

Cactaceae
Systematics Initiatives

32



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Front Cover

'Oh, I do like to be beside the seaside...' (especially the Pacific Ocean); *Copiapoa cinerea* in one of its more obese and prolific manifestations sunbathing near Paposo, 45 m, 10 Feb 2013. [DH]

CCC3 goes ahead!

I'm pleased to say that arrangements for the preparation of a new edition of the CITES Cactaceae Checklist were finally agreed last month. The time-frame has contracted somewhat as the CITES Plants Committee requires a full draft of the Checklist in the required format by 1 October this year. That format was substantially agreed in 2009, when the Committee accepted my proposal to reduce the page size to the international standard size A5 and to include cross-references to the illustrations in NCL. Citation of authors' names etc was also discussed but no decision taken. It is possible that if the NCL text is made available on line, and searchable, this might be unnecessary.

What *is* necessary, as I pointed out in my two previous CSI editorials, if the Checklist and Lexicon are to be accepted as 'standard references' by CITES, is that changes to the previous edition are widely agreed and acceptable to the Plants Committee to minimize updates to the CITES permit system and confusion among users generally. The main changes we have to consider concern which *genera* to accept in the light of molecular data (and their interpretation) now available, bearing in mind that practical means of identification must have priority over phylogenetic hypothesis. In the case of *Opuntia*, the consensus since the work of Dickie & Wallace and Stuppy has been for division, and this does not create a serious problem for the CITES authorities. In the more difficult case of tribe Trichocereae, Anceschi & Magli (CSI 31: 24–27) have argued that the logical solution is expansion of *Echinopsis* rather than division in line with the clades proposed by Schlumpberger. But neither of these solutions is likely to satisfy users, the first being too radical and the other unsupported as yet by a workable key to identification, let alone independent confirmation. By way of compromise I have suggested adopting the idea of *alternative names* suggested by Berger and revived at the NCL group discussion last May (see CSI 31: 5–7). Gordon Rowley (next page) is suggesting an alternative compromise he calls 'bipartite classification', using the standard infrageneric categories to accommodate clades within genera, which (as it happens) is how I am proposing to deal with a comparable situation in my 'other family', the Commelinaceae, where the choice concerning *Tradescantia* seems to be one large genus (perhaps 150 species) or 20 or more small ones, some difficult to distinguish without DNA or other data not readily or currently available.

The problem of deciding which *species* to recognize is also susceptible to solution by compromise – by treating doubtful cases as subspecies, forms or cultivars (not *varieties*, please, as this category is ambiguous). Such has been the flood of so-called "new species" since CCC2, however, I have to draw the line somewhere and say I will not accept them at any rank without independent evaluation. Elsewhere (Mamm. Postscripts 3: 21–22. 1991) I have stated my personal criteria for accepting new taxa in my pet genus *Mammillaria* (*sensu* Hunt!) but I am very happy to have been offered the article that follows concerning probably the next most 'popular' genus with growers and amateur taxonomists. Unsolicited, and contributed by botanists who live, so to speak, among the plants, it might equally be taken to heart by *aficionados* of another of the 'difficult' genera, *Copiapoa*, that has priority, alphabetically, as a topic for this issue.

D.H.

New species in *Gymnocalycium*: a call for common sense

Pablo Demaio & Jorge Chiapella

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“I have been struck with the fact, that if any animal or plant in a state of nature be highly useful to man, or from any cause closely attract his attention, varieties of it will almost universally be found recorded. These varieties, moreover, will be often ranked by some authors as species.” (Charles Darwin, “The Origin of Species”, 1859)

The proper delimitation of plant species is far away from being an issue restricted to debate among professional botanists. Topics like teaching and communication about biodiversity, ecological and biogeographical research, and conservation and management of biological resources all depend on the precise identification of species. The more precisely and exactly diagnosed a species is, the easier it will be to teach about it, to communicate its particular features, to elaborate conservation policies.

Until 1859, most of naturalists thought that species were fixed and immutable entities, produced by special creation, without variation among individuals. With the publication of *The Origin of Species* Charles Darwin conceived a better, simple, and comprehensive explanation about why there are so many different species on the earth. A key concept in his theory of evolution was that populations in the wild have morphological variability (we know today that this morphological variability mostly represents genetic variability, but it is also linked to environmental factors). New species arise only if genetic variation is present, if environmental constraints are acting, or geographic isolation happens, or if some or all of the previous are somehow interacting, driving parental species to change. More recently, Ernst Mayr produced a useful definition of a species: a group of interbreeding natural populations that are reproductively isolated from other groups. Of course there are many other species concepts which rely on different features. In any case species are entities of the real world, and the role of taxonomists is not only to discover and describe them, but to put them into a comprehensive scheme of classification. This is not a trivial work, since the days are over when a taxonomist's only job was to travel to exotic, faraway lands, to collect the first plants they stumbled upon, to produce some description and to hope that the newly described species would gain acceptance among fellow botanists.

Although every modern biologist knows these facts, it seems that many cacti enthusiasts often forget it, thinking about species like pre-Darwinian naturalists, disregarding natural variability of the populations. This noxious attitude is usual in cactus taxonomy, and *Gymnocalycium* doesn't escape from this mistake. Every year “new” species, subspecies and varieties are described and published.

But, are they really new different taxa? If you think you have found a new species, you have to demonstrate it. This is just an elemental rule of scientific procedure. A new species is a hypothesis, a provisional assumption, and the author has to show all of the evidence that prompted him/her to propose such hypothesis. However, evidence of a new species is not

only a *typus* specimen sent to a herbarium. A *typus* specimen is a necessary reference, but it hardly constitutes proof. An author postulating new taxa should at least present :

- data from several individuals taken from a wild population, with distinctive morphological features, with statistically tested differences from other similar populations around it, considering that most of morphological characters used in cacti are homoplastic and therefore not enough to differentiate species; *and/or*
- evidence of reproductive isolation between this population and other similar populations around it; *or*
- evidence of molecular markers showing clearly differences between this population and other similar populations around it; *or*
- chromosomal evidence showing clearly differences between this population and other similar populations around it; *or*
- ecological evidence, like stable phenological differences in the wild, different habitat, *et cetera*.

Furthermore, a certain curiosity about the evolutionary circumstances of the forces driving the establishment of a new species in the area is also needed. Is there something in the geological or ecological conditions in which the “new” species is found to account for the occurrence of a speciation event?

Please gentle reader, be kind and search for any of the evidence mentioned before in the recent descriptions of new species of *Gymnocalycium*. Could you find it? We are sure you could not. Publishing new species in *Gymnocalycium* (or subspecies, or whatever taxonomic rank you might think of) has become a kind of race, with no rules. It is time to talk about this. Botanists in general, but even more those working with cacti, have to agree of how to evaluate the finding of real new species carefully and critically.

The authors are botanists who live and work in the province of Córdoba, Argentina, the Mecca of *Gymnocalycium* collectors, and in particular one of us (PD) has strolled during the last 15 years in every corner of all type of environments of Córdoba – mountains ranges, hills, savanna-like Chaco plains, salt flats – looking for cacti, especially *Gymnocalycium*. And before us, better botanists have done the same. The hard fact of the variability of species in *Gymnocalycium* in the wild is something you see every day, and that for sure the evolution of morphological patterns in the genus is very complex. And we just can’t understand how people who never walked before on these environments easily find new species in a two-week expedition. Something is wrong with this.

