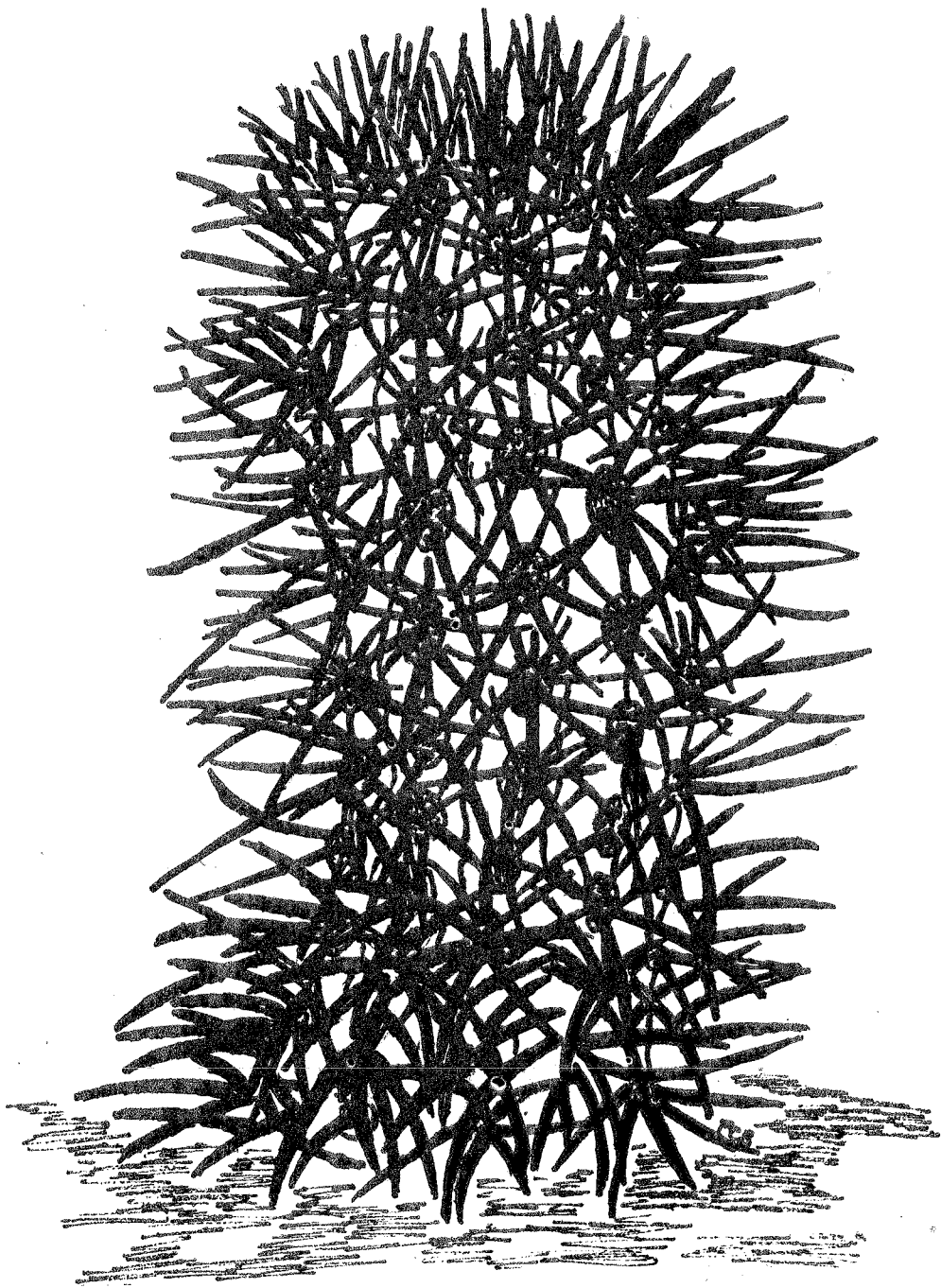


THE CHILEANS '90

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Copiapoa atacamensis

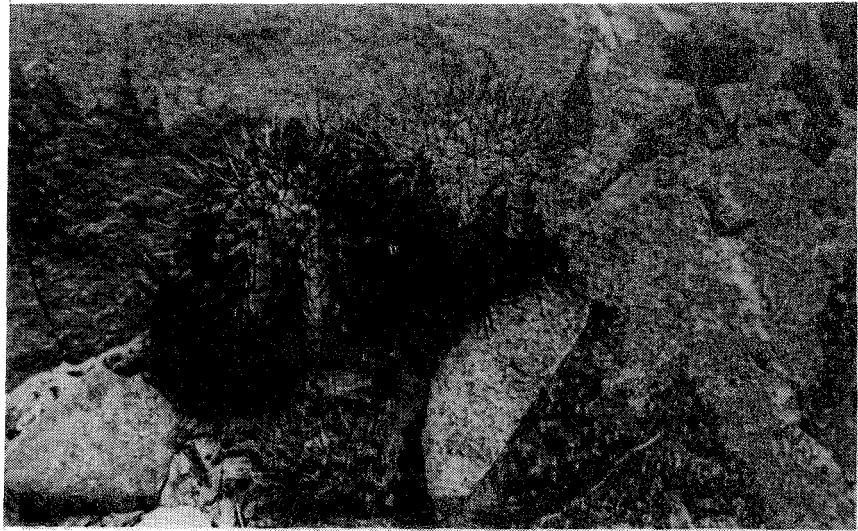
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**COPIAPOA
BOLIVIENSIS**

Blanco Encalad.
600m

Photo

- R Ferryman



**COPIAPOA 'MARGINATA'
KZ 90**

El Cobre

Dodonaeus 5, 6, 1968

Photo - K Knize



EULYCHNIA IQUIQUENSIS & COPIAPOA Kz72

Near Antofagasta Photo - K. Knize

Dodonaeus 6, 5, 1968

Up until the last few years it had always been assumed that it was Thomas Bridges who collected the very first example of *Copiapoa marginata* when he visited Cobija, then the port of entry on the Pacific coast which was the start of the trail to the highlands of Bolivia. However, no plants which would meet the original description of *C. marginata* have yet been found at Cobija, whilst plants which do conform to that description abound along the hilly coast from south of Caldera to as far north as Chanaral. In the 1960's and 1970's imported plants of elongated forms of *Copiapoas* became readily available under names such as *streptocaulon* or *marginata*. These were discussed in *The Chileans* (No.37 p.18), where it was suggested that those plants which had been collected from the neighbourhood of Antofagasta over the course of the last fifty years and brought into cultivation under the name of *C. marginata*, required a new name. Following on from that discussion there is now ample evidence available that Thomas Bridges did visit the locality near Caldera where columnar-growing *Copiapoa* are found, which were then sent from Chile to Europe, where they were distributed as *Echinocactus marginatus*, and under other names. Ritter himself in his *Kakteen in Sudamerika* acknowledges that *Copiapoa marginata* emanates from near Caldera and not from Antofagasta as he had first believed.

It may be argued that Bridges could possibly have found his *Echinocactus marginatus* on his visit to Cobija, but any such contention must explain the contradictory evidence. Firstly, no collector has yet reported finding any *Copiapoa* in the vicinity of Cobija. It might even be suggested that Bridges could have travelled some distance up or down the coast whilst he was at Cobija and so discovered the plant at an isolated spot where it was not subsequently rediscovered. However, the slides taken by R.Ferryman on his trip to the Grand North in the course of his first visit to Chile, showed clearly that the coastal hills drop steeply down to the shore along a considerable length of that part of the coast on which Cobija lies. At the Chileans' Autumn Weekend we also heard the graphic account from R.Ferryman of his trek along the face of this slope from an accessible point overlooking the sea, to take a closer look at some *Eulychnias* which were to be seen perhaps a km or so away. Eventually they were reached after a couple of hours struggling along the slope, across steep-sided quebradas and over patches of loose sand.

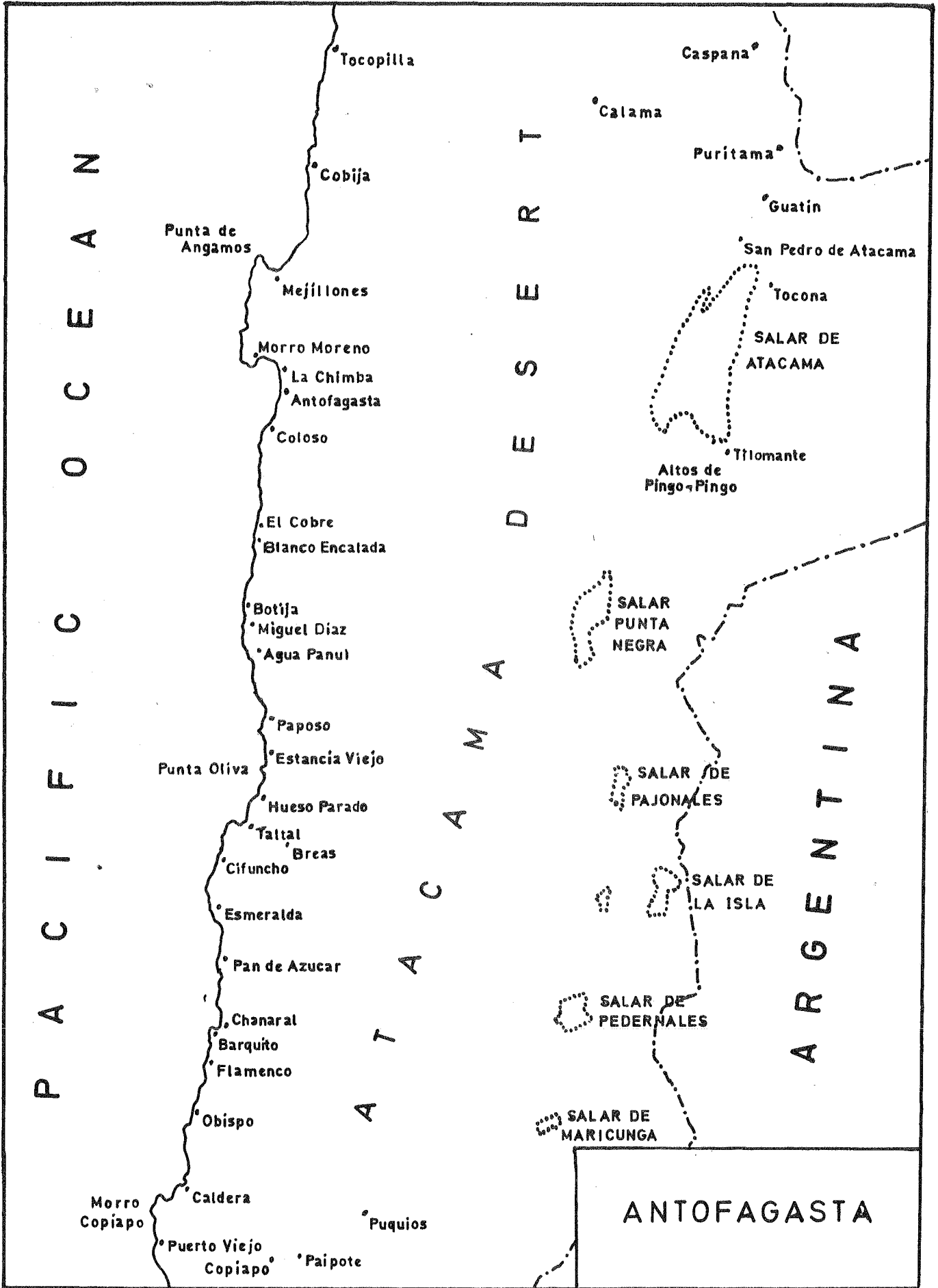
In 1841 no European would consider such a hike - the peons walked, the Europeans rode; travel up and down the coast from Cobija was virtually impossible in the saddle. Today it is perhaps difficult to appreciate the pioneering nature of travel outside the established trade routes of the day. Practicalities of travel such as the absence of maps, the few places where provisions were available, must be related to 1841 and not to today's situation. It may be argued that Bridges may have seen plant life on shore whilst passing Mejillones; that he calculated the distance from there to Cobija was roughly similar to the cross-desert trek he had previously made from Copiapo to Totorillo; that he organised transport and provisions in Cobija for a visit to Mejillones; that he took a practical route away from the immediate coast; and so collected some *Copiapoa* there. However, there is and was no fresh water at or near Mejillones and the logistics would seem to be against survival on such a trek. For practical purposes it appears that all Bridges' elongated *Copiapoas* were collected in the area around port Copiapo and Caldera.

It may be considered that that *Copiapoa atacamensis* could have been collected somewhere near Antofagasta in the area where Dr. Rose collected it in 1914, or even south of Antofagasta in the Bolivia of that time. In his original description of *Echinocactus boliviensis*, Pfeiffer gave Bolivia as the country of origin. The traditional boundary between Chile and Upper Peru (later Bolivia) lay at Hueso Pardo, just to the north of Taltal (*Chileans* 40, p.30). If the plant which Pfeiffer described really had come from Bolivia, it could have come from anywhere between that boundary and Antofagasta. However Bolleart specifically notes the problems of trying to disembark anywhere along this stretch of then quite uninhabited coast (*Chileans* 40, p.30). Along this coast no means of life support were available to a traveller at that time. It is indeed surprising that Philippi, on his pioneering trip through the Atacama desert, did venture further north than Paposo, as far as El Cobre. Philippi also managed to land at Mejillones from a boat; it should be borne in mind that Philippi was travelling as a representative of the Chilean government and with a government ship at his disposal. However there is no record of any traveller or collector visiting any part of this coast between Bridges' 1844 visit to Cobija and Philippi's 1854 visit to that coast.

When Dr. Rose collected on the hilly slopes above Antofagasta in 1914 he came across some globular- to ellipsoid-bodied plants which he took to be *Copiapoa marginata*. In selecting this name he greatly confused subsequent discussion of the location and habit of this plant. It is quite clear that for some time Ritter accepted the name *marginata* for the *Copiapoa* found near Antofagasta (*Copiapoa streptocaulon*, *Chileans* 38) which he illustrates in *K.u.a.S* 12.1.1961. The comments made on this subject by Backeberg in his *Die Cactaceae* merely added further to the confusion. Plants found near Antofagasta by Knize during his earliest collecting trips to Chile were identified as Kz 90; in the 1968 *Dodonaeus*, Knize published photographs both of single headed specimens and of plants with up to about half a dozen heads, all globular or perhaps slightly elongated-globular. The comment by Britton and Rose that their "*Copiapoa marginata*" from Antofagasta was globular, later elongated, was always a puzzle to me, since neither the photographs or comments from either Ritter or Knize alluded in any way to elongated growth. So I decided to purchase a couple of plants of Kz 90 from Knize, both of which arrived as nice globular specimens with bodies about 60mm across; they looked quite similar to the photograph of Kz90 in the 1968 *Dodonaeus* and equally similar to plants photographed by R.Ferryman near Blanco Encalada. One of these two plants was kept in my own collection and the other was passed on to E.W.Bentley. My own plant remained globular, or, as some may put it, did not grow much. Imagine my astonishment a year or two later to find the other plant well cultivated by E.W.Bentley and grown to the shape shown on the cover! Had Britton and Rose seen the same sort of thing happen to their own plant in cultivation, hence their description of "later elongated"? So I thought! Had I consulted Ritter's *Kakteen in Sudamerika* Vol.3 after its publication I would have found his "*Copiapoa boliviana*" was globular "later 200-500mm long, occasionally up to 1m long, procumbent". It was the slides shown to *The Chileans' Weekend* by R. Ferryman which were taken in the course of his first trip to Chile which showed clearly to me for the first time that this plant did indeed grow "later elongated".

.... from I.M.Johnston, "Flora of northern Chile".

Much of the early collecting in this region was done at Cobija which is situated between Antofagasta and Tocopilla and is now practically deserted. During most of the first half of the last century it was one of the principal ports-of-call between Coquimbo and Arica. It was under the Bolivian flag and from it started one of the main routes to the Bolivian plateau. Hugh Cuming visited and collected at Cobija in September 1828. Unfortunately his plants were distributed under a general printed label reading "Cobija, Iquique, et Arica" and consequently the precise source of the specimens is unknown. The elder Hooker has made further confusion by labelling them in his herbarium as from Peru and even from



Lima! D'Orbigny collected about Cobija in April 1830. A large collection was made there by M. Gaudichaud who was in the port July 1-3 1836. Thomas Bridges did a little collecting at Cobija in September 1844. His plants were distributed with those he collected on the plateau and in the Amazon forests, and like them merely labelled as from Bolivia. About Antofagasta little collecting has been done. Herzog collected there in September 1911, and Rose in October 1914. The only collections I know of from Tocopilla were by Vidal in September 1889 and Gulland in 1918. Collections from Iquique were made by Salinas in December 1913 and at Quebrada de Huantaca near Iquique by Martens in September 1904. Recently Werderman collected in the Quebrada Huantajaya near Iquique; Rose also collected at Iquique in 1914. There is very little literature bearing directly upon the Flora of the nitrate coast.

..... from R. Ferryman

There is little doubt in my mind that the plant illustrated by Pfeiffer as *Echinocactus marginatus* is of a plant that originates from the Copiapo-Caldera area. Copiapoa abound in the coastal flats and hills throughout the area from Morro Copiapo to Caldera and beyond. Several forms exist amongst which are more columnar types, presumably *Echs. columnaris* sensu Pfeiffer but doubtless only a form. The plants from Antofagasta are quite different and as H. Middleditch proposed in these pages some time ago they deserve a different name. Confirmation can be found by comparing the description by Britton and Rose with their illustration. The plants of Copiapoa which I found in the Antofagasta area correspond with the description by Britton and Rose rather than with the illustration. The question is simply what to call the plant now that *C. marginatus* has been tied to Caldera.

The description of *Echs. bolivianus* by Pfeiffer is quite clearly of a Copiapoa. Although it is far from a comprehensive description it contains enough clues. The mention of 'epidermis decidua' (deciduous epidermis) is a phrase that I can well liken to the plants I found. It is not strictly true of course but I can understand the impression given. Similarly the plants are not green with white clothing but more a dirty grey with the white infusion typical of Copiapoa. They are in my opinion the scruffiest looking of all Copiapoas and when covered in dense fog look as though they need a good wash!

The plants I found (RMF 39) were particularly abundant in the coastal hills around Morro Moreno through the Bay of Chimba continuing south to the sparse representation at Blanco Encalada. I found no sign of Copiapoa around Cobija. The Bay of Chimba would have represented a safe harbour for Bridges' vessel on route to Cobija: upon landing he would have found little difficulty in finding the Copiapoa for there are no high mountains running right to the edge of the sea at that spot. My collection list shows the field data as sea level to 200 metres altitude. Confirmation of the ease of landing here at Chimba can be seen by the fact that Antofagasta now represents a large fishing port and dock. Antofagasta is of course situated in the Bay of Chimba.

The plants themselves grew in areas overlooking the sea in a habitat that I can only describe as resembling a builders' yard. Rubble of all shapes and sizes strewn about with the plants preferring the base of rocks or, where possible, genuine ground. A few grew from cracks and crevices between rocks but most preferred a more open aspect. Soil was fairly soft uniform earth into which the plants put down a fairly substantial tap root. The area was obviously damp for lengthy periods as lichen grew quite abundantly on the Copiapoa and on the *Eulychnia*. During our two day stay in that locality the mist never cleared for any length of time. Little other vegetation could be found - the odd dead or dying shrub was all that could be seen. Cerro Moreno was reached from an inland road linking Antofagasta with Cobija/Tocopilla. The road travels along the coast for most of its length as far as the Mejillones peninsula, from where the road continues straight on but the coast becomes distant. On regaining the coast at the northern end of the Mejillones peninsula it is possible to turn west and follow the coastline back to Cerro Moreno, but it is a very arduous journey. From Cobija to Mejillones I saw no Copiapoa, either at the coast or in the hills. As to how far the Copiapoa may be distributed along the peninsula north from Cerro Moreno would be pure speculation on my part; Ritter makes no mention of their appearance any further north than La Chimba. The southernmost occurrence is at Blanco Encalada where it grows amongst rocks in the ravines. Here the appearance of the plants is much nicer. Further south the larger Copiapoas take over and although one would need a lifetime to explore the coast from Blanco Encalada to Taltal it seems to me unlikely that it would appear here, this view being based upon the field work which I have managed to carry out around Papos and Taltal.

The plants at Caldera which Ritter calls *Copiapoa calderana* = *Copiapoa lembckeii* Backbg. bear an uncanny resemblance to the *C. boliviana/atacamensis*, part of which may be attributable to the extent of their variation. It may be recollected that Pfeiffer also described *Echinocactus (Copiapoa) bridgesii* as coming from Bolivia whereas Ritter puts it amongst the collection of Copiapoa in the surroundings of Caldera. The illustration entitled *C. bridgesii* on the front cover of Chileans No.37. is of a plant that I would consider to be *Copiapoa boliviana*.

..... further from H. Middleditch

It is most interesting to hear that *Copiapoa atacamensis* is to be found within striking distance of the shore line around what is now Antofagasta. As R. Ferryman has stopped in that locality he will have seen for himself the apparent convenience with which a collector could make a landing at that spot; he also suggests that Bridges may have landed there. However, it may be as well to bear in mind that Bridges would travel out to Cobija on a commercial boat that would earn its keep by anchoring at recognised ports of call and dropping off or collecting freight or passengers. There would be no question of a "request" stop for one passenger who was a working foreman on an Estate in central Chile; even less likely would be a "request" stop on the stretch of coast between Blanco Encalada and Mejillones where there was no trade to be had and no-one to trade with. In 1835-1842 there was no port at Antofagasta, nor yet any habitation. In addition we find Isaiah Bowman (*Desert Trails of Atacama*, 1924) writing of his visit to Antofagasta that "it has a very poor natural harbour; vessels must anchor in the open roadstead and discharge by lighter to the small moles. Exposure to the southwest gales can entail further delay".

It was Cumming who sailed up the coast of Chile in his own yacht, stopping at his pleasure, so he could have landed and collected this Copiapoa. But his interest was collecting shells. He sailed home to Europe westwards across the Pacific, collecting again on the shores of the East Indies. It seems unlikely that he would involve himself with plants on such an extended return journey to Europe, particularly since his interest in plants arose only from acting as selling agent for Bridges. Even if Bridges had succeeded in making the return trip from Cobija to waterless Mejillones, no Copiapoa were seen by R. Ferryman in the vicinity of Mejillones.

Thus it would seem to me, on the basis of sheer practicalities of travel at that time, that we are still left without any definite evidence that the plants described by Pfeiffer as *Echinocactus boliviensis* could have come from the vicinity of what is now Antofagasta; there appears to me to be every probability that they could have come from a part of Chile visited at that time by utilising the facilities then available. At the time of Pfeiffer's description, there were occasions when cacti from one country were incorrectly attributed to another, whilst cacti from South America were even attributed to Mexico. Hence the possibility cannot be ruled out altogether that the plant on which Pfeiffer based his description

had been collected much further to the south, perhaps as far south as Caldera again.

At the time when this plant was originally collected, there was a great deal of interest in Europe in acquiring previously unknown plants from overseas. This was fuelled by the spread of wealth to manufacturers and merchants arising out of the exploding industrial revolution in Western Europe, whilst the retention of collected plants was made possible by the novel victorian conservatories. In turn, this required merchants to acquire and distribute plants, but - most importantly - it required travellers and collectors to venture abroad where plants could be found, dug up, and shipped back to Europe. There would certainly be some plants which could well have been collected by whim, but the great majority of collections were made by travellers who had been promised cash in return for their efforts - quite possibly with money up front to cover some of their initial expenses. Any merchant or collector who put up either money or promise of cash might approach either someone who was going abroad for the first time, or else a person who had already been abroad; if the latter, it might even be a traveller who already knew where plants were to be dug up, who knew what transport problems had to be faced. Put yourself in the position of a plant merchant or rich collector who is proposing to put up front money together with a promise of cash for plants, in 1842. Would you pick on Bridges, who can tell you that he has been to the Rio Copiapo where there are these unknown cacti by the boatload, growing not far off the coast, where there is a port which is visited by a regular and reliable sailing ship service? Where they can be transported to the coast as complete large plants, not just the crown like the *Copiapoa echinoides* he had collected on his previous trip, at Totoral? Or would you pay out your money and promises to someone who is off to the wilds of Africa or Amazonia in the hope of finding something? Before you handed over your money, would you extract a promise from Bridges that after he left Valparaiso in 1844, he would stop off at the Rio Copiapo and collect several cases of plants which he could have forwarded - as was common practice - in his absence either to Valparaiso or even to Europe. If you had been in Bridges shoes, knowing the frequency of the sailing service up the coast of Chile and the relative ease of breaking his journey at Rio Copiapo, would you have accepted the money and executed the commission?

If this explanation is accepted, then *Echinocactus bolivianus* Pfeiff, syn. *Copiapoa bolivianus* Ritt., does not refer to the plants in question growing between Antofagasta and Blanco Encalada. It was one of the load of plants collected by Bridges not too far from Caldera in 1844, many of which were listed in Schelhouse's catalogue of 1846. At that time both *Echinocactus marginatus* and *E. columnaris* were stated to be from Valparaiso, whereas they were from the area between Morro Copiapo and Caldera; *Echinocactus bolivianus* and *E. bridgesii* were both stated to be from Bolivia (Abbildung und Beschreibung Bluhender Kakteen Vol.2 Part 3 Plate 14, 1846, Pfeiffer & Otto) and it is highly probable that they, too, were from the area round the mouth of the Rio Copiapo.

The realities of trade and business of the 1840's would therefore appear to suggest that the plant collected near Antofagasta and named *Copiapoa marginata* by Britton & Rose, was indeed a previously uncollected sort and if this is accepted then the name *Copiapoa atacamensis* is justified. Even leaving aside the questionable basionym for Ritter's name of *Copiapoa bolivianus* for this plant, Ritter's name was published in 1980 in his Vol 3 of *Kakteen in Sudamerika*; *Copiapoa atacamensis* was published in 1979 in *Chileans* 37.

An article written by Knize which appeared in the 1968 *Dodonaeus* journal described a trip to the north of Chile and illustrated those plants which had for so long been called *Copiapoa marginata*, giving a location at El Cobre. The route from Blanco Encalada to Antofagasta was travelled by Ritter both on his own and in company with Buining; in Vol 3 of his *Kakteen in Sudamerika* he gives a distribution for *C. boliviana* from Chimba (north of Antofagasta) to Blanco Encalada. However, the slides which R. Ferryman took on his trip to this area and which were shown to the Chileans Weekend gave a clearer and far more comprehensive picture of the consistent appearance of this plant over its distribution range and the nature of its surroundings. They were a most valuable contribution to gaining a clear understanding of the identification of this plant and its ecology.

..... further from R. Ferryman

There are indeed *Eulychnia* growing near Antofagasta in isolated fashion on the hillsides as depicted in Knize's photograph, but in places they also form quite dense forests. By this I mean forests in the cactus sense with plants ten or twenty meters or more apart, not forests in the tree sense. From Tocopilla down to Antofagasta there are quebradas cut into the hills with spurs like that on the photograph, but only with a good distance between; similarly on the Cerro Coloso just south of Antofagasta these entries are well spaced out. However on Cerro Morreno and at La Chimba, which is almost behind Antofagasta, the lowermost slopes being part of the town, the number of entries increase to the extent that one can just about walk from one to another. The picture is typical of the northern end of this stretch of coast, down to Antofagasta, where the plants are doing battle to survive. When we visited Tocopilla this year the entire population of some 30 to 50 *Eulychnia* were dead.

Copiapoa boliviana has a wide distribution around Antofagasta and to the south. On Cerro Moreno it can be found quite low down on the slopes, similarly at La Chimba. It occurs somewhat higher up on the Cerro Coloso and again fairly low down in the Quebradas that lead to the ocean between Antofagasta and Blanco Encalada. The base of these slopes varies in its distance from the sea but this *Copiapoa* is usually the first trace of vegetation to be seen. At Morro Moreno they start at around 200ft altitude; this probably represents a km or two from the sea and no more than 15 minutes hike from the base of the Cerro. La Chimba however has a low flat bottom to the mountains that now accommodates the town of Antofagasta - I suppose this represents three to four kms from the sea, but again the plants are found quite low down in the mountains. The *Eulychnias* only grow up on the mountainside as depicted in the photograph, together with an occasional *Copiapoa*. At the base of the hills where there are *Copiapoa*, there are no *Eulychnias*. The *Copiapoa* also grow on the flatter areas at the foot of the mountains at odd places between Blanco Encalada and Paposo.

..... from F. Vandenbroeck

On the northernmost coast of Chile plant growth only resumes to the south of the important harbour of Antofagasta. There is a road from Antofagasta going via El Cobre and Blanco Encalada to Paposo. Along this road one can find both *Copiapoa solaris* (syn. *C. ferox*) and *Copiapoa boliviana*, growing together. The two species are very distinctive and I do not remember seeing any hybrids. In this same area I also found a small *Pyrrhocactus*. From this area *P. glaucescens* is reported but the plants do not correspond with Ritter's picture of this species. Many plants of *C. solaris* and *Pyrrhocactus* were either dying or dead, due probably to the excessive drought. *Copiapoa boliviana* seemed better adapted to the harsh climate.

In regard to *Copiapoa solaris*, Ritter gives some odd considerations regarding this species. According to him, this plant grows best above the boundary of the actual mist zone i.e. in a zone which is mainly sunny and where the mist only occasionally reaches. The species name 'solaris' which he gave to this plant refers to its sun-loving nature. These views are opposed by some. From my own observations, these plants grow near Blanco Encalada at sea level and in small groups which appeared fresh and healthy. More to the east, further from the coast and at a greater altitude the plant

mounds were larger, but very clearly the plants here had to endure the intense sunlight. In all clumps the north side (facing the sun) was considerably dilapidated. At higher altitudes the clearly imposing clumps were always withered. It is evident that the optimum zone for growth of *C. solaris* lies at a moderate altitude of about 700m, as indicated by the size of the plant clumps. It is unlikely that this plant originally grew above the mist zone; Ritter's suggestion (Kakteen in Sudamerika Vol.III pp.1051-1052) that scanty rain falls at long intervals on the heights is unlikely, bearing in mind the almost total absence of any accompanying vegetation.

Climatologically it has been established that the pronounced desert character of the northern Atacama district is becoming more extreme. The increasing aridity of the climate in this region and the consequent decrease in mist formation restricts the plants to a zone where at least a little water penetrates. The species currently occurring here should be considered to be no more than a relict of a much richer botanical diversity in former times.

..... from J.Harding, Journal Royal Geographical Society, 1877

That part of the desert of Atacama in which I was engaged was the southern part of the coast province of Bolivia. I was chiefly occupied as engineer in the construction of a railway from the port of Antofagasta to Las Salinas. On the coast the temperature is very equitable, varying in Antofagasta from a maximum in summer of 82°F in the shade, to a minimum of 52° in the winter. There are usually two or three slight showers of rain fall during the winter, but seldom enough to wet the surface of the ground. The wind is almost invariably a gentle sea breeze by day and a land breeze by night. Passing the coast range, the climate changes wonderfully. In the Salar de Carmen, although only 6 miles in a straight line from the sea and 1700 feet high, the cold in winter is very severe and the wind blows a gale almost every day. At La Salinas I have registered a minimum shade temperature at 7 a.m. in the winter of 7°F and at 11 o'clock the same day 98° in the shade. In summer the shade temperature ranges between about 40° at night and 130° in the day. The air is so dry that a piece of thick notepaper if folded and pressed with a paper knife will break in two when opened out.

A JOURNEY TO THE ATACAMA DESERT By R.A. Philippi 1853-1854

Translated by H. Middleditch

In the stretch of territory from the R. Copiapo to Cobija, between the ocean and the Argentine provinces, lies the waste of Atacama. The Chilean government and people are scarcely acquainted with it. The boundaries of Chile, Bolivia, and the Argentine provinces are not yet established there. Very commonly held is the belief that an enormous wealth of precious metals is buried there. It was of some importance to become acquainted with what the mineral resources offered for mining and other commerce. On this and other grounds the Chilean government commissioned me to explore the Atacama desert.

In preparing for this trip I found nobody in Santiago with knowledge of the area. I did not know whether to take mules or horses, where I would find a guide, and so on. I hoped that I could enlighten myself on these matters in Copiapo. Dr. Segeth loaned me a travelling barometer. Prof. Domeyko provided me with a psychrometer; in Valparaiso I found a sextant, but had no luck in obtaining a chronometer. My travelling companion would be Herr W. Doll, a surveyor for many years resident in Chile.....All preparations being affected, on 22 November we sailed out on the brigantine which the government had placed at my disposal.....on 29 November we dropped anchor in the harbour of Caldera close besides the Portland, the English admiral's ship..... from Hueso Parado towards Migual Diaz we reached Hacienda Paposo.... On 22 December we finally got together a sufficient number of mules to take myself, Herr Doll and a servant to El Cobre..... We camped overnight not far from the ocean; the timber from Lecheros and cactus soon provided a fine flickering fire. There was no water there. As soon as grey dawn appeared we departed and after a sharp ride of almost 1 hour we reached the Panul water....Towards midday we ascended to the Agua Migual Diaz, which lies in a valley at 861 feet above sea level. The vegetation there was similar to that near Paposo but much sparser; there was a berberis new to me. [36 other species of plants are listed from 33 different families/genera including "many cacti"].

At 2.30 p.m. we left Agua Migual Diaz. On from here the vegetation decreased strikingly. In the valley of Botijas which is about 2 leagues away from Migual Diaz, lived the uncle of our guide and muleteer with his wife and children. He was occupied in collecting the water which occurred there and conveying it down to the beach for transport by boat twice a week to Cobre, for the mules which are used to bring the ore down to the shore there.

The mountains here consist of Syenite, which crumbles into grit and buries the major peaks in debris and coarse sand. The feldspar is prominent throughout, greyish white, occasionally with small pale red flecks. The quartz is smoke grey. The hornblende is black and occupies two or three times the volume of the black mica; only rarely is there a leaf of white mica. Beyond the odd Echinocactus there is not a trace of vegetation in this desolate grit and rubble. We left there at 6 p.m. going uphill to look for a campsite for the night. We passed a valley that had vertical sides 80ft high in which the strata was laminated horizontally. The muleteer by great exertion climbed up on to the firm rocks on which grew some Calandrina discolor, a few columnar cacti, and Pitcairnia chrysantha. Despite their spines the plump leaves of the last-named served the beasts for sustenance for this night, and the wood of the cactus was our firewood. Water was completely absent. The place is called Chagua de Jote; it lies some 400ft above sea level, some half hour away from it and 15-16 leagues from Paposo.

The following day, 24 December, we set off at daybreak on our track which was similar to that followed the previous afternoon. On the way we met with two females mounted on horseback who came from Cobre. They were very dark, burnt by the sun. After 3 hours sharp riding we caught sight of the tip heaps of a copper mine; soon after that we saw a Chilean flag and as we rounded the small foothill a little bay in which the barque lay at anchor came into view, and finally the establishment El Cobre hidden under crags.

The owner, Don Jose Antonio Moreno, received me most hospitably. I found here the weekly newspaper, a very good kitchen, wine and good water from Valparaiso! For the first time since leaving Chanaral I had bread to eat. Herr Moreno had often travelled over the desert and certainly as an intelligent and alert observer; I acknowledge a great deal of very valuable information from him about the area and have later found all his views to be correct throughout. The small bay where he is established has been called El Cobre from time immemorial and probably even the Changos obtained copper from here. Herr Moreno reopened the copper mine six months ago and currently employs 60 workers. The area could not be more impoverished: no water, no fodder for man nor beast; the animals carry ore down to the beach and transport water and provisions back to the mine. Everything - clothing, tools, firewood, cooking utensils, harness, horse-shoes, and so on, must be brought in by ship.

The vegetation is extremely insignificant. At a couple of hundred feet in height were *Cereus*, a species of *Echinocactus*, *Oxalis gigantea*, and the *Opuntia* with the numerous long whitish spines which I had noted near Cachinal.

The mountains to the east certainly rose up to about 3000ft, all their sides were quite lacking in vegetation. Why do the clouds and mist persist only in the neighbourhood of Paposo and not also further to the north?

