

# SUCCULENTA

The image shows the cover of a magazine titled 'SUCCULENTA'. The title is written in a large, bold, yellow, 3D-style font at the top. The background is a close-up photograph of two vibrant pink cactus flowers in full bloom, set against a bed of smooth, white and light-colored pebbles. The flowers have many petals and prominent yellow stamens. To the left, a portion of a green cactus with dark spines is visible. At the bottom, there is a line of text in a smaller, yellow, sans-serif font providing publication details.

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# SULCOREBUTIA, FOOD FOR TAXONOMISTS ?

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**Many cactus lovers seem to have an opinion about the nomenclature of their plants. But do they really know the right name? Who will determine this and in particular, how is it done ? We haven't heard the last word on this subject.**

## **Creation of the image**

My cactus hobby was scarcely born, when I met Karel. He presented himself as a very experienced collector. He invited me emphatically to visit him. And not to hesitate in asking any question. As I was eager to learn, I accepted the invitation gladly. Within a week I had entered his sanctum. It was indeed a paradise. Many plants were in bloom. In every flowerpot there was a label with a name on it. This was quite a different experience to “*120 cacti in colour*”.

Shortly before the visit somebody asked me, if I already had the “hand of the negro” (*Maihuenopsis clavarioides*). I hadn't a clue, but Karel showed me the plant immediately. He was definitely an expert. “But”, he said, “my great love are the jonias.” These I also knew nothing of but Karel showed me them and he was entranced as he described the beautiful flowers. At that very moment Jaap and Gijs dropped in by chance.

“These are not jonias”, Jaap corrected. “These are lobivias. To be more precise *Lobivia jajoiana* from Bolivia.” Karel looked bewildered but kept silent but Gijs raised his eyebrows: “Actually they are *echinopsis* from Argentina.” As if stung by a wasp Jaap turned. “They do come *for sure* from Bolivia. After all you can derive

this from the name. And I heard it personally from Backeberg. He cannot be wrong, as he found them himself. You say *Echinopsis*? You mean *Echinocactus*? But these plants are lobivias and there is an end to it !”

I didn't understand much of it. Had Jaap really had contact with the famous Backeberg ? On the other hand Gijs didn't look stupid. I chose to keep silent as I didn't want to be thought a dummy. A few minutes later the guests left. Karel muttered to me in offended tones “This Jaap, he must always know better! The only thing that really matters is that you understand me. Isn't that right?”

Not lobivias, but *sulcorebutias* took up all my attention during the next years. But during my search for possible correct names I have heard such conversations over and over again. Apparently we cactus lovers feel a deep satisfaction in such talks, in which expertise is not really required. I believe that German-speaking people use the word “*Bierernst*” in such cases. I was struck by the fact that many statements made no reference to supporting information. Observations were hardly done, but this was no obstacle to having a firm opinion.

For the record I should say that names used in this article correspond to what

hobbyists on the continent use to say. This does not mean that the original authors will always still support these names. Therefore author citations are omitted in this paper.

### **Rebutia**

A small plant with the name *Echinopsis minuscula* was brought onto the market by Pierre Rebut. It probably came originally from Argentina. Later on such plants were indeed found in the province of Tucuman.

In 1895 the genus *Rebutia* was defined by K. Schumann. (Fig 1) Schumann had observed that the new plant did not bloom from the areole, so it could not be an echinopsis. The plant itself resembled an echinocactus or malacocarpus, but as the flo-

wer originated from outside the areole, it had to be related to *Mamillaria* “without any doubt”. The shape of the corolla and the pericarp however prevented classifying it here. Some years later Schumann withdrew *Rebutia*.

28 years later on Spegazzini (1923) defined the genus *Aylostera*. The decisive feature was a partial fusion of the style and the tube. Using this characteristic one could clearly distinguish *Aylostera* from *Rebutia*. (Fig 2 and Fig 3)

Have the observations of Schumann been checked? I suppose so. Though I was never able to confirm that *Rebutia* did *not* bloom from the areole. But I have heard amateurs discussing seriously about pistils which had grown together to the tube opposite free pistils.



Fig. 1 *Rebutia minuscula*



Fig. 2 Flowersection *Rebutia minuscula*



Fig. 3 Flowersection  
*Aylostera schatzliana* JK423



Fig. 4: *Browningia candelaris* (Photo: Craig Howe)  
<http://cactiguide.com/cactus>

Berger (1929) did not mention a genus *Aylostera*. He recognized only one genus *Rebutia*. He wrote: "Small plants, roughly globular, reminiscent of mamillaria, with tubercles in spiral rows and small spines. Flowers from the older areoles, often originating close to the base, small, funnel-shaped, with slender tube, open by day. These small plants from the mountains are not to be classified in *Echinocactus* nor in *Echinopsis*."

