

SULCOREBUTIA, FOOD FOR TAXONOMISTS ? (Part 2)

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Fortunately Nol Bredero and John Donald (1981) were able to make the final pronouncement. They wrote enthusiastically : „Ein Problem ist gelöst!“ Bredero had discovered that in case of sulcorebutias, tiny hairs are found behind the scales on the pericarp, which never occur in weingartias. The consequence was a recombining of *Weingartia purpurea* and *toretorensis* to *Sulcorebutia*. The sulco-collectors were satisfied because now it had been concluded unambiguously, that Brandt was wrong. I was so naive as to check this characteristic and offer my findings to a group of specialists. I narrowly escaped excommunication but they advised me to visit a reliable optician urgently. The characteristic was cherished for years in circles of sulco-friends. But these days even the most fervent adherents will have recognized that most of the sulcos don't have hairs behind the scales on the pericarp, while some weingartias, for example HS 164, do.

Günter Hentzschel (1999) also emended the genus *Sulcorebutia*. He thought the shape of the scales on the pericarp to be of major concern. Only the scales of the genera *Weingartia* and *Gymnocalycium* were similar to those of *Sulcorebutia*. A second characteristic was the non-branching funicles of *Sulcorebutia* as opposed to the plural branching of the two other genera. This suggested a close relationship between *Weingartia* and *Gymnocalycium*. Alas the observation of the plural branching of funicles in fruits of weingartias turned out to be an illusion, as was later confirmed by Hentzschel.

Consensus classification

Donald worked on his concept of clines. That's why he took all plants in the wider surroundings of Cochabamba to belong to *S. steinbachii*. It was also part of the spirit of the times to reduce the number of names. In 1984 Donald participated with the Huntington Botanical Expedition Boli-



Fig. 7: *Sulcorebutia steinbachii* JK 94



Fig. 8. Flowersection *Sulcorebutia steinbachii* G123

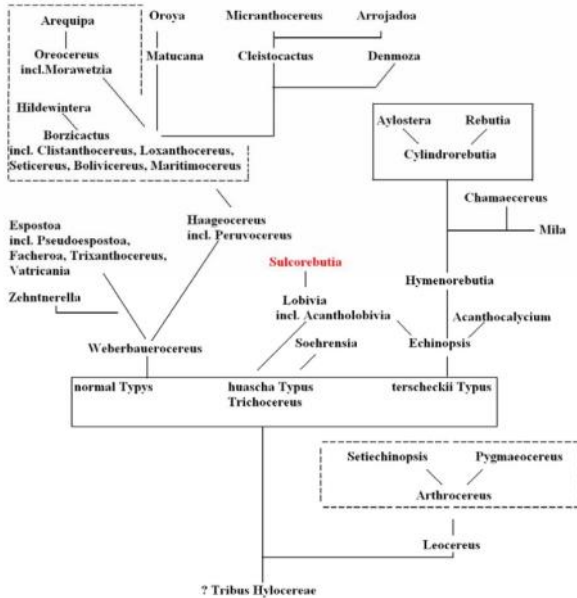


Fig. 9. Cladogram according to Buxbaum

via. In his list of fieldnumbers of 1989 still seven varieties of *S. steinbachii* were mentioned, namely *steinbachii*, *glomerispina*, *tuberculato-chrysantha*, *gracilior*, *kimnachii* (n.n.), *horrida* and *polymorpha*. According to the Cites Cactaceae Checklist of Hunt (1999) all these names of varieties should be allowed to expire and in the meantime the name of the genus was changed to *Rebutia*. A workgroup of the International Organization for Succulent Plant Study (IUBS-IOS) was established in 1984 in order to obtain consensus in the classification of the Cactaceae. Some years later the CITES Plants Committee proposed that the IOS should be supervisor of the compilation of what was going to be the first edition of the checklist. Amateurs were confused. How to interpret this list? Some believed, it was only meant for the use of government customs and excise organisations. Or was it a list with obligatory names superseding all

earlier work? The editors of Succulenta seem to have supported this opinion for a while.