Herr Moreno firmly advised me against trying the land route to Mejillones, along which I would be in danger of dying of thirst together with my animals on the way, since he could obtain no reliable guide for me. The few watering spots which occur over this long stretch of 30 leagues at Agua Buena and La Chimba, lie not only off the route but significantly well away from it and high in the valleys. They are virtually impossible to find for those not conversant with them. Not long ago two Chileans from Cobija had attempted to take this route; they were soldiers in the Peruvian army, deserters and only wanted to go along the sea to their homeland. One of them succumbed to exhaustion and thirst half way along the route and his companion arrived in Cobre more dead than alive. Had he found no-one here, he too would not have lived to tell the tale. Herr Doll would not be deterred by this and would at least search over the 5 leagues as far as Agua Buena. After one and a half days he came back extremely exhausted and half dead of thirst, without having found it. He reported that the coast offered nothing of interest to him and specifically not a trace of vegetation. Under these circumstances I resolved to go on by ship again and to land only in the bay of La Chimba and in that of Mejillones, to look for deposits of Guano.

Herr Doll, who suffered severely from seasickness, decided to return to Paposo by land whilst I proceeded by sea. We had a fresh wind as far as the Isla Blanca, so called due to the layer of bird droppings which covers its surface, so it appears white. There was no trace of plants or insects there....On 29 December at 4 in the afternoon we lifted anchor to sail to the Bay of Mejillones. By dawn the following day we were already north of Morro Moreno and at 9 we turned the Punta de Angamos; right behind it on the shore we saw huts, men, and a boat; evidently people go there for Guano. A boat with four men rowed in front of us in the direction of Cobija....they informed us that they drew their water from a spring at the foot of Morro Moreno, some 12 hours away, but it was of poor quality. Their firewood was the timber from *Cereus* which they had to drag from a good two hours away on the crown of the Morro Mejillones. I went off in the longboat but the surf was so high on the shore that we did not risk a landing and returned to the ship.....

On 31 December I made an attempt to climb up the 2500ft high Morro Mejillones which lay about 2 hours away in a SSW direction. Few excursions have been so painful for me....On this day the sun was at its zenith, when the bare rock and rubble was almost burning hot. Not a patch of shade was thrown and not a breath of air; the time from 9 to 3 is unquestionably not suitable for making a fatiguing trek. More than once I was so exhausted that I wondered whether I could go on any further. And what was the result of the whole effort? Very little. For a time the track goes along the beach in a southerly direction. The strata is horizontal. Then the footpath climbed a steep rise parallel to the shore, over rubble which often gave way underfoot and rolled down to the ocean. One spot appeared to me so nasty that I did not dare cross it but preferred to climb a valley straight up to the top. Finally it brought me to a pretty broad, level ridge about 1500ft high which fell steeply to either side. There was not the slightest trace of a plant, nor insect, to be seen but thousands and thousands of empty shells of a land snail. What had devoured the snails? and where did they live?

On this ridge we followed the footsteps taken by the guano collectors on their expeditions to the peak of the Morro for fire wood. This peak rose up like a cone from a roughly 2000ft high level. At this height is some vegetation; here and there an *Echinocactus* is to be seen, a *Solanum*, a *Frankiana*, a *Dinemandra*, an *Alona* or *Tetragonia*, usually withered. The cactus, mostly the eleven-angled *Eulychnia breviflora* starts at this height and then becomes frequent right on the peak. At its foot I found an *Alstromeria* in flower, a remarkable picture in the harsh surroundings. I was astonished to find here many tracks and droppings of Guanacos. Is there more vegetation here in winter? I was too exhausted to climb up the peak itself and satisfied myself by going round to the east side of it. There I could overlook the whole of the sandy desert that stretched as far as Chimba, 22 sea miles long and 3 sea miles broad. Then I turned back. I satisfied myself that the plateau at some 1800 to 2000ft high, falling steeply to the west, which forms the coast from Chanaral, continues uninterrupted as far as Cobija....On 2 January we sailed away in the direction of Paposo.

..... from H. Middleditch

If all the travellers along the Atacama coast as well as the residents regularly used dead *Eulychnia* stems for firewood, and if R.Ferryman can still find dead specimens today at places on this same stretch of coast, then there must have been a tremendous number of dead *Eulychnias* there at the beginning of the 19th century. Bearing in mind both the dryness of the atmosphere and the previous absence of disturbance from external sources, these dead *Eulychnia* could have accumulated over many hundreds of years. Or even over thousands of years. The snail shells, and the guanaco tracks, could equally have been centuries old.

..... from John Ball Notes of a Naturalist in South America 1887

On the 5th May we were before Antofagasta. A gentleman who resided here for some time assured me that at intervals of five or six years a heavy fall of rain occurs here. At such times not only the coast region, but the Atacama desert lying between the coast and the cordillera is speedily covered with fresh vegetation, which after a few months dries up and disappears. At such times the Guanacos descend from the mountains and actually reach the coast.

..... from R. Ferryman

There are new mining developments afoot at El Cobre and to this end the road there from Paposo has been improved. It is now about the standard of an English B class road, so it took us little more than an hour to do the journey over this distance, this year.

GYMNOCALYCIUM PARAGUAYENSE - A GOOD SPECIES By Bohumil Schutz

Translated by E.W.Bentley from Friciana Rada VI June 1966

The discovery of Paraguayan cacti had its beginning towards the end of the previous century. Prof. Schumann published the first comprehensive account in M.f.K. 1899/1900. At that time Dr.David Anisits, Hermann Grosse, Dr.Lindemann, Dr.Malme and Dr.Hassler were active in Paraguay. The last-named came to Asuncion in the year 1885 and stayed there to his life's end. The botanical material collected on his expeditions was worked on in collaboration with Prof. Chodat of Geneva. Some genera were farmed out to specialists. One of these was Prof. Schumann.

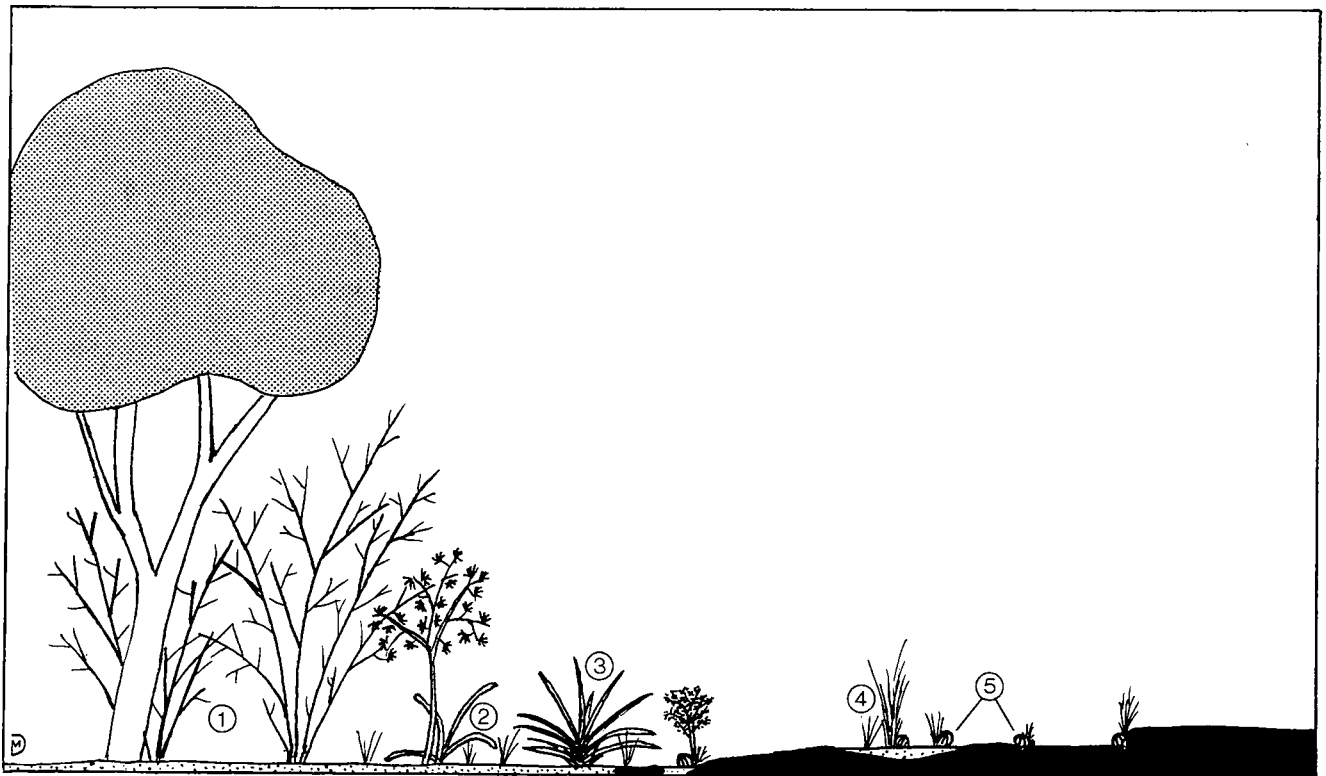
All the cacti found by Hassler between 1885 and 1896 were the subject of the above-mentioned studies in M.f.K. 1899/1900. Here he wrote about *Echinocactus denudatus*: "under the name of forms or varieties of *E.denudatus* we have received from Paraguay a large series of quite different forms. All differ distinctly from the type. None of the bodies showed the so characteristic and so strikingly beautiful form of the spider cactus with the thick, bloated, straight-running



Gymnocalycium fleischerianum

Photo - Buining

Succulenta 49.5.1970



Vegetation profile at Sandstone patches

1. Surrounding vegetation - Trees and bushes

2. Monvillea sp.

3. Bromelia

serra 4. Grasses 5. Gymnocalycium sp.

Black - solid sandstone Dotted - earth

D, METZING

ribs, few in number, and the closely adpressed spines. The special characteristics of these forms were:- the increased number of ribs, frequently in conjunction with a flattening of the body, which was even almost cake-like. Combined with the flattening was a more or less deep junction of the ribs. The spines were frequently directed upwards and occurred in great number. Ferd. Haage junior took this occasion, by combining the characteristics mentioned, to designate the resulting forms as varieties and described these. Another variety, distinguished by taller growth and almost sharp-edged ribs had already been named before this by Mundt."

By this latter is meant *E.denudatus* var. *golziana* Mundt. The description is included in the report of the nomenclature commission in M.f.K. 1897 p.169:- "The commission is of the opinion that in considering the plant, only the species *E.denudatus* Lk. & Otto is involved, but on account of the not so flat-domed, but fairly sharp ribs and on account of the not adpressed, curved, pale, but spreading, stiff, slightly bent, darker, spines, a good variety is represented for which Herr Mundt's name is kept in reserve".

In the supplement to the *Gesamtbeschreibung* for 1902 we find in the key to the sub-genus *Hybocactus*, under *Gymno. megalothelos*, this note: "here belong probably several of the forms itemised as varieties of *E.denudatus*." This is very important because here, for the first time the view is advanced that the Paraguayan plants do not belong to *E.denudatus*.

And then in 1903 in the M.f.K., in an article by E.Dams, there appeared the name of *E.paraguayensis* K.Sch.. After this we often read about this species. Prof. Schumann as editor would certainly have permitted no misuse of this name. In spite of this Britton & Rose stated that the plant was never described. Even Backeberg accepted this statement. Then a short time ago the *Bulletin de l'Herbier Boissier* came to light in the *Bibliothek des Botanischen Instituts Brunner Universitat*. There I looked for Dr.Hassler's work. But my surprise was great when I read the title of the section: *Cactaceae det. K.Schumann*.

This means that Prof. Schumann not only determined the cacti for Dr.Hassler but that he himself had worked on this section. There are to be found there several first descriptions by Prof. Schumann. Among them is, however, the naming of *E.paraguayensis* as a species in its own right. In a short Latin diagnosis it is mentioned as differing from *E.denudatus* in those of a flower with a purple throat. Other differences are in the report of the nomenclature commission, to which K.Schumann refers. One thing does not tally here, and specifically the naming of the variety. Since however page 169 of Vol.VII of the M.f.K. was mentioned, it is clear that here is meant the same plant that Mundt has called var. *golziana*.

The difference between *E.paraguayensis* and *E.denudatum* are so important that I concur with the view of Schumann that we have a good species. Older illustrations too, which are reproduced here [in *Friciana* - H.M.] as well as the illustration of two imports that A.V.Fric once brought back and which still live in our collections, confirm the assessment. Seedlings of Echs. (or now, following recombination) *Gymno. paraguayense* are easy to distinguish from those of *Gymno. denudatum*. They have a different body colour, angled ribs divided by cross-furrows and no spider-like spines. Areole wool is not yellow, but white and areoles soon become naked. The buds are reddish-brown, greenish in *denudatum*. Also seeds are considerably smaller and easy to differentiate.

... from H.Middleditch

It is quite clear from this article written by Dr.Schutz that he has been at some pains to search the literature in order to determine the origin of the name *Gymno. paraguayense*. However, there remains the implications of the "one thing that does not tally", i.e. the name *v.golzianus* given to this plant by Mundt. An examination of all the original references in the *Journal of the German Cactus Society* would appear to suggest that this may have some bearing upon the nomenclature.

The meeting of the German Cactus Society held in the December of 1896 was reported in the January issue of M.f.K.VII for 1897, as follows:- "Herr Walter Mundt tabled a very interesting plant which had reached him among others from Paraguay. In habit resembling the same as *Echs. denudatus* Lk.& Otto, but is distinguishable from it on account of a greater number of ribs (seven) and dark brown spines. A further feature of the plant produced, is the existence of central spines, which are absent with *Echs.denudatus* Lk.& Otto. The view was put forward that the name *Echinocactus megalothelos* Sencke Cat. ought to be correct. For comparison with this plant Herr Mundt showed a fine specimen of *Echs. denudatus* Lk.& Otto, a good 20cm in diameter [8 inches - H.M.]".

In the M.f.K. VII.11.1897 there appears on p.169 the report of the nomenclature commission, correctly reproduced by Schutz, but the paragraph which he quotes above is preceded in the original context by:- "In August of this year an *Echinocactus* was passed on to the nomenclature commission from Herr W.Mundt, that originated from a large batch of imported cacti from Paraguay, with the question, whether the commission could recognise it as a new species. There the plant displayed a flower bud, so it was transferred to the Royal Botanic Garden for cultivation; after it had unfolded, a verdict about it was possible."

Then in the M.f.K. VII.12.1897 there is a brief article written by W.Mundt entitled *Echs. denudatus v.golzianus*, which states "I have named this novelty after the distinguished cactus expert and owner of one of the largest cactus collections, Herr masterbuilder E.Golz in Schneidermuhl, about which the decision of the nomenclature commission has been reported in the November issue [i.e. M.f.K. VII.11.1897 - H.M.]. I received the specimens of this plant back in January, from Paraguay, and believed that I had found a hitherto unknown species among them. In order not to resurrect once again just a fairly well-known vintage species as a novelty, I left the aforesaid plant to the examination of the nomenclature commission in accordance with our rules. They have awarded only to me the authority for naming it. Since I first placed this plant as long ago as February in the hands of the Society for their evaluation, I have had some suggestions that I should relinquish this right, as various others would have wished"

As noted above, in the January 1897 issue of M.f.K, it is stated that a plant was tabled at the December meeting of the Society, having recently arrived from Paraguay; however, in the M.f.K. for December 1897 we find Mundt saying that the plant was received from Paraguay in January 1897. One may presume that this is a minor inaccuracy and that both notices refer to the same plant. Whilst the nomenclature commission state in M.f.K. VII.11.1897 that they had received the plant concerned from Mundt in August of that year, Mundt himself observes in M.f.K. VII.12.1897 that the plant was put into the "hands of the society" in February of that year. The cactus literature is rife with discrepancies of this nature; the only practical course is to accept them as discrepancies and presume that one and the same plant is involved in each case. There are two very important points which need to be highlighted. Firstly, a search of M.f.K. for the period up to August 1898 has revealed no mention of *Echinocactus denudatus v.paraguayensis* either in these quoted extracts or elsewhere in the text. Secondly, the plant in question was named *Echinocactus denudatus v.golzianus* in M.f.K. VII.12.1897.

Another article appears in M.f.K. VIII.9.1898 in which Karl Hirscht describes a holiday trek round various well-known public and private collections in Germany. After a visit to the Berlin Royal Botanic Gardens, followed by a call on

Herr Brunnow in Naumberg, he travels on to Dresden where he is joined by Dr. Schumann and Herr Schwarzbach. They travel by river steamer from Dresden to Copitz to visit Herr Bauer and "...soon we found ourselves right in Copitz. Hospitably Herr Bauer bade us welcome and gladly showed us those cacti he grew in the garden, in cold frames, and in the greenhouse. A large bed in the open air is planted out with *Echinopsis* and with *Echs. denudatus* Lk. & Otto v. *paraguayensis* Mundt. A splendid specimen of a pink flowering *Echinopsis* made a fine show embellished with numerous flowers and already shimmering on the arrival of the guests. In the cold frames were bedded out a great many plants of the sharp-ribbed *Echinocactus ottonis* Lem. v. *paraguayensis* F. Haage jnr., which were introduced in the previous year, which displayed astonishing floriferousness; also here had come developments such as fruits and flower remains which quite covered these plants. Equally numerous were the already known and likewise very readily flowering *Echinocactus denudatus* Lk. & Otto v. *paraguayensis* Mundt, before us in individual heads of extraordinary size. Then Herr Bauer showed us the variety of *Echinocactus pumilis* Lem. imported in the current season, which certainly on account of the divergent habit should preferably be considered a species of its own".

It seems that this is the first occasion on which the name *Echs. denudatus* v. *paraguayensis* appears in print. This name is attributed to Mundt, but it has not been possible to find a publication of this name by Mundt anywhere in the literature.

There is an account in M.f.K. X.8.1900 of the regular meeting of the German Cactus Society in Berlin, which notes: "from Herr Walter Mundt, Pankow, two of last year's seedlings which were overwintered in the house, which in spite of precious little care which they have endured during this season, had now attained such development that the small plant bodies were already able to form some buds. They are the charming and free-flowering paraguayen forms of *Echinocactus ottonis* Lk. & Otto and of *denudatus* Lk. & Otto."

In the same year we find Schumann describing quite a number of new discoveries from Paraguay and in M.f.K. X.12.1900 he notes, under *Echs. denudatus*, that "under the names of forms or varieties of *Echs. denudatus* we have received from Paraguay an extensive range of truly diverse habit. All diverge clearly from the Type. The particular characteristics of these forms are: the increase in the number of ribs, frequently in conjunction with a flattening of the body, that even becomes almost cake-like. Along with the flattening there appears a more or less deep separation of the ribs. The spines are frequently erect and occur in larger numbers. A further variety, distinguished by its taller growth and almost sharp-edged ribs, has already been named by Mundt."

In the context of this last sentence, Schumann probably had in mind *Echs. denudatus* variety *paraguayense*, which does not appear to have been published, either by Mundt or anyone else prior to this date. At this point it may be as well to occupy a moment reflecting upon Schumann's position in the cactus world in Germany at that time. He was Curator of the Royal Botanic Gardens at Dahlem, Berlin; he was Director of the German Cactus Society; he was Editor of their Journal, the *Monatsschrift für Kakteenkunde*; and although no membership list of the "nomenclature commission" has come to light, it does seem highly probable that Mr. Schumann and that body were effectively synonymous. The German Cactus Society Journals of the day do tend to convey the impression that Mr. Schumann improved upon Alice - what he said once was right. It is not too difficult to guess who was pressing Mundt to "...relinquish his right .. to name (his plant from Paraguay)" - M.f.K. VII.12.1897.

So if Schumann had made up his mind that this plant should be *Echs. denudatus* v. *paraguayensis* then as Editor of M.f.K. he would imprint his stamp on the identity of these plants seen during the course of the trip to Dresden and Copitz (M.f.K. VII.9. 1898) erroneously attributing the varietal name to Mundt. However, only *Echs. denudatus* v. *golzianus* was validly described by Mundt. In the 1903 *Bulletin de l'Herbier Boissier* Vol.3 (second series), Schumann described *Echs. paraguayensis* with the synonym *Echs. denudatus* v. *paraguayensis* Mundt M.f.K. VII. 169; Schumann thus fell into a trap of his own making. If he had referred back to page 169 of that Journal he would have found there no mention of that varietal name; as Schutz points out, it does not tally.

The I.C.B.N. article 33.2 states that "a new combination is valid only when its basionym, or, for new names, the name to be replaced, is clearly indicated and a full bibliographic reference is given". The basionym and the bibliographic reference quoted by Schumann viz "*Echinocactus denudatus* v. *paraguayensis* Mundt M.f.K. VII p.169" did not and does not exist, so that this basionym was and is invalid; if the ICBN requirement i.e. article 33.2 was in existence in 1903 then *Echinocactus paraguayense* Schumann 1903 would also become invalid. As the 1903 name was quoted by Schutz as the basionym for *Gymnocalycium paraguayense*, then this name in turn may also be invalid. It appears to have been the intention of Schumann to raise a variety to species level, in which case the name should have been *Gymnocalycium golzianum*! If it is accepted that the Schumann and Schutz names are invalid, then this plant is without a valid name. Or is it? The ICBN Rule requiring the basionym and bibliographic reference to be quoted in full was not in existence in 1903; so in that case, Schumann's *Echinocactus paraguayense* would stand as a new name.

..... from G.J.Swales

At the end of his article about *Gymnocalycium paraguayense*, Schutz says that the seeds of this species are smaller and easy to differentiate from *G. denudatum*. Now the seeds of *G. denudatum* are one of the larger seeds of this genus, measuring some 2mm or more across. The surface of the testa is almost smooth and a matt to slightly lustrous black in colour. When viewed sideways the body of the seed broadens out near the base with a sharp radius and the hilum end of the seed has a shallow vee shape. There is a small amount of variation between seeds but this can be expected since seeds of a species are not really all absolutely identical, but the features noted are quite consistent.

On the other hand the seeds of *G. paraguayense* are smaller, the surface of the testa is formed of prominent humps and it is a shiny black. However these seeds vary appreciably in size, in shape, and in prominence of the hilum margin. They are possibly the most variable sort of *Gymnocalycium* seed that I have come across. My seed samples have been obtained from a wide variety of sources, including many of the normal commercial suppliers and when I have opened a packet of seed of *G. paraguayense* purchased from say Uhlig or De Herdt, I could almost separate the contents into three different sorts.

In addition to seeds of *G. paraguayense* I have also acquired seeds under the name of *G. denudatum* v. *paraguayense*. The question here is - what really are these plants? The seed is smaller than normal seed of *G. denudatum*, usually it has little or no broadening at the hilum end, generally the seed is not symmetric i.e. the hilum is not quite basal, but the surface of the testa is pretty smooth and matt, like that of *G. denudatum*.

The seed of *G. paraguayense* is basically similar to that of *G. fleischerianum* and also of *G. chiquitanum*. Both *G. paraguayense* and *G. fleischerianum* come from the same patch of Paraguay and *G. chiquitanum* comes from S.E. Bolivia, so they are all at the north-eastern extremity of the area of distribution of *Gymnocalycium*. All these seeds have the shiny black tuberculate testa surface, they are almost symmetrical or have but a slightly oblique hilum, and have a nipped-in waist immediately above the hilum. The flowers on all these three sorts are white with a red throat.

ECHINOCACTUS PARAGUAYENSIS REDISCOVERED By Dr.B.Schutz
Translated by W.W.Atkinson from Succulenta 50.6.1971

The name *Echinocactus paraguayensis* K.Sch. first appeared in the cactus literature in *Monatsschrift f. Kakteenkunde* 1903 p.49. It is in an article about cacti in Paraguay by Dr.E.Hassler. Here we can read that this species grows exclusively in the Cerro de Acahay, and that one finds only a few other species there. ... I got hold of the original article by Hassler, which appeared in 1903 in a genuine scientific botanical journal "Bulletin de l'Herbier Boissier". To my amazement I read "Cactaceae det. K.Schumann". That is to say that Prof. Schumann not only identified the cacti from Paraguay for Dr.Hassler, but he himself had written the article. In amongst the new descriptions herein there appears: "*Echinocactus paraguayensis* K.Sch. = *Echinocactus denudatus* Lk & Otto v. *paraguayensis* Mundt M.f.K. VII 169. Petalis albis basi interna purpureis. Inter rupes Chololo in vallis fluminis Y-aca. Dec. n. 6693. Note: I now believe that this plant is a genuine species, distinguished from *Echinocactus denudatus* by the number of ribs, the spines, and the flower colour."

One thing does not seem to tally. In the part of M.f.K referred to, Mundt gives the varietal name *golziana*. Without any doubt Prof. Schumann was referring to just this variety. The plants were imported at the end of last century and the beginning of this, being offered and also illustrated as *Echinocactus denudatus* v.*paraguayensis*. The most recent habitat collected plants were brought back by A.V.Fric in 1929. One of these imports is still in my collection. Many generations of seedlings were grown by the well-known cactus nurseryman Z.Fleischer of Brunn. However, because Backeberg announced his opinion that these were hybrids, it became necessary to import new plants from Paraguay to clarify the point.

Knowing that my friend Gunther Moser was on good terms with A.M.Friedrich, I passed on the whole story with precise details of the habitat location as given by Hassler, together with the request to persuade Herr Friedrich to visit the spot. The request to search for these plants remained initially without result, but at last I received the welcome news from my friend Moser that on 18 May 1969 the plant had again been found in Chololo-i, precisely where the original plants had been collected. It is seemingly very rare and grows on the steep sides of a river valley. I have been able to study this plant in the collection of my friend Moser in Kufstein, and without any doubt it is the old *Gymnocalycium paraguayensis*.

A GRINGO ON THE HUNT FOR CACTI By W.Knoll
Translated by K.Wood-Allum from the G.O.K. Journal 1974

Shortly after I had come back from Cordoba, my uncle had to go to Asuncion, to seek out a doctor there, and I was able to accompany him. Whilst my uncle was making his enquiries, I took a stroll a little further through the city and came across a street whose name seemed somehow familiar to me. Suddenly I recalled that in this street dwelt the wellknown Paraguayan cactus collector Adolf M.Friedrich and by means of the telephone directory I then discovered his correct address.

On approaching Herr Friedrich, I was welcomed by him in person in a very friendly manner and first of all taken through his tropical garden. It would be foolish to enumerate all the genera and species that one could list which Herr Friedrich grew there, but it seems to me to be worthwhile mentioning an extremely comprehensive collection of orchids, in which Herr Friedrich is particularly interested. Naturally there is no lack of cacti from all parts of Paraguay and the motherplant of all the "screw-cacti" existing in the world left me green with envy.

In the course of a short conversation, Herr Friedrich declared himself ready to undertake a collecting trip in Paraguay, together with myself, provided I shared the problems of organisation and finance. Naturally I was highly delighted to accept. By good fortune my uncle had already been considering for some time that he would go on an expedition in to the Paraguayan Chaco and thus we resolved to undertake the trip together. Also my nephew Oscar was one more for the party and inside a week we had all our requirements collected together.

On the morning of a very beautiful August day we left Resistancia behind, reaching Asuncion towards noon and taking up our temporary quarters in an hotel. Around noon we drove off with Herr Friedrich towards the first site along a splendid motor road to the southeast. About 90 km away from Asuncion, we pulled up to look for cacti and right besides the route, between flat rocks, we then found *Frailea schilinskiana* (WO 71). The plants always grew singly in just a little quite red earth, now and again partially hidden under bunches of grass or drawn fairly well down into the ground. Whilst Herr Friedrich eagerly sought after specimens of this species, Oscar and I crossed over a small stream and climbed a little higher through low-growing thickets and over gravel-like fields. On a shady forest edge stood fine plants of *Notocactus ottonis* (WO 72); we found yet more of this species at other sites, but they proved to be different in habit and in their spination.

The forenoon of the following day was occupied with travel preliminaries and with minor repairs to the van. At noon we were on our way once again with Herr Friedrich, this time though on the route towards Encarnacion. In the neighbourhood of the small village of Chololo-Y we came to a halt. Herr Friedrich summoned a pair of children and showed them a specimen of collected cactus, upon which the children faithfully promised to collect some of them. We could surely count on the children probably bringing us *Frailea friedrichii* and *Notocactus ottonis*, but of the *Gymnocalycium denudatum* also existing somewhere in this neighbourhood we had faint hope.

After the children set away, we drove farther on towards Pirareta (= the spot where many fish are to be found). A glass-clear stream plunges downwards over boulders and the fish scurrying to and fro did credit to the place-name. On the edges of the stream stood dense subtropical forest, really much too humid there for cacti. However, barely had we scrambled a little higher up over a pair of flat rocks than we found the first plants of *Gymnocalycium fleischerianum* (WO75). I was astonished that this species can flourish here between rocky slabs and boulders in such a small amount of soil. On top of that it struck me that this species is so evidently variable in its spination, one could sometimes quite fairly distinguish three or four different species or varieties. In addition it happens that plants which are exposed to the full glare of the sun turn a violet brown colour. The afternoon passed away with zealous searches, repeatedly interrupted by exclamations of surprise from both my companions when they had once again found somewhere an especially handsomely spined specimen.

As darkness began to fall we left behind this so hospitable spot and made our way back on our return journey. In doing so we took a short cut over an earth track and in the process we had a close shave; in a quite inoffensive looking pothole the van sank so deep that even the headlamps were covered, and only with an exceptional effort did we get through. The three of us sat there in the van were up off our seats, no doubt somewhat anxious, but we still got a damp

backside.

However the surprises for this day were not at an end even yet; as we reached Chololo-Y, the children were already on the look-out for us with some well-filled sacks. And what came to light out of them on that day? In addition to fine plants of *Frailea friedrichii* (WO73) and *Notocactus ottonis* (WO91) there was also a real bumper quantity of the true *Gymnocalycium denudatum* (WO74), splendid examples and in many forms as well.

Perhaps I can discuss a few things about this species here and relate its history. About 70 years ago this species was discovered in Paraguay at an undisclosed site. The knowledge of the exact recorded site became lost and so up to 1972 it could not be collected again. Then Herr Friedrich came by chance towards Chololo-Y on one occasion and found this species here once again, though only a few plants and not particularly fine-looking ones at that. For the first time the children showed us a much bigger site, that we would certainly not have found if we had been on our own.

Rapidly we packed up the plants, gave the children a truly generous tip - they were as happy as a wren in January - and we set out on the last kilometers back to Asuncion. A good supper and a still better Paraguayan beer finished off this successful and memorable day. On the afternoon of the third day we really wanted a rest in readiness for our long drive into the Chaco, however Oscar and I sought out the habitat site of the *G.denudatum*, to take some photographs. The children came with us again, because by ourselves we would never have found the very well concealed spot in question.

..... from H.Middleditch

In the foregoing account by W.Knoll, read *G.paraguayense* for *G.denudatum*. It would be very nice to have it confirmed that WO 74 flowered with a wine-red throat.

..... from J.Lambert

My 1986 trip to Argentina commenced at the extreme north-east corner of that country, in the province of Misiones, right on the Brazilian border. Here we visited the waterfalls at Iguazu, from where we moved southwards along the main road to Posadas. Here we crossed the Rio Parana to Encarnacion on the Paraguayan bank and took the main road to Asuncion. Until we reach Villa Florida, the country remains humid and marshy, with only a few big cerei as far as cacti are concerned. After crossing the river Tehicuary, the landscape becomes somewhat drier, and just past Caapucu we find *Cereus xanthocarpus*. This species is well represented and I notice it farther along the road, near Quiindy and Paraguari. A somewhat different form thriving in the same biotype is Ritter's *Cereus lanosus*.

From Paraguari we made a detour to the right, in the direction of Piribeby. We pass Chololo-i and search vainly for *Gymnocalycium paraguayense*, mentioned by Moser from this locality. According to Arlzberger, however, the data concerned from Moser is inaccurate. Unfortunately the sun is already sinking when we finally reach Pirareta. This obliges us to make a choice between viewing the waterfalls or searching for cacti. Naturally we plump for the latter, the more so as this does not really amount to a waterfall after Iguazu!

We learn from an indian, with whom my Argentinian companion talks in guarani, that the cacti alongside the waterfalls had been completely eradicated by the tourists. However, he agrees to take us to the spot where an untouched population still thrives. To our delight, we discover here some *Gymnocalycium fleischerianum*, which we photograph in its natural state, and of which we cautiously collect a pair of offsets and a ripe fruit, without removing the mother plant. At this same spot we also find a *Monvillea rhodoleucantha*. Then it was on to Asuncion for the night.

..... from F.Vandenbroeck

During our trip to Paraguay we visited Chololo-i where we looked for *Gymnocalycium paraguayense* near the waterfall and on sloping rocky places away from the river, but we did not find any of these plants. Near the Pirareta waterfall, which had a similar landscape to that of Chololo-i, we did find *G.fleischerianum*. In nature this plant seems to be on the verge of extinction. Before my departure to Paraguay I was able to see the pictures which were taken by the late Mr. Buining of the habitat of this species. According to these pictures the plants must have been pretty numerous, some of them growing very close to the waters of the Rio Yhaca.

The banks of this river consist of gently sloping rocky terraces, sparsely covered with bushes and grasses, among which *Monvillea*, *Piptanthocereus*, and bromeliads, are to be found. Farther away from the river there is a dense bush forest interspersed with palm trees. We had been searching for hours upstream and downstream of the waterfall before finding a small group of *G. fleischerianum*. Later, nearer sunset, we were so fortunate as to find a small but intact population on a somewhat hidden spot further away from the river. Here the plants showed all forms and degrees of spination. In Succulenta for May 1972 Mr. Gunther Moser boasts of possessing over 190 imported plants of *G.fleischerianum*. This is deplorable because I doubt whether there are 200 plants left in nature. I fear the species has been decimated by overcollecting and also by local tourists who take the plants home as a souvenir, so I was told. The main threat to the habitat of the plant is the possible transformation of the area into a recreational park, which lack of money still prevents at the moment. Sadly enough this enchanting place is now almost devoid of this beautiful species, once occurring there in large numbers.

..... from J.Piltz

During his time in South America, Fric had searched the area around Encarnacion and met with a *Notocactus* which he named *Echinocactus multiflorus*. This was later transferred by Buining into *Notocactus ottonis* as var. *multiflorus*. We came across what we believe to be this same plant in the vicinity of Encarnacion, although it appears to us to be more likely related to *N.linkii* rather than to *N.ottonis*. Somewhat further along our route towards Asuncion we were in the area near Caacupe where we came across an orange flowering *Notocactus ?ottonis*. A plant with a flower of a similar colour is known from collections in Czechoslovakia.

Near Pirareta there are indeed no *Gymnocalycium* to be found in the immediate vicinity of the waterfall, but they do begin to appear at a distance of about 300m from the waterfall. There are even plans afoot to build weekend houses in the vicinity of the waterfall. The vegetation of the surrounding area has been modified by burning carried out to clear it for agriculture; some ground has been put to use for growing sugar cane and some for raising cattle. It is still possible to see many burnt plants. The secondary growth consists of grass tussocks, bromeliads, herbs, shrubs, and the occasional tall tree; it is quite probable that the original vegetation included many more tall trees.

Here and there occur patches of flat rocky outcrops, usually about the size of half a tennis court, with hollows and steps. This is where the *Gymnocalycium* grow, in crannies and hollows on these rocks where a little soil has lodged and around the edges of the rock hummock where the soil cover is shallow. Here we stood over some *Gymnocalycium* which did not seem to me to look quite like *G.fleischerianum* and I said to L.Bercht that I thought they might be *G.paraguayense*, but he in turn said they were not like the *G.paraguayense* that he had received from Arlzberger. But we were shown by Arlzberger two different populations of *G.paraguayense* which were not identical, from sites not a great distance apart. We also found a further population that even Arlzberger did not know about, where the plants were

growing in a coarse greyish-white sand at the edge of a flat rocky outcrop.

Not only do both *G.fleischerianum* and *G.paraguayense* vary in habit from one population to another, but at first glance there may not be any obvious differences between these two species. However, *paraguayense* does tend to grow flatter than *fleischerianum* and it always has a notch or cross-furrow immediately below the areole, which is never found in *fleischerianum*. Both species have flowers with the wine red throat but the outer petals of *paraguayense* are shorter and the flower does not open as widely as *fleischerianum*. There are occasionally female flowers to be found on *paraguayense*, but never on *fleischerianum*.

During the dark winter months we should have an opportunity to make a comparison of the seeds.