I have some problems with this statement. Schumann used the observation that the plant did *not* bloom from the areole to distinguish the genus. The same characteristic was denied by Berger. What did he observe *instead* to be still able to recognize a genus *Rebutia*, distinguishable from other genera? We never will know. Berger mentioned 6 species: *Rebutia minuscula*, *R. deminuta*, *R. pseudominuscula*, *R. pygmaea*, *R. fiebrigii* and *R. steinmannii*, of these the first four species came from Argentina and the others from Bolivia.

Does the characteristic, that pistil and tube are partially grown together, make any sense? One taxonomist will find it important, the other will dismiss it. It was Ritters opinion (1980), that the fusion hardly had taxonomical meaning, as it would have developed simultaneously in different separated lines.

Often the characteristic is ignored. How would early taxonomists have decided whether a plant belonged to *Rebutia* or not? I suspect that plants have been thought to be related after a rough observation without a real check. Is this strange? No, because such things still happen. It would be really strange, if one used a formal checklist of characteristics for every new plant to decide to what genus it belongs. The rough and ready ap-

proach is a natural way of acting, but of course it contains the risk of overlooking a significant characteristic.

In the same way relationship between plants will not only be accepted, but denied as well. Many people do accept *Rebutia minuscula* and *Aylostera pseudominuscula* to belong to the same genus. But who will for example expect a closer relationship between *Rebutia minuscula* of only a few centimetres diameter and height and a 5 meters high *Browningia candelaris*? (Fig. 4) Nobody, don't you think? More about this later.

During the winter of 1929-1930 the plant collector José Steinbach sent Werdermann a plant, which "probably had been discovered in the wider surroundings of Cochabamba (Bolivia) at an altitude of 2500 m". Unfortunately Steinbach died shortly afterwards. Werdermann identified the plant as "doubtless" being related to *Echinocactus minusculus*. "Schumann had defined, based on its special flowering characteristics, the genus *Rebutia*, which he withdrew later on, and classified this plant as *Echinopsis*." But Werdermann himself argued to reintroduce the genus *Rebutia* for this "well characterized" group. It may be just me, but I have no idea what Werdermann meant with "well characterized". Did he possibly understand *Rebutia* in the same way as Berger?

Werdermann (1931) described the "interesting" plant, which strongly resembled *Lobivia boliviensis* qua habitus, but nevertheless was called *Rebutia steinbachii* based upon one single flower. A second opinion was impossible, as the plant had died when the publication was made. By the way, to my mind a plant can be called interesting if it is little known and in the mean time is or will be desirable. It must be more than just a study-



object. The possession of interesting plants works as a lure for collectors by the way. Thinking back, I guess, that in this sense Karel did not have many interesting plants. Who indeed wants a “jonias”?

### **Weingartia**

As far as I know all authors mentioned above were professional botanists. But amateurs also made their contributions heard. Very well known was Curt Backeberg. According to Wikipedia he met the Czech plant collector Alberto Vojtěch Frič by chance in 1927. His stories stimulated a desire for adventure in Backeberg who decided to import cacti himself. He was so strongly fascinated by these plants that he published a lot, for example the standard work of six parts „Die Cactaceae“.

In 1933 Backeberg defined the genus *Spegazzinia* with the species *fidaiana* (Fig. 5) and *neumanniana* (Fig. 6). Backeberg had observed a hairless scales tube with these plants, while the flowers kept open until they had wilted. This last characteristic has not been noticeable with my own plants. Had Backeberg really compared the tube with the one of *Rebutia minuscula*? Albert Hofman (personal statement) assumes that the first description of the species *fidaiana* (*fidana* Hunt 2006) has to be taken as a first description of the genus also. Well, I have seen quite some cacti with this label, but with many of them I really had doubts that these were the plants meant by Backeberg. Are experts more flexible than me? Or is there insufficient quality of reference data (incomplete, inaccurate), as a result of which classifications are also unreliable. Another genus *Spegazzinia* already existed at the time. Therefore Werdermann changed the name of the genus into *Weingartia* (1937) without supplying extra information. Later on *Echinocactus cummingii*, which had been described already

in 1849, was also classified in this genus. The result of this remarkable sequence of events was nevertheless correct. Not only was the genus *Weingartia* poorly defined by the summary description of Backeberg, the plant description also was not clear, as the name had been used twice, for completely different plants, which had died long ago. Boom (1962) dedicated a very worthwhile article to the correct name of the plant. He ended his account with the remark “Look at what the consequences may be if the international Rules for Botanical Nomenclature are not employed in the right way; it is definitely necessary, that everybody, who is occupied with the taxonomy of cacti (and of course of all other plants) acquaint themselves with the proper use of these rules. Especially in the case of cacti much incompetent work has been done in this area.“ Then a list of names followed. In case of ambiguous names was added „quoad descr.“

*Echinocactus cummingii* Salm-Dyck non Hopffer, Cat. Hort. Dyck.: 174 (1849), (1850), Allg. Gartenz. 11, 225 (1843).

