THE FAMILY TREE (2)

Johan Pot

Islands

Dr. John Donald reported with excitement after his first Bolivia trip about populations of Sulcorebutia, which were similar to islands. I must admit, that the significance of this comment was not clear to me for years.

Donald had observed that there was a strict separation between the usually very small populations. Could somebody explain the reason for this? Or was it pure coincidence?

Anyone going to discover sulcorebutias will quickly look, based on experience, for a particular type of landscape. Sulco seem pickier than *Echinopsis obrepanda* for example, which can apparently thrive in all sorts of places. There is no evidence that sulcorebutia can survive in the areas between the "islands".

How can such an island ever been inhabited? Perhaps the conditions in the intermediate areas have been beneficial long ago? Actually, I don't know any reason to believe so. At the moment I can only report, that plants or seeds in one way or another ended up on these islands. They came *out of the blue*. Maybe they were transported by the wind, perhaps by an animal, perhaps by both. There are indications that relatively large distances can be bridged. Cor Noorman found in a population of *Sulcorebutia krahnii* a plant, which could have come from a place 60 km away. Johan de Vries discovered a large population of *Sulcorebutia langeri* 50 km from the known populations. Between these *langeri* populations nobody has ever discovered another sulcorebutia.

Such migration may still occur. They call this gene flow. But there is no reason to believe that on such an island material always comes from the same source. This allows major differences to occur between neighbouring islands if the original inhabitants cross with newcomers4. For this reason, it is not convenient to classify plants only on the basis of their habitat.

Can an immigrant in a population actually participate in reproduction? You might suspect that if fertilization does not work in the greenhouse, it will be the same in nature.

In the spring of 2013 a group of six cactus lovers tried to perform pollinations between plants of different populations. Three of them did not produce any positive results. I was one of the lucky ones who succeeded in harvesting seeds. But oddly enough this year, half of the 460 pollinations of plants of the same population produced no result.

For years, Willi Heil has pollinated plants very carefully. The plants used were not only isolated, but he put a non pollinated plant next them. If this plant would show a fruit, it was an indication of an unintended pollination. Heil said that pollination of three plants generally yielded significantly better results than of two plants.

Is this not a remarkable fact? We accept more or less similar looking plants from

1 One might use the word *hybrid*, if it is clear that the original inhabitants and the newcomers belong to different species. But does this have meaning, if they produce fertile offspring?

the same population as members of the same species. Yet more than once fertilization is not successful. Of course this can not be concluded after one single attempt. But Johan de Vries also reported that he had never harvested from any seeds of *Sulcorebutia callecallensis* VZ 56.

Recently the editors of Succulenta wrote in a commentary: *Biologists mean by 'species' a group of plants which produce fertile offspring in nature.* This is clear, isn't it? The plants of a species produce fertile offspring. Is the converse true? If plants of a group produce fertile offspring, do they belong to the same species? How about: "If plants of a group do not produce fertile offspring, they do not belong to the same species?" Do the plants with fieldnumber VZ 56 not belong to the same species? And, assuming if my suspicion is true, do the gymnocalycium and Weingartia who once did have fertile offspring, belong to the same species?

Dr. Werner Kunz (2012) summarizes his article with *'From Darwin the scientific world pursues classification of the present situation on earth, dividing the evolving biodiversity of organisms into taxonomic categories. It must seem surprising to us that a generally applicable concept of species has not been found so far. A reason for this is the desire to assign to the concept of species a real, independent existence. But in reality 'species' is a nominalistic concept, a construction of thinking man. Evolutionary lineages and reproductively isolated populations do not exist in reality. As a consequence, they also do not have the characteristics and properties of substantial material. Populations can neither mate nor reproduce sexually. Only individuals can. It is not the species, which is changed by evolution, but only the individual is subject to mutation and selection. The unit of evolution is the organism, not the population.'*

Volker Storch does not agree with Kunz: *'What is a human being?" A human being is a being that can think and speak", will often be the answer. This sounds logical at first. But are people who cannot think clearly and /or are deaf and dumb, not people? The more you think about it: the question of being human sounds so simple – yet nobody is able to give a precise definition. At least not a concise one. Nevertheless, no one doubts that there are people'*. He wonders how programs against eradication of species can take shape, if the concept species is controversial. *'Practice has shown that this is quite possible, if one accepts some ambiguities. Because nature cannot wait until we have discussed at length, into which exact categories animals and plants, which are apparently endangered by extinction, should be classified. Also a vague concept of species has proved useful millions of times, just like the concept, that human beings exist.'*

Storch might choose certain characteristics that are not appropriate for the intended purpose. But I suspect that this approach is widespread.

