. The change that has taken place in this period cannot fail to be one of the greatest interest from the standpoint of evolution.

Professor Flinders Petrie has drawn my attention to the fact that the ages at death of a certain number of Egyptian mummies in the Roman period have been recorded and are published

---

* *Phil. Trans.*, Vol. 186, A. p. 375. Formulae for determining the mode and the probable error of its determination from the moments directly are given in a paper: "On the Mathematical Theory of Errors of Judgment, with Special Reference to the Personal Equation," in type for the *Phil. Trans.* See also *R. S. Proc.* Vol. 68, p. 369.

† See *R. S. Proc.* Vol. 67, p. 169.

by Professor W. Spiegelberg of Strassburg in his work: *Aegyptische und griechische Eigennamen aus Mumienetiketten*, 1901.

I presume that the individuals whose ages are thus preserved would not belong in bulk to the lowest nourished classes of the Egyptian population, so that they do not represent in any way a selected short-lived class. If anything we may suppose them to belong to the rather better classes, and at a time when a fairly settled civil government was in existence. We may reasonably treat the material as a fair sample of what one of the most stable populations of 1900 to 2000 years ago could manage in the matter of longevity. Unfortunately the material is very sparse. Professor Petrie provides me with the following table:

Age at Death	♂	♀	Age at Death	♂	♀
1½	1	1	*33*	2	1
1⅚	1	—	*35*	—	3
2	2	—	*36*	2	2
3	3	2	*37*	1	—
4	4	3	*40*	3	3
5	3	—	*46*	1	—
6	—	2	*48*	2	—
7	1	—	*50*	5	1
8	—	—	*52*	3	1
9	—	1	*54*	—	1
10	1	1	*55*	2	1
11	1	1	*59*	1	—
14	2	1	*60*	4	1
16	1	1	*62*	1	—
17	2	3	*63*	1	—
18	—	2	*65*	2	—
19	1	2	*68*	2	—
20	2	3	*70*	—	1
21	1	6	*70·5*	1	—
22	2	2	*72*	3	—
23	1	2	*84*	1	—
24	2	—	*90*	1	—
25	5	4	*96*	—	1
26	4	1			
27	1	1		82	59
28	—	1			
29	1	1			
30	1	2		Total 141	
32	1	—			

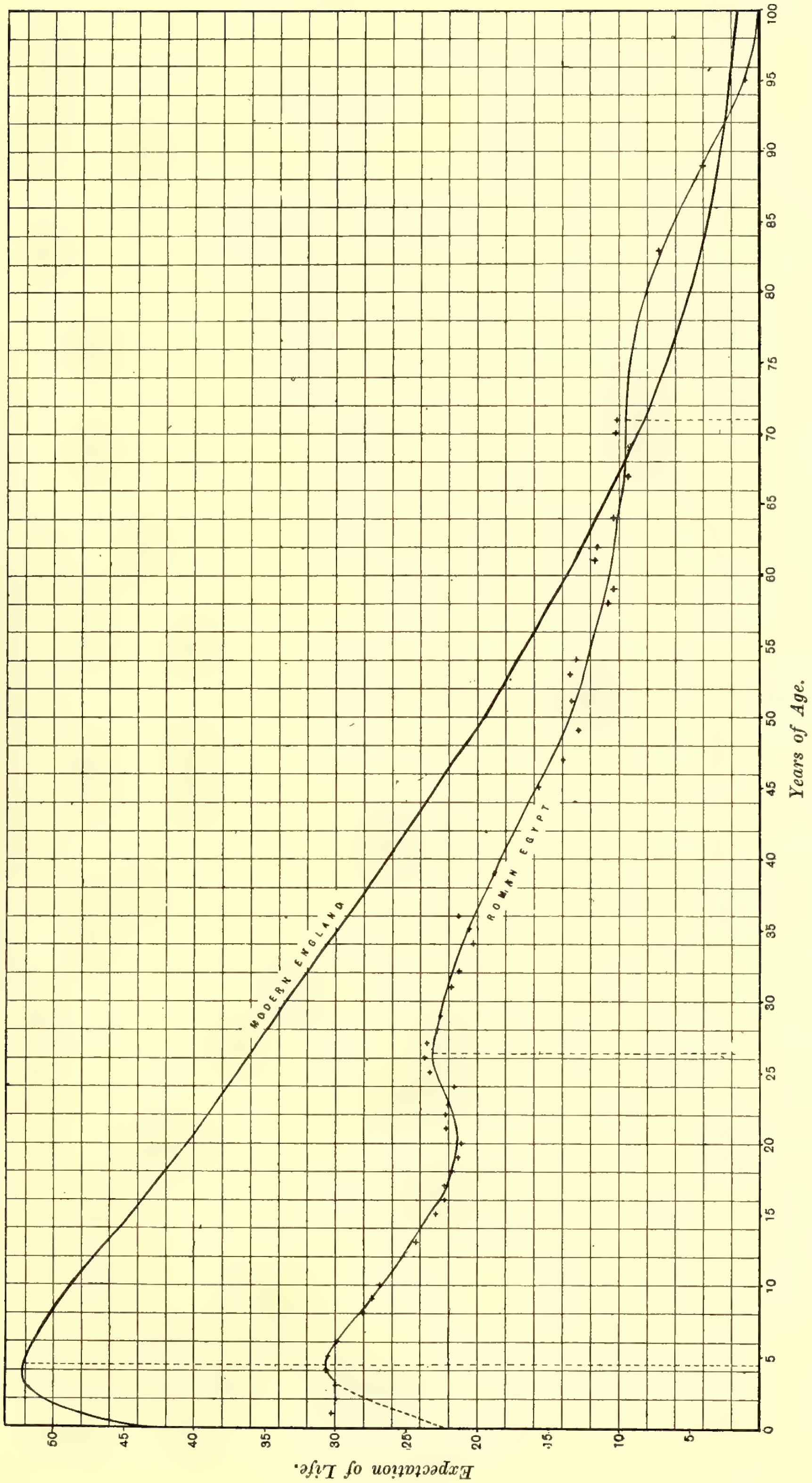
In dealing with this table I have not ventured to separate the ♂ and ♀ mortality, the numbers are far too insignificant. In the second place I have used expectation of life, and not a mortality table, because at any rate after two years of age we get up to a good age the means of more or less considerable numbers. Evidently there are hardly any entries in the first year of life, there is no fair representation of infantile mortality. Baby mummies, if they ever existed, have disappeared out of all due proportion.

In the diagram I have indicated by a small cross each expectation of life that it was possible to calculate, and I have further placed upon the diagram the English expectation of life based upon the average of male and female*. Now if we neglect infancy and extreme old age we really get a very fair sweep showing the general trend of the Egyptian mortality. I have very roughly adjusted the data representing them by the continuous curve. No elaborate adjustment was

---

* Ogle's Table, 1871—80.

Comparison of Expectation of Life at an Interval of 2000 years.



permissible, but from this adjustment, or even without it, the following results are clearly obvious:

(i) That up to the age of 68 the English expectation of life is greater, and in the earlier part of life immensely greater, than that of 2000 years ago.

In the course of those centuries man must have grown remarkably fitter to his environment, or else he must have fitted his environment immeasurably better to himself. No civilised community of to-day could show such a curve as the civilised Romano-Egyptians of 2000 years ago exhibit. We have here either a strong argument for the survival of the physically fitter man, or for the survival of the civilly fitter society. Either man is constitutionally fitter to survive to-day, or he is mentally fitter, i.e. better able to organise his civic surroundings. Both conclusions point perfectly definitely to an evolutionary progress.

(ii) The Egyptian curve differs fundamentally from the English in exhibiting apparently three maxima instead of one. These maxima must arise from the mortality curve itself being multinodal.

The first maximum is at about 4·5 years of age and corresponds to the English at about 4 years of age*. This is associated with the point where the especial dangers of infantile and childhood mortality have been surpassed.

The second Egyptian maximum occurs between 26 and 27 and possibly marks the period where the dangers of youthful mortality have been surmounted. The mortality of youth in our English case is a rather small component (greater for the French) and is centred at 22·5 years, becoming insignificant at 35.

I take it that this was much emphasised in Ancient Egypt, and reached its maximum considerably earlier. The third Egyptian maximum is less definite, because there are so few cases of extreme longevity to base the means upon. But it seems to me, looking at the points between 60 and 70, clearly impossible for the expectation of life curve to approach the axis without a point of inflexion and a maximum somewhere about 71—possibly earlier, even as early as 65, but the data are not sufficient to determine the point at all closely†. If this third maximum really exists, it would probably mean that the "mortality of middle life" which in the case of England is centred about 41·5 and ceases about 70 was much more definitely marked in Egypt. [See my paper in the *Phil. Trans.*, Vol. 186, p. 408, and especially Plate 16.] That the expectation of life for a Romano-Egyptian over 68 was greater than for a modern English man or woman is what we might expect, for with the mortality of youth and of middle age enormously emphasised only the very strongest would survive to this age. Out of 100 English alive at 10 years of age 39 survive to be 68; out of 100 Romano-Egyptians not 9 survived.

Looking at these two curves, we realise at a glance either the great physical progress of man, which enables him far more effectually to withstand a hostile environment, or the great social and sanitary progress he has made which enables him to modify that environment. In either case we can definitely assert that 2000 years has made him a much "fitter" being. In this comparison it must be remembered that we are not placing a civilised race against a barbaric tribe, but comparing a modern civilisation with one of the highest types of ancient civilisation.

That a man of 25 years to-day lives on an average 15 more years than a man of 25 years did 2000 years ago is surely very strong evidence that man has progressed substantially in this period.

K. PEARSON.

---

* I have turned down the Egyptian curve in a perfectly arbitrary manner by a broken line to indicate to the reader, looking at the diagram, that the infantile mortality is practically unrecorded.

† I am inclined to think the Egyptian curve for a considerable period must be concave to the horizontal axis, instead of throughout convex as in the case of the English.

VOLUME I APRIL, 1902 No. 3

ON THE SYSTEMATIC FITTING OF CURVES TO OBSERVATIONS AND MEASUREMENTS.

BY KARL PEARSON, F.R.S., University College, London.

CONTENTS.

Introductory Note.

ONE of the most frequent tasks of the statistician, the physicist, and the engineer, is to represent a series of observations or measurements by a concise and suitable formula. Such a formula may either express a physical hypothesis, or on the other hand be merely empirical, i.e. it may enable us to represent by a few well selected constants a wide range of experimental or observational data. In the latter case it serves not only for purposes of interpolation, but frequently suggests new physical concepts or statistical constants.

In any given case the formula or curve to be fitted to the data is:

(i) Directly given by physical theory;

(ii) Chosen on the basis of a physical hint;

(iii) Although purely empirical, suggested by experience of goodness of fit in like cases;

(iv) Quite unknown and to be chosen solely by examination of the material.

Now, as I hope to indicate in this paper, half the difficulty of curve-fitting in practice lies in the choice of a suitable curve. Especially in Case (iv) it is only a very considerable experience in curve-fitting that can lead to a suitable choice among all the possible algebraic, exponential and trigonometrical curves that suggest themselves.

The hasty assumption of some physicists and many engineers that a parabola of the form

$$y = c_0 + c_1 x + c_2 x^2 + c_3 x^3 + \dots$$

is always a good thing is to be deprecated, as may be seen at once by considering what a poor fit is obtained in this way to material really expressed by such curves as

$$y = y_0 e^{-cx^2}, \quad y = y_0 \sin nx, \quad y(x+c) = b^2, \text{ etc.}$$

To assume a curve of this form we must show the *rapid* convergency throughout the proposed range of the Maclaurin Expansion, and this is not always feasible.

The present paper does not concern itself with the choice of suitable curves, but only with the determination of the constants, when the form of the curve has been selected. This I readily allow to be the easier half of the task.

So far I have not, however, been able to find any systematic treatise on curve-fitting. It is usually taken for granted that the right method for determining the constants is the method of least squares. But it is left to the unfortunate physicist or engineer to make the discovery that the equations for the constants found in this manner are in nine cases out of ten insoluble, or a solution so laborious that it cannot profitably be attempted.

The present paper endeavours to indicate a systematic method for fitting curves. It is not claimed for it:

(i) That it will succeed in giving the values of the constants in every conceivable case.

I can only say that after an experience of some eight years' use by my fellow-workers, students and myself we have found it applicable to a vast range of physical and statistical data.

(ii) That it will give absolutely the "best" values of the constants in all cases.

I endeavour to show that it must give *good* values. The definition of "best fit" is more or less arbitrary, and for practical purposes, I have found that with due precautions as to quadrature, it gives, when one can make a comparison, sensibly as good results as the method of least squares.

Finally it is an advantage to have a systematic method of approaching curve-fitting problems, which at any rate gives practically excellent values for the constants in a very great number of cases in which the method of least squares is admittedly of no service at all.

(1) *General Theorem.*

A series of measurements or observations of a variable y having been made, corresponding to a series of values of a second variable x, it is required to determine a good method of fitting a theoretical or empirical curve $y=\phi(x, c_1, c_2, c_3, \dots c_n)$, *where* $c_1, c_2, c_3, \dots c_n$ *are arbitrary constants, to the observations for a given range* $2l$ *of the variable x.*

Such problems in curve-fitting recur with great frequency in physical, biological and statistical investigations. The usual *theoretical* rule is to use the method of least squares, but if the constants c_1, c_2, c_3, ... c_n are involved in a complex manner the equations obtained by the method of least squares become unmanageable, and we find physicist and statistician remarking that "the increased accuracy of the result obtainable by least squares would not be an adequate return for the labour involved," and then falling back on some more or less questionable process of determining the constants. This process may be graphical or arithmetical, but it is usually unsystematic in character and elastic in result. The object of the present paper is to give a systematic method of fitting curves to observations, which I have reasonable ground for considering a good one, and which at any rate for a great variety of problems leads us to easy and simple results.

The assumption to be made solely for the *proof*, but not in practice, is that $y=\phi(x)$ can be expanded by Maclaurin's Theorem, and that the resulting series converges fairly rapidly. Let the expansion be:

$$y = \phi(0) + x\phi'(0) + \frac{x^2}{1.2}\phi''(0) + \frac{x^3}{1.2.3}\phi'''(0) + \dots$$

$$= \alpha_0 + \alpha_1 x + \alpha_2 \frac{x^2}{1.2} + \alpha_3 \frac{x^3}{1.2.3} + \dots, \text{ say.}$$

Here $\alpha_0, \alpha_1, \alpha_3, \dots$ etc. will be functions of the n constants $c_1, c_2, \dots c_n$ of the curve. Hence theoretically we can find the n c's in terms of $\alpha_0, \alpha_1, \alpha_2, \dots \alpha_{n-1}$. We should thus be able on substitution to express $\alpha_n, \alpha_{n+1}, \dots$ etc. in terms of $\alpha_0, \alpha_1, \dots \alpha_{n-1}$. Now consider the first n α's as the constants of our curve and it will be expressible in the form:

$$\begin{aligned} y = \alpha_0 + \alpha_1 x + \alpha_2 \frac{x^2}{1.2} + \dots + \alpha_{n-1}\frac{x^{n-1}}{\underline{|n-1}} \\ + \phi^n(\alpha_0, \alpha_1, \alpha_2, \dots \alpha_{n-1})\frac{x^n}{\underline{|n}} \\ + \phi^{n+1}(\alpha_0, \alpha_1, \alpha_2, \dots \alpha_{n-1})\frac{x^{n+1}}{\underline{|n+1}} \\ + \text{etc.} \qquad\qquad \text{(i).} \end{aligned}$$

Next let y' be the ordinate corresponding to x given by observation, then $y - y'$ will be the distance between the theoretical and observed curves at the point corresponding to x, and our object is to make the values of this as small as possible by a proper choice of $\alpha_0, \alpha_1, \alpha_2, \dots \alpha_{n-1}$. This may be done by the method of least squares or making

$$\int (y - y')^2\, dx = \text{a minimum.}$$

This obviously gives a very good method, if not the "best," a term incapable of definition. The resulting equation, since y is the variable, is

$$\int (y - y')\, \delta y\, dx = 0 \qquad\qquad \text{(ii).}$$

Now δy depends on the variation of $\alpha_0, \dots \alpha_{n-1}$, or

$$\begin{aligned} \delta y = {} & \delta\alpha_0 + \delta\alpha_1 x + \delta\alpha_2 \frac{x^2}{1.2} + \delta\alpha_3 \frac{x^3}{1.2.3} + \dots + \delta\alpha_{n-1}\frac{x^{n-1}}{\underline{|n-1}} \\ & + \left(\frac{d\phi^n}{d\alpha_0}\delta\alpha_0 + \frac{d\phi^n}{d\alpha_1}\delta\alpha_1 + \dots + \frac{d\phi^n}{d\alpha_{n-1}}\delta\alpha_{n-1}\right)\frac{x^n}{\underline{|n}} \\ & + \left(\frac{d\phi^{n+1}}{d\alpha_0}\delta\alpha_0 + \frac{d\phi^{n+1}}{d\alpha_1}\delta\alpha_1 + \dots + \frac{d\phi^{n+1}}{d\alpha_{n-1}}\delta\alpha_{n-1}\right)\frac{x^{n+1}}{\underline{|n+1}} \\ & + \text{etc.} \\ = {} & \delta\alpha_0\left(1 + \frac{d\phi^n}{d\alpha_0}\frac{x^n}{\underline{|n}} + \frac{d\phi^{n+1}}{d\alpha_0}\frac{x^{n+1}}{\underline{|n+1}} + \dots\right) \\ & + \delta\alpha_1\left(x + \frac{d\phi^n}{d\alpha_1}\frac{x^n}{\underline{|n}} + \frac{d\phi^{n+1}}{d\alpha_1}\frac{x^{n+1}}{\underline{|n+1}} + \dots\right) \\ & + \delta\alpha_2\left(\frac{x^2}{1.2} + \frac{d\phi^n}{d\alpha_2}\frac{x^n}{\underline{|n}} + \frac{d\phi^{n+1}}{d\alpha_2}\frac{x^{n+1}}{\underline{|n+1}} + \dots\right) \\ & + \dots \end{aligned}$$

$$
\begin{aligned}
&= \delta\alpha_0 \left\{1 + \frac{d}{d\alpha_0}\left(\frac{x^n}{\underline{|n}}\,\phi^n(\theta x)\right)\right\} \\
&\quad + \delta\alpha_1 \left\{x + \frac{d}{d\alpha_1}\left(\frac{x^n}{\underline{|n}}\,\phi^n(\theta x)\right)\right\} \\
&\quad + \delta\alpha_2 \left\{\frac{x^2}{1\,.\,2} + \frac{d}{d\alpha_2}\left(\frac{x^n}{\underline{|n}}\,\phi^n(\theta x)\right)\right\} \\
&\quad + \text{etc.} \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(iii)},
\end{aligned}
$$

where θ lies between 0 and 1, and $\frac{x^n}{\underline{|n}}\,\phi^n(\theta x) = R$, say, represents the remainder after n terms of Maclaurin's expansion.

Substituting in (ii) and rearranging, it becomes

$$
\begin{aligned}
&\left\{\int (y - y')\left(1 + \frac{dR}{d\alpha_0}\right)dx\right\}\delta\alpha_0 \\
&+ \left\{\int (y - y')\left(x + \frac{dR}{d\alpha_1}\right)dx\right\}\delta\alpha_1 \\
&+ \left\{\int (y - y')\left(\frac{x^2}{1\,.\,2} + \frac{dR}{d\alpha_2}\right)dx\right\}\delta\alpha_2 \\
&+ \left\{\int (y - y')\left(\frac{x^3}{1\,.\,2\,.\,3} + \frac{dR}{d\alpha_3}\right)dx\right\}\delta\alpha_3 \\
&+ \ldots = 0.
\end{aligned}
$$

But the quantities $\alpha_0, \alpha_1, \alpha_2, \ldots \alpha_{n-1}$ are at our choice, and therefore to satisfy this equation, their variations must be independently zero. Thus we have the following equations to find $\alpha_0, \alpha_1, \ldots \alpha_{n-1}$:

$$
\left.
\begin{aligned}
\int (y - y')\left(1 + \frac{dR}{d\alpha_0}\right)dx &= 0 \\
\int (y - y')\left(x + \frac{dR}{d\alpha_1}\right)dx &= 0 \\
\int (y - y')\left(\frac{x^2}{1\,.\,2} + \frac{dR}{d\alpha_2}\right)dx &= 0 \\
\int (y - y')\left(\frac{x^3}{1\,.\,2\,.\,3} + \frac{dR}{d\alpha_3}\right)dx &= 0 \\
\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots&
\end{aligned}
\right\}
\quad\ldots\ldots\ldots\ldots\ldots\ldots\text{(iv)}.
$$

Now let A be the area, $A\mu_1$, $A\mu_2$, $A\mu_3$, $A\mu_4$, ... etc. be the first, second, third, fourth, etc. moments of the theoretical curve, and A' be the area, $A'\mu_1'$, $A'\mu_2'$, $A'\mu_3'$, $A'\mu_4'$, ... the like moments for the observation curve, moments being taken round the axis of y (which is of course any axis). Then the above equations may be written

$$\left.\begin{aligned}
A &= A' - \int (y - y') \frac{dR}{d\alpha_0}\, dx \\
A\mu_1 &= A'\mu_1' - \int (y - y') \frac{dR}{d\alpha_1}\, dx \\
A\mu_2 &= A'\mu_2' - 2 \int (y - y') \frac{dR}{d\alpha_2}\, dx \\
A\mu_3 &= A'\mu_3' - \underline{|3} \int (y - y') \frac{dR}{d\alpha_3}\, dx \\
A\mu_4 &= A'\mu_4' - \underline{|4} \int (y - y') \frac{dR}{d\alpha_4}\, dx \\
&\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots \\
A\mu_{n-1} &= A'\mu'_{n-1} - \underline{|n-1} \int (y - y') \frac{dR}{d\alpha_{n-1}}\, dx
\end{aligned}\right\} \quad \text{...............(v).}$$

Now the integral term in these equations must clearly be small because

(i) It involves the small factor $y - y'$.

(ii) R, the remainder, $= \frac{x^n}{\underline{|n}} \phi^n (\theta x)$ will by hypothesis be small, if n is at all considerable. Hence neglecting the integral terms, we find

$$\left.\begin{aligned}
A &= A' \\
\mu_1 &= \mu_1' \\
\mu_2 &= \mu_2' \\
\mu_3 &= \mu_3' \\
&\cdots\cdots \\
\mu_{n-1} &= \mu'_{n-1}
\end{aligned}\right\} \quad \text{.......................................(vi).}$$

Or, the constants of the theoretical curve are to be found by equating its area and first $n - 1$ moments to the area and first $n - 1$ moments of the observed curve. These results having been obtained we may at once replace $\alpha_0, \alpha_1, \alpha_2, \ldots \alpha_{n-1}$ by the real constants $c_1, c_2, c_3, \ldots c_n$ of the theoretical curve, and we obtain the rule:

To fit a good theoretical curve $y = \phi (x, c_1, c_2, c_3, \ldots c_n)$ *to an observed curve, express the area and moments of the curve for the given range of observation in terms of* $c_1, c_2, c_3, \ldots c_n$, *and equate these to the like quantities for the observations.*

The moments may be taken about any axis parallel to y likely to simplify the results, e.g. the mid-vertical of the range or in other cases the centroid vertical.

Returning to equations (v) we see that the solution (vi) is even more approximate than might at first sight be imagined. For if we render identical the first

$n-1$ moments of the two curves, the higher moments of the curves become *ipso facto* more and more nearly identical the larger n may be. But such a term as

$$\int (y-y')\frac{dR}{d\alpha_s}\,dx$$

vanishes if the higher moments are equal; for we may write

$$R = \frac{x^n}{\underline{|n}}\phi^n(0) + \frac{x^{n+1}}{\underline{|n+1}}\phi^{n+1}(0) + \ldots,$$

and accordingly

$$\begin{aligned}\int (y-y')\frac{dR}{d\alpha_s}\,dx = {} & \frac{d\phi^n(0)}{d\alpha_s}\frac{1}{\underline{|n}}(A\mu_n - A'\mu_n') \\ & + \frac{d\phi^{n+1}(0)}{d\alpha_s}\frac{1}{\underline{|n+1}}(A\mu_{n+1} - A'\mu'_{n+1}) \\ & + \frac{d\phi^{n+2}(0)}{d\alpha_s}\frac{1}{\underline{|n+2}}(A\mu_{n+2} - A'\mu'_{n+2}) \\ & + \text{etc.}\end{aligned}$$

Thus if $A = A'$, we have the factors $(\mu_n - \mu_n')$, $(\mu_{n+1} - \mu'_{n+1})$, $(\mu_{n+2} - \mu'_{n+2})$, etc. Thus besides the smallness of the factors $\frac{\phi^n(0)}{\underline{|n}}$, $\frac{\phi^{n+1}(0)}{\underline{|n+1}}$, ... etc., depending on the hypothesis of convergency in Maclaurin's expansion, we have the smallness of the factors $\mu_n - \mu_n'$, $\mu_{n+1} - \mu'_{n+1}$, ... depending on the fact that if $n-1$ moments of a curve are equal, the succeeding ones will be nearly equal.

We conclude accordingly that equality of moments gives a good method of fitting curves to observations. It is this method of moments which I venture to suggest as a good systematic process, preferable to those in ordinary use when the method of least squares is too laborious or impracticable, for determining the constants of empirical or theoretical curves from observation. This is really the method which has been long in constant use for fitting the normal curve of errors $y = y_0 e^{-x^2/2\sigma^2}$ to observations; it has been largely adopted by myself in fitting skew frequency curves to observations*; and it becomes identical with that of least squares when we fit parabolic curves of any order to observations. It is then no approximation, but the accurate solution, for the expansion by Maclaurin's Theorem is finite.

One great advantage of the method, as will be illustrated below, is that it enables us to determine in many cases the whole of the theoretical curve from a part, if the observations can only be made along a portion of the range.

There are three essentials to the application of the method:

(*a*) We must be able to ascertain the moments of the theoretical curve in terms of $c_1, c_2, c_3, \ldots c_n$.

---

* *Phil. Trans.*, Vol. 186, A, pp. 343—414.

(*b*) We must know how to find the moments of any system of observations.

(*c*) The expressions for the moments in terms of $c_1, c_2, c_3, \dots c_n$ must be such that we can easily solve the system of equations (vi).

I propose now to consider these points in some detail, starting with the second.

(2) *On the discovery of the area and moments of a curve given by a series of isolated observations.*

The isolated observations may be of two kinds:

(*a*) *Actual measurements may have been made of the ordinates of the curve at p points.*

This is the most common case in physical investigations, but it is not infrequent in economic and actuarial enquiries, e.g. the mean age of bridegroom for brides of a given age, or the mean number of years of insurance of those that die at a certain age.

(*b*) *The actual measurements may represent the areas for certain base elements, p in number, of a given curve.*

This latter is the usual case of frequency observations. We determine the number of individual cases which fall into each of a small series of ranges of some vital or economic variable, e.g. the number of deaths, which under certain circumstances occur in each year of life, the number of individuals which fall into each small range of a particular organ or character, etc. This is the type of data on which "frequency curves" are based. Actually (*b*) would sensibly coincide with (*a*) if we took our elementary ranges for classification, extremely small. This, owing either to roughness and paucity of data, or to the immense labour involved, is very often practically impossible. Not uncommonly (*a*) is used for (*b*), and for a great majority of cases the work is close enough for the value of the observations. But for fine and important work it is desirable to keep the two classes of cases essentially distinct.

Case (i). *p ordinates of a curve are observed or measured, to find its area and moments.*

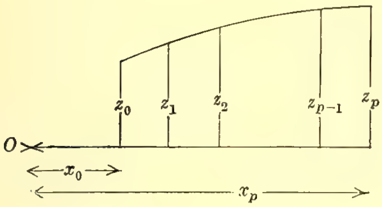
What we require is clearly

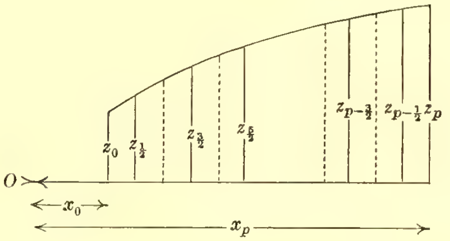
$$A\mu_n = \int_{x_0}^{x_p} yx^n dx,$$

from a knowledge of $z = yx^n$ at p points. The answer to this problem is familiar, and consists in the choice of a good quadrature formula. Whether we are dealing with the ordinates y simply, or the more complex moments, yx^n, will make theoretically no difference, except that in the latter case we may have to go to higher differences for the purpose of reaching accuracy.

If I venture here to deal at some length with quadrature formulæ, it is because the choice of a good formula is essential to the application of the method of moments. At the same time, although much will be familiar, there are new and novel points to which I want to draw special attention. For this portion of my paper I am chiefly indebted to the kindness of Mr W. F. Sheppard of Trinity College, Cambridge. I told him that I wanted the best correctional terms for the tangential and chordal areas, and the working-out of the system of formulæ is entirely due to him.

An area may be looked at as given in two ways: (i) by extreme ordinates, or (ii) by mid-ordinates. The former we will represent by $z_0, z_1, z_2, z_3, \ldots z_p$ and the latter by $z_{\frac{1}{2}}, z_{\frac{3}{2}}, z_{\frac{5}{2}}, \ldots z_{p-\frac{3}{2}}, z_{p-\frac{1}{2}}$. These ordinates will be supposed taken at equal distances, h, and for the purposes of practical calculation, h can nearly always be taken as our horizontal unit. We have thus the two schemes:





For these cases we have respectively the Euler-Maclaurin formulæ

$$\int_{x_0}^{x_p} z\,dx = A_C + h\,(\gamma_1\Delta - \gamma_2\Delta^2 + \gamma_3\Delta^3 - \gamma_4\Delta^4 + \ldots)\,(z_0 + z_p) \ldots\ldots\ldots\ldots (\alpha),$$

$$\int_{x_0}^{x_p} z\,dx = A_T - h\,(\gamma_1'\Delta - \gamma_2'\Delta^2 + \gamma_3'\Delta^3 - \gamma_4'\Delta^4 + \ldots)\,(z_{\frac{1}{2}} + z_{p-\frac{1}{2}}) \ldots\ldots (\beta).$$

Here $$A_C = h\,(\tfrac{1}{2}z_0 + z_1 + z_2 + \ldots + z_{p-1} + \tfrac{1}{2}z_p),$$

and $$A_T = h\,(z_{\frac{1}{2}} + z_{\frac{3}{2}} + z_{\frac{5}{2}} + \ldots - z_{p-\frac{3}{2}} + z_{p-\frac{1}{2}}),$$

are respectively the areas of the chordal and tangential series of trapezia. Thus the formulæ (α) and (β) give the corrections which are respectively to be added and subtracted from what we may term for brevity the chordal and tangential areas in order to obtain the curved area.

In the above formulæ Δ operating on z_p and $z_{p-\frac{1}{2}}$ must be taken *backwards*, i.e. $\Delta z_p = z_{p-1} - z_p$ and $\Delta z_{p-\frac{1}{2}} = z_{p-\frac{3}{2}} - z_{p-\frac{1}{2}}$, while

$$\Delta z_0 = z_1 - z_0, \quad \Delta z_{\frac{1}{2}} = z_{\frac{3}{2}} - z_{\frac{1}{2}}.$$

The values of the coefficients γ are as follows* :

$\gamma_1 = \cdot 083,3333$ $\qquad \gamma_1' = \cdot 041,6667$
$\gamma_2 = \cdot 041,6667$ $\qquad \gamma_2' = \cdot 041,6667$
$\gamma_3 = \cdot 026,3889$ $\qquad \gamma_3' = \cdot 038,7153$
$\gamma_4 = \cdot 018,7500$ $\qquad \gamma_4' = \cdot 035,7639$
$\gamma_5 = \cdot 014,2692$ $\qquad \gamma_5' = \cdot 033,1918$
$\gamma_6 = \cdot 011,3674$ $\qquad \gamma_6' = \cdot 030,9989$
$\gamma_7 = \cdot 009,3565$ $\qquad \gamma_7' = \cdot 029,1253$
$\gamma_8 = \cdot 007,8925$ $\qquad \gamma_8' = \cdot 027,5110$
$\gamma_9 = \cdot 006,7858$ $\qquad \gamma_9' = \cdot 026,1066$
$\gamma_{10} = \cdot 005,9241$ $\qquad \gamma_{10}' = \cdot 024,8732$
$\gamma_{11} = \cdot 005,2367$ $\qquad \gamma_{11}' = \cdot 025,7807$
$\gamma_{12} = \cdot 004,6775$ $\qquad \gamma_{12}' = \cdot 022,8052$
$\gamma_{13} = \cdot 004,2150$
$\gamma_{14} = \cdot 003,8269$

Now the Euler-Maclaurin formulæ possess marked merits and defects:

(*a*) The correction terms being usually small, they equally weight all the observations in the bulk A_C and A_T of the formula†. This is of much importance when the observations are liable to considerable error.

(*b*) They will give the best possible results if we go to the complete system of differences for the p ordinates.

But:

(*c*) To do this involves in most cases very great labour. The coefficients γ do not converge very rapidly, and the Δ's in many practical cases, especially of frequency, do not get rapidly small.

(*d*) If we stop at the third or fourth difference, then the γ coefficients are not the best coefficients by which to multiply the successive differences, but the best coefficients differ considerably from these if p be not very large.

In order to get over (*c*) a number of formulæ have been used which depend upon the number of ordinates used being a multiple of 2, 3, 4, 6, etc. Thus we have the following rules:

Simpson's Rule ($2p$ elements),

$$\int_{x_0}^{x_{2p}} z\,dx = \tfrac{1}{3}h\,\{z_0 + 2\,(z_2 + z_4 + \dots + z_{2p-2}) + z_{2p}$$
$$+ 4\,(z_1 + z_3 + \dots + z_{2p-1})\} \quad \dots\dots\dots\dots\dots\dots\dots\dots (\gamma).$$

---

* Calculated from the formulæ given by De Morgan; *Differential and Integral Calculus*, Art. 61, p. 262.

† Except in the case of the first and last ordinates of A_C, which clearly can only be given half weight.

Newton's Rule ($3p$ elements),

$$\int_{x_0}^{x_{3p}} z\,dx = \tfrac{3}{8}h\,\{z_0 + 3\,(z_1 + z_2 + z_4 + z_5 + z_7 + z_8 + \ldots) + z_{3p} + 2\,(z_3 + z_6 + \ldots + z_{3p-3})\} \quad\ldots\ldots\ldots\ldots(\delta).$$

*Boole's Rule** ($4p$ elements),

$$\int_{x_0}^{x_{4p}} z\,dx = \tfrac{2}{45}h\,\{7z_0 + 14\,(z_4 + z_8 + \ldots + z_{4p-4}) + 7z_{4p} + 32\,(z_1 + z_3 + z_5 + \ldots + z_{4p-1}) + 12\,(z_2 + z_6 + z_{10} + \ldots + z_{4p-2})\} \quad\ldots\ldots\ldots\ldots(\epsilon).$$

Weddle's Rule ($6p$ elements),

$$\int_{x_0}^{x_{6p}} z\,dx = \tfrac{3}{10}h\,\{z_0 + z_2 + z_4 + z_8 + z_{10} + \ldots + z_{6p-2} + z_{6p} + 2\,(z_6 + z_{12} + \ldots + z_{6p-6}) + 5\,(z_1 + z_5 + z_7 + \ldots + z_{6p-1}) + 6\,(z_3 + z_9 + z_{15} + \ldots + z_{6p-3})\} \quad\ldots\ldots\ldots\ldots(\zeta).$$

All these rules give with increasing exactness the value of the integral, but they suffer under obvious disadvantages:

(*a*) The number of elements cannot often be selected beforehand, and if for example there be 7 or 11 or 13 a new rule has at once to be worked out.

(*b*) The multiplying different ordinates by different factors is a source fruitful of arithmetical slips.

(*c*) The multiplying of certain ordinates by factors much larger than others, multiplies the error made in the determination of certain ordinates largely. We do not give equal weight to all the ordinates.

Thus, while formulæ like (ϵ) or (ζ) give extremely good results, especially for the integration of continuous mathematical functions, and this with less work than (α) or (β), they do not seem advantageous for what we may term observation-curves. Accordingly Mr Sheppard has determined† the best coefficients for the corrections to the chordal and tangential areas when one, two or three differences only are used. He has provided the following quadrature formulæ which seem to me of much interest and practical value.

---

* I do not know who originated this rule; it is given in Boole's *Finite Differences.*

† Mr Sheppard, since this memoir was written, has given the proofs of his formulæ, *L. Math. Soc, Proc.*, Vol. 32, p. 270.

Case (i). *Bounding ordinates or chordal area known.*

(*a*) *One Difference:*

$$\text{Area} = A_C + \frac{1}{12}\frac{p}{(p-1)}\{(z_1 - z_0) - (z_p - z_{p-1})\}\,h \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(\eta).$$

If we take $p/(p-1)$ to be approximately unity, this formula reduces to (α) retaining only the first difference.

(*b*) *Two Differences:*

$$\begin{aligned}\text{Area} = A_C &+ \frac{1}{120}\frac{p(15p-26)}{(p-1)(p-2)}\{(z_1 - z_0) - (z_p - z_{p-1})\}\,h \\ &- \frac{1}{120}\frac{p(5p-6)}{(p-2)(p-3)}\{(z_2 - z_1) - (z_{p-1} - z_{p-2})\}\,h \ldots\ldots\ldots\ldots(\theta).\end{aligned}$$

(*c*) *Three Differences:*

$$\begin{aligned}\text{Area} = A_C &+ \frac{1}{5040}\frac{p(763p^2 - 3444p + 3636)}{(p-1)(p-2)(p-3)}\{(z_1 - z_0) - (z_p - z_{p-1})\}\,h \\ &- \frac{1}{1260}\frac{p(119p^2 - 504p + 432)}{(p-2)(p-3)(p-4)}\{(z_2 - z_1) - (z_{p-1} - z_{p-2})\}\,h \\ &+ \frac{1}{5040}\frac{p(133p^2 - 462p + 360)}{(p-3)(p-4)(p-5)}\{(z_3 - z_2) - (z_{p-2} - z_{p-3})\}\,h \ldots\ldots(\iota).\end{aligned}$$

Case (ii). *Mid-ordinates or tangential area known.*

(*a*) *One Difference:*

$$\text{Area} = A_T - \frac{1}{24}\frac{p}{(p-2)}\{(z_{\frac{3}{2}} - z_{\frac{1}{2}}) - (z_{p-\frac{1}{2}} - z_{p-\frac{3}{2}})\}\,h \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(\kappa).$$

(*b*) *Two Differences:*

$$\begin{aligned}\text{Area} = A_T &- \frac{1}{960}\frac{p(80p-177)}{(p-2)(p-3)}\{(z_{\frac{3}{2}} - z_{\frac{1}{2}}) - (z_{p-\frac{1}{2}} - z_{p-\frac{3}{2}})\}\,h \\ &+ \frac{1}{960}\frac{p(40p-57)}{(p-3)(p-4)}\{(z_{\frac{5}{2}} - z_{\frac{3}{2}}) - (z_{p-\frac{3}{2}} - z_{p-\frac{5}{2}})\}\,h \ldots\ldots\ldots\ldots(\lambda).\end{aligned}$$

This formula has many advantages, it is more exact than (κ), and although less so than (μ) is sufficient for most practical purposes. It weights in the bulk of the formula, A_T, all the ordinates equally and thus is superior to those of Case (i) which give only half-weight to the terminal ordinates. In order to facilitate its use, writing it in the form

$$\text{Area} = A_T - P\{(z_{\frac{3}{2}} - z_{\frac{1}{2}}) - (z_{p-\frac{1}{2}} - z_{p-\frac{3}{2}})\}\,h + Q\{(z_{\frac{5}{2}} - z_{\frac{3}{2}}) - (z_{p-\frac{3}{2}} - z_{p-\frac{5}{2}})\}\,h,$$

the values of P and Q have been tabulated for 8 to 20 ordinates inclusive by Mr Leslie Bramley-Moore. They are as follows:

p	P	Q
8	·128,6111	·109,5833
9	·121,2054	·094,6875
10	·115,8854	·085,0694
11	·111,8779	·078,3668
12	·108,7500	·073,4375
13	·106,2405	·069,6644
14	·104,1825	·066,6856
15	·102,4639	·064,2756
16	·101,0073	·062,2863
17	·099,7569	·060,6170
18	·098,6719	·059,1964
19	·097,7214	·057,9731
20	·096,8818	·056,9087

This formula will give results more close as a rule than Simpson's, and it possesses the great advantage of only weighting particular ordinates in the correctional terms.

(*c*) *Three Differences:*

$$\begin{aligned}\text{Area} = A_T &- \frac{1}{80640}\frac{p\,(9842p^2 - 53970p + 70407)}{(p-2)(p-3)(p-4)}\{(z_{\frac{3}{2}} - z_{\frac{1}{2}}) - (z_{p-\frac{1}{2}} - z_{p-\frac{3}{2}})\}\,h \\ &+ \frac{1}{40320}\frac{p\,(4802p^2 - 23016p + 22905)}{(p-3)(p-4)(p-5)}\{(z_{\frac{5}{2}} - z_{\frac{3}{2}}) - (z_{p-\frac{3}{2}} - z_{p-\frac{5}{2}})\}\,h \\ &- \frac{1}{80640}\frac{p\,(3122p^2 - 12222p + 10935)}{(p-4)(p-5)(p-6)}\{(z_{\frac{7}{2}} - z_{\frac{5}{2}}) - (z_{p-\frac{5}{2}} - z_{p-\frac{7}{2}})\}\,h \\ &\qquad\qquad\ldots\ldots\ldots\ldots\ldots\ldots\ldots(\mu).\end{aligned}$$

Special and occasionally useful Cases.

Case (iii). *Mid-ordinates and two extreme ordinates known.*

(*a*) *One Difference:*

$$\text{Area} = A_T - \frac{1}{12}\frac{2p}{(2p-1)}\{(z_{\frac{1}{2}} - z_0) - (z_p - z_{p-\frac{1}{2}})\}\,h\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(\nu).$$

If $2p/(2p-1)$ be taken as approximately unity, this becomes a formula well-known on the continent as Parmentier's.

(*b*) *Two Differences:*

$$\begin{aligned}\text{Area} = A_T &- \frac{1}{180}\frac{2p\,(40p-57)}{(2p-1)(2p-3)}\{(z_{\frac{1}{2}} - z_0) - (z_p - z_{p-\frac{1}{2}})\}\,h \\ &+ \frac{1}{180}\frac{2p\,(5p-6)}{(2p-2)(2p-3)}\{(z_{\frac{3}{2}} - z_{\frac{1}{2}}) - (z_{p-\frac{1}{2}} - z_{p-\frac{3}{2}})\}\,h\ldots\ldots\ldots\ldots(\xi).\end{aligned}$$

Case (iv). *Bounding ordinates with the two mid-ordinates only of the terminal elements.*

(*a*) *One Difference:*

$$\text{Area} = A_C + \frac{1}{6}\frac{2p}{(2p-1)}\{(z_{\frac{1}{2}} - z_0) - (z_p - z_{p-\frac{1}{2}})\}\,h \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (o).$$

(*b*) *Two Differences:*

$$\text{Area} = A_C + \frac{1}{120}\frac{2p\,(30p-29)}{(2p-1)(p-1)}\{(z_{\frac{1}{2}} - z_0) - (z_p - z_{p-\frac{1}{2}})\}\,h$$
$$- \frac{1}{120}\frac{2p\,(10p-9)}{(2p-3)(p-1)}\{(z_1 - z_{\frac{1}{2}}) - (z_{p-\frac{1}{2}} - z_{p-1})\}\,h \;\ldots\ldots\ldots\ldots\ldots (\pi).$$

If p be fairly large this is not very divergent from

$$\text{Area} = A_C + \tfrac{1}{4}\{(z_{\frac{1}{2}} - z_0) - (z_p - z_{p-\frac{1}{2}})\}\,h - \tfrac{1}{12}\{(z_1 - z_{\frac{1}{2}}) - (z_{p-\frac{1}{2}} - z_{p-1})\}\,h \ldots\ldots (\rho),$$

which may be obtained directly by a double application of Simpson's Formula, and is somewhat more exact than the latter.

It is, perhaps, worth while exhibiting the sort of relative exactness to be obtained by the whole series of formulæ on a special example, say $\int_0^1 \frac{dx}{1+x}$ for 12 or 13 ordinates. We find

$$\int_0^1 \frac{dx}{1+x} = \cdot 693{,}147{,}18.$$

	Divergence
(α), with four differences,	$+ \cdot 000{,}000{,}25$
(β), with four differences,	$- \cdot 000{,}000{,}59$
(γ)	$+ \cdot 000{,}001{,}48$
(δ)	$+ \cdot 000{,}003{,}28$
(ϵ)	$+ \cdot 000{,}000{,}07$
(ζ)	$+ \cdot 000{,}000{,}04$
(η)	$+ \cdot 000{,}014{,}59$
(θ)	$+ \cdot 000{,}000{,}93$
(ι)	$+ \cdot 000{,}000{,}07$
(κ)	$- \cdot 000{,}014{,}93$
(λ)	$- \cdot 000{,}001{,}26$
(μ)	$- \cdot 000{,}000{,}12$
(ν)	$- \cdot 000{,}003{,}91$
(ξ)	$- \cdot 000{,}000{,}14$
(o)	$+ \cdot 000{,}008{,}12$
(π)	$+ \cdot 000{,}000{,}22$
(ρ)	$+ \cdot 000{,}001{,}27$

It may be noticed also that*

$$A_C = \cdot 693{,}580{,}83, \text{ or } \Delta = + \cdot 000{,}433{,}65,$$
$$A_T = \cdot 692{,}930{,}49, \text{ or } \Delta = - \cdot 000{,}216{,}69.$$

The latter is less divergent from the true value than the former, but they differ by as much as 1 in 3200 and 1 in 1600 respectively from the true value. On the other hand the worst of the above quadrature formulæ (κ) and (η) give results only about 1 in 48,000 in error, while the best, like Boole's or Weddle's Rules, or (ι) and (μ), vary from about 1 in 6,000,000 to 1 in 17,000,000, while (ξ) and (π) are almost as good. When we are dealing with frequency we probably never, and often when we are dealing with measurements, physical or economic, we do not, know our data with anything like the accuracy of 1 in 48,000. We conclude therefore that we may expect good results from most of these formulæ. But some remarks on their relative goodness may be of service. In the first place the Euler-Maclaurin formulæ (α) and (β) with *four* differences are not nearly as good as Mr Sheppard's new formulæ (ι) and (μ) using only *three* differences, and not so good as (ξ) or (π) with *two* differences. It seems to me accordingly that unless we are prepared to go to great labour and calculate high differences, (ι), (μ), (ξ) or (π) are the best formulæ to use, and that for nearly all practical purposes (θ) and (λ) are quite accurate enough for use. Boole's Rule (ϵ) and Weddle's Rule (ζ) give splendid results, but great care must be taken when we apply them to somewhat irregular observations of physical quantities and to frequencies, and not to the evaluation of mathematical integrals, for in the bulk of the formulæ they weight and largely weight certain ordinates, and thus may tend to emphasise errors in particular observations.

(3) It seems well to illustrate the application of these formulæ to a special case, although in doing so I anticipate some of the results to be reached later. Let us try and fit by the method of moments a parabola of the third order to the following data:

$x = 0$	$y = \cdot 382$	$x = \cdot 6$	$= 1{\cdot}270$
1	·674	·7	1·215
2	·923	·8	1·137
3	1·104	·9	·989
4	1·214	1·0	·819
5	1·273		

These data are really a series of measurements on Aneroid Barometers published by Dr Chree in a paper in the *Phil. Trans.*, Vol. 191, A., p. 448. They will serve as well as any others, however, as an illustration of method.

---

* Clearly: Area $= A_T - \frac{1}{3}(A_T - A_C)$, nearly. This is a very useful formula—based on an assumption as to parabolic segments like Simpson's—when both extreme and mid-ordinates are available.

We want to determine the values of the constants a, b, c, d, when a curve of type

$$y = a + bx + cx^2 + dx^3$$

is fitted to the above data.

In using the method of moments we require to evaluate $S(yx^n)$ up to $n = 3$ from a knowledge of its value at a number of isolated points. In order to do this we require to use a quadrature formula, and the exactness of our results will increase as we use better formulæ. The object of this illustration is to show the increasing accuracy of different quadrature formulæ. The actual values of a, b, c, d are given in terms of the moments in the second part of this paper. In calculating the moments $x = 5$ was taken as origin, and in each case the same quadrature formula was used for the area and all the moments. The following methods were used,—R. M. S. stands for root mean square of the error of ordinate at the 11 given points:—

I. The curve was taken through four selected points. This method was adopted by Dr Chree, and I have merely transferred the result obtained by him to the centre of the range:

$$y = 1{\cdot}269{,}100 + {\cdot}024{,}000x - {\cdot}027{,}320x^2 + {\cdot}000{,}969x^3,$$

$$\text{R. M. S.} = {\cdot}0126.$$

II. The area and moments were evaluated by treating A_T as if it were A:

$$y = 1{\cdot}270{,}290 + {\cdot}033{,}402x - {\cdot}026{,}806x^2 + {\cdot}000{,}3279x^3,$$

$$\text{R. M. S.} = {\cdot}0094.$$

III. The area and moments were evaluated by Parmentier's Rule, or (ν) with $2p/(2p-1)$ put unity:

$$y = 1{\cdot}263{,}808 + {\cdot}032{,}311x - {\cdot}026{,}380x^2 + {\cdot}000{,}4113x^3,$$

$$\text{R. M. S.} = {\cdot}0089.$$

IV. The area and moments were evaluated by Simpson's Rule (γ):

$$y = 1{\cdot}270{,}130 + {\cdot}027{,}046x - {\cdot}027{,}180x^2 + {\cdot}000{,}7326x^3,$$

$$\text{R. M. S.} = {\cdot}0070.$$

V. The area and moments were evaluated by Sheppard's Rule (λ):

$$y = 1{\cdot}268{,}898 + {\cdot}029{,}388x - {\cdot}026{,}853x^2 + {\cdot}000{,}5764x^3,$$

$$\text{R. M. S.} = {\cdot}0057.$$

VI. The curve was fitted by the method of least squares:

$$y = 1{\cdot}268{,}800 + {\cdot}028{,}700x - {\cdot}026{,}880x^2 + {\cdot}000{,}6167x^3,$$

$$\text{R. M. S.} = {\cdot}0055.$$

Now these results show us at once that with (λ) we have a fit by the method of moments sensibly as good as that obtained by the method of least squares. Had we used (ι) or (μ) there could not have been any difference in the R. M. S. between the method of moments and the method of least squares. There is of course a distinction between the two methods which it is important to bear in mind,

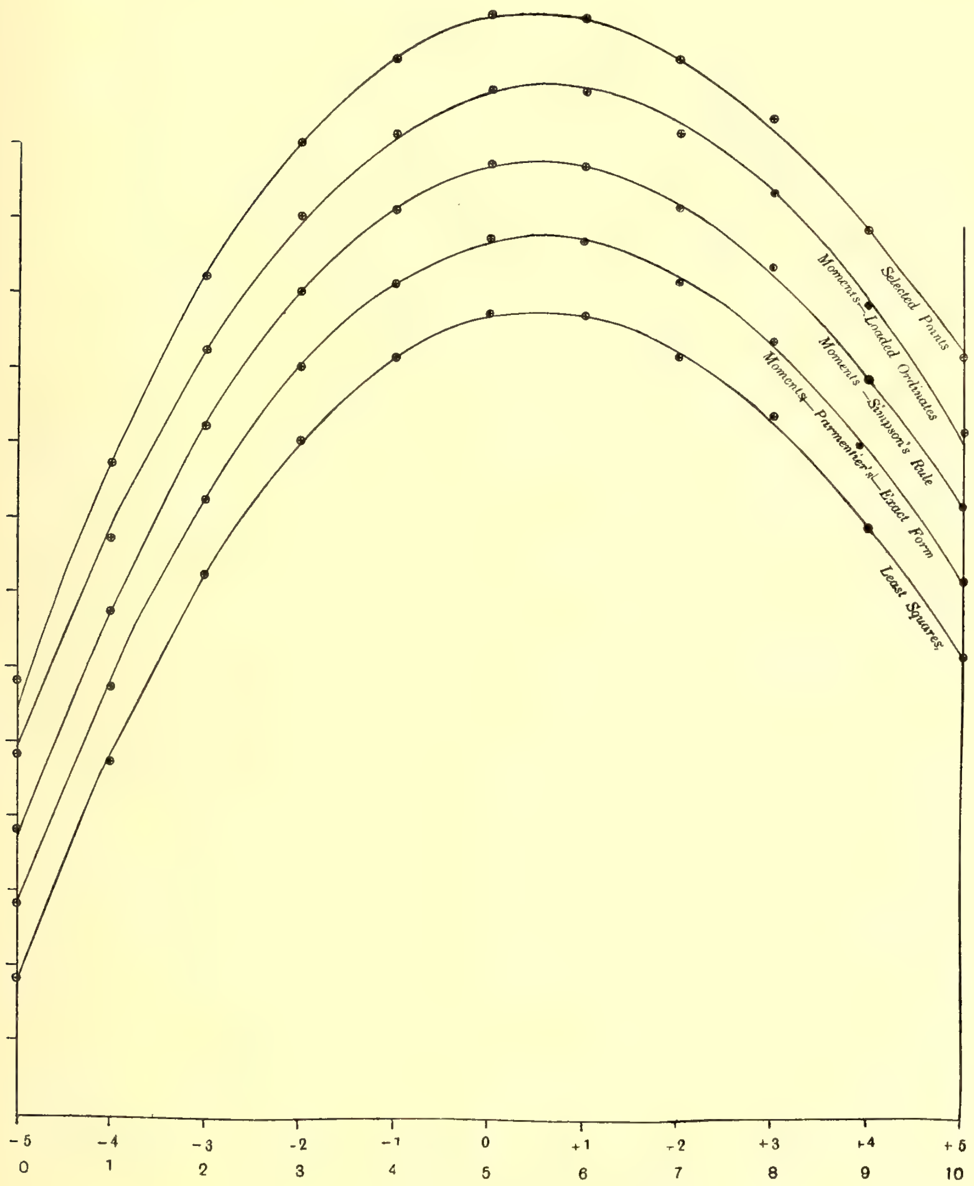


FIG. 1. Fitting of Parabolas to Observations.

namely: the method of least squares takes a curve which passes with the least root mean square deviation from 11 observation points; the method of moments takes a curve which has the least root mean square deviation from *all* the points of some smooth curve with a moment system determined by the 11 points. Hence it is quite possible that the method of moments may actually give better results than the method of least squares in such a case as the above, if after the determination of the curve it becomes necessary to compare experience and observation at other points than the eleven used in the first determination.

Fig. 1 gives the theoretical curves and the points of observation in cases I, II, IV, V, and VI.

(4) CASE (ii). *The frequency z of individuals falling within p elementary ranges of a total range ph is observed or measured, to determine the true mean and moments of the system.*

Let $y = f(x)$ be the curve giving the frequency distribution, and z_r the frequency observed within the range of the variable x from $x = x_{r-1}$ to $x = x_r$. Then what we actually observe are

$$z_1 = \int_{x_0}^{x_1} y dx, \quad z_2 = \int_{x_1}^{x_2} y dx, \ \dots \ z_p = \int_{x_{p-1}}^{x_p} y dx.$$

Let N be the total number of observations, or

$$N = z_1 + z_2 + \dots + z_p.$$

For the nth moment about a line through the origin perpendicular to the range we require

$$N\mu_n' = \int_{x_0}^{x_p} x^n y dx.$$

Now let
$$Z = \int_{x}^{x_p} y dx,$$

i.e. be all the frequency from $x = x$ to $x = x_p$, or above the value x. Then

$$Z_0 = \int_{x_0}^{x_p} y dx, \quad Z_1 = \int_{x_1}^{x_p} y dx, \ \dots \ Z_p = \int_{x_p}^{x_p} y dx$$

are known and given by

$$N, \quad z_2 + z_3 + \dots + z_p, \quad z_3 + z_4 + \dots + z_p, \quad z_4 + z_5 + \dots + z_p, \ \dots\dots \ z_p, \quad 0.$$

Since $dZ/dx = -y$, we have

$$\begin{aligned} N\mu_n' &= -\int_{x_0}^{x_p} x^n \frac{dZ}{dx} dx \\ &= \Big[-Zx^n\Big]_{x_0}^{x_p} + n\int_{x_0}^{x_p} x^{n-1} Z dx \\ &= Z_0 x_0^n + n\int_{x_0}^{x_p} Z x^{n-1} dx. \end{aligned}$$

Thus
$$\mu_n' = x_0^n + \frac{n}{N}\int_{x_0}^{x_p} Z x^{n-1} dx \quad \dots\dots\dots\dots\dots\dots\dots\dots\text{(vii)}.$$

This is the fundamental formula for finding the true moments of frequency distributions from the grouped frequencies. The rule is clear. In order to evaluate $\int_{x_0}^{x_p} Zx^{n-1}dx$, since we know the value of Zx^{n-1} for $x = x_0, x_1, x_2, \dots x_p$, we have to find the area of a curve of which we are given $p+1$ ordinates; we have accordingly to use the best available quadrature formulæ, taking care that the exactness of the formula corresponds to the degree of the moment investigated.

For practical working, since $Z_0 = N$ is large, it is convenient to take $x_0 = 0$, and our formula then becomes

$$\mu_n' = \frac{n}{N}\int_0^{x_p} Zx^{n-1}dx \quad \dots\dots\dots\dots\dots\dots\dots\dots\text{(viii)}.$$

Here we must be very careful to notice that our origin is the start of the base-element in which the frequency begins, that $Z_0 = N$ is the total frequency, and Z_p is zero, and that x_p is measured to the end of the last base-element h for which we are considering the frequency. Thus a length $x_p + h$, and not x_p, would be the total range we should obtain by plotting the frequencies z as if they were ordinates at the middle of the elements. This process therefore tends to exaggerate the range. As a rule it is convenient in frequency distributions to determine the μ's about the mean. In this case they may be found from the μ''s about any other line by the formulæ

$$\left.\begin{aligned}
&\mu_1 = 0,\\
&\mu_2 = \mu_2' - \mu_1'^2,\\
&\mu_3 = \mu_3' - 3\mu_1'\mu_2' + 2\mu_1'^3,\\
&\mu_4 = \mu_4' - 4\mu_1'\mu_3' + 6\mu_1'^2\mu_2' - 3\mu_1'^4,\\
&\mu_5 = \mu_5' - 5\mu_1'\mu_4' + 10\mu_1'^2\mu_3' - 10\mu_1'^3\mu_2' + 4\mu_1'^5,\\
&\mu_6 = \mu_6' - 6\mu_1'\mu_5' + 15\mu_1'^2\mu_4' - 20\mu_1'^3\mu_3' + 15\mu_1'^4\mu_2' - 5\mu_1'^6
\end{aligned}\right\} \dots\dots \text{(ix)}.$$

Should the frequency observations we are dealing with cover a complete distribution we can proceed somewhat differently. Let $y = f(x)$ be the frequency distribution and let it be absolutely confined within the range l of x. If we take $x = 0$, at one end of this range we have for the integral curve $Z = \int_x^l ydx$. Now, whatever be the form of the frequency distribution, whether it gives a curve of high contact or not at $x = 0$ and $x = l$, it must follow, if the range be absolutely limited, that $Z = 0$ for $x = l$, and $Z = \text{constant} = N$ at $x = 0$. But usually there is contact of a high order at one or both ends of the range. I shall therefore work out the modifications which must be made in the moments when there is high contact at one end at least, say at $x = l$.

Thus for $x = l$, we have

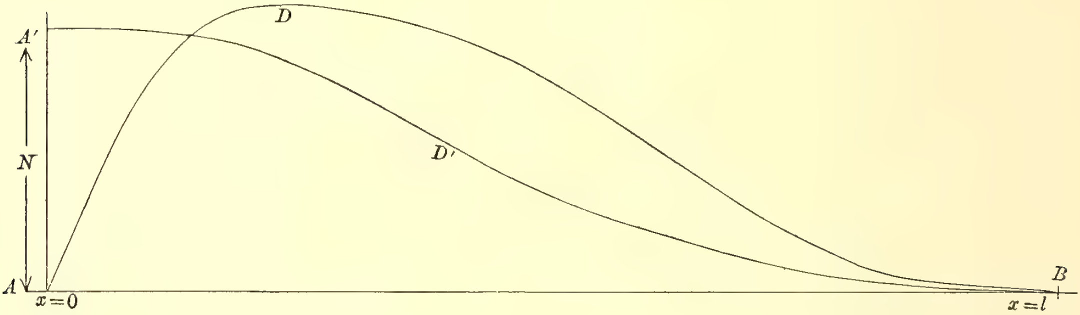
$$Z = 0, \quad \frac{dZ}{dx} = -y = 0, \quad \frac{d^2Z}{dx^2} = 0, \quad \frac{d^3Z}{dx^3} = 0, \text{ etc.},$$

and for $x = 0$ we have

$$Z = N, \quad \frac{dZ}{dx} = 0, \quad \frac{d^2Z}{dx^2} = a_2, \ldots \frac{d^sZ}{dx^s} = a_s, \ldots,$$

where $a_2, a_3, \ldots a_s, \ldots$ define the contact of the integral curve at the origin with the line $Z = N$, and will be supposed for the time being known.

The frequency curve and its integral curve will accordingly take the form indicated in the diagram below.



Now by the Euler-Maclaurin formula

$$\int_0^l Z'dx = (\tfrac{1}{2}Z_0' + Z_1' + Z_2' + \ldots + Z'_{l-1} + \tfrac{1}{2}Z_l')\, h$$
$$- h\left[\frac{B_1h}{\underline{|2}}\frac{dZ'}{dx} - \frac{B_3h^3}{\underline{|4}}\frac{d^3Z'}{dx^3} + \frac{B_5h^5}{\underline{|6}}\frac{d^5Z'}{dx^5} - \ldots\right]_0^l.$$

The expression in square brackets vanishes at the upper limit not only for $Z' = Z$, but for $Z' = Zx^s$,—since every differential containing either Z or one of its differential coefficients is zero at $x = l$.

At the lower limit we have by applying Leibnitz's Theorem

$$\left[\frac{d^n(Zx^s)}{dx^n}\right]_0 = n(n-1)(n-2)\ldots(n-s+1)\frac{d^{n-s}Z}{dx^{n-s}}$$
$$= \frac{\underline{|n}}{\underline{|n-s}}\, a_{n-s},$$

provided n be greater than s, otherwise it is zero, unless $n = s$, when we have the value $\underline{|n}\, N$.

Now let C_sh stand for the chordal area

$$(\tfrac{1}{2}Z_0x_0^s + Z_1x_1^s + Z_2x_2^s + \ldots + \tfrac{1}{2}Z_l l^s)\, h,$$

where of course $x_0 = 0$, $x_1 = h$, $x_2 = 2h$, etc., if h be the base element. Then we easily find

$$\left.\begin{aligned}
\int_0^l Z\,dx &= C_0 h - \frac{h^4}{720}a_3 + \frac{h^6}{30240}a_5 - \\
\int_0^l Zx\,dx &= C_1 h + \frac{h^2}{12}N - \frac{h^4}{240}a_2 + \frac{h^6}{6048}a_4 - \\
\int_0^l Zx^2\,dx &= C_2 h + \frac{h^6}{1512}a_3 - \frac{h^8}{28800}a_5 + \\
\int_0^l Zx^3\,dx &= C_3 h - \frac{h^4}{120}N + \frac{h^6}{504}a_2 - \frac{h^8}{5760}a_4 + \\
\int_0^l Zx^4\,dx &= C_4 h - \frac{h^8}{1440}a_3 + \frac{h^{10}}{15840}a_5 - \\
\int_0^l Zx^5\,dx &= C_5 h + \frac{h^6}{252}N - \frac{h^8}{480}a_2 + \frac{h^{10}}{3168}a_4 -
\end{aligned}\right\} \text{............ (x),}$$

where the values of the Bernoulli numbers have been substituted. Now let us use (viii) and write $a_s/N = a_s'$, then we find

$$\left.\begin{aligned}
\mu_1' &= \frac{C_0 h}{N} - \frac{h^4}{720}a_3' + \frac{h^6}{30240}a_5' - \\
\mu_2' &= \frac{2C_1 h}{N} + \frac{h^2}{6} - \frac{h^4}{120}a_2' + \frac{h^6}{3024}a_4' - \\
\mu_3' &= \frac{3C_2 h}{N} + \frac{h^6}{504}a_3' - \frac{h^8}{9600}a_5' + \\
\mu_4' &= \frac{4C_3 h}{N} - \frac{h^4}{30} + \frac{h^6}{126}a_2' - \frac{h^8}{1440}a_4' + \\
\mu_5' &= \frac{5C_4 h}{N} - \frac{h^8}{288}a_3' + \frac{h^{10}}{3168}a_5' - \\
\mu_6' &= \frac{6C_5 h}{N} + \frac{h^6}{42} - \frac{h^8}{80}a_2' + \frac{h^{10}}{528}a_4' -
\end{aligned}\right\} \text{.................. (xi).}$$

If the base element be as usual unity, then we have simply to put $h = 1$ throughout. If there be high contact at $x = 0$, then we have simply

$$\mu_s' = \frac{sC_{s-1}}{N} - (-1)^{\frac{1}{2}s} B_{s-1} h^s,$$

where $B_{2r} = 0$, for in this case $a_2' = a_3' = a_4' = \text{etc.} = 0$.

We can modify (xi) in the following manner. Since Zx^s, $s > 0$, vanishes at both ends of the range we have, substituting the value of Z (see p. 282), and putting the base element unity:

$$\begin{aligned}\text{Chordal area of } Zx^s = {} & (z_0 + z_1 + z_2 + \dots + z_n)\, 0^s \\ & + (z_1 + z_2 + \dots + z_n)\, 1^s \\ & + (z_2 + z_3 + \dots + z_n)\, 2^s \\ & \dots\dots\dots\dots\dots\dots \\ & + (z_{n-1} + z_n)(n-1)^s \\ & + z_n n^s \\ = {} & z_n (1^s + 2^s + 3^s + \dots + n^s) \\ & + z_{n-1} (1^s + 2^s + 3^s + \dots + (n-1)^s) \\ & + z_{n-2} (1^s + 2^s + 3^s + \dots + (n-2)^s) \\ & + \dots\dots\dots\dots\dots\dots \\ & + \dots\dots\dots\dots\dots\dots \\ & + z_2 (1^s + 2^s) + z_1 (1^s) \quad \dots\dots\dots\dots\text{(xii)}.\end{aligned}$$

Now $1^s + 2^s + 3^s + \dots + n^s$ can be summed by a Bernoulli's numbers series, i.e.

$$1^s + 2^s + 3^s + \dots + n^s = \frac{n^{s+1}}{s+1} + \tfrac{1}{2} n^s + \frac{sB_1}{|\underline{2}} n^{s-1}$$

$$- \frac{s(s-1)(s-2)}{|\underline{4}} B_3 n^{s-3} + \frac{s(s-1)(s-2)(s-3)(s-4)}{|\underline{6}} B_5 n^{s-5},$$

the series ending with a constant or n, according as s is odd or even.

Now we may write on the right-hand side $n + \frac{1}{2} - \frac{1}{2}$ for n, and we find accordingly that

$$\left.\begin{aligned} 2(1 + 2 + 3 + \dots + n) &= (n + \tfrac{1}{2})^2 - \tfrac{1}{4}, \\ 3(1^2 + 2^2 + 3^2 + \dots + n^2) &= (n + \tfrac{1}{2})^3 - \tfrac{1}{4}(n + \tfrac{1}{2}), \\ 4(1^3 + 2^3 + 3^3 + \dots + n^3) &= (n + \tfrac{1}{2})^4 - \tfrac{1}{2}(n + \tfrac{1}{2})^2 + \tfrac{1}{16}, \\ 5(1^4 + 2^4 + 3^4 + \dots + n^4) &= (n + \tfrac{1}{2})^5 - \tfrac{5}{6}(n + \tfrac{1}{2})^3 + \tfrac{7}{48}(n + \tfrac{1}{2}), \\ 6(1^5 + 2^5 + 3^5 + \dots + n^5) &= (n + \tfrac{1}{2})^6 - \tfrac{5}{4}(n + \tfrac{1}{2})^4 + \tfrac{7}{16}(n + \tfrac{1}{2})^2 - \tfrac{3}{64} \end{aligned}\right\} \dots\text{(xiii)}.$$

In these we can write n, $n-1$, $n-2$, $n-3$, etc., successively for n, but $z_r (r + \frac{1}{2})^s$ is the sth moment of z_r about one end of the range, and $\overset{n}{\underset{0}{S}} z_r (r + \frac{1}{2})^s$ is the sth moment of the system of *grouped* frequencies about one end of the range. Let us call this $N\nu_s'$. We can now rewrite equations (xi) in terms of the ν''s. We first note, however, that when $s = 0$:

$$\begin{aligned}\mu_1' &= \frac{1}{N}\int_0^l Zdx = \frac{1}{N}\,\text{(chordal area) by (x)} \\ &= (\tfrac{1}{2} Z_0 + Z_1 + Z_2 + \dots + \tfrac{1}{2} Z_n)/N \\ &= \{(n + \tfrac{1}{2})\, z_n + (n - 1 + \tfrac{1}{2})\, z_{n-1} + (n - 2 + \tfrac{1}{2})\, z_{n-2} + \text{etc.}\}/N \\ &= \nu_1'.\end{aligned}$$

Hence finally reintroducing h for base unit, and substituting in (xi), we have, if $\nu_n' = S(z_r x_r^n)$:

$$\left.\begin{aligned}
\mu_1' &= \nu_1' - \frac{h^4}{720} a_3' + \frac{h^6}{30240} a_5' - \\
\mu_2' &= \nu_2' - \frac{h^2}{12} - \frac{h^4}{120} a_2' + \frac{h^6}{3024} a_4' - \\
\mu_3' &= \nu_3' - \frac{h^2}{4} \nu_1' + \frac{h^6}{504} a_3' - \frac{h^8}{9600} a_5' + \\
\mu_4' &= \nu_4' - \frac{h^2}{2} \nu_2' + \frac{7}{240} h^4 + \frac{h^6}{126} a_2' - \frac{h^8}{1440} a_4' + \\
\mu_5' &= \nu_5' - \frac{5}{6} h^2 \nu_3' + \frac{7}{48} h^4 \nu_1' - \frac{h^8}{288} a_3' + \frac{h^{10}}{3168} a_5' - \\
\mu_6' &= \nu_6' - \frac{5}{4} h^2 \nu_4' + \frac{7}{16} h^4 \nu_2' - \frac{31}{1344} h^6 - \frac{h^8}{80} a_2' + \frac{h^{10}}{528} a_4' -
\end{aligned}\right\} \quad \text{(xiv)}.$$

These are the formulæ for finding the first six moments about one end of the range when we have found the ν's or the "rough moments," i.e. the frequencies grouped at the mid-points of the elements, about the same end. Putting

$$a_2' = a_3' = a_4' = a_5' = \ldots = 0,$$

we have the corrections first given by Mr W. F. Sheppard (*Proceedings of the London Mathematical Society*, Vol. XXIX. p. 368) for the case of high contact at both ends of the range.

It remains now to be considered how we can determine good values for the a's. I assume that the form of the integral curve near the origin can be closely given by a parabola of the 5th order. This, since $Z = N$ and $dZ/dx = 0$ for $x = 0$, must be

$$Z = N\left(1 + \frac{a_2'}{\underline{|2}} x^2 + \frac{a_3'}{\underline{|3}} x^3 + \frac{a_4'}{\underline{|4}} x^4 + \frac{a_5'}{\underline{|5}} x^5\right),$$

whence we have as required $\quad \left(\dfrac{d^s Z}{dx^s}\right)_0 = a_s' N = a_s.$

Now when $x = \epsilon$, $x = 2\epsilon$, $x = 3\epsilon$, $x = 4\epsilon$, let

$$Z = N(1 - n_1), \quad N(1 - n_1 - n_2), \quad N(1 - n_1 - n_2 - n_3), \quad N(1 - n_1 - n_2 - n_3 - n_4).$$

Thus we find:

$$\begin{aligned}
-n_1 &= \frac{a_2'\epsilon^2}{\underline{|2}} + \frac{a_3'\epsilon^3}{\underline{|3}} + \frac{a_4'\epsilon^4}{\underline{|4}} + \frac{a_5'\epsilon^5}{\underline{|5}} \\
-(n_1 + n_2) &= \frac{a_2'\epsilon^2}{\underline{|2}} 2^2 + \frac{a_3'\epsilon^3}{\underline{|3}} 2^3 + \frac{a_4'\epsilon^4}{\underline{|4}} 2^4 + \frac{a_5'\epsilon^5}{\underline{|5}} 2^5 \\
-(n_1 + n_2 + n_3) &= \frac{a_2'\epsilon^2}{\underline{|2}} 3^2 + \frac{a_3'\epsilon^3}{\underline{|3}} 3^3 + \frac{a_4'\epsilon^4}{\underline{|4}} 3^4 + \frac{a_5'\epsilon^5}{\underline{|5}} 3^5 \\
-(n_1 + n_2 + n_3 + n_4) &= \frac{a_2'\epsilon^2}{\underline{|2}} 4^2 + \frac{a_3'\epsilon^3}{\underline{|3}} 4^3 + \frac{a_4'\epsilon^4}{\underline{|4}} 4^4 + \frac{a_5'\epsilon^5}{\underline{|5}} 4^5.
\end{aligned}$$

By actual solution of these equations I find* :

$$\left.\begin{aligned}
a_2'\epsilon^2 &= -\frac{415n_1 - 161n_2 + 55n_3 - 9n_4}{72} = \gamma_2,\\
a_3'\epsilon^3 &= \frac{755n_1 - 493n_2 + 191n_3 - 33n_4}{48} = \gamma_3,\\
a_4'\epsilon^4 &= -\frac{119n_1 - 97n_2 + 47n_3 - 9n_4}{6} = \gamma_4,\\
a_5'\epsilon^5 &= \frac{125n_1 - 115n_2 + 65n_3 - 15n_4}{12} = \gamma_5
\end{aligned}\right\}\text{............(xv).}$$

These values of γ can be easily calculated. Now let $h/\epsilon = \rho$. Then if we take h equal unity as usual, $1/\rho$ will measure the fraction ϵ is of h. Of course very usually $\rho = 1$, but this is not necessary; thus in certain diseases the frequency of cases in each of the first five years of life is often recorded, but later only in five-year periods. Making these substitutions we may write our final results for the moments:

$$\left.\begin{aligned}
\mu_1' &= \nu_1' - \frac{\rho^3\gamma_3}{720} + \frac{\rho^5\gamma_5}{30240} -\\
\mu_2' &= \nu_2' - \frac{1}{12} - \frac{\rho^2\gamma_2}{120} + \frac{\rho^4\gamma_4}{3024} -\\
\mu_3' &= \nu_3' - \frac{1}{4}\nu_1' + \frac{\rho^3\gamma_3}{504} - \frac{\rho^5\gamma_5}{9600} +\\
\mu_4' &= \nu_4' - \frac{1}{2}\nu_2' + \frac{7}{240} + \frac{\rho^2\gamma_2}{126} - \frac{\rho^4\gamma_4}{1440} +\\
\mu_5' &= \nu_5' - \frac{5}{6}\nu_3' + \frac{7}{48}\nu_1' - \frac{\rho^3\gamma_3}{288} + \frac{\rho^5\gamma_5}{3168} -\\
\mu_6' &= \nu_6' - \frac{5}{4}\nu_4' + \frac{7}{16}\nu_2' - \frac{31}{1344} - \frac{\rho^2\gamma_2}{80} + \frac{\rho^4\gamma_4}{528} -
\end{aligned}\right\}\text{............... (xvi).}$$

(xvi) and (xv) form the solution of the problem. It is of course sometimes more convenient to use (xi) directly. In any individual case we must first find the ν''s—generally only ν_1' to ν_4' are needful—about one end of the range. Then we calculate the γ's and ρ and so determine the μ''s by (xvi). Then by (ix) we find the values of the μ's transferred to the centroid. Of course the process will be much simplified if there be high contact at both ends, for then all the γ's may be put zero. The methods here indicated seem of such importance that it is desirable to fully illustrate them in various special examples, each of which has been selected with a view of illustrating some peculiar point or difficulty. My first two examples are illustrations of the fitting of skew frequency curves; my third deals with the fitting of sine curves when only a portion of

---

* The reader must remember that n_1, n_2, n_3 and n_4 are not the *total* frequencies in the first four elements, but the *proportional* frequencies.

the observations are known; my fourth deals with the representation of vital statistics by Makeham's curve and my fifth with the deduction of the curve of errors from partial observations of frequency.

(5) *Illustration I. On the mean variability and distribution of fecundity in* 2000 *thoroughbred broodmares.*

By fecundity of the mare is here meant the ratio of the number of yearling foals she has actually produced to the number of her opportunities. The base-elements were taken $\pm \frac{1}{30}$ on either side of $0, \frac{1}{15}, \frac{2}{15}, \frac{3}{15}, \ldots \frac{14}{15}, 1$. Thus fecundity from 0 to 1 was divided into 16 grades, respectively denoted by *a, b, c, d, ... l, m, n, p, q*. The data were extracted from the stud-books, every mare having had at least 8 or more coverings.

I propose in this first illustration to go through the whole of the work of dealing with the frequency distribution as it may be unfamiliar to many of my readers, and yet it is really very simple. I shall first suppose the curve to have high contact at the terminals of the range, and work out the ν's and deduce the μ's by Sheppard's corrections: see p. 287. This, however, is not in this case legitimate from mere inspection of the curve, and therefore we ought to start by using (xiv). Working out the μ's in the latter way also we can compare the results actually obtained. It will be sufficient to go as far as μ_4.

Grade	Frequency z	x	zx	zx^2	zx^3	zx^4
a	0	−9	− 0	+ 0	− 0	+ 0
b	2	−8	− 16	+ 128	− 1024	+ 8192
c	7·5	−7	− 52·5	+ 367·5	− 2572·5	+ 18007·5
d	11·5	−6	− 69	+ 414	− 2484	+ 14904
e	21·5	−5	− 107·5	+ 537·5	− 2687·5	+ 13437·5
f	55	−4	− 220	+ 880	− 3520	+ 14080
g	104·5	−3	− 313·5	+ 940·5	− 2821·5	+ 8464·5
h	182	−2	− 364	+ 728	− 1456	+ 2912
i	271·5	−1	− 271·5	+ 271·5	− 271·5	+ 271·5
j	315	0	−1414		−16837	
k	337	+1	+ 337	+ 337	+ 337	+ 337
l	293·5	+2	+ 587	+ 1174	+ 2348	+ 4696
m	204	+3	+ 612	+ 1836	+ 5508	+ 16524
n	127	+4	+ 508	+ 2032	+ 8128	+ 32512
p	49	+5	+ 245	+ 1225	+ 6125	+ 30625
q	19	+6	+ 114	+ 684	+ 4104	+ 24624
	2000		+2403	+11555	+26550	+189587
			−1414		−16837	
			+ 989		+ 9713	
			$\nu_1' = \cdot 4945$	$\nu_2' = 5\cdot 7775$	$\nu_3' = 4\cdot 8565$	$\nu_4' = 94\cdot 7935$

Using (xiv) with the α's zero to find the moments we have

$$\mu_1' = \cdot 4945, \quad \mu_2' = 5\cdot 694{,}167, \quad \mu_3' = 4\cdot 732{,}875, \quad \mu_4' = 91\cdot 933{,}917,$$

and hence by (ix)

$$\mu_1 = 0, \quad \mu_2 = 5{\cdot}449{,}636, \quad \mu_3 = -3{\cdot}472{,}584, \quad \mu_4 = 90{\cdot}747{,}281.$$

It will thus be seen that the determination of the four μ's about the mean is neither a long nor a difficult process. I will now proceed to find them *de novo* by applying (viii).

Grade	Frequency	Z I.	x	Zx II.	Zx^2 III.	Zx^3 IV.
a	0					
		2000	0	0	0	0
b	2					
		1998	1	1998	1998	1998
c	7·5					
		1990·5	2	3981	7962	15924
d	11·5					
		1979	3	5937	17811	53433
e	21·5					
		1957·5	4	7830	31320	125280
f	55					
		1902·5	5	9512·5	47562·5	237812·5
g	104·5					
		1798	6	10788	64728	388368
h	182					
		1616	7	11312	79184	554288
i	271·5					
		1344·5	8	10756	86048	688384
j	315					
		1029·5	9	9265·5	83389·5	750505·5
k	337					
		692·5	10	6925	69250	692500
l	293·5					
		399	11	4389	48279	531069
m	204					
		195	12	2340	28080	336960
n	127					
		68	13	884	11492	149396
p	49					
		19	14	266	3724	52136
q	19					
		0	15	0	0	0

The chordal areas are here:

Chordal area of Z = 17,989,
" " Zx = 86,184,
" " Zx^2 = 580,828,
" " Zx^3 = 4,578,054.

Whence by (xi) with the a's zero, the moments about one end of the range:

$$\mu_1' = 8{\cdot}9945,$$
$$\mu_2' = 86{\cdot}350{,}667,$$
$$\mu_3' = 871{\cdot}242,$$
$$\mu_4' = 9156{\cdot}074{,}667.$$

Using (ix):

$$\mu_2 = \quad 5\cdot449{,}637,$$
$$\mu_3 = - \ 3\cdot472{,}590,$$
$$\mu_4 = \quad 90\cdot747{,}442.$$

The divergence between these results and those given by Mr Sheppard's process is very small and solely due to the arithmetic being cut off at the sixth place of decimals in the multiplication. Thus μ_2' above really ends with $\dot{6}$, and this difference is sensible in the fourth place of decimals of μ_4 when we multiply μ_2' by $6\mu_1'^2$.

Now let us see if Mr Sheppard's process is in this case justified; let us no longer put the a's zero, i.e. no longer suppose high contact at the high fecundity end of the curve. We have

$$n_1 = 19/2000, \quad n_2 = 49/2000, \quad n_3 = 127/2000, \quad n_4 = 204/2000.$$

Hence we find from (xv)

$$\gamma_2 = - \cdot035{,}729, \quad \gamma_3 = \cdot080{,}344, \quad \gamma_4 = - \cdot136{,}750, \quad \gamma_5 = \cdot080{,}625.$$

This leads us by (xvi) to

$$\mu_1' = \nu_1' - \cdot000{,}1089, \qquad \mu_2' = \nu_2' - \tfrac{1}{12} + \cdot000{,}2525,$$
$$\mu_3' = \nu_3' - \tfrac{1}{4}\nu_1' + \cdot000{,}1510, \quad \mu_4' = \nu_4' - \tfrac{1}{2}\nu_2' + \tfrac{7}{240} - \cdot000{,}1886,$$

or the μ''s are only influenced in the fourth place of decimals. Substituting the values of ν_1', ν_2', ν_3' and ν_4' about one end of the range just found, we have

$$\mu_1' = \ \cdot494{,}391, \quad \mu_2' = \ 5\cdot694{,}4195,$$
$$\mu_3' = 4\cdot733{,}026, \quad \mu_4' = 91\cdot933{,}7284,$$

which lead by (ix) to

$$\mu_2 = \quad 5\cdot449{,}997,$$
$$\mu_3 = - \ 3\cdot471{,}085,$$
$$\mu_4 = \quad 90\cdot745{,}703.$$

We see that modifications are in the third place of decimals of μ_3 and μ_4 and in the fourth of μ_2. Clearly we are not justified theoretically in assuming high contact at the high fecundity end of the frequency curve, but for most practical problems Sheppard's corrections would in this case have been quite sufficient. The actual slope of the tangent to the frequency curve at the end of the range would be

$$-\frac{d^2Z}{dx^2} = \gamma_2 = \cdot035{,}729,$$

which is of course fairly small. Thus if there be not high contact at one end of the curve, but the slope of the tangent be not large, Sheppard's corrections will still give the substantial part of the required correction. If, however, as in the mortality due to various diseases the curve meets the axis at a considerable angle, we must endeavour to determine in some such manner as the above the value of the corrective terms.

Suppose we attempt to fit a curve of the form

$$y = y_0\left(1+\frac{x}{a_1}\right)^{m_1}\left(1-\frac{x}{a_2}\right)^{m_2}$$

to the above data. The values of the constants in terms of the moments are given in *Phil. Trans.* Vol. 186, A., pp. 367 *et seq.*, and we find

$$y = 342{\cdot}187\left(1+\frac{x}{47{\cdot}13579}\right)^{82{\cdot}6261}\left(1-\frac{x}{12{\cdot}11059}\right)^{21{\cdot}2291}.$$

The mean being at $j + {\cdot}4945$, and the mode, which is the origin, at $j + {\cdot}7970$. Here j denotes a fecundity of 9/15, and 1/15 is the unit of fecundity. Fig. 2 shows that we have a very reasonable fit,—a curve which effectively represents the phenomenon.

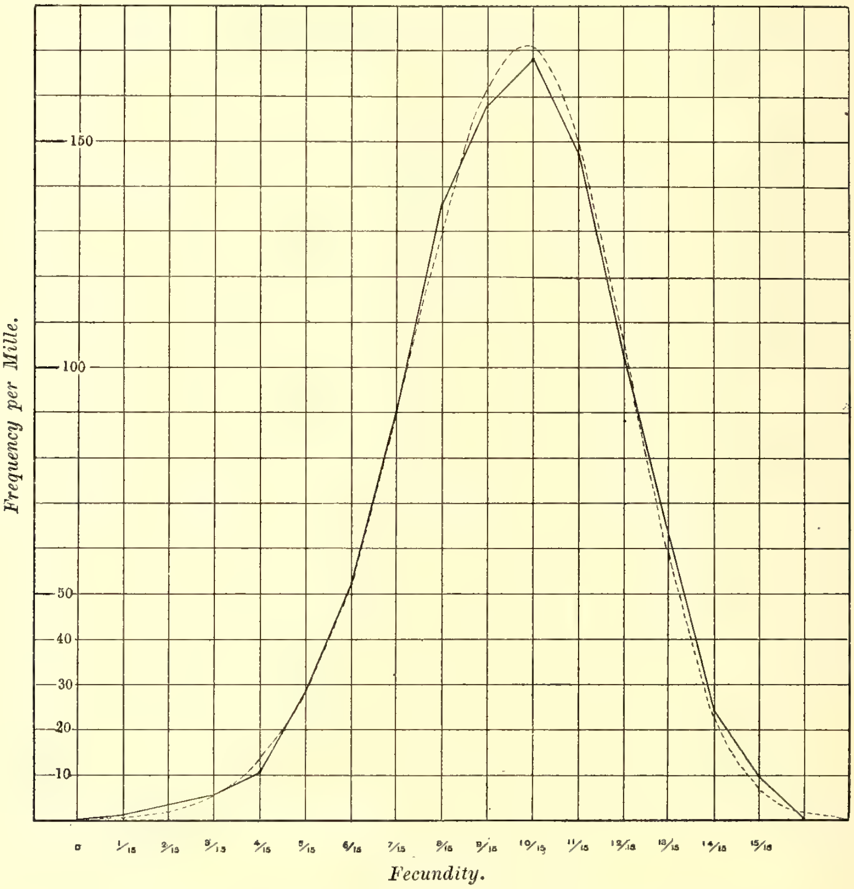


Fig. 2. Fecundity of 2000 Brood Mares, 8 or more coverings.

(6) *Illustration II.* Half the battle of curve-fitting is to select a suitable type of curve for representing the observations. Mere increase of the number of

constants will often give far less advantageous results than the choice of a more suitable form even with fewer constants. I will endeavour to illustrate this by the following system of frequencies due to data from a game of 'patience*':

Value of character	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22–28
Frequency	—	—	—	3	7	35	101	89	94	70	46	30	15	4	5	1	—	—	—

The possible range is 4 to 28.

Now we find the mean of the character to be 11·86, and let us assume the form of the curve to be

$$y = y_0 \left(1 + \frac{x}{a}\right)^p e^{-\frac{px}{a}}.$$

Here the origin is at the mode or maximum ordinate which is at a distance a/p from the mean. We have thus only three constants p, a and y_0 at our disposal. We shall show in the sequel that we get a better fit than if we disposed of *seven* constants in a curve of the form

$$y = a_0 + a_1x + a_2x^2 + a_3x^3 + a_4x^4 + a_5x^5 + a_6x^6.$$

The data appear to give high contact at both ends, and therefore Sheppard's modifications would give the best values of the moments. But for the purposes of illustration we will treat the data as giving a polygonal curve, and assume our object to be that of finding a curve going as close to this polygon as possible. Methods of finding the moments of a polygonal area will be given in the second part of this paper. Formulæ for our present purpose will be found in *Phil. Trans.* Vol. 186, A., p. 350. There results for the moments about the mean in the present case

$$\mu_2 = 4{\cdot}3231, \quad \mu_3 = 4{\cdot}6804, \quad \mu_4 = 59{\cdot}683.$$

Hence we deduce†

$$y = 98{\cdot}762 \left(1 + \frac{x}{7{\cdot}4449}\right)^{13{\cdot}7530} e^{-1{\cdot}8473x}.$$

The distance from the mean to the mode, which is the origin, is ·5413. Thus the modal value is at 11·3187, and the range starts at 3·2931.

The ordinates corresponding to the observations are given in column two of the Table in Art. 13 of this memoir. Fig. 3 shows a reasonable fit. The Table compares these results with the successive parabolas up to the sixth and shows how a well selected curve with three constants can easily be superior to one with seven constants‡.

---

[*] Thiele; *Forelaesninger over almindelig Iagttagelseslaere*, p. 12.

[†] The formulæ for p, a, and y_0 in terms of the moments are given, *Phil. Trans.*, Vol. 186, A, p. 373.

[‡] This point is of special importance, for objections have been raised against the skew frequency curves just referred to on the ground that they give better fits than the normal curve because they have one or two more constants as the case may be. This is true, but they also give better fits than some other curves with double their number of constants!

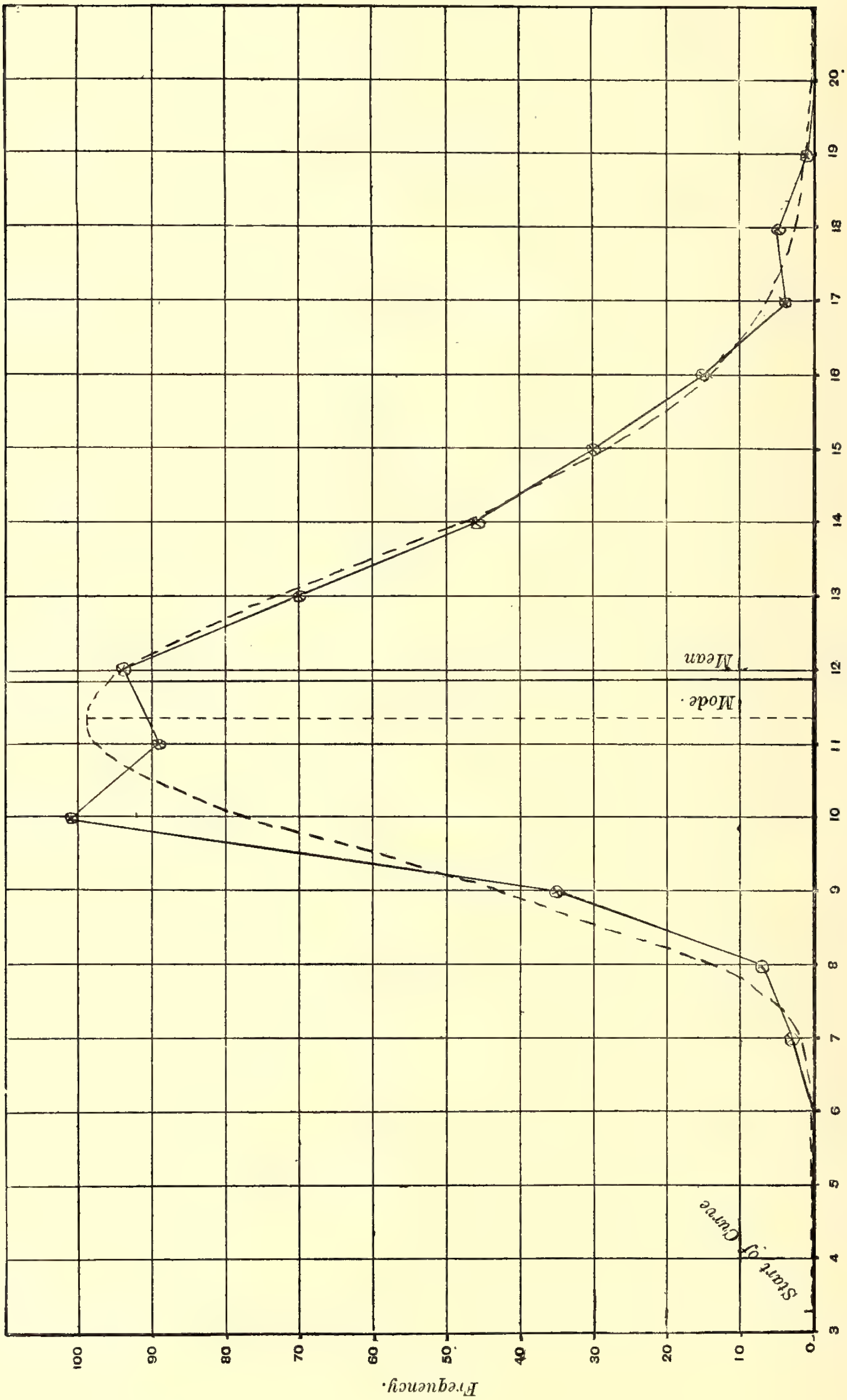


Fig. 3. Thiele's Observations.

Now either of the curves in Illustrations I. and II. is a good example of the impossibility of using the method of least squares for systematic curve-fitting. The reader need only attempt to write down the type-equations, which must be solved to find the constants, and he will realise the simply appalling amount of lengthy approximations which must be carried out even after *rough* values of these constants have been guessed by some one or other means.

But it is not only algebraic and exponential curves for which the method of least squares fails; it fails also for trigonometrical curves. I will now illustrate this in the very simplest case possible.

(7) *Illustration III.* Let it be required to fit the simplest sine-curve

$$y = a \sin (nx + \alpha)$$

to the aneroid barometer observations in the Illustration in § 3. Let us write the equation in the form

$$y = A \sin nx + B \cos nx \text{..........................(xvii).}$$

Then the three type-equations to find n, A and B, arising from applying the method of least squares, are the following:

$$\begin{aligned} AS(\cos^2 nx) + \tfrac{1}{2}BS(\sin 2nx) \quad &= S(y \cos nx), \\ \tfrac{1}{2}AS(\sin 2nx) + BS(\sin^2 nx) \quad &= S(y \sin nx), \\ AS(yx \sin nx) - BS(yx \cos nx) &= \tfrac{1}{2}(A^2 - B^2)\, S(x \sin 2nx) - ABS(x \cos 2nx). \end{aligned}$$

Here S denotes a summation with regard to the eleven values of x and y given on p. 279; after these have been substituted in the summations, we must eliminate A and B, and we shall then have an equation to determine n. Afterwards the values of A and B must be found by substituting the value of n in the two first equations. We may leave this as an exercise to those readers who have faith in the method of least squares applied to curve-fitting! *

Now let us turn to the method of moments. There are three constants to be found, so we must find the area and the first two moments of the observations and of the theoretical curve.

Taking the origin at the middle of the range $2l$, and writing M_0, M_1, M_2 for the area and first two moments, we have

$$M_0 = \frac{2B \sin nl}{n}, \quad M_1 = 2A\left(\frac{-l \cos nl}{n} + \frac{\sin nl}{n^2}\right),$$

$$M_2 = 2B\left\{\frac{l^2 \sin nl}{n} + \frac{2l \cos nl}{n^2} - \frac{2 \sin nl}{n^3}\right\}.$$

---

* The equation to find n is intractable even if we place the origin at the centre of the range and evaluate by trigonometry the summations not involving y or x outside the trigonometrical terms.

Put $\beta = \frac{1}{2}\left(\frac{M_2}{M_0 l^2} - 1\right)$ and $z = nl$, then we find

$$\frac{1}{z} + \beta z = \cot z \quad \text{(xviii)},$$

$$B = \frac{M_0}{2l}\frac{z}{\sin z}, \quad A = -\frac{M_1}{2l^2}\frac{1}{\beta \sin z} \quad \text{(xix)}.$$

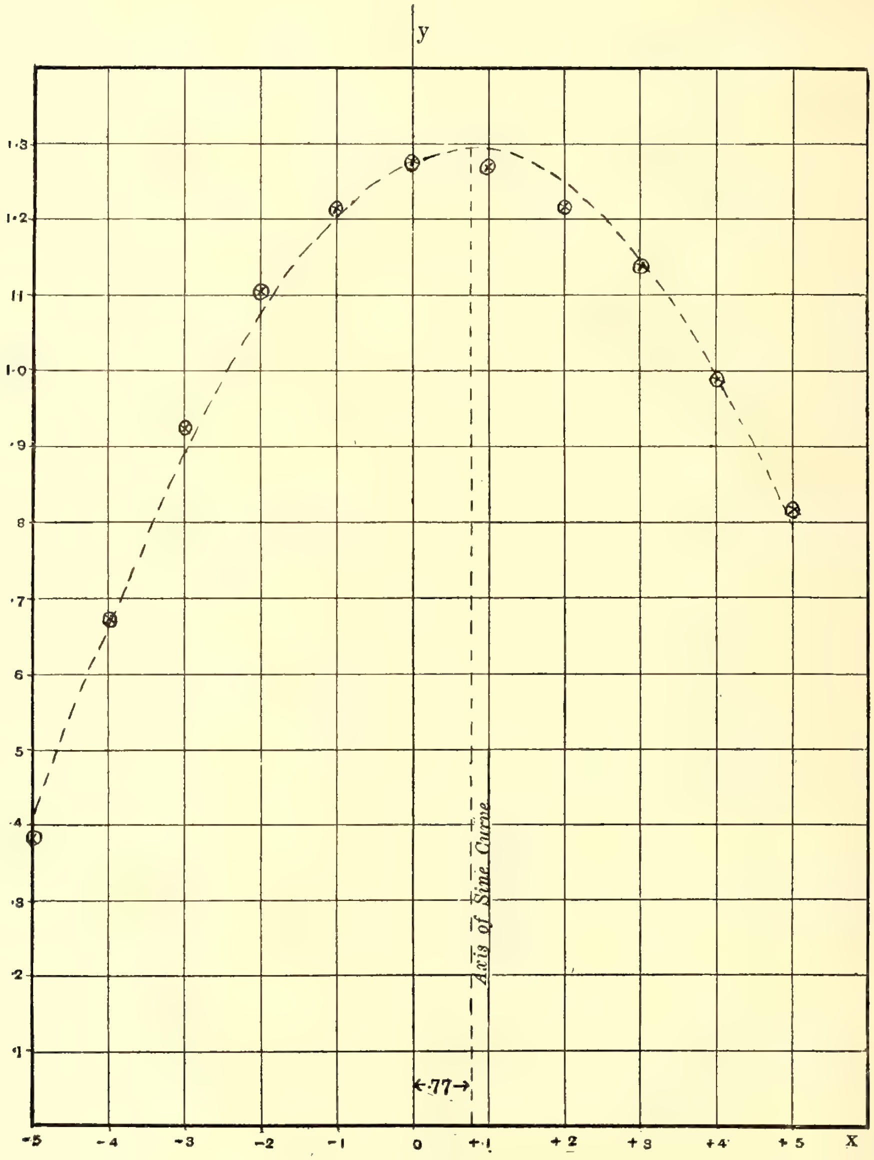


Fig. 4.

These are the general equations for fitting the sine curve (xvii) by the method of moments to any series of observations. We see how simple is this method as compared with that of least squares; we must first find a root of (xviii) and this value of z substituted in (xix) will give us A and B.

For the special case of the barometer observations I suppose our ordinates placed at the middle of the elements so that the range $2l = 11$. I then find the moments by an application of (λ) on p. 276, using the values of P and Q given for $p = 11$ on p. 277. This gives

$$M_0 = 10\cdot979{,}4240, \quad M_1 = 4\cdot420{,}0564, \quad M_2 = 86\cdot783{,}0235,$$

and thence $\beta = -\cdot369{,}353.$

To solve (xviii), the hyperbola

$$y = \frac{1}{z} - \cdot369{,}353z,$$

and the transcendental curve $y = \cot z$

were roughly plotted and observed to intersect about $z = 1\cdot2$. Using Newton's method of approximation, I found with Miss M. A. Lewenz's aid:

$$z = 1\cdot1867, \quad z = 1\cdot1844, \quad z = 1\cdot184{,}4132,$$

which last value is practically exact. This gave

$$n = \cdot215{,}348 = 12^\circ\, 20'\, 19'',$$
$$A = \cdot213{,}545, \quad B = 1\cdot276{,}288.$$

Whence the required curve is

$$y = \cdot213{,}545 \sin(\,215{,}348x) + 1\cdot276{,}288 \cos(\cdot215{,}348x),$$

or

$$y = 1\cdot29403 \sin\{(12^\circ\, 20'\, 19'')\, x + 80^\circ\, 30'\, 5''\},$$

the latter form allowing of easy calculation of the ordinates.

We have

x	Observed y	Calculated y
-5	·382	·417
-4	·674	·669
-3	·923	·891
-2	1·104	1·071
-1	1·214	1·201
0	1·273	1·276
$+1$	1·270	1·292
$+2$	1·215	1·250
$+3$	1·137	1·148
$+4$	·989	·993
$+5$	·819	·793

The root mean square error of the ordinates is ·0233. Thus the fit is by no means so good as that of the parabola of the third order $y = a_0 + a_1x + a_2x^2 + a_3x^3$

dealt with on pp. 279—281. But there was no reason for supposing *a priori* the observations to be suitable to sine curve representation, and the sine curve has one less constant. The fit is illustrated in the accompanying Fig. 4, and is seen to be by no means bad. The data were merely selected, as equally good with any others, to exemplify the process of fitting a sine curve.

(8) *Illustration IV. To fit Makeham's Curve to Mortality Statistics.*

Given a mortality table—i.e. a table which gives the number of survivors out of n people born in the same year at each year of age of the group—then if l_x denotes the number who attain the age of x, the table will be closely represented between the ages 20—25 to 85—90 by Makeham's formula, i.e.

$$l_x = ks^x (g)^{c^x} \quad \text{.................................(xx)},$$

where k, s, g and c are constants to be determined from the data of the table.

Now there will be some 60 to 70 corresponding values of x and l_x and it is a quite hopeless task to think of discovering the values of k, s, g and c for the equation as it stands. If we take logarithms the equation may be written:

$$L_x' = K' + xS' + G'c^x,$$

where the capitals are the logarithms of the small letter quantities. The determination of K', S', G' and c by the method of least squares is still impracticable. Of course four corresponding values of L_x' and x would give K', S', G' and c, but such a selection of four arbitrary values out of 60 or 70 is unsatisfactory in the extreme. Accordingly Messrs G. King and G. F. Hardy have determined values of these constants by a process of averaging series of corresponding values of L_x' and x, so that the final values of the constants shall depend on as much of the table as possible*. The values reached for the constants are good, but no doubt better ones could be found, and the process from the standpoint of systematic curve fitting is unsatisfactory. It involves empirical trials—e.g. "various groupings were tried, and the best was found to be, four groups of eighteen years of life each" (*Text-book*, p. 82)—and therefore follows no general rule for curve fitting.

Accordingly it seems very desirable to indicate how the method of moments can be applied to Makeham's formula.