With unbelieving astonishment circles of amateurs heard that the members of the workgroup looked for consensus and found it by the raising of hands. Was this science or some pragmatic acting in which a majority prescribed what names everyone else was allowed to use? Would somebody dare to doubt the expertise of the members of the work group? The latter had indeed all won their spurs. It

was said about Backeberg that he recognized a not-yet described plant in an unfamiliar collection immediately. Would all members of the workgroup have such knowledge at their disposal? Or did they have other skills by which they could create an acceptable project together? This question had been answered before, for example by Reto Nyffeler and Urs Eggli (1994). „The classification of the cacti has been determined during the last decennia to a large extent by amateurs (persons without basic knowledge of systematic botany)“. This has left deep traces as a result of which cacti now are in the bad books of scientific botany. “However many interested amateurs have enlarged the body of knowledge about cacti by their detailed specific knowledge of forms and by their fieldwork, equally often they have caused confusion by their lack of knowledge of important biological concepts (evolution, variability, biology of populations, and so on) and of rigorous



Fig. 10: *Weingartia neocumingii* HS 93



Fig. 11: *Sulcorebutia totorensis* JK 152



Fig. 12: *Sulcorebutia jolantae* HS 68a



Fig. 13: *Sulcorebutia mentosa* JD 175b

working methods.“ Nyffeler and Eggli argued further: “It is the task of systematic botany, to bring about organisation, in the huge number of plant forms according to the postulated phylogensis.“

The IOS-workgroup had started with a list of „undisputedly“ accepted genera. For other genera there was a vote following a discussion. I gladly accept that all members had significant observations and conclusions at their disposal, which meant their opinions were valuable. But we do not know these observations and conclusions. Is my question justified, is a *sulcorebutia*-specialist automatically qualified to vote about *mamillaria* ? By the way who supplied the (hypothetical) history of descent ? Which method did this person

use to collect his data ? Perhaps he used only morphological data.

David Hunt (2006) published the New Cactus Lexicon as a kind of honour to Backeberg. I can imagine that the „splitter“ Backeberg would have felt somewhat displeased if he had seen the book. The genera *Sulcorebutia* and *Weingartia* had been merged into *Rebutia*. Hunt argued, that *Rebutia* could be seen as a genus of “convenience“. It could possibly be biphyletic or polyphyletic. I have not the faintest idea what is meant by these two sentences. The list of names of species was not synonymous with the Cites Checklist. I did not find any explanation. In summary dogmas seem to be circulated and have to be followed blindly



Fig. 14: *Sulcorebutia albissima* JK 39

by allegedly incompetent people like me. Even if they have real doubts about the postulated relationships. Sometimes I wondered if Jaap might have been a member of the workgroup.

Also on the level of species we have come across a similar situation. Willi Gertel (1996) for example mentioned *Sulcorebutia steinbachii* ssp. *tiraquensis* var. *totoensis* subvar. *totoensis*. We might hope that this was based on a profound investigation so that this combination of names would conform to the phylogenesis. But I could not find any information about what exactly was investigated. By the way, could we accept a more close relationship with *S. jolantae*? (Fig. 11 and fig. 12) Or even with some populations those called *S. frankiana*? Using morphological characteristics or isoenzymes this might be possible. Would it be an argument to see for example *S. frankiana* as a subspecies of *S. steinbachii*? I do not expect much enthusiasm for amateurs putting forward such ideas. But they are inspired by objective data.

Interesting are comments of L. Ratz in this

context. (2005) Can a layman have an opinion about the method of the botanist? "Botany is a natural science and from this perspective one can judge when scientific statements are justified according to scientific criteria." and further on:

"The basis of all natural scientific investigations are only objective data." Obviously Ratz was not very convinced about the basis for all sorts of comments, concerning the (cactus) taxonomy.