Classification

Perhaps the argument of Kunz is not refutable. Perhaps the concept of species is merely the result of our intuition. But it turns out to be good enough for many people to understand nature effectively, as Storch argues. I know of no one who completely renounces the concept in practice.

So it is certainly wise to try to *recognize* a species as well as possible. And this turns out to be not very easy in the case of

Weingartia. I blame this on not very consistent handling of characteristics.

To get an impression of the extent to which characteristics within a population are constant I have examined 170 populations , of which I show the first 26 in Table 1.

Plants were tested on 15 characteristics .

FIELD-NUMbER	NAME	Number	PERIANTH RED 97	PERIANTH YELLOW 95	FILAM. RED 75	FILAM. YELLOW 83	STIGMA STYLE 81	FILAM. PERIC. 76	F-TUBE ANGLE_B 76	RADIALS NUMBER 83
WR289	S.rauschii	5	1-100	0-100	2-100	0-100	1-100	2-100	0-100	1-100
HJ0441	W.frey-juckeri	4	0-100	1-100	0-100	2- 75	2-100	0-100	2-100	1-100
HS098	W.longigibba	4	0-100	1-100	0-100	2-100	2-100	0-100	2-100	2-100
WR042	W.neumanniana	5	0-100	1-100	0-100	2-100	2- 80	0-100	2-100	0-100
GR21	S.hertusii	4	1-100	0-100	2-100	0-100	1-100	2-100	0- 75	1-100
BLMT89	W.kargliana	6	0-100	1-100	0-100	2- 50	2-100	0- 83	2-100	0-100
MC5532	S.menesesii	4	0-100	1-100	0-100	2-100	2-100	0- 75	1-100	2-100
EH6236	S.pasopayana	4	1-100	1-100	2-100	1-100	1-100	1- 50	1- 75	0-100
JK163B	W.sucrensis	4	0-100	1-100	0-100	2-100	2-100	0- 50	2-100	2-100
JK503	S.sp.Torotoro	4	1-100	1-100	1- 75	2-100	1-100	0-100	2-100	2-100
JK073	S.sp.Barranca	5	1-100	0-100	1- 80	0-100	0- 80	2-100	0-100	1-100
L382	S.tarabucoensis	5	1-100	1-100	2- 60	1- 80	1-100	1- 80	0- 80	0-100
L387	S.pasopayana	5	1-100	1-100	2- 80	0- 60	0-100	2-100	0- 80	0-100
LH08	S.augustinii	4	1-100	0-100	2-100	0-100	1-100	0- 50	1-100	2-100
JPNEU	W.neumanniana	4	0- 75	1-100	0-100	2-100	1- 50	0-100	2-100	0-100
HJ1164	S.tarvitaensis	5	1-100	0-100	1- 80	0- 80	1-100	2-100	0- 80	1- 80
CB	C.knizei	3	0-100	1-100	0-100	2-100	1- 67	0- 67	2- 67	0-100
HJ0962	S.sp.Co.Lampasillo	4	1-100	1-100	1- 75	1-100	1-100	2-100	1- 75	0- 50
HJ1135	S.azurduyensis	5	1-100	1-100	0-100	2-100	1- 80	1- 80	1-100	1-100
HS121	S.rauschii	5	1-100	0-100	2-100	0-100	1- 80	2- 80	0- 80	1- 80
HS149	S.totorensis	6	1-100	1- 83	0-100	0- 83	1-100	0-100	2-100	2-100
HE223	S.carichimayuensis	4	1-100	1-100	0-100	2- 75	1- 75	0- 50	1- 75	1-100
HJ0950	W.sp.Zurima	5	0-100	1-100	0-100	2- 80	2-100	0- 80	2-100	2- 60
HJ0994	S.sp.Zudañez	4	1-100	0-100	2-100	0-100	1-100	1- 75	0- 50	1-100
HS125	S.sp.Mandinga	5	1-100	0-100	2-100	0-100	1-100	2- 60	0-100	1-100
JK152	S.totorensis	4	1-100	1-100	0- 75	0- 75	1-100	0-100	2- 75	1- 50