*Lobivia cumingii* (Hopff.) Br. & R., The Cact., 5, (1922), quoad descr.

*Oroya cumingii* Kreuz., Verzeichnis: 39 (1935).

*Spegazzinia cumingii* (Hopff) Backbg., Kakt. ABC: 298 (1935), quoad descr.

*Weingartia cumingii* (Hopff.) Werd. ex Van Oosten, Succulenta 21: 129 (1939), quoad descr.

*Weingartia neocumingii* Backbg., Kakt. and. Sukk., 1: 2 (1950).

*Gymnantha cumingii* (Hopff.) Ito, Expl. Diagr.: 53 (1957), quoad descr.

*Gymnocalycium neocumingii* (Backbg.) Hutch., Cact. & Succ. J. (U.S.): 29 (1957).

*Gymnocalycium cumingii* (Br. & R.) Hutch., Nat. Cact. & Succ J.: 14 (1959).

I can imagine that not only the interested

amateur will be lost here, the professional-taxonomist will also perhaps do a double-take. How could the name of a species be connected to different genera so often in only 35 years ? Searching for explanations, one is reminded of the speed with which Jaap solved taxonomical problems. Personally I would guess that the poor quality of information is the cause of this chaos.

### ***Sulcorebutia***

Backeberg defined the genus *Sulcorebutia* (1951). Here is an attempt to interpret the Latin text : „Plants caespitose, with rather small offsets, ribs tuberculous; tubercles lobivoid, axeshaped (!), with crack (!); flower funnel form, originating from the circle round expanded crack, with the colour of the morning sun, with scales, hairless (!) fruit still unknown – Bolivia, near Colomi (Cochabamba) in an altitude of 3400m (Cardenas). Typus: *Rebutia Steinbachii* Werd.”

The characters followed by “(!)” will have been the main criteria. It is nice to conclude, that this text differs from the one in “Die Cactaceae” (1959).

Werdermann described the colour of the flower to be red, Backeberg mentioned “the colour of the morning sun” like the one of *Rebutia violaciflora*. (Fig. 7) Previously I had heard the explanation, that some taxonomists were rather flexible in their interpretation of the colour red. Later on Pip Smart told me, that Martin Cárdenas from Cochabamba had looked for years for a plant like the one of Werdermann, with positively real red flowers. He never found one. Obviously his quest brought him to the area east of Cochabamba. Backeberg got his plants from Cárdenas. It seems plausible, that the type-plant defining the genus comes from a population different to the one described by

Backeberg. Is this really important ?

Because herbarium material of the type-plant of Werdermann no longer existed, in 1999 a neotype was deposited in the herbarium of the Städtische Sukkulentensammlung in Zürich. This suggests it should be a plant from the original population. Would this plant completely correspond with the description of Werdermann ?

David Hunt (2006) explained: “The type (which does not have to be an average or ‘typical’ specimen of the species or other taxon concerned) gives the botanist an absolutely fixed point of reference from which to judge whether other specimens to which the name has been applied are correctly identified or not.” Then the neotype was superfluous, if it was identified, based on the plant of Werdermann, to belong to the same species. However if the identification was different, the deposit would lead to a paradox.

To Backeberg the crack (sulco) at the upper side of the tubercle was a distinguishing characteristic of the genus, therefore the name *Sulcorebutia*. Moreover he mentioned the lobivoid appearance of the plant and the hairless flower. The totally free pistel was mentioned in the English language comment. (Fig. 8)

Although Cárdenas sent Backeberg a couple of plants, he rejected this genus. There was a rumour that he objected in the first place to the amateurish procedure of Backeberg. Anyway, he rejected *Sulcorebutia* and described for example *Rebutia arenacea* and *R. glomeriseta* in 1951, *R. tiraquensis*, *R. totorensis* and *Aylostera Krugeri* in 1957 and even *Weingartia torotorensis* in 1971, all of which were recombined to *Sulcorebutia*. I never understood why Cárdenas believed to recognize in this plant an aylostera (with fused pistil and scales on the tube covered by



Fig. 5: *Weingartia fidaiana*



Fig. 6: *Weingartia neumanniana*

hairs).