Some projects

Data, immediately apparent from observing the plant, turned out to lead to shaky conclusions as you can see from the above remarks. This may be made even clearer by the next example. A plant in the greenhouse of Karel became 15 cm high. A cutting of this plant which I received got to only 10 cm. Circumstances and care influence the outward appearance. Buxbaum had restricted himself to characteristics of flower and seed as these would always represent „primitive“ characteristics, meaning that the pressure of the environment would have less effect on them. A further clarification : if a certain characteristic is found on several plants of the whole group, it can be interpreted to be „primitive“. It is unlikely that such a characteristic came into being many times at different places independently of each other.

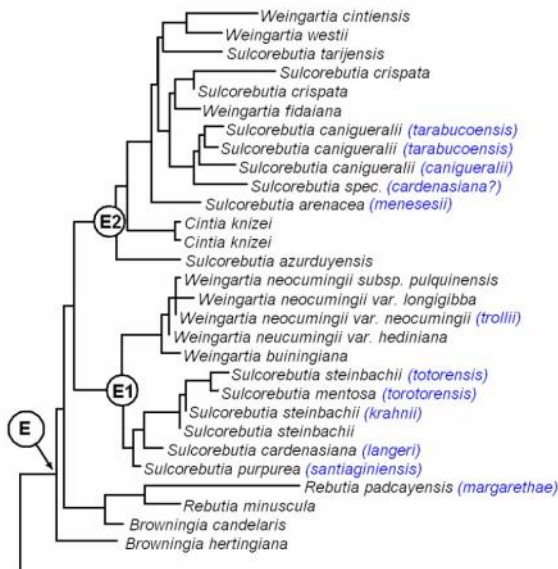


Fig. 15. Cladogram according to Ritz

One can wonder if a characteristic easily changes from one manifestation into another and afterwards back into the original, creating a kind of blinking light situation. Let us start with a population, of which all plants have brownish black spines. By a favourable mutation this population will obtain only white spined plants. But after some generations a next mutation causes only brownish black spines again. Nobody will take such a story to be probable but rather assume that the brownish black spine of the first population, for example *Sulcorebutia mentosa* (Fig. 13), is a primitive characteristic. Likewise the white spine in the other population, for example *Sulcorebutia albissima* (Fig. 14) will be a primitive characteristic. There is no known report of a population in which both forms appear, next to each other. In the *Cites Checklist* is *S. albissima* taken as a synonym of *S. mentosa*. By the text above I do not intend to deny a rela-

tionship. I simply cannot understand the decision to avoid the name „albissima“. Was this choice purely subjective? All statements about relationships are based on probability. Therefore all of them are hypotheses.

One can wonder how to evaluate a hypothesis when faced with evidence that in view of the hypothesis is improbable.

Meanwhile the magic word seemed to be DNA, but this was not a realistic approach for the common amateur.

In 2004 I was a member of a group of enthusiastic amateurs who tried to investigate relationships, using selected isoen-

zymes. One can take the characters of these isoenzymes to be primitive. After all, if a mutation is not favourable, there is serious chance, that the individual with such a mutation will have died because of a failing digestion, before it got descendants.

The result of comparing of isoenzymes showed that *Sulcorebutia* is less similar with *Rebutia*, but it was not really possible to separate it from *Weingartia*. Would Brandt be declared right after all? Some participants did not appreciate the results of the project very much, perhaps because it often did not confirm their postulated relationships. Of course one can question the supposition of actually knowing the correct outcomes in advance. If that were the cases the interpretation of results may well be biased to confirm such supposition. Detlev Metzger rejected the results of the project stating that the database was too small. When I asked him too small for what, he answered with an amiable smile.



Fig. 16: *Sulcorebutia* spec.