I shall take l for the range of the mortality table to be dealt with and my origin of x' at the mid-point of this range. If x_0 be the age corresponding to the origin, I shall write

$$l_{x'} = k's'^{x'/l} (g')^{c'^{x'/l}} \quad \text{.................................(xxv)},$$

whence we see that:

$$\left.\begin{aligned} s' &= s^l \\ c' &= c^l \\ g' &= g^{c^{x_0}} \\ k' &= ks^{x_0} \end{aligned}\right\} \quad \text{.................................(xxvi)}$$

---

* *Journal of Institute of Actuaries*, Vol. XXII. p. 200, or G. King: *Institute of Actuaries Text-book*, Vol. II. p. 79 *et seq.*, especially p. 82.

at once connect the ordinary constants s, c, g, k with my s', c', g', k'. Clearly k' is a number of living persons like k, and s', c' and g' are mere numerical quantities independent of what unit of duration of life we may select—day, month, year, etc.—while the s and c of the usual notation involve the unit in which life is measured—a theoretical, although scarcely practical disadvantage. Taking logarithms I now, dropping dashes, write the formula

$$L = K + S\frac{x}{l} + Ge^{2nx/l} \quad \text{(xxvii)},$$

where

$$e^{2n} = c' \quad \text{(xxviii)}.$$

We must now proceed to find the area and first three moments A, $A\mu_1$, $A\mu_2$, $A\mu_3$ of (xxvii) about the middle of the range l. If we then equate these to the moments found from the table we shall have equations to determine K, S, G and n and therefore k, s, g and c in a perfectly direct and systematic manner, using all the data provided. We have

$$A = \int_{-\frac{1}{2}l}^{\frac{1}{2}l} L\,dx = Kl + \frac{Gl}{2n}(e^n - e^{-n}).$$

Or if

$$\alpha_0 = A/l \quad \text{(xxix)},$$

$$\alpha_0 = K + G\frac{\sinh n}{n} \quad \text{(xxx)},$$

$$A\mu_1 = \int_{-\frac{1}{2}l}^{+\frac{1}{2}l} Lx\,dx.$$

Or if

$$\alpha_1 = 12A\mu_1/l^2 \quad \text{(xxxi)},$$

$$\alpha_1 = S + 6G\left\{\frac{\cosh n}{n} - \frac{\sinh n}{n^2}\right\} \quad \text{(xxxii)},$$

$$A\mu_2 = \int_{-\frac{1}{2}l}^{+\frac{1}{2}l} Lx^2\,dx.$$

Or if

$$\alpha_2 = 12A\mu_2/l^3 \quad \text{(xxxiii)},$$

$$\alpha_2 = K + 3G\left\{\frac{\sinh n}{n} - \frac{2\cosh n}{n^2} + \frac{2\sinh n}{n^3}\right\} \quad \text{(xxxiv)},$$

$$A\mu_3 = \int_{-\frac{1}{2}l}^{+\frac{1}{2}l} Lx^3\,dx.$$

Or if

$$\alpha_3 = 80A\mu_3/l^4 \quad \text{(xxxv)},$$

$$\alpha_3 = S + 10G\left\{\frac{\cosh n}{n} - \frac{3\sinh n}{n^2} + \frac{6\cosh n}{n^3} - \frac{6\sinh n}{n^4}\right\} \quad \text{(xxxvi)}.$$

From (xxxii) and (xxxvi) we have:

$$\alpha_3 - \alpha_1 = G\left\{\frac{4\cosh n}{n} - \frac{24\sinh n}{n^2} + \frac{60\cosh n}{n^3} - \frac{60\sinh n}{n^4}\right\} \quad \text{(xxxvii)}.$$

From (xxxiv) and (xxx) we have:

$$\alpha_2 - \alpha_0 = G\left\{\frac{2\sinh n}{n} - \frac{6\cosh n}{n^2} + \frac{6\sinh n}{n^3}\right\} \qquad \text{(xxxviii)}.$$

Eliminating G between (xxxvii) and (xxxviii) and writing β for $(\alpha_3 - \alpha_1)/(\alpha_2 - \alpha_0)$, or:

$$\beta = \frac{4\left(\dfrac{20\mu_3}{l^3} - \dfrac{3\mu_1}{l}\right)}{\dfrac{12\mu_2}{l^2} - 1} \qquad \text{(xxxix)},$$

a constant to be found from the moments of the mortality table, we have after some reductions:

$$\tanh n = \frac{2n^3 + 30n + 3\beta n^2}{\beta n^3 + 12n^2 + 3\beta n + 30}.$$

Or, substituting for the hyperbolic tangent:

$$e^{2n} = \frac{(\beta+2)\,n^3 + 3\,(\beta+4)\,n^2 + 3\,(\beta+10)\,n + 30}{(\beta-2)\,n^3 - 3\,(\beta-4)\,n^2 + 3\,(\beta-10)\,n + 30} \qquad \text{(xl)}.$$

This equation will give n to any degree of approximation required. Then (xxxviii) gives G, and (xxxii) gives S, and (xxx) K, whence all the constants of the solution can be found.

To solve (xl) an approximate value of $n = n_0$ is easily found; for $c = e^{2n/l}$ has been found from previous experience to have a logarithm very nearly ·04. Starting from this value of n_0 successive approximations can be obtained by Newton's method. Thus, put $n_0 + h$ in (xl) and neglect h^2, we find if $e^{2n} = N/D$, where N stands for numerator and D for denominator:

$$h = \frac{e^{2n_0} - \dfrac{N_0}{D_0}}{\dfrac{1}{D_0}\left(\dfrac{dN}{dn}\right)_0 - \dfrac{N_0}{D_0^2}\left(\dfrac{dD}{dn}\right)_0 - 2\dfrac{N_0}{D_0} - 2\left(e^{2n_0} - \dfrac{N_0}{D_0}\right)} \qquad \text{(xli)}.$$

Writing

$$Y_1 = 2n^3 + 3\beta n^2 + 30n \qquad \text{(xlii)},$$

$$Y_2 = \beta n^3 + 12n^2 + 3\beta n + 30 \qquad \text{(xliii)},$$

we have

$$e^{2n} = \frac{Y_2 + Y_1}{Y_2 - Y_1} \qquad \text{(xliv)},$$

$$D = Y_2 - Y_1, \quad N = Y_2 + Y_1$$

$$\frac{dD}{dn} = \frac{dY_2}{dn} - \frac{dY_1}{dn}, \quad \frac{dN}{dn} = \frac{dY_2}{dn} + \frac{dY_1}{dn} \qquad \text{(xlv)},$$

where

$$\frac{dY_1}{dn} = 6n^2 + 6\beta n + 30, \quad \frac{dY_2}{dn} = 3\beta n^2 + 24n + 3\beta \qquad \text{(xlvi)}.$$

Thus every part of (xli) can be readily found numerically by calculating Y_1, Y_2, and their differentials, as given by (xlvi), for any value of n_0. Of course for accurate calculations we must go to 9 to 12 places of decimals and the ordinary tables of logarithms are of no service. For the numerical illustration now to be given a large Brunsviga calculator was used, and exponentials and reciprocals found so as to be true to twelve places of figures. The calculations were of course long and laborious, and I owe an immense amount of solid help to my former colleague, Mr Leslie Bramley-Moore, for independent arithmetic and for verification of my own calculations.

I selected the mortality table given in the Text-book for actuaries, but I limited the range l of life to the 60 years from 25 to 85 inclusive. I did this because the data after 85 is really sparse, because the material before 25 begins to diverge from Makeham's law, and lastly because as a mere illustration of method it is a sufficiently big task to calculate area and moments for a system of 61 ordinates. Using z_0 to z_{61} at equal distances I could apply Weddle's Quadrature Rule (see (ζ) of p. 275), in which I have great confidence for a fairly smooth curve like that given by the mortality table. The ordinates, of course, are $z = L = \log l_x$ for the area, xz for the first, x^2z for the second, and x^3z for the third moment, where attention must be paid to the sign of x.

The following values were found:

$$\begin{aligned} A &= 221\cdot843{,}235 \\ A\mu_1 &= -275\cdot103{,}222 \\ A\mu_2 &= 64{,}464\cdot355{,}986 \\ A\mu_3 &= -162{,}062\cdot316{,}564. \end{aligned}$$

Whence

$$\alpha_0 = 3\cdot697{,}387{,}25\dot{0}, \quad \alpha_2 = 3\cdot581{,}353{,}110{,}\dot{3}$$
$$\alpha_1 = -\cdot917{,}010{,}74\dot{0}, \quad \alpha_3 = -1\cdot000{,}384{,}670{,}\dot{1}4\dot{8}.$$

These lead to

$$\beta = \cdot718{,}529{,}308{,}595.$$

By a rougher quadrature process I got for β for the whole range from 20 to 90

$$\beta = \cdot801{,}086{,}783.$$

The value of β as found by (xl) from the n which corresponds to Messrs King and Hardy's c is:

$$\beta = \cdot804{,}162{,}\dot{5},$$

but their range is from 17 to 88 years.

The next point is the solution of (xl). Working in the manner indicated with

$$\beta = \cdot718{,}529{,}309,$$

and calculating necessary terms to 12 places of decimals, we found the following series of approximations to the value of n

$$2\cdot7,\quad 2\cdot8,\quad 2\cdot807{,}68,\quad 2\cdot807{,}312,\quad 2\cdot807{,}346{,}8,\quad 2\cdot807{,}343{,}62,$$
$$2\cdot807{,}343{,}87 \text{ and } 2\cdot807{,}343{,}873.$$

This value is correct to the last figure or we have

$$n = 2{\cdot}807,343,873.$$

Hence by using the exponential theorem:

$$e^n = e^2 \times e^{\cdot 807} \times e^{\cdot 000,343,873},$$
$$= 16{\cdot}565,858,706,268.$$

Similarly

$$e^{-n} = {\cdot}060,365,117,060.$$

Thus

$$\sinh n = 8{\cdot}252,746,794,593,$$
$$\cosh n = 8{\cdot}313,111,911,675.$$

Hence we determine from (xxxviii):

$$G = -{\cdot}064,875,005,350,$$

and from (xxxii):

$$\frac{S}{l} = -{\cdot}002,866,074,767.$$

Finally from (xxx)

$$K = 3{\cdot}888,100,258.$$

Calculating: $c = e^{2n/l}$ we have:

$$c = 1{\cdot}098,096,393,273,$$

which I believe is true to the last figure.

The value of c as found by Messrs King and Hardy is:

$$c = 1{\cdot}095,612,204.$$

The difference is partly due to difference of range, partly due to method of calculation.

Thus finally we obtain for L_x, the logarithm of the number of survivors of age $55 + x$ years:

$$L_x = 3{\cdot}888,100,258 - x \times {\cdot}002,866,074,767 - {\cdot}064,875,005,350\,(1{\cdot}098,096,393,273)^x.$$

In comparing our formula with others of a like kind, it must be remembered that our x is measured from 55 years as origin. For use it may be noted that the reciprocal of c is

$$\frac{1}{c} = {\cdot}910,666,865,065,$$

which will be wanted when x is negative.

Clearly c and $\frac{1}{c}$ are wanted to many places of figures as they have to be raised to high powers. The values of L_x for $x = -30$ to $+30$ were found by repeated multiplication with a Brunsviga, so that in no part of the work has a table of logarithms been used.

I give here a table of the observations and calculated values and add Messrs King and Hardy's results*, asking the reader, however, to remember that these

---

* *Text-book of Institute of Actuaries*, Part II. p. 88.

were based on the range 17 to 88 and therefore are not strictly comparable with mine based on a different range.

Life Table ($L_x = \log l_x$).

		Calculated differences*				Calculated differences	
Age	Observed	By moments	By averages	Age	Observed	By moments	By averages
25	3·96833	+ ·00184	+ ·00043	*56*	3·81354	+ ·00046	+ ·00039
26	·96609	+ 83	− 45	*57*	·80339	+ 75	+ 59
27	·96307	+ 56	− 59	*58*	·79289	+ 71	+ 44
28	·96025	+ 5	− 97	*59*	·78184	+ 47	+ 09
29	·95684	+ 8	− 81	*60*	·77069	− 50	− 100
30	·95363	− 13	− 91	*61*	·75695	+ 21	− 41
31	·95003	− 1	− 67	*62*	·74259	+ 55	− 20
32	·94682	− 34	− 89	*63*	·72729	+ 73	− 15
33	·94319	− 32	− 76	*64*	·71075	+ 95	− 6
34	·93957	− 37	− 72	*65*	·69294	+ 112	− 2
35	·93578	− 34	− 59	*66*	·67359	+ 138	+ 13
36	·93219	− 60	− 76	*67*	·65281	+ 148	+ 12
37	·92833	− 68	− 76	*68*	·63099	+ 87	− 57
38	·92416	− 56	− 56	*69*	·60628	+ 123	− 27
39	·91967	− 23	− 16	*70*	·57894	+ 212	+ 58
40	·91503	+ 12	+ 25	*71*	·55390	− 161	− 314
41	·91072	0	+ 19	*72*	·52602	− 504	− 652
42	·90615	− 1	+ 22	*73*	·48994	− 306	− 443
43	·90147	− 8	+ 19	*74*	·45432	− 460	− 579
44	·89685	− 40	− 9	*75*	·40598	+ 321	+ 229
45	·89169	− 38	− 5	*76*	·36295	+ 202	+ 147
46	·88629	− 34	0	*77*	·31403	+ 266	+ 261
47	·88082	− 48	− 13	*78*	·26403	− 7	+ 51
48	·87463	− 16	+ 18	*79*	·20713	− 80	+ 59
49	·86847	− 18	+ 15	*80*	·14362	− 29	+ 210
50	·86179	+ 1	+ 31	*81*	·07776	− 332	+ 29
51	·85456	+ 39	+ 65	*82*	·00212	− 306	+ 205
52	·84693	+ 77	+ 99	*83*	2·92001	− 343	+ 348
53	·83947	+ 56	+ 72	*84*	·81934	+ 694	+ 1600
54	·83194	− 5	+ 4	*85*	·73319	− 578	+ 584
55	·82363	− 40	− 38				

* Calculated less observed values.

By the method of moments the mean difference is ·00116 and by the method of averages it is ·00126. The improvement is not very sensible, but the method of fitting being brought under a general rule is an advantage of great importance. The actuaries have adopted Makeham's formula to express the life-table, but it cannot be considered to give good results for the down slope of old age, say 68 onwards. From the mathematical standpoint better results would be obtained by the choice of other functions. I have selected Makeham's solely with a view of illustrating the application of my general method to a somewhat complex arithmetical problem.

(*To be continued.*)

ON THE SOURCES OF APPARENT POLYMORPHISM IN PLANTS, ETC.

(EDITORIAL.)

In the last number of *Biometrika* a note was inserted in the *Miscellanea** warning biometricians against laying too great stress on "apparent modes" obtained by the mere inspection of frequency polygons. It was pointed out that the significance of such modes could only be tested by an application of the mathematical theory of random sampling. Now a great deal of argument in favour of the dimorphic and polymorphic character of plants has been based solely on emphasising irregularities in the seriation, which have no importance when the deviations due to random sampling are properly allowed for. Nor has the application of this very rough form of graphical analysis tended to error in botanical investigations only. We have also in mind the bold resolutions by craniologists of small groups of human skulls into distinct local races, because they formed peaked frequency distributions. On application of the theory of random sampling such peaks have on more than one occasion been found to be of no significance, and further they have been seen to actually disappear when a much larger number of skulls were available for measurement.

Again, within the limits of the same homogeneous race by measuring groups of individuals at different stages of seasonal or secular growth, or subject to different conditions of environment, we may easily obtain significant bi-modal or even multimodal frequency distributions, which have no relation whatever to the existence of dimorphic forms or to "petites espèces." For example, the offspring of Shirley poppies all grown from Hampden stock in six different parts of this country showed in the number of stigmatic bands on the capsules modes significantly different from each other and from the parent stock. There is no doubt that, had the countings been mixed together without attention being paid to local environment, we should have spoken of the flower of this poppy as polymorphic in character, or suggested the existence of "petites espèces." A still further emphasis of the irregularity of the seriation would have arisen, if the capsules had not been

---

* Vol. I. p. 260.

taken in their entirety at the end of the flowering season, but 500 to 1000 gathered at one date from one crop as a sample of the crop, and 500 to 1000 from a second crop at another date. But this latter method of gathering is what actually has been adopted in most counting on wild flowers. So far as we are aware no attempt has yet been made to count any character in all the flowers, throughout the whole season, of some given plant on a definite small area. Thus the polymorphism so often noted may be wholly or in part due to heterogeneity introduced by the collector himself; he gathers from different localities at different parts of the flowering seasons. A "different locality" may mean either side of an east and west hedge, and a "different part of the flowering season" the same day for the population on either side of this hedge.

The importance of these considerations is so great that it seems absolutely necessary to ascertain the influence of seasonal and environmental changes on plants before we conclude as to their polymorphic character or as to the existence of "petites espèces" from a discussion of frequency distributions. In order to emphasise these points the following three papers are now published to show the changes which arise in the statistical constants when the gatherings are made at different periods in the season, and further to indicate how the theory of statistics can be applied to test the significance or non-significance of differences in statistical constants.

Mr Yule shows us how the influence of year, date of gathering, and environment on *Anemone nemorosa* affects the statistical constants. The differences are quite as significant as those which in other cases have led to the suggestion of "petites espèces."

Mr Tower indicates a similar seasonal change in the case of the mode of *Chrysanthemum leucanthemum.* He proposes a new definition for the term 'mode,' but the word 'mode' was introduced into statistics with a perfectly definite sense, and it seems undesirable now to alter it. "The average prevailing state of one or more characters of a homogeneous lot of individuals" is not a biometric definition. It might refer to any constant whatever of the frequency,—to the mean, the mode, the variability, or indeed to the whole frequency distribution itself. The now established use of the word 'mode' is for that value of an organ or character, at which the frequency of the population per unit of the character or organ is a maximum,—the frequency 'per unit of the character' being used, if the character be not discrete, in the sense of the infinitesimal calculus. The definition is clear; it belongs to the theory of statistics to show us how to determine whether there is one or more true modes, and if there be, to settle the degree of their significance. A frequency distribution with more than one true mode is multi-modal, but although the population will then probably be heterogeneous, it is not shown to be polymorphic. We take it that polymorphism means the existence at the same instant of the season under the same environment in a homogeneous population of two types. The object of the present series of papers is to indicate that much of the multimodalism interpreted in the case of flowers as polymorphism is due either to misinterpretation of the criterion of significance, i.e. is not true multi-modalism at all, or, if such, is due to some heterogeneity of period, of season or of environment introduced by the gatherer.

It is not contended that there is not a great deal of true polymorphism in plants—this is beyond dispute—only its true appreciation can scarcely be realised until the influences of environment and of stages in season on the modal values have been exhaustively studied. Take the case of the Fibonacci series

3, 5, 8, 13, 21, 34, 55, etc.,

which has been so fully considered by Dr Ludwig*. There is no doubt that these numbers recur with somewhat remarkable persistency in the plant kingdom. Each number is the sum of the two immediately preceding it, and there may well be some mechanical explanation of the building up of flowers, by which type added to type is more probable than progression by units. But if we take *Chrysanthemum leucanthemum* itself, we can, not only pass at different periods of the season from one Fibonacci mode to a second, but other modes also come in which tend to destroy our faith in the absolute truth of the Fibonacci series. Mr Tower's second mode is quite definitely at 33 and not the Fibonacci 34. He has also in both his sub-groups an apparent mode between 22 and 25, which is only screened and not lost when he combines them. His series 284 and 168 are somewhat small for this sort of work, but Mr Yule and one of the editors found in 1133 heads gathered in 1895, during some weeks in the Lake District (Keswick), a distribution which indicated modes between 14 and 16 and between 24 and 26, as well as the widespread typical mode of 21. 8, 13 and 34 as modes were unrepresented †. In Dr Ludwig's own classical series of 17,000 heads there is a significant mode between 24 and 26. Whatever be the value of the Fibonacci series, it seems impossible to look upon it as providing the only numbers which can arise as modes for the rays of the ox-eyed daisy. Its real significance can only be tested when all the flowers on a given small area are observed throughout the season, and the number of rays counted and the date noted as each flower opens‡. Only thus shall we be able to test whether the mode changes continuously during the season, or springs from one number to a second, and, if the latter, whether these numbers are or are not really the Fibonacci series.

---

* See especially his "Ueber Variationscurven und Variationsflächen der Pflanzen," *Botanisches Centralblatt*, Bd. LXIV. 1895.

† See p. 319 below.

‡ Even in this case the flower should not be cut off, but effectively marked. The removal of flowers may tend to influence the characters of later flowers on the same plant.

Variation of the number of sepals in *Anemone nemorosa.*

By G. UDNY YULE.

[Received January 9, 1902.]

It is a question of some interest how far local races of plants vary from year to year. An abnormal characteristic, or a larger proportion of abnormal individuals, may be exhibited at some one time by one local race as compared with another, but unless the same race be re-observed, it cannot be certain that the abnormality is not merely a temporary condition, due to an unusually wet or dry season, or to the fact that the different races compared were observed at different times in the season. With individuals so largely subject to external influences as plants a good deal of caution must be used in drawing conclusions.

In the spring of 1898, between the 20th and 23rd of April, I counted the number of sepals on three different series of *Anemone nemorosa* in the neighbourhood of Bookham, Surrey. The three places from which they were taken are within a mile or two of each other, and on the same clay subsoil. *A* was a copse by Banks Common, Effingham ; the underwood had been recently cut, so the place was fairly exposed, there being few large trees. *B* was a spot in one of the Eastwick woods ; the underwood had not been cut for a long time, so it was close growing and the ground very sheltered. *C* was a narrow strip of copse, only a few yards wide, between two fields in the parish of Little Bookham. It sloped slightly down a hill. The underwood was low, about a year old, so the situation may be called exposed. The sepals were counted on the spot, a thousand being taken in each place. The flower is a delicate one, and it is necessary to take a good deal of care not to count specimens that have lost one or more sepals ; I never admitted a flower that dropped a sepal on being shaken or blown. The frequencies are given in the first three columns of Table I. *B* exhibits the largest proportion of sixes and the least variability, *C* the lowest proportion of sixes and the greatest variability, *A* is intermediate.

A fortnight later the strip of copse *C* was revisited. It was late in the season, the anemones were half over and the 500 which were counted nearly cleared the strip. The frequencies per 1000 are given in Column 4. It will be seen that the distribution is quite different to that of Column 3 ; flowers with five and six sepals are more frequent, with seven or more sepals less frequent, than earlier in the season. The S.D. is however sensibly the same, being in both cases markedly higher for *C* than for either *A* or *B*. The intervening fortnight had been wet.

In the spring of 1899 the two places *A* and *C* were again visited and 500 flowers counted at each ; it will be noted that the visit was made nearly a fortnight earlier than in the preceding year. The frequencies per 1000 are given in Columns 5 and 6. The distribution for *A* resembles the *B* distribution of the previous year more nearly than the *A* distribution, the number of six-sepal flowers having risen from 515 to 614 per thousand. *C* has not been similarly affected at all, the distribution for April 8th—12th, 1899, being very like the distribution for April 21st—22nd, 1898.

TABLE I.

Frequencies of specimens of Anemone nemorosa with different numbers of sepals gathered in different places in the years 1898—1900.

Year		1898				1899		1900
Place		*A*	*B*	*C*	*C*	*A*	*C*	*C*
Column		**1**	**2**	**3**	**4**	**5**	**6**	**7**
Date		April 20–23	April 21–23	April 21, 22	May 7	April 9	April 8–12	April 15
Number of sepals	*4*	—	3	—	—	—	2	—
	5	7	31	12	34	20	28	6
	6	515	657	448	576	614	460	380
	7	414	271	363	276	306	390	448
	8	49	35	135	92	44	94	138
	9	13	2	33	14	14	24	24
	10	1	1	5	4	2	2	4
	11	1	—	4	—	—	—	—
	12	—	—	—	4	—	—	—
Total		1000	1000	1000	1000	1000	1000	1000
Number gathered		1000	1000	1000	500	500	500	500
Mean number of sepals		6·55	6·31	6·76	6·51	6·42	6·63	6·81
S.D. of sepals ...		0·68	0·62	0·90	0·87	0·69	0·81	0·80

On April 15th, 1900, *C* was visited for the fourth time, the distribution being given in Column 7. It will be seen that it has changed its character very considerably, *seven*-sepal flowers being now more numerous than sixes. In the April gatherings of 1898 and 1899 the frequency of sixes was roughly 450 per 1000, and the probable error of this on a gathering of 500 blossoms is only 14 or 15, so it is very unlikely that the low figure noted in 1900 was a mere random deviation. But if *one* local race can change its character as much as this from year to year, what stress can be laid on differences between local races only noted at one time in one year? The only point in which the race *C* has *constantly* differed from *A* and *B* is its greater variability, as measured simply by the S.D.

I regret I found it impossible to visit either *A* or *B* in 1900, and in 1901 I had no opportunity for observation at all.

I cannot suggest any definite reason for the change in the *C* distribution in 1900. The low underwood of 1898 had grown to a height of six feet or more in 1900, thus rendering the ground more shady, and also screening the wind to some extent. This is an approach towards the conditions of place *B*, but the distribution is not at all like that of *B* for 1898. The growth of underwood in place *A* may, possibly, account for the change in that distribution between 1898 and 1899, but there can be no certainty about such a conclusion.

In 1898 and again in 1900 I noted the flowers of place *C* in several different lots as I worked my way down from the upper to the lower end of the strip of copse. In 1898 the lots from the upper end exhibited a large excess of sixes, the lots from the lower end an excess of sevens : in 1900 there was an excess of sevens in both cases. Has the race at the top been swamped by a race from the lower end of the copse ? I do not think it very likely, as 1899 shows no sign of such a process. The actual figures from my notes are as follows :

TABLE II.

Relative distributions of Flowers from the top and bottom of the Copse C, 1898 and 1900.

		1898		1900	
		Top	Bottom	Top	Bottom
Number of sepals	*5*	8	4	3	—
	6	295	153	103	87
	7	178	185	126	98
	8	58	77	39	30
	9	8	25	8	4
	10	4	1	1	1
	11	1	3	—	—
Total		552	448	280	220

I hope to be able to continue these observations, and have merely put these notes together to illustrate the fact that a "local race" must be observed for, at least, some years before its characteristics as compared with other races can be known. All plants may not fluctuate so much as these Anemones but it cannot be assumed that they do not.

Variation in the Ray-flowers of *Chrysanthemum leucanthemum* L. at Yellow Springs, Greene Co., O., with remarks upon the Determination of Modes.

BY W. L. TOWER, Yellow Springs, Ohio.

[Received November 11, 1901.]

AT Yellow Springs, Greene Co., O., *Chrysanthemum leucanthemum* L. occurs in only two localities, one and one-half miles apart. Between these localities are cultivated fields and woodlands, and except in these two spots this species is not known to occur within five miles of Yellow Springs. So considerable a degree of isolation for this species is uncommon in the eastern United States, and affords a good opportunity to determine whether it has produced any change in the modes,—8, 13, 21, 34,—as they have been determined by Ludwig (1895, 1896 a, b, 1898 a, b) in Germany.

Of the two localities, one is a field of about five acres' area, lying upon a hillside which slopes southward towards the Little Miami River. This place,—one mile south of the Yellow Springs, and one hundred and fifty yards east of the Little Miami R.R.,—is uniformly but not thickly covered by the plants. The second locality is one half mile west of Antioch College, where a few plants grow in some glacial gravels.

My material all came from the first locality, there being too few heads of flowers at the second to be of use. In collecting the specimens, lots were obtained on July 5 and July 30, and only fresh, fully blossomed heads were counted. All that were injured, wilted, or had begun to go to seed were rejected, since many, if not all such individuals, had lost a greater or less number of rays. In gathering the material I walked at random across the field, picking the heads in the most mechanical manner possible, and then rejecting those that were too old or that had been injured. The two lots collected gave quite different results as regards the number of ray flowers in the heads, and had one lot only been taken it would have been almost certain to have forced the conclusion that the species had changed in this locality from the ancestral condition of Europe.

Lot No. 1. *Collected July* 5, 1901.

The rays in 284 heads were counted and were found to vary in number from 16 to 39. The polygon of distribution (Fig. 1) shows two strongly developed modes, on 22—25, and on 33, each surrounded by a considerable body of variates and with a deep sinus between the two modes. The mean of the lot was 27·87 rays, or 3·25 above the mean for lots one and two combined, which is the mean for the season. The modes of this lot do not fall upon those numbers which were found by Ludwig to be the modes of this species in Germany. The modes of my lot 22—25, and 33, have no relation to the series of Fibonacci, 8, 13, 21, 34, which are the modes in Europe. The difference between my results and Ludwig's as well as the discrepancy of Lucas's (1898) results do not indicate a change of modal condition in America but are due to an entirely different cause. This cause I shall briefly discuss in the latter part of this paper.

At the time this first lot was collected a considerable number of the heads had already passed their prime and begun to lose their ray flowers. These were rejected from the material used. Counts of some of this rejected material showed that all of the heads had a large number of rays and that they would have fallen in the group about the mode on 33. It is quite possible that had I made a collection of material a few days earlier, the specimens I was forced to reject, being then in their prime, would have fallen about 34 as a mode, thus conforming with Ludwig's results.

In the variates which are grouped about the lower mode 22—25, there is an evident skewness toward a lower number of rays, and there is no clearly defined modal number. This condition is associated with the time in the blossoming period when the material was taken.

Lot No. 2. *Collected July* 30, 1901.

The second lot of material, although from the same field as the first, and taken both in the same mechanical way and with the same precautions, showed, when the rays were counted, a condition that was decidedly different from that of the earlier material. In 168 heads the rays varied in number from 12 to 34, with modes on 13 and 21 (Polygon, Fig. 2). The mode on 33 (34) rays found in the first lot (Fig. 1) disappeared in the second; the lower mode on 22—25 was replaced by a strong one on 21 and a new mode on 13 appeared. The mean of this second lot was 21·38 or 3·26 below the mean for the generation.

The Modal condition of the rays at Yellow Springs, O.

To obtain the modes about which the number of rays tend to gather in this locality, the two lots of variates must be combined, since neither lot 1 nor 2 represents the whole condition as regards variation in the rays for the generation from which the lots were taken. When the two lots are combined into one polygon of distribution, there are represented in it variates from the early, middle and late parts of the generation, and it shows fairly well the entire generation for 1901. In this polygon (Fig. 3) 452 variates, with a range from 13 to 39, are found grouped about three modes 13, 21, 33, with the mode on 21 strongest, 33 next, and 13 last. This corresponds closely with Ludwig's results, differing only in that the highest mode in my counts falls upon 33 instead of 34 rays. The reason for this apparent shifting of the mode 34 I have called attention to in the account of Lot No. 1. The mean for the generation is 24·625.

In studying the variations of the rays, florets and bracts of Asters, Shull (1902) found that those heads which blossomed first had a prevailingly larger number of parts than those which appeared later in the season. The polygons of distribution for the heads were found to be multimodal in every species studied, and these modes were correlated with the time in the season when the heads appeared. Material taken early in the season gave modes on high numbers with almost no variates in the lower part of the range; material taken in the middle of the season gave modes on the mid-range numbers with variates over the entire range; and material taken at the end of the season gave modes upon the lower numbers with a range limited to the lower and middle numbers. Material taken at only one time would not in this case have given data of any value. In *C. leucanthemum* L. the heads which blossom first have a prevailingly larger number of parts than those which follow later in the season. The observations of Shull (1902) upon specific plants of Asters and my own upon *C. leucanthemum*, where every flower that appeared during the growing season upon marked plants was studied, show that in individual plants there is no tendency to have even a majority of the heads in one modal group, but in every plant the heads are distributed over the entire range of variation observed for the species. It has been pointed out by Shull that probably the heads which blossom first are the buds which are formed first and have a maximum amount of nourishment and space for growth, while the later formed buds have progressively less space and nourishment and this causes a decrease in the number of parts in the heads of composite plants.

These observations will, I believe, sufficiently explain the difference between my two lots of material. In the first lot there are represented the first blossomed heads with a prevailingly larger number of parts, and in the second lot the high numbers have disappeared and the lower modes have appeared. Neither of the two lots alone can give data of much value as regards variation, but the two (or more) combined lots represent, as I have pointed out, the condition for the season, which is the thing sought.

As regards the observations of Lucas's (1898), I believe that the deviation of the modes of his counts from those of Ludwig can be fully explained by the above observations, and the fact that Lucas evidently took his material at one time or very nearly so. His determinations are for momentary states in the season and are comparable roughly with my lots 1 and 2. In the material from Yarmouth and Grand Pic, Nova Scotia, Lucas found modes on 22 and 29, and in that from Cambridge and Milton, Mass., the modes were on 21 and 29. The Nova Scotia lot has the mode on 29 well developed and separated by a deep sinus from the lower mode on 22. The group about the mode on 29 is strongly skewed towards the higher numbers, which may indicate a tendency in this group to move away from the lower mode. In the lot from Massachusetts the higher mode has almost disappeared, as in my Lot No. 2, and there is a strong modal group about 21.

The difference between Lucas's two lots is explained by the fact that the season is some weeks earlier in Massachusetts than in Nova Scotia, and, although the lots were taken at near dates, the Nova Scotia lot represents a mid-season condition, somewhat later than my Figure 1, and the Massachusetts lot represents a condition late in the season, like that of my Figure 2. The criticism of Ludwig, that if Lucas had counted more heads he would have found modes on the series of Fibonacci, 8, 13, 21, 34, &c., is only partly true. Lucas's polygons of distribution are even in outline and evidently contain a sufficient number of variates, and I doubt very much if a much larger number of heads *taken at the same time* would have materially changed his results. If Lucas had made counts of material taken at different times during the generation he would in all probability have found modes corresponding to those found by Ludwig in Germany or to those at Yellow Springs, O.*

From the above observations upon *C. leucanthemum* L. and Shull's results upon Asters, together with an exactly similar series of observations upon several species of insects, it seems that the determination of a "place-mode" on the mode of any character is not a simple matter. The following definition of a "place-mode," given by Davenport (1898), seems to me inadequate in view of the evidence. He says: "I use the word 'place-mode' to embody a well known idea, namely, that a species has a different *mode*......in different localities.......... It fixes the condition of a species in a particular locality at a particular time; it affords a base from which we may measure any change which the species has undergone in the same locality after a certain number of years." The statement "It fixes the condition of a species in a particular locality at a particular time.........," does not express, I believe, the idea contained in the word "place-mode." Thus, either of my lots 1 or 2, or Lucas's lots fix the condition of the rays for *C. leucanthemum* for a particular locality at a particular time, and therefore each of my two polygons represents a "place-mode" for the rays of *C. leucanthemum* at Yellow Springs. If this is true then there are two different "place-modes" for the rays of this species at Yellow Springs during one and the same season. It is not thinkable that there should be two "place-modes" for the same species and character, at the same place and during the same season. Consequently, my two lots as well as Lucas's are not "place-modes," but are, as I have before stated, momentary states in the progressive variation of a given season.

It is well known that the several climatic factors are potent in producing variation, not only in the characters of animals and plants, but also in the dates between which a given species will appear. Thus, seasons vary, are hot or cold, early or late, moist or dry, &c., and species of plants and animals are governed largely by these conditions. For example, *C. leucanthemum* may in favorable years begin to blossom by May 15, or even earlier, or in unfavorable ones the blossoming period may not begin until June 15; hence, if, in two successive seasons, one early and the other late, collections of this species were made on July 1, the two collections would not represent homologous points of time in the two seasons. It is evident that the data obtained from such material would not represent "place-modes," neither would the two lots be comparable in any way, so that indications of change could be detected. I believe, therefore, that the "place-mode" for a species or for a character of one species should represent the average prevailing condition at a given place during a period of observation continued through years or long enough to eliminate the effect of secular climatic fluctuations.

To determine the prevailing condition for a given place and time, the variates should be taken uniformly throughout the season and with as little selection as possible. This would give a "secular-mode," since it would reveal the condition as regards variation as it exists for the place and season and we should then know just how much abmodality is exhibited by different parts of the same lot of variates, their relative strength and permanency, and the direction of variation from year to year, or from decade to decade.

---

* It has been shown by Ludwig (1895) that *C. leucanthemum* has a strong mode on 21 in the lowland counties of Germany, and that lots from the mountain region have modes on 13 or even 8 strongly developed. In some extremely fertile places he found strong modes on 34. This variation of modes with locality, soil and climate Ludwig believes to be largely due to nutrition. He has observed the same facts in other species of plants.

In several species of insects I have found that progressive variation in a season is important, and any data for the determination of modes is of little use, unless, in gathering it, the fact be kept in mind that the individuals of a season differ from one another progressively throughout the generation. Thus, material taken at approximately one time gave me data which seemed to indicate a rapid change from year to year, and this seemed the proper conclusion to draw from my data, until I discovered that my successive yearly lots of material were not at all capable of comparison, for the reason that momentary states only were represented and not the conditions for the different years.

Statistical Biology seeks to determine the exact status of species as regards variation, expressed in modes, abmodalities and abnormalities; the direction, rate, and causes of variation in species; the suppression of old modes, the rise of new ones, and the shifting of modes; and the inheritance and permanency of these characters and changes. With such data, accurately determined for a number of species for a period of years, it will be possible to test the validity and broad application of some of the fundamental theories upon which modern Biology is built. In the gathering of these data, however, the most scrupulous care must be exercised to avoid error from undue selections, and to have the data cover as near as can be for each modal determination, one entire generation.

Seriation of counts of the rays of C. leucanthemum from Yellow Springs, Greene Co., O.

Classes	Lot No. 1, July 5, 1901	Lot No. 2, July 30, 1901	Totals
12	—	1	1
13	—	8	8
14	—	3	3
15	—	6	6
16	1	8	9
17	—	9	9
18	—	8	8
19	2	12	14
20	8	19	27
21	17	26	43
22	23	11	34
23	22	10	32
24	21	10	31
25	22	8	30
26	19	5	24
27	16	4	20
28	14	6	20
29	12	4	16
30	10	2	12
31	16	4	20
32	18	2	20
33	29	1	30
34	20	1	21
35	6	—	6
36	6	—	6
37	—	—	0
38	—	—	0
39	2	—	2
Totals	284	168	452

Summary.

1. The ray-flowers in the heads of *Chrysanthemum leucanthemum* L. at Yellow Springs, Greene Co., O., were found to vary from 12 to 39 in number, and were grouped about the series of Fibonacci, 13, 21, 33 (34), as modes, with 21 as the primary, and 33 (34) and 13 as secondary modes. The species in this locality shows for this generation no change from that of Europe.

2. A "place-mode" is the average prevailing state of one or more characters of a homogeneous lot of individuals [i.e. of the same pleomorphic condition and stage of development] characteristic of a particular place and season, as determined by observations carried on long enough to eliminate the effects of secular climatic fluctuations.

3. A "secular-mode" is the prevailing state of one or more characters of a homogeneous lot of individuals, of the same pleomorphic condition and stage of development, for a particular place and year.

BIBLIOGRAPHY.

Davenport, C. B., 1899. The Importance of Establishing Specific Place-Modes. Science, N. S. Vol. ix, No. 220, pp. 415—416.

Lucas, F. C., 1898. Variation in the Number of Ray-Flowers in the White Daisy. American Naturalist, Vol. xxxii, No. 379, pp. 509—511.

Ludwig, F., 1895. Ueber Variationskurven und Variationsflächen der Pflanzen. Bot. Centralb. lxiv, 1—8, 2 taf.

Ludwig, F., 1896 a. Weiteres über Fibonacci-Kurven und die numerische Variation der gesammten Blüthenstände der Kompositen. Bot. Centralb. lxviii, 1 et folg. 1 taf.

Ludwig, F., 1896 b. Eine fünfgipfelige Variations-Kurve. Ber. Deutsch. Bot. Ges. xiv, 204—207.

Ludwig, F., 1898 a. Die pflanzlichen Variations-Kurven und die Gauss'sche Wahrscheinlichkeitskurven. Bot. Centralb. lxxiii, 241—250, 1 taf.

Ludwig, F., 1898 b. Ueber Variationskurven. Bot. Centralb. lxxv, 97—107; 178—183, 1 taf.

Shull, Geo. H., 1902. A Quantitative Study of Variation in the Bracts, Rays and Disc florets of *Aster shortii* Hook., *A. Novae-Angleae* L., *A. puniceus* L., and *A. prenanthoides* Muhl., from Yellow Springs, Ohio. American Nat., Vol. xxxvi, No. 422.

EXPLANATION OF PLATE.

The polygons of distribution are all from counts of the ray-flowers in the heads of *Chrysanthemum leucanthemum* L. from Yellow Springs, Greene Co., O. The mean for the season is represented by the heavy line m_1 m_1 m_1 m_1 about which the polygons are centered to facilitate comparison.

Fig. 1. Polygon of distribution of Lot No. 1, collected July 5, 1901. Modes on 22—25 and 33 rays. Mean (m_2 m_2) 27·87. $n=284$.

Fig. 2. Polygon of distribution of Lot No. 2, collected July 30, 1901. Modes on 13 and 21. Mean (m_3 m_3) 21·30. $n=168$.

Fig. 3. Polygon of distribution for the "character-mode" for the generation of 1901. Mode 21, secondary modes 33 (34) and 13. Mean (m_1 m_1 m_1 m_1) 24·62. $n=452$.

FIG. 1

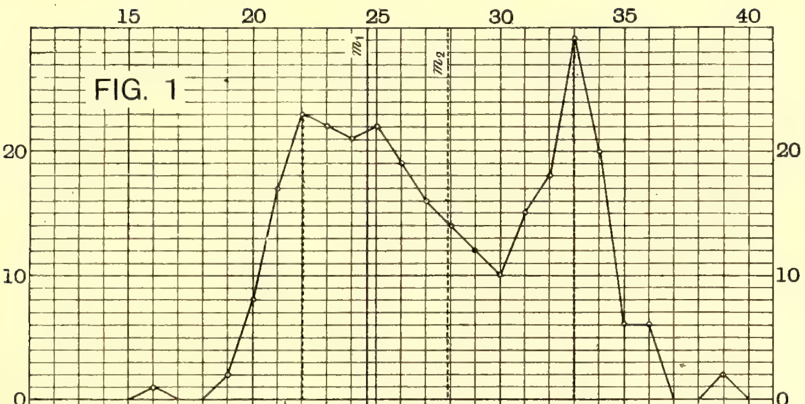


FIG. 2

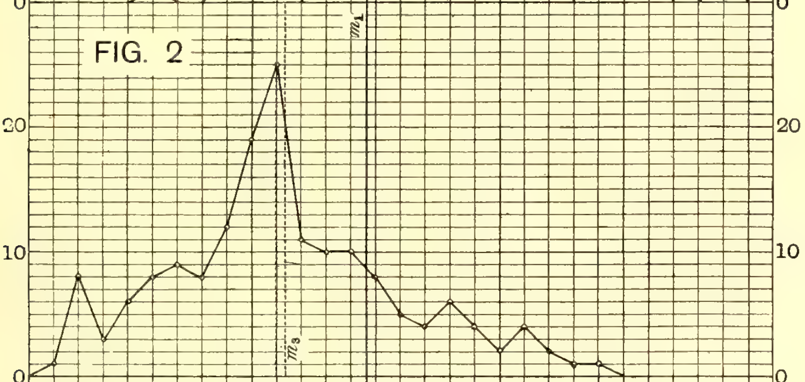


FIG. 3

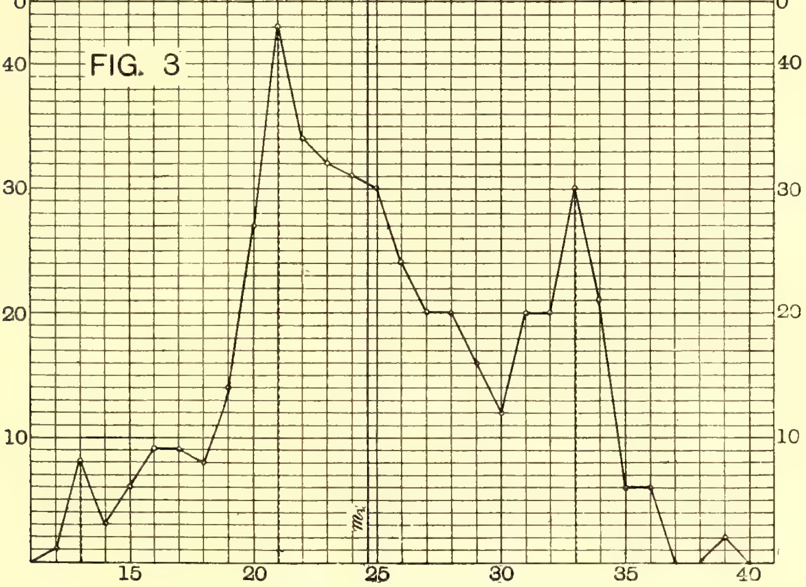


Fig. 1. $n=284$; $m_2=27{\cdot}87$; *modes* $=22,\ 33$.

Fig. 2. $n=168$; $m_3=21{\cdot}36$; *modes* $=13,\ 21$.

Fig. 3. $n=452$; $m_1=24{\cdot}62$; *modes* $=13,\ 21,\ 33$.

Dr Ludwig on Variation and Correlation in Plants.

By ALICE LEE, D.Sc.

A NUMBER of points arise from Dr Ludwig's paper in the October number of *Biometrika* which deserve to be considered from the standpoint of statistical theory. I have accordingly worked out the statistical constants of the material given by him, with the following results.

Ficaria verna		Mean	S. D.	Correlation
Greiz A (1000)	Petals	8·286	1·3382	·2439 ± ·0201
	Sepals	3·695	·8524	
Greiz B (1000)	Petals	8·401	1·2660	·2181 ± ·0203
	Sepals	3·669	·8174	
Greiz C (1000)	Petals	8·486	1·2761	·2705 ± ·0198
	Sepals	3·649	·8417	
Greiz D (300, in 1900)	Petals	8·597	1·4342	·2540 ± ·0364
	Sepals	3·556	·8267	
Greiz E (300, in 1901)	Petals	8·467	1·2459	·2586 ± ·0363
	Sepals	3·753	·8539	
Greiz F (400, in 1901)	Petals	8·4175	1·1618	·3379 ± ·0299
	Sepals	3·640	·8349	
Greiz G* (1000)	Petals	8·232	·9954	·2480 ± ·0200
	Sepals	3·437	·7033	
Mean (A, B, C and G)	Petals	8·351	1·2189	·2451
	Sepals	3·650	·8037	

The probable error of the petal means for A, B, C or G = ·026 about, and of their standard deviation = ·018.

The probable error of the sepal means = ·017 about, and of their standard deviation ·012 for the same series. We may therefore conclude that there will be a sensible difference in the petal means when they differ by two to three times ·04, and in petal variabilities when they differ by two to three times ·03. In the sepals the means must differ by two to three times ·024 and the variabilities by two to three times ·02.

We see that sensible differences occur, especially between the A and G series, as to the variability of both petals and sepals and as to the mean of the latter. But these changes, while demonstrating that the four series are not random samples of the same population made at the same time, are not by any means greater than the same plant in the same locality at different periods of its season or the same plant in different districts at the same period has been known to give. They are well within even the limits of local environmental or seasonal changes†.

A similar remark applies to the divergencies between the components D, E, F of the series C.

---

* The reduced variabilities of this series indicate that it was gathered in a different place or at a different season from A, B and C.

† Compare for example the divergencies for *Papaver Rhaeas* given by Pearson, *Phil. Trans.* Vol. 197, A, p. 312, for neighbouring districts, and by MacLeod for *Ficaria ranunculoides* during the flowering season, *Biometrika*, Vol. I. p. 125.

Taken as a whole the mean results of A, B, C and G may be held to represent the state of things at Greiz. But it seems of vital importance in future to record (i) the period in the season at which the flowers were gathered and (ii) the differences in local environment, if any, of the different series.

The coefficients of variation deduced from the mean values are 14·6 for petals and 22·0 for sepals. These are well within the range of the coefficients of variation determined for the vegetable kingdom*; and as usual much higher than the ordinary values in the animal kingdom.

Dr Ludwig's next results are for *Ficaria verna* at Gera, Trogen and Gais. I find the following values:

Ficaria verna		Mean	S. D.	Correlation
Gera, H (1000)	Petals	8·225	1·1113	·1928 ± ·0205
	Sepals	3·309	·6406	
Gera, I (675)	Petals	8·058	·9432	− ·0188 ± ·0260
	Sepals	3·293	·5384	
Gera, K (712)	Petals	8·263	·5032	·1954 ± ·0203
	Sepals	3·228	·5454	
Trogen, L (285)	Petals	8·144	·7663	− ·1821 ± ·0386
	Sepals	3·386	·5487	
Gais, M (184)	Petals	8·560	·9499	·1964 ± ·0478
	Sepals	4·679	·5252	
Gais, N (1000)	Petals	9·722	1·2819	·2237 ± ·0203
	Sepals	4·538	·6158	

Now these results are extremely anomalous. While Gera H is in fair agreement with Gera K so far as means and correlations are concerned, there are remarkable changes in the variabilities, especially of the petals. Both, however, differ hopelessly from Gera I in the correlation between the numbers of petals and sepals. This value is about ·2 in H and K and zero in I. I venture to think, either that there is some error here, or that this result needs investigation of a special kind. Usually we find a correlation of ·2 to ·25 between petals and sepals, but either at some period of the season or with a special environment this correlation can be reduced to zero. Further in Trogen L, which has very much the same means as Gera H, the correlation has become *negative*, or the greater the number of petals the fewer the sepals! Gais in the neighbourhood of Trogen, while giving flowers with remarkably high means in both characters, still exhibits a correlation of a positive kind, not very far from the value of Gera H and K, or in the longer series not widely divergent from the results for Greiz (·224 as compared with ·245)—indeed within the probable error of the difference. If the zero correlation at Gera and the negative correlation at Trogen be verified by further countings, then, I think, it will probably be found that the correlation between the numbers of petals and sepals varies with the period of the season, and may pass through zero from positive to negative values. If this result be confirmed it seems of considerable importance from the standpoint of plant economy.

We have seen that the Gais means differ very sensibly from the Greiz, while the difference in the correlation of petals and sepals is not significant. A change of environment or a collection at a different part of the season may easily show much change in means or variability. So far there does not seem any good reason for supposing the Gais and Greiz *Ficaria verna* to be of different race, "petites espèces" as Dr Ludwig calls them. By this, I understand, that if

---

* *Phil. Trans.* Vol. 197, A, p. 361.

their environments were interchanged, they would not at once interchange also their statistical constants. Dr Ludwig, however, gives a double correlation table for pistils and stamens for Gais and Greiz, and considers that the difference here confirms his view of a difference of local race. I have worked out the correlation in the two cases, and find the following results:

Ficaria verna		Mean	S. D.	Correlation
Gais (80)	Pistils	18·1125	4·2885	·3913 ± ·0639
	Stamens	23·8250	2·8872	
Trogen (385)	Pistils	13·2635	3·0606	·5328 ± ·0290
	Stamens	20·3682	3·8234	

Now compare these results, obtained, indeed, from very small numbers, with Professor Weldon's results for MacLeod's statistics of *Ficaria ranunculoides*.

Ficaria ranunculoides		Mean	S. D.	Correlation
Early Flowers (268)	Pistils	17·4478	3·8942	·5065 ± ·0306
	Stamens	26·7313	3·7609	
Late Flowers (373)	Pistils	12·1475	3·3878	·7489 ± ·0153
	Stamens	17·8633	3·2984	

It will I think be clear that the differences between the means and correlations in the second table are sensibly as great as the differences between the like quantities in the *Ficaria verna* at Gais and Greiz. It is conceivable therefore that a difference in the periods of the seasons at either place would well account for the differences in the "correlation-fields" without any necessity for supposing difference of race. We require in fact to know how the means, variabilities and correlations of the characters of a plant change (i) with its season and (ii) with the influence of environment, before we can formulate a test for racial differences.

On p. 25 of his memoir Dr Ludwig gives a table for the correlation of ♀ and ♂ flowers in the Blütenköpfchen of *Homogyne alpina*.

I find for the 162 individuals dealt with:

Homogyne alpina	Mean	S. D.	Correlation
♀ flowers	10·537	2·6303	·3735 ± ·0456
♂ flowers	31·8333	7·3924	

Here again it would be of much interest to know if this relationship is maintained throughout the whole flowering season.

Finally I have dealt with Herr Heyer's elaborate system of measurements of 12000 needles from *Pinus silvestris*. I find

Pinus silvestris	Mean	S. D.	Coefficient of Variation
Lower Branches ...	22·163 ± ·048	4·474 ± ·034	20·19
Middle Branches ...	26·524 ± ·055	5·167 ± ·039	19·48
Upper Branches ...	25·949 ± ·062	5·858 ± ·044	22·57

Here the differences in the means and variabilities of needles from different parts of the tree are quite sensible. The variability of the needles as judged by the S. D. increases as we go upward, but the length of the needles does not. It would be of great value, as bearing upon the growth of the tree, to ascertain whether the above relations are due (i) to the special environment of the particular tree dealt with or (ii) to the period in the seasonal growth (February to April) at which the needles were measured.

An examination of Dr Ludwig's polygons on p. 22 seems to suggest that the classification of "lower," "middle" and "upper" branches is not a very satisfactory one. There may be some more fundamental classification having relation to position on branch or to light and shade, and the "lower," "middle," and "upper" branches, while having needles belonging to all these classes, have a greater frequency of one or other class peculiar to themselves.

The coefficients of variation are somewhat greater than have been found for the ash, beech or chestnut (number of veins in leaves), but almost the same as for the variation of prickles on the leaves of holly*.

I have noticed the following *errata* in Dr Ludwig's paper. Table E, p. 15, totals in last row, for 56 read 55, for 45 read 44, for 24 read 23: Table H, p. 16, totals in last row, for 29 read 24. In the Table of *Pinus silvestris*, p. 21, the total at the bottom of column headed 11 should be 47 and not 46.

---

* *Phil. Trans.* Vol. 197, A, p. 361.

Variation in Ray-flowers of *Chrysanthemum leucanthemum*, 1133 heads gathered at Keswick, during July, 1895, by K. Pearson and G. U. Yule.

THE following table gives the unreduced raw material:

Number of Rays	Frequency	Number of Rays	Frequency
11	1	*24*	33
12	3	*25*	33
13	23	*26*	24
14	36	*27*	16
15	36	*28*	6
16	46	*29*	11
17	46	*30*	5
18	77	*31*	10
19	78	*32*	4
20	151	*33*	11
21	286	*34*	1
22	132	*35*	1
23	63		
		Total	1133

ON THE FUNDAMENTAL CONCEPTIONS OF BIOLOGY.

BY KARL PEARSON.

THE contrast between the old and new methods of dealing with biological conceptions has been recently emphasised by the publication of my memoir on Homotyposis*, and of Mr W. Bateson's criticism of it entitled, "Heredity, Differentiation, and other Conceptions of Biology†." To the biometrician it is a *sine qua non* that the conceptions upon which the theory of evolution is founded shall be concisely defined. Under such conditions only can they be quantitatively expressed, and without quantitatively exact expression it is impossible to use statistical methods. If the question be raised: Why are statistical methods to be used? the answer is clear: Because the whole problem of evolution is a problem in vital statistics—a problem of longevity, of fertility, of health, and of disease, and it is as impossible for the evolutionist to proceed without statistics, as it would be for the Registrar-General to discuss the national mortality without an enumeration of the population, a classification of deaths, and a knowledge of statistical theory. Yet this it seems to me is precisely what the school of biologists represented by Mr Bateson are attempting to do. I speak advisedly of the "school of biologists," for the matter is much wider than an individual controversy between Mr Bateson and myself. His paper which directly or indirectly attacks all the biometric work of the past ten years was published by the Royal Society at the recommendation and with the approval of its Zoological Committee. That Committee embraced some of the most distinguished English biologists, and we may therefore reasonably suppose that they attach meaning and weight to the terms used by Mr Bateson. They have made themselves a party to the controversy by allowing the issue under their aegis of extremely disputable matter, and matter which I believe can

---

* "Mathematical Contributions to the Theory of Evolution. IX. On the Principle of Homotyposis and its relation to Heredity, to the Variability of the Individual and to that of the Race. Part I. Homotyposis in the Vegetable Kingdom." *Phil. Trans.* Vol. 197, pp. 285—379 (Dulau and Co., Soho Square, London).

† *R. S. Proc.* Vol. 69, pp. 193—205.

be shown to have no basis whatever beyond that of confused and undefined notions. It will seem almost incredible to those readers of *Biometrika* who have been working for years statistically that some of these notions can still be accepted and propounded. They will say that variation, correlation, and heredity are concepts of which they have quite clear and quantitatively definite ideas; yet they will be startled to find how little the great body of English biologists have yet studied, or at any rate digested the biometric work of the last eight years. But the fact has to be recognised; biometricians have not only to collect material, analyse it, and see its bearing on vital phenomena, but they have still to convince the great body of biological workers that their methods are the *only* logical methods for solving, not necessarily every problem, but certainly many problems in the evolution of life.

It is therefore with considerable sense of the gravity of the contest that I take up the gauntlet thrown down by Mr Bateson, but it seems necessary to do so for the sake of our infant science. I should have been content for the present to continue my own work, leaving the old school of biologists rigidly alone. It is Mr Bateson who has forced the controversy by a brilliant but logomachic attack. He does not attempt to meet biometric conclusions by new measurements, he appeals to the significance of words, and to what he holds to be fundamental biological conceptions. A reply to Mr Bateson must therefore in the first place be an analysis of terms, and only in the second place a personal defence. The discussion accordingly tends to become dialectical, rather than ontological; we have to discuss the definition and use of words, rather than put observation against observation, fact against fact. Partly on this account,—because the controversy may be long and disputatious, and so, even were it free*, hardly fitting to the proceedings of a learned society,—partly because it is of fundamental interest to all biometricians, I have changed the *venue* to this journal.

In the paper of Mr Bateson's to which I have referred there is a very free use of the terms *Variation*, *Discontinuity*, *Differentiation*, etc., but he does not provide a definition of any one of these terms. He must therefore either be using them (i) in the sense of the memoir which he is criticising, or (ii) in the sense accepted by biological writers, or (iii) in some sense of his own which he has elsewhere defined. Now I will at once put aside (ii) for I can find no common denominator in the use of these terms by biological writers. If it exists at all, I must presume that Mr Bateson has not neglected it, when he formed his own conceptions on these points. Mr Bateson is therefore either using his terms in my sense, which I believe is in the main in accordance with current biometric practice, or he is using them in some other sense, somewhere or other defined by himself.

(i) *Is Mr Bateson using these terms in their current biometric sense?*

We might possibly expect such use from him when he is criticising a biometric memoir. But unfortunately Mr Bateson and I speak in totally different tongues.

---

* I am officially informed that I have a right to a rejoinder, but only to such a one as will not confer on my opponent a right to a further reply!

When one opponent has not even a preliminary training in biometry, and the other fails to attach any clear ideas to the terms used by his antagonist, used apparently as if they had universally accepted weight, it seems very hard to find a common ground for discussion. Let the reader not suppose this to be an exaggerated statement of the case. Consider the terms Variation, Correlation, Regression. There is nothing more familiar to the biometrician who has had experience of vital statistics than the distinction between a standard deviation measuring variability and a coefficient of correlation measuring degree of likeness or association. If he has only worked out the constants for one correlation table between two different organs he has learnt the distinction between these characters. He knows that any degree of correlation may be associated with any degree of variability. He knows that regression is not peculiar to heredity nor to identity in the organs compared. Now in my memoir I define homotyposis as the resemblance of certain like parts, it is therefore a *correlation*, and whatever its numerical value it may be associated as my memoir shows with all sorts of values of variation*. This is perfectly obvious to the biometrician so soon as he has realised the numerical definitions attached to these terms. Now Mr Bateson writes:

"An 'undifferentiated series of like parts' means only a series of like parts which have varied and are varying among themselves but little. A series of highly variable like parts is a series in which differentiation exists or is beginning to exist in complex and irregular fashion" (*R. S. Proc.* Vol. LXIX. p. 197).

And again: "If differentiation exists and is not recognised the apparent homotyposis due to individuality will, as Professor Pearson perceives, be immediately lowered" (*Ibid.* p. 169).

Now I have tried to understand what is the meaning Mr Bateson attaches to the terms used in these sentences and it appears to me as a direct result of the words cited that high variation is associated with low correlation and *vice versa*; or that variation and correlation have in Mr Bateson's biological usage a significance which is diametrically opposed to their numerical definition by the biometrician. We are obviously using the same words for very different quantities. Thus our use of the terms variation and correlation is clearly not the same. Nor is it better in the matter of regression. Throughout all Mr Bateson's writings, as well as in his criticism of my paper, there runs a hopelessly confused notion of what we are to understand by regression. The concept of regression is equally obscure in Professor Hugo de Vries' ideas on the establishment of breeds. Any population tabled for two characters in each individual or in each related pair, whether it be a population of coin-tossings, dice-throws, earwigs, or butterflies' scales, exhibits the phenomenon of regression, and this whether it is dimorphic or monomorphic, or exhibits continuous or discontinuous variation (in one of Mr Bateson's senses). All the statistician means by regression is this: If

---

* See, for example, p. 327 of my memoir, *Phil. Trans.*, Vol. 197, A., where it is shown how very sensibly reducing the variation of a character in the hart's-tongue fern does not sensibly alter correlation.

all the organs A of a certain size or value have associated with them an array of B-organs having a definite mean value, then this mean value changes with the change of A. The distribution of the means of B-arrays for given values of A, whether expressed by curve or table, is in its most general sense the phenomenon which Mr Galton has termed regression. Thus there is regression which may be determined between the number of court and plain cards in a hand at whist, between the head-lengths of two brothers, and between a measurement on the *imago* and another on its pupal case. Regression in its essence has no special relation to vital phenomena, nor to any hypothesis of parental foci and stable population. It is a fundamental conception of the theory of statistics*.

It will be clear to the reader that Mr Bateson does not use these terms in the biometric sense, possibly because he has not the preliminary biometric training. He is, of course, perfectly free to use them in his own sense, *except on an occasion when he is attacking a biometric memoir.* In replying to Mr Bateson, if I use the words referred to in the biometric sense, then we have absolutely no common ground. On the other hand it is somewhat unusual in a discussion to give entirely different meanings to the terms originally used, and leave your adversary to find out with what significance you may be using them. Indeed Mr Bateson seems to rejoice in the idea that all definition is impossible. The kernel of his argument is that variation cannot be distinguished from differentiation; possibly for this reason he avoids defining either term. He tells us that my memoir fails because this distinction cannot be made (p. 197, etc.). It is not a little curious to find Mr Bateson later admitting in a supplementary note that "these two classes of variation can broadly be recognised and treated as distinct" (p. 204), the two classes being apparently what he terms "Differentiant" and "Normal" types of diversity. But this I suppose was necessary in order to save his own theory that evolution takes place solely by the former kind of diversity,—i.e. the one which Mr Bateson asserts I cannot discriminate. He tells us that: "The attempts to treat or study them" (the context suggests his differentiant and normal variations) "as similar is leading to utter confusion in the study of evolution" (p. 204). But if we cannot distinguish them, how are we to study them by different methods? Either they are distinguishable, in which case his criticism of my memoir is idle, or they are not distinguishable, in which case his theory of evolution by "differentiant variation" is also idle†. *Man soll das Kind nicht mit dem Bade verschütten!*

But it is not only such terms as variation, correlation, and differentiation which Mr Bateson uses in a totally diverse sense from that used by me in my paper. In these cases, indeed, he departs from the current biometric senses, and we must search his own writings if we are to attach any meaning to them. But Mr Bateson takes away even my own definition from a word coined by myself. I have

---

* Half the obscurity consequent on its use by non-statistically trained biologists would possibly have been avoided had it been called "progression"!

† It is idle to attribute evolution to a factor you cannot distinguish from non-effective factors!

repeatedly said that I mean by the Principle of Homotyposis, "a *numerical* appreciation of the likeness and diversity among homotypes," and again, "the quantitative measurement of the degree of resemblance between undifferentiated-like organs being, so far as I am aware, a quite novel branch of investigation, I venture, with some hesitation, to introduce certain terms." Notwithstanding this definite statement as to what I mean by homotyposis Mr Bateson tells me that he should welcome my paper as an attempt—the only one so far as he knows—to emphasise and develop a conception introduced by him, namely, that "*the resemblance which we call heredity may be a special case of the phenomenon of symmetry*" (p. 194). "The principle that Professor Pearson calls 'homotyposis' I have expressed by the statement that the variations of parts repeated in series *may* be 'similar and simultaneous.' Beyond this we cannot yet go. Professor Pearson's statement of the principle fails to recognise one of the most important features of homotyposis. Expressed in my own terms, Professor Pearson's 'homotyposis' is the principle of 'similar and simultaneous variation' restricted to *undifferentiated like parts*" (p. 201).

Frankly I have not the least idea of what this "principle of symmetry" may be, or how "symmetry" on p. 194 is the same as "similar and simultaneous variation" on p. 201. I suppose they are definite biological conceptions, but to my purely mathematical mind both "symmetry" and "similarity" in this sense convey no meaning at all. As according to Mr Bateson "it would be easy to suggest terms better adapted to the expression of these conceptions" I heartily wish he had done so. My confusion, however, only becomes intensified when he tells us that he anticipates that "the largely analogous phenomena of rhythmical vibration will provide ready metaphors from which to construct a terminology" (p. 195, ftn.). I have had to consider largely symmetry, similarity and rhythmical vibration in the course of my studies, but how my mathematically concise notions on these points apply to, say, two leaves growing at different parts of a tree I am unable to appreciate. I venture to think that they are when applied without definition to vital phenomena, *idola fori*; precisely illustrations of that vague biological use of the well-defined terms of exact science against which I have elsewhere strongly protested*. My own strong opinion is that biological conceptions can be accurately defined, and so defined measured with quantitative exactness. We are only at the beginning of this new scientific era at present and I may well fail with imperfect biological training to give proper definitions myself. But I should be far readier to admit that there is nothing at all in the principle of homotyposis than to allow it to be placed in the same category as a "principle of symmetry" = a "principle of similar and simultaneous variation" = a principle which if it were not premature could be expressed in metaphors drawn from the "largely analogous phenomena of rhythmical vibration." This is the sort of language we know so well in mediæval works on physics. As it was cast out from physics, so it must disappear from biology.

---

* *The Grammar of Science*, 2nd Ed. p. 333. I can find no *precise* definition of these "principles" in Mr Bateson's *Materials for the Study of Variation.*

I have said enough to show that Mr Bateson and I do not speak the same language. I must now pass to my second point:

(ii) *What is the sense in which Mr Bateson uses his terms?*

In order to answer this question I was forced to postpone my reply to Mr Bateson until I had read his *Materials for the Study of Variation.* But I fear I am not much wiser now that I have done so. Mr Bateson nowhere gives concise definitions, to which he consistently keeps in the course of his treatise. His whole thought seems in flux, and if the reader believes he has Mr Bateson's sense on one page, he will find that the context connotes something totally different on the next.

Variation. I start first with Mr Bateson's definition of variation: "For though on the whole the offspring is like the parent or parents, its form is perhaps never identical with theirs, but generally differs from it perceptibly and sometimes materially. To this phenomenon, namely the occurrence of differences between the structure, the instincts or other elements which compose the mechanism of the offspring, and those which were proper to the parent, the name *Variation* has been given" (*Materials*, p. 3).

Mr Bateson suggests that Specific Differentiation has resulted from this Variation. Later he tells us that:

"To study Variation it must be seen at the moment of its beginning. For comparison we require the parent and the varying offspring together" (p. 7).

There is no doubt here as to Mr Bateson's meaning: variation is a study of the difference between two organisms which stand in the relation of parent and offspring, and to study it we require *both* these organisms for comparison.

Now two points appear to flow from these statements:

(i) Mr Bateson's conception of variation is not that of a measure of the deviations of a population from its mean. To the biometrician variation is a quantity determined by the class or group without reference to its ancestry. To Mr Bateson it is a measure of the deviation of the offspring from the parent. The biometric expression for such a measure might well be taken for any law of distribution, as the root mean square of such deviations, or:

$$\sqrt{(m_1 - m_2)^2 + (\sigma_1 - r\sigma_2)^2}.$$

In other words it would involve m_1, m_2, the means of the parental and filial generations, their variabilities or standard deviations σ_1, σ_2, and the coefficient r of parental inheritance. This is a highly complex expression, and it is noteworthy that the data for determining it are not in *one single case* given by Mr Bateson. In the great mass of cases for which I have seen data—at least 60 and probably 100 now—the population is either stable or approximately so, thus $m_1 = m_2$ and $\sigma_1 = \sigma_2$ nearly. In other words, unless $r = 1$, i.e. inheritance be complete, *the offspring on the average differs by a finite quantity from the parent.* This is true whatever be the nature of the distribution of the character in the fraternity. The coefficient of parental inheritance judged from upwards of 50 cases in insect, animal, and plant life is about ·4 to ·6. We may conclude then that whatever character we choose to deal with we shall find "discontinuity" between parent and offspring. Such "discontinuity" has nothing specially to do with vital deviations or with inheritance, it is a simple fact of the statistical distribution of any two quantities not perfectly correlated,—e.g. the number of trumps in two partners' hands at whist.

(ii) In collecting the materials for a study of variation as defined by Mr Bateson we must give particulars of *both* parent and offspring. We do not know whether a character in the offspring is a variation or not until we have a knowledge of the parent. The biometrician's definition of variation involves only a knowledge of the distribution of a character in a population; its relation to the distribution in the parent population involves a study of heredity. Mr Bateson includes under variation three distinct studies: (*a*) a change of type between parental and filial population, (*b*) a change in variability (in the biometrician's sense), and (*c*) an investigation of heredity.

Mr Bateson scarcely mentions heredity throughout the whole of his bulky volume. He does not compare parent and offspring, and thus in not one of the cases cited by him is there evidence whether or not the instance described is one of variation or not according to his own definition of variation! That is to say he tacitly drops the "Individual Variation" as he has defined it, and which he suggests is the source of "Specific Differentiation" and goes off to something else. In the bulk of cases this consists in comparing two or more members of a population,—*not* parent and offspring,—and treating their difference as a variation. This divergence between theory and practice renders it impossible to follow Mr Bateson when he uses the term "variation" in his criticisms on my memoir.

Discontinuity. We have already seen that when correlation is imperfect, whatever be the distribution of two characters, then statistical theory shows us discontinuity, and measures its average value. If this was all Mr Bateson meant by "discontinuity," he would be in the biometric camp. But in the course of his *Materials* he gives several further definitions, to which I must refer:

"The chief object, then, with which we shall begin the Study of Variation will be the determination of the nature of the series by which forms are evolved."—"The first questions that we shall seek to answer refer to the manner in which differentiation is introduced in these series. All that we know is the last term of the series. By the postulate of Common Descent we take it that the first term differed widely from the last, which nevertheless is its lineal descendant: how then was the transition from the first term to the last term effected? If the whole series were before us, should we find that this transition had been brought about by very minute and insensible differences between successive terms in the series, or should we find distinct and palpable gaps in the series? In proportion as the transition from term to term is nominal and imperceptible we may speak of the series as being **Continuous**, while in proportion as there appear in it lacunae, filled by no transitional form, we may describe it as **Discontinuous**....To decide which of these agrees most with the observed phenomena of Variation is the first question which we hope by the Study of Variation to answer." (*Materials*, pp. 14—15.)

Mr Bateson even suggests that for long periods the change may have been continuous and these periods interrupted by breaches of continuity. Now the "series" thus spoken of is not of course in this passage, nor is it indeed elsewhere in the work properly defined. It appears to consist of an individual and its ancestry. But is the series to consist (i) of the individual and one of its ancestors of each generation, or (ii) of the individual and something corresponding to my generalised "midparent" in each generation, or (iii) to the type individual of each generation? In the first two cases continuity is practically impossible unless the coefficient of parental heredity is unity, and this is contrary to every measurement of heredity yet made. All such series are of course discontinuous. If (iii) be Mr Bateson's series, although it does not appear to be*, the answer can only be found by comparing populations of different generations. This, however, is nowhere done in Mr Bateson's work. But if (i) or (ii) is Mr Bateson's idea of "series," then it follows that:

(*a*) We know such series to be discontinuous; this flows at once from our knowledge that parental heredity is less than unity.

(*b*) The only way Mr Bateson can test such discontinuity by a study of variation is to stick to his first definition of variation as the difference between parent and offspring.

As, however, he has entirely dropped it in his book, that book contributes absolutely nothing to the question of whether such series are "continuous" or "discontinuous." The statement of one coefficient of parental heredity for one character in one race would go far further to settle the point.

Now Mr Bateson's definitions of variation and of discontinuous series are in complete accordance with each other, only he has not used them in his treatise. Further they have no application at all to the problem of homotyposis, for we know every member, not merely the last of the homotypic series, and the variations dealt with are not deviations between parent and offspring in an unknown series.

We must then look further and see if we can find another definition of discontinuity given by Mr Bateson. Without a fresh definition of "series" or of "variation" we find on p. 38 of the *Materials* the "further meanings of Discontinuous Variation" explained by the help of examples. The first illustration used is that of a dimorphic male beetle. Mr Bateson gives a frequency polygon for the

---

[*] He compares in his work over and over again members of the *same* generation, and speaks of their differences as "discontinuous variation."

size of the cephalic horn. He states that "he is not acquainted with evidence as to the course of inheritance in these cases, and I do not know whether 'high' and 'low' males may be produced by one mother" (p. 40)*. In other words Mr Bateson admits that he is not considering either variation or discontinuity in the light of his own definitions. Discontinuous variation means now for him a dimorphic distribution of a character in one generation, even when we are quite ignorant of whether the immediate or the evolutionary ancestry proceeded by continuous or discontinuous series in the sense of the earlier definition. Seeking for further light, we find Mr Bateson suggesting "that the separation of the males into two groups was a case of characters which do not readily blend, and are thus exempt from what Galton has called the Law of Regression." Since eye-colours do not readily blend, and as I have shown in 20 to 30 sets of relationships, undoubtedly obey the law of regression, this does not throw more light on Mr Bateson's second definition. It is, however, an illustration of what I have above referred to, Mr Bateson's confused state of mind as to regression. On p. 42, however, Mr Bateson tells us that the existence of intermediate links between the types of dimorphic forms "does not touch the fact that the Variation may be Discontinuous, for we are concerned not with the question whether or no all intermediate gradations are possible, or have ever existed, but with the wholly different question whether or no the normal form has passed through each of these intermediate conditions. To employ the metaphor which Galton has used so well—and which may prove hereafter to be more than a metaphor—we are concerned with the question of the position of Organic Stability; and in so far as the intermediate forms are not or have not been positions of Organic Stability, in so far is the variation discontinuous."

This is the third definition of discontinuity implying a new definition of variation given within fifty pages!

The first depends on variation defined as a deviation between parent and offspring being finite. This is true for many, and possibly for all living forms whatever their distribution. It is simply a statistical result of a correlation coefficient less than unity.

The second definition refers to one generation alone, and depends upon a recognition of statistical heterogeneity in the distribution of the population. Mr Bateson apparently supposes such heterogeneity is associated with bimodal polygons.

The third definition has nothing whatever to do with variation or heredity as far as I can understand. If m_1 be the mean for one generation and m_2 the mean for the next, the variation is to be treated as continuous or discontinuous according as $m_2 - m_1$ is sensible or insensible. Now I should consider $m_2 - m_1$ as a measure of the *change in type* produced by environment, natural selection, or other

---

* What is equally or more important, we do not know if they may be due to one father, or indeed produced at one mating.

source of change. But what permits of a change in the type? Why, the existence of variation in the biometrical sense. If Mr Bateson terms the change in type variation, what name does he give to the distribution of deviations which alone render this change of type possible? It is perhaps needless to add that, if discontinuous variation be summed up in the problem of whether positions of organic stability have ever existed in forms intermediate between recognised dimorphic types, Mr Bateson has not discussed discontinuous variation at all in his work, for we are certainly not given any data for dimorphic populations at different stages of their evolution, nor even statistics for several local races of one species of recognised dimorphic character. To those who have studied my memoir on Homotyposis there will be no need to say that discontinuity as described in either Mr Bateson's first or his third definition has no bearing whatever on that subject. If Mr Bateson, however, relies solely on his second definition, namely, that variation is discontinuous when there is heterogeneity in the statistical distribution of frequency, then we may reasonably expect from him a study of frequency distributions. Will he tell us what he understands by homogeneous and heterogeneous distributions? Writing of a discontinuously varying population in 1897*, Mr Bateson says:

"When such a population is seriated in respect of the varying character for statistical study in the manner with which naturalists have been familiarised by the writings of Galton and others the curve of variation has not one peak as in a monomorphic species, but has at least two peaks."

Of course, from the statistical point of view this is an impossible definition of heterogeneity. Not only may two or many peaks occur in perfectly homogeneous material, but no peaks whatever in certainly heterogeneous material. It all depends on whether the peaks are significant or not, and on the distance between the modes of the mixed material. Indeed, if Mr Bateson's second definition be his final one, it can only be applied by a mathematical biologist, for the discrimination of modes is a most complex problem involving the theory of errors of random sampling†. Further the resolution of heterogeneous frequency distributions in biology will depend: (i) on an intimate and extensive knowledge of the distribution of frequency for organic characters for many types of life; (ii) a selection on philosophical or on empirical grounds of theoretical distributions to represent these; (iii) a test in any individual case of whether these theoretical distributions represent the observed facts within the errors of random sampling, and (iv) supposing they do not, their resolution into component distributions.

To sum up, Mr Bateson has given three distinct definitions of discontinuity:

(*a*) A variation is treated as a deviation between parent and offspring, and variation is discontinuous if this be finite.

---

* *Science Progress*, N. S. Vol. I. No. 5, October.

† It would be interesting to know what degree of heterogeneity Mr Bateson supposes to exist in his "low" male group of Java beetles (p. 39). They have at least three apparent modes. Are the "low" males in themselves trimorphic?

Such discontinuity must exist if correlation be not perfect, it is a well recognised result of statistical theory.

(*b*) A variation is treated as a finite deviation in type between one generation and a second.

Only a comparison of parental and filial populations can test the existence or non-existence of such discontinuity. Measurements of such populations have been over and over again published by biometrical workers on heredity. I believe Mr Bateson has not published a single such comparison in his book.

(*c*) Discontinuity is attributed to distributions of frequency which are bimodal or multi-modal.

The distinction of true from apparent modes is a very delicate problem in the logic of chance. But it is exactly the statistical processes of the higher mathematics—which Mr Bateson tells us have gone wide of their mark, if that be the elucidation of evolution—by which alone we can hope to solve the problem which according to this definition of Mr Bateson's is involved in discontinuity, and according to the biometrician lies in the heterogeneity of frequency or the differentiation of the organ in question.

Thus Mr Bateson has given us three definitions. Which of them is to be considered as fundamental or primary? I do not know. *Not one of them has been used in his own treatise to test whether the cases he adduces are variations, or, if so discontinuous variations.* Which of them am I to suppose he refers to when he criticises my memoir on Homotyposis? I do not know, I can only try them all.

Now Homotyposis has nothing whatever to do with a comparison of deviation between parent and offspring, nor has it anything whatever to do with the question of whether the type changes infinitesimally or finitely between successive generations. Hence the only possible definition that applies to homotyposis is that considered under (*c*) above. I charitably suppose Mr Bateson to refer to his definition of 1897, and not to those of 1894, although he has in the words cited on p. 328 above expressly told us that discontinuity of variation is not this, but something very different.

If so, the whole point between Mr Bateson and myself turns on whether or not it is possible in the bulk of cases to detect heterogeneity or not in a frequency distribution. I contend that the mathematical statistician is doing this every day, but I also contend that the validity of his processes cannot be judged by biological reasoning. Mr Bateson's only hope lies in a discussion of the logic of chance, he must criticise the mathematical bases of the theory of statistics. To assert without a knowledge of the mathematics of the problem that a frequency distribution cannot be resolved, is like a statement made by one ignorant of harmonic analysis that curves cannot be analysed by a Fourier's series.

In short if Mr Bateson means by discontinuous variation, what I understand by heterogeneity of frequency, he can only question the adequacy of our tests by a complete study of the mathematical methods of modern biometry. The moment he does this he will have to recognise that his own treatise on variation contributes nothing whatever to the study of discontinuity.

I have said enough, perhaps, to show that Mr Bateson and I do not use the same language, and to indicate how very difficult controversy must be when we have no common definitions. Yet many biologists will read Mr Bateson's paper who have neither the opportunity nor perhaps the inclination to study my original memoir. In biology I have been told that a statement made by any individual biologist is considered true until some other biologist takes the trouble to contradict it. Then it is considered doubtful and one authority is weighed against a second. In case absence of contradiction should imply acceptance of statements as true, I wish to state once for all that for years I have not replied to English or German critics because the publication of further results obtained by biometric methods seemed the best answer to those who suppose silence synonymous with discomfiture. But if one is forced against one's will into controversy, let it be complete; and so let me state once and for all that I consider Mr Bateson's peculiar theory of evolution by discontinuous variations untenable. It is, as he recognises, quite incompatible with much of my own work on evolution. I have not, however, spent my energies in criticising it, nor do I intend to do so on the present occasion. I doubt even whether I fully understand what he means by the term "discontinuous"; I am far from certain that he himself is clear on the point—several definitions may be extracted from the *Materials*. But I do know that I have gone through hundreds of populations now, each involving several hundred up to a thousand individuals for a great variety of characters in both the animal and plant kingdoms, and I find, when really comprehensive populations are examined, so little of anything like this discontinuous variation in which Mr Bateson puts his faith*, that I doubt whether it has any statistical validity in that *mass* struggle for existence which occurs in nature. On the other hand, taking variation in its biometric sense for a continuous homogeneous distribution of frequency, I do find definite evidence of progressive change in races. I think we have now sufficient data, for example, to show that selection has taken and is taking place in man. If we take a long series of measurements of the skull in prehistoric and dynastic Egypt there can, from the measurements themselves, be no reasonable doubt that we are dealing with the same race, nor again in the case of Englishmen to-day and of Englishmen 250 years ago. But a change, a

---

* Mr Bateson cites Dr F. Ludwig's interesting researches as showing "discontinuous variation" in plants, and speaks definitely of the "laws such distributions commonly obey." Here again we have evidence of the impossibility of testing the truth without adequate statistical theory. In many cases the multimodal character of Dr Ludwig's curves is simply due to the divergencies of random sampling, and without a theory of the probable errors of random sampling we may make "discontinuous variations" out of statistically insignificant differences! In other cases there is undoubted heterogeneity, but whether Mr Bateson will consider it due to "discontinuous variation" when he sees its real cause is another matter. The clue to the mystery was given in a note to Part I. of *Biometrika* and is more fully developed in a series of papers in this Part.

significant change, has in both instances taken place between the earlier and later series. There is nothing here of the nature of "discontinuous variation." You may go through hundreds of skulls and find occasionally discontinuity in one of Mr Bateson's senses, interparietals, wormian bones, and cases of fused atlas, but it is not in the direction of these things (even if they be truly discontinuous, which I doubt) that evolution has taken place. It is rather in the non-exceptional characters which vary with what Mr Bateson would call normal variation. Of course Mr Bateson may say that there is really differentiation there; it is he, however, who identifies "differentiant" diversity and "discontinuous variation." If then we have no reason to suppose that any of these *marked* cases of discontinuity have been sufficiently numerous and sufficiently profitable to lead to survival (without artificial protection), why should we suppose that those he merely asserts exist, but which he says cannot be distinguished*, have been what the marked cases have not been, i.e. the material for evolution? Fix for a moment our attention on man; his races are distinct, and their distinguishing characteristics are in large part, at the very least, those which we know to give continuous frequency distributions. Take the case of the skull; as soon as 20 to 40 measurements are taken on a population we see at once the special features which separate and connect that population with other local races. We see at once broad relations connecting ancient and modern Egypt, mediæval and modern English, Aino and Japanese; we see also the well-marked differences of such groups. In no case, however, is it what Mr Bateson terms a "discontinuous variation," still less a "meristic variation," which differentiates the skulls of local races. Mr Bateson cites with approval Virchow's statement that "every deviation from the type of the parent animal must have its foundation on a pathological accident†." Well, the markedly "discontinuous" variations of the skull, which I personally should describe as due to pathological accidents, are precisely those which, whether they are more or less frequent in one or another race, do *not* form the distinguishing racial characters of the races. Mr Bateson tells us that a study of the continuous variations such as I have made in my memoirs goes "wide of its mark, if that aim is the elucidation of evolution." I believe, if we can once grasp how the local races of man, even in one organ like the skull, have become differentiated from one or more common stocks, we shall have reached the first definite stage in the solution of the problem of evolution. But the worker who endeavours to solve this question of the local races in man by tabling either "discontinuous" or "meristic" variations will make small progress. And if the continuous variations can be shown to be a sufficient source of the divergent characters in the local races of man, and the so-called discontinuous variations to have no importance, we have at least a probable basis for attacking the problem

---

* "The attempt to exclude differentiation by definition must constantly fail in practice" (p. 205). That is the "issue in a word" according to Mr Bateson.

† *Materials for the Study of Variation*, p. 74. Of course every individual deviates from the type of its parent, as everyone who has measured a population of parents and children must recognise. But as usual the word "type" is here being used biologically or without quantitative definition.

of local races in other cases, and ultimately, perhaps, the differentiation of species. But the safe way to reach the latter is through the problem of local races.

If Mr Bateson wishes to attack the problem of evolution by what he terms discontinuous variation, he must go far further than forming a useful catalogue of museum and collectors' deviations from "type." He must trace first whether in any given case they are or are not inherited, secondly he must discover whether or not the individuals who possess them are more fertile than the "type," thirdly whether the death-rate is with regard to them selective or non-selective. Shortly starting with a race having among its members a few with a recognisably discontinuous variation, he must show how its descendants at a later period have the discontinuous variation of the earlier period as a dominant character. In other words he must deal with the vital statistics of a population, or proceed *biometrically*.

Mr Bateson writes (p. 202) as if I were to-day inclined to allow more to "discontinuous variation" than I did in 1895, and this although he cites on p. 204 a passage from my memoir on Homotyposis of 1901 practically identical in spirit with a second he has cited previously from my memoir of 1895. The only basis for his belief lies in the fact that I should heartily welcome any attempt to demonstrate by a satisfactory statistical investigation—none other is valid—that a significant change has occurred in any wild species in its natural environment by a "discontinuous variation" which is sufficiently marked to be distinguishable from a continuous series of variations *. Till such a biometric investigation as I have suggested is made I must adhere to my statement as to the distribution of variation, for it accords well with the populations I have myself examined and measured in the case of both animals and plants. These populations may be far fewer than those upon which Mr Bateson bases his statements, but as far as I know he has not published large series of the frequencies of various organs in different populations†, which would enable me to test whether or not my "description accords ill with the observed facts of variation." It does not accord ill with the many series I have myself published or with the still more numerous data which I have still unpublished, and which have also influenced my judgment on this point. To sum up then, "discontinuous variation," which is sufficiently marked to be separable from continuous variation, is so infrequent (I do not say it does not occur) as to be statistically negligible for the purpose

---

* Careful selection of slight variations appears to be effective in the case of artificial selection. See, for example, Sir W. T. Thiselton Dyer: The Cultural Evolution of *Cyclamen latifolium*, "The striking results obtained by cultivators have been due to the patient accumulation by selection of gradual but continuous variation in any desired direction." *R. S. Proc.* Vol. 61, p. 147.

† In Mr Bateson's *Materials* in hardly any case are statistics of the general population given. In three cases—those of the common earwig (p. 40), the Java beetle *Xylotrupes gideon* (p. 39), and the cockroach *Blatta Americana* (p. 417)—are statistics given for the general population of a locality. In none of these cases is evidence given as to the inheritance of the "discontinuous variation," and in one it is suggested that the variation is possibly due to regeneration. It would not I presume be difficult to test the question of inheritance by separating the dimorphic forms; and one instance of death-rate correlated with such dimorphism in a population would in my opinion be worth a whole catalogue of "meristic variations."

of vital statistics. It must be admitted at once, however, that in this discussion of evolution by discontinuous variations I have used the term not in the precise sense of any of the definitions discussed on p. 329, but in what appears to be the sense actually adopted by Mr Bateson in the body of his treatise, i.e. as a name for any abnormal value of a character in an individual which has not been linked up in continuous series with the bulk of the so-called normal population; whether the abnormal character is pathological or not, whether it could or could not be linked up if a large enough population were taken, is as a rule not discussed*.

Yet it is to this vague "differentiant variation," represented in his book by apparently unlinked character values, that Mr Bateson appeals for the basis of his theory of evolution. It is because I do not, according to him, recognise its existence that my memoir on homotyposis is idle. Nay, rather because it is unrecognisable! According to Mr Bateson it crosses and re-crosses normal variation in such a manner that the two cannot be distinguished. What in my memoir on homotyposis I do recognise and try to avoid is a frequency distribution, the elements of which are not homogeneous, i.e. are not due to the same group of chance-causes, but are compounded of two or more series due wholly or in part to different groups of chance-causes†. This is what I understand by differentiation, but it is something totally different from Mr Bateson's "differentiant variation," as illustrated in his treatise. It is, however, all that my memoir is concerned with, and I do not hold the tests for such differentiation peculiarly hard to apply.

Mr Bateson takes the case of a syllid with numerous segments apparently undifferentiated but with marked differentiation of the segments at the posterior and anterior ends. How, he asks, are we to consider which, if any, of these segments are suitable for investigating homotyposis? Probably I should not take such a case for studying homotyposis at all, for each segment may bear an *organic* relation to its neighbours; there may well be a condition—as of fitting of adjacent parts—which is expressly excluded in the production of pure homotypes. But if Mr Bateson desires to know how I should determine whether there was *differentiation* of any significance between two of these segments for any chosen character, the biometric answer is perfectly simple. Measure the characters of these two particular segments in a sufficiently large population and determine whether the differences of the means and of the standard deviations are or are not sensible in comparison with the probable errors of those differences. If they are not, then

---

* Of course Mr Bateson has distinctly stated that the continuity of the frequency distribution has nothing to do with his definition of discontinuity (see p. 328, above). But he certainly does not apply any other test than the apparent discontinuity of the frequency to the bulk of his cases,—he never applies any other of his own definitions.

† E.g. n p-sided teetotums might stand for n chance-causes; each on a spin of the whole system would give results peculiar to that spin. A frequency curve based on those spins would be "homogeneous"; but if in the middle of the operations, m of the n teetotums were replaced by q-sided teetotums there would be differentiation in the frequency in my sense.

one set of segments may be looked upon as equivalent to a random sample of the other set and there is no class-differentiation. This method is so familiar to statisticians, who are using it every day to test whether a class is or is not differentiated from the general population, that it appears somewhat surprising that Mr Bateson should believe we are in the habit of detecting differentiation solely by an inspection of modes in seriations*. Whenever therefore there is a suspicion that "homologous" organs or parts, differing in (*a*) period of production, (*b*) regions of the organism where they are produced, (*c*) environmental conditions generally, are really differentiated, there is no difficulty for the trained biometrician in actually testing whether this differentiation exists, and, if so, the extent of it. The influence of such factors in the differentiation of homologous parts might be expressed in the following definition:

If there be correlation between the means of the homologous parts produced and (*a*) the period of life at which they are produced, (*b*) the part of the organism in which they are produced, or (*c*) the environmental conditions under which they are produced, then we may call the arrays of organs produced under constant type conditions (*a*), (*b*) or (*c*) differentiated classes of homologous organs. But if the correlation between the mean characters of the arrays of organs and the factors (*a*), (*b*) or (*c*) be small or evanescent we term the organs undifferentiated.

The differentiation in *Nigella* was recognised by the correlation between the segmentation and factor (*b*) long before the frequency diagram was reached. But surely one who has been through hundreds of distributions of variation in all kinds of types of life would recognise differentiation in the heavy line distribution of Diagram I. long before the next stage of determining the correlation was completed. Compare the distribution for *Nigella* with that for the veins in beech leaves of Diagram II. The variation in the latter is within the limits of random sampling a normal chance distribution. The former is seen at once to be heterogeneous. A change of environment alone suffices to emphasise the differentiation. The seed from the *Nigella* capsules was sown again in the following year, but very thinly, so that many capsules were produced on the side shoots: see dotted line in Diagram I. In this crop there was an average of 20 capsules to a plant, whereas 4 was about the average in the parental series. The distribution is so markedly bimodal that even Mr Bateson's third definition of discontinuity† would exclude it from my "undifferentiated" like organs. Here the frequency of 5 segments is to that of 8 as 8 is to 3, but in a third sowing they were even as 9 to 2. Hence it follows that the number of capsules with about five segments or "low" capsules can be increased or diminished in relation to the "high" capsules by the environment of the plant. The material is therefore in my sense of the word heterogeneous or the like organs are differentiated, they stand in

---

* The latter may be sometimes of use to suggest further examination, but this method is often a real danger when it is used by those ignorant of the extent of seriations which are solely due to random sampling.

† See p. 330.

Nigella Hispanica. Segmentation of Seed Capsules.
——— Parents. ------ Offspring.
Distribution of 2500 Capsules taken for comparison.

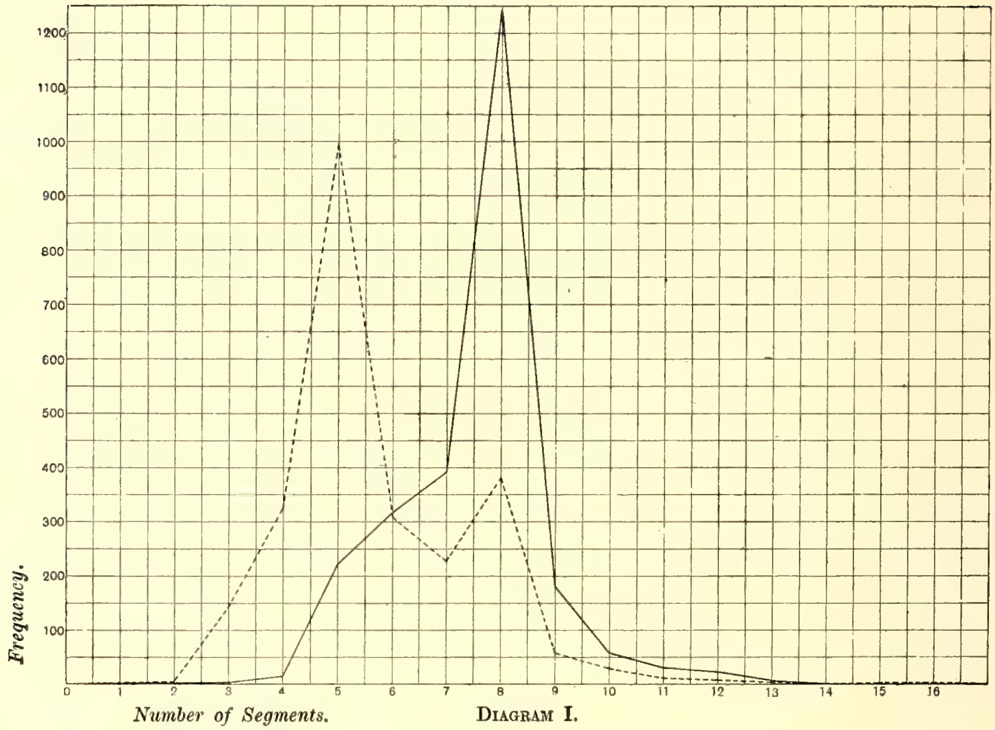


DIAGRAM I.

Veins in Beech Leaves illustrating close approach to homogeneous material.
2600 Leaves. ------ Normal Curve.

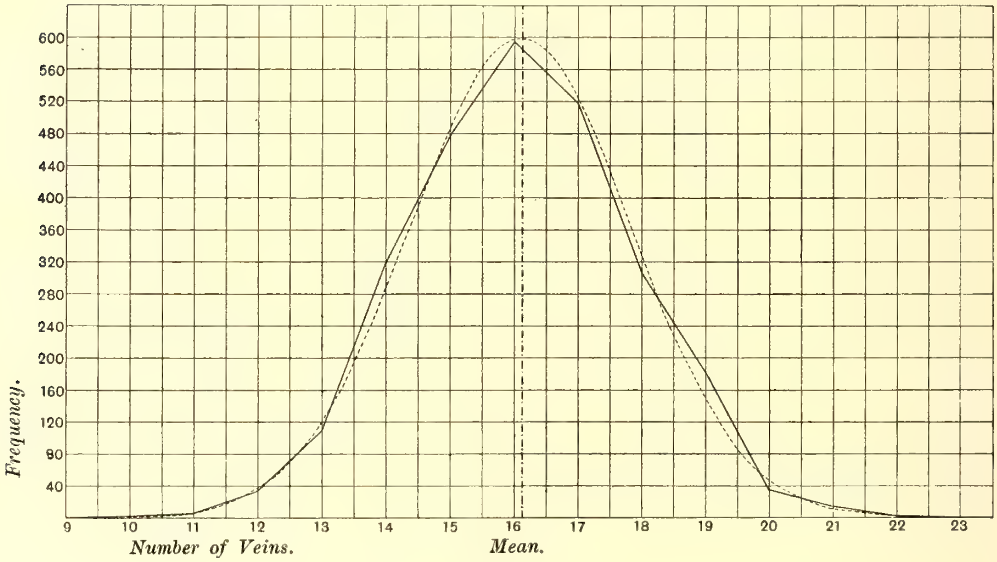


DIAGRAM II.

different relations to the same environment. I should myself be quite content to reject the *Nigella* for homotypic purposes on the basis of the continuous line, but further experiments were made with the offspring of the first year's *Nigella* to fix absolutely the nature of the differentiation*.

It may be of interest to note that a purely algebraical attempt to resolve the 1899 *Nigella* into its components led to the result

$$r = 1{\cdot}17 - {\cdot}9\rho,$$

where r is the homotypic correlation and ρ the organic correlation between high and low capsules on the same plant. At present I have not data to determine ρ, but if its value be as I suspect from like cases in plants at least as high as r, then the homotyposis of *Nigella* will lie between ·5 and ·6, i.e. about three times its apparent value as affected by differentiation. The labour of such an investigation is only justified in the present case, because Mr Bateson appears to think that the biometrician has no power of detecting differentiation or having detected it of analysing his material. I think if Mr Bateson were better acquainted with the really large amount of work which has been done in detecting class and race differentiation by our modern methods he would speak less confidently of the difficulties which he, without having applied these methods, feels certain must arise in the practice of them.

Turning now more directly to the problem of homotyposis, I believe that Mr Bateson would have understood my paper better had he not read his own in February 1901—a date some weeks before my own paper was notified as accepted and ten months before it was printed and available for study. Thus he entirely misunderstands the relationship between fraternal correlation and homotypic correlation. He appears to think that I am working with an analogy of some sort, and writes: "it would then be expected that the correlation between those repeated parts of the same individual would be similar to that between the germ cells of its parents" (p. 195). Of course the argument has nothing to do with "the repeated parts of the same individual." It is briefly the following:

(i) The organ or character C in an individual A, putting aside the influence of environment, is determined in some way unknown to us by the characters or organs of the two gametes E and F from which was formed the zygote out of which A sprung.

(ii) The organ or character C' in another individual B will also be determined by the characters or organs of the two gametes E' and F' from which its zygote was formed.

(iii) Hence if A has hereditary relationship to B the correlation between their like characters C and C' must ultimately be deducible from the relationship between the gametes E, F, E', F'. In particular if A and B are brothers their correlation for any character depends upon the fact that E and E' are products of one gonad and F and F' products of a second gonad.

---

* I have very heartily to thank Mr A. G. Tansley for taking charge of this crop.

(iv) Hence the fraternal correlation must be a function of the correlation between characters or organs of the gametes put forth by the same gonad.

So far I do not see that any exception can be taken to the argument; then, by the adoption of, I think, fairly legitimate limitations, it is shown mathematically that the *average* correlation between brothers for any character will be equal to the *average* correlation between the characters of gametes. To test this theory we must then endeavour to find out what is the quantitative relationship between the organs or characters of gametes. Now the production of gametes seems a process analogous to that of the production of any like organs by an individual, and the average value of the correlation of such organs ought to give us a value approximating to that of the average correlation between the scarcely measurable organs and characters of the gametes themselves. Such is the general tenor of my reasoning. What kind of like organs ought then to be dealt with in order to compare the results with those for the relationship between pairs of brothers? Our data for brothers were drawn from types of life—man, horse, dog—in which there was no sensible class differentiation, tested by either biological or biometric methods*. Such differentiation where it exists must either be a result of environment, in particular of nurture, of period of production, or of *differentiation in the gametes themselves.* Its absence accordingly was the very sufficient reason for comparing the correlation of characters in the gametes with the correlation of *undifferentiated* like organs. Hence the source of my definition of homotypes as "undifferentiated like organs." It will be seen at once that the whole of Mr Bateson's argumentation is purely idle, and it is more than idle, it is, I venture to think, largely captious. Had I been comparing brothers of differentiated classes A and B, I should have tested whether for the characters I was dealing with differentiation did or did not exist—a test I again say we are perfectly able to make†. I should then have correlated A with A, and B with B, and probably A with B, but not mixed pairs of A and B with pairs of A and A. This is precisely what we do with pairs of brothers and pairs of sisters, where there is a sexual differentiation. The possibility of dealing with pairs of A and B without introducing heterogeneity may surprise Mr Bateson, but I fear the mathematics of this must be passed over on the present occasion.

Now Mr Bateson's charges were:

(i) Differentiation between like organs is not distinguishable.

My reply is that so far as it produces an effect comparable with the errors of random sampling it is distinguishable by well-known tests.

---

* Stature and forearm, for example, have been frequently tested for heterogeneity.

† Perhaps it may be as well to note that differentiation for the biometrician denotes heterogeneity of *mean* and standard deviation for two or more parts of the population for the differentiated character, and this can be found by breaking up the population into classes. For Mr Bateson it seems as I have indicated to mean something quite different, but as he avoids in his paper definition I am unable to say what. No biometrician could use variation and differentiation as in any way synonymous.

(ii) Differentiation exists between pairs of brothers, and therefore I ought to have included the correlation of differentiated like organs in forming my average.

My reply is that I was dealing with types of life in which differentiation between pairs of brothers is not sensible, and therefore I was perfectly justified in seeking the correlation of undifferentiated like organs, my homotypes.

Mr Bateson wants to know what I should do if I had to deal with fraternal correlation in a community of ants. I reply that nobody at present knows anything whatever about heredity in a confraternity of ants, and that until some attempt has been made to apply the exact methods of biometry to such communities it is impossible for him to assert either that differentiation is so imperfect that it cannot be determined, or still more that if it exists without being sensible it would have any sensible effect on fraternal correlation*. Mr Bateson will I hope pardon me if I say that a quantitative study of variation and heredity in ants, starting with those genera where differentiation can be detected, would be a far more valuable test of my views, than any amount of appeal to what may or may not be the case under circumstances which nobody has tested.

(iii) Mr Bateson charges me with being compelled "to pick and choose" my cases, and he puts this charge in a manner which anyone reading his paper without studying mine—and there will be many such among biologists—would undoubtedly interpret to signify doctoring of returns.

"In plain language, we shall have to pick and choose our cases, and the value of our coefficient of homotyposis will depend entirely on how we do it. Has not Professor Pearson himself been so compelled in more than one of his examples, notably in that of *Nigella*?" (p. 202).

Any reader of this passage would think that *Nigella* and other things had been discarded by me after I had found their coefficient of homotyposis to be out of keeping with my theory. But what are the facts clearly stated in my paper? Why, that after the first few *Nigella* were examined *they were recognised to be differentiated*, long before their coefficient was found. That again I was distinctly warned not to include *Asperula odorata* and *Scolopendrium vulgare* by well-known botanists before I had even begun to collect them; that *Malva rotundifolia* was collected with a full knowledge that it had spread by stolons, and that I did collect and measure the homotyposis in all these things, because I wanted to appreciate the effect of differentiation and common origin on the coefficient of homotyposis. And having done this, did I put them on one side as I really ought

---

* It is possible that the correlation between pairs of brothers both in the army and pairs one in and one out might differ slightly for men. Mr Bateson might be puzzled to know how to rate a pair one of whom was a volunteer. I happen to know the effect of volunteering on fraternal correlation. It is sensible but of no practical importance. That one quantitative fact is of more value I take it than Mr Bateson's sweeping statement about ants whose heredity nobody has yet studied by exact methods, i.e. "average fraternal correlation, I think, has no meaning, still less an ascertainable value in these cases" (p. 201).

to have done? Not at all! I did not choose to leave them out of account because I recognised how a certain type of critic might think he saw his opportunity. This is what I wrote in August 1900—and Mr Bateson's paper was read in February 1901:—

"In summing up my results and comparing them with those obtained for fraternal correlation by my co-workers and myself I felt some difficulty. If I made a selection of what I considered the best homotypic correlation series, and the best fraternal correlation, I might lay myself open to the charge of selecting statistics with a view to the demonstration of a theoretical law laid down beforehand. Accordingly I determined to include all my homotypic results, except those for the absolute dimensions of mushroom gills and ivy leaves, where it was pretty evident that we had to a greater or lesser degree an influence exerted by the growth factor" (*Phil. Trans.* Vol. 197, A, p. 355).