The description of new taxa of *Gymnocalycium* should be the result of an extremely cautious and sceptical process involving morphological and molecular studies, together with critical assessment of wild populations. Furthermore we researchers and collectors must increase communication among us, in order to establish methodological agreements for the description and identification of species. Only in that way we will achieve a realistic and definitive panorama of the diversity of these fascinating plants.

As usual I would be glad to receive readers’ comments on the above contribution. With CCC3 in mind, I am also wondering whether it might be practical to arrange an ICSG conference in southern South America later this year to discuss current problems in cactus taxonomy. If you might be interested and able to attend, please let me know. – D.H.

Pereskia – one genus or two?

Gordon Rowley

Summary. *Pereskia* is paraphyletic, and the two major clades call for taxonomic recognition. In the absence of clear-cut distinctions the genus is retained intact but segregated into two subgenera, of which *Leuenbergeria* is published as new.

Seeking the origin and evolution of such extraordinary plants as cacti from among less specialized ancestors has always posed problems. Lack of fossil evidence is not least of these. Molecular taxonomy has provided a leap forward, but also brought setbacks when classification based upon imputed common ancestry conflicts with traditional concepts based on gross morphology. This is the case for *Pereskia*, a genus of 17 species (Hunt 2006) looked upon as the nearest we have to living survivors of primitive cacti. In the course of its long history it has accumulated genes determining many features we associate with advanced survival strategies: tuberous roots, areoles, spines, epigyny, dioecy and other floral quirks. It is tempting to regard it as a relict genus, to be cherished alongside such “living fossils” as tree ferns, cycads and *Ginkgo*.

Curt Backeberg took a typically individual view of the scene, as summed up in his *Lexicon* of 1966, with two genera, *Peireskia* and *Rhodocactus*, covering 24 species in all, but times have changed. Beat Leuenberger provided us with all that one could desire in his excellent monograph of 1986. This includes three keys for the identification of his 17 species (“Sp. A” is now *P. marcanoi*). One key covers sterile specimens, another flowering plants, and the third specimens in fruit. There are also first class line drawings of individual species, so it is no problem that the two subgenera proposed here share so many features in common. Perhaps one day a more intensive search and computer compilation will come up with a better set of criteria. At least it would be some advance on Joel Lodé’s sole distinction of the two clades on the distribution of stomata!

In the absence of clear-cut differentia to separate the northern and southern clades readily, recognition of two genera is here rejected in favour of a lower rank: subgenera (see table). The approach is what might be called ‘bipartite classification’ in which major categories (families, genera and species) would be retained in the traditional sense based on gross morphology, and subordinate categories (subfamilies, subgenera and subspecies) employed to reflect supposed phylogeny*. Could this prove to be a blessed compromise to resolve the present conflict between traditional and molecular taxonomy? One is reminded of similar dilemmas where DNA sequencing has upset current concepts of Didieraceae and Portulacaceae.

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*See Hunt, D. (1999). The opuntoids, all sections or all genera? *Cact. Consensus Init.* 8: [3–] 7: ‘a major advantage of the “all sections” approach is that it enables one to build an *infrageneric* hierarchy which can reflect the hierarchy suggested by a cladogram.’ – Ed.

PERESKIA Mill.

Subgenus *Pereskia*

Type: *P. aculeata* Mill. in Gard. Dict. ed. 8, unpaginated. LT: Dillenius, Hort. Elth. t. 227, fig. 294 (1732).

Shrubs or trees slow to form bark and lose bark; shoots mainly stout, 4 mm or more in diameter (slim and prickly in *P. aculeata*). Dwarf sideshoots (brachyblasts) present or absent.

Flowers large, rotate and scattered or small, cupped and aggregated.

Fruits turbinate to pyriform or black and shiny.

‘Southern Clade’ (9 spp.)

P. aculeata, *P. bahiensis*, *P. diaz-romeroana*, *P. grandifolia*, *P. horrida*, *P. nemorosa*, *P. sacharosa*, *P. stanantha*, *P. weberiana*

Subgenus *Leuenbergera* Rowl. **subgen. nov.**

Type: *P. lychnidiflora* DC., Mem. Mus. Nat. Hist. 17: 75, t. 18 (1828). T: “Cactus fimbriatus” in Moçiño, Fl. Mex. ined. no. 1689 (Hunt Inst. Bot. Doc., Pittsburgh).

Shrubs or trees with early maturing bark replacing green tissue; twigs slender, 4 mm or less in diameter (to 5 mm in *P. lychnidiflora*); never prickly; brachyblasts present (lacking in *P. bleo*).

Flowers mostly large and rotate.

Fruits ±globose, not black.

‘Northern Clade’ (8 spp.)

P. aureiflora, *P. bleo*, *P. guamacho*, *P. lychnidiflora*, *P. marcanoi*, *P. portulacifolia*, *P. quisqueyana*, *P. zinniiflora*.



Pereskia grandifolia BR: Minas Gerais, Lavras, 10 Oct 1988, *Harley* 24802 [NT]



Pereskia zinniiflora (*male*) CU: Cienfuegos, cult. Jard. Bot. de Cienfuegos, 4 Apr 2005 [NT]

NCL updates etc

In this issue David Hunt considers recently proposed taxa of Copiapoa with the help of comments and photos by Graham Charles [GC] and comments by Paul Hoxey [PH], and reviews recent publications and proposed changes to the classification of Gymnocalycium.

COPIAPOA

If the information I have is correct, no less than seven new species of *Copiapoa* and two heterotypic subspecies have been proposed shortly before or since NCL was published in 2006. Having sat in on ‘Copiapoathons’ in the past, and having now seen a bit of the Atacama flora for myself, I cannot say I would want to recognize any more new taxa above the level of form or *forma* without a clear understanding by the author(s) of the relationship of the supposed novelty in terms of the main species-groups, together with a plausible identification key and, ideally, some meaningful DNA data by way of confirmation. The paucity of morphological characters available to enthusiasts to distinguish their discoveries may explain why so much depends on location, location, location, i.e. locality data. But comparisons with the next-door population or one or two other possibly related taxa do not impress me. Those describing new taxa rarely refer to the species-groups distinguished by Ritter (1980/KS 3: 1048) or to Nigel Taylor’s tentative identification key (Taylor 1981/CSJGB 43(2/3): 49–60; 1989/EGF 3: 252–255) or our subsequent efforts (Hunt 2001/CSI 12:15–17). Nigel’s observation that the presence/absence of mucilage in the stem might be a useful diagnostic character (Taylor 2001/CSI 12: 18), perhaps correlated with soft body-texture and tuberous rootstock, does not seem to have been followed, though rather easily tested in the field.

