# THE FAMILY TREE (1)

*Johan Pot*

**It is very difficult to recognize unambiguous relationships within the genus *Weingartia* as understood in the broadest sense. Obviously not everyone interested sees them in the same way. Is the family tree not well understood? Does a clear family tree really exist? Characteristics, which justify a classification into a few, but still recognizable species, are hard to find.**

## A variety

In 1962 Friedrich Ritter described *Sulcorebutia verticillacantha*.[1] He had found in the mountains "über Sayari" a population unknown to him. Ritter realized that these plants belonged to the genus *Sulcorebutia*. In what way? It is true that Backeberg set up the genus, but Ritter doubted the observations underlying his description. I quote: "But even after deleting this illusory peculiarity on which Backeberg founded his new genus, this species[2] seems to differ sufficiently from all species of related genera, so it should be considered to be a new genus" Unfortunately these differences were not explained by Ritter. This did not seem to bother the amateur collector nor the scientist. They recognized the genus intuitively and that was good enough. It was not until 1999 that Hentzschel found a useful characteristic: the shape of the scales on the pericarp with which we can distinguish *Weingartia* (including *Sulcorebutia*) from all other genera. I do not reject recognition by intuition in advance. Nobody will write his plant labels only after consulting a list of characteristics. But what to do if the plant characteristics do not match the name at all?

Years ago Rudolf Oeser confessed to me frankly, that he had committed a sin in his youth. I braced myself, because like many others I like a spicy story. Well, that was pretty disappointing. In his hubris Oeser (1984) had described *Sulcorebutia verticillacantha* var. *chatajillensis* (Fig. 1) and understood afterwards, that he had published a duplicate description of *Sulcorebutia alba* Rausch. In other words: *Sulcorebutia verticillacantha* var. *chatajillensis* **is** *Sulcorebutia alba*.

Most colleagues use terminology like: "This plant **is** a [name]." Some even increase the weight with "This plant **is unequivocally** a [name]." But in all cases the speaker means: "I **call** this plant [name]" which is often quite a different matter.

John Pilbeam (1985) had the same opinion as Oeser. But Karl Augustin et al. (2000) proposed *Sulcorebutia losenickyana* var. *chatajillensis* as a new combination. This was soon corrected into *Sulcorebutia vasqueziana* var. *chatajillensis* by Willi Gertel (2001), after which in 2010 the name was changed into *Sulcorebutia vasqueziana* subsp. *chatajillensis*. Augustin and Hentzschel (2008) for their part

1 Often the names *Weingartia* and *Sulcorebutia* will be used in the classical way. It's a contrived attempt for the sake of those readers, who can unambiguously distinguish between these two genera, although no (combination of) characteristics to support this are known.

2 In this quote Ritter used the word "type" to mean the species of the genus *Sulcorebutia*, being *Rebutia steinbachii*.

**Fig 1: *Sulcorebutia verticillacantha* var. *chatajillensis*. Photo of one of the original plants, taken by Rudolf Oeser †.**

considered *chatajillensis* to be synonymous with *vasqueziana* var. *losenickyana*.

Obviously the sin of Oeser's youth was not really serious, because the name *chatajillensis* is still in use, however as a subspecies of *S. vasqueziana*. Was this choice made intuitively, or was it supported by an objective investigation? Of course it would be ironic, if there was a scientific reason to maintain the original generic name *verticillacantha*.

During the SSK-AFLP®-Project (2007) it was investigated among other things, to what extent the result of this method was congruent with the one of chloroplast-markers in 2005.

Congruence was found in the clade verticillacantha among others, and *S. vasqueziana* was located here as well. Already in 1962 Ritter had suggested such a relation with his *S. verticillacantha* var. *verticosior*. But both Dr. Hunt (2006) and Gertel (2010) connect *S. verticillacantha* with *S. steinbachii*, perhaps intuitively. Or were they able to deduce a recent common ancestor of these two taxa?

**Common ancestor**

With 'smartly' selected pieces of DNA it has been possible to find a common primordial mother. Using this method researchers suspect the cradle of mankind to be located in eastern Africa. Mind you, suspect. The data are subjected to a probability, that produces this outcome. A similar study, but for cacti, was carried out by Dr. Ritz (2007). It was found that all surveyed plants of *Cintia*, *Sulcorebutia* and *Weingartia* had a common primodial mother, which was not shared with plants of different genera. This result seems to be an acceptable argument to only recognize the genus *Weingartia* (the oldest name, thus having priority) instead of the three genera *Cintia*, *Sulcorebutia* and *Weingartia*.

