## **TEPHROCACTUS**

Incl. Maihueniopsis, Cumulopuntia, Puna and related genera



T. molinensis (Speg.) Backgb. Photo by R. Geissler.

## **STUDY GROUP**

Vol.7 No.1 March 2001

## Secretary's Page

Four more members have joined us since the last journal: Cesare Serra, ITALY, Mrs. A.C. Nunn, London, David Parker, Birmingham and Mr Elton Roberts, U.S.A. I wish them good fellowship and hope they will take an active part in what we are trying to achieve, namely to find out more about our plants.

All articles and comment should be send to the Co-Editors:

Subscriptions and any other correspondence should be sent to the Secretary. If you write to one of our Officers and expect an answer, please remember to include a S.A.E.

Subs. remain at £10.00 per annum for the UK and Europe (European members please note: "no Euro-cheques are accepted by our Banks – Please send £ Notes") Overseas members £14.00 or \$25.- USA (in \$ bills only). <u>Please make all cheques payable in sterling</u> to: "Tephrocactus Study Group (not individuals). May I also remind you to let me know any change of address, Tel. No. or code.

Please remember members may advertise their "WANTS" and "SURPLUS PLANTS" free in the Journal, in no more than 30 words

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## A SALUTARY LESSON ON GROWING IN A COLD FRAME

It was a lovely sunny morning in May. The birds were singing and it was due to be the hottest day of the year so far, although I didn't realise it at the time. I went around opening greenhouse windows, pushing back doors and opening up the coldframe ... er, well no, actually I'd forgotten to open up the coldframe in my haste to get out and enjoy the day.

In the early part of the afternoon after a morning of unbroken, hot sunshine, I sauntered down to the bottom of the garden to see how my plants were enjoying the weather. To my horror I noticed that the cold frame (N/S facing) was still shut up tight. No problem I thought, open it up now and all will be well. The thermometer was off the scale. I also thought that one or two plants looked a bit pale in places but assumed they would soon recover. Wrong!

Over the next few days and weeks the full extend of the damage became apparent. One by one plant segments began to go pale, then a straw colour and finally shrivel up. Approximately 50% of the 100 or so plants in the frame were affected to one degree or another. In most cases this was just a segment or two which were easily removed with no lasting damage to the plant. Others were not so fortunate. The worst affected were my plants of *M.darwinii* and *M.hickenii*, which were only salvaged by removing one or two healthy joints. In a lot of cases it was only old growth that was affected, seemingly ageing several years in a few days.

So what can be learnt from this. I would not have believed that Opuntias, of all plants, could have been affected by a few hours roasting, however, there was no air movement and the plants were very dry at the roots. Although they were watered immediately, the damage had of course been done. Obviously in a cold frame there is far less overhead space than in a greenhouse so the available air moisture was soon used up.

Although if you looked at the plants today you would only notice a few with remaining damage (and this will be pruned out in time) it is a lesson I have learnt and I now pay more attention to opening the frame than I did previously!

**Martyn Collinson** 

## THE AUGUST MEETING

A reminder that the meeting will be held at Slimbridge on 5<sup>th</sup> August commencing at 10am. The discussion topic will be *T. darwinii, T. hickenii and T. platyacantha*. Information on the plants was given on P417/8 in the last issue of the Journal. The lecture will be by Graham Charles who will be showing slides of the Tephrocacti (sensu Backeberg) which he has seen on several expeditions to South America. Please will you inform Rene if you are coming to the meeting?

## PLANTS IN MY COLLECTION

For a few years I have had a problem occurring mainly with *T. subinermis* and to a lesser extent with one clone of *T. bolivianus*. While a few plants get black sooty deposits on the areoles, on these the segments themselves go black and rot. It appears to happen mainly in the growing (and therefore watering) season, and affects new growth, although not all of it. I remove the damaged segments and the lower parts of the plant appear normal. I have tried reducing the amount of water given, but it appears to make no difference. As the plants are near the door, which is open most from of late spring to autumn, it does not seem as though lack of ventilation is the cause. One plant especially is badly affected, having lost most of last year's growth. In winter the problem does not seem to occur, even in spite of the damp weather in December – the hydrometer often showing 90%. I have had these specimens over twelve years, and for the first few years there was no trouble at all.

Two plants growing side by side on the bench are very similar in habit, being low growing and spreading outwards. One is *T. sp. from Patagonia* (ex A. Hill, collected by Van Vliet and passed on by P. Van de Weede). The other is *Opuntia fragilis* (ex Mottram, from the Peace River, Northern Alberta). Though the habitats of these two are thousands of miles apart, from a distance they appear quite alike. *Opuntia fragilis* has smaller segments – the oldest being slightly flattened and having shorter finer spines. It seems that growing close to the ground is one way of surviving the very strong winds in these areas.