To help judge the merits or otherwise of the recent novelties for potential inclusion or mention in NCL2, I have sought comments from Graham Charles and Paul Hoxey, who have accumulated more knowledge of the genus and its subtleties than I can possibly hope to. The recent new names, in alphabetical order and associated publication data are as follows:

14542 *C. algarrobensis* Katt 2012/CSJA 84(2): 69, *nom. inval.* [T (*see note*): CL, Atacama, known only from a hill group E of Algarrobal [50 km N of Vallenar], c. 750 m, FK 530.] ~ *C. megarhiza*

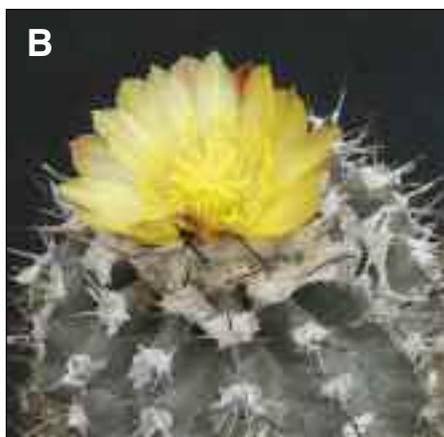
• ‘The purported holotype specimen at SGO [117450] is reported to be an illustration, which is contrary to ICN Arts 40.2, 40.4’ (Eggli & Nyffeler 2013/RPS 63: 12). The plant was compared by the author with *C. megarhiza* and *C. echinoides* but was tuberous-rooted, a key difference from the latter taxon. Seeds have been distributed by Kattermann with the above FK number.

14193 *C. angustiflora* WaltH, Chas & Mchl in WaltH+Mchl 2006/BCSJ 24(4): 185–192. T: CL, Antofagasta (not Atacama, as stated in the title, l.c. 188), Quebrada Guanillos, 10 Mar 2005, Walter HW 436 (SGO 153173, holo.). **Distri:** ‘Guanillos valley and some km to the north and south’. ~ *C. mollicula*

Similar to *C. mollicula* but **bo** smaller, subglobose, grey brown; **ri** narrow, well-developed, notched into tubercles with a small pointed chin; **sp** shorter and finer; **fl** very trumpet-shaped, short and narrow; **pc** almost tubular; **nc** narrow, elongated; **hmr** almost lateral.

• Formerly confused with *C. esmeraldana*, then thought to be a form of *C. humilis*, this evidently enigmatic plant, which occurs sympatrically with *C. hypogaea* ssp. *lauri*, between the northern and southern populations of *C. montana* sens. lat. (including the local forms described by Ritter as *C. olivana* and *C. mollicula*) is apparently another such form.

The authors provide comparative tables in support of their decision to treat *C. angustiflora* as a species, but no key to the one ‘good’ (or reliably recognizable!) pre-Ritter species, *C. humilis* and its many geographical forms, or *C. montana*, if that also deserves the rank of species it has in NCL. As one of the co-authors, Graham Charles considers *C. angustiflora* deserves recognition as a distinct species.



A: *Copiapoa angustiflora* CL, Antofagasta, Quebrada Guanillos, GC 303. cult. GC [GC]; **B:** *C. mollicula* CL, Atacama, mts N of the airstrip at Chañaral, Tom Jenkins TJ 81 [GC]; **C:** *C. mollicula* (*C. leonensis*) CL, Atacama, NE of Caldera, Agua Leones, FK 444, cult. GC [GC].

Paul Hoxey, while admitting that it is tricky to know how to classify small isolated but allied populations also considers *C. mollicula* and *C. angustiflora* distinct enough to be separated from *C. montana*.

My feeling, on the contrary, is that comparison of ‘diagnostic’ features by means of a table tends, as in this case, to prove that *NONE* of the characters is diagnostic, and that the taxa compared are at best *subspecies* – a typical case of ‘things look bigger close-up’ (Hunt’s First Law of Psychotaxonomy’!).

Distinct-looking populations they may be but that doesn't make them *species*. At a stretch we could call them *subspecies* or but as with the distinctive variants of *C. cinerea* I think the category of *forma* would be more appropriate or perhaps informal geographical names, such as 'Guanillos form'. [DH]

14017 *C. megarhiza* ssp. *parvula* Mchl+WaltH 2005/KuaS 56(11): 297. **T:** CL, Atacama, prov. Copiapó, E of Caldera, Mar 2005, *Walter & Mächler* HW 391 (SGO). → *C. megarhiza*

• A small-bodied form of *C. megarhiza* occurring N of ssp. *megarhiza* with <22 narrow, prominent-ly tuberculate ribs, longer, thinner spines and somewhat smaller fruits and seeds.

14490 *C. coquimbana* ssp. *andina* Schb+Keim 2009/CCo 13(1): 13. **T:** CL, prov. Atacama, 50 km NE of Vallenar, 1200–1400 m, NW-facing slopes in crevices on hard igneous rocks, 5 Nov 2008, *Schaub & Keim* s.n. (SGO 157240, holo.). → *C. coquimbana*

• An inland population disjunct from the main coastal populations on the coast [GC]. Apparently the same probably Ritter's *C. coquimbana* var. *armata* [Ritt 1980/KSA 3: 1075, fig. 1002 (*photo: Buining*), dept. La Serena, upper reaches of Río Choros, *Wagenknecht* in FR 1461 (SGO 124929)]. The highest of the *C. coquimbana* forms and probably the most distinct [PH]. Fig. 1002 was photographed in Wagenknecht's garden, 13/14 Jan 1969, and is reproduced as fig. 75 in Paul Hoxey's book *A Journey with Friedrich Ritter along the coast of Chile and Peru* (2013). Disjunction might justify recognition of the form as a subspecies, but when compared with var. *coquimbana* the main difference (in a very variable species) appears to be the chestnut-brown colour of the spines [DH].

14522 *C. griseoviolacea* Schb+Keim 2011/CCo 14(4): 9–15. **T:** CL, Atacama, SE of Huasco, north-facing hills and dry riverbeds S of the Huasco river, 320–850 m, 9 Sep 2010, *Schaub & Keim* s.n. (SGO 159676, holo.). → *C. echinoides*

• Probably a redescription of *C. cuprea* Ritt. (→ *C. echinoides*) (Hoxey 2012/CSI 27: 10–11.)

14389 *C. leonensis* Schb+Keim 2006/CCo 10(2): 118–126. **T:** CL, 27 Aug 2004, *Schaub & Keim* s.n. (SGO 151588, holo.). → *C. mollicula*

• Said by the authors to differ from *C. humilis* by its hard body and large hard rootstock. A southern population of *C. mollicula* [PH].

14573 *C. sarcoana* Schb+Keim 2012/CCo 16(2): 4–16. **T:** CL, Atacama, around Sarco, on inclined coastal plains, 0–300 m, June 2011, *Schaub & Keim* s.n. (SGO 161062, holo.). → *C. coquimbana*

• Compared with the very variable *C. coquimbana*, of which it is perhaps just a form.

14491 *C. schulziana* Schb+Keim 2009/CCo 13(1): 18. **T:** CL, Atacama, c. 30 km from the coast, w of Domeyko, on a N side lateral valley of Quebrada Chañaral, 550–650 m, 6 Nov 2008, *Schaub & Keim* s.n. (SGO 157239).