FIELD-NUMBER	NAME	Number	RIBS NUMBER 77	PERIANTH ROUND 81	PERIANTH EDGE 77	OFFSET 89	CENTRAL-SPINE 94	RADIAL COLOUR 78	RADIAL ROUGH 88	AVERAGE CONSTANT
WR289	S.rauschii	5	1-100	1- 90	1-100	1-100	0-100	2- 90	0-100	98.6
HJ0441	W.frey-juckeri	4	0-100	1-100	0-100	0-100	1-100	2-100	2-100	98.3
HS098	W.longigibba	4	0-100	1- 63	0-100	0-100	1-100	1- 75	2-100	95.8
WR042	W.neumanniana	5	0- 80	1- 70	0-100	0-100	1-100	2-100	0-100	95.3
GR21	S.hertusii	4	1- 75	1- 63	1-100	1-100	0-100	0-100	0- 88	93.4
BLMT89	W.kargliana	6	0-100	1- 92	1- 67	0-100	1-100	2-100	0-100	92.8
MC5532	S.menesesii	4	2-100	0- 63	0-100	0- 75	1-100	1- 88	1- 88	92.6
EH6236	S.pasopayana	4	0-100	1-100	0-100	1-100	0-100	2-100	1- 63	92.5
JK163B	W.sucrensis	4	2- 75	1- 75	0-100	0-100	1-100	1- 88	2-100	92.5
JK503	S.sp.Torotoro	4	1- 75	1-100	1- 75	0-100	1-100	0- 63	0-100	92.5
JK073	S.sp.Barranca	5	1- 60	1-100	0-100	1- 60	0-100	0-100	1-100	92.0
L382	S.tarabucoensis	5	0- 80	1-100	1-100	1-100	0-100	2-100	0-100	92.0
L387	S.pasopayana	5	0-100	1- 80	1-100	1-100	0-100	2-100	0- 80	92.0
LH08	S.augustinii	4	2-100	1- 63	1-100	1-100	0-100	2- 63	1-100	91.7
JPNEU	W.neumanniana	4	0-100	1-100	1- 75	0-100	1- 75	2-100	0-100	91.6
HJ1164	S.tarvitaensis	5	0- 80	0- 90	0- 80	0-100	0-100	2-100	0-100	91.3
CB	C.knizei	3	0-100	1- 83	0-100	0-100	0-100	-	-	91.0
HJ0962	S.sp.Co.Lampasillo	4	0-100	1- 88	0- 75	1-100	0-100	0-100	1-100	90.8
HJ1135	S.azurduyensis	5	0-100	0- 60	1- 60	0-100	0-100	0-100	0- 80	90.6
HS121	S.rauschii	5	1- 60	1- 80	1-100	1-100	0-100	2-100	0-100	90.6
HS149	S.totorensis	6	0- 33	1- 92	1- 67	0-100	1-100	2-100	0-100	90.5
HE223	S.carichimayuensis	4	0-100	1-100	1-100	1-100	0-100	2- 75	0-100	90.0
HJ0950	W.sp.Zurima	5	0- 80	1- 60	0-100	0-100	1-100	1- 90	2-100	90.0
HJ0994	S.sp.Zudañez	4	1- 75	1- 75	0-100	1-100	0-100	0-100	1- 75	90.0
HS125	S.sp.Mandinga	5	1- 80	0- 50	0- 80	1-100	0-100	0- 90	0- 90	90.0
JK152	S.totorensis	4	2-100	1-100	1- 75	0-100	1-100	2-100	0-100	90.0

Table 1

Two or three categories were assigned to each of the characteristics. This data was collected by field number and it was determined which category is most prevalent for each characteristic. The most prevalent value is shown in the table along with the percentage of plants having this value. A field number was included only if it contained four or more plants.

For example, there are five plants of *S. rauschii* in the database. The characteristic Perianth red has category = **1** if true, otherwise zero, and is true in this case for all five plants, so the score is 100%. If the characteristic is 100% constant, the corresponding cell is coloured yellow.