In my collection is a plant labelled *T. microsphaericus*, which it obviously is not – it bears no resemblance to the illustration in Backeberg. For the last two years it has formed buds early in the year. They grow slowly and then suddenly dry up after watering, which I usually begin in late March, having sprayed earlier, depending upon the weather. Already (mid January) I can see at least four small buds and other tufts, which might be buds or segments beginning to form. This year I will not give any water to this plant until later and see whether it will flower. It is on a top shelf in full sun so I hope that it will not feel too ill-treated.

Bill Jackson has asked what compost we all use (P372). I have no special mixtures. I use 50% John Innes No. 2 and 50% alpine grit with some sharp sand added. As to fertiliser, I use a very weak solution of Chempack No. 8 about once a month when watering.

Geraldine Dyson.

## CUMULOPUNTIA IGNESCENS (VPL) RITTER

I am grateful to Roger Moreton for translating Ritter's description of this plant. Photographs of plants in habitat appeared on P 369 and P 376 in last March's issue of our Journal. The erect spines can be seen on the single segment on P376.

Ed.



## **CUMULOPUNTIA IGNESCENS (VPL) RITTER.**

Syn. Opuntia ignescens Vaupel 1913, Bot. Jahrb. Engler, Beibl. 111,S.30 Syn. Tephrocactus ignescens (Vpl.) Backbg. 1935-Syn. Tephrocactus ignescens (Vpl.) Backbg. var. steinianus Backbg. 1956 Syn. Tephrocactus flaviscoparius Ritt. Nom. Nud.

Forming hemispherical or flatish clumps, without a tuberous root. The <u>segments</u> 3 - 6 cm long 1.5 - 3 cm thick, bright green to bluish green, almost cylindrical, blunt below, tapering higher up; segments towards the end with long pronounced tubercles, flattened towards the lower end. <u>Areoles</u> round to oval, mostly 2 - 3 mm across with white felt, copious towards the underside, missing from the lower fifth to a quarter of the segment, dense towards the growing end. On spineless areoles are yellow glochids. <u>Spines</u>; occurring only on the quarter to half of the segment, mostly in thick tufts of 6 - 20, standing upright, brush-like, but also standing out diagonally from the segment, needle – like, straight, somewhat rigid, at times also thin bristle- like and then rather bent, normally 3 - 8 cm long, mostly also with several fine short ones: colour golden yellow to orange red or reddish brown, not shiny, the finest ones lighter to white.

<u>Flowers</u> (recorded from 7 blooms) lateral 35 - 50 mm long, about the same width, open without perfume. Ovary green 20 - 35 mm long, 15 - 21 mm thick at the edge of the tube, smooth and naked as far as the areoles which are on the upper end and close underneath they are whitish, 1 - 2 mm across, with small slightly broadened reddish scales and with upright bundles of bristle-like, paler, still growing tiny spines. Nectary groove pale yellow, about 2mm high tightly around the pistil. Tube above this pale inside 12 - 22 mm long funnel shaped. Stamens all about the same length, 7 - 12 mm, the lower ones very thin, white, the upper ones thick, pale yellow. Anthers pale yellow. Pistil white 2 - 4 mm thick somewhat thinner above, very thin at the base. Style 18 - 25 mm long from which 2 - 3 mm above, the 7 - 8 pale yellow stigma lobes arise and which stand deeper than the highest anther. Petals 15 - 21 mm long, 7-15 mm wide, bluntly rounded above, often somewhat crenate, narrowing below, cinnabar to orange yellow, more rarely carmine or golden yellow, the outer ones also carmine to purple.

<u>Fruit</u> cylindrical to barrel shaped somewhat tapered near to the upper end glossy an d without tubercles 3 - 4.5 cm long, 17 - 25 mm thick, pale yellow to bright green. <u>Ovary</u> 10 - 15mm wide, 15 - 18mm deep, somewhat wrinkled on the upper edge with prominent white areoles 2 - 3 mm across with upright bundles of 6 - 12fine spiny bristles, reddish brown to light yellow 2 - 6 cm long, without glochids. <u>Seed capsules</u> 10 - 12 mm across, roundish, not fleshy. <u>Seeds</u> almost spherical about 3mm across, scarcely narrowed at the base, pale yellowish brown, rather smooth; aril ridge somewhat raised, somewhat scanty or less wide; the ribs starting at the base, typical also of *C. pentlandii*, are here less raised and shorter.

Type locality: near Sumbay on the Arequipa – Juliaca Peru railway line at the height of 3850m.

Friedrich Ritter. Kakteen in Sudamerika. Vol. 3. P. 880. Translated by Roger Moreton.