• Differs from *C. coquimbana* in the mostly solitary stems <32 × 25 cm, 'pistachio green throughout' and napiform rootstock. Further study needed [PH]

14574 *C. superba* Schb+Keim 2012/CCo 16(2): 17–29. **T:** CL, Antofagasta, N of Chañaral, on coastal hills, June 2011, *Schaub & Keim* s.n. (SGO 161070, holo.).

Bo forming clumps <60 cm; **br** 'light brownish green' at first, coated 'light bluish grey' [pruinose?] when older; **r** fibrous; **ri** <19, narrow; **sp** long, thin recurving, at first orange to brown; **fl** 38 × 32 mm [*C. grandiflora* 50 × 55 mm], yellow; **fr** <15 × 12 mm, greenish to red; **sd** 1.5 × 1 mm, black, glossy.

• Inadequately known. Compared with nearby *C. grandiflora* and said to be 'totally different' but no attempt was made to differentiate it from other members of the genus.

I would greatly appreciate feedback from other readers who have experience of these plants or opinions on their status. Meanwhile, what follows is by way of a mid-issue whistle-stop field-trip to see some of the most spectacular copiapoaes *where they grow*...

Chile's coastal rock garden:

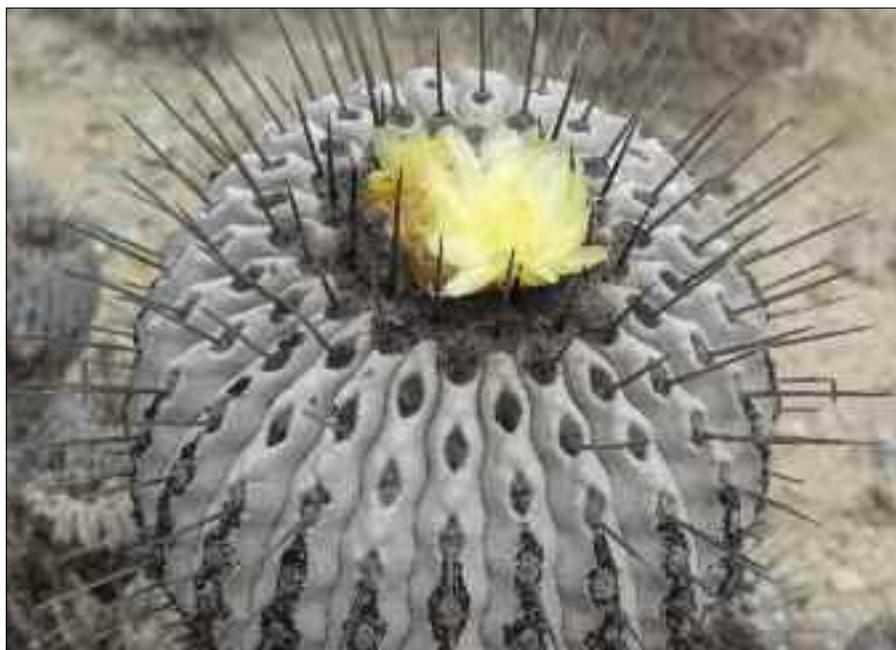
Copious and colourful Copiapoas on permanent display

As I said in CSI 30, I booked my first visit to Chile last year some weeks before the discussion in CSI 29 concerning *Eulychnia* led me to divert my attention to that genus rather than the opuntioids (principally *Maihueniopsis*) and *Copiapoa* (initially the main attraction). A fortnight in the field was clearly nowhere near enough to learn much about one genus, let alone three, and many of the scattered inland localities for *Maihueniopsis* were out of reach in the time available. Happily the many coastal sites for *Copiapoa* are easily reached from the Panamerican highway and have been much visited and photographed by cactophiles. But I make no apology for filling a few pages in this last of the present series of CSI with my own snapshots of these Chilean gold-medal rivals to the cacti of all the other 'centres of diversity' of the family in Latin America. That they are photogenic is an understatement; I don't recall ever uttering the word '*fantastic!*' out loud quite so often as during my brief few days north and south of Taltal!

Serendipity played some part in my luck in seeing so many of the plants in flower and under blue skies. I had intended going before Christmas but had to postpone the trip till the end of January when I thought flowering would be over. I was wrong!



Copiapoa coquimbana CL, Atacama, N of La Serena, Llanos de Choros, 230 m, 2 Feb 2013.



Copiapoa dealbata CL, Atacama, N of Huasco, PN Llanos de Challe, 35 m, 3 Feb 2013.



Copiapoa dealbata CL, Atacama, N of Huasco, PN Llanos de Challe, 35 m, 3 Feb 2013.



Copiapoa cinerascens CL, Atacama, Quebrada Pan de Azucar, 70 m, 8 Feb 2013.



Copiapoa cinerea [columna-alba] CL, Atacama, Quebrada Pan de Azucar, 70 m, 8 Feb 2013



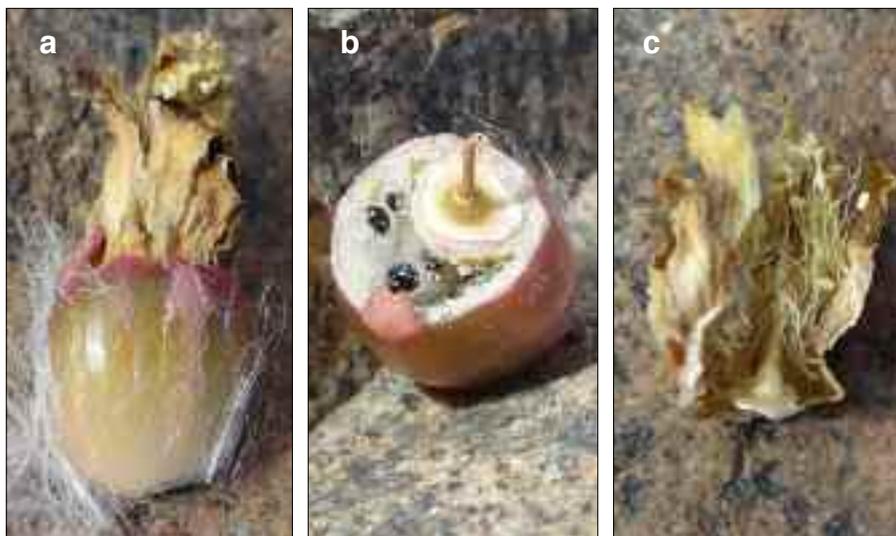
C. cinerea [albispina] CL, Antofagasta, s of Taltal, 50 m, and coastline beyond the site, 9 Feb 2013.



Cylindropuntia desertorum CL, Atacama, inland from Cifuncho, 400 m, 9 Feb 2013.



Copiapoa cinerea [albispina] CL, Atacama, coastal slopes 10 km N of Taltal, 90 m, 10 Feb 2013.



C. cinerea [albispina] CL, Atacama, coastal slopes 10 km N of Taltal, 90 m, 10 Feb 2013; **a**, fruit prior to abscission of floral remnant; **b**, another fruit and seeds after abscission of perianth and stamens, but with the operculum and style still attached; **c**, an abscised floral remnant with operculum attached.