On the far right we find 98.6% as the average of the percentages of the 15 characteristics of *S. rauschii*. This is the highest score of the table. Apparently, I have expressed by a number, that the characteristics chosen for *S. rauschii* have little variation. In other words, *S. rauschii* is easily recognizable. This is true for 26 of the 170 populations , of which the characteristics are constant for an average of 90% or more. The lower the average, the more difficult to it will be to recognize a plant of that taxon.

Directly underneath the names of the characteristics in the table is shown for what percentage that feature is constant for all 170 records. The characteristics *Perianth red* (97%), *Perianth yellow* (95%) and *Central spine* (94%) stand out as the most consistent.

Since I want to avoid getting bogged down into details I will explain just these three features here.

Images of flower sections were taken with a photoscanner. Using these pictures I tested the upper part of the perianth at ¼ distance from the edges for colour. It was not difficult to express the colours of all 1900 measured petals in numbers[2]. This is useful for quantifying the otherwise vague characteristics of “noticeable presence of violet red pigment” and “noticeable presence of yellow pigment.” A biologist might argue that much colour information is lost in this way. He is undoubtedly right, but the scanner does not read it. Neither is it perceptible to the human eye.

In the example of *S. rauschii* I find for *Perianth red* indicator 1 = presence of violet red pigment and *Perianth yellow* indicator **0** = absence of yellow pigment.

Our subjective experience of a truly red flower colour, in this scheme will indicate both violet red pigments and yellow pigments as present.

In conversations with other collectors I noted that the concept central spine gave rise to disagreement. For my concept of central spine I refer to Figure 6. I call the brown spines *central spines* and the white ones *radial spines*.

Fig 6: By way of illustration *Sulcorebutia tiraquensis* KK1770: the author describes the brown spines as “central spines” and the white ones as “radial spines”.

2 For the filaments this approach does not work effectively. This may be the reason for the low percentages in the top of the columns.

The practical utility is obvious. If we have a plant with central spines in front of us, we do not have to look for a name among plants *without* central spines in general. Also a magenta flower with only violet red pigment will usually not be found in a population with yellow flowering plants without violet red pigment. If such a phenomenon occurs, one could seriously wonder how this should be interpreted. For example, in the case of a population at Redención Pampa, where yellow and magenta flowering plants coexist, but mixed colours are not present. Gertel (2011) calls them conveniently Sulcorebutia gemmae.

These characteristics can be used in a recognition key. But do they also say something about relationships? For example, are all weingartias without central spines closely related? Gene flow not only makes the recognition difficult, but also the determination of relationships. Yet I dare to presume that all weingartias without central spines have recently had a common ancestor. This expectation is reinforced, if another characteristic indicates the same populations. In 2004, members of the SSK, the Studiengemeinschaft Südamerikanische Kakteen, compared data from 11 isoenzyme systems of including Weingartia/ Sulcorebutia. This data was provided by Dr. Monika Konnert. I have reviewed it again, identifying in which case an allele for enzyme system isocitrate dehydrogenase (IDH) had designation 2. In 79 of 87 populations with IDH-2, plants without central spines were recorded.

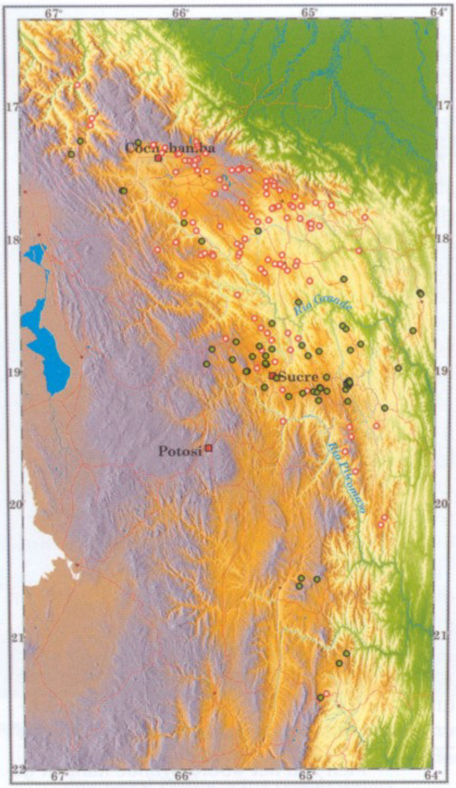

Fig. 7: Of the populations which are indicated by circles, both the status of isoenzym system IDH as that of the central spine are known. In the case of green circles, *IDH-2* applies at the same time with *no central spine*.