## **OBSERVATIONS ON TEPHROCACTUS SEEDLINGS (SOWN 1996)**

Fig.1 shows two plants of T. alexanderi DJF397 (Villa Mervil) and on the right one plant of T. alexanderi DJF469 (Piedre Pintado, La Rioja). The first two would appear to be fairly "typical" alexanderi whereas the La Rioja plant is quite distinctive having more and whiter spines - it is also slower growing and sensitive to overwatering. It is interesting to note that in the first 2-3 years of growth, T. alexanderi is elongated/ columnar. In fact the plants do not look like an Opuntia at all and could easily be mistaken for a Cereus or Echinocereus. Only when it starts to offset does the original growth shrink and become more rounded, with the new growth conforming to the more typical pattern.

We then come onto T. alexanderi v. bruchii and here the problems start. Fig.2 shows on the left, two plants of v. bruchii from Poman, La Rioja (very dark spines according to the description!) and four of v. bruchii DJF465 (Fiambala, Cat) - highly variable according to the description. Yes, I would agree with that! Of the first two, one is almost the same as the "typical" alexanderi but admittedly the central spines are slightly darker (although this is not clear in the photo.) The other is quite different but could be a smaller variety of the plant (bottom, right). This leaves the middle pair which are splendid plants and quite "geometricus" in habit but the spination is not quite right. However, neither are they anything like the earlier forms. The one on the far right is again, quite distinctive. The word "variable" sums it up - I have tried to apply Backeberg's' key to these plants without success. Perhaps someone else could comment.

Fig.3 shows T. alexanderi v. "geometricus". Three plants from Loro Huasi, Cat (DJF319) and the little one (bottom right) DFJ320 from Fiambala. Well, are they geometricus? I suppose only time will tell. To me the large ones look more like aoracanthus but who knows. The little dark one has very small spines pressed close to the body but is very slow growing. Fig.4 shows three plants of T. aoracanthus from various localities in S Juan. (DJF337, 472 & 474). Again they all look slightly different. The plant on the left put on some good, spiny growth this year and I think will become a very attractive plant. Incidentally, if you look at the photograph of a geometricus seedling in Michael Keisling's' new book it will be seen to look nothing at all like the later "round balls" so typical of the plant.

All the seeds were sown in separate pots so it is unlikely that any "jumped" from one pot to the other, but some of the variations are quite extraordinary.

More observations from other members who have grown from seed would be most welcome by the group I am sure, however unscientific you may think they are. Of course the next update will be when they flower but I would advise you not to hold your breath!

Martyn Collinson

## YOUR COLLECTION

Members are invited to write an article on your collection for publication in the Journel. The emphasis can be on anything you wish: favourite plants, housing, cultivation, propagation, problems etc.

## **TEPHROCACTUS AND OTHER PRICKLY PEARS.**

This is my own, personal reaction to this new book. Having purchased it direct from the author, for £20 inc. P&P from Germany, I was pleasantly surprised, when first opening and scanning, to see how well it was laid out. Being a grower who enjoys the plants for what they are all this techno-speak, which seems to be the current mode, floats over me. But my impressions of the chapters are as follows.

## **Plant Diseases**

A well written and detailed account, interspersed with good clear photographs, of the various afflictions suffered by this type of plant in our damp climate, together with good information on various methods of treatment.

## Propagation

Covers grafting in excellent decail with good photographs. Seed raising is also well covered showing one of the many methods that the author uses.

## **Classification Problems**

Short, concise information in everyday English. No techno-speak to confuse the reader. At present there seems to be a reclassification on almost a yearly basis with many plants having up to four or five different names. This chapter does not suffer from this blight. Cultivation

Good, concise information without over elaboration.

**Plant Descriptions** 

This covers the bulk of the book. An excellent photograph of flower and body shows each plant and there is good information including location and habitat data.

## **Explanation of Genus Synonyms**

The title sounds more frightening than the text, which is well written and covers all the types that we TSG members grow.

## Index of Synonyms

This is very good and blows away any misconceptions derived from 20<sup>th</sup> Century man's obsession with short cuts.

**Anti-Pest Preparations** 

Good, detailed information about the various concoctions available (not always here in the UK) for dealing with pests and diseases.

Conclusions

The book has 317 pages of good quality, long lasting paper. It is a handy size for use in the greenhouse for identification purposes. It is well written and supported by excellent photographs and, one very good point, it is written by a 'hands on' grower. Being a firm believer in KISS\* this book meets all my expectations and more; money well spent.

E. Fletcher.

\* Editor's Note: "Keep It ? Simple" ?

## THANK YOU.

Thank you to the several members who have replied to the requests to let Bill Jackson know what potting compost they use. He would be pleased to hear from more members. Also thank you to those who expressed a view on the possibility of an index.



Fig. 1. T. alexanderi. Photo by M. Collinson.

Fig. 2. T. alexanderi v. bruchii. Photo by M. Collinson.