Copiapoa cinerea [haseltoniana] CL, Antofagasta, 17 km N of Taltal, 25 m, 10 Feb 2013.



C. cinerea [haseltoniana] CL, Antofagasta, between Planta Paposo and Paposo, 50 m, 10 Feb 2013.



Copiapoa echinoides CL, Atacama, sw of Copiapó, near Playa del Medio, 115 m, 13 Feb 2013.



Copiapoa megarhiza echinata CL, Atacama, 5 km s of Playa del Medio, 150 m, 13 Feb 2013.



Copiapoa megarhiza echinata (?) CL, Atacama, 5 km s of Playa del Medio, 150 m, 13 Feb 2013.



Copiapoa dealbata with *C. echinoides* CL, Atacama, 26 km N of Carrizal Bajo, 210 m, 13 Feb 2013.



Copiapoa echinoides CL, Atacama, 26 km N of Carrizal Bajo, 210 m, 13 Feb 2013.



Copiapoa dealbata CL, Atacama, 26 km N of Carrizal Bajo 210 m, 13 Feb 2013.



Copiapoa dealbata CL, Atacama, 25 km N of Carrizal Bajo 210 m, 13 Feb 2013.

Best in Show?

One of the cinereas, surely? but a very difficult decision for the judges, especially as (thanks to those not-quite-so-pretty eulychnias!) there wasn't time to see any of the populations of the *C. cinerea* 'Formenkreis' inland and upland from Taltal so vividly depicted in the second of Rudolf Schulz and Attila Kapitany's 'warts and all' (their term) account of the genus*. If one needed any encouragement or excuse to return to Taltal, a glance at their very fine book should be more than enough. Despite the all too obvious evidence of increasing aridity in some areas, the silence and unearthly sense of timelessness in the Atacama, especially when among such natural wonders as these, is such that differences of opinion over what to call them seem of no importance whatever. Nevertheless I'm happy to say that, give or take what may in any case be ephemeral preferences for 'species' *versus* 'subspecies', a high level of agreement seems to have broken out in this instance between the UK and Australia! (see the Appendix to the Australian book, pp. 230–233, where the names used are compared with those in the then recently published NCL). I fear a similar degree of consensus may take longer to achieve between the UK and *Austria* in regard to the next genus discussed! – Ed. →→→

*SCHULZ, R. & KAPITANY, A. (2006). *Copiapoa* 2006. Schulz Publishing, Teesdale, Australia.

GYMNOCALYCIUM

'Seeing the list of 'new' gymnocalyciums on p. 20 of CSI 29, I can't help thinking that if the same criteria were applied to people there would be dozens of new human species on my street alone! I'm as keen as the next man to recognize and name differences to avoid confusion but, surely, this is what cultivar names are for.' – Bob Humphrey (16 Jan 2013).

It is true that discussion of anything to do with this popular genus in our NCL meetings at Milborne Port has tended to induce yawns if not snores from one or more of those present. However, with two recent and attractively produced surveys of the genus to consider in conjunction with two molecular studies, it is time for a strong cup of coffee and leather gloves (at least for the timid) with which to grasp the nettle.

- CHARLES, G. (2009). *Gymnocalycium* in habitat and culture. Pp. 288. Self-published, Stamford, UK.
- DEMAIO, P.H., BARFUSS, M.H.J., KIESLING, R., TILL, W., & CHIAPPELLA, J.O. (2011). Molecular phylogeny of *Gymnocalycium* (Cactaceae): Assessment of alternative infrageneric systems, a new subgenus, and trends in the evolution of the genus. *Amer. J. Bot.* 98(11): 1841–1854.
- MEREGALLI, M., ERCOLE, E. & RODDA, M. (2010). Molekulare Phylogenie versus Morphologie: Die infragenerische Klassifikation der Gattung *Gymnocalycium* (Cactaceae) in neuem Licht. [Molecular phylogeny vs. morphology: Shedding light on the infrageneric classification of *Gymnocalycium* (Cactaceae)]. *Schumannia* 6: 257–275.
- METZING, D. (2012). Nacktdistel und Spinnenkaktus. Die Gattung *Gymnocalycium*. Pp. 144. Deutsche Kakt. Ges., Adelsdorf, DE.

At the outset of these notes, I must make it clear that, as with *Copiapoa*, I claim no special knowledge of the genus, and merely intend to comment on the taxonomic differences of opinion that emerge from the four specialist treatments listed and draw my own conclusions. Nevertheless, I have to admit I sympathize with Bob Humphrey and his remarks above. I also agree very strongly with the introductory remarks at the start of the paper by Pablo Demaio et al. (2011) and the short and unsolicited article Pablo sent me via e-mail in March 2013 (see pages 4–5).

The treatment of the genus for NCL was much influenced by the authors of three of the four accounts to be compared here, namely Graham Charles, Detlev Metzting and Massimo Meregalli, and more than half the illustrations in the 'Atlas' volume were contributed by Graham. Various proposed amendments to the NCL treatment have already been presented and discussed in CSI 29: 11–15 (2013) and a number of corrections made in the updated reprint edition of the 'Atlas' volume published later in 2013. The misidentified image for ❁ 273.3 *G. robustum* has been replaced; locality data have been added to the captions for ❁ 268.4 and ❁ 274.2; and the identification of the following plates adjusted to take account of status changes proposed in CSI 29 and/or recommended by both Graham and Detlev: ❁ 276.2 *G. chacoense* [3]; ❁ 276.3 *G. paediophilum* [3]; ❁ 277.5 *G. horridispinum* ssp. *horridispinum* [3]; ❁ 278.1 *G. horridispinum* ssp. *achirasense* [3]; and ❁ 285.1 *G. megatae* [5].

What the molecules say

Meregalli et al. (2010) based their cpDNA survey on *atpB*, *rbcL*, *trnK rps16*, *trnL* and *trnF* sequences from 36 samples of about 30 species in all the widely recognized subgenera based on seed-morphology, plus subg. *Pirisemineum*. Their results indicated that the genus is monophyletic and comprises three major and well-supported clades only partly consistent with the traditional morphological classification: Clade A, comprising a major part of subg. *Microsemineum*; Clade B, the rest of subg. *Microsemineum* plus subg. *Trichosemineum*, *Gymnocalycium* and *Macrosemineum*; Clade C, corresponding to subg. *Muscosemineum* and *Pirisemineum*. i.e. species typical of the Chaco ecosystem in s Bolivia and Paraguay plus *G. saglionis* widespread in Argentina.

Within Clade A, the analyses did not differentiate any ‘internal’ groups except a sister-relationship between *G. glaucum* and *G. carminanthum*, but the placement of *G. saglionis* (the type species of subg. *Microsemineum*) was not constant when individual markers were analyzed, varying from basal to Clade A, basal to the complex of Clades A+B (thus confirming its present-day usage or requiring the other *Microsemineum* species to be transferred to a new subgenus) or even basal to Clade C. Thus no change in the subgeneric classification could be proposed at present.