In Fig. 7, all plant populations are indicated, for which in SulcoMania* IDH data are present simultaneously with data of central spines. In the cases of IDH-2 combined with the charisteristic of no central spine, corresponding circles are green, otherwise they are white with a red border.

Is it so important? I think so. The presence of a central spine appears not to be a coincidence. Plants with central spines will usually have (at least in part) other ancestors than plants without central spines, because the latter also have IDH-2 isoenzyme. Note that both in the north and the south of the habitat green circles are present while the greatest density is near Sucre. This result is due partially to the choice of the plants under investigation. But I consider it to be sufficiently reliable to find no evidence for a continuous habitat for such plants of the Ayopaya area (top left) to Tarija (right). This is depicted in Fig. 8 and Fig. 9. So in the past plants or parts of them had to travel quite a distance before remote areas were inhabited. This puts the idea under pressure, that neighbouring popula-

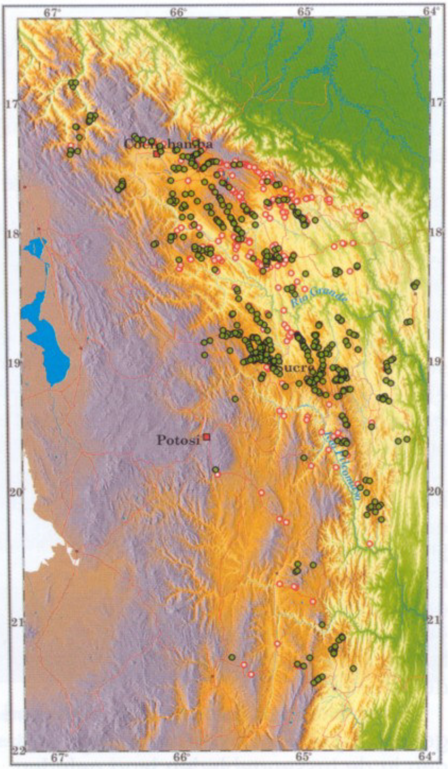

Fig. 8: All populations included in *SulcoMania* with data on *central spines*. Green circles establish populations of plants without a central spine.

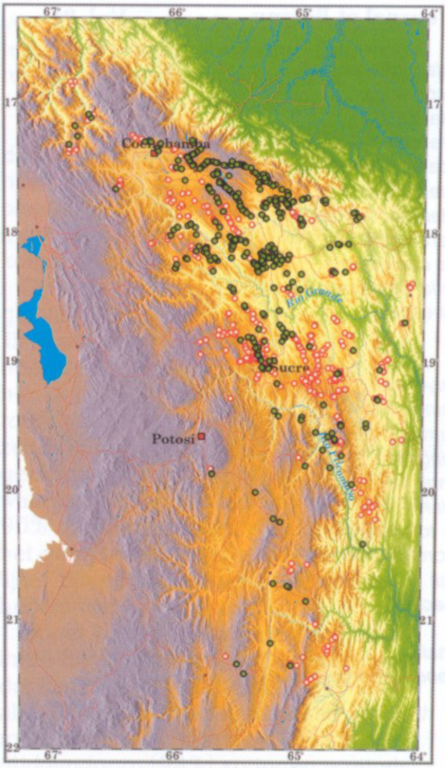

Fig. 9: All populations included in SulcoMania with data on central spines. Green circles establish populations of plants with a central spine.

tions would be naturally closely related. Also note further that some plants in the Ayopaya area have central spines. S. glomeriseta from this area always has central spines. (Fig. 10)

I have supplemented the cladogram of Dr. Ritz with details on IDH and central spines (Fig. 11). Except *S. langeri* all plants with IDH-2 are in the upper clade. One and the same mother plant in the recent past does not necessarily mean that a majority of the ancestors belonged to the same primordial population.

There are more mutually reinforcing characteristics. For example if the radial spines have false lobes, they are light in

Fig. 10: *Sulcorebutia glomeriseta* with clearly observable central spines.