..... from D.Metzing

I would be inclined to agree that the notch in the ribs is the best character for distinguishing between *G.paraguayense* (M.35, M.40) and *G.fleischerianum* (M.4, M.9, M.32 & M.41). They were all on the flattish patches of rock, the *G.fleischerianum* being found on four different patches and the *G.paraguayense* on two other patches of rock. We never found the two sorts growing together at the one place but I never saw any particular ecological difference between the habitat of one sort and that of the other. The roots on *G.fleischerianum* were up to one metre in length. The flowers on these plants are white with a pinkish throat, pink filaments, and a fairly long tube. The flowers on the *G.paraguayense* had a shorter tube, with a red throat.

..... from H.Middleditch

A number of articles dealing with the *Gymnocalycium* of Uruguay, Paraguay, and Rio Grande do Sul, written by K.Prestle, appeared in the 1991 issues of *Succulenta*. This included a set of flower sections of *G.denudatum*, *G.fleischerianum*, and *G.paraguayense*. The flower section of *G.paraguayense* was of a longish tubed flower which lacked any sign of a red throat, which would suggest that it was not really a representative of this species.

From the comment (above) it appears that *G.fleischerianum* tends to grow somewhat taller than fat in comparison with *G.paraguayense*. Does this mean that the plant which was brought to the December 1896 meeting of the German Cactus Society which was "another variety, distinguished by the taller growth, which had already been named by Mundt" (i.e. *Echinocactus denudatus* v. *golzianus*) was what we now recognise as *G.fleischerianum*, whereas the plants with "flatter, cake-like" bodies also mentioned at the December 1896 German Cactus Society meeting are what we would now regard as *G.paraguayense*. Does this mean that there should be added to the name *Gymnocalycium fleischerianum* Backeberg *Die Cactaceae* III 1959, "synonym *Echinocactus denudatus* v. *golzianus* Mundt M.f.K. 12, 1897"?

THE ECOLOGY OF SOME GYMNOCALYCIUM HABITATS IN PARAGUAY By D.Metzing

From *Gymnocalycium* (Austria) 2(2) 1989

The distribution area of *Gymnocalycium* in eastern Paraguay is confined to the Cordillera de los Altos. This mountain area lies to the east of the Ypacaray trench and rises to some 400 m altitude. These mountains are formed of silurian sandstone, which weathers to a red earth. The climate of this area is subhumid, without any marked dry periods. Temperatures are markedly influenced by the wind direction: the north wind brings sultry warm air and through thunderstorms the greatest amount of rainfall. The south wind brings cooler dryer air masses. Dew also forms at night on account of the radiation cooling which is more pronounced in the Cordillera de los Altos than in Asuncion. The natural vegetation is semi evergreen forest, characterised by a partial leaf-fall during the winter months. Much of the natural forest has been cleared by burning for agriculture, for sugar plantations or cattle raising.

Within this area are to be found cacti from the genera *Gymnocalycium*, *Notocactus*, and *Frailea*. They mainly occur at places where the sandstone outcrops in the form of flat patches or even small plateaus. Here the predominant vegetation gives way to a plant association of xerophytic character which can endure the greater dryness, even though there is a relatively high rainfall of 1400 mm annually. The reason for this is that the soil at the edge of the sandstone patches is only very shallow and in consequence it dries out very rapidly. Still less favourable are the conditions in hollows in the surface of the stone into which some earth has collected. These spots are often completely lacking in vegetation because of the run-off or flooding resulting from the downpours. On this account they can only be colonised by plants which migrate from safer rooting places by means of runners, such as *Tradescantia*, *Selaginella* or the ground-lichen *Cladonia*.

Where the earth has collected in crevices in the stone, it is then colonised by perennial plants. These are grasses, Bromeliaceae and even small bushes in a moderate depth of crevice. On the edges of the patches of sandstone where the earth becomes deeper the vegetation steadily becomes higher, until finally it becomes dense thickets or forest. In this way the sandstone plates form dry islands in the middle of the subtropical forest, insofar as this has not already been destroyed. Outside of these rock patches the smaller growing cacti - *Gymnocalycium*, *Notocactus*, and *Frailea* - cannot compete with the natural vegetation. Whilst the macroclimate exhibits no arid period, there are arid periods throughout the year in the microclimate of rock patches. This is demonstrated by the existence of the xerophytic vegetation at these spots. A climatic gradient is presented by the ever lower growing vegetation inward from the margin of the rock flats. The incident light steadily increases from the edge to the centre, as far as the leaves of the plants are concerned. The moisture content of the ground is more constant at the margins on account of the vegetation covering and towards the centre fluctuates, up to extreme fluctuation. The water can stand for some time in the hollows in the flat sandstone; it cannot sink in and runs over the surface, so that the cacti can sometimes be standing in water. Not taken into account in the data for annual precipitation is the nightly dew.

The vegetation of the sandstone flats also bears the stamp of the influence of man. Where these habitat locations have been encroached upon by the fires set away for the purpose of putting land into cultivation, the perennial plants have been more or less damaged.

Whilst the forms of *Notocactus ottonis* v. *paraguayensis* are to be found only at the margins of the sandstone flats in the shelter of bushes, frequently at the base of termite mounds, the *Gymnocalycium* advance further on to the stone flats. Colonisation follows by preference the shelter of other plants, such as dwarf bushes, hummocks of moss, or grass, which afford favourable chances of survival for the young plants. Older plants of *G.fleischerianum* can stand all on their own, but their spination is much more robust.

Together with *G.fleischerianum* there is found *Frailea friedrichii*, *F.cataphracta* and *F.knippeliana*. The plants of *G.paraguayense* are to be found more in the shade, under bushes, and they tend to draw down into the ground to a greater degree during the winter months.

PARODIAS - HIGHLAND PLANTS? From G.Hole

My conversation with J.Piltz has left me with the impression that Parodias are to be found at fairly high elevations. Whatever there may be in the way of trees or bushes in the valley bottoms or on the valley sides, it seems that you have to climb up until this sort of vegetation has pretty well disappeared before the Parodias begin to appear. In particular I remember him telling me about one such climb, which finally reached a more level patch of ground above the bush zone, which was liberally scattered with rocks. Piltz said that there were so many Parodias occupying the patches between the rocks that he could hardly find a place to put his feet down without treading on these plants.

..... from W.Verheulpen

It is rather inappropriate to categorize all Parodias as high mountain dwelling plants. The plants of this genus occur over an enormously wide ranging area that embraces all kinds of soil and habitat conditions, at altitudes ranging from 500m to almost 4000m.

..... from H.Middleditch

In describing the surroundings of Alemania, both J.Lambert and F.Vandenbroeck mention that they found Parodia growing in association with Echinopsis and Trichocereus, all amongst bushes which stood a good two metres high. This might suggest that Parodias may not be confined to those higher altitudes where grasses are sparse and bushes few and far between.

..... from J.Lambert

Parodia tuberculosi-costata was found near Alemania on a stony slope (schists) with brown earth, amidst other cacti (Gymnocalycium, Cleistocactus, Opuntia...) and bushes. Some Parodias do indeed prefer more steepish biotypes or even cliffs, but this is not an absolute rule. There are indeed a number of places where the Parodias grow above the zone of woodland, which one has first to cross before getting to them.

..... from H.Middleditch

But I see from my field number list of B numbers that one or two Parodias appear to grow at relatively low altitudes, not far above the level of the Chaco plain. These are B24 P.macrancistra at 700 m., B67 P.sanagasta at 800 m., and B155 P.microsperma at 600 m. There is also Lau 559 from Tala, prov. Tucuman, which appears to lie at an equally modest altitude. Certainly the great majority of Parodias do appear to grow at much higher elevations in comparison with these particular sorts, which not only seem to be members of the group of Parodia with small seeds, but may all be variations on P.microsperma.

LAU PARODIA By W.Weskamp

Translated from K.u.a.S. 27.5.1976

Out of the 30 smooth glossy-seeded species of the sub-genus Parodia known today, only four have straight central spines. All the others always have one central spine more or less hooked to curved at the tip. These four are P.malyana, from Ancasti; P.uhligiana, R.20 from between Cachi and Cachipampa and Lau 568 from near Amblayo; P.fechseri of which Lau 458 comes from south of Cachi; and P.rigidispina. In 1947 Krainz published P.rigidispina in "Neue und seltene Sukkulenten". The species has been collected again in the meantime and very probably by Fechsner. They were distributed by the firm SPI under the name P.fechseri, without providing any data on habitat location.

Now we have an interesting discovery which in my opinion belongs unequivocally to the aforementioned forms. Lau made this find near Antilla in the extreme southeast of province Salta at about 500 m altitude. The Parodia Lau 576 grows completely isolated on the peak of a small hill which rises here about 100 m above the plain. Lau had to open a way from the road to the place with a machete. The find is interesting on this account, because Antilla is over 200 km as the crow flies from Cachi. In the meantime Lau 576 has been described as Parodia campestre by Brandt in the Danish magazine Kaktus 10 No.3, 1975 pp.61-64.

..... from H.Middleditch

Does this mean that we have an isolated example of a Parodia which is not within the microsperma species complex, but which grows at one of the lowest altitudes at which any Parodia has been found growing?

..... from J.Lambert

On my 1988 trip to Argentina I twice tried vainly to get to Antilla. I started first from Rosario de la Frontera, and subsequently from Tucuman, but had to turn back each time because the road was cut off by water, due to heavy rains. But in 1990 I did succeed in getting to Antilla. But as far as any Rebutia or Parodia campestre are concerned, I might just as well have spared myself the trouble! The general appearance of the area is rather flat and bushy, where cacti are represented by Eriocereus tortuosus, Cleistocactus, and Opuntia.

PARODIA COLLECTED BY J. & B.PILTZ IN NORTH ARGENTINA BETWEEN 1976 AND 1980

By W.Weskamp

Translated from K.u.a.S 32.8.1981

In the vicinity of El Cadillal dam in Tucuman province, which lies in the eastern outliers of the Andes and where the altitude has dropped to 600-700 m, Frau Piltz who was searching for Tillandsias in the middle of an extensive deciduous woodland quite unexpectedly found a population of Parodias P.150. Such a location has not recurred again. But some years ago Rausch wrote to me that Parodia saint-pieana grew near Valle Grande (Prov.Jujuy) within a forested area, and he also enclosed a photograph. Similarly on rocky cliffs at the Cadillal reservoir Lau found a Parodia Lau 437 which differed somewhat from P.150. Once again P.150 was then found to the south of Vipos - P.151.

These Parodia are in my opinion only forms of Parodia talaensis Brandt Lau 559. At this point I must make a small diversion in order to be explicit. As is well known the first Parodia was described by Weber as Echinocactus microspermus and that was 85 years ago. The description itself is inadequate and indeed no-one would be able to determine the species from it. Schumann first gave an exact description of the Weber plants, especially of the flowers. Weber had received the plants from Schickendantz who at that time was a lecturer in Tucuman, but without precise data on habitat location. According to that, these plants had small yellow scales on the tube and fire coloured (orange red) outer petals, golden yellow to orange inner petals. An outstanding colour!

Some years later Schumann described an imported plant which the firm of De Laet (Belgium) had introduced as Echinocactus microspermus v. macrancistrus, of which no-one knows who collected it and what its origins were. The

flower tube of this variety has long lilac-pink scales and this colour is often carried on to the tips of the always yellow flower petals; we misled everyone about this variety and place it mistakenly in the all-embracing form-circle of *Parodia microsperma*. Within that is *P.talaensis*, *P.thionantha*, *P.campestre*, the undescribed *Parodia* sp. from Trancas (which Ritter considers to be the type species), the *Parodia* sp. from Tucuman or the *Parodia* Lau 443 from Sierra Medina. For the most part resident in Tucuman province, they all belong in the form-circle around *Parodia microsperma* v. *macrancistra* (K.Sch)Borg, which to me would stand as a species in its own right, namely as *Parodia macrancistra* n.n. But *Parodia microsperma*, which nobody knows any longer, may possibly have been long since drowned in one of the many reservoirs constructed at a later period, or obliterated as a sacrifice to road construction or some other action of civilisation.

We find a continuation of the form circle of *Parodia macrancistra* farther on to the north, specifically near Lumbra on the banks of the Rio Juramento in province Salta. The P.153 found here is *Parodia setifera*, whose habitat location was for so long unknown to us. It has more or less twice as many radial spines as the others, but central spines similarly variable in length and similar flowers. Unfortunately it is not to be denied that Ritter's *P.uebelmanniana* comes from the same location and is thus identical with *P.setifera*. There is no doubt here.

On a return trip in 1980 the *Parodia* P.235 which is completely unknown to me was found at 1100 m on the east side of the Andes, a little way south of Salta City. The central spines up to 30 mm long are brown to dull black in the crown, later becoming almost white with darker straight to weakly curved tips. They are nice plants. The *Parodia* P.247 occurs near Trancas in the south of Salta province and from its habit it is the Brandt described *P.weberiana*, for which he quoted no habitat location. He gave yellow and orange as flower colours for it, so that for the reds of *Parodia* P.247 no contradiction is required.

..... from A.Wheatcroft

I have looked and better looked at this last phrase written in German in the original article, and find it impossible to make a satisfactory translation into English. Several of my colleagues who are fluent in German are likewise unable to make sense out of it.

..... from H.Middleditch

In his 1976 commentary Weskamp produced a collective description for all those *Parodias* of the *microspermae* group which possessed straight spines. This included Lau 576 described by Brandt as *P.campestre*. In 1981 Weskamp says that *P.campestre* belongs to the species complex round *P.macrancistra* which, as the name suggests, has a prominent and hooked central spine. There seems to be some conflict here.

..... from G.Hole

In my own collection I have plants of both *microsperma* and *macrancistra*, as well as seedling plants (about two inches in diameter) of *talaensis*, *campestre*, P.150 and P.151, *setifera* and *uebelmanniana*. At this age these plants all look very much alike; it is quite possible that there may be differences in the radial spine count, but because of the number of radial spines, the way they intermingle with those from adjacent areoles, and the white woolly hair, it is virtually impossible to make a count of the radial spines. The only way to do it accurately is to remove one areole from the plant, and I am not much inclined to do that. All of them have four central spines which are roughly similar in disposition, colour, and length. There is a slight difference in the habit of the central spines - on some plants they lie in among the radials, on others they stand out more, away from the radials. On *Parodia campestre* the central spines are rather long and straight; some of the central spines do have a slight kink at the end but my plants of this name are without hooked centrals. If the hooked centrals are to be considered as a feature of the *macrancistra* group then I would not see *campestre* falling within this particular group.

There is also some difference in the body shapes. Firstly I would be inclined to describe *setifera* and *uebelmanniana* as cheese shaped, or depressed globular - they are most unlikely to grow into elongated plants. By comparison both P.150 and P.151 are fairly globular and the spination does not stand away from the body; I gather that Piltz' view is that P.150 and 151 represent the original *P.macrancistra*, from Vipos. On the other hand my own mature plants of *talaensis* and *campestre* are now about six inches tall and some two inches in diameter and I regard this as a typical growth form for these two species. This characteristic shows up at quite an early stage - my young seedlings of *P.campestre* are larger than the others of the same age in this group. There are no obvious differences in the size, shape, and colour of the flowers throughout this group although the wool on the flower tube of *P.macrancistra* is darker.

For practical purposes all these plants are solitary, whereas the *Parodia* from the *Microsperma* group which grow much further south near Sanagasta, on the slopes of the Sierra Velasco, are clumping plants.

..... from F.H.Brandt

Parodia weberiana. A species which produces very variable flowers, from golden yellow to a reddish orange colour. The spination is also very variable, from pale horn-coloured central spines up to clear brown, but there are always only four central spines of which however 1 to 3 can be hooked. The plants raised from seed obtained from Uhlig under the description *sp.nov.* *Higueritas* are in all respects like the Type of this species with their variable flower colours. The location was kept secret by the collector, but the description of the seed as "*Higueritas*" probably refers to the finding place of the seed, also hence of the species. I found a place of this name in Prov. Catamarca in the vicinity of the town of Tinogasta. Consequently the habitat locality for *P.weberiana* is in the vicinity of Tinogasta and south from there, by the place *Higueritas*, prov. Catamarca.

..... from H.Middleditch

Weskamp states that Brandt quoted no habitat location for his *P.weberiana*, but the foregoing information came directly from Brandt himself, before the publication of Weskamp's article. However, my own map of Catamarca province does not mark any place by the name of *Higueritas* near Tinogasta. Bearing in mind that *P.weberiana* appears to belong to the group round *P. microsperma*, a location to the north or north-west of Tucuman city would seem to be more probable. Indeed Weskamp says that *Parodia* P.247 from near Trancas has the habit of *P.weberiana*. Running out of the Cumbres Calchaquies and down past Trancas is the Rio Acequiones (see map Chileans '46 p.34) whose upper reaches accept the waters of the Rio *Higuerita*. The junction of the two rivers is no great distance off the road from Trancas to San Pedro de Colalao. It would seem to me that this is more likely to be the *Higueritas* quoted by Brandt as the location of the Uhlig seed.

..... from J.Lambert

I did collect what I consider as the original form of *Parodia microsperma* in the region of Las Tacanas and Gonzalo, prov. Tucuman, which lie not far from San Pedro de Colalao. I expect an article about this to appear in *Succulenta* in due course. I did not collect any *P.macrancistra* in the localities mentioned in the literature for this species, but I did find a

population near El Tala, which one would expect to be *P.talaensis*. But in my opinion it is only a form of *P.macrancistra* - which, in turn, may only be a variety of *P.microsperma*

PARODIA MICROSPERMA "REDISCOVERED" By J.Lambert

Translated by H.Middleditch from Succulenta 68.7/8.1989

The importance of *Parodia microsperma* lies partially in the fact that it is the Type species of the genus. Indeed the very first *Parodia* to be described was *Echinocactus microspermus* Weber, published in 1896. At that time the name of the genus did not exist, but it was erected by Spegazzini in 1923. In time it became evident that the name of the species had been particularly fortunately selected, because really the majority of *Parodias* (the whole of the subgenus *Parodia*) was characterised by the small dimensions of the seed. At this point we will stop with the history of the species itself. It was found by Schickendantz, a teacher in Tucuman, who was however more specialised in minerology, and sent the plants to Weber in Paris for him to study. The precise origin was not disclosed by Weber, although possibly it could be found in the correspondence between Schickendantz and Weber, but nothing has been made public about this.

Because the diagnosis by Weber was rather brief, a more detailed description was made by Schumann in 1902. Whereas Weber really laid stress on the changing colours of the flower (see below), Schumann says nothing about this, from which it may be presumed that his description was based upon a limited number of plants.

After the first imports at the end of the nineteenth century, it seems that the species has not been collected again. Already in 1902 Schumann writes that it has become scarce and years later Backeberg was obliged to state that according to what was said it had vanished out of collections. In his book about the genus *Parodia*, Weskamp himself goes so far as to suppose that the species had been exterminated in the wild. We find the one and only tip about their origin in the jottings of Fric who mentions that the species occurs in the vicinity of Hualinchay (formerly Quelonchoi). Herzog, who visited this place, found only a *Rebutia* species there.

On our last trip to Argentina we had among other objectives resolved to investigate this particular matter a little more closely and so we went off to the stretch around Hualinchay. The first thing to be established was that it was not surprising that Herzog had only met with a *Rebutia* at this place, because the hills around Hualinchay and also the trails that lead up to this place, are evidently much too damp to accommodate *Parodias*, even in the uppermost spots.

A distribution area is seldom really restricted to the immediate surroundings of a well-defined spot, and so we looked for this plant somewhat further on. This turned out to be a good move, indeed it was not long before we discovered a first population of the species, JL-252, in the hills above Las Tacanas. This place lies along the road from San Pedro de Colalao to Gonzalo, but in fact at not much more than 10 km from Hualinchay as the crow flies. The plants grow there on open places between the bushes, in very little earth on flaking stones, at an altitude of 1200m.

What we were immediately struck by were the remarkable conical tubercles and the relatively short central spines, two features which were also quoted in the description by Schumann. The plants were not quite in flower, but from some buds one could deduce that the flowers should be fiery-red, at least on the exterior. Meanwhile darkness was drawing in and we turned back towards San Pedro de Colalao to stop there overnight. The following day we went further up the road to Gonzalo and once again we had some good luck.

A second population (JL 254) awaited us just before the Rio Rearte at 1300 m altitude, this time on a rocky slope, on which however humus-rich earth appeared to be present. Various plants stood here in full bloom and we were able to establish that next to the red flowers, orange ones also occurred.

The climax was however still to come; along the road from Gonzalo towards Choromoro, close to La Higuera, we found a third and last population (JL 254b) on a steep rocky cliff. A real display of fireworks! The flowers of the very numerous plants displayed all possible colour combinations of red to yellow, through orange. Thus we noted among others, red flowers with a greenish tube, red scales, yellow throat, and red filaments with yellow bases; yellow flowers with pale yellow tube, light reddish scales and yellow filaments; yellow flowers with pink tube and red filaments; and - what have you! Thus Weber was perfectly right when he spoke of the changing colour of the flowers.

[A full description follows of a plant with an "average" orange-red flower] As it may be seen there exists - apart from certain colour variations of the flower - a remarkable agreement between this description and that of Schumann, so that we were not in any doubt that we had really and truly found the original form there. The sole difference worthy of mention lies in the measurement of the seed, but in our opinion the Schumann text must hide a misprint here. Indeed he first writes that the seed was extraordinarily small, then after that giving a total length (with strophiole) of 1.5 mm. This is self-contradictory and in fact it ought to have been 0.5 mm.

Now something about the possible affinities of *P.microsperma* with other species, so we also look at at which of the forms are to be met with in the surrounding area. To the west, at about 40 km as the crow flies, one finds *P.rigida*. When we make a comparison between this form and a yellow-flowering *P.microsperma*, the differences are few. In fact the uppermost woolly areoles and the short central spines tend to recall the image of *P.microsperma* rather than that of *P.catamarcensis*, with which Weskamp considers *P.rigida* as a close relation.

This immediately brings up the ideas from Kiesling, who maintains that all small *Parodias* from the region of Cafayate are only forms of *P.microsperma* (including *P.tuberculi-costata*, etc. ...). Without going so far, Dietrich Herzog considers on his side that too many names have been erected too quickly, and that one ought to reckon with a greater degree of variability within any given species.

In the north we find on one hand *P.talaensis* (which I would feel is only a form of *P.macrancistra*) and on the other hand *P.weberiana*. With this last species Brandt lays stress upon the very variable colour of the flower.

In the east lies the distribution area of *P.macrancistra*. This species was first described by Schumann in 1902 as a variety of *P.microsperma*. Accepting that the central spines are longer and the flower size larger, but are these features now only varietal rather than good enough for the basis of a species? The fact that the scales on the flower tube are red and not yellow, as quoted by Weskamp, is not a decisive argument, since we have seen that with *P.microsperma* the scales can also display various colours.

From the immediate surroundings to the south there are no *Parodias* known, but from 25 to 30 km away as the crow flies one arrives in the area around Tafi del Valle, from where we know of *P.tafiensis* and somewhat more to the north-west, *P.spaniosa*. These species grow, however, under quite different circumstances in the high mountains, and display (especially *P.tafiensis*) clearly distinguishing i.e. "good" characters which make them into valid species.

If we go on here somewhat lengthily over the possible affinities between other named species and *P.microsperma*, it is not just to raise the doubts of the cactophiles, but because this in fact forms part of a much more general problem. It is

so that various authors take up very divergent standpoints in regard to the *Parodias* from Argentina. In a book which is expected to be published shortly, Kiesling declares that in his view only 8 valid species of *Parodia* would exist in his country. If we were to take the monograph by Weskamp into our hands, even after leaving out synonyms and dubious species, we would have some fifty species. As always we try to take up neither the standpoint of a splitter nor that of a lumpner, but as far as possible to look at the facts in the wild and in cultivation. Probably the reality lies somewhere between the two extreme figures that we quote here, but it will still demand quite a lot of work and study, before we get a clear and (something like) indisputable judgement on this matter.

..... from Editor, Succulenta

With this article the author had sent to me four slides as illustrations of the colour differences among the flowers. These slides complemented the article very well. To process the slides for lithographing they were sent up to a photographer. Alas they did not arrive there. If the slides are eventually found again, they will be inserted

HAS PARODIA MICROSPERMA REALLY BEEN REDISCOVERED? By W.Weskamp.

Translated by H.Middleditch from Succulenta 68:12.1989

The habitat location for *P.microsperma* which has not been known for 95 years could at last have been found by the Belgian field-investigator J.Lambert. In this journal he mentions three populations. It is interesting that these populations lay not very far from the village of Trancas. In the vicinity of this village Ritter found plants with which he also claimed that he had made a rediscovery of the species.

In particular J.Lambert found the flower colours of importance in coming to the conviction that he had found the Type species of the genus *Parodia*. Indeed the interpretation of the flower colour is the decisive factor in the determination of the species. According to Weber, the original author, the colours change from yellow and golden yellow to orange. Because the rules require, without exception, that all parts of a newly described taxon come from one specimen, the so-called holotype, the stated colour must be based upon one flower. Thus we are concerned here with one flower that is orange at the base, then goes over into yellow-orange to yellow at the margin, like a flame. This interpretation entirely agrees with Schumann's description, which indicates that the outermost petals of a fire-colour, with a somewhat darker tinted middle stripe (orange-red, orange-yellow). Amongst others, *P.spegazziniana* Brandt flowers in this way. We can leave out of consideration that Backeberg in his *Kakteenlexikon* and Haage in his *Kakteen A-Z* make *Parodia microsperma* flower yellow.

When we start from my determination of the flower colour, then the *Parodia* found by J.Lambert on a rocky cliff by La Higuera, on the road from Gonzalo to Choromoro, is not the species sensu Weber. Because on the one hand these *Parodias* had no fire-like flowers, but each of them had yellow or red flowers; on the other hand the red scales on the receptacles of these *Parodia* JL 254b ought to be yellowish, according to Schumann.

As the *Parodias* JL-254a, growing in the vicinity of the Rio Rearte, had greenish-yellow scales according to J.Lambert, then this proves that different forms occur at the two growing places, which are not the same. Apart from that, I completely exclude the fire-red (in contradistinction to fire-colour) flowers as mentioned for the *Parodia* JL-252 from Las Tacanas and the JL-254a already mentioned.

I also do not believe that, at the end of the previous century and prior to that time, Schickendantz would have made such laborious trips to the district visited by J.Lambert, the more so as *Parodia* populations also occur closer to his Tucuman home.

From my point of view I maintain that *Parodia microsperma* no longer exists. To conclude I still ought to add that according to Schumann the innermost flower petals of *P.microsperma* pass from golden yellow into orange, and are thus bi-coloured.

..... Reaction from J.Lambert [Also in Succulenta 68.12.89]

The argument by W.Weskamp that the different colours ought to be present in one and the same flower does not appear to us to be a deciding factor. As we wrote, with the population JL-254b we could observe the most divergent colour combinations, whereby a principle such as that quoted by W.Weskamp certainly is not excluded. Also the colour of the scales on the flower tube proved to be variable, that is to say now a greenish yellow, then again more reddish tinted. It is especially regrettable that a number of slides went missing, since these really would be able to put the matter quite explicitly.

..... from H.Middleditch

There appears to be a useful pointer to the routes which Schickendantz might have followed in riding to and from Tucuman, in the earlier account of the visit to Tucuman undertaken by Burmeister.

TRAVELS THROUGH SOME NORTHERN PROVINCES OF THE LA PLATA STATES By H.Burmeister.

Translated by H.Middleditch from Neumann's Zeitschrift fir Allgemeine Erdkunde Vol IX 1860

[En route from Cordoba to Tucuman] ...25 July. We left Tres Pozos and soon we saw the mountain ridge in the far distance. As the Sierras came nearer, we left the undeveloped Camp and found ourselves riding between numerous settlements, from where we could still not get a good view of the Sierra or of the city Ten minutes from the Quinta the road curved to the west and ascended a low mound which formed the eastern bank of the river. From there we had a fine view of the city, the Sierra behind it, the wide plain in front, with the river in the foreground. We crossed without difficulty to the far bank. The city looked very smart with orange trees in all the courtyards and gardens. ... Behind the city rose the Sierra, thickly wooded.

The northern part of the province is mountainous and completely lacking in extensive plains. The south-eastern part is an un interrupted plain, which is traversed from north to south by the Rio Tala and watered by numerous tributaries descending from the Sierra Aconquija.

From the market place, or any main street running east and west, may be seen the central part of the lengthy mountain system with its huge backbone of jagged peaks which rise above the smaller parallel range that lies to the east of it and which advances to within three leagues of the city. ... It was over this Sierra that I rode, to the Estancia San Xavier on the far side. The first part of the ride was over the plain, through Buena Yerba. From Servir Peridondo to the foot of the Sierra, the plain was covered with open bushes, occasionally interrupted by green patches of turf. Immediately

at the foot of the Sierra, even on the plain itself, the tall, dense, forest commenced. It is a very imposing growth with trees having 4 to 5 feet thick trunks, branching at 10 to 12 feet height and carrying their branched canopy above that. On the high branches can be seen a very slender leaved plant that is probably a *Tillandsia*. Various bushes form an undergrowth beneath the laurels, in which ferns frequently grow. Thin climbing plants trail from bush to bush.

At the foot of the mountains the forest runs out into the plain as long as it has a steady fall. This stretch is called the Falda. As one climbed higher the laurels became left behind and another, more open, forest took their place. This difference may already be observed at a distance from the Sierra, largely due to the different colours of the two forest zones. In this more open forest the trees are shorter, the trunks less stout, the canopy is more open and uneven.

As long as we rode through dense forest, little of the bedrock could be seen, as it was covered with a productive layer of soil. Towards the crown of the ridge, where the fairly steep road has been made passable artificially, it cut into the bedrock and thereby exposed the flaky, fairly soft, micaceous schist, whose bedding plane sloped towards the west to Aconquija and likewise extended from SW to NE. The bedrock is fairly soft so that it weathers easily on exposure to the air into fine gravel and sand, which covers the track. Its ingredients are fine-grained and on the whole give almost the appearance of a mica-rich, iron-shot sandstone of yellowish brown colour. The whole of the Sierra Tucuman appears to consist of this material. As the crown of the ridge came closer, the forest faded away entirely and left the uppermost crown bare. The ground here was covered with tall grasses giving it a Pampas-like impression. The pass was over the lowest spot on the ridge, where we rested for a while. The temperature required here to boil water was 77.2° which amounts to a height of 3662 feet, whereas Tucuman lies at 2208 feet or 750 m.

After a short stop we rode further over the undulating crown of the Cuesta and after a quarter of an hour we came to the other, western, slope, which falls into the valley of St. Xavier. We saw the estancia lying at our feet. The valley is very largely bare, with occasional open thickets, which were reminiscent of the upper forest of the Sierra. The remainder was covered with high grasses just like the Cuesta, which makes very valuable animal fodder. The occupier sells an excellent cheese under the name of Tafi cheese. The place after which this very good cheese is named lies 12 leagues from here in a similar treeless valley through which a small river flows. The road to Tafi passes through St. Xavier and crosses two Cuestas on the way to that place. I have not taken that route myself, but it was described to me by those who have travelled it a great many times as rather difficult. The road from Tucuman to Cafayate and San Carlos also goes through Tafi. From Tafi one rides north in its river valley and then crosses the pass to descend into the valley in which lies Santa Maria and Tolombon.

In the valley of St. Xavier there is a small stream which runs southwards to join the R. Lueles. The sides consisted of metamorphic rocks, whose debris filled the bed of the stream with small boulders, but which did not outcrop on account of the deep loamy covering of the valley sides. Here my brief excursion came to an end and we turned round to go back to Tucuman.

..... from H. Middleditch

It transpires that Schickendantz also made use of this same route from Tucuman to Tafi del Valle which is described by Burmeister. In the following document there is a pretty comprehensive account of the horseback journies undertaken by Friedrich Schickendantz from Andalgala via Santa Maria to Tucuman, from Andalgala via Campo Pucara to Tucuman, from Andalgala to Catamarca and return by another route, also from Andalgala via Belen to Tinogasta and back to Alpasinche. By comparison with these extensive journies, going up to Hualinchay from Tucuman is hardly more than an easy day's ride. So that when Weskamp expresses doubts whether "Schickendantz would have made such laborious trips to the district visited by J. Lambert" I feel obliged to assume that he has made this statement without the encumbrance of facts. If Weskamp makes a habit of this sort of thing it may be advisable to carry out a check on the other statements he makes, too.

PHYSICAL-GEOGRAPHIC SKETCH OF THE NORTH-WESTERN PARTS OF THE ARGENTINIAN PROVINCES OF TUCUMAN AND CATAMARCA. By H. Burmeister.

Translated by H. Middleditch from Petermann's *Geographische Mittheilungen* 1868 Nos. 2, 4, & 6

..... from H. Burmeister

Herr Schickendantz who is at present an official at a foundry works at Pilciao, 4 leagues south of Andalgala, has traversed the whole terrain from there in different directions.

..... from F. Schickendantz

From Tafi there are three routes to Tucuman -

1. Over the Cienega and then the following cuesta to San Xavier.
2. Over the Tablas, that is the grass-rich plateau south of the Cienega to the Quebrada de Lueles and Lueles itself.
3. Through the Quebrada del Portrero negro, which comes out near Fronterita, over Carpinchanco to Tucuman.

I am familiar only with the first route; the others should both be more convenient, but are considerably longer.

Climbing out of Tafi, going towards the east, the route goes up a steep and high slope which is slippery and hence dangerous after rain. Arriving at the top there is a grassy plain sloping away gently to the southeast, over which the trail goes in the same direction towards Cienega, an Estancia of J. M. Teran from Tucuman. Here there begins a precipitous wild-looking valley which receives its waters from the surrounding high mountain peaks, which runs to the south flowing past Anfama down below and forms the start of the Rio Lueles. A perilous path often only a handspan wide pulls out of Cienega towards the summit of the facing mountain ridge and from there descends still more steeply, but less rocky, to the other side of the valley where the hamlet of Anfama lies. On this section I have seen for the first time the gnarled, rose-leaved Quinoa tree. The pumas are abundant in this area.

From Anfama a further mountain is ascended, to pass over it by the dizzy Cuesta de Junta, which is a very awkward piece of work, especially downhill. Coming to the bottom there is an Estancia of the same name, 2 leagues from Anfama, where the Rio de Anfama joins up with the Rio de Juntas. If it is not running too full, one may reach Siambon in a short space of time by riding in the bed of the combined rivers, but if not, one has then to climb up a steep mountain ridge and only reach Siambon after a long laborious ride. At the latter place the most significant heights of the route lie behind and the warmer climate of the plain of Tucuman already makes itself perceptible. The ride becomes more pleasant, since the track goes forward less steeply and the area round about is more delightful. By the Portrillos, an Estancia of the family Frias, I have seen the orchids in full flower which you [Burmeister] mentioned in the account of your journey when riding from San Xavier; they are yellow coloured. With San Xavier there is a magnificent view from the first of the wooded

forehills over the Estancias of the broad, boundless plain to the east as well as of the huge mountains behind to the west. At the foot lies the almost tropical forest with its stately laurel trees, its climbing plants, its parasitic luxuriant banana plantations and its elegant fern fronds, going right up to the first houses of Yerba Buena, whilst from there on the slim white towers of the matrix of Tucuman rise up out of the dense dark green of its numerous orange trees - a long-awaited sight for me, as to all homecomers.

..... from H.Burmeister

W.Boden previously lived in Santa Maria and has made the journey from there to Tucuman and back several times.

..... from W.Boden

From San Xavier the route goes along the valley ascending to the north, until one rides up the moderately steep climb of Cuesta de Piquerilla where the Estancia los Portrerillos lies on the heights. Beyond, the track goes downhill into the valley of the Rio Juntas towards Siambon, 3 leagues from San Xavier, a small hamlet of some settlers on the bank of the southward flowing river. One now keeps to this valley for some time and rides alongside or in the river itself towards the north, until one has reached the Estancia de Juntas, 2 leagues from Siambon. Here the Rio de Anfama coming from the northwest unites with the Rio de Juntas.

Beyond Juntas there is the Cuesta de Anfama or de Juntas to be traversed, which forms the watershed between the two rivers, and to the right is named after the one and to the left after the other. It is a much higher and steeper mountain ridge which must be traversed by means of a troublesome and in places menacingly dangerous path, until one arrives at the Estancia Anfama on the other side at 2 leagues distance from Juntas. It lies here on the above-mentioned river from which it takes its name. Together with the Rio de Juntas it forms the major source of the waters which flow into the Lueles in the plain and also receives those from the rivulet of San Xavier.