Now what about these mushroom gills and ivy leaves? Why, that they were measured with *the definite intention of using only the ratio or index for homotyposis*, because we were already very familiar with the correlation due to growth. When one has been in the habit of forming correlation tables between age and growth and knows that this correlation can be as large as ·5, one does not blindly confuse the growth and the homotyposis factors. What would Mr Bateson have said if I had determined fraternal correlation in head length between minor brothers without reducing them first by means of the growth correlation table to a standard age? Yet how does Mr Bateson refer to my necessary exclusion of the ivy leaf and mushroom absolute measurements, an exclusion designed *ab initio** from the homotypic values?

"The values found range from ·1733 to ·8607. Reasons are put forward for excluding some of the highest and for doubting the validity of others, especially some of the lower ones" (p. 196).

Take this in conjunction with the passage in which Mr Bateson speaks of my having been compelled to pick and choose my results and I assert that the impression formed upon the reader will be an entirely erroneous one. These passages would no doubt have been modified had Mr Bateson not hastened to print and read his paper before anything but the abstract of my own was available for criticism. Nothing has been chosen or excluded *after* we knew what its homotyposis was, neither *Nigella* nor hart's-tongue nor woodruff, they are *all* used for the average. The table stands exactly as it was intended it should stand when the material was settled upon and before the constants were calculated.

Mr Bateson cannot maintain logically a double position: (i) that it is wrong to exclude differentiated organs and (ii) that I pick and choose. For the organs which I know to be differentiated have actually been included because I knew beforehand what sort of criticism my paper would rouse.

---

* "This series was originally undertaken by Dr E. Warren, using as his character the index or ratio of length to maximum breadth. It was hoped that in this manner the growth factor might be fairly well eliminated," *Phil. Trans.* Vol. 197, A, p. 240, see also pp. 338, 339.

I might with perfect justification have excluded several cases from my Table. For example I gave the correlation between cephalic indices of brothers to be ·3790 and between tempers to be ·3167. I knew these were very low and based on unreliable material, but I gave everything I had at that date. From far larger and more reliable material I know now that the correlation of cephalic index between brothers is ·4861 and of temper ·5068. I contented myself with entering the older values with due words of warning.

But, perhaps, Mr Bateson means that I have excluded things to which no reference is made in the paper at all? He writes:

"Yet another and even clearer illustration. The two claws of a crab are a pair of homotypes. Their homotypic correlation in respect of any character, length, for example, might be determined" (p. 199).

Well, I do know the correlation between several characters which occur in the claws of crabs. It never once entered my mind to include them. I know further the correlation between right and left corresponding bones of the skeleton in man for a considerable number of races—for the skull, the hand, the chief long bones etc., etc., perhaps in all for forty or fifty cases. If such things are homotypes according to Mr Bateson I have been guilty of excluding correlations which would bring up the homotypic average to somewhere between ·9 and 1! But these are not homotypes in my sense of the word, which may perhaps aid Mr Bateson to see that his "principle of symmetry" is not synonymous with homotyposis. The correlation between right and left member is in my sense of the word *organic* and not *homotypic*. I term the correlation between two members "organic" when its value is wholly or partly determined by the fact that for the welfare of the individual the members must within certain limits "fit," they have a function to perform in common and their mutual relationship has been controlled during its evolutionary development by the existence of this common end. In homotyposis this purpose, i.e. the performance of a common function as controlling the relationship, has either no existence or is insensible—the mutual relationship is due to the individuality of the producer, and is practically uncontrolled by the importance to the individual of the homotypes performing at some time related but really differentiated parts in a common function. It is not vitally important to a beech tree that one leaf having 12 veins upon it another gathered from anywhere-else on the tree should also have 12 or nearly 12. But it is important to a man that if he has one femur of 456 mm. the other femur should be within a few millimetres of the same length*. Thus in right and left-hand members we have a differentiation in function, one member could not possibly replace the other, and if the differentiation were not visible at once to the biologist, it would be evident at once to the biometrician in the angle means. Such cases therefore are very fully excluded by the definition of homotypes, and I confess I do not understand how Mr Bateson finds

---

* In an early stage of evolution possibly all correlation was nearly homotypic, but selection of "fitting" individuals would soon emphasise the "organic" factor, i.e. it would reduce the variability of the array associated with individuals of a given size.

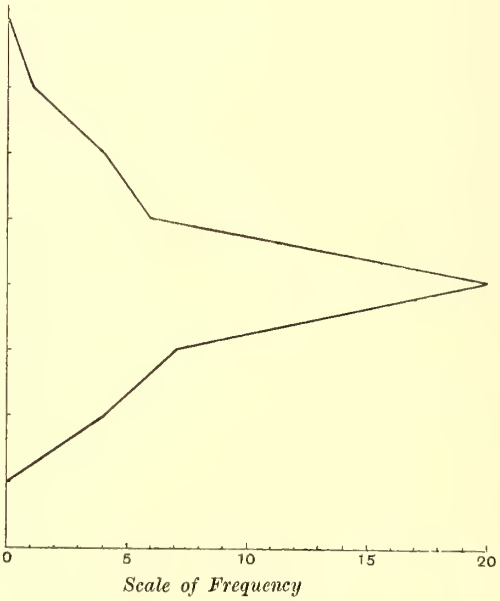
the two claws of a crab an "even clearer illustration" of homotyposis than digits III and IV in a Deer! I suppose it must be in some way because he supposes "bilateral symmetry" to be really the basis of homotyposis.

In summing up my results on homotyposis and fraternal correlation Mr Bateson writes:

"Professor Pearson attaches importance to the rather close similarity between the two average values. We are bound, therefore, to remark as a suspicious circumstance that the range of values is so wide, and that the average value should so nearly approach the mean of the whole possible range; but upon this point I do not propose to dwell, preferring to deal with more general aspects of the problem" (p. 196).

Naturally I do propose to dwell upon it, because the paragraph contains two things: (i) a hint that destructive criticism could be raised at this point, if Mr Bateson pleased, and (ii) a hint as to the assumptions that Mr Bateson is willing to make with regard to heredity and to homotyposis. He is apparently prepared for fraternal correlation having *with equal probability* any value between ·2 and ·7. Unless there is no clustering of fraternal correlations round a definite point of the range Mr Bateson's remark about the mean of the whole possible range is purely idle. Hence we must be prepared for a very agnostic state of mind on the subject of heredity in Mr Bateson. He has no views as to what fraternal relationship is like at all; it may lie indifferently anywhere between ·2 and ·7! It seems rather late in the day for this sort of opinion, especially in one who has expressed his mind rather definitely on the importance of the statistical treatment of heredity. Here are the total series—42 cases, the majority running to 1000

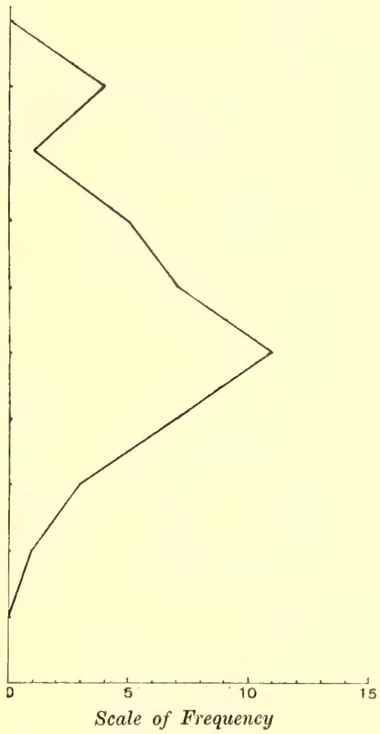
	Cases
·05—·15	0
·15—·25	1
·25—·35	4
·35—·45	6
·45—·55	20
·55—·65	7
·65—·75	4
·75—·85	0
Total	42



pairs and covering many types of life—which have to the present date been determined for fraternal correlation.

Now take the 39 cases for homotyposis which have so far been determined. Of these 22 are in the vegetable kingdom, and 17, not yet published, in the animal kingdom. We get the distribution given below. In both these distributions there is no approach to uniform scattering throughout the range of observed values. In the second we see the lump produced by the three cases of *Nigella, Malva* and *Asperula*, which were doubted before they were dealt with. The mean of the forty-two fraternal correlations is now ·495 and of the thirty-nine homotypic correlations ·499. We have still not enough material to reach a typical distribution in either case, but we have evidence more than enough to see—notwithstanding the very great difficulties of the investigation—that the coefficients tend to cluster about ·5. Mr Bateson says I "attach importance to the rather close similarity between the two average values." I attach just so much as the probable error of 39 or 42 observations admits. My own words were:

	Cases
0—1	0
1—2	4
2—3	1
3—4	5
4—5	7
5—6	11
6—7	7
7—8	3
8—9	1
9—10	0
Total	39



"Now I do not propose to lay great stress on what at first sight might look like a most conclusive equality between the mean values of homotypic and fraternal correlations....... I am quite aware that a few further series added to either the homotypic or fraternal results might modify to some extent this equality" (*Phil. Trans.* Vol. 197, A, p. 358).

My conclusion was that the homotypic factor and the fraternal correlation had both values lying between ·4 and ·5. If I incline more definitely to the higher limit to-day, it is because we have far more and better data available for both heredity and homotyposis than we had when my memoir was written in August 1900.

My sole reason in anticipating unpublished results here is to emphasise how very unadvisable such a statement as that made by Mr Bateson is, if he has not data of his own upon which he can base such an opinion as that: there is no clustering of hereditary or homotypic constants. Without a strong opinion that such is the case I presume he would not have felt bound to remark on the "suspicious circumstance." It is exactly the same rapid jumping to conclusions which I also note when Mr Bateson says that I restrict myself to undifferentiated like parts and miss the point that relationship is not lost when we pass to differentiated parts. If Mr Bateson had studied my paper he would have found fundamental theorems in *cross-homotyposis* fully considered, and possibly had he studied my other memoirs previously published he would have found more than one theorem in cross-heredity, and have known that we had worked out whole series of correlations between differentiated parts in related organisms.

In conclusion then I must state that Mr Bateson much confirms my belief in biometry. He tells us that neither differentiation between like organs nor between brothers can be biologically detected. I believe it can be detected, where it has any sensible influence, by biometric methods. He tells us further that evolution takes place by "specific," "differentiant" or "discontinuous" variation, not by normal variation, so that the statistical work of my co-workers and myself "has gone wide of its mark if that mark is the elucidation of Evolution" (p. 203). There is only one conclusion to this argument, namely, that as long as Mr Bateson refuses to apply mathematics to biology, he will not be able to discriminate this mysterious* "differentiant" variation from normal variation, and he too will not be able to elucidate Evolution. Personally I do not despair, for I see great progress in the last eight years, and it is chiefly marked by a tendency to define, so that we can be quantitatively exact and so drop *nomina quae carent rebus*.

Twenty years hence our successors, working by improved methods and with better training, will no doubt reach fitter definitions and more exact values for vital coefficients. But of one thing I am sure: Biometric methods will not then have to justify themselves to a non-mathematical biological world; mathematical knowledge will soon be as much a part of the biologist's equipment as to-day of the physicist's. Its function will not be to replace observation by symbols—all the biometric workers that I know even to-day are striving to keep in touch with nature—but to interpret observation in certain fields of biological enquiry, especially in that of Evolution, where without mathematics further progress has become impossible; impossible, for the very simple reason that we have to deal with the vital statistics of large populations, where no tabulation of individual instances can possibly lead to definite conclusions.

---

* "Subtle and evasive quality of differentiation" is Mr Bateson's term.

DATA FOR THE PROBLEM OF EVOLUTION IN MAN.

A SECOND STUDY OF THE VARIABILITY AND CORRELATION OF THE HAND.

By M. A. LEWENZ, B.A., and M. A. WHITELEY, B.Sc.

(1) A first study* of the hand was published in 1899 by Miss Whiteley and Professor Pearson who dealt with the measurements of the first joints of the fingers in 551 pairs of female hands. In that memoir they had in view a full statistical reduction of Dr Pfitzner's measurements on the hand skeleton†, proposing to compare his and their own results. Although Dr Pfitzner has only measured about 50 female hands, still the large number of bones he has dealt with and the numerous correlations to be found rendered the arithmetical work by no means light. After Miss Whiteley had progressed some way with the reduction of the metacarpal bone correlations, she was unable to continue the work, which was then taken in hand and completed by Miss Lewenz. To her accordingly most of the numerical constants given in this paper are due‡.

Although the paucity of data is a great drawback, it yet seemed better in the first place to reduce the short series of female instead of the longer series of male hands, because only this sex had been dealt with in the first study. We hope eventually to obtain measurements on the male hand and compare them with Dr Pfitzner's male series.

In every case we give the probable error of our determinations which are naturally large. In all the workings grouping has been avoided and standard deviations and correlations found by taking actual sums of the squares and of the products of deviations. In the statement of general results we have, bearing in mind the largeness of the probable errors, endeavoured to express only conclusions which result from an examination of several series and not from single instances.

(2) We may divide our discussion into four parts: (i) the comparison of the absolute lengths and variabilities of different bones in both hands; (ii) the

---

* *R. S. Proc.* Vol. 65, pp. 126—151.

† *Schwalbes Morphologische Arbeiten*, Bd. I. pp. 21—35, and Bd. II. pp. 99—106.

‡ I am responsible for editing and to some extent rearranging Miss Lewenz's material. K. P.

correlation of the same bones in different digits of the same hand; (iii) the correlation of different bones of the same digit; and (iv) the correlation of the same bones of the same digits in right and left hands. To have correlated every bone of every digit of both hands would have been an almost insuperable task, and further would not have led to any very important results. What we had in view was the threefold problem:—to what extent do the parts of the same digit fit each other, to what extent do the digits of the hand fit each other, and to what extent do the two hands fit each other.

The remarkably high degrees of correlation between the parts of the hand and of one hand with the other indicated by our first study are amply verified when the individual parts of the skeleton are dealt with. The hand is a most highly correlated mechanism, and given one long bone of one digit, the range of variation occurring in any other long bones of the same or the other hand is wonderfully small. It is hard to hold any other view than that this degree of fitting is the result of selection for physical use. It is striking to compare the high correlations obtained for the parts of both English and German female hands with the correlations which we find for bones of the other chief organ of man's supremacy, the head. Skill in the use of the head and hand has been man's chief source of success, but while the instrument of physical superiority is a highly correlated mechanism, the seat of mental superiority, the skull, is probably the least correlated portion of the human body! Of course, if we could in any way subject to measurement and correlate the soft parts of the head, the organs of sense, and in particular the folds and commissures of the brain we might find high degrees of relationship. We should expect sensory and mental fitness to depend upon high degrees of correlation between the parts of the sensory and mental organs. But it still remains a noteworthy fact that the bony parts of the skull are on the average not correlated with even a third of the correlation of the parts of the hand, and this fact alone seems to account for the small apparent relationship between intellectual ability and measurements on the head. We should expect to find the parts of the organism on which intellectual efficiency depends highly correlated like the parts of the hand on which physical efficiency depends; the absence of high correlation in the parts of the skull suggests that it is not chiefly upon its case that brain efficiency turns.

(3) Considering first the absolute size and variability of the parts of the hand we shall adopt the following notation: R = right hand, L = left hand, m = metacarpal bone, p = first or proximal phalanx, s = second or middle phalanx, d = third or distal phalanx, the subscripts 1, 2, 3, 4, 5 will refer to the thumb, index, middle, ring and little finger respectively. Thus: Rd_1, Lp_4... would refer to the distal phalanx of the thumb of the right hand, and the proximal phalanx of the ring finger of the left hand, and so on.

In Table I. are given the means, standard deviations and coefficients of variation of the bones of both hands. In each case the number on which the constants are based is stated.

TABLE I.

	Mean		Standard Deviation		Coefficient of Variation	
Bone	*R*	*L*	*R*	*L*	*R*	*L*
(45) (45)						
m_1	41·27±·28	40·91±·25	2·80±·20	2·53±·18	6·78±·48	6·18±·44
m_2	62·20±·36	61·56±·34	3·55±·25	3·40±·24	5·71±·41	5·52±·39
m_3	59·64±·33	59·33±·31	3·28±·23	3·04±·22	5·50±·39	5·12±·36
m_4	53·80±·30	53·36±·30	2·96±·21	2·96±·21	5·49±·39	5·55±·39
m_5	50·07±·29	49·47±·28	2·87±·20	2·83±·20	5·73±·41	5·71±·41
(46) (47)						
p_1	27·52±·18	27·40±·19	1·79±·13	1·94±·13	6·51±·46	7·09±·49
p_2	36·96±·21	36·79±·21	2·15±·15	2·15±·15	5·81±·41	5·85±·41
p_3	41·04±·22	40·91±·21	2·22±·16	2·14±·15	5·42±·38	5·24±·36
p_4	38·76±·23	38·38±·21	2·34±·16	2·17±·15	6·04±·42	5·65±·39
p_5	30·59±·19	30·26±·18	1·92±·13	1·86±·13	6·27±·44	6·15±·43
(46) (47)						
s_2	22·22±·15	22·21±·15	1·55±·11	1·55±·11	6·95±·49	7·01±·49
s_3	26·96±·18	26·87±·18	1·82±·13	1·84±·13	6·74±·48	6·85±·48
s_4	25·59±·18	25·49±·18	1·82±·13	1·86±·13	7·13±·50	7·27±·51
s_5	18·13±·16	17·98±·14	1·62±·11	1·45±·10	8·95±·63	8·08±·57
(43) (44)						
d_1	20·56±·12	20·39±·12	1·17±·08	1·21±·09	5·68±·41	5·93±·43
d_2	15·98±·11	16·09±·10	1·06±·08	0·95±·07	6·68±·49	5·90±·43
d_3	16·65±·13	16·77±·12	1·27±·09	1·15±·08	7·65±·56	6·83±·49
d_4	17·16±·12	17·20±·12	1·20±·09	1·22±·09	6·99±·51	7·08±·51
d_5	15·51±·11	15·64±·11	1·11±·08	1·09±·08	7·14±·52	6·96±·50
(44) (44)						
D_1	47·86±·32	47·78±·31	2·84±·22	2·75±·22	5·93±·47	5·76±·45
D_2	75·16±·41	75·09±·43	3·68±·29	3·90±·31	4·90±·38	5·19±·41
D_3	84·68±·50	84·49±·49	4·49±·35	4·30±·34	5·30±·42	5·07±·40
D_4	81·46±·49	81·19±·51	4·44±·35	4·57±·36	5·45±·43	5·63±·44
D_5	64·19±·43	63·89±·39	3·92±·31	3·49±·27	6·10±·48	5·46±·43

Now looking at the column headed "mean" we see that for the metacarpal bones, the proximal phalanges and the middle phalanges the right hand is larger than the left. And in the same manner the total thumb (D_1) and finger lengths of bone (D_2, D_3, D_4, D_5) are larger on the right than the left. If we take the distal phalanges, then it would appear that the left was larger for all the fingers and the right for the thumb only. But if we take the 37 cases in which we have bones from both hands of the *same* individual, the means work out:

Rd_1	Ld_1	Rd_2	Ld_2	Rd_3	Ld_3	Rd_4	Ld_4	Rd_5	Ld_5
20·62	20·41	15·97	16·03	16·68	16·68	17·16	17·14	15·51	15·57

or there is no sensible difference between the distal phalanges of right and left hand except in the case of the thumb. Applying the same test—means for pairs of corresponding bones on right and left hands of same individual—we find that in the 19 cases dealt with the right is larger in 13 cases, in a 14th case the means are equal; in 5 cases only is the left larger. In not one of these 5 cases is the difference of the means more than a small fraction of the probable error of the difference, while in 9 out of the 13 cases in which the right-hand bone is larger the difference is as large as the probable error. We must therefore conclude that:

Judged by the skeleton the right hand is larger than the left, but the preponderance of the right hand decreases as we pass downwards from the metacarpal bones to the distal phalanges, where it vanishes. This is in accordance with our result for finger joints*, and in disagreement with Dr Pfitzner's conclusions from the same data; he considers that there is no quantitative difference between right and left for the simple anatomical parts of the hand skeleton.

Turning to absolute variability, we find the standard deviation larger in 11 cases for the right hand, 5 for the left and in 3 exactly equal from the above Table. Taking only cases in which we have bones from the two hands of the same individual we have 7 cases of right preponderance, 9 of left, and 3 of equality.

Lastly, considering relative variability we have from the above Table 11 cases in which the right hand has a greater coefficient of variation and 8 cases in which the left preponderates. Dealing with the smaller number of cases in which we have pairs from both hands, we find the order inverted and 9 cases for the right and 10 for the left.

The average coefficient of variation for pairs from both hands is 6·14 for the right and 6·06 for the left. It would be unwise to base any statement as to the relative variability of the hands on such a slight difference as this, considering that we are dealing with only 37 to 40 pairs of hands. In the finger measurements the left hand was found slightly more variable than the right, but it was remarked: that "the divergence is not one on which real stress can be laid considering the probable error of the coefficient of variation†."

We can therefore merely state that we find no sensible difference in variability for the two hands.

Turning now to relative variability (coefficients of variation) of the respective fingers, we note that in the case of the first joints of the fingers on the living subject the order of variability for the *left hand* was:

(i) Little Finger. (ii) Ring Finger. (iii) Index Finger. (iv) Middle Finger.

If we take the average variability of all the phalanges we have for our present data:

---

* *R. S. Proc.* Vol. 65, p. 129.

† *Loc. cit.* footnote, p. 129.

Left Hand	Little Finger	6·75
	Thumb	6·40
	Ring Finger	6·28
	Index Finger	6·07
	Middle Finger	6·01

This order is therefore, with the introduction of the thumb, identical with the result obtained from English ♀ hands.

If we turn to the right hand, the order for the finger joints was:

(i) Little Finger. (ii) Ring Finger. (iii) Middle Finger. (iv) Index Finger.

This order has been considerably modified in our values for the German measurements:

Right Hand	Little Finger	7·02
	Index Finger	6·79
	Middle Finger	6·33
	Thumb	6·32
	Ring Finger	6·26

the last three coefficients not being sensibly different from each other.

Looking at the probable errors of our results, it is hardly possible to assert more than that the most variable finger is the little finger.

If we take the total length of the fingers we have the following results:

Right Hand		*Left Hand*	
Little Finger	6·10 ± ·48	Thumb	5·76 ± ·45
Thumb	5·93 ± ·47	Ring Finger	5·63 ± ·44
Ring Finger	5·45 ± ·43	Little Finger	5·46 ± ·43
Middle Finger	5·30 ± ·42	Index Finger	5·19 ± ·41
Index Finger	4·90 ± ·38	Middle Finger	5·08 ± ·40

These agree in making index and middle fingers least, thumb, little and ring fingers most variable; results, which are in accordance with the finger-joint determinations*.

Hence, without pressing to finer shades of difference which are not warranted owing to our paucity of data, we may conclude that (*a*) the index and middle fingers are the least variable, (*b*) the little finger and the thumb the most variable, and (*c*) the ring finger intermediate between these two groups.

The middle and index fingers would appear to be the most useful, the ring and little fingers the least useful. It is therefore somewhat surprising to find the thumb grouping itself with the latter; we must, however, bear in mind that looking at mammals generally the thumb exhibits greater differences than any other digit.

---

* *Loc. cit.* p. 129.

If instead of considering the individual digits we consider mean variability of the individual bones we find:

Right Hand		*Left Hand*	
Middle Phalanx	7·15	Middle Phalanx	7·24
Distal Phalanx	6·83	Distal Phalanx	6·54
Proximal Phalanx	6·01	Proximal Phalanx	6·00
Metacarpal	5·84	Metacarpal	5·62

The order is thus the same for both hands and the agreement between the two hands in each case fairly good. This may accordingly be taken to be the natural order of *relative* variability in the bones—the two larger bones being relatively less variable than the two smaller, but of the two smaller the lesser is the less variable.

(4) We now pass to the subject of correlation, and will consider first the correlations of the metacarpal bones. We have the following results:

TABLE II.

Metacarpal Bones of Right Hand.

45 Cases	Rm_1	Rm_2	Rm_3	Rm_4	Rm_5
Rm_1	1	·813 ± ·034	·816 ± ·034	·731 ± ·047	·658 ± ·057
Rm_2	·813 ± ·034	1	·943 ± ·012	·914 ± ·017	·858 ± ·027
Rm_3	·816 ± ·034	·943 ± ·012	1	·946 ± ·011	·887 ± ·021
Rm_4	·731 ± ·047	·914 ± ·017	·946 ± ·011	1	·929 ± ·014
Rm_5	·658 ± ·057	·858 ± ·027	·887 ± ·021	·929 ± ·014	1

TABLE III.

Metacarpal Bones of Left Hand.

45 Cases	Lm_1	Lm_2	Lm_3	Lm_4	Lm_5
Lm_1	1	·785 ± ·039	·791 ± ·035	·705 ± ·051	·665 ± ·056
Lm_2	·785 ± ·039	1	·936 ± ·013	·907 ± ·018	·888 ± ·021
Lm_3	·791 ± ·035	·936 ± ·013	1	·928 ± ·014	·877 ± ·023
Lm_4	·705 ± ·051	·907 ± ·018	·928 ± ·014	1	·947 ± ·010
Lm_5	·665 ± ·056	·888 ± ·021	·877 ± ·023	·947 ± ·010	1

Now it will be seen at once that these are high correlations and we may deduce the following results:

(*a*) Of the 10 coefficients of correlation of the metacarpal bones 7 are greater for those of the right hand than for those of the corresponding pair in the left. In only 3 does the left predominate. The differences are, however, in each case within the probable errors of the differences. It is therefore hardly possible to assert definitely that the metacarpal bones of the right are more closely correlated than those of the left hand.

(*b*) A metacarpal bone has always more correlation with a second of the same series than with any other more distant metacarpal bone*.

The only exception to this rule is the correlation of the left-hand metacarpal bone of the little finger with those of the middle and index fingers, but in this case the difference of these correlations is well within the probable error of the difference, i.e. it is not significant.

The manner in which the correlation of digits arranges itself according to situation is a striking demonstration of how it is truly organic in character, and how impossible it is to treat such organs as "homotypes†."

We next turn to the proximal phalanges and we have the following results:

TABLE IV.

Proximal Phalanx. Right Hand.

46 Cases	Rp_1	Rp_2	Rp_3	Rp_4	Rp_5
Rp_1	1	·837 ± ·030	·818 ± ·033	·803 ± ·045	·854 ± ·027
Rp_2	·837 ± ·030	1	·937 ± ·012	·893 ± ·020	·894 ± ·021
Rp_3	·818 ± ·033	·937 ± ·012	1	·949 ± ·010	·916 ± ·016
Rp_4	·803 ± ·045	·893 ± ·020	·948 ± ·010	1	·917 ± ·016
Rp_5	·854 ± ·027	·895 ± ·021	·916 ± ·016	·917 ± ·016	1

TABLE V.

Proximal Phalanx. Left Hand.

47 Cases	Lp_1	Lp_2	Lp_3	Lp_4	Lp_5
Lp_1	1	·871 ± ·024	·864 ± ·025	·823 ± ·032	·777 ± ·039
Lp_2	·871 ± ·024	1	·910 ± ·017	·879 ± ·023	·857 ± ·026
Lp_3	·864 ± ·025	·910 ± ·017	1	·927 ± ·014	·859 ± ·026
Lp_4	·823 ± ·032	·879 ± ·023	·927 ± ·014	1	·908 ± ·017
Lp_5	·777 ± ·039	·857 ± ·026	·859 ± ·026	·908 ± ·017	1

Here the correlations are, if anything, somewhat higher on the average than in the case of the metacarpal bones.

---

* Whiteley and Pearson: *loc. cit.* p. 131.

† Bateson: *R. S. Proc.* Vol. 69, p. 199; *Biometrika*, Vol. I. pp. 341, 342.

(*a*) In seven cases out of ten the right hand is more highly correlated than the left, but the differences are again not markedly significant.

(*b*) A proximal phalanx has always more correlation with a second of the same series than with any other from which it is separated by the second.

There is no exception to this rule in the left hand. There are non-significant exceptions in the case of the correlation of the right-hand index finger proximal phalanx with those of the ring and index fingers. There is an apparently significant exception in the case of the right-hand thumb and little finger great phalanges. But this is not confirmed in the case of the left hand. The little finger does, however, show a tendency to disregard the "rule of neighbourhood" and we hope to return to this point when dealing with the male hand.

Taken as a whole, however, the principle (*b*) above enunciated for metacarpal bones and proximal phalanges may be considered as well verified. This is in good agreement with the result obtained for the first joints of the hands which consist of the proximal phalanx, the head of the metacarpal bone, together with certain soft parts*. We shall see that it is less generally true for the middle and distal phalanges.

The middle phalanges give the following correlations:

TABLE VI.

Middle Phalanx. Right Hand.

46 Cases	Rs_2	Rs_3	Rs_4	Rs_5
Rs_2	1	·900 ± ·019	·910 ± ·017	·760 ± ·042
Rs_3	·900 ± ·019	1	·937 ± ·012	·754 ± ·043
Rs_4	·910 ± ·017	·937 ± ·012	1	·840 ± ·029
Rs_5	·760 ± ·042	·754 ± ·043	·840 ± ·029	1

TABLE VII.

Middle Phalanx. Left Hand.

47 Cases	Ls_2	Ls_3	Ls_4	Ls_5
Ls_2	1	·924 ± ·014	·930 ± ·013	·823 ± ·032
Ls_3	·924 ± ·014	1	·965 ± ·007	·764 ± ·041
Ls_4	·930 ± ·013	·965 ± ·007	1	·857 ± ·026
Ls_5	·823 ± ·032	·764 ± ·041	·857 ± ·026	1

The correlations here are on the whole less than for the larger bones, but are still very high. In every case the left-hand middle phalanges are more highly correlated than the right-hand corresponding pairs.

---

* Whiteley and Pearson: *loc. cit.* p. 131.

We see again that the marginal fingers as a rule have least correlation. There are three insignificant deviations from the rule that a bone has always more correlation with a second of the same series than with any other bone from which it is separated by the second. There is one significant deviation, e.g. the correlations of the little finger middle phalanx with those of the index and middle fingers of the left hand.

Lastly, turning to the distal phalanx we have the following Tables:

TABLE VIII.

Distal Phalanx. Right Hand.

43 Cases	Rd_1	Rd_2	Rd_3	Rd_4	Rd_5
Rd_1	1	·605 ± ·065	·631 ± ·062	·733 ± ·048	·678 ± ·055
Rd_2	·605 ± ·065	1	·797 ± ·038	·786 ± ·039	·797 ± ·038
Rd_3	·631 ± ·062	·797 ± ·038	1	·861 ± ·027	·770 ± ·042
Rd_4	·733 ± ·048	·786 ± ·039	·861 ± ·027	1	·831 ± ·032
Rd_5	·678 ± ·055	·797 ± ·038	·770 ± ·042	·831 ± ·032	1

TABLE IX.

Distal Phalanx. Left Hand.

44 Cases	Ld_1	Ld_2	Ld_3	Ld_4	Ld_5
Ld_1	1	·698 ± ·052	·739 ± ·046	·750 ± ·045	·587 ± ·067
Ld_2	·698 ± ·052	1	·859 ± ·027	·815 ± ·034	·798 ± ·037
Ld_3	·739 ± ·046	·859 ± ·027	1	·917 ± ·016	·753 ± ·049
Ld_4	·750 ± ·045	·815 ± ·034	·917 ± ·016	1	·795 ± ·037
Ld_5	·587 ± ·067	·798 ± ·037	·753 ± ·049	·795 ± ·037	1

Here again the correlations have still further fallen; there is no balance of correlation significantly in favour of either hand. The marginal digits are again the least correlated. There are further interesting and significant deviations from the rule as to the relationship of correlation to neighbourhood. The distal phalanx of the thumb in *both* hands is most highly correlated with the distal phalanx of the ring finger and the correlation drops in either direction from this. The little finger distal phalanx is more highly correlated in both hands with that of the index finger than with that of the closer middle finger. It would thus appear that special relations hold for the marginal digits at least in regard to the distal phalanges, if not also for the middle phalanges.

We may add to these results for individual bones similar results for the whole bone length of the four fingers.

TABLE X.

Bone Length of Fingers. Right Hand.

44 Cases	R_2	R_3	R_4	R_5
R_2	1	·962 ± ·008	·949 ± ·010	·897 ± ·020
R_3	·962 ± ·008	1	·960 ± ·008	·920 ± ·016
R_4	·949 ± ·010	·960 ± ·008	1	·933 ± ·013
R_5	·897 ± ·020	·920 ± ·016	·933 ± ·013	1

TABLE XI.

Bone Length of Fingers. Left Hand.

44 Cases	L_2	L_3	L_4	L_5
L_2	1	·952 ± ·009	·943 ± ·011	·900 ± ·019
L_3	·952 ± ·009	1	·948 ± ·010	·889 ± ·021
L_4	·943 ± ·011	·948 ± ·010	1	·893 ± ·021
L_5	·900 ± ·019	·889 ± ·021	·893 ± ·021	1

These results bring out strongly:

(*a*) the high degree of correlation between the whole fingers, if anything slightly greater for the right than the left hand*;

(*b*) the rule that a finger has always more correlation with a second than with any finger from which it is separated by the second.

There is here no exception whatever to this rule; and

(*c*) the marginal fingers have the least correlation and the little finger always less than the index.

(5) Let us now consider the correlation of bones of the same digit, i.e. discuss longitudinal and not lateral relationship. In the first place we take the thumb.

TABLE XII.

Bones of the Thumb. Right Hand.

43 Cases	Rm_1	Rp_1	Rd_1
Rm_1	1	·821 ± ·034	·552 ± ·072
Rp_1	·821 ± ·034	1	·581 ± ·068
Rd_1	·552 ± ·072	·581 ± ·068	1

---

* This is not in accordance with the result from finger-joint measurements (*R. S. Proc.* Vol. 65, p. 131), but the advantage of the right is really *very* slight.

TABLE XIII.

Bones of the Thumb. Left Hand.

45 Cases	Lm_1	Lp_1	Ld_1
Lm_1	1	·825 ± ·032	·528 ± ·073
Lp_1	·825 ± ·032	1	·538 ± ·072
Ld_1	·528 ± ·073	·538 ± ·072	1

Here the values are less intense than is the rule in the case of the lateral relationship. Further there is no significant difference between right and left-hand thumbs. Lastly, each bone is more nearly correlated with its immediate neighbour, than with the one from which that neighbour separates it.

Let us see how far these results hold for the other digits.

TABLE XIV.

Bones of the Index Finger. Right Hand.

46 Cases	Rm_2	Rp_2	Rs_2	Rd_2
Rm_2	1	·837 ± ·030	·798 ± ·036	·534 ± ·071
Rp_2	·837 ± ·030	1	·834 ± ·030	·489 ± ·076
Rs_2	·798 ± ·036	·834 ± ·030	1	·516 ± ·073
Rd_2	·534 ± ·074	·489 ± ·076	·516 ± ·073	1

TABLE XV.

Bones of the Index Finger. Left Hand.

46 Cases	Lm_2	Lp_2	Ls_2	Ld_2
Lm_2	1	·797 ± ·036	·691 ± ·052	·518 ± ·073
Lp_2	·797 ± ·036	1	·862 ± ·026	·504 ± ·075
Ls_2	·691 ± ·052	·862 ± ·026	1	·481 ± ·077
Ld_2	·518 ± ·073	·504 ± ·075	·481 ± ·077	1

Here the values are again less intense than for the lateral relationship. There is possibly slightly more correlation in the right than in the left hand. Each bone is more highly correlated with a second than with a bone from which the second separates it; this rule, however, is broken through in the case of the distal phalanx, which in both index fingers is most closely correlated with the bone furthest removed, i.e. the metacarpal bone.

TABLE XVI.

Bones of the Middle Finger. Right Hand.

46 Cases	Rm_3	Rp_3	Rs_3	Rd_3
Rm_3	1	·810 ± ·034	·743 ± ·039	·701 ± ·051
Rp_3	·810 ± ·034	1	·900 ± ·019	·693 ± ·052
Rs_3	·743 ± ·045	·900 ± ·019	1	·680 ± ·054
Rd_3	·701 ± ·051	·693 ± ·052	·680 ± ·054	1

TABLE XVII.

Bones of the Middle Finger. Left Hand.

45 Cases	Lm_3	Lp_3	Ls_3	Ld_3
Lm_3	1	·797 ± ·037	·680 ± ·054	·686 ± ·053
Lp_3	·797 ± ·037	1	·836 ± ·034	·571 ± ·068
Ls_3	·680 ± ·054	·836 ± ·034	1	·602 ± ·064
Ld_3	·686 ± ·053	·571 ± ·068	·602 ± ·064	1

Here there is in every case a right-hand preponderance. The correlations are also greater than for the thumb or index finger bones. Lastly, the rule as to neighbourhood is again significantly broken for both hands in the case of the distal phalanx which is in both cases most highly correlated with the corresponding metacarpal bone.

TABLE XVIII.

Bones of the Ring Finger. Right Hand.

45 Cases	Rm_4	Rp_4	Rs_4	Rd_4
Rm_4	1	·806 ± ·035	·799 ± ·036	·697 ± ·052
Rp_4	·806 ± ·035	1	·899 ± ·019	·626 ± ·061
Rs_4	·799 ± ·036	·899 ± ·019	1	·667 ± ·056
Rd_4	·697 ± ·052	·626 ± ·061	·667 ± ·056	1

TABLE XIX.

Bones of the Ring Finger. Left Hand.

45 Cases	Lm_4	Lp_4	Ls_4	Ld_4
Lm_4	1	·792 ± ·038	·732 ± ·047	·583 ± ·066
Lp_4	·792 ± ·038	1	·844 ± ·029	·625 ± ·061
Ls_4	·732 ± ·047	·844 ± ·029	1	·596 ± ·065
Ld_4	·583 ± ·066	·625 ± ·061	·596 ± ·065	1

The correlations are here less for the left than for the right-hand ring-finger bones. The law of neighbourhood is again broken for the distal phalanges of both right and left fingers, the metacarpal bone in the first case and the proximal phalanx in the second being the most closely correlated bones.

TABLE XX.

Bones of the Little Finger. Right Hand.

45 Cases	Rm_5	Rp_5	Rs_5	Rd_5
Rm_5	1	·813 ± ·034	·633 ± ·060	·513 ± ·074
Rp_5	·813 ± ·034	1	·810 ± ·035	·638 ± ·060
Rs_5	·633 ± ·060	·810 ± ·035	1	·433 ± ·082
Rd_5	·513 ± ·074	·638 ± ·060	·433 ± ·082	1

TABLE XXI.

Bones of the Little Finger. Left Hand.

44 Cases	Lm_5	Lp_5	Ls_5	Ld_5
Lm_5	1	·805 ± ·036	·574 ± ·068	·454 ± ·081
Lp_5	·805 ± ·036	1	·685 ± ·054	·462 ± ·080
Ls_5	·574 ± ·068	·685 ± ·054	1	·361 ± ·089
Ld_5	·454 ± ·081	·462 ± ·080	·361 ± ·089	1

Here the correlations have fallen again considerably, but the right hand is still more highly correlated than the left. The rule of neighbourhood is again broken in the case of the distal phalanges, which for both right and left little fingers are most closely correlated with the corresponding proximal phalanges.

To sum up our results for longitudinal relationship of the bones of the hand, we conclude that:

(*a*) The right hand exhibits somewhat more correlation than the left.

(*b*) The thumb, index and little finger, the "marginal digits," have less correlation than the middle and ring fingers, the "central digits."

(*c*) The proximal phalanx exhibits more correlation with the other longitudinal bones than the other phalanges or the metacarpal bones. The distal phalanx exhibits least correlation,—the metacarpal bone and the middle phalanx having about equal correlation and standing in this respect between the proximal and distal phalanges.

(6) Our last series of correlations will be between the corresponding bones of both hands.

We may arrange these in a single table as follows, adding the results obtained by Whiteley and Pearson for the first joint of the living finger:

TABLE XXII.

Pairs of Corresponding Bones in two Hands.

Bone	Number	Thumb	Index	Middle	Ring	Little
Metacarpal	39	·974 ± ·006	·990 ± ·002	·985 ± ·003	·946 ± ·011	·955 ± ·010
Proximal Phalanx	41	·944 ± ·008	·938 ± ·013	·952 ± ·010	·948 ± ·011	·934 ± ·014
Middle Phalanx	41	—	·882 ± ·023	·908 ± ·019	·959 ± ·009	·874 ± ·025
Distal Phalanx	37	·796 ± ·041	·793 ± ·041	·852 ± ·030	·899 ± ·021	·863 ± ·028
Total Bone Length	44	·945 ± ·012	·975 ± ·006	·971 ± ·006	·976 ± ·005	·960 ± ·009
First Joint	551	—	·925 ± ·004	·934 ± ·004	·929 ± ·004	·904 ± ·005

It would thus appear that the metacarpal bones and the proximal phalanges of the two hands are more highly correlated than the first joints of the two hands in the case of the thumb and all the fingers. Further the correlation seems to decrease for each finger as we pass down from metacarpal to distal phalanx. Lastly, the middle and ring fingers of the two hands are on the whole more closely correlated than the thumb, index and little fingers. Or, the principle that the "marginal digits" exhibit less correlation than the "central digits" remains true, if instead of correlating different bones of the same digit of the same hand, we correlate the same bones of the same digits of different hands. It will be noted that the correlations of right and left metacarpal bones are as high as, if not higher than, the values which have been obtained for the right and left long bones of the human skeleton[*].

(7) *Concluding Remarks.*

In drawing general conclusions we must at once warn the reader to notice again the size of our probable errors. We look upon the present study as one of suggestion rather than of definite statistical proof. Until we have at least 250 to 500 pairs of hand skeletons measured we cannot draw absolutely definite conclusions. We shall consider our arithmetical labours, great as they have been, amply repaid, if they lead to further bone measurements, so that the excellent work of

---

[*] Warren: *Phil. Trans.* Vol. 189, B, p. 178. Whiteley and Pearson: *R. S. Proc.* Vol. 65, p. 132.

Dr Pfitzner may be extended on wider material and for other races*. But there is no doubt that the hand is a most interesting study, and the results already reached serve to indicate a variety of new problems to be studied in other digits than those of man, problems which will, if answered, help to throw light not only on the sources of efficiency in such organs, but probably also on the nature of their growth and evolutionary development.

In the first place we see that local relationship influences the variability and the correlation of the hand bones. There is a correlation between the part of the organism at which the homologous part is produced and its characters and relationships to other parts. In other words the relation of digits is organic and not homotypic.

(i) Considering first size, we note that the bones of the right hand appear to be on the whole larger than those of the left. In this respect we have agreement with Dr Warren's results for measurement of the humerus, radius and ulna which are larger on the right side, while the leg bones, femur, tibia and fibula are less on the right†. It would be interesting to know whether in this the bones of the foot resemble those of the hand or the other bones of the leg.

(ii) There is no significant difference in either absolute or relative variabilities between right and left-hand bones. This agrees with Dr Warren's results for the long bones of the skeleton‡.

(iii) There is a slight, but we cannot say definitely significant, preponderance in the correlations of the right-hand bones over those of the left.

(iv) The highest correlations occur between corresponding bones of the right and left hands. These are as high as any right and left-hand relations between parts of the human skeleton yet investigated.

(v) The next highest correlations are between lateral and not between longitudinal neighbours. Each bone being on the average more nearly related to the corresponding bone on the next digit, than to the adjacent bone on the same digit.

(vi) Dividing the hand into marginal members, i.e. thumb, index and little fingers, and central members, i.e. middle and ring fingers, and the bones into "lower bones," i.e. distal and middle phalanges, and "upper bones," i.e. metacarpal bones and proximal phalanges, the correlations roughly speaking are highest for the upper bones of the central members and become less as we move out from this upper centre towards the lower and marginal parts of the hand. This is true whether we take pairs in lateral or in longitudinal series.

---

* It is almost impossible to overestimate the importance of the work done at Strassburg at Professor Schwalbe's initiative. The raw material already published is of the highest value. Unfortunately the statistical methods adopted are occasionally inadequate and some of the conclusions reached demand, even if true, far more elaborate statistical demonstration.

† *Phil. Trans.* Vol. 189, B, pp. 146, 157, 162, 165 and 169.

‡ *Ibid.* p. 190.

(vii) Generally there is a "rule of neighbourhood," i.e. any bone is more closely correlated with a second of the same series than with any other from which it is separated by that second. Speaking roundly this is true for both lateral and longitudinal series; but there are apparently significant deviations from this rule, the most notable of which are, perhaps, those of the distal phalanges which on all the fingers of both hands tend to be more highly correlated with the metacarpal bones or the proximal phalanges than with the middle phalanges. The middle phalanges, however, obey the general rule.

(viii) The lower bones of the marginal members tend on the whole to be most variable; thus the thumb and little finger are the most variable members and the middle and distal phalanges the most variable bones. The middle phalanx is, however, more variable than the distal, and there are individual exceptions to the rule noted in the body of this paper.

On the whole our sparse data seem to indicate a regular and continuous distribution of both variation and correlation following local position for the bones of the hand, and one is strongly impelled to believe that a knowledge of this systematic distribution obtained from adequate data would reveal much that is yet obscure to us in interdigital relationship, and in the nature of the transition from homotypic to organic correlations.

ON THE INHERITANCE IN COAT-COLOUR OF THOROUGHBRED HORSES (GRANDSIRE AND GRANDCHILDREN).

By N. BLANCHARD, B.A., Caius College, Cambridge.

Dr E. Warren's recent paper on inheritance in *Aphis* incidentally draws attention* to the need for determining further correlations between grandparent and offspring. At the suggestion of Professor Karl Pearson I have worked out two further cases for the inheritance of coat-colour in thoroughbred horses. Using his index of coat-colours for the chief sires I extracted from Weatherby's Studbooks the coat-colours of 1000 colts and their paternal grandsires, and of 1000 fillies and their paternal grandsires. The correlation Tables I. and II. were then formed in the manner described in Pearson and Bramley-Moore's memoir† on inheritance of coat-colour in thoroughbred race-horses. These tables were then reduced to the fourfold division :

Paternal Grandsires

		Bay and darker	Chestnut and lighter	Totals
Colts	Bay and darker ...	494	213	707
	Chestnut and lighter	143	150	293
	Totals	637	363	1000

Paternal Grandsires

		Bay and darker	Chestnut and lighter	Totals
Fillies	Bay and darker ...	485	237	722
	Chestnut and lighter	119	159	278
	Totals	604	396	1000

---

* *Biometrika*, Vol. I. p. 129.

† *Phil. Trans.* Vol. 195, A, pp. 92 *et seq.*

Using the notation of Professor Pearson's memoir on the correlation of characters not quantitatively measurable*, we find in the first case:

$$h = {\cdot}35046, \quad H = {\cdot}37518,$$

$$k = {\cdot}54466, \quad K = {\cdot}34395,$$

and the equation:

$$\cdot 33819 = r + {\cdot}09544\, r^2 + {\cdot}10283\, r^3 + {\cdot}06186\, r^4 + {\cdot}02483\, r^5 + {\cdot}04403\, r^6 + {\cdot}00566\, r^7 + {\cdot}03311\, r^8,$$

the root of which is $r = {\cdot}3238$.

In the second case we have

$$h = {\cdot}26470, \quad H = {\cdot}38521,$$

$$k = {\cdot}58881, \quad K = {\cdot}33367,$$

and the equation:

$$\cdot 38054 = r + {\cdot}11203\, r^2 + {\cdot}09312\, r^3 + {\cdot}02836\, r^4 + {\cdot}02367\, r^5 + {\cdot}02651\, r^6 + {\cdot}00290\, r^7 + {\cdot}02230\, r^8,$$

the root of which is $r = {\cdot}3609$.

TABLE I.

Colts and Paternal Grandsires.

Grandsires

			1	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	Totals
			bl.	bl./br.	br./bl.	br.	br./b.	b./br.	b.	b./ch.	ch./b.	ch.	ch./ro.	ro./ch.	ro.	ro./gr.	gr./ro.	gr.	
Colts	*1*	bl.	—	—	—	5	—	—	12	—	—	7	—	—	—	—	—	—	24
	2	bl./br.	—	—	—	1	—	—	4	—	—	6	—	—	—	—	—	—	11
	3	br./bl.	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	1
	4	br.	—	—	—	28	—	—	75	—	—	43	—	—	—	—	—	—	146
	5	br./b.	—	—	—	—	—	1	2	—	—	2	—	—	—	—	—	—	5
	6	b./br.	—	—	—	5	—	—	27	—	—	18	—	—	—	—	—	—	50
	7	b.	—	—	—	49	—	—	284	—	—	136	1	—	—	—	—	—	470
	8	b./ch.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	9	ch./b.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	10	ch.	—	—	—	25	—	—	117	—	—	149	1	—	—	—	—	—	292
	11	ch./ro.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	12	ro./ch.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	13	ro.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	14	ro./gr.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	15	gr./ro.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	16	gr.	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	1
Totals			—	—	—	114	—	1	522	—	—	361	2	—	—	—	—	—	1000

---

* *Phil. Trans.* Vol. 195, A, pp. 1—47.

TABLE II.

Fillies and Paternal Grandsires.

Grandsires

Fillies		*1* bl.	*2* bl./br.	*3* br./bl.	*4* br.	*5* br./b.	*6* b./br.	*7* b.	*8* b./ch.	*9* ch./b.	*10* ch.	*11* ch./ro.	*12* ro./ch.	*13* ro.	*14* ro./gr.	*15* gr./ro.	*16* gr.	Totals
1	bl.	—	—	—	1	—	—	9	—	—	7	—	—	—	—	—	—	17
2	bl./br.	—	—	—	2	—	—	2	—	—	7	—	—	—	—	—	—	11
3	br./bl.	—	—	—	—	—	—	1	—	—	1	—	—	—	—	—	—	2
4	br.	—	—	—	15	—	—	72	—	—	36	—	—	—	—	—	—	123
5	br./b.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6	b./br.	—	—	—	9	—	—	26	—	—	21	—	—	—	—	—	—	56
7	b.	1	—	—	56	—	—	291	—	—	165	—	—	—	—	—	—	513
8	b./ch.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
9	ch./b.	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	1
10	ch.	—	—	—	16	—	—	100	—	—	155	1	—	—	—	—	—	272
11	ch./ro.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
12	ro./ch.	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	1
13	ro.	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	1
14	ro./gr.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
15	gr./ro.	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	1
16	gr.	—	—	—	—	—	—	1	—	—	—	1	—	—	—	—	—	2
Totals		1	—	—	99	—	—	504	—	—	394	2	—	—	—	—	—	1000

Putting these two results together with Pearson and Bramley-Moore's* for the maternal grandsires and adding other known grandparental series, we have :

	Thoroughbred Horse. Coat-colour	Man.† Eye-colour	Basset Hound.‡ Coat-colour	Daphnia §	Aphis ‖
Mat. Grandfather and Son ...	·3590	·3717	– ·0032	—	—
Mat. Grandfather and Daughter	·3116	·2969		—	—
Pat. Grandfather and Son ...	·3238	·4213	·2144	—	—
Pat. Grandfather and Daughter	·3609	·3802		—	—
Mat. Grandmother and Son ...	—	·2623	·2215	—	—
Mat. Grandmother and Daughter	—	·3180		·2720	·3208 ·1766 ·2305
Pat. Grandmother and Son ...	—	·2722	·0976	—	—
Pat. Grandmother and Daughter	—	·2205		—	—
Mean	·3388	·3166	·1326	·2720	·2426

---

* *Phil. Trans.* Vol. 195, A, p. 93.

† *Ibid.* p. 106.

‡ Pearson: *R. S. Proc.* Vol. 66, p. 157.

§ Warren: *R. S. Proc.* Vol. 65, p. 154.

‖ Warren: *Biometrika*, Vol. I. p. 139.

It would thus appear that there is very little difference in the degrees of resemblance between grandparents and grandchildren in the cases of the most complete data, i.e. those for the horse and man. The dog presents curious anomalies, so curious that one must feel grave doubts as to the accuracy of the record. Taking the whole series as it stands the mean grandparental correlation is about ·27. It is doubtful however whether the parthenogenetic grandmothers ought not to be treated as "midgrandparents." In this case the grandparental relation for Daphnia reduces to ·1360 and for Aphis to ·1213—results more closely in accordance with the Basset Hound value than with those for the horse and man. What we need in order to throw light on the whole subject is the measurement of grandparental inheritance for "blending" characters in the case of sexual reproduction. At present we have only data for alternative characters for sexual reproduction and blending characters for parthenogenetic reproduction, and it is by no means certain that the comparison is a fair one. In the case of man it is not easy to obtain numerous data for a blending character in grandparents and offspring, for such characters are rarely put on record: possibly something might be done in the case of measurements of the cephalic index in not too old grandparents and young grandchildren. As a rule, however, we cannot obtain directly adult characters for both. The breeding of small mammals or insects *ad hoc* seems the best solution of the difficulty. At any rate it is clear that we want further observations on grandparental inheritance and if possible on material where the influence of environment and the death-rate are not so great as in Daphnia and Aphis. From a wide range of series of both blending and alternative characters it is now known that the parental correlation is about ·45, but until we know the grandparental correlation with equal certainty it is impossible really to determine the weight to be given to earlier stages of the ancestry. What, however, is clear at present is that the values thus far found are inconsistent with Mr Galton's original $\frac{1}{9}$[*], with the ·15 deduced by Professor Pearson[†] from his fuller treatment of Galton's Law, or even very satisfactorily with the ·25 which he sets as a limit in his paper on the Law of Reversion[‡]. It is to be hoped that biometricians will turn their attention to this important point by making direct observations of blending characters in grandparents and their sexually produced grandchildren.

---

[*] *Natural Inheritance*, p. 133.

[†] *R. S. Proc.* Vol. 62, p. 397.

[‡] *Ibid.* Vol. 66, p. 149.

PROFESSOR DE VRIES ON THE ORIGIN OF SPECIES*.

By W. F. R. WELDON, F.R.S.

In the first volume of his *Mutationstheorie* Professor de Vries has defined and illustrated his conception of the fundamental phenomena on which the process of organic evolution depends. He has done this so fully that it seems permissible to discuss the essential features of his position without waiting for his promised second volume.

Professor de Vries takes the refreshingly unusual course of trying to make clear at the outset what he means by a species. The ultimate systematic unit which he recognises is the "elementary species," or the limited species of such botanists as Jordan, such zoologists as Bourguignat—the smallest group of individuals which can be shown to differ, and to produce offspring which differ, from other groups in any certain number of characters. The characters of such elementary species are normally constant through successive generations. It is usually possible to arrange a number of such elementary species in a series, so that each species, although it differs from its neighbours in each of a, generally large, number of characters, does so to a very slight extent, and the series is therefore nearly continuous. Such a series of groups forms a Linnean species, expressed by the ordinary binomial nomenclature. In some cases the boundaries of a Linnean species are mere matters of convention ("Sache des sogenannten systematischen Tactes"), in others there are at intervals gaps in the series of elementary species which form natural boundaries for the Linnean groups. Such gaps are in general due to the extinction of previously existing elementary species. "Die Linné-schen Arten entstehen durch den Untergang einzelner elementaren Arten aus der bis dahin ununterbrochener Reihe. Dieses Entstehen ist also ein rein historischer Vorgang, und kann nie die Gegenstand experimenteller Forschung werden." (p. 44.)

---

* *Die Mutationstheorie, Versuche und Beobachtungen über die Entstehung der Arten im Pflanzenreich*, Bd. 1. Leipzig, 1901.

The essential problem is therefore that of the origin of "elementary species," and the *Mutationstheorie* is a statement of the process by which this is believed to be effected.

In an elementary species Professor de Vries recognises two distinct phenomena which produce differences between individuals: *Variation proper* (Variabilität im engeren Sinne, oder die individuelle Variabilität) and *Mutation.*

Variation proper is a phenomenon which regularly occurs in every generation, producing a series of differences between individuals such that the distribution of the various kinds of individuals in every generation obeys the laws of chance. Such variation can never lead to a permanent change in the mean characters of the species; and if by stringent selection among such variations the mean character of the race is for a time changed, removal of the selection will be quickly followed by regression to the old mean of the species.

Mutation, on the other hand, is a phenomenon which occurs intermittently, and has not been shown to obey any ascertained law of magnitude or of frequency. An individual which exhibits a mutation belongs already to a new species; and its offspring exhibit regression not to the old specific mean, but to a new one. The whole sum of the differences between two "elementary species," as enumerated in a long systematic diagnosis, may constitute a single mutation, and we are told "Dennoch hat man diese ganze Diagnose als den Ausdruck eines einzigen Merkmales zu betrachten, als eine Einheit, welche als solche entstanden ist, als solche verloren werden kann, deren einzelne Faktoren aber nicht voneinander getrennt in die Erscheinung treten können. Theoretisch haben wir eine solche Gruppe von Eigenschaften gleichfalls als eine Einheit, als ein ganzes Merkmal zu betrachten." (p. 42.)

Without mutation, therefore, no new species can be established; when a mutation has occurred a new species is already in existence, and will remain in existence, unless all the progeny of the mutation are destroyed. The only influence which natural selection can exert upon the course of evolution is that due to the total destruction of species. The phrase "survival of the fittest," as describing a process of evolution, ought to be replaced by "survival of the fittest species."

The fundamental statements, on which the whole *Mutationstheorie* rests, are those concerning the regression of the offspring to one mean if their parents only vary, and to another if their parents exhibit mutations.

The view of regression among the offspring of merely varying individuals is supported mainly by an appeal to experience. A summary of the results achieved in horticulture is held to show that a large number of florists' races have been obtained by crossing; and where stable races have been certainly obtained without cross breeding their existence is attributed to mutation. The main part of the evidence for the asserted instability of forms produced by long-continued selection consists of facts which do not seem to me conclusive. Thus under the heading "*Das Verhalten der veredelten Rassen beim Aufhören der Selection,*" among the

Fig. 1.
First Embryo: 72 Hours of Incubation.

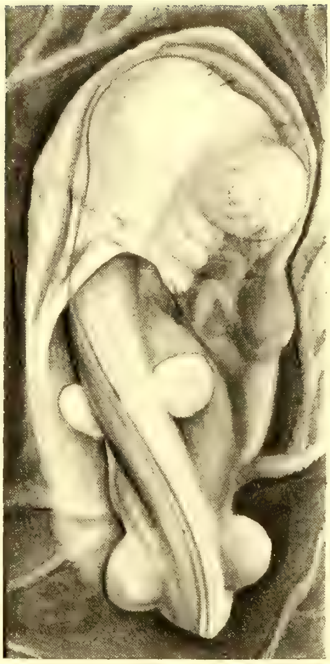


Fig. 2.
Second Embryo: 96 Hours of Incubation.

first cases mentioned is that of certain wheat. The race to which this wheat belonged originated in temperate Europe, but by selection among plants grown in Norway, near the northern limit of possible culture, a form was produced which ripened earlier, and had heavier seeds than the parent form. Seeds from this form, when sown in more southern countries, gave rise after a few generations to plants which resembled the parent race. Here we have obviously to consider not only the cessation of selection, but the change in external conditions, as affecting the result. Again Professor de Vries himself shows that the number of supernumerary carpels in the fruit of *Papaver somniferum polycephalum*, produced by plants grown from seed of the same parental fruit, varies enormously (from 150 to one or two!) according to the amount of nutrition supplied during particular stages of growth; he says deliberately that the selection of plants with the greatest number of carpels is simply the selection of the best nourished individuals; and yet the reduction in the number of extra carpels after cessation of selection is quoted as proof that the results of selecting mere variations are unstable.

Now it cannot be too strongly insisted upon that every character of an animal or of a plant, as we see it, depends upon two sets of conditions; one a set of structural or other conditions inherited by the organism from its ancestors, the other a set of environmental conditions. There is probably no race of plants or of animals which cannot be directly modified, during the life of a single generation, by a suitable change in some group of environmental conditions.

The work of Dareste, Driesch, Herbst, and others has shown that some of the most normal and universal phenomena of animal development are each directly dependent for their occurrence upon a certain group of external conditions. Referring to the recent work of Herbst* for a valuable and suggestive summary of work already done, I take this opportunity of illustrating the connection between normal development and environmental conditions by a new example. It is well known that a hen's egg, at the normal temperature of incubation, loses roughly half a gram of water per day, by evaporation through the shell; as a result of this the density of the medium by which the embryo is surrounded increases, and the bulk of this medium is so diminished as to produce the air space at the broad end of the egg. Some years ago I attempted to replace the water lost by evaporation without preventing the process of evaporation itself†. A hole was made in the broad end of the egg-shell and the subjacent membranes, into which one end of a siphon, filled with water, was fitted. The other end of the siphon was placed in a reservoir of water, and the whole apparatus placed in an incubator. In from 20 to 30 per cent. of embryos treated in this way the amnion was largely or entirely absent after incubation for three or four days. In Fig. 1 (Plate III.) I have drawn an embryo, observed after 72 hours of incubation, and it will be seen that this embryo projects into the albumen without a trace of amniotic covering, like the embryo of a shark. In Fig. 2 a more usual condition is represented. The embryo

---

* Curt Herbst: *Formative Reize in der thierischen Ontogenese*, 8vo. Leipzig, 1901.

† Preyer has shown that an egg, incubated in an atmosphere saturated with water, cannot develope.

(96 hours old) has practically no amnion at the sides, and none at all behind; but the head projects into an amniotic sack, which is widely open posteriorly. Now under a wide range of conditions, including all the differences between development within a uterus or within an egg-shell, the chance that a bird, or reptile, or mammal will not develope an amnion is of about the same order as the chance that it will not develope a head; and the amnion is probably the less variable structure of the two. The production of an amnion is a phenomenon which I think Professor de Vries will certainly not include in his category of individual variations, and yet it can be completely suppressed during the life of a single individual by changing the appropriate group of external conditions. Until we know far more than we know at present about the relation between an organism and its environment, it is simply useless to discuss the stability of characters, whether "variations" or "mutations," except under environmental conditions which are as constant as we can make them during the period under discussion.

The characters which give their value to the improved races of wheat, and to many of our cultivated plants, are admittedly in large part the direct result of cultivation under special conditions; and in order to judge whether the effect of selection on such plants is permanent we must grow them without selection under the same carefully arranged conditions of nutrition as those adopted for the culture of the race during the operation of selection. The evidence as it stands gives little or no indication what their behaviour under such circumstances would be.

Apart from cases in which the cessation of selection has been accompanied by a change in the conditions of culture, the proof that selected varieties are unstable is actually made to include cases in which selection has been deliberately reversed, such as those of Buckman and Watson on parsnips and cabbages, quoted by Darwin.

For the reasons indicated, the discussion of the facts relating to the stability of selected races, given by Professor de Vries, seems to me to be largely irrelevant.

The view that the focus of regression in the offspring of a merely variable species is constant is substantially that adopted as a limiting hypothesis by Professor Pearson in 1895[*]. Professor Pearson at that time put forward two limiting hypotheses; one that the focus of regression is as Professor de Vries supposes, fixed, the other that the focus of regression changes with every generation. The whole of the work since published, both by Mr Galton and by Professor Pearson and his pupils, goes to show that the focus of regression, for each generation, *is its own mean:* hence the array of offspring, produced by parents who differ by a fixed amount *from the mean of the parental generation*, will have a mean deviation *from the mean of the whole filial generation* such that the ratio

$$\frac{\text{mean deviation of offspring from filial mean}}{\text{deviation of parents from parental mean}}$$

will be equal to the coefficient of regression.

---

[*] "Mathematical Contributions to the Theory of Evolution, III." *Phil. Trans.*, A, 1895, pp. 253—318.

A clear proof that Professor Pearson's view of the facts of regression is wrong, although it is in accord both with the theory of chance and with the results of the numerous statistical studies of inheritance which he and his pupils have made during the past seven years, is absolutely essential, if the view held by Professor de Vries is to be maintained. No proof whatever is offered throughout the *Mutationstheorie*. The only observations which bear upon the point, and are sufficiently extensive to serve as serious evidence, are the observations on maize. In 1886 Professor de Vries had a race of maize plants in which the mean number of rows of seeds per head was 12 to 14. By a process of selection, sowing in the first year seed from a head with 16 rows, and in later years seeds from plants with a greater number of rows, he succeeded by 1894 in producing a race in which the mean number of rows per head was 20—a number which rarely occurred, and was practically never surpassed, in the original race. The means of the successive generations are given graphically on p. 53 of his work; but I find it difficult to reconcile the diagram with the statements on p. 88; it is therefore impossible to discuss these results in detail, but certainly neither the diagram, nor the statement on p. 54 that the line on the diagram which shows the mean character of each race "nähert sich im Laufe der Jahre derjenigen der Aussaatkolben immer mehr," is consistent with the view that the ratio between the deviation of the parents of any generation from the original race-mean, and the deviation of their offspring from the same original race-mean, is even approximately constant.

A further proof that regression to the original race-mean does not occur is given by the subsequent history of this maize. In 1897 an attempt was made to reverse the process of selection, and for this purpose individuals were chosen out of the generation of that year which had only 16 rows of seeds per head, the mean of the generation being 20. Now 16 is a greater number than the mean number of rows in the original race; and if regression to the original race-mean occurred, the number of rows of seeds per head among the offspring of individuals with 16 rows should have been at all events less than 16. As a matter of fact it was 20! Not only so, but the individuals with the smallest number of rows per head were taken out of this generation, and their offspring had on an average 18 rows per head. From these again the individuals with the smallest number of rows of seeds were chosen as parents, and the mean number of rows in the third generation was 14—16.

So that this experiment, taken as a whole, forms a fairly conclusive proof that the statements concerning the focus of regression on which the whole theory of the instability of varieties depends, are erroneous, and a main part of the argument fails.

In supposing that his view of regression is identical with that of Mr Galton, Professor de Vries seems to overlook a fundamental difference between the two. When Mr Galton says that parents which exhibit a known deviation D in any character produce offspring whose mean deviation is $\frac{1}{3}D$, he is careful to explain that the parents spoken of are the whole series of parents of their generation which exhibit this deviation D, and that the ancestry of these parents is supposed to be an average sample of the whole antecedent generations, or to have zero deviation. If not only the parents, but the grandparents exhibit deviation D, it is clear from *Natural Inheritance*, pp. 134—137 ("Separate Contribution of Each Ancestor") that the mean deviation of the offspring will be *more than* $\frac{1}{3}D$; while with increase in the number of generations during which the ancestors have exhibited this deviation the ratio between the mean filial deviation and D continually approaches unity. This is stated still more clearly in the memoir on Basset Hounds*. Professor de Vries, however, regards the ratio between parental and filial deviation as fixed, so that the only ancestors whose peculiarities directly affect the individuals of a generation are their immediate parents. Mr Galton's view of the effect of regression follows inevitably from the general theory of chance, if we regard the character of an individual as a phenomenon due to a series of complex groups of causes, among which are the characters of each ancestor. The view which Professor de Vries implicitly adopts, that the characters of remote grandparents are of no effect except indirectly by determining the characters of parents, will not commend itself to naturalists as in accord with experience.

The statements as to the character of regression among the offspring of "mutations" are also unsupported by anything like satisfactory proof. The view held is clearly set forth in many parts of the work, but especially in *Die Lehre von der einseitigen Steigerung von Variabilität durch Auslese*, pp. 416—422. In this section many cases, which are commonly adduced as evidence of the production of stable races by selection of variations, are treated as examples of mutation; and the treatment brings out clearly the nature of the conceptions involved. An example cited is that of *Anemone coronaria*, quoted also by Darwin. The Rev. W. Williamson, after cultivating this plant for some years, found an individual with a single small additional petal. Among the offspring of this plant more supernumerary petals appeared; and by continued selection during some years, a "double" variety was established. On such cases the following comment is offered (pp. 419—420): "Eine solche Verbesserung geschieht, wenn sie einmal möglich ist, rasch und mit zunehmender Geschwindigkeit. Daher die Vorstellung von der zunehmenden Variabilität. Die Erklärung liegt aber einfach darin, dass man, wie im vorigen Paragraphen erörtert wurde, in Bezug auf das neue Material anfänglich Minus-Varianten findet, welche, sobald sie isolirt sind, im Folge des Regressionsgesetzes, sich nicht dem Merkmal der Art, sondern dem Mittelwerth der neuen Varietät nähern." That is to say, the original *Anemone coronaria* which presented an extra petal had undergone a change, which involved not only the obvious structure of one abnormal stamen, but the whole power of hereditary transmission. It and its offspring belonged thenceforth to a new "elementary species," and therefore exhibited regression to a new mean, involving the possession of double flowers. The only thing which a cultivator can do towards producing such a double flowered species is to watch for the first appearance of a mutation, and if

---

* *R. S. Proc.* Vol. LXI. p. 403.

a favourable mutation has occurred to isolate the offspring. Regression will lead such offspring to assume the mean character proper to the new species; and when this has been attained selection can do nothing more of permanent effect until the occurrence of a new mutation. Indeed, if the individual which exhibits the mutation should not be a "Minusvariant," but should be at the mean of the new species, all the attainable improvement will have been effected at once, and no further step can be made without a new mutation.

The existence of the very remarkable form of regression here postulated can only be proved by full evidence of the correlation between parents and offspring in cases which are said to be due to mutation; but such evidence is never provided by Professor de Vries.

The nearest approach to an adequate account of the relation between a mutating individual and its offspring is given in the case of the form called *Trifolium pratense quinquefolium**. In 1886 Professor de Vries found by a roadside several wild plants of *T. pratense*, bearing leaves with four or five leaflets in addition to normal leaves with three leaflets. Two of these plants were removed to his garden and cultivated. In 1889 the two plants together bore 64 leaves with four, and 44 with five leaflets, among a very large number of normal leaves. Data by which the deviation of these plants from the mean of their parental generation could be determined are not given. In 1889 the seed from these plants was collected, and in 1890 there were 100 offspring of the first generation. "About half" of these bore only normal leaves, and were destroyed. The remaining half bore some leaves with four leaflets, and some with five; but the proportion of abnormal leaves is not recorded. The four best plants were saved for seed, and the rest destroyed. In August—September 1890, the four seed-plants had amongst them 69 leaves with four leaflets (64 according to the *Mutationstheorie*) and 44 with five. Of the plants with some abnormal leaves which were not saved for seed the best twelve had amongst them 48 leaves with four and 11 with five leaflets.

The whole number of leaves on yearling plants is less than the number on older plants, so that the four plants chosen for breeding, and some of the plants thrown away, had a larger proportion of abnormal leaves than their parents; but the proportion of abnormal leaves among the whole hundred offspring—the mean character of the offspring from which regression must be determined—was certainly *less* abnormal than that of the parents, and did not exhibit an increase in the number of abnormal leaves, such as should follow from regression to a new "specific mean" with many leaflets, if the hypothesis put forward by Professor de Vries were true. This postulated regression failed to occur in spite of the fact that the offspring were reared in a garden, under conditions shown to favour an excessive production of abnormal leaves.

---

* I have consulted the memoir "Over het omkeeren van Halve-Galton-Curven," *Botanisch Jaarboek*, x., 1898, as well as the account given in the *Mutationstheorie*.

The second generation of offspring, produced in 1891, contained many plants. Three hundred of these plants, which were examined in August, bore

Leaves with 3 leaflets	7189
Leaves with 4 or 5 leaflets	1177
	8366

or an average of four abnormal leaves per plant through the whole series of individuals. The abnormalities were more evenly distributed than in the first generation, since 80 per cent., instead of fifty per cent., bore *some* abnormal leaves. An observation later in the season showed that some half-dozen plants had produced leaves with six leaflets; and the best plant of all produced finally 36 per cent. of abnormal leaves, including six with six leaflets.

Here, after selection during two generations, the variability has increased in one direction, as the ordinary theory would lead one to expect: but there is again no evidence that the mean character of the generation has "regressed" towards a type with many leaflets.

From this time onwards the conditions of culture and of selection were changed. The seeds produced in 1891 were sown in a greenhouse, and were transplanted from seed pans, after the appearance of the third leaf, each into a pot of well manured garden soil. Only those in which the third leaf possessed a supernumerary leaflet were preserved, and of these there were only 18 out of several hundreds of seedlings; we have therefore no means of comparing the mean abnormality of this generation with that of previous generations; all we can learn from this and from the subsequent observations is that under conditions of culture favourable to the production of supernumerary leaflets the percentage of such leaflets among the *extreme* offspring of stringently selected ancestry increased. The large amount of destruction which occurred in this and in subsequent generations clearly shows, however, that there was no regression of mean character in the direction of a new specific type during any part of the experiment. Thus in 1893 the seed of the 18 plants selected in 1892 produced 3409 seedlings, of which only 938, or less than 30 per cent., exhibited the abnormal third leaf for which their parents were selected.

In subsequent years a still more stringent form of selection was adopted. The seed produced by each parent was sown separately, and the percentage of seedlings in which the third leaf was abnormal was noted in each case. Not only were all seedlings rejected in which the third leaf was not abnormal, but the seedlings preserved were taken only from those families which contained a percentage of abnormal individuals.

By proceeding in this way a race of clover has been established in which the modal number of leaflets is approximately five, and deviations occur with fairly symmetrical frequency in either direction. Leaves with more than seven leaflets rarely occur, and when they do an explanation is offered which removes them from the category of the other variations. Leaves with less than three leaflets are also rare.

Since the race is now constant, Professor de Vries suggests that without a new mutation its character cannot be further changed. Experiments to test this supposition, which he has doubtless made, are not described.

The result is of very great interest, but there is no scrap of evidence to show that any part of it is due to the remarkable form of regression to which it is ascribed. Professor de Vries has proceeded throughout his experiment as if Mr Galton's view of inheritance applied to the character selected, and the results obtained are in exact accordance (so far as they can be judged from the data given) with the truth of that view.

The whole book is full of records of experiments as interesting and instructive as the record of work on Clover referred to; especially a large part of it is devoted to the wonderful forms of *Oenothera lamarkiana* which Professor de Vries has raised. But I cannot find evidence that in any one of these numerous experiments the kind of regression ascribed to the offspring of mutations has actually occurred.

The only difficulty in reconciling results, such as those obtained by Professor de Vries, with Darwin's theory of Natural Selection as it is commonly understood seems to me to arise from a belief that the operation of natural selection is of necessity slow, while many new races have certainly been established in a few years. In speaking of the possible slowness of selection in a wild state I think Darwin was influenced first by his constant desire to present his own theory in a way which should give the fullest opportunity for reasonable objection, and secondly by the perception that selection might often be indirect, and therefore fail to act so rigidly upon a particular character as the selection of a human breeder can act. He certainly realised that it can act very quickly under favourable conditions. The case of artificial selection has been very fully discussed by Professor Pearson, on the basis of Mr Galton's work and his own, in his "Law of Ancestral Inheritance*," and it is shown that a view of regression essentially identical with that stated by Mr Galton (but not with that attributed to him by Professor de Vries) leads to the expectation that by selecting parents of constant character for some six or eight generations it will be possible to produce a race of offspring whose mean will closely approximate to that of the selected parents; and further, that after some dozen generations of such selection, the mean character of race will be permanent.

This result of Professor Pearson's is in complete accord with the experimental results obtained by Professor de Vries; it is in complete accord with the little we know concerning the history of domestic races of animals and plants, but it

---

* *R. S. Proc.* XLII. p. 386.

completely excludes any such remarkable form of regression as that which Professor de Vries describes but fails to demonstrate among the offspring of his "mutations."

I feel confident that when this result is better understood than it is at present such naturalists as Professor de Vries and Mr Bateson will abandon their attempts to distinguish between "variations" and "mutations," or between "normal" and "differentiant" variations. These attempts appear always to rest upon a fancied relation between the phenomenon of "regression" and the stability of specific mean character through a series of generations which a little knowledge of the statistical theory of regression will show to be wholly imaginary.

ON THE INFLUENCE OF PREVIOUS VACCINATION IN CASES OF SMALLPOX.

By W. R. MACDONELL, LL.D.

In *Biometrika*, Vol. I. Part II. p. 177 *et seq.*, Professor Karl Pearson's method of finding the correlation coefficients and other constants of characters not quantitatively measurable* was extensively applied to the case of characters quantitatively measurable, in order to avoid the very considerable labour involved in forming correlation tables of the usual detailed kind. I have since used the method in an investigation in which quantitative scales are unobtainable, and to which therefore it is peculiarly applicable, viz., the degree of effectiveness of vaccination in smallpox, and the object of this note is to give my results. I propose to show the correlation, first, between degree of effective vaccination and (1) strength to resist smallpox and (2) type of disease; and secondly, between type of disease and (1) degree of foveation, (2) scar area and (3) number of scars. The data have been extracted from the First Report of the Vaccination Commission, 1896, from a Report by Dr R. S. Thomson and Dr E. L. Marsh on the cases admitted to the City of Glasgow Smallpox Hospital, Belvidere, during the epidemic outbreak in 1892–5, and from the *Times* newspaper of November 30 and January 13 last.

1. The Commissioners' Report, pp. 55—58, gives statistics of the following epidemics: Sheffield 1887–8, London 1892–3, Dewsbury 1891–2, Warrington 1892–3, Leicester 1892–3 and Gloucester 1895–6; the facts were obtained from the local reports upon the epidemics in the six towns, and with regard to these reports the Commissioners write as follows (§ 212): "It is quite possible that the "classification" (vaccinated and unvaccinated) "may not be strictly accurate, "though great pains appear to have been taken to make it so. Doubtful cases "were in general included amongst the vaccinated class, and care was taken to see "that none should be included in the unvaccinated class except those who properly "came within it. Where the doubtful cases were separately stated in the reports 'we have added them to the vaccinated class for the purpose of our calculations."

---

* *Phil. Trans.* Vol. 195, pp. 1—47.

In Sheffield, Warrington, Leicester and Gloucester the doubtful cases do not appear to be stated separately; in London there were 191 doubtful cases, of whom 44 died, and in Dewsbury 24, of whom 2 died.

The figures for the six towns can then be arranged in the following table:—

TABLE I.

Epidemics for Six Towns.

	Recoveries	Deaths	Totals
Vaccinated...	8283	461	8744
Unvaccinated	1499	822	2321
Totals	9782	1283	11065

The constants h and k were calculated, and the equation for r, the coefficient of correlation between degree of effective vaccination and strength to resist the disease, found in the usual way.

$$h = 1{\cdot}19554, \quad k = {\cdot}80726,$$

$$ {\cdot}032834r^6 + {\cdot}014289r^5 + {\cdot}148325r^4 - {\cdot}024924r^3 + {\cdot}482556r^2 + r = {\cdot}888664,$$

whence $$r = {\cdot}6561 \pm {\cdot}0092.$$

On account of the magnitude of the epidemic in Sheffield, I have calculated the result for that town separately.

TABLE II.

Sheffield.

	Recoveries	Deaths	Totals
Vaccinated...	3951	200	4151
Unvaccinated	278	274	552
Totals	4229	474	4703

$$h = 1{\cdot}27716, \quad k = 1{\cdot}18833,$$

$$ {\cdot}097083r^7 + {\cdot}008170r^6 + {\cdot}119614r^5 + {\cdot}137450r^4 + {\cdot}043352r^3 + {\cdot}758844r^2 + r = 1{\cdot}336056,$$

whence $$r = {\cdot}7694 \pm {\cdot}0124.$$

The Leicester and Gloucester epidemics are of special interest owing to the practice of vaccination having fallen into disuse in these towns for some years prior to the epidemic. They are therefore shown separately in the two following tables.

TABLE III.

Leicester.

	Recoveries	Deaths	Totals
Vaccinated ... Unvaccinated	197 139	2 19	199 158
Totals	336	21	357

$$h = 1{\cdot}56497, \quad k = {\cdot}14444,$$

$$\begin{aligned}{\cdot}055622r^7 + {\cdot}016221r^6 + {\cdot}136492r^5 - {\cdot}015457r^4 + {\cdot}236484r^3 \\ - {\cdot}113022r^2 - r + {\cdot}587355 = 0,\end{aligned}$$

whence $$r = {\cdot}6112 \pm {\cdot}0728.$$

TABLE IV.

Gloucester.

	Recoveries	Deaths	Totals
Vaccinated ... Unvaccinated	1091 454	120 314	1211 768
Totals	1545	434	1979

$$h = {\cdot}77455, \quad k = {\cdot}28434,$$

$${\cdot}040653r^6 - {\cdot}005035r^5 + {\cdot}064292r^4 + {\cdot}061288r^3 + {\cdot}110118r^2 + r = {\cdot}649608,$$

whence $$r = {\cdot}5897 \pm {\cdot}0198.$$

The Commissioners' Report, p. 59, also gives the results of an examination of 10403 cases at the Homerton Hospital between the years 1873 and 1884, and of 2584 cases at the Fulham Hospital between the years 1880 and 1885; these are exhibited in the following Table.

TABLE V.

Homerton and Fulham Hospitals.

	Recoveries	Deaths	Totals
Vaccinated ... Unvaccinated	9328 1424	1132 1103	10460 2527
Totals	10752	2235	12987

$$h = \cdot 94596, \quad k = \cdot 86115,$$

$$\cdot 063062 r^6 + \cdot 011756 r^5 + \cdot 161372 r^4 + \cdot 004529 r^3 + \cdot 407310 r^2 + r = \cdot 732600,$$

whence $$r = \cdot 5760 \pm \cdot 0089.$$

These figures include among the vaccinated 1561 doubtful cases, of whom 440 died; if these are excluded altogether the table becomes

TABLE VI.

Homerton and Fulham (doubtful cases excluded).

	Recoveries	Deaths	Totals
Vaccinated...	8207	692	8899
Unvaccinated	1424	1103	2527
Totals	9631	1795	11426

$$h = 1 \cdot 00650, \quad k = \cdot 76829,$$

$$\cdot 059812 r^6 + \cdot 003304 r^5 + \cdot 154271 r^4 - \cdot 0008905 r^3 + \cdot 386642 r^2 + r = \cdot 865473,$$

whence $$r = \cdot 6615 \pm \cdot 0083.$$

The Glasgow statistics have now to be dealt with; they are given on p. 10 of the Report referred to above. The doubtful cases, 20, of whom 5 died, are not included.

TABLE VII.

Glasgow.

	Recoveries	Deaths	Totals
Vaccinated...	622	21	643
Unvaccinated	31	26	57
Totals	653	47	700

$$h = 1 \cdot 49766, \quad k = 1 \cdot 39567,$$

$$\cdot 051044 r^8 + \cdot 099843 r^7 + \cdot 004768 r^6 + \cdot 221286 r^5 + \cdot 069366 r^4 + \cdot 196370 r^3 + 1 \cdot 045120 r^2 + r = 1 \cdot 617863,$$

whence $$r = \cdot 7783 \pm \cdot 0365.$$

Finally I give the result of an examination of the statistics published in the *Times* of January 13 last, which give particulars of 1017 cases of smallpox in

London during the present epidemic. These figures deal only with the cases that were completed in 1901; I regret that I have not seen later figures than these. The doubtful cases, 63, of whom 41 died, have been excluded.

TABLE VIII.

London, for the year 1901.

	Recoveries	Deaths	Totals
Vaccinated...	652	108	760
Unvaccinated	96	98	194
Totals	748	206	954

$$h = \cdot78603, \quad k = \cdot82972,$$

$$\cdot071607r^6 + \cdot001780r^5 + \cdot149636r^4 + \cdot019844r^3 + \cdot326092r^2 + r = \cdot7101,$$

whence $$r = \cdot5779 \pm \cdot0311.$$

On November 30 last the *Times* gave similar particulars for 330 cases completed up to that date, which are shown in

TABLE IX.

London, to November 30, 1901.

	Recoveries	Deaths	Totals
Vaccinated...	195	45	240
Unvaccinated	30	60	90
Totals	225	105	330

$$h = \cdot47281, \quad k = \cdot51572,$$

$$\cdot053861r^6 + \cdot021001r^5 + \cdot077122r^4 + \cdot094990r^3 + \cdot121918r^2 + r = \cdot762763,$$

whence $$r = \cdot6605 \pm \cdot0406.$$

It will be noticed on comparing this result with the previous one that the correlation diminished as the epidemic progressed; this will be an interesting point to investigate again when later figures are available.

The foregoing results may now be collected in the following Table.

TABLE X.

Coefficient of Correlation, r, between effectiveness of vaccination and strength to resist the disease.

	r	Doubtful cases
For the 6 towns enumerated... ...	·6561 ± ·0092	Included in vaccinated
„ Sheffield	·7694 ± ·0124	„
„ Leicester	·6112 ± ·0728	„
„ Gloucester	·5897 ± ·0198	„
„ Homerton and Fulham Hospitals	·5760 ± ·0089	„
„ „ „ „	·6615 ± ·0083	Excluded
„ Glasgow	·7783 ± ·0365	„
„ London, 1901 Epidemic ...	·5779 ± ·0311	„
„ London, 1892–3 Epidemic* ...	·5954 ± ·0272	„

In Sheffield and Glasgow the correlation is nearly the same, and considerably higher than elsewhere; in the other towns it is remarkably uniform, the coefficient approximating to ·6. It will also be noted that the correlation in the present epidemic is nearly the same as that in the epidemic of 1892–3. We have clearly in this coefficient a fairly stable statistical constant for smallpox epidemics.

2. Coming next to the correlation between degree of effective vaccination and type of disease, I divide the types into two classes, (1) Mild, = mild, varioloid, and discrete, and (2) Severe, = coherent and confluent, and exhibit in Table XI. the statistics of the cases whose types were observed in the Sheffield, Dewsbury, Leicester and Warrington epidemics. The London figures for 1892–3 are excluded because a somewhat different classification was adopted there. No figures appear to be available for Gloucester. (See Report of Commission, pp. 66—69.)

TABLE XI.

Sheffield, Dewsbury, Leicester and Warrington.

	Mild	Severe	Totals
Vaccinated...	2229	505	2734
Unvaccinated	229	804	1033
Totals	2458	1309	3767

$$h = \cdot 39212, \quad k = \cdot 60009,$$

$$\cdot 001160 r^7 + \cdot 050798 r^6 + \cdot 016967 r^5 + \cdot 073650 r^4 + \cdot 090250 r^3 + \cdot 117624 r^2 + r = \cdot 959775,$$

whence $$r = \cdot 7935 \pm \cdot 0093.$$

---

* Pearson: *Phil. Trans.* Vol. 195, p. 43.

In Glasgow, the classification is (i), Mild = discrete, and (ii), Severe = confluent and haemorrhagic, which appears to be practically the same as in the above four towns. The figures from which Table XII. is formed are taken from the Report of Drs Thomson and Marsh, p. 11.

TABLE XII.

Glasgow.

	Mild	Severe	Totals
Vaccinated...	608	45	653
Unvaccinated	9	48	57
Totals	617	93	710

$$h = 1{\cdot}12179, \quad k = 1{\cdot}40323,$$

$$\cdot 090996r^7 - \cdot 007112r^6 + \cdot 122066r^5 + \cdot 117763r^4 + \cdot 041736r^3 + \cdot 787065r^2 + r = 1{\cdot}801254,$$

whence $$r = \cdot 9123 \pm \cdot 0181.$$

This high correlation between vaccination and type is in agreement with the comparatively high correlation between vaccination and strength of resistance in Glasgow.

3. Table XIII. is formed to show the correlation between degree of foveation and type in 631 cases of vaccinated persons in Glasgow who took smallpox. (See Report, p. 13.)

TABLE XIII.

Glasgow.

Scars	Mild	Severe	Totals
Foveated ...	479	24	503
Unfoveated...	107	21	128
Totals	586	45	631

$$h = 1{\cdot}46625, \quad k = \cdot 83150,$$

$$\cdot 029479r^5 + \cdot 099698r^4 - \cdot 059144r^3 + \cdot 609594r^2 + r = \cdot 489370,$$

whence $$r = \cdot 3951 \pm \cdot 0594.$$

4. Table XIV. gives the facts as to scar area in the same 631 cases.

TABLE XIV.

Glasgow.

Area of Scar	Mild	Severe	Totals
Over half square inch	379	16	395
Half square inch and under	207	29	236
Totals	586	45	631

$$h = 1{\cdot}46625, \quad k = {\cdot}32125,$$

$${\cdot}017164r^6 + {\cdot}105170r^5 - {\cdot}048332r^4 + {\cdot}171870r^3 - {\cdot}235517r^2 - r + {\cdot}373833 = 0,$$

whence $$r = {\cdot}3520 \pm {\cdot}0584.$$

5. Table XV. gives the facts as to number of scars in these 631 cases.

TABLE XV.

Glasgow.

Scars	Mild	Severe	Totals
Two and upwards	320	16	336
One	266	29	295
Totals	586	45	631

$$h = 1{\cdot}46625, \quad k = {\cdot}08153,$$

$${\cdot}012675r^4 - {\cdot}190374r^3 + {\cdot}059772r^2 + r = {\cdot}233054,$$

whence $$r = {\cdot}2323 \pm {\cdot}0616.$$

This value of r is unexpectedly small, but it is confirmed by the facts observed in Sheffield in 1887–8 and London in 1892–3; these are given in Table XVI., from which doubtful cases, where the records with respect to the nature of the vaccination were incomplete, are excluded. (See Report of the Commission, pp. 71—74.)

TABLE XVI.

Sheffield and London.

Scars	Mild	Severe	Totals
Two and upwards	1855	161	2016
One or none ...	325	64	389
Totals	2180	225	2405

$$h = 1{\cdot}31930, \quad k = {\cdot}98736,$$

$${\cdot}069843r^5 + {\cdot}138433r^4 - {\cdot}0031005r^3 + {\cdot}651312r^2 + r = {\cdot}280372,$$

whence $$r = {\cdot}2418 \pm {\cdot}0325.$$

It is obvious that in dealing with the last four tables we have descended to a much lower plane of correlation, and the results may possibly somewhat modify medical opinion as to the degree of significance of foveation, number of scars and scar area.

I understand that the figures relating to the recent smallpox epidemic in Glasgow will soon be available, and no doubt more statistics of the present London epidemic will be issued shortly; their publication will furnish a mass of extremely interesting and valuable material for statistical work. It is to be hoped that information will soon be given regarding the social rank and occupation of the patients, as an investigation of the type and mortality of the disease in the different classes of the community seems to me a very important line of statistical inquiry, having regard to the state of the controversy at the present time.

Our numbers demonstrate that high correlation exists between the presence of the vaccination scar and both the recovery from and the mildness of the attack. To complete a logical demonstration, however, of the effectiveness of prior vaccination in cases of smallpox we at least require to determine the correlation between the physique and nourishment of the attacked—to some extent indicated by their social class—and the presence or absence of the scar.

MISCELLANEA.

Local Death Rates.

Would it not be worth while for an evolutionist statistician to give some attention to the mass of material accumulated in the Decennial Supplements to the reports of the Registrar-General for England and Wales? These contain for each intercensal decade (1851—60, 1861—70, etc.) a series of tables giving the deaths from different causes and at successive age-groups for each of the 632 Registration Districts in England and Wales. The mean population during the decade at each age-group is also tabulated, so that the rates can be easily worked out. The correlations of death rates at different ages would, for instance, form a very interesting study. Thus the question suggests itself, e.g. are the childhood and adult death rates for different districts always positively correlated—i.e. should we in general expect to find a high adult rate where there is a high mortality in infancy and childhood? Very little inspection will show that the general death rates are thus positively correlated, but it is at least open to question whether death rates from specific causes are so; I would instance the death rates from diseases of the nervous system. Should death rates from some causes show a much lower correlation than others, the question would arise whether the reductions might be due to the selectivity of the death rate; were the death rate highly selective, a high infantile or childhood mortality might lead to a reduced adult mortality and so to an actually negative correlation. It is doubtful however whether this would really occur: high death rates are in general due to bad local conditions of one sort or another, and it must be remembered that any selectivity of the death rate acting on the young may be counterbalanced by a corresponding weakening of the survivors due to these very conditions. It must also be borne in mind that death rates have changed with great rapidity in many parts of England, and that the adults now existing are the survivors of a much severer childhood mortality than the present. Unless, moreover, a careful selection be made and the rapidly growing urban districts taken by themselves, the influence of migration may make itself felt. From the point of view of selection many difficulties might be avoided if a group of districts with little migration could be formed, and the *change* in childhood death rate between the two decades, say 1851—60, 1861—70, for each district compared with the *change* (in the corresponding age-groups) of adult death rate between 1871—80 and 1881—90. Were the childhood death rate markedly selective one would expect the districts exhibiting the greatest decreases of childhood death rate in the earlier period, to exhibit the smallest decreases of adult death rate in the later—i.e. the changes would be negatively correlated. Any investigation would certainly present great difficulties as to interpretation of results, but it would seem worth undertaking.

G. U. YULE.

VOLUME I AUGUST, 1902 No. 4

THE MOST SUITABLE PROPORTION BETWEEN THE VALUES OF FIRST AND SECOND PRIZES.

BY FRANCIS GALTON, F.R.S.

A CERTAIN sum, say £100, is available for two prizes to be awarded at a forthcoming competition; the larger one for the first of the competitors, the smaller one for the second. How should the £100 be most suitably divided between the two? What ratio should a first prize bear to that of a second one? Does it depend on the number of the competitors, and if so, in what way? Similar questions may be asked, but will not be answered here, when the number of prizes exceeds two. What should be the division of the £100 when three prizes are to be given, or four, or any larger number?

The interest of this memoir does not depend solely upon the answer to the above questions, but more especially on its bringing to evidence a new property of the law of frequency of error, upon which I stumbled while engaged upon a side branch of the inquiry. The problem then before me (of which the results are still unpublished) was the probability that the winner of a first or of a second prize in a given year, would succeed in winning first or second prizes in subsequent years. The data assumed the following form:—100 winners of a first place supplied $m(1)$ winners of a first place, and $n(1)$ winners of a second place in subsequent years, while 100 winners of a second place supply $m(2)$ winners of a first place and $n(2)$ winners of a second place. What are the future prize-winning capacities of winners of first and second places respectively? Let the most appropriate values of first and second prizes be called α and β, then

$$\frac{\alpha}{\beta} = \frac{\alpha . m(1) + \beta . n(1)}{\alpha . m(2) + \beta . n(2)},$$

whence $\frac{\alpha}{\beta}$ can be determined.

Having found its value for the cases with which I was dealing, I sought to compare it with another obtained through the ordinary law of frequency of error, on the following bases:

(1) I concluded that when only two prizes α and β are given, their values should not be proportioned to the absolute merits of the two competitors, but to their respective *excesses* of merit above the third competitor, who receives no prize at all. Let $[A]$, $[B]$, and $[C]$ be the first, second, and third competitors, and a, b, c the marks allotted to them, then I conceive the most suitable relation of α to β is as $(a-c)$ to $(b-c)$, and not as a to b.

(2) If there be n competitors, considered as random samples from a large body among whom merit is normally distributed, the most reasonable presumption is that they will tend to occupy n equally probable positions. In the ordinary table of the Probability Integral the argument is $\pm hx$, whose values range from 0 to ± infinity, and the tabular values are those of $\Theta(hx)$ ranging from 0 to ± 1. For the present purposes $\Theta(hx)$ must be taken as the argument, running from -1, through 0, to $+1$, and hx becomes the tabular value. If there be n competitors the most equable, and therefore the most probable distribution of them along the scale of $\pm\Theta(hx)$, is that one competitor should fall into each of the n equidistant stalls ($\frac{1}{2}n$ stalls lying on either side of 0), the septa that enclose those stalls being situated at $0, +2, +4, \dots +n$ on the positive side and at $0, -2, -4, \dots -n$ on the negative side. I assume that each competitor fills his stall, and that his position is expressed with needful precision by the middle of the stall. Consequently the places of the several competitors will be taken to be at $+1, +3, +5, \dots + (n-1)$ on the positive side and at $-1, -3, -5, \dots -(n-1)$ on the negative side. Their position is purely a question of evenly distributed probabilities, entirely unconnected with the law by which the values of hx to which they refer are established. At the same time I am aware that others may hold that this method fails in accuracy, by treating the curve of distribution as a polygon, but I shall not stop to argue the point further because the difference of result is too small to weigh in the present argument. Following a nomenclature already adopted, in which the words 'centile' and 'decile' occur, I will call the n values in any array corresponding to those of $\Theta(hx) = \pm 1, \pm 3, \pm 5, \dots \pm (n-1)$, by the name of "equi-postiles," and those of the septa between which they stand by that of "equi-partiles."

(3) Thus far it has been implied that the value of n is known, but, as a matter of fact, it seems usually impossible to arrive at even a grossly approximate idea of the number of *virtual* competitors; which far exceeds their *actual* number in all important competitions. The number of runners in the Derby are few, but they include the best horses out of a multitude of thoroughbreds, who are all qualified for entry but whose owners keep them back because their chance of winning was found by trial performances to be *nil*. The same happens in University scholarships, in the principal athletic sports, and in all competitions that arouse a widely felt and keen desire for distinction.

Therefore being ignorant of n, I selected a few widely different values of it for trial and worked out the $\Theta(hx)$ values of $[A]$, $[B]$, and $[C]$ by the formula

$\frac{1}{n}((n-1), (n-3), (n-5))$. Then I took from the Probability Integral Tables the corresponding values of hx.

As an example of the complete process let $n = 10$, then the most probable values of $\Theta(hx)$ for the ten competitors will, according to my assumption, be

$$-0{\cdot}9,\quad -0{\cdot}7,\quad -0{\cdot}5,\quad -0{\cdot}3,\quad -0{\cdot}1,\quad +0{\cdot}1,\quad +0{\cdot}3,\quad +0{\cdot}5,\quad +0{\cdot}7,\quad +0{\cdot}9.$$

They are separated by equal distances from one another and by the half of those distances from the septa, including the terminals, that enclose them.

Confining ourselves to the first three terms on the positive side, that is to $+0{\cdot}9$, $+0{\cdot}7$ and $+0{\cdot}5$, we find from the Probability Integral Tables that the corresponding values of hx are $+1{\cdot}1631$, $+0{\cdot}7329$, $+0{\cdot}4770$.

The percentage values of $(a-c)$ and $(b-c)$ (as described above in (2)), are quickly derived from these. We will call them X and Y, and their sum S.

$$\begin{array}{l|l} ha = 1{\cdot}1631 & h(a-c) = 0{\cdot}6861 \\ hb = 0{\cdot}7329 & h(b-c) = 0{\cdot}2559 \\ \cline{2-2} hc = 0{\cdot}4770 & hS \qquad = 0{\cdot}9420 \end{array}$$

$$X : 100 :: h(a-c) : hS; \quad Y : 100 :: h(b-c) : hS.$$

Whence $X = 72{\cdot}8$, $Y = 27{\cdot}2$.

Thus h disappears from the result while m, the Mean, does not come under consideration. If it had been taken into consideration by writing $m+a$ for a, $m+b$ for b, and $m+c$ for c, it would have been eliminated by the subtractions, as h was by the divisions.

Similarly if n be taken $= 1000$, the values of $\Theta(hx)$ for $[A]$, $[B]$, and $[C]$ would be $+0{\cdot}9990$, $+0{\cdot}9970$, and $+0{\cdot}9950$ which give $ha = +2{\cdot}3268$, $hb = +2{\cdot}0985$, and $hc = +1{\cdot}9849$.

Proceeding in this way for many widely different values of n, I found to my astonishment that the resultant X and Y values for those of $n = 10$ and above, came out curiously alike, as is shown in Table I.

TABLE I.

n	X	Y	$X+Y$
3	66·7	33·3	100·0
5	71·0	29·0	„
10	72·8	27·2	„
20	73·8	26·2	„
50	74·3	25·7	„
100	74·5	25·5	„
1,000	75·1	24·9	„
10,000	75·3	24·7	„
100,000	75·4	24·6	„

The values of X between those corresponding to $n = 50$ and $n = 100{,}000$ range within a difference of 1·1. The smallest possible class in which c is not negative, consists of five individuals, and even here the proportion of X to Y is as 71·0 to 29·0, which does not differ grossly from that in a class of 100,000 where it is 75·4 to 24·6. Nay, even taking the smallest possible class which is of three individuals, in which the values of ha, hb, hc are respectively equal to $-c$, 0, and $+c$, the value of $h(a-c) = 2hc$ and that of $h(b-c) = hc$. Consequently $S = 3hc$, therefore $X = 100 \times \frac{2}{3}$, and $Y = 100 \times \frac{1}{3}$, $= 66·7$ and $33·3$ as in the Table.

The rationale of the approximate uniformity of the value of X and Y seems well worthy of a more searching mathematical investigation than I am competent to make. It seems difficult to doubt that this curious property of the terminal equi-postiles is associated with others whose character cannot now be foreseen.

Comparison with facts. Many serious objections present themselves *à priori* to the useful application of this theory, among which is the partial non-conformity of examination marks with the law of frequency, especially at either end of the series, one of which is precisely the part here in question. I therefore put the theory to test by procuring through the kindness of friends a large number of sets of marks in various Civil Service examinations. I took them just as they came and found the X and Y values for each case, as in the following example.

No. 268.

$$\begin{array}{r|r} a = 1801 & a - c = 130 \\ b = 1712 & b - c = 41 \\ \cline{2-2} c = 1671 & S = 171 \end{array}$$

$$X : 100 :: 130 : 171 ; \quad Y : 100 :: 41 : 171$$

$$X = 78·0 ; \quad Y = 22·0 ; \text{ Total } 100.$$

I grouped these values into fives, each page of my MS. book containing that number, then into twenty-fives, and so on. Individually their values ran very irregularly, but the groups of 25 began to give hopeful indications which were fully confirmed by larger groupings, as is shown in Table II. where the X values alone are entered. Those of Y are of course complementary to them.

Thus far the evidence that the calculation was correct in principle seemed conclusive, owing to its being so remarkably well confirmed by observation. In fact, I lived for a few days in a fool's paradise, thinking that such was the case, until with the desire of probing the matter more thoroughly, I made a Table of the distribution of the individual observations. The result is shown in Table III., which shattered my sanguine hopes. If the principle upon which the calculation is based had a contributory effect to any noticeable degree, in producing the mean value of 73·4 as shown in Table II., there would have been a concentration of values about that point in Table III. But there is nothing of the kind. The values are pretty equably distributed between 50 and 100, with a slight but distinct tendency in the smaller values to be the more numerous. This seems due to the fact that the curve of distribution (*see Natural Inheritance*) is always convex towards its axis; consequently $b - c$ is on the average less than $\frac{1}{2}(a - c)$.

TABLE II.

Values of X derived from 300 *Lists of Marks in various Civil Service Examinations.*

Mean values of successive groups of		
25 cases	50 cases	100 cases
74·4	74·5	73·6
74·6		
70·6	72·6	
74·7		
75·7	73·3	72·9
70·9		
70·4	69·6	
68·8		
73·5	73·5	73·7
73·5		
74·7	73·9	
73·1		

Mean of all 300 cases, 73·4.

Subject to this qualification, the Mean is no more than the average of random values between certain limits. Those limits are created by the conditions (1) that b cannot exceed a though it may be equal to a, in which case one of the limits is $100(a - c)$ divided by $2(a - c)$, or 50; (2) that b cannot exceed c though it may be equal to c, in which case the other limit is $100(a - c)$ divided by $(a - c) + 0$, or 100.

TABLE III.

Distribution of 300 *Observed Values of X.*

50—	*55—*	*60—*	*65—*	*70—*	*75—*	*80—*	*85—*	*90—*	*95—*	Total
40	36	27	31	32	23	34	30	26	21	300
76		58		55		64		47		300
166					134					300

Therefore it appears to be merely a coincidence that calculation and observation lead to much the same conclusion. The principle on which the former is based is practically neutral in its effect on the observed results, neither contributing to nor conflicting with them in a sensible degree. The curious property of the foremost equi-postiles that it discloses, must rest its claims to interest upon its own merits and not upon any effective aid that it might be supposed to afford to solving the question of the most suitable proportion between the values of first and second prizes.

What I profess to have shown is

(1) that in the three topmost equi-postiles of a normal series, whose measures are a, b, and c, the value of $(a-c)$ is roughly three times as great as that of $(b-c)$, almost independently of the number of individuals in the series and quite independently of its Mean and of its Modulus of Variability.

(2) that observation leads to practically the same result as calculation, but almost wholly for a different reason.

(3) that when only two prizes are given in any competition, the first prize ought to be closely three times the value of the second.

I now commend the subject to mathematicians in the belief that those who are capable, which I am not, of treating it more thoroughly, may find that further investigations will repay trouble in unexpected directions.

Note on Francis Galton's Problem.