↑ Fig. 3. T. alexanderi v. geometricus. ↓ Fig. 4. T. aoroacanthus. Both photos by M. Collinson.



4.4

## SECRETS OF SPINES.

The accepted belief, that cactus spines provide protection against animals and the sun's rays, does not cover the subject fully. Detailed observation through a microscope reveals interesting biological puzzles. While evolutionary adaptation to specific environments can explain many features there are still many secrets hidden at scales not visible to the naked eye. Moreover, there are wide differences in spination between groups of cacti even down to different species of the same genus. These are the products of evolution but it is by no means obvious how or why they have arisen. The attached photographs, of spines from Pterocacti and related Opuntioids, show some of the elaborate detail existing at microscopic levels.

The internal structure of spines is identical for all nine Pterocactus species. The cross section shows tiny plates joined together in an apparently chaotic manner creating a sponge like structure. This is light, because of the empty spaces between the plates, but at the same time, resistant to mechanical forces. In this way spines can maintain their defensive functions even without firm anchorage to the areoles. These spaces also mean that there is a degree of thermal insulation between the inside and outside of the spine, some protection from the sun's rays. They also allow access of water and water vapour. Slow evaporation of this can, possibly, raise the local humidity just around the skin of the plants. Putting spines in water does confirm this absorption. They become soft and flexible, hardly able to prick the skin.

The outer layers are built of similar components but the plates are larger and layered horizontally around the core making a more weather resistant coating. Sometimes there are longer air channels but, generally, the plates are tightly packed providing a cover for the inside of the spine. This cover is more complicated and it is not always easy to distinguish the separate components but there are fibres intertwined together to make a sort of rope fabric. This must make a major contribution to the stiffness and strength of the spine. Towards the spine end these fibres bend towards the surface creating specific appendices or acanthas. Their function is easily guessed. Spines enter the skin easily but are much more difficult to take out. The appendices work like fishing hooks.

Since this property leads directly to the loss of segments it is natural to wonder how such a mechanism can help the plant adapt to its environment. My favourite hypothesis assumes that this contributes to vegetative propagation and distribution. Certainly *P. kuntzei* and *P. megliolii* do have the ability to lose segments in Autumn and Winter and these, spread by wind and shifting sands, can give rise to new plants during the next growing season. While other Pterocacti are not known to regenerate in this way they do lose segments very easily. Perhaps the evolution of this mechanism is in progress, not yet complete for all the species. But it is all speculation and, without detailed observation in the field, this explanation for the existence of barbs must remain just a theoretical hypothesis.

Glochids also carry barbs but, unlike spines, are easily detached from the plant. They stay in the skin of any attacker, and cause discomfort, for a longer period. This must be a serious deterrent to would be predators.

This in turn suggests an alternative reason for the barbs on spines. It may be that the plant accepts the sacrifice of a few segments to protect the remainder. This is the strategy used by bees. Using their sting they lose internal organs and die. This sacrifice by individuals protects the hive and queen. Many species of starfish act in the same way. In the moment of danger their main shield may spontaneously separate from some of their arms. All parts might regenerate but some will have been sacrificed to the predator. For them too, segmentation is one means of reproduction.

Based on the above examples it can be concluded that the function of hooks on the ends of spines may be limited only to the plant's protection from predators and has no role in vegetative reproduction. However, it should also be noticed that both functions mentioned above could be complimentary. We have to always remember that evolution is caused by totally accidental mutations though, due to the existence of natural selection, it may be expected that some profitable features will be preserved.

In taking these photographs I was hoping to characterise the spine surface of other Pterocactus species. Among other things I hoped find reasons to support or reject the idea that skottsbergii and hickenii are forms of the same species. The boundaries between other species are not clear-cut either. Some specialists point to the existence of forms intermediate between australis and fischeri and again between hickenii and auracanus. (K. Gilmer). Unfortunately it turns out that mere characteristics of spine surface cannot provide unambiguous answers to these questions. Seen through the electron microscope, structures in the short spines of *P. gonjianii* are the same as those in the much bigger spines of *P. hickenii*. However, *P. australis* does show a new structure, something like the bark of coniferous trees. I think that it may be a quite separate species. However, remembering the variability of these plants, it is certainly necessary do more, and more detailed, studies over a range of clones before leaping to conclusions.

I have looked briefly at the chemical composition of spines. There are no surprises. As in most biological specimens we find mainly Carbon, Hydrogen and Oxygen. Other elements are present only as traces.

Piotr Swiatoniowski.

The following photographs have been reduced slightly to fit four to a page. Piotr has kindly said we can retain the photographs on disc. If anyone wants full size copies please contact the Editor.