Fig. 17: *Sulcorebutia cardenasiana* HS 41

No, he did not know the aim, nor had he read the report (Pot 2006). Afterwards this group of amateurs happened to be able to initiate dna-research. Christiane Ritz and others (2007) published results based on chloroplast markers : indeed it was not possible to separate *Weingartia* and *Sulcorebutia*. The original group of rebutias from Argentina and *Browningia* could be seen as a satellite group of the weingartias. The relationship with *Aylostera*, *Mediolobivia* and *Gymnocalycium* was shown to be significantly less strong. Therefore *Rebutia minucula* and *Aylostera pseudominuscula* could not belong to the same genus, contrary to what had been believed for many years. (Fig. 15) Note, that I put alternative names, used by amateurs, into the clad gram behind the ones used by Ritz. Again some people were not fully satisfied with this result. For example how can one reconcile, that *Browningia* and *Rebutia* are that close in the clad gram ? Did they suspect a too small database in this case as well ? Or were the browningia samples not very reliable on further consideration ? Or would it have been better to use different markers, assuming that this was possible ? Gordon Rowley (2009) proposed a genus *Rebutia*, in which he included the original rebutias,

the weingartias and the sulcorebutias, but not the aylosteras and mediolobivias. It struck me that he showed only a part of the clad gram of Dr.Ritz, from which the browningias had been removed. I don't know the algorithm behind this clad gram. But in the cladograms prepared by me the position of every individual is determined by the presence of *all* other individuals. If a plant is removed, different arithmetical averages will be found and therefore the position of some of the plants may change. For the record : the vertical distances in these cladograms have no other purpose than to keep the diagram legible. Thus they have nothing to do with the closeness of the relationship. Ritz stated, that her project confirmed the observation of the branched funicles by Hentzschel. The same goes for the hairless pericarps of *Weingartia*, *Sulcorebutia* and *Rebutia*. Obviously the error of Hentzschel had not been discovered and Brederoo and Donald seemed to have done careless observations. In my opinion a spicy detail is the names used for species, which would correspond to those in „Das große Kakteenlexikon“ of Anderson in the case of the rebutias and the sulcorebutias and to „Die Gattung Weingartia“ of Augustin and Hentzschel in the case of weingartias. It is reassuring



Fig. 18. Hybrid of *Weingartia fidana* (fig. 5) with *Sulcorebutia tarijensis* JK242

to see that in the clad gram all canigueralii's, all neocumingii's and all steinbachii's are close together. The position of *S. cardenasiana* did surprise me. In the upper part you find a *S. spec.* (Fig. 16). This might be a very interesting plant, not only because it is not well known and wanted. Though it has been discovered at the same latitude as *S. tarijensis*, it reminds one very much of *S. cardenasiana*. (Fig. 17) Suppose – which I do not suspect of course – that some error did creep in. In that case *S. cardenasiana* would occur in two different clades. This I would consider very questionable.

West European amateurs of the continent prefer to keep old, established names, even if they are not quite correct. It was a relief that the „cardenasiana“ mentioned in the clad gram is called *S. langeri* every time in the known collections. Some amateurs wondered why Ritz did

not immediately recombine all sulcorebutias to *Weingartia*. I imagine it was not her intention at all. Ritz is a geneticist. A geneticist is not by definition a taxonomist. By the way, it is not known to me if taxonomist is a recognised profession with a protected status. Possibly everybody is allowed to call him- or herself a taxonomist.

Günter Hentzschel and Karl Augustin (2008) published the second part of „Die Gattung *Weingartia*“. They concluded that morphologically no fundamental differences existed between *Weingartia*, *Sulcorebutia* and *Cintia* concerning body, flower and fruit. Also they had made several attempts to hybridise, between these genera, the results of which could be considered rather successful, while they had hardly any successful results with other genera. The consequence of these observations was the recombining of all sulcore-