On the other side of the Rio de Anfama one must again ascend the mountainside up to the Cuesta Grande de la Cienega, a broad grass covered plateau, on which lies the Estancia de la Cienega, 3 leagues from Anfama. The river describes a great arc to the north around the high plateau and terminates in this direction in a marshy depression close to which the Estancia is laid out, from which it takes its origin. To the south the high plateau continues as a flattened mountain ridge, which represents the watershed between the Rio Famalla and Rio Lueles and in the west becomes the border of the Quebrada del Portrero Negro. When the western margin of the plateau is reached, close to the cienega, then one descends into the valley of Tafi. The distance between the two points would amount to about 3 leagues.

..... from H.Middleditch

Unless we hear to the contrary from J.Lambert, I imagine that he did not venture along any perilous paths or up such troublesome steep ascents as those here described, which suggests to me that Schickendantz would have had no great problem had he chosen to ride round any *Parodia* sites near Hualinchay. However, I suspect that he was never near that particular place and did not find his *Parodia microsperma* there, for the very simple reason that the calls of his business did not take him in that direction. On the other hand the slopes of the Andes directly to the west of Tucuman city lie between the known finding places for *Parodia* which are near Tafi and near Hualinchay. This area is crossed by the old route from Tucuman to Tafi which is described above, both by Schickendantz and by Boden. From Tucuman this old route first climbed through well-nigh forested ground, as Schickendantz tells us, as far as San Xavier. This may possibly be comparable in its humid nature to the environment on the route up to Hualinchay, which J.Lambert indicates does not support *Parodia*.

In the northern part of Tucuman province the main river is the Rio Sali which runs very roughly in a north to south direction, approximately parallel to the main chain of the eastern Andes which here is called the Cumbres Calchaquies. The Rio Sali runs in a valley which is composed to a great extent of alluvial material of geologically recent deposition, whilst the ridge of the Cumbres Calchaquies is composed of ancient granite and gneiss. Between these two, it may be seen from the geological map of Tucuman published by Kuhn & Rohmeder in "Estudio Fisiografico de los Sierras de Tucuman" in Univ. Nacional Tucuman, Mon. Inst. Est. Geog. Facult ad de Filosofia y Letras, No.3 1943, that there are parallel bands of strata which likewise run almost north to south. These bands of strata occur around Hualinchay and Gonzalo in the north and also to the west of Tucuman city in the area crossed by Schickendantz and Boden. However, the east-west spread of this strata is somewhat broader around Gonzalo than on the line of the old route from Tucuman to Tafi, where the granite and gneiss which forms the backbone of the high Cumbres Calchaqui outcrops at about Anfama. The *Parodia microsperma* were found by J.Lambert near Hualinchay and Gonzalo at altitudes of 1200 to 1300 m, whilst Anfama lies at 1750 m altitude. From the point of view of associated vegetation, geology, and altitude, I would have imagined that the stretch between San Xavier and Anfama would be the most likely area in which the original *Parodia microsperma* was found.

..... from F.Kuhn & G.Rohmeder, Estudio Fisiografico de los Sierras de Tucuman 1943

Before the construction of the automobile road along the line of the R.Tafi del Valle, access to the valley of Tafi from Tucuman, if not undertaken by plane, was somewhat inconvenient. One route between the plain and Tafi was via San Javier, Siambon, Las Juntas, Anfama and La Cienega, whilst other routes crossed the spurs and ridges lying between that route and the new road.

..... from H.Middleditch

Once a decent road was opened downstream from Tafi del Valle as far as the plain, I strongly suspect that traffic would follow this newer, longer, but more convenient route whilst the more direct route, as described by Burmeister, Schickendantz, and Boden, would fall into decline and disuse. The old route would be reduced to carrying traffic to and from the few Estancias along its length and it is unlikely that those making use of this route would have any interest in the local weeds. As time passed and more reliance became placed upon wheeled transport, the old route would become no-man's land to most collectors. In consequence, had the original *Parodia microsperma* been found on that route, it would have subsequently become "lost", but not eliminated, just overlooked.

From these considerations I feel that the view expressed by Weskamp that *Parodia microsperma* no longer exists in the wild is presumptuous and premature. However it would be very useful to find an even better indication of the possible origin of *Parodia microsperma*, should such exist, rather than rely solely on dead reckoning.

..... from J.Lambert

You may care to consider that a *Parodia microsperma* form is to be found in the field number list of Lau, under his number 575, for which the finding place is given as Raco. This is not a very great distance to the north of the route which you describe that was taken by Schickendantz. After it was suggested by H.Middleditch that this locality might have rewarded a search for *microsperma*, I also went there, but it was altogether disappointing. Perhaps it has changed quite a bit since Lau visited it: it has become a residential and holiday resort, complete with country club, etc. ... where the cacti do not have many chances of surviving! I did not see anything which would suggest that *Parodias* might grow there - for one thing, the place as a whole is too moist.

The first systematic study of the vegetation of Argentina was made by Dr.Lorentz, who came from Germany to fill the chair of Botany in the old University of Cordoba. Before his time, amateurs like Bunbury and Gillies had made chance investigations and small collections. Dr.Lorentz was afterwards assisted by another botanist, G.Hieronimus, who came from Germany to the University of Cordoba as botanical assistant.

The benefits of having established a scientific centre in the country are more keenly appreciated day by day; new researches are undertaken with the certainty for the collectors that the collections and the results of their studies will always fall into the hands of competent persons who know how to value them and render them useful to science. Collections of great importance ... will soon arrive, and a magnificent collection sent by F.Schickendantz has just been determined by Prof. Hieronymus, although he has published nothing as yet. He is also occupied at present in determining an interesting collection from Patagonia made by Prof. Berg and F.Moreno.

..... from H.Middleditch

It may be possible to track down the material from this particular collection and establish whether collecting places are clearly indicated. In the meantime, perhaps we may turn to the observation made by Weskamp that "Because the rules [of nomenclature] require, without exception, that all parts of a newly described taxon come from one specimen, the so-called holotype, the stated colour must be based upon one flower."

..... from G.Rowley "Name that Succulent"

Another bizarre notion is that an author selects his holotype first and then describes it in as minute detail as possible. This would really put the cart before the horse.

..... from G.J.Swales

The International Code for Botanic Nomenclature is reissued after each Congress and supercedes all previous Codes, to be superceded in turn after the following Congress. I only have available to me one complete issue of the Codes and hence my comments only relate to that particular version. In that issue I am not able to find anything which requires an author to confine himself to one plant or to one flower when he is undertaking the description of a new species. If an author has a number of plants before him which he is using for the purpose of preparing a description of a new species, with a view to indicating the extent of the variation that exists, then he can only pick one of them as the Type specimen. So I am not surprised by the comment from G.Rowley. I can also see the advantage of including in the first description an indication of the nature and degree of the variation which is evident to the author, so again it does not surprise me that the ICBN does not call for a first description to be based upon a single specimen.

..... from R.Mottram

There are no rules in the I.C.B.N. or elsewhere regarding the number of plants required in order to draw up a first description. Ideally, the author should give a full concept of the species by including as much of the variation between individual plants in his description as possible. In practice, this is often tedious, and in a great many other cases only one specimen is available to the author, so the description is in reality just of one plant. The pity is that authors seldom say if their description is based upon one plant, many, or a whole population.

Thus a description which includes, for example, "ribs 9 to 11", could mean that one plant has say 9 ribs below or 11 above. Or it could mean that some plants are exclusively 9 and others are exclusively 11. We do not know unless the author tells us. Characters are often subject to great variation, even on the same plant at different stages of maturity. On the columnar species, rib counts vary with height and age, as on one of my *Cleistocactus baumannii*. Other specimens may have a different rib count at different heights, and only access to more material and careful observation can provide a more universal picture. The work involved is immense, tedious, and unrewarding, and authors vary in accordance with the material available to them and the patience they can muster.

On the other hand, authors should make their descriptions as clear as possible, and a mass of data may be very confusing. Thus a good author will also highlight the diagnostic characters i.e. those characters which clearly distinguish the taxon from related taxa, either in a write-up following the description, or by underlining or italicising the salient features.

..... from H.Middleditch

It would appear that Weskamp is not correct in stating that the ICBN require all parts of a new description to be based upon one specimen.

..... from G.Rowley

I would only add that Weskamp seems to confuse nomenclature (covered by the Codes) and taxonomy (which is not) - a common failing.

..... from H.Middleditch

I have to admit that the implications of this last comment from G.Rowley were far from obvious on first reading. But I think he means that the process of describing a plant is taxonomy i.e. how long or short is the description, how many plants and flowers you refer to in the process, what parts of the plant and/or flower you include in the description, and so on. When it comes to selecting a name, then and only then do matters of nomenclature arise, which are covered by the Codes. If that is indeed the case then the statement by Weskamp that " .. the rules require, without exception, that all parts of a newly described taxon come from one specimen, ... " is quite fallacious. In consequence the rest of his argument on this particular point is almost irrelevant.

..... from J.Brickwood

Weskamp divides *Parodia* into various sections more or less in accordance with the seed. His Section *politispermae* contains the type species of the genus so that in accordance with the ICBN rules this section name is invalid and it should be "Section *Parodia*". I understand that he has been sent both English and German copies of the relevant ICBN rules by R.Mottram, but apparently he still remains convinced that he is right.

In his review of *Parodia*, Weskamp includes a description of *P.microsperma*; this is not specifically stated to be a reproduction of Weber's original description but although it follows the original fairly closely it does not reproduce it exactly. For the record it may be as well to reproduce the original description accurately.

Republic Argentina, Catamarca, Tucuman. Small species, distinctive on account of its hooked spines. Body solitary, subglobose, 5 to 10 cm in diameter, tuberculate. Tubercles subconical, barely confluent, but separated, arranged in 20 ribs or series of spirals. External spines 11, white, radiating, of 4 to 6 mm in length. Central spines 3 to 4, reddish, of 5 to 10 mm in length of which the lower points outwards or at an angle, hooked. Flowers numerous and very fine, 3 to 4 cms in diameter, varying in colour, orange yellow or golden yellow. Style and stigma yellowish. Berry hairy. Seeds remarkable for their extreme smallness, almost like dust.

..... from H.Middleditch

The original French gives the flowers "variant de couleur" which my dictionary indicates may mean changing colour, or varying in colour. My own experience of *Parodia* flowers is that the depth of colour pales to some degree after the first day on which the flower is open, due to the flower enlarging in size. Hence it may be that Weber was also noting this feature i.e. the flowers changing colour. Nevertheless I suspect that it is the range of flower colour to which he refers, a belief that I feel is supported by the comments from J.Lambert on this specific point.

..... from J.Lambert

Weber's description of *P. microsperma* is remarkably accurate. A few more details may be added, for instance that if the young plants are indeed subglobular, older specimens become more cylindrical with age - as indeed do many *Parodia*! This may also be seen in the excellent drawing by Gurke in Schumann's "Bluhende Kakteen" where the plant is somewhat higher than broad.

SULCOREBUTIAS FROM AROUND SUCRE. From W.Gertel

In the course of my second trip to Bolivia we travelled from Cochabamba towards Epizana and then on to Aiquile. Here we were marooned for two whole days by the pouring rain. The owner of the lodgings recommended us not to go to the Laguna area immediately after the rains, so we decided to go on to Sucre and explore the area around Aiquile on our way back. On the following day there was a clear sky so we set off for Sucre. On the way there, in the valley of the Rio Novillero, we saw some *Tillandsias* again. When Ed Kirschneck climbed up the tree he also found some *Pfeiffera*. We approached ever closer to the river crossing at Puente Arce. At these lower altitudes, the natives are able to make a better living than in the higher areas and the buildings are also better. Here we even found *Pereskias* all covered with *Tillandsias*. Down in the river valley the most characteristic plant was *Neocardenasia herzogiana*, with spines up to about ten inches or so in length. *Sulcorebutias* are absent at altitudes below 2000 m whereas numbers of *Weingartias* and *Parodias* colonise almost every hill along the Rio Chico.

We used the first day at Sucre to check over, oil and grease the jeeps. Meanwhile we looked around on the Cerro Churuquilla for *S.canigueralii* which we found there without difficulty, as they were more numerous than they had appeared to be three years previously. It was very clear that the population stretched over the whole of the mountain plateau. In 1983 the plants were so shrunken that they were rarely to be found. It makes a big difference whether you collect in the middle of the dry season, or towards the end when flowers or buds guide you towards the plants. About half way up Cerro Churuquilla we came to Recolleta San Francisco where Padre Canigueral lived, who actually found *S.canigueralii*. We then climbed up the Cerro, with a statue of Christ on top; all around the statue you find *S.canigueralii*. In recent years many eucalyptus trees have been planted on the slopes of the hill and eucalyptus do not allow any other vegetation to grow under their canopy. There are no eucalyptus around the crown of the hill yet. Here we found plenty of *S.canigueralii*, with plenty of offsets, so we did not have to take a single complete plant. My three companions made use of the afternoon to search for *S.canigueralii v. appanata* in the vicinity of the cement works, whilst I employed the time at the local Institut Geografic Militar in order to purchase some maps and to make a brief telephone call home. From that it transpired that nothing had been heard from us up until then.

On the next day we made a trip to Cerro Chatajilla. On the way there we first visited the habitat of my *G.25*, before we branched off the main road to Cerro Chatajilla. On the way we came across *Trichocereus tarijensis*. As we reached an altitude of about 3200 m, we began to stop and look out for *Sulcos* and finally struck oil. They were of a type which we had already seen some kms behind us on the main road, with flowers of magenta, via red, to reddish yellow (but this is common for the whole area along the road to Ravelo). We found these plants growing from well below the summit up to the top of the Cerro Chatajilla. As we travelled further in the direction of the pass we established that the composition of the population shifted towards the type of plants described by Rolf Oeser as *S.chatajillensis*, but there was no pure population of these either on the pass or on the adjoining mountains. Just beyond the summit there is the Capilla de Chata and around it we found a fine location of *S.chatajillensis*. Some of them were completely hidden and we probably would not have found them if they had not been in flower. There were also some *Tephrocactus* here. (I knowingly employ here an invalid name, as I have already done before, since between us all we are really almost agreed that the species name *verticillacantha* is absolutely wrong at this place here). It will need to be considered to what extent the name *S.chatajillaensis* can be retained, as usually still ascribed to *Sulcorebutias* from this area.

The following day was one of the most remarkable and productive of the whole expedition. We left Sucre very early and drove in the direction of Alamos. We first saw the habitat of my *G.21b* and then naturally the Type locality for *S.vasqueziana* on the Barrancas pass, where some of the plants (unlike three years before) were already in flower. Of all the *Sulcorebutias* I saw in Habitat, this is definitely the tiniest. I have not seen a single one which was bigger than half an inch across. As we scaled some more hills a few kms further on, we experienced the first surprise of the day. We found not the expected *S.vasqueziana* again, but another sort of *Sulcorebutia*, having nothing to do with any other *Sulcorebutia* known from this road, which even today is a puzzle to me. Perhaps they belong to Rausch's "dung-heap" of *Sulcos* but they were a definitely divergent population. There was one very remarkable clone with huge magenta flowers up to 7 cm in diameter.

The next surprise was shortly after the first ominous toll bridge. After we had ascended only a few metres, we found a fantastic flowering meadow, whose blooms consisted of the flowers of *S.frankiana*. So magnificent a sight I had never seen before. Hundreds and thousands of *S.frankiana*, and many of them in flower, from magenta via red to reddish yellow and orange. We photographed them all like mad. Slowly we walked over the high mountains, without the population changing in any way. Then, at about 3000 m came the break. The *S.frankiana* disappeared abruptly and perhaps 5 or 10 m further on there was the first *S.alba*. I spent quite some time in a search in the hope of finding *S.frankiana* and *S.alba*

growing side by side - but no! It is almost unbelievable that two *Sulcorebutias* grow on the self-same location, occasionally not 10 m away from each other, which flower at the same time, but none are to be found so close together that they can be said to grow side by side. Equally no trace of transitions or hybrids. In my opinion, no better criterion can be possible to validate each species. I had certainly heard of this phenomenon beforehand but that does not have the same impact as seeing it for ones-self.

Since time was pressing we gave up any ideas of climbing more hills before Alamos, but drove through the river valley in which lay the small hamlet, over the bridge which was being built three years earlier, and then near Alamos searched for *S. frankiana* v. *aureispina* - but without success. Even at the location specified by Swoboda we found at first only quite normal *S. frankiana*, although the flowers there were all magenta. Then on a closer inspection we found a few clones with yellow spines in the crown. There were multiheaded plants in which the yellow spines were just discernable. I have come to the conclusion that the v. *aureispina* has no justification, but is simply a mood of Nature, which occurs in only a few spots between all the other *S. frankiana*. We should loose this name forthwith.

Further on we went to reach Chimuri. After drawing one blank we finally found, on a very rocky mountain, the so-called *S. pedroensis* - as anticipated - a name which would be better invalidated, since it is indeed only a magenta flowering form of *S. alba*. Two or three of the plants we came across posed us a problem though, conforming in no way whatsoever with the prevailing form. The clone was robust, with stiff outstanding spination and had not the least similarity to the normal *S. pedroensis*. Are they just sports, mutants, or a new species? If these plants are something like *S. pedroensis* sensu Swoboda and Augustin, then the name may eventually be recognised.

The surprise of the day still awaited us. Right up on the mountain summit, somewhat apart from the other *Sulcorebutias*, Edmund and Franz came across a small population of *S. frankiana* all with red flowers. That alone was still not a specially great surprise, if one disregarded the fact that these *S. frankiana* grew above *S. alba*, at an altitude which is really uncommon for *S. frankiana*. The real surprise was the size of the plants. Whilst in the other habitat locations *S. frankiana* reached barely more than 3-4 cm across, at this spot they grew to the size of around 10 cm. We could scarcely believe it, they were the largest *Sulcorebutias* that we had seen up to now. Unfortunately it was already pretty late and we had no time to search around any further, so that we do not know whether this small patch is the only occurrence of the huge *S. frankiana*, nor did we find any more of the other strongly spined *Sulcorebutias*. It is however certain that this location near Chimuri is among the most interesting that we have found in the course of our travels. I am moreover quite convinced that the area between Alamos and the Rio Caine in the north is still good for some surprises. It is indeed almost worth a trip to explore this area alone.

On the way from the Rio Mizque towards Sucre in 1989 our travelling companions ran into problems with their jeep, so that it was pretty late when we arrived at our hotel in Sucre. On the following day this vehicle was in for repair so whilst the wives had a look round Sucre, Helmut and I went out on our own towards Alamos and Chimuri. Naturally my main objective was the huge plants of *S. frankiana* which I had found with Franz Kuhhas and Ed Kirschneck. It had annoyed me for three long years that I had only glanced over these fine plants. Now, in this year, they were not going to be just glanced at, since they stood in full flower. Many had a regular crown of flowers. Whilst three years before the *S. alba* G.47 with magenta flowers which grew on this same mountain were in bloom, in this year I was able to establish that the flowering time of these *S. alba* was already over. Only isolated plants still had flowers or some buds. To my surprise I found *S. frankiana* G.47a alongside *S. alba* G.47. I even managed to photograph two plants which stood only 30 cm from one another. It was quite clearly a case of sympatric occurrence of two basically different *Sulcorebutias*, without being able to find any sort of hybrid or transition forms. Strange to say the habitat location of the huge plants of *S. frankiana* was confined to one mountain flank, whilst the *S. alba* grew everywhere else.

We continued to drive still a short distance further in the direction of Ravelo and on this stretch we also found *Sulcorebutia* G.103. A precise identification of this find is pretty difficult since the plants throughout were very small and were completely withdrawn into the ground. They are in any case as short spined as the *S. alba* from near Chimuri and they gave an impression of being much darker, many of them even black. It remains to await how they develop. On the return trip we once again searched for *S. frankiana* v. *aureispina*, but could find no other place other than the location of our G.46 from 1986 and there we again found only brownish spined plants.

..... from J.Pot

When we were in Bolivia in 1990, we followed the road from Epizana to Aiquile, then southwards again descending to under 2000 m where it was quite warm, crossing the Rio Grande at Puente Acre. It was getting fairly near to Sucre before we got back up to 2800 m again. At 14 km from Sucre we saw a promising mountain ridge, so we stopped for three of the party to climb up to it. The promising ridge was further away than I had originally thought, but eventually we got there. I found *Lobivia cinnabarina* and a *Parodia*, but no *Sulcos*. The other two went over to the other slope and found some multiheaded plants of *S. canigueralii*. This was probably the place where Kohres had collected his plants. The cement works looked as though they were quite near but we had to go another 9 km to get there because of the many turns in the road.

Two of the party left us at Sucre and took the vehicle for a two-day trip towards Potosi. Whilst they were away, I took a taxi to Hacienda Barranca in order to collect some *Lobivia obrepanda*; we stopped at the Hacienda, a small house perhaps 5 m long and 3m wide. They evidently knew about the gringos who search for cacti. They immediately set off up the path with pickaxes and set away to root out all the cacti. I immediately began to shout "enough" but was still not able to prevent the number of proffered plants being larger than was planned. Some large, old specimens were collected. After Barranca we saw to our left a stony ridge, where we quickly found whitespined *Sulcorebutia*, which looked much like the plants that we had found two years before to the south of Barranca.

Some kms further on there was an ideal hill, although rather steep. I recalled what Leo van de Hoeven had said about the location of *S. frankiana* and *S. alba* and gave myself a good chance of finding them here. It was only a good 100 metres climb, often over difficult ground. Up above there was everything present that would make a seeker of *Sulcorebutias* happy, except *Sulcos*.

A couple of kms further on we stopped once again. On the right hand side of the road stood two small hills and here we found *S. frankiana*. Now for *S. alba*! It was known that both sorts grew on the one hill and that the locations were clearly divided. In spite of our searches we did not find *S. alba*. The next hill looked steep but it had a sort of path upwards. The taxi driver spoke Quechua but the locals could not tell us anything about cacti. Nevertheless we decided to go and look. We went up along the path and halfway up I looked casually down to the ground. Here was *S. alba* in great numbers and growing in a different manner to that which I had expected. All the other *Sulcorebutias* which we had found around Sucre were withdrawn into the ground, but the *S. alba* at this spot formed nice little round heads above the ground.

On the following day I again took the same taxi, this time taking the road which I had found on the map, going northwards between the cement works and Barranca. The landscape did not look so marvellous for a *Sulcorebutia*-seeker; there were many fields and sometimes a deep valley. I began to doubt the correctness of my choice, all the more so as this road would shortly come to a dead end in a valley. At the height of the cement works it was somewhat rough ground, but when I reached it I realised there would be no *Sulcos* there as experience had taught me that *Sulcos* are to be found only in terrain showing no signs of erosion. But we did find *Lobivia cinnabarina*.

Must we go on further still? The altimeter indicated 2700 m and up to now I knew that *Sulcos* scarcely ever grew under 3000 m in the area of Sucre. But I had looked ahead, where there was a stony ridge, and still wished to take a chance. The taxi driver would probably not go that far with me, as the road was only poor and he was careful about his vehicle. This ridge should really be able to offer *Sulcos*, but the altimeter indicated barely 2800 m. We searched for some time, found *L.cinnabarina* again, but of *Sulcos* we saw not a trace. Just as I felt like giving up, we discovered a *S.cangueralii* with roughly 50 heads. That gave us heart. With great pleasure I took a cutting from this plant; we immediately found a second one, and others after that. These plants grew on an extraordinarily small patch, perhaps two square metres. They were mostly large plants, from which a cutting was easily taken. On account of their colour they were barely distinguishable from the stones.

After spending a few days on a trip to Tarabuco we returned to Sucre. We were thinking of travelling via Ocuri and Uncia to Oruro. As far as I knew, this particular road had not been travelled by any other cactus collectors. How far would *Sulcos* be found there? Even outside Ravelo we had to cross a river. Just at that moment a large group of llamas appeared. They crossed the water and ran past our vehicle, whilst we captured them on camera. During our previous trip to Bolivia we had hardly seen any llamas and now we saw a considerable number all at once. After about 5 km we stopped. The hill consisted of large, smooth stones. Here and there it was encroached upon and at these places plants could grow in the "soil" that had gathered there. The *Sulcos* that we found there were like those from the other side of Ravelo, although the terrain was completely different from that at our first stop before Ravelo.

Setting off once again, we climbed steadily higher and at the third stop we found plants with hard, black spines. Was this still a form of *S.losenickyana*? A motorcyclist came up to us and asked us what we were looking for. He said that the plants also grew further up and I gathered that he would ride in front and would point them out to us. It was dismally cold here, the wind howled over the plateau and it was half overcast. I realised that this could really have an effect on me and I thought of my small plants in my greenhouse basking in the sun at 40°C.

Very shortly we came to a junction and took the branch going north; shortly afterwards we came across someone who told us that the road led to Ocuri. Afterwards we discovered that the other turn also led to Ocuri but was a much better road than the one we had taken. The landscape was most interesting, undoubtedly a paradise for a geologist, but here and there barely passable. I also doubted whether any other cactus collectors would come this way after us. Our last find was at about 3700m. We were almost blown over by the wind and one of our party was the hero of the day when he found two multi-headed *Sulcos*. I was able to take cuttings of both of them. After that we searched twice in vain. Shortly before Ocuri we arrived on an enormous plateau and there we met once again the good road from Ravelo. Perhaps the motorcyclist was standing waiting for us somewhere along that stretch.

..... from L.Hoevens

Travelling south from Aiquile there are *Sulcorebutias* to be found quite close to the edge of the road, at about 2300 m altitude. But the nearer we got to Puente Arce the less the altitude and *Weingartia* put in an appearance. By the time we reached the river we were down to 1800 m and coming near *Parodia* territory. Not far from the Rio Grande we came across *P.ocampo* and also some *Weingartias* in flower. In a little canyon away from the river, at about 1900 m, we came across a *Blossfeldia* on near-vertical rocks, the largest heads about half an inch in diameter. From Sucre we went to the site of *S.canigeralii*, then along the road to Los Alamos for *S.vasqueziana*. Here the plant bodies might be half an inch in diameter but were about one inch across the spines, but even the plants with a body half this size were still about an inch across the spines. At 3000 m we met with *S.sucrensis*, then later on with *S.alba*. Next we met with a *S.alba* with a red flower, then some more *S.sucrensis*. We then found both *S.sucrensis* and *S.alba* at virtually the same spot, with *S.frankiana* not far away.

..... from J.R.Kirtley

On the trip to Bolivia which was made with B.Bates in November 1989, we drove from Sucre along the road to Ravelo, stopping at locations 35 km and 44 km out from Sucre and another on the return journey at 4 km out of Ravelo. It had rained heavily for about two hours on the previous evening, the storm passing in the direction of Ravelo and unloading such a quantity of water that the parched earth could not absorb it. The resulting run-off had washed out part of the road, which was passable only with care.

At the first stop we explored a steep boulder-strewn hillside of red sandstone which rose to the north above the road. Interspersed among the rocks were shrubs, grasses, bromeliads. The earth itself was covered with what appeared to be blackened and burned vegetation, but on closer examination was found to be dried moss and lichen. *Sulcorebutia* were found, many growing horizontally in the vertical rise between the boulders, where the ground was "stepped". (The *Sulcorebutias* at Sacaba and Colomi grew in the same fashion). Plants observed here ranged from 1 cm to 3 cm in diameter, mostly bearing fine adpressed spines, with the odd plant having stronger, yellowish spines; many of these plants were drawn down into the earth, so that in some cases the plants grew either level with, or below ground.

The second stop was made where a small stream meandered from near the road into a small side valley. Here I crossed to the north side of the road again, to explore an outcrop of coarse-grained red sandstone, the strata of which ran at an angle of perhaps 30° from the horizontal. Grasses, dwarf shrubs and bromeliads grew in the depressions and crevices in the strata. Here small pools had formed, following the evening's rain. Dropping onto hands and knees, I scoured the cracks and crevices, eventually finding *Sulcorebutia*. Plants here were similar to those 9 km further south, with the addition of the robust, heavily spined plants which we know as *S.losenickyana* Rausch. The earth in the crevices was very wet, the moss and the lichen already beginning to turn green. Some plants bore brown marks as though they were suffering from rot caused by too much wetness, although this may have been caused by low temperatures. Plant body size ranged from 1 cm to 4 cm, spination from thin and wispy, through all the combinations to long and robust. Spine colour ran from white, through all shades, to dark brown. In my scramblings across the rock, I disturbed a frog coloured bright green and yellow, which was duly photographed. Going back to the vehicle, we then moved on over a very fertile plain into Ravelo, where we took refreshments before beginning the return leg.

We stopped at about 4 km out of Ravelo to search another sandstone hillside; we were at some 3200 m altitude. Here we met a Dutch couple who were staying at our hotel, and who had travelled out by truck and were looking for a lift back. We explained what we were about and appreciated the help that was offered. Terrain and vegetation here was

similar to that at other locations, but the ground was not wet, apparently having missed the storm. This hillside produced the most interesting plants yet encountered, some little more than 5 mm in diameter and others with a diameter of more than 6 cm. The smaller plants grew horizontally, the larger plants vertically. Spination once again ranged from thin to robust, the larger plants conforming to Rausch's *S. losenickyana*. One of our Dutch friends brought me a large *S. losenickyana*, together with the lump of moss and earth in which the roots were embedded. Imagine my surprise when I found growing in the moss and soil around those roots, three small finely spined plants, no more than 1 cm in diameter. This was the first time that I had observed these two sorts growing in close proximity to one another. At the time I wondered if the small plants were in fact seedlings, or immature forms of the larger and more robustly spined plants.

These plants had been in flower earlier than the time of our visit there. Only occasionally were there buds still to come, and these indicated red flowers. Fruit and seed were collected, however most of these were immature, resulting in poor germination.

The clones of *S. losenickyana* which I have in my collection do not show the very strong spination which was exhibited by the plants we found here, although they are distinctive. Having studied all the plants from this area which I am growing, and having recalled the memories of the collecting experience, checking notes and slides on many occasions, the conclusions that I have come to are these:- that Rausch found and described an extreme form of the plants in this area as *S. losenickyana*. There is a general hotch-potch of *Sulcorebutias* of all sizes, bearing spines of many different forms, lengths, and colours abounding here. Having seen so many intermediate forms (between the very small *S. sucrensis* form and the robust *S. losenickyana* form), that I cannot at this time come to any other conclusion except that they are all but one extremely variable species.

..... from W.Rausch K.u.a.S. 25.3.1974

At the time the four of us spent a half day along the road and we could only find four plants, and these are so anomalous, that only with difficulty can they be accommodated with any of the well-known *Sulcorebutia*. I name these after Egon Losenicky who looked after my collection during my travels in the field.

..... from H.Middleditch

It appears that the sloping mountainside on which these plants were found by J.Kirtley faced south, so presumably the *Sulcorebutias* growing in the steep or near vertical steps may well have been in full shade for the whole of the day. From the habitat slide the whole site appears to be very exposed but nevertheless I suspect that the *Sulcorebutias* growing here on the more nearly horizontal ground would have some shade for some part of the day. Because of the moss and lichen which was reputedly in numerous patches here and there, I also suspect that this would provide a reservoir of moisture to a number of these *Sulcorebutia*, even between rainfalls.

..... from J.D.Donald

The plants from the region Sucre to Ravelo are very diverse changing almost from kilometre to kilometre, a region which Rausch called 'the dung-heap of *Sulcorebutia*'. Little sense could be made of the numerous phenotypes that occurred in the local populations. The photograph taken by J.Kirtley shows, I suspect, a plant which I believe is Rausch's *S. losenickyana* in company with some small plants of *S. brevispina*. This diagnosis is based upon the fact that Rausch told me that only *S. losenickyana* has such long spines in the *Sulcorebutia* population around Sucre. It is an oddity, in that there is no single locality exclusively of *S. losenickyana*. It is simply a random variant. The *losenickyana* type and the *brevispina* type are established phenotypes that persist and are found at odd intervals along the whole route from east of Sucre as well as west, between Zudanez to the east to Ravelo and beyond in the west, arcing also southwards into the Department of Potosi near Colavi and also from Ravelo north eastwards towards Poroma, quite a range.

..... from W.Gertel

Although I do not think that I have been in exactly the same place as where the heavily spined *Sulcorebutia* was found by J.Kirtley and B.Bates, these *Sulcorebutias* from between Sucre and Ravelo are far from being uniform. They belong to what we usually call *S. spec Sucre*. The only valid name for these plants is *losenickyana*, but Rausch picked some very extraordinary plants for his first description, which means that *S. losenickyana* actually only covers an extreme form of the *Sulcorebutias* of this area. In a wider sense all these plants belong to *S. canigueralii* - a population which extends from Sucre to places even beyond Ravelo. This population is extremely variable. Rausch once used the expression "Dung-heap of Sucre" and he also said that it would be impossible to describe anything from there, because one hardly finds two similar plants on the same site. Besides WR.64, possibly Lau.375 and Lau.954 belong here as well as all the HS plants from HS 1 to HS 12, as well as my own collections of G21, G25, G26, G43 and many others. Even *S. chatajillensis* belongs to this group of *Sulcorebutias* although this particular population is more or less uniform.

..... from H.Middleditch

The first description of *S. losenickyana* was published by Rausch in K.u.a.S 25:3.1974, whilst in his *Kakteen in Sudamerika* Ritter says that he had his *S. sucrensis* in manuscript before that, but acknowledges that the Rausch name has priority. Hence Ritter accepts his *S. sucrensis* as a synonym of the Rausch *S. losenickyana*. However, I find that the two descriptions are far from identical in a number of respects:-

	Rausch	Ritter
Body	Up to 6 cm.	Up to 3.5 to 4 cm.
Spines	Divergent, somewhat curved back to the body Up to 25 mm long Centrals 0-1, up to 17 mm long	Comblike Up to 6-12 (-20) mm long Rarely a central spine

The Rausch description of *losenickyana* states clearly that the spination is "strahlig", which is beam, jet, or cone-like, thus implying an acute angled cone, which is in conformity with the illustration that accompanies his description. It is quite definitely not "strählig" which would mean comb-like. One would have imagined that there was quite an appreciable difference in habit between comb-like and pretty well correct spination. Once again referring to the illustration accompanying the original Rausch description of *losenickyana*, the central spines give the impression of being longer than the others, which is not the impression that I obtain from Rausch's own description. In Ritter's description of *S. sucrensis*, the comb-like spination (which infers that the spines are more or less appressed to the body), the spine length, and the body size, seem to me to be very reminiscent of the general appearance of the *Sulcorebutias* which come from this area under various names such as *alba*, *frankiana*, *ritteri*, *vasqueziana*, and *canigueralii*. My copies of slides taken in habitat by J.Donald of *S. canigueralii*, *S. vasqueziana* and of *S. frankiana* are of quite small heads almost level with, or projecting only a short distance above, the ground, spination short and enwrapping the body. A similar impression was conveyed by the slides of the *Sulcorebutia* growing in this area which were shown to us by both L.Hoeven and K.Preston-

Mafham at the 1989 Chileans' Weekend. We have been told by almost all those who have been out to Bolivia that these plants shrink back into the ground in the dry season, an action that would be impractical if the spines did not hug the body. The *Sulcorebutia* with the robust, projecting spination which was found by J.Kirtley is not difficult to equate with the Rausch *S.losenickyana*, but very difficult to associate with the Ritter *S.sucrensis*.

However, in K.u.a.S 41:10.1990 there are two colour photographs of *S.oenantha*, one having the typical body-hugging spination and the other having projecting spines with the centrals on the upper half of the body facing virtually upwards. The difference is as much as that between the descriptions of *S.losenickyana* by Rausch and of *S.sucrensis* by Ritter.

..... from J.Kirtley

Robustly spined plants can, and indeed do, withdraw into the ground during drought conditions, as do the more finely spined plants. On the Ravelo road I was able not only to note this but also to record it on slide. Plants of all forms grow both above and below ground level - however, the most common sight was of small-bodied, fine spined plants below ground, the larger bodied, more robustly spined plants being above ground. At Sacaba and Colomi the same mixture of plant forms exist, growing in very similar conditions, vertically and horizontally. These, too, were also noted and recorded on slide. Large bodied plants with strong spination (*S.steinbachii* forms) are found withdrawn below ground alongside small bodied, fine-spined plants. I found no intermediate forms here! One answer is that the roots must have some means of securely anchoring the plant in order to achieve any degree of withdrawal. Those plants which grow only above ground may have no other choice!