#### **TEPHROCACTUS MOLINENSIS**

In early December 1995 1 obtained a cutting of *T. molinensis*. Its size was about 25 x 30 mm, my #0107. After some time the cutting rooted and there it sat. It did not produce any new pads or dry up but just sat there. I tried repotting, different compost, water, no water, high shelf, low shelf but to no effect. At the end of March last year I decided on drastic measures. I sliced the top off with a sharp knife. OK. On the 11<sup>th</sup> May it was growing a new pad on the top. It could have done that four years ago!

Rob Seward.



A) General view of inner and outer Structure. P. Australis KG 2011





B) Cross section, outer tunicle and inner structure. P hickenii KG 2466



D) Cross section, specific tubules in inner structure. P. Megliolii KG 2029.

C) Surface of a spine. P australis KG 1952



E) Fibres on a spine surface.P. Hikenii KG 2152



G) Specific hooks on a spine surface. P. Fischeri KG 2001



F) Surface of a spine and delicate hair. P. Gonjanii KG 1715



H) Close-up of specific hooks.P. Hickenii f. Skottsbergii KG2151

## AUSTROCYLINDROPUNTIA SUBULATA F. AUREA.

Figs 5 & 6 show photographs of my *A. subulata f. aurea*. I hope that you have more such unusual specimens in your collections. I am sure that, even if most of us prefer plants, which look like, those in the wild, cristate, monstrous and chlorophyll-less plants are necessary supplements to any collection.

The illustrated clone produces much more wool than a typical *A. subulata*. As may be obvious in the illustration, it grows about two times slower than the normal form. The clone is quite sensitive, especially to some orange-brown smut (Staganospora assans). Evidence of this is easily noticed on the golden leaves. The stock, also *A. subulata*, is completely immune to this fungus.

Piotr Swiatoniowski..

## VARIEGATION IN OPUNTIAS.

There are several reasons why a cactus will vary from the usual colour. Some species develop a different colour in the resting period due to the lack of water. Renewal of normal cultivation in the growing period sees a return of such a plant to the normal colour. However, too much sun with possibly lack of water in the growing period will cause a plant to dry up and one sees an intensification of the red pigments as the plant tries to react and survive. If the plant is then given the correct cultivation and can be encouraged to start growing it will return to the normal colour of the species. Lack of nutrients in the soil can cause a plant to develop yellow patches. If the plant is repotted or given a fertiliser feed then the plant should revert to normal. When a virus attacks a plant its presence can sometimes be detected by a series of small patches of light green/yellow which mottle the epidermis. A plant may also change colour as it is in the process of dying and such a change is often an indication that there is no hope of saving the plant. None of these causes of colour change are desirable nor can the plants be truly classified as variegated. Nearly twenty years ago I started to collect variegated cacti. On a visit to various nurseries in Europe I had to make decisions as to whether a plant was possibly showing signs of variegation or came under one of the above categories. I confess that I was not always correct in my diagnosis and did waste some money but I also managed to add some plants to my collection. However, a friend quietly told me that several people had asked him why I was always buying plants that were dying!

I have had cacti of various genera in my collection which have come under one or other of the above categories. The virus flecking is often seen in the stocks of grafted plants but I have never seen it in Opuntias (a virus might be present but not show). I have had one plant of *M. glomerata/hypogeae* show sign of mineral deficiency but it recovered after several feeds of liquid fertiliser. This is the only Opuntia that I can recall ever showing a colour change.

True variegation obviously comes from some upset in the genetic makeup of the plant and there is a permanent change to the normal pigment of the epidermis. Lack of chlorophyll has the result that the action of photosynthesis cannot take place in that area. Thus a plant completely without chlorophyll cannot produce its required nutrients. To enable such plants to survive they therefore need to be grafted on a green stock so that the stock can provide the entire nutrient to the scion. If the variegated plant has some green epidermis then it may not be necessary to graft it for it to survive. Generic hybrids sometime show yellow patches. Abnormal coloured clones of various genera and species are frequently seen as grafts for sale. However, the *Austrocylindropuntia subulata* in Piotr's photographs is the first time I have seen a chlorophyll-less Opuntia. Does any member possess another one, either fully or partially chlorophil-less, or one showing some other form of variegation? Please let us know and if possible enclose a photograph.

Ed.

## THE SHOWING OF TEPHROCACTI (THROUGH THE EYES OF GARETH DARBON).

Depending on the Judge, the showing of Tephrocacti can be a risky business. Apart from the peril of transportation, opinions differ and therefore the judging does accordingly. A Judge normally goes with popular opinion e.g. Tephrocactus molinensis, which is not grown true to type, will possibly win when a better-grown, smaller growing plant of a different species is present.