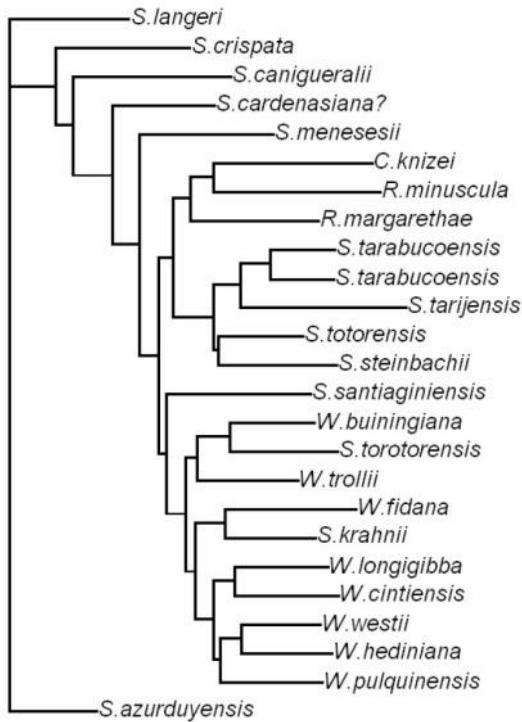


Fig. 19. Cladogram compiled by CactusData

butias to *Weingartia*, which had partially been done already by Brandt, about 30 years before. However some oddities struck me. At page 771 hybrids are shown of *S. canigueralii* and *W. neocumingii* (fa. *sucrensis*). 14 km from Sucre, along the road to Aiquile, sulcorebutias and weingartias, usually called *canigueralii* and *neocumingii* (fa. *sucrensis*) as well, grow next to each other. A natural hybrid was never reported from that place. Should I believe now, that these plants somehow got the wrong names? Or have *canigueralii*-populations different characteristics, by which some will yet hybridise easily with *W. neocumingii* and others will not at all? Hybridising of different species of *Weingartia* with each other or *Sulcorebutia* with each other was not tested by

Hentzschel, because „sufficient hybrids by chance already existed in the collections“. Pip Smart showed me years ago with pride a few hybrids, caused by pollination on purpose. This had not been an easy result. Johan de Vries and I have the same experience. (Fig. 18)

I assume the remark about hybrids by chance can easily mislead us. The presence of fruits is not necessarily evidence of hybridization. This is also well explained by non hybridized pollination, which is very likely to occur as can be seen by the following. Let us accept that on a table in the garden there are 100 plants, which all have one and only one partner for fertilization. Thus on this table exist 50 possible matches. Suppose all plants bloom at the same time with only one flower, and every flower is equally attractive to a bumblebee. The insect has landed on a flower. After some time it will ascend and land again. For every flower the chance that it will land on it is 1 of 100, or 1%. So there is a chance of 99% = 0,99 that the flower of the match plant is not visited. After a second landing this chance will be $0,99 \times 0,99 = 0,992$. After a number n landings the chance has become $0,99^n$. If $n = 459$, the chance of no visit has become less than 1%. So while it may be true that all sulcorebutias are strongly related, this suggestion has not been confirmed by observations of fertilization by chance. There is still another consideration. If hybridization in nature were that easy, one would not expect so many strongly differing populations so close together. The question of relationships motivated me to create a database, which at the moment contains almost 50 characteristics of 1700 individual plants of the genus *Weingartia*. Which characteristics are indeed significant and which are not? From the above

we've learned that experts often disagree. Perhaps the following leads to a sensible choice. A characteristic which is fairly constant for the whole population can be used for this population, but perhaps not for another one. *Sulcorebutia mentosa* for example has magenta flowers, never red or yellow. Thus the colour of the flower is a significant characteristic in this case. In different populations, which usually called *S. losenickyana* all sorts of colours occur. For such populations this characteristic will not be of much value.

Now I selected plants, similar to the plants in the clad gram of Ritz. I selected characteristics which can be interpreted as significant for *all* these plants. In case of the rebutias it was not possible to check this, as there was only one plant of each species available.

The following characteristics underlie the clad gram. (Fig. 19)

In my estimation both cladograms show a fair amount of similarity. Of course we should not forget that the method of Ritz did not investigate relationships at the level of species, while my own method is not really meant for judgements at the level of genera. But after the critical words of Eggli/Nyffeler and Hunt I take this

result as encouraging.

The key to recognition (Pot 2009) was based on this database. Last year somebody discovered, that in this key *Sulcorebutia steinbachii* was recognized in different ways, in which moreover the appearance of the populations were different. Actually he had chosen to look for all steinbachii's in one search operation, like in the New Cactus Lexicon for example.