..... from K.Preston-Mafham (At the 1989 Chileans' Weekend)

From La Paz we travelled via Oruro to Cochabamba, where we made one or two local trips before setting off for Sucre. At Cuesta de Huankani on the road to Colomi we came across hundreds and hundreds of *S.steinbachii* with all different flower colours, and also with a range of spination, some very spiny and some almost spineless. I got the idea that there seemed to be large heads with long spines and small heads with shorter spines, but no intermediates, and I did not get the impression that one developed into the other. At other locations one can find only one flower colour and spine type. There are some plants here with dozens of heads that are up to two feet across, but hardly stick up above the ground at all.

We then went over the hill to Cliza, finding *S.taratensis*. There were plenty of plants, thousands of them. Not only do they hug the ground like alpines, you can even walk over them and not even see them, although some of the multi-headed plants were almost a foot across. Some plants growing near a rock may stick two or three inches out of the ground and these will be very spiny. All around are flowers of different colours, yellow and gold, through red to magenta, sticking out of the ground, but some plants with ten or twelve heads had only one flower.

Taking the road to Epizana we pass through Kayrani and on towards Lopez Mendoza. Here we collected seed of *S.tiraquensis* v. *longiseta* KPM.157, but the plants were not in flower. The plants were very variable ranging from tiny short spines to long golden spines and everything in between: an incredibly variable population. The varietal name does not seem to have any justification. We are keeping in our own collection twenty plants grown from this seed in order to find out what sort of variation they display.

..... from J.Pot

When we made our first trip to Bolivia in 1988 we made some very thorough preparations, but we did not know the exact locations of many plants discovered by other collectors who had travelled in Bolivia. A good example is at Monte Puncu, the mountain on which *S.tiraquensis* is found. Everyone seems to find this plant quite easily at this place. We took our map, found when we were at Monte Puncu, got out, and climbed. After climbing for one km we came to the conclusion that we were in the wrong place. We knew that Albert Buining visited the type locality of *S.tiraquensis* when he was 70 years old. It was a very hard walk for him, but he succeeded.

This one km that we had climbed was very hard for us, even though I am 45 years old and my companion was aged 35. So we should have been elsewhere. We went on further down the road and after about 3 km we did discover *S.tiraquensis*; these plants all had black spines, except just one with yellow spines (JK 18). We were not even sure that these plants were *S.tiraquensis sensu stricta*.

The next place we looked, not far away, I discovered one "normal" *tiraquensis*, but in the ground next to it there were other *Sulcorebutias* (JK 20). After one year in my greenhouse they remain small with reddish brown spines and either no central spines or just short ones. Some km further to the east we made another stop and discovered flat dark green plants, 12 cm in diameter (JK 22). Were they *tiraquensis*?

When we visited Monte Puncu on our 1990 trip to Bolivia, we visited the finding place of JK 20 again and found it had vanished - a farmer had made a field there. No plant left! In the neighbourhood of the field, however, we discovered plants like JK 20a, which is identical to JK 20. I would not regard it as a normal *tiraquensis*, as the flowers are often bicoloured and rather small. By now some of the JK 20 from our 1988 trip show some longer spines, but others remain with very short spines.

If one plant has adressed spination and another has somewhat projecting spines, the two plants can have a totally different appearance - but if you count the spines then they can be the same. Even if the plants differ appreciably in the appearance of their spination, they may still come from the same population. For example, I have six clones of HS 188; one has long, projecting spines, another has short, pectinate spines. Of course, if one wishes, one can believe that Swoboda made a mistake in his identification; but among my own JK 26 I can see very different plants, although I am quite sure that they came from the same population. They all have the same flower and they flower at the same time.

..... from J.D.Donald

The observation of small pectinate-spined plants accompanying larger long spined plants, often at the base of the larger plant is a fairly commonplace occurrence for some species in *Sulcorebutia*. I observed it several times. Often these smaller plants are seedlings or juvenile offsets of the parent plant, rarely will they be of a different species, but again that does occur in some populations. I suppose that I had better be a little circumspect when I use the category of species, as it depends of course upon one's viewpoint. In my case, you are probably well aware that I consider this category is much over employed in the *Sulcorebutia*. There are possibly only six to eight that I can really believe as genuinely botanically distinct species. So when two or more alleged 'species' occur in close proximity to each other, it is well to suspect that they might be more closely related than their initial appearance might suggest. Of course because of their distinctly different appearance they are, in all probability, separate genotypes, but still members of the same species population showing quite a substantial natural phenotypic diversity. Amongst the populations north of Cochabamba, I have observed what I believe to be a case of dimorphism between juvenile and mature forms of the same plant. Some times it is possible for the juvenile form to persist into adulthood without transformation to the adult form. This occurs in some forms of

S.steinbachii, particularly with the form 'tuberculao-chrysantha' and 'gracilior'. The former is the long-spined adult form and the latter is the juvenile of the same variety. I collected several plants of the former to which were physically attached "gracilior" as offsets. In time these offsets became like their parents but amongst several seedlings also raised from the same original plant, there are some 'gracilior' that remain juvenile.

SULCOREBUTIA VERTICILLACANTHA V.CHATAJILLENSIS By R.Oeser
Translated by H.Middleditch from K.u.a.S 35:10.1984

In May of 1973 I received some plants from Domdey, who was at that time a German teacher at the German school in Sucre. Amongst them were plants of a population of *Sulcorebutia* from a well defined and relatively compact habitat location in the area of Sucre. From the very beginning they gave a clear impression of being good uniform plant material. Their outward appearance resembled in many respects the imported plants collected by Rausch in 1971 as *S.alba* WR.472 from the district of Los Alamos, also from the area of Sucre. Because of the uncertain identification my plants stood undescribed in my collection until now, under close observation until a description would eventually be possible.

In September of 1979 Gerhard Kohres, to whom I had given the precise habitat data, brought along from the same field location a further, larger number of plants, which collectively increased the impression that these were a new variety of the well known *S.verticillacantha*. Flower and seed studies which were undertaken by A.J.Brederoo confirmed this supposition. Since their existence in a relatively narrowly defined area was located by two different collectors and in such remarkable uniformity, that it was evidently a new variety from the wide form-circle of the already well-known *S.verticillacantha* varieties, from which many have erroneously been described as individual species.

...The flower colour in the Type is clear red with a paler throat towards orange. There are also found scattered throughout the the population markedly varying flower colours, although the majority of the plants flower like the Type. An isotype in my collection flowers with a dark velvety red, tinted bluish at the margin. Other plants from the Kohres collection flower sporadically pale red to magenta, one plant dark orange to yellow. These variations in flower colour, although appearing only randomly, are of no taxonomic moment, since we find this variability of the flower colours with many *Sulcorebutia* spp. The flower pigment is a Betacyanide which can produce all the colour shades involved here. Nature frequently experiments in many ways and varieties with the flower colours in the course of evolution, in order to find the most fitting flower colour for the pollinator and thus most favourable for survival in these high Andes.

This now newly described variety of *S.verticillacantha* stands close to *S.ritteri* but differs from it in spination, flower form and colour and has a different and clearly separated distribution area. In the whole of the area to the north, northwest, and west of Sucre there occur numerous similar variations, of which some have already been described, such as *S.vasqueziana* v.*albspina* and *S.vasqueziana* v.*applanata*, which by a comparison of all features are likewise found not to be in accordance with this now newly described variety.

In 1974 Rausch described his WR.477 as *S.losenickyana* based upon a find on the road from Sucre to Ravelo. At this spot he found only four specimens of these plants altogether. These plants have been spread round European collections as propagations of the original in only three different clones. On the same route at Km.38 Ritter found his *S.sucrensis* in only one example, which he later identified as the Rausch described *S.losenickyana* on the basis of only one still extant slide of the plant. From finds north of the country town of Ravelo, collected by Domdey, there proved to be more specimens clearly also *S.losenickyana*, with no variations in the flower colour and spine length. Walter Rausch noted conversationally that at his habitat location he could perhaps have found only the edge of the population and that these could be distributed more widely elsewhere.

In 1979 Kohres collected plants at the 38 km point on the same road, from which some had the typical habit, spination, and flower of *S.losenickyana*. Other plants of this collection are clearly *S.verticillacantha*. In 1983 Swoboda also found on this road, but directly behind Sucre airport, likewise a *S.losenickyana*. Consequently it is proved by many finds that these plants grow in one area at different places within stands of various forms of *S.verticillacantha*.

... from H.Middleditch

When he refers to *S.losenickyana*, does Oeser have in mind a plant with more or less outstanding spination as opposed to the body-hugging spination commonly associated with *Sulcorebutias* from around the Sucre area? Is Oeser stating that the projecting form of spination has been found not only in the vicinity of Km 38 on the Sucre-Ravelo road but also scattered here and there among other field locations of *Sulcorebutia*? Would those *losenickyana* have been typical semi-underground plants with enwrapping spination, or larger plants with projecting spination growing at a spot where withdrawal below ground level in the dry season is more difficult?

A WRINKLE ON THE WRINKLES - CUTICLE? - ARILLUS LAYER?
from H.Middleditch

On various occasions at several Chileans' Weekends, we have looked at seeds from a number of genera. Most of these seeds have a black exterior. This black coat consists of a layer of cells which form the outer shell of the seed and is called the testa. When a slide of a seed is projected on to a screen the magnification will be round about 300. At this magnification it is often quite easy to see the individual testa cells on many cactus seeds. In some seeds the outside of the testa cell may be almost flat in which case the surface of the seed looks fairly smooth, so that it can be difficult to pick out the individual testa cells. Or the outside of the testa cells may be convex, or humped, so that the surface of the seed is more or less covered with hummocks, each hummock being an individual testa cell. Shiny black seeds or matt black seeds seem to be very common among South American cacti. But in addition there are quite a number of seeds which have what appears to be a fairly uniform brown or grey brown coating. Sometimes this brown coating covers the whole, or almost the whole, of a seed. But on many seeds the centre of each testa cell appears to be black and this is surrounded by a brown coating over the outer part of each testa cell. You might imagine that the brown coating had been rubbed off the high spots, that is, off the tops of the hummock formed by each testa cell.

Nowadays it is not uncommon for various cactus journals to include electron microscope photographs of seeds. In these photographs it is easy to see that there are fine wrinkles that stand up on the external surface of the testa cells. If a comparison is made between an optical photograph of a shiny black seed and an electron microscope photograph of the same sort of seed, we find on the S.E.M. photograph that the testa cells are covered by either indistinct or fine wrinkles of

low altitude. On an S.E.M. photograph showing the testa cells covered by what appears to be tall, thick, or crested wrinkles, the same seed will appear brown or grey-brown on an optical photograph. When each testa cell on a seed exhibits a black centre and a brown surround on an optical photograph, then on an S.E.M. photograph it will usually have fine, shallow, or insignificant wrinkles at the centre of the testa cell surrounded by coarse, deep wrinkles out to the margin of the cell. An article by Barthlott and Voit which reviews various features of cactus seeds appears to define this wrinkled surface layer as the cuticular layer.

..... from R.Mottram

As far as the cuticular layer is concerned, all standard dictionaries I have consulted are agreed that the cuticle is an impervious, noncellular layer secreted by epidermal cells. e.g. Little & Jones, A Dictionary of Botany (1980) "a noncellular layer of waxy or fatty materials on the outer walls of epidermal cells". Ivimey-Cook, Succulents: A Glossary of Terms (1974) "An impervious layer secreted by the epidermis, generally as a protection against undue water loss". Abercrombie, Hickman & Johnson - Penguin Dictionary of Biology (1979) "Superficial non-cellular layer covering animal or plant; secreted by epidermis".

..... from G.J.Swales

The outer wall of the testa cell is constructed of several layers of long chain molecules forming a basketwork with apertures through which water would be able to pass like going through a sieve. I have an S.E.M. photograph to demonstrate this very feature to our own students. In order to waterproof the seed the epidermal cells secrete a wax-like substance which is exuded through these relatively large apertures. This substance is deposited on the outer surface of the testa cell as the cuticular layer. The lower parts of this layer remain within the interstices of the basketwork of the outer cell walls so that in this way the cuticle is locked on to the surface of the testa. Because of this it is mechanically impossible to remove the cuticular layer from the testa. In addition the cuticular layer does not possess a cellular structure.

..... from "Botany" Weier, Stocking & Barbour 1970

[Of plants in general]... The epidermis is usually a single superficial layer of cells covering all other primary tissues and protecting them from drying out and, to some extent, from mechanical injury. ... The outer tangential wall of cells exposed to the air is usually thicker than the other walls of the cells, and its surface layer is usually coated with a waxy substance called cutin. This superficial layer of cutin is termed the cuticle. ... The cuticle is quite impervious to water and gases.

..... from H.Middleditch

My first encounter with any reference to a cuticular layer on cactus seed was in the article by Barthlott and Voit. At that time I have to admit that I did not quite understand this particular feature, but by now I think that I have a rather better picture of the nature of the cuticular layer. Because the brown colour (optically) or wrinkled layer (by S.E.M.) is pretty consistent in micro-disposition in any particular seed or group of seeds e.g. *Austrocactus*, it was always unsatisfactory to try and explain it away as a membranous remnant of the funicular flesh. It is probable that the epidermal cells on seeds are just as capable of excreting a cuticular layer as any other sorts of epidermal cells on plants or on animals. In addition, the testa cells on any one seed may well be consistent in the thickness and nature of cuticular layer that they produce, and also moderately consistent in a given species. Which will of course be why we can find consistently brownish seed or consistently black seed associated with many particular species.

..... from J.Lambert

When looking at the selection of slides of the seed collected by R.Ferryman in Chile, I was struck by the fact that seeds of the same species, or even in the same population, showed quite a bit of variation indeed. For example, in both *Neochilenia andicola* and its variety *robusta*, the size of the seed not only fluctuates from 1.3 to 1.7mm but the look of the testa differed greatly from one sample to another, from nearly pure black to completely covered by a pale brownish or greyish cuticle. The same remark can be made about *Neochilenia curvispina*, *N.paucicostata* and *Neoporteria littoralis*, whilst in *Neochilenia taltalensis* the cuticle varies from weak to quite prominent. In addition, in some cases, the black surface of the testa seems to suggest that it is not naked, but that the cuticle itself has turned black.

..... from G.J.Swales.

The outer layer of testa cells on a seed is a normal epidermal cell like the outer layer of cells on other living things. I see no reason why it should not function in any different manner to the epidermal cells on, for example, cactus stems and bodies, which produce an especially thick cuticular coating. Even flower petals have a cuticular layer, as I was informed by a botanist who had worked her way through a surprising variety of the world's flora checking this very point.

On any seed of a given species each testa cell may well be working to a similar cuticle production programme, so each cell is likely to produce a similar amount of cuticular material and also to deposit it in a similar manner on the external surface of the testa cell. In this way the pattern of deposition is likely to be similar on all testa cells and this is often what we see under the electron microscope. What is far from clear is why some species exude so much deposit that it forms into folds and wrinkles which project appreciably above the surface of the testa cell, again all to a pretty consistent pattern per species. When we look through a microscope or camera lens at a cuticular deposit which is flat and mechanically bonded to the outer wall of the cell, we do not see the thin cuticular layer but only the (usually) black surface of the testa cell. Where the cuticular layer has buckled and wrinkled there may well be an air gap between these wrinkles and the surface of the testa cell, so at these places the cuticular layer appears white in the electron microscope and brown to the eye or through an optical microscope.

..... from H.Middleditch

Does this mean that the naked black surface mentioned by J.Lambert will have a thin cuticular layer, but so thin that it does not obscure the view of the black testa cells which it is covering? In addition to the variation in the brownish colour of the outer coat of the *Neoporteria* seed, observed by J.Lambert, we have also been told by F.Fuschillo that among certain species of *Gymnocalycium* seeds which he has studied, there are some of a brown colour and some of black. This difference in seed colour is apparently found not just in one plant but even in one fruit. Is this due to a difference in the micro-wrinkles of the cuticular layer, or simply a difference in the patchy membranous layers which are already known to occur erratically over the surface of the seed testa?

There are quite a number of cactus seeds which display a pale brown surface covering which is quite different to the cuticular layer. This brown covering appears to be some sort of membranous layer which does not cover the whole surface of the seed in a uniform manner. It is more obvious over a part or parts of the seed surface, and wherever it is more obvious it gives the impression that the brownish material is thicker. There seems to be no consistency in the disposition, form, or extent of these apparently thickened patches. The brown membrane may possibly extend over the

whole of the seed but it is much easier to see it where it is folded into a pleat or folded over itself, like a macro wrinkle over the cuticular wrinkles. These brown patches may be seen e.g. on seeds of *Gymnocalycium brachypetalum* P 101, *Acanthocalycium thionanthum*, *Weingartia cintiensis*, to mention just a few out of many, as well as on several of the Matucana seeds such as *M. krahnii* which were discussed at the 1987 Chileans weekend. One or two S.E.M. photographs of Matucana seed which have appeared in the Dutch Cactus Journal recently, display this additional membranous layer very well.

..... from R.Mottram

It is doubtful if the macro wrinkles displayed by this outermost layer on the seeds of *M. krahnii* and others have any definite form. I look upon this outer layer as the remains of the outer integument. In fruits where the seeds are released dry, the outer integument remains on the seed, drying onto the already hardened testa. As it dries, it shrinks to form the macro wrinkles we have seen on slides. It also follows that the overlay may be irregular, with some areas of the testa revealed in patches, or extra material forming the macro folds.

..... from H.Middleditch

In the dicotyledons, the ovule at the point of fertilisation possesses two integuments, an outer and an inner. As it ripens into a seed the outer integument develops and forms the layer of testa cells. This is the only outer integument which exists in an ovule, so that the brown membranous material which exists on certain seeds must have another source.

..... from R.Mottram

I agree that the testa is normally referred to as the outer integument, but this assumes that only two integument layers are present. When there are three layers, then the outermost layer is not the testa but the aril e.g. Blackmore & Tootill, *The Penguin Dictionary of Botany* (1984) "Occasionally a third integument is formed which becomes conspicuous as an aril following fertilisation".

..... from H.Middleditch

As far as I understand the terminology, an integument is an outer layer of cells in the Ovule, whilst the testa of a seed and any layer of cells immediately below the testa is formed from the outer or inner integument of the ovule. No reputable botanical author that I have read appears to put forward more than two integuments for an ovule. Having become a seed, the terminology changes from that applied to an ovule and there are no longer any integuments on a seed. The statement by Blackmore and Tootill that "the third integument ...becomes an aril" is totally unsupported by all data I have accessed. In the glossary to Esau K. *Anatomy of seed plants 1977* the definition of aril as "a fleshy outgrowth enveloping the seed and usually arising at the base of the ovule" appears to be in conformity with accepted botanical terminology.

..... from G.J.Swales

When a flower is fertilised the ovules commence to change into seeds. The ovule has both an inner and an outer integument; the testa of the ripe seed is formed from the outer integument and with relatively few exceptions it is the surface of these testa cells that we see when we look at a seed. However, it is of course perfectly correct for R.Mottram to say that when a third layer grows over the outer integument, it is usually an aril. There are several well-known seeds which exhibit an aril that are often quoted and illustrated in text books.

On the other hand an aril is just like the inner and outer integuments in that it is constructed of individual cells, hence it is much thicker than a cuticular layer. An aril can usually be separated without too much difficulty from the testa, although I must say that I have not tried doing this on any *Opuntia* seed. The aril and the cuticular layer are thus two coverings of a quite different nature.

..... from H.Middleditch

In his 'Morphology of Cacti' Buxbaum says of the seeds of *Opuntia*, that 'they possess a so called "third integument"' - inverted commas by Buxbaum around the words third integument. He goes on to say that this "third" integument is not a true integument and therefore not part of the testa, but because it originates from the funiculus it must be termed an "arillus" or "arillus cover" - again, inverted commas by Buxbaum, no doubt because he is using a term unfamiliar to professional botanists.

..... from R.Mottram

And it is the non-uniform membranous coating that I would interpret as the arillus cover.

..... from H.Middleditch.

Indeed some terminology or other must be utilised and arillus is certainly less clumsy than 'brown membranous coating':

..... from R.Bregman

The membranous layer around the seeds of several South American cacti has been bothering me for years. Based on many observations both under the scanning and light microscopes I have come to the conclusion that this layer is of funicular origin. Therefore the term aril or arillus layer is correct. At this point I disagree with Prof. Barthlott from Germany who believes that this layer is the outermost cuticular layer. I am still working on this problem.

..... from H.Middleditch

The use by Bregman of the term aril in this context does not appear to be quite appropriate, if one accepts the application of the term aril as quoted by Corner (*Seeds of Dicotyledons 1976*) and as illustrated by him, also as illustrated by Kapil R.N., J.Bor & F.Bouman, *Seed appendages in Angiosperma*, (*Bot. Jahrbuch. Syst.* 1980). Both works demonstrate that the aril is an outgrowth from the ovule. Although the aril may be parted from the seed where the two are only in contact, the aril must be physically broken off the seed at the point of attachment i.e. where the outgrowth commenced in the ovule. There is no evidence at all that the outermost membranous layer, when present, has grown from any part of the cactus seed. This leaves the term arillus layer as a possible alternative; as noted above, it appears to have been used in this context by Buxbaum.

However Buxbaum says in K.u.a.S 17.6.1966 p.102 'the arillus skin ...on ripe seeds...generally attracts attention by its brown colour. Many "brown" seeds are really black but have a brown arillus skin!'. The inverted commas were applied round the word brown by Buxbaum. It is not clear from this statement whether Buxbaum is referring to the cuticular layer, or the non-uniform brown membranous coating, or both. Certainly Buxbaum did not use an S.E.M. for his seed studies, but only a 30 to 50 magnification optical microscope - see the summary of his equipment listed in K.u.a.S. 17.7.1966 p.124. The existence of cuticular wrinkles was evidently known to him, to judge by the two examples sketched in Fig.13 p.152 K.u.a.S. 17.8.1966 but he does not appear to have discussed this particular detail in his writings on cactus seeds. If he knew about the cuticular layer, was his reference to 'brown arillus skin' intended to apply solely to the non-uniform brown membranous coating?

..... from G.J.Swales.

It would be preferable to try and be more certain about Buxbaum's intentions for the use of the word *arillus*. The English edition of Buxbaum's *Morphology of Cacti* is divided into three sections; section 1, Roots and stems, is edited by E.B.Kurtz; section 2, Flowers, without either author or editor being defined; and section 3, Seeds, again edited by E.B.Kurtz. The editor's preface is dated 1950. What was the actual terminology in the original German edition? Or was there ever a German edition at all? Did Buxbaum's material only appear subsequently in German in the Buxbaum-Krainz *Die Kakteen* over the approximate period 1960-1973? Is there any reference to this so-called *arillus* layer in the German text of that publication, and if so exactly what word does the author use? Did Abbey Garden Press simply make use of Buxbaum's manuscript material and edit it into their publication, inventing a suitable English word for something that had never been published in German?

..... from P.Braun

The data included in Buxbaum's *Morphology of Cacti* which was written in English and published by Abbey Garden Press only appeared in German at a later date in the *Krainz Die Kakteen*; even later still it was republished by the DDR Literaturschau.

..... from H.Middleditch

In the Buxbaum-Krainz *Die Kakteen*, under *Corryocactus*, it is noted that the seed is "starke bedeckung mit einer Arillushaut" i.e. heavily coated with an *arillus* skin. Available information on *Corryocactus* seed is sparse so that whether Buxbaum was studying a seed with a well wrinkled cuticular layer or a brown membranous coating cannot be readily established. Again in *Die Kakteen*, this time under *Pyrrhocactus*, Buxbaum observes that a certain group of species have seed agreeing with the morphology of *Corryocactus* seed in respect of the "sehr dicken Arillushaut" i.e. very thick *arillus* skin. In *Pyrrhocactus* Fig 9 of Buxbaum's *Die Kakteen* the sketches are unfortunately of seeds having two types of surface. One with the uniform overall grey-brown coating that we have seen on quite a number of *Neoporteriae* seed collected in habitat by R.Ferryman, which might be reasonably presumed to be a thick cuticular layer. The other is clearly sketched as a patchy layer so one would reasonably assume it to be non-uniform membranous coating that occurs over the cuticular layer.

Still in *Die Kakteen*, now under *Gymnocalycium*, there is a nice sketch of a seed of *G.baldianum* with an incomplete brown membranous covering, described as a form with a "besonders starker Arillushaut" i.e. an especially prominent *arillus* skin. Is this supposed to be a form of *Gymnocalycium* with a prominent *arillus*, or a form of *G.baldianum* with a prominent *arillus*, or a form of *Gymnocalycium baldianum* seed with a prominent *arillus*? Another nice sketch is of *G. lafaldense* with a partial covering of brown membranous layer described as a "zarte, durchscheinende Arillushautfetzen" i.e. shreds of fragile, transparent *Arillus* skin. Prefacing the discussion as a whole, all the *Gymnocalycium* seeds are divided into eleven types, with a brief description of each type of seed. Here the following appears, with the word *arillushaut* rendered as *arillus* skin:

- 1a. Uruguayense, subseries *uruguayense*. *Arillus* skin absent or only small remnants.
- 1b. Uruguayense, subseries *denudata*. No reference to *arillus* skin.
2. Baldianum. *Arillus* skin well developed and thick, to thin, scrappy, or absent.
3. Lafaldense. *Arillus* skin very thin transparent scraps.
4. Mostii. *Arillus* absent.

No *Arillus* skin on:

5. Unidentified. [Possibly *guanchinense* - H.M.]; 6. *Chiquitanum*; 7a. *Castellanosii*; 8. *Quehlianum*; 9. *Horridispinum*; 10. *Saglione*; 11. *Pflanziana*; 12. *Schickendanziana*.

In his 1966 articles on seed in K.u.a.S. Buxbaum again uses the term *Arillushaut*. However in opening his article concerned with a mucilaginous testa coat on seed of *Hylocereus polyrrhizus* in N.C.S.S. Jnl 18.3.1963 p.40, Buxbaum refers to the 'so-called "aril-cover" in the *Opuntioideae*' - Buxbaum's inverted commas around the words *aril-cover*. One is tempted to wonder if the translator took the German *arillushaut* and made *aril-cover* out of it because he was familiar with the term *aril* and unfamiliar with the term *arillus*?

..... from R.Kiesling, *Maihueiopsis*, *Tephrocactus*... *Darwiniana*, 25(1-4) 1984

The term *aril* is applied to distinct structures which partially or completely surround the seeds and which may originate from the funiculus, umbilicus, micropyle, chalaza or raphe - see Font Quer, *Diction. de Botanica*, 3rd edition, 1970. Thus it is convenient to denominate this "third integument" of the *Opuntioideae* as a "funicular aril", according to R.N.Kapil et al, *Bot Jahrbuch* 101(4):566 198)

..... from H.Middleditch

It would appear that Buxbaum did adopt the term "arillushaut" or *arillus* skin for the additional membranous layer which may be observed on certain seeds. It appears that the use of the term third integument to describe this layer is confined to certain authors and this terminology would not appear to be accepted by botanists in general. At the present time the nature and origin of this extra layer does not appear to have been investigated and no clear appreciation of it seems to have been published. Under these circumstances it might be acceptable to describe it as an *arillus* layer for the time being. We can note its existence or absence on cactus seeds but beyond that it appears to be without value in seed studies at the present time.

GETTING TO GRIPS WITH MAIHUENIA SEED. From R.Moreton

After reading in Chileans '47 about the sort of conditions which cacti seeds have to put up with in the high Andes, I began to wonder if I was trying to get them to germinate at too high a temperature. So I took some pots of *Tephrocactus*, *Maihueia*, and *Austrocactus* seed out of my propagator at 70°F and put them at 55°F. There was further germination, but there was no problem with the root failing to penetrate down into the compost.

..... from H.Middleditch

But that might have been due to putting them through a dual temperature cycle and not purely to the lower temperature. There is still a small quantity of P & W *Maihueia* seed left which could be tried solely at the lower temperature.

..... from R.Moreton

The further batch of *Maihueia* seed have been sand-papered to break through the testa coat. This is not very easy to do, because the seeds are so slippery. I used the usual sort of school rubber with a tiny depression in one end and this is just enough to hold the seed in place whilst it is given four or five rubs with fine grade sandpaper, which is all that I find

is necessary. The seed was then soaked in water for 24 hours and at the end of that time most of the seed had swollen up, which I take to be a good sign. This seed was divided into two parts, one lot being put into a temperature of 55-60°F and the other part put into a cold frame for a few weeks over the coldest part of winter, as I noticed that in one of Ritter's old catalogues he refers to the *Maihuenia* as "Frost germinators".

There were various other seeds put out in the cold frame with the *Maihuenia* - some alpines and even a rose; they must all have been subjected to several night frosts. All the seed that was put out in the cold frame gave quite good germination. By now, some of every sort of the P & W seed has germinated. I think that last year's seeds did not germinate until they had had a cold spell like this. In regard to problems with the long "stem" - or root - on *Maihuenia* at the cotyledon stage, pushing out of the compost, even with a layer of gravel on the surface it still happens.

There is much useful information in Thompson and Morgan's "Growing from Seed", The two following extracts seem to contain some quite useful information:-

Understanding Seed by P.Thompson

Seeds of all kinds of plants respond to their surroundings and use what they sense as a guide to the time when they germinate. Everything they experience has a meaning in relation to this event, and can influence whether the seed remains as it is or produces a seedling. Constant temperatures and total darkness are perceived as the result of deep burial within the soil; temperatures close to freezing point, maintained consistently for several weeks on end are interpreted as the passing of winter, with the promise of spring to come; high temperatures and drought suggest the dangers of a Mediterranean summer. If the origin of a plant is known it is often possible to deduce the way its seeds will respond and to adapt our seed growing conditions accordingly.

Lettuce seeds still germinate as though they were wild plants growing in their original home in the Mediterranean. They still associate high temperatures with the risk of summer drought, and fail to germinate if sown when the soil is too warm. They still associate low temperatures with the security of their winter growing season and germinate, and grow most satisfactorily when they are cool. Celery, appreciated by the Romans, still refuses to germinate when temperatures remain constant, but responds to the fluctuating ups and downs associated with day and night. These are needed to persuade this waterside plant, whose seeds are distributed by floating on water, that it has been stranded on solid ground, where it has a chance of germinating, and is no longer immersed in the water which changes temperature so much less quickly.

Seeds of many species, particularly those from deciduous woodlands, frequently possess complicated and subtle methods of regulating the times when they germinate. Deciduous woodlands are challenging places in which to live. The competition from the overhead canopy of leaves is severe, and many perennial plants persist only if they are able to grow and function at times when the trees are leafless. This means that the seeds must germinate some time during the autumn or winter so that the seedlings can become established in spring and early summer before the tree's leaves are fully grown. They manage this in a variety of ways, but one, which is quite commonly found, is by producing seeds in which the embryo is only partially formed. Inevitably, these cannot germinate until the embryo has completed its development.

Seed dormancy and the inexperienced sower. By J.Kelly

...It is not reluctance to germinate that characterises 'difficult' seeds, but a need to do so in the most beneficial circumstances. Neither is it that these conditions should be the optima for germination itself, what is sought is an environment in which the seedling can grow as safely as possible to maturity

Some seeds, produced in vast quantities by their parent plants, germinate rapidly and wherever there is enough moisture and oxygen and the temperature is right. If thousands perish in a day or a week, the species does not care, enough will survive to perpetuate the line. Others, though, are produced in smaller quantities and must be sure of germinating where years of growth can be considered likely. The mechanisms that such plants use can (for our purpose as gardeners, although not strictly true in the botanical sense) be lumped together under the heading of dormancy.

The different kinds of embryo dormancy are of interest to scientists, but what matters to us is the question of whether the delay in germination is likely to be caused by impermeability or chemical barriers on the one hand, or by the need for vernalisation (the 'experience' of spring) on the other.

Many hard seeds resist the ingress of water until their seed coats have degraded and without imbibition a seed cannot germinate. In Nature this can happen in several ways, many of which serve also to remove chemicals from the seed coats which act as inhibitors as well. A seed, lying in the ground for a long time, will have its coat attacked by fungi, which soften it and eventually break its integrity, allowing water to penetrate. Another, finding itself in the gut of a bird, will endure in a few hours the seed coat destruction from acid that would take a year or two to accomplish in the soil.

Dormancy is often to do with inhibitive substances in the seed coats or with the coats themselves being softened only by prolonged immersion in water. In these cases, soaking will usually break the dormancy..... although gardeners have soaked recalcitrant, hard-coated seeds in sulphuric acid.

What I can say is that soaking in hot water will do just about everything that gut acids or prolonged contact with moisture can do. Abrading the seeds with sandpaper, or chipping them with a sharp knife, are good methods, but tiresome. Water, just a shade under boiling, may be poured over the seeds in a mug, and then left to cool and stand for twenty-four hours.

The vernalisation process is kidding a seed that it has passed through the rigours of a winter and that spring has arrived and all is safe for germination. What you do is to condense the seasons to just a few weeks by alternatively chilling and warming the seeds. Take no notice of those who tell you to freeze them. Germination is all about the movement of water, and it is very still indeed when frozen. Just before it freezes, however, it expands, and that tells the seed a lot, especially as it contracts again on thawing. It is not a bit of good subjecting dry seed to temperature variation of this kind, it must be in contact with water.

Small plastic bags, in which the seed is placed, mixed with a small quantity of damp peat, are the answer. Put in the cooler part of the domestic refrigerator, for three weeks at a time, followed by short periods of warmth in the kitchen, is the answer. After three doses of cold, sowing the whole lot, peat and all, in warmth, should wake up the most sleepy of seeds.

Growing Trilliums from seed By P.Christian

.....very old or very dry seed may be soaked for no more than a few hours in tepid water to which a few drops of washing-up liquid have been added. The washing-up liquid helps the seed to take up water. This is a useful tip also for other difficult or hard-coated seed such as cyclamen, iris, strelitzias and even sweet peas, but long soaking should be

avoided as prolonged and over-rapid absorption of water is harmful to many seeds, not just trilliums, the seed literally bursts internally and dies.

Most gardeners will be familiar with the idea that many seeds require a period of cold before they will germinate. In nature this ensures that seeds of such species do not germinate as soon as they ripen in autumn, but instead wait until the spring when growing conditions are favourable. Trilliums take the idea one step further and many species exhibit a phenomenon called complete double dormancy. This means that the seed requires a cold period followed by a warm period to allow the roots to break dormancy (corresponding to the first winter and spring) followed by another cold period and a final warm period before seed leaves appear after the second winter period. The first leaves do not thus appear until a period of almost 18 months after sowing. It is easy to see why some growers might throw the pots away as useless after just one year.

Chilly treatment for Peruvian lilies By P.Thompson

Garden lore has it that the germination of seeds which are naturally reluctant to produce seedlings is improved by soaking them in warm water for twenty four hours before sowing them. Occasionally this works. This fable, repeated in every book and seed catalogue, sufficed until somebody who really needed to know tried it out and found it was a waste of warm water and time.

At Kew, or to be more precise in the Physiology section located at Wakehurst Place in Sussex, experiments had been going on for some years which looked at the conditions under which seeds germinated. A request arrived from the Parigo Horticultural Company who were breeding new strains of alstroemeria; this request arrived when other experiments were being done on the germination responses of bluebells and the hoop petticoat daffodil, amongst a number of other monocotyledonous plants. These had one thing in common; they germinated more freely at comparatively low temperatures provided that for several weeks after being sown they had been kept very warm. It is the exact opposite of the process which alpine gardeners know so well, and have used for many years, when they sow seeds in a cold frame in January to be frosted and thoroughly chilled; after which the seedlings appear as temperature rises during the spring.

The first experiments produced encouraging results but was never enough to persuade every viable seed to germinate. A treatment was tried out which involved excising a small amount of tissue lying immediately above the embryo, after making a small tear in the seed coat. First attempts were a disaster when this operation was done before sowing the seeds; every one of them rotted away during the initial phase at high temperatures. However, when delayed until this initial phase was completed and done immediately before the seeds were transferred to low temperatures, the removal of tissue close to the embryo was successful, and resulted in the rapid germination of practically every viable seed in the samples being tested.

Among the tested samples were nine different species, almost all of them collected during the expedition of Watson, Cheese and Beckett, for which field notes giving precise information on where they had been collected had been deposited at Kew. These were from a variety of places ranging from sea level to the high, dry, Andean Alps; some from the frost-free, mist-shrouded coastal plain which runs besides the Pacific Ocean near Valparaiso, some from the hot, dry sub-alpine hills and valleys inland around Santiago, and others from more southerly, cooler lowland plains among the forests of southern beech and pine.