The class that normally gives the Tephrocacti a chance to shine is the small pot size classes, where small growing, choice plants can often be over looked as the Judges don't recognise them to be as such. and so will therefore regularly be drawn to immature, fast growing species e.g. Tephrocactus fauxianus that really should only be looked at in the larger classes. The things that should be in the smaller classes are slow growing plants like Tephrocactus alexanderi forms including bruchii and geometricus. Others that should also be considered are floccosus types in a 4" pot.

I feel that there are a number of intermediate species that grow large, but take a long time to reach this size and as such may be looked at in both small pot and larger pot classes e.g. *Puna subterranea* and some *Pterocactus*, especially forms of *australis*.

I also feel that Tephrocacti should only be shown in the specific Opuntia group classes, as, in the main, the only plant that gets shown outside this is Tephrocactus geometricus. This I feel is an easy plant to grow and will grow at least a pad a year or produce flowers, but as it is rare in cultivation at the moment, it is looked upon far too favourably.

Depending on whether or not the Judge grows, or has an interest in the plants, the responses to judging this class may differ, as is the case with most groups. We can't all grow every plant known to us as collectors - we don't, in the main, have the room. The exhibitors who are particularly interested in any group will always tell the Judge that he / she has got it wrong, that's their prerogative, but perhaps we should ensure that Judges at National Shows are experts in the specific plants they are asked to judge.

G. Darbon.

## PLANTS WANTED, SALES & SWOPS.

Members are reminded that small advertisments can be inserted free in the Journal for the above items.



## **TEPHROCACTUS**

Incl. Maihueniopsis, Cumulopuntia and all related genera



Two month old grafted "peel" (just visible at base) of O. pachypus K. Sch. Magnification x 1 . 5 Photo by P. Swiatoniowski

# STUDY GROUP

## Secretary's Page

Four more members have joined us since the last journal: Valerie Miller from the USA, Michael Russe, North Yorks., John Marley, Monmouth, Rolando Barcenas, Italy and Mats Rolfson from Sweden. I wish them good fellowship and hope they will take an active part in what we are trying to achieve, namely to find out more about our plants. Their experience with our plants is always very welcome!

All articles and comment should be send to the Co-Editors:

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## THE MEETING ON SUNDAY AUGUST 12.

The meeting will again be held at the Slimbridge Village Hall, commencing at 10am. The meeting was originally scheduled to be held on the previous Sunday, August 5<sup>th</sup>. However, unaware of our meeting, the auction had been arranged of the plants of the late Alan Craig on that date. Some members intending to come to our meeting also wanted to go to the auction. We therefore changed the date of our meeting. If you do intend to come to the meeting on August 12<sup>th</sup> please will you inform Rene as soon as possible?

After a short business meeting there will be a discussion on *T. darwinii, hickenii* and *T. platyacanthus.* Descriptions of these were printed in TSG Vol. 6 No 4. December 2000 P 417-418 to aid pre-study of the August topic. Please bring any examples of these plants or what you consider to be closely related. Also please bring any other plants you think will be of interest or which you would like identified (we will try to help). You are also welcome to bring plants for sale. We ask for 10% of sales to be donated to Group funds. There is no charge for the meeting. All members of the TSG are welcome and you may bring guests who are not members. Light refreshments will be available at the meeting. We shall be going to a nearby pub that supplies good food for lunch. After lunch we are to have a talk by Graham Charles on the Tephrocacti sensu Backeberg that he has seen on his visits to South America.

## **BORN FROM A FEW AREOLES**

There are not many Opuntias that need grafting as a method of propagation. Most of these hardy plants will form new roots from a single cutting without any problems. For a very long time A. malyana was believed as impossible to cultivate without grafting but even this fallacy is now seen to be restricted to a particular clone, or clones. However, there are species, like Austrocylindropuntia pachypa, which are difficult to obtain for just this reason, unusual though it is among the Opuntioideae. This magnificent species, from the Chosica Valley in Peru, is almost always solitary and is a funny exception to the general rule of segmentation. In this case, and for some other, slow growing Opuntias, grafting a small piece of 'peel'\* with just a few areoles is a good solution. This type of grafting, which requires very little material, is impossible with most cacti but for our favourite Opuntias it is unbelievably easy. Many growers would be very satisfied if Geohintonia Mexican, Mammillaria pectinifera and similar Mexican rarities could be propagated this way. I have used this method successfully on a few, slow growing species such as Pt. Hickenii f. skottsbergii and A. pachypa and offer below notes on my observations.