According to the NGL a steinbachii is recognized by: „Body very variable, clustering; stems depressed-globose; tubercles in circa 13 spirals; central spines 1-3, < 2 cm, almost black; radial spines 6-8, < 2.5 cm, ascending, almost black; flower c. 3.5 × 3.5 cm; hypanthium well-developed; perianth scarlet to magenta, rarely yellow, musty-cented; stigma lobes white. “ How many steinbachii's would be recognized by Hunt in my collection with these data ? How many plants of different species would he recognize to be *steinbachii* ? Can a steinbachii have yellow spines as well, or an orange flower, or a lack of central spines ? Under which conditions will a sulcorebutia not correspond to the image of *steinbachii* ? Obviously I do not understand the standards of Hunt. Nor do I know on what they are based.

position of radial spines	distance lower filaments to pericarp / length pistil	shape of scales on the tube
colour of radial spines	length of filament	colour perianth in top
structure of surface of radials	distance anther to edge perianth	colour perianth above upper insertions
length radials	distance stigma to perianth	colour stigma
position central spines	diameter of stigma	colour filament
shape of the plant body	angle tube below insertions	colour throat
length of the pistil	shape of perianth	colour scales on the tube
area of the insertions of filaments	edge of perianth	colour scales on the pericarp

How to continue ?

Last summer I was visited by a very serious amateur. He confided to me with a serious look, that he would accept a classification in future only if it had been fully based on dna-research. Of course I appreciate this very much. But in the meantime I'm a little worried. Until now I have not heard of any research that will give solutions at the level of species. But suppose it does happen. Will we, amateurs, be able to do something with it ? Will relationships be confirmed by easily observable characteristics ?

Basically they are trying to construct a system based on relationships. But at the same time we want to know what we are speaking about. DNA-research will be a „black box“ to most of the cactus amateurs. A solid system, which however is not understood because of obscure backgrounds has little sense in amateur circles. The same goes for a classification based on carefully hidden morphological observations.

Maybe Karel was close to the truth when he said: „The only thing that really matters is that you understand me. Isn't that right ?“ Suppose somebody speaks about for example *Sulcorebutia tarabucoensis*, which has according to the first description few brown radial spines, about 8 ribs and red-yellow flowers. But he means a plant with many white radial spines, about 13 ribs and a magenta flower, from a very different population. My reaction would be, this person does not know *Sulcorebutia tarabucoensis*. He for his part perhaps believes that I do not understand the range of variability of this species. Thus we will not understand each other.

Professionals have not succeeded in constructing an unequivocal, generally accepted definition of the concept „species“.

How will somebody be able to draw conclusions about the range of variability of these undefined units ? Who will be able to draw conclusions about strong relationships between such units without defined ranges of variability ? Who will be able to understand that nevertheless, numerous relationships are conjured out of a hat ?

I am not a taxonomist. But it is my impression, that empirical experts of the previous century could not have suspected for a moment the variety of forms of some plant families. People have found during the last decennia unexpectedly many populations of *Weingartia*, of which the majority is well distinguishable from all other populations. Nobody – with or without a definition of the concept „species“ – will expect to recognize a thousand species.

But also nobody is able at the moment to create an understandable and acceptable survey of relationships in this genus. Of course one can ignore this multitude of forms. But it does not make any sense to dispose of all unknown populations as „flowerpot species“. The same goes for accepting all relationships blindly without supporting data.

Moreover now we are dealing with new modern technology, which may put postulated relationships in a very different light. I can easily imagine a Gordian cactus knot, which we will not be able to untangle by continuing as we have in the past. Yet I keep hoping for new approaches, which will lead us out of the dilemma. Maybe one day the knot will be untangled in a more sophisticated way than used by the legendary Alexander. But some splitting may be not avoidable. People like Nyffeler and Eggli may find my considerations anarchistic. This however is not my aim. I would welcome an

explanation by an expert about the whole area in clear and workable terms. Maybe this paper will give the initial impetus.

I like to thank Jim Gras for proofreading the English text.

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