(1) THE problem proposed by Mr Galton is one of very great interest and, somewhat generalised, probably of wide application to a number of important biometrical investigations. In its generalised form it seems to open up possibilities of deducing statistical constants from comparatively small samples, for it provides us for the first time, I believe, with the most probable relationships between the individuals forming a random sample. I would state the problem as follows:

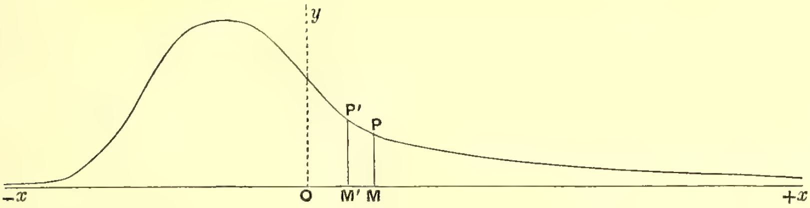
A random sample of n individuals is taken from a population of N members which when N is very large may be taken to obey any law of frequency expressed by the curve $y=N\phi(x)$, $y\delta x$ *being the total frequency of individuals with characters or organs lying between* x *and* $x+\delta x$. *It is required to find an expression for the average difference in character between the* p^{th} *and the* $(p+1)^{th}$ *individuals* when the sample is arranged in order of magnitude of the character.*

I propose to call this general problem: *Francis Galton's Individual Difference Problem in Statistics*, or more briefly *Galton's Difference Problem*. It will be seen at once to carry us from the consideration of the means and standard deviations of mass aggregates and arrays to the average interval between individuals of those aggregates. We may still deal with *averages*, but we fix our attention no longer on the whole population, but on definite individuals in its ordered array. This I believe to be a real advance in statistical theory.

---

* Clearly a knowledge of the average difference in character of adjacent individuals involves also a knowledge of the average difference in character between any two individuals.

(2) Let the figure represent any frequency distribution given by $y = N\phi(x)$, where we may suppose the limits, if finite, to be extended, if necessary, from $+\infty$ to $-\infty$ by zero ordinates. We make no hypothesis as to the nature of the distribution, or the position of the origin; as a corollary we will deal with the case of normal distribution. Let N be the number of individuals or the area of the curve*, A the area to the left of any ordinate $PM = y$, at a character-value $OM = x$. Thus the area to the right is $N - A$. Then, if $a = A/N$, we shall have:



$$a = \int_{-\infty}^{+x} \phi(x)\, dx \quad \text{.......................................(i)},$$

an integral which may be supposed known when the distribution of the general population is known.

We first note that the chance of any random individual having a character less than $x = A/N = a$, and having a character greater than $x = (N - A)/N = 1 - a$. Now let $OM = x_p$ correspond to the p^{th} individual's character reckoned downwards and $OM' = x_{p+1}$, to the next or $(p+1)^{\text{th}}$ individual's character. Then we require first to find the mean value of $M'M = x_p - x_{p+1}$, there being $p - 1$ individuals to right of PM and $n - p - 1$ individuals to left of $P'M'$ in the sample of n individuals we are selecting out of the population. The chance of an individual falling at M is given by $y_p \delta x_p / N$, and of one at M' by $y_{p+1} \delta x_{p+1}/N$; the chance of an individual to left of $P'M' = A_{p+1}/N$ and to right of $PM = (N - A_p)/N$. The total chance therefore of an individual at M, another at M' and $n - p - 1$ to left of $P'M'$ and $p - 1$ to right of PM

$$= \frac{y_p \delta x_p}{N} \times \frac{y_{p+1} \delta x_{p+1}}{N} \times \left(\frac{A_{p+1}}{N}\right)^{n-p-1} \times \left(\frac{N - A_p}{N}\right)^{p-1}.$$

But clearly we could permute the two individuals as well as those to right and left of PM and $P'M'$ and must introduce the factor $\underline{|n} \,/\, (\underline{|n-p-1}\ \underline{|p-1})$.† To get the average we must multiply the chance thus obtained by the corresponding $x_p - x_{p+1}$ and first integrate from $x_{p+1} = -\infty$ to x_p and then for x_p from $-\infty$ to $+\infty$. For, the p^{th} and $(p+1)^{\text{th}}$ individuals may be anywhere in the range provided (i) there are no individuals between them, (ii) the $(p+1)^{\text{th}}$ is anywhere below the p^{th}, (iii) $p - 1$ individuals fall above the latter, and (iv) $n - p - 1$ individuals below the former. Hence if we write x' for x_{p+1}, x for x_p, a' for A_{p+1}/N, a for A_p/N, y_0' for y_{p+1}/N, y_0 for y_p/N, we have for χ_p the average interval between the p^{th} and $(p+1)^{\text{th}}$ individuals:

$$\chi_p = \frac{\underline{|n}}{\underline{|n-p-1}\ \underline{|p-1}} \int_{-\infty}^{+\infty} dx \int_{-\infty}^{+x} dx'\, y_0 / y_0'\, a'^{\,n-p-1} (1-a)^{p-1} (x - x') \quad \text{............(ii)},$$

where by (i)

$$\frac{da'}{dx'} = y_0', \quad \frac{da}{dx} = y_0 \quad \text{.......................................(iii)}.$$

---

* Since $\int_{-\infty}^{+\infty} y\, dx = N$, it follows that $\int_{-\infty}^{+\infty} \phi(x)\, dx = 1$.

† We have to find the permutations of n things which may be distributed into four groups which contain respectively $p - 1$, $n - p - 1$, 1, and 1 individuals. This is the same as the number of ways in which out of n factors $(x + y + z + u)$ we can pick out $(p - 1)$ x's, $(n - p - 1)$ y's, one z and one u, i.e. the coefficient of $x^{p-1} y^{n-p-1} zu$ in $(x + y + z + u)^n$. But this coefficient is $\underline{|n} \,/\, (\underline{|p-1}\ \underline{|n-p-1}\ \underline{|1}\ \underline{|1})$. I owe this method of looking at the factor to Dr L. N. G. Filon.

Consider first the x' integral, i.e.

$$I=\int_{-\infty}^{+x} dx'\, y_0'\, a'^{\,n-p-1}(x-x')=\int_{-\infty}^{+x} da'\, a'^{\,n-p-1}(x-x'),$$

and integrate it by parts. It equals:

$$\left[\frac{a'^{\,n-p}}{n-p}(x-x')\right]_{-\infty}^{+x}+\int_{-\infty}^{+x}\frac{a'^{\,n-p}}{n-p}\,dx',$$

or between limits:

$$=\frac{1}{n-p}\int_{-\infty}^{+x} a'^{\,n-p}dx'=\frac{1}{n-p}\,U,\text{ say.}$$

Thus:

$$\chi_p=\frac{\underline{|n}}{\underline{|n-p}\;\underline{|p-1}}\int_{-\infty}^{+\infty} y_0\, U(1-a)^{p-1}dx,$$

$$=\frac{\underline{|n}}{\underline{|n-p}\;\underline{|p-1}}\int_{-\infty}^{+\infty} U(1-a)^{p-1}da,\text{ by (iii),}$$

$$=\frac{\underline{|n}}{\underline{|n-p}\;\underline{|p}}\left\{\left[-U(1-a)^p\right]_{-\infty}^{+\infty}+\int_{-\infty}^{+\infty}\frac{dU}{dx}(1-a)^p dx\right\},$$

or, taking the value between limits and substituting $\frac{dU}{dx}$, we have

$$\chi_p=\frac{\underline{|n}}{\underline{|n-p}\;\underline{|p}}\int_{-\infty}^{+\infty} a^{n-p}(1-a)^p dx \quad \text{......................................(iv).}$$

This is the complete solution of Galton's difference problem*.

An interesting theorem which results from this has been given me by Mr W. F. Sheppard; namely: the average differences between successive individuals are the successive terms in

$$\int_{-\infty}^{+\infty}\{a+(1-a)\}^n dx$$

when the subject of integration is expanded by the binomial theorem.

Given any law of frequency $y_0=\phi(x)$, we must first find a from (i), and then when tables of a have been made, calculate χ_p by quadratures from (iv). This will be fairly easy, if the distribution be assumed to be normal, for then tables of a, or tables which readily give a, already exist, and quadratures may be used on (iv) to any degree of accuracy required. This has been done by Mr Sheppard in the cases cited below for comparison with Mr Galton's results.

It will be seen that the fundamental difference between the above theory and Mr Galton's lies in the assumption of the latter, that the individual results of a special examination give a sensibly normal distribution. The above theory only assumes that the competitors are a perfectly random sample from material which if it were indefinitely large would obey the law of frequency $y_0=\phi(x)$. Of course, if we want to compare with Mr Galton's results, we must assume this law to be the normal law, but we still have the great generalisation that the actual competitors are only a random sample from a great bulk of material following this law. In any individual examination, it may be quite possible—especially if the competitors are few—that the first man stands anywhere, even below mediocrity, and the chance of this is allowed for in this the full mathematical theory.

---

* This result is due independently to Mr W. F. Sheppard and myself. I had stated Mr Galton's problem to him, and said that I had reduced it to a determination of $\int_{-\infty}^{+\infty} A^r dx$. He sent me, practically by return of post, the answer in the above notation, suggesting quadratures as the best practical solution, and pointing out the theorem referred to in the text.

(3) Another method of reducing the integral in (iv) without quadratures is, perhaps, of interest. I have found it convenient in other cases, where the integral limits are, or can be safely extended to, $\pm\infty$. Suppose we require to find:

$$I=\int_{-\infty}^{+\infty} U\,dx.$$

Let m be the value of x for which U reaches the maximum value U_m and let $u=\log U$; thus $(du/dx)_m=0$, unless $U_m=\infty$. Then we find:

$$\begin{aligned}U=U_m e^{\frac{1}{2}\left(\frac{d^2u}{dx^2}\right)_m \xi^2}\Bigg\{&1+\tfrac{1}{6}\left(\frac{d^3u}{dx^3}\right)_m \xi^3+\tfrac{1}{24}\left(\frac{d^4u}{dx^4}\right)_m \xi^4\\ &+\tfrac{1}{120}\left(\frac{d^5u}{dx^5}\right)_m \xi^5+\tfrac{1}{720}\left[\left(\frac{d^6u}{dx^6}\right)_m+10\left(\frac{d^3u}{dx^3}\right)_m^2\right]\xi^6\\ &+\tfrac{1}{5040}\left[\left(\frac{d^7u}{dx^7}\right)_m+35\left(\frac{d^3u}{dx^3}\right)_m\left(\frac{d^4u}{dx^4}\right)_m\right]\xi^7\\ &+\tfrac{1}{40320}\left[\left(\frac{d^8u}{dx^8}\right)_m+56\left(\frac{d^3u}{dx^3}\right)_m\left(\frac{d^5u}{dx^5}\right)_m+35\left(\frac{d^4u}{dx^4}\right)_m^2\right]\xi^8\\ &+\text{terms in } \xi^9 \text{ and higher powers}\Bigg\} \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\text{(v)}.\end{aligned}$$

Now since U is a maximum, d^2U/dx^2 and generally d^2u/dx^2 will be negative. The limits of ξ where $x=m+\xi$ will also be $\pm\infty$, and the integral of U can thus be expressed in terms of the well-known area and moments of the probability curve. In the first place, if $1/\sigma^2=-d^2u/dx^2$

$$\int_{-\infty}^{+\infty} e^{-\frac{1}{2}\xi^2/\sigma^2}\,\xi^{2i+1}d\xi=0$$

if i be an integer.

Further:

$$\int_{-\infty}^{+\infty} e^{-\frac{1}{2}\xi^2/\sigma^2}\,\xi^{2i}\,d\xi=(2i-1)(2i-3)\ldots\ldots 3\,.\,1\,\sqrt{2\pi}\,\sigma^{2i+1}.$$

Hence writing $a_q=\left(\dfrac{d^qu}{dx^q}\right)_m$, we find:

$$I=\int_{-\infty}^{+\infty} U dx=U_m\sqrt{2\pi}\,\frac{1}{\sqrt{-a_2}}\left\{1+\frac{a_4}{8a_2^2}-\frac{a_6+10a_3^2}{48a_2^3}+\frac{a_8+56a_5a_3+35a_4^2}{384a_2^4}-\text{etc.}\right\}\ldots\ldots\ldots\text{(vi)}.$$

The successive terms often converge with such rapidity that two or three of them are quite sufficient for practical purposes.

To apply this to our special case, we note

$$U=a^{n-p}(1-a)^p,$$

$$u=\log U=(n-p)\log a+p\log(1-a),$$

$$\frac{du}{dx}=\frac{1}{U}\frac{dU}{dx}=\left(\frac{n-p}{a}-\frac{p}{1-a}\right)\frac{da}{dx}.$$

Hence if U be a maximum, we have $du/dx=0$, and

$$a=(n-p)/n, \qquad 1-a=p/n\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\text{(vii)}.$$

Thus m is to be found from

$$\frac{n-p}{n}=\int_{-\infty}^{m} y_0\,dx,$$

or, since

$$1 = \int_{-\infty}^{+\infty} y_0\, dx,$$

$$\frac{p}{n} = \int_m^{\infty} y_0\, dx \quad\text{...(viii).}$$

We find at once:

$$U_m = \frac{(n-p)^{n-p}\, p^p}{n^n} \quad\text{...(ix).}$$

It remains to find the successive differentials of u for $x=m$. Let us write the value of y_0 at $x=m$, simply y_m, and we shall then have

$$(da/dx)_m = y_m, \quad d^2a/dx^2 = y'_m, \quad d^3a/dx^3 = y''_m, \text{ etc.}$$

We find:

$$a_2 = -\frac{n^3}{(n-p)\,p}\, y^2{}_m$$

$$a_3 = -\frac{2n^4(n-2p)}{(n-p)^2\, p^2}\, y^3{}_m - \frac{3n^3}{(n-p)\,p}\, y_m y'_m$$

$$a_4 = -6n^4\left(\frac{1}{(n-p)^3} + \frac{1}{p^3}\right) y^4{}_m - \frac{12n^4(n-2p)}{(n-p)^2\, p^2}\, y^2{}_m\, y'_m - \frac{n^3}{(n-p)\,p}\,(3y'^2{}_m + 4y_m\, y''_m)$$

$$a_5 = -24n^5\left(\frac{1}{p^4} - \frac{1}{(n-p)^4}\right) y^5{}_m - 60n^4\left(\frac{1}{(n-p)^3} + \frac{1}{p^3}\right) y^3{}_m\, y'_m$$

$$-10n^3\left(\frac{1}{p^2} - \frac{1}{(n-p)^2}\right)(3y_m\, y'^2{}_m + 2y^2{}_m\, y''_m)$$

$$-5n^2\left(\frac{1}{p} + \frac{1}{n-p}\right)(2y'_m\, y''_m + y_m\, y'''_m)$$

$$a_6 = -120n^6\left(\frac{1}{(n-p)^5} + \frac{1}{p^5}\right) y^6{}_m - 360n^5\left(\frac{1}{p^4} - \frac{1}{(n-p)^4}\right) y^4{}_m\, y'_m$$

$$-30n^4\left(\frac{1}{(n-p)^3} + \frac{1}{p^3}\right)(9y^2{}_m\, y'^2{}_m + 4y^3{}_m\, y''_m)$$

$$-30n^3\left(\frac{1}{p^2} - \frac{1}{(n-p)^2}\right)(y'^3{}_m + 4y_m\, y'_m\, y''_m + y^2{}_m\, y'''_m)$$

$$-n^2\left(\frac{1}{n-p} + \frac{1}{p}\right)(10y''^2{}_m + 15y'_m\, y'''_m + 6y_m\, y^{\text{iv}}{}_m)$$

etc. etc. ....(x).

These quantities may be calculated fairly easily when y is known as a function of x, the coefficients of the y terms in n and p repeating themselves in each a.

(4) Let us apply these results to the special case when the distribution from which the material is drawn is supposed to obey the normal law. In this case, if s be the standard deviation of the material from which the sample is made:

$$y = \frac{1}{\sqrt{2\pi}\, s}\, e^{-\frac{1}{2}x^2/s^2},$$

$$a = \int_{-\infty}^{+x} y\, dx,$$

$$\chi_p = c \times \int_{-\infty}^{+\infty} a^{n-p}(1-a)^p\, dx, \text{ if } c = \frac{\underline{|n}}{\underline{|n-p}\;\underline{|p}}.$$

Write $x = sx'$, then, if $ys = y'$

$$y' = \frac{1}{\sqrt{2\pi}} e^{-\frac{1}{2}x'^2},$$

$$a = \int_{-\infty}^{+x'} y'\, dx',$$

$$\chi_p = cs \int_{-\infty}^{+\infty} a^{n-p} (1-a)^p \, dx' \quad \text{.............................(xi).}$$

Hence dropping dashes we have:

$$\frac{n-p}{n} = \int_{-\infty}^{+m} \frac{1}{\sqrt{2\pi}} e^{-\frac{1}{2}x^2} dx,$$

or:

$$\frac{n-2p}{n} = \sqrt{\frac{2}{\pi}} \int_0^m e^{-\frac{1}{2}x^2}\, dx \quad \text{.....................................(xii).}$$

Thus as soon as n and p are known m can be found from tables of the probability integral. Then we may find y_m from

$$y_m = \frac{1}{\sqrt{2\pi}} e^{-\frac{1}{2}m^2} \quad \text{...(xiii),}$$

or tables of the ordinates of the normal curve.

We easily find by differentiating (xiii) that:

$$y'_m = -my_m, \quad y''_m = (m^2-1)\, y_m, \quad y'''_m = m(3-m^2)\, y_m$$
$$y^{\mathrm{iv}}_m = (3 - 6m^2 + m^4)\, y_m, \quad y^{\mathrm{v}}_m = m(10m^2 - 15 - m^4)\, y_m \quad \text{..................(xiv).}$$

Substituting in (x) we find:

$$\alpha_2 = -n^2 \left\{\frac{1}{n-p} + \frac{1}{p}\right\} y^2_m \quad \text{...(xv).}$$

$$\alpha_3 = -2n^3 \left\{\frac{1}{p^2} - \frac{1}{(n-p)^2}\right\} y^3_m + 3n^2 \left\{\frac{1}{n-p} + \frac{1}{p}\right\} my^2_m \quad \text{..............................(xvi).}$$

$$\alpha_4 = -6n^4 \left\{\frac{1}{p^3} + \frac{1}{(n-p)^3}\right\} y^4_m + 12n^3 \left\{\frac{1}{p^2} - \frac{1}{(n-p)^2}\right\} my^3_m$$
$$- n^2 \left\{\frac{1}{n-p} + \frac{1}{p}\right\} (7m^2 - 4)\, y^2_m \quad \text{...(xvii).}$$

$$\alpha_5 = -24n^5 \left\{\frac{1}{p^4} - \frac{1}{(n-p)^4}\right\} y^5_m + 60n^4 \left\{\frac{1}{p^3} + \frac{1}{(n-p)^3}\right\} my^4_m$$
$$- 10n^3 \left\{\frac{1}{p^2} - \frac{1}{(n-p)^2}\right\} (5m^2 - 2)\, y^3_m + 5n^2 \left\{\frac{1}{p} + \frac{1}{n-p}\right\} m(3m^2 - 5)\, y^2_m \ \text{...(xviii).}$$

$$\alpha_6 = -120n^6 \left\{\frac{1}{p^5} + \frac{1}{(n-p)^5}\right\} y^6_m + 360n^5 \left\{\frac{1}{p^4} - \frac{1}{(n-p)^4}\right\} my^5_m$$
$$- 30n^4 \left\{\frac{1}{p^3} + \frac{1}{(n-p)^3}\right\} (13m^2 - 4)\, y^4_m + 30n^3 \left\{\frac{1}{p^2} - \frac{1}{(n-p)^2}\right\} m(6m^2 - 7)\, y^3_m$$
$$- n^2 \left(\frac{1}{p} + \frac{1}{n-p}\right) (31m^4 - 101m^2 + 28)\, y^2_m \quad \text{.....................................(xix).}$$

If the α's be found from these equations, then by (xi) and (vi):

$$\chi_p = s \frac{\underline{|n}}{\underline{|n-p}\ \underline{|p}} \frac{(n-p)^{n-p} p^p}{n^n} \sqrt{2\pi} \sqrt{\frac{(n-p)p}{n^3}} \frac{1}{y_m}$$
$$\times \left\{1 + \frac{\alpha_4}{8\alpha_2^2} - \frac{\alpha_6 + 10\alpha_3^2}{48\alpha_2^3} + \text{etc.}\right\} \quad \text{................................(xx).}$$

Here the term in a_3^2 is generally the largest, a_4 the next and a_6 the least.

We can write the terms in the curled brackets:

$$c_1 = -\frac{10a_3^2}{48a_2^3} = \tfrac{5}{6}\frac{(n-2p)^2}{n(n-p)p} - \tfrac{5}{2}\frac{n-2p}{n^2}\frac{m}{y_m} + \tfrac{15}{8}\frac{(n-p)p}{n^3}\frac{m^2}{y^2_m} \quad\text{..................(xxii).}$$

$$c_2 = \frac{a_4}{8a_2^2} = -\tfrac{3}{4}\frac{(n-p)^3+p^3}{n^2(n-p)p} + \tfrac{3}{2}\frac{n-2p}{n^2}\frac{m}{y_m} - \tfrac{1}{8}\frac{(n-p)p}{n^3}\frac{7m^2-4}{y^2_m} \quad\text{............(xxiii).}$$

$$\begin{aligned} c_3 = -\frac{a_6}{48a_2^3} = &-\tfrac{5}{2}\frac{(n-p)^5+p^5}{n^3(n-p)^2p^2} + \tfrac{15}{2}\frac{(n-p)^4-p^4}{n^4(n-p)p}\frac{m}{y_m} \\ &-\tfrac{5}{8}\frac{(n-p)^3+p^3}{n^5}\frac{13m^2-4}{y^2_m} + \tfrac{5}{8}\frac{(n-2p)(n-p)p}{n^5}\frac{m(6m^2-7)}{y^3_m} \\ &-\tfrac{1}{48}\frac{(n-p)^2p^2}{n^6}\frac{31m^4-101m^2+28}{y^4_m} \quad\text{.................................(xxiv).}\end{aligned}$$

And thus:

$$\chi_p = s\frac{\underline{|n}}{\underline{|n-p}\,\underline{|p}}\frac{(n-p)^{n-p}p^p}{n^n}\sqrt{2\pi}\sqrt{\frac{(n-p)p}{n^3}}\frac{1}{y_m}\{1+c_1+c_2+c_3+\dots\} \quad\text{......(xxv).}$$

The solution of the problem is now purely arithmetical, although of course laborious.

(5) We may note some special cases.

Corollary (i). *Suppose both n and p large and not nearly equal.*

Since if q be large

$$\underline{|q} = \sqrt{2\pi q}\, q^{-q}e^{-q},$$

we have

$$\chi_p = s\frac{1}{n\, y_m}\{1+c_1+c_2+c_3+\dots\} \quad\text{..............................(xxvi),}$$

a much simpler form.

Corollary (ii). *Suppose n large and p small.*

$$\chi_p = s\frac{\sqrt{2\pi p}\,p^p e^{-p}}{\underline{|p}}\frac{1}{n y_m}\{1+c_1+c_2+c_3+\dots\} \quad\text{.....................(xxvii).}$$

Corollary (iii). Suppose n large, and that we consider Mr Galton's special problem of the ratio of the distance between the first and second to the distance between the second and third in a graduated array. Then

$$\frac{\chi_1}{\chi_2} = \frac{e}{2\sqrt{2}}\frac{y'_m}{y_m}\frac{1+c_1+c_2+c_3+\dots}{1+c_1'+c_2'+c_3'+\dots} \quad\text{...........................(xxviii),}$$

where undashed letters refer to quantities for $p=1$ and dashed letters to the same quantities when $p=2$.

(6) As a first series of illustrations, let us apply these results to Mr Galton's consideration of the proportion of money to be given in prizes, supposing only two prizes, for the cases $n=3$, 10, 50, 100, 1000.

The following table contains the chief values*. We write:

$$\chi_p = s\times\phi(p)(1+c_1+c_2+c_3+\dots) \quad\text{..............................(xxix).}$$

Then, if $d_{rr'}$ be the difference measured in variability units between the r^{th} and r'^{th} individuals,

$$d_{rr'} = \{\chi_r+\chi_{r+1}+\chi_{r+2}+\dots\dots+\chi_{r'-1}\}/s,$$

---

* I owe to Dr Alice Lee, not only a careful revision of my numbers, but an extension of this table.

and Mr Galton takes as a reasonable measure of the prizes $100d_{13}/(d_{13}+d_{23})$ and $100d_{23}/(d_{13}+d_{23})$ per cent. of the prize money. These are obtained from the last two rows of the table.

Table of Data for Two-Prize Ratios.

$n=$	*3*	*10*	*50*	*100*	*1000*
m	·43074	1·28155	2·05375	2·32635	3·09040
$\log y_m$	$\bar{1}$·560,6213	$\bar{1}$·244,2739	$\bar{2}$·685,0071	$\bar{2}$·425,7300	$\bar{3}$·527,0311
m'	− ·43074	·84162	1·75069	2·05375	2·87830
$\log y_m'$	$\bar{1}$·560,6213	$\bar{1}$·447,0995	$\bar{2}$·935,3726	$\bar{2}$·685,0071	$\bar{3}$·801,9239
$\phi(1)$	·833,910 *	·524,952 *	·380,906 †	·345,992 †	·274,009 †
$\phi(2)$	·833,910 *	·342,013 *	·222,691 †	·198,170 †	·151,399 †
c_1	+ ·004,736	+ ·031,971	+ ·070,072	+ ·084,161	+ ·119,233
c_2	+ ·011,633	− ·005,875	− ·032,216	− ·042,268	− ·066,830
c_3	− ·002,055	+ ·000,204	+ ·002,068	+ ·001,656	− ·001,876
$1+c_1+c_2+c_3$	1·0143	1·0263	1·0399	1·0435	1·0505
c_1'	+ ·004,736	+ ·007,686	+ ·027,170	+ ·035,035	+ ·055,246
c_2'	+ ·011,633	+ ·002,355	− ·010,553	− ·016,108	− ·030,375
c_3'	− ·002,055	− ·000,327	+ ·000,443	+ ·000,517	− ·000,170
$1+c_1'+c_2'+c_3'$	1·0143	1·0097	1·0171	1·0194	1·0247
$\chi_1/s=d_{12}$	·8458	·5388	·3969	·3611 ‡	·2879
$\chi_2/s=d_{23}$	·8458	·3453	·2265	·2020 ‡	·1551
$d_{13}/(d_{13}+d_{23})$	·667	·719	·733	·736	·741
$d_{23}/(d_{13}+d_{23})$	·333	·281	·267	·264	·259

The results are in fairly close agreement with those obtained from Mr Galton's investigation, which puts the first and second individuals in the places they would hold if the sample of the competitive population were actually arranged according to the normal law. His proposition that if there be two prizes they should embrace 75 and 25 per cent. respectively of the prize money is seen to be a sound rule for practical purposes when n is at all large, and might well be impressed upon the powers that rule such distributions not only in the educational world, but in rifle, athletic, sporting and agricultural competitions.

(7) We may next consider how the divergencies between individual members of an array vary when we take the pair close to one end of the array, or nearer to the centre. Let us suppose the array to contain 100 individuals; we already know the differences between the 1st and 2nd, and the 2nd and 3rd individuals. We will now find the differences between the 25th and 26th and the 50th and 51st. In other words we will determine $\chi_{25}(100)$ and $\chi_{50}(100)$. We can easily find these expressions in the more general case for n fairly large §; we have:

$$\chi_{\frac{1}{2}n}(n)=s\times\frac{2\cdot 506{,}628}{n}\left(1+\frac{\cdot 035{,}398}{n}-\frac{\cdot 012{,}327}{n^2}\right)\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(\text{xxx})$$

and

$$\chi_{\frac{1}{4}n}(n)=s\times\frac{3\cdot 146{,}865}{n}\left(1+\frac{\cdot 072{,}942}{n}-\frac{\cdot 026{,}989}{n^2}\right)\ldots\ldots\ldots\ldots\ldots\ldots\ldots(\text{xxxi}).$$

---

* Calculated from (xxv).

† Calculated from (xxvii).

‡ Mr W. F. Sheppard sends me as the values for these constants deduced by quadratures ·3594 and ·2018, which thus show that our method is sufficiently approximate.

§ i.e. using (xxvi).

These will give the corrective terms in the brackets close enough, even if n be as small as 10. The terms outside the brackets will need determining by (xxvii) instead of (xxvi) if n be less than 30, say. We see that (xxx) gives us the average difference between the mediocre individuals and (xxxi) the difference between two individuals at the quartile. Roughly the differences in the two cases are as 5 to 6. But if we compare the extreme individuals' difference for $n=100$, we have

$$\chi_1 = {\cdot}3611 \times s, \quad \chi_2 = {\cdot}2020 \times s, \quad \chi_{25} = {\cdot}0315 \times s, \quad \chi_{50} = {\cdot}0251 \times s.$$

Thus the interval between extreme individuals is more than ten times the interval between mediocre individuals.

Now, of course, the normal distribution in a general sort of way indicates that the differences between modal, or what the biologists term 'normal,' individuals are very small. But Mr Galton's difference problem enables us for the first time to quantitatively appreciate how much wider the differences are between the extreme (or biologists' 'abnormal' individuals) and modal (or normal) individuals. Now the range of a distribution being somewhat about 6*s*, we see that extreme individuals may be separated by as much as $\frac{1}{17}$ of the range, while modal individuals have only a difference of $\frac{1}{250}$th of the range, and even individuals at the quartile only a difference of $\frac{1}{200}$th of the range.

It is not possible to pass over the general bearing of such results on human relations. If we define 'individuality' as difference in character between a man and his immediate compeers, we see how immensely individuality is emphasised as we pass from the average or modal individuals to the exceptional man. Differences in ability, in power to create, to discover, to rule men, do not go by uniform stages. We know this by experience, but we see it here as a direct consequence of statistical theory, flowing from a characteristic and familiar chance distribution. We ought not to be surprised, as we frequently are, at the results of competitive examination, where the difference in marks between the first men is so much greater than occurs between men towards the middle of the list. In the same way the individuality of imbeciles and criminals at the other end of the intellectual and moral scales receives its due statistical appreciation.

We stand in a better position to judge the pathological from the merely exceptional, mere isolation no longer leads us to doubt the position of an extreme outlying error, observation or individual*.

In short Galton's difference problem leads us to look upon samples of populations and even on populations themselves, no longer as arrays of continuously varying individuals, but as systems of discrete units. We see discontinuity in every sample and in every population. We obtain a new and most valuable conception of a normal or standard population. It is one in which each individual is separated from his immediate neighbours, when the whole is arranged according to any character, by definite calculable intervals. These intervals are, of course, the *average* intervals which would be found by taking the mean of many such samples or populations, but they are none the less of extreme suggestiveness. Just as the *continuous* representation by a frequency curve is only an ideal representation of the observed facts, so we now reach an ideal representation of the actual *discontinuity* in the given population. As in the case of many physical investigations, so we find in statistical theory both continuous and discontinuous representations of the phenomena equally important and equally valid within the legitimate limits of interpretation.

(8) As a last illustration I propose to investigate the value of χ when $n=2$, and $p=1$. We easily find:

$$m=0, \quad y_m = \frac{1}{\sqrt{2\pi}},$$

---

* I propose on another occasion to consider the application of Galton's problem to a new theory for the rejection of outlying individuals.

and from (xxii)...(xxiv):

$$c_1 = 0, \quad c_2 = \cdot 017{,}699, \quad c_3 = -\cdot 003{,}081,$$
$$\chi = s \times 1\cdot 127.$$

Since a_5 the next term vanishes, I believe this result is probably true to the last figure. Anyhow I think we may say that if the individuals be taken at random from a population, then the probable value of the standard deviation of that population is nearly $\frac{23}{26}$ of the difference between the two individuals. Thus by averaging the differences between pairs of individuals taken at random we can obtain fairly readily an appreciation of the standard deviation, i.e. the variability of the general population. Further, if we take individuals, not quite at random, but from correlated groups, e.g. pairs of brothers selected at random, the $\frac{23}{26}$th of the average difference of the pairs will be the standard deviation of the correlated groups, e.g. a group of brethren; hence the degree of relationship between such correlated individuals may be determined. This is only a suggestion of one of the many possible uses of Galton's difference problem. It opens up, indeed, many new methods of inquiry, the effectiveness of which, however, can only be tested by their application in actual statistical practice. It must suffice for the present to have indicated that this difference problem marks a new, and very probably a most important, departure in statistical theory.

KARL PEARSON.

THE RELATION OF BINARY FISSION TO VARIATION.

By J. Y. SIMPSON, D.Sc.

It has hitherto been commonly assumed that binary fission as a method of reproduction amongst unicellular organisms is mere duplication, and that the daughter-products of the process are exactly alike. The generalisation was probably in the first instance botanical; thence it spread to zoology, and from lack of investigation has been accepted within the most recent times*. Its use in theory is obvious. In binary fission we do not look for variation; accordingly we are left with an excellent rationale of conjugation, and so, finally, of sexual reproduction, viz. a means to produce variation in the interests of evolution. As the result of certain considerations I have been led to the view that binary fission is not mere duplication, and that the products of such division are more or less unlike. A similar conclusion has been already reached by Ernest Warren in his studies on Parthenogenesis. Thus in a paper entitled "An Observation on Inheritance in Parthenogenesis"† he states: "From twenty-three Daphnia, themselves originating from Parthenogenesis, broods were produced consisting of three to six individuals. The parents were measured, and the offspring were allowed to grow up. On measuring the offspring it was at once obvious that the children of the same brood exhibited very considerable variability."

Perhaps it should be stated here that the contention is not that there is *always* variation in binary fission. Of this I have very little doubt, but it would be exceedingly difficult, perhaps impossible, to prove; for where the variation was not quantitative it might be qualitative. Indeed there were many instances where the products of binary fission did appear to be singularly alike, and they might well have been called duplicates so far as their qualities were revealed by a magnification of 625, but these instances were by no means the majority. On the other hand there were also numerous instances where even with a magnification of 105 it was evident on careful examination that there was considerable dissimilarity between the daughter forms.

---

* Adam Sedgwick's Presidential Address, Section D, *Report of British Association for* 1899, pp. 759, 760.

† *Proceedings of the Royal Society*, Vol. LXV. p. 155.

The species that I have specially examined in this connection are *Paramœcium caudatum* and *Stylonichia pustulata*, but the data submitted in this paper deal only with *Paramœcium*. The points to which attention was directed were :—

(*a*) the total length,

(*b*) the extreme breadth,

(*c*) the index $\frac{\text{breadth}}{\text{length}}$.

In (*a*) differences in daughter forms were found ranging from 0 to 25μ, and in (*b*) from 0 to 20μ. A difference of from 1 to 20μ was also found in the distance between the two contractile vacuoles.

It will be noticed that greater length in one member of a divided pair was not necessarily associated with less breadth.

The following Table gives the lengths and maximum breadths in each of 100 pairs of *Paramœcium caudatum*, expressed in μ, the unit of measurement being 5μ. It also gives the value of the index $\frac{\text{length}}{\text{breadth}}$.

The differences in general outline—e.g. one of the daughters was sometimes found to have a distinctive bend or curve in its contour—were best brought out by means of micro-photography. As micro-photography has not as yet been generally applied to living *Ciliata*, I give a few notes on the method employed. Ordinary Ilford chromatic plates were used, as also a Leitz 3 lens, which with the associated extension gave a magnification of about 80. The pictures were all taken instantaneously by incandescent light. The proportion of failures was very high, about 70 per cent. The chief difficulty was in obtaining a cell small enough to be wholly included within the magnification of the lens. Ultimately a block of soft paraffin was employed in which a hole was pierced with a fine needle. It was then sectioned with the microtome, and in this way by regulating the thinness of the section, a cell was obtained with the minimum of water in which the infusorian could live and yet be in focus all the time. A cover-glass was then superimposed, and as it was held in position by a generous application of vaseline round the edge, I was able to take the photographs in a horizontal position. Some of the photographs were sadly lacking in definition, but it is a matter of extreme difficulty to calculate and adapt that amount of water in the cell which is sufficient for the free movement and life of the protozoon and yet is not too great to allow it to get out of focus during the exposure.

An interesting question is raised as to the exact stage at which these measurements should be taken so as to represent this variation in its truest and greatest degree. It is very natural to suppose that for some little time after division the two daughter *Paramœcia* will be imperfectly formed, e.g. across the line of fission: we may imagine that the posterior end of the anterior daughter and the anterior end of the posterior one are not fully developed, as

TABLE I.

Length, Breadth, and Breadth-Length Index in each member of 100 *Pairs of Paramœcium caudatum, produced by asexual division.*

First Member			Second Member			First Member			Second Member			First Member			Second Member		
Length	Breadth	Index	Length	Breadth	Index	Length	Breadth	Index	Length	Breadth	Index	Length	Breadth	Index	Length	Breadth	Index
265	65	·245	245	60	·245	225	65	·289	220	65	·295	245	75	·306	235	85	·362
280	65	·232	275	55	·200	215	70	·326	210	70	·333	235	75	·319	230	70	·304
250	45	·180	230	50	·217	230	70	·304	210	90	·429	230	80	·348	220	70	·318
270	60	·222	250	70	·280	220	80	·364	210	85	·405	205	85	·415	200	85	·425
285	85	·298	280	85	·304	225	55	·244	215	60	·279	230	65	·283	230	60	·261
255	70	·275	250	75	·300	215	65	·302	200	70	·350	255	75	·294	250	75	·300
185	70	·378	180	65	·361	200	75	·375	200	75	·375	250	70	·280	245	60	·245
230	75	·326	220	70	·318	220	85	·386	220	75	·341	200	55	·275	195	60	·308
215	70	·326	210	80	·381	225	65	·289	225	65	·289	265	70	·264	250	65	·260
210	65	·310	195	75	·385	245	70	·286	240	75	·313	255	75	·294	245	80	·327
175	50	·286	170	55	·324	230	70	·304	230	65	·283	250	80	·320	245	80	·327
240	70	·292	235	80	·340	230	70	·304	220	65	·295	240	65	·271	230	60	·261
210	55	·262	200	55	·275	235	65	·277	225	65	·289	220	70	·318	220	65	·295
240	70	·292	230	85	·370	240	55	·229	225	55	·244	225	60	·267	220	65	·295
245	70	·286	235	65	·277	245	65	·265	245	60	·245	220	55	·250	210	60	·286
235	75	·319	225	70	·311	230	70	·304	220	60	·273	250	75	·300	245	80	·327
220	75	·341	215	70	·326	225	60	·267	220	55	·250	255	70	·275	250	70	·280
240	65	·271	235	75	·319	230	60	·261	225	60	·267	260	90	·346	255	85	·333
280	80	·286	270	65	·241	220	60	·273	215	65	·302	230	55	·239	215	60	·279
250	70	·280	245	75	·306	235	70	·298	220	70	·318	220	60	·273	205	65	·317
210	70	·333	205	70	·341	215	70	·326	215	70	·326	240	65	·271	225	65	·289
185	65	·351	185	60	·324	250	75	·300	245	75	·306	220	55	·250	205	55	·268
225	80	·356	225	75	·333	240	65	·271	235	70	·298	235	55	·234	235	50	·213
190	55	·289	175	65	·371	255	80	·314	245	70	·286	275	85	·309	275	75	·273
250	65	·260	240	70	·292	275	80	·291	275	75	·273	205	65	·317	200	60	·300
240	75	·313	230	80	·348	265	75	·283	250	70	·280	215	55	·256	205	60	·293
200	60	·300	190	60	·316	230	55	·239	205	60	·293	250	75	·300	235	70	·298
170	60	·353	165	55	·333	300	95	·317	300	95	·317	235	70	·298	220	75	·341
200	55	·275	190	55	·289	235	85	·362	225	75	·333	210	60	·286	210	55	·262
220	75	·341	200	75	·375	250	65	·260	245	60	·245	250	75	·300	250	75	·300
230	60	·261	220	65	·295	190	60	·316	185	55	·297	250	80	·320	245	65	·265
220	65	·295	210	60	·286	235	60	·255	225	60	·267	255	70	·275	245	80	·327
235	60	·255	220	60	·273	230	60	·261	225	70	·311	225	65	·289	205	70	·341
												240	70	·292	230	70	·304

indeed would appear to be the case for a short interval. Accordingly one should perhaps wait till the daughters are full-grown: on the other hand it may then be objected that the differences that are noticeable may be due to feeding or some modification of the environment. On the whole probably the fairest time at which to make the comparison is just shortly after division. I have attempted comparison at both stages. The measurements are taken at intervals of from one to thirty-two hours after division. A few further observations and statistics show a continued tendency to vary in the second generation.

The correlations between divided individuals have been determined for length, breadth, and the index $\frac{\text{breadth}}{\text{length}}$.

The compilation of the correlation tables was undertaken, and the computations connected with them were performed by Miss Lee, D.Sc., whose assistance I would gratefully acknowledge. The values obtained are given in the following tables:—

TABLE II.

Constants of the Series.

Organ	Mean	Standard Deviation
Length	229·050	19·152
Breadth	68·125	9·155
Index	29·913	4·029

TABLE III.

Correlations.

Organs	Correlations
Lengths of 1st and 2nd Members	·914 ± 0·011
Breadths of 1st and 2nd Members	·782 ± 0·026
Breadth of 1st and Length of 2nd Member	·447 ± 0·054
Index of 1st and 2nd Members	·664 ± 0·038
Length and Breadth of 1st Member	·421 ± 0·055

These values are remarkably high, compared with the homotypic values obtained by Professor Pearson in a large number of cases among animals and plants. Among the causes which obviously tend to raise the apparent correlation between the members of a pair resulting from division are (1) differences in the length of time which elapsed between division and measurement, involving the inclusion in the Table of individuals differing considerably in age; (2) differences in the treatment of different pairs, which the most careful culture cannot wholly prevent. The second of these causes can hardly be allowed for; an attempt was however made to provide a means of estimating the effect of the first, by measuring a smaller series of individuals at known intervals of time after division, and again at a known interval of time after the first measurement. The difficulties of restoring the individuals to normal conditions of life after a first measurement were so great that the series obtained is small: and the measurements were made at a different time of year. The principal difficulty is associated with the artificial means employed to quiet the protozoa so as to admit of accurate measurements being made. Many methods have been suggested, from the use of narcotics downwards. I found the application of 5°/$_0$ solutions of gelatine to be as effective and less deleterious than any other agent. But it is obvious that the differences in environment—from a normal medium to one to which such gelatine has been added—when repeated more than once especially within a short period for purposes of measurement must have some influence upon later growth even when the greatest care is taken, and it is highly probable that this influence is seen in the difference in index correlation to which Professor Pearson calls attention in his note below.

Note on Dr Simpson's Memoir on *Paramœcium caudatum.*

(1) Dr Simpson's results are clearly not purely homotypic as far as the correlations between pairs of members are concerned, but largely affected by the growth factor, the measurements of pairs having been made at all sorts of intervals after division. They cannot therefore be used in their present condition as a means of appreciating homotyposis in the results of linear division. In order to reach pure homotyposis we must obtain measurements of growth in length and breadth during a variety of periods after division. The labour, however, of obtaining measurements on pairs of members at a definite interval after division being very great, Dr Simpson was only able to provide the set of measurements in the accompanying Table IV. These involve eleven pairs measured twice and three of them measured thrice. It will be seen at once that the growth is very irregular, and even more so in breadth

TABLE IV.

	Divided	Measured	Length	Breadth	Measured	Length	Breadth	Measured	Length	Breadth
1st pair	May 22, 11 a.m.	5 p.m.	210	70	May 23, noon	225	75	May 24, 11.30 a.m.	225	80
			230	55		245	65		245	70
2nd pair	May 22, 11 a.m.	4.30 p.m.	230	80	May 23, 1 p.m.	240	90			
			245	70		260	90			
3rd pair	May 22, 10 a.m.	4 p.m.	225	60	May 23, 1.30 p.m.	230	75			
			220	60		225	70			
4th pair	May 22, 5 a.m.	10 a.m.	210	75	May 23, 12.30 p.m.	220	75	May 24, noon	225	75
			220	75		230	75		235	80
5th pair	May 22, noon	5.30 p.m.	240	85	May 23, 1.15 p.m.	245	85	May 24, 1.30 p.m.	245	85
			230	80		235	80		235	80
6th pair	May 22, 6 a.m.	3 p.m.	220	65	May 23, 4 p.m.	230	70			
			215	70		220	75			
7th pair	May 23, 5.30 p.m.	6.30 p.m.	175	65	May 24, 1 p.m.	230	85			
			180	60		240	80			
8th pair	May 23, 4.30 a.m.	6 p.m.	195	60	May 24, 1.30 p.m.	230	80			
			220	70		245	90			
9th pair	May 23, 4 a.m.	6.15 p.m.	230	60	May 24, 12.30 p.m.	250	70			
			215	75		240	90			
10th pair	May 23, 4 a.m.	6.45 p.m.	225	70	May 24, noon	260	95			
			235	65		270	85			
11th pair	May 23, 1 p.m.	5.45 p.m.	205	60	May 24, 2 p.m.	220	75			
			230	70		235	90			

than in length. Thus of the six individuals measured thrice, four exhibited no change at all in length, three no change at all in breadth during the last 24 hours. It seems therefore that individuals either cease growing at a certain interval after division or else have periods of rest. The unit of 5 μ does not seem sufficiently small to obtain accurate results for breadth measurements. For, anything between 72·5 and 77·5 would be registered as 75. But anything just below 72·5 or just above 77·5 would be registered as 70 or 80 respectively. Thus a real growth just less than 5 μ might not appear at all, and a real growth just greater than 5 μ might appear as a growth of 10 μ. As the standard deviation in breadth for the whole series is less than twice 5 μ, this cannot fail to have serious influence in dealing with such a short series as 11 pairs. Hence the conclusions reached below are purely tentative, but they will suffice to show the importance of allowing for the growth factor in dealing with like cases of homotyposis.

(2) In order that we may safely apply the results deduced for growth from the present series to Dr Simpson's earlier series, we must determine whether the former may, as far as its statistical constants are concerned, be considered a random sample of the latter. Accordingly the new measurements were taken as if they had been 44[*] original measurements of length, breadth, etc., on separate individuals.

TABLE V.

Constants of Two Series.

Organ	Mean		Standard Deviation		Coefficient of Variation	
	100 series	44 series	100 series	44 series	100 series	44 series
Length	229·050	227·954	19·152	18·099	8·361	7·940
Breadth	68·125	74·205	9·155	9·997	13·439	13·472
Index	29·913	32·557	4·029	3·470	—	—
Interval from division in hrs.	—	18·023	—	11·153	—	—

TABLE VI.

Correlations of Two Series.

Organs	100 series	44 series
Lengths of 1st and 2nd Members	·914 ± ·011	·698 ± ·074
Breadths of 1st and 2nd Members	·782 ± ·026	·574 ± ·097
Breadth of 1st and Length of 2nd Member[†]	·447 ± ·054	·641 ± ·060
Indices of 1st and 2nd Members	·664 ± ·038	·114 ± ·100
Length and Breadth of 1st Member[†] ...	·421 ± ·055	·594 ± ·066

---

[*] In order not to weight individuals and to leave the series as homogeneous in age-variation as possible the three pairs measured thrice were only included as far as their first two measurements are concerned.

[†] It is a remarkable result that breadth of second member is more highly correlated than breadth of first member with length of first member in both Dr Simpson's series. How far is some element of "compensation in division" indicated?

The least examination of these two series shows that one cannot be considered as a random sample of the other. Not only are the means of breadth and index sensibly different, but there has been a general lowering of the direct and a raising of the cross-correlations, which is far beyond the limits suggested by even the large probable errors. As for the index-correlation, its immense change is very mysterious*. There has been either considerable change in environment, in average interval from division at which measurements were taken, or, as suggested in (1), the breadth to 5 μ is really not close enough to get consistent results.

It appears therefore that, as the second series cannot be looked upon as a random sample of the first, it would be idle to attempt to use results for growth deduced from it to correct for the growth factor the values of the correlations obtained in the first series. Accordingly all that seemed possible was to apply the growth results obtained for the short series to the correlations obtained for the short series and so reach some appreciation of the screening influence of growth on pure homotyposis.

(3) The interval after division was accordingly correlated with length, breadth and index for the 44 cases, with the following results:

TABLE VII.

Interval and Size.

Organ	Correlation
Length and Interval ...	·547 ± ·071
Breadth and Interval...	·538 ± ·072
Index and Interval ...	·273 ± ·094

It will be seen at once that growth-correlation is a most sensible factor in the resemblance between individuals when pairs of them are measured at the same interval after division. It is least for the index, but is quite sensible even in this case. In other words, the index is a more reliable character than absolute length or breadth to deduce *pure* homotyposis from†, but clearly the average percentage growths in length and in breadth are not equal, and accordingly the index itself alters with the interval from division.

The lengths between 3 and 38 hours interval from division were plotted to the intervals; the results were, of course, very irregular, but a straight line certainly would represent them as effectively as any curve. For the data available accordingly the regression line of age and size was taken as the average curve of growth. In other words, for an organ x the growth in time τ was taken to be $\frac{r_{xt}\sigma_x}{\sigma_t}\tau$, where r_{xt} was the correlation for interval from division and size tabled above in VII, σ_x the standard deviation of the organ x and σ_t that of the intervals of time given in Table V. Thus if x be the observed size of an organ at time t its most probable size, at the mean interval $\bar{t}$ after division, would be

$$x' = x - \frac{r_{xt}\sigma_x}{\sigma_t}(t - \bar{t}).$$

---

* The value of the index-correlation was also determined for the second series indirectly from the formula *R. S. Proc.* Vol. 60, p. 493. The value found was ·130, which agrees as well as could be expected with the direct value ·114 given above.

† Used by K. Pearson for gills of mushrooms and by E. Warren for ivy leaves for this reason: see *Phil. Trans.* Vol. 197, A, pp. 240, 338.

Let y be the observed size of any other organ at the same interval after the mean time, then its probable size at the mean time is given by

$$y' = y - \frac{r_{yt}\sigma_y}{\sigma_t}(t - \bar{t}).$$

Clearly means of x' and y' will be equal to means of x and y, and we easily find*

$$\sigma^2_{x'} = \sigma^2_x(1 - r^2_{xt}), \qquad \sigma^2_{y'} = \sigma^2_y(1 - r^2_{yt}) \text{..........................} \text{(i)}$$

$$R_{x'y'} = \frac{\rho_{xy} - r_{xt}r_{yt}}{\sqrt{1 - r^2_{xt}}\sqrt{1 - r^2_{yt}}} \text{..........................} \text{(ii)}$$

where $R_{x'y'}$ is the correlation of the x' and y' characters allowing for growth and ρ_{xy} of x and y, or without allowance for growth.

If x and y are the same organ for the pair, we have

$$R = \frac{\rho - r^2}{1 - r^2} \text{..........................} \text{(iii)}$$

where r is the correlation of growth and interval of time for the organ in question.

If we apply these formulae to the values of ρ given in Table VI and to the values of r_{xt} given in Table VII, we deduce the values of the homotypic correlations, when allowance is made for growth. We find

TABLE VIII.

Homotyposis allowing for Growth †.

Lengths of 1st and 2nd Members	·569 ± ·074
Breadths of 1st and 2nd Members	·400 ± ·151
Length of 1st and Breadth of 2nd Member ...	·491 ± ?
Indices of 1st and 2nd Members	·043 ± ?
Length of 1st and Breadth of 1st Member ...	·425 ± ?

The first two of these results are quite as satisfactory as we could expect from such data. They fit in fairly well with the homotypic correlation distribution clustering about ·5. The fourth is very improbable. But the drop from ·664 to ·114 in index correlation between Dr Simpson's first and second series of measurements is so inexplicable that we cannot possibly expect anything from the index figures. The third result clearly does not satisfy the theorem I have given for cross-homotyposis ‡. But the lowness of the index correlation, and the fact that the length of the 1st member is more highly correlated with the breadth of the 2nd member than with that of the 1st would undoubtedly be explicable, if, as I have suggested, there be some principle of "compensation in division" at work. We can only hope that at some future time we may have available a much more extensive system of growth measurements, made on the *same* long series as the homotypic measurements. Without this correspondence it is impossible to distinguish how much of the degree of resemblance between members resulting from the same division is due to growth and how much to pure homotyposis. But the results for growth in Table VII combined with those for pure homotyposis in Table VIII suffice to show that great care must be used in not treating what are really coefficients of gross resemblance as in Table V—due to pure homotyposis + growth + individual environment of the pairs of members—as in any way a proper measure of the first factor, i.e. of pure homotyposis only.

---

* These formulae may be obtained by straightforward algebra from the above results, but they are really simple cases of a theory developed at length in an unpublished memoir on "Selection allowing for Growth"; (ii) is clearly the partial correlation coefficient for the time-factor constant.

† Probable errors in first two cases calculated from unpublished formulae.

‡ *Phil. Trans.*, Vol. 197, A, p. 287.

A SECOND STUDY OF THE VARIATION AND CORRELATION OF THE HUMAN SKULL, WITH SPECIAL REFERENCE TO THE NAQADA CRANIA.*

By CICELY D. FAWCETT, B.Sc., assisted by ALICE LEE, D.Sc., and others,
University College, London†.

CONTENTS.

(1) *Introductory and Historical.*

THE present investigation was commenced in 1895, but the long series of measurements involved and the elaborate numerical calculations necessary, have delayed the completion of the work until the present time. It forms part of a more general scheme for determining the size, variability and correlation of the chief organs and characters in man, which has been in progress at University College for

---

* The Editors of *Biometrika* have been assisted in publishing this memoir in its present form by the allotment by the President and Council of the Royal Society of a grant for this purpose from their Publication Grant Fund.

† I am responsible for the editing and arrangement of Miss Fawcett's material. The present memoir is to some extent a product of cooperation among the biometric workers at University College. On Miss Fawcett, however, by far the most arduous part of the task has fallen. K. P.

some years past. When this scheme was started but little had been done to obtain a scientific measure of the variability and correlation of the parts of the human body. Innumerable anthropometric, including craniometric measurements, had been made and published but very little had been done in determining scientifically their statistical constants. In fact there was considerable danger that the want of proper statistical theory would bring the science of craniology into discredit with archaeologists. The manner in which variation is dealt with even in such a classical work as Rütimeyer and His's *Crania Helvetica* is astonishing to the statistician who has realised the nature of the distribution of any character in a homogeneous population. A considerable population can be measured and we can determine whether or no it is sensibly differentiated from a second statistically defined population. But to classify a few individuals into different races by means of two or three measurements, such as the cephalic index, the length, or the facial angle,—before the correlation and the variation of these characters have been determined for even a single race—is a very dangerous proceeding, and calculated to bring craniometry into discredit*.

It was with a view accordingly of providing anthropologists with the needful constants for determining racial differences that the scheme spoken of was started. It consisted partly in the reduction of existing published measurements, and partly in the measurement of new and large series, where such were not already available. A fairly comprehensive series of determinations of variability in man were made by Dr Alice Lee, Mr G. U. Yule, and Professor K. Pearson, and published by the latter in his *Chances of Death and other Studies in Evolution*, Vol. I. pp. 256—277. Further a considerable quantity of new material was collected and reduced in a series of papers entitled: *Data for the Problem of Evolution in Man*, published by the Royal Society in their *Proceedings* and *Transactions*.

The first really scientific determination of the variability of the skull was published by Stieda in 1882†, but the value of his paper lies only in the hint that the mathematical methods used by Quetelet and Galton in anthropology ought to be applied to craniology. He does not apply his method to any extensive series of comparative results nor extend it to tests of racial differentiation. A much more complete series for the variation of the parts of the skull is given in the paper in *The Chances of Death* referred to above (see pp. 323—372). The first determination of the correlation of any parts of the skull was, we believe, made in 1895 and published by Professor Pearson in his memoir on *Regression, Heredity, and Panmixia*‡. He correlated length and breadth of skull in modern Germans, modern French, and in the Naqada crania, which had just then reached England. Further correlation results, giving the values in the case of length, breadth, and

---

* Nothing is here said of the power of distinguishing races which an anatomical craniologist may possess after long experience of types. But many such craniologists make their *ultimate* appeal—and this without the requisite statistical knowledge—to craniometry and not to anatomical appreciation.

† "Ueber die Anwendung der Wahrscheinlichkeitsrechnung in der anthropologischen Statistik." *Archiv für Anthropologie*, Bd. XIV. SS. 167—182.

‡ *Phil. Trans.* Vol. 187, A, pp. 279—281.

height, and of the two cephalic indices, were published in the memoir on *Spurious Correlation* of 1896[*]. Soon after this Dr Alice Lee took up the subject and wrote an academic dissertation on the correlation of the skull in 1899, which appeared in the *Phil. Trans.* as *A First Study of the Correlation of the Human Skull*[†]. Her memoir deals chiefly with the correlation between the capacity of the skull and its length, breadth, and height for a variety of races, but incidentally a good many other correlations are considered. Meanwhile Dr Franz Boas had published in 1899 an interesting paper especially drawing attention to the correlation of the cephalic index with the capacity[‡]. Roughly we may say that the result of these investigations is to show:

(i) that the correlation of the parts of the skull varies remarkably from one local race to a second;

(ii) that the correlation of the measurements most commonly made is remarkably small, as compared with that of the principal dimensions of the human body, e.g. the long bones of the skeleton or the bones of the hand.

These conclusions, definite as far as they go, rather strengthened than weakened the need of the investigation we had been for some years engaged upon. Our object was to determine in this "Second Study of the Correlation of the Human Skull" whether, confining our attention to a single race and dealing with a greater range of characters, we should find any more highly correlated than those referred to above. We desired further to place before the reader the full statistical treatment of a large series of skulls, adopting the modern methods of reduction, with a view, if possible, of making these methods more generally known to craniological investigators. The publication of raw material is always of value, but we are convinced that until the statistical constants for variation and correlation have been calculated for such material, but little can be safely asserted as to racial relationship from purely craniometric investigations.

(2) *Material.*

The great difficulty of the statistician in approaching craniological problems is the absence of sufficient material. This is an unavoidable difficulty which must be faced. It is very hard to obtain a homogeneous group of skulls, even 50 in number, and these again must be distributed between the two sexes. The probable errors therefore of constants determined from such series are proportionately large. For statistical purposes much of the craniometric data published by anatomists is hopelessly inadequate, and if we are to trust reliable craniometric judgments rather than anatomical appreciations[§], we must impress upon craniologists the

---

[*] *R. S. Proc.* Vol. 60, p. 495.

[†] *Phil. Trans.* Vol. 196, A, pp. 225—264.

[‡] *The American Naturalist*, N. S. Vol. I. p. 448.

[§] We do not contest the value of the anatomical appreciation in the hands of the master, but we do contest the cloaking of such appreciation by an apparent array of craniometric data, which are statistically inadequate.

need to largely increase the number of crania dealt with, whenever this can be done without introducing doubts as to homogeneity.

In our present investigation we have been lucky in obtaining,—not what the mathematician would term a statistically adequate series, we cannot get skulls like crabs, butterflies, or leaves by the thousand, but—an unusually long series for a craniometric investigation. Professor Karl Pearson requested Professor Flinders Petrie before he left for Egypt in 1894 to procure for him if possible 100 skulls of a homogeneous race. Early in 1895 Professor Petrie reported that he was able to send to England a remarkably fine anthropological collection—the entire skeletons as well as crania—of the now well-known Naqada race, embracing more than 400 individuals. This collection was packed and brought to England at the expense of the late Mr A. B. Pearson-Gee, who, hearing that the project might fall through, generously provided the requisite funds. The whole material was deposited at University College, where Mr Herbert Thompson in 1895 made for Professor Pearson five measurements on 301 skulls; these were used in Professor Flinders Petrie's *Naqada and Ballas*. In 1896 Dr Ernest Warren undertook the measurement of the chief bones of the skeletons, and his results were published in the memoir of 1897: *An Investigation of the Variability of the Human Skeleton: with especial reference to the Naqada Race*[*]. Shortly after, at the suggestion of Professor Pearson, Miss C. D. Fawcett undertook to complete the measurements on the crania. We have thus during a number of years had at our disposal a splendid collection of upwards of 400 skulls. We cannot too heartily thank Professor Flinders Petrie for his kindness in this matter. We are also much indebted to Dr E. Warren for aid. Where possible his sexing of the skeletons has been adopted for the sexing of the skulls, because that sexing was based on a very careful process (*loc. cit.* p. 138) of examination of the long bones, the hip-bones, and the skulls. When this determination was not available appeal was made to Professor G. Thane's determination of sex used in the case of the skulls measured by Mr Herbert Thompson. Lastly in those cases—fairly numerous—in which the skulls had no corresponding skeletons or had not been sexed by Professor Thane, Dr Warren has most kindly come to our aid, and given us his judgment as to sex. It will be found in the tables that the skulls are classed as male (♂), female (♀), doubtful but probably male (♂ ?), doubtful but probably female (♀ ?), and finally sex quite doubtful (?). For the remarks on the anatomical peculiarities of the skulls placed in the last column of the tables of the appendix of measurements we have to thank in particular Professor W. F. R. Weldon, F.R.S., who most kindly went through the whole series with this end in view, and Professor G. Thane who examined some thirty selected crania.

---

[*] *Phil. Trans.* Vol. 189, B, pp. 135—227.

(3) *Brief Account of the Naqada Race.*

Some details as to the Naqada race may be stated here. We owe this summary to the ready response of Professor Flinders Petrie to an appeal for aid in this matter. At Naqada in Upper Egypt there existed one of the largest prehistoric cemeteries yet known, in which about 2000 graves were opened by Professor Petrie and his fellow-workers in the early months of 1895. Since then other cemeteries at Abadiyeh, Hu, and other sites have also been excavated, and the general results have been summed up in *Diospolis Parva*, 1901. The conclusion reached is that the prehistoric cemeteries of Egypt date approximately between 7000 and 5000 B.C., or we may consider the Naqada crania as about 8000 years old. The people whose remains were thus discovered were highly skilful in mechanical work, such as flaking flints and cutting vases in the hardest stones, but they had small skill in copying animate forms. In this they contrast strongly with the artistic powers of the next race, who founded the dynastic history. The portraiture remaining of the prehistoric people shows at the beginning a few examples of the steatopygous race of Bushman type; these are always female figures and perhaps represent the last captive survivors of palaeolithic man in Egypt. The great body of the race was of one type, strongly like the races on either hand of Egypt, the Libyans of Africa, and the Amorites of Syria. The small difference of cephalic index between the prehistoric Egyptians and the cognate peoples of modern Algeria (1800 miles distant and 8000 years later) is even less than that between modern Italians and their forefathers 2000 years earlier. The type in external appearance may be summed up from portraiture as having a well-formed head with finely domed top; a long, slightly aquiline nose; good lips, and a pointed beard. The hair was brown*, abundant and wavy; the eyes, as shown in paintings of the Amorites, were blue.

(4) *Measurements made and Methods of Measurement.*

In determining what measurements should be made on the Naqada crania, we were largely guided by the following consideration: one or another measurement may be ideally good from the anatomist's standpoint, but the chief use of craniometry is for comparative purposes, and what will be of most value will be, not to add new types of measurement, however desirable in themselves, but to make such measurements as will bring the Naqada skulls into relationship with as many measured series as possible. Now there can be no doubt, we think, that the German system as expressed in the concordat of the German craniologists known as the *Frankfurter Verständigung*, whatever be the defects of its individual measurements, covers, in the great catalogue of the German anthropological

---

* Some of the skull-boxes contained the dry scalp with the hair upon it in a remarkable state of preservation. It was a dark brown in short curly twists. In two cases there were locks of some brilliant golden hair, but on careful examination, for which I have to thank Dr W. A. Osborne, dark brown single hairs were extracted from it, and it appeared that the whole had been *bleached*; possibly, this is the earliest case on record of the hair-dyer's handicraft.

collections, by far the largest mass of material yet measured by a nearly uniform system. It will take an army of calculators their lifetimes to reduce that raw material to statistical shape. Accordingly we have largely adopted[*] the series of measurements and the nomenclature of the Frankfurt Concordat as the basis of our treatment of the Naqada skulls. One or two exceptions to this (as the inclusion in our tables of Flower's ophryo-occipital length, which had already been taken by Mr Herbert Thompson) will be dealt with below.

The *Frankfurter Verständigung über ein gemeinsames craniometrisches Verfahren* was settled at a meeting of the German Anthropological Society held at Frankfurt, August 14–17, 1882, and has been accepted by Kollman, Virchow, Ranke, Ecker, His, Schwalbe, Welcker, v. Török, Stieda, Rüdinger, and other well-known German craniological investigators. It was first published in the *Correspondenz-Blatt d. deutsch. anthrop. Gesellsch.* Bd. XIV. S. 1, and offprints may be obtained from Prof. J. Ranke in Munich[†]. The fundamental conception of this concordat is the measurement of lengths and angles in relation to a certain conventional plane now termed the "German horizontal plane." This plane is defined as:

> Jene Ebene, welche bestimmt wird durch zwei Gerade, welche beiderseits den tiefsten Punkt des unteren Augenhölenrandes mit dem senkrecht über der Mitte der Ohröffnung liegenden Punkt des oberen Randes des knöcheren Gehörganges verbinden.

Unfortunately for this definition the four points defining two straight lines—the two lowest points on the under rims of the eye-sockets and the two highest points on the upper rim of the auricular passages—do not necessarily lie in one plane, although the divergence from coplanarity as a rule has small importance. In the present case the craniophor to be presently described swings the skull from the highest points of the upper rims of the auricular passages[‡], and the third point to determine the horizontal plane was taken from the under rim of the left eye-socket, when this was available, as was generally the case.

The determinations of the horizontal plane were made by a Ranke's craniophor and a Spengler's pointer belonging to Professor Pearson. These were made for him by Bernard Wiedermann of Munich, who also provided a Ranke's goniometer. These instruments were made under the personal supervision of Prof. J. Ranke, whom we have to thank for his great kindness in this matter. The craniophor, the Spengler's pointer, and the goniometer are illustrated in the accompanying plate, and a fuller description of them is given by Professor Ranke himself in his *Beiträge zur physischen Anthropologie der Bayern*, Bd. II. S. 11 *et seq.* The fundamental idea of the craniophor is a couple of horizontal bars with axes in the same line, which can be inserted in the auricular passages, so that the skull swings freely from the uppermost points of the upper rims of the auricular orifices.

---

[*] With one important exception: see p. 415.

[†] A reprint with modifications, the source of which is not apparently stated, is given by E. Schmidt: *Anthropologische Methoden*, Leipzig, 1888, pp. 320–31.

[‡] The line through these two points will be spoken of as the auricular axis.

Alternative bars are provided in case these orifices are very small. On the base of the instrument is a pointed rod, which can be clamped in any position, and which enables the operator to fix a third point on the base of the skull. Spengler's pointer is merely a scriber, the horizontal awl or pointer of which can be set to the top line of the horizontal bars. It is then moved to the front of the skull, and this is turned round its auricular axis until the lowest point of the under rim of the eye-socket is in contact with the point of the awl. The skull is then held in this position by adjusting the pointed clamping rod attached to the base.

A horizontal rod sliding on a graduated vertical bar attached to one of the supports of the "auricular axis," then gives the auricular height of the skull, that is, the vertical height of the skull measured perpendicular to the horizontal plane in a line perpendicular to the auricular axis. The goniometer is an instrument almost sufficiently described by its figure. It consists of two parallel horizontal bars terminating in points, which retaining their parallelism can be moved at will in a vertical plane. The points of these two bars can be brought into contact with any two points of the skull in one vertical plane. A rod can then be adjusted so as to be paralled to the line joining these two points, and the angle between this line and the horizontal is read off on a protractor scale attached to the instrument.

The *Frankfurter Verständigung* defines the horizontal length—*gerade Länge*—of the skull as follows:

> Von der Mitte zwischen den Augenbrauenbogen, *arcus superciliares,* auf den Stirn-Nasenwulst, zu dem am meisten vorragenden Punkt des Hinterhaupts parallel mit der Horizontalebene des Schädels gemessen.

A footnote states that the measurement may be taken with the callipers or Spengler's craniometer, but the adjustment to the median and horizontal planes is not described. We adopted the following method which had been previously used by Professor Pearson for taking the horizontal length. The craniophor was placed in use upon a drawing-board covered with good millimetre ruled paper, the auricular axis of the instrument was adjusted so as to be parallel to the ruling. A solid truly vertical block made by the Cambridge Instrument Company was now brought in contact with the back of the skull and its bottom edge brought parallel to the ruling. This gave us a true vertical tangent plane to the skull parallel to the auricular axis. The Concordat says nothing about the measurement of the horizontal length, when the most projecting point of the back of the skull does not lie in the median plane. Such skulls frequently occur, and in such cases the horizontal length as above defined would be skewly measured. We have always taken it *horizontal and perpendicular to the auricular axis.* A similar block cut away above the base, but having a projection of the same thickness as the base and capable of adjustment to any position or height, was then brought in contact with the most projecting point of the forehead with one or both superciliary ridges, or the *glabella** as the case might be, and its base made parallel to the auricular

---

* A projecting edge on the front block allows the measurement to be *always* made from the glabella itself when this is preferred.

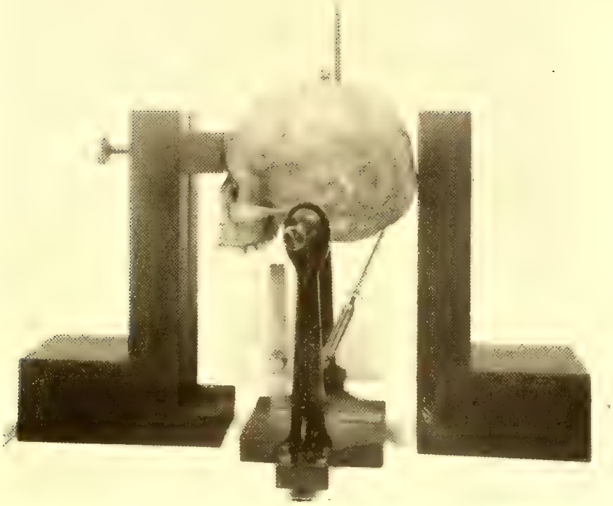


Fig. 1.
Craniophor and Block-Squares.

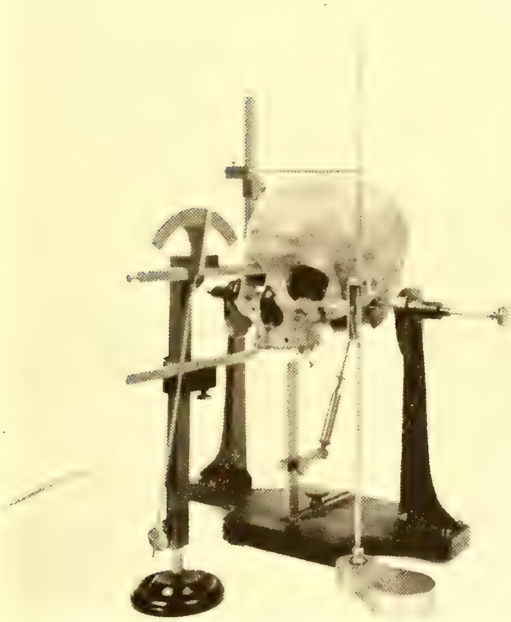
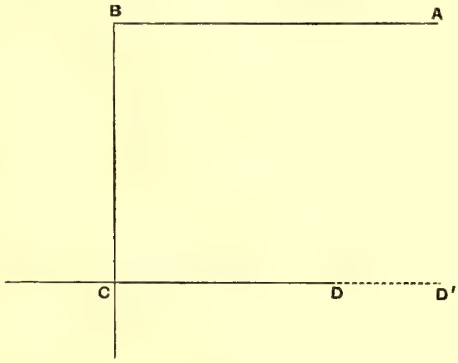


Fig. 2.
Craniophor, Goniometer, and Scriber.

axis by means of the millimetre paper. The horizontal length of the skull was then determined by measuring with a millimetre scale the distance between the bases of the vertical blocks. This was measured on both sides of the blocks to insure that they were truly parallel. It will be seen that this measurement diverges somewhat from the *gerade Länge* of the Frankfurt Concordat. But the *gerade Länge* as measured by the German craniologists with the *callipers,* the eye judging of the horizontality of the instrument, seems to us liable to an error of 1 to 2 millimetres—an error of the same order as the thickness of the superciliary ridges. Taken with the callipers in nine cases out of ten it will be found to agree with the German horizontal length, and in the tenth it is very difficult to determine whether the difference is due in part or not to the hand in holding the callipers. The length determined by aid of the blocks described above would be the exact horizontal length of the skull, if photographed to life size by a camera on the auricular axis at a considerable distance, or it would be the horizontal length of a projection on a plane perpendicular to the auricular axis made by a line parallel to that axis and moving round always in contact with the surface of the skull. We shall consider below what differences there are in mean and variability, of the length of the skull as measured in the three different ways*.

Finally attention may be drawn to the spanner used, which is rectangular in shape and graduated on three sides. It was devised by Professor J. Ranke but is a modification of Virchow's spanner. Diagrammatically it consists of three rods *ABCD,* of which *CD* can be pulled out to *D′* so that *AD′* is parallel to *BC,* and further *CD* can be slid parallel to *AB* along *BC.*



Thus by taking *BC* parallel to any given direction it is possible by one adjustment to determine the distance between *AD* resolved along and perpendicular to this direction, i.e. we can read off *BC* and *DD′*. By this instrument the vertical difference in height of points not necessarily in the same vertical line can be obtained with considerable accuracy. In addition to these instruments we used a steel tape and a small pair of callipers.

---

* Measurements by Mr A. Martin Leake showing how slight are the differences produced by using the English or German methods are cited by Pearson: *The Chances of Death,* Vol. I. p. 270.

The following forty-eight measurements and indices were found for the skulls as far as their condition allowed of the determination:

(*a*) Capacity. Our method of measuring this is dealt with at length below (C).*

(*b*) Flower's ophryo-occipital length. From ophryon to occiput measured with the callipers (F).

(*c*) Greatest length, from glabella to the most projecting point at the back of the skull (L).

(*d*) Horizontal length, measured as described above (L').

(*e*) Greatest horizontal breadth of the skull, measured with the callipers (B).

(*f*) Least breadth of forehead, from one temporal line to the other, as measured with the callipers (B').

(*g*) Height of skull, from the middle of the anterior edge of the *foramen magnum*, the basion, to the point on the top of the skull vertically above it, perpendicular to the German horizontal plane, measured with the callipers with the skull on the craniophor (H).

(*h*) Auricular height, measured with vertical scale and sliding rod of craniophor (OH). See p. 414. For skulls which cannot be placed on the craniophor, the auricular height has been determined by the distance from the auricular axis (see p. 413) to a point on the sagittal suture 2—3 cms. behind its meeting point with the coronal suture, or behind the bregma. Taken with callipers described above, D being inserted in the auricular orifice and CD being made to coincide as closely as possible with the auricular axis "by appreciation." Such height is marked by (h) in the Tables of measurements.

(*i*) Length of skull base, from the basion to the middle of the fronto-nasal suture, the nasion (LB).

(*j*) Horizontal† circumference of the skull, measured directly above the superciliary ridges and round the most projecting point of the back of the head with the steel tape (U).

(*k*) Sagittal or median circumference of the skull, measured from the nasion over the top of the head to the middle of the posterior rim of the *foramen magnum*, or the opisthion. Taken with the steel tape (S).

(*l*) Cross-circumference of the skull, measured in a vertical plane (perpendicular to the "horizontal plane") from the upper rim of one auricular passage to that of the other. Taken with the steel tape (Q).

(*m*) Face height, measured from nasion to the lowest median projection of the mandible. Measured with the callipers (GH).

(*n*) Upper face height, measured from the nasion to the middle of the central process of the upper jaw between the middle incisor teeth, i.e. the alveolar point. Taken with the callipers ($G'H$).

---

* Letters in brackets indicate the symbols used for each measurement.

† Not necessarily 'horizontal' in the sense of the "horizontal plane."

(*o*) Face breadth, being the distance from one zygomatic maxillary suture to the other. The measurement must be taken from the lower end of these sutures, from the lower front rim of one cheek-bone to that of the other. Taken with the callipers (GB).

(*p*) Zygomatic breadth, from the outermost point of one zygomatic arch to the like point on the other. Taken with the callipers (J).

(*q*) Nasal height, measured from the nasion to the lowest edge of the pyriform aperture. Taken with small callipers (NH).

(*r*) Nasal breadth, greatest breadth of the nasal aperture, wherever it may be. Taken with small callipers (NB).

(*s*) Breadth of orbit (O_1), for both left (L) and right (R) eyes; the greatest breadth from side to side of orbit wherever found, measuring from inner margin to inner margin. Taken with small callipers (see p. 430, below).

(*t*) Greatest height of orbit (O_2), for both left (L) and right (R) eyes; taken as closely as possible perpendicular to O_1 by aid of small callipers.

(*u*) Length of palate (G_1); measured from the point of the *spina nasalis posterior* to the inner wall of the alveolar rim between the middle incisors, with small callipers (see p. 429, below).

(*v*) Breadth of palate (G_2); between the alveolar walls at the second molars. With small callipers.

(*w*) Profile length. From the basion to the alveolar point (GL).

(*x*) Condylar width (W_1). Greatest width of mandible at condyles, from outside of one condyle to outside of second. With callipers.

(*y*) Angle width (W_2). Greatest width of mandible at angles; from outside of one angle to outside of the other. With callipers.

(*z*) Greatest height of mandible (h_1); measured from lowest median projection to top of process between middle incisors. With small callipers.

(*zz*) Distance between *foramina mentalia*. With small callipers.

From these measurements the following indices were determined:

(α) The cephalic index ($100\ B/L'$) for greatest length.

(β) The cephalic index ($100\ B/L$) for horizontal length.

(γ) The ratio of height to length ($100\ H/L'$) for greatest length.

(δ) The ratio of height to length ($100\ H/L$) for horizontal length.

(ϵ) The height to breadth index ($100\ H/B$).

(ζ) The breadth to height index ($100\ B/H$).

The determination of both (ϵ) and (ζ) was for purposes of spurious correlation: see p. 461, below.

(η) The face height to face breadth index (100 GH/GB).

(θ) The upper face height to face breadth index (100 $G'H/GB$).

(κ) The nasal breadth to nasal height index (100 NB/NH).

(λ) The orbital height to orbital breadth index for both right and left eyes (100 $O_2/O_1 \, . \, R$ and 100 $O_2/O_1 \, . \, L$).

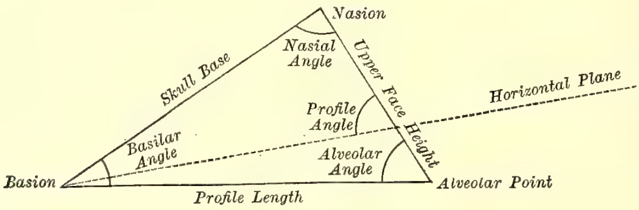
(μ) The palate breadth to palate length index (100 G_2/G_1).

(ν) The length to breadth index (100 L'/B) for greatest length.

(π) The length to height index (100 L'/H) for greatest length.

(ν) and (π) again were only calculated as subsidiary calculations involved in other investigations.

A knowledge of the profile length (GL), the skull basis (LB), and the upper face height ($G'H$) gave us the sides of the triangle formed by the mid-point on the frontal nasal suture, the nasion, the mid-point on the alveolar rim between the middle incisors, the alveolar point, and the mid-point of the anterior rim of the *foramen magnum*, the basion.



We placed these lengths on the trigonometer made for Professor Pearson by the Cambridge Instrument Company, and read off at once the three angles of this triangle*. But knowing the profile angle we were able to discover the angles between the skull base and the profile length and the horizontal plane. We have therefore besides the profile angle the following five angles:

(*aa*) Profile angle (P), measured with Ranke's goniometer as described above: p. 414.

(*bb*) Angle between upper face length and profile length, the *alveolar angle* ($A\angle$). Determined by length measurements and trigonometer as above described.

(*cc*) Angle between skull basis and upper face length, the *nasial angle* ($N\angle$). Determined as in (*bb*).

---

* Professor Thane having much emphasised the importance of these angles, the trigonometer in question was devised for readily obtaining them with sufficient practical accuracy.

(*dd*) Angle between skull basis and profile length, the *basilar angle* ($B\angle$). Determined as in (*bb*).

(*ee*) Angle between skull basis and the German horizontal plane (θ_1), the *basio-nasal horizontal angle*. Determined as in (*bb*), with the use of profile angle as found by goniometer.

(*ff*) Angle between profile length and the German horizontal plane (θ_2), the *basio-alveolar horizontal angle*. Determined as in (*ee*).

Of the six angles, we are inclined to lay most stress on $A\angle$, $N\angle$, and $B\angle$. We found considerable difficulty in determining the profile angle $P\angle$ with satisfactory accuracy from the goniometer, and of course a knowledge of θ_1 and θ_2 depends on the knowledge of $P\angle$.

(5) *On the Determination of the Capacity.*

We devote a special section to the consideration of the method of determining the capacity. In the first place this is admittedly a difficult point even in sound skulls; in the next place our skulls were exceedingly fragile, and notwithstanding very great care in the handling, more than one came to pieces in the process of measurement. Shot of course cannot be used with skulls 7000 to 8000 years old, and even sand is dangerous, so that we only used it for a comparatively few crania. Again, in many of the crania there was a considerable amount of caked and dry desert sand. To extract this by gentle tapping or shaking was an extremely difficult process, and a good deal of it was practically unreachable by any other method. Rough tapping would of course quicken the extraction, but might easily end in the destruction of the skull. In the moving about of the skulls and their handling in the course of their six years at University College most of this desert sand has, we think, been got rid of, but we believe that the smaller values of the capacity found by Mr Herbert Thompson for the same skulls, are to some extent, but by no means entirely, due to the imperfect elimination of this sand. In the next place the difficulty of getting out all the material, sand or seed, placed in a fragile skull is very real, and after testing we found it equally satisfactory to measure our material before it was put in, and not on removal. Thus complete removal of the material from the skull was unnecessary, and a double system of shaking on putting in and taking out was avoided. We commenced by filling the measuring glass with more material than required for the skull, shaking down and reading off. Then we filled the skull, shaking and tapping so far as possible, then we measured the comparatively small remainder, shaken down again in the measuring glass, and the difference gave the skull capacity.

In order to test the observer's own agreement with herself thirty skulls were measured, using (1) mustard seed, (2) rape seed, and (3) silver sand. The method with each material was the same. The results are as follows:

TABLE I.

Capacity of Thirty Skulls, diversely measured.

No.	Sex	Mustard	Rape	Sand
1212	♂	1273	1275	1265
1474	♀	1347	1345	1340
1483	♀	1378	1379	1372
1586	♂	1305	1310	1300
1587^{1}	♂	1445	1440	1435
1651	♂	1288	1283	1295
1666	♀	1300	1303	1295
1683	♂	1245	1243	1245
1743	♀	1327	1325	1317
Q. 408^{c}	♀	1305	1302	1300
1890	♂	1425	1420	1410
Q. 83	♂	1398	1397	1395
1787	♂ ?	1557	1545	1525 ?*
1804	♀	1274	1272	1270
1814^{b}	♀	1215	1220	1220
1825	♀	1200	1205	1205
1827	♀	1187	1190	1190
1875	♂	1285	1288	1285
1878	♀	1310	1315	1308
Q. 466^{4}	♂	1260	1265	1255
B. 5	♀	1355	1365	1345
B. 21	♀	1225	1225	1218
T. 19	♂	1353	1358	1356
T. 23	♀	1280	1275	1265
T. 23^{d}	♂	1253	1260	1250
T. 23^{f}	♀	1277	1280	1270
T. 29^{A}	♂	1220	1230	1225
15	♀	1187	1185	1180
121	♀	1445	1440	1435
724	♀	1205	1205	1200
Mean		1304·1	1304·8	1299·0

Now these results are in very fair agreement, if anything the estimate by sand is slightly less than those by seed. The remainder of the skulls were only done twice, once by mustard seed and once by rape seed, and this merely for verification. Clearly the same person using different material, sand and seed, and consequently rather different methods of packing can obtain results in good agreement among themselves. But if different persons repeat the process or use a different process will they reach sensibly the same values?

Now some of the Naqada skulls were measured independently by four different observers. Thus the capacity of No. 1308 as determined by Professor Thane (mean of two determinations) was 1201, Mr Quebell (mean of two) 1238, H. Thompson (mean of two) 1214, and by C. D. Fawcett (mean of three) 1217. Again, *B* 24^{b} was determined by Professor Thane at 1454, by H. Thompson at 1481 and by C. D. Fawcett at 1497. Further, *B* 21 was estimated by Professor Thane at 1156,

---

* The skull broke under the last measurement, so that it is somewhat doubtful.

by Herbert Thompson at 1173 and by C. D. Fawcett (mean of three determinations) at 1222. Of course some of this difference may be due, as we have suggested, to the gradual elimination of more of the desert sand, but the bulk of it was undoubtedly personal equation. Either C. D. Fawcett packed less tightly in the measuring glass or more tightly in the skull, than H. Thompson did; either way the result is that there is a difference in the determinations which may run up to 20 or 30 cm.^{3}, and much the same holds, if either of our estimates are compared with those of the far-more practised Professor Thane. We look upon the capacity-determination as subject to large personal equation, and we do not see how to correct it. As we got practically identical results with seed or sand, so we believe different observers watching each other's methods would rapidly learn to do the same, but whether such accordant results would give the true capacity of the skull, we are more inclined to doubt.

Herbert Thompson measured 39 male and 55 female Naqada crania. C. D. Fawcett measured 69 males and 98 females. Further, Dr Alice Lee has estimated the mean capacity of these crania from the values of length, breadth and height measured by C. D. Fawcett*. She has also determined from the Leipzig collection of Theban mummy skulls the mean capacity of 201 males and 96 females†. The following are the results:

TABLE II.

Mean Capacity.

Material	Sex	No.	Mean Capacity
Naqada (H. Thompson)	♂	39	1339
" "	♀	55	1243
Naqada (C. D. Fawcett)	♂	69	1387
" "	♀	98	1279
Naqada (Dr Lee's Formula)	♂	69	1377
" "	♀	98	1255
Ancient Egyptians	♂	201	1390
" "	♀	96	1254

Now allowing for the difference of the sample it is clear that there must be a large personal equation element in the differences between H. Thompson and C. D. Fawcett, upwards of 30 to 40 cm.^{3} Roughly weighting his and her ♂ series at 6 and 8, his and her ♀ series at 7 and 10 respectively, we find:

Mean capacity for Naqada ♂ crania = 1366,
" " " " ♀ crania = 1264.

---

* *Phil. Trans.* Vol. 196, pp. 225—264.

† *Loc. cit.* p. 246. A longer series of Egyptian mummies with much the same results is given by Pearson: *Chances of Death*, Vol. I. p. 339.

Personally we would give no greater weight to these results, which took days and days of laborious measurement, than we would to the values 1377 and 1255 obtained in a few minutes from the length, breadth and auricular height by Dr Lee's formula*. Nor should we be prepared to admit any difference upon which an argument as to race might fairly be based between our measurements on the Naqada skulls, Dr Lee's estimate of their capacity, or the values given for the capacity of the ancient Egyptians†. We think it reasonable to suppose the skull capacity of the male Naqada to have been 1370—1380 cm.^{3} and of the female 1250—1260 cm.^{3}, and that there is no sensible change in capacity between this proto-Egyptian race and the Theban people, whose crania are classed as 'mummy skulls' at Leipzig. We shall compare these Egyptians in other characters later with the Naqada race.

(6) *On the Degree of Homogeneity possessed by the Naqada Crania.*

It seemed of importance to determine whether our material might fairly be treated as homogeneous, and Professor Petrie kindly provided us with a list which divided the graves into three classes, namely interments of 30—40 S.D., those of 41—69 S.D., and the latest, of 70—80 S.D. As many skulls as possible were dated roughly from this list, but these were unfortunately a very small proportion of the whole number. Professor Petrie tells us that the smaller and poorer graves which contained only one or two indistinctive pots were not worth plundering, so that in these the skeletons and skulls were found intact. On the other hand the richer graves with plenty of good and distinctive pots had nearly all been plundered, and the skull had very possibly been destroyed or had perished in the process. Hence in very many cases, where the skull was found no dating was possible.

We selected length and breadth as suitable characters and obtained the following results:

TABLE III.

Mean L and B for early and late groupings.

	No.	I 30—40 S. D.	No.	II 40—80 S. D.	No.	III Whole Series‡
L ♂	13	184·88	5	183·45	129	184·64
L ♀	23	177·98	9	175·00	163	177·08
B ♂	13	131·71	6	137·92	129	134·82
B ♀	21	129·54	9	131·53	163	131·31

---

* *Loc. cit.* "mean formula" Table XIX., p. 244.

† The more so as the great disproportion of the sexes for the ancient Egyptians suggests that certain of the larger ♀ crania have been sexed as ♂ crania.

‡ The results here and in Table IV. were deduced from smaller series than were found ultimately available for Tables V. and IX.

Now it will be noticed that the numbers for the later period are quite insignificant, so small indeed as to be largely affected by age at death of the individuals. We can hardly get a real random age distribution in 5 to 9 skulls!

Turning first to the length, the means and standard deviations of I, II and III were calculated and hence the probable error of the differences of the means was found. The results were as follows:

$$♂\ M_{\text{I}} - M_{\text{III}} = \cdot2413 \pm \cdot9882$$
$$♂\ M_{\text{II}} - M_{\text{III}} = -1\cdot1933 \pm 2\cdot0817$$
$$♂\ M_{\text{I}} - M_{\text{II}} = 1\cdot4346 \pm 2\cdot2534$$

Thus in no case was there a sensible difference in length, within the limits of random sampling, of the three groups.

For the females

$$♀\ M_{\text{I}} - M_{\text{III}} = \cdot8943 \pm \cdot5570$$
$$♀\ M_{\text{II}} - M_{\text{III}} = -2\cdot0840 \pm 1\cdot2746$$
$$♀\ M_{\text{I}} - M_{\text{II}} = 2\cdot9783 \pm 1\cdot3589$$

The differences here are all larger than their probable errors, but only in one case slightly more than twice the probable error. It is impossible again to assert that there is a real class difference.

Turning now to the breadth we have

$$♂\ M_{\text{I}} - M_{\text{III}} = -3\cdot1152 \pm \cdot7221$$
$$♂\ M_{\text{II}} - M_{\text{III}} = 3\cdot0938 \pm 1\cdot3378$$
$$♂\ M_{\text{I}} - M_{\text{II}} = -6\cdot2090 \pm 1\cdot4703$$

Now these differences appear significant; in two cases they are four times and in the third case twice their probable errors. But on closer examination of the individual crania we doubted whether the apparent sensibility of the differences in the first and third lines is not largely due to the existence of two or three rather juvenile skulls in the first series.

For the females

$$♀\ M_{\text{I}} - M_{\text{III}} = -1\cdot7674 \pm \cdot6367$$
$$♀\ M_{\text{II}} - M_{\text{III}} = \cdot2278 \pm \cdot8208$$
$$♀\ M_{\text{I}} - M_{\text{II}} = -1\cdot9952 \pm \cdot9831$$

The last two results are hardly to be classed as sensible differences, the first is possibly such.

If the reader will now turn to the frequency diagrams for the breadth of skull in ♀ and ♂ (p. 445) he would expect to find in the former modes at about 129·5 and 131·5 and in the latter modes at 132 and 138, if there be a real difference in the two series of early and late skulls. The conspicuous female maxima are at

126, 131 and 133·5 and the conspicuous male maxima at 131, 135 and 138·5. This again is very inconclusive agreement.

Lastly, if the material were markedly heterogeneous the variability in breadth and length of skull ought to be large as compared with admittedly homogeneous material. We give some comparative results in the following table of standard deviations and coefficients of variation:

TABLE IV.

Variability in L and B of Naqada Crania compared with Homogeneous Series.

Series	L ♂		L ♀		B ♂		B ♀	
	S. D.	C. of V.	S. D.	C. of V.	S. D.	C. of V.	S. D.	C. of V.
Naqada ...	5·722	3·092	5·247	2·963	4·612	3·421	4·490	3·420
Bavarian* ...	6·088	3·371	6·199	3·571	5·849	3·887	4·891	3·394
Aino*	5·936	3·195	5·453	3·077	3·897	2·759	3·662	2·677
French† ...	7·202	3·966	6·435	3·651	6·068	4·206	5·062	3·674
English‡ ...	6·446	3·435	6·536	3·665	4·976	3·554	5·062	3·781

Judged by length we see that for both sexes the Naqada series is less variable than Bavarian, Aino, French, and English. Judged by breadth the Naqada are more variable than the Aino, less than French and English and differ little from the Bavarian skulls. Accordingly it would appear that the Naqada series is quite comparable in homogeneity with any modern series of skulls of like number, even with such very homogeneous material as the "Altbayerisch" crania of Professor Ranke and the Whitechapel crania of Professor Thane. We think therefore that we are justified in treating our material as homogeneous and in speaking of a Naqada *race* and not merely of the Naqada crania.

(7) *Mean Values of the Cranial Characters of the Naqada Race and their Comparison with those of allied and other Races.*

The accompanying Tables V^a and V^b contain our principal results for the mean values of the chief characters. We give the number of skulls dealt with for each mean and the probable errors calculated in the usual manner. We also give for comparison the values which were available for the following races:

(*a*) Maciver's measurements on the earliest inhabitants of Abydos reduced by Miss M. A. Lewenz and Professor Pearson. These results are based on very

---

* Dr A. Lee: *Phil. Trans.* Vol. 196, A, p. 230.

† Deduced by C. D. Fawcett from Broca's measurements of the skulls in the Paris catacombs.

‡ Unpublished measurements on Professor Thane's Whitechapel Skulls.

few crania and the measurements as stated in Maciver's tables, not sufficiently definite to give the indices accurately[*].

(*b*) Ancient Theban Mummies. The means were found by Professor Pearson and Dr Alice Lee from Dr E. Schmidt's measurements published in the Leipzig portion of the German Anthropological Catalogue. Their date is approximately 1500 B.C.

(*c*) Modern Egyptian skulls collected chiefly by Mook from a cemetery near Cairo, extracted from the German Anthropological Catalogue, and reduced by Dr Alice Lee. They are almost certainly Copts.

(*d*) Modern Negroes from the North of Africa. These have been extracted from the German Anthropological Catalogue, and were reduced by Mr N. Blanchard.

Another table (see Table VI.) for modern Negroes but for a few characters only is given later. These were chiefly from Algiers, the Soudan and the West Coast

---

[*] We must express our entire disagreement with the method of exhibiting measurements adopted in D. Randall Maciver's *The Earliest Inhabitants of Abydos*, Oxford, 1901. His series are somewhat smaller than ours, running in each case from about 20 to 60 measurements. He says that they belong to two, and perhaps, to three periods. Now it is perfectly certain that no graphical method will enable us to distinguish in the case of small frequency distributions like this whether a divergence is due to random sampling or to difference of race. We can only proceed as we have attempted to do in Art. 6 by calculating the means and their probable errors, and then ascertaining the degree of significance of the differences. Mr Maciver has not given us a single mean, a single variability, or a single probable error. Until he does this no statistically trained craniologist would venture to draw any conclusions whatever from his results. To those who know the large errors of random samples when the sample consists of not more than 20 to 60 individuals, especially in the case of low correlation, it will be an obvious truth that no conclusions at all can be drawn from the graphical display of these results. For variation the rough observations must be replaced as we have endeavoured to do by the smooth curves. For correlation, the only profitable proceeding is to calculate by Bravais' formula the coefficient of correlation. Hundreds of correlation tables have been formed and published by the workers at University College during the last few years, but although our numbers are often twice to thrice Mr Maciver's, we refrain from publishing a single correlation table on the present occasion. We feel quite convinced that a correlation table with even 100 entries is in 9 cases out of 10 only misleading to the *eye*. What we want is the correlation coefficient and its probable error. For example in Table 14 Mr Maciver gives the correlation table of breadth and height of orbit for male skulls, only 27 in number! We see no conclusion whatever which can be safely drawn from a mere table of this kind. Although when we state for this race that the correlation between breadth and height of orbit is ·43 ± ·06, we can at once make a comparison with other races, and present our material in an intelligible form to the trained statistician. On p. 2 of his memoir Mr Maciver states: "The methods commonly employed for recording anthropometrical data are unsatisfactory in the extreme......Those who realize the importance of full publication yet usually content themselves with flinging together simple lists. Even for a small series of examples such a list is cumbrous and when the series is a long one it becomes most tedious and distracting. The entire labour of tabulation is left to be done by the reader who must be possessed of exceptional patience to undertake it at all." We can only criticise this by saying that Mr Maciver leaves the entire labour of calculation to be done by the reader, and that we should have been grateful had he flung a simple list together. Throughout his measurements never once apparently has a skull stood midway between two units! It is impossible to read the indices accurately, and to determine such an important correlation as that between the orbital and nasal indices would take hours of work and "a reader possessed of exceptional patience," if indeed it could be properly done at all. For some purposes Mr Maciver's tables might possibly be of use as a supplement to a "simple list," but we trust sincerely they may not again *replace* ordered Tables of measurement.

TABLE V^a. *Comparison of Naqada Means with those of other Races.*

MALE.

Character and Reference Letter	Naqada and allied Races										Unallied Races			
											Advanced		Primitive	
	Naqadas		Maciver's Prehistoric Egyptians		Theban Mummies		Modern Egyptians		Modern Negroes		'Altbayerisch' (Modern German)		Aino	
	No.	Mean	No.	Mean	No.	Mean	No.	Mean	No.	Mean	No.	Mean	No.	Mean
(a) C	88	1381·0 ±7·7	—	—	164	1387·63	59	1356·1	39	1347·7	100	1503·5	76	1462
(b) F	121	183·96 ± ·37	44	182·36	—	—	—	—	—	—	—	—	—	—
(c) L	139	185·13 ± ·33	42	184·50	169	181·94	60	177·03	39	182·85	100	180·58	88	185·8
(d) L'	121	184·87 ± ·37	—	—	—	—	—	—	—	—	—	—	88	184·6
(e) B	139	134·87 ± ·26	42	132·38	169	136·63	59	136·76	39	133·15	100	150·47	88	141·2
(f) B'	140	91·06 ± ·27	—	—	168	93·83	60	94·08	32	95·91	72	103·70	88	96·2
(g) H	134	135·21 ± ·31	34	133·94	169	136·05	59	137·24	39	135·31	99	133·78	88	139·5
(h) OH ...	140	115·59 ± ·25	—	—	169	114·34	60	116·07	27	115·04	100	120·75	88	119·3
(ι) LB	109	99·34 ± ·31	24	100·92	169	100·63	59	100·95	27	102·96	85	100·30	88	105·4
(j) U	118	511·02 ± ·81	—	—	168	510·76	59	501·76	32	508·47	99	524·35	88	522·5
(k) S	119	373·02 ± ·74	—	—	168	372·44	59	365·95	32	367·72	80	365·10	77	372·8
(l) Q	116	304·22 ± ·63	—	—	169	306·07	59	311·81	27	306·85	87	329·70	84	328·5
(m) GH ...	28	112·02 ± ·76	—	—	151	114·31	23	115·74	29	115·21	—	—	53	118·3
(n) $G'H$ ...	85	67·59 ± ·30	22	70·64	169	68·78	56	68·69	31	68·00	56	70·80	73	69·8
(o) GB ...	82	95·85 ± ·37	—	—	161	95·66	56	95·29	27	96·63	49	95·10	76	102·1
(p) J	53	125·63 ± ·48	22	126·45	166	128·33	57	125·82	32	129·47	56	135·00	74	137·3
(q) NH ...	91	48·94 ± ·21	25	50·36	169	50·61	59	50·68	27	49·52	70	50·90	79	50·5
(r) NB	86	25·12 ± ·14	25	25·10	166	25·26	58	24·93	34	27·29	70	24·80	78	25·6
(s) O_1L ...	82	43·11 ± ·16	27*	38·07	169*	38·14	59*	38·15	27	39·56	71*	39·90	79*	40·9
(s') O_1R ...	81	42·60 ± ·16												
(t) O_2L ...	80	32·67 ± ·17	27*	32·67	169*	32·96	59*	32·49	27	34·19	71*	33·72	79*	34·9
(t') O_2R ...	72	31·87 ± ·18												
(u) G_1	75	55·80 ± ·28	—	—	160	46·25	52	47·10	26	49·42	56	44·30	75	53·0
(v) G_2	75	40·33 ± ·29	—	—	151	40·83	52	39·98	26	39·73	53	33·20	74	38·2
(w) GL ...	81	94·72 ± ·34	24	96·12	169	95·79	45	97·47	27	105·33	—	—	69	104·9
(x) W_1	41	110·49 ± ·87	—	—	153	118·08	12	112·92	16	117·00	—	—	61	102·0
(y) W_2	51	93·58 ± ·67	—	—	154	97·08	13	95·31	16	97·15	—	—	—	—
(z) h_1	60	32·91 ± ·28	—	—	—	—	—	—	—	—	—	—	59	33·8
(z') f	49	44·44 ± ·25	—	—	—	—	—	—	—	—	—	—	—	—
(aa) $P\angle$ ...	62	84°·41 ± ·25	—	—	47	85°·49†	13	84°·15	11	82°·00	40	89°·10	67	82°
(bb) $A\angle$ ...	73	72°·82 ± ·32	—	—	—	—	—	—	—	—	—	—	—	—
(cc) $N\angle$ ...	73	66°·60 ± ·30	—	—	—	—	—	—	—	—	—	—	—	—
(dd) $B\angle$ ...	73	40°·73 ± ·22	—	—	—	—	—	—	—	—	—	—	—	—
(ee) θ_1	62	28°·29 ± ·29	—	—	—	—	—	—	—	—	—	—	—	—
(ff) θ_2	62	11°·30 ± ·34	—	—	—	—	—	—	—	—	—	—	—	—
(α) 100 B/L' ...	101	72·70 ± ·19	—	—	—	—	—	—	—	—	—	—	—	—
(β) 100 B/L ...	130	72·99 ± ·17	42	71·78	169	75·07	59	77·27	39	72·87	100	83·20	88	76·5
(γ) 100 H/L' ...	98	73·20 ± ·18	—	—	—	—	—	—	—	—	—	—	—	—
(δ) 100 H/L ...	131	73·30 ± ·16	34	72·60	169	74·71	59	77·46	39	74·15	99	74·20	88	75·6
(ε) 100 H/B ...	131	100·47 ± ·28	34	101·18	169	99·64	59	100·56	39	101·75	—	—	—	—
(ζ) 100 B/H ...	131	99·76 ± ·27	34	98·84	169	100·36	59	99·44	39	98·28	99	89·10	88	98·8
(η) 100 GH/GB	25	117·06 ± ·72	—	—	148	112·20	23	120·74	23	113·02	—	—	49	115·9
(θ) 100 $G'H/GB$	76	70·63 ± ·35	—	—	155	72·32	55	72·09	27	70·37	48	74·50	65	68·4
(κ) 100 NB/NH	77	51·08 ± ·32	25	50·03	166	50·07	58	49·36	27	55·43	70	48·70	78	50·7
(λ) 100 O_2/O_1; L	77	74·89 ± ·39	27*	86·00	169*	86·54	59	85·23	27	87·20	71*	84·5	80*	85·3
(λ') 100 O_2/O_1; R	76	74·87 ± ·39												
(μ) 100 G_2/G_1 ...	67	71·94 ± ·61	—	—	133	88·51	47	85·02	25	81·52	47	74·4	72	72·1

* One eye only taken and which not stated.

† From measurements on Theban mummies of the Mook Collection at Munich.

TABLE V^{b}. *Comparison of Naqada Means with those of other Races.*

FEMALE.