Within Clade B, several subclades were revealed. These were better supported in the Bayesian Inference than the Maximum Parsimony, where there were more polytomies. The first subclade, group (B1) would correspond to subg. *Macrosemineum*, though not consistently delimited in respect of *G. paraguayense*. It was thought that hitherto inconclusive discussion of the relationships of *G. angelae*, *G. hyptiacanthum* (*G. uruguayense sensu NCL*) and *G. mesopotamicum* might be clarified by analysis of other *Macrosemineum* species such as *G. denudatum* and *G. horstii*. The authors also suggest that the B2 grouping of *G. bruchii* and the problematic *G. rauschii* might point to the latter having been a form or hybrid of *G. bruchii*, not a plant from Uruguay where it was allegedly collected. The B3 group includes all species examined that belong to subg. *Gymnocalycium*. ‘This is probably the most complex group of species in the genus, showing a relatively high uniformity of characters and a tendency to segregate into isolated populations, each one slightly but consistently differentiated morphologically. This has led to the description of several microspecies, but the value of many of them is poorly understood. Molecular evidence confirms this uncertainty’.

Still within B3, a monophyletic unit including *G. capillaense*, *G. gibbosum* and *G. erolesii* (referred to *G. schroederianum* in NCL) is moderately supported, but the authors say that more species and markers need to be tested to clarify the affinities in this group.

In view of their seed-morphology, the inclusion of the subgenus *Trichosemineum* in Clade B, as group B4, was unexpected. In that their distribution and ecology is similar to that of the southern members of subg. *Microsemineum* and *G. castellanosii* (subg. *Microsemineum*) has a papillate seed, a possible link could be expected, but the molecular data firmly associate these species with subg. *Gymnocalycium* and *Macrosemineum*. ‘It seems that too much emphasis was given to the seed differences of *Trichosemineum* with respect to those of *Macrosemineum* and *Gymnocalycium*, shared to large extent between the three subgenera.’ The example of *G. robustum* (subg. *Gymnocalycium*) and *G. quehlianum* (*G. stellatum sensu NCL*, subg. *Trichosemineum*), sympatric in n Córdoba and indistinguishable vegetatively, was given.

Clade C was found to be ‘clearly isolated’, containing the species of subg. *Muscosemineum* and *Piriseimineum* plus those of the *G. chiquitanum* complex, all native to the Chaco ecosystem, as mentioned already. They cluster into two well-supported groups, one (C1) equivalent to subg. *Muscosemineum* (with separate lineages for *G. schickendantzii* and for *G. anisitsii* with *G. eurypleurum*) the other (C2) for *G. pflanzii sens. lat.* plus *G. chiquitanum* and *G. chacoense*. These C2 species had more usually been associated with subg. *Microsemineum*.

The broader molecular survey of the genus by Demaio et al. (2011*) was based on 78 samples of 52 species sequenced for the the plastid markers *atpI-atpH*, *petL-psbE*, *trnK-matK* and *trnT-trnL-trnF*. It further confirmed the monophyly of *Gymnocalycium* and basic support for the seed-based infrageneric classification favoured in NCL and other treatments. They also considered *G. saglionis* the earliest-diverging (i.e. basal) taxon in the genus, presenting it as a monospecific Clade (A), and taking the positive option suggested by Meregalli et al. of requiring a new subgenus for the other *Microsemineum* species, as adopted by Metzger in his book, i.e. subg. *Scabrosemineum* Demaio et al. l.c. 9 (2011). This is represented by Clade C in their scheme, while Clade B is reserved for subgenera *Piriseimineum* and *Muscosemineum* and Clade D includes subgenera *Macrosemineum*, *Trichosemineum* and *Gymnocalycium*.

Clade B (corresponding to Clade C in the Meregalli et al. scheme) is clearly resolved by Demaio et al. into two subclades, B1 for *G. pflanzii* and *G. chacoense* (subg. *Piriseimineum*), B2 for *G. anisitsii*, *G. eurypleurum*, *G. marsoneri*, *G. mihanovichii*, *G. schickendantzii* and *G. stenopleurum*.

Clade C, the new subg. *Scabrosemineum*, typified by *G. monvillei*, is described as follows:

Plants often large (10–30 cm diameter), roots usually fasciculate, also napiform. Stems frequently solitary or scarcely proliferous, globular, or depressed, ribs mostly tuberculate, rounded, areoles large, spines mostly long, frequently strong. Flowers large, campanulate, growing near the apex. Fruit nearly globose, longitudinally splitting. Seeds small, 0.6–1 mm, brown to dark brown, finely or roughly tuberculate, hilum micropylar region smaller than the seed diameter. Southern Bolivia to northern and central Argentina, in rocky outcrops in mountain ranges.

The numerous subclades in this clade and also Clade D (subgenera *Macrosemineum*, *Trichosemineum* and *Gymnocalycium*) are shown in the cladograms (figs. 1–3) in their paper.

The authors conclude with a section headed ‘Morphological trends in *Gymnocalycium*’ and conclude that these include a tendency during diversification towards reduced plant size, the development of napiform rootstock and fruit and seed modifications promoting dispersal by ants (myrmecochory), also enabling and expansion into cooler regions. More morphological and molecular data will be necessary to resolve relationships in their Clades C and D, especially D2, accompanied by detailed and reliable biogeographical analysis.

*The authors cite an earlier paper: DEMAIO, P., BARFUSS, M.H.J., TILL, W. & CHIAPPELLA, J.O. (2010). Phylogenetic relationships and infrageneric classification of the genus *Gymnocalycium*. Insights from molecular data. *Gymnocalycium Sonderausgabe* 2010: 925–946. This has not been seen by me. – Ed.

Implications and inferences for NCL2

As the two molecular systematic studies now available are largely congruent and clarify relationships within the subgenera already widely recognized, few I think would disagree with Detlev Metzging in adopting the trifold division of *Microsemineum* formally proposed by Demaio et al. At the species level it is a different question. Demaio & Chiapella (above, pp. 4–5) have made their position very clear, and there is little evidence in their phylograms that their cpDNA markers reliably differentiate many of the more recently described taxa from those described earlier; in several of the subgenera, the species are, as the authors say more than once, ‘morphologically very similar’. And, as yet, we who do not call ourselves experts do not have any means, other perhaps than Graham Charles’s distribution maps, and subject to knowledge of where a plant is or was growing, of identifying it. Until we have a good dichotomous key, or series of keys, of the kind Beat Leuenberger provided for *Pereskia*, most of the so-called species described since the days of Britton & Rose (and several of the 23 they accepted – without proposing any new ones!) have yet to be evaluated with the scientific rigour judged necessary by our Argentinian friends.

To attempt make the point, whilst accepting that enthusiasts who cultivate these plants need ‘handles’ for them and any suggestion of ‘lumping’ is likely to be very unpopular, I have drawn up the following ‘back to basics’ table, including nearly all the taxa accepted by the authors of the two handbooks (Charles and Metzging) or sampled in the molecular studies, to suggest how (or where) I think a process of rationalization might begin:

Bold print indicates species that are ‘basic’ (*not basal!*) to each group on grounds of nomenclatural seniority. All others are listed in normal or plain (‘Roman’) print except those in *italic* which, for the purpose of the list, are dismissed as synonyms or rejected for lack of acceptable typification or neotypification. (NB as a pragmatic solution to the *G. quehlianum/stellatum* controversy, I propose listing both names as synonyms of the ‘morphologically very similar’ *G. bodenbenderianum*.)