John Donald (1971) made a stand against the opinion of Cárdenas. First he mentioned that deliberate hybridization between *Sulcorebutia* and *Rebutia* had brought no descendants. One can pollinate *Sulcorebutia* and *Lobivia* with *Chamaecereus*, but not with *Rebutia*. Also one can pollinate *Weingartia* and *Sulcorebutia* with each others, but again not with *Rebutia*. Also hybridization between *Rebutia* and *Lobivia* is not possible.

Cárdenas had criticized the European taxonomists because of the lack of field experience with *Rebutia*, as a result of which they were not in a position to develop a proper classification. Such remarks are rather familiar to me in circles of current day specialists. Donald however had observed hundreds of plants transported directly from their natural surroundings, so he had a right to speak. In his opinion the separations between species was blurred and he recognized clines, for example *Sulcorebutia candiae*, *S. mensesii*, and *S. xanthoantha* and *Sulcorebutia kruegeri*, *S. arenacea*, *S. caineana* and *S. breviflora* (*brachyantha*). At last Donald compared *sulcorebutias*, *rebutias*, *weingartias* and *lobivias* with each others based on 9 main characteristics, divided

into 30 sub characteristics. The presence of these sub characteristics was measured. It resulted in the following table:

L	15	10	10	1
W	10	15	13	4
S	10	13	15	4
R	1	4	4	15
	L	W	S	R

The main characteristics were (1) structure of the rib, (2) tubercle, (3) position of the areole, (4) structure of the areole, (5) appearance of the flower, (6) structure of the receptacle, (7) insertion of the filaments, (8) fruit and (9) seed.

I'm afraid I do not understand this explanation of Donald very well.

Franz Buxbaum (1967) did recognize the genus *Sulcorebutia*. He believed he was able to map the connections of taxonomical units as determined by ancestry. Of course these units had to be monophyletic. He intended that a group of taxonomical units should have a common ancestor, from which all the members of subgroups - but no other groups - are descended. Buxbaum compiled the following cladogram - in this case a phylogenetic tree. (Fig. 9)

### Shifting insight

It is not clear to me how Ritter recognized that for example *Aylostera krugeri* was a sulcorebutia, if he only used the characteristics of the genus *Sulcorebutia* mentioned by Backeberg. Maybe he did something like this: B looks like A, C looks like B, D looks like C, and so D looks like A? Ergo D is related to A?

Donald also accepted a considerable list of sulcorebutias, doubtless by a process of shifting insight. This led automatically to an extension of interpretation, which was followed by an amendment to the genus (1972). In this the crack on the tubercle, what gave the genus its name, was still mentioned.

Fred H. Brandt (1977) surprised friend and foe alike by the joining of *Weingartia* and *Sulcorebutia*. He wrote: "As a result of observations spanning many years as well as investigations of the plants and seeds of these two genera I have come to the conclusion that these genera are one closed unit and thus have to be joined." Consequently all sulcorebutias were recombined. It is a pity that I could not recover details of his observations. Nor do I know what he investigated. When I was introduced shortly afterwards to well-known sulc-specialists I was pressed emphatically to forget Brandt. Had he committed a heresy? Inquiry brought no more than a recurrence of the statement.

Donald and Brederoo studied for a considerable time to decide if or how to separate the genera. It was not very easy to obtain an unambiguous judgement. Perhaps one had to think of three strongly related groups: (I) the group of the southern weingartias with *Weingartia fidaiana* as a species of reference, (ii) the group of the northern weingartias *W. neocumingii* as a species of reference (Fig. 10) and (iii) the group of the sulcorebutias with

*Sulcorebutia steinbachii* as a species of reference. Actually I did not find any clear motivations for these suggestions. By the way, according to Donald *Weingartia neumanniana* was found in the area of the Bolivian-Argentinian border, though Backeberg himself stated the area near Humahuaca, 125 km more southward.

Friedrich Ritter (1980) concluded that the observation of Backeberg concerning the tubercle was based on an illusion. There was no crack at all, only a fold. This phenomenon was found in other genera as well, for example in *Weingartia*. But Ritter did recognize the genus *Sulcorebutia* because "the short, light edged scales of the flower are not these of *Rebutia*".

Moreover he had observed, that the areoles (?) on the flowers of *Rebutia* (as far they were not hairless) except wool, almost always bear several to many bristles. Ritter believed *Rebutia* to be a genus on its own, to which also *Aylostera* with its hairy scales belonged. In spite of this remark I cannot build on the considerations of Ritter. Nevertheless they were good enough for a series of recombinations, like *Sulcorebutia arenacea*, *krugeri*, *tiraquensis*, *totorensis*, which obviously were accepted by everybody. Of course it was annoying, that this seemed to be overruled by the activities of Brandt. Were the arguments of Ritter more convincing?



Cover :

Fig. 7 - *Sulcorebutia steinbachii* JK 94

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Until today, this English version (translation by Johan Pot) was unpublished.

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