Character and Reference Letter	Naqada and allied Races								Unallied Races			
									Advanced		Primitive	
	Naqadas		Maciver's Prehistoric Egyptians		Theban Mummies		Modern Egyptians		'Altbayerisch' (Modern German)		Aino	
	No.	Mean	No.	Mean	No.	Mean	No.	Mean	No.	Mean	No.	Mean
(*a*) *C*	123	1287·9 ±5·4	—	—	74	1250·27	27	1213·96	100	1335·50	51	1308
(*b*) *F*	168	177·41 ± ·29	57	177·46	—	—	—	—	—	—	—	—
(*c*) *L*	185	177·47 ± ·28	58	177·90	77	174·81	27	171·41	—	173·45	62	177·2
(*d*) *L′*	159	177·89 ± ·29	—	—	—	—	—	—	—	—	62	176·2
(*e*) *B*	185	131·50 ± ·23	58	132·12	77	134·31	27	131·07	100	143·98	63	136·8
(*f*) *B′*	181	88·23 ± ·20	—	—	77	91·40	27	91·70	83	96·30	62	92·4
(*g*) *H*	169	129·47 ± ·25	49	128·33	77	130·51	27	130·30	96	128·01	62	135·1
(*h*) *OH*	174	113·11 ± ·20	—	—	76	109·75	26	109·27	100	114·17	63	115·0
(*i*) *LB*	141	94·86 ± ·25	45	97·64	76	95·29	26	96·00	90	95·80	60	100·3
(*j*) *U*	146	493·74 ± ·64	—	—	77	494·75	27	485·11	99	501·40	62	501·7
(*k*) *S*	151	363·64 ± ·70	—	—	76	357·93	27	352·00	90	353·40	53	360·7
(*l*) *Q*	151	296·49 ± ·46	—	—	77	296·21	27	295·63	97	318·70	61	317·1
(*m*) *GH*	27	109·92 ± ·74	—	—	72	107·43	12	104·00	—	—	21	112·1
(*n*) *G′H*	119	65·84 ± ·28	42	67·40	74	65·96	26	63·50	66	66·80	41	65·5
(*o*) *GB*	113	92·51 ± ·28	—	—	71	91·61	26	92·88	66	89·70	46	96·7
(*p*) *J*	63	117·04 ± ·47	42	119·55	77	121·22	26	120·62	67	126·30	44	129·7
(*q*) *NH*	123	46·73 ± ·19	46	47·67	74	48·36	26	45·31	72	48·20	46	47·4
(*r*) *NB*	119	24·31 ± ·11	46	23·86	72	24·21	26	24·62	72	23·70	45	24·7
(*s*) O_1L	118	41·70 ± ·14	*46	37·22	*73	37·04	*26	36·85	*79	38·32	*49	39·8
(*s′*) O_1R	113	41·30 ± ·14										
(*t*) O_2L	117	32·16 ± ·13	*46	32·22	*75	32·25	*26	32·50	*79	33·18	*50	33·9
(*t′*) O_2R	116	31·87 ± ·14										
(*u*) G_1	103	53·89 ± ·26	—	—	69	44·13	25	46·08	57	42·20	38	51·4
(*v*) G_2	103	38·87 ± ·22	—	—	69	38 67	25	39·04	55	32·10	46	37·4
(*w*) *GL*	106	91·02 ± ·30	45	93·69	74	91·93	26	94·96	—	—	43	100·1
(*x*) W_1	52	106·37 ± ·67	—	—	69	112·85	12	106·50	—	—	38	95·6
(*y*) W_2	59	87·62 ± ·55	—	—	70	89·2	12	86·75	—	—	—	—
(*z*) h_1	75	31·61 ± ·21	—	—	—	—	—	—	—	—	32	32·6
(*z′*) *f*	60	43·12 ± ·20	—	—	—	—	—	—	—	—	—	—
(*aa*) *P*∠	89	84°·49 ± ·26	—	—	28	85°·57†	—	—	61	88°·8	41	81°
(*bb*) *A*∠	93	72°·62 ± ·22	—	—	—	—	—	—	—	—	—	—
(*cc*) *N*∠	93	66°·40 ± ·31	—	—	—	—	—	—	—	—	—	—
(*dd*) *B*∠	93	40°·98 ± ·19	—	—	—	—	—	—	—	—	—	—
(*ee*) θ_1	89	29°·09 ± ·25	—	—	—	—	—	—	—	—	—	—
(*ff*) θ_2	89	11°·82 ± ·26	—	—	—	—	—	—	—	—	—	—
(α) 100 *B*/*L′*	146	73·86 ± ·16	—	—	—	—	—	—	—	—	—	—
(β) 100 *B*/*L*	169	74·19 ± ·16	58	74·27	77	76·53	27	76·61	100	83·10	62	77·6
(γ) 100 *H*/*L′*	141	73·11 ± ·16	—	—	—	—	—	—	—	—	—	—
(δ) 100 *H*/*L*	166	73·22 ± ·16	49	72·14	77	74·69	27	76·09	96	73·90	61	76·7
(ε) 100 *H*/*B*	169	98·66 ± ·25	49	97·13	77	97·26	27	99·50	—	—	—	—
(ζ) 100 *B*/*H*	163	101·59 ± ·25	49	102·96	77	102·81	27	100·50	96	88·80	62	98·8
(η) 100 *GH*/*GB*	22	118·87 ± ·97	—	—	71	112 75	12	113·43	—	—	21	115·9
(θ) 100 *G′H*/*GB*	106	70·36 ± ·27	—	—	71	72·27	26	68·54	58	74·40	60	67·7
(κ) 100 *NB*/*NH*	113	52·31 ± ·31	46	50·14	72	50·23	26	54·58	72	49·20	44	50·5
(λ) 100 O_2/O_1; *L*	111	76·91 ± ·29	*46	87·04	*73	89·81	*26	87·69	*79	86·60	48	85·2
(λ′) 100 O_2/O_1; *R*	113	77·32 ± ·48										
(μ) 100 G_2/G_1	97	72·30 ± ·41	—	—	60	88·34	24	84·40	51	76·00	38	72·8

* One eye only taken and which not stated.

† From measurements on Theban mummies of the Mook Collection at Munich.

of Africa. They were copied from the MSS. of Broca in Paris for Professor Pearson through the courtesy of M. Manouvrier.

To these allied races the data for two other races have been added.

(*e*) Ranke's measurements on "Altbayerisch" skulls from his *Beiträge zur physischen Anthropologie der Bayern*, Bd. I.

(*f*) Koganei's measurements on Aino skulls. *Beiträge zur physischen Anthropologie der Aino, Mitteilungen aus der medicinischen Facultät der k. j. Universität.* Tokio, 1894, Bd. II.

We are thus able to compare the Naqadas with both a highly civilised modern race and an extremely primitive type.

TABLE VI.

*Modern Negroes**.

Character and Reference Letter	Male		Female	
	No.	Mean	No.	Mean
(*a*) C ...	54	1430	23	1256
(*c*) L ...	54	185·04	23	174·52
(*e*) B ...	54	135·20	23	130·52
(*g*) H ...	54	134·77	23	126·91
(β) $100B/L$	54	73·28	23	74·85
(δ) $100H/L$	54	72·83	23	72·75

Before we discuss the bearing of these tables we must refer to one or two matters with regard to the general measurement of cranial characters. It must first be observed that it is extremely difficult to fix a conventional measurement by a printed statement. The skull is not a system of geometrical points, and a personal view as to what is to be taken as the "point" may quite well bias one way or the other a measurement by one or more millimetres. For example in measurements made from the German horizontal plane, one investigator may take it from the auricular axis and the left eye, another from the right eye and a third from what he considers the mean of the two. These differences will easily lead to as much as 1° difference in its position. Thus the personal equation is far more important in the measurement of some characters than the errors of random sampling, even though our samples are small. We cannot safely conclude as to racial difference from differences greater than those of random sampling, when we are treating certain organs of which the measurement is difficult of exact definition.

---

* From Broca's MSS. measurements at Paris. Chiefly but not entirely from Algiers, the Soudan and the West Coast of Africa. The capacities are as in the case of all Broca's measurements exaggerated. If we modify them by the reduction suggested in *Archiv f. Anthropologie*, Bd. XIII. Suppl. S. 78 we find Negro ♂ = 1347, ♀ = 1175, values much nearer to those given in Table V. for the modern Negro.

One of the worst of such measurements is the palate length G_1. According to the original version of the Frankfurt Concordat this was to be measured from the point of the spine to the inner wall of the alveolar rim between the middle incisors. This is the measurement we attempted to make. We believe it is also the measurement attempted by Koganei on the Aino. Now the spine has frequently been broken or worn away and accordingly the measurement cannot always be made. Hence the Germans have begun to take the palate length from the *base* of the spine, which is not in agreement with their own Concordat. Further, what is "the inner wall of the alveolar rim between the middle incisors"? The inner wall runs in and almost encircles the teeth and its fine edge is very generally broken in on ancient skulls. Accordingly "the inner wall between the middle incisors" has recently come to be taken as the mid-point of a line tangential to the tops of the inner wall on the two incisors*. An examination of the bases of the skulls in our photographs will show what a great difference this interpretation of the original Concordat makes. The major part of the divergence of G_1 for Naqada from G_1 for the Theban Mummies and modern Egyptians is solely due to this change of convention and not to racial differences.

Dr W. R. Macdonell has kindly made a series of measurements on this point. First he has dealt with the length of spine and finds the following results:

TABLE VII.

Length of Spina nasalis posterior.

ENGLISH SKULLS			NAQADA SKULLS		
Number	Sex	Mean	Number	Sex	Mean
19	♂	3·55 mm.	20	♂	5·05 mm.
17	♀	3·19 mm.	18	♀	4·00 mm.

There is thus a very sensible difference between the length of spine in English and prehistoric Egyptian skulls, and further the omission or inclusion of the spine is a matter in the average of 3 to 5 mm. difference in the measurements. In the next place Dr Macdonell undertook a series of measurements (*a*) from the tip of the spine to the inner wall of the alveolar margin between the middle incisors and (*b*) from the tip of the spine to the niveau of the inner alveolar margin on the middle incisors.

---

* See E. Schmidt: *Anthropologische Methoden*, S. 238. He takes, however, the "Basis dieser Spina nasalis posterior" to be the direction for measurement of the Frankfurt Concordat, which is precisely opposite to the "Spitze der Spina" in the printed versions of the Concordat kindly provided by Professor Ranke. Further Dr Schmidt prints *Basis* for the original *Spitze* in the version of the Concordat he gives in an appendix without commenting on the change.

He found for the Naqada skulls:

Mean difference 20 ♂ skulls 4·87 mm.
" " 18 ♀ " 4·02 mm.

Hence for the Naqada skulls we have the following results:

Mean reduction in length of palate due to measuring by E. Schmidt's convention instead of in strict accordance with the wording of the Frankfurt Concordat

= 9·92 mm. for males, and = 8·02 mm. for females.

Hence to compare our measurements on the Naqada skulls with Schmidt's on the Theban Mummies, with those on the modern Egyptians and probably with those on the Negroes, we must subtract the above quantities from the mean values of G_1 in Table V. We find:

TABLE VIII.

Palate Measurements on Naqada Skulls.

	Length of Palate (G_1)	Palate Index ($100G_2/G_1$)
♂	45·88	87·90
♀	45·87	84·74

Comparing these results with those given in the table it will be seen: first, that the chief sexual difference in the Naqada skulls depends upon the length of spine; and secondly, that the palate measurement no longer forms a character by which we can definitely distinguish between prehistoric and historic Egyptians. For, the breadths of the palate are sensibly the same in both sexes for Naqadas, Thebans and Copts. In the lengths we have for males: Naqadas 46, Thebans 46, Copts 47, and for females: Naqadas 46, Thebans 44, Copts 46. No weight can safely, considering the numbers dealt with, be put on these somewhat irregular slight differences, nor on the resulting differences in the palate index.

Another very questionable measurement is the breadth of the orbit O_1. The Frankfurt Concordat runs: "Von der Mitte des medialen Randes der Augenhöhle bis zum lateralen Rand der Augenhöhle." This reads as if the medial rim were a perfectly definite margin which is very rarely the case. E. Schmidt has got over the difficulty by taking a more or less definite point for the nasal end of the measurement. He writes: "Als Maasspunkt am inneren Rande ist die Stelle zu wählen, wo die hintere Kante der Thränengrube oben mit Stirn-Thränenbeinnaht zusammentrifft (*Point lacrymal* der französischen Schule)."[*]

This is fairly, but not universally definite. But it by no means necessarily gives the "grösste Breite des Augenhöhleneinganges," whatever that may be, of the Frankfurt Concordat.

---

[*] *Anthropologische Methoden*, S. 236.

Ranke admits* the difficulty as to the position of the callipers point on the nasal side and draws: "eine Verbindungslinie zwischen den stets deutlichen inneren Ausläufern des oberen und unteren Augenhöhlenrandes." He further states that "über den wahren Verlauf dieser Verbindungslinie kann ein ernsthafter Zweifel niemals bestehen." Unfortunately with the experience of the Naqada skulls before us, we cannot fully agree in this opinion. Is the "Verbindungslinie" to be a geodesic between the last sensible points of the upper and lower orbital rims, or is it to be a curve on the nasal wall following the general curvature indicated by the sweep of the orbital rims? The difference in the breadth of the orbit with one or other of these interpretations will frequently amount to several millimetres. Ranke probably had one or other of these ideas clearly in his mind—he does not say which—and so passed over the other. Intermediate stages between the two are possible and thus the personal equation again looms largely in the background. In our measurements an attempt was made to follow the curvature of the rim, and not to form a geodesic. The result is that the breadth of the Naqada orbit comes out larger and the orbital index less than those of the Thebans or Copts which were measured probably by Schmidt's convention.

Dr W. R. Macdonell kindly measured 40 ♂ orbits and 30 ♀ orbits for the Naqadas by what we may term the 'geodesic' method for comparison with those obtained by the 'curvature' method. The results show: first, that the difference is in both sexes greater for the left than for the right eye; secondly, it averaged about 1·9 mm. for male and 1·6 mm. for female eyes. Taking the average of both orbits, this would reduce our measurements as follows:—

Maximum Breadth of Naqada Orbit O_1.

♂ 40·8 mm. ♀ 39·9 mm.

These are practically identical with the values given by Koganei for the Aino, but still differ sensibly from the values for the Thebans and Copts. There is no doubt, however, that these were measured from the "lacrymal point." Maciver does not tell us how he measured the breadth of his orbits, but just as our measurements for the height of the Naqada orbit agree perfectly with his, and with those for Thebans and Copts, his for the breadth are also in perfect agreement with those for Thebans and Copts. We think we may therefore conclude that so far from there existing any sensible distinction between the shape or size of the orbit between prehistoric and historic Egyptians, there really is absolute identity if the orbit be measured in the same manner.

We are now in a better position to look through Table V. and compare our allied races. We turn first to the Naqada of 6000 B.C., the Thebans of 1500 B.C. and the Copts of to-day, three races dividing an interval of about 7000 to 8000 years into two periods of 3000 to 4000 years. It is impossible not to be impressed at once with the striking likenesses between these three groups, especially when we notice how small are the number of skulls upon which occasionally (as for

---

* *Beiträge zur physischen Anthropologie der Bayern*, Bd. I. *Die Bildung der Augenhöhlen*, S. 94.

modern ♀ Copts) the means are based. Compare for example the ♂ Naqadas in capacity (C), horizontal circumference (U), sagittal circumference (S), vertical circumference (Q), breadth of face (GB), nasal height (NH), nasal breadth (NB), orbital breadth* (O_1), orbital height (O_2), palate length (G_1)†, palate breadth (G_2), profile length (GL), profile angle (P), height-breadth index (100 H/B), nasal index (100 NB/NH), orbital index (100 O_2/O_1), and palate index (100 G_2/G_1), with the like quantities for the Thebans, and it seems impossible to hold that we are dealing with really different races or even with a mixture of races. Evidence in the same direction if less definite may be drawn from the auricular height (OH), the skull-base (LB), the height-breadth index (H/B) and having regard to Maciver's measurements and those on the Copts, the zygomatic breadth (J).

Turning to the females we reach much the same results; in several cases where the Naqada appear to differ slightly from the Thebans, they are seen to be intermediate between the latter and the Copts or Maciver's measurements show us how a slight difference in sample or method of measurement brings Naqadas into line with Thebans or Copts or both. The Germans and Aino, and in many cases the Negroes show us how widely divergent different races can be from a group like the Naqada-Theban-Copt series. Compare for example the Germans and Aino with the Egyptian group in ♂ skull circumferences (U, S, Q) and the Negroes and the Egyptian group in nasal breadth, orbital height, length of profile, or palate index. In some respects, indeed, the *modern* Negro is closer to the later Egyptians, Thebans or Copts, than to the Naqadas. He stands on the whole nearer to the Egyptian group than to races like the German or Aino, but it seems impossible to assert that he is closer to the Naqadas than to the later Egyptians. If the historic Egyptians are to be treated as distinct from the Negroes, then certainly the Naqadas are. Skulls of a negroid type may be found among our material, and possibly competent type-craniologists would have cast them out at once, but our general averages are quite sufficient to show that we are dealing with distinct races, and one which 6000 or 7000 years ago was as distinct from the Negro as it is to-day.

If we accept the general proposition that the Naqadas, Thebans and Copts are for a number of characters so closely related that we are bound to consider them as in bulk the *same* stock, we are still forced to the conclusion that in certain characters a progressive evolution has taken place, for these characters have substantially changed. If it be asserted that the change is due to racial mixture, then the fact that other characters have remained practically stationary is very difficult of interpretation, for the result of mixture would be to alter these also. On the other hand, the comparatively small correlations of the parts of the skull‡ show that if certain characters in the skull be modified by selection, the change in other characters need not be significant, if the latter have not themselves been directly selected. The most noteworthy of these changing characters are the

---

* See remarks, p. 431.

† See remarks, p. 430.

‡ See the section on correlation below.

decreasing length (L), the increasing breadth (B), the increasing frontal breadth (B'), the increasing auricular height (OH) and the increasing total facial height (GH) for the males. For the females the Copt crania are so few in number that they are scarcely of value for comparative purposes. Still the main changes—decrease in length and increase in breadth—are well marked between Naqadas and Thebans. These changes appear again in the progressively increasing values of the two indices B/L and H/L.

The following table shows the most significant changes and places for comparison alongside them changes that have gone on within England itself. From these results, we think, it must be concluded that the changes between prehistoric, historic and modern Egyptians, even when they are greatest, are not greater than we find between different classes of the same community, or between members of the same community at a few centuries interval. We are not therefore compelled to assume that admixture of races to which the "fixed-type" craniologist is always appealing. The supposition of an intraracial evolution, tending in both Egyptian and English cases in the same direction—conveniently if not quite exactly described as towards increased brachycephaly—is the most reasonable view of the facts.

TABLE IX.

*Male Crania.**

Character	Naqadas	Thebans	Copts	17th Century English	Modern English	
					Criminal	Upper Class
Capacity ...	1381	1388	1355	1522	[1378]†	[1431]†
Length ...	185·13	181·94	179·11	187·65	180·44	182·51
Breadth ...	134·87	136·63	136·51	140·00	139·30	142·96
Cephalic Index	72·99	75·07	77·30	74·73	77·23	78·33

But if changes in the mean length of the skull of 3 to 7 mm., in the mean breadth of the skull of 2 to 3 mm. and in the cephalic index of 3 to 4 points can arise in periods of 300 to 6000 years, it seems possible that a period of 100,000 years with evolution working only at the same rate would suffice to have modified the skull from a form which the craniologist would hesitate to term human. In other words the pace of evolution may be far greater than has hitherto been suggested. The problem of the evolutionist is not to show that at all times all characters in all species are rapidly changing, but that at some times some species are changing at a rate which on the whole would suffice to bring the biological record into synchronism with geological limits.

---

[*] Measurements on Prof. Thane's 17th century skulls by A. Martin Leake and G. U. Yule. For the modern English: see Macdonell, *Biometrika*, Vol. I. p. 190.

[†] Deduced from Dr A. Lee's formula, *Phil. Trans.* Vol. 196, A, p. 252.

We may turn now to the actual means for the Naqada race and give these numbers their classes according to the conventions of the *Frankfurter Verständigung.*

TABLE X.

Specification of Naqada Race.

Character	Class		Remarks
	♂	♀	
(β) $100B/L$	Dolichocephaly ...	ditto	Female significantly less dolichocephalic
(γ) $100H/L$	Orthocephaly ...	ditto	Sexes equally orthocephalic
(aa) Profile Angle	Mesognathy ...	Mesognathy	Sexes alike
(η) Facial Index	Narrow faced ...	ditto	Sexes practically alike
(θ) Upper Face Index ...	Narrow upper face	ditto	Sexes alike
Zygom. Face Index* ...	Chamaeprosopy ...	Leptoprosopy	♂ index = 89·17 ; ♀ index = 93·92†
Zygom. Upper Face Index*	Leptoprosopy ...	Leptoprosopy	♂ index = 53·80 ; ♀ index = 56·25‡
(λ) Orbital Index	Chamaeconchy ...	Chamaeconchy	Actual Frankfurt Concordat results as in Table V. Female sensibly less so than male
ditto	Hypsiconchy ...	Hypsiconchy	Maciver-Schmidt method of measurement. Female differs sensibly from male
(κ) Nasal Index	Platyrrhiny ...	Platyrrhiny	The male is on the borders of mesorrhiny and differs sensibly from female
(μ) Palate Index	Leptostaphyline ...	Leptostaphyline	Actual Frankfurt Concordat results as in Table V. Sexes alike
ditto	Brachystaphyline ...	Mesostaphyline	Schmidt's convention as in Table, p. 430. Sexes sensibly different
Alveolar Index*§ ...	95·35	95·95	Sexes practically alike

We are dealing therefore with a long-headed narrow-faced race with a flat nose and rather round orbits. Comparing the sexes we note that male and female are significantly different in cephalic and zygomatic face indices, further in the shapes of the orbit and of the nose; or greater tendencies to brachycephaly, leptoprosopy, hypsiconchy, and platyrrhiny seem secondary sexual characters in the female. These results receive confirmation from a sexual comparison of the

---

* A formula for calculating the index from the means of the lengths is given by Pearson: *R. S. Proc.* Vol. LX. p. 492, but the corrective terms modifying the simple ratio of the means are insensible to two places of decimals in the present cases.

† For Ainos zygomatic face index for ♂ = 86·2, for ♀ = 86·4.

‡ For "Altbayerisch" skulls zygomatic upper face index for ♂ = 52·4, for ♀ = 52·8, and for Ainos for ♂ = 50·8, for ♀ = 50·5.

§ The alveolar index, $= 100GL/LB$, is not considered and classified in the *Frankfurter Verständigung*, so the actual values are here given.

Q. 392

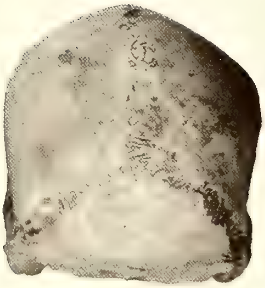
T. 10^{B}

T. 29^{A}

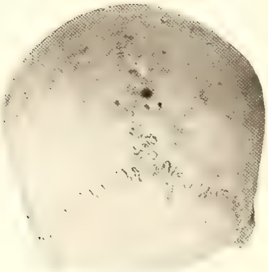
Q. 392

T. 10^{B}

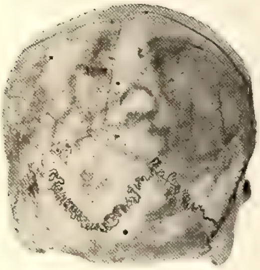
T. 29^{A}



Q. 392



T. 10^{B}



T. 29^{A}

Male Naqada Crania.

Plate VI.

Q. 758

530

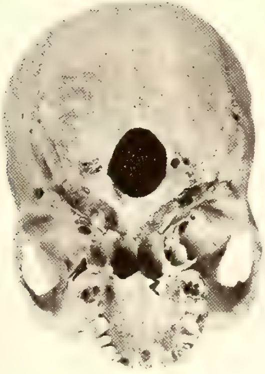
1755

Q. 758

530

1755

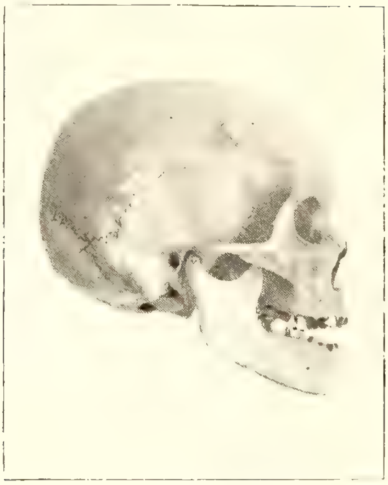
Q. 392

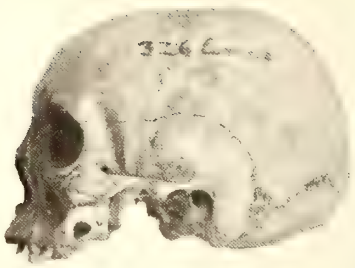


T. 10^B

T. 10^B

Male Naqada Crania.


1308

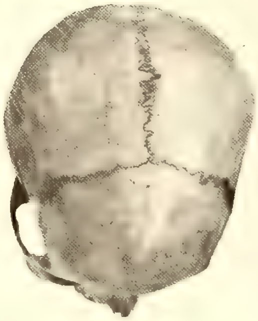

Q. 326 Lower


1308


Q. 326 Lower

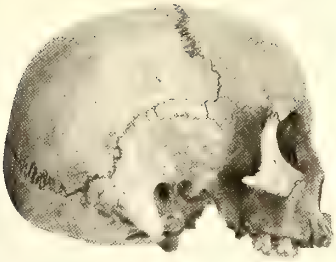

R. 2


Q. 326 Lower

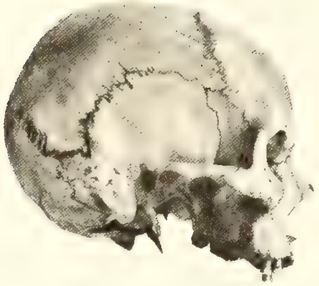

R. 2

Female Naqada Crania.

Plate VIII.



Q. 408^{D}

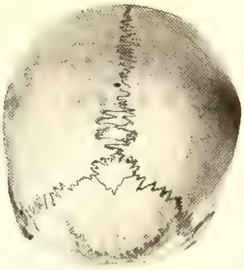


R. 2

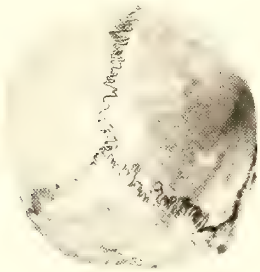
Q. 326 Lower

Q. 408^{D}

R. 2

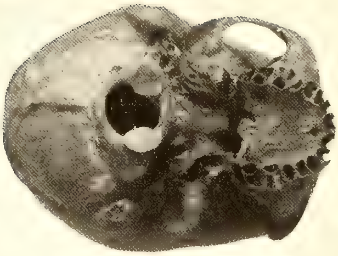


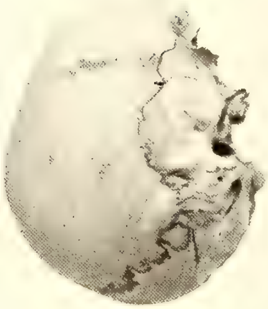
Q. 408^{D}

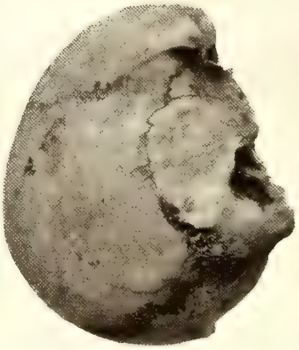


R. 2

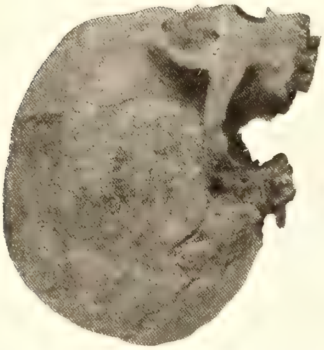
Female Naqada Crania.

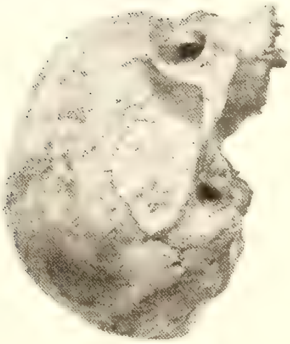

1446


Q. 359^2


Q. 383


R. 5


1377

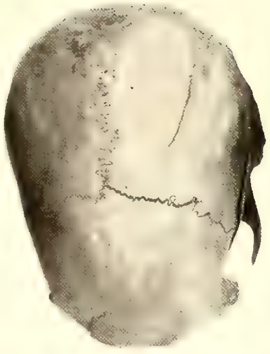

1300


Q. 791


1644

Abnormal and Special Naqada Crania.

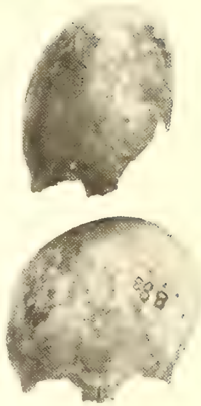
Plate X.



1587^4

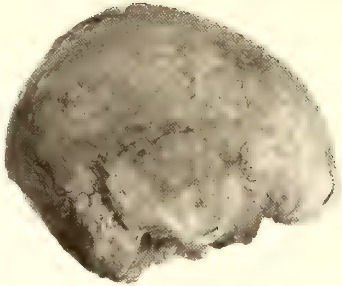
Q. 18

1031

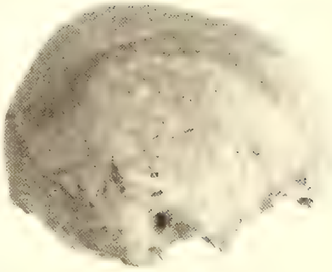


B. 83

1587^1



1587^4



Q. 513

T. 5^F

Abnormal and Special Naqada Crania.

same characters in the other Egyptian series. Comparing the German measurements we do not find any sensible sexual differences in head, face or nose indices, but there are such differences in the orbital and palate indices and in the same directions as in the Egyptian series. In the Ainos it is only in brachycephaly that the women show sensible difference from the men. Hence we cannot assert that the secondary sexual characters of one race are those of all races.

The platyrrhine character of the nose and the leptostaphyline form of the upper jaw together with the values for the auricular height (OH) and the cross-circumference (Q) seem not entirely consonant with what Professor Petrie tells us of the portraiture* (see p. 412 above). It is possible that this portraiture was to some extent conventional, or that it in fact gives some other type than the Naqada. Still the crania agree with the portraiture in not giving a markedly primitive type. There is no trace of "neanderthaloid" skulls in the collection. This is more or less confirmed by the probable Naqada stature, which is well above that of continental neolithic or palaeolithic man, above the French and South German commonality of to-day, just above the English criminal classes and only 2 to 3 cms. below the upper-class English. Its nearest equivalent is the modern Nubian†. There is nothing, whatever, which would lead us to believe that we are dealing with a markedly primitive type, although occasionally primitive characteristics, such as a markedly curved dental arch or strongly emphasised upper temporal lines, may be found among the material. A very fair notion of the cranial characters in general may be obtained from Plates V—VI, which give typical male skulls, Plates VII—VIII which give typical female skulls and Plates IX—X which reproduce special features of anatomical or pathological interest‡.

These plates, together with the measurements, will enable the reader to confirm the views above expressed, i.e. (i) that the Naqadas are not a race of markedly primitive character, (ii) nor very much nearer to the Negro than the historic Egyptians or the Copts of to-day. It will need a far more comprehensive study of modern and ancient Negro crania than has yet been made to see in its due proportions this Negro and Egyptian relationship. Meanwhile our study of the skulls seems to be in complete agreement with Dr Warren's conclusions as drawn from the long bones:

> "Here then in the New Race we have a hardy and vigorous people, as shewn by the pronounced pilastre of the femur and the platycnemia of the tibia. Just as is observed in so many races, in

---

* Prof. Petrie's description of the portraiture undoubtedly seems to refer to a race differing far more from the historic Egyptians than the cranial measurements of the Naqadas appear to allow for.

† Pearson: *Phil. Trans.* Vol. 192, A, pp. 211 and 243.

‡ We owe the photographs from which these plates were prepared to the patience and care of Mr D. Radford Sharpe, to whom our most hearty thanks are due. The skulls were placed on cups on a horizontal table and the camera vertically above them, and sensibly at the same distance from them all. To avoid increasing the very heavy labour—in the dull weather in which it was necessary to carry out the work, some exposures required upwards of 30 minutes—two and sometimes three skulls were taken on the same plate. This will suffice to explain why the adjustment in some pairs seems at fault—the right and left-hand skulls, even if placed truly horizontal, were seen from rather different relative standpoints.

some characters the New Race was advanced or modern; in others it was inferior or primitive. On the whole, the proportions of the limb-bones to one another may be said to have approached those of the Negro, while the sacral and scapular indices were almost identical with those of Europeans*."

In total height and auricular height of skull, in height and breadth of face, in nasal height, in cephalic index and upper face index the Naqadas approached the modern Negro. But in nasal breadth, height of orbit, palate length and nasal index they are closer to the Germans. In length of skull, sagittal circumference, facial index, breadth-height ratio and nasal index they are, perhaps, closest to a primitive race like the Aino. Thus the cranial results fully bear out the judgment from the long bones, that the race was in some characters advanced or modern, in others inferior or primitive.

(8) *General points.*

Of other points to which attention may be drawn in our general table of means we note:

(*a*) The mean male skull with the possible exception of the breadth of the orbit is significantly larger than the mean female skull.

(*b*) While the height of the male skull appears to be significantly greater than the breadth, that of the female seems significantly less. (Cf. (ϵ) and (ζ) in Tables V^a and V^b.)

(*c*) The left orbit for both measurements in both sexes appears larger than the right orbit. Thus the orbit gives the advantage to the left, which agrees with the predominating side of the skeleton found by Dr Warren in the case of femur, tibia and fibula, whereas in the case of humerus, radius and ulna the right-hand side was the larger†.

(*d*) The advantages of measuring Flower's length, the maximum length, and the horizontal length (F, L and L'), are seen to be remarkably small. When the sample numbers as many as 160 to 200 skulls—a number very rarely reached in craniological investigations, the differences between F, L and L' are not beyond the limits of random sampling for female skulls, and the difference between F and L' scarcely beyond these limits for male skulls‡. The indices calculated for L and for L' are sensibly identical within the limits of random sampling, and it is accordingly for all purposes of racial comparison and relationship not worth the labour to measure these three lengths separately. One or other may be from the anatomical standpoint a better measurement to make, but the craniologist who knows either F,

---

* *Phil. Trans.* Vol. 189, B, p. 191. An Investigation of the Variability of the Human Skeleton with special reference to the Naqada Race.

† *Phil. Trans.* Vol. 189, B, pp. 159 *et seq.* The right is the larger side also in the case of the hand: see Whiteley and Pearson, *R. S. Proc.* Vol. 65. p. 129 and Lewenz and Whiteley, *Biometrika*, Vol. I. p. 348.

‡ The results would probably have been still closer, if the means had been struck for the same skulls.

L or L' knows the other two for his population as far as the errors of random sampling allow him to trust his measurements*. Of course the result might be different could we deal with 500 to 1000 crania of one sex and race, but there is small hope of such numbers in the majority of cases. It is inexpedient, while there is so much to be done, to multiply measurements which differ by quantities of the order of the probable error of their determination†.

(9) *On the Variability of the Naqada Race.*

Before we consider the actual frequency distributions and the graphical graduations of some of the more important characters, it may be well to examine the numerical values of the variations as a whole. These are given with the probable errors in Table XI.

We have here for the first time a moderately extensive table of the variability of the characters of the human skull, and further the probable errors are given in each case so that the significant differences can be discriminated. The misfortune is that there is so little material available for comparative racial purposes. Only one or two organs have so far had their variability measured for anything like a long series of races. The capacity of the skull has been fairly completely dealt with by Pearson‡. We extract the following coefficients of variation for comparison:

	♂	♀		♂	♀
17th Century English	7·68	8·15	Etruscans	9·58	8·54
Parisian French	7·36	7·10	Egyptian Mummies	8·13	8·29
Italians	8·34	8·99	Naqadas	7·72	6·92
Germans	7·74	8·19	Negroes	7·07	6·90
Ainos	6·89	6·82	Modern Egyptians	8·59	7·17
Low Caste Panjabis	7·24	8·99			
Polynesians	8·20	5·55			
Kanakas	7·37	6·68			
Andamanese	5·04	5·59			

It will be seen at once that the Naqadas show no exceptional variability, but have their men and women somewhat less equal in variability than is the case with the Egyptian mummies or the Negroes. The Naqada values lie between those of the Negroes and those of the Egyptians but they are not, except in the case of the women to the negresses, really close to either.

---

* This point had been already demonstrated for the cephalic index, see Pearson: *The Chances of Death*, Vol. I. p. 270 footnote.

† Thus although L should generally be greater than L' and $100B/L$, $100H/L$ less than $100B/L'$, and $100H/L'$, still the difference of the samples on which L or L' are taken is quite sufficient in itself to upset such relations. Of course it is only possible to get L' when both auricular passages and one or other eye-socket remain perfect; thus we can usually find L for more skulls than L'. I should therefore be inclined to suggest that F and L' should be dropped in future systems of measurement.

‡ *The Chances of Death*, Vol. I, pp. 328—349. The short series of Naqada skulls given on p. 339 of that work must now be replaced by the complete series given in this paper.

TABLE XI.

Variability of the Naqada Race.

Character	Male			Female		
	Mean	Standard Deviation	Coefficient of Variation	Mean	Standard Deviation	Coefficient of Variation
C	1380·99 ± 7·67	106·68 ± 5·42	7·72 ± ·39	1287·91 ± 5·42	89·18 ± 3·84	6·92 ± ·30
F	183·96 ± ·37	6·03 ± ·26	3·28 ± ·14	177·41 ± ·29	5·59 ± ·21	3·15 ± ·12
L'	184·87 ± ·37	5·99 ± ·26	3·24 ± ·14	177·89 ± ·29	5·51 ± ·21	3·10 ± ·12
L	185·13 ± ·33	5·75 ± ·23	3·17 ± ·13	177·47 ± ·28	5·57 ± 20	3·14 ± ·11
B	134·87 ± ·26	4·60 ± ·19	3·29 ± ·13	131·50 ± ·23	4·55 ± ·16	3·45 ± ·12
B'	91·06 ± ·27	4·82 ± ·19	5·29 ± ·21	88·23 ± ·20	3·94 ± ·14	4·47 ± ·16
H	135·21 ± ·31	5·38 ± ·22	3·98 ± ·16	129·47 ± ·25	4·74 ± ·17	3·66 ± ·13
OH	115·59 ± ·25	4·46 ± ·18	3·86 ± ·16	113·11 ± ·20	4·00 ± ·14	3·54 ± ·13
LB	99·34 ± ·31	4·85 ± ·22	4·88 ± ·22	94·86 ± ·25	4·44 ± ·18	4·68 ± ·19
U	511·02 ± ·81	13·00 ± ·57	2·54 ± ·11	493·74 ± ·64	11·47 ± ·46	2·27 ± ·09
S	373·02 ± ·74	11·91 ± ·52	3·19 ± ·14	363·64 ± ·70	12·77 ± ·50	3·51 ± ·14
Q	304·22 ± ·63	10·11 ± ·45	3·32 ± ·15	296·49 ± ·46	8·07 ± ·33	2·72 ± ·11
GH	112·02 ± ·76	5·97 ± ·54	5·33 ± ·48	109·92 ± ·74	5·68 ± ·52	5·17 ± ·18
$G'H$	67·59 ± ·30	4·11 ± ·21	6·08 ± ·31	65·84 ± ·28	4·47 ± ·20	6·87 ± ·30
GB	95·85 ± ·37	4·97 ± ·26	5·18 ± ·27	92·51 ± ·28	4·41 ± ·20	4·77 ± ·21
J	125·63 ± ·48	5·22 ± ·34	4·16 ± ·27	117·04 ± ·47	5·58 ± ·34	4·77 ± ·29
NH	48·94 ± ·21	3·00 ± ·15	6·13 ± ·31	46·73 ± ·19	3·18 ± ·14	6·81 ± ·29
NB	25·12 ± ·14	1·98 ± ·10	7·89 ± ·41	24·31 ± ·11	1·80 ± ·08	7·28 ± ·32
O_1L	43·11 ± ·16	2·14 ± ·11	4·97 ± ·26	41·70 ± ·14	2·21 ± ·10	5·30 ± ·24
O_1R	42·60 ± ·16	2·14 ± ·11	5·02 ± ·27	41·30 ± ·14	2·22 ± ·10	5·38 ± ·24
O_2L	32·67 ± ·17	2·31 ± ·12	7·06 ± ·38	32·16 ± ·13	2·12 ± ·09	6·58 ± ·29
O_2R	31·87 ± ·18	2·32 ± ·13	7·27 ± ·41	31·87 ± ·14	2·18 ± ·10	6·85 ± ·38
G_1	55·80 ± ·28	3·62 ± ·20	6·49 ± ·36	53·69 ± ·26	3·98 ± ·19	7·41 ± ·35
G_2	40·33 ± ·29	3·75 ± ·21	9·29 ± ·51	38·87 ± ·22	3·32 ± ·16	8·55 ± ·40
GL	94·72 ± ·34	4·58 ± ·44	4·84 ± ·26	91·02 ± ·30	4·63 ± ·21	5·09 ± ·24
W_1	110·49 ± ·87	8·24 ± ·61	7·46 ± ·56	106·37 ± ·67	7·20 ± ·48	6·77 ± ·45
W_2	93·58 ± ·67	7·13 ± ·48	7·62 ± ·51	87·62 ± ·55	6·30 ± ·39	7·19 ± ·45
h_1	32·91 ± ·28	3·27 ± ·20	9·93 ± ·62	31·61 ± ·21	2·68 ± ·15	8·47 ± ·47
f	44·44 ± ·25	2·55 ± ·17	5·73 ± ·39	43·12 ± ·20	2·29 ± ·14	5·31 ± ·33
$P\angle$	84°·41 ± ·25	2°·87 ± ·17	—	84°·49 ± ·26	3°·66 ± ·18	—
$A\angle$	72°·81 ± ·33	4°·16 ± ·23	—	72°·62 ± ·22	3°·14 ± ·16	—
$N\angle$	66°·60 ± ·30	3°·77 ± ·21	—	66°·40 ± ·31	4°·48 ± ·22	—
$B\angle$	40°·73 ± ·22	2°·80 ± ·16	—	40°·98 ± ·19	2°·76 ± ·14	—
θ_1	28°·98 ± ·29	3°·34 ± ·20	—	29°·09 ± ·25	3°·44 ± ·17	—
θ_2	11°·30 ± ·34	3°·92 ± ·24	—	11°·82 ± ·26	3°·65 ± ·18	—
$100B/L'$	72·70 ± ·19	2·88 ± ·13	3·96 ± ·18	73·86 ± ·16	2·89 ± ·11	3·92 ± ·12
$100B/L$	72·99 ± ·17	2·80 ± ·12	3·83 ± ·16	74·19 ± ·16	3·12 ± ·11	4·20 ± ·11
$100H/L'$	73·20 ± ·18	2·77 ± ·13	3·85 ± ·17	73·11 ± ·16	2·81 ± ·11	3·85 ± ·15
$100H/L$	73·30 ± ·16	2·73 ± ·11	3·73 ± ·16	73·22 ± ·16	2·96 ± ·11	4·04 ± ·15
$100H/B$	100·47 ± ·28	4·73 ± ·20	4·72 ± ·20	98·66 ± ·25	4·66 ± ·17	4·73 ± ·18
$100B/H$	99·76 ± ·27	4·53 ± ·19	4·54 ± ·19	101·59 ± ·25	4·74 ± ·18	4·66 ± ·18
$100GH/GB$	117·06 ± ·72	5·30 ± ·51	4·53 ± ·43	118·87 ± ·97	6·73 ± ·68	5·67 ± ·58
$100G'H/GB$	70·63 ± ·35	4·52 ± ·25	6·41 ± ·35	70·36 ± ·27	4·15 ± ·19	5·91 ± ·27
$100NB/NH$	51·08 ± ·32	4·18 ± ·23	8·18 ± ·44	52·31 ± ·31	4·86 ± ·22	9·28 ± ·42
$100O_2/O_1 : L$	74·89 ± ·39	5·06 ± ·27	6·76 ± ·37	76·91 ± ·29	4·57 ± ·21	5·94 ± ·27
$100O_2/O_1 : R$	74·87 ± ·39	5·00 ± ·27	6·68 ± ·36	77·32 ± ·48	4·78 ± ·21	6·18 ± ·28
$100G_2/G_1$	71·94 ± ·61	7·36 ± ·43	10·23 ± ·60	72·30 ± ·41	5·93 ± ·29	8·20 ± ·40

If we turn to the length, breadth, and height of the skull, our data for comparison are still more sparse. We find:

Race.	Length.		Breadth.		Auricular Height.	
	♂	♀	♂	♀	♂	♀
Bavarian *	3·37	3·57	3·89	3·39	4·47	3·91
English †	3·44	3·66	3·55	3·78	—	—
French ‡	3·97	3·65	4·21	3·67	—	—
Naqada	3·17	3·14	3·29	3·45	3·86	3·54
Aino *	3·20	3·08	2·76	2·68	3·67	3·18

Again the Naqada lie between the 'primitive' Aino and the high modern civilisations.

Of circumferential measurements we can only compare:

Race.	Horizontal Circumference.		Vertical Circumference.	
	♂	♀	♂	♀
Bavarian §	2·86	3·09	—	—
Ancient Egyptian ‖	2·74	2·85	2·67	2·84
Naqada	2·54	2·27	3·32	2·72
Row Grave German §	2·70	2·40	—	—

This table shows us much the same state of affairs, and we mark as usual the advance in variability with advancing civilisation. Only one other absolute dimension¶ are we able to give a comparison with; namely the length of the palate. We have:

Race.	Length of Palate.	
	♂	♀
Bavarian	6·42	6·85
Naqada	6·49	7·41

Thus Bavarian and Naqada data both agree in making the woman more variable in palate length than the man. All the above are comparisons of coefficients of variation.

For angular measurements we can only quote the variability in profile angle of the Bavarians:

Race.	Standard Deviation.	
	♂	♀
Bavarian	2°·79	3°·59
Naqada	2°·87	3°·66

---

* Dr Alice Lee: *Phil. Trans.* Vol. 196, A, p. 230.

† Unpublished data for the Whitechapel skulls deduced by Prof. Pearson.

‡ Unpublished reductions of measurements in Broca's manuscripts by C. D. Fawcett.

§ Pearson: *The Chances of Death*, Vol. I. pp. 356–7.

‖ Deduced from Dr Alice Lee's data. *Phil. Trans.* Vol. 196, A, p. 262.

¶ Raw material enough is of course available, but the object of this present memoir is rather to show how craniological results are to be presented from the statistical side than to publish long reductions of other investigators' measurements.

In the case of both races for palate and profile the females are markedly more variable than the males. Further the ancient is here more variable than the modern race.

Lastly we turn to the indices. Here somewhat more material is available; we give now the standard deviations:

Race.	*B/L*		*H/L*		*H/B*	
	♂	♀	♂	♀	♂	♀
Bavarian*	3·50	2·97	—	—	—	—
English†	3·31	3·37	—	—	—	—
French‡	4·43	4·19	3·53	3·67	4·74	4·31
Naqada	2·80	3·12	2·73	2·96	4·73	4·66
Egyptian Mummies*	3·35	3·36	—	—	—	—
Modern Egyptians*	5·42	5·10	—	—	—	—
Negroes§	2·77	3·52	—	—	—	—
Panjabi Low Caste§	2·98	3·75	—	—	—	—
Aino§	2·41	2·54	—	—	—	—
Row Grave Germans§	2·28	2·35	—	—	—	—

Clearly the Naqada race has less variability than that of the highly advanced modern races, but as much or more than 'primitive' peoples like the Aino and Row Grave Germans. The very wide-spread evidence of increased variation as we pass from uncivilised and primitive people may of course be due to increased racial admixture as man grows older, or it may, as we believe, be due to less stringent dependence for survival on the physical characters in civilised man. But whatever its sources it seems fairly manifest in investigations of this kind, and must always be borne in mind when we are comparing races at different stages of development and at different historical periods.

For the remaining ratios we have not much comparative data. We note the following standard deviations:

Race.	Upper Face Index.		Orbital Index.		Nasal Index.	
	♂	♀	♂	♀	♂	♀
Bavarian‖	3·26	3·33	6·66	5·22	4·43	4·61
Naqada	4·52	4·15	*L.* 5·06 *R.* 5·00	4·57 4·78	4·18	4·86

There is not the same marked increased variability in the modern race here. But we see that for both races the woman is less variable in orbital index¶ and slightly more variable in nasal index than the man.

---

* Dr Alice Lee: *Phil. Trans.* Vol. 196, A, pp. 230 and 232.

† Unpublished values for the Whitechapel skulls deduced by Prof. Pearson.

‡ Deduced from Broca's MSS. measurements of the catacomb skulls by C. D. Fawcett.

§ Pearson: *The Chances of Death*, Vol. I. pp. 350—371, where more data as to the cephalic index will be found.

‖ Pearson: *The Chances of Death*, Vol. I. pp. 325—328.

¶ Confirmed by Pearson's reduction of Waischenfeld skulls, see *loc. cit.* p. 327.

Generally it will be realised that the cranial characters are not highly variable like the parts of plants, and that there is considerable agreement between results from very different races.

Turning now to the graphical and analytical representation of cranial variability, it was impossible to give diagrams of the 47 characters dealt with for the two sexes. Accordingly a choice was made of 12 characters, and the curves calculated and plotted in the 24 cases corresponding to these characters for the two sexes. The laborious calculations required are largely due to Dr Alice Lee, and for the draughtsmanship involved in converting her numbers into diagrams we have to thank Mr Karl Tressler, formerly one of the demonstrators in the Department of Applied Mathematics at University College, London. The general fit and smoothness of Mr Tressler's diagrams is the best verification of the accuracy and completeness of Dr Lee's work.

Now these diagrams are given with a view to bringing out a number of points. First and foremost to emphasise that no stress ought to be laid on the "peaks" of such frequency distributions as occur in most craniological investigations. These "peaks" are chiefly due to the fact that we are dealing with *very small* random samples. Few craniologists can work with more than 30 to 100 measurements of any character in one sex. In the next place sex-determination is only a *probable* determination; further, diversity of age, occasional foreign skulls, postmortem deformation and other causes produce heterogeneity. Lastly where a cemetery or local burial place has been plundered, we are certain to get family groups of skulls. All these causes tend to emphasise the irregularity of the distribution even beyond the limits of random sampling. We can only ask, are these small samples more variable—variability being measured by the standard deviation—than the largest and most homogeneous series known to us, such a sample for example as the "Altbayerisch"? If they are not, and the Naqadas are certainly not, then it is very unwise—nay, quite unjustifiable—to argue from a series of peaks as to racial mixtures. Even if like peaks occur in the two sexes for several characters the sources of heterogeneity given above are often ample explanation.

What we do see from these diagrams is that the curves which have already proved themselves sufficient for many frequency distributions in other living forms will suffice to graduate and smooth the frequency polygons obtained for short series of craniological measurements. They will serve for comparative purposes as soon as further series of craniological measurements have been reduced in the statistical manner advocated in this memoir. When once the craniologist has learnt to look upon his many peaked sample as the disturbed form of such smooth distributions, he will hesitate to make statistically unwarranted racial differentiations on the basis of such peaks. The question will then become: Is any system of peaks, allowing not only for random sampling, but for the above-mentioned sources of heterogeneity, really significant? In the state of our present ignorance, it is safer to be sceptical as to significance, than to build up on a statistically unsound foundation elaborate theories of racial mixture and racial relationship.

The curves we give in our diagrams are precisely similar to—showing neither greater nor less range than—those to which human, including cranial characters, closely approach in distribution, when we actually have the measurements on a large homogeneous population*. Hence we may fairly take them as representing cranial variation until more material is made available for comparison, especially in races of unquestionable purity.

Adopting the notation of the memoir on skew variation† we have the following system of constants for the 12 characters given in the first column, the second column gives the number of skulls upon which the calculations were based; these neither in total number, nor, if in total number, as individuals, are identical with those used for the results in Tables V^{a} and V^{b}. Thus the means and variabilities differ slightly, but always within the limits of probable error of random sampling.

TABLE XII.

Analytical Constants of Curves.

Character	No.	Unit	μ_2	μ_3	μ_4	β_1	β_2	Criterion	Mean	Mode	Skewness‡
L ♂	141	1 mm.	34·2740	13·5652	3120·00	·0046	2·6560	·7017	184·91	185·17	−·0450
L ♀	187	1 mm.	30·5756	19·9421	2903·14	·0139	3·1054	−·1691	177·38	177·07	·0559
B ♂	139	1 mm.	21·5419	28·6959	1561·16	·0824	3·3642	−·4813	134·77	134·19	·1247
B ♀	181	1 mm.	20·8101	1·3987	1125·26	·0002	2·5984	·8038	131·56	131·51	·0103
H ♂	134	1 mm.	28·9647	32·8925	2287·03	·0445	2·7260	·6815	135·21	134·47	·1385
H ♀	169	1 mm.	22·5375	21·5598	1464·20	·0406	2·8826	·3565	129·46	128·91	·1147
OH ♂	139	1 mm.	20·7052	9·0319	1449·16	·0092	3·3803	−·7331	115·57	115·39	·0398
OH ♀	175	1 mm.	15·7281	−23·7639	927·09	·1451	3·7477	−1·0600	112·57	113·14	−·1450
U ♂	116	3 mm.	20·0378	21·3658	1120·35	·0567	2·7903	·5896	510·98	508·97	·0356
U ♀	148	3 mm.	15·7876	18·0031	790·96	·0824	3·1734	−·0997	493·92	492·20	·1390
S ♂	120	3 mm.	15·4287	6·3013	761·12	·0108	3·1074	−·3623	373·06	372·51	·0425
S ♀	153	3 mm.	16·7380	8·2974	844·62	·0147	3·0156	·0129	364·31	363·57	·0606
Q ♂	115	2 mm.	25·8917	50·4615	1998·67	·1467	2·9814	·4773	304·29	301·97	·2279
Q ♀	150	2 mm.	18·9225	·2075	1129·17	·0000	3·1536	−·3071	296·49	296·48	·0011
G′H ♂	84	1 mm.	17·1063	−17·9839	788·55	·0646	2·6948	·8043	67·61	68·35	−·0450
G′H ♀	117	1 mm.	18·5722	−5·4600	942·44	·0047	2·7323	·5494	65·05	65·23	−·0422
NB ♂	84	1 mm.	3·7738	1·4915	38·7783	·0414	2·7229	·6783	25·17	24·91	·1333
NB ♀	116	1 mm.	3·0685	1·0370	28·1413	·0372	2·9888	·1341	24·28	24·10	·1010
NH ♂	88	1 mm.	8·9208	−2·6028	250·32	·0095	3·1455	−·2623	49·04	49·17	−·0450
NH ♀	121	1 mm.	10·2777	−1·5166	265·10	·0021	2·5097	·9870	46·69	46·81	−·0359
B/L ♂	137	1 point	7·7552	2·9348	151·05	·0185	2·5114	1·0326	72·97	72·67	·1087
B/L ♀	181	1 point	9·4455	4·6788	261·60	·0260	2·9322	·2136	74·23	73·96	·0868
H/L ♂	131	1 point	7·2499	4·2003	166·40	·0463	3·1658	−·1926	73·47	73·20	·1013
H/L ♀	159	1 point	8·6228	5·5811	219·76	·0486	2·9556	·2345	73·31	72·96	·1196

---

* See for example: Bavarian Skulls, Cephalic Index. *Phil. Trans.* Vol. 186, A, p. 388 and diagram; *Biometrika*, Vol. I. p. 158. Breadth and Height of Head in Criminals, *Biometrika*, Vol. I. pp. 183—5 with diagrams.

† *Phil. Trans.* Vol. 186, A, pp. 343—414.

‡ Skewness = distance from mean to mode divided by the standard deviation.

These results were in fact first obtained from the measurements, but at a later date further skulls were found available for measurement. The third column gives the unit in terms of which the corresponding moment constants in the next three columns are expressed. Then follow the values of the numerical constants β_1, β_2 and the criterion $6 + 3\beta_1 - 2\beta_2$. Finally the mean and mode in the usual units and the skewness of the distribution. The actual equations to the 24 curves deduced from these constants with the origin in the same unit as the mean are given on the diagrams below. In these curves the unit of y is one individual per unit of x, and the unit of x is the unit given in the third column of Table XII. As a matter of practical use, it is sufficient in these cases to treat y as given by a scale of absolute frequency, which is indicated on the vertical to the left of the diagram.

Several points may be drawn from these analytical results.

(*a*) The skewness is negative in five cases only, or, looking at the diagrams, the mode is greater or falls to the right of the mean in only 5 out of 24 cases. But supposing the skewness to be merely a result of random sampling and not due to any bias in the organs in question, the probable error of skewness would be $\cdot 67449 \sqrt{3/2n}$ where $n =$ number of individuals dealt with*. We therefore conclude that for $n = 81$ to 144 observations, the probable error of skewness would be between ·09 and ·07. Thus in four out of the five cases, the skewness is only about half its probable error; in the fifth, the auricular height of the female, the skewness $= - \cdot 1450 \pm \cdot 0624$, and this may possibly be considered as significant. Of the positive skewnesses 8 are insignificant, and 11 certainly or probably significant. We may therefore conclude that:

In measurements on the skull, if the mode and mean do not coincide, the mean will almost invariably be greater than the mode.

(*b*) Next let us examine the probable errors of $\sqrt{\beta_1}$, β_2 and the criterion†.

The probable error of $\sqrt{\beta_1}$ runs in our case from ·14 to ·18 roughly, according to the number in the series, that of β_2 from about ·28 to ·37 and that of the criterion from about ·56 to ·74. We notice at once that β_2 differs from 3 in only very few cases by an amount which is significant having regard to its probable error; the same is again true of the criterion, which differs from zero by quantities of the order of the probable error. $\sqrt{\beta_1}$ has deviations from zero, which are upwards of double its probable error in two or three cases, but on the whole we may conclude that:

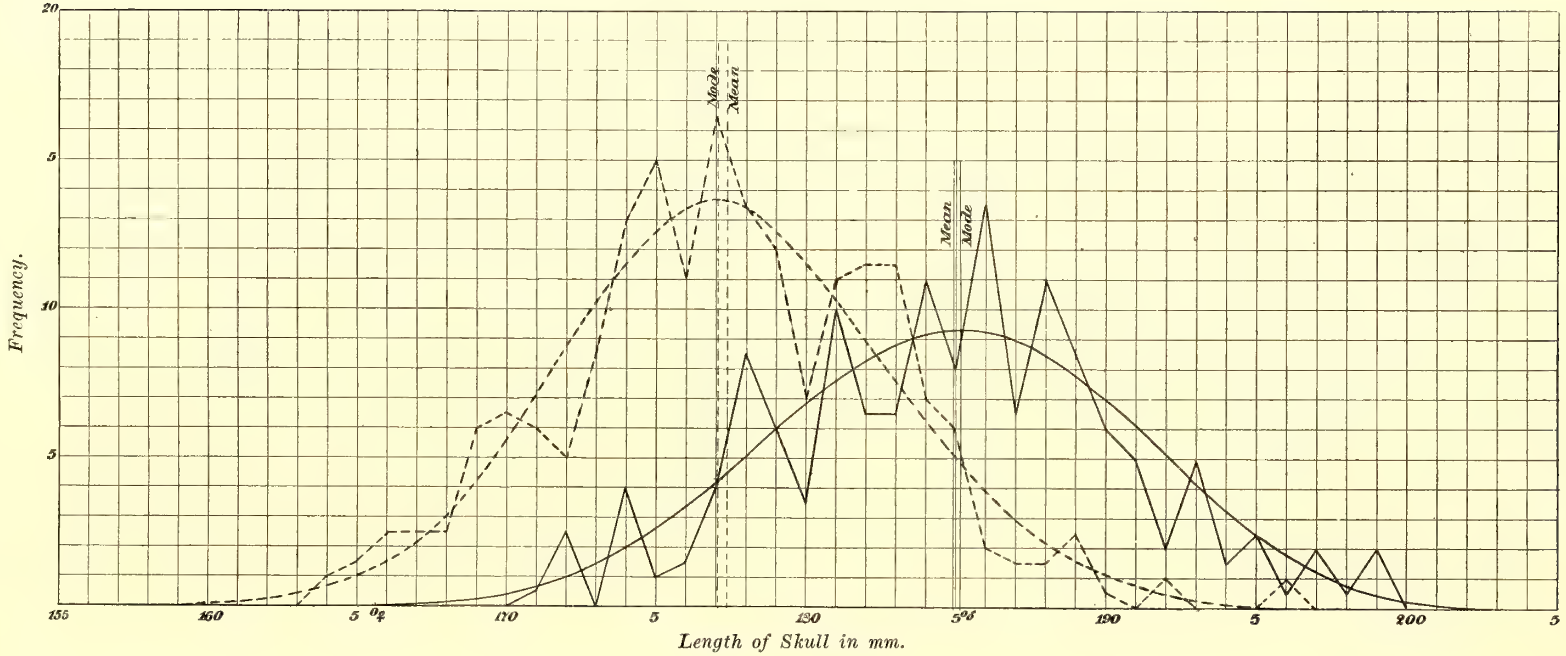
With series of skull measurements such as the present, which are long for the craniologist, if short for the statistician, we shall reach for most practical purposes adequate graphical representations of the frequency by using the normal curve of deviations: $y = y_0 e^{-x^2/(2\sigma^2)}$.

---

* Pearson: "On the Mathematical Theory of Errors of Judgment," *Phil. Trans.* Vol. 198, A, p. 278.

† *Loc. cit.* p. 278.

DIAGRAM I. *Length of Skull, L.*



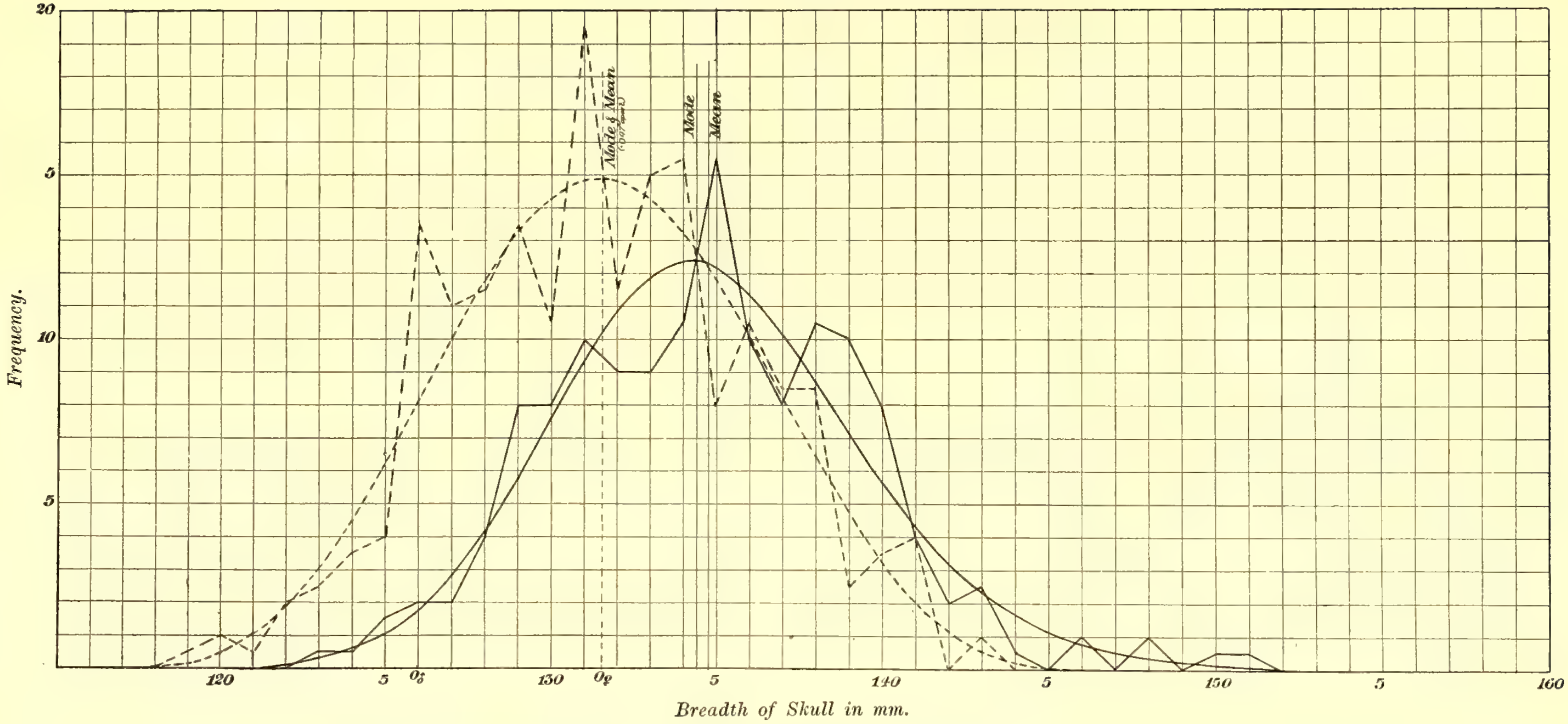
$$♂ \text{———} \quad y = 9{\cdot}311\left(1+\frac{x}{24{\cdot}679}\right)^{6{\cdot}554}\left(1-\frac{x}{20{\cdot}962}\right)^{5{\cdot}567}.$$

Origin, 185·17.

$$♀ \text{- - - -} \quad y = 1{\cdot}288\cos^{76{\cdot}196}\theta \, e^{19{\cdot}063\theta}, \quad x = 45{\cdot}820\tan\theta.$$

Origin, 165·61.

DIAGRAM II. *Breadth of Skull, B.*



$$♂ \text{ ——— } \quad y = 1{\cdot}732 \cos^{30{\cdot}449}\theta \, e^{11{\cdot}056\theta}, \quad x = 22{\cdot}665 \tan\theta.$$

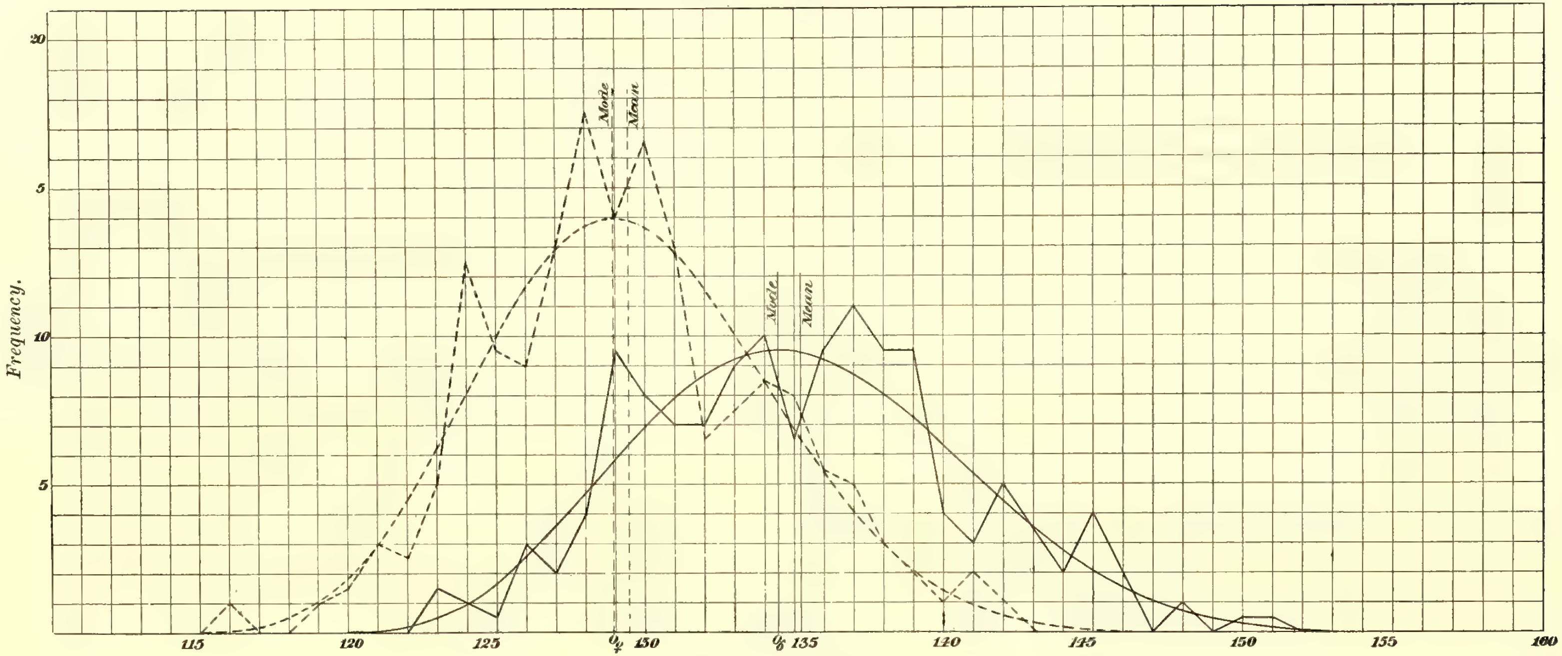
Origin, 125·96.

N.B. The draughtsman has in this curve inadvertently plotted the mode at 134·4 instead of 134·19.

$$♀ \text{ - - - - } \quad y = 14{\cdot}89 \left(1 + \frac{x}{16{\cdot}124}\right)^{4{\cdot}880} \left(1 - \frac{x}{16{\cdot}686}\right)^{5{\cdot}050}.$$

Origin, 131·51.

DIAGRAM III. *Height of Skull, H.*



Height of Skull in mm.

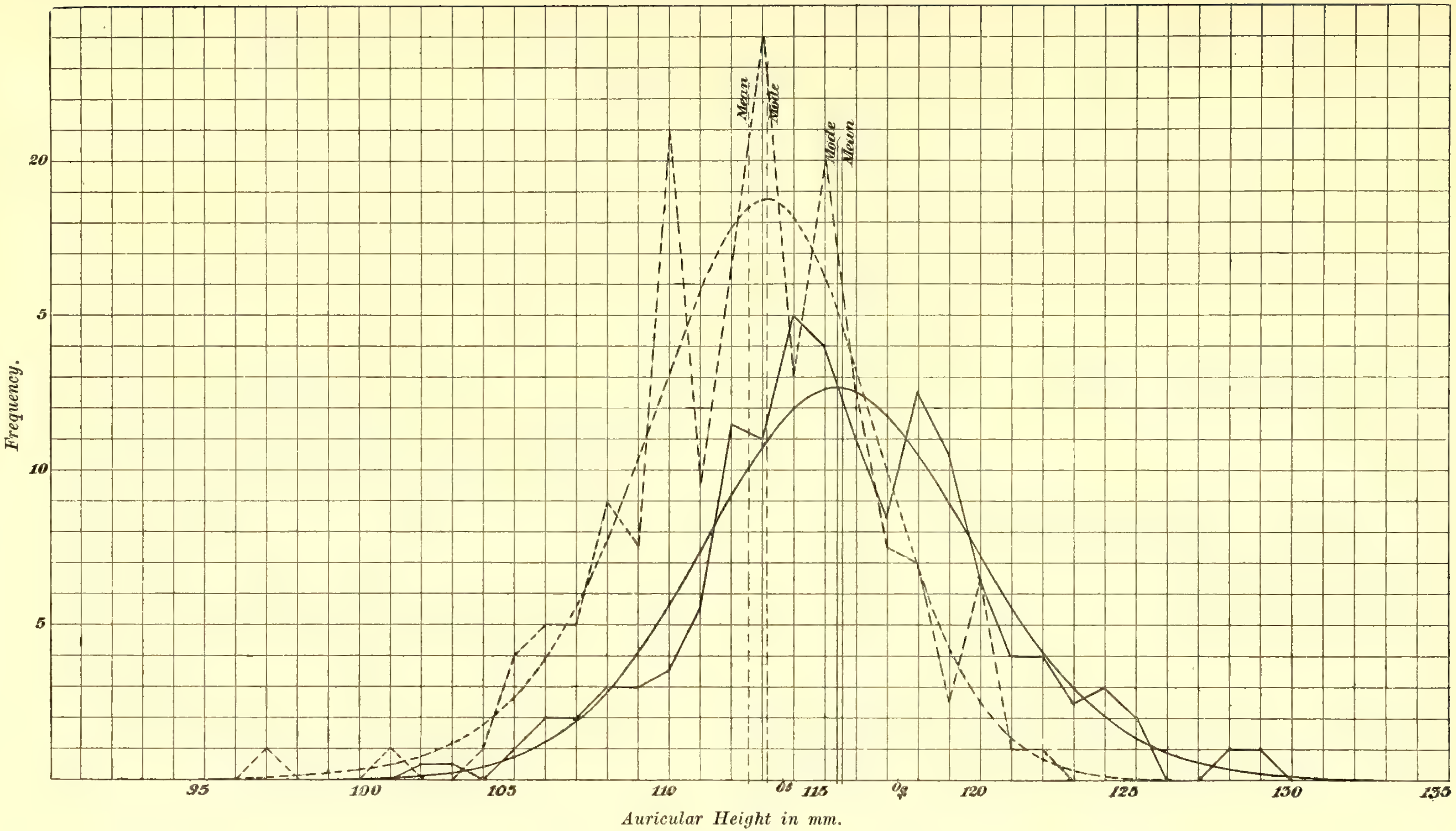
♂ ——— $y = 9{\cdot}517\left(1+\frac{x}{16{\cdot}405}\right)^{4{\cdot}791}\left(1-\frac{x}{27{\cdot}437}\right)^{8{\cdot}013}.$

Origin, 134·47.

♀ - - - - $y = 13{\cdot}936\left(1+\frac{x}{19{\cdot}554}\right)^{10{\cdot}129}\left(1-\frac{x}{36{\cdot}426}\right)^{18{\cdot}870}.$

Origin, 128·91.

DIAGRAM IV. *Auricular Height, OH.*



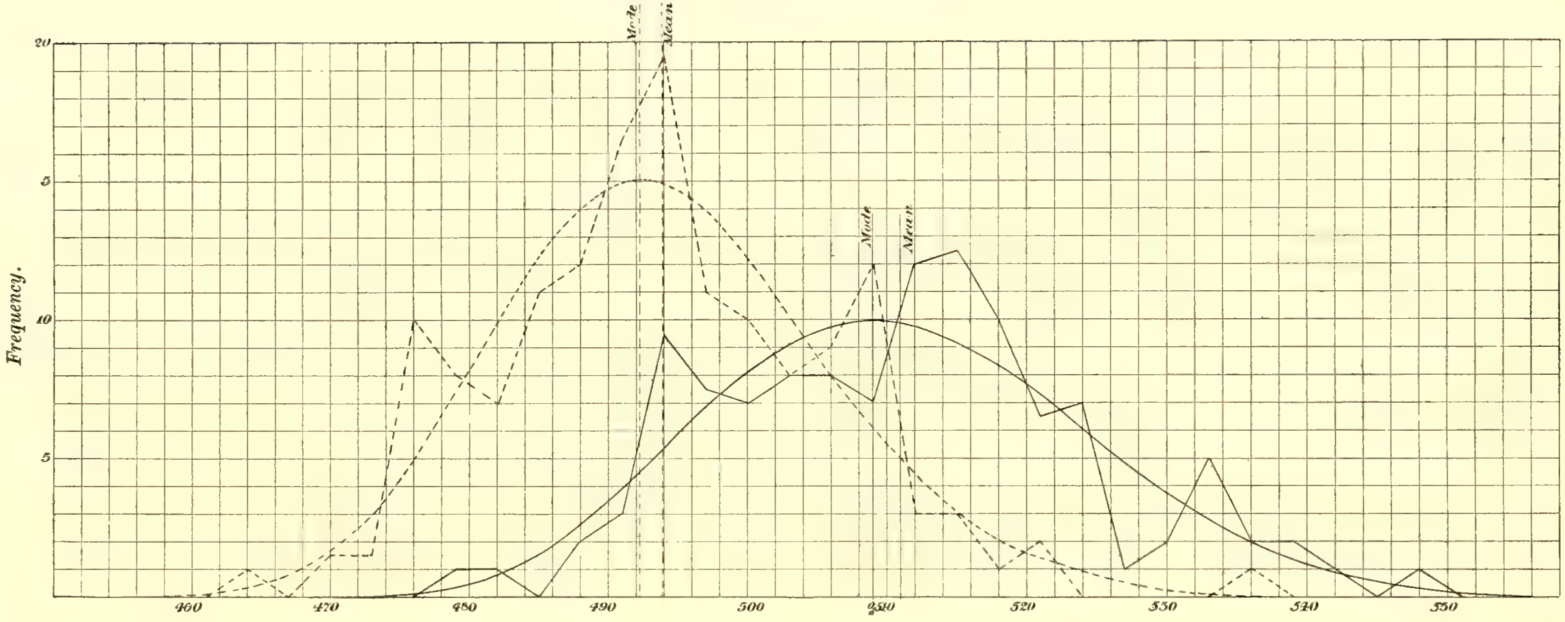
♂ ——— $y = 11{\cdot}66 \cos^{24{\cdot}407}\theta\, e^{1{\cdot}896\theta}$, $x = 19{\cdot}430 \tan\theta$.

Origin, 113·67.

♀ - - - - $y = 8{\cdot}71 \cos^{16{\cdot}731}\theta\, e^{-5{\cdot}101\theta}$, $x = 15{\cdot}887 \tan\theta$.

Origin, 117·38.

Diagram V. *Horizontal Circumference, U.*



Horizontal Circumference in mm.

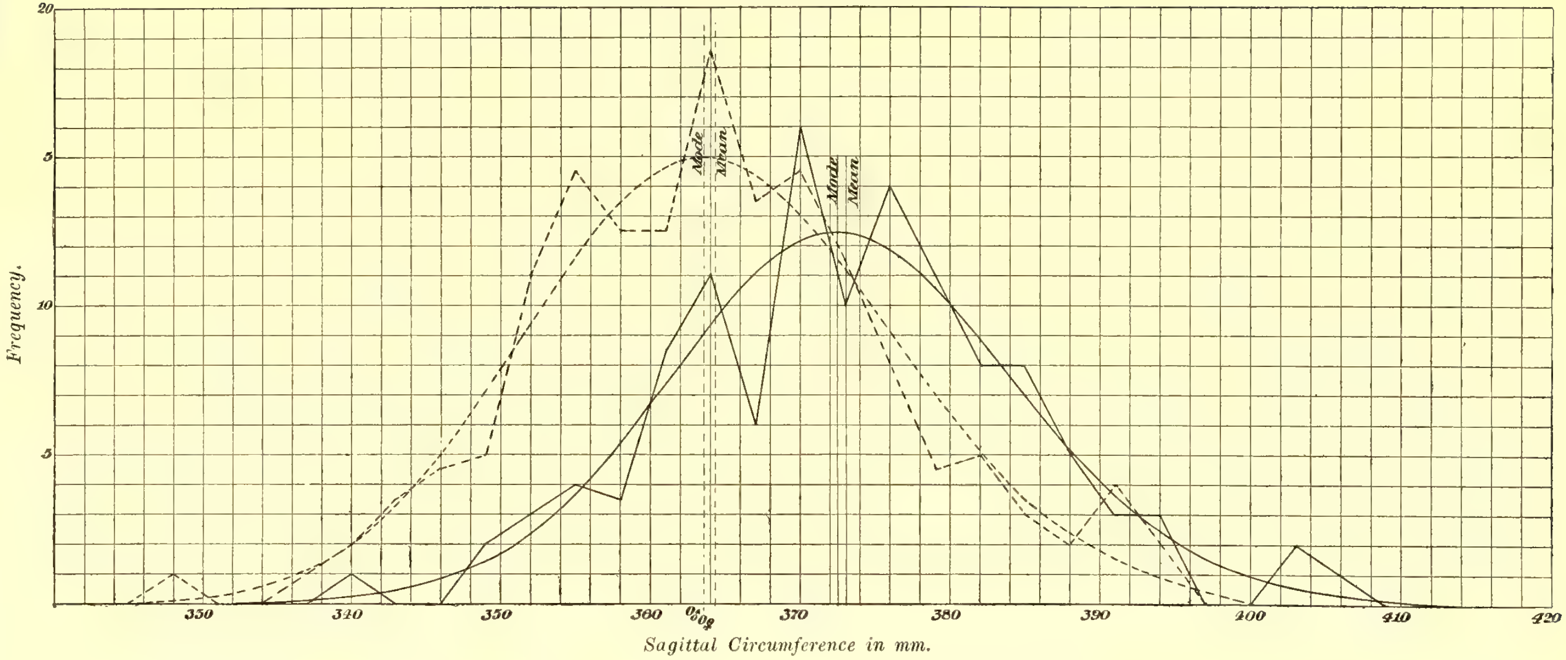
♂ ——— $$y = 9{\cdot}997\left(1 + \frac{x}{14{\cdot}119}\right)^{5{\cdot}514}\left(1 - \frac{x}{25{\cdot}929}\right)^{10{\cdot}127}.$$

Origin, 508·97.

♀ - - - - $$y = \text{Antilog}\,(\overline{37}{\cdot}033{,}1864)\cos^{127{\cdot}8556}\theta\, e^{165{\cdot}1064\theta}, \quad x = 26{\cdot}915\tan\theta.$$

Origin, 387·99.

DIAGRAM VI. *Sagittal Circumference, S.*



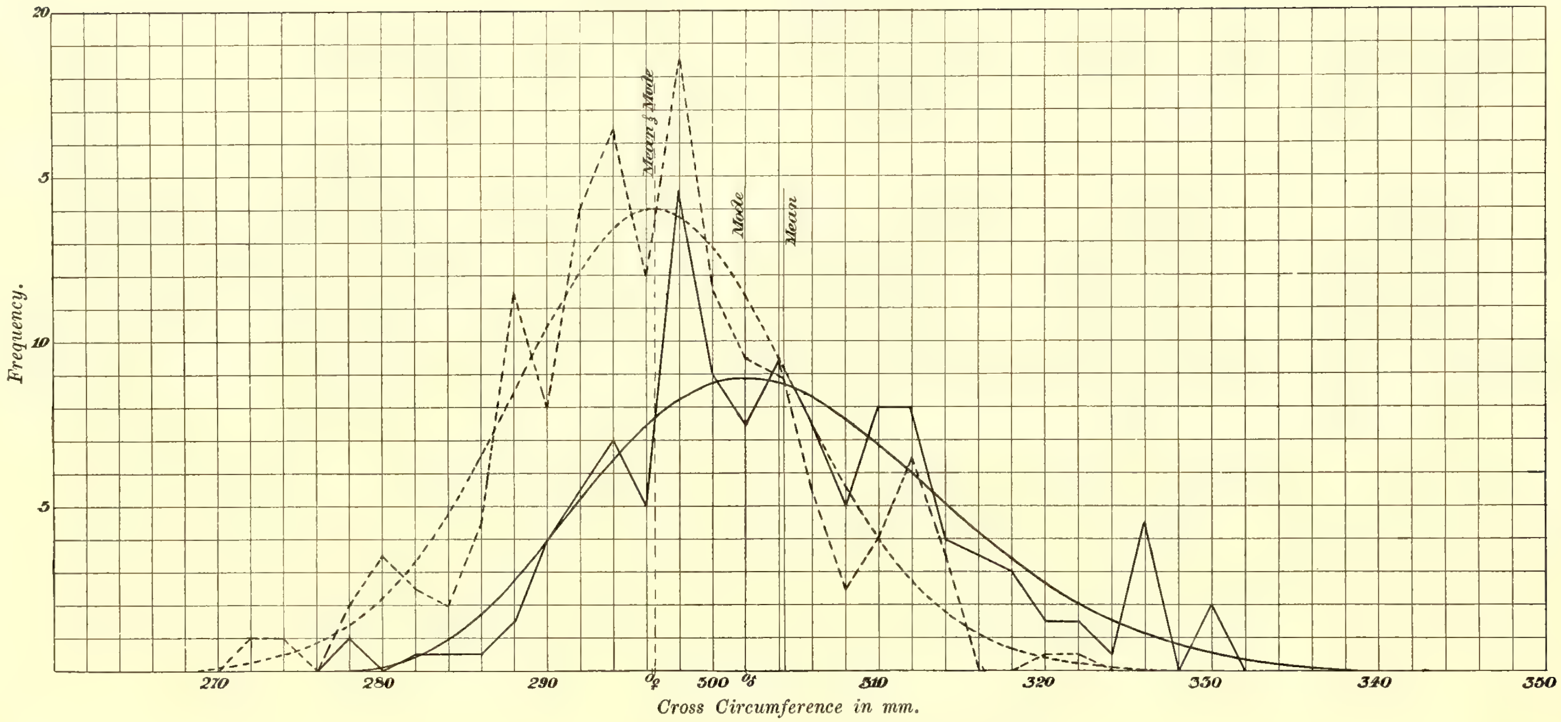
♂ ——— $y = 8{\cdot}4102 \cos^{38{\cdot}211}\theta \, e^{5{\cdot}489\,\theta}, \quad x = 23{\cdot}045 \tan \theta.$

Origin, 362·58.

♀ - - - - $y = 26{\cdot}30 \left(1 + \frac{x}{67{\cdot}264}\right)^{271{\cdot}341} e^{-4{\cdot}034x}.$

Origin, 363·57.

DIAGRAM VII. *Cross Circumference, Q.*



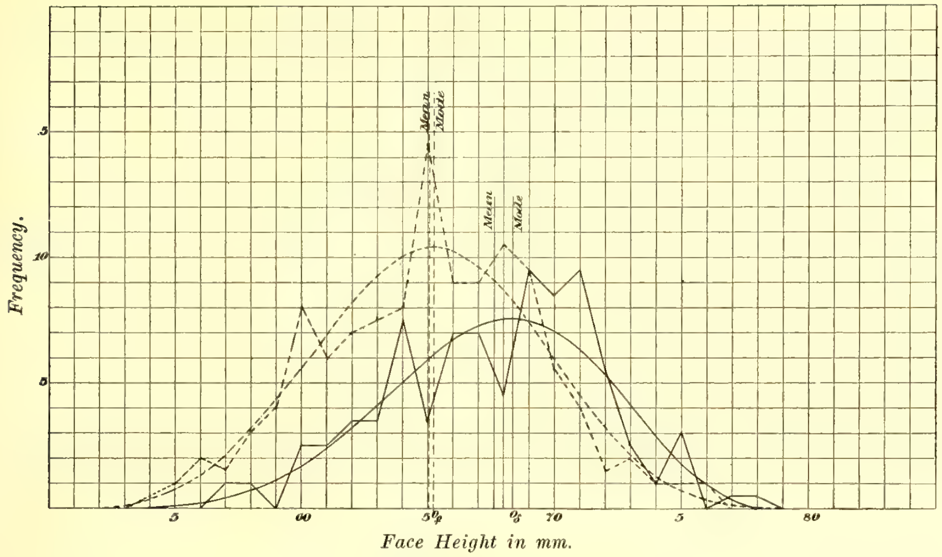
♂ ——— $y = 8{\cdot}90 \left(1 + \frac{x}{14{\cdot}416}\right)^{5{\cdot}464} \left(1 - \frac{x}{55{\cdot}573}\right)^{15{\cdot}598}$.

Origin, 301·97.

♀ - - - - $y = 14{\cdot}00 \cos^{44{\cdot}075}\theta\, e^{{\cdot}166\theta}, \quad x = 27{\cdot}879 \tan\theta$.

Origin, 296·27.

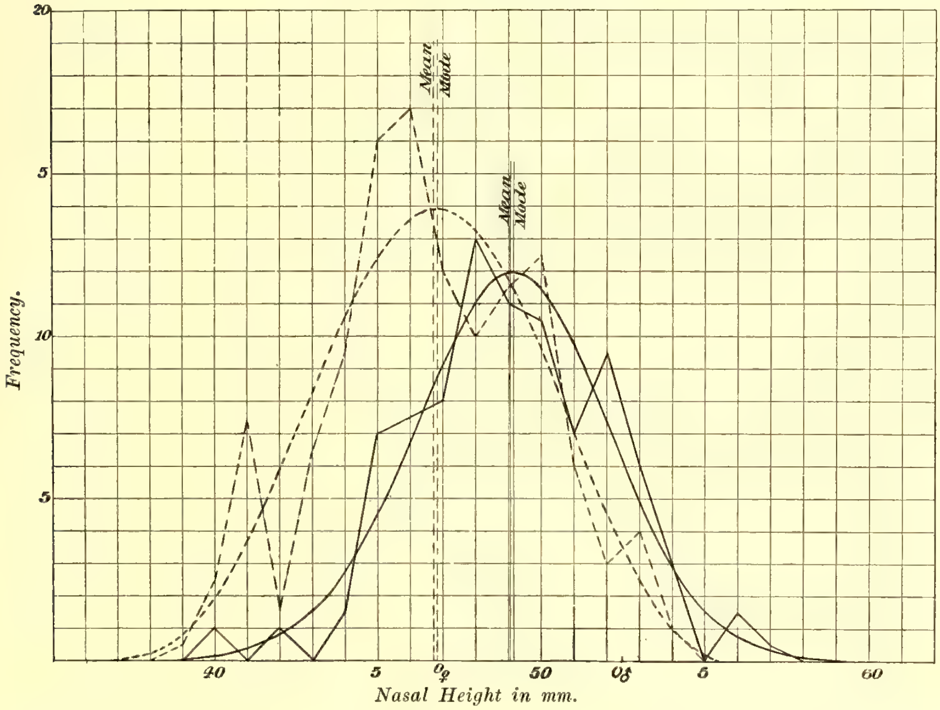
DIAGRAM VIII. *Upper Face Height, G′H.*



♂ —— $y=7\cdot57\left(1+\frac{x}{19\cdot913}\right)^{6\cdot544}\left(1-\frac{x}{11\cdot005}\right)^{3\cdot616}$.   ♀ - - - - $y=10\cdot42\left(1+\frac{x}{20\cdot987}\right)^{9\cdot186}\left(1-\frac{x}{17\cdot555}\right)^{7\cdot684}$.

Origin, 68·35.   Origin, 65·23.

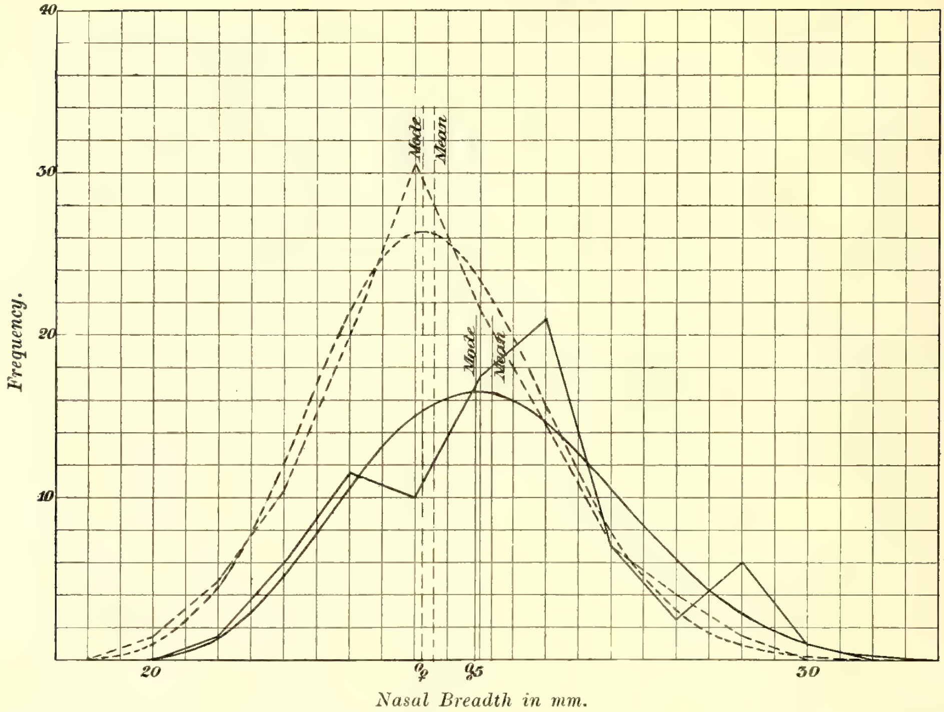
DIAGRAM IX. *Nasal Height, NH.*



♂ —— $y=6\cdot19\cos^{50\cdot860}\theta\, e^{-8\cdot195\,\theta}$, $x=20\cdot378\tan\theta$.   ♀ - - - - $y=13\cdot92\left(1+\frac{x}{10\cdot756}\right)^{3\cdot767}\left(1-\frac{x}{9\cdot703}\right)^{3\cdot398}$.

Origin, 52·46.   Origin, 46·81.

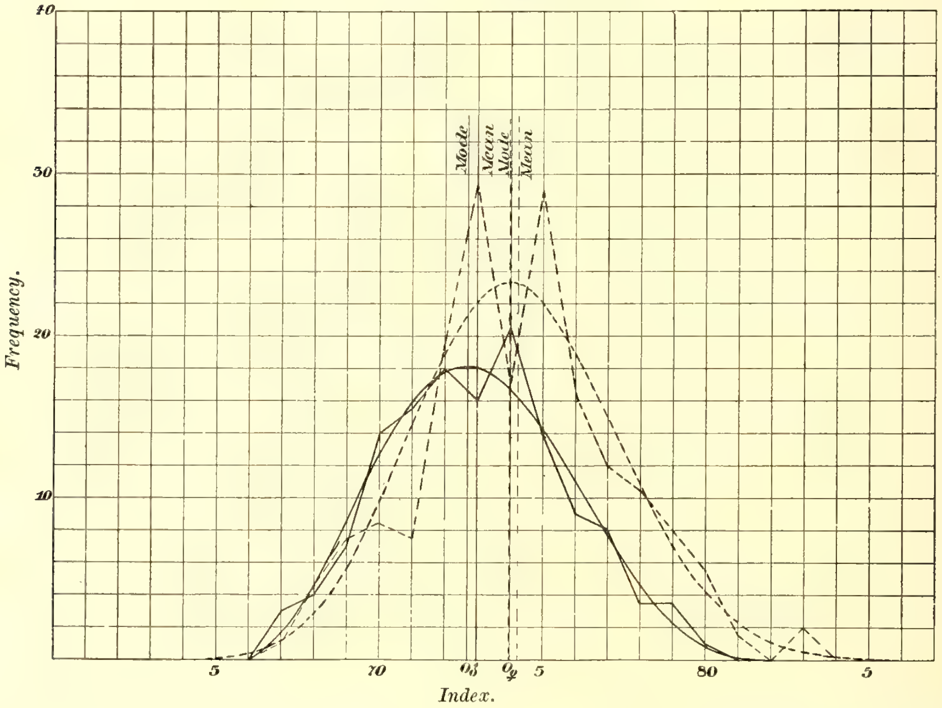
DIAGRAM X. *Nasal Breadth, NB.*



♂ —— $y=16\cdot52\left(1+\frac{x}{5\cdot994}\right)^{4\cdot871}\left(1-\frac{x}{9\cdot844}\right)^{8\cdot003}$. Origin, 24·91.

♀ - - - - $y=26\cdot30\left(1+\frac{x}{10\cdot381}\right)^{24\cdot463}\left(1-\frac{x}{25\cdot836}\right)^{60\cdot843}$. Origin, 24·10.

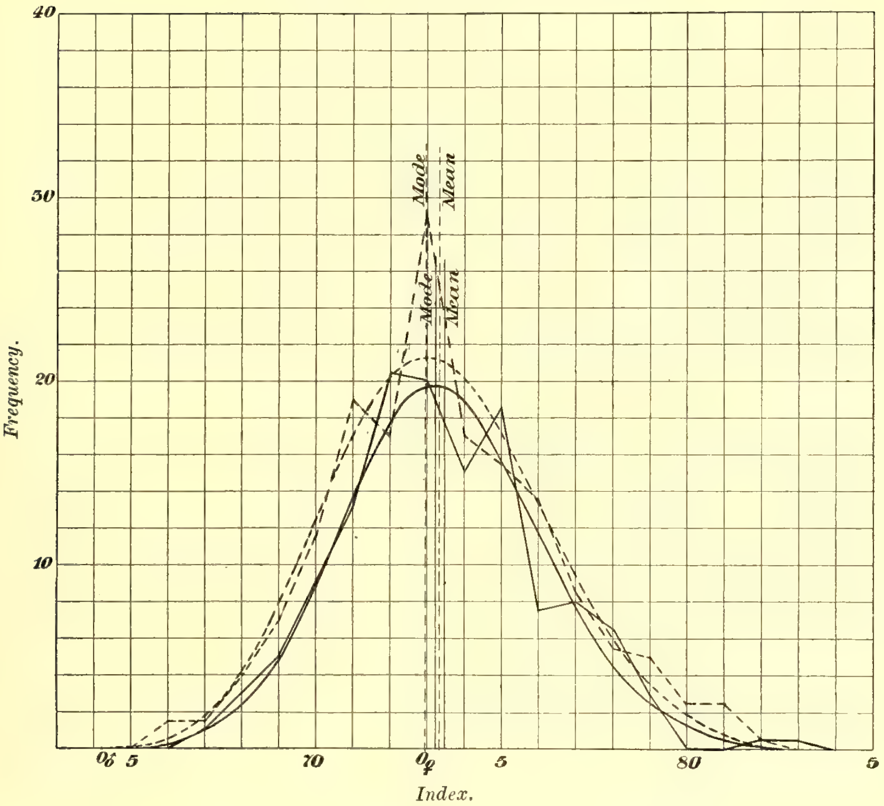
DIAGRAM XI. *First Cephalic Index, B/L.*



♂ —— $y=18\cdot12\left(1+\frac{x}{7\cdot408}\right)^{2\cdot835}\left(1-\frac{x}{10\cdot033}\right)^{3\cdot840}$. Origin, 72·67.

♀ - - - - $y=23\cdot24\left(1+\frac{x}{16\cdot576}\right)^{18\cdot016}\left(1-\frac{x}{30\cdot874}\right)^{33\cdot555}$. Origin, 73·96.

DIAGRAM XII. *Second Cephalic Index, H/L.*



♂ ——— $y = \cdot 01954 \cos^{68\cdot 023}\theta\, e^{31\cdot 185\theta}$, $x = 19\cdot 632 \tan \theta$.

Origin, 64·20.

♀ - - - - $y = 21\cdot 39 \left(1 + \frac{x}{13\cdot 721}\right)^{14\cdot 401} \left(1 - \frac{x}{30\cdot 861}\right)^{32\cdot 390}$.

Origin, 72·96.

Professor Weldon having urged the importance of comparing the actual goodness of fit of the skew and normal curves in at least one instance, we selected the breadth of the skull as an illustration, because this is a measurement, which, owing to the peaks of its curves, has already been used as an argument for racial mixture in the Naqada material. The female data (broken line, Diagram II., p. 445) provide a "mountain range" which many non-statistically trained craniologists would at once interpret as signifying racial mixture. Let us examine these data from the mathematical standpoint. The equation to the normal curve is: $y = 15\cdot 829\, e^{-\frac{1}{2}(x/4\cdot 5618)^2}$, the origin being at 131·56 mm. and the standard deviation being 4·5618 mm. The equation to the skew curve is given at the foot of the diagram, p. 445. The following are the observed and calculated frequencies:

		Calculated				Calculated	
mm.	Observed	Skew	Normal	mm.	Observed	Skew	Normal
119 & under	0·5	0·2	0·7	132	11·5	14·8	15·8
120	1·0	0·5	0·6	133	15·0	14·3	15·0
121	0·5	1·0	1·1	134	15·5	13·3	13·8
122	2·0	1·9	1·7	135	8·0	11·8	11·9
123	2·5	3·1	2·7	136	10·5	10·1	9·9
124	3·5	4·6	4·0	137	8·5	8·3	7·8
125	3·0	6·3	5·6	138	8·5	6·4	5·9
126	13·5	8·2	7·5	139	2·5	4·7	4·2
127	11·0	10·1	9·6	140	3·5	3·2	2·9
128	11·5	11·8	11·7	141	4·0	2·0	1·9
129	13·5	13·2	13·5	142	0·0	1·1	1·2
130	10·5	14·3	14·9	143 & over	1·0	1·0	1·1
131	19·5	14·8	15·7	Totals	181	181	180·7

Applying the test for goodness of fit*, we find for skew curve: $\chi^2 = 15{\cdot}65$ and $P = {\cdot}90$, and for normal curve: $\chi^2 = 17{\cdot}46$ and $P = {\cdot}83$. In other words, if the skulls, of which the Naqadas are a sample, obeyed the former frequency distribution we should get a more peaked polygon in 90 out of every 100 trial samples of 181 crania; and if they obeyed the latter, or normal distribution, in 83 out of every 100 trials. We see therefore quite clearly in a special case: first, that the multimodal appearance of short series of crania such as those represented in our diagrams may be wholly due to random sampling and be no sign of racial heterogeneity; secondly, that for many cranial series the normal curve, if it presents a worse fit than the skew curve, still gives a distribution quite good enough for most craniometric purposes.

This is not to be interpreted as meaning that skewness and the distinction of mode and mean are not to be regarded, when enough material is available. They may ultimately lead the craniologist to important conclusions such as that suggested above on p. 443, but the data provided by most craniological series is not sufficient to determine them with significance. In other words, our present more elaborate investigations justify for short series of craniological characters the use of the normal curve. It has already been shown that such use is justified as a first approximation in the case of many other characters in man†.

(10) *On the Correlation of Cranial Characters.*

Attention has already been drawn to the fact that the correlation of cranial characters in man is remarkably low and is also very irregular from race to race‡. Unfortunately cranial series are always relatively small, and a small series combined with low correlation means a high probable error. Further, if we add possible

---

* See *Biometrika*, Vol. I. p. 155.

† It is widely recognised now that the normal curve is not a general description of frequency. Hence its use for each class of cases must be independently demonstrated. Its simplicity and easy theoretical handling make any justification of its use such as the above of great importance.

‡ Pearson: *Phil. Trans.* Vol. 187, A, 1896, pp. 279—281; Boas: *American Anthropologist*, Vol. I. 1899, pp. 448 *et seq.*; Lee: *Phil. Trans.* Vol. 196, A, pp. 228, 229.

heterogeneity, errors of sexing, and clustering due to family entombments to these factors, we find that cranial characters do not provide the same sound basis for investigation that is provided by a small series of long bones. Nevertheless this fact in itself—irregular and low correlation of cranial characters—is of great importance and well worth ample demonstration. If craniologists as a body would once realise it, many wide generalities which now pass muster as craniological laws would be tacitly dropped. We may more or less safely argue from one race to a second in a long bone result, but it is almost impossible from a minute study of the individual crania of one race to argue as to what will hold for the crania of a second race. These low correlations of cranial characters and their divergent values from race to race seem to indicate that if the skull has been the subject of extremely stringent and varied selection, then this selection has not tended in the direction of relative proportioning of parts.

In calculating the correlation of cranial characters a selection had to be made among the 47 characters, the means and variabilities of which had been dealt with—otherwise we should have had 1081 possible pairs of characters to consider, and the arithmetic involved would have been interminable. Accordingly some 37 pairs of characters were taken as being those of special interest, and their correlation coefficients calculated for both sexes. In all these cases since the series are small it was considered best not to group the measurements into correlation tables but to proceed to the coefficient by calculating the sum of the products of the actual pairs of measurements. This involved considerably more labour, but it makes the results somewhat more reliable. It also frees us from the danger of drawing hasty conclusions from the graphical presentation of correlation results exhibiting the wide eccentricity due to the random character of small samples (see p. 425 footnote).

TABLE XIII.

Correlation of Naqada Cranial Characters.