A. subulata and A. cylindrica seem to be the most popular stocks for other Opuntias. They are not the best though if we are going to graft a piece of peel. Because there are no growing points these common stocks produce many offsets that cramp proper growth. The sleeping areoles of the slow growing scion lose priority to the faster growing areoles on the stock. This can reduce or even prevent growth of the scion. It is possible, of course, to cut out the stock areoles and this usually works, but it does spoil the plant's beauty at the same time. I recommend O. ficus-indica as a competent stock for the following reasons. Firstly, like other 'platy' opuntias, it very seldom shoots out new pads after grafting. Secondly it does seem to stick to the scion very successfully and can live on for many years. Poor resistance to cold appears to be the only weakness of this stock but then, A. subulata is not frost hardy either. In my experience some clones of this are very sensitive to stagnant cold and will freeze to death if the temperature drops below 4C.

It is important to remember that only a fully developed areole can form a characteristic segment. Peel from an undeveloped part of the plant won't be able to shoot out a proper joint. (A similar thing happens with undeveloped cuttings as they first take root. This quite striking phenomenon occurs with Austrocylindropuntia as well as with other, related genera. It perhaps deserves separate discussion.)

The grafted fragment of A. pachypa needs a few weeks to start growing, but after this we will be able to enjoy our new plant. Please, look at the pictures, (front cover and Figs 1 - 2) be encouraged and try to propagate your own specimens.

I have tried a much larger scion of *A. pachypa* on *Hylocereus undatus*. This fast growing stock was not suitable for this scion. The scion did grow (Fig 3) but the result was not typical resulting in symptoms similar to poverty of light. It was interesting too that the whole process was extremely slow, much slower than I expected. There has to be some definite reason for this. Perhaps important differences between growth hormones; possibly this may have something to do with the alkaloids found in *A. pachypa* but which have not been reported for *H. undatus*.

Another experiment using Pereskiopsis spathulata as a stock failed for some reason.

Special thanks to Roger Moreton who has kindly sent me *A. pachypa*. I wish also to ask all of you for more information about this species which is not very often mentioned in the literature. Can someone write an article on this theme?

Piotr Swiatoniowski.

\* Piotr has told me that he obtained the "peel" by using a razor blade to cut off a strip of the stem from the side of the plant. He then trimmed the strip by slicing from the strip some of the parenchyma\*\*, leaving about 3 - 3.5mm of the soft tissue under the epidermis. At this stage the vascular bundles (which go from the main central vascular bundle strip to each areole) of every areole on the peel, could be seen. Thus these vascular bundles could link up to those on the stock and the "sleeping growing points" hidden in the mature areoles could become active.

\*\*Parenchyma = the unspecialised and undifferentiated living tissues making up the bulk of many plant organs. Quote from "Glossary of botanical terms with special reference to Succulent Plants" compiled by Urs Eggli. Ed.

## EMAIL ADDRESSES.

When we tried to contact members to inform them of the change of the date of the August meeting it became clear that we did not have all the email addresses of members and some addresses we had were incorrect. If you have an email address and did not receive an email informing you of the change of date please will you inform Rene of your email address? Also if you do change your email address please will you inform Rene?

## YOUR COLLECTION

Members are invited to write an article on your collection for publication in the Journal. The emphasis can be on anything you wish: favourite plants, housing, cultivation, propagation, problems etc.

## **TEPHROCACTUS SEED SOWING 1999/2000**

In the habitat of small Opuntia the regeneration though distribution by seed is quite common despite the fact that many segments of Opuntia break off very easily, particularly during periods of drought. (A. verschaffelti, M. nigrispina. T molinensis.

In habitat many fruits are distributed by agriculture and grazing cattle, who eat them and distribute seed in the surrounding area. Ants may also contribute by distributing seed by carrying it some distance. Also seeds and segments of many species are washed and carried away through heavy rains so that they will reach and populate new areas. Reading all the negative reports from cacti friends and articles in other literature about the difficulties of raising Opuntia from seed, I had tried a large number from seed and found out some interesting things in the process. Obtaining seeds is not really too difficult. Mesa Garden, Piltz and Kohres offer quite a large selection. Some even with habitat details of the original material. Most seed offered through commercial channels does not give accurate dates when the fruits were harvested. A number of the small Opuntia form fruit containing a thick fleshy slime which may well contain a substance which blocks germination for a year or more. A similar phenomenon can occur in the fruit of wild roses where the seed usually germinates quite freely in the second year.

It is not possible to sow all the Opuntia species, as most are not available through commercial channels. This means giving a somewhat incomplete picture of what can be germinated. Here is just a list of seeds that have germinated freely for me:

Maihuenia

- Tephrocactus articulatus, alexanderi, alexanderi. f. geometricus, aoracanthus. Maihueniopsis glomerata, mandragora. ingens? (El Loa, RH 1797), camachoi

- Pterocactus, fresh seed Airampoae spec. Mendoza Micropuntia pulchella Cylindropuntia versicolor etc.

With Austrocylindropuntia one has to wait at least a full year to see a complete germination result. With Cumulopuntia it takes an age and one should reserve a place where seed can dry out and be watered again over several years (even occasionally outdoor in warm rain!) Care must be taken to protect seedlings from fungal attacks by using light fungicides. Early transplanting as soon as possible is essential. Spring (April/May) and August/September appear to be when most seeds germinate best.