Columns following the names indicate their clade disposition according to Demaio et al. (**D**) and Merregalli et al. (**M**), then their status according to NCL (**H**), Charles (2009) (**C**) and Metzging (**G**). a = accepted; n = not accepted; p = provisionally accepted; s = subspecies; ® = rejected

Taxon	D	M	H	C	G
	Microsemineum				
saglionis	A	A	a	a	a
	Piriseimineum				
chacoense	C2	B1	n	a	a
chiquitanum	C2		a	a	a
paediophilum			n	a	a
pflanzii argentinense	C2	B1	a	a	n
pflanzii	C2	B1	a	a	a
pflanzii zegarrae	C2	B1	a	a	n

Muscososemineum					
anisitsii	C1	B2	a	a	a
anisitsii damsii		B2	a	a	n
eurypleurum	C1	B2	a	a	a
<i>friedrichii</i>			n	n	a
marsoneri		B2	a	a	a
marsoneri matoense			a	a	n
megatae		B2	n	a	?
mihanovichii		B2	a	a	a
schickendantzii	C1	B2	a	a	a
schickendantzii delaetii			p	a	?
stenopleurum		B2	a	a	n
Scabrosemineum					
albiareolatum	A		a	a	a
bayrianum		C	p	a	a
<i>cardenasianum</i>			n	a	n
<i>carminanthum</i>	A		n	n	n
castellanosii	A	C	a	a	a
castellanosii acorugatum			p	n	n
castellanosii ferocius		C	p	a	a
glaucum ferrarii		C	p	a	n
glaucum glaucum	A	C	a	a	a
horridispinum		C	n	a	a
horridispinum achirasense	A	C	n	a	a
hossei	A	C	a	a	a
monvillei		C	a	a	a
mostii		C	a	a	a
mostii valnicekianum		C	n	a	a
nigriareolatum			a	a	a
oenanthemum		C	a	a	a
pugionacanthum		C	a	a	a
rhodantherum		C	a	a	a
ritterianum		C	a	a	a
spgazzinii	A	C	a	a	a
spgazzinii cardenasianum			a	n	a
Macrosemineum					
angelae		B1	p	a	a
buenekeri			s	a	a
denudatum		D	a	a	a
horstii		D	a	a	a
<i>hyptiacanthum hyptiacanthum</i>			®	a	a
<i>hyptiacanthum netrelianum</i>		D	®	a	n
<i>hyptiacanthum uruguayense</i>	B1	D	n	a	n
mesopotamicum	B1	D	a	a	a
paraguayense	B1	D	a	a	a
uruguayense			a	n	

Trichosemium					
bodenbenderianum	B4	D1	a	a	a
<i>intertextum</i>	B4		n		→ ochoterenae
ochoterenae	B4		a	a	a
<i>quehlianum</i>		D1	®	a	a
ragonesei			a	a	a
<i>stellatum</i>			n	n	n
					→ bodenbenderianum
					→ bodenbenderianum
Gymnocalycium					
<i>amerhauseri</i>		D2	p	a	a
andreae		D2	a	a	a
<i>andreae carolinense</i>	B3		p	n	n
baldianum		D2	a	a	a
<i>berchtii</i>			p	a	a
bruchii	B2	D2	a	a	a
calochlorum		D2	a	a	a
capillaense	B3		a	a	a
<i>carolinense</i>			n	a	a
<i>erinaceum</i>	B3	D2	a	a	a
<i>eroplesii</i>	B3		n		→ schroederianum
<i>fischeri</i>		D2	n	a	a
gibbosum	B3	D2	a	a	a
kieslingii		D2	a	a	a
<i>kroenleinii</i>			a	a	a
<i>leptanthum</i>	B3		®		
<i>neuhuberi</i>			a	a	a
<i>rauschii</i>	B2		®		
<i>reductum leeanum</i>		D	p	a	n
<i>reductum reductum</i>		D2	p	a	a
robustum	B3	D2	a	a	a
schroederianum		D2	a	a	a
<i>schroederianum bayense</i>			n	a	a
<i>schroederianum boessii</i>			n	a	
<i>striglianum</i>		D2	a	a	n
<i>taningaense</i>			a	a	a
<i>uebelmannianum</i>		D2	a	a	a
					→ gibbosum

Throwing down the gauntlet

To suggest there are only 27 ‘good’ species of *Gymnocalycium*, compared with twice that number accepted by Graham Charles and a quarter of the number accepted by the late Hans Till (*Gymnocalycium Sonderausgabe*, 2008) may be considered parsimonious (where did I hear that word before?). Nevertheless I invite all critics of my injudicious and highly provocative assessment to submit their objections to me – with the *caveat* that I reserve the right to reject them if not accompanied by a dichotomous identification key to the species they claim to be worthy of recognition! – Ed.

NCL Illustrations: Corrigenda

Page xiii, col. 2: ❀ 521.2-3 for *E. calochlora* [Lo] read ❀ *E. calorubra* [Lo]

Page 241, captions to ❀ 214.2 and ❀ 241.3: *delete* [Lo]

(*E. calochlora* is a true *Echinopsis*; the combination *Lobivia calochlora* (K. Schum.) Schlumpb. 2012/CSI 28: 30 was made in error and is not accepted by the author.)

Page 460, ❀ 460.5 for *Cumulopuntia sphaerica* read **Cumulopuntia leucophaea**

Page 466, ❀ 466.6 for *Pterocactus araucanus* read **Pterocactus hickenii**

Page 521, ❀ 521.1 for *Borzicactus icosagonus* read **Borzicactus icosagonus** (ssp. *roseiflorus*)

Page 523, ❀ 523.1 for *Sclerocactus scheeri* [1] (*Ancistrocactus megarhizus*) please read **Coryphantha glanduligera** (see below)



Coryphantha glanduligera MX: Nuevo León, Aramberri, Sandía-La Ascension, 1540 m [GH] (as *Ancistrocactus megarhizus*)



Sclerocactus scheeri [1] (*Ancistrocactus megarhizus*) MX: Tamaulipas, MEX 85, 47 km s of Cd. Victoria, 415 m, 24 Aug 1971 [DH 710425]

Thank you to Paul Hoxey for drawing my attention to the misidentifications of ❀ 460.5 and 523.1. As Editor I was careless not to check the identification I received with the scan, but (apart from the unhooked spines) the plant certainly reminded me of the plant I was shown some 40 years earlier by Hernándo Sánchez-Mejorada, though that was in Tamaulipas.

To Norbert and Elisabeth Sarnes I am indebted for their re-identification of ❀ 466.6 based on the spination etc (and the unlikelihood of *P. araucanus* occurring in eastern Chubut). Perhaps the slide had been mislabelled.

Moving on – more images, please!