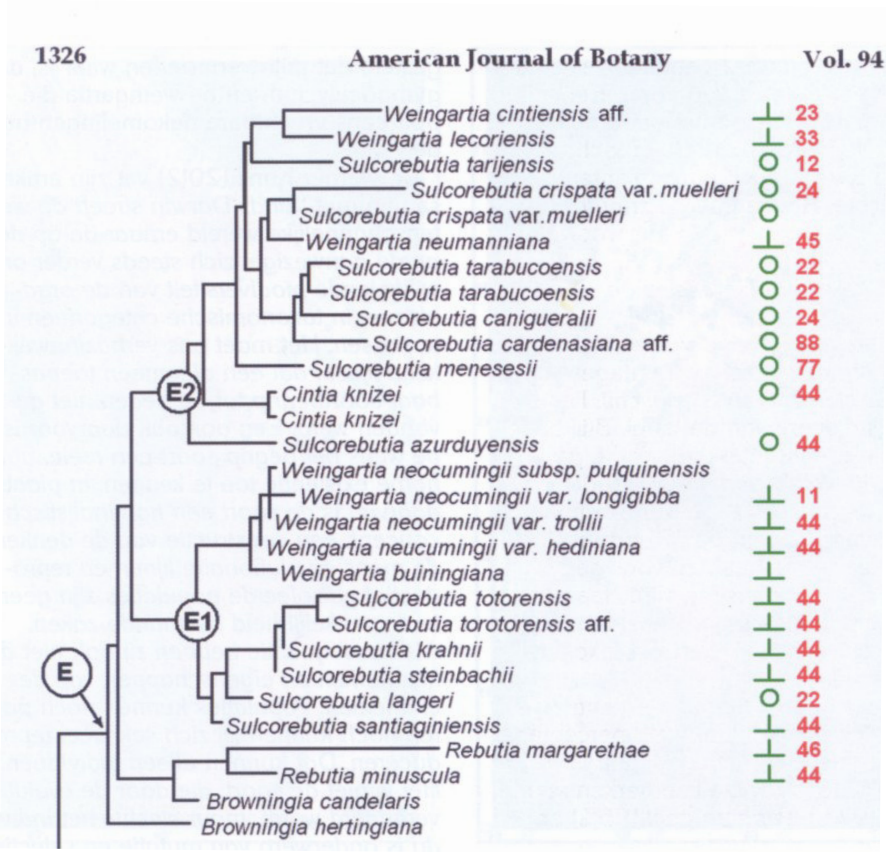

Fig. 11: Editing of a part of the cladogram of The molecular phylogeny Ritz (2007). ⊥ = central spine present, o = central spine absent. The numbers indicate the status of isoenzym system IDH.

colour with a dark point, like the radial of *Gymnocalycium pflanzii*. This is the case in 83 of 94 records.

Beware, this is not always true of the converse. In the case of plants with false lobes on the radial spines, it is also true that the length of the stigma is 20% or more of the length of the style. This, too, is not always true of the converse. It applies to 75 of the 94 populations, mostly classical Weingartia, but not all.

Figure 12 shows the presence of white radial spines. This has some similarities with the absence of the central spine, but then again that does not apply to the populations in the northwest and in the southeast.

I have not (yet) found better examples of correlation.

Conclusion

In general, only a small correlation can be found between the various characteristics. It seems that each population is affected by various migrations. Of the 170 populations of Table 1, there are only 26 constant characteristics at a level of 90% or more. The lower this number, the less applicable is the close relationship to other populations. Or rather: the assumed

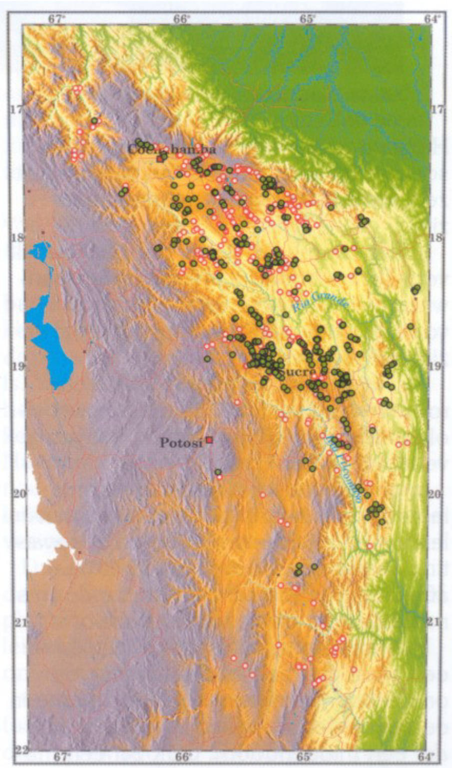

Fig. 12: All populations included in SulcoMania with data on colour of the radial spines. Green circles represent populations of plants with white radial spines.

close relationship. Evidently this does not bother some authors. They make combinations enthusiastically regardless of whether plants are similar or not. To my way of thinking that soon leads to an unworkable concept.