..... from R.Moreton

In addition to the Thompson and Morgan "Growing from Seed", if you buy seeds from them by post you get a copy of their "germination times" which gives a list of about 90 different germination requirements and each packet of seeds is coded to correspond. This year I grew some *Pulsatilla alpina sulphurea* and got virtually 100% germination by following their instructions whereas previously I have had no success at all.

There is still one problem with the *Maihuenia* seed. At the cotyledon stage some of the seedlings fail to shed the seed coat and it gets trapped part way up the stem. Although I have attempted to remove this, it usually results in damage to the stem and consequent loss of the seedling.

..... from H.Middleditch

So what are their instructions for growing *Maihuenia* from seed?

..... from R.Moreton

Thompson & Morgan do not provide any instructions for *Maihuenia* seed.

..... from Brooklyn Botanic Garden Record May 1984

Stratification is the storing of seeds for a period of after-ripening in a moist medium at a low temperature, usually a little above freezing. Stratify at 35° to 40°F, which is about the temperature range of the normal household refrigerator. Recommended period of stratification ranges from 15-40 days for *Berberis* spp. to 120-180 days for *Magnolia* spp. [Stratification periods are quoted for 36 spp.]

Some seeds benefit by first being stored at a higher temperature before being given the cold treatment. For example *Crataegus* spp (hawthorn) requires 120 days at 77°F followed by 180 days at 41°F. [Temperatures and periods are quoted for nine other examples].

The term dormancy is applied to the inability of a seed to germinate. Dormancy may be due to factors within the seed itself. In some seeds the embryo itself is not completely organised at the time the seed is shed. In such cases not much can be done to speed up germination. For example, ginkgo and English and American hollies will germinate only after the embryo has had an opportunity to mature at favourable temperatures and humidity. This may take as long as two or three years.

Germination of many seeds is delayed because they have hard seed coats. This condition is particularly common among members of the legume family. A seed coat is protection for the seed, but it can also prevent germination by keeping out water and air. In many cases the effect of the hard seed coat can be overcome by nicking it (scarifying) with a knife or file. Scarification of the seed coat will greatly hasten germination. To scarify a small quantity of hard coated seeds quickly and easily, roll a sheet of emery cloth or sand paper with rough side in, and insert it into a tall jar or can of about one quart capacity. Drop in the seeds, cap the jar, and shake vigorously until the seed coats are worn thin.

GERMINATING TEPHROCACTUS SEED From C.Holland

Over the last three years I have accumulated large numbers of seed of various *Opuntiae*. Germination from sowings has been very variable. My largest sowing so far has been of 390 *Maihuenia philippi* ex KK, but from the look of the seed these are clearly *Maihueniopsis*. They are small and brown, not large and glossy black. This has failed to produce a single seedling, although this is probably not significant as it is quite likely to be poor seed from the supplier than anything else. However, when I received some more seeds from Knize recently, I happened to go out to meet the P.O. van as it drew up. The inside of the van was like an oven and the surfaces of the packages were pretty warm, to say the least - this was early July and real summer outside temperature. Not an ideal environment for seeds, I would imagine. So quite by chance I may have discovered, perhaps, why different seed consignments from the same source can be of such variable germination quality in my experience.

Most disappointing though are various *Austrocylindropuntia* spp. which have been poor or failed in the past and I have also drawn a blank this year despite having tried the new chipping method that I heard about. Chipping I found easier than sandpapering, although you lose some of the seed when chipping by damaging the embryo. Really it is not chipping, more a matter of breaking off a piece of the seed coat. To test this out, I took a batch of 50 seeds of *Maihuenia patagonica*, and chipped about half of them, leaving the others unchipped. No presoaking was given. These seeds were sown in my normal manner, that is in a compost made up of 3 parts grit, 3 parts perlite, 1 part J.I., 1 part sand, and one part peat based potting compost. The type of sand used is quite important though not vital - I use Foremost brand horticultural sand myself. I am not sure if this is a quartz sand or granite, however its structure is such that it provides conditions of water retention and aeration that plants love! The pots are soaked in a solution of Chinosol then put into a plastic bag which is sealed up. They are left in full sun in an unheated, well ventilated greenhouse; at that time of year there is enough sun and warmth to encourage germination. As soon as about 3 or 4 seeds have germinated, I open the top of the plastic bag to reduce humidity and also to introduce some air to the surface of the compost. From then on it is a case of making sure that the compost is kept moist enough so that the seedlings will not dry up. A few more seeds germinate after that. Gradually, over several weeks, the sides of the plastic bag are pulled down in order to acclimatise the seedlings until such time as they can be completely exposed to their surroundings.

The germination was about even between the chipped and unchipped seeds. But from what I have read in Chileans No.47 about germination of the P & W seed, I am now tending to chip the aril more extensively than I have in the past, or in more than one place.

It is surprising that you should ask (in Chileans No.47) if anyone wanted to try soaking seed in enzymes used for home brewing, as I have already done this with both seed of *Maihuenia* and *Maihueniopsis*. The enzyme was obtained from Boots's home brewing section and in fact there were two sorts, one a "Starch and protein digesting enzyme", the other a "Pectolytic enzyme". This is used in the quantity of one heaped teaspoon to a breakfast cup full of water. This is not a method that I use with all my seeds of *Maihuenia* and *Maihueniopsis*; some of them are put to soak for a day in tepid water before they are sown.

Recently I ran out of the enzymes from Boots which I had been using and unfortunately it seems that Boots now only do a pectolytic enzyme, and not the combined proteinase-*anylase* enzyme. However I finally managed to get separate pectolase and *anylase* enzymes from a home brewing shop, but no proteinase. These new batches are infinitely superior in terms of solubility compared to the old Boots material. Merely by inspection they appear to be crystalline whereas the Boots material was like an amorphous powder, forming a suspension as opposed to a solution, most of which would come out of suspension and deposit all over the seeds during soaking. Even so, I have observed partial hydrolysis of the aril e.g. two batches of *Tephrocactus dactylifera*, one soaked in water and the other in a solution of *anylase* + proteinase + pectinase enzymes. The former are obviously wet but the latter, after thorough draining and several washings, were sticky.

As an experiment I divided a batch of seed of *Tephrocactus bruchii* into five separate lots, one of which has been given an enzyme soak, one lot chipped, another lot both chipped and enzyme soaked, another lot simply soaked in water, and a control lot without any pretreatment at all. But only a single germination took place!

..... from H.Middleditch

If indeed the system in nature is for the fruit to be taken by some animal and eaten, then (discounting seeds destroyed in the process) there are likely to be quite a few seeds which suffer mechanical damage due to chewing as well as being soaked in some sort of enzyme, before passing out of the animal. Is it possible that a combination of chipping and enzyme soaking (in tepid water?) may yield acceptable germination results?

..... from D.Aubrey-Jones

It is interesting to hear about the use of the enzymes which have been used to pre-treat some seeds that have proved difficult to germinate successfully. However, the enzymes that have been mentioned above are not really the most effective for digesting cellulose, indeed they probably have little if any useful effect. It is the cellulase enzymes that are required for this. Although the enzymes used for wine-making are available through retail outlets, I would be very surprised if it was possible to obtain a cellulase enzyme through a retail outlet. This is a very specialised material and probably only exists in a few specialised research institutes who are concerned with agriculture and in particular with ruminant animals. Their work involves a detailed understanding of the function of the rumen and for this purpose they need to obtain samples of fluid from the rumen - which will contain cellulase, besides much else. To this end they will anaesthetize an animal and insert a plastic tube into the rumen with an external valve or tap - a very minor operation that does not interfere with the normal functions of the animal in any way. In this way a sample can be obtained of the rumen contents more or less when required.

..... from H.Middleditch

Does this mean that we need to approach an Agricultural Institute that is involved in animal husbandry to see what it might be possible to obtain in the way of cellulase enzymes?

..... from W.Christie

Regarding the rumen enzymes. It is indeed possible for me to get samples of rumen contents. However, you should be aware that rumen contains a highly concentrated population of bacteria, protozoa and fungi and it is these that produce the enzymes, including the cellulases. I doubt whether the fluid travels well as the micro-organisms have to be active and viable. No single enzyme is involved but a family of them, so that isolation of the type of concentrate analogous to that in home brewing is impracticable. If you have a suitable number of appropriate seed, I could do some trial incubations with rumen fluid prior to sowing some seed on your behalf. Incidentally, I tried cutting off the hilum with the extra

Tephrocactus and Maihuenia seeds which you sent to me. I got excellent germination, but all the seedlings keeled over with fungal infections almost immediately.

..... from H.Middleditch

In the course of the last couple of years I have been able to look at or be acquainted with problem seedlings of Maihuenia in various collections - where other seedlings usually germinate and grow on every year in appreciable numbers with no real problems. Almost without exception these Maihuenia seedlings displayed a long stem of barely matchstick thickness at the cotyledon stage. I am beginning to suspect that this is etiolation, which in turn weakens the resistance of the young seedling.

..... from C.Holland

Thank you for the print sent by a Chileans member of his Maihuenia seedlings at the cotyledon stage. I am inclined to agree that the stem may be described as etiolated. My own seedlings have a much shorter stem.

..... from J.Arrowsmith

Some time ago I was at Brookside nursery in north Wales where they were selling small plants of Maihuenia which they appear to have grown themselves from seed. Perhaps we could find out how they set about sowing the seed

..... from A.Butler

We grew the Maihuenias just like any other alpines - sow the untreated seeds in pots in a gritty compost, with a top dressing of grit, water and leave outside exposed to the elements until something comes up. Then bring into a sheltered but unheated greenhouse, to grow on until they are large enough to transplant. Do not be in too much of a rush to bring them in! Ours were fairly neglected so the plants had to be quite big to get noticed!

ARROJADOA FLOWERS From R.Ferryman

In 1978 I acquired some seeds of *Arrojadoa penicillata* which were no problem to germinate and grow on. Within four years the terminal cephalium was produced and flowers appeared. The rate of growth of the stems is little short of phenomenal - about three or four feet per stem per year - so that in order to keep the plant within bounds I have to prune it to within a few inches of soil level at the end of every year before the onset of winter. Some basal shoots several inches long which are the new season's growth are left untouched. They can be relied upon to put on a fair amount of growth in the following spring and to produce a new cephalium and also a set of flowers. A new segment will start growing from the terminal cephalium quite soon after that cephalium is formed. Some stems will produce as many as three cephalia at intervals along the stem in a single season. One or two flowers often appear from the cephalia part-way down the stem; they probably come from buds that were initiated when that cephalium was terminal but were arrested when the plant put its effort into new growth through that cephalium. Once the new terminal cephalium starts to produce flowers then the residual buds in the older cephalium start into growth again.

My plants of *A.dinae* and *A.eriocaulis* were also grown from seed; these are also pruned back regularly. Both *eriocaulis* and *dinae* grow from a tuber which are now beginning to resemble a good *Pterocactus* tuber. Whilst in Holland I visited the collection of R.Klinzer who has a fine selection of *Arrojadoa*, both named specimens and others under field references. He, too, commented that he had to prune his *Arrojadoa* every year. He often gave away numerous cuttings of both *A.dinae* and *A.eriocaulis* but was convinced that they would not root, the only way to grow these two species being from seed. The stem of *A.eriocaulis* is much the more woolly of the two.

On the other hand *A.rhodantha* makes a stouter and more segmented form of growth so it only requires to be pruned every two or three years. With regular pruning of both *A.penicillata* and *rhodantha* the base of the plant became rather untidy so these plants are now the third or fourth generation from seed. It is not easy to catch the flowers open in order to pollinate them. I am pretty sure that the flowers are not open for days at a time but only at odd times, possibly not even during specific times of day. It is just a case of being there at the right time to use a brush to get them to set fruit. Once the fruit is set it develops quite quickly into a berry the size of a small gooseberry, initially black and then turning greenish red. The fruit is full of squashy pulp and will detach from the plant when ripe without splitting. The seed is viable and germinates readily.

Five years ago I was away in Chile during January with the greenhouse heating left to run itself on the thermostat. On my return I was greeted by my neighbour who was keeping an eye on the collection with the sad news that the heating had tripped out and had evidently been off for a few days, when outside temperatures had been well below zero. The max-min thermometer recorded a low around freezing point. To my amazement the anticipated losses did not materialise - perhaps a dozen plants were lost, but it was the taller Brazilian plants which lacked the closeness of other stems and bodies that suffered marking and die-back. *Arrojadoa penicillata* was only slightly marked but the more thin and twiggy stems of *A.dinae* and *A.eriocaulis* were badly marked.

If my *Arrojadoa* were left unpruned for several years they would produce great long stems with a dozen or more cephalia at intervals. In the course of discussing *Arrojadoa* with P.Braun at the 1987 Chileans' Weekend it transpired that this sort of state is rarely attained in the wild. It may be due to animals knocking off or breaking the stems, or even to stormy weather, but some form of natural pruning evidently does take place.

All my flowering *Arrojadoa* can be pollinated without any trouble and do set seed, but I have not found a great interest in the seed. Perhaps these plants were in demand as novelties a few years ago and are now taken for granted. But the ever increasing stringency of import controls may possible change that situation.

..... from J.Arnold

I must admit to being surprised by the suggestion that *A.dinae* will not root from cuttings. My own top cutting of this species is only about 120mm long and it does have a terminal cephalium. At the present moment it has just started to put down roots and I cannot say that it has definitely become established, but it is certainly not showing any reluctance to do so - so far! My plant of *A.eriocaulis* is on a low graft and is certainly woollier than any other *Arrojadoa* that I know of. I also have one or two other *Arrojadoa* grown from seed and I would not have said that they pose any real problem.

..... from W.Christie

My plant of *Arrojadoa penicillata* was grown from Köhres seed sown in 1983. In addition to the main stem, there are about half a dozen stems from the base, none longer than about 20 cm. The main stem is about five feet high and it is managing (just) without any supplementary support. It has now flowered in 1990 for the first time. On the other hand, *A.eriocaulis* has flowered at only 3 years from seed.

..... from P.Bint

In 1976 I obtained a stem of *Arrojadoa penicillata* from Southfield nursery and it put out its first flowers in 1979. Since then it has continued to flower pretty regularly, but only put on a small amount of extra growth. The first few flowers in a season may just come in ones and twos, but the flowers generally appear in groups. Many flushes of blooms occur in the season, invariably influenced by weather conditions. That is to say that hot weather induces flower growth and cool, damp weather inhibits the same. I would agree that an entire flush of flowers do not open simultaneously but I cannot substantiate the theory that they open for a limited period during the day, as mine appear to be open throughout the day. From recollection, they appear to last 48 hours. The idea that an open flower does not have all its petals reflexed outwards agrees completely with my own impressions. *Arrojadoa penicillata* certainly appears to be the most open of floral displays in that the outer petals do reflex somewhat and the inner petals open far enough to provide a tube somewhat in the manner of *Cleistocactus* flowers. My recollection of *A.rhodantha* is of a similar situation but my experience of other species is non-existent.

I did have one other specimen but briefly, *A.eriocaulis*, which was sickly when arriving in my collection and it did not respond to treatment. I have seen slides of other *Arrojadoas* where flowers were not open but that only captures an instant situation. I would gladly grow and comment upon any *Arrojadoas* if material were available. I have been searching the literature available to me and am struck by the dearth of information on this genus. Most publications seem to have heard of *A.rhodantha* and *penicillata* but little else. In 'Cacti and their cultivation' by Martin, Chapman and Auger they are dismissed as 'not grown in cultivation'. Several are referred to in *Chileans Vol.6 No.32* either as nomen nudum or field collected numbers. I wonder where these plants are now? All in all a seriously depleted genus with regard to documentation.

..... from A.Johnston

My two plants of *Arrojadoa* flower and set fruit quite regularly. The fruit is a dark red colour and remains about pea size for a few weeks, then almost overnight it expands to about double that size. It is still dark red in colour round the top but shades out through bright pink to pale pink round the base. The fruit is then somewhat turnip shaped, hemispherical at the top and tapering towards the bottom. If the spines hold the fruit in place it can then be removed very easily, but sometimes a fruit will fall off the plant.

..... from A.W.Craig

In September 1986 I obtained a couple of cuttings of *Arrojadoa penicillata* about 12 inches long, although I did have my doubts as to whether they would survive over the winter. To my surprise they rooted down fairly readily in the propagator and were taken out of it during the month of April. Within two or three months they had produced a terminal cephalium, which then surprised me still further by putting out a bunch of flowers. I was anxious to pollinate the flowers in order to obtain seed; although one or more of the flowers in the bunch was open for almost a whole week, only one (or sometimes two) of the flowers in the bunch were open at any one time and then only for a duration of a few hours. The petals were really very solid, almost candle like in their rigidity so that if the flower was not open it was very difficult to prise the petals apart in order to pollinate the stigma. Attempts to pollinate flowers which were not open met with no success.

When the flower is open the stigma is just about flush with the top of the petals and pollination of open flowers has resulted in fruit of depressed globular shape, about 10mm across. Initially the fruit is immersed in the cephalium so that it is not visible. Apart from trying to prise apart the bristles of the cephalium to see if a fruit has been set, a gentle tug at the dead flower remains indicate its existence. About six to eight weeks after flowering any fruit seems to detach itself from the areole and is pushed partially out of the cephalium, from where it can be simply picked off the plant. The fruit is usually a pinkish green colour, the walls of the fruit are still flexible and are not split, but the seed seems to be ripe at this stage.

In August 1987 I obtained from Windyridge nursery several more short cuttings of *Arrojadoa - albiflora, aureispina, dinae, and eriocaulis v.albicornata* together with two or three which only had field data as identification. These were all about 2 to 3 inches in length and again were set in the propagator. To my surprise all of them had put down roots by October. They have to be supplied with some moisture over the winter or else they are liable to shrink so much that they may not restart into growth in the spring. In order to encourage growth I decided to keep them in the propagator, which is set to bring in the heater below 70°F, during the summer. However the *Arrojadoa* did not seem to like being watered during the summer and showed no inclination to grow; indeed they looked almost unhappy. But in September they all started to put on new growth. My two original plants also put out new growth from this time after standing practically still all summer.

..... from G.Charles

Arrojadoa have surprised me by the long period over which they remain in flower. In 1986 I acquired an *Arrojadoa multiflora* which was grafted on to an *Echinopsis*. In April 1987 it put out a flush of buds, something like a dozen, following this by a further flush later in the same month. It continued on more or less in the same way right up until August. They were quite difficult to pollinate because the flowers were hardly ever open when I looked at them, so they must only open briefly. The petals are very stiff and it is virtually impossible to force them apart. There also seems to be some variation in the number of flowers in a cephalium that will open at one time and the length of time that they stay open. On one particular day in August my *Arrojadoa dinae* had a bunch of thirteen flowers of which only two were open at nine in the morning and they were still open at teatime. Out of a bunch of seven flowers on *A.eriocaulis v. albicornata* three were open at nine a.m., then by lunch time six were open and by three p.m. they were all closing up.

..... from P.Allcock

I do have quite a number of different species of *Arrojadoa* and several of these now carry a cephalium and produce flowers. All of them flower very late in the year, about September-October time, when I imagine that daylight lengths will be about the same as in Brazil. All the flowers in one cephalium do not open at the same time but at some period in the day I may perhaps find all of the flowers open. On *A.rhodantha* and *A.aureispina* the inner petals do not even open out to form a cylindrical tube of petals but remain bent slightly inwards, leaving an opening through which the stamens and stigma can be seen. As I only have one each of these plants I have not tried crossing the flowers to set fruit. But I do have two plants of *A.multiflora* and I have tried crossing flowers between the two plants, both by using a brush and by taking anthers off one flower with tweezers and pushing them on to the stigma of the other flower - but neither method has yielded any fruit so far. The flowers on *A.multiflora* are both longer than on any other *Arrojadoa* flower that I have seen and the petals also open wider, so it lacks the slight constriction at the mouth of the tube found on quite a few species of *Arrojadoa* flowers and has longer, paler petals.

When I first saw a slide of flowers on *A.eriocaulis v.albicornata* at the Chileans' Weekend (from memory it was

shown by G.Charles) I thought it was very similar to Ritter's *A.multiflora*, but now I have had both out in flower at the same time it is easy to spot the differences. On *v.albicoronata* the flower petals are either straight upright or slightly incurved and of course the petals are bicoloured in comparison with the paler flowers on *multiflora*. Fortunately I have four plants of this particular variety of *eriocaulis*, one pair from different clones and another pair with somewhat stronger spines and again of different clones. Flowers have appeared not only from the terminal cephalium but also one or two from an old cephalium part way down the stem; once again, all attempts at setting fruit have not met with success.

My *A.eriocaulis* was raised from seed sown in 1982. The main stem is now some 15 inches high and it has produced a cephalium. There were signs of buds in November 1987 but it was too late in the year for the flowers to mature. So far I have had no difficulty in getting cuttings of this species to root down. Whether grown from seed or from a cutting the "tuber", which I would rather call a thickened woody rootstock, is produced in the course of a few months. Cuttings taken from *A.eriocaulis v.albicoronata* have likewise rooted down without any particular trouble, being no different to cuttings taken from any other species. The woolliest - or hairiest - species is *A.eriocaulis*, which has very woolly areoles all the way down to the base. Short hair in the areoles is a common feature for *Arrojadoa* even on *penicillata* and *rhodantha*, where they are not really very obvious at first glance.

Quite different to all my other *Arrojadoa* is *A.albiflora* because of the stoutness of the body - it must be a good inch in diameter - and the 6mm long white spines, some with dark tips. Without a name is *Arrojadoa HU 364*. Also I do have *A.penicillata v. spinosior* but to me it does not really look a great deal different from the type.

..... from H.Middleditch

My first reaction on seeing *A.albiflora* was one of surprise at the stoutness of the stem, but having been prompted to refer to the original description by Buining and Brederoo in *Succulenta* for Feb. 1975, I see that the stems can be up to 6cm in diameter, which is over two inches thick. This species was found by Buining and Horst in 1972 and again in 1974 near Urandi, in southern Bahia, a place which lies possibly 25 miles away from both Mutas to the north and Monte Azul to the south, from where *A.rhodantha* has been reported. I see that I have a handwritten note in my HU list saying that HU 364 is a form of *Arrojadoa aureispina*.

..... from P.Allcock

Certainly I cannot yet see any difference between HU 364 and *A.aureispina*.

..... from F.Ritter, *Kakteen in Sudamerika Vol.1*

Britton and Rose declared correctly that *Arrojadoa* is day-flowering. Rose must indeed have been aware of that since he portrayed a flowering branch of *Arr. rhodantha*, which he had collected himself in the wild. Werdermann correspondingly wrote about *Arr. rhodantha*: "The flowers open in full sunlight in the morning hours". Backeberg, who never saw *Arrojadoa* in the wild, as opposed to the habitat knowledge of Rose and Werdermann, quoted these extracts, asserting however that *Arrojadoa* is night flowering and comments upon the quotation from Werdermann "This should really read: the solid wide opening flower is still open in the forenoon".

I therefore reproduce my own observations on the *Arrojadoa* flower. 1. Flower from Monte Azul, Minas Gerais. Flower open in the morning with the dawn and closed before dusk. 2. Flower from Ourives, Bahia was still closed at sunrise, opening some hours later with increasing warmth of the day, closed about two hours before sunset, although the temperature had still not fallen. 3. Flower from Mutas near Guanambi, Bahia, was well open at 11.00 a.m. 4. Flower from Anace near Conquista, Bahia; the stem had been collected in habitat the day before; flower was open at the crack of dawn, closing about 9 to 10 o'clock in the morning; the same stem in the following year in open air cultivation in Chile, the flower was still closed at 2 o'clock at night, starting to open very slowly at three hours before dawn and was fully open at dawn; at about 9 to 10 o'clock in the morning it began to close slowly, being fully closed before noon, not opening again.

As may be seen, *Arr. rhodantha* is day-flowering, however in regard to the opening time of the flower it appears to vary regionally within the period of daylight. That will indeed be a reflection of the regionally different species of flower pollinators. For *Arr. penicillata* from Ourives, Bahia, I noted at that location: the flower opens first in the afternoon and already closes after a few hours with the onset of dusk, not opening again. In contrast to that, Backeberg asserts in his handbook "Flowers in the night lax and wide opening.". Similarly my first scan of my further notes about *Arr. penicillata* from Itaobim, Minas Gerais "All flowers of the different specimens in habitat open between 2.30p.m. and 3.30p.m. in the afternoon"; closing time was not observed. The flowering time of *Arr. albiflora* occurs in the morning and midday, that of my *Arr. multiflora* at the time of sundown, after dark it closes again.

VOYAGE IN THE INTERIOR OF BRAZIL, 1830 By Auguste de St.Hilaire.

Translated by H.Middleditch

I left Villa Fanado on 18 May 1817 for the village of Chapada, going in the direction of the caatingas. Having passed through the carrasqueinos around Villa Fanado, I reached a stony terrain covered with trees having gnarled branches. Chapada lay on a lengthy ridge and is overlooked by hills. Around the village much of the original woodland had been destroyed; none of the bushes spread over the ground were in flower. From Chapada I set off for Sucuriu, five leagues away, along a road which rose and fell the whole way. The terrain is reddish, stony, and of remarkable aridity. As to the vegetation, it exhibited a multitude of variations, which were perhaps accounted for by the altitude of the mediocre earth. Throughout this section I saw only one dwelling, scarcely any flowers, few birds and almost no insects other than a few butterflies. On leaving Chapada, the first hills are covered with short trees. Sometimes the carrascos are visible at a lower level. Elsewhere the small gnarled trees arise from a dried up pasture. Finally, on approaching Sucuriu, are the caatingas, trees higher than in the carrascos but less vigorous than the virgin forests, from which they are also distinguished by losing their leaves every year.

Between Chapada and Sucuriu the caatingas are not the only woods which the season had largely stripped of their attire. All the other sorts of wood had suffered the same fate; some trees had retained no leaves at all. Among those I saw almond and peach trees which were covered with flowers before bearing any new foliage. From the tint of purple or yellow in those leaves remaining on the trees I might have imagined myself in one of our own woods at home in the autumn. Here and there some palms were to be seen, with perfectly straight trunks, some 25 to 40 feet high. The dessicated leaves droop around the trunk, surmounted by six green, curved, leaves and crowned by a floral spathe.

At some distance from Chapada I had seen signs of the Caatingas, but beyond the carrascos immediately after Sucuriu I was soon within this singular region and I passed through it over a distance of about 33 Portugese leagues. The caatingas are intermediate between the carrascos and the virgin forest; but they are distinguished from the latter since they

lose their leaves each year. The caatingas occur not only in the province of the Mines but also in Bahia, Pernambuco, Parahyba and Rio Grande do Norte; the woods present great differences in their vegetation in accordance with the latitude. In this area, the rains last six months and cease in February when the temperature falls little by little. Then the leaves of the caatingas start to fall and by June the trees are almost entirely leafless. Then in the month of August the new shoots appear on the trees and quite remarkably this usually takes place before the rains. Soon these arrive, the temperature rises daily and the vegetation regains its verdure.

At the time of my journey, the caatingas were almost entirely leafless. These woods display variations similar to the carrascos, but it is at the margins of their extent that their variations are most prolific. Between Sucuriu and Setuba it formed a thick undergrowth, climbing plants and trees ten to twenty feet high, from the midst of which arose trees of more average height. Between Setuba and Boa Vista, further from the margin of the carrascos, I saw new differences. The trees arising above the bushes were taller and further apart. Great lianas surrounded the trees as in the virgin forests, they hung high up on the branches and made a huge network which crossed in all directions. A much-branched cactus, spiny and deeply fluted, whose stem and branches were no more than the thickness of two fingers, seemed to twine between the leafless branches of the adjacent trees, and by its green colour, contrasted with the grey bush all around. One other cactus, which I had already seen near to Rio de Janeiro (*Cactus heterocladius*, D'Orbigny) raised its branches amidst the maze of lianas, its trunk, which tapered gradually from base to crown, was covered with cushions of spines disposed in a quinaux; it displayed various tiers of verticillate branches, horizontal, rounded and seven in number at each whorl; these branches, like those of the fir tree, are proportionately shorter in length as they occur further towards the top of the plant and in turn they carry secondary branches, flattened and oblongovate, which could in the same way be taken for leaves,

In general the cactus seem to belong to the caatingas next to the Arassuahy and the Jequitinhonha, because I have never come across a single species in the district of the Mines nor in the carrascos. I saw cacti used as hedges on the banks of the R. de Sao Francisco, but I have never recorded in my diary that I had seen plants of these genera, either in the caatingas which adjoins the same river from Pedras dos Angicos as far as Salgado, or in those which are scattered amidst the Sertao. I have in the meanwhile presumed that there are bound to be cactus to be found in those parts of the desert which are dryer than where I have travelled and Messers Spix and Martius have fully confirmed this conjecture. On the other hand I have never seen any melastomaceae in these same caatingas, whilst those with capsular fruit are common in the campos and those with fleshy fruits in the forests.

During the journey from Sucuriu, it was the southern winter and I met with considerably elevated temperatures. The ground was dried up and the grass of the caatingas was scorched by the sun. I found not a single flower in the midst of this, almost no insects, and the birds had largely taken refuge in the vicinity of habitations or on the banks of streams. Between Sucuriu and Setuba the ground was laced with cracks, just like bare patches back home in France at the end of the autumn, which are ponded during the winter. On this arid terrain and surrounded by leafless trees, arose large solitary cacti (*Cactus candelabrisformis* D'Orbigny) of a dark green; their stems displayed five spiny ribs, quite vertical, and of uniform thickness throughout its height, and near the half-way level it carried a whorl of similar branches, curved like a candelabra.

Between Setuba and Fazenda Boa Vista do Calhao, I crossed two rivers which flowed into the Arassuahy - the Setuba and the Gravatai. At Fazenda Boa Vista on the banks of the Arassuahy, the fresh verdure which bordered the river contrasted with the greyish tints of the nearly leafless caatingas. In the caatingas one may see on the trunks of the trees or sometimes on the large branches, hummocks of rounded to ovoid shape. These hummocks, made of earth, are nothing other than the habitations of white ants. When the tree trunk is not very large, the hummock almost entirely surrounds it. The ants reach their home by a track covered by a tunnel almost built of earth, about an inch wide and less in height, which starts at the base of the tree. That night we stopped at Piauhy.

From Piauhy the following day's journey was five leagues as far as Teixeira. Between Teixeira and Inhumas, I passed through countryside scarcely any different from that which I had traversed for several days. In patches with some moisture the vegetation exhibited a fine verdure. In certain places the route crossed extensive black rocks, smooth, hump-backed, which are at the same level as the rest of the ground and form like a sort of pavement. Amidst these rocks arise only some cacti. All around these bare patches were trees of the structure found in the carrascos. Among the leafless trees was one covered with fine yellow flowers.

After travelling 4 leagues from Inhumas, we came upon a hut at Estrato de Sao Joao. The following day we continued through leafless caatinga; in a couple of hollows the trees carried fine verdure, proving a shade quite obscured from the sun. This oasis extended over a distance of no more than one hundred feet, followed again by the caatingas. In other places the caatinga disappeared and we crossed small plains, surrounded on all sides by mountains; these plains seemed to me to have been covered with water in summer; they only carried the odd leafless tree and large herbs scorched by the sun. Elsewhere the route crossed over extensive rocks, black, smooth, hump-backed, just like those which I had seen before reaching Inhumas. Shrubby trees no more than five to six feet high surrounded these rocks and amidst them grew many sorts of cacti of very different forms. One sort, located much closer to the shrubby trees, had a glaucous trunk, upright, spiny, with five sides, four or five feet high, whose whorl of branches curved like a candelabra. A second species looked like a *Cactus opuntia* (*Cactus opuntiodes* D'Orbigny) but the sections of the stem or branch which occur between the joints are not as stout and are more oval. Another cactus, less spiny, reaches barely one foot in height, and forms a stout cylinder (*Cactus brevicaulis* D'Orbigny). Yet others of the thickness of a little finger, crawl like serpents. Finally one species, even more unusual, arises between the slits in the rocks, a hemisphere of three to nine inches in diameter, with six ribs, sinuous, armed with radiating spines; on the crown of this hemisphere arose a cylinder about six inches tall, which carries thick transverse ribs and which is covered by compressed brown wool. The flowers appear at the very blunt crown of this cylinder and due to their smallness they are almost entirely hidden amongst the wool which surrounds them (*Cactus aphananthemum* D'Orbigny).

The cactus are just as common in the woods which surround the place where I stopped, but the bromeliads also grow there in great abundance. These plants grow together there and cover extensive areas, with their elongated leaves. Amidst the rocks I also noticed a small tree loaded with a tremendous number of individuals belonging to the parasitic species of *Tillandsia*, which looked like the bearded *T. usnia* and which moved with the slightest breeze.

As it was more than a dozen leagues from Estrato to San Miguel, travellers with loaded mules must sleep in the open. The air was warm and did not auger a bad night, but I had not long been asleep when the temperature changed, becoming cold and humid. On the following day another four leagues brought us to Bom Jardim. Beyond Bom Jardim a great change came over the terrain; it was no longer open or sandy, the trees were no longer leafless. It was the end of the caatingas.

..... from H.Middleditch

I suspect that the "much branched cactus whose stems and branches were no more than the thickness of two fingers" which seemed to "twine between the trees", was an *Arrojadoa*. On the occasion of the visit of P.Braun to The Chileans' Weekend we had on display a map of eastern Brazil on which were plotted all the locations for *Arrojadoa* which had been found in the literature. The southern most location was indeed in the general vicinity of the confluence of the rivers Arassuahy and Jequitinhonha, close to the area described above by St.Hilaire. In the description of *Arrojadoa penicillata* v. *spinosior* Bng & Brdr in *Succulenta* 50.1:1980, the habitat location is given as 30 km to the west of Itaobim, to the south of the R.Jequitinhonha. This is within a few kms of where St.Hilaire saw the thin-stemmed cactus twining between the trees. The 1980 description of *A.penicillata* v. *spinosior* is also accompanied by a photograph of an *Arrojadoa* whose stems are twining in and out of a low-growing bush.

It is interesting to note the lapse of some 150 years between the first report of this cactus and the recording of the southernmost location for *Arrojadoa*. It is not too difficult to identify as a *Melocactus*, St.Hilaire's low growing globular cactus with the terminal cylinder covered by wool.

..... from Buining & Brederoo, *Pilosocereus magnificus* sp.nov. C.& S.J.U.S. XLIV 1972

Habitat: on dry bare flat rocks close to the Rio Jequitinhonha. Holotype HU 224. Leopold Horst and Buining were at the habit at on 5 June 1968. They found at the same location two unknown species of *Melocactus* and fine groups of *Buiningia aurea*. Among the surrounding dry xerophytic bushes they also found *Arrojadoa penicillata* and *Tacinga funalis*.

..... from H.Middleditch

This *Pilosocereus* is described as "Plant branching candelabra-like from near the base, 1.5 to 2 m high, branches glaucous azure-blue, ribs 6 to 7." I am prepared to accept that this is St.Hilaire's cactus with "an upright glaucous trunk 4 or 5 feet high whose branches curved like a candelabra". The photograph taken in habitat by Buining which accompanies the original description shows some straggly bushes perhaps 3 m high whilst standing immediately in front of them is *Pilosocereus magnificus* - "located close to the shrubby trees" just as St.Hilaire describes.

..... from L.Hoeven

Yes indeed I did find *Arrojadoa* which I did not identify, growing in and out of bushes, at the type location of *Pilosocereus magnificus* which is up the R.Jequitinhonha from Itaobim. In addition I found *A.dinae* in 1983, it grows around the village of Jacarici in Bahia; also *A.eriocaulis*, east of Mato Verde. Then to the west of Grao Mogol I discovered *A. eriocaulis* v. *albicoronata*, before it was described. The typical way it grows is to propagate itself on underground roots and in my opinion it should have been given a name in accordance with this form of growth. All these plants grow in the company of bushes.

..... from J.Arnold

My recollection of the slide of *Arrojadoa eriocaulis* v. *albicoronata* which was shown by L.Hoeven at the Chileans' Weekend is of a group of stems separated by a few inches where they rose out of the ground. As for it being stoloniferous, this is what was said, although I do find it somewhat surprising.