Nevertheless, I would like to present the following light-hearted thought. I assert that I am descended from the emperor Charlemagne. Of course you will want to

see hard evidence immediately in writing, but I cannot provide this explicitly. Instead, I offer you a simple calculation. Let us assume that Charlemagne had two children, a boy and a girl. Let us assume that this also applies to these children and all generations to come. Suppose there were three generations per century. From the year 800, there have been $12 \times 3 = 36$ generations. In 2013, the number of descendants of Charlemagne therefore would be $2^{36} = 68{,}719{,}476{,}736$. That is almost 10 times as much as there are people on our planet today. I do not think it likely that I would not be one of these 69 billion people. In other words, I expect to be a descendant of Charlemagne. But mind you, I do not claim that this descent took place purely in the male line. In this example, that chance would be only 1 in 34.5 billion. After all the 36th generation would consist of 34.5 billion boys and 34.5 billion girls.

Of course you can reject this story by doubting my assumption of two children per ancestor[3]. But this does not apply to the converse. Every person has two parents, four grandparents, eight great-grandparents, and so on. At the time of Charlemagne almost 69 billion ancestors of every now living human being have lived theoretically. You will say, this is not possible. I agree with you. It is only acceptable, if many ancestors were common to all currently living persons in this story. Perhaps they lived on a kind of island, in this example, an island of nobles. Then they might have developed special characteristics, such as blue blood, which are not observed outside that island. You could almost think of inbreeding. I must be descended from an illegitimate child of one of these islanders, for my blood is just red.

This light-hearted thought is meant to explain, that actually we are not able to imagine a complete family tree. The same applies to the results of Dr. Ritz. It's true a probable primordial mother of weingartias inclusive sulcorebutias was found, but she was in her time not the only cactus. Most likely the plant was part of a population. What happened to the descendants of the other members of population of the primordial mother? Did all of them become extinct? Is that plausible?

Must we really accept, that all now living weingartias descended from this one primordial mother?

Although it is hard to comprehend, I cannot find room for an alternative interpretation here. So the answer will have to be affirmative.

Yet it is pointless to depart from a primordial weingartia as a single individual plant. I see rather a development of a (large?) number of populations, which had mutually genetic exchange and still have.

This process is far from complete. It is difficult to chart. It may even be completely impossible. I am far from certain, that knowledge of the entire genome will lead to an understandable family tree, in contradiction to what a reputable scientist recently suggested.

**Unexpected fertilization**

In the movie "La guerre du feu" of 1981, based on the novel of the same name from van J.-H. Rosny aîné a group of Neanderthals lose their fire. Because none of the members of the tribe is able to make fire himself, three young men are sent out to find new fire. This is an adventurous mission. Along the way they free a woman of the kind *Homo sapiens* who had been captured by an enemy tribe.

3 If you are interested, you may consult Wikipedia and discover that it was a lively existence at the court of Charlemagne.

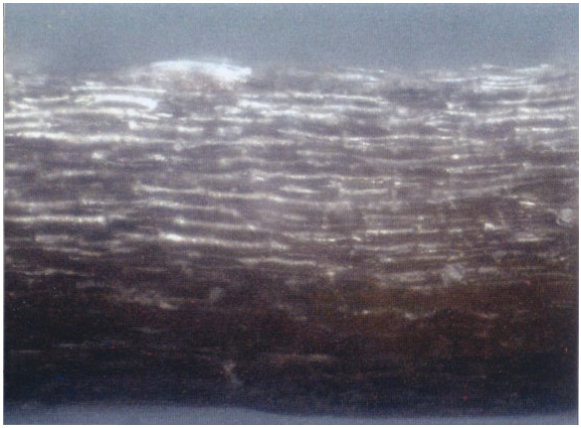

**Fig 2: Radial spine of *Sulcorebutia losenickyana* JK 206. Through a magnifying glass the spine is perceived as smooth.**

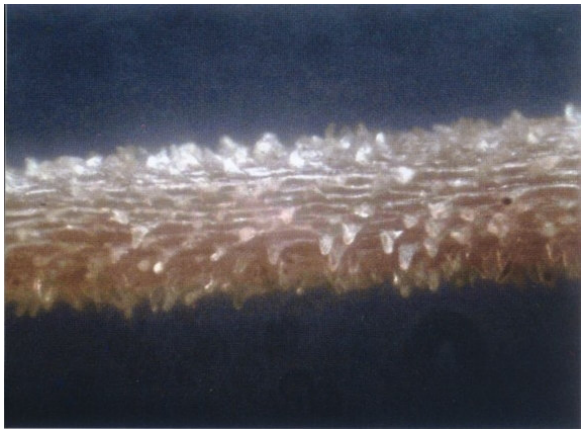

**Fig. 3: Radial spine of *Sulcorebutia albissima* JK 39. On the surface bulges grow, which are indicated by Vanmaele by "lobes".**

Something beautiful blooms between the woman and one of these three Neanderthals. While making the film advice from Desmond Morris was used. Perhaps an attempt to strengthen the scientific status of the movie?