Pair of Characters	No.	♂	No.	♀	Pair of Characters	No.	♂	No.	♀
L & H ...	134	·489 ± ·044	163	·283 ± ·048	H/L & NB/NH ...	71	– ·050 ± ·080	107	– ·132 ± ·064
L & B ...	139	·344 ± ·050	183	·143 ± ·049	H/L & O_2/O_1 (L) ...	70	·175 ± ·078	100	– ·002 ± ·067
B & H ...	129	·273 ± ·055	163	·119 ± ·052	H/L & O_2/O_1 (R) ...	73	·170 ± ·077	102	– ·016 ± ·067
					H/L & G_2/G_1 ...	59	·148 ± ·086	87	– ·041 ± ·072
C & H ...	86	·642 ± ·043	114	·519 ± ·046					
C & B ...	89	·434 ± ·058	123	·532 ± ·044	B/L & NB/NH ...	75	– ·148 ± ·076	110	– ·050 ± ·064
C & L ...	89	·501 ± ·054	123	·599 ± ·039	B/L & O_2/O_1 (L) ...	72	·106 ± ·079	102	– ·085 ± ·066
C & Q ...	84	·656 ± ·042	118	·603 ± ·039	B/L & O_2/O_1 (R) ..	74	·165 ± ·076	104	– ·036 ± ·066
C & U ...	84	·681 ± ·040	115	·723 ± ·030	B/L & G_2/G_1 ...	60	·317 ± ·078	88	·109 ± ·071
U & Q ...	84	·512 ± ·054	115	·454 ± ·050					
					NB/NH & O_2/O_1 (L)	77	– ·276 ± ·071	111	– ·263 ± ·060
O_1 & O_2 (L)	81	·434 ± ·061	108	·477 ± ·050	NB/NH & O_2/O_1 (R)	76	– ·323 ± ·069	113	– ·279 ± ·059
O_1 & O_2 (R)	82	·405 ± ·062	112	·510 ± ·047	NB/NH & G_2/G_1 ...	67	– ·194 ± ·079	97	·026 ± ·068
NB & NH	84	·343 ± ·065	116	·125 ± ·061					
G_1 & G_2	73	·202 ± ·076	105	·501 ± ·049					
$G'H$ & GB	77	·385 ± ·065	101	·479 ± ·050	G_2/G_1 & O_2/O_1 (L)	62	·118 ± ·085	93	·108 ± ·069
					G_2/G_1 & O_2/O_1 (R)	65	·177 ± ·081	94	·216 ± ·066

TABLE XIV.

Correlation of Naqada Cranial Characters and Comparison with French Values.

Pair of Characters	Naqada Race						French Race *					
	♂			♀			♂			♀		
	No.	Gross	Spurious	No.	Gross	Spurious	No.	Gross	Spurious	No.	Gross	Spurious
B/L & *H/L*	130	·284 ± ·054	·604 ± ·037	166	·371 ± ·046	·438 ± ·043	860	·489 ± ·018	·464 ± ·019	340	·576 ± ·024	·477 ± ·028
B/H & *L/H*	131	·595 ± ·038	·603 ± ·037	163	·527 ± ·039	·552 ± ·037	,,	·419 ± ·020	·527 ± ·017	,,	·417 ± ·030	·541 ± ·026
H/B & *L/B*	131	·601 ± ·037	·509 ± ·043	163	·594 ± ·035	·508 ± ·040	,,	·586 ± ·016	·508 ± ·018	,,	·503 ± ·027	·482 ± ·028
B/L & *H*	130	− ·176 ± ·056	—	169	− ·101 ± ·053	—	,,	− ·040 ± ·024	—	,,	·068 ± ·036	—
B/H & *L*	131	·001 ± ·058	—	163	− ·115 ± ·053	—	,,	− ·170 ± ·023	—	,,	− ·143 ± ·036	—
H/L & *B*	131	− ·214 ± ·056	—	166	− ·003 ± ·054	—	,,	·126 ± ·023	—	,,	·211 ± ·035	—
B/L & *L*	130	− ·551 ± ·041	− ·770 ± ·023	169	− ·560 ± ·037	− ·624 ± ·033	,,	− ·652 ± ·014	− ·686 ± ·013	,,	− ·720 ± ·018	− ·705 ± ·018
H/L & *L*	131	− ·333 ± ·052	− ·623 ± ·036	166	− ·514 ± ·039	− ·651 ± ·031	,,	− ·548 ± ·017	− ·677 ± ·013	,,	− ·622 ± ·022	− ·677 ± ·020
B/L & *B*	130	·594 ± ·038	·798 ± ·021	169	·695 ± ·028	·740 ± ·024	,,	·699 ± ·012	·727 ± ·011	,,	·729 ± ·017	·714 ± ·018
H/L & *H*	131	·660 ± ·033	·782 ± ·023	166	·677 ± ·029	·759 ± ·023	,,	·639 ± ·014	·736 ± ·011	,,	·694 ± ·019	·736 ± ·017

---

* Paris Catacomb Crania deduced from copies of Broca's MSS. measurements sent by the kindness of M. Manouvrier to Prof. Pearson.

We must now proceed to consider these tables in detail, and compare the results with those for other races as far as such are yet known.

Length, Breadth and Height Correlations.

We note great sexual differences in these results, the males being twice as highly correlated as the females, but length and height in this dolichocephalic race are the most closely associated pair. We have here a good deal of material for comparison. We collect all available data in the following table:

TABLE XV.

Race	*L* and *H*				*L* and *B*				*B* and *H*			
	♂		♀		♂		♀		♂		♀	
	No.		No.		No.		No.		No.		No.	
French[a]	860	·294 ± ·022	340	·132 ± ·036	860	·089 ± ·024	340	− ·042 ± ·037	860	·224 ± ·023	340	·229 ± ·035
German[b]	100	[− ·096 ± ·067]*	99	[·314 ± ·061]*	100	·286 ± ·062	99	·488 ± ·052	100	[·072 ± ·067]*	99	[·276 ± ·063]*
English (Criminals)[c] ...	—	—	—	—	3000	[·402 ± ·010]†	—	—	—	—	—	—
English (Middle Classes)[c]	—	—	—	—	1000	[·345 ± ·019]†	—	—	—	—	—	—
Aino[b]	87	[·501 ± ·054]*	63	[·349 ± ·075]*	87	·432 ± ·059	63	·376 ± ·073	87	[·345 ± ·064]*	63	[·178 ± ·082]
Naqada	134	·489 ± ·044	163	·283 ± ·048	139	·344 ± ·050	183	·143 ± ·049	129	·273 ± ·055	163	·119 ± ·052
Sioux Indians[d]	57	· 36 ± · 08	—	—	57	· 24 ± · 08	—	—	57	· 00 ± · 09	—	—
Living Sioux[d]	—	—	—	—	243	[· 24 ± · 04]†	—	—	—	—	—	—
Eskimo[d]	—	—	—	—	47	· 47 ± · 08	—	—	—	—	—	—
Indians, B. Columbia[d] ...	—	—	—	—	?	[· 08]†	—	—	—	—	—	—
Shuswap Indians[d] ...	—	—	—	—	?	[· 04]†	—	—	—	—	—	—
Badenser[d]	—	—	—	—	?	· 09	—	—	—	—	—	—
Bágdi Caste, Bengal[d] ...	—	—	—	—	?	[· 13]†	—	—	—	—	—	—

* Auricular and not total height used and so not properly comparable.

† Measurements on living head and not skull.

(a) See footnote p. 456. (b) See A. Lee: *Phil. Trans.* Vol. 196, A, p. 231.

(c) Macdonell: *Biometrika*, Vol. I. pp. 181 and 188.

(d) Boas: *American Anthropologist*, Vol. I. p. 453.

It is impossible to disguise one's disappointment at the great range of results here exhibited. We see that the Aino are in fair agreement with the Naqada values, and both show uniformly greater correlation in the male. Both also are in fair accordance with the English measurements as far as the latter go. Even if we discard the last four series, no numbers being given, we are met at once by the remarkably low French results, where not only do both sexes show a low value, but they compare fairly well with results found for independent series of French skulls by Professor Pearson in 1896, i.e. Length and Breadth, Parisians, ·05 ± ·06 and French Peasants ·13 ± ·07*.

The German results also are very puzzling, even when we note that the total height has been replaced by the auricular height; for the same change has been made without apparent influence in the case of the Aino. The German sex differences are so remarkable and the divergencies from other races so anomalous, that one is driven to question the accuracy of measurement, of record or of arithmetic, until one sees the names of the measuring craniologist and the calculator! Clearly much more work remains to be done here, if possible on larger series, and where, if it can be provided for, craniologist and calculator are one. Meanwhile we must confess that the Naqada, Aino and English results strike us as the least improbable and inconsistent of the total material.

The craniologist who seriously examines these results must be convinced how little a study of one race can tell him of what is likely to occur in a second.

Capacity, and Height, Breadth and Length.

We have a certain amount of comparative data already available which is given in the table below:

TABLE XVI.

Race	*C* and *H*				*C* and *B*				*C* and *L*			
	♂		♀		♂		♀		♂		♀	
	No.		No.		No.		No.		No.		No.	
Naqada	86	·642 ± ·043	114	·519 ± ·046	89	·434 ± ·058	123	·532 ± ·044	89	·501 ± ·054	123	·599 ± ·039
German†	100	·243 ± ·064	99	·451 ± ·054	100	·672 ± ·037	99	·706 ± ·034	100	·515 ± ·050	99	·687 ± ·037
Aino†	76	·544 ± ·054	52	·521 ± ·068	76	·561 ± ·053	52	·502 ± ·070	76	·893 ± ·016	52	·663 ± ·053
Sioux ‡	57	· 44 ± ·07	—	—	57	·67 ± ·05	—	—	57	· 54 ± ·06	—	—
Means	—	·47	—	·50	—	·58	—	·58	—	·61	—	·65

---

* *Phil. Trans.* Vol. 187, A, p. 280.

† A. Lee: *Phil. Trans.* Vol. 196, A, p. 231.

‡ Boas: *The American Anthropologist*, Vol. I. p. 458. Boas apparently correlates the lengths with the cube root of the capacity, but the coefficients ought from theoretical considerations to be sensibly the same as if he had correlated with the capacity itself.

The means have been taken without weighting the series. On the whole the results here are much more consistent than in the last table, the Germans being most anomalous in C and H ♂, and the Aino in C and L ♂. The means probably give very fair average results, which will serve for general racial comparison. They show on the whole no very marked sexual preponderance in correlation, although such undoubtedly exists in individual races, e.g. German female is sensibly more, Aino female is sensibly less, correlated than the male. There is small doubt that these capacity correlations come out better than those between the lengths chiefly because they are higher, and accordingly the probable errors of random sampling less. Possibly the best method of dealing for the present with the latter will be to investigate them for long series of measurements on the living, replacing the total height of the skull by the auricular height of the head.

Capacity and Circumferences.

We have at present very limited data for these cases, only indeed the Thebans and Naqadas:

TABLE XVII.

Race	C and U				C and Q				Q and U			
	♂		♀		♂		♀		♂		♀	
	No.		No.		No.		No.		No.		No.	
Naqada*	84	·681 ± ·040	115	·723 ± ·030	84	·656 ± ·042	118	·603 ± ·039	84	·512 ± ·054	115	·454 ± ·050
Theban Mummies†	202	·813 ± ·016	96	·826 ± ·022	202	·788 ± ·018	96	·673 ± ·038	202	·665 ± ·027	96	·625 ± ·042

Considering the close relationship of Naqadas and Thebans these results are singularly different, but we must remember that it is precisely in length and breadth that differentiation has been shown to have taken place, and these would directly affect the correlationship of circumferences.

The historic Egyptians show in every case higher correlation than the Naqadas, and in both series with one exception the males are more highly correlated than the females. The divergence between the sets of values for two so closely allied series indicates how difficult it would be to reconstruct satisfactorily skull-capacity from any general formula connecting capacity and the circumferences.

---

* These results are for a rather more extensive series of skulls than those dealt with by Dr A. Lee: *Phil. Trans.* Vol. 196, A, p. 261.

† *Phil. Trans.* Vol. 196, A, p. 262.

Facial and Palate Measurements.

The correlation of breadth and height of orbit is fairly constant, say ·45, for both eyes in both sexes, the female orbit has its parts, however, somewhat more highly correlated. The same sexual advantage occurs more markedly in the length and breadth of palate and significantly, if less markedly, in height and breadth of face. On the other hand the male breadth and height of nose are more closely correlated.

We have absolutely no data available at present for comparative purposes.

Index Correlations.

Here again the results of the present paper embrace nearly all we at present know*. But they lead to several suggestive hints for further investigation. We may note that:

(*a*) In both sexes chamaecephaly is associated with platyrrhiny.

(*b*) In the male chamaecephaly is also associated with chamaeconchic and brachystaphyline characters, but in the female there is no really sensible relationship between the shape of palate or orbit and the height-length cephalic index. What little relationship there is tends to mark an association between chamaecephaly and hypsiconchic and leptostaphyline characters.

(*c*) In both sexes brachycephaly is associated with the leptorrhine and brachystaphyline characters.

(*d*) In the male brachycephaly is associated with hypsiconchic, in the female with the chamaeconchic character; in the latter sex the association is much smaller.

(*e*) In both sexes there is a quite sensible association of platyrrhiny with chamaeconchy, or when the nose is flat the eye is oval.

(*f*) In the male platyrrhiny is associated with the leptostaphyline character, but in the female there is practically no relationship of the nose and palate characters.

(*g*) In both sexes there is sensible correlation between the palate and orbital indices, hypsiconchy being associated with brachystaphyline characters.

All these results, it must be borne in mind, are relations between the characters of individuals *within the race*, and indicate how, if an individual differ from the mean in one character, he or she will be likely to differ from it in a second. They must not be extended without further consideration from association of deviations within the race to association of racial characters. Still a consideration of Table XIV. shows that they may be suggestive also in this direction. The correlation

---

* Dr Lee's correlations of cephalic index and capacity for Germans, Ainos, Thebans, Copts, Malays, Etruscans and French are the only other cases we know of: see *Phil. Trans.* Vol. 196, A, p. 232.

of the *mean values* of the chief cranial characters in 50 to 100 races would be a most valuable investigation, breaking practically untrodden ground. We want an *interracial* as well as this *intraracial* correlation, to show us how far it is safe to generalise from what occurs within a race to what will happen when we compare races together. If platyrrhiny is associated with chamaeconchy for individual Naqadas, is it right to generalise and say that the platyrrhine races of men are also chamaeconchic? Probably, but there is no proof, until someone has actually worked out interracial coefficients of correlation.

Cephalic Indices and Length, Breadth and Height.

The correlations of the cephalic indices are of such importance that a special consideration of them as well as their relationships to length, breadth and height was desirable. For comparative purposes the Paris catacomb crania were worked out at the same time. The formulae used in the investigations were those given by Professor Pearson in his paper "On a form of Spurious Correlation which may arise when Indices are used in the Measurement of Organs*." In every case where it exists, the value of spurious correlation has been calculated: see Table XIV. p. 456. In order, however, to test the accuracy of the results reached by these formulae from the recorded values of the coefficients of variation (Table XI.) and the length, breadth and height correlations (Table XIII.), in two cases the correlations were worked out *ab initio*, namely from the data given in the Appendix of measurements to this memoir. There resulted:

Correlated Pair	From Formula	From Measurements
B/L and H/L ♂	·490 ± ·018	·500 ± ·018
B/L and H/L ♀	·576 ± ·024	·572 ± ·025

The results are in such good agreement, well within the limits of the probable errors, that it seems unnecessary to deduce in future any such correlations directly from the measurements.

Now turning to our results themselves, Table XIV., we see that where the spurious correlation exists it is at least of the same order and very frequently sensibly larger than the gross correlation. In fact the organic correlation between L, B and H often tends to reduce the result considerably below the value it would have if the lengths, breadths and heights had been selected from the records in random triplets, i.e. below the spurious correlation. Thus the correlation of the two chief cephalic indices for male Naqada is reduced from ·604 to ·284. In the case of the French males it is raised from ·464 to ·489, which is fairly in keeping with Professor Pearson's result for Bavarians ♂, i.e. ·401 to ·486†, the only other comparable values at present known.

---

* *R. S. Proc.* Vol. 60, p. 493. Formula (iv) and (vi).

† *R. S. Proc.* Vol. 60, p. 495.

It will be seen at once from these results that if an individual tends to brachycephaly he will also tend to hypsicephaly. Further, we ask, are the brachycephalic races hypsicephalic and the dolichocephalic races chamaecephalic? Again we want our interracial coefficients of correlation to answer this problem satisfactorily.

If we turn to the correlation between the indices and the lengths they do not involve, we find:

(*a*) For both sexes in Naqadas brachycephaly is associated with hypsicranial characters. Among the French there is practically no association at all.

(*b*) For both sexes of the Naqadas hypsicephaly is associated with platycranial characters. On the other hand, for both sexes in the French, hypsicephaly is united with stenocranial characters.

(*c*) In both races platycephaly is associated with brachycranial characters—the association vanishing, however, for the Naqada males, and being more marked for the French.

Lastly turning to the relations between the indices and the absolute lengths they involve, we conclude that

(*d*) Dolichocephaly and chamaecephaly in both races are associated with macrocranial characters.

(*e*) Brachycephaly is associated with platycranial characters in both races.

(*f*) Hypsicephaly is associated in both races with hypsicranial characters.

In (*d*), (*e*) and (*f*) the association is really produced entirely or almost entirely by the spurious correlation.

Here, again, a whole range of racial problems are suggested which can only be dealt with by interracial correlation investigations.

It should be noted that throughout the above statements we have used technical terms for brevity in an *intraracial* sense.

By dolichocephalic, chamaecephalic and stenocephalic crania *within the race* we understand those of individuals having their B/L, H/L and B/H indices below the racial mean; by brachycephalic, hypsicephalic, platycephalic crania, those of individuals having the corresponding indices above the mean. By brachycranial, stenocranial and chamaecranial characters we describe those of individuals with cranial length, breadth and height below the racial mean, and by macrocranial, platycranial and hypsicranial characters those of individuals with length, breadth and height above the racial mean. As soon as we can find interracial means—means of racial means—we shall be able to use these terms also in an interracial sense, which will be somewhat less arbitrary than that of the *Frankfurter Verständigung*; that concordat really fixes rough numbers to represent unknown interracial results.

Here we must conclude for the present our discussion of the correlation of cranial characters. We see that except where one of the characters is capacity, all such correlations as have hitherto been calculated are remarkably small and remarkably divergent from race to race. Where they appear to be high, as in Table XIV., the result is solely due to what has been termed spurious correlation. Of course when many more characters have been dealt with, some high organic correlations may be discovered, but this discovery can hardly now upset the general principle that the bulk of cranial characters have far smaller correlation than the larger bones of the skeleton or than the bones of the hand. This principle must somehow be deducible from the general course of cephalic evolution in man, but until many more races have been statistically treated, if possible in far larger cranial series, and what we have termed interracial correlation coefficients have been determined, it would be idle to speculate on what this low correlation of cephalic characters really denotes from the standpoint of evolution*. One must be content at present to accumulate material for future interpretation. But enough has been indicated in this memoir to show the wide field which is open to craniologists, who will adopt the more recent mathematical methods in statistics. There is an immense amount of work to be done in tabling both intraracial and interracial means, variabilities and correlations. In many cases the measurements are already provided, but for certain characters it is very desirable that a revised and more definite concordat, international if possible, should supplement the *Frankfurter Verständigung*. The object of the present memoir has been to point out some defects of the older methods and to emphasize the importance of the new. The conviction of the mathematical contributors to this memoir is, that if they have done but little it is not the fault of the new methods, but of their individual want of skill, especially their want of previous anatomical training.

The measurements and calculations given in detail or in the form of coefficients in this memoir have been the labour of upwards of six years, and it may be proper to again refer here to the distribution of labour, and the many friends to whom acknowledgment of assistance and counsel is due. Two-thirds of the measurements and the first work on most of the calculations are due to the author; an independent verification of her calculations and a considerable number of additions are due to Dr Alice Lee, whose name is therefore added to the title. To Miss M. Lewenz are due the solutions of the triangles on which all the angle measurements depend, and the determination of the angular means and variabilities. To Mr N. Blanchard we owe the reductions of the Negro crania.

We have already referred to Mr Karl Tressler's work on the diagrams; to Mr Radford Sharpe's preparation of the photographs for the plates; to Mr Herbert Thompson's determination of some of the length, breadth and height measurements and a first series of capacities; and to Dr E. Warren's help in a variety of ways, especially in determination of sex and in the suggestions which flowed from his memoir on the Naqada skeletons. To Professor Thane we owe the most cordial thanks for his ever ready advice and assistance, and the same is true of Professor

---

* Compare, however, the relatively lower correlations of certain parts of the hand-skeleton, *Biometrika*, Vol. I. p. 359.

Weldon, who spent several days in going through the material with a view to noting anatomical peculiarities and selecting typical crania for reproduction. Lastly to Professor Flinders Petrie we owe not only the information acknowledged earlier, but the skulls themselves.

If the professed craniologist should feel aggrieved that such splendid material should have fallen at first into mathematical hands, he may console himself with the knowledge that the crania will be available for further work when they ultimately reach the Anatomical Museum at Cambridge. He must also remember that the material was dug up and brought to this country with this direct purpose in view—that it should be used for the illustration of statistical methods as applied to craniometry,—and that however little sympathy he may have with these methods, without them the present material would certainly not have been brought to England when and how it was. Let us hope that he will in the end pardon the method and even the errors of this paper for the sake of such material as it has indirectly made available for craniological purposes.

(11) *Summary of Conclusions.*

(*a*) Craniometry cannot in future content itself with either the raw measurements, tables of mere averages, or graphical exhibition of correlation results, but must adopt the methods of modern statistical investigation, tabulating means, variabilities, correlations, and their probable errors in order to draw safe inferences and make racial comparisons.

(*b*) The prehistoric Egyptians as represented by the Naqada crania appear to be as homogeneous as most short series which pass muster as racial unities.

(*c*) The Naqada race does not appear substantially nearer to the Negro—as judged by his modern representative—than the historic Egyptian as sampled in the Theban mummies or than the modern Copt.

(*d*) In some features only the Naqada crania are "primitive or inferior," in others they are "advanced or modern." In some characters they resemble the Negro, in others the European.

(*e*) The close resemblance in the majority of characters of Naqadas, Thebans and Copts leads one to believe that one is examining substantially the same race at intervals during 8000 years.

(*f*) The progressive divergence in certain characters of these three series of crania ought, we hold, to be attributed to an evolution tending in a fixed direction. If this be so we have an actual measure of the rate at which evolution can modify characters.

(*g*) The relationship between cranial characters as exhibited by their coefficients of correlation in the case of the Naqada and other races is seen to be low, and to vary much from race to race. It is therefore very doubtful how far it is legitimate to press results found for individuals of one race upon those of another. We cannot pass from intraracial to interracial conclusions, but we must work towards a knowledge of interracial correlation, and the first step in this direction should be to obtain the average values of some 40 to 50 characters in 50 to 100 races measured on some uniform plan. Only on such interracial correlations will it be possible to establish a properly founded statistical theory of race in man.

DESCRIPTION OF PLATES.

PLATE IV.

Craniometric Apparatus: see pp. 413—14.

PLATE V. *Male Crania.*

Q. 392 (Top)	T. 10^{B} (Top)	T. 29^{A} (Top)
Q. 392 (Front)	T. 10^{B} (Front)	T. 29^{A} (R. profile)
Q. 392 (Back)	T. 10^{B} (Back)	T. 29^{A} (Back) *Torus occipitalis*

PLATE VI. *Male Crania* (continued).

Q. 758 (Top)	530 (Top)	1755 (Front and mandible) Persistent frontal suture
Q. 758 (R. profile)	530 (R. profile and mandible)	1755 (R. profile and mandible)
Q. 392 (Base)	T. 10^{B} (Base)	T. 10^{B} (R. profile)

PLATE VII. *Female Crania.*

1308 (R. profile and mandible) Q. 326 Lower (L. profile)
1308 (Front) Q. 326 Lower (Front) R. 2 (Front)
Q. 326 Lower (Top) R. 2 (Top)

PLATE VIII. *Female Crania* (continued).

Q. 408^{D} (R. profile) R. 2 (R. profile)
Q. 326 Lower (Base) Q. 408^{D} (Base) R. 2 (Base)
Q. 408^{D} (Back) R. 2 (Back)
Juvenile, interparietal and wormian bones

PLATE IX. *Abnormal and Special Crania.*

1644 (R. profile) Curved dental arch	Q. 791 (R. profile) Depressed obelion	1300 (R. profile) Negroid, prognathous	1377 (R. profile) Fused atlas and axis
R. 5 (R. profile) Very strongly marked lower temporal line	Q. 383 (R. profile) Markedly convex and sloped occipitals	Q. 359^{2} (R. profile)	1446 (Base) Fused atlas

PLATE X. *Abnormal and Special Crania* (continued).

1587^{4} (Top) Much emphasised upper temporal lines	Q. 18 (Top) Scaphocephalic	1031 (Base) Markedly arched palate, characteristic worn molars	B. 83 (Domes) Much distorted infant skulls *
1587^{4} (Back) Much emphasised upper temporal lines	1587^{4} (R. profile)	Q. 513 (R. profile) Sloped occipital *Torus occipitalis*	T. 5^{F} (Top) Os anti-epilepticum, persistent frontal suture

---

* To indicate the immense possibilities of *postmortem* deformation, here of course on exaggeratedly plastic material.

(12) *Appendix of Tables of Cranial measurements.*

The Naqada Crania, as already stated, were brought to England in 1895, and deposited at University College, London. Unfortunately the cramped accommodation of that institution did not permit of any proper room being appropriated to them in term time. They occupied two small lecture rooms while Mr H. Thompson was measuring F, L, B, H and C. They were then transferred during vacation time to a gallery of the Anatomical Museum where Dr E. Warren was dealing with the skeletons. Thence they passed to the Biological Laboratory, where C. D. Fawcett started her work. Changes there involved their being removed to the South Library, where they were much in the way of other workers, and finally they were deposited in the Instrument Room of the Department of Applied Mathematics. It is necessary to emphasise these points, for they will explain why (i) it has never been convenient to have the whole series of skulls out for examination at the same time; (ii) it has only been possible with great labour to reach any individual skull required for re-examination, since the boxes had to be stacked in columns eight or ten high and in rows four or five deep. In many cases the skulls had been placed at some time or other by the packers in the wrong boxes, and quite a number of skulls or boxes had duplicate numbers. Lastly the whole material was (and remains of course) excessively fragile, and the repeated removals, however carefully conducted, were very far from desirable. In any case where a skull was known to be more perfect when first examined by Mr Thompson, than when the series was last gone through in January 1902, the *Remarks* contain the words "when broken?"

In the last examination of the whole series, Professor Pearson, with assistance in holding two or three fragments together, added several hundreds of measurements to C. D. Fawcett's series. Many of these are queried, but the remainder were obtained too late to be included in the determination of the statistical constants. Thus 28 additional values of the face-height of which only 55 values had been previously taken were found, and some of the other measurements were increased in the same proportion. They are now available, however, for any later comparison with other material.

At the same time Professor Weldon went through the whole series, dealing with probable age, sex in some unsettled cases and abnormalities. He further selected types for photographic reproduction. A select series was further considered later by Professor G. Thane, who kindly described some of their chief anatomical peculiarities.

But while most hearty thanks are due to Professors Thane and Weldon and also to Dr E. Warren for their aid, which has been most generously given whenever asked for, the statistical workers at University College must take upon their own shoulders all errors of description and blunders of measurements which occur in these tables. They have done their best to reduce them to a minimum, but the magnitude of the task undertaken, the time during which it has been in progress, and the novelty of the attempt—the endeavour to apply modern statistical methods for the first time to an original series of craniological data—must be some excuse for imperfections which undoubtedly will be found. The main object in view has been to indicate the mathematical theory which it will be necessary for the craniologist in the future to apply to similar data.

The following abbreviations have been used in the *Remarks*. cr. = *cranium*, i.e. skull + mandible. cal. = *calvarium* = skull − mandible. f. stands for face. Thus cal. − f. = skull − mandible − face bones, or what some Germans write *calvaria*, the "Hirnkapsel." dome = what some Germans write *calvaria'*, or the "Schadeldach" alone. Of course there are different stages of all these classes. Thus 'cal. − part f.' means that a portion of the face has disappeared.

TABLE I.

Fa					Mandible				Remarks
GH	$G'H$	GB	J	NH	W_1	W_2	h_1	f	
—	—	—	—	—	—	—	—	—	br. cr. ad. sut. closing
—	—	—	—	—	—	—	—	—	cal. – f. ad.
—	—	—	—	—	—	—	—	—	br. cal. – f. when br. ?
—	—	—	—	—	—	—	—	—	br. cal. when br. ?
—	60	95	119	44	—	—	—	—	cr. ad.
—	—	—	—	—	—	—	—	—	cal. – f.
—	68	103	125·75	47	—	—	—	—	cr. ad. supraorb. foramen
—	—	—	—	—	—	—	—	—	cal. occipital and atlas fused
—	—	—	—	—	—	—	32	—	frags. of dome and m.
—	—	—	—	—	—	—	—	—	dome frags. ad. partly persist. front. sut.
115	69·25	97	122 ?	50·5	88	100	33·75	44	cr. y. ad.
—	67	100·25	—	50	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	—	frag. of dome
—	—	—	—	—	—	—	—	—	cal. – part f. adolesc.
117 ?	72·5	92	—	56·5	105·75	120 ?	36·5	47	cr. ad. supraorb. foramina. small wormian b.
—	—	—	—	—	—	—	—	—	br. dome
—	58	79	—	49	—	—	—	—	badly br. cal.
113 ?	71	—	—	49	98·75	106	32 ?	48	badly br. cr.
—	—	—	—	—	—	—	—	—	cal. – f. y. ad. interpar. and wormian bs.
—	—	—	—	—	—	—	—	—	cal. – f. ad.
—	—	—	—	—	—	—	—	—	br. dome old.
102·5	60	89	—	42	91	107 ?	30 ?	43	br. cr. ad.
—	—	—	—	—	—	—	—	—	cal. – f. ad.
—	—	—	—	—	—	—	—	—	dome badly br.
—	59·75	85·5	—	46	—	—	—	—	br. cal. [and sagit. suts. closed
—	—	—	—	—	—	—	—	—	dome v. asymmetrical s. interpar. b. coron.
—	—	—	—	—	93	118	32	43	cal. – f. + m. ad. coron. and sagit. suts. closed
—	69	98	143	51·3	—	—	—	—	cal. base br. ad. 3rd occipit. condyle
—	68·5	—	—	48	—	—	—	—	dome + frags. of f. y. ad.
—	—	—	—	—	—	—	—	—	br. dome
—	67·5	90	—	49	—	—	—	—	br. cal. adolesc.
—	—	—	—	—	—	—	—	—	br. cal. – f.
—	68·8	93	115 ?	49	—	—	—	—	cal.
106	64	89·5	121·75	47·5	177·75	112·25	28	43·5	cr. ad.
—	—	—	—	—	—	—	—	—	dome ad.
—	—	—	—	—	—	—	—	—	cal. – f.
—	—	—	—	—	—	—	—	—	br. dome + frags. y. ad. asym. pariet.
—	—	—	—	—	—	—	—	—	br. cal. – f. ad.
—	—	—	—	—	—	—	—	—	frag. of dome + frontal
—	—	—	—	—	—	—	—	—	cal. – f. ad.
—	—	—	—	—	—	—	—	—	br. cal. – f.
—	69	104	—	52	—	—	—	—	cal. ad. ? m. curved dental arch
—	61 ?	—	—	—	—	—	—	—	f. + frags. adolesc. supraorb. foram.
—	66·5	95·5	—	50·5	—	—	—	—	br. cal.
—	—	—	—	—	—	—	—	—	cal. + frags. of f. y. ad. pterion
109 ?	69·5	101	—	47·7	—	—	34·5	—	cr. ad. supraorb. foram. persistent front. sut.
—	—	—	—	—	—	—	—	—	frags.
—	65	93	124·6	45	—	—	—	—	cal. old.
—	—	—	—	—	93	110?	33	—	cal. – f. y. ad. [and occipital
—	65	89·5	116	46	—	—	—	—	cal. y. ad. small wormian bs. between l. parietal
—	60·5	90	—	47	—	—	—	—	cal. y. ad. wormian bs. l. ant. condylar foram.
—	—	—	—	—	71	88	28	38	cal. – f. + m. adolesc. hair [fails
—	68	86	114	50	—	—	—	—	cal. y. ad.
108	65	96	120	46	84·25	104	30	43	cr. ad. hair
—	—	—	—	—	—	—	—	—	cal. – f. ad.
—	70·75	102	131	51·5	—	—	—	—	br. cal.
116	69	97	—	48	87·5	108	33	43·5	cr. ad.
110	65·5	95·5	120 ?	46	89·5	98·5	32	43·5	cr. y. ad.
—	—	—	—	—	82	86	29	—	frags. y. ad.
—	75	96	—	53	99 ?	—	32 ?	—	frontals + parietals + frags.
106·25	68	98	122	48	94·5	106 ?	31·25	44·5	cr. ad.
—	—	—	—	—	95	112·5	29	45·5	frags. of cr. ad. wormian bs.
—	—	—	—	—	—	—	31	—	cal. – f. ad. inclined occipital
—	—	—	—	—	85 ?	—	31	46·5	cal. – f. + frags. + m. ad. [hair
—	72	96	—	52		—	—	—	cr. ad. small interpar. b. very l. styloid proc.

—78 frags. $B'=94{\cdot}5$.—86 parietals.—ne.—336, 339, 378, 388, 398 frags.—427 dome old.—436, 492 frags.—

s. persistent front. sut.—502, 523, 52

MEASUREMENTS OF NAQADA CRANIA.

TABLE I.

				Lengths								Circumferences			Face										Palate		Indices												Angles						Mandible				Remarks
Series No.	Grave No.	Sex	C	F	L'	L	B	B'	H	OH	LB	U	S	Q	GH	$G'H$	GB	J	NH	NB	O_1 L	O_1 R	O_2 L	O_2 R	G_1	G_2	B/L'	H/L'	B/L	H/L	B/H	GH/GB	$G'H/GB$	NB/NH	O_2/O_1 L	O_2/O_1 R	G_2/G_1	GL	$N\angle$	$A\angle$	$B\angle$	θ_2	θ_1	$P\angle$	W_1	W_2	h_1	f	
1	3	♀	—	—	—	179?	—	—	—	104 (h)	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	br. cr. ad. sut closing
2	7	♀	—	179	—	179	138	92	—	115 (h)	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	77·1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	cal. – f. ad.
3	12	♂	—	184·5	—	183	135·5	—	132·5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	74·0	72·4	102·3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	br. cal. – f. when br.?
4	13	♀	1151	165		166·2	129·5	—	133·5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	77·9	80·3	97·0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	br. cal. when br.?
5	15	♀	1184	166	166	166·5	129·6	87	124	110	86	476	357	289	—	60	95	119	44	22·5	42	42·75	33	32·75	50	40·5	78·1	74·7	77·8	74·5	104·5	—	63·2	51·1	78·6	76·6	81·0	83	68°·8	74°·6	36°·6	4°·9	31°·7	79°·5	—	—	—	—	cr. ad.
6	16	[illegible]		188		188	[illegible]		[illegible]																				[illegible]	[illegible]	[illegible]																		cal. [illegible] f
7	52	♂	1345	181	182	182	134·5	89·5	128	112·5	99	507·5?	360?	293·5?	—	68	103	125·75	47	25·5	44	44·5	36	35	55	44	73·9	70·3	73·9	70·3	105·1	—	66·0	54·3	81·8	78·7	80·0	99·25	73°·1	72°·2	34°·7	10°·05	24°·65	82°·25	—	—	—	—	cr. ad. supraorb. foramen
8	53		[illegible]	[illegible]		[illegible]			[illegible]																				[illegible]	73·4	[illegible]							[illegible]						[illegible]					[illegible]
8	85			—	—	178?	136?	94	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	76·4?	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	32	—	frags. of dome and m.
9	108		—	—	—	188?	136·5	103	—	106?		—			—														[illegible]																				[illegible] frags. ad. partly persist. front. sut
10	114		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	310	115	[illegible]	[illegible]	[illegible]	[illegible]	23	41	42·8	32·5	32	53·5	[illegible]	78·8	74·2	78·8	74·2	[illegible]	118·6	71·4	[illegible]	[illegible]	74·8	[illegible]	[illegible]	68·1	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	88	[illegible]	33·75	44	cr. [illegible] ad.
11a	121	♀	1440	—	181·5	179	139·5	90·5	141	119	94·5	509	382	313	—	67	100·25	—	50	25	41	40	34·75	35	51?	40	70·9	77·7	78·0	78·8	99·0	—	66·7	50·0	84·8	87·5	78·4	—	—	—	—	—	—	—	—	—	—	—	[illegible]
11b	160	?	—	—	—	190	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	frags. of dome
12	193	♂	1264	182	181	181·5	128	92	134·5	114	93	498	376	303	—	—	—	—	—	—	44	—	36	—	—	—	70·7	74·3	70·2	74·1	95·2	—	—	—	81·8	—	—	—	—	—	—	—	—	—	—	—	—	—	cal. – part f. adolesc.
13	245		1433	185	188·5	188	139	100	129·5	114·8	100	521	370	313·5	117?	72·5	92		56·5	26	45·5	43·5	33·5	34·5	54·5	44·5	73·7	68·7	73·9	68·9	107·3	127·2?	78·8	46·0	73·0	79·3	81·7	93·5	64°·8	75°	40°·2	9°	31°·2	84	105·75	120	30·5	47	cr. ad. supraorb. foramina small wormian b
14	282		—	—		—	—	89·5?	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	br. dome
15	314	[illegible]		—	182?	—	—	85	—	116	80?	—		308?		58	[illegible]		49	27	38	38	33	31	45	42							[illegible]	55·1	86·8	81·6	[illegible]												[illegible] br. cal
16	315			[illegible]		[illegible]	[illegible]		[illegible]					[illegible]	113	71			[illegible]	25				32	[illegible]	44			[illegible]	[illegible]	[illegible]			51		[illegible]	[illegible]	[illegible]						75·5	[illegible]	[illegible]	[illegible]	[illegible]	[illegible] br. cr
17	316		[illegible]	[illegible]		[illegible]	[illegible]			[illegible]		[illegible]		[illegible]													71·5	72·3	[illegible]	[illegible]	[illegible]																		[illegible] interpar. and wormian bs
18	328	♂?				174	128·5	70	123·5	112 (h)	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	73·9	71·0	104·0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	cal. – f. ad
19	342	♀	—	179·5	176	181	131·5	91·5		112	—	500	—	294	—	—	—	—	—	—	—	—	—	—	—	—	74·7	—	72·7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	br. dome old.
20	358				166?	160	128	[illegible]	[illegible]	111 (h)	[illegible]		[illegible]		102·5	[illegible]	[illegible]		42	[illegible]	42	43	32	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	58·3	[illegible]	[illegible]	[illegible]	95·5	74	[illegible]	[illegible]	[illegible]	25	[illegible]	[illegible]	[illegible]	[illegible]	43	br. cr. ad
21	366		1373	[illegible]		[illegible]	[illegible]	[illegible]		116		512	304	305	—														74·9																				cal. – f. ad.
22	368	♂?	—	—	185·5	—	138	—	139	117·5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	74·4	74·9	—	—	99·3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	dome badly br.
23	384	?	—	—	177	—	—	87	—	123	93	488	387·5	314·5	—	50·75	85·5	—	46	22	38	37	30	29	52·5	36							[illegible]	47·8	78·9	78·3	68·6												
24	386							[illegible]		[illegible]					—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	br. cal [and sagit. suts. closed
25	391	♀		176	177	176	138	93	120	111·75?	93·5	502	[illegible]	308	—	—	—	—	—	—	—	—	—	—	—	—	78	67·8	78·4	68·2	115·0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	dome v. asymmetrical s. interpar. b. coron.
26	393		[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		317·5		[illegible]	[illegible]	143	51·3	25	[illegible]	47	[illegible]	35·5	59	[illegible]	72·2	71·0	72·5	72·0	100·7		70·4	48·7	[illegible]	75·5	[illegible]	97·5	59·7	[illegible]	[illegible]	[illegible]	30·3	84	63	118	32	43	cal. f. m. ad. coron. and sagit. suts. closed
27	396					197·5	150·5	94·5	147·75	112 (h)		547		—	—																																		[illegible] ad. 3rd occipit. condyle
28	426	♂?	—	—	—	—	—	84	—	—	—	—	—	—	—	68·5	—	—	48	22	—	39	—	33·5	49?	36?	—	—	76·2	74·8	101·9	—	—	45·8	—	85·6	73·5	—	—	—	—	—	—	—	—	—	—	—	dome + frags. of f. y. ad.
29	519a	♀	—	—	186	184	127?	88·5	131	113·75	99	—	382	298	—	67·5	90	—	49	23·75	41	43	32	31·5	51·5	36	68·3	70·4	69·0	71·2	96·9	—	75·0	48·5	78·0	73·3	69·9	95·5	69°·0	76°·5	33°·6				—	—	—	—	br. dome
30	522														—																																		br. cal. adolesc.
31	527			[illegible]	[illegible]	177	[illegible]	[illegible]	129	106	90	485	350	272?	—	68·5	93	115?	49	20	41	39	32	32	60	43	72·4	72·7	72·6	72·9	99·6	—	74·0	53·1	78·0	82·1	71·7	—	—	—	—	—	—	—	—	—	—	—	br. cal. f
32	530	♂	1320	177·5	178	178	130·5	89	123	111	92	499	351	208	106	64	89·5	121·75	47·5	27	42	42	31	31	58	38	76·7	69·1	76·7	69·1	111·0	118·1	71·5	56·8	73·8	73·8	65·5	95	75°·1	69°·1	35°·2	11°·77	30°·13	81°·17	177·75	112·75	28	43·5	cal
33	531					[illegible]		[illegible]		[illegible]		494		—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	73·4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	cr. ad
34a	534	♂	—	174·5	—	178	134·5	—	126					—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	75·6	70·8	107·0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	dome ad
34b	536	?	—	—	—	191·5	—	—	—				384	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	cal. – f.
35a	541	♀	—	191	188	189·5	139·5	93	134·5	120	96	521?	390	321?	—	—	—	—	—	—	—	—	—	—	—	—	74·2	71·5	73·6	71·0	103·7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	br. dome + frags. y. ad. asym. pariet.
35b	548	?	—	—	—	—	138?	85	—			—		—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	br. cal. – f. ad.
36	554?	♂		—	—	177	133·5	95·75	134	112·5	101·5	495?	355	296?	—	—	—	—	—	—	—	—	—	—	—	—	—	—	75·4	75·7	99·6	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	frag. of dome + frontal
37	592	♂	—	—	191	191	129	89·8	133	111·6?	—	518?	378·5	293?	—	—	—	—	—	—	—	—	—	—	—	—	67·5	69·6	67·5	69·6	97·0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	cal. – f. ad.
38	598	♂?	—	—	189	190	130·5	95	136	119	96	533?	384?	321·5	—	69	104	—	52	25	42	—	32	—	55	46·5	73·8	72·0	73·4	71·6	102·2	—	66·3	48·1	76·2	—	84·5	—	—	—	—	—	—	—	—	—	—	—	br. cal. – f.
39	618	♂?	—	179		180	126·5	89·5	131		—	—		—	—	61?	—	—	—	—	39·5	—	31	—	—	—	—	—	70·3	72·8	96·6	—	—	—	78·5	—	—	—	—	—	—	—	—	—	—	—	—	—	cal. ad. ? m. curved dental arch
40	646	♂	—	—	192?	—	—	92	—	123	101	—		—	—	66·5	95·5	—	50·5	25·5	39·5	39·75	26·5	25	59	35	—	—	—	—	—	—	69·6	50·5	67·1	62·9	58·6	—	—	—	—	—	—	—	—	—	—	—	f. + frags. adolesc. supraorb. foram.
41	660	♂	—	179	182	181	128·5	89	127	113·5		498	361	292	—	—	—	—	—	—	—	—	—	—	—	—	70·6	69·8	71·0	70·2	101·2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	br. cal.
42a	693		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]		[illegible]		[illegible]																																			cal. + frags. of f. y. ad. pteron
42	695														109?	[illegible]	[illegible]		47·75	26	[illegible]	45	[illegible]	35	57	[illegible]	[illegible]	72·3	75·4	[illegible]	[illegible]	107·0	[illegible]	54·5	73·4	[illegible]	77·2	97	74°·7	[illegible]	35	13°·2	21°·8	[illegible]			34·5		cr. ad. supraorb. foram. persistent front. sut
43	717		[illegible]	[illegible]	[illegible]	172	134	[illegible]	[illegible]	111·25	93	488·5	354	297	—	65	93	124·6	45	24·5	42·25	42	32	31·8	—	—	77·5	72·3	77·9	72·7	107·2	—	69·9	54·4	75·7	75·7	—	82	61°·1	82°	36°·9	6°·5	30°·4	88°·5	—	—	—	—	frags.
44	718	♂	1150	180	186·5	184	132	97	124·5	110·5	102	593	361	293	—	—	—	—	—	—	—	—	—	—	—	—	70·8	[illegible]	[illegible]	[illegible]	[illegible]	—																	cal. old.
45	724	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]		[illegible]	[illegible]		[illegible]		[illegible]	[illegible]	116	46	[illegible]	[illegible]	[illegible]	31·5	31	[illegible]	32·5	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	74·2	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	33		cal. f. y. ad. [and occipital
46	732		[illegible]	[illegible]	[illegible]		[illegible]		[illegible]	[illegible]	[illegible]	[illegible]		[illegible]		60·5	[illegible]		47	24	[illegible]	[illegible]	[illegible]	32	45·5	[illegible]	[illegible]	[illegible]	[illegible]	75·1	106·2		67·2	51·0	88·2	82·0	[illegible]	85·5						89·5					[illegible] wormian bs between l. parietal
47	733																										73·4	73·4	73·1	73·2	100·0	—																	cal. y. ad. wormian bs l. ant. condylar foram.
48	749	♀?	1175	175	174	174	129·5	90	131	114·8	92·5	486	365	[illegible]	—	68	86	114	50	22	40	41	33	32	51·5	38	74·4	75·3	74·4	75·3	[illegible]		79·1	44·0	82·5	78·0	73·8	82·25	61°·2	77°·7	[illegible]	[illegible]	[illegible]	80	71	88	28	38	cal. – f. + m. adolesc. hair [fails
49	751	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]		11																																							cal. y. ad.
50	751		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]		[illegible]		[illegible]	108?	[illegible]	[illegible]	[illegible]	[illegible]	25·5	42	41·5	32	[illegible]	54	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	112·5	[illegible]	55·4	[illegible]	[illegible]	82·1	91	70°·2	[illegible]	34°·1	5°·5	[illegible]	81	84·25	[illegible]	[illegible]	43	cr. [illegible] hair
51	758	♂	—	—	195·5	—	—	96	—	120	107	534·5	394	310	—	70·75	102	131	51·5	26	44	42·5	31·5	31	64	46	72·8	69·6	73·2	69·9	104·7	—	69·4	50·5	71·6	72·9	71·9	—	—	—	—	—	—	—	—	—	—	—	cal. – f. ad
52	801	♂	1310	177	178	178	132·5	90	130	114·5	102	[illegible]	363	[illegible]	116	69	97		[illegible]	26	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	74·4	[illegible]	74·4	[illegible]	[illegible]	[illegible]	71·1	54·2	72·1	72·1	[illegible]	—	—	—	—	—	—	—	—	—	—	—	br. cal.
53a	832		[illegible]	[illegible]		[illegible]	[illegible]			[illegible]		[illegible]			110	65·5	95·5	[illegible]	[illegible]	20	[illegible]	44·75	31	32	54·5	[illegible]	71·1	[illegible]	[illegible]	[illegible]	[illegible]	115·2	[illegible]	50·5	[illegible]	71·5	71·5	[illegible]	67°·1	[illegible]	[illegible]	8°·2	27°·4	[illegible]	[illegible]	[illegible]	33	43·5	cr. ad
53	842																																					95	68°·6	77°·5	33°·6	9°·5	24°·1	87	[illegible]	95·5	32	43·5	cr. y. ad
54	844			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]																																			82	86	29		frags. y. ad.
55	862		[illegible]	[illegible]	[illegible]	[illegible]	131	[illegible]	[illegible]	[illegible]																																			[illegible]		[illegible]		frontals + parietals + frags.
55a	864	♂	—				—	88	—	—						75	90		53	24	[illegible]	44	34	31			75·1	77·3	74·7	[illegible]	97·2		[illegible]	45·5	77·3	[illegible]													cr. ad.
56	867	♂?	1395?	184	186·5	185·5	144·6	93	131·5	121·8	—	517·5	382?	[illegible]	[illegible]	65	[illegible]	122	48	25·5	40·5	41	29	29·75	52·8	38·75	71·5	70·1	72·0	70·5	101·9	108·4	69·4	53·1	71·6	72·6	73·4	80	64°·2	78°·9	36°·9	10°·1	26°·8	89°	94·5	106?	31·25	44·5	frags. of cr. ad. wormian bs
57	871					[illegible]							[illegible]		—	—	—	—	—	—	—	—	—	—	—	—	77·5	70·5	[illegible]	70·0	[illegible]		—		—	—		—	—	—	—	—	—	—	95	112·5	20 31	45·5	cal. f. ad. m. med. occipital
58	884		[illegible]	[illegible]	184·5	185·5	126·5	89·5	134	115·75	97·5	508	370	[illegible]	—	72	96	—	52	26	49	48·5	37·5	37	50	37	69·6	72·6	70·0	73·0	95·9	—	[illegible]	[illegible]	76·5	[illegible]	[illegible]	92	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	85?	—	31	46·5	cal. – f. + frags. + m. ad. [hair cr. ad. small interpar. b. very l. styl. ad proc.

Miscellanea: 18 frags.—28 frags. B' = 96 [illegible].—29 frags.—39 frontal + br. parietals.—71 frags. B' = 88·5.—78 frags. B' = 94·5.—86 parietals.—101 frags. of two crania.—108^b, 108^c, 118, 116, 140, 144, all frags.—201 frags. B' = 86·5.—212^a frags. B' = 99.—212^b frags. B' = 91. 217 frags.—279 frags. of several crania.—284 frags. of dome.—336, 339, 378, 388, 398 frags.—427 dome old.—436, 492 frags.—496 frags. persistent front. sut.—502, 523, 525, 560, 594 *b* and *c*, 662, 683, 695 all frags.—712 frags. + broken frags. of ribs, vertebrae and fingers.—708 *B* frags. B' = 93·5.—711 frags. B' = 94.—823 frags.—851 frags.

TABLE II.

				MANDIBLE				Remarks
GH	*G′H*	*GB*	*J*	W_1	W_2	h_1	*f*	
—	61·5	—	—	—	—	—	—	cal. y. ad.
—	—	—	—	—	—	—	—	dome, frags. of f. + m. pterion [on r.
—	—	—	—	—	—	—	—	cal. – f. wormian b. between pariet. and occip.
—	—	96	—	—	—	—	—	cal. ad.
104	62·5	95	124	91	117	30·5	45·5	cr. old, pterion, supraorb. foram.
106·75	62·5	93	114	92	109·5	31	43	cr. ad. pterion on r.
120	66·5	93·5	—	98	117·25	34·25	46	cal. + br. m. ad. hair
—	62	88 ?	—	—	—	—	—	f. + frags. adolesc.
—	72	107	—	—	—	—	—	cal. old, suts. nearly gone
—	65	—	—	—	—	—	—	cal. + frags. of f. y. ad.
—	60·5	92·5	—	—	—	—	—	cal.: negroid, prognathous, lower margin nasal aperture rounded off, very small nasal spine, almost suppressed, flat nasal bone
—	—	—	—	—	—	—	—	cal. + frags. of f. adolesc. persist. front. sut.
100	63	90·5	118	85	106·75	29	43	cr. ad.
—	—	—	—	—	—	—	—	frags. without frontal, y. ad.
—	72·5	—	—	—	—	—	—	frags. + frags. of another skull, eyes remain
—	—	—	—	—	—	—	—	frags. of dome
—	—	—	—	—	—	—	—	frags. of dome
—	—	—	—	—	—	—	—	much br. cal. old, large interpar. b.
—	—	97·5	—	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	br. dome
—	62	92	111	—	—	—	—	cal. – f. adolesc. hair
103·75	60	91	113	83·25	106	33	41	cr. ad. wormian bs. hair
—	60	95	—	—	—	—	—	br. cal.
—	—	—	—	—	—	—	—	br. cal.
—	—	—	—	—	—	—	—	dome y. ad.
—	—	—	—	90	106	30	—	cal. – f. + frags. ad.
—	69 ?	93·5	—	—	—	—	—	cal. old, small wormian b.
—	—	—	—	—	—	—	—	br. dome
103·5	61	92·75	—	—	—	29·25	—	cr. + br. m., flattened at back
117	70	95	—	94·5	110·5	35	47	cr. ad. fused atlas, right only ankylosed,
—	55·5	91	—	—	—	—	—	br. cal. child [probably effect of disease
—	70	98·25	—	—	—	—	—	br. cal. ad.
116·5	69·5 ?	97·5	120	93	101·75	31	41	cr. ad.
—	—	—	—	100	118·5	36	48	dome + m.
—	58	90	119·5	—	—	—	—	cr. adolesc.
—	—	—	—	95	116	30·5	44	cal. – part of f. ad. pterion, hair
—	—	—	—	—	—	—	—	frags.
112	65·5	92	—	102·5	—	33	43	cr. ad.
—	—	—	122 ?	—	—	—	—	cal. – f. ad. supraorb. foram. injured frontal
—	—	—	—	—	—	—	—	cal. – parts of f. adolesc. interp. and wormian bs. persist. front. sut.
—	65	101	—	—	—	—	—	cal. ad. sagit. suts. largely closed
—	69·5	94	120	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	m. br. cal. ad.
—	66·5	98	117·5	—	—	—	—	cal. old, sm. interp. b.
—	—	—	—	—	—	—	—	dome
105·5	62	93	—	85·25	108	30	42·5	cr. ad. union of atlas with occipital bone, secondary result of disease, interpar. and wormian bs.
—	69	87	123·5	—	—	—	—	cal. ad. curved dental arch
—	—	—	—	—	—	—	—	cal. – f. ad. irregul. wormian bs.
—	—	—	—	—	—	—	—	br. cal., when br. ?
—	66	93	—	—	—	—	—	cal. ad. pterion, interp. b.
—	—	—	—	96	117·5	35·25	—	frags. of cal. + m. [some hair
—	71	—	—	—	—	—	—	much br. cal. ad. place for extra r. molar,
—	—	—	—	95·75	101·5	35	43	cal. – f. + br. m. old
114·75	69	97	120	89·5	111	35	44	cr. br. f. pterion
107·5	64·25	—	116 ?	78	96·5	32	44	cr. ad. hair, outgrowth
108	—	—	—	83·5	109·25	28·75	—	br. cal. – f. + m. when br. ? persist. front. sut.
118	69·75	102	123·5	83 ?	102	31·25	41·5	cr. ad. [proc.
—	—	—	—	—	—	—	—	cal. – f. y. ad. supraorb. foram. long styloid
—	69	—	—	95·5	—	33·75	46	br. cal. + br. m. ad. hair
111	71·5	—	—	—	—	—	—	cr. old, l. side f. br.

nea: 1047 frags.—1234^{a} frags

MEASUREMENTS OF NAQADA CRANIA—*continued*.

TABLE II.

Serial No.	Grave No.	Sex	C	Lengths								Circumferences			Face										Palate		Indices											GL	Angles						Mandible				Remarks
				L	L'	l	B	B'	H	OH	LB	U	S	Q	GH	G'H	GB	J	NH	NB	O₁ L	O₁ R	O₂ L	O₂ R	G₁	G₂	B/L	H/L	B/L'	H/L'	B/H	GB/GH	G'H/GB	NB/NH	O₂/O₁ L	O₂/O₁ R	G₂/G₁		N∠	A∠	B∠	θ₁	θ	P∠	W₁	W₂	h₁	f	
59	1031		[illegible]	[illegible]	[illegible]	[illegible]	121·2	92	120	115	97	492·5	367	[illegible]		61·5			44	25		38		29·5	58	35·5	[illegible]	70·7	[illegible]	71·3	[illegible]			56·8		77	60·4	97	73°·75	73°·75	32°·5	[illegible]	23°·5	82°·75			—		cal. y. ad.
60	1037			181		[illegible]	[illegible]			[illegible]								—	—	—	—	—	—	—	—	—	—	—	75·5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	dome, frags. of f. + m. pterion [on r.
61	1037			[illegible]		[illegible]	[illegible]											—	—	—	—	—	—	—	—	—	—	—	76·4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	cal. – f. wormian b. between pariet. and occip.
62	1101		[illegible]	188	185	[illegible]	128	[illegible]	131	117·5	[illegible]	[illegible]	[illegible]	[illegible]			90	—	48	25·75	44·5	45	33·5	34	—	38	69·2	70·8	68·3	69·9	97·7	—	—	53·6	75·3	75·6	—	91·5	70°·2	72°·8	37°	13°·2	23°·8	86°	—	—	—	—	cal. ad.
63	1102		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	535	[illegible]	[illegible]	114	62·5	95	124	46	25	43	43·5	32	30·5	57	43	73·5	73·2	73·3	73·0	100·4	109·5	65·8	54·3	74·4	70·1	75·4	99·75	74°·3	72°·7	33°	12°·3	20°·7	85°	91	117	30·5	45·5	cr. old, pterion, supraorb. foram.
64	1105		[illegible]	[illegible]	[illegible]	170	[illegible]	[illegible]	[illegible]	108	91·5	[illegible]	367·5	282	[illegible]	[illegible]	93	114	47	22	44	44	34	34·75	50·5	39	75·4	76·2	75·1	76·5	98·8	114·8	67·2	46·8	77·3	79·0	77·2	91·5	72°·2	72°	35°·8	9°·75	26°·05	81°·75	92	109·5	31	43	cr. ad. pterion on r.
65	1109			[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	114	[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	—	45	24	44·25	43	30·5	31·5	50·5	40·5	74·4	73·9	74·9	74·3	100·7	128·3	71·1	53·3	68·9	73·3	71·7	—	—	—	—	—	—	—	98	117·25	34·25	46	cal. + br. m. ad. hair
65	1201							[illegible]								[illegible]	[illegible]		49	24	—	—	31	—	47	35	—	—	—	—	—	—	70·5	49·0	—	—	74·5	—	—	—	—	—	—	—	—	—	—	—	f. + frags. adolesc.
66	1212		[illegible]	175·5	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	115·5	[illegible]	[illegible]	[illegible]	[illegible]		72	[illegible]	—	50·5	29	46	45	36·5	37·5	62	—	75·6	70·1	77·1	71·5	107·8	—	67·3	41·4	79·3	83·3	—	99	69°·8	74°·6	35°·6	7°·65	27°·95	82°·25	—	—	—	—	cal. old, suts. nearly gone
67	1234			[illegible]			[illegible]	[illegible]		[illegible]		[illegible]		[illegible]		65		—	49·5	24	—	39	—	28	—	37·5?	73·7	—	73·7	—	—	—	—	48·5	—	71·8	—	—	—	—	—	—	—	—	—	—	—	—	cal. + frags. of f. y. ad.
68	1300	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	95	494	358	[illegible]		66·5	92·5		45	22	41	43	31	33	58	39	72·8	69·0	[illegible]	[illegible]	[illegible]		65·4	49·0	75·6	76·7	67·2	101	81°·5	68°	30°·5	14°·5	16°	82°·5	—				cal. + negroid, prognathous, lower margin nasal aperture rounded off, very small nasal spine, almost suppressed, flat nasal bone
69	1307			175·5	171	173	[illegible]	88	124	108	[illegible]	487	351	[illegible]													[illegible]	[illegible]	73·4	[illegible]	[illegible]																		cal. + frags. of f. adolesc. persist. front. sut.
70	1308		[illegible]	[illegible]				85	[illegible]	113·25	[illegible]	475	354	[illegible]	[illegible]	63	[illegible]	118	41	24·5	[illegible]	[illegible]	27	27·25	52	35	[illegible]	77·1	76·2	77·4	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	67·3	97	[illegible]	[illegible]	32	[illegible]	[illegible]	[illegible]	85	106·75	29	43	cr. ad.
71	1315						[illegible]		142	[illegible]																					97·2																		frags. without frontal, y. ad.
71'	1319															77·5			51	24	[illegible]	43	31·5	[illegible]										47·0	74·1	79·1	—												frags. + frags. of another skull, eyes remain
72	1329			[illegible]		[illegible]	[illegible]		[illegible]									—	—	—	—	—	—	—	—	—	—	—	75·3	69·8	107·9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	frags. of dome
73	1335			[illegible]		171	[illegible]		[illegible]									—	—	—	—	—	—	—	—	—	—	—	77·2	71·9	107·3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	frags. of dome
74	1336			[illegible]		[illegible]	[illegible]																						65·7								—	—											much br. cal. old, large interpar. b
75	1338	♀	1225		172	171	131	88	136	113	89	476·5	354	292	—	—	97·5	—	45	27	42	42	31	30·5		-	76·2	79·1	76·6	79·5	96·3	—	—	60·0	73·8	72·6	—	92·5	77°·7	69°·8	32°·5	9°·7	22°·8	79°·5	—	—	—	—	cal. ad.
76	1338a	♀		—	—	184	125·8	—	—	—	—		—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	68·4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	br. dome
77	1344							81		[illegible]		[illegible]	[illegible]	[illegible]		62	[illegible]	111	[illegible]	21	[illegible]	[illegible]	33·5	31·5	50	47·75	74·1	[illegible]	75·2	[illegible]	[illegible]		67·4	45·7	[illegible]	[illegible]	[illegible]	—											cal. – f. adolesc. hair
78	1349		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	81	[illegible]	115	92	[illegible]	374	[illegible]	105·75	[illegible]	94	113	47	25	44	44	[illegible]	[illegible]	[illegible]	[illegible]	70·2	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	65·0	53·2	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	33	41	cr. ad. wormian bs. hair
79	1350			[illegible]			[illegible]		[illegible]		[illegible]		[illegible]			[illegible]	[illegible]		41·5	24	44	[illegible]	31	32	55	[illegible]			[illegible]	71·5	[illegible]		63·2	53·7	70·5	73·0	[illegible]	—											br. cal.
80	1358	♂	1301	185	180	187	137	94	136·5	117	100	512	383	305	—	—	—	—	—	—	—	—	—	—	-	—	73·7	73·4	73·3	73·0	100·4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	br. cal.
81	1358a	♀	—	184	184	184	125·8	92·5	—	114·5?	—	—	367	—	—	—	—	—	—	—	—	—	—	—	—	—	68·4	—	68·4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	dome y. ad.
83	1363	♀	-	175		174·5	132	91·75	132·5	116	90	495	371	305?	—	—	—	—	—	—	—	—	—	—	—	—	—	—	75·6	75·0	99·6	—	—	—	—	—	—	—	—	—	—	—	—	—	90	106	30	—	cal. – f. + frags. ad.
83	1364		[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	351	[illegible]		[illegible]	[illegible]		52·75	[illegible]	[illegible]		[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	79·5		[illegible]	97·5	[illegible]	[illegible]	37·3	15·1	22·8	85			-		cal. old, small wormian b.
84	1373										—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	br. dome
85	1376	♂	1310	187	188	[illegible]	136·5	85	134	113	100·5	516·5	371·5	[illegible]	103·5	61	92·75		46	23	45	44·5	[illegible]	[illegible]		[illegible]	72·6	[illegible]	72·8	[illegible]	[illegible]	111·6	65·8	[illegible]	65·6	67·4	—	97·5	71°·2	77°·3	31°·5	8°·2	26°·3	81°·5			[illegible]		cr. + br. m., flattened at back
86	1377		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	110·5	93·5	[illegible]	[illegible]	[illegible]	117	[illegible]	[illegible]		47·5	[illegible]	[illegible]	41·5	31	[illegible]	50	[illegible]	[illegible]	[illegible]	73·7	70·5	[illegible]	123·2	73·7	[illegible]	78·5	77·1	70·8	[illegible]	[illegible]	[illegible]	36·8	7·15	29·65	83·75	94·5	110·5	35	47	cr. ad. fused atlas, right only ankylosed,
87	1379				[illegible]		[illegible]	[illegible]					[illegible]			[illegible]	[illegible]		[illegible]	23	[illegible]	[illegible]	33	[illegible]			69·7		66·7				61·0	53·5	[illegible]	86·5													br. cal. child [probably effect of disease
88	1382			[illegible]		[illegible]	[illegible]	[illegible]		[illegible]						[illegible]	[illegible]		47·25	25	41·75	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]			70·7				71·2	[illegible]	[illegible]	70·3	70·2	-											br. cal. ad.
89	1395		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	110·5	[illegible]	[illegible]	120	45·75	[illegible]	44·5	44	32	31	50	[illegible]	71·6	71·9	71·4	71·7	[illegible]	110·5	71·3	61·5	71·9	[illegible]	67·9	91·75	68·5	72·7	35·8	11·8	27	84·5	93	101·75	31	41	cr. ad.
90	1401				[illegible]			[illegible]		116	99·5	—	371	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	100	118·5	36	48	dome + m.
91	1406	♂?	1149	170	171	160	126	85	121·5	110	93·75	470·5	[illegible]	285		58	90	110·5	41·5	23·5	41	38	29·5	31	49	38·5	73·7	[illegible]	74·6	73·7	101·2		64·4	56·6	[illegible]	81·6	78·6	87·5	68°·2	[illegible]	32°·1	8°·55	23°·55	88°·25				—	cr. adolesc.
92	1410		1254	176·5	[illegible]	177·5	133	—	134	117	94·5	—	305	301	—	—	—	—	—	—	43·5	—	34·5	—	—	—	74·7	75·3	74·9	75·5	99·3	—	—	—	79·3	—	—	—	—	—	—	—	—	—	95	116	30·5	44	cal. – part of f. ad. pterion, hair
93a	1411	?	—	—	—	—	—	83	—	—	—	—	342*	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	frags
93'	1412	♂	1305	173	175·5	174	135·5	88	126	111	101·5	495	353	303	112	65·5	92	—	50·5	22·5	40	30·5	33	32·5	56	30	77·2	71·8	77·9	72·4	107·5	110·0	71·2	44·6	82·5	82·3	60·6	97	68°·8	76°·9	34°·3	5°·6	28°·7	82°·5	102·5	—	33	43	cr. ad.
94	1417		[illegible]	[illegible]				84·5	128	111·75	101	470	340	295·5	—	—	—	122	—	—	—	—	—	—	—	—	77·6	75·5	77·7	75·7	102·7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	cal. – f. ad. supraorb. foram. injured frontal
95	1419		1265	[illegible]	[illegible]	174	[illegible]	93·6	127	109	92	494	350	298	—	—	—	—	—	—	—	—	—	—	—	—	74·6	72·4	75·3	73·0	103·1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	cal. – parts of f. adolesc. interp. and wormian bs. persist. front. sut.
96	1421		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	122	[illegible]	521	[illegible]	[illegible]		65	101		47	[illegible]	45	[illegible]	32	32·25		42·5	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		64·4	55·3	[illegible]	66·4		97·5											cal. ad. sagit. suts. largely closed
97	1422		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	52·5	[illegible]	43	[illegible]	[illegible]	[illegible]	57	45	[illegible]	[illegible]	[illegible]	[illegible]	103·4		73·9	[illegible]	70·2	70·2	75·9	[illegible]	67·5	75·4	37·1	[illegible]	29	83·5					cal. ad.
98	1424						[illegible]	93	[illegible]	[illegible]	97·75	—	356·5	304?	—	—	—	—	—	—	—	—	—	—	—	—	75·9	70·7	75·9	70·7	107·2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	m. br. cal. ad.
99	1441	♂	1454	187·5	180	188	134·5	86·5	138	120·5	99	512·5	375	307	—	66·5	98	117·5	44·5	22·5	42·5	43·5	33	33·5	57·5	38	72·3	71·2	71·5	73·1	97·5		67·0	50·6	77·6	77·0	66·1	100·5	75°·2	71°·0	32°·9	7°·35	25°·55	70°·25		—			cal. old, sm. interp. b
100	1443									[illegible]		515															67·2		67·9																				dome
101	1446		[illegible]		[illegible]					[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	93		[illegible]	[illegible]		42		[illegible]	55	[illegible]	74·1	[illegible]	[illegible]	[illegible]	[illegible]	113·4	66·7	51·8		[illegible]	[illegible]	87·25	71	72·2	[illegible]	12·8	24	85	85·25	105	30	42·5	cr. ad. union of atlas with occipital bone, secondary result of disease, interpar. and wormian bs.
102	1452	♂	1400	180	185	183	133·5	85	134	112	98	496	366	[illegible]		69	87	123·5	52	22	[illegible]	41·5	[illegible]	[illegible]	61	42	72·2	72·4	73	73·2	99·6		79·3	42·3	79·3	71·1	65·9	94								-		—	col. ad. curved dental arch
103	1454		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]													60·5	73·5	70·1	74·3	94·7						—		70·4	71·9	37·7	13·1	24·0	85					cal. – f. ad. irregu. wormian bs.
104	1457			[illegible]		[illegible]	[illegible]		[illegible]																				72·5	69·1	105·0															-		-	br. cal., when br.
105	1459		[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	93		46	23		42·5		30			70·1	72·9	71·5	73·3	105·5		71·0	[illegible]		84·7		85	67·2	74·9	37·9	18·1	[illegible]	93					cal. ad. pterion, interp. b
106	1461			[illegible]			[illegible]			[illegible]		[illegible]	[illegible]																73·1																90	117·5	35·25		frags. of cal. + m. [some hair
107	1463			[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		71			47			[illegible]		33·5			[illegible]	[illegible]	72·2	71·2	101·5					84·8		60·5	62·9	86·2	36·9					—			much br. cal. ad. place for extra r. molar,
108	1474		[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	112	[illegible]	[illegible]	[illegible]	[illegible]													74·4	72·2	74·1	72·2	[illegible]												—	—	95·75	101·5	35	43	cal. – f. + br. m. old
109	1485		[illegible]	[illegible]			[illegible]			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	97	[illegible]	[illegible]	22	42	42	[illegible]	30·25	55	34	[illegible]	70·5	[illegible]	70·8	98·2	118·3	71·1	44·0	72·4	69·7	61·8	90·5	64	80·1	35·9	2·9	33	83	89·5	111	35	44	cr. br. f. pterion
110	1487	♂	[illegible]		[illegible]		[illegible]			[illegible]	[illegible]				[illegible]	64·25		110	45	25	40·5	42	31	[illegible]	—		67·7	74·7	68·0	76·0	[illegible]			55·0	76·5	71·4		97·75	71·9	[illegible]	32·2	11·1	21·1	87	78	96·5	32	44	cr. ad. hair, outgrowth
111	1488			[illegible]		[illegible]									108	—	—	—	—	—	—	—	—	—	—	—	—	—	78·7	74·9	105·2	—	—	—	—	—	—	88·75	—	—	—	—	—	—	83·5	109·25	28·75	—	br. cal. – f. + m. when br.? persist. front. sut.
112	1489		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	115	100	496	365	294	118	69·75	102	123·5	52·5	24	42	41·5	31·75	31			65·9	73·2	66·8	71·2	89·9	115·7	68·4	45·7	75·6	74·7		98	61°·4	85°·0	37°·7	4°·1	35°·6	85°	[illegible]	102	31·25	41·5	cr. ad. [proc
113	1494		[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	118·5	102·5	512	[illegible]														70·1	73·3	69·0	72·8	[illegible]																	—	cal. – f. y. ad. supraorb. foram. long styloid
114	1499		[illegible]	[illegible]			[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	377	302	—	69	—	—	47	—	—	43	-	35	—	—	75·2	76·9	74·2	75·8	97·8	—	—	—	—	81·4	—	97	71°·6	73°·9	34°·5	10°·6	23°·9	84°·5	95·5	—	33·75	46	br. cal. + br. m. ad. hair
115	1499		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	125	113	98	476	351	287	111	71·5	—	—	54	28	—	45	—	33·5	56	41	72·1	72·7	71·5	72·0	99·2	—	—	51·9	—	74·4	73·2	91	65°·5	78°·4	36°·1	3°·6	32°·5	82°	—	—	—	—	cr. old, l. side f. br.

Miscellanea: 1047 frags.—1234* frags. 1251 frags.—1302 frag. *B'*=91.—1304 a frag.—1396 frags.—1417 two boxes of frags.—1413 frags.—1426 frags.—1451 br. cal. of child.—1460 frags.—1504 frags.

TABLE III.

					MANDIBLE				
GH	$G'H$	GB	J	∠	W_1	W_2	h_1	f	Remarks
—	73·75	95	117·5	·17	—	—	—	—	cal. ad. v. flat occipital
—	—	—	—	—	—	—	27	47·5	cal. – f. + m. ad.
—	—	—	—	—	97	118	34	44·5	cal. – f. + m. ad.
—	—	—	—	—	—	—	—	—	cal. adolesc.
—	74?	99	131·5	·25	—	—	—	—	cr. – m. y. ad. supraorb. foram.
—	66	91·5	—		86	97	33	46	cr. ad. suts. oblit.
114	69	92·5	127·75	—	86·5	101	27	42	frags. of cr. ad. antral abscess.
—	71	97	—	—	87	106·5	32·5?	—	br. cr. ad. [incl. occip. scalp + hair
—	—	—	—	—	83	100?	30?	41	cal. – f. + br. m. ad. supraorb. foramina, v.
—	64	96	129	·5	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	—	br. dome + frags.
—	—	—	129·5	—	—	—	—	—	br. dome + frags. of f.
—	—	—	—	—	97	112	29	45	cal. – f. + m. [marked
—	—	—	130?	—	110	125	33	48	cal. – f. + m. old. upper temporal lines very
—	—	—	—	—	—	—	33	41	dome + frags. ad. scalp + hair
—	—	—	—	—	82	107?	30·5	40	frags. of cal. – f. ad.
—	—	—	—	—	—	—	—	—	br. cal. when br.?
—	65	88	—	—	—	—	—	—	br. cal. old. hair
—	—	—	—	—	—	—	—	—	dome y. ad.
116	70·5	106	129·5	—	101	111	36	46	cr. ad. supraorb. foramina. all cervical vertebrae
115·25	71·5	96	130	·25	91·5	110	33·5	43	cr. ad. sutures nearly closed
—	73?	93	—	·75	—	—	35	—	cal. + part of m. y. ad.
—	64	96	—	·5	—	—	—	—	cal. y. ad.: curved dental arch associated with deep glenoid fossa and very prominent eminentia articularis. long hair
—	66	—	—		—	—	—	—	cal. br. f. ad.
107	70	95	—	—	88	117	31	44	br. cr. ad.
118·5	72	—	130		88	105	41·25	47·5	cr. br. f. ad. supraorb. foram.
—	—	—	—	—	84·75	102·75	37	—	dome + m. temp. bs. scalp + hair
—	68·5	91	120	·5	—	—	—	—	cal. ad. supraorb. foram. curved dental arch
—	70·5	100·75	115·75	·5	—	—	37	—	cal. + frag. of m. old
—	67	92·75	115		—	—	—	—	cal. ad.
120·5	70	90	113·5		81	103	31·5	41	cr. ad.
—	65	90·5	—	—	80	95	25	40	br. cr. – frags. of f. etc. ad.
—	—	—	—	—	—	—	—	—	dome + frags. ad.
109·75	64	105·5	124?	·5	84·5	110·5	32	48	cr. ad. curved dental arch
—	—	—	—	—	—	—	—	—	cal. – f. + frags. ad. wormian b.
—	72	—	136	—	—	—	—	—	br. cal. ad.
—	—	—	—	—	91	109	30·5	—	frag. cal. + atlas verteb. + m.
—	67	89·5	—	·75	86	105·5	28·5	43	cr. ad. wormian bs.
—	75	95	131?		—	—	—	—	cal. ad.
—	66·5	96·5	—	·75	—	—	—	—	cal. ad.
—	—	—	—	—	90	—	33	—	br. cr. child?, worn teeth
—	65	89	—	·25	—	—	—	—	cal. ad. sm. wormian bs.
—	72	90	—	—	—	—	—	—	f. + frags. y. ad.
113·75	70·25	91	126?		93	—	34·5	42·5	cr. ad.
—	—	—	—	—	—	—	—	—	dome + temp. bs. + frags. of f. adolesc.
—	65	96·5	112·5		—	—	—	—	cal. ad. supraorb. foramina
110	62	92	—	·5	82·5	101	30·5	43	cr. ad.
—	—	—	—	—	81	108	33	—	cal. – f. ad. r. pterion. supraorb. foramina
—	67	91·5	—	—	93	110	37	42	br. cr. old, much inclin. occip. depression
108	64	94·25	116	·5	87·5	106	31·5	41	cr. ad. [above λ.
110	68	94	117	·75	80	101·5	33·5	41	cr. ad. teeth worn and distorted
—	—	—	—	—	—	—	—	—	frag.
—	67	83	115·25		—	—	—	—	cal. ad. pterion
120	70·75	100	130·3	·25	102	117	33·25	45	cr. y. ad. persistent frontal sut.
—	77·5	106	133·5		—	—	—	—	cal. ad.
—	68·5	—	—	·25	—	—	—	—	cal. old
—	67	98·25	116	·5	—	—	—	—	cal. ad. sm. interpar. b. persist. frontal sut.
—	57	87	—	—	—	—	—	—	br. cal. child
108	63·5	93·5	121·5	·5	82·75	102·75	33	41	cr. y. ad.
—	65?	85	—	·5	—	—	—	—	cal. deformed l. jugal
—	—	—	—	—	—	—	—	—	cal. – f. ad.
—	68·5	105·5?	—	—	—	—	—	—	cal. in frags. ad. nasal and other suts. closed
102	67	88	123·5	·5	—	—	31	42	cr. br. m. ad. sm. wormian bs. between pariets.

sed.—1648 frags.—1654 W. frag.—295.—1677^{b} dome + frags.—1680 cal. ad.—1734 frags.

MEASUREMENTS OF NAQADA CRANIA—*continued.*

TABLE III.

No.	Grave No.	Sex	C	F	L'	L	B	B'	H	OH	LB	U	S	Q
				Lengths								Circumferences		
116	1505		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	120	114·75	95	505	379	293
117	1506				[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	116·25	102?	519·5?	369?	309
118	1507			[illegible]	[illegible]	[illegible]	135	92	146	128?	109	521?	391	326?
119	1515		[illegible]	[illegible]	[illegible]	[illegible]	122	82	129·5	113·5	92	491	366	285
120	1545		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	116	[illegible]	[illegible]	[illegible]	[illegible]
121	1545·		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	127	[illegible]	[illegible]	497	357	[illegible]
122	1546	♂?	—	178	181?	181	135	83	139	124 (h)	[illegible]	[illegible]	363	
123	1557							88·5						
124	1562			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	113	[illegible]	492·5	361	[illegible]
125	1566		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	131·5	[illegible]	[illegible]	[illegible]	370	[illegible]
126	1567	♂	—	—	185	—	—	84	—	110·5?	—	500?	361	288
127	1567·1		1440	[illegible]	[illegible]	188	131·5	[illegible]	136	115·5	101·5	511	380	300
128	1567·3			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]		497	[illegible]	[illegible]
129	1567·4				[illegible]	[illegible]	133	[illegible]		117	110?	515	[illegible]	[illegible]
130	1592					[illegible]	[illegible]	[illegible]		112·5 (h)				
131	1594			[illegible]	[illegible]	[illegible]	120	[illegible]	122·5	111	[illegible]	480	[illegible]	[illegible]
132	1595			[illegible]		[illegible]	[illegible]		[illegible]					
133	1604					[illegible]	125	[illegible]	127					
134	1607					[illegible]	122	[illegible]				471	352·5	
135	1609			[illegible]	[illegible]	[illegible]	137	[illegible]		121	105·5	524		[illegible]
136	1641		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	134	118	[illegible]	515	[illegible]	[illegible]
137	1643		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	134	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
138	1644		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	132	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
139	1645			[illegible]	[illegible]	[illegible]	120	[illegible]	131	115·5	[illegible]	495	307	[illegible]
140	1647		[illegible]	[illegible]	[illegible]	[illegible]	133	[illegible]	[illegible]	122	[illegible]	514	379	[illegible]
141	1651		1288	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	128	[illegible]	[illegible]	505	[illegible]	[illegible]
142	1653						[illegible]			[illegible]				
143	1657		[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	113·5	[illegible]	[illegible]	[illegible]	[illegible]
144	1666		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
145	1670		[illegible]		[illegible]	[illegible]	132·5	[illegible]	[illegible]	118	[illegible]	[illegible]	[illegible]	[illegible]
146	1672		[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	110	[illegible]	490	[illegible]	[illegible]
147	1673·2				[illegible]			[illegible]		117		509		[illegible]
147	1673·3					182	122	85						
148a	1675	♂?	1338	—	187	183	125·5	93	134·5	116·5	103	504	366	297
148	1676				[illegible]	[illegible]		[illegible]		—	—	505	375	
149	1677				[illegible]	[illegible]		[illegible]	[illegible]	112 (h)	103	523?	385	—
150	1682							[illegible]		110 (h)	105		[illegible]	
151	1683		1244		[illegible]	[illegible]	[illegible]	[illegible]	135	115·5	[illegible]	494	[illegible]	[illegible]
152	1684		[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	527·5	[illegible]	[illegible]
153	1690		[illegible]		[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
154	1709			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	117·75	[illegible]	[illegible]	[illegible]	294
155	1715		[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	112	[illegible]	[illegible]	[illegible]	[illegible]
156	1729				[illegible]	[illegible]	[illegible]					505		
156a	1733		[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	115	[illegible]	513·5	[illegible]	305
156b	1735				[illegible]	[illegible]	[illegible]	[illegible]		[illegible]		472	[illegible]	273
157	1737		[illegible]	[illegible]	[illegible]		122	[illegible]	128	111	[illegible]	[illegible]	[illegible]	[illegible]
158	1743		[illegible]		[illegible]	[illegible]	134	[illegible]	135·5	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
159	1745		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	97·5	127	114	[illegible]	[illegible]	305	[illegible]
160	1748		[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	137	[illegible]	[illegible]	512	371	304
161	1751 W		[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	114·5	[illegible]	482	[illegible]	299
162	1751		[illegible]	174	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	114	[illegible]	[illegible]	[illegible]	[illegible]
163	1753					[illegible]	[illegible]							
164	1754		[illegible]			[illegible]		[illegible]	[illegible]	113·5	[illegible]	[illegible]	[illegible]	214
165	1755		[illegible]		[illegible]	[illegible]	[illegible]		[illegible]	121·5	103	517	375	312
166	1762				[illegible]	191	131·5	95·5	145	113	109	524?	374	296
167	1773	♂	1490	184	186·5	184·5	136	97	133	120	90	519	376	314
168	1774	♀	1203	182	180	180	129	87·5	126·5	112	92	494	370·5	293·5
169	1775			[illegible]		[illegible]	[illegible]	[illegible]						
170	1781		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	543·5	[illegible]	316
171	1782		[illegible]		[illegible]	[illegible]	127	[illegible]	[illegible]	113·5	[illegible]	[illegible]	[illegible]	235
172a	1782·		1423	[illegible]	191	[illegible]	[illegible]	94	142	119	113·5	525·5	377·5	312·5
172	1786			—	—	181	—	97	—	—	—	—	369	—
173	1787		1517		[illegible]	181	147	[illegible]	134	114·5	[illegible]	513·5	[illegible]	314

No.	Grave No.	GH	G'H	GB	J	NH	NB	O_1 L	O_1 R	O_2 L	O_2 R	G_1	G_2
		Face										Palate	
116	1505		73·75	95	117·5	51	25	42·5	42	33·5	33	55	37
117	1506	—	—	—	—	—	—	—	—	—	—	—	—
118	1507		—									–	–
119	1515	—	—	—	—	—	—	—	—	—	—	—	—
120	1545		74?	[illegible]	134·5	52·5	[illegible]	43	47	40	[illegible]	55	42·75
121	1545·		66	91·5		47	24	44	45	34·5	36	56·5	40·5
122	1546	114	60	92·5	127·75	51	25	40·75	47	34	33	57·5	[illegible]
123	1557		71	97		51	27	44·5	42·5	30	32	59·75	45
124	1562												
125	1566		64	90	129	49·5	21·75	41·25	42	30·25	31·5	60	43
126	1567	—	—	—	—	—	—	—	—	—	—	—	—
127	1567·1				129·5				44		33		
128	1567·3												
129	1567·4				130?							–	
130	1592												
131	1594												
132	1595	—	—	—	—	—	—	—	—	—	—	–	—
133	1604		65	88		45·5	22·5	[illegible]	[illegible]	33·5	33·5	52	
134	1607												
135	1609	116	76·5	100	120·5	50	26	42·25	41·25	29·5	30·5	61?	47
136	1641	115·25	71·5	96	130	50·25	24·5	44	43·5	35·75	34	56	40·5
137	1643		73?	93		45·75	25	44		37	35	64·75	40
138	1644		64	96		48	26	47	42·5	31	31		42
139	1645		66			50	20·5		40·5		33	55	43
140	1647	107	72	95		51	21	41	36?	31·5	30·75	54·75	33
141	1651	115·5	72		[illegible]	52			41·5		33·5	52	
142	1653												
143	1657		65·5	91	[illegible]	47·75	25·5	[illegible]	[illegible]	27·5	[illegible]	51·75	45
144	1666		72·5	100·75	115·75	46	20·5	43·5	44·5	33	37·75	50	47
145	1670		67	92·75	115	50	25·5	38·5	40	30·5	31	57	37
146	1672	[illegible]	70	90	113·5	46	21·75	38	41·5	33	31	62	34·5
147	1673·2		65	90·5		45	23·5	42·5	42·5	32	31	52	37·5
147	1673·3	—	—	—	—	—	—	—	—	—	—	—	—
148a	1675	109·75	64	105·5	124?	46	24·5	41	42·5	29·5	28	52	47
148	1676	—	—	—	—	—	—	—	—	—	—	—	—
149	1677	—	72	—	136	53	26	39	37	34	33	50	42?
150	1682												
151	1683		67	91·5		48·5	25	42·5	41	33	31	50	36
152	1684		75	95	[illegible]	54	27	45·75	40·5	34·75	33	50·5	[illegible]
153	1690		76·5	96·5		50	25	41	44	34	33·5	53·5	38·5
154	1709												
155	1715		65	[illegible]		44	22	39	39·5	32	29	50·5	38
156	1729		72	90		46	26	43·5	45	37·5	37·5	55	35·5
156a	1733	113·75	70·25	91	120?	49·5	23	42	41·75	32·5	32·5	50	39
156b	1735												
157	1737												
158	1743		65	[illegible]	[illegible]	40	22·5	38	38	38	28	51	37·5
159	1745	[illegible]	62	62		43	22	43·5	42·5	32	31	48	32·5
160	1748												
161	1751 W		67	91·5		48	24	41?	42	[illegible]	[illegible]	53·5	38
162	1751	[illegible]	74	94·25	110	45	23	42·5	43·25	37·5	42·5	53·75	41·75
163	1753	110	[illegible]	84	117	46	25	40	40	34	[illegible]	59	41·5
164	1754												
165	1755		67	83	115·75	45	23·5	42·5	43·5	32	33	52	35
166	1762	120	75	[illegible]	[illegible]	53	20·5	45	42·5	31·5	31	61	47·25
167	1773	—	77·5	106	133·5	52	27	44	41·5	34	33	61	44
168	1774	—	68·5	—	—	49	24	42	—	29	—	51	43
169	1775	—	67	98·25	116	48	25	41·5	40·5	31·5	31	56	46
170	1781		57	87		41	24		[illegible]		37·5	35	28
171	1782	158	63·5	83·5	121·5	45·5	24·5	42·5	43	32·5	27	59	37·5
172a	1782·		[illegible]	88		53	21·5	41·5	38·5	33	30		
172	1786	—	68·5	105·5?	—	50	27	40	41	32·5	32	—	41
173	1787	[illegible]	67	88	123·5	47	23	44	42·5	32·5	31·5	51	34·5

No.	Grave No.	B/L'	H/L'	B/L	H/L	B/H	GH/GB	G'H/GB	NB/NH	O_2/O_1 L	O_2/O_1 R	G_2/G_1
		Indices										
116	1505	72·0	69·5	72·3	69·9	103·5		77·4	49·0	78·8	78·6	67·3
117	1506						—	—	—	—	—	—
118	1507	70·1	75·8	70·7	76·4	92·5		–				
119	1515	68·2	72·3	68·5	72·8	94·2	—	—	—	—	—	—
120	1545	68·5	70·1	68·7	[illegible]	97·8		74·7	55·2	61·2	63·8	77·7
121	1545·	72·4	70·2	[illegible]	69·8	103·1		72·1	51·1	78·4	[illegible]	71·7
122	1546	74·6	76·8	74·6	76·8	97·1	123·2	74·6	49·0	72·7	70·2	[illegible]
123	1557							73·2	52·9	67·4	75·3	75·3
124	1562	70·9	72·6	[illegible]	72·9	97·3						
125	1566	72·4	78·8	72·2	78·6	[illegible]		66·7	40·8	73·3	[illegible]	71·7
126	1567	—	—	—	—	—	—	—	—	—	—	—
127	1567·1	70·3	72·7	69·9	72·3	96·7					75·0	
128	1567·3	70·3		69·4								
129	1567·4	69·3		69·6								
130	1592	—		72·6								
131	1594	73·5	71·4	72·8	70·8	102·9						
132	1595	—	—	74·0	71·3	103·9	—	—	—	—	—	—
133	1604			[illegible]	75·1	98·4		73·9	49·5	86·3	83·8	
134	1607			70·5								
135	1609	71·5		71·7			109·4	66·5	[illegible]	69·8	73·9	77·0
136	1641	66·0	[illegible]	67·0	68·9	98·1	120·1	74·5	48·8	81·3	78·2	65·2
137	1643	70·9	72·0	70·5	71·6	98·5		78·5	51·3	81·8		61·8
138	1644	71·9	72·1	71·5	71·7	99·6		66·7	60·5	66·9	70·5	
139	1645	71·1	72·2	70·9	72·0	98·5			53·0		81·5	74·1
140	1647	70·7	73·4	71·1	73·8	96·4	115·8	73·7	41·2	76·8	78·8	60·3
141	1651	[illegible]	[illegible]	71·5	[illegible]	101·2					80·7	
142	1653											
143	1657	[illegible]	74·5	72·7	74·6	97·1		75·3	[illegible]	73·8	72·8	[illegible]
144	1666	68·8	72·0	69·2	[illegible]	94·7		[illegible]	[illegible]	[illegible]	75·3	[illegible]
145	1670	73·0	75·2	72·4	77·0	[illegible]		72·2	51·0	79·2	79·5	64·9
146	1672	72·2	74·0	71·8	74·2	96·8	133·9	77·8	57·7	86·8	74·7	55·0
147	1673·2							71·8	52·2	75·3	72·1	72·1
147	1673·3	—	—	67·0	—	—	—	—	—	—	—	—
148a	1675	67·1	71·9	68·6	73·5	93·3	104·0	66·7	53·3	72·0	65·0	77·0
148	1676	—	—	72·2	—	—	—	—	—	—	—	—
149	1677	—	—	67·7	75·5	89·7	—	—	49·0	87·2	80·2	84·0
150	1682											
151	1683	73·2	70·3	74·2	77·1	95·9		74·9	51·5	77·0	[illegible]	65·2
152	1684	72·6	70·1	72·1	75·3	[illegible]		78·6	50·0	75·0	71	—
153	1690	72·0	[illegible]	72·6	[illegible]	93·8		68·0	50·0	73·3	70·2	71·8
154	1709	74·9	68·7	75·4	66·1	102·1						
155	1715	80·6	74·0	81·3	74·6	[illegible]	—	73·0	50·0	82·0	73·4	75·3
156	1729			75·0				80·0	50·5	80·2	83·3	70·0
156a	1733	75·2	70·1	79·3	[illegible]	97·8	125·0	77·2	40·5	77·4	77·8	78·0
156b	1735			72·2								
157	1737											
158	1743	72·2	75·7	71·8	75·3	95·3		67·4	48·7	73·7	73·7	73·5
159	1745	[illegible]	76·6	[illegible]	77·4	96·7	119·6	67·4	51·2	73·3	72·0	67·7
160	1748	68·1	71·5	67·6	72·9	95·3						
161	1751 W	69·9	73·0	71·2	71·5	95·0		73·2	50·0	75·0	71·4	71·0
162	1751	73·1	77·6	73·3	77·8	94·3	114·6	67·9	51·1	70·5	75·1	77·7
163	1753	74·0	73·1	75·0	73·0	102	117·0	72·3	54·3	77·5	75·0	70·3
164	1754			72·2								
165	1755	72·8	70·9	72·0	70·0	94·7		80·7	52·2	75·3	71·3	67·3
166	1762	71·3	74·5	71·7	[illegible]	95·7	120·0	70·8	50·0	[illegible]	72·0	77·4
167	1773	67·4	74·4	68·8	75·9	90·7	—	73·1	51·9	77·3	79·5	72·1
168	1774	72·9	71·3	73·7	72·1	102·3	—	—	49·0	69·0	—	84·3
169	1775	71·7	70·3	71·7	70·3	102·0	—	68·2	52·1	75·9	70·5	82·1
170	1781			71·2			–	65·5	51·2			73·7
171	1782	[illegible]	[illegible]	[illegible]	[illegible]	100·4	115·5	67·9	53·8	71·8	67·4	67·0
172a	1782·	71·0	75·1	75·4	70·0	93·2		70·5	43·0	70·5	93·5	
172	1786	72·3	74·3	73·0	75·1	97·2	—	65·0?	54·0	81·2	78·1	—
173	1787	77·0	73·2	77·2	72·8	[illegible]	115·9	70·1	49·9	73·9	74·1	67·0

No.	Grave No.	GL	N∠	A∠	B∠	θ_2	θ_1	P∠	W_1	W_2	h_1	f	Remarks
			Angles						Mandible				
116	1505	91	69	72	39	12 17	26 53	84 17					cal ad v. flat occipital
117	1506	—	—	—	—	—	—	—	—	—	27	47·5	cal. - f. + m. ad.
118	1507	—	—	—	—	—	—	—	97	118	34	44·5	cal - f. + m. ad
119	1515	—	—	—	—	—	—	—	—	—	—	—	cal. adolesc.
120	1545	104·75	67°0	78°2	34°2	6°05	28°15	84°25		—			cr m y ad supraorb foram
121	1545·	98·75	70 3	74 1	35 3	[illegible]	28 7	81	86	67	33	40	cr ad sut [illegible]
122	1546								86·5	111	27	42	frag f cr ad antral abscess
123	1557								97	106·5	32·5?		br cr ad [incl. occip scalp + hair
124	1562								83	100?	[illegible]	41	cal f + br m ad supraorb foramina, v
125	1566	94	67	79 1	33 9	[illegible]	24 5	88 5	–	–			cal ad
126	1567	—	—	—	—	—	—	—	—	—	—	—	br. dome + frags.
127	1567·1		–										br dome + frags of f
128	1567·3		–						97	112	[illegible]	45	cal f + m [marked
129	1567·4								116	125	33	48	cal f + m old upper temporal lines very
130	1592										33	41	dome frags ad scalp + hair
131	1594								82	107?	30·5	40	frags of cal f ad
132	1595	—	—	—	—	—	—	—	—	—	—	—	br. cal. when br.?
133	1604		–										br cal old hair
134	1607												f [illegible] ad
135	1609	-							101	111	30	40	cr ad supraorb foramina all cervical vertebrae
136	1641	98·5	73 2	71 1	35 7	9 15	20 55	[illegible] 25	91·5	110	33·5	41	cr ad sutures nearly closed
137	1643	97·5	73 1	65 7	38 2	6 05	32 15	74 75			35		cal part of m y ad
138	1644	92·5	68 8	76 8	34 4	5 7	28 7	82 5					cal y ad curved dental arch associated with deep glenoid fossa and very prominent eminentia articularis. Long hair
139	1645	96	68 6	77 9	33 5	[illegible]	15 1	87				—	cal br f ad
140	1647								88	117	31	44	br cr ad
141	1651	98	71	70 3	38 7	12 7	20	[illegible]	88	105	41·25	47·5	cr br f ad. supraorb foram.
142	1653								84·75	102·75	37	—	dome + m temp ls scalp + hair
143	1657	91	65 1	[illegible]	[illegible]	[illegible]	[illegible]	86 5				—	cr ad s supraorb foram curved dental arch
144	1666	97·5	60 1	[illegible]	37 1	6 3	32 4	87 5			[illegible]	—	cal + frag of m old
145	1670	93·5	68 8	71 9	30 3	7 1	20 2	82		–			cal ad
146	1672	92·25	65 5	79 1	35 4	4 9	30 5	84	81	103	31·5	41	cr ad
147	1673·2								80	95	25	40	br cr frags of f [illegible] ad.
147	1673·3	—	—	—	—	—	—	—	—	—	—	—	dome + frags. ad.
148a	1675	101·5	80°4	67°1	32°2	21°1	11°1	88°5	84·5	110·5	37	48	cr ad curved dental arch
148	1676												cal. - f. + frags. ad. wormian b.
149	1677	—	—	—	—	—	—	—	—	—	—	—	br cal. ad.
150	1682								91	109	[illegible]	-	frag cal + atlas verteb + m
151	1683	90·75	86 8	64	35 2	17 75	17 45	81 75	86	105·5	25·5	43	cr ad wormian bs
152	1684	101·25	69 7	[illegible]	37 4	9 1	25 3	82				-	cal ad.
153	1690	98	71	74 5	34 5	7 25	27 25	81 75				—	cal ad
154	1709	90·5							90	—	33		br cr [illegible], worn teeth
155	1715	83·5	72	[illegible]	35 5	12 75	25 75	82 25					cr ad sm wormian bs
156	1729												f + frags y ad
156a	1733	90·5	71 8	69 3	35 9	10 7	19 2	80	93		34·5	42·5	cr ad
156b	1735											—	dome temp ls + frags of f adolesc.
157	1737												
158	1743	93	71	73 6	35 4	15 4	[illegible]	80					cr ad supraorb foramina
159	1745	88	68 3	70 4	35 3	7 1	28 2	83 5	82·5	101	30·5	43	cr ad
160	1748		—						81	108	33	—	cal f ad [illegible] pterion supraorb foramina
161	1751 W	-							93	110	37	42	br cr old, inner inclin occip depression
162	1751	91	69 2	77	33 8	9 5	24 3	86 5	87·5	106	31·5	41	cr ad [above
163	1753	90	72	73 6	34 4	6 15	28 25	79 75	80	101·5	33·5	41	cr ad teeth worn and distorted
164	1754											—	frag
165	1755	92·5	72 6	71 2	[illegible]	10 8	25 4	82				-	cal ad. pterion
166	1762	91	60 4	77	36 6	7 25	29 35	84 25	102	117	33·25	45	cr y ad persistent frontal sut
167	1773	100·25	64°5	78°3	37°2	10°7	26°5	89°	—	—	—	—	cal. ad.
168	1774	95·5	75°3	65°8	38°9	19°45	19°45	85°25	—	—	—	—	cal. old
169	1775	91	71	72 5	36 5	[illegible]	27 5	81 5				-	cal ad sm interpar b persist frontal sut.
170	1781		—										br cal child
171	1782	92	70	75 5	34 5	13	21	88 5	82·75	102·75	33	41	cr y. ad
172a	1782·	74	55 5	84	49 5	1 5	30	88 5					cr deformed l jugal
172	1786	—	—	—	—	—	—	—	—	—	—	—	cal. - f. ad.
													cal in frags ad nasal and other suts. closed
173	1787	89·75	67	75 8	37 2	9 7	27 5	85 5	–		31	42	cr. br. m ad. sm wormian bs. between parietals

Miscellanea: 1515 two super. maxillae, 1608 frags + hair. 1636 cal. old. par. ant. closed.—1648 frags.—1654 W. frag.—1669 frags. of dome.—1673 cal. in frags.—Another 1675 cal. - f. adolesc. persist. front. sut. worm. b. $L=179{\cdot}5$, $B=132{\cdot}5$, $B'=96$, $H=122$, $OH=110\,(h)$, $LB=96$, $U=504$, $S=361$, $Q=295$.—1677[b] dome + frags.—1680 cal. ad.—1734 frags.

TABLE IV.