..... from H.Middleditch

Another reference to *Arrojadoa penicillata* v. *spinosior* is made by Heek & Strecker in K.u.a.S. 36.5.1985. In the course of their 1981 trip to Brazil they were travelling south from Vitoria de Conquista along the main highway; at Itaobim they turned up the valley of the R.Jequitinhonha and there met with *Buiningia aurea* HU.116, *Melocactus multiceps* HU.112, *Pilosocereus magnificus* HU.224, and *Arrojadoa penicillata* v. *spinosior* HU.113. Then Heek & Strecker continued up the Jequitinhonha valley to Porto Alegre, opposite Itinga (on the northern bank of the river) where they again found *A.penicillata* v. *spinosior*. For practical purposes they appear to have been in the same area as the type locality for *Pilosocereus magnificus*. It would appear that the cactus described by St.Hilaire as "a stout cylinder reaching barely one foot in height" may well be *Buiningia* (*Coleocephalocereus*) *aurea*.

The location for *A.penicillata* v. *spinosior* may be found on the map of Eastern Brazil which appears on page 113 of Chileans No.44, on which the R.Jequitinhonha is plotted. This map also locates Villa Fanado de Minas Novas, Chapada, Arrasuai (San Antonio de Calhao), and (San Miguel de) Jequitinhonha. These changes in place-names are of course quite common throughout South America, as has been mentioned previously in these pages.

..... from K.Preston-Mafham

All the *Arrojadoa* I saw in Brazil were small plants, usually upright and self-supporting; we did not see any making the large bramble-like growth displayed in the photographs published in *Succulenta*.

..... from F.Vandenbroeck

During our 1989 visit to Brazil we found forms of *A.rhodantha* widely distributed. Other species we found are *A.aureispina* between Caitite and Bom Jesus de Lapa, also different forms of *A.penicillata* and *A.eriocaulis* (possibly var. *albicoronata*) near Grao Mogol. In most cases *Arrojadoa* grows on level sandy ground amidst bushes. In one instance we saw some fine well-developed specimens of *A.rhodantha* growing on a high steep rocky ledge together with *Coleocephalocereus goebelianus*. We also found *A.aureispinus* growing on rocks together with *Zehntnerella*. *Arrojadoa eriocaulis* grew in very exposed positions, but since it had very short stems, it hardly needs any support. As far as I know, the only *Arrojadoa* which twines in and out of bushes is *A.penicillata*. We saw it growing in this way at several places between Iacu and Milagres, near Jeque, and west of Itaobim. I did not see it growing any taller than about 1.5 m and never over the top of any bushes. Its method of growth could be compared with some species of *Monvillea*, which also liked to have some support from bushes or branches. The other species of *Arrojadoa* have stems strong enough to be self-supporting.

The natural vegetation in Brazil has by now been largely destroyed. There are not many places left where the original caatinga forest can be seen. Earlier reports speak of tropical vegetation where now there is nothing left. As an ecologist I was saddened by the progressive destruction of all this natural vegetation. In Minas Gerais the endless monotonous eucalyptus forests are appalling. They seem to be completely devoid of any other plants or any animals. Now and then small patches of natural forest can still be seen (but these are mostly in the process of being burnt) which give one an idea of the original variety of trees, bushes, herbs, epiphytes, and so on. The vegetation upstream from Itaobim, as described by St.Hilaire, no longer exists.

PUTTING THE HEAT ON ARROJADOA From P.Allcock

All my Arrojadoda are grown together in pots which are stood in trays that are suspended over the air heating cable, all surrounded by a topless enclosure of polythene sheet. The warm air will percolate up between the trays. The Arrojadoda look as though they would like to keep growing in the winter so I do have to keep them watered, definitely not with a spray but selectively so as to avoid the roots drying up. If the fine roots are allowed to dry up it seems to be an extremely difficult job getting them to regenerate from the main woody root. In fact the best course is to take a cutting and start again if the finer roots have been allowed to dry up completely. Whenever I see signs of the ribs shrinking on any plant I give them all a dribble of water as I expect the pots to be all equally dry. Feed is never added to the water during the winter months as I do not want to encourage the plants into active growth but just to keep them plump and ticking over.

..... from G.Charles

My plants of Arrojadoda are housed with various other less-hardy plants in a purpose-built enclosure within the greenhouse. This is set to maintain 15° to 20°C by means of a 300 Watt heating cable, which is run round the inside of the enclosure just above soil level. I believe that soil heating cables which are buried in sand work better if the sand is kept moist. But if you want to keep the sort of plants that need this special treatment over winter then you need to keep the sand dry in order to keep down air humidity. Even more important however is the state of the soil the plants are rooted into. It should not be absolutely dry for long periods but just slightly moist. Cables run underneath the pots will dry out the plant compost too much and dry up the roots to the detriment of next season's growth. If the air temperature is to be maintained at say 55°F then soil heating cables will maintain too high a soil temperature for inactive roots.

..... from S.Stringer

All my Brazilian plants are given a free root run in a raised bed with round about eight inches of compost. During winter I have a sheet of polythene draped over a couple of horizontal canes above the tops of the columnar plants. The soil heating cable was in the bottom of the bed, covered by a sheet of plastic, with a thermostat in the air immediately above the soil, set for a minimum of 10°C. In late spring of 1987 I noticed that the *Pilosocereus* were at a standstill when normally they are in strong growth. All the plants were uprooted and all the roots that had grown to within three inches of the heating cable had been completely dried up. Now I have changed to using a tubular heater above the soil.

..... further from P.Allcock

I find it difficult to understand how a soil heating cable covered by a bed of sand (or grit) can function effectively unless the bed of sand is always kept moist, so that the heat can be transmitted throughout the bed by the moisture. In this way a thermostat which is laid upon, or buried in the bed of sand, will function properly. If the bed is once allowed to go dry, I would expect that dry sand will be a fairly poor conductor of heat, so that it will become very hot immediately around the heating cable and this will not be apparent to the thermostat, so overheating will occur and roots will get dried up.

..... from R.Ferryman

All my Arrojadoda are grown in pots sunken into a false bed. Alongside, in similar conditions, are *Discocactus*, *Uebelmannia*, *Pilosocereus*, etc. No heat is added directly to this area but it is at the warmer end of the glasshouse, with 2kW of "soil" heating cable in the immediate vicinity and a further 5kW of heating in the close vicinity. A minimum of 45° is the aim although this year it is being increased to 50° F due to a change in heating patterns. I calculate that I will lose some 10°F in my glasshouse along its length. I have experimented over the years with various methods but remain totally against confinement in hot boxes or propagators. All too often I have heard it said that these warm loving plants do not do well in such situations. Why? Heating cable manufacturers recommend laying the cable in sand and keeping the sand moist in order to avoid hot spots. Few people abide by these rules for over wintering and the result is that hot spots do occur. Even with the thermostat set at 50°F temperature at soil level can be 80°F - higher than they receive during growing times in the wild, so encouraging the roots to grow without any encouragement for the bulk of the plant. The effect is made worse if one uses air thermostats to control soil warming, particularly if plants are packed into propagators (as is usually the case!) without air space between the pots; again an air temperature of 50°F could easily mean 80° F at root source. The plants obviously become weary. Free air circulation and even warmth are the only answer. If one must use propagators, keep the pots apart to allow the air to circulate, leave the propagator slightly open to allow air to leave and enter, preferably two small gaps rather than one bigger one, in order to allow air flow. Closed boxes lead to stale air - poison to plants. I offer this advice based upon practical experience and of course the basics of the technique is how I earn a living!

..... from H.Middleditch

I can understand the point that R.Ferryman makes about the need for air circulation, since the air itself is a very poor conductor of heat. String vests can keep the body warmer than a thick vest solely because of the static pockets of air trapped in it. Double glazing acts as an insulation not because of the glass but because of the insulation value of the static air layer trapped within. Likewise it is the static air trapped in the miniscule interstices between the grains of sand which prevents the heat from a soil heating cable from being transmitted through a dry bed of sand. Although a "soil" heating cable wrapped round the inside of a propagator would set up a circulation of air to some degree, I would have imagined that a surprising temperature gradient could still exist within the propagator. Two thermometers strategically placed may well indicate this.

..... from A.C.J.Hall

My first propagator was one of those small green trays with Perspex domes and circular vents in the top. Regardless of how carefully I sterilised everything I always ended up with a lush forest of liverworts and mosses. I decided that I would have to keep the air moving in order to prevent any formation of stagnant air pockets in which these lower plants thrive. My "propagator" now consists of a four foot long section of staging, enclosed by polythene sheeting over a lightweight frame. The polythene sheeting is run from near greenhouse roof level right down to ground level but is kept about four inches away from the side and roof of the greenhouse. Within this propagator there is room for a four feet wide staging but I only have a two foot six width, standing next to the centre walkway and well clear of the side of the greenhouse. There is a 150 watt heater slung under the staging and a 300 watt soil heating cable laid in a sand bed on top of the staging. These are controlled by an air thermostat set at 20°C.

In order to ensure that there is an adequate circulation of air within this enclosed section, I have within it two small fans, one located in the topmost corner and the other opposite the staging, both directing airflow across the plants on the staging. These are brought into operation at half-hourly intervals by a separate time switch and run for fifteen minutes. This ensures that the warm air gets round all the plants, tall or short, instead of relying on natural convection. The control circuit for these two fans is arranged so that they will also cut in via a relay when the heater is cut off by the thermostat.

The idea here is to ensure that no cold pockets of air are allowed to form even if there is residual heat in the soil bed. In addition I have a second thermostat which is right in the topmost part of the enclosed section, set at 35°C. Once this temperature is reached then a third fan, also right at the very top of the propagator, comes into operation; this draws air out of the top of the propagator and discharges it into the general body of the greenhouse. Whenever the heating is in operation in the propagator section there is bound to be a gradual trickle of warm air through this fan, out of the propagator and into the greenhouse; this will take place even when this extractor fan is at a standstill; this means that there is a small but steady change of air in the propagator. In the course of a day, or perhaps even half a day, I expect there will be a complete air change within the propagator.

Originally I used some inexpensive Japanese made fans as air movers but they had a limited life, so now I am using some four inch German made fans which are probably ex-computer cooling fans. These are obtained from the local electrical bits-and-pieces shop and they have proved to be very reliable at a cost of 16p each. The sand bed containing the soil heating cable is always kept moist as I believe that this improves the distribution of the heat. But the plants and cuttings are never placed on top of this sand bed, always in compost-filled pots or trays which are then stood on the heated bed.

..... from F. Wakefield

My greenhouse is heated by gas and because the heater is right in the middle of the greenhouse I need to make sure that the heat gets all round the greenhouse and does not just lodge up in the roof space. At one end of the greenhouse I have a ducted fan set in the top corner which comes into operation whenever the heater starts to operate. This blows sufficient air through the top of the greenhouse to make a plant in a hanging basket at the opposite end move slightly. At the same time it does not interfere with proper combustion in the heater. This same fan is coupled up via a reverse thermostat so that on a warm day it will also start up and keep air circulating. In this way I avoid cold spots in the greenhouse when the heater is on and I also seem to have kept clear of any problems with scorch in hot weather. I would certainly agree that it is very necessary to maintain a positive air circulation, whether it is a greenhouse or a "propagator" and not just rely on natural convection to try and ensure avoidance of hot or cold spots.

ARROJADOA - Locations

Arrojadoa are to be found growing in the eastern part of Brazil. Almost all the following recorded finding places may be located on the map of Eastern Brazil which appeared in Chileans No.43.:-

albiflora	Urandi
aureispina	NW of Cataite
v.guanambensis	Guanambi
beatae	SW Montes Claros
canudoensis	Canudos
dinae	Urandi
eriocaulis	Mato Verde; Espinoza
v.albicornata	Grao Mogol
horstiana	?Porteirinha; Espinoza
multiflora	Brejinho
penicillata	Calderao; Ourives (Itaobim); Villa Fatima to Feira de Santana; Jequie to Vitoria de Conquista
v.decumbens	Iacu; Milagres to Jequie
v.seabrensis	?Seabra
v.spiniosior	E. of Itaobim; W. of Itaobim - Porto Alegre
form	Porto Novo
rhodantha	Sao Raimundo de Nonato; Anace; Monte Azul; Ourives; Mutas (Guanambi)
v.occibahiensis	Serra Ramalho
v.reflexa	Rio Paramirim

PHOTOGRAPHING WHICH TEPHROCACTUS? From J.R.Kirtley

During the journey through Bolivia in November 1989, undertaken in conjunction with Brian Bates, Tephrocacti were observed growing both on the altiplano and in the southern highlands. In attempting to identify these plants, I have placed them into five groups, based upon my notes and photographs. These groups are broadly based on the style of growth and the general appearance of the plants. Those plants which I would place in the first group were observed at a number of places between the shores of Lake Titicaca in the north and on the main road approaching Potosi, in the south, at altitudes between 3500 and 4060 m.

After leaving La Paz and heading south over the altiplano, the very first cactus that we met with was a few km south of Calamarca, a Tephrocactus growing on flat, stony ground, only a few metres from the road. Here we found four small plants with tufts of grass growing among the segments, each plant being about 10-15 cm in diameter and 4-6 cm high, of an overall green colour. The base of the plant was raised four or five cms above the otherwise flat ground. This gave the distinct impression that the ground at one time had been level with the base of the plants but that over a number of years the ground surface had been scoured away by the strong winds which we encountered on the altiplano, leaving the plants raised up on a miniature pedestal of ground. We quickly got used to seeing the clouds of dust raised up by a whirlwind over the altiplano. From the top of a suitable hill we could see several of these clouds of dust coming from all directions. We were caught in these dust clouds once or twice - first you saw it, then you heard it, then you were in the middle of it, then quite abruptly it was gone.

Each of the segments on these plants is divided into very elongated tubercles, which taper off downwards to merge into the body and terminate abruptly at the upper end so that the top of the tubercle forms a sort of step, with an areole on top of each step. The size of the segments could not be determined as all of them were partially buried. Only the upper areoles of each segment (even new segments) carried spines which were from 2 to 5 cm in length, projecting straight out from the plant or bent outwards from the segment, reddish brown to straw colour. The flowers were about 4 cm in

diameter with yellow petals shading to deep yellow at the tips. The outermost petals (not the uppermost scales) bore a distinctive red tinge; the red colour could also be seen on the backs of the petals of the unopened flower buds shortly before they open. The anthers, filaments, style and stigma were whitish yellow. After travelling south for a further one km, more plants with this same appearance were found growing among grass tufts on a steeply sloping hillside, among rocks and boulders in company with forms of *Lobivia pentlandii*.

On the road from Panduro to Eucalyptus, observations were made of *Tephrocacti* growing at four different locations. A few km from Panduro a sprawling clump with pale spines and emerald green epidermis was photographed right on the edge of the road. The hummock was one metre by half a metre across and 30 cm high. The segment size appeared to be larger than on the other *Tephrocacti* which we had seen up until this point. The buds and flower remains indicated a yellow flower.

At some 2 km before Eucalyptos we met with plants similar to those seen at Calamarca, growing deep in the flat stony soil with only the upper portion of each segment above ground. The segments were again very tuberculate. Flowers yellow, about 5 cm diameter, with broad petals, but here some flowers had green stigma lobes.

At a location some 8 km from Eucalyptos there were plants of similar appearance growing in similar circumstances as at the foregoing site. At this location the spines tended to be straw coloured. For the first time some fairly short thin spines were noticed at the base of the emergent flower buds. One plant had half a dozen buds with scales at the top of the tube so closely spaced that they almost formed a complete ring. We also found the same hummock-shaped sort of plants growing vertically on the banks of dry streams which might suggest that they had started off growing horizontally but stream bank erosion had left them suspended in a vertical position. On the hill behind Oruro more hummocks of *Tephrocacti* were observed which were similar to the other plants seen up until this point but they had a denser spination. Here the *Tephrocacti* were growing in company with *Lobivia pentlandii* forms.

About one km before we reached Pazna and at no great distance from Lake Poopo we stopped briefly and found one (and only one) large hummock of *Tephrocactus* some 80 cm across and 60 cm high, which was growing on the slope above the road among granite rocks. A fair bit of mining appeared to have been done in this locality in past years and this rocky slope could have been a tip. The spines were nearly white, very pale compared with other *Tephrocacti* that we had seen up to now. Flowers 4 cm in diameter, golden yellow, paler towards the centre, petal tips verging to orange, with short, thin spines around the top of the tube. There were numerous flowers on this plant, all emerging in a group on one side of the plant, up to as far as the crown of the hummock. On first looking at my slide of this plant, I took the flowers to be on its northern side, but after seeing that the shadow of the hummock was cast at the same side as it bore the flowers, I came to the conclusion that the flowers were all on the south side of the plant.

To the south of Pazna, at Challapata, the road turns away from the altiplano and into the mountains. At Cieneguillas, some 35-50 km north of Potosi, there came the first real change in the appearance of the *Tephrocacti*. Plants forming both small and large hummocks grew here in close proximity to one another, although only one small hummock was photographed. This had the same sort of stepped tuberculate segments but also displayed some differences, compared with the plants that we had seen up to that point. This plant formed a tighter hummock, the segments were about the same size but with a much denser, straw coloured, spination that blended in well with the dry leaves of the grasses growing through it. The hummock was about 45 cm diameter by 20 cm high. At the top of the tube there was a dense ring of long thin bristly spines which were so long that they reached virtually up to the mouth of the open flower. The flowers were up to 5 cm in diameter, tangerine red, the colour appearing to become denser towards the tips of the very broad petals. There were a large number of reddish yellow filaments, the stigma lobes, style and anthers being whitish.

When returning to La Paz later on in the course of our trip, a stop was made close to Panduro where hummocks of *Tephrocacti* grew both in deeply shaded spots between bushes and among large rocks and boulders as well as on exposed open ground. On the shaded plants the segments had little or no spination, perhaps because they did not require the spines until they outgrew or lost the shade. Those plants growing on open ground bore the normal spination.

From La Paz we had a run out to the shores of Lake Titicaca where we found more small hummocks of *Tephrocacti* which were similar in all aspects to those found at Calamarca and also between Panduro and Eucalyptos.

..... from H.Middleditch

In Chileans No.46 a summary was provided of those parts of Bolivia which Pentland visited during his residence in that country. In effect he entered Bolivia by the traditional route from the coast at Cobija inland to Oruro, then on to La Paz. Whilst in Bolivia he travelled between La Paz and Sucre and he also visited Cochabamba. In addition he made a trip to Arequipa. It is quite probable that the plants which were later named *Tephrocactus bolivianus* and *T.pentlandii* were collected by Pentland in the course of one or the other of these travels. They were most likely to be growing close alongside his route rather than some distance away from it. The roads which run between La Paz and Sucre remain unaltered from Pentland's time until today, so that the plants seen close to the highway by J.Kirtley would be the same sort as those seen by Pentland. Thus it is quite probable that the original *T.bolivianus* or the original *T.pentlandii* came from one or other of the locations noted above by J.Kirtley.

Although a steadily increasing number of European cactophiles now visit Bolivia almost every year, few - if any - seem to have paid the same sort of attention to the *Tephrocacti* which lie alongside this main route as our two Chileans' members. The slides which were taken of the *Tephrocacti* at each location mentioned above were shown at the Chileans' 1990 Weekend and although the plants were not all of identical habit, they were certainly similar. It appears that Kiesling has not visited Bolivia and almost certainly has not travelled the road from La Paz via Potosi to Sucre. Hence he has not seen the sort of plants which we saw on the slides taken by J.Kirtley. On that account one might question the basis which Kiesling uses to separate what he understands as *T.bolivianus* from what he understands as *T.pentlandii*. Evidently he does see two distinctive sorts; but the sort to which he attributes the name *pentlandii* does not appear on the slides taken by J.Kirtley.

It appears that Pentland visited both Cochabamba and Sucre, so there is always the possibility that Pentland may have travel led from Cochabamba to Sucre. This would involve taking the traditional road leading out of Cochabamba in roughly an eastward direction. Following this road roughly half way to Santa Cruz, he would then turn more or less due south and continue in the same general direction until he arrived at Sucre. At an elevated point on the first half of this route, near Tiraque, is to be found a *Tephrocactus*. Both K.Preston-Mafham and L.Hoeven showed slides to the Chileans' Weekend of this sort of *Tephrocactus*. In comparison with the plants photographed by J.Kirtley, it appears to consist of rather smaller segments which are very closely packed together, with spines less in number and shorter in length. It is always possible that Pentland may have collected a specimen of this plant as well as a specimen of those growing between La Paz and Oruro. On arrival in Europe the two sorts would certainly have displayed a different habit and may possibly have formed the basis for the original introduction of the names *bolivianus* and *pentlandii*. More detailed

information about the *Tephrocactus* found along the road to the east of Cochabamba would probably make a valuable contribution to this particular problem.

PUSHING OUT THE FRUIT - PUSHING IN THE DENTS? from F.Fuschillo

Of the seed which I have looked at and photographed, provided they are fully ripe, most appear to be well formed without any abnormality. On one occasion I looked at about 1500 seeds of the *G.pflanzii* group and they were one and all pretty much the same.

But very often Brazilian cactus seed have flat or slightly concave patches instead of the normal convex shape. Many of these seeds have humped testa cells but these humps are very poorly developed, if at all, over the flat or somewhat concave patches. It looks as if the seeds have been pressed together when ripening and have indented each other. If the seed on *Melocactus ripens* before the fruit is squeezed out of the cephalium, then surely the fruit must be very compressed whilst it is still within the cephalium. Do other Brazilian cephalium bearing plants also ripen their seed whilst the fruit is still within the cephalium? Is this why the seeds are so often mis-formed?

..... from S.Stringer

Although I have sometimes found completely flat seed in *Melocactus* fruits I suspect that this is caused by a physiological defect rather than compression within the cephalium, the seeds being sterile. I have also noticed that *Melocacti* fruits are produced within the less dense part of the cephalium which covers an area from the centre to approximately half way across the radius of the cephalium and I think it is unlikely that sufficient pressure will build up within this area to compress the seeds out of their proper shape whilst they are being formed. In addition, some *Melocacti* have cephaliums which are primarily composed of woolly hair rather than bristles e.g. *M.guitartii* and *M.maxonii*, so that upon examination it will be found that there is ample room within the cephalium for the fruits to develop. By comparison I have a plant of *M.zehntneri* with a very tight mature cephalium which in recent years has shown an increasing tendency to retain the fruits within the cephalium when ripe rather than expelling them - is this a common occurrence? These fruits can be removed with tweezers when dry and invariably contain ripe, perfectly formed, seed.

..... from A.Hill

On my *Melocactus neryii* there are ten rings of dried fruit. All but three of these fruits show either the tuft of the dried flower just sticking up out of the wool of the cephalium, or a very short part of the seed case - about 4 mm. The other three fruits are sticking well up, to a length of 9 mm; these are on the most recent flowering rings. Tugs at both fruit immersed in the cephalium and those projecting out of it suggest that there is no real difference in the way the fruits are held. The long fruits feel just as tight as the others. I must admit that when I first heard the stories about fruit being expelled out of the cephalium I thought it was a nonsense.

..... from W.Clarke

From April to September scarcely a day will pass without a flower on one or other of my *Melocacti*. Shortly after the last of the flowers, the first fruits begin to appear and continue to be produced into the next flowering season. It would seem that the fruits take about six months to mature and are produced over the same length of time that the plants were in flower i.e. six months or so. The fruit certainly comes out of the cephalium very promptly; I can walk in the greenhouse one day and see no fresh fruit on a cephalium, the next day there can be half a dozen fruits there. All fruits appear from the top of the cephalium and not from the sides. I have never seen a seed pod being pushed out of the cephalium and this prompts the question "Does it only happen at night in the dark?" I am inclined to think so.

The fruits on my *Melocacti* are all conical in shape, two and a half to three times as long as broad, tapering to a sharp point at the bottom. Size varies, as does the colour, from red on *M.ernestii* to pale pink on *M.multiceps* with others in between. Seed pods produced by HU 436 and *multiceps* in particular, are nice and fat when they pop out of the cephalium. These fruits are not squashed flat by the tightness of the cephalium but quickly deflate and become flat after they have been pushed out. My *M.multiceps* is about six inches in diameter with a cephalium about two inches high, shaped like a short cylinder with a rounded top. The older part turns black in colour and old dried-up fruits project from the sides - they were originally on top but come on to the sides as the cephalium grows. *Melocactus* HU 436 is almost 5 inches in diameter with a cephalium barely 1 inches across, only slightly humped. Both of these plants were grown from seed. Then again until this year I had never seen a fruit pushed out from *M.ernestii*, although the colour of the fruit can be seen here and there over the cephalium in winter. In the summer, when these fruits have dried up inside the cephalium, they betray their existence by the tiny dried up flower remains. Then they can be pulled out and many of them do contain seeds. It appears to me that only fruit containing seed are pushed out of the cephalium but those which are not pushed out may or may not contain seed. Dry fruits can be rubbed between the fingers and to release the seed, most of which germinates well if sown.

..... from A.W.Craig

I have a few *Melocacti* which carry a cephalium. They all flower profusely and appear to be self-fertile as the seed pods are always full of seed, which germinates almost 100% after sowing. The seed pods get pushed out of the cephalium very quickly. There can be no visible fruit on a cephalium early in the day and yet the cephalium can push out dozens of fruit before the day is out. On a good warm day 30 or 40 fruit pods can appear. The cephalium is so compact you wonder how they could all have been fitted in. How big can the fruit become before it is pushed out of the cephalium? The fruits look to be turgid as soon as I see them, but I would not be sure if the seed was ripe at that point in time. I would not expect that the wedge effect of fruit shape was sufficient to push it out of cephalium. If the fruit is tugged gently a day or two days later it will detach; after a few days it will be pushed completely out of cephalium, the fruit severed from plant. Sometimes fruits drop right off the plant and into the pot.

..... from P.Leigh

I have several cephalium sized *Melocacti* and have grown *M.conoides*, *bahiensis*, and *matanzanus* to the cephalium stage. The seed pods certainly differ in the way they behave. The seed pods are not pushed out of the cephalium at all on *M. matanzanus*, they tend to dry up still within the cephalium and have to be pulled out using tweezers in order to have a look at the pod and the seeds. They are often very difficult to pull out. On *Melocactus neglectus* the seed pod emerges for a quarter of its length without ever being fully ejected from the cephalium. The seed pod on *M.bahiensis* is expelled for three quarters of its length when ripe; several weeks later, when it has dried up, it tends to come out completely and fall down on to the top of the cephalium. I do wonder if the ejection process is determined by how the plant is growing at the time?

..... from J. Arnold

Some fruits are indeed held erect above the cephalium on a number of *Melocacti*, whilst others are pushed right out. But I do not think that this difference occurs between one species and other but rather at different times of the year on the same species. I will keep a look out for *Melocactus* fruit beginning to appear.

..... from G. Charles

I do have a number of Brazilian *Melocacti* with cephalia. Most of the fruits seem to stay put in the cephalium and dry up in situ, but certainly I find some fruits which have fallen out whilst still red and swollen. Whether both occur on the one plant, I will have to check.

..... from R. Ferryman

Yes, fruits do set on my *Melocacti*. If left they will simply be pushed out of the cephalium but in the past I have removed them while they are still within the cephalium and they have torn away. Nothing dramatic you understand, just a simple tear. Certainly I have come across fruits on both Brazilian and Cuban *Melocactus* that have dried and remain within the cephalium. This is most evident if a plant dies, when if the cephalium is pulled apart I have found several fruits contained within it in different layers i.e. not just a single season's fruit.

..... from J. Griffin

In my own collection there are three *Melocacti* that are carrying cephalia. On *M. broadwayei* the cephalium is very bristly indeed all over; on *M. conoides* the cephalium is much more woolly but there are bristles projecting out of the sides of the cephalium, whilst on *M. glaucus* the cephalium consists only of wool. These plants are kept in the propagator over winter and not given any water; *M. broadwayei* has had to be kept indoors because of lack of room in the propagator, but it has also been kept dry. On *M. broadwayei* the flowers appear in ones and twos, from almost anywhere in the cephalium, from near the middle to close to the edge of the cephalium. Even in the middle of winter this plant has put out one quite turgid fruit at a point only about 1 cm from the edge of the cephalium. The fruit is filled with a runny jelly and holds only about ten seeds.

Melocactus conoides pushes out dozens and dozens of fruit, which fall down on the cephalium and even tumble down the plant into the pot. These are about 2 cm long and are quite flat when they are pushed out of the cephalium. It is quite easy to see the black seeds through the wall of the fruit and when the fruit is opened there is no pith at all inside it. The inside is packed with seeds, there must be about 25 in a fruit.

My plant of *M. glaucus* was grown from seed and the body is now over 4" wide. Over the winter of 1988/89 it started to show a thickish tuft of wool right in the crown which grew rapidly during the following season into a cephalium. By the following winter the cephalium already measured about one inch across. It has had an enormous number of flowers in the course of the year, sometimes in a complete ring, and has pushed out a great many fruits. Some of these have fallen off the cephalium and either become caught up in the spines or fallen right down on to the pot. By the middle of winter some of these fruits have now dried off and these were probably formed earlier in the season, but there are still one or two turgid fruits among those lodged in the spines or lying on the pot, so these are probably more recent. In addition to the turgid fruits projecting from the cephalium it is also possible to see dotted here and there below some of the flower remains the pink tops of more fruit which are still two or three mm within the cephalium. The expelled fruits are fat, short, pink and about 1 cm long. They are turgid and full of pith but the wall is a translucent pink colour so that it is still possible to see that there are seeds inside, numbering about 15 to 20.

..... from P. Allcock

On my own *Melocacti* I find that the fruits do come up out of the cephalium but they simply stay standing upright for a while, perhaps a day or two, and then fall down. I am convinced that the seeds are ripe before the fruit is pushed up out of the cephalium. If a fruit is partially projecting out of the cephalium it can be pulled away from the plant and it will leave a hole in the cephalium where it came from. By touching the cephalium around this point the wool and fibres forming the cephalium will close up almost immediately and the hole will disappear. But if a fruit is pushed out by the cephalium, there is no sign of a hole where the fruit came from. This would make you think that the fruit had been squeezed up out of the cephalium. When a fruit is examined while it is still fresh it is semi-transparent, sufficiently so as to be able to make out many individual seeds; it also contains a fair amount of squelchy pulp or jelly like material. Could this squelchy material be dry whilst the fruit is ripening and then when the seeds are ripe does this squelchy material take up water, so expanding the fruit into a slim conical shape so that the fruit forces itself out of the cephalium?

..... from M. Muse

The fruits which have appeared on my *Melocactus disciformis* HU.132 and on *M. glaucescens* HU.219, are both pink and tapered cylindrical, round at the base with a basal aperture. The cephalium does close up after the fruits are expelled and no exit pore is apparent after a few days. In the course of examining the cephalium under a 10 times magnification it is obvious that the cephalium bristles are modified spines since they are tapering, awl-shaped at their junction with the cephalium, and pointed at the tip.

The HU.219 has a cephalium 4.5 cm in diameter and a body of 9 cm diameter which is still undergoing rib division at four separate points. The outer part of the cephalium is pinkish white due to the reddish colour of the bristles whereas the centre is white. The entire cephalium is studded with barely visible dried flower remains which form a circle within the inner white portion of the cephalium. In early February the floral remains on HU.219 protruded a mere 2 mm above the cephalium and were immovable, indicating that the fruits were still attached. Last summer, fruits littered the cephalium, the flanks of the plant, the top-dressing at the base of the plant and the bench around the pot. There must have been 50-60 fruits in all and those that I examined contained few seeds, remained turgid for several weeks and were slightly acid to the taste. I did not measure the fruits or the dried floral remains. Because they remain turgid for so long they also bounce off the spines when they topple from the edge of the cephalium, which is why some were found on the bench.

The cephalium on HU.132 is also pinky white, 4.2 cm in diameter on a body 11 cm in diameter, where rib-division has ceased. This cephalium, too, is packed with dried flower remains which radiate out from the centre of the cephalium in a counter-clockwise series of spirals. Both plants were watered in the second week of December. On 5 January the upper portion of a fruit showed itself above the wool and bristles at the outermost edge of the cephalium on HU.132; by 14 January the fruit was almost clear of the cephalium and canted over at an angle. Within a week it was no longer turgid, but had started to dry up. The floral remains are complete, pink at the tips of the withered petals and measuring 8 mm long overall. The fruit is tapering, carrot shaped, whitish around the basal pore and 2.2 cm long. A second fruit was evident only by the 8 mm long floral remains protruding fully above the cephalium. A gentle pull indicated that the fruit was already detached from the plant body. These floral remains are well in (about 1 cm) from the edge of the cephalium. In contradistinction to the solitary fruits which appeared very early in the year, during May about 30 fruits appeared

which were not turgid, but flattened and translucent so that the seeds were visible.

..... from F.Vandenbroeck

Whilst we were travelling in Venezuela we met with many Melocacti. To the east of Chacopata we came across a population of Melocacti where every possible stage of flowering and fruiting could be observed, even on one and the same cephalium. To the north of Cariaco there were many fruits protruding from the cephalia which were damaged so that the black seeds were visible in a whitish jelly. We were later given to understand that the birds seem to eat the fruits. Fruits toppling off the plants and on to the ground are quite frequent.

..... from K.Preston-Mafham

In Mexico, in Peru and in Brazil the fruits pop out of the Melocactus cephalium and you find them lying around the place. I have never seen anything take the fruits off Melocactus, but I can imagine that rodents might take them at night. I have a photograph taken in Mexico of a grasshopper eating a Melocactus fruit.

..... from H.Middleditch

Is it possible that any other Brazilian cacti may ripen their seeds when their enlargement is constrained?

..... from R.Ferryman

You can walk past *Coleocephalocereus* which has been in flower and there will be no sign of fruit. After attending to one or two jobs in the greenhouse you can walk back again and the *Coleocephalocereus* will have a full size fruit. They come out just as fast as *Buiningia*.

..... from A.W.Craig

I can imagine that the seed could be forced pretty firmly against each other whilst a fruit is still in the cephalium of a Melocactus, but I cannot see this happening with seeds in a *Discocactus* fruit which is well clear of the plant body. So I cannot see this accounting for the flat patches on the S.E.M. shots of seed of *Discocactus multicolorispinus* in K.u.a.S. 32.3.1981.

..... from H.Middleditch

But in that very publication alongside the other S.E.M. shots is one of a part of the flattened patch of seed coat which shows both the usual rectilinear outline of the testa cells together with another offset pattern of similar outlines. As the author indicates, this second pattern has been impressed from an adjacent seed, whilst the testa was in the process of hardening. So appreciable pressure between adjacent seeds does appear to occur in a *Discocactus* fruit, however unexpected it may be.

..... from P.Allcock

The flowers on *Discocactus* die down almost level with the body of the plant, and then the fruit elongates upwards, which is why it is whitish in colour. This may also account for the seeds being pretty well compressed against each other in the early stages of the fruit formation.

EOMATUCANA OREODOXA By F.Ritter

Translated by H.Middleditch from *Kakteen in Sudamerika* 4.1981.