Of course, it is still questionable whether *Homo sapiens* ever really met a *Homo neanderthalensis*, although they both occurred for example in Germany. Compare this with the following. Near the village of Torotoro in Bolivia you can find footprints of dinosaurs. On the same grounds sulcorebutia are found. Is it plausible therefore that such plants have previously served as food for herbivorous dinosaurs? It is apparent here a common habitat alone does not lead to any conclusions.

According to Wikipedia the *Single origin hypothesis* is supported by most scientists, which implies a strictly separated evolution of modern humans and Neanderthals. Corresponding characteristics would occur independently of each other.

But most people outside Africa have parts of Neanderthal DNA linked. That would make any Neanderthals (who lived a lot more than 36 generations ago) plausible distant ancestors. Or is it possible that these similarities in DNA could arise independently in both lines of development?

Which expert puts us on the right track? Do we recognize a distinct species *Homo neanderthalensis*? Or is it wise to consider Neanderthal as a form of *Homo sapiens*? I see in this issue something in common with the history of *Weingartia*.

Perhaps it is essential to choose from two assumptions. Does the status of a characteristic change spontaneously, allowing the population to adapt to the environment? Or are these properties themselves conservative, with populations being affected from the outside as a result of migration? If the newly arrived feature is beneficial for adaptation to the environment it will be preserved. I myself believe to see many clues for the last assumption.

Wim Vanmaele (1983) pointed two differences in the structure of the epidermis of the radial spine. Radials of *S. breviflora* resemble strongly those of *W. neocumingii*. Vanmaele spoke of *false lobes* on the spines. Because of such spines they could have classified *breviflora* within *Weingartia* in those days. But at the time nobody felt the need.

Other spines can be smooth or they have bulges, called *lobes* by Vanmaele (Fig. 2, 3 en 4 ). I find it amazing, but 30 years after publication the interest in this observation still seems extremely small.

Often the spines of weingartia's which resemble *neocumingii* have a light colour with a dark tip. I can imagine that all weingartias including sulcorebutias with such radial spines had a recent common ancestor. However they will not have been the first weingartias, because such a radial spine occurs in only a part of the whole habitat.

Recently I was at a meeting of cactus lovers. An image of *Gymnocalycium pflanzii* was shown. My attention was drawn to the bright radial spines with conspicuous black tips. They reminded me of the radial spines of weingartia, referred to above. However *Weingartia* and *Gymnocalycium* are different genera. So it will probably have no significance, if the radials have the same colour.

At my request Ludwig Bercht

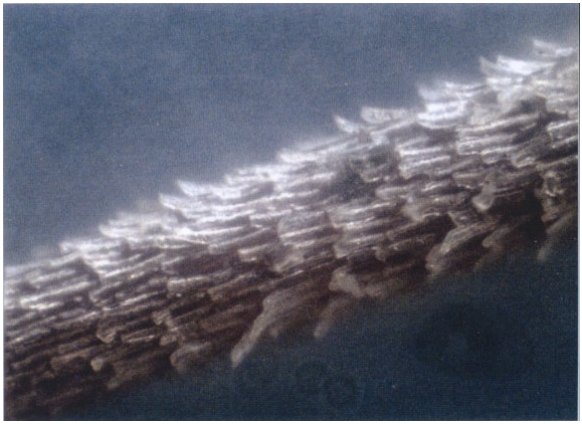

**Fig 4: Radial spines of *Weingartia frey-juckeri* HJ 441. The epidermis of the spine breaks transversely, after which the part that is closest to the tip is curling upward. Vanmaele speaks of "false lobes".**

**Fig 5: Radial spine of *Gymnocalycium pflanzii* KK 850.**

sent me some samples of radial spines of *Gymnocalycium pflanzii* KK 850, of which I made microscopic recordings (Fig. 5). To my surprise, I just found false lobes similar to those on the spines of many classic weingartia's.

The light spine with a dark tip was already found with the naked eye. Did this combination of two characteristics in both genera arise independently of each other? Or has there been an unexpected fertilization?

Do other genera exist in this area with such spines? I don't

know of them, but someone else might. In that case, I would be happy to be corrected. Otherwise, I prefer the assumption of an unexpected fertilization.

I have looked at such radial spines of three gymnocalyciums, which grow more than 1,100 km apart. This is significantly more than the 300 km that separate the extreme habitats of weingartias with such spines. Therefore, I assume that in *Gymnocalycium* the characteristic was present earlier than in *Weingartia*.

Can we now claim, that Weingartia is descended from Gymnocalycium? In a sense, yes, but if such a fertilization occurred one (or more) times 50 generations ago, the gymno would only have been one of 1,000 billion ancestors. The number of theoretical ancestors in case of (for example) 50000 generations grows beyond my imagination. Therefore, preference should be given to seeing both weingartias and gymnocalyciums as 'residents' of islands. In this case each of the two islands is called a *genus*.

**To be continued**

**Photographs by the author**

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Originally published in **Succulenta** 93:2 (2014) (pp. 87-92).

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