					Mandible				
GH	G'H	GB	J	N	W_1	W_2	h_1	f	Remarks
—	—	—	—	—	—	—	—	—	cal. – f. ad.
126 ?	69·5	99	—	49	98·75	—	38·25	46	cr. ad. supraorb. foramina, sm. interpar. b.
—	58·5	84	112	44	83	96	27 ?	41	cr. ad. g. develop. wormian bs. pterion
—	—	94	—	43	—	—	—	—	cal. ad. closed suts. [suture
107	60·75?	94·5	—	46	89	108·5	34·5	41	br. cr. ad. pterion on both sides, persist. frontal
—	66	90	—	48	—	—	—	—	cr. ad.
—	—	—	—	—	—	—	—	—	br. cal.
—	65·5	—	—	50	—	—	28	37·5	cr. old [ture to left
—	64·5	89·75	123·75	48	—	—	30	—	cal. + frag. m. adolesc. frontal sinus and aper-
—	68	98	123·5	49	—	—	—	—	cal. ad.
—	66·5	100·5	—	48	—	116·5	32	47	frags. of cr. large interpar. b.
107	65·5	88?	—	50	89·5	107·5	31	37	cr. ad. pterion, abnormal teeth in l. jaw
—	—	95	—	41	—	—	—	—	cal. ad. r. pterion
—	69·5	91	114	49	—	—	—	—	cal. ad.
112·5	70	97	120 ?	46	90	106	32	44	cr. br. base, ad.
—	—	—	113	—	89·25	105	31·75	43	cr. and frags.
109	68·5	95·5	122·5	48	86·75	109	32	—	cr. ad. r. and l. pterion
115·25	68·5	105·5	125	50	97	112·5	35	45·5	cr. y. ad.
—	—	90	—	44	99	117	32	43·5	cr. old
—	67	90·5	—	47	—	—	—	—	cal. ad.
124	76	104	135·5	53	101	125	40·75	42·5	cr. old
106·5	63	87·5	—	44	80·5	98·5	31·5	41·5	br. cr. y. ad.
—	73	97	125·5	56	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	—	br. when ?
—	72	95	125	53	—	—	—	—	cal. ad. supraorb. foramen
—	61	91	—	44	—	—	—	—	cal. ad. [bs.
—	71	100·25	118 ?	53	—	—	—	—	cal. ad. two supraorb. foramina and wormian
—	70	95	—	52	—	—	—	—	f. + frags. supraorb. foramen
—	—	—	—	—	—	—	—	—	cal. – f. child.
—	62	—	—	46	—	—	—	—	cal. ad. supraorb. foramen
—	63·5	89·5	—	47	—	—	—	—	br. cal.
—	—	—	—	—	—	—	—	—	cal. – f. pterion
—	—	—	—	—	—	—	—	—	scaphocephalic calvaria, abnormal
—	—	—	—	—	—	—	—	—	br. cal. – f. y. ad. small interpar. coronal
—	—	—	—	—	—	—	—	—	cal. + frags. of f. y. ad. [suture closed
—	—	—	—	—	—	—	—	—	br. dome
—	66	89	—	45	—	—	—	—	cal. ad. l. pterion
—	—	—	—	—	—	—	—	—	cal. – f. ad.
—	64	103	124·5	51	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	—	br. when ?
—	—	91	—	48	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	—	frags. ad.
—	60	90	118	40	—	—	—	—	cr. ad. wormian bs. long styloid process
—	—	—	—	—	—	—	—	—	cal. – f. – b. ad. interparietal. inclined occipital
—	—	—	—	—	—	—	—	—	dome + frags.
—	62	94	120	46	—	—	—	—	cal. old abnorm. pterion
—	62·5	85 ?	121 ?	41	—	—	—	—	cal. ad.
—	57	96	124	45	—	—	—	—	cal. ad. wormian b.
—	—	97	130	48	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	—	frag.
—	68·5	90	—	53	—	—	—	—	br. cal. adolesc. ?
—	72	105 ?	—	51	—	—	—	—	br. cal. ad.
—	66·5	—	—	49	—	—	—	—	br. cal. old, sutures closed [obliterated
—	71	94·5	124	46	—	—	—	—	cal. old, coronal sut. interp. and occipit. sut.
—	67·25	93	—	48	—	—	—	—	cal. ad.
—	54·5	92	—	46	—	—	—	—	cal. ad.
—	63	96·25	128	47	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	—	br. when ?
—	61	94	—	43	—	—	—	—	frags. of cal.
—	—	—	—	—	—	—	—	—	frags. with a little hair

anea : 1817 dorsal vertebrae.—19

MEASUREMENTS OF NAQADA CRANIA—*continued.*

				Lengths								Circumferences			Face										Palate		Indices												Angles						Mandible				Remarks
Serial No.	Grave No.	Sex	C	F	L	L	B	B'	H	OH	LB	U	S	Q	GH	G'H	GB	J	NH	NB	O_1 L	O_1 R	O_2 L	O_2 R	G_1	G_2	B/L'	H/L'	B/L	H/L	B/H	GH/GB	G'H/GB	NB/NH	O_2/O_1 L	O_2/O_1 R	G_2/G_1	GL	NL	AL	BL	θ_2	[illegible]	PL	W	W'	h_1	r	
174	1795	♂			[illegible]	185·5	140	[illegible]	[illegible]	[illegible]	[illegible]	519	[illegible]	[illegible]													[illegible]	73·3	75·4	[illegible]	100·7		—																cal. - f. ad
175	1796	♂	[illegible]		182	184	131	[illegible]	139	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		40·7	[illegible]		40		[illegible]	52·75	37·3	72·0	70·4	71·2	75·5	94·2	127·3	70·2	52·3		[illegible]	79·7	99	71·1	74·9	37	6·1	[illegible]	78	[illegible]		35·25	40	cr. ad. supraorb. foramina, sm. interpar. b.
176	1799	♀	[illegible]		[illegible]	174	[illegible]	[illegible]	141	[illegible]	94·5	481	353	208		58·5	84	112	44·5	21·75	42·5	42·75	32·75	37·5	54	35·75	73·1	81·5	72·7	81·0	89·7	—	69·6	48·9	77·1	[illegible]	66·2	[illegible]	70·1	77·7	32·2	8·3	[illegible]	86	[illegible]	[illegible]	27	41	cr. ad. g. develop. wormian bs. pterion
177	1804a	♀	[illegible]	197	[illegible]	[illegible]	134	[illegible]	132	[illegible]	[illegible]	479	[illegible]	[illegible]			[illegible]		43	25	43·25		31·5			41·5	80·2	[illegible]	80·5	[illegible]	[illegible]			[illegible]	72·8			85	67·3	77·0	35·1	10·0	24·2	88·5					cal. ad. closed suts. [suture
178	1804b	♂	[illegible]		[illegible]	[illegible]	[illegible]	95·5	137·75	[illegible]	[illegible]	493	[illegible]	[illegible]	107	60·75	94·5		46	20·5	44	42·5	34·5	32·5	53	[illegible]	70·6	77·2	[illegible]	76·5	[illegible]	113·2	64·3	44·0	78·4	[illegible]	70·0	[illegible]							80	[illegible]	31·5	41	br. cr. ad. pterion on both sides, persist. frontal
179	1806	♂	[illegible]		188	[illegible]	[illegible]	95	142	[illegible]	95·5	518	355	310		[illegible]	[illegible]		48	26	44·25	42·5	37·5	32·5	59·5	[illegible]	73·4	75·5	75·2	77·4	97·2		73·3	53·2	73·4	70·5	[illegible]	97	[illegible]	77·3	37·3	11·7	25·0	[illegible]					cr. ad.
180	1807	♀	[illegible]		177·5	[illegible]	[illegible]	87	134	114	95·3	491	[illegible]	[illegible]													72·1	75·5	72·5	75·9	[illegible]																		br. [illegible]
181	1814 A	♂	[illegible]		175	174·5	124	[illegible]	142	116·5	97·75	473	355	[illegible]		[illegible]			[illegible]	23		[illegible]		33	58·5	37	[illegible]	81·1	71·0	81·4	87·3			46·0		75·0	63·3	92·5						87			[illegible]	57·5	cr. old [ture to left
182	1814 B	♀	[illegible]	175	[illegible]	[illegible]	[illegible]	82	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	278		64·5	86·75	123·75	48	23	42·75	45·5	34	34	52·75	38·5	72·2	68·8	[illegible]	69·0	104·0		[illegible]	[illegible]	70·8	78·2	74·0	93	[illegible]	75·3	34·8	[illegible]	[illegible]	[illegible]			[illegible]		cal. + frag. m. adolesc. frontal sinus and aper-
183	1816	♀	1225	180	180·5	[illegible]	131	[illegible]	128	[illegible]	[illegible]	501	[illegible]	286		68	68	123·5	49	26·5	44	44	37·75	34·75	59·75	[illegible]	72·6	[illegible]	72·8	71·1	[illegible]		69·4	54·1	81·3	[illegible]	[illegible]	93	[illegible]	73·0	37·2	8·1	29·1	82					cal. ad.
184a	1820 E	♂				[illegible]	131·5			[illegible]		535				67·5	[illegible]		48	[illegible]	42·5	43	32	32	59·5				65·7				61·2	58·3	75·3	74·4										110·5	32	47	frags. of cr. large interpar. b.
184b	1820 W	♀	[illegible]		181	[illegible]	131	[illegible]	[illegible]	100·5	102·5	[illegible]	[illegible]	[illegible]	107	65·5	[illegible]		50	25		42·5		35	61	44·5	72·4	70·8	72·8	[illegible]	94·2	121·6	74·4	50·0		82·4	[illegible]	94	65·1	86·8	34·1	[illegible]	[illegible]	82·75	80·5	107·5	31	37	cr. ad. pterion, abnormal teeth in l. jaw
185	1825	♀	[illegible]	175	175·5	[illegible]	[illegible]	87	128	111·5	[illegible]	478·5	[illegible]	[illegible]			[illegible]		51	25	41	39·5	31	[illegible]	-	38·5	71·8	72·0	72·0	73·1	95·4		—	[illegible]	75·0	71·7		[illegible]	68·0	81·5	[illegible]	3·5	29·4	85					cal. ad. r. pterion
186	1827	♀	[illegible]	[illegible]	181	[illegible]	[illegible]	84	120	[illegible]	91·75	498	[illegible]	[illegible]		66·5	91	114	49	24	41	40	33	32·5	53	35·75	70·9	71·3	70·5	[illegible]	[illegible]		76·4	49·0	[illegible]	81·3	67·5	91·5	71	71·1	37·9	15·65	25·25	85·75					cal. ad
187	1832	♀			[illegible]	178	134·5	88·5	127·5	[illegible]		494·5		[illegible]	112·5	70	97	[illegible]	40·5	27	38	38·5	33	32·5	59	41·5	74·9	[illegible]	75·6	71·0	105·5	116·0	72·2	58·1	86·8	84·4	[illegible]							83	[illegible]	[illegible]	32	44	cr. br. base, ad.
188	1860	♀		[illegible]	176	177	[illegible]	82·5	[illegible]	113	[illegible]	[illegible]	374	[illegible]				113								—	73·9	73·9	73·4	73·4	100·0		—												80·25	105	31·75	43	cr. and frags.
189	1863	♀	1251	[illegible]	[illegible]	[illegible]	127·2	[illegible]	123·5	108·75	[illegible]	[illegible]	[illegible]	281	[illegible]	68·5	95·5	122·5	48·5	26	40	41	32·5	31·5	58	36	70·7	68·6	71·1	[illegible]	103·0	114·1	71·7	53·6	81·3	70·8	62·1	91·5	70·3	73	[illegible]	11	25·7	84	86·75	[illegible]	32		cr. ad. r. and l. pterion
190	1875	♂	[illegible]		[illegible]	179	134	[illegible]	[illegible]	112	[illegible]	491	351·5	[illegible]	115·25	68·5	105·5	125	[illegible]	23·5	40	44·5	35	33·25	60	[illegible]	75·7	70·3	74·9	75·4	[illegible]	109·2	64·9	47·0	[illegible]	74·7	66·3	[illegible]	72	71·8	[illegible]	13·2	23	85	97	112·5	35	45·5	cr. y. ad.
191	1878a	♀	[illegible]	177·5	[illegible]	175	[illegible]	85·5	[illegible]	115	94·5	492·5	[illegible]	[illegible]			[illegible]		[illegible]	24·5	40	[illegible]	[illegible]	30·5	—		70·2	75·0	70·0	70·3	100·7			55·7	75·0	75·6		97	73·5	69·5	37	13·5	23·5	84	[illegible]	117	[illegible]	13·5	cr. old
192	1878	♂	[illegible]		[illegible]	[illegible]	[illegible]	84	144	114	98·5	511	[illegible]	[illegible]		67	90·5		[illegible]	[illegible]	44	43	35	32·5	52·5	[illegible]	69·7	77·2	70·3	77·8	[illegible]		74·0	48·2	70·5	75·6	74·3	91·25											cal. ad
193	1879	♂	1415		[illegible]	[illegible]	131·5	93·5	[illegible]	110	107	[illegible]	[illegible]	[illegible]	124	[illegible]	[illegible]	135·5	53	28	48·25	44·5	32	33·5	57	37	65·1	[illegible]	66·3	67·3	[illegible]	119·2	73·1	52·8	[illegible]	75·3	64·9	100·5	65·5	78	30·5	[illegible]	20·5	[illegible]	101	125	[illegible]	42·5	cr. old
194	1884	♀			[illegible]	173·5		83	[illegible]	112	[illegible]		[illegible]		[illegible]	63	87·5		44·5	24	39	[illegible]	[illegible]	28	48	30		72·3		72·5		121·6	72·0	53·9	74·4	71·8	75·0	88·5	71·3	74·5	[illegible]	10·75	23·45	85·25	[illegible]	[illegible]	31·5	41·5	br. cr. y. ad.
195	1890	♂	[illegible]	[illegible]	187	187	130	81	[illegible]	117	[illegible]	[illegible]	[illegible]	[illegible]		73	97	125·5	50	25·5	[illegible]	[illegible]	30	29·5	57·5	[illegible]	72·7	73·3	72·7	73·3	[illegible]		75·3	45·5	[illegible]	73·8	80·0	97·5	69·6	71·8	35·6	11·15	20·65	83·75					cal. ad.
196	1893	♂		[illegible]		184	[illegible]																						70·9																				br. when ?
197	1899	[illegible]	[illegible]	181	[illegible]	181	[illegible]	91	132	110	95	[illegible]	[illegible]	303		72	[illegible]	[illegible]	53	27·75	43·5	41·5	37·75	32·5	62	39	74·3	73·3	73·9	72·9	101·3		75·8	52·4	75·3	75·9	69·9	92·5	64	73	38°	11·17	20·83	84·17					cal. ad. supraorb. foramen
198	1902	[illegible]	[illegible]	[illegible]	[illegible]	176·5	126	88·5	131	111	[illegible]	488	[illegible]	[illegible]		[illegible]	[illegible]		44·75	22	41	42	33	34	55	43	72·8	75·7	71·4	71·2	96·2		67·0	49·2	[illegible]	81·0	78·2	86·75	69·2	70	34·8	7·5	27·3	83·5					cal. ad. [bs.
199a	1904a	[illegible]	[illegible]	184·5	[illegible]	183	135·6	94·5	131	[illegible]	[illegible]	513·5	[illegible]	[illegible]		71	[illegible]	[illegible]	53	25	45	40·5	37	30·5	58	44	74·1	71·6	74·1	71·6	104·3		70·8	47·2	[illegible]	78·5	75·9	94·75	[illegible]	71·5	39·3	13	20·3	84·5					cal. ad. two supraorb. foramina and wormian
199b	1904b	[illegible]				[illegible]		[illegible]								[illegible]	[illegible]		[illegible]	23	41	44·5	34	34	45·5	30	—						73·7	44·2	82·0	[illegible]	85·7		—										f. + frags. supraorb. foramen
200	1910	[illegible]	1288	[illegible]	174	174·5	131	[illegible]	126	110	97	[illegible]	344	287			—						—			—	75·3	72·4	75·1	72·2	104·0																		cal. - f. child.
Q GRAVES																																																	
201	Q 9a	♀		[illegible]	[illegible]	[illegible]	[illegible]	90·5	133	113		491	[illegible]	268		62			40·5	24	38		30	—	[illegible]	37	70·8	75·6	76·4	75·1	101·7	—		51·0	78·9		74·0	89	—		—	-		86°	—	—		—	cal. ad. supraorb. foramen
202	Q 9	♀		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	[illegible]		[illegible]	[illegible]		47	[illegible]	[illegible]	41	[illegible]	31·5	51·5	35	72·3	[illegible]	72·0	75·3	96·9	—	70·9	45·0	[illegible]	70·8	67·9	-	-		—			—	—	—		—	br. cal.
203a	Q 13	♀		[illegible]	[illegible]	185	[illegible]	83	127 ?	111·5		504·5	375·5	[illegible]											—		73·0	69·0	73·0	69·0	100·3	—					—		—	-	—			—	—	—	—	—	cal. [illegible] pterion
203b	Q 18	[illegible]				[illegible]	124	[illegible]					—									—		—	—	—			[illegible]			—					—	—			—			—	—	—	—	—	scaphocephalic calvaria, abnormal
204	Q 30	♀		[illegible]	[illegible]	—	135	[illegible]		114 ?		[illegible]		[illegible]										—	—		75·2		76·7			—					—	—		-	—			—	—	—	—	—	br. cal. - f. y. ad. small interpar. coronal
205	Q 37	♂		[illegible]		[illegible]		[illegible]	145	113 (h)		—	405	327									—		54	[illegible]	[illegible]			72·0		—					74·1		—	—	—			—	—	—	—	—	cal. + frags. of f. y. ad. [suture closed
206	Q 43	♂				[illegible]	[illegible]	[illegible]															—			—			[illegible]			—					—	—			—			—	—	—	—	—	br. dome
207	Q 54	♀	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	91	133·5	111·5	96·5	497·5	361	[illegible]		[illegible]	89		45	23	44	44·5	33		53·5	[illegible]	73·1	75·0	[illegible]	74·8	97·5	—	74·2	51·1	75·0		67·3	90	72·3	73·2	34°·5	9·8	24·7	83°	—	—	—	—	cal. ad. l. pterion
208	Q 59	♂	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	94·5	127·7	114	102·5	515·5	372	268													[illegible]	67·2	69·8	67·1	[illegible]	—	—								—			86°·5	—	—	—	—	cal. - f. ad.
209	Q 60	♂			[illegible]	187	[illegible]	[illegible]		117·5	—	511	[illegible]	[illegible]		[illegible]	[illegible]	124·5	51	25·75	[illegible]	42	30	29	57·5	[illegible]	71·1	—	71·1			—	62·1	[illegible]	71·4	[illegible]	77·2		—		—				—	—	—	—	cal. ad
210	Q 82	[illegible]		181		[illegible]	137		[illegible]		—														-			—	75·2	75·2	100·0	—	—				—	—	—			-			—	—	—	—	br. when ?
211	Q 83		[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	97	[illegible]	[illegible]	[illegible]			[illegible]		[illegible]	[illegible]	43	41·5	32·5	[illegible]	-		76·8	72·9	77·3	73·3	105·4	—	—	53·6	[illegible]	73·5	—	88·5	64·2	80·2	[illegible]				—	—	—	—	cal. ad.
211	Q 87					[illegible]	134	[illegible]		[illegible]	—																		72·4			—	—					—	—						—	—	—	—	frags. ad
212	Q 92		[illegible]	[illegible]	[illegible]	[illegible]	134	84	[illegible]	[illegible]	84	[illegible]		[illegible]		[illegible]	[illegible]	118	[illegible]	[illegible]	41	41	[illegible]	30·5	50·5	35·75	79·3	73·4	[illegible]	73·8	[illegible]	—	66·7	61·3	71·4	74·4	70·8	85	73·2	71	35·8	9·75	20·15	80°·75	—	—	—	—	cr. ad. wormian bs. long styloid process
213	Q [illegible]			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	—	[illegible]		534		312										—			71·3		[illegible]			—	—				—		—					—	—	—	—	—	cal. - f. - b. ad. interparietal. inclined occipital
214	Q [illegible]							[illegible]																—								—	—						—					—	—	—	—	—	dome + frags.
215	Q 103		[illegible]	174	[illegible]	174	[illegible]	84	134	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	45	43·5	34·5	34	51	[illegible]	72·3	70·6	[illegible]	[illegible]	94·4	—	66·0	[illegible]	[illegible]	78·2	74·5	95	74	70·8	35·2	7·7	27·5	[illegible]	—	—	—	—	cal. old abnorm. pterion
216	Q 103	♀	[illegible]	[illegible]	178	178	[illegible]	[illegible]	[illegible]	[illegible]	94	[illegible]	[illegible]	[illegible]		[illegible]	85	[illegible]	44	[illegible]	43	43	31	31	51	[illegible]	72·8	73	72·8	73	[illegible]	—	73·5	54·0	72·1	72·1	70·6	89·5	68·4	77·7	[illegible]	10·65	[illegible]	[illegible]	—	—	—	—	cal. ad.
217	Q 105	♂	[illegible]	[illegible]	[illegible]	172	[illegible]	[illegible]	132·8	114	[illegible]	495	359·5	[illegible]		[illegible]	[illegible]	124	45	23·75	38	[illegible]	30	[illegible]	52·5	40	[illegible]	[illegible]	75·8	77·2	[illegible]	—	59·4	52·8	78·9	74·4	87·6	95	68·2	80·4	31·4	[illegible]	[illegible]	[illegible]	—	—	—	—	cal. ad. wormian b.
218	Q 107	♂		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]			[illegible]	[illegible]	48	[illegible]	43	[illegible]	33	32·5	[illegible]	40·5	71·8	72·0	71·7	72·0	99·7	—	—	60·4	73·3	74·7	—	[illegible]	71·2	74·6	34·2	[illegible]	22·05	86°·75	—	—	—	—	cal. ad.
219	Q 107								—	[illegible]														—			—					—	—				—	—			-			—	—	—	—	—	frag
220	Q 107	[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	112	[illegible]	501		285		[illegible]	[illegible]		53	25	[illegible]	43	31·5	33			72·5	68·7	72·1	68·3	105·0	—	76·1	47·2	70·8	70·7			-		-			—	—	—	—	—	br. cal. adolesc ?
220	Q 127					[illegible]		[illegible]	—							72	[illegible]		51	28	45	—	35		58	44	—					—	68·6	54·9	77·8		75·9			—				—	—	—	—	—	br. cal. ad.
220	Q 116					[illegible]	141	[illegible]	—	112 (h)		[illegible]	[illegible]			[illegible]			49		35		35	—			—		77·0			—	—					-						—	—	—	—	—	br. cal. old, sutures closed [obliterated
221	Q 131		[illegible]	[illegible]	[illegible]	[illegible]	132·5	[illegible]	134·5	115·75	[illegible]	[illegible]	[illegible]	301		71	94·5	124	40·5	28	43	42·5	34·5	32	59	34·5	66·8	67·8	[illegible]	67·9	98·5	—	75·1	60·2	80·2	75·3	58·5	90·5	69·6	75·0	34·8	7·4	27·4	83°	—	—	—	—	cal. old, coronal sut. interp. and occipit. sut.
222	Q 137		[illegible]	177·5		[illegible]	[illegible]	[illegible]	[illegible]	114	[illegible]	492·5	[illegible]	267		[illegible]	[illegible]		[illegible]	25	[illegible]	42	32·75	33	59·5	37·5	73·4	75·8	73·0	75·5	96·8	—	72·3	51·5	75·3	78·6	63·0	[illegible]	75·5	70	34·5				—	—	—	—	cal. ad.
223	Q 144	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	110	[illegible]	509·5	[illegible]	[illegible]		[illegible]	92		40	27	40	41	30	30·5			74·1	[illegible]	74·1	[illegible]	101·5	—	59·2	58·7	75·0	74·4		89	65·5	82	32·5	4°	28·5	80	—	—	—	—	cal. ad.
224	Q 162a		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	119	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	128	47	28	43	41	30	30	51	42	72·2	74·1	72·4	74·4	97·5	—	65·5	[illegible]	69·8	73·2	82·4	99	66·6	83	30·4	1·25	[illegible]	84·25	—	—	—	—	cal. ad.
225	Q 162b			[illegible]		[illegible]	140·5		[illegible]															—		—			[illegible]	71·5	100·7	—	—					-	—			-			—	—	—	—	br. when ?
225b	Q 162c							[illegible]								[illegible]	[illegible]		43	23	[illegible]	30·5	31	32	49	37						—	64·9	53·5	80·1	87·0	80·4								—	—	—	—	frags. of cal.
226	Q 171			[illegible]	[illegible]	[illegible]	[illegible]	88·5	[illegible]	115		[illegible]	[illegible]	[illegible]													69·8	73·3	70·2	73·7	95·3	—	—					-	—	—	-	—			—	—	—	—	frags. with a little hair

Miscellanea: 1817 dorsal vertebrae.—1901 br. dome + frags. of m.—Q 48 B' = 94.—Q 53 frags.—Q 63 B' = 87.—Q 68 frags.—Q 77 B' = 83.—Q 116 frags.—Q 138 frags.—Q 148 frags.—Q 149 frags.

TABLE V.

				Mandible				Remarks
GH	$G'H$	GB	J	W_1	W_2	h_1	f	
—	63	98	— 5	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	dome + frag. ad.
—	—	—	—	—	—	—	—	cal. – f. ad.
—	—	—	—	—	—	—	—	br. cal. – f. old
—	—	—	—	—	—	—	—	frags. dome
—	—	—	—	—	—	—	—	frags. br. cal. when br.?
—	66	93	—	—	—	—	—	cal. – f. ad.
—	68·5	95	122	—	—	—	—	br. cal. y. ad. [persist. front. sut.
—	57	88	—	—	—	—	—	cr. – occipit. y. ad. wormian and interpar. bs.
—	65·5	99	116 5	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	dome. hair
—	—	—	—	—	—	—	—	dome + frags. y. ad.
—	—	—	—	—	—	—	—	frags.
—	—	—	—	—	—	—	—	cal. – f. ad. [foram.
—	—	—	—	—	—	—	—	cal. – part of f. ad. small interpar. b. supraorb.
—	—	—	—	—	—	—	—	br. cal – f. ad. asymet. large interpar. b.
—	—	—	—	—	—	—	—	back of skull + frags. ad.
—	69	94	125 5	—	—	—	—	cal. ad. pterion
—	—	—	—	—	—	—	—	br. cal. – f. ad.
—	—	—	—	—	—	—	—	br. dome + crystallised frags.
—	66	86	—	—	—	—	—	br. cal. ad.
—	67	90	117	—	—	—	—	cal. ad.
—	67·5	—	—	—	—	—	—	br. cal. ad. supraorb. foramina
—	66	93	123 5	—	—	—	—	cal. ad.
—	56?	85·5	—	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	frags. golden (artificially coloured?) hair
—	58	89	—	—	—	—	—	cal. br. f. adolesc. persist. front. sut.
112·5	63	92·5	124·	100·5	—	30·5	50	cr. ad. portions of scalp; abnorm. jugal.
—	66	90	117	—	—	—	—	cal. ad. supraorb. foram.
—	64·5?	88	118	—	—	—	—	cal. y. ad. supraorb. foramen
—	56	81	—	—	—	—	—	cal. adolesc.
109·5	69·25	—	—	—	—	—	—	cal. – parts of f. ad.
—	75	104	121·	—	—	—	—	cr. ad. hair [vertebra, etc.
—	—	—	—	—	—	—	—	cal. – f. ad. little hair, interparietal b. atlas
—	68	94	113	—	—	—	—	cal. ad. small and very heavy
—	73	99	113·5	—	—	—	—	cal. ad. flat nasal bones, prenasal fossa on
—	—	—	—	92	98	33	—	br. cr. when br.? [face, diseased teeth
—	—	—	—	—	—	—	—	cal. – f. y. ad. wormian bs. like Q 383 but
—	—	100	128 5	—	—	—	—	cal. ad. [less marked
—	64	95	— 5	—	—	—	—	cal. ad. [edly convex and sloped; interpar. b.
—	—	—	—	—	—	—	—	cal. – f. ad. nuchal portion of occipital mark-
—	60	86	— 5	—	—	—	—	cal. y. ad.
—	60·5	104·5	(135 5	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	cal. – f. ad. interpar. b.
—	—	—	—	—	—	—	—	dome. old
—	—	—	—	—	—	—	—	cal. – f. ad. parietal sutures closed
—	64	96	118 5	—	—	—	—	cal. ad.
—	69	87	124·	—	—	—	—	cal. ad.
—	59	96·5	125·	—	—	—	—	cal. ad. [other wormian bones
—	63	86	114?5	—	—	—	—	cal. juvenile circa 17 yrs. large interpar. and
—	70	88	123 5	—	—	—	—	cal. ad. hair. pterion
—	61·5	92·25	121	—	—	—	—	cal. ad.
—	—	—	—	91·5	105·25	33	46	cr. y. ad.
—	70·75	100	119	—	—	—	—	br. cal. pterion. wormian bs.
—	—	—	—	—	—	—	—	br. cr. adolesc. spheno-pariet. sut. closed
—	64·5	—	—	—	—	—	—	frag.
—	61	—	—	89	—	27·5	45·5	br. cr. ad.
—	62·5	94·5	—	—	—	—	—	br. cr. y. ad.
—	—	—	—	—	—	—	—	br. cal. ad.
—	65	96·75	122	—	—	—	—	cal. ad. pterion on both sides
—	—	—	—	—	—	—	—	br. cal. – f.
—	—	97·5	— 5	—	—	—	—	cal. – f. adolesc.
—	—	—	—	—	—	—	—	cal. – f. ad.
—	—	—	—	—	—	—	—	cal. – f. ad.
—	—	—	—	—	—	—	—	dome. ad.

rt of f. ad. covered with cry

MEASUREMENTS OF NAQADA CRANIA—*continued.*

TABLE V

Serial N	Grave N	Sex	C	L	L	L	B	B'	H	OH	LB	U	S	Q
				Lengths								**Circumferences**		
227	Q 173	[illegible]	[illegible]	183	183	185·1	128·7	97	134	110	[illegible]	507	352	302
228	Q 174	[illegible]		181		180	128·5	88·5		[illegible] (h)		495	305	284
229	Q 176 b	[illegible]	[illegible]	[illegible]	[illegible]	178·5	131·0	83	133·5	115	101	505	304	294
230	Q 185	[illegible]		185	180	185	132·5	96		[illegible]		512·5	377·5	305
231	Q 186	[illegible]								97 (h)				
232	Q 191			[illegible]		187	[illegible]		[illegible]					
233	Q 208 3	[illegible]		[illegible]	191	194·5	[illegible]	[illegible]		112		533		304
234	Q 210	[illegible]		183	182	182	[illegible]	83	135	112	103		380	313
235	Q 212 2	[illegible]		172	171	[illegible]	[illegible]	86·5		[illegible]		474		[illegible]
236	Q 212 4	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	125	[illegible]	97·5	497	358	288
237a	Q 214	[illegible]		[illegible]		172·5	[illegible]			[illegible] (h)				—
237b	Q 215	[illegible]				[illegible]	[illegible]							—
238	Q 217	[illegible]		[illegible]		[illegible]	[illegible]		[illegible]					
239	Q 225 b	[illegible]	[illegible]	184·5	[illegible]	[illegible]	[illegible]	88	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	312?
240a	Q 225 W		[illegible]	170·5	175	[illegible]	[illegible]	90	[illegible]	112	95·5	490·5	357	309
240b	Q 229					[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	354	281?
241	Q 238			190		192	[illegible]		133·1					
242	Q 241a	[illegible]	1423	[illegible]	180	179·7	[illegible]	[illegible]	135	115	[illegible]	[illegible]	301·5	310
243a	Q 241	[illegible]		[illegible]		[illegible]	[illegible]	[illegible]	120·5	[illegible] (h)			[illegible]	
243b	Q 245					[illegible]	[illegible]	[illegible]				[illegible]	[illegible]	—
244a	Q 252	[illegible]				184	[illegible]	[illegible]		[illegible] (h)		[illegible]	[illegible]	
244b	Q 254	[illegible]	[illegible]	[illegible]	[illegible]	177	[illegible]	90	125	[illegible]	90	492	357	[illegible]
245	Q 265	[illegible]		[illegible]	[illegible]	181	135·5	83	[illegible]	[illegible]	—	488	374	[illegible]
246	Q 272	[illegible]	[illegible]	[illegible]	[illegible]	177·5	[illegible]	85	135·5	113·25	[illegible]	490	370·5	312
247	Q 276	[illegible]	[illegible]	[illegible]	[illegible]	172·1	[illegible]	[illegible]	122	[illegible]	90	[illegible]	340	[illegible]
248	Q 276					[illegible]	131·5	[illegible]					[illegible]	
249	Q 299	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	119	100	91	[illegible]	[illegible]	[illegible]
250	Q 305a	[illegible]	1421	[illegible]	[illegible]	184	[illegible]	88·5	137	[illegible]	102	501·5	377	307
251	Q 305	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	124·1	[illegible]	[illegible]	493·5	[illegible]	[illegible]
252	Q 311			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	92	[illegible]	[illegible]	[illegible]
253	Q 314			[illegible]		[illegible]	130	87·5	[illegible]				373	
254	Q 315				[illegible]	[illegible]				125·5	98·5		[illegible]	332
255	Q 331	[illegible]	1224	[illegible]	181	[illegible]	[illegible]	[illegible]	127	[illegible]	98	[illegible]	[illegible]	[illegible]
256	Q 323			[illegible]	[illegible]			90		117	[illegible]	510	[illegible]	[illegible]
257	Q 336 a	[illegible]	[illegible]	[illegible]	[illegible]	171	131·1	[illegible]	128	117·75	90·75	488	[illegible]	308
258	Q 326		[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	123·5	[illegible]	93	[illegible]		[illegible]
259	Q 358 2			[illegible]		[illegible]			[illegible]					
260	Q 359 2	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	113	[illegible]	481	[illegible]	[illegible]
261	Q 359 3		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]
262	Q 365		1415	[illegible]	[illegible]	182	135·5	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
263	Q 383		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	111	145	120	[illegible]	520	[illegible]	[illegible]
264	Q 384	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	91	[illegible]	[illegible]	[illegible]
265	Q 392		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	117	105	513	370	[illegible]
266a	Q 396			[illegible]	[illegible]	[illegible]	134	88·5	[illegible]	117	[illegible]	513	375	[illegible]
266b	Q 395	[illegible]				[illegible]	132	92						
267	Q 400			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	142·5	[illegible]	102	[illegible]	[illegible]	[illegible]
268	Q 403	[illegible]	[illegible]	181·5	[illegible]	184·5	[illegible]	90·5	[illegible]	111	[illegible]	[illegible]	[illegible]	[illegible]
269	Q 405			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	103	[illegible]	373	[illegible]
270	Q 409	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
271	Q 406	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	129	[illegible]	[illegible]	[illegible]	87	[illegible]	[illegible]	[illegible]
272	Q 408		[illegible]		[illegible]	[illegible]	[illegible]	91	140	117·0	102·75	[illegible]	[illegible]	[illegible]
273	Q 411	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	482	[illegible]	[illegible]
274	Q 413	[illegible]								[illegible]				[illegible]
275	Q 420	[illegible]			[illegible]	170	122	87·5			93·5			
276a	Q 428							[illegible]				—		
276b	Q 430					[illegible]	[illegible]	[illegible]						
277	Q 436	[illegible]		[illegible]	170	177	[illegible]	78	[illegible]	[illegible] (h)		[illegible]		
278	Q 437	[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	114·75	[illegible]	[illegible]	[illegible]	[illegible]
279	Q 435			[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]			[illegible]	
280	Q 445	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	127	[illegible]	137	115	97·5	[illegible]	[illegible]	[illegible]
281	Q 466 3	[illegible]	[illegible]	177	[illegible]	[illegible]	[illegible]	88	[illegible]	111	94	495	[illegible]	292
282	Q 466 4	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	131	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
283	Q 485 A	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	375	[illegible]
284	Q 487	[illegible]			[illegible]	[illegible]	[illegible]	91	[illegible]	110·5	108	[illegible]	374·5	[illegible]
285	Q 488	[illegible]		[illegible]		[illegible]	[illegible]	[illegible]	132·5	—	—	—	—	—

Serial N	Grave N	GH	G'H	GB	J	NH	NB	O_1 L	O_1 R	O_2 L	O_2 R	G_1	G_2
		Face										**Palate**	
227	Q 173		63	98		49	23	42·5	42	30·75	29·5	47	—
228	Q 174	—		—	—								
229	Q 176 b		—				—		—				
230	Q 185	—		—		—		—			—	—	
231	Q 186	—			—		—					—	—
232	Q 191				—	—			—				
233	Q 208 3		66	93		49	25	44·5		34			
234	Q 210		68·5	95	122	45·5	28	42·5	41	29	29	57·5	[illegible]
235	Q 212 2		57	88		39·5	25·5	39·5	37·5	33	34	45	34
236	Q 212 4		65·5	99	116·5	43	27·5	45	45	31·5	31·5	54·5	39
237a	Q 214	—	—	—	—						—	—	
237b	Q 215					—							
238	Q 217		—	—					—	—		—	
239	Q 225 b				—					—			
240a	Q 225 W		—	—					42·5		33		
240b	Q 229		—									—	—
241	Q 238												
242	Q 241a		69	94	125·5	47·5	23	45·75	44·25	33·5	33·5	50	40
243a	Q 241			—	—								
243b	Q 245			—			—						
244a	Q 252	—	60	86		47	23	41	42	34	33	51	34
244b	Q 254		67	90	117	47·5	23·75	42	39·5	33	33·5	52·5	38
245	Q 265		67·5			48	24		41		33	48·75	38·5
246	Q 272	—	69	93	123	47	24	40·5	39·5	30	28·5	50	41·5
247	Q 276	—	56?	85·5	—	42·5	21	39·25	39·5	32·75	33	47·75?	
248	Q 276		—										
249	Q 299	—	58	89		[illegible]	23	38·5	37·5	26·5	30	52	37·5
250	Q 305a	112·5	63	92·5	124·5	45·5	24·5	40	41	28·5	29	53	
251	Q 305		66	90	117	49·5	23	42·5	41·5	32·5	33·5	50·75	30·75
252	Q 311		[illegible]	88	118	48	26	43	42	33·75	33	[illegible]	35
253	Q 314		56	81		41	22	36·5	37	33	33	39	37
254	Q 315	109·5	69·25			51	25		41		33	57·5	40
255	Q 331	—	75	104	121·5	52·25	25	44	43	36	[illegible]	58	44
256	Q 323	—											—
257	Q 336 a		68	94	113	46·5	26	39	38·75	27·5	27·25	62	45
258	Q 326	—	73	99	113·5	49·75	26·5	43	42	34·75	34·5	52·5	43
259	Q 358 2												
260	Q 359 2												
261	Q 359 3			100	128	53·75	[illegible]	44·75	45·75	30·5	34		48
262	Q 365		64	95		44·5	28·5	41·5	42·5		31	52	39·5
263	Q 383	—	—										
264	Q 384	—	60	86		47	23·5	40	40·5	30	30·5	51	37
265	Q 392		[illegible]	104·5	(135?)	53	30·5	42	41	33·5	33	54	41·5
266a	Q 396				—		—						
266b	Q 395		—								—		
267	Q 400	—											
268	Q 403		60	90	118	47	25·5	45	44·5	38	37	53	42
269	Q 405		60	87	124·5	52	24	[illegible]	41	30	30	45	41
270	Q 409		59	90·5	125·5	44	[illegible]	43	42	30	31	55·5	40
271	Q 406		63	86	114	49	23·5	41·75	40·75	34	34·5	52·5	41·5
272	Q 408		70	88	123	50	23·25	42	41	38	30·5	50	[illegible]
273	Q 411		61·5	92·25	121	47	23	43	43	32	32	58	45
274	Q 413										—	52·5	39
275	Q 420		70·75	100	119	52	25·5	45	44	35	30·5	60	41
276a	Q 428		—										
276b	Q 430		64·5			49	24·5		[illegible]	34	34	44?	41
277	Q 436		61			42·5			40		28		
278	Q 437		62·5	94·5		49·5	25·75	42·5	39	24·75	27	56	38
279	Q 435												
280	Q 445	—	65	99·75	122	40·5	24	41	38	29·5	[illegible]	57·5	35·5
281	Q 466 3												—
282	Q 466 4			97·5		45·5	27	44·5	43·5	30	29·5		42·5
283	Q 485 A												
284	Q 487		—	—				—					—
285	Q 488	—	—	—	—	—	—	—	—	—	—	—	—

Serial N	Grave N	B/L	H/L	B/L	H/L	B/H	GH/GB	G'H/GB	NB/NH	O_2/O_1 L	O_2/O_1 R	G_2/G_1	GL
		Indices											
227	Q 173	70·3	73·2	70·3	73·2	96·0	—	64·3	46·9	72·3	70·2		89
228	Q 174	—		71·4					—				
229	Q 176 b	73·9	75·0	73·6	74·7	98·6	—						—
230	Q 185	71·2		71·6									—
231	Q 186					—	—				—	—	—
232	Q 191	—		71·2	74·0	96·2	—				—		
233	Q 208 3	72·5		71·2		—	—	71·0	51·0	70·9	—	—	—
234	Q 210		74·2		74·2			72·1	[illegible]	68·2	72·7	66·1	98·5
235	Q 212 2	69·0		70·3	—			64·8	[illegible]	[illegible]	[illegible]	75·0	
236	Q 212 4	78·1	71·0	79·0	71·8	110·0	—	66·2	64·0	70·0	70·0	71·6	91
237a	Q 214	—		79·4	—	—	—				—		—
237b	Q 215			70·2		—	—				—		—
238	Q 217	—		73·1	72·4	101·0						—	—
239	Q 225 b	75·2	72·8	75·4	72·9	103·3						—	—
240a	Q 225 W	79·8	73·7	74·2	73·1	108·3					77·6	—	
240b	Q 229	—		73·2	70·7	95·5?							
241	Q 238			74·4	69·1	107·3						—	—
242	Q 241a	75·8	75·0	[illegible]	[illegible]	101·1		73·4	48·4	73·2	75·7	71·4	95
243a	Q 241			73·7	69·7	105·7				—		—	
243b	Q 245	—		71·4									
244a	Q 252	—		76·6	—	—		70·7	48·9	83·0	78·5	66·7	
244b	Q 254	71·3	71·8	72·1	72·3	99·7		74·4	50·0	78·0	84·8	72·4	94·25
245	Q 265	69·5	72·0	69·2	71·8	96·5			50·0		80·5	79·0	89?
246	Q 272	78·3	75·3	79·4	70·3	124·1		71·0	51·1	74·1	72·2	74·1	92
247	Q 276	73·3	70·7	73·5	70·0	103·7	—	65·5	49·4	83·4	83·5		85
248	Q 276			74·3		—			—				
249	Q 299	73·1	[illegible]	73·5	66·5	110·5		65·2	57·5	76·6	80·0	72·1	88
250	Q 305a	70·3	74·1	79·7	74·5	94·9	121·6	68·1	53·8	71·3	70·7		93
251	Q 305	70·8	70·3	77·0	70·5	109·3		73·3	46·1	76·5	80·7	78·3	88·75
252	Q 311	76·4	72·0	77·3	[illegible]	100·1	—	73·3	54·2	78·5	78·6	80·5	89
253	Q 314	—		[illegible]	73·9	98·9			—		—	—	—
254	Q 315								49·0	—	75·0	69·0	
255	Q 331	[illegible]	70·2	73·7	[illegible]	103·9		72·1	47·8	81·8	83·7	74·1	95·5
256	Q 323									—			
257	Q 336 a	72·8	70·9	73·4	71·5	102·7		72·3	55·9	70·5	70·3	72·0	93·25
258	Q 326	69·5	[illegible]	[illegible]	[illegible]	104·0		73·7	53·3	80·8	82·1	81·9	90
259	Q 358 2	—			75·5							—	—
260	Q 359 2	75·4	[illegible]	[illegible]	78·1	97·1						—	
261	Q 359 3	67·1	72·0	69·3	72·2	[illegible]			53·2	81·6	74·3		93
262	Q 365	73·4	71·8	74·5	72·8	103·3		67·4	64·0		72·9	70·0	93
263	Q 383	72·1	70·7	72·4	77·0	94·0			—				
264	Q 384	74·7	77·0	75·1	77·4	97·1	—	69·8	50·0	75·0	75·3	72·5	92
265	Q 392	73·0	73·8	70·4	73·6	95·7		57·0	57·5	79·8	80·5	76·8	100·5
266a	Q 396	71·8	[illegible]	72·0	74·2	97·1							
266b	Q 395		—	75·4		—			—			—	—
267	Q 400	70·4	70·4	75·8	75·8	100·0	—				—		
268	Q 403	74·0	70·7	73·4	[illegible]	104·7		66·7	54·3	84·4	85·1	79·2	—
269	Q 405			65·9	74·2	94·7	—	79·3	40·2	90·0	87·8	91·1	88·75
270	Q 409	71·3	70·8	71·1	[illegible]	[illegible]	—	61·1	66·5	66·8	73·8	72·1	—
271	Q 406	72·5	[illegible]	72·5	70·5	102·8		[illegible]	45·0	81·4	84·7	79·0	80
272	Q 408	68·8	78·8	68·8	78·8	87·2	—	79·5	46·5	[illegible]	89·0	80·0	93·25
273	Q 411	70·0	[illegible]	77·5	70·7	109·6		66·7	48·9	74·4	74·4	77·0	90·5
274	Q 413											74·3	—
275	Q 420	69·3		69·3				73·8	49·0	77·8	83·0	65·3	99
276a	Q 428						—			—			
276b	Q 430			[illegible]					50·0		—	—	—
277	Q 436	72·2	[illegible]	71·8	73·0	97·5				—	[illegible]	—	
278	Q 437	[illegible]	73·3	73·8	71·2	99·5		66·1	52·0	58·2	66·2	68·0	88
279	Q 435	75·6	74·7	75·1	[illegible]	101·2				—		—	
280	Q 445	72·5	78·2	73·4	70·2	[illegible]		67·2	51·0	72·6	78·9	67·6	90·75
281	Q 466 3	[illegible]	72·9	70·0	73·5	104·2							
282	Q 466 4	67·3	70·1	67·6	70·4	[illegible]			59·3	67·4	67·8		91·5
283	Q 485 A	75·4	74·2	74·5	[illegible]	[illegible]							—
284	Q 487	69·0	70·0	70·8	71·9	95·4	—		—	—	—	—	—
285	Q 488	—	—	76·1	74·6	101·9	—	—	—	—	—	—	—

Serial N	Grave N	N∠	A∠	B∠	θ_2	θ_1	P∠	B	B	h	f	Remarks
		Angles						**Mandible**				
227	Q 173	68·1	75	36·9	10·5	20·4	85·5					cal ad
228	Q 174	—			—	—						dome + frag ad
229	Q 176 b		—	—								cal f ad
230	Q 185		—	—	—							br cal f old
231	Q 186			—								frags [illegible]
232	Q 191				—		—					frags br cal [illegible]
233	Q 208 3		—	—	—			—				cal f ad
234	Q 210	72·3	75	32·7	10	22·7	85					br cal y at. [persist front sut
235	Q 212 2				—							cr [illegible] y ad [illegible] interpar bs
236	Q 212 4	71·8	74·3	33·9	11·2	22·7	85·5					cal ad
237a	Q 214											[illegible] hair
237b	Q 215		—									+ frag y ad
238	Q 217	—			—							frags
239	Q 225 b				—						—	cal f ad [foram
240a	Q 225 W											[illegible] part of f ad [illegible] supraorb
240b	Q 229		—									br cal f ad [illegible] large interpar b
241	Q 238	—			—							back of skull frags ad
242	Q 241a	65·5	80·2	34·3	6·3	28	80·5					ca ad pterion
243a	Q 241	—										br cal f at
243b	Q 245	—										br, dome crystallised frags
244a	Q 252		—	—	—							cr ca [illegible]
244b	Q 254	71	73·8	35·2	13·2	22	87					cal ad
245	Q 265	—					80					br cal ad supraorb foramen
246	Q 272	69·9	75·1	35	6·4	28·6	81·5					cal ad
247	Q 276	68·8	80·2	31	4·8	20·2	85			—		ca ad
248	Q 276		—				—	—		—		frags golden (artificially coloured?) hair
249	Q 299	70·8	76·9	32·3	5·1	27·2	82					cal br f adolesc. persist front sut
250	Q 305a	64·8	82·6	32·3	1·1	31·2	84	100·5		30·5	50	cr ad portions of scalp, abnormal jugal
251	Q 305	69·1	72·3	38·6	17·7	20·9	90					cal ad supraorb foram
252	Q 311	69·1	74·4	36·5	13·6	22·9	88					cal y ad supraorb foramen
253	Q 314		—									cal adolesc
254	Q 315	—	—		—							cal parts of f ad
255	Q 331	69	72·8	38·2	8·2	30	81				—	cr ad hair [vertebra, etc.
256	Q 323											cal f ad little hair, interparietal [illegible]
257	Q 336 a	69·1	75·1	35·8	11·9	23·9	87					cal ad small and very heavy
258	Q 326	67·5	72·5	40	11	[illegible]	83·5					cal ad flat nasal bones, prenasal fossa on
259	Q 358 2							92	[illegible]	53		br cr when fr [face, diseased teeth
260	Q 359 2						—				—	cal f y ad wormian bs like Q 383 but
261	Q 359 3	64·2	80·2	35·6	7·3	28·3	87·5					cal ad [less marked
262	Q 365	70·3	76·9	32·8	5·6	27·2	82·5					cal ad [edly convex and sloped, interpar b
263	Q 383											cal f ad [illegible] portion of occipital mark
264	Q 384	74·7	72	33·3	9·5	23·8	81·5					cal y ad
265	Q 392						81·5					cal ad
266a	Q 396	—				—		—				cal f ad interpar b
266b	Q 395	—										dome old
267	Q 400			—			—					cal f ad parietal sutures closed
268	Q 403					—	82·45				—	cal ad
269	Q 405	—					91				—	cal ad
270	Q 409	—	—				—				—	cal ad [other wormian bones
271	Q 406	71·3	72·9	35·8	8·6	27·2	81·5					cal juvenile circa 17 yrs large interpar. and
272	Q 408	65	84	31	4·5	35·5	79·5					cal ad hair pterion
273	Q 411	[illegible]	73·1	34·5	[illegible]	24·6	83					cal. ad
274	Q 413		—					91·5	105·25	33	46	cr y ad
275	Q 420	—		—	—		78				—	br cal pterion wormian bs
276a	Q 428		—									br. cr adolesc spheno pariet sut. closed
276b	Q 430		—				—				—	frag
277	Q 436						—	86		27·5	45·5	br cr. ad
278	Q 437	63·5	82·1	34·4	7·9	26·5	90				—	cr cr y ad.
279	Q 435			—								br cal ad
280	Q 445	66·2	70·7	34·1	4·3	29·8	84					cal ad pterion on both sides
281	Q 466 3	—										br cal f
282	Q 466 4	70	74·7	35·3	10·8	24·5	85·5					cal f adolesc
283	Q 485 A						—				—	cal f ad
284	Q 487	—	—	—	—	—	—	—	—	—	—	cal f ad.
285	Q 488	—	—	—	—	—	—	—	—	—	—	dome ad

Miscellanea: Q [illegible] cal.—part of f. ad. covered with crystals and no measurements possible.—Q 212 frags. of two crania.—Q 230 frags. + mand. of child.—Q 251 frags.—Q 260 frags.—Q 264 frags.—Q 334 frags.—Q 364 frags.—Q 411^{b} frags

TABLE VI.

					MANDIBLE				
GH	*G′H*	*GB*	*J*	*N*	W_1	W_2	h_1	*f*	Remarks
—	58	89	—	40	—	—	—	—	cal. ad. very dendritic sutures
—	—	—	—	—	—	—	—	—	cal. – f. ad.
—	—	—	—	—	—	—	—	—	cal. – f. torus occipitalis
—	—	—	—	—	—	—	—	—	cal. – f. ad.
—	—	—	—	—	—	—	—	—	br. cal. – f. ad.
—	—	—	—	—	—	—	—	—	br. dome ad.
—	—	—	—	—	—	—	—	—	frags.
—	66	101·5	112·5	50·	—	—	—	—	cal. ad.
—	65	98	114?	44	—	—	—	—	impf. cr. ad. [foram.
—	—	—	—	—	—	—	—	—	cal. – f. + frags. y. ad. interpar. b. supraorb.
—	61	96	—	48	—	—	—	—	br. cal. ad. pterion
—	—	—	—	—	—	—	—	—	cal. – f. interpar. bone
—	—	—	—	—	—	—	—	—	br. cr. y. ad. cervical verteb.
—	66·5	87·5	—	45·	—	—	—	—	br. dome + f. ad.
—	—	—	—	—	—	—	—	—	br. cal.
—	—	—	—	—	—	—	—	—	br. cal. – f. ad.
—	—	—	—	—	—	—	—	—	br. cal. – parts of f. ad.
—	—	—	—	—	—	—	—	—	cal. juvenile. ? diseased
—	71	93?	—	49·	—	—	—	—	f. + frags. ad.
—	64?	92·5	115	48	—	—	—	—	cal. y. ad. pterion, small interpar. b.
—	—	—	—	—	—	—	—	—	frags.
—	68	—	—	50·	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	—	br. dome
—	—	—	—	—	—	—	—	—	cal. – part of f. ad.
—	66	102	130·75	49	—	—	—	—	br. cal. and frags.
—	68·5	90	—	48	—	—	—	—	cal. old. much closed sutures
—	64·5	88	123	44	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	—	br. cal. when br. ?
—	—	—	—	—	—	—	—	—	cal. – f. + frags. ad.
—	69·5	101	130	51	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	—	br. dome ad.
—	—	—	—	—	—	—	—	—	br. cal. – f. when br. ?
—	—	—	—	—	—	—	—	—	br. cal. – f. ad.
—	62·5	85	114·5	46	—	—	—	—	cal. y. ad. supraorb. foram. obelion depressed
—	65	91	117	46	—	—	—	—	cal. adolesc. pterion
—	58?	83·5	—	39·	—	—	—	—	br. cal. child
—	—	—	—	—	—	—	—	—	dome. ad. frags. of other crania
—	68	97·6	—	46	—	—	—	—	frags. + f. ad.
—	—	—	—	—	—	—	—	—	frags.
—	—	—	—	—	—	—	—	—	dome ad.
—	—	—	—	—	—	—	—	—	br. when br.?
—	—	—	—	—	—	—	—	—	frags. of cal. ad. wormian b.
—	64·5	96·4	—	52	—	—	—	—	cal. old
—	—	99·5	—	48·5	—	—	—	—	cal. f. br. old. small interpar. b.
—	—	—	—	—	—	—	—	—	dome + br. frontal
—	60	90	—	44	—	—	—	—	cal. – f. adolesc.
—	—	—	—	—	—	—	—	—	cal. – f. ad. interpar. b.
—	60	89	119?	46	—	—	—	—	cal. adolesc.
—	67·5	99·5	118	51	—	—	—	—	cal. ad.
—	—	—	—	52	—	—	—	—	cal. ad. wormian b. in sagit. sut.
—	—	—	—	—	—	—	—	—	frag.
—	64	95	—	45	—	—	—	—	br. cal. ad. interpar. b.
—	—	—	—	—	91?	119?	33	44	cal. – f. + m. ad. suts. largely closed
—	67·5	94	123·5	48·5	—	—	—	—	cal. ad.
—	61	87·5	—	45	—	—	—	—	cal. ad.
09·25	66?	87	120	52·2	90·5	—	29?	43	cr. ad. supraorb. foram.
—	71	99·5	—	53	—	—	—	—	f. + m. + frags. ad. supraorb. foram.
—	—	—	—	—	—	—	—	—	br. dome. ad. sagit. sut. closed.
—	65	96·5	—	44	—	—	—	—	f. + frags. y. ad. supraorb. foram.
09	—	84	116	45	88·5	109	31	42	cr. ad.

Miscellanea: Q 506 fra

MEASUREMENTS OF NAQADA CRANIA—*continued*

TABLE VI.

				Lengths								Circumferences			Face										Palate		Indices												Angles						Mandible				Remarks
No.	Gr. No.	Sex	C	L	L	L	L	B	H	OH	LB	U	S	Q	GH	G'H	GB	J	NH	NB	O_1 L	O_1 R	O_2 L	O_2 R	G_1	G_2	B/L	H/L'	B/L'	H/L	B/H	GH/GB	G'H/GB	NB/NH	O_2/O_1 L	O_2/O_1 R	G_2/G_1	GL	NL	AL	BL	θ_1	θ_2	PL	W_1	W_2	h_1	f	
286	Q 496	[illegible]	1275	174		178	[illegible]	84	[illegible]	111·75	93	509·5	305	292		58	89		40	23	38·5	37·5	29·5	30	52	37·5			78·7	73·0	107·7		65·2	57·5	76·6	80·0	72·1		—		—		—			—	—		cal. ad. very dendritic sutures
287	Q 509	[illegible]	[illegible]	174	[illegible]	175·5	[illegible]		129	[illegible]		493		[illegible]			—		—	—		—		—		—	77·1	72·9	77·8	73·[illegible]	105·[illegible]		—			—	—	—			—		—			—	—		cal. f. ad.
288	Q 511	[illegible]	[illegible]	175	178	177	[illegible]	[illegible]	127·5	[illegible]	93	491	303	288					—	—		—		—		—	71·0	71·6	72·0	72·0	100·0		—			—	—	—	—	—	—		—				—		cal. f. torus occipitalis
289	Q 513	[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	121	[illegible]	[illegible]	[illegible]	[illegible]					—	—		—		—		—	73·[illegible]	[illegible]	74·0	74·[illegible]	[illegible]·7		—			—		—			—		—				—		cal. f. ad.
290	Q 528	[illegible]		[illegible]		[illegible]	[illegible]		[illegible]										—	—		—		—		—			77·9	74·2	105·0		—			—		94	—		—		—				—		br. cal. – f. ad.
291	Q 544	[illegible]	—	[illegible]	[illegible]	[illegible]	139·7	92·5	[illegible]	118	100·25	530	[illegible]	[illegible]					—	—		—		—		—	72	71·8	71·6	71·4	100·4		—			—					—		—				—		br. dome ad.
292	Q 547		—			[illegible]	141	86											—	—		—		—		—		—	66·8				—	—		—					—		—				—		frags.
293	Q 549	[illegible]	1120	[illegible]	[illegible]	[illegible]	[illegible]	85	137	113	[illegible]	510	371	299		60	[illegible]	112·5	50·5	24	42·6	41·5	35	35·5	53	46	73·4	73·7	[illegible]	73·4	99·6		65	47·5	82·2	85·5	86·8	—			—		—	86			—		cal. ad.
294	Q 549			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		118		[illegible]	372	305		65	[illegible]	114	44	24	41	41	31	31·5	54	35	70·5	—	71·7				66·4	54·5	75·6	76·8	64·8	—	—		—	—	—	—			—		impf. [illegible] ad. [foram
295	Q 5[illegible]4			[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	112	[illegible]	515	[illegible]	311								—				—	—	—	70·7	[illegible]	[illegible]		—								—		—	—			—		cal. – f. + frags. y. ad. interpar. b. supraorb.
296	Q 5[illegible]5						[illegible]	87·5	[illegible]	118 (h)				298		61	90		48	23	41·5	40	31·5	[illegible]	47	34	—	—			[illegible]5		63·5	47·9	75·9	[illegible]	72·3	—			—		—	—			—		br. [illegible] ad. pterion
297	Q 567			177	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	91·5	49[illegible]·5	372·5	312												—	74·6	74·9	75·2	75·5	[illegible]		—			—					—		—	—			—		cal. f. interpar. bone
298	Q 569 A			[illegible]		182	[illegible]	87		[illegible] (h)													—			—	—		74·2				—		—	—					—		—	—		—	—		br. cr. y. ad. cervical verteb.
299	Q [illegible]83			[illegible]		185	[illegible]	[illegible]					[illegible]			[illegible]	87·5		45·5	24	43	43	34·5	31·5	55·5	36·5	—		73·7				76·0	52·7	[illegible]	73·3	65·8				—		—	—	—		—	—	br. dome + f. ad.
300	Q [illegible]590				[illegible]	[illegible]		[illegible]		[illegible]	[illegible]		[illegible]						—							—	—	78·1		79·[illegible]			—			—					—		—	—	—		—	—	br. cal.
301	Q 593					[illegible]		[illegible]	[illegible]	[illegible]	[illegible]·75		[illegible]						—							—	—		[illegible]·9			—	—	—		—	—				—		—	—	—		—	—	br. cal. f. ad.
302	Q 598			[illegible]		[illegible]	141·2		[illegible]										—						—	—	—		74·3	71·6	103·8	—	—			—	—	90·25			—		—	87	—		—	—	br. [illegible] parts of f. ad.
303	Q 610			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	116	[illegible]	475	354·5	291					—						—	—	75·6	75·3	[illegible]·4	70·1	100·4	—	—			—					—		—		—		—	—	cal. juvenile [illegible] diseased
303	Q [illegible]							[illegible]								71	[illegible]		[illegible]·5	25·5				34	53	45	—	—				—	76·4	51·5		—	84·9	—	—						—		—	—	f. + frags. ad.
304	Q [illegible]24		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]·5	[illegible]	113	[illegible]	[illegible]	[illegible]	[illegible]		64·[illegible]	92·5	115	48	25	44	44	33·75	33	[illegible]	45	75·9	75·6	75·4	75·1	100·4	—	69·2	52·1	76·7	75·0	—	85·5	68	74·6	37°·4	11·4	26	86°	—		—	—	cal. y. ad. pterion, small interpar. b.
305	Q [illegible]26							[illegible]		[illegible]					—				—							—	—	—				—		—	—	—	—						—	—	—		—	—	frags.
306	Q [illegible]31		[illegible]	[illegible]	[illegible]	177	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	491·5	305	[illegible]		[illegible]			50·5	23·5	41		32·5		53·75	38	74·2	74·5	73·2	73·4	99·6	—		46·5	79·3	—	70·7	93·5	72	70·2	37·8	13°·5	24·75	83·25	—		—	—	cal. ad.
307	Q [illegible]34							84					[illegible]						—							—	—	—				—		—	—	—					—	—		—	—		—	—	br. dome
308	Q [illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	88	[illegible]	11[illegible]	[illegible]	[illegible]·5	[illegible]	[illegible]					—							—	74·7	72·4	75·7	73·4	103·1	—		—	—	—					—	—		—	—		—	—	cal. part of f. ad.
309	Q [illegible]53				[illegible]			[illegible]		[illegible]	101·5	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	[illegible]·75	49	[illegible]·5	44·5	45	33	33·5	52	41		—				—	[illegible]·7	60·2	74·2	74·4	78·8				—			—	—		—	—	br. cal. and frags.
310	Q [illegible]73		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	122·5	[illegible]	531	[illegible]	314		[illegible]·5	[illegible]		48	23	41·5		32			—	71·2	74·6	71·2	74·6	95·5	—	76·1	47·9	71·9	—		94·5	63·2	83	33°·8	2·5	31·3	[illegible]·5	—		—	—	cal. old much closed sutures
311	Q [illegible]703		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]·5	[illegible]	114	[illegible]	[illegible]	374	292		[illegible]·5	88	123	44	20	43·5	44	30	31	—	—	[illegible]·7	72·9	69·5	72·7	95·6	—	73·3	59·1	[illegible]·0	70·5	—	94·25	69	78·2	32°·8	8·8	24	87	—		—	—	cal. ad.
312	Q 703					[illegible]	[illegible]		[illegible]																	—	—		69·6	68·2	[illegible]·1	—		—		—		—							—	—	—	—	br. cal. when br.?
313	Q [illegible]54			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	118	[illegible]	515	[illegible]	[illegible]												—	70·3	71·7	70·5	71·9	[illegible]	—		—		—		—		—	—				—	—	—	—	cal. – f. + frags. ad.
314	Q [illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	118	[illegible]	535	[illegible]	[illegible]		[illegible]	101	[illegible]	51	20	44	44	33	32	50·5	37·5	67·5	71·2	67·5	71·1	94·9	—	65·8	51·0	75·0	72·7	63·0	99	67·2	77·6	35°·2	7·4	27·8	85	—		—	—	cal. ad.
315	Q [illegible]766			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	[illegible]										—		—	77·2	70·9	76·5	70·2	[illegible]·4	—		—		—		—			—				—	—	—	—	br. dome ad.
316	Q [illegible]776				[illegible]			[illegible]		118		[illegible]	[illegible]	305												—						—				—		—			—				—	—	—	—	br. cal. – f. when br.?
317	Q [illegible]790	[illegible]	[illegible]	[illegible]	[illegible]	181·5	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]01·5	[illegible]												—	72·5	75·0	72·7	75·2	[illegible]·7	—				—		—							—		—	—	br. cal. – f. ad.
318	Q 791		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]·75	[illegible]	[illegible]	[illegible]·5	[illegible]	371·5	[illegible]		62·5	86	114·5	46	22·75	[illegible]	42·5	34	33	50	33	73·9	77·7	74·9	78·8	95·1	—	73·5	49·5	79·1	77·6	66·0	87·5	69	75	36	9·75	26·25	84·75	—		—	—	cal. y. ad. supraorb. foram. obelion depressed
319	Q [illegible]797		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]·5	[illegible]	[illegible]	[illegible]·5	488	[illegible]	[illegible]		[illegible]	[illegible]	117	46	24·5	[illegible]·5	[illegible]·5	32	31	61	44	71·3	71·9	71·7	72·0	[illegible]	—	71·4	53·3	81·0	78·5	72·1	91	73·6	70·3	36·1	12·2	23°·9	82·5	—		—	—	cal. adolesc. pterion
320	Q 79[illegible]			[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]			[illegible]	83·5		[illegible]·5	22·5	[illegible]	[illegible]·5	[illegible]	[illegible]·5	46	[illegible]	71·7	81·3	70·5	[illegible]	88·3	—	69·4	57·[illegible]	81·0	78·7	65·2	86·75							—		—	—	br. cal. child
321	807					[illegible]	[illegible]																				—		71·1		—	—				—	—	—							—		—	—	dome. ad. frags. of other crania
321	[illegible] 1272							[illegible]·5		[illegible]						[illegible]	97·0		40	23	[illegible]·5	[illegible]	30	29	60	37	—					—	69·7	50·0	75·9	74·4	61·7	—							—		—	—	frags. + f. ad.
322	[illegible]			[illegible]		[illegible]	[illegible]·5		[illegible]																	—	—		[illegible]·3	[illegible]·5	[illegible]·0	—	—			—	—	—							—		—	—	frags.
322	[illegible]					[illegible]	[illegible]					529	[illegible]									—				—	—		73·7		—	—	—	—	—	—		—							—		—	—	dome ad.
																							B GRAVES																										
323	B 2			[illegible]		[illegible]	[illegible]		[illegible]								—												[illegible]·2	71·7	93·9			—												—			br. when br.?
323	B 4					[illegible]	[illegible]	[illegible]				525·[illegible]		—			—												[illegible]·3									—	—	—			—			—			frags. of cal. ad. wormian b.
324	B 5	[illegible]	[illegible]	[illegible]	170	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	102	[illegible]	357	313		[illegible]·5	96·4		52	24	43·5	41	34	35·5	—		77·0	79·5	78·4	80·5	97·5		66·9	46·2	78·2	80·0		90·75	62	83·3	34·7	3·2	31·5	80·5		—			cal. old
325	B 12				[illegible]	[illegible]		[illegible]	113	[illegible]·5	105	518	383	308			99·5		48·5	27·5	44	43	33·5	43										56·7	76·1	76·7		98								—			cal. f. br. old. small interpar. b.
326	B 15					[illegible]			[illegible]	[illegible] (h)							—													70·8					—			—	—	—						—			dome + br. frontal
327	B 1			[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]				[illegible]	90		44	[illegible]	[illegible]	[illegible]	[illegible]	30				—	80·[illegible]	78·3	102·6		66·7	45·5	79·5	75·9	—	—								—			cal. – f. adolesc.
328	B 16					[illegible]	[illegible]	[illegible]		[illegible]																			72·0														—			—			cal. – f. ad. interpar. b.
329	B 21		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	112	[illegible]	477	359	295		60	89	119·[illegible]	40	22	39	[illegible]	31	32	46·5	34	74·8	75·6	74·8	75·0	[illegible]		65·5	47·8	80·1	84·2	73·1	83·25						84·5		—			cal. adolesc.
330	B 23		[illegible]	182	[illegible]	[illegible]	[illegible]	[illegible]·25	130	117·25	97	502	[illegible]·5	[illegible]		67·5	99·5	118	51	20	42	42	[illegible]·5	33	53·[illegible]	[illegible]	73·5	75·1	73·3	74·9	97·8		67·8	51·0	77·4	78·6	73·6	91	66·2	76·8	37	9·2	27·8	86		—			cal. ad.
331	B 24		[illegible]	[illegible]·5	185		[illegible]	[illegible]	[illegible]	125	100·5	523	385	320					52	20	43·75		31·5		45	[illegible]	70·[illegible]	78·4	75·5	78·6	96·0			55·8	70·4		80·1	90	68·6	77	34·4	8	20·4	85		—			cal. ad. wormian b. in sagit. sut.
331	B 24						[illegible]		11																						97·9						—									—			frag.
332	B 30			[illegible]·5	[illegible]	[illegible]·5		[illegible]	[illegible]	[illegible]·5	97		[illegible]7			[illegible]	[illegible]		45	21	41	30	[illegible]·75	[illegible]	54	42		71·1		71·9			[illegible]7·4	46·7	75·[illegible]	74·4	74·1	90·5	66·1	79	34·9	5·25	29·6[illegible]	84·25		—			br. cal. ad. interpar. b.
333	B 31			[illegible]		[illegible]	[illegible]·5	[illegible]	[illegible]	[illegible]·25	[illegible]	[illegible]	351	[illegible]													69·7	69·4	70·4	[illegible]·2	100·4						—								91·[illegible]	119·[illegible]	33	44	cal. – f. + m. ad. suts. largely closed
334	B 36	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]·5	[illegible]	[illegible]	[illegible]	[illegible]	98·5	517	351	312		[illegible]·5	94	123·5	45·5	23	42	42	[illegible]	[illegible]	53·[illegible]	35	72·[illegible]	71·1	72·8	71·7	101·5		71·8	47·4	[illegible]·0	[illegible]·0	65·3	95	[illegible]·2	75·7	35·1	[illegible]·8	24°·3	[illegible]·5					cal. ad.
335	B 39		[illegible]	[illegible]			[illegible]		[illegible]	[illegible]·5	[illegible]		[illegible]2·5	[illegible]		[illegible]	87·5		45	20	41	43·5	31·5	31·5	52	30·5	74·3	72·3	75·1	73·2	102·7		[illegible]·7	57·8	76·8	72·4	70·0	92·5	67·0	70·8	32·6	8·2	24·4	88					cal. ad.
336	B 47		[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	123	[illegible]	[illegible]·5	[illegible]·5	325	100·75	[illegible]	87	[illegible]	52·25	25	[illegible]	41	33·5	42	50·[illegible]	42	78·5	78·[illegible]	78·5	78·5	100·0	125·0	75·9	47·8	83·8	78·[illegible]	84·0	91	64	81	35	7·75	27·25	88·75	9[illegible]·5		29·[illegible]	43	cr. ad. supraorb. foram.
337	B 56			[illegible]·5		[illegible]·5	[illegible]	[illegible]·5	[illegible]							71	[illegible]·5		53	25	44·5	43	31·5	33	57	41			73·0	[illegible]·1	104·1	—	71·4	47·2	[illegible]·8	[illegible]·7							—			—	—		f. + m. + frags. ad. supraorb. foram.
338	B 60					[illegible]	[illegible]	[illegible]	[illegible]	[illegible]			[illegible]																[illegible]	[illegible]·5	88·7	—						—					—						br. dome. ad. sagit. sut. closed.
338	B 76	[illegible]				[illegible]		7		[illegible] (h)						[illegible]5	90·5		44	22		38	34	34	51	40						—	67·4	50·0		80·5	78·4						—						f. + frags. y. ad. supraorb. foram.
339	B 77		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]·7	[illegible]	89	484	351·5	288	[illegible]		84	116	45	24	[illegible]·5	40	32·5	31·5			72·4	72·2	72·6	72·4	100·[illegible]	129·8		53·3	82·3	78·8		85	68	76	36	8	28	84	88·5	109	31	42	cr. ad.

Miscellanea; Q 506 frags.—Q 512 frags.—Q 686ᶜ frags.—Qua¹ frags.—B 10 frags.—B 19 ♂ frontal bone + frags. B' = 92.—B 62ᵃ frags.—B 62ᵇ frags. B' = 93·5.—B 65 frag.

TABLE VII.

GH	G'H	GB	J	Mandible W₁	W₂	h₁	f	Remarks
				W_1	W_2	h_1	f	
—	—	—	—	94	101·5	31	—	dome + m.
—	—	—	—	—	—	—	—	frags. when br.?
—	—	—	—	—	—	—	—	br. dome
—	—	—	—	75·75	92	30·75	41	frags. of cr. + m. when br.?
—	—	—	—	—	—	—	—	cal. − f. y. ad. upwd. inclin. of occipit.
—	—	—	—	—	—	—	—	dome
—	63	93	—	—	—	—	—	cal. y. ad. small interpar. b. very dendritic [suts.
117	70	96	121	98	118?	33	48	cr. old.
—	63·5	110	128	—	—	—	—	cal. ad. skin
—	—	—	—	—	—	—	—	frags. ad.
—	—	—	—	—	—	—	—	dome + frags of f. large interpariet. b.
—	59	—	—	—	—	—	—	br. cal. ad.
—	63	95	119·	—	—	—	—	cal. base br. y. ad.
—	—	—	—	—	—	—	—	cal. − f.
—	67·5	94	—	—	—	—	—	br. cr. ad.
—	64	95	—	—	—	—	—	f. + frags. ad.
—	—	—	—	—	—	—	—	frag.
—	—	—	—	91	—	35?	—	br. dome. ad. large interpar. b.

GH	G'H	GB	J	W₁	W₂	h₁	f	Remarks
—	—	—	—	—	—	—	—	dome. ad.
—	72	97	125	—	—	—	—	br. cr. ad.
—	67	90	—	—	—	—	—	br. cal. ad.
111·75	69·6	99	129	94	118·75	34·75	47	cr. ad. supraorb. foram. partly fused interp.
—	—	—	—	—	—	—	—	dome. ad.
105·25	60	90	—	88	—	28	45	cr. old
—	—	—	—	—	—	—	—	cal. − f. ad. much inclined occipital
—	64	87	126	—	—	—	—	cal. ad.
—	66·5	92	—	—	—	—	—	cal. adolesc.
—	56	79	—	—	—	—	—	br. cal. adolesc. wormian bs. os antiepilepti- [cum. persist. front. sut.
—	—	—	—	—	—	—	—	br. cal. − f.
—	67?	94	131	—	—	—	—	cal. ad. supraorb. foram.
—	61	97	—	—	—	—	—	cal. adolesc.
97	60	88	112	85·5	98	27·5	44	br. cr. child
—	71·25	95	133	—	—	—	—	cal. ad. small interpar. b. deformed l. malar
—	52	—	—	—	—	—	—	br. cal. ad.
103·5?	—	—	—	84·5	95	28·5	—	cal. + frags. y. ad.
—	—	—	—	—	—	—	—	cal. − f. ad.
—	71·5	101·5	126	—	—	—	—	cal. ad. supraorb. foramina, much obliter. suts.
—	68·75	92·5	118	—	—	—	—	cal. ad.
—	—	—	—	—	—	—	—	cal.
—	—	—	—	—	—	—	—	cal. − f. ad. persist. frontal suture
105·5	61·2	88	114	99	107·5	37?	40·5	cr. ad.
—	60	89·3	—	—	—	—	—	cal. adolesc. without wisdom teeth
—	65	—	125	—	—	—	—	cal. with br. f. y. ad.
—	—	—	—	—	—	—	—	cal. − f. inclined occipital
—	67·5	—	—	—	—	—	—	cal. + part of f. ad.
—	—	—	—	—	—	—	—	br. dome. adolesc.
—	59·6	94·8	119	—	—	—	—	cal. ad.
118	71	101·5	127	98·75	109·5	35·5	46	cr. ad. short torus occipitalis, central part interparietal lozenge-shaped, bone closed above with parietals, strong interparietal groove along mid $\frac{2}{3}$ths of sagittal suture
—	71	99?	—	—	—	—	—	br. cr. pterion
—	—	—	128	—	—	—	—	cal. − part f. y. ad.
—	—	—	—	—	—	—	—	cal. − f. ad. small interpar. b. pterion
—	—	—	—	85	115	35·5	—	cal. − f. + m. ad.
120?	70?	91	118	89	104·5	26	44	cr. ad.

Miscellanea: B 83

				Lengths								Circumferences			Face										Palate		Indices												Angles						Mandible				
Series No	Grave No	Sex	C	F	L′	L	B	B′	H	OH	LB	U	S	Q	GH	G′H	GB	J	NH	NB	O_1 L	O_1 R	O_2 L	O_2 R	G_1	G_2	B/L′	H/L′	B/L	H/L	B/H	GH/GB	G′H/GB	NB/NH	O_2/O_1 L	O_2/O_1 R	G_2/G_1	OL	N∠	A∠	B∠	θ_2	θ_1	P∠	W_1	W_2	h_1	f	Remarks
340	B 78	♀	—	176	—	176	132·5	—	135·2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	75·3	76·8	98·0	—	—	—	—	—	—	—	—	—	—	—	—	—	94	101·5	31	—	dome + m.
341	B 83	[illegible]		180		182	134		131																—				73·6	72·0	102·3	—																	frag. [illegible]
342	B 86	[illegible]		[illegible]		[illegible]	[illegible]		130·5	106 (h)																			72·3	78	92·7																		[illegible]
343	B 90	[illegible]		183		182	126·5	88	141	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—		—	—	69·5	77·5	89·7								—	—	—	—	—	—	75·75	92	30·75	41	frags. of cr. + m. [illegible]
344	B 100		1321	175·5	177	[illegible]	133·5	91	130	118	96	493	357	[illegible]											—		75·4	76·8	75·9	77·3	98·2																		cal. [illegible]
345	B 107 A			172·2		[illegible]	[illegible]		131	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	76·1	75·9	100·2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	dome [suts.
346	B 112	[illegible]	1168	166	[illegible]	[illegible]	[illegible]	95	124	112	88·5	471·5	342	281		63	93		48	24	42	41	36	35	56	39·5	74·3	76·1	74·3	76·1	97·6	—	67·7	50·0	85·7	85·4	70·5	92·5	67°·1	78°·9	34°	2°·27	30°·73	82°·17					cal. [illegible] small interpar. b. [illegible]
347	B 113		[illegible]	192	188·5	190·5	134·5	90	137	120	107·5	521	390	[illegible]	117	70	99	121	51	26	40	42	36	35	58	42	71·4	75·3	[illegible]	74·2	94·7	121·0	[illegible]	51·0	[illegible]	83·3	72·4	105						85·5	98	[illegible]	33	48	cr. [illegible]
348	B 115	[illegible]	[illegible]	176	177·5	177·5	131	91	129·5	109	99	487 ?	354	290 ?	—	63·5	110	128	46	26	43	41	34	34	53·5	39	73·8	73·0	73·8	73·0	101·2	—	57·7	50·5	79·1	82·9	72·9	[illegible]	66·3	80·6	33·1	3·1	29·2	84·5					cal. [illegible]
349	B 118	[illegible]	[illegible]	185	[illegible]	183	132·5	91	128	112	101·5	510	372	298	—	—	—	—	—	—	—	—	—	—	—	—	72·4	69·9	71·6	69·2	103·5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	frags. ad.
350	B 119 A	♀		182·2	—	182	130	90·5	—	107·75 (h)	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	71·4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	dome + frags of f. large interpariet. b.
351	B 119b	♀	1158	167	167	167	128·5	87·5	124	106	89	475	354	282	—	59	—	—	44	23	—	39·5	—	30	53	35	76·9	74·3	76·9	74·3	103·6	—	—	52·3	—	75·9	62·3	86·5	70°·4	75°·8	33°·8	5°·2	29°	81°		—			br. cal. ad.
352	B 120	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	127	115·2	[illegible]	[illegible]		[illegible]		63	95	111·75	45	24	37·5	35	28	27	52	36	73·7	71·3	73·3	[illegible]	[illegible]		66·3	53·3	74·7	77·1	69·2	92	68·2	78·3	33·5	6·7	26·8	[illegible]					[illegible]
353	B 121			[illegible]		179·5	139·5		136																				77·7	75·8	102·6																		cal. f.
354	B 122	[illegible]			188·5	187·5		90·5		108 (h)						67·5	94		50	24·5	41	39·5	32	32	54	40							71·8	49·0	78·0	81·0	74·1												br. cr. ad.
355	B 125	[illegible]		181		183	140·5		124							64	95		43	25	41	44	28	29	55	37·5			76·8	67·8	113·3		67·4	58·1	68·3	65·9	68·2												f. + frags. ad.
356	B 129	[illegible]					[illegible]																																										frag.
357	B 131						[illegible]			[illegible]																																			91		35		br. dome. ad. large interpar. b.
	T GRAVES																																																
358	T 4a	[illegible]						84		100 (h)		508	—	—					—	—	—		—										—		—	—	—	—	—	—	—	—	—	—	—	—	—	—	dome. ad.
359	T 4b	♂	1370	185	186	186	134·7	91·5	133·5	115	100	512	375	302	—	72	97	125	54	26	41	40·5	33	34	56	42	72·4	71·8	72·4	71·8	100·9	—	74·2	48·1	80·5	84·0	75·0	97·5	69°·1	73°·4	37°·5	8°·6	28°·9	82°	—	—	—	—	br. cr. ad.
360	T 4c							[illegible]		[illegible]						67	90		45·2	21	40		31		[illegible]	[illegible]							74·4	46·5	77·5		79·0												br. cal. ad.
361	T 4		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	135·2	[illegible]	[illegible]	518	371	[illegible]	111·75	66·6	[illegible]	127·5	50	26·5	41	41	28·5	28·5	58	45·8	77·5	73·3	[illegible]	72·7	105·8	112·9	70·3	53	69·5	69·5	79·0	101	66·0	79·8	34·2	4·1	30·1	[illegible]	93	118·75	34·75	47	cr. [illegible] supraorb. foram. partly fused interp.
362	T 4	[illegible]			177·5	175	[illegible]	82	129·5	108		451·5		289													68·8	72·2	69·4	72·8	95·4																		dome. ad.
363	T 5 A	♀	1423	181	180	178	134·5	95	131	113	95	508	377	304	105·25	60	90	—	46	26	42	41	32	32	53·5	40	74·7	72·8	75·6	73·6	102·7	116·9	66·7	56·5	76·2	78·0	74·8	87·25	64°·9	79°·0	35°·2	10°·1	25°·1	90°	88	—	28	45	cr. old
364	T 5 B		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	113·6	94·4	493	352	[illegible]													75·0	71·0	75·4	71·4	[illegible]																		cal. f. ad. much reclined occipital
365	T 5		[illegible]	[illegible]	[illegible]	181	[illegible]	[illegible]	[illegible]	121·5	94·5	515	352	320		64	87	120·5	48·5	24	41	40	29	29	54	40	77·8	73·9	77·5	73·5	105·3		73·6	49·5	70·7	72·5	74·1	90	67·0	76·2	36·8	10·3	25·6	[illegible]					cal. ad.
366	T 5	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	132·5	[illegible]	[illegible]	[illegible]		494	355	[illegible]		[illegible]	[illegible]		46·5	24	39·5	38	30·5	31·5	54·5	42	[illegible]	72·1	[illegible]	72·0	104·3		72·3	[illegible]	77·2	82·9	77·1	98·75	66·0	77·5	36·5	5·1	31·4	82					cal. [illegible] persist. front. sut.
[illegible]	T 5	[illegible]				[illegible]						[illegible]	340	[illegible]		[illegible]	79		47	[illegible]	[illegible]	37	32	34	[illegible]	[illegible]	70·5	70·5	70·5	70·5	[illegible]		[illegible]	42·0	61·4	[illegible]	80·8												br. cal. [illegible] wormian [illegible]
367	T 8	[illegible]		[illegible]		[illegible]	[illegible]			105 (h)	[illegible]	391·5																	[illegible]																				br. cal. – f.
368	T 10 B	♂	1424	185	187	186	140·7	93	129	115	96	519	375	311	—	67 ?	94	131·6	49	26·3	44	43	32	32	50	36·5	75·2	69·0	75·6	69·4	109·1	—	71·3	53·7	72·7	74·4	73·0	87	63°·1	79°·8	37°·1	6°·2	30°·9	86°	—	—	—	—	cal. ad. supraorb. foram.
369a	T 14	♀		173·2	175	173·5	129·5	88	124	100	94	487·5	353·5	287		61	97		45·5	27	44	43	34	32	52	40·5	74·0	70·9	74·6	71·5	104·4		62·9	59·3	77·3	74·4	77·0	90	68°·8	76°·9	34°·3	10°·85	23°·45	87°·75					ca. [illegible]
369	T 15					[illegible]	131	[illegible]	131	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	97	66	88	112	44	25	37	35·5	33	32	45	32·5			75·7	75·7	100·0	110·2	[illegible]	56·8	89·2	90·1	72·2								85·5	98	27·5	44	br. cr. [illegible]
370	T 15		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	118	[illegible]	[illegible]	[illegible]	311		71·25	95	[illegible]	48	21·5	[illegible]	43	34·4	32	55	[illegible]	[illegible]	73·8	74·6	73·8	[illegible]		75·0	44·8	[illegible]	74·4	66·9	64	63·2	86·5	30·3	9·25	27·25	86·75					cal. [illegible] small interpar. [illegible] malar
370	T 15 B					[illegible]	[illegible]	[illegible]				[illegible]				52			35·5	25		30	[illegible]	26·5					75·7					[illegible]		75·7													br. [illegible] ad.
371	T 15		[illegible]	171·5	[illegible]	[illegible]	132·5	[illegible]	133	118	91	483	372	310	103·5						43		35		—		70·6	77·3	77·1	77·6	[illegible]					81·4		85	66·8	79·3	33·9	7·95	25·65	87·25	84·5	95	28·5		cal. + frags. [illegible]
372	T 18			[illegible]		181	131·5	88·5		117 (h)		[illegible]													—				72·7																				cal. f. ad.
373	T 19		[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	115	[illegible]	[illegible]	[illegible]	[illegible]		71·5	104·5	[illegible]	[illegible]	20	44	[illegible]	[illegible]	33·5	56	45·5	73·3	71·0	74·4	71·7	[illegible]		70·1	51·5	[illegible]	[illegible]	81·3	90·5	66·8	77·8	35·4	7·95	27·45	85·75					cal. ad. supraorb. foramina, [illegible] obliter. suts.
374	T 20		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	93	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		68·75	92·5	118	50·5	24	44·5	43	33·25	32	57·5	[illegible]	68·7	71·0	[illegible]	71·7	[illegible]		74·3	47·5	74·7	74·4	70·1	96	72·1	75·1	30·8	8·1	31·5	78·5					cal. ad.
375	T 20	[illegible]						[illegible]		116·75	101		305·5	[illegible]											—																								cal.
376	T 20c	[illegible]	—	177·2	—	170	129	—	132·5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	72·5	74·4	97·4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	cal. – f. ad. persist. frontal suture
377	T 23	♀	1273	178	178	178	132·5	84·3	125	113	92·5	492	363	300	105·5	61·2	88	114·25	40	24	38	39	28·6	28·4	53	35·5	74·4	70·2	74·4	70·2	106·0	119·9	69·5	60·0	75·3	72·8	67·0	91	72°·1	75°·3	32°·6	5°·7	26°·0	81°	99	107·5	37 ?	40·5	cr. ad.
378	T 23		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	481	348	[illegible]		[illegible]	86·3		41	24	[illegible]	38	27·25	28·5	54	38	78·8	68·2	75·8	[illegible]	[illegible]		67·2	58·5	[illegible]	75·0	70·4	85	71·2	73	35·8	5·5	30·3	78·5		—			cal. ad. [illegible] without wisdom teeth
379	T 23			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		111		510		[illegible]		65		125·5	46·5	25	45		33				71·7		72·1					53·7	73·3									[illegible]					cal. with [illegible]
380	T 23		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	115·5		514	370	[illegible]													77·2	73·4	77·2	73·4	105·2																		cal. f. [illegible] occipital
381	T 23	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	134·5	[illegible]	[illegible]	110·5	99	515	377	[illegible]		67·5			49			43		31·5			71·7	69·4	71·8	[illegible]	[illegible]					73·3	—	92·75	66·4	77·8	35·8	11·95	23·85	[illegible]					cal. + part of f. ad.
382	T 23	[illegible]		177		[illegible]	[illegible]		[illegible]																				71·9	73·2	102·3																		br. dome [illegible] ad.
383	T 23	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	90·5	128	101·8	[illegible]	511	370	298		59	94·5	116	43·5	27	40·5	39·5	30·9	30·5	54·5	38	75·3	70·1	74·3	69·2	107·4		62·4	62·1	76·3	77·2	69·7	92·5	68·2	79·8	32	1·45	[illegible]	81·25					cal. ad.
384	T 29 A	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	135	[illegible]	[illegible]	114	[illegible]	[illegible]	[illegible]	[illegible]	118	71	101·5	127	49	25	42	42	32·5	32	52·5	39	74·2	70·0	[illegible]	71·3	104·7	116·3	70·6	51·0	75·6	76·2	74·3	91·25	67·8	75·2	37	10·3	[illegible]	85·5	88·75	[illegible]	35·5	46	cr. ad. short [illegible], central part interparietal lozenge-shaped, bone closed above with parietals, strong interparietal groove along mid ⅔ths of sagittal suture
385	T 35	[illegible]		181		186	139	99	131	110 (h) ?						71	99 ?		53	29		46		34	53	40·5	—		74·7	70·4	106·1		71·7	54·5		73·9	70·4												br. cr. pterion
386	T 36		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	125	[illegible]	523	[illegible]	[illegible]				[illegible]			44		30		—		68·4	71·4	68·3	71·2	95·0				68·2		—	—											cal. part f. [illegible] ad.
387	T 37	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]											—		73·0	71·3	[illegible]	71·3	103·6																		cal. f. ad. small interpar. b. pterion
388	T 39	[illegible]	[illegible]	197·5	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	119	[illegible]	[illegible]	[illegible]	310													70·3	70·4	75·9	70·1	[illegible]														85	115	35·5		cal. f. [illegible] ad.
389	T 56	[illegible]	1275		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	111	95·5	[illegible]	[illegible]	290	120 ?	70 ?	91	118·5	50	24	43	42	36	35	51	37	70·0	73·0	70·6	73·5	104·2	131·9 ?	70·9	48·0	83·7	83·3	72·3	80	62	78·5	30·5	8·5	31	87	86	104·5	20	41	cr. ad.

Miscellanea: B 83 *bis* two much distorted infants' skulls.—B 94 frag. $B' = 89$.—B 102 frag.—T 5 D frags.—T 10 A frags. dome.—T 31 frags.—T 43 frags. of infant's skull

TABLE VIII.

	GH	G'H	GB	J		Mandible W				

	GH	$G'H$	GB	J		W_1	W_2	h_1	f	Remarks
?	—	56·5	84	111		—	—	—	—	cal. child. interpar. b.
	—	59·25	85	124	5	—	—	—	—	cal. ad.
	—	58·5?	85	119		—	—	—	—	cal. ad.
	—	71·5	96	124	75	—	—	—	—	cal. ad.
	—	62	88	128		—	—	—	—	cal. ad. temporal fossa rather small, lower temporal line very strongly marked; small prenasal fossa; constriction across parts of parietal bones; sutures in temporal fossa mostly closed
	—	67	93	126	5	—	—	—	—	cal. ad.
	—	66	95	122	5	—	—	—	—	cal. ad. molars gone
?	—	68	97	126	5	—	—	—	—	cal. ad. scalp
	—	—	—	—		—	—	—	—	br. or lost cr.
	—	—	—	—		96·5	109	35	44	cal. – f. + frags., long styloid processes
	103 ?	63 ?	92	118		88	102	26·5	43	br. cr. ad.
	—	—	—	—		—	—	—	—	cal. – f. ad. inclined occipital
	—	—	—	—		—	—	—	—	cal. – f.
?	—	—	—	—		—	—	—	—	frags. dome
	—	65	87	—		—	—	—	—	br. cal. ad.
	—	—	—	—		—	—	—	—	br. dome. y. ad.
	—	—	—	—		—	—	—	—	frags. ad. long hair
	—	75	—	—		—	—	—	—	br. cal. ad.
	—	60	92·5	—		—	—	—	—	f. + frags. ad.
	—	71	96	120		—	—	—	—	f. + frags. ad.
	—	—	—	—		—	—	—	—	br. dome + frags. y. ad.
?	—	72	94	117?		—	—	—	—	br. cal. much inclined occipital
	—	54	80	—		—	—	—	—	f. + frags. child

MANDIBLE

W_2	h_1	f
—	27·5	49
—	31·5	—
—	27	42
—	30·75	47·5
116	39	47
—	31 ?	—
—	36	—
—	24·5 ?	—
—	31·5	45·5
—	29	46
—	33	46
—	30	—
—	34	—
115·25	29	42
—	27	—
—	27	45
118·5	33	44
109	33	43
—	31	—
—	32	—
—	34	47
103·5	34·5	46
—	33	48
—	30	44
—	31	—

f
43
42
40
47·5
41·5
50
44
41·5
44
45·5
43
—
45·5
—
41
47
45
44
46
40·5
44
45
48
—

R GRAVES, UNNUMBERED BOXES, etc.

Series No.	Grave No.	Sex	C	Lengths								Circumferences			Face										Palate		Indices											GL	Angles						Mandible				Remarks
				F	L'	L	B	B'	H	OH	LB	U	S	Q	GH	G'H	GB	J	NH	NB	O_1 L	O_1 R	O_2 L	O_2 R	G_1	G_2	B/L'	H/L'	B/L	H/L	B/H	GH/GB	G'H/GB	NB/NH	O_2/O_1 L	O_2/O_1 R	G_2/G_1		N∠	A∠	B∠	θ_2	θ_1	P∠	W_1	W_2	h	f	
390	R 1	♀	1090	163·5	164?	164	129	84	121·5	108·5	88	464?	328?	290?	—	56·5	84	111	41	21	41	39	31	30	45	36	78·7	74·1	78·7	74·1	106·2	—	67·3	51·2	75·6	76·9	80·0	83	67°·5	78°·4	34°·1	11°·6	22°·5	90°	—	—	—	—	cal. child. interpar. b.
391	R 2	♂?	1215	163·8	165	165	137·5	85	121	112	88	478	345	300		59·75	85	124	48·5	24	41	40	32·5	32·5	48	40	83·3	73·3	83·3	73·3	113·6		69·7	49·5	79·3	81·3	83·3	85	69	74·7	36·3	13·8	22·5	88·5				—	cal. ad.
392	R 3	♂	1355	178	178·5	179	134	[illegible]	130	115	95	501·5	360	306		58·5?	85	119	49	22	44	43·5	34	34	43?		75·1	72·8	74·9	72·6	103·8		68·8	44·9	77·3	78·2		88·5	66	78·6	35·4	13·4	22	92	—	—	—		cal. ad.
393	R 4	♂?	1373	182	181·5	183	135·5	96·5	135·7	111	106	540·5	366	296	—	71·5	96	124	51·5	28	41	43	36·5	36·5	57·5?	39	74·7	74·8	74·0	74·2	99·9	—	74·5	54·4	89·0	84·9	67·8	91	—	—	—	—	—	89°·75	—	—			cal. ad.
394	R 5	♂	1458	183	185·5	185	139	93	137	113	97·75	515·5	374	298	—	62	88	128	48·5	27	44·75	44·75	33	31·5	54	42	74·9	73·9	75·1	74·1	101·5		70·5	57·7	73·8	70·4	77·8	95	65°·1	77°	37°·9	7°	30°·[illegible]	84					cal. ad. temporal fossa rather small, lower temporal line very strongly marked; small prenasal fossa; constriction across parts of parietal bones; sutures in temporal fossa mostly closed
395	R 6	♂	1434	181	182·5	185	140	91	135·8	113	90	508	373·5	304	—	67	93	126	45	22·5	40	40	33	31	55	44	76·7	74·4	75·7	73·4	103·1	—	72·0	50·0	82·5	77·5	80·0	90	64·1	81	34·9	3·5	31·8	84·5					cal. ad.
396	R 7	♀	1284	172·5	175·5	174	136	90	124	110	94	490?	353·5	274?	—	66	95	122	49	23·75	41·5	40	35	34·75	60·5	38·5	77·5	70·7	78·2	71·3	109·7	—	69·5	48·5	84·3	86·9	63·6	91·75	69·7	74	36·3	6·5	29·8	80·5					cal. ad. molars gone
397	R 8	♂	1275	183	188?	186	128	90·5	123	112?	96·5	534?	372?	288?		68	97	120	50	24·5	42·5	43	35	34·25	57	42	68·1	65·4	68·8	66·1	104·1	—	70·1	49·0	82·4	79·7	73·7	96	71·3	72·2	36·5	13·3	23·2	85·5	—				cal. ad. scalp
398	I MAST	?		184		188	130·7		131																	—		–	72·7	69·7	104·4	—	–	—	–	—	—	—	—	—	—	—	—	—	—	—	—	—	br. or lost cr.
399	U 6?	♂	—	—	—	184	130	—	141	120	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	70·8	76·6	92·2	—	—	—	—	—	—	—	—	—	—	—	—	—	96·5	109	35	44	cal. f. + frags., long styloid processes
400	G	[illegible]	1215	171	170·5	171	136	91	126	110·5	92·5	485	355	298	103?	63?	92	118	46	24	37	39	33·5	33	48?	43?	79·8	73·9	79·5	73·7	107·9	112·0?	68·5	52·2	90·5	84·6	89·6	80·75						81	88	102	20·5	43	br. cr. ad.
401	G 6	♀	1200	175	175	175	136	85	128	113	97·75	[illegible]	[illegible]	[illegible]											—	—	77·7	73·1	77·7	73·1	106·3	—													—		—	—	cal. – f. ad. inclined occipital
402	nn (vii)	♀	—	—	180	181	—	89	130	108·5	98	—	363	—	—	—	—	—	—	—	—	—	—	—	—	—	—	72·2	—	71·8	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	cal. f.
403	nn (viii)	?	—	—	—	—	—	—	—	115?	—	—	—	315?	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	frags. dome
404	nn (iii)	♀				177		[illegible]	132·5	116 (h)	93		[illegible]		-	65	87		44·5	23	40	40	34	33	51	35	—	-	-	75·4	—	—	74·7	51·7	85·0	82·5	68·6	-											br. cal. ad.
405	nn (i)	♀?				168	124		—	95 (h)	—	—				—	—	—	—	—	—	—	—	—	—	—	—	—	73·8	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	br. dome. y. ad.
406	nn (ii)	♀	—	179·2	—	180	130?	—	124	95 (h)	95	—	359	—	-	—	—	—	—	—	—	—	—	—	—	—	—	—	72·2	68·9	—	—	—	—	—	—	—	-	-	-					-			-	frags. ad. long hair
407	nn (ix)	?						94		112·5 (h)	[illegible]	516	[illegible]			75			39·25		42		32·5				—								77·4														br. cal. ad.
408	nn (x)	♂	1315			184	139	87·5	140	118·75 (h)	95·5		[illegible]			60	92·5		46	24	36·5	40	31	34·75	52·5	43			75·5	76·1	99·3		64·9	52·2	86·1	86·9	81·9	96·5						79	—			-	f. frags. ad.
409	nn (xi)	?			183·5	183·5		[illegible]	137	113·5	[illegible]		[illegible]			71	96	[illegible]	51	27·5	44	44	33·5	34	56	30		74·7	—	74·7			74·0	53·9	76·1	77·3	53·6	—						—					f. + frags. ad.
410	[illegible]	♂				182		88	137	112 (h)			[illegible]														-			75·3															—				br. dome + frags. y. ad.
411a	[illegible]	♂?			170·5	174		[illegible]	143·5	112·5	[illegible]	492?	369·5	297?		72	94	[illegible]	53	26?			35·5	30	50·5?	30		81·3		82·5			77·7	49·0	—		60·2	93	71·3	69·4	37·3	13·6	25·7	83?	—				br. cal. much inclined occipital
411b	S	?						92								54	80		41	21	37·5	35·5	32	[illegible]	48·5	32							67·5	51·2	85·3	84·5	66·0	—							-			—	f. + frags. child

Miscellanea: *nn* (vi) frags. of two skulls.

MANDIBLES WITHOUT CRANIA, SEXED IN A FEW CASES FROM LONG BONES OF SKELETONS.

Series No.	Grave No.	Sex	W_1	W_2	h_1	f
412	81	?			27·5	49
413	108c	?		—	31·5	—
414	113c	?			27	42
415	116	?	91·5	—	30·75	47·5
416	131		97	116	39	47
417	252	?	—	—	31?	—
418	269	?			30	
419	269 *bis*	?	—	—	24·5?	—
420	273	?	—	—	31·5	45·5
421	279	?	—	—	29	46
422	343	♂	—	—	33	46
423	380	?	—	—	30	—
424	382	♂	—	—	34	—
425	408	?	88	115·25	29	42
426	408 *bis*	?	—	—	27	—
437	528	?	82	—	27	45
428	577	?	90	118·5	33	44
429	590	?	93	109	33	43
430	594*b*	♂	—	—	31	—
431	674	?	—	—	32	—
432	685	?	91	—	34	47
433	696	♀	87	103·5	34·5	46
434	711	♀	97?	—	33	48
435	716	♀	—	—	30	44
436	728	?			31	

Series No.	Grave No.	Sex	W_1	W_2	h_1	f
437	731	♀			34·5	—
438	736	?	105?		33·5	50
439	739	?	—	—	25	—
440	740	?	75	—	29?	45
441	803	?	—	—	30	—
442	873	?		—	28	—
443	879*b*	?	—	—	32?	—
444	880	♂	—	—	34	—
445	1032	?	—	—	32	—
446	1037*c*	?	90	—	31	46
447	1201	?	—	—	30	—
448	1251	?	96·75	109·5	33	48
449	1304	?	78·75	—	29	45·5
450	1333	?	—	—	35	—
451	1504	?	82	—	34	43
452	1558	?	81	105·25	28·5	42·5
453	1573	?	92	102	37	46
454	1577	?	80·5	97	27·75	41·5
455	1592 *bis*	?	—	—	35	43
456	1612	?	97	113	39·5	47·5
457	1614	?	91	113·5	32	41
458	1614 *bis*	?	—	—	27	—
459	1614 E	?	75	100	33	44
460	1621	?	79·5	—	28	42·5

Series No.	Grave No.	Sex	W_1	W_2	h_1	f
461	1653 *bis*	?	85	103·5	35	40
462	1654 E	?	86		40	42
463	1654 W	?	74·5	—	32	40
464	1656	?	87·5	114?	31·5	48
465	1669	?	95	—	39	43
466	1729	♂	87·75	101·75	33·5	
467	1734	?	—	—	28	—
468	1739 high	?	79·5	97	32·5	39
469	1756	?	85	105	32	52
470	1789	?	88	110·5	28	44
471	1813	?	82	113·5	31·25	46·5
472	1858	?	75	105	29·75	40·5
473	1866	♂	94·5	—	38·5	42
474	1901	?	103	—	31·5	43
475	1909	?	90	107·25	29·5	43
476	1914	♀	88·5	108	35·75	43
477	B 34*b*	♀?	78·5	—	27	31
478	B 42	?	—	—	27	—
479	B 48	♂	97	—	34·5	47
480	B 80	?	—	—	34	44
481	B 94	♀	91·5	—	31·5	43
482	B 105*a*	?	—	—	34	—
483	B 106	♂	—	—	36	43
484	B 107	♀			33	

Series No.	Grave No.	Sex	W_1	W_2	h_1	f
485	B 108	♀	86	107	—	43
486	B 119*c*	♀	83·5	—	34	42
487	Q 336	?	85	109·75	37·5	40
488	Q 358(2)	?	94	100	32·5	47·5
489	R A	?	88	112·5	23·75	41·5
490	R B	?	100	100	34·75	50
491	R C	?	89	106	27·5	44
492	R D	?	96	120	33·25	41·5
493	R E	?	86	113	30	44
494	R V	?	80	100	26·5	45·5
495	T 4 A, D, E, F?	?	92	—	27	43
496	T 4 C *bis*	♂	—	—	40	—
497	T 10 A	♂	92·75	113	34	45·5
498	T 23 *bis*	?	—	—	30	—
499	T 29 *bis*	?	—	—	33·5	41
500	T 30	♂	105	115	34	47
501	T 57	♂	101·5	111·5	38·5	45
502	A	?	81·5	108·75	31	44
503	B	?	—	—	37	46
504	C	?	—	—	32	40·5
505	D	?	—	—	25	44
506	*nn*	?	86	117·35	—	45
507	β	?	—	—	28	48
508	φ	?	—	—	38	—

m. = mandible. br. = broken. frag. = fragment. A mere reference to a suture denotes that it is abnormal in character. Again *Pterion* denotes some abnormality of the sutures at or near the pterion; the irregularities here were so diverse that they could not be properly indicated without individual drawings. In the age appreciations, which are of course only approximate, *child* = less than 15 years, *adolesc.* 15 to 20, *y. ad.* 20 to 30, *ad.* 30 to 50, and *old* above 50.

Finally it may be noted that, besides the boxes included in the *Miscellanea*, which correspond to definite graves, there are in the collection (i) a number of boxes containing minute fragments of skull bones, in most cases of a considerable number of skulls mixed together; (ii) boxes containing skulls or odd bones of deer, goat, dog, etc.; and (iii) boxes containing mandibles of man: these latter are mostly broken or are fragments only; many of them do not belong to the skulls. Those that do belong to the skulls have such measurements as were possible given under the skull number, and a further series of jaw measurements is given in a separate table. The teeth in these jaws are, if anything, more fragile than the cranial bones. Mr Augustus Winterbottom, F.R.C.S., to whom we have shown specimens, finds in them no marked difference from modern teeth. In some cases they are remarkably ground down,—a result possibly due to eating either grain or grain ground in soft stone mills.

BIOMETRISCHE UNTERSUCHUNGEN ÜBER DIE SPIELARTEN VON HELIX NEMORALIS.

Von C. HENSGEN, Plettenberg, Westfalen.

I.—*Einleitung.*

Die hier mitgetheilten Untersuchungen über die Varietäten von *Helix nemoralis,* die Entwickelung der Zeichnung auf dem Gehäuse sowie die Vertheilungsverhältnisse der so entstehenden Spielarten betreffend, bildeten Vorarbeiten, welche früher von mir lediglich zur persönlichen Orientirung ausgeführt wurden, um eine Grundlage zu Züchtungsversuchen zu bilden, betreffend Constanz und Variabilität dieser Formen bei gegebenen natürlichen und künstlichen Lebensbedingungen.

Wenn ich diese Arbeiten hier veröffentliche, so nehme ich an, dass dieselben auch ein allgemeineres Interesse beanspruchen dürften, weil wir es hier mit einem Untersuchungsmaterial zu thun haben, wie es unsere einheimische Fauna für derartige Arbeiten kaum besser zu bieten vermöchte.

Ich habe in dieser Arbeit auch die combinatorische Bestimmung der Variationen mit berücksichtigt, da dieselben bei allen derartigen Untersuchungen anwendbar sind, sofern sich für die einzelnen Varietätenformen der Individuen gewisse Begrenzungsgebiete feststellen lassen, und sich so bei der Vergleichung der theoretischen Möglichkeiten mit den natürlichen Ergebnissen schon gewisse Schlussfolgerungen ergeben. Ganz besonders ladet das Material, welches die Stylommatophoren dem Forscher bieten zu Züchtungsversuchen ein. Die Organisation dieser Mollusken als Zwitter würde schon an und für sich in dieser Beziehung interessante Resultate versprechen, als in Bezug auf die Befruchtung und über die Entwickelung der Jungen, sowie deren Vertheilung und Eigenschaften, bezogen auf das Elternpaar, nur sehr wenig bekannt zu sein scheint*.

In Bezug auf den bei Eintritt der kälteren Jahreszeit sonst beginnenden Winterschlaf der Thiere kann ich mittheilen, dass dieser bei Züchtungsversuchen

---

* Vide: Sporleder, A., Fortges. Beobachtungen über die Wachsthumszeit einiger Schnecken, *Malakozoolog. Blätter,* Bd. VII. (1861). Bronn's *Kl. u. Ordn. d. Thierreiches,* Bd. III.

in genügend temperirten und feucht gehaltenen Räumen vollständig wegfällt. Ich hatte seiner Zeit (1879) auf Fürsprache von Prof. Oscar Schmidt von Prof. de Bary in Strasburg Erlaubniss erhalten eine Anzahl von mit Drahtgittern verschlossenen Kästen in den Gewächshäusern des alten botanischen Gartens aufstellen zu dürfen und habe daselbst verschiedene Arten der Stylommatophoren bei vollkommener Lebensthätigkeit überwintert. [Diese Untersuchungen betrafen damals Absonderung und Beeinflussung der Bildung der Schalen.]