Body very soft and fleshy, in nature quite level with the surface of the ground or humped hemispherically upwards, but with age the underground part extends appreciably, in that the older parts of the body are drawn deep into the ground. The body terminates below with a robust swollen root without a narrowed neck. Scarcely or not at all offsetting of its own accord but frequently grazed by livestock as a result of which large clumps often form. Individual flowering size heads are 25-80 mm across, the crowns barely depressed, spiny. Ribs on flower bearing plants 7-12, very blunt with narrow, markedly snaking furrows and rounded flanks, 3-6 mm high, with rounded tubercles; the notches insignificant or up to half the depth of the rib furrows, often with horizontal cross furrows (see picture). Areoles few, white felted, 1-3 mm diameter, at the highest point of the tubercle, not raised, 8-15 mm apart. Flowers from the crown, actinomorphic, 4-6 cm long, slender, opening wide, without scent. The flowers open initially in the hours of darkness or not until the daylight of early morning; they close halfway through the afternoon, to open again in the dark or towards morning. Pericarp green, even reddish brown towards the top, without tubercles, about 3 mm long and thick, with some narrow, triangular, very pointed greenish scales of 1.2 to 2.5 mm in length, with either a few white hairs or quite bare. The outside of the wall (the cortical side) is not hyaline. Nectar chamber tubular, 8-15 mm high, 1.5 to 2 mm broad, with little nectar, pale, often orange or vermilion towards the top, and becoming narrower upwards without any thickening of the wall so that on account of the bases of the lowermost stamens projecting outwards towards the style it is closed off without open access. Upper part of tube slender, funneliform, 13-30 mm long, 5-7 mm wide at the junction with the petals, white internally, greenish to red-brown externally with a few more or less red-brown scales with some above where they change over to outer petals. Tube mostly bare, sometimes with a few white hairs. Filaments white below, usually lobster red towards the top, anthers and pollen pale yellow; stamen insertion distributed over the whole of the upper part of the tube, not crowded at the top margin; the anthers reach more or less to the base of the petals, to almost half way up them. Style almost white to pale green, 35-45 mm long, of which the 1.5 to 3 mm long slightly spreading bright green to bright yellow stigma lobes lie between the anthers or somewhat higher. Inner petals spread in a funnel shape and are often bent outwards, 15-25 mm long, 5-8 mm wide, somewhat spatular with a narrower base, pointed to almost rounded above, bright golden yellow to orange red below, orange to lobster red towards the tips.; the outer ones narrower, more or less with longer pointed ends, somewhat shorter and purple; all colours are of high density. Five flowers were recorded from different specimens at the Type location. Spines 4 - 12, straight or bent, needle-like, flexible, half porrect or more so, brownish becoming grey, with distinctive dark tip; spines around the areoles, when there are one or two in the middle these are 1.5 to 4 cm long, the remainder 7 to 30 mm long. Fruit like a *Matucana* fruit, some 14 mm long, 8 mm thick, fairly flesh-like, only 3 mm thick above, pale green, red-brown above, somewhat rib-like, otherwise smooth, furnished like the pericarpel; the fruit splits open into vertical strips in the lower half of the sides almost as far as the bottom, and the edges of the slits fold inwards thus widening the gaps out of which the seeds fall from the threadlike dried up funicles; the funicles are little branched or unbranched. Seeds about 1.7 mm long, 1.3 mm wide, 1.0 mm thick, prominently arched dorsally; the testa black, dull, with rows of fine humps lined up alongside each other; the hilum very oblique ventrally, oval, about 1 mm long. Type locality in the high mountains on both sides of the ravine of Rahuapampa, at an altitude of about 3000m., Department Ancash, Peru. Discovered by me in 1964.

..... from H.Middleditch

There is a part of this description which puzzles me - the mention of shallow cross furrows as well as horizontal

cross furrows. However, the photograph in K.in S.A. shows a plant with what I would consider to be a pretty hefty horizontal wrinkle running across two or three ribs in the lower part of the body. Perhaps this is what is meant by 'horizontal cross furrows'? Do the flowers open and close in cultivation in the manner which Ritter indicates? Surely this is not usual for *Matucana* flowers? If his statement is correct it would suggest that these particular plants are orientated towards a different pollinating agent compared to all the rest of the *Matucana*.

..... from P.Conrad

It will be almost exactly twenty years ago that I managed to purchase a plant of *Eomatucana oreodoxa*, the first I had ever seen at that time. It was a two headed specimen that was obviously quite old. This plant measured about six inches across showing only about 1 inch of plant body, the rest either having become very corky and lost its spines or else having been below ground level. The roots were very large and tuberous. It established itself fairly quickly and then became covered with offsets - as many as 21 - coming from the old spineless areoles. The really surprising thing was that the offsets formed aerial roots when very small indeed, when only 3 or 4 mm in diameter.

..... from G.Charles

A KK collected plant of *Matucana oreodoxa* that I obtained was only 1 inch across but the root was very nearly as thick as the body. My other plants of this species are of unsubstantiated origin, although one came from De Herdt as a fair sized plant in the early 1970's and this flowers well. Because of the rootstock, these plants require a wider pot than you would expect from the size of the body, rather than extra depth. Seedlings also produced a thick root, although they are really too small to compare yet. and it is too soon to look at the offsets which have been taken off and rooted down, I also have one offset from A.Craig from his Lau 273 and strangely enough this has not flowered.

The flowers are symmetrical, of varying tube length and the colour also varies from yellow-orange to red. The fruit is quite like that of other *Matucana*, less fleshy than *M.madisoniorum/paucicostata* and the fruits on my *M.oreodoxa* were also smaller, but still splitting open with several vertical splits.

..... from A.W.Craig

On a visit to Denis Sargent in the Isle Of Wight in 1972 I purchased a couple of imported plants of Lau 273 *Matucana oreodoxa*. One of these is two headed and has a very large tuberous root. The second one has eight heads, each up to 1 inch in diameter, with an enormous tuberous root that nearly fills a 4 inch diameter pot. Neither of these plants have produced any flowers. In addition I have a plant of KK 1591 which has a generally similar appearance to the Lau plants but carries spines which are more curved.

One of the several offsets was removed from the Lau 273 and grafted, then subsequently re-established on to its own roots, where it has set about producing a thick rootstock of its own. The spination is about twice as long as that on the plant it came from. It has now grown to a head of three inches in diameter which is about twice the size of the individual heads on its parent plant. It has now started to put out offsets of its own. Also unlike its parent, this plant now flowers. The flower stands straight upright, with a yellow tube and orange-red petals. There are tiny dark scales along the tube, the uppermost scales having an orange red margin with a dark red median stripe that runs well down the tube. Flowers have also appeared from the sides of the plant.

..... from S.Stringer

I have two plants of *Matucana oreodoxa*, one a seedling and the other a KK import. Both have very large rootstocks, which grow about twice as fast as the body (and rot very easily). Although I have had both of them about seven or eight years, neither have flowered yet, nor have they produced any offsets.

..... from D.Aubrey-Jones

One of my plants of *Matucana oreodoxa* came to me without any data on its origin. It put out a flower in mid-March which is different from any other *Matucana* flowers I have seen. In particular, it closes up partially at night time and I am fairly sure that the flowers on all my other *Matucana* stay more or less wide open during the night. Also it has very few scales on the flower tube whereas my other *Matucana* flowers display a goodly number of scales on the tube. The flower is almost rotate, just very slightly one-sided. The stamens are not bunched up together like they are in all my other *Matucana* flowers, but spread out like they are in, say, *Rebutias*. This plant is in a square pot which is amply large enough for the size of plant body, but I can see that the sides of the pot are bulging visibly, so I will have to move it into a larger pot later in the season.

At the end of March both my other two plants of *M.oreodoxa* came into bud. The smaller plant of KK origin and some 2 inches across, bore five buds, whilst the larger plant, some 3 inches across, had about a dozen buds. This gave me an opportunity both to take a flower section and also to set some fruit. I have already taken some time lapse photographs of other *Matucana* flowers and in conjunction with measurements of the flower it became clear that all parts of the flower continued to grow after the flower first opened, but at different rates. This produces the well-known phenomenon of the anthers overtaking the stigma during the period that the flower remains open. This does not happen in *M.oreodoxa*.

At the same time that one of the buds started to open on *M.oreodoxa*, a bud also opened on my *M.aurantiaca*. Of course the flower on *M.oreodoxa* is much taller and more slender than the flower on *M.aurantiaca*, but they are both nearly rotate. To my surprise they both started to close up very slightly between 2 and 3 in the afternoon, closing up altogether quite rapidly at about 4 pm. They opened out together again early the following morning. By 8 am in the morning the flowers were at least 75% open. So my own observations appear to agree with Ritter's statement on the flowers of *M.oreodoxa* - "The flowers open initially in the hours of darkness or not until the daylight of early morning; they close halfway through the afternoon, to open again in the dark or towards morning".

..... from R.Bregman,

Succulenta 68.12.1989

The two species *Matucana oreodoxa* and *M.aureiflora* seem from the point of view of habit rather to belong at home in the *aurantiaca*-group but from the point of view of the seed structure they both belong at home in the *haynei* group. Also the flower departs so markedly from the typical *Matucana* flower that Ritter feels obliged to give both sorts a separate status, respectively belonging to the genus *Eomatucana* and the subgenus *Incaia*. Yet both sorts form typical *Matucana* seed and fruit, for which reasons we provisionally attribute these plants to the genus *Matucana*, pending further ideas.

Seed ca. 1.2 to 1.7 mm tall, 1.3 to 1.8 mm broad, 0.8 to 1.0 mm thick, embryo with sharply bent cotyledon. Dull black, covered with irregularly tattered yellowish-brown arillus remnants; dorsally and ventrally little or not broadened; outer surface with irregular grooves and humps; cuticle with multiple creases; testa cells at the apical part somewhat convex, at the basal part quite flattened. Hilum usually somewhat sunken; hilum material well developed so that the "hilum-cup" is thereby completely filled, at best there is a little aperture in the hilum at the height of the funicular remnant.

Succulenta 69.1.1990 Translated by W.W. Atkinson

Matucana oreodoxa is distinctly different from the other *Matucana* spp., in flower structure as well as in habit. The flower in particular is unique in the genus, although it mostly resembles *M. madisoniorum*. The flowers of both spp. are slender, actinomorphic, and totally lack the ring of primary stamens at the bottom of the flower tube, which close off the nectar chamber in other *Matucana* spp. On the basis of this similarity, Ritter put both spp. in a separate genus: *Eomatucana*. We have already written that this is based upon an unfortunate mistake; the two species are definitely not so closely related. As against the above-mentioned similarities of flower there must be taken into account a few notable differences: as well as the taxonomically less important differences such as the length and colour of the flower and the hairiness of the tube, *M. oreodoxa* is notable for the very fleshy petals, and the fact that the flower stays closed at nights and in the early mornings. The flower of *M. madisoniorum* stays open day and night.

Is *M. madisoniorum* less an *Eomatucana* (=primitive *Matucana*) more a really highly developed species? *Matucana oreodoxa* could very well be the relic of an ancient form; the plant grows in a region where other original *Matucana* spp. grow, the areole is small, and until now no near relatives have been found. Again, the embryo of *M. oreodoxa* is fairly primitive in form, certainly for a *Matucana*; it strongly resembles that of *Cleistocactus*. From an evolutionary standpoint, it could be argued that Ritter's view, that *M. oreodoxa* should be put in a separate genus, is not so daft after all, but in view of the similarity to *M. madisoniorum* in flower structure, the setting up of a new genus *Eomatucana* should be abandoned for practical reasons. It is virtually impossible to separate a genus *Eomatucana* (without *M. madisoniorum*!) from the genus *Matucana* by means of a description. Characteristics of seed and fruit cannot be used for this, as *M. oreodoxa* forms typical *Matucana* seed and fruit.

Matucana oreodoxa was discovered by Ritter in 1964 and described by him in 1965. The type specimen is deposited at the herbarium of the University of Utrecht.

Distribution: *Matucana oreodoxa* has a small distribution area, in the valley of the R. Puchca, a tributary of the Rio Maranon. The type location is in the hills on both sides of the Rahuapampa saddle, Dept. Ancash, Peru. According to Ritter it grows there at an elevation of 3000m. *Matucana oreodoxa* seems to be rare in nature. Many have sought it in vain.

Succulenta 69.3.1990 Translated by W.W. Atkinson

Concerning taxonomy, more or less the same can be said for *M. aureiflora* as for *M. oreodoxa*. *Matucana aureiflora* also differs from other *Matucana* spp. in habit and flower. The presumption exists that this is also a relict species which has had an independent development over a long time. Perhaps it is an intermediate form between *Matucana* and *Oroya*. Further research should show this. The position of the spines certainly looks more like an *Oroya* than a *Matucana*. The flower is also very strange for a *Matucana*: short funneliform, like a *Mila* or a *Lobivia* flower and golden-yellow in colour.

For this species Ritter introduced the subgenus *Incaia* (originally written mistakenly as *Incaica*) thus a rung lower on the taxonomic ladder than *Eomatucana* which was given the status of a genus. Apparently Ritter found the presence or absence of a diaphragm at the bottom of the flower of significant importance in this respect. In contrast to all other *Matucana* spp. (Except *M. madisoniorum*) *M. oreodoxa* lacks the diaphragm, but it is present in *M. aureiflora*, albeit useless because no nectar is produced. According to Ritter this flower is pollinated by bees which only collect pollen, and not by nectar-sucking humming birds, as is the case of the other *Matucana* spp. This seems a plausible explanation for the disappearance of the red flower colour. Red is recognised by birds but not by bees.

Matucana aureiflora was discovered by Ritter in 1964 and described by him in 1965. The type specimen is deposited at the herbarium of the State University of Utrecht.

Distribution. *Matucana aureiflora* is only known from the immediate neighbourhood of the town of Cajamarca, Type location Banos del Inca, near Cajamarca, Peru. Populations of *M. aurantiaca* occur in the same area, a fact which Ritter perhaps took into consideration for his opinion that *M. aureiflora* and *M. aurantiaca* are closely related. In general people decide (too) quickly that plants growing near to each other are therefore closely related. This is not however always the case, and certainly not in the relationship between *M. aureiflora* and *aurantiaca*. The two spp also differ ecologically. *Matucana aureiflora* grows on grassy slopes with mineral soil. In the area we searched, the number of ants nests was high. Some plants grew on a nest (or rather in it, for only the top of the plant stuck out). Evidently seeds are distributed by the ants. *Matucana aurantiaca* grows (at least near Cajamarca) amongst rocks where some humus has collected. The difference in substrate shows in the different root structures. *Matucana aureiflora* has a tap root; *M. aurantiaca* has a fine, surface root system. Unlike *M. aurantiaca*, *M. aureiflora* is easy to cultivate, but difficult to flower.
..... from D. Aubrey-Jones

From the translations of the Bregman articles concerning *Matucana* which I have seen, it puzzles me as to why Bregman creates a group within *Matucana* which includes *oreodoxa*, *aureiflora*, *haynei* and *comacephala*. As Bregman himself suggests, *oreodoxa* and *aureiflora* are in many ways rather different and would appear to be outliers of the *Matucana* group. His sole reason to form this group would appear to be seed structure. From the brief examination that I had been able to make of *Matucana* seeds, I doubted whether the seeds of the above mentioned species were much different to quite a number of other *Matucana* species.

Working from the seed photographs that Bregman uses to illustrate his articles, together with a friend who has done some work in the past on cacti seeds, we sorted the species blind. That is, we erased the species names so that we did not know which seed was from which species and simply placed like with like, or with nearly like. The exercise was quite revealing and the following species appeared to show a broadly similar seed structure: *huagalensis*, *intertexta*, *celendinensis*, *myriacantha*, *haynei*, *aureiflora*, *oreodoxa* and *ritteri*. Indeed, the seed of *aurantiaca* and *fruticosa* appear to be not that dissimilar to the foregoing group and they could all be put together in a large group.

Obviously this is far from being a rigorous piece of scientific research. Ideally many seeds from each species should be examined under the microscope directly and should be measured, etc. Nevertheless the foregoing should provide a rough and ready test of major seed differences.

..... from H. Middleditch

In the above series of articles by Bregman, the description of *M. oreodoxa* is to a large extent a repeat of that given by Ritter, with two exceptions. Ritter provides no details of spination, whereas Bregman does. But Ritter states categorically that the owing to the lowermost parts of the lowermost filaments projecting out from the tube wall towards the style, that in *M. oreodoxa* the nectar chamber is closed off without ready access - "geschlossen ist ohne freien spielraum", whereas Bregman states equally plainly that the flower "lacks the ring of primary stamens at the bottom of the flower tube which in other *Matucanas* close off the nectar chamber". Surely both these statements can hardly be correct?

PROBLEM NEOWERDERMANNIA from K. Preston-Mafham

On our way from Oruro to Cochabamba we came across a patch of Neowerdermannia - there were hundreds and hundreds of them at that particular spot. These plants flower in mid-winter, when the days are warm but the nights are sub-zero. We did collect some seed but only three seeds germinated out of a couple of hundred.

..... from R.Moreton

Certainly I find Neowerdermannia give very poor germination results with me. 1% is good! Of various species of seed which I received from R.Ferryman, the Neowerdermannia were the only ones that gave no germination.

..... from H.Middleditch

Is it worth suggesting a soak in warm water and then a 60 day spell in the domestic refrigerator before sowing?

PROBLEM PILOSOCEREUS SEED From R.Moreton

The Melocactus seed which had been collected by Vandenbroeck during his visit to Venezuela germinated fairly well, but nothing in the Pilosocereus. This is another group which habitually gives poor germination. Are there any ideas for getting improved results?

..... from W.Christie

Most of the seeds collected by Vandenbroeck are doing well, only a few not germinating, including the Pilosocereus species.

..... from J.Griffin

Some of the Pilosocereus seed has germinated for me.

THE PROBLEM ERDISIA - OR AUSTRACACTUS - OR OPUNTIA. RMF 81 / 101

..... from H.Middleditch

From slides shown at the Chileans' Weekends it is perfectly clear that the seeds of these plants are distinctly different from those of other Erdisia or Austrocactus seed. They have been photographed by F.Fuschillo, but we do not have them on an S.E.M. photograph. It is just possible that this could be arranged, if half a dozen seeds can be obtained for this purpose.

..... from R.Ferryman

None of my plants and cuttings set any fruit this year. It may have been due in part to my being away from home at the best time for pollinating them. But having said that, quite a lot of flowers aborted this year, which rather surprised me. Given better luck in the coming season it may be possible to provide 5 or 6 seeds for an S.E.M. photograph.

..... from G.Charles

Last year I was very fortunate in having a flower on the rooted cutting which came originally from R.Ferryman's collected plant. When I had a close look at the flower, the stamen arrangement was quite clearly the same as we have seen on Austrocactus, both in this journal and at the Chileans' Weekend. There was a bunch of stamens near the base of the style, then a wide gap free of stamens up to the base of the petals, where there was a ring of stamens. This is quite different to the flower section shown to us by R.Ferryman and illustrated in this journal, but what is perhaps even more surprising, the field number was the same in both cases. As all my spare time was being taken up by site preparation for the replacement greenhouse, I was not able to take a slide of the flower section, but I will certainly do so when the next opportunity presents itself.

..... from J.Arnold

But that really is most extraordinary. How can there be two sorts of stamen disposition in two flowers on what is effectively the same plant?

..... from H.Middleditch

Does this help to explain why the description given by Ritter of the flower section on this plant (reproduced in Chileans No .47) differed from the slide and sketch of the flower section taken by R.Ferryman?

AND A PROBLEM PYRRHOCACTUS from F.Vandenbroeck

We left for an extended trip to Argentina on November 6th and came back on December 27th, visiting not only the Sierra Cordoba, but also travelling from the Quebrada Humahuaca, via Salta and Cafayate, down to Mendoza. Once again we had a look at the Pyrrhocactus near Malargue which we had come across during our trip in 1986, but this time it had some yellow flowers. This species also occurs near San Rafael and on the Cuesta de los Terneros. We made some very thorough searches but we did not find Ritter's *P. platyacanthus*. We did find plants with flattish spines, that looked very different from *P. straussianus*, but later on they turned out to be *Austrocactus*.

..... from H.Middleditch

In his *Kakteen in Sudamerika* Ritter gives body dimensions of 6-7 cm diameter and 20 cm tall for his *P.platyacanthus*; he does not describe the flower, but gives "numerous white bristles" on the pericarpel and tube, suggesting that he may have found a dried-up flower. The seed "is 2.5 mm long, 2 mm wide, and 1 mm thick, the basal end almost snout-like with the hilum in a ventral notch; testa dark brown, matt, with large cross-ripples, the whole covered with very fine tubercles; hilum ventral, roundish, white, sunken; above the basal point, slightly separated from the hilum, lies a small micropyle."

From the review of *Pyrrhocactus* in Chileans No.45 together with the comparison between *Austrocactus* seed and *Pyrrhocactus* seed presented in Chileans No.46, there does not appear to be anything in Ritter's foregoing description which would categorically exclude this plant being an *Austrocactus*.

SOME PARODIA NAMES from J.Lambert

In talking to the 1991 Chileans' Weekend about the Parodias which I have seen in Argentina, I made use of certain specific names which, if one adheres to the ICBN Rules, are to be considered invalid. Indeed, in a note which appeared in *Bradleya* 3. 1985, Urs Eggli published a classified list of "Backeberg's invalid cactus names", among which are to be found no less than 16 species and 8 varieties of Parodia. With few exceptions, the main reason for invalidation of the names is the fact that no holotype was deposited. Hence article 9.5 of the ICBN Rules was transgressed, and consequently Eggli sets out to write down his list with the cold precision of a Swiss watchmaker. Moreover, as article 9.5 was only enforced since 1.1.1958, he logically applies it only to the names published after this fateful date. Which leads to the aberrant situation that names published in part 1 of "Descriptiones Cactacearum Novarum" (1957) are valid, whilst those published in exactly the same way in the subsequent parts II and III are invalidated!

If one accepts this view, later synonyms of which a type has been deposited will replace the original names given by Backeberg. However, in all cases where no such synonyms are available, anyone who would like to put his name after a species might deposit a type, give it any arbitrary name, and simply duplicate Backeberg's original description! This is hardly the purpose of the ICBN and it seems to me that here is a case where one should avoid a too rigid application of the rules. In other words, let us discard the letter of the law, to consider its principle, which is to maintain stability and avoid confusion. Indeed, when accurate descriptions, complete with latin diagnoses and illustrations were published, there is but little room left for confusion, even in the absence of Type-material.

In our particular case, there is not a single Parodia connoisseur who would hesitate as to what is meant, for example, by *P.fescheri* or *P.tafiensis*. Let us suppose for just a moment that Backeberg did deposit types of the controversial species, but that these somehow got lost or destroyed afterwards. In such a case, there would be no problem of invalidation, the only requirement being that neotypes be deposited in replacement of the lost material.

So why could not this solution be adopted? It would solve the problem in a simple and elegant way, confirming names that have been in use for decades now. In addition, it would be paying a tribute to Backeberg, who certainly made a number of mistakes, but to whom we are still indebted for the greatest monument ever written on Cacti. Of course, I am well aware that such a decision would suppose proposals for conservation by the Nomenclature committee on Spermatophytes, which would in turn need to be approved by the General Committee. So let us hope that this present plea may be heard or read by some competent people, who might be willing to take it into consideration.

..... from H.Middleditch

In pointing out the position with certain Backeberg names in relation to the relevant ICBN Rules, Urs Eggli does no more than document the factual situation. But, as J.Lambert observes, this leaves a rather unhappy vacuum. Anyone who wishes to do so can select what they consider to be a suitable specimen of one of the invalid Backeberg Parodia species, deposit it in a recognised herbarium, and erect a new epithet for it, with their name as author. In this way we will be burdened with more synonyms. It would be a relief to have something comparable to "*Hatiora herminiae* (Campos-Porto & Castellanos) Backbg ex Barthlott comb. nov.; synonyms *Hatiora herminiae* Campos-Porto & Castellanos 1941; *Hatiora herminiae* Backbg 1959, nom.invalid (Art.33.2)" which tidies the situation quite neatly without any clutter of avoidable extra names. It also circumvents the bureaucratic maze pointed out by J.Lambert.

..... from W.Verheulpen

Some of these names have already been changed by Weskamp, as follows:-

P.tolombana Wesk. sp.nov. Syn. *P.rigida* Bckbg, *P.dextrohamata* Bckbg. Type DH135 from near Tolombon
P.cebilarensis Wesk.sp.nov. Syn. *P.rubelliamata* Bckbg. Type DH 113 from El Cebilar, Quebrada de Rio de Sauce.

P.cachiana Wesk. sp.nov. Syn. *P.kiliana* Backbg. Type DH 104 from Cachi Adentro
P.lembeckei Wesk. sp.nov. Syn. *P.tafiensis* Backbg. Type P.157 from Abra del Infernillo.
P.tumbayana Wesk. sp.nov. Syn *P.setosa* Backbg. Type P.171 from Tumbaya
P.belenensis Wesk. sp.nov. Syn. *Parodia fuscato-viridis* Backbg. Type JL-164 from Quebrada de Belen.

THE LUMPING ALTERNATIVE From F.Wakefield

I have been checking my plants against the names that are published in the Preston-Mafham Dictionary of cacti and the contraction in the number of species is staggering, especially in the Neopteraria. The photographs are very good and in most cases it is very easy to recognise the plants, but when one looks at the number of species contracted into one, it is not so easy, as there is no indication of the range of variation. I have always thought that this genus is particularly variable and that some of the discussion on identification made far too much of minor differences.

CHILEANS 1992 YEAR BOOK - COMPENDIUM OF FIELD NUMBERS

This publication updates the Lau, HU, Rausch and Ritter field number lists which appeared in our 1972 Year Book, together with the addition of half a dozen further field number lists. Because of both its bulk and availability, the KK field number list is not included. This compendium is intended to assist in identification of a plant for which a field number is known, and also to enable an impression to be obtained of the approximate locality in which each number occurs. Pin-point locations are not provided. This publication is available from the Chileans' Membership Secretary at a cost to members of £6.40 (inc. p & p), and £7.10 or \$18 overseas.

SUBSCRIPTION RENEWAL - CHILEANS VOLUME 15

A subscription renewal form for Chileans Volume 15 will be enclosed with the present issue. Almost all back numbers are still available from our Membership Secretary.

TRAWLING FOR GYMNOCALYCIUM - WITH A FINE NET From F.Fuschillo

Since my visit to the I.o.S Congress in Frankfurt I have been able to keep in touch with a number of cactophiles in

Europe who have been very helpful to me with information about *Gymnocalycium*. There are a group of *Gymnocalycium* enthusiasts in Austria, who produce their own periodical, and it appears that four or five of their members are making regular trips out to Argentina to search out *Gymnocalycium* habitats. As far as I am aware, Strigl and Neuhuber are among their number. It sounds to me as if they set about this by dividing up a patch of ground on the map into even smaller areas and then fly out there to go over each piece of ground almost inch by inch, in search of *Gymnocalyciums*. In this way they must be collecting some very valuable information and getting to know a great deal about these plants.

..... from H.Till

We have applied ourselves to the problem of the individual spp. in their habitat, not only at odd places as has generally already been done, but also to the investigation of their whole distribution and multiplicity of forms. So we arrange for each trip, which area will be explored by preference. The produce of each journey then comes to me and all are evaluated by me according to a predetermined scheme. Only thus do we obtain in the course of time a good and also correct survey of the genus *Gymnocalycium*, its species and forms. So my friends fly each year to Argentina in two or three groups, in accordance with the route we have planned. As a pensioner I myself am naturally not able to afford a trip each year, but I am looking forward to my fourth trip to South America.

In 1987 I stood with Franz Strigl gazing at the west side of the Sierra Ambato, which rises up to about 3000 m in height; as Rausch has rightly said, everything is on a much larger scale than the mountains I know in my homeland. So Rausch tells me that to get to the site of *G.tillianum* from that spot and back, plus looking for the plants, needs three days. Already I have made three visits to the Cerro Uritorco in the Sierra Chica, in Cordoba province. This is a large mountain and I had already spent several days searching over it, and I know it from the north to the south-west side. We had found several populations of *G.andreae* elsewhere in the Sierra Grande, always in small local populations, and in August of last year we also found a population on the west side of the Cerro Uritorco. Of *G.riojense* alone I have already come across 62 individual populations, and there one must learn to pay close attention to the boundaries with the related forms like *G.kozelskyanum* or *G.piltziorum* and several more.

On my 1990 trip to Argentina I once again obtained some interesting information. Once again I was able to establish that individual species are restricted to a certain area, to which they are confined by specific barriers which they are not able to overstep. In this way isolated populations are formed, between which no gene interchange can take place. I therefore consider that it is essential to investigate the spatial extent of the isolated populations. Hence I have again been able to establish that much information which we have obtained from earlier field collectors was inaccurate, most were even incorrect.

..... from K.u.a.S. December 1991

There will be a spring meeting of the International *Gymnocalycium* Friends in Salzburg from 3-5 April 1992, on the theme of *Gymnocalycium riojense*.

AND A FINE NET FOR NOTOCACTI from N.Gerloff

So far I have been able to make two visits to Rio Grande do Sul. On my 1991 trip I travelled for a week with Nilson and Hamester, from Porto Alegre via Santa Maria, Santiago to Itaquí on the R.Parana, returning via Uruguiana, Alegrete, Bage and Cacapava do Sul, with many side diversions enroute. This was followed immediately by a trip with Nilson and Stockinger from Porto Alegre to Sao Gabriel, Dom Perdido, and Bage, with a run to the Uruguayan border and also to Pelotas, before returning to our starting point. In the course of my journeys there I have probably established something approaching two hundred individual locations for *Notocacti*.

My friend Konrad Herm, who is Editor of *Internoto*, has also visited Rio Grande do Sul in company with W.Uebelmann in 1988 and last October he made another visit there.

..... from A.Hofacker

My second visit to Rio Grande do Sul was made in 1990 and so far I suppose I may have come across some one hundred and fifty separate locations for *Notocacti* there.

..... from J.Brickwood

The Hungarian Cactus Society have produced a fairly comprehensive review of *Notocacti* field numbers. Apart from extracts from the Ritter, Rausch, van Vliet, Knize and HU field number lists, it also includes lists of *Notocacti* collected by Arzberger, Gutierrez, Stockinger, Ruoff, Nilson, Polz, Prestle, Schlosser, Waras, Knoll, and Abrahams. Some of these amount to a column or two, but the list from Abrahams runs to 386 numbers and that from Prestle to 450 numbers. There is also a correlation list of field numbers and locations (such as there are) in alphabetical order of species. These would all appear to be almost exclusively from Uruguay and Rio Grande do Sul.

ONE THAT GOT AWAY? from R.Moreton

Recently I acquired a small plant of *Melocactus gruberi*. Do you know anything of it? From a series of articles in K.u.a.S. in 1983-84 I see that Gruber was in Venezuela so I presume that it is a Venezuelan species. Even as a small plant it is quite distinctive which is more than can be said for a good many *Melocacti*.

..... from H.Middleditch

A search of the indices of *Succulenta*, etc., has not brought to light either a description or other reference to *M.gruberi*.

..... from R.Gruber

Up to now the *M.gruberi* has not been described but I have heard that it will shortly be described by N.Taylor. Both N.Taylor and D.Hunt have visited all the places where I came across cacti during my trip to Venezuela.

..... from D.Hunt

Publication of an illustrated report on the visit to Venezuela has been deferred while work is being undertaken on an overall checklist of cactus names.

..... from R.Gruber

All my slides, and notes, and the plants originally collected in Venezuela have now been sent to Zurich.

..... later from R.Moreton

From the review of *Melocactus* in *Bradleya* 9/1991, I see that *Melocactus andinus* has been validly published, synonym *M. gruberi* n.n.

This jubilee is more than just grounds for pride and joy. It should also be an occasion for serious reflection. The author has taken over as first Chairman at the request and suggestion of the D.K.G. Council in a difficult situation. The Society today no longer has its earlier importance. On a broad front it has become disassociated from scientific botany; it is hardly involved with the I.o.S which is steadily growing in importance. Our editorial board receives no longer sufficiently suitable manuscripts; instead large working groups with to some extent eminent publications have separated off. Important areas of research are published above all in anglo-saxon countries. As a result [our] membership numbers are declining.

....Succulenta in 1992 From Succulenta 12.1991

At the beginning of 1980 we had 4075 members; in 1985 there were 3040, in 1990 we still had 2750 and now there are about 2400. In future the magazine will appear once every two months instead of once every month.

PITAHAYA FRUIT From R.Moreton

Recently I saw some fruit on sale in Sainsbury's supermarket described as Pitahaya fruit from Columbia. Attached to it was a short length of three-winged stem with what looked like the odd areole, so I am wondering if this is a cactus fruit. I thought that I would easily find out what it was, but even after perusing all my cactus books, I am not much wiser. Perhaps it is *Mediocactus*? The fruit is yellow in colour and fairly large for a cactus fruit, roughly the shape and size of a large lemon, (12 cm long and 7 cm thick) quite tuberculate, like a pine cone when open, but having a scale or leaflet at each hump. When it is cut open the wall is about three mm in thickness, quite turgid and the inside of the fruit is filled with a juicy pulp. Embedded in this are the seeds which are just over a mm in size, but there is far more pulp than seeds. Having extracted most of the seed I put it to one side for a while. When I came to sow I first gave it a soaking in water and was surprised to find it swelled up until it looked just like frog spawn. This could be an outer coating and it might inhibit germination. I have sown some of the seed and wait to see what comes up.

..... from H.Middleditch

One of these fruits was brought to the 1991 Chileans' Weekend and cut in half; the general consensus was that it was reasonable to eat. The pot full of seedlings was also brought along by R.Moreton, the seedlings being three winged and at that time about four inches tall. Looking through the pages of Backeberg's *Die Cactaceae* I was struck by the apparent large size of the fruit on his Fig.711 *Mediocactus coccineus*. The "young fruit" in this illustration is not only almost globular but also far more tuberculate than the Pitahaya fruit, but the same species in Fig. 710 carries a lemon shaped and almost tuberculate fruit, which could be the mature condition. There does not appear to be any indication of fruit size in *Die Cactaceae*, but Britton & Rose give 7 cm length, which is approaching the size of the Pitahaya fruit. However, both Britton & Rose and Backeberg illustrate and describe the fruit as spiniferous whereas the Pitahaya fruit is spineless. There is a note on P.796 of *Die Cactaceae* to the effect that "the unspined fruit pictured by Soulaire in "Cactus" 6 of 1946, is *Hylocereus*, not *Mediocactus*".

..... from R.Mottram

The illustration in *Cactus* (France) is described as a *Mediocactus* fruit, but I am inclined to agree with Backeberg that it is very probably a fruit of *Hylocereus*, as it is quite free of spines. The fruit is more or less spherical, not a lemon shape. There is not much in the way of podaria, but the scales are pretty large, approximately the same size as on *Eriocereus* fruit, but not as large as on the Pitahaya fruit. The external surface displays some signs of shallow corrugations - it is not as smooth as fruit on *Harrisia*. The writer describes it as Pitahaya fruit.

..... from H.Middleditch

Geographically *Hylocereus* does occur in Columbia whilst *Mediocactus* does not. Fruit sizes for *Hylocereus* are around the 7 to 10 cm which does match the Pitahaya fruit, but all fruit colours quoted by Backeberg seem to be reds, not yellow.

..... from M.Muse

On the basis of the commentary in *Die Cactaceae* Vol.2 and Fig.732, the Sainsbury Pitahaya fruit would seem to be *Hylocereus microcladus*, which emanates from Columbia. As the fruit matures it becomes swollen so that the nett effect is to flatten the tubercles so that they are less prominent and seem fewer. In short, the fruit is a dead ringer for the fruit of *H.trigonus* in Fig.729 of *Die Cactaceae* Vol.2, except for their canary yellow colour!

..... from H.Middleditch

Perhaps we should not entirely discard the possibility of *H.polyrhizus*, with a fruit 10 cm long (scarlet again!) which is said to have the largest scales of any *Hylocereus* species, on the ovary and fruit. This is also from Columbia.

..... from J.Madsen

The Pitahaya from Columbia seems to have become very popular in Europe recently, although I have not yet seen it in Denmark. In my treatment of the *Cactaceae* of Ecuador I included the plant as *Selenicereus megalanthus*. However its generic position is doubtful and it should perhaps be placed in *Mediocactus*. The species is widely cultivated in Columbia. It seems as if the cultivars lack spines or else the fruits are harvested before the spines are produced. Ripe fruits in Ecuador have spines 5 to 10 mm long. Dr. Werner Hoffmann and some of his students are familiar with these cultivars in Columbia.

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Special interests

Austrocactus	A.Johnston, 11 Malvern Rd., Scunthorpe DN17 1EL
Cereanae	G.J.Charles, Briars Bank, Fosters Bridge, Ketton, Stamford PE9 3UU
Cleistocactus	T.Lavender, Kalanchoe, Market Place, Tetney DN36 5NN
Copiapoa	A.W.Craig, 32 Forest Lane, Kirklevington, Yarm TS15 9LY
Discocactus	R.Moreton, 91 Umberslade Rd., Selly Oak, Birmingham, B29 7SB
Echinopsis	M.Muse, 32 Fielding Rd., Birstall, Leicester, LE4 3AJ
Frailia	J.Forrest, Spring Garden, 2 Dargaber Rd., Quarter, Hamilton ML3 7XQ
Gymnocalycium	F.Fuschillo, 55 Emberton Court, Torpion St., London EC1V 0EP
Lobivia	R.Purslow, 130 Brighton Rd., South Croydon, CR2 6AE
Matucana	D.Aubrey-Jones, 62 Rosehill Park, Caversham, Reading RG4 8XF
Melocactus	J.Arnold, Suffolk House, 2 Oak Hill, Washingborough, LN4 1BA
Neoporteriae	R.M.Ferryman, Nichelia, The Street, Stonham Aspal IP14 6AH
Opuntia	R.Crook, 35 Cardinal Close, Worcester Park, Surrey KT4 7EH
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