Recently a cactus celebrity assured me that, without doubt, no more than seven species of Sulcorebutia exist. Unfortunately he could not explain how he arrived at this number, but claimed however, that we should think of groups of related plants, which he called for convenience "complex". For example one could speak of a crispata-complex.

I was curious how representatives of this complex were identified and suggested that these plants might have a magenta flower. However, this was thought too restrictive. Yellow flowering crispata's would exist as well. I freely admit that I was somewhat surprised by my apparent lack of knowledge. His statement, however, allowed no room for doubt. *S. heliosoides* (Fig. 13) grows in the "crispata area" and is consequently part of the "crispata-complex". In popula

Fig. 13: Sulcorebutia heliosoides GC 12

Fig. 14: Sulcorebutia crispata G 52

terms: *S. heliosoides* **is** a *crispata* (Fig. 14).
Immediately I thought “No, **you call** this plant *crispata*, but **I do not**.” Perhaps Hunt might call *S. heliosoides* a ‘flowerpot species’. In that case it did not exist. Even clearer.

Biologists may see patterns that elude me. But every collector of Sulco’s immediately know what I'm talking about when I speak of *S. heliosoides.* I therefore reject reasoning, which appears to be based on intuition and as a result leads to only a vague concept or perhaps misleading conclusions.

If populations clearly differ visually, I prefer to do not give them the same name. Even if they have had recently common ancestors to a significant degree. The latter I cannot determine. And so far no one has shown convincing results in that direction.

Plants with a name are handled more carefully than plants without a name. That serves a similar purpose to the one pursued by Volker Storch and therefore defends an intuitive representation of the concept of species. All collectors of plants of the genus *Weingartia* together manage a wealth of data for research into a young genus that is at the beginning of the formation of fixed patterns of features. This data (read plants) we should cherish, because it can be the basis of interesting studies. Through publications, populations are less likely to fade into forgetfullnes. Here the rank of the taxon is of secondary interest. The consequence will probably be, that established systematicians will get headaches from the results. To me that seems to be less painful than the compulsive declaring of different taxa to be synonymous or making inconsistent combinations.

A family tree at generic level can perhaps be suggested. As soon as this goes down to smaller units, the concept gets more fuzzy.

Still, I think I have identified some characteristics, which can be observed by anyone and which could well lead to the first vague contours of a hypoyhetical family tree. This can be a guideline for classification in the genus Weingartia.

Finally

What is the situation with *Sulcorebutia verticillacantha* var. *chatajillensis*? In table 1 it is a taxon with a low level of constant characteristics, namely 75.5%. This makes it difficult to recognize. If Oeser had committed a sin in his youth, I think it would be that of describing a hardly recognizable taxon.

Probably a layman, after seeing a *chatajillensis*, will not think spontaneously of a *Sulcorebutia verticillacantha*, but also not of a *Sulcorebutia vasqueziana* or *Sulcorebutia losenickyana* sensu stricto. Unless he is guided primarily by the absence of central spines.

I myself am inclined to identify these plants with "Weingartia spec. of Chataquila". In this
way I do not deny the existence of the population, but I do not classify the plants because of insufficient recognition.

* SulcoMania is a project based on a large database with data from Weingartia including Sulcorebutia. By means of a computer program, these data can be used in different ways. The whole is supported by 10 000 images and a number of maps. For further information, please contact the author.

I would like to thank Jim Gras for proofreading the English translation.

Tabel 1 : Characteristics of a number of populations of individual plants have been collected. The data of the populations are displayed from left to right. Categories are assigned to the characteristics. Left in a cell you find the category that is most common in the population. In addition, the score for that category is stated. If all plants of a population ("fieldnumber") belong to the same category, you find the value "100" in the cell and the cell is coloured yellow. Along the top row you can find indications of the selected features and below the percentage for which this attribute has the same average number of categories per field number.

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