II.—*Combinatorische Berechnungen.*

Für alle Untersuchungen, welche die Vergleichung bestimmter Eigenschaften von Individuen zum Zwecke haben, ist es von Wichtigkeit die Zahl aller durch die Variabilität dieser Eigenschaften *überhaupt möglich werdenden characteristischen Formen* kennen zu lernen, so fern dieselben innerhalb gewisser durch die organische Anlage bedingter Grenzen sich festlegen lassen. Man ist dann in der Lage die gesetzmässig möglichen Formen mit den in der Natur wirklich beobachteten in Vergleich ziehen zu können, woraus sich weiter bereits oft wichtige Resultate für die biologische Forschung ergeben können.

Für die Varietäten, welche bei den Mollusken in Folge Variation in der Zeichnung der Schalen und Gehäuse entstehen können, sind in Bezug auf die Lage jener Zeichnungen bestimmte feste Gebiete gegeben, in Abhängigkeit von der Lage und Vertheilung der Farbdrüsen im Mantel der Thiere, welche Drüsen ja die Ausscheidung des Farbstoffes an den nächstliegenden Stellen der Umhüllung bedingen. Bei den Streifenvarietäten der Heliceen, welche im Besonderen hier in Betracht kommen, handelt es sich daher bei der combinatorischen Berechnung um Combinationen ohne Wiederholung und ohne Inversionen. Es ist nun ferner bei der Feststellung möglicher Formen darauf Bedacht zu nehmen, dass in allen Fällen, bei denen durch Verbreiterung einzelner nebeneinander liegender Streifen eine Verschmelzung dieser zu einem gemeinschaftlichen Bande stattfindet (wodurch eine scheinbare Verminderung der absoluten Streifenzahl eintritt) dies sowohl in der combinatorischen Berechnung als auch bei der Classificirung der in der Natur beobachteten Funde zum Ausdruck gelangt. Ich bezeichne daher die durch die normalen Lagegebiete von Drüsen erzeugten farbigen Ausscheidungen als *Streifen*, die durch Zusammenfliessen solcher Streifen erzeugten Verbreiterungen als *Bänder*. Die Bezeichnung der Streifen selbst ist die übliche, ausgedrückt durch die Zahlenreihe 1, 2, 3... u.s.w. und dabei ausgehend von dem, dem Nabel am nächsten liegenden Streifen. Bei den Bändern sind die, die betheiligten Streifen anzeigenden Zahlen durch umfassende Klammern gekennzeichnet.

A. *Die combinatorisch möglichen Formen in den Variationen der Streifung, welche durch Verschwinden von* n *Streifen oder durch deren Zusammenfliessen zu Bändern entstehen können, als Formen von Combinationen ohne Wiederholung und ohne Inversionen.*

I. *Keine Streifen*

Summe $S=1$.

II. *Ein Streifen*

$$S=n.$$

III. *Zwei Streifen oder deren Band*

$$S=\frac{n\,(n-1)}{1\,.\,2}+(n-1)=\tfrac{1}{2}[n^2+n-2].$$

IV. *Drei Streifen und deren Bänder*

$$S=\frac{n\,(n-1)\,(n-2)}{1\,.\,2\,.\,3}+(n-2)+(n-2)\,(n-1)$$

$$=\tfrac{1}{6}[n^3+3n^2-10n].$$

V. *Vier Streifen und deren Bänder*

$$S=\frac{n\,(n-1)\,(n-2)\,(n-3)}{1\,.\,2\,.\,3\,.\,4}+(n-3)+(n-3)\,(n-2)$$

$$+\tfrac{1}{2}(n-3)\,(n-2)+(n-1)\,\frac{(n-2)\,(n-3)}{1\,.\,2}$$

$$=\tfrac{1}{24}[n^4+6n^3-25n^2-30n+72].$$

VI. *Fünf Streifen und deren Bänder*

$$S=\frac{n\,(n-1)\,(n-2)\,(n-3)\,(n-4)}{1\,.\,2\,.\,3\,.\,4\,.\,5}+(n-4)+(n-3)\,(n-4)$$

$$+(n-2)\,\frac{(n-3)\,(n-4)}{1\,.\,2}+(n-1)\,\frac{(n-2)\,(n-3)\,(n-4)}{1\,.\,2\,.\,3}+(n-3)\,(n-4)$$

$$+\tfrac{1}{2}(n-3)\,(n-4)$$

$$=\tfrac{1}{120}[n^5+10n^4-45n^3-190n^2-584n]$$

u.s.w. für 6, 7.........Streifen.

B. *Gesonderte Berechnung, getrennt nach den theoretisch möglichen Formen an* (1) *Streifen,* (2) *Bänderformen, sowie* (3) *Combinationen von Streifenformen mit Bänderformen für* n *gegebene Elemente.*

1. *Streifenformen*

$S=2^n$ (einschliesslich der Form $n=0$, der streifenlosen Form).

2. *Bänderformen:*

a. Für ein Band

$$S=\tfrac{1}{2}n\,(n-1).$$

b. Für zwei Bänder:

α. Bei betheiligten 4 *Streifen und Bändern von je* 2 *und* 2 *Streifen*

$$S=\tfrac{1}{2}(n-2)\,(n-3).$$

β. Bei 5 *Streifen und Bändern von je* 3 *u.* 2 *oder* 2 *u.* 3 *Streifen*

$$S=(n-3)\,(n-4).$$

γ. Bei 6 *Streifen und Bändern von je*

$$\left.\begin{matrix}2\ \textit{u.}\ 4\\ 4\ \textit{u.}\ 2\\ 3\ \textit{u.}\ 3\end{matrix}\right\}\ \textit{Streifen}$$

$$S=\tfrac{3}{2}\,(n-4)\,(n-5).$$

δ. *Bei 7 Streifen mit Bändern von je*

$$\left.\begin{array}{l} 2 \text{ u. } 5 \\ 5 \text{ u. } 2 \\ 3 \text{ u. } 4 \\ 4 \text{ u. } 3 \end{array}\right\} \textit{Streifen}$$

$$S = 2\,(n-5)\,(n-6),$$

und so weiter.......

c. *Für drei Bänder:*

c. *I.* α. *Bei je* 2, 2 *und* 2 *Streifen*

$$S = \tfrac{1}{2}[(n-4)\,(n-5) + (n-5)\,(n-6) + (n-6)\,(n-7) + \ldots + 2\,.\,1].$$

c. *I.* β. *Bei je*

$$\left.\begin{array}{l} 2,\ 2 \text{ u. } 3 \\ 2,\ 3 \text{ u. } 2 \\ 3,\ 2 \text{ u. } 2 \end{array}\right\} \textit{Streifen}$$

$$S = \tfrac{3}{2}[(n-5)\,(n-6) + (n-6)\,(n-7) + \ldots + 2\,.\,1].$$

c. *I.* γ. *Bei Bändern von je*

$$\left.\begin{array}{l} 2,\ 2 \text{ u. } 4 \\ 2,\ 4 \text{ u. } 2 \\ 4,\ 2 \text{ u. } 2 \end{array}\right\} \textit{Streifen}$$

$$S = \tfrac{3}{2}[(n-6)\,(n-7) + \ldots + 2\,.\,1],$$

und so weiter.

c. *I.* δ. *Bei Bändern von je*

$$\left.\begin{array}{l} 2,\ 2 \text{ u. } (n-4) \\ 2,\ (n-4) \text{ u. } 2 \\ (n-4),\ 2 \text{ u. } 2 \end{array}\right\} \textit{Streifen}$$

$$S = \tfrac{3}{2}[1\,.\,2].$$

Oder total:

$$S = \tfrac{1}{2}(n-4)\,(n-5) + \tfrac{4}{2}(n-5)\,(n-6) + \tfrac{7}{2}(n-6)\,(n-7) + \tfrac{10}{2}(n-7)\,(n-8) + \ldots\ldots$$

c. *II.* *Bei je*

$$\left.\begin{array}{l} 2,\ 3 \text{ u. } 3 \\ 3,\ 2 \text{ u. } 3 \\ 3,\ 3 \text{ u. } 2 \end{array}\right\} \textit{Streifen}$$

$$S = \tfrac{3}{2}[(n-6)\,(n-7) + (n-7)\,(n-8) + \ldots\ldots].$$

c. *III.* α. *Bei je*

$$\left.\begin{array}{l} 2,\ 3 \text{ u. } 4 \\ 2,\ 4 \text{ u. } 3 \\ 3,\ 2 \text{ u. } 4 \\ 3,\ 4 \text{ u. } 2 \\ 4,\ 3 \text{ u. } 2 \\ 4,\ 2 \text{ u. } 3 \end{array}\right\} \textit{Streifen}$$

$$S = \tfrac{1}{2}\,(3\,.\,2)\,[(n-7)\,(n-8) + (n-8)\,(n-9) + \ldots\ldots].$$

c. III. β. Bei je

$$\left.\begin{array}{l} 2,\ 3\ u.\ 5 \\ 2,\ 5\ u.\ 3 \\ u.\ s.\ w. \end{array}\right\}\ \textit{Streifen}$$

$$S=\tfrac{1}{2}(3.2)[(n-8)(n-9)+(n-9)(n-10)+\ldots\ldots]$$

u. s. weiter.

c. III. γ. Bei je

$$\left.\begin{array}{l} 2,\ 3\ u.\ 6 \\ 2,\ 6\ u.\ 3 \\ u.\ s.\ w. \end{array}\right\}\ \textit{Streifen}$$

$$S=\tfrac{1}{2}(3.2)[(n-9)(n-10)+\ldots\ldots].$$

Oder total:

$$(3.2)[(n-7)(n-8)+2(n-8)(n-9)+3(n-9)(n-10)+\ldots\ldots+\ldots\ldots].$$

c. IV. Bei je

$$\left.\begin{array}{l} 2,\ 4\ u.\ 4 \\ 4,\ 2\ u.\ 4 \\ 4,\ 4\ u.\ 2 \end{array}\right\}\ \textit{Streifen}$$

$$S=\tfrac{3}{2}[(n-8)(n-9)+(n-9)(n-10)+\ldots\ldots].$$

c. V. Bei je

$$\left.\begin{array}{l} 2,\ 4\ u.\ 5 \\ 2,\ 5\ u.\ 4\ u.\ s.\ w. \\ 2,\ 4\ u.\ 6 \\ 2,\ 6\ u.\ 4\ u.\ s.\ w. \\ 2,\ 4\ u.\ 7 \\ 2,\ 7\ u.\ 4\ u.\ s.\ w. \end{array}\right\}\ \textit{Streifen}$$

$$S=\tfrac{1}{2}(3.2)[(n-9)(n-10)+2(n-10)(n-11)+\ldots\ldots+\ldots\ldots]$$

u. s. weiter.

Für eine 4m. Anzahl, 5m. Anzahl u. s. w. von Bändern lassen sich so alle möglichen Combinationen von Bändern zusammenstellen.

III.—*Combinationen dieser Bänderformen mit Streifenformen.*

Hierfür ergiebt sich allgemein:

Sind $c_{p,q,r,s\ldots\ldots}$ die Anzahl Combinationen von *Bändern*, bestehend aufeinanderfolgend aus p, q, r, $s\ldots$ zusammengeschlossenen *Streifen*, so ist die Anzahl der Combinationen dieser Bänder mit 1, 2, 3 u. s. w. Streifen

$$S=C_{p,q,r,s\ldots\ldots}\times(2^{n-p-q-r-s\ldots\ldots}-1)$$

wobei $n=1$ resp. 2, 3, u. s. w. ist.

Die Grösse der Betheiligung der einzelnen Streifen bei der Bildung sämmtlicher combinatorisch möglichen Formen ist eine ziemlich gleiche für die hier in Betracht kommenden Untersuchungen*, wie sich aus der nachstehenden Zusammenstellung ergiebt. Wir werden später sehen, wie das Vorherrschen einzelner Streifen und

---

* $n=5$.

Varietäten bei den beobachteten Funden, dieses Verhältniss der Vertheilung recht wesentlich verändert (Tabelle D).

Vertheilung des Vorkommens der Streifen enthalten in den Combinationen.

Bezeichnung der Streifen. Streifen No.	Mit			Streifen		Summe
	1	*2*	*3*	*4*	*5*	
1	1	5	13	20	16	55
2	1	2+4	8+8	16+8	16	63
3	1	1+2+3	7+6+4	16+8	16	64
4	1	1+1+1+2	6+6+4	16+8	16	63
5	1	1+1+1+2	5+4+4	12+8	16	55

IV.—*Die Sammlungen.*

Nach der Belagerung und Einnahme von Strasburg in 1870 war ein grosser Theil der zerstörten Angriffsfront des Nordens der Befestigung in Rücksicht auf die geplante Erweiterung derselben eine Reihe von Jahren liegen geblieben. Unter solchen Verhältnissen waren für die Entwickelung einer reichen Klein-Fauna Bedingungen geschaffen, wie sie sich kaum günstiger denken lassen, und so fand sich auch hier *Helix nemoralis* auf dem Mauerwerk der Befestigungen (alles noch nach dem alten Vauban'sche System aufgeführt) so wie dem sich anschliessenden hochwuchernden Strauchwerk in zahlreichen Exemplaren vor.

Wie weit hier in unmittelbarster Nähe des geräuschvollen Grossstadtlebens eine in sich abgeschlossene Wildniss entstanden war, bewiesen die an sonnigen Mittagen aus Mauerspalten und Schlupflöchern hervorgekrochenen Nattern[*] von oft beträchtlicher Grösse, die zusammengerollt auf dem Strauchwerk sich sonnten. Verwilderte Hauskatzen zogen in den Schusslöchern ihre Jungen auf und einmal traf ich an hellem Nachmittage sogar eine Katze (die doch sonst das nasse Element möglichst scheut) mitten im Wassergraben bis an den Kopf im Wasser still auf dem Schilfe liegend, um einem in der Nähe mit ihren Küken herumschwimmenden Wasserhuhn aufzulauern.

Diese kurzen Angaben mögen genügen um zu zeigen, in welchem stillen und abgeschlossenen Zustande die von mir abgesuchten Fundorte seinerzeit[†] sich befanden.

Wenn ich bei den vorliegenden Sammlungen der Begrenzung der Fundorte durch mehrfache Wasserläufe sowie künstliche nasse Gräben eine gewisse Bedeutung für die Funde beilege, so bin ich mir andererseits des bedingten Werthes solcher Absperrungen wohl bewusst, denn obwohl einerseits die primitive Ent-

---

[§] Hauptsächlich *Tropidonotus natrix*.

[†] An diesen Stellen erheben sich jetzt der Kaiserpalast, die Bibliothek, die Gebäude der Kaiserstrasse sowie die neuen Universitätsbauten.

wickelung der Locomotion der Pulmonaten gegen eine weitgehende active Wanderung dieser Mollusken spricht, so sind andererseits doch vielfache Beobachtungen vorhanden, die auf eine bemerkenswerthe Ueberschreitung des eigentlichen Verbreitungsgebietes hinweisen, und kann ich die Mittheilungen von Kobelt* nur bestätigen. *Helix pomatia* z. B. fand ich in einer Anzahl Enclaven, die für das Kalkbedürfniss der Thiere besonders günstige Bedingungen boten, innerhalb eines Zeitraumes von 10 Jahren jedes Jahr regelmässig in gut entwickelten starkschaligen Exemplaren wieder, und zwar in den Thaleinschnitten von Lenne, Bigge und Veischede im Sauerlande, am Schloss zu Bilstein, auf Ruine Schnellenberg bei Attendorn, Ruine Schwarzenberg und Schloss Brüninghausen bei Plettenberg, Schlossberg zu Altena und Schloss zu Hohenlimburg, trotz des für *H. pomatia* verhältnissmässig hochgelegenen rauhen Gebietes†.

Immerhin wird man aber zugeben müssen, dass bei den so zahlreich (an den Fundorten I, II, IV u. V besonders recht übereinstimmend) beobachteten Individuen von *H. nemoralis*, hier für diese Art wenig Veranlassung gegeben war ihr von mehrfachen Wasserlinien begrenztes Gebiet durch active Wanderung in grösserer Zahl zu verlassen oder von aussen her einen grösseren Zuzug zu erhalten, so dass die vorgefundenen Vertheilungsverhältnisse der Varietäten auch auf eine gewisse natürliche Grundlage zurückgeführt werden können.

Dass übrigens schon geringe Aenderungen in den allgemeinen Lebensbedingungen auf die Variabilität, in Bezug auf die Entwickelung der Streifung, Einfluss ausüben können, ergiebt sich schon deutlich aus einer Vergleichung der Vertheilungsverhältnisse von den obenerwähnten vier Fundorten I, II, IV u. V mit den Funden der Sammlung III, worauf ich noch zurück kommen werde.

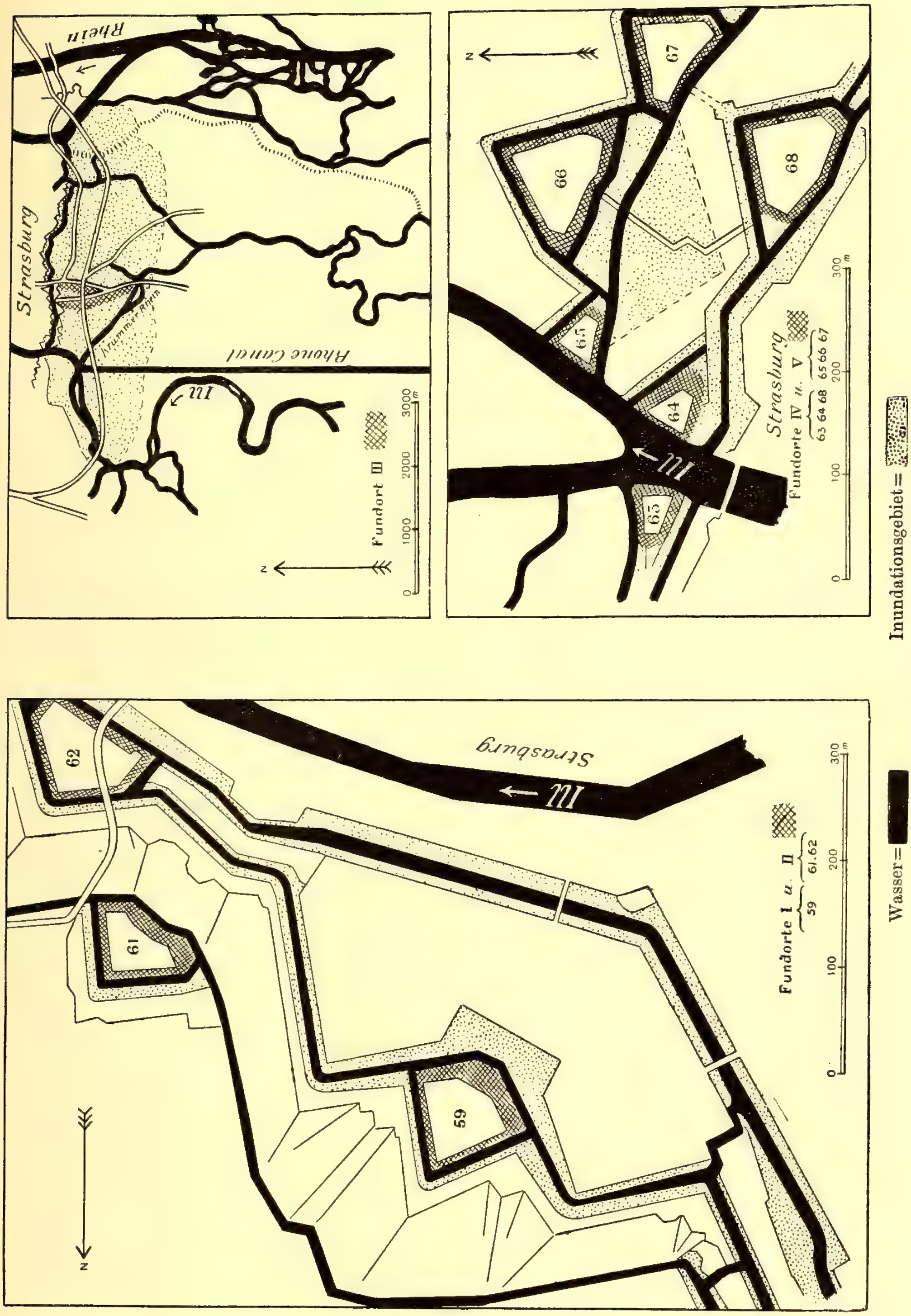
Die Sammlungen für jeden einzelnen Fundort fanden hintereinander an demselben Tage statt und zwar wurde dazu ein solcher ausgesucht, bei welchem auf ein zahlreiches Verlassen der Schlupfwinkel der Thiere zu rechnen war, also warme Witterung nach vorausgegangenem Regen bei zugleich hoher Dampftension der Luft. Letzteres ist sehr wichtig, da die Pulmonaten sehr empfindlich gegenüber dem Gehalte der Luft an Wasserdampf sind und bei scheinbar feuchter Witterung (nassem Boden) aber sehr trockner umgebender Luftströmung sich oft zurückgezogen halten, wie ich dies auch durch Versuche bei künstlich hergestellten verschiedenen Gehalten von Luft an Wasserdampf feststellen konnte.

Für die Bestimmungen der Art wurden von jeder Sammlung eine Anzahl Exemplare sowie alle zweifelhaften Funde näher untersucht, Form der Kiefer und

---

* Kobelt, *Studien zur Zoogeographie*, Wiesbaden 1897–98, pag. 29 u. folg. Siehe auch Müllenhof, *Die Ortsbewegung der Thiere.*

† Auf den primären Lagerstätten des Kalksteins bei Attendorn im Biggethale sowie zu Lethmate an der Lenne habe ich *H. pomatia* nicht angetroffen. Es scheinen hiernach der aus Aetzkalk hergestellte Mauerkalk und Weisskalk für die Thiere besonders günstig für die Assimilation zu sein, wofür auch anderweitige Beobachtungen sprechen. Dieses durchsuchte Gebiet hat eine Länge von über 80 Kilometern.



Wasser = ■ Inundationsgebiet = ▨

Karte der Fundorten.

die Zähnung der Radula controllirt, sowie besonders die Form der Liebespfeile, soweit solche vorhanden waren, festgestellt*.

Auch wurde zur Aufgabe gemacht, an jedem Fundorte das bestimmte Gebiet auf alle möglichen zugänglichen Exemplare abzusuchen, und ferner neben ausgewachsenen Exemplaren auch alle jungen Thiere mit zu berücksichtigen. Für das Fundverhältniss zwischen ausgewachsenen und jungen Thieren ist allerdings ein persönlicher Fehler immer insofern vorhanden, als die jungen Thiere weniger leicht gefunden werden können, indessen zeigt doch auch hier die Vergleichung der Verhältnisse zwischen alten und jungen Thieren eine befriedigende Annäherung der Werthe. Eine Abweichung in den Verhältnissen alter und junger Thiere muss aber nothwendig noch aus dem andern Grunde eintreten (der mich ja veranlasste auf diesen Umstand bei Sammlungen Rücksicht zu nehmen), als nämlich Zeichnungen oft erst nahe dem Rande zu auftreten. Zur Beleuchtung dieser Thatsache sind in der Tabelle C diese unvollkommenen Spielarten und die Betheiligung der einzelnen Streifen dabei besonders zusammengestellt. Auf der Karte, Seite 475, sind die Fundorte mit besonderer Berücksichtigung der wasserführenden Begrenzungen wiedergegeben.

V.—*Zusammenstellungen und Folgerungen.*

Die Vergleichung der Werthe in den Rubriken der Tabelle A für die Streifenformen, beobachtet an den fünf Fundorten und nach dem Vorkommen von 1, 2... bis 5 Streifen zusammengestellt, sowie die hiernach auf den Figuren 1 bis 5 wiedergegebenen graphischen Darstellungen zeigen eine in der Hauptsache gleiche Zu- und Ab-nahme in den Curvenstücken, so dass die Schlussfolgerung wohl gestattet sein dürfte, dass auf Grundlage der Fundmengen sich hier schon Näherungswerthe an die wirkliche Vertheilung an diesen Fundorten berechnen lassen.

Ein wesentlicher Unterschied zwischen den Funden von I, II, IV u. V und den Funden von III zeigt sich (wie schon früher erwähnt wurde) insofern, als bei III die streifenlose Form bedeutend zurücktritt, und andererseits die vollkommenste Form 1.2.3.4.5 mit ihren Nebenspielarten eine beträchtliche Zunahme erfahren hat, wie dies in den späteren Curven Fig. 3, *a* u. *b* noch deutlicher zum Vorschein kommt. Dieses Resultat steht aber in vollkommenem Einklang mit der allgemeinen Beobachtung, dass Entwicklung von Farbe und Zeichnung im Thier- und Pflanzenreiche in einem gewissen Zusammenhang mit der Stärke und Dauer der Belichtung stehen und das gebotene Maximum an Licht durchaus nicht fördernd für die Entwicklung farbiger Spielarten ist. Für den Fundort III ergaben sich aber bei der daselbst vorhandenen günstigen Belaubung auch günstigere Bedingung zu einer kräftigerer Entwicklung von Farbenvarietäten, während umgekehrt die stets dem ganzen Tageslichte angesetzten Fundorte von I, II, IV u. V eine

---

* Siehe hierüber auch: Schmidt, A., *Der Geschlechtsapparat der Stylommatophoren* (Berlin 1885), sowie von demselben: Artenunterschied von *H. nemoralis* und *H. hortensis* unter Berücksichtigung der Liebespfeile, *Zeitschr. f. Malakozoologie* VI. S. 49 (1849).

TABELLE A, I.

Fundorte	Zahl der Streifen						Summe
	0	*1*	*2*	*3*	*4*	*5*	
I *a*	339	14	5	86	5	39	488 } 1297
j	569	25	35	141	9	30	809
II *a*	138	37	3	100	1	17	296 } 545
j	133	28	14	53	6	15	249
III *a*	127	54	2	58	7	211	459 } 930
j	171	39	1	82	6	172	471
IV *a*	228	100	35	263	7	31	664 } 1505
j	382	150	81	192	3	33	841
V *a*	136	72	19	120	4	20	371 } 864
j	213	74	39	146	3	18	493
Summe *a*	968	277	64	627	24	318	2278
j	1468	316	170	614	27	268	2863
Total	2436	593	234	1241	51	586	5141

a = ausgewachsene, *j* = junge Thiere.

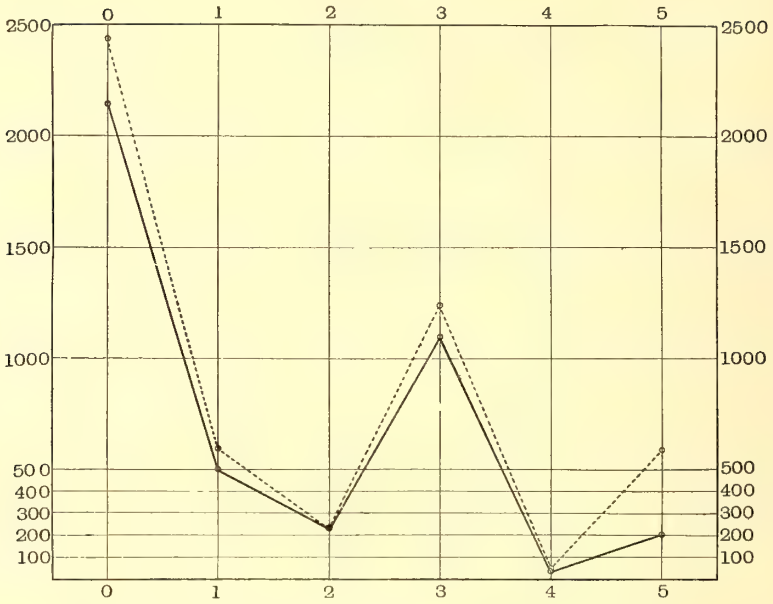
TABELLE A, II.

Vergleichswerthe der Fundmenge für (*a*) 100 *ausgewachsene und* (*j*) 100 *junge Thiere von jedem Fundort berechnet.*

Fundorte	Zahl der Streifen					
	0	*1*	*2*	*3*	*4*	*5*
I *a*	69·47	2·87	1·02	17·62	1·02	8·00
j	70·33	3·09	4·33	17·43	1·11	3·71
II *a*	46·62	12·50	1·01	33·78	0·34	5·74
j	53·41	11·24	5·62	21·29	2·42	6·02
III *a*	27·67	11·76	0·44	12·64	1·53	45·97
j	36·31	8·28	0·21	17·41	1·27	36·52
IV *a*	34·34	15·06	5·27	39·61	1·05	4·67
j	45·42	17·84	9·63	22·83	0·36	3·92
V *a*	36·66	19·41	5·12	32·34	1·08	5·39
j	43·20	15·01	7·91	29·61	0·62	3·65

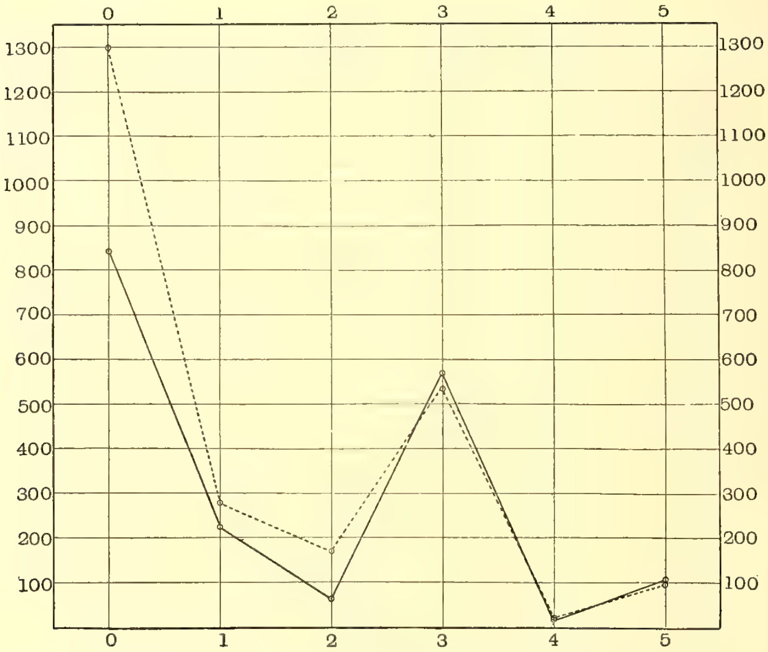
a = ausgewachsene, *j* = junge Thiere.

Zunahme an streifenlosen Exemplaren aufwiesen. Es ist daher auch auf diesen Umstand insofern mit Rücksicht genommen, als die Vergleichung und Zusammenfassung der Ergebnisse der letzteren Fundorte, der unter den gleichen Lebensbedingungen sich entwickelnden Thiere, auch in den gesonderten Curven auf Fig. 1 u. 2 (sowie später noch Fig. 3 *a* u. *b*) zum Ausdruck gelangt ist und, wie die Zahlen zeigen, auch nicht unberücksichtigt bleiben darf.



Funde 1—5 zusammen 5141. Funde 1, 2, 4 u. 5 zusammen 4211.

Fig. 1. Funde 1—5 addirt (punktirte Linie); Funde 1, 2, 4 u. 5 addirt (continuirliche Linie).



Ausgewachsene Thiere 1819. Junge Thiere 2392. Summe 4211.

Fig. 2. Funde 1, 2, 4 u. 5, die gleichen Lebensbedingungen entsprechen.

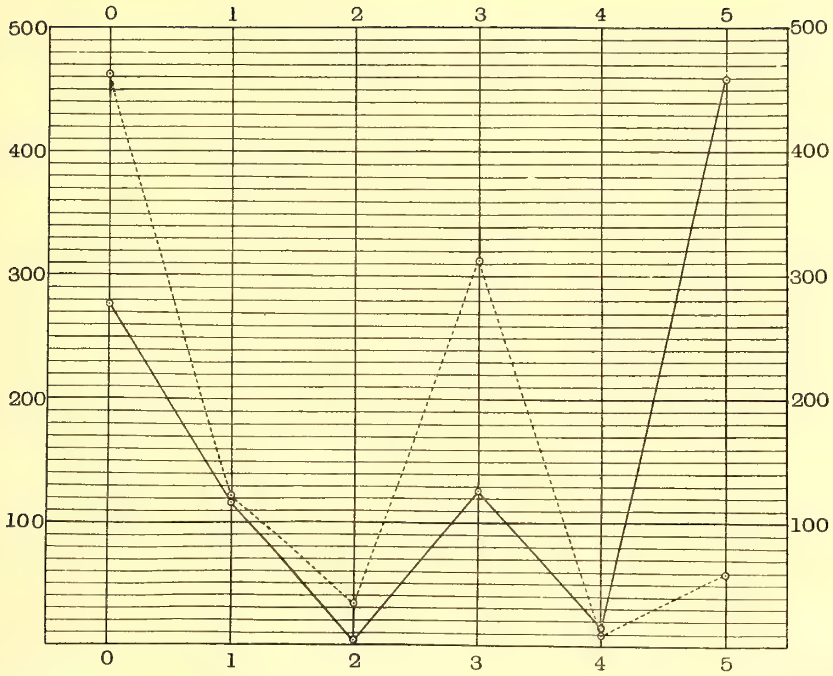


Fig. 3. Berechnetes Vorkommen der Streifen bei je 1000 ausgewachsenen Thieren (*a*) von den Fundorten 1, 2, 4 und 5 (punktirte Linie) und (*b*) vom Fundort 3 (continuirliche Linie).

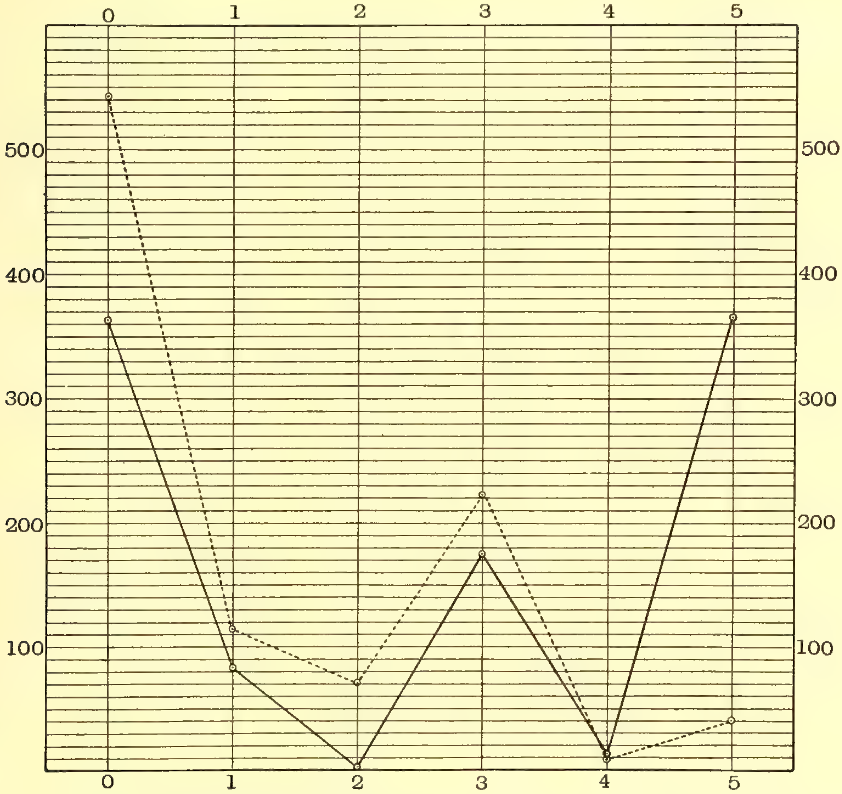


Fig. 4. Berechnetes Vorkommen der Streifen bei je 1000 jungen Thieren (*a*) von den Fundorten 1, 2, 4 und 5 (punktirte Linie) und (*b*) vom Fundort 3 (continuirliche Linie).

TABELLE B.

Wiedergabe sämmtlicher möglicher und beobachteter Varietäten aller fünf Fundorte.

(*a*=ausgewachsene, *j*=junge Thiere.)

	Lage der Streifen resp. Bänder	Fundorte										Summe			
		I		II		III		IV		V					
		a	*j*	*a*	*j*	*a*	*j*	*a*	*j*	*a*	*j*	*a*	*j*	*a+j*	
	0	339	569	138	133	127	171	228	382	136	213	968	1468	2436	
Gebiet von einem Streifen	*1 . . . .*	—	—	—	—	—	—	—	—	—	—	—	—	—	Ein Streifen 593
	. 2 . . .	—	2	—	—	—	—	—	—	—	—	—	2	2	
	. . 3 . .	14	23	37	28	54	39	100	150	72	74	277	314	591	
	. . . 4 .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
Gebiet von zwei Streifen	*1 2 . . .*	—	2	1	2	—	1	—	1	1	1	2	7	9	Zwei Streifen 234
	(1 2) . . .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 . 3 . .	4	33	2	12	2	—	35	79	17	38	60	162	222	
	1 . . 4 .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 . . . 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. 2 3 . .	1	—	—	—	—	—	—	1	—	—	1	1	2	
	. (2 3) . .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. 2 . 4 .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. 2 . . 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. . 3 4 .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. . (3 4) .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. . 3 . 5	—	—	—	—	—	—	—	—	1	—	1	0	1	
	. . . 4 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. . . (4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
Gebiet von drei Streifen	*1 2 3 . .*	80	140	93	53	52	80	207	181	102	135	534	589	1123	Drei Streifen 1241
	(1 2) 3 . .	6	1	6	—	6	2	56	11	18	10	92	24	116	
	1 (2 3) . .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	(1 2 3) . .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 2 . 4 .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	(1 2) . 4 .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 2 . . 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	(1 2) . . 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 . 3 4 .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 . (3 4) .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 . 3 . 5	—	—	1	—	—	—	—	—	—	—	1	—	1	
	1 . . 4 5	—	—	—	—	—	—	—	—	—	1	—	1	1	
	1 . . (4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. 2 3 4 .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. (2 3) 4 .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. 2 (3 4) .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. (2 3 4) .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. 2 3 . 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. (2 3) . 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. 2 . 4 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. 2 . (4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. . 3 4 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. . (3 4) 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. . 3 (4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. . (3 4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	

TABELLE B—*Fortsetzung.*

	Lage der Streifen resp. Bänder	Fundorte										Summe			
		I		II		III		IV		V					
		a	*j*	*a*	*j*	*a*	*j*	*a*	*j*	*a*	*j*	*a*	*j*	*a*+*j*	
Gebiet von vier Streifen	*1 2 3 4 .*	5	9	1	2	3	3	—	1	—	—	9	15	24	Vier Streifen 51
	(1 2) 3 4 .	—	—	—	—	—	—	—	—	1	—	1	—	1	
	1 (2 3) 4 .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 2 (3 4).	—	—	—	—	—	—	—	—	—	—	—	—	—	
	(1 2 3) 4 .	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 (2 3 4).	—	—	—	—	—	—	—	—	—	—	—	—	—	
	(1 2)(3 4).	—	—	—	—	—	—	—	—	—	—	—	—	—	
	(1 2 3 4).	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 2 3 . 5	—	—	—	4	1	2	7	2	2	3	10	11	21	
	(1 2) 3 . 5	—	—	—	—	—	—	—	—	1	—	1	—	1	
	1 (2 3) . 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	(1 2 3) . 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 2 . 4 5	—	—	—	—	3	1	—	—	—	—	3	1	4	
	(1 2) . 4 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 2 . (4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
	(1 2) . (4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 . 3 4 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 . (3 4) 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 . 3 (4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 . (3 4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. 2 3 4 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. (2 3) 4 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. 2 (3 4) 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. 2 3 (4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. (2 3 4) 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. 2 (3 4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. (2 3)(4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
	. (2 3 4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
Gebiet von fünf Streifen	*1 2 3 4 5*	35	30	16	14	174	159	31	33	19	17	275	253	528	Fünf Streifen 586
	(1 2) 3 4 5	4	—	1	1	17	3	—	—	1	1	23	5	28	
	1 (2 3) 4 5	—	—	—	—	1	—	—	—	—	—	1	—	1	
	1 2 (3 4) 5	—	—	—	—	4	6	—	—	—	—	4	6	10	
	1 2 3 (4 5)	—	—	—	—	12	1	—	—	—	—	12	1	13	
	(1 2)(3 4) 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	(1 2) 3 (4 5)	—	—	—	—	3	1	—	—	—	—	3	1	4	
	1 (2 3)(4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
	(1 2 3) 4 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 (2 3 4) 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 2 (3 4 5)	—	—	—	—	—	2	—	—	—	—	—	2	2	
	(1 2 3 4) 5	—	—	—	—	—	—	—	—	—	—	—	—	—	
	1 (2 3 4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
	(1 2 3)(4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
	(1 2)(3 4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
	(1 2 3 4 5)	—	—	—	—	—	—	—	—	—	—	—	—	—	
	Summe	488	809	296	249	459	471	664	841	371	493	2278	2863	5141	

Gestatten die an den einzelnen Fundorten gesammelten Thiere eine Vergleichung der Streifenformen in ihren Vertheilungsverhältnissen zu einander innerhalb eines jeden Fundgebietes, so ist bei einer Vergleichung verschiedener Fundorte stets auch zu berücksichtigen, dass hier die einzelnen Fundmengen wesentlich eine Vergleichung der Streifenformen in den Zahlenwerthen und somit auch in den hiernach construirten Curven beeinflussen können. Um daher die Werthe der verschiedenen Fundorte vergleichsfähig zu machen, ist eine Umrechnung aller Werthe auf eine für alle Funde gemeinsame gleiche Gesammtmenge S erforderlich.

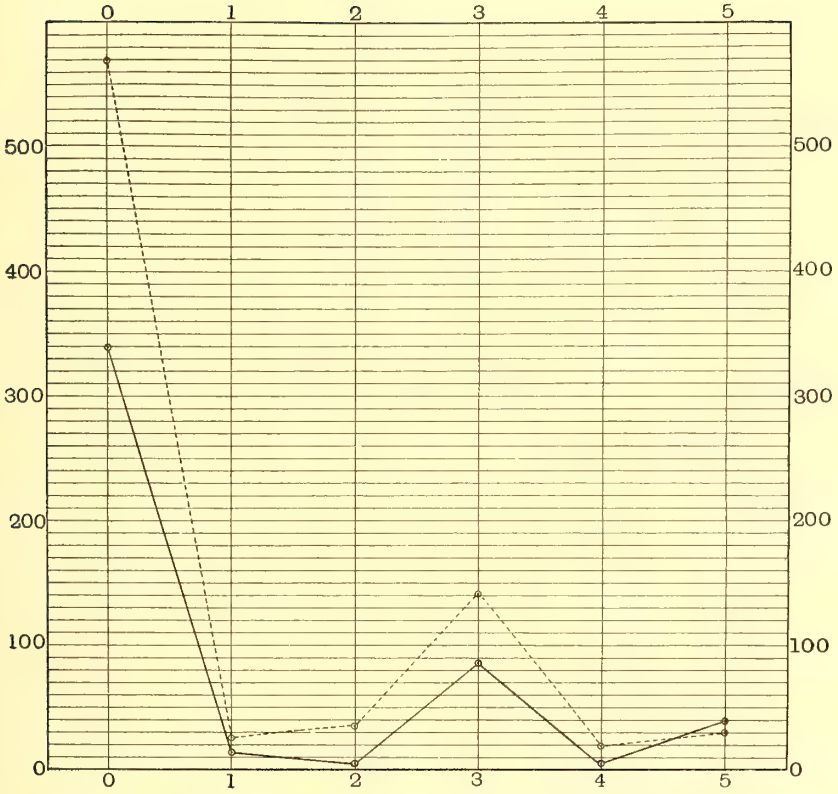
Bei solcher Umrechnung müssen die ausgewachsenen und die jungen Thiere natürlich getrennt betrachtet werden; in Tabelle A, II sind für $S = 100$ ausgewachsene resp. junge Individuen von jedem Fundort die Vergleichswerthe zusammengestellt.

Es ist deutlich ersichtlich wie bei den zusammengefassten Werthen von I, II, IV u. V die Annäherung der Werthe aneinander beträchtlich zugenommen hat. Ich habe es für wichtig gehalten hier auf die verschiedenen Arten der Vergleichung und die verschiedene Interpretation solcher Sammelwerthe besonders aufmerksam zu machen, weil sie manches lehrreiche für künftige Arbeiten in dieser Richtung enthalten dürften. Hier haben wir es bei den Pulmonaten in Folge der geringen Locomotion mit einem Untersuchungsmaterial zu thun, von dem sich auf verhältnissmässig kleinem Raum (also bei gleichen Lebensbedingungen) leicht grössere Mengen bei einiger Sorgfalt sammeln lassen, da das einmal gefundene Object sich nicht durch die Flucht zu entziehen vermag, wodurch das Werthverhältniss der Funde unter einander für die späteren vergleichenden Untersuchungen gesteigert wird.

Ist also einerseits eine gewisse Vorsicht geboten Schlussfolgerungen auf Grund der Vergleichungen der Spielarten innerhalb eines Fundortes sowie auf Grund der Werthe verschiedener Fundorte zu ziehen, so können sich andererseits auch wieder eine Reihe Regelmässigkeiten ergeben, die eine gemeinschaftliche Vergleichung erlauben und sich gewissermassen gegenseitig controlliren. Ersichtlich ist aber auch (wie die Wahrscheinlichkeit der Schlussfolgerungen ja allgemein mit der Menge der Funde zunimmt), dass immerhin ein hinreichendes Material vorhanden sein muss.

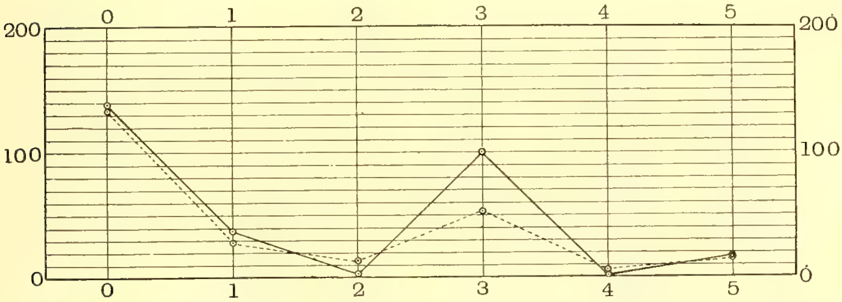
Ich hoffe noch Gelegenheit zu haben in einer späteren Mittheilung über andere Funde gerade diesen Punkt und dessen Einfluss auf die Menge der gefundenen sogenannten seltenen Varietäten näher beleuchten zu können.

Aus den hier vorliegenden Untersuchungen ergiebt sich jedenfalls aber auch, wie diese eine schätzenswerthe Unterlage für Züchtungen bilden können und Fragen hervorrufen, welche Biologie und vergleichende Anatomie zusammen zu beantworten haben werden und wie derartige Sammlungen bei Berücksichtigung aller gegebenen Bedingungen an den Fundorten, aus ihren Zahlenwerthen Hinweise ergeben können, nach welcher Richtung hin weitere Untersuchungen besonders wünschenswerth erscheinen.



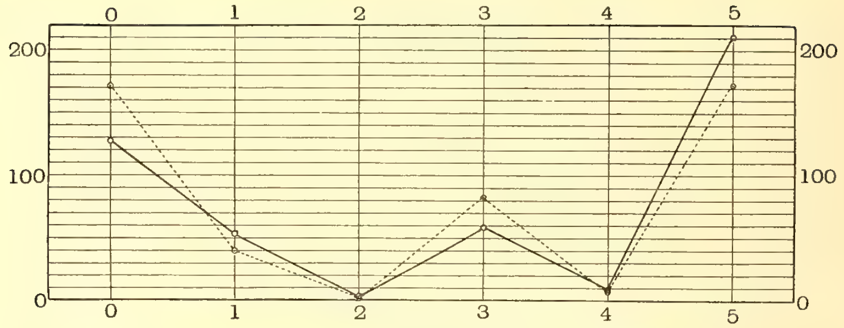
Ausgewachsene Thiere 488. Junge Thiere 809. Summe 1297.

Fig. 5. Fundort I. Werk 59. Siehe Karte S. 475. Junge Thiere - - - - - Ausgewachsene ———



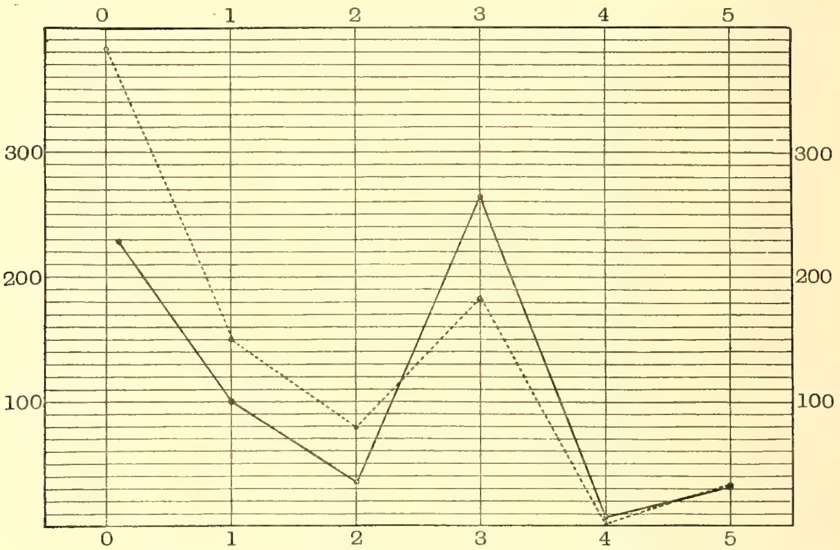
Ausgewachsene Thiere 296. Junge Thiere 249. Summe 545.

Fig. 6. Fundorte II. Werke 61 u. 62. Siehe Karte S. 475. Junge Thiere - - - - - Ausgewachsene ———



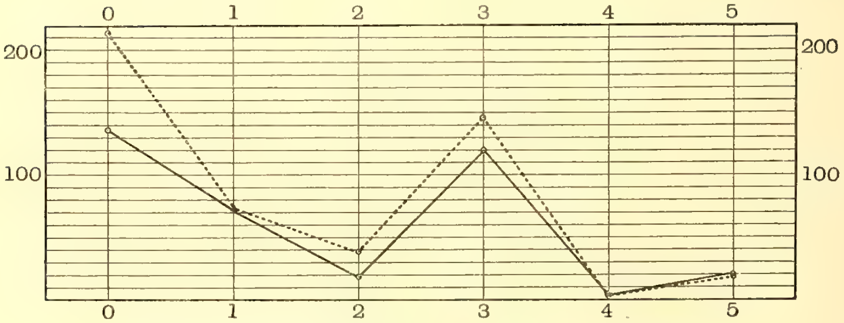
Ausgewachsene Thiere 459. Junge Thiere 471. Summe 930.

FIG. 7. Fundort III. Garten mit Gelände. Siehe Karte S. 475. Junge Thiere - - - - - Ausgewachsene ———



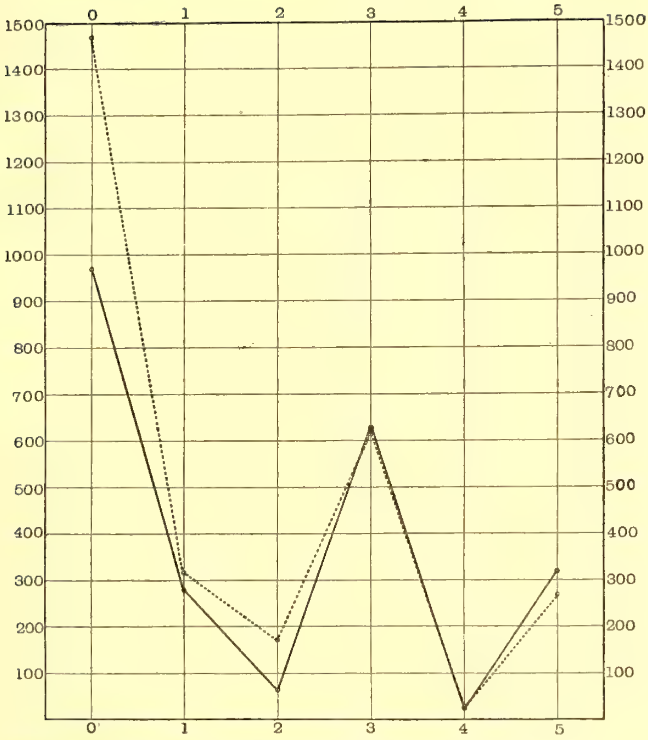
Ausgewachsene Thiere 664. Junge Thiere 841. Summe 1505.

FIG. 8. Fundorte IV. Siehe Karte S. 475. Werke 63, 64 u. 68.



Ausgewachsene Thiere 371. Junge Thiere 493. Summe 864.

FIG. 9. Fundorte V. Siehe Karte S. 475. Werke 65, 66 u. 67.



Ausgewachsene Thiere 2278. Junge Thiere 2863. Summe 5141.

Fig. 10. Funde von I bis V zusammen.

Wir haben bisher, die allgemeine Vertheilung der Varietäten nach der Anzahl betheiligter Streifen untersucht und darauf die Vergleichswerthe basirt. Welche Gebiete der Farbdrüsen dabei dominiren ist daraus nicht scharf ersichtlich. Bei diesen Sammlungen wurde aber zugleich Rücksicht genommen auf die gesonderte Zählung sämmtlicher vorkommenden Varietäten und dabei zugleich die Vollkommenheit der Entwicklung der Streifung mit in den Kreis der Beobachtung eingeschlossen. Diese Beobachtungen sind hier in der nachfolgenden Tabelle B, der eigentlichen Haupttabelle, zusammengestellt.

Die Zahlen dieser Tabelle zeigen einen ganz besonders interessanten und characteristischen Verlauf. Auch hier kommen, wie in den früheren Zahlen der Tabelle A, und in den Curven der Fig. 1—10, die beobachteten Maxima deutlich wieder zum Ausdruck in den betreffenden Streifengebieten, wir sehen aber zugleich, wie an den Gesammtmengen verhältnissmässig nur wenig Formen (von den 89 theoretisch möglichen) sich betheiligen, und wie die Formen, aus dem Gebiete der drei Streifen 1, 2, 3 sowie im Gebiete der fünf Streifen diese einzige mit einigen Nebenformen *das ganze* Gebiet der Variationen beherrschen, so dass jetzt schon gewisse Fragen und Erklärungen herausgefordert werden, welche spätere experimentelle Untersuchungen auf Werth und Berechtigung zu prüfen hätten. Hierbei dürfen wir aber das Folgende nicht ausser Acht lassen.

Bei der Untersuchung der Varietäten ist von der bekannten Bezeichnung der Reihenfolge der Streifen ausgegangen und hierauf sind die Berechnungen basirt, da von einer bestimmten Ordnung ja doch auszugehen ist. Auch für künftige Untersuchungen wird es sich empfehlen dies beizubehalten, auch schon mit Rücksicht auf die frühere Litteratur hierüber. Indessen muss bei allen derartigen Arbeiten niemals vergessen werden, dass hier lediglich ein Hülfsmittel für die Untersuchung vorliegt *und der Weg den die Natur zur Bildung und Vervollkommenung von Varietäten einschlägt nicht der gleiche zu sein braucht,* d. h. der Weg vom Einfachen zum Vollkommenen ja nicht an die Reihenfolge 1, 2, 3... u. s. w. gebunden ist.

Dies vorausgeschickt, wird auch die Bedeutung der Generaltabelle B klarer ersichtlich werden, wenn wir die Betheiligung der einzelnen Streifen in den beobachteten Varietäten feststellen, was in der nachfolgenden Tabelle C, I, zum Ausdruck kommt, aus welcher die Betheiligung der Zahl nach ersichtlich ist, und die Reihenfolge der Streifen resp. der Lagegebiete der Farbdrüsen ergiebt:

Streifen	3.	1.	2.	4.	5.
Vorkommen	2699	2119	1899	626	625

Da selbstverständlich ein Streifengebiet mehreren Varietäten gemeinschaftlich ist, so haben wir auch zugleich das Vorkommen der einzelnen Varietäten nach Zahl zu prüfen und erhalten dann die Werthe der Tabelle C, II, in welcher einmal I, II, IV u. V zusammengefasst sind (nach früherer Begründung) und auch alle Funde I—V zusammen.

TABELLE C, I.

Betheiligung der einzelnen Streifen.

Beobachtete Varietäten	Keine Farbenausscheidung	Streif 1	Streif 2	Streif 3	Streif 4	Streif 5
0	2436	—	—	—	—	—
. 2 . . .	—	—	2	—	—	—
. . 3 . .	—	—	—	591	—	—
1 2 . . .	—	9	9	—	—	—
1 . 3 . .	—	222	—	222	—	—
. 2 3 . .	—	—	2	2	—	—
. . 3 . 5	—	—	—	1	—	1
1 2 3 . .	—	1123	1123	1123	—	—
1 . 3 . 5	—	1	—	1	—	1
1 . . 4 5	—	1	—	—	1	1
(1 2) 3 . .	—	116	116	116	—	—
1 2 3 4 .	—	24	24	24	24	—
1 2 3 . 5	—	21	21	21	—	21
1 2 . 4 5	—	4	4	—	4	4
(1 2) 3 4 .	—	1	1	1	1	—
(1 2) 3 . 5	—	1	1	1	—	1
1 2 3 4 5	—	528	528	528	528	528
(1 2) 3 4 5	—	28	28	28	28	28
1 (2 3) 4 5	—	1	1	1	1	1
1 2 (3 4) 5	—	10	10	10	10	10
1 2 3 (4 5)	—	13	13	13	13	13
(1 2) 3 (4 5)	—	14	14	14	14	14
1 2 (3 4 5)	—	2	2	2	2	2
Summe	2436	2119	1899	2699	626	625

TABELLE C, II.

Betheiligung der Varietätenformen.

Beobachtete Formen	Funde von I, II, IV, V alt u. jung zusammen		Funde von III alt u. jung zusammen		Funde I—V zusammen
0	2138		298		2436
. 2 . . .	2		—		2
. . 3 . .	498		93		591
1 2 . . .	8		1		9
1 . 3 . .	220		2		222
. 2 3 . .	2		—		2
. . 3 . 5	1		—		1
1 2 3 . .	991	1099	132	140	1239
(1 2) 3 . .	108		8		
1 . 3 . 5	1		—		1
1 . . 4 5	1		—		1
1 2 3 4 .	18	19	6	6	25
(1 2) 3 4 .	1		—		
1 2 3 . 5	18	19	3	3	22
(1 2) 3 . 5	1		—		
1 2 . 4 5	—		4		4
1 2 3 4 5	195	203	333	383	586
(1 2) 3 4 5	8		20		
1 (2 3) 4 5	—		1		
1 2 (3 4) 5	—		10		
1 2 3 (4 5)	—		13		
(1 2) 3 (4 5)	—		4		
1 2 (3 4 5)	—		2		

Aus diesen Zusammenstellungen ergiebt sich, wenn wir die in der Hauptsache in Frage kommenden 5 Spielarten (einschliesslich ihrer Verschmelzungsformen) vergleichen, das folgende proportionale Verhältniss:

	a.—Für die aus I, II, IV u. V zusammengezogenen Werthe		*b.*—Für die aus I—V zusammengezogenen Werthe	
Streifenform	Beobachete Menge	Proportionales Verhältniss	Beobachtete Menge	Proportionales Verhältniss
0	2138	10·53	2436	10·97
. . 3 . .	341	1·68	591	2·66
1 . 3 . .	220	1·08	222	1·00
1 2 3 . .	1099	4·09	1239	5·58
1 2 3 4 5	203	1·00	586	2·63

und die Reihenfolge nach Fundmengen stellt sich hiernach in das Verhältniss

Für *a*		Für *b*	
Form 0	—— 10·53	Form 0	—— 10·97
1 2 3 . .	—— 4·09	1 2 3 . .	—— 5·58
. . 3 . .	—— 1·68	. . 3 . .	—— 2·66
1 . 3 . .	—— 1·08	1 2 3 4 5	—— 2·63
1 2 3 4 5	—— 1·00	1 . 3 . .	—— 1·00

Durch derartige Eingrenzung lässt sich feststellen wo die Ausgangspunkte der Varietätenbildung zu suchen sind und welcher Werth gewissen Streifengebieten in Bezug auf die Bildung der nächstwerwandten Varietäten zukommt. Die einfache Beobachtung einer sogenannten seltenen Varietät beweist noch nicht ob man es mit einem seltenen Zwischenglied zwischen den anschliessenden Formen, mit einem Rückschlag, oder mit einer Neubildung zu thun hat. Nach den Bewerthungen der Fundresultate, wie sie hier in verschiedener Richtung durchgeführt wurden ergiebt sich, dass das Ausgangsgebiet aller Streifenbildungen im Gebiete des Streifens 3 zu suchen ist. Nach dieser Richtung hin sind spätere weitere Sammlungen mit zu untersuchen und zwar wird dann dieses Streifengebiet auch bei Sammlungen der übrigen Arten der Stylommatophoren vorherrschen müssen.

In den über 5000 Exemplare umfassenden Sammlungen nach der Gesammtübersicht (vide Tabelle B) haben sich nur die folgenden Spielarten gefunden:

1. *Streifenformen*

0	1 2 . . .	1 2 3 . .	1 2 3 4 .	1 2 3 4 5
. 2 . . .	1 . 3 . .	1 . 3 . 5	1 2 3 . 5	
. . 3 . .	. 2 3 . .	1 . . 4 5	1 2 . 4 5	
	. . 3 . 5			

Summa 14.

2. *Bänderformen keine.*

3. *Streifenformen mit Bänderformen*

(1 2) 3 . .	(1 2) 3 4 .	(1 2) 3 4 5
	(1 2) 3 . 5	1 (2 3) 4 5
		1 2 (3 4) 5
		1 2 3 (4 5)
		(1 2) 3 (4 5)
		1 2 (3 4 5)

Summa 9.

Oder total, einschliesslich der streifenlosen Form, 23.

Nach brieflicher Mittheilung wurden früher von A. Schmidt* während seiner langjährigen Untersuchungen die folgenden Spielarten beobachtet:

An Streifenformen 22

0	1 2 . . .	1 2 3 . .	1 2 3 4 .	1 2 3 4 5
1	1 . . . 5	1 2 . 4 .	1 2 3 . 5	
. 2 . . .	. 2 3 . .	1 2 . . 5	1 2 . 4 5	
. . 3 . .	. . 3 4 .	1 . 3 4 .	1 . 3 4 5	
. . . 4 .		1 . 3 . 5		
. . . . 5	. . 3 . 5	. 2 3 4 .		

---

* Schreiben von 25. xi. 78. Vide auch: Ad. Schmidt, *Beiträge zur Malakozoologie*, Berlin 1857; sowie Hall, *Zeitschr. der gesammten Naturwissensch.* Bd. viii.

An Bänderformen 9

(1 2) . . .	(1 2 3) . .	(1 2) (3 4) .	(1 2) (3 4 5)
. (2 3) . .		(1 2 3 4) .	(1 2 3) (4 5)
. . (3 4) .		(1 2) . (4 5)	

An Streifen mit Bänderformen 17

(1 2) 3 . .	1 2 (3 4) .	(1 2) 3 4 5	(1 2 3) 4 5	(1 2 3 4) 5
(1 2) . 4 .	(1 2) 3 4 .	1 (2 3) 4 5	1 (2 3 4) 5	1 (2 3 4 5)
. 2 (3 4) .	(1 2) 3 . 5	1 2 (3 4) 5	1 2 (3 4 5)	
. . 3 (4 5)	(1 2) . 4 5	1 2 3 (4 5)		

Zugleich erwähnte Schmidt den Fund von einem 6-streifigen Bande. Letzterer Fund ist sicher auf eine Theilung einer der fünf Streifen zurückzuführen. Jedenfalls wird es gewagt sein alle derartige Spaltungen als neue Varietäten anzusprechen. Wir wissen das bei *H. pisana* diese Spaltung der Streifen ganz allgemein vorkommt*. Ehe hier nicht über die Lage der Drüsen und deren anatomischen Bau näheres vorliegt, sollte man um Verwirrungen vorzubeugen sich zur Zeit auf die 5 Lagegebiete beschränken.

Neben den seltenen Zwischenformen zwischen den Gruppen der Hauptformen der Sammlungen ist auch das unvollkommene Auftreten der Eigenschaften der Varietäten nicht unberücksichtigt zu lassen, denn hier müssen bei grösseren Sammlungen sowie bei der Vergleichung des Materials verschiedener Sammlungen an gleichem Fundorte aber aus verschiedenen Zeitperioden, sowie beim Vergleichen der Funde auseinanderliegender Fundorte aus der Vertheilung und Art dieser unvollkommenen Varietätenformen gewisse Schlussfolgerungen möglich werden inwieweit wir es dabei mit einem Vorwärtsschreiten oder einem Rückschritt in der Varietätenbildung zu thun haben. Die Zusammenstellung der Tabelle D enthält hier alle derartigen bei sämmtlichen fünf Fundorten beobachteten unvollkommen ausgebildeten Formen. Da hier eine Anzahl Streifenformen erst in der letzten Windung sowie am Saum auftreten, wird auch hier das Verhältniss bei ausgewachsenen und jungen Thieren schon ein ungleiches sein müssen und ein Urtheil darüber, wie sich die jungen Thiere weiter entwickelt haben würden ist natürlich vorläufig ausgeschlossen. Auch hier kann der Züchtungsversuch manches Resultat erzielen, das in der freien Natur schwerlich zu erhalten ist. Zugleich ist in der Zusammenstellung weiter noch angegeben, welche Varietäten eine *rothe* Grundfarbe des Gehäuses besassen (alle übrigen variirten von hellgelb bis tief citronengelb).

Auch bei diesen beobachteten unvollkommenen Formen sehen wir, wie die Fundmengen auch bei den Hauptformen ein Maximum zeigen. Welche Deutung ist hier nun zulässig? Sind diese unvollkommenen Formen ein Beweis für das Vorwärtsschreiten in der Bildung vollkommener Streifenformen bei 1.2.3 und in der Richtung nach 1.2.3.4.5, oder sind hier Rückgänge, Verkümmerungen

---

* Auch *H. pomatia* zeigt ja vielfach eine Auflösung der breiten Streifen in eine oft grosse Anzahl feiner Linien.

TABELLE D, I. *Beobachtete Varietäten der fünf Sammlungen die unvollkommen entwickelte Streifenformen aufwiesen und deren Vertheilung auf die fünf Fundorte.*

	. 2 . . .	*. . 3 . .*	*1 2 . . .*	*1 . 3 . .*	*. 2 3 . .*	*. . 3 . 5*	*1 2 3 . .*	*(1 2) 3*	*1 . . 4 5*	*1 2 3 4 .*	*(1 2) 3 4 .*	*1 2 3 . 5*	*1 2 3 4 5*	Vertheilung auf die einzelne Fundorte
I *a*					*2* schw. 1		kleiner Streif am Saum ... ... 4 l. W. ... ... 5 ½ l. W. ... ... 8 nur l. W. ... 13 *1* nur in f. L. am Saum ... ... 1 *1* in *2* f. L. ... 1			*1* schw. 1 *3* schw. 3			*4* schw. 8 *5* schw. 5 *4* u. *5* schw. 3	53
I *j*	in *2* f. L. 1	schw. 23	schw. 1	*1* schw. 12 *1* u. *3* schw. 1			sehr wache angedeutet ... 8 nur kleine Striche am Saum ... 13 ¼ l. W. ... 24 l. W. ... ... 25 mehr als eine W. 20 schw. ... ... 1 *2* u. *3* schw. ... 1			*4* schw. 6 *3* u. *4* schw. 1			*4* schw. 7 *5* schw. 1	145
II *a*			*2* schw. 1	*1* schw. 2			*1, 2* schw. ... 1 *1, 2, 3* schw. ... 63 *3* schw. ... ... 2 *2, 3* schw. ... 1 *1, 2, 3* in f. L. 1						*4* schw. 4 *5* schw. 2 *4, 5* schw. 2	79
II *j*			*2* schw. 2				*2* schw. ... ... 36 *3* schw. ... ... 1 *1, 2, 3* schw. ... 1			*3, 4* schw. 1		*3* u. *5* zz. 1	*4* schw. 3 *5* schw. 1 *4* u. *5* schw. 3	49
III *a*				*3* schw. 1			*2* schw. ... ... 5 *1* u. *2* schw. ... 2 *1, 2,* u. *3* schw. 1			*4* schw. 2 *1, 2, 4* schw. 1		*1–5* schw. 1	*4* schw. 2 *5* schw. 2 *4, 5* schw. 7 alle schw. 5	29
III *j*							*2* schw. ... ... 34 *3* schw. ... ... 2 *1, 2* schw. ... 7			*1, 2, 4* schw. 2 *4* schw. 1			*3* schw. 1 *4* schw. 5 *5* schw. 6 *4, 5* schw. 13 *2, 3, 4, 5* schw. 3 alle schw. 5	79
IV *a*				*3* schw. 5			*1* schw. ... ... 1 *2* schw. ... ... 5 *3* schw. ... ... 1 *1, 2, 3* schw. ... 1	*1* schw. 2 *3* schw. 1				*5* schw. 1	*4* schw. 4 *5* schw. 2 *4, 5* schw. 5	31
IV *j*				*1* schw. 1 *1, 3* schw. 1			*2* schw. ... ... 1 *1, 2* schw. ... 3 *2, 3* schw. ... 3	*1, 2* schw. 3				*5* schw. 2	*4* schw. 3 *4, 5* schw. 4	21
V *a*			*2* schw. 1	*3* schw. 1		*5* schw. 1	*1, 2* schw. ... 1 *2* schw. ... ... 5 *3* schw. 1	*1, 2* schw. (l. W.) 2			*3, 4* schw. 1	*1, 2, 3, 5* schw. } 1	*4* schw. 3 *5* schw. 6 *4, 5* schw. 5	29
V *j*			*1, 2* schw. 1	*1* schw. 11 *1* schw. a. l. W. 1			*1, 2* schw. ... 5 *2* schw. ... ... 15 davon *2* in *2* Streifen getheilt *3* schw. 1	*1, 2* schw. 1	*5* schw. in *3* f. L. 1			*5* schw. 3	*4* schw. 4 *5* schw. 2 *4, 5* schw. 4	49
Summe	1	23	6	36	1	1	325	9	1	18	1	9	133	564

f. L. = feinen Linien; l. = letzte; W. = Windung; zz. = zweizeilig; schw. = schwach.

in den Farbdrüsen die Ursache? Für beide Deutungen liessen sich vielleicht plausible Erklärungen erbringen, beweiskräftig für das eine oder andere werden nur zahlreiche Sammlungen und die experimentelle Untersuchung der Bedingungen der Varietätenbildung sein können.

Bei vorstehenden Betrachtungen der Fundergebnisse mag vielleicht noch mancherlei unberücksichtigt geblieben sein, was sowohl bei der Sammlung der Varietäten wie der nachherigen Untersuchung der Beachtung werth erscheinen möchte. Hier kann, nachdem in der "Biometrika" ein Organ geschaffen ist, welches an geeigneter Stelle einen Austausch von Ansichten und Wünschen gestattet, solches nur fördernd für die biologische Forschung sein und wird dies dann späteren Untersuchungen zu Gute kommen. Es werden sich dann aus dem zusammengetragenen Material Normen ausarbeiten und Grundsätze festlegen lassen, welche die wichtigsten Untersuchungsbedingungen und Hinweise enthalten die bei Sammlungen und der Verarbeitung des Materials im Auge zu behalten sind. Was z. B. für die Flora in Bezug auf den Einfluss solarer Wärme, der Feuchtigkeit und Insolation Gültigkeit besitzt und bei der Deutung des eigenartigen Verlaufes der Vegetationslinien* nicht vernachlässigt werden darf, muss

TABELLE D, II.

Verhältniss zwischen ausgebildeten und unvollkommenen Varietätenformen.

	I	II	III	IV	V
Gesammtfunde	1297	545	930	1505	864
Darunter streifenlose Formen ...	908	271	298	610	349
Bleiben Streifenformen	389	274	632	895	515
Darunter unvollkommen ausgebildet	198	128	108	52	78
Bleiben wirklich ausgebildete ...	191	146	524	843	437

[Auch hier müsste für genauere Vergleichsverhältnisse natürlich Umrechnung, auf gleiche Fundmenge basirt, stattfinden.

Betreffs der Grundfarbe der Gehäuse ist zu bemerken dass rothe Grundfarben gefunden wurden bei

	I	II	III	IV	V
a	7	10	8	21	11
j	14	16	19	15	12

alle übrigen zeigten hellgelbe bis intensiv citronengelbe Farbe.]

---

* 1. Drude, O., *Die Anwendung physiologischer Gesetze zur Erklärung der Vegetationslinien*, Göttingen 1876. Siehe auch die daselbst citirten Quellenschriften.

2. Tschaplowitz, F., *Untersuchungen üb. d. Einwirkung der Wärme u. d. anderen Formen d. Naturkräfte auf die Vegetationserscheinungen.* 1882.

auch für die Fauna der Pulmonaten und deren Entwicklung zu Recht bestehen und neben anderen Existenzbedingungen auch das Variiren nach verschiedener Richtung hin beeinflussen. Hierauf ist namentlich dann bei der Vergleichung der Funde an verschiedenen auseinanderliegenden Fundorten Rücksicht mit zu nehmen.

Die Möglichkeit der Vergleichung der einzelnen Thierklassen und Arten aus verschiedenen weit auseinanderliegenden Zeiträumen ist selbstverständlich eine recht beschränkte, trotz der zahlreichen (auch fossilen) Funde die fortdauernd gemacht werden, wir würden sonst auf die Fragen über das Variiren und gewisse Constanz von Variationen bestimmtere Antworten geben können. Bilden gerade hier die Weichthiere eine besonders wichtige Klasse, wodurch dieselben sich als Leitfossilien ganz unentbehrlich für die Geologie machen, so ist in Bezug auf Abstammung und Entwicklung recht wenig zu erfahren, trotz der Massenfunde, weil wir es hier nur mit der Umhüllung, in den meisten Fällen sogar nur mit Pseudomorphosen derselben zu thun haben. Wenn daher Fleischmann* selbst die Stelle citiren kann: "Andererseits erhalten wir aber durch das genannte Studium dieser Schalenreste nicht den mindesten Aufschluss über die specielle innere Organisation der Thiere, welche diese äusseren Cuticular-schalen absonderten"† so ist dieser Thatsache ihre Berechtigung nicht abzusprechen.

Wenn wir uns aber bescheiden und nur so weit zurückgreifen, als uns fossile Funde bewahrt sind, welche noch die Zeichnung auf der Schale erkennen lassen, d. h. noch wirkliche Schalenreste der Untersuchung uns zugänglich machen, so müssen sich auch aus der Bestimmung und Variabilität dieser Zeichnungen Schlussfolgerungen in Bezug auf die derzeitige Entwicklung der Farbdrüsen machen lassen, und hier lassen sich doch schon recht beträchtliche Zeiträume zurückverfolgen.

Ich hoffe noch Gelegenheit zu haben über diesbezügliche Funde aus dem Mainzer Becken namentlich aus dem in geologischer Beziehung so interessanten Mosbacher Sande bei Biebrich berichten zu können.

---

* *Die Descendenztheorie*, Leipzig 1901.

† Häckel, E., *Systematische Phylogenie*, Bd. II. 1896.

Auf die in dieser Mittheilung nur kurz angedeutete Art der combinatorischen Berechnung möglicher Formen, getrennt nach Streifen und Bänderformen, werde ich an anderer Stelle ausführlicher zurückkommen. Ich möchte dabei zugleich zeigen wie die Zahl der theoretisch möglichen Formen eine wesentliche Beschränkung erfährt, wenn auf die organischen Verhältnisse Rücksicht genommen wird, und diejenigen Formen eliminirt werden wo eine Verschmelzung zu Bänderformen durch die natürlichen Lageverhältnisse in der Natur eine wesentliche Erschwerung erfahren muss, so dass eine Annäherung an die aus den Funden berechneten Vertheilungsverhältnisse eintreten kann.

C. H. August 26ten. 1902.

CAMBRIDGE: PRINTED BY J. AND C. F. CLAY, AT THE UNIVERSITY PRESS.

BIOMETRIKA.

The Editors of *Biometrika* conclude with Part IV. their first volume, and desire at the same time to say a few words to their friends and subscribers. The experience of this year has shown the Editors how difficult it is to fulfil all their promises within the limited space at their disposal. They have accordingly sought in this volume rather to show the variety of subjects to which biometric methods apply, than to give their readers bibliography and exegesis. Thus this volume has been principally occupied with original memoirs in which the Editors have endeavoured to maintain a high standard of both statistical method and observational care. They hope in their second volume to give a completer critical record of earlier and current biometric work and at the same time to deal more fully with the terminology and methods of statistical research in biology.

The success of *Biometrika* has been greater in its first year than its founders ventured to anticipate. Except in one or two isolated cases it has met with a friendly welcome as an earnest endeavour to supply a real scientific want. But this success and the desire of the Editors to show what new fields biometry opens up have led to the extension of the Journal much beyond the size originally proposed. Subscribers to the first volume have received nearer 500 than the promised 400 pages, together with a mass of numerical measurements and data, which must have permanent value, however much biological or statistical theories may develop. The Publishers on their side have in every way—in printing, tabulation and illustration, much of which has been of a difficult if not novel character—supported the endeavour of the Editors to produce a journal which will well bear comparison with the leading periodicals devoted to other special branches of science.

All labour, however, of both Editors and Publishers must be unavailing unless *Biometrika* arouses and maintains in its supporters and subscribers what may perhaps be termed a *personal* interest in its success. This can be exhibited materially not only by the renewal of existing subscriptions, but by the procuring of additional ones. The number of subscribers to *Biometrika* has doubled since it was started; it is most important that none of these friends of its infancy should now desert it. If each existing subscriber would make a strenuous effort to procure one or more additional subscribers, the Editors might hope that the subscription list for the second year would again be doubled; the journal would then become self-supporting and its existence would be permanently secured. There are several University and public libraries, not only in Great Britain, but on the continent and in America which do not yet appear to have heard of *Biometrika*, and these can only become subscribers through the influence of local friends.

In a second manner also personal interest in the Journal can be effectively exhibited, namely by sending to the Editors, not only original papers, but material for notes in the *Miscellanea*, offprints for notice and suggestions or criticisms upon what has appeared or should appear in its pages. The Editors accordingly appeal to their friends and subscribers for renewed and additional aid in these matters.

It would greatly oblige the Editors and facilitate the work of the publishers if new subscribers would tear off and return to Messrs C. J. CLAY and SONS the notice attached below; it may be sent through any bookseller.

PRESS NOTICES.

Nature.—"An undertaking which deserves support from all workers in science who are interested in the theory of organic evolution in its broadest application.......We cordially wish the new Journal the success which it deserves."

British Medical Journal.—"The appearance of a new quarterly journal for the statistical study of biological problems is further evidence of the important position which mathematics is beginning to take in biology....... We congratulate the Cambridge University Press on the publication of this new and handsome quarterly, which will, we believe, do great service in advancing the science of biology, and aid in elucidating many problems of medicine which lend themselves to investigation by modern statistical methods."

Les Temps.—"*Biometrika*, fondée par les biologistes anglais qui ont le plus travaillé pour faire accepter par les naturalistes l'idée nouvelle, et pour la développer et propager dans le monde scientifique, *Biometrika* qui, en réalité procède de la pensée de Darwin dont les leçons ne furent pas, en Angleterre, comme en France, étouffées sous les clameurs de maîtres en place qui anathématisaient toute doctrine dont ils n'étaient point les auteurs, *Biometrika* marque incontestablement une orientation nouvelle."

Revue Scientifique.—"*Biometrika* est essentiellement le résultat de l'orientation nouvelle donnée aux études biologiques par les travaux de M. F. Galton, et plus récemment, de M. Weldon, de M. Bateson, de M. Karl Pearson, etc., travaux qui tendent tous à mesurer, et à exprimer par des chiffres, le sens et le degré des variations, au lieu de se contenter de signaler l'existence de celles-ci. Par conséquent, nous assistons ici à une invasion des mathématiques dans le domaine des choses de la vie ; et, à vrai dire, nous n'y pouvons voir aucun inconvénient.......Ce premier numéro de *Biometrika* donne une très bonne idée du but poursuivi, et des méthodes par lesquelles il convient d'y tendre, en même temps que de la variété des problèmes susceptibles d'être abordés ; nous ne pouvons qu'engager les naturalistes,—ceux du moins qui veulent faire œuvre de biologistes, et ne pas se contenter de décrire et enregistrer des espèces tout simplement—à examiner avec attention ce fascicule. Beaucoup d'entre eux, nous en sommes persuadés, voudront connaître de plus près la méthode nouvelle, et nous espérons que beaucoup voudront l'appliquer, et apporter leur pierre à l'édifice commun."

BIOMETRIKA. NEW SUBSCRIBERS' FORM.

To Messrs C. J. Clay and Sons,
Cambridge University Press Warehouse,
Ave Maria Lane,
London, E.C.

Please send me Volume I of Biometrika *at a cost of* 30/- *and enter my name as a subscriber to that Journal until further notice at* 30/- *net per volume of four parts, or bound in Buckram* 34/6 *net. I desire that Volume I and the further parts as they appear shall be sent to the address given below.*

Signature

Address

⁂ This form may be transmitted through any bookseller.

Vol. I. Part I. **October 1901**

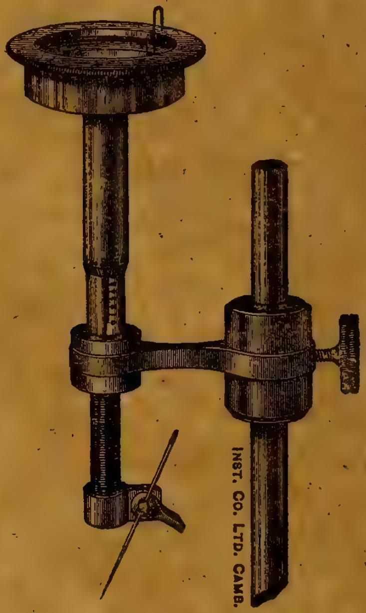
BIOMETRIKA

A JOURNAL FOR THE STATISTICAL STUDY OF BIOLOGICAL PROBLEMS

EDITED
IN CONSULTATION WITH FRANCIS GALTON
BY
W. F. R. WELDON KARL PEARSON
AND
C. B. DAVENPORT

CAMBRIDGE
AT THE UNIVERSITY PRESS
LONDON: C. J. CLAY AND SONS, AVE MARIA LANE
AND H. K. LEWIS, GOWER STREET
NEW YORK: THE MACMILLAN COMPANY
LEIPSIC: BROCKHAUS

Price Ten Shillings net.


INST. CO. LTD. CAMB.

Vol. I. Part II. January 1902

BIOMETRIKA

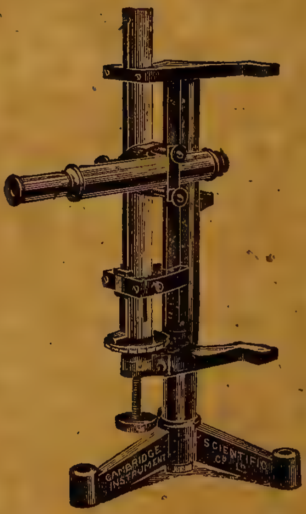
A JOURNAL FOR THE STATISTICAL STUDY OF BIOLOGICAL PROBLEMS

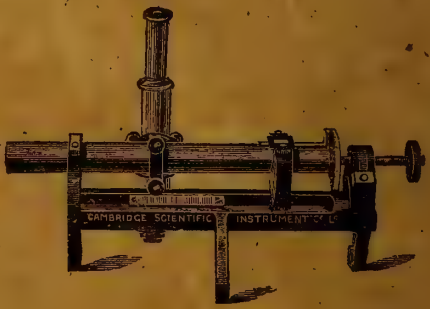
EDITED
IN CONSULTATION WITH FRANCIS GALTON
BY
W. F. R. WELDON KARL PEARSON
AND
C. B. DAVENPORT

CAMBRIDGE
AT THE UNIVERSITY PRESS

LONDON: C. J. CLAY AND SONS, AVE MARIA LANE
AND H. K. LEWIS, GOWER STREET
NEW YORK: THE MACMILLAN COMPANY
LEIPSIC: BROCKHAUS

Price Ten Shillings net.


CAMBRIDGE INSTRUMENT
SCIENTIFIC CO. LD


CAMBRIDGE SCIENTIFIC INSTRUMENT

Vol. I. Part III. April 1902

BIOMETRIKA

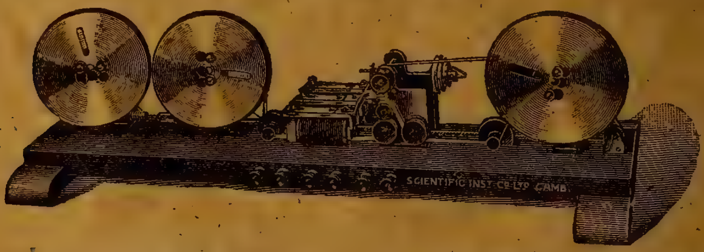
A JOURNAL FOR THE STATISTICAL STUDY OF BIOLOGICAL PROBLEMS

EDITED
IN CONSULTATION WITH FRANCIS GALTON
BY
W. F. R. WELDON KARL PEARSON
AND
C. B. DAVENPORT

CAMBRIDGE
AT THE UNIVERSITY PRESS

LONDON: C. J. CLAY AND SONS, AVE MARIA LANE
AND H. K. LEWIS, GOWER STREET
NEW YORK: THE MACMILLAN COMPANY
LEIPSIC: BROCKHAUS
BOMBAY AND CALCUTTA: MACMILLAN AND CO., LIMITED

Price Ten Shillings net.


SCIENTIFIC INST. Co. LTD. CAMB.

Vol. I. Part IV. **August 1902**

BIOMETRIKA

A JOURNAL FOR THE STATISTICAL STUDY OF BIOLOGICAL PROBLEMS

EDITED
IN CONSULTATION WITH FRANCIS GALTON
BY
W. F. R. WELDON KARL PEARSON
AND
C. B. DAVENPORT

CAMBRIDGE
AT THE UNIVERSITY PRESS

LONDON: C. J. CLAY AND SONS, AVE MARIA LANE
AND H. K. LEWIS, GOWER STREET
NEW YORK: THE MACMILLAN COMPANY
LEIPSIC: BROCKHAUS
BOMBAY AND CALCUTTA: MACMILLAN AND CO., LIMITED

Price Ten Shillings net.


SCIENTIFIC INST. Co. LTD. CAMBRIDGE

OXFORD UNIVERSITY PRESS.

BIOLOGICAL SERIES. (Translations of Foreign Memoirs.)

Memoirs on the Physiology of Nerve, of Muscle, and of the Electrical Organ. Edited by Sir J. Burdon-Sanderson, M.D., F.R.SS.L. & E. Medium 8vo, 21*s.*

The Anatomy of the Frog. By Dr Alexander Ecker, Professor in the University of Freiburg. Translated, with numerous Annotations and Additions, by George Haslam, M.D. Medium 8vo, 21*s.*

Essays upon Heredity and kindred Biological Problems. By Dr August Weismann. Authorized Translation. Crown 8vo.

Vol. I. Edited by E. B. Poulton, S. Schönland, and A. E. Shipley. *Second Edition.* 7*s.* 6*d.*
Vol. II. Edited by E. B. Poulton and A. E. Shipley. 5*s.*

The Earliest Inhabitants of Abydos. (A Craniological Study.) By D. Randall-MacIver, M.A. Portfolio, eight plates, and sixteen tables. 10*s.* 6*d. net.*

The Structure and Life-History of the Harlequin Fly (Chironomus). By L. C. Miall, F.R.S., and A. R. Hammond, F.L.S. With One Hundred and Thirty Illustrations. 8vo, 7*s.* 6*d.*

Alternating Generations; a Biological Study of Oak Galls and Gall Flies. By Hermann Adler, M.D. Translated and Edited by C. R. Straton, F.R.C.S. Ed., F.E.S. With coloured Illustrations of forty-two Species. Crown 8vo, cloth extra, 10*s.* 6*d. net.*

A Monograph, Structural and Systematic, of the Order Oligochaeta. By Frank Evers Beddard, M.A., F.R.S. *With Plates and Illustrations.* Demy 4to, 42*s. net.*

On certain Variations in the Vocal Organs of the Passeres. By J. Müller. Translated by F. J. Bell, B.A., and edited by A. H. Garrod, M.A., F.R.S. With Plates. 4to, 7*s.* 6*d.*

Annals of Botany. Edited by Isaac Bayley Balfour, M.A., M.D., F.R.S., D. H. Scott, Ph.D., F.R.S., and W. G. Farlow, M.D.; assisted by other Botanists. Royal 8vo. Published quarterly, price 14*s.* per number.

Complete Catalogue Post free on application.

LONDON: HENRY FROWDE, OXFORD UNIVERSITY PRESS
WAREHOUSE, AMEN CORNER, E.C.

CAMBRIDGE UNIVERSITY PRESS.

CAMBRIDGE GEOGRAPHICAL SERIES.

General Editor, F. H. H. Guillemard, M.D., late Lecturer in Geography at the University of Cambridge.

Man, Past and Present. By A. H. Keane, F.R.G.S., late Vice-President Anthropological Institute, Corresponding Member Italian and Washington Anthropological Societies. Crown 8vo. With Illustrations. 12*s.*

"We may conclude by the expression of our sense of the high value of Mr Keane's work, which will be acceptable alike to the advanced student of ethnology and to all those interested in the natural history of their own race. (R. L.)"—*Nature.*

"This important work is intended as a supplement to the Author's *Ethnology.* It deals systematically with the primary divisions of mankind taking up each group in turn, and the statements are fortified by an immense amount of references, which the advanced student will find of the utmost service."—*Geographical Journal.*

Ethnology. By the same Author. With numerous Illustrations. Second Edition. Crown 8vo. 10*s.* 6*d.*

"A handy but comprehensive work on ethnology has long been required alike by the student and general reader, and it is with pleasure and interest that we welcome the appearance of a book which is claimed by the author to be a synthesis and a trustworthy guide......We recommend it as a most useful introduction to a very complicated study; and as the author has brought together and abstracted a large number of references, the student can use the book as a point of departure, and thus it will serve as a base for a more extended or detailed survey of this really important branch of science."—*Nature.*

"This book ought to be bought by every public library, member of parliament, colonial official, missionary, captain of a man-of-war, and novelist."—*Speaker.*

LONDON: C. J. CLAY and SONS,
CAMBRIDGE UNIVERSITY PRESS WAREHOUSE,
AVE MARIA LANE.

CONTENTS

The Frontispiece gives the Statue of **Charles Darwin** by Hope Pinker presented to the University of Oxford by Professor E. B. **Poulton**, F.R.S. It is reproduced from a negative by Mrs E. B. **Poulton** and presented to Biometrika by L. S.

Biometrika will appear about four times a year. A volume containing about 400 pages, with plates and tables, will be issued annually.

Papers for publication should be sent to Professor W. F. R. **Weldon**, Merton Lea, Oxford, to Professor **Karl Pearson**, 7, Well Road, Hampstead, London, or to Professor C. B. **Davenport**, University of Chicago, Illinois, U.S. It is very desirable that a copy of all measurements made, not necessarily for publication, should accompany each paper. In all cases the papers themselves should contain not only the calculated constants, but the distributions from which they have been deduced.

Papers will be accepted in German, French or Italian. In the first case the manuscript should be in Roman not German characters.

Contributors receive 25 copies of their papers free. Fifty additional copies may be had at cost price about 6/- per sheet of eight pages: these should be ordered when the final proof is returned.

All communications, other than those relating to contributions, should be addressed to the University Press, Cambridge.

The subscription price, payable in advance, is 30*s.* *net* per volume (post free); single numbers 10*s.* net. Subscriptions may be sent to Messrs C. J. Clay & Sons, Cambridge University Press Warehouse, Ave Maria Lane, London, either direct or through any bookseller.

Publications of the Cambridge University Press.

Reports of the Anthropological Expedition to Torres Straits by the members of the Expedition. Edited by A. C. Haddon, Sc.D., F.R.S., Fellow of Christ's College, Lecturer on Ethnology in the University of Cambridge. Volume II. (Physiology and Psychology). Part I. Introduction and Vision. *Now ready.* Price 9*s.* net.

It is expected that the work will be completed in five volumes.

The price of each part will vary according to its size and the number of the illustrations. The price of separate parts will be net, but to subscribers to the whole set a reduction will be made of one-third of the published price. It is estimated that the subscription price to the complete set will be about five pounds.

The Fauna and Geography of the Maldive and Laccadive Archipelagoes. Being the Account of the Work carried on and of the Collections made by an Expedition during the years 1899 and 1900 under the leadership of J. Stanley Gardiner, M.A., Fellow of Gonville and Caius College and Balfour Student of the University of Cambridge.

Volume I. Part I. *Now ready.* Price 15*s.* net.
Volume II. Part II. will be published on April 15, 1902.

The work will be complete in eight parts, forming two volumes; each part will contain about 120 pages of text, demy quarto, and will be fully illustrated by lithographic plates and text illustrations prepared by Mr Edwin Wilson of Cambridge.

Subscription for the whole work £4. 4*s.* net. The separate parts will be published at 15*s.* net.

Zoological Results based on material from New Britain, New Guinea, Loyalty Islands and elsewhere, collected during the years 1895, 1896, and 1897. By Arthur Willey, D.Sc. Lond., Hon. M.A. Cantab., F.Z.S., Lecturer on Biology in Guy's Hospital Medical School.

Part I., with Eleven Plates. Demy 4to. 12*s.* 6*d.*
Part II., with Twelve Plates. Demy 4to. 12*s.* 6*d.*
Part III., with Ten Plates. Demy 4to. 12*s.* 6*d.*
Part IV., with Twenty Plates. Demy 4to. 21*s.*
Part V., with Twenty-one Plates. Demy 4to. 21*s.*

The entire work will be completed with the publication of Part VI., which will contain Dr Willey's Monograph on *Nautilus* and other articles, including an account of the Ascidians by Prof. W. A. Herdman, D.Sc., F.R.S.

Index Nominum Animalium. Compiled by C. Davies Sherborn under the supervision of a Committee appointed by the British Association and with the support of the British Association, the Royal Society and the Zoological Society. Volume I. (1758—1800).
[*Ready about Oct.* 1902.

This volume will consist of about 1300 pages, and include upwards of 60,000 entries, and will be issued at the price of twenty-five shillings net. The right is reserved of raising the price after publication.

LONDON: C. J. CLAY AND SONS,
CAMBRIDGE UNIVERSITY PRESS WAREHOUSE,
AVE MARIA LANE.

PAPERS for all PURPOSES.

PRINTING for all PURPOSES.

Papers of all qualities ruled to any pattern.
Ruled Papers for Correlation Tables.
Manuscript Books in great variety.

LANGLEY & SONS, Euston Printing and Stationery Works,
George Street, London, N.W.

Two minutes from Gower Street Station. Three minutes from University College,
Two minutes from Euston Station.

Telephone: No. 727 K. C. Telegrams: "PRESSLY LONDON."

CONTENTS

Biometrika will appear about four times a year. A volume containing about 400 pages, with plates and tables, will be issued annually.

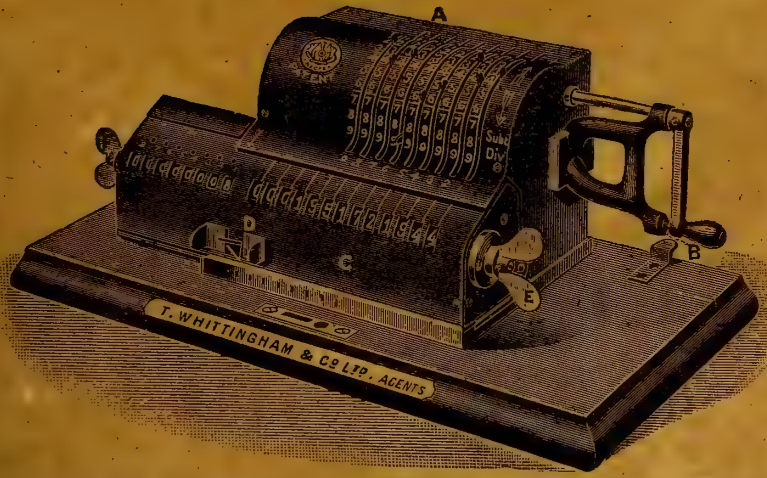
Papers for publication should be sent to Professor W. F. R. WELDON, Merton Lea, Oxford, to Professor KARL PEARSON, 7, Well Road, Hampstead, London, or to Professor C. B. DAVENPORT, University of Chicago, Illinois, U.S. It is very desirable that a copy of all measurements made, not necessarily for publication, should accompany each paper. In all cases the papers themselves should contain not only the calculated constants, but the distributions from which they have been deduced.

Papers will be accepted in German, French or Italian. In the first case the manuscript should be in Roman not German characters.

Contributors receive 25 copies of their papers free. Fifty additional copies may be had at cost price about 6/- per sheet of eight pages: these should be ordered when the final proof is returned.

All communications, other than those relating to contributions, should be addressed to the University Press, Cambridge.

The subscription price, payable in advance, is 30*s. net* per volume (post free); single numbers 10*s.* net. Subscriptions may be sent to Messrs C. J. Clay & Sons, Cambridge University Press Warehouse, Ave Maria Lane, London, either direct or through any bookseller.


A
B
C
D
E
T. WHITTINGHAM & Co LTD. AGENTS

CONTENTS

Biometrika will appear about four times a year. A volume containing about 400 pages, with plates and tables, will be issued annually.

Papers for publication should be sent to Professor W. F. R. Weldon, Merton Lea, Oxford, to Professor Karl Pearson, 7, Well Road, Hampstead, London, or to Professor C. B. Davenport, University of Chicago, Illinois, U.S. It is very desirable that a copy of all measurements made, not necessarily for publication, should accompany each paper. In all cases the papers themselves should contain not only the calculated constants, but the distributions from which they have been deduced.

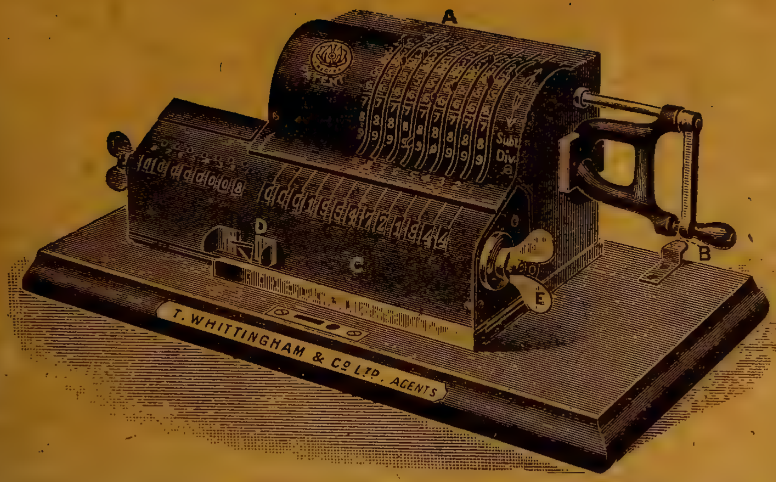
Papers will be accepted in German, French or Italian. In the first case the manuscript should be in Roman not German characters.

Contributors receive 25 copies of their papers free. Fifty additional copies may be had at cost price about 6/- per sheet of eight pages: these should be ordered when the final proof is returned.

All communications, other than those relating to contributions, should be addressed to the University Press, Cambridge.

The subscription price, payable in advance, is 30*s.* *net* per volume (post free); single numbers 10*s.* net. Subscriptions may be sent to Messrs C. J. Clay & Sons, Cambridge University Press Warehouse, Ave Maria Lane, London, either direct or through any bookseller.

THE "BRUNSVIGA" CALCULATING MACHINE



will Multiply, Divide, Add, Subtract, and consequently will perform all numerical complications in plain figures or decimals. Invaluable for compiling Statistics, working out Ratios, Averages, Percentages, Birth Rates, Death Rates, Interests, Foreign Exchanges, &c., &c.

Size A. Products 18 Figures; Multiplier or Quotient 10 Figures. **£27. 10s.**
Size B (the most popular). Products 13 Figures; Multiplier or Quotient 8 Figures. **£20.**
Size C. Products 10 Figures; Multiplier or Quotient 8 Figures. **£15.**

Agents—T. WHITTINGHAM & CO., Limited, Export Stationers and Printers,
35, BUCKLERSBURY, LONDON, E.C.
REGISTERED OFFICE AND WORKS—12, LITTLE TRINITY LANE.

OXFORD UNIVERSITY PRESS.

ANNALS OF BOTANY. Edited by ISAAC BAYLEY BALFOUR, M.A., M.D., F.R.S., of the University of Edinburgh; D. H. SCOTT, M.A., Ph.D., F.L.S., of the Royal Gardens, Kew; W. G. FARLOW, M.D., of Harvard University, U.S.A., assisted by other Botanists. Royal 8vo, paper covers.

The subscription price of each Volume is £1. 10s. *payable in advance. Intending subscribers should send their names, with subscription, to* Mr HENRY FROWDE.

⁂ Complete sets (Vols. I–XIV, 1887–1900) are offered for the present at the following prices, viz. £20 unbound, £23 bound.

Vol. XVI, No. LXI, with 9 plates, 14*s.*

CONTENTS.

VINES, S. H.—*Tryptophane in Proteolysis.*
LANG, W. H.—*On the Prothalli of* Ophioglossum pendulum *and* Helminthostachys zeylanica.
MASSEE, G., and SALMON, E. S.—*Researches on Coprophilous Fungi.* II.
FORD, Miss S. O.—*The Anatomy of* Ceratopteris thalictroides (L.).
GAGER, C. S.—*The development of the Pollinium and Sperm-cells in* Asclepias Cornuti, *Decaisne.*
THISELTON-DYER, Sir W. T.—*Morphological Notes.* V and VI.
NOTES.

Vol. XVI, No. LXII, with 9 plates, 14*s.*

CONTENTS.

YAPP, R. H.—*Two Malayan 'Myrmecophilous' Ferns,* Polypodium (*Lecanopteris*) carnosum (*Blume*), *and* Polypodium sinuosum, *Wall.*
WARD, H. M.—*On the Relations between Host and Parasite in the Bromes and their Brown Rust,* Puccinia dispersa (*Erikss.*).
HILL, T. G.—*On Variation in the Flowers of certain Species of Primula.*
COPELAND, E. B.—*The Mechanism of Stomata.*
THISELTON-DYER, Sir W. T.—*Morphological Notes.* VII.
FARMER, J. B., and HILL, T. G.—*On the Arrangement and Structure of the Vascular Strands in* Angiopteris erecta, *and some other Marattiaceae.*
NOTES.

Complete Catalogue post free on application.

London: HENRY FROWDE, Oxford University Press Warehouse, Amen Corner, E.C.

CONTENTS

(All Rights reserved)

The Editors wish to acknowledge the receipt of papers from A. C. **Dimon**, G. U. **Yule**, W. L. **Byrne**, W. **Palin Elderton**, H. **Whitehead**, and A. O. **Powys**.

Mr W. **Bateson** hopes to send at a later date a reply to the Paper "On the Fundamental Conceptions of Biology" which appeared in Part III.

Biometrika will appear about four times a year. A volume containing about 400 pages, with plates and tables, will be issued annually.

Papers for publication should be sent to Professor W. F. R. **Weldon**, Merton Lea, Oxford, to Professor **Karl Pearson**, 7, Well Road, Hampstead, London, or to Professor C. B. **Davenport**, University of Chicago, Illinois, U.S. It is very desirable that a copy of all measurements made, not necessarily for publication, should accompany each paper. In all cases the papers themselves should contain not only the calculated constants, but the distributions from which they have been deduced.

Papers will be accepted in German, French or Italian. In the first case the manuscript should be in Roman not German characters.

Contributors receive 25 copies of their papers free. Fifty additional copies may be had at cost price, about 6/- per sheet of eight pages: these should be ordered when the final proof is returned.

All communications, other than those relating to contributions, should be addressed to the University Press, Cambridge.

The subscription price, payable in advance, is 30*s.* *net* per volume (post free); single numbers 10*s.* net.—Parts I–IV forming Volume I, 30*s.* *net*, or bound in Buckram 34/6 *net*. Subscriptions may be sent to Messrs C. J. Clay & Sons, Cambridge University Press Warehouse, Ave Maria Lane, London, either direct or through any bookseller.