The 16-page supplement included in the interim edition of the NCL Illustrations published last August was just a taster for the much more extended supplement I should like to produce in two or three years' time. So please help if you can by providing good images of any of the

accepted taxa not hitherto illustrated, or of any taxa, accepted or not, that show variants or forms ‘that look different’ (‘TLD’) or aspects not previously illustrated, its fruits, for instance, its appearance in habitat as opposed to cultivation, or in a different part of its geographical range. The scope for images that will assist users of the book to identify or compare taxa is endless.

Other work in progress...

Now that the preparation of a new edition of the *CITES Cactaceae Checklist* is formally agreed, I have to give priority to this in conjunction with the update of the NCL text volume. Meanwhile, on a shorter time-scale, somewhat governed by hard-pressed contributors, there are two more volumes of *Succulent Plant Research* (SPR) on the stocks, the ‘Ritter in Colour’ project and a successor to my ‘Mammillaria Postscripts’ booklet series of 1989–1999, ‘Huitzilopochtli’.

Progress with ‘Ritter’s Cacti in Colour’

As reported in CSI 27: 4–9 (2012), with a sample treatment for *Opuntia*, most of the original colour slides Ritter used to illustrate his *Kakteen in Südamerika* are extant, along with some not used, and have been scanned (by Paul Hoxey) for potential publication in colour. Meanwhile I had compiled an Excel file listing the data on each slide (slide number, figure number (Abb.) if published, Ritter’s identification, FR number, film and exposure numbers). At that stage it was my idea to publish all the images in a single volume uniform in size with NCL (‘American A4’) with with the relevant data from Ritter’s original captions and text references, plus a separate commentary.

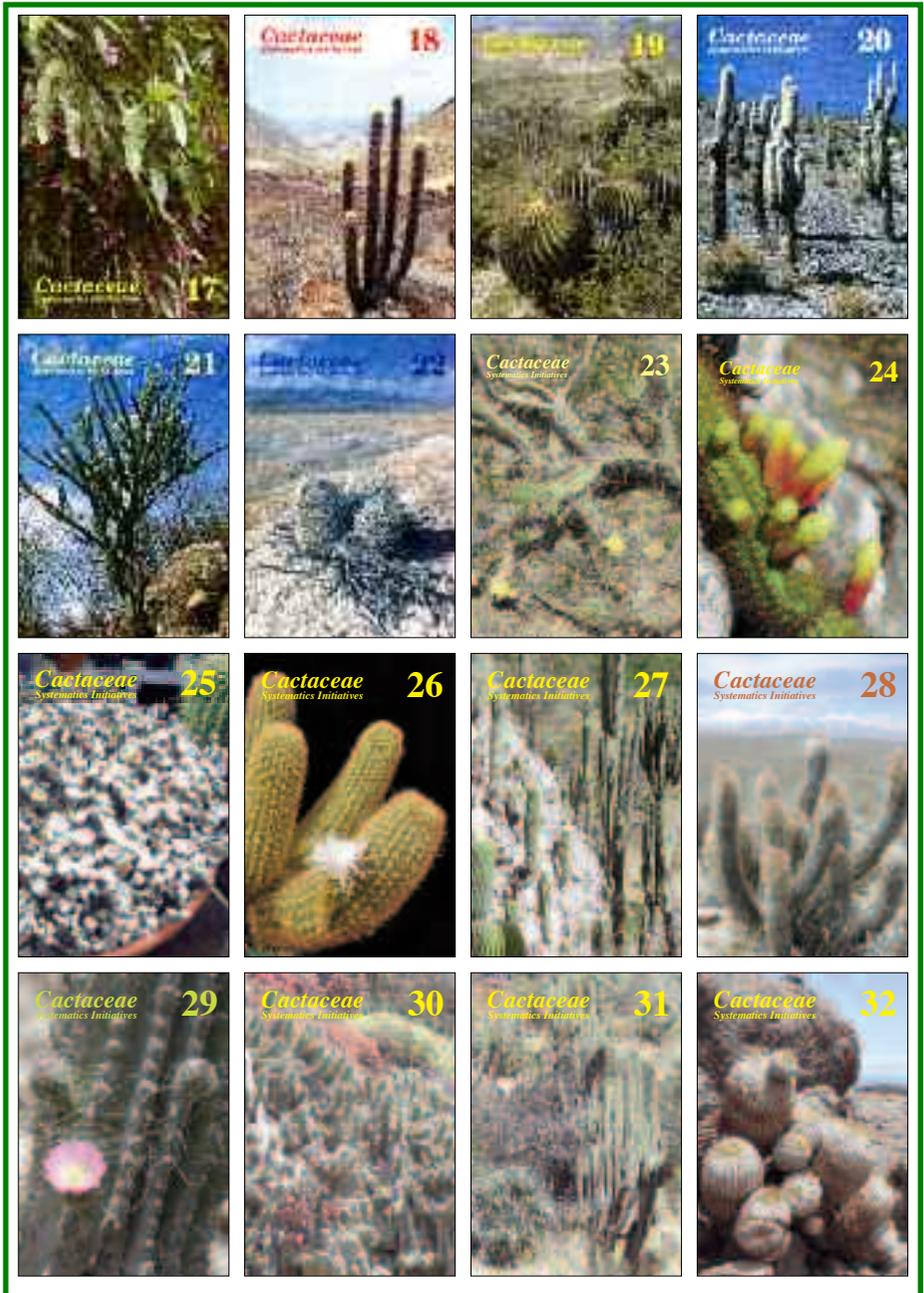
Subsequently, I discussed the project with Urs Egli, and he was able to add locality data and collection dates from Ritter’s slide list to the Excel file, thus potentially adding considerable value to the proposed commentary. In view of the extra work and time involved we agreed it would be more practical and economical to publish the the work, initially at least, as a series of fascicles each devoted to one or more individual genera. In December 2012 formal agreement was reached with the Director of the Städtische Sukkulenten-Sammlung Zürich for Urs to collaborate with us in that way as a co-author and supply data associated with the slides derived from archival material conserved at SSZ.

To ‘set the ball rolling’ I then produced an A5 booklet* containing the available scans of *Copiapoa* (that genus being one of Ritter’s principal interests), plus blanks for the missing slides, as a basis for discussion and decisions concerning the format and content of the eventual version to be published. At the NCL group meeting in May 2013 the scans were reviewed and current views on the taxonomy and identity of the plants discussed, and it was agreed that some of the initial scans needed further ‘photoshopping’ (this is now in the hands of Paul Hoxey and Graham Charles). Then, on 22 September 2013, I had a preliminary discussion at SSZ, after which Urs prepared initial ‘test data’ for some of the FR numbers for which Ritter gave multiple collection localities. I trust a definitive version of the *Copiapoa* fascicle will see the light of day sometime this year.

Recent Studies in the Opuntioideae

This is the provisional title of the next volume of *Succulent Plant Research* and is a sequel to SPR 6, *Studies in the Opuntioideae* (Hunt & Taylor eds, 2002). As with the earlier volume the papers contributed concern the systematics of representatives of the subfamily in both

*Copies still available from me (UK £3 (6 × 2nd Class stamps); or Euros 5, incl. postage) – DH.



These issues of Cactaceae Systematics Initiatives are all still available