Here are a few notes I made during the course of the year:

In autumn '99, or to be exact, 31.10.'99 (Kohres) and 6.11.99 (Mesa Garden/Lutz) I started sowing under artificial light. This time, after all, is the beginning of spring in the Southern Hemisphere. Seed which had not germinated by the 16.1.2000 was substituted with other seed (Mesa: *T aoracanthus*, Piltz: *M. camachoi* and *glomerata*, Nyffeler – Eggli *Pterocactus*. According to Dr Eggli the *Pterocactus* seed was collected January 1996, which explains that only two Pt. *kuntzei* (N&E 380), an unusually spined form of *kuntzei* had germinated. Two lots of ten seed were sown. Other seed of *P kuntzei* germinated 100% from DJF!

On the 5.3 (Mesa 2nd lot) and 8.3 (Succeed) I made another sowing and brought out the seed which was left to dry out last January. All were re-moistened and put in the cold greenhouse.

In general most of the seeds germinated best within 14 to 21 days. A lot made the first seed leaves and those with the largest leaves grew the strongest and had the strongest spination. Some however remained at the first—leaf—stage and died shortly after, although a few grew on. At that stage one should play midwife and help the seedlings by

removing the remaining seed coat from the leaves with a pair of tweezers! Some made three cotyledons (*T. articlatus, Maihuenia patagonica, Airampoae and T. alexanderi*) and some even became double headed.

At the first sowing the seed-compost was purely mineral components, but for the next sowing I added some coconut fibre. The temperature in the propagator was between 17- $33^{\circ}$ C and the pots were standing in plastic trays with transparent lids (Fig 4). The five daylight-tubes were 20cm above. The larger grit (2 — 5mm) in the compost helped to prevent the surface from forming a crust. However, indoor artificial light does not produce such strong and compact seedlings as natural light.

During June the compost was allowed to dry out. In August it was again watered whereby more seeds germinated as a result! For example *M. cylindrarticulata* Kohres, that had not germinated for ten months and also two *Maihuenia* from W. Smith (Volcan/Chile). Also the obscure *A. steiniana*? with four seedlings, which also took four years to germinate. The last seedling showed at the end of October (*M. nigrispina*).

Because of the longer nights, the compost stays moist longer and germination continues uninterrupted. In general it would be interesting if one could obtain dated seed which could be grown under optimum conditions and accurately documented.

Cyril Hunkeler.

#### **IGNORANCE IS BLISS**

I read with interest Mr. Fletcher's article on Austrocylindropuntia malyana/Tephrocactus malyanus in the December Journal Vol. 6 No. 4 P419 - 421.

I have had a grafted specimen for a number of years, which looks very similar to the one in Fig. 9 in the article. Since I do not much like 'lollipops' I had been meaning to degraft and re-root one head, but have never got round to it. At the BCSS National Show last year Terry Edney was selling grafted specimens, at a reasonable cost, and I bought one specifically to de-graft. I took care to select the most robust looking plant.

Although somewhat late in the season (mid August) I decided to chop at once, rather than wait until the spring, I followed my usual re-rooting technique, setting the cutting on silver sand. The sand is watered, but then allowed to dry out completely before being watered again. The cutting rooted inside three weeks. I was obviously pleased at this. I was therefore very surprised to read that this species is very difficult to root. I can only put my success down to beginner's luck and the fact that ignorance is bliss!

Mr. Fletcher speculates that the difficulty in re-rooting this species may be due it being grown for too long a period on a graft. My plant was presumably a recent propagation so perhaps this is the answer.

C. Parker.

## **AUCTION OF PLANTS.**

The plants in the very large collection of one of our late members, Alan Craig, will be auctioned on August 4<sup>th</sup> and 5<sup>th</sup>. Alan lived at Yarm in Cleveland and specialised in South American cacti. Details of the auction and plants for sale can be obtained from Mr B. Burke, 23, Jessica Way, Waterside, Leigh, WN7 4QG. Please send a stamped (two first class stamps) self-addressed A4 envelope which will be returned to you in July with the required information.

## TIME FOR CHANGE

I have now held the National Collection for Tephrocactus sensu Backeberg, for about ten years and amassed a great many species, varieties and forms of that group and it is ever growing in number. Unfortunately, as things are going, I am running out of space fast. I find it also quite difficult to do justice plants and the collection as a whole. It is quite a task to look after so many plants, tying to pot on and propagate at least one or two from each plant as an insurance against possible loss or damage over time. The main thing is that I must make space for new plants as they come along. One must not stand still in the quest for new plants and information as well as looking for plants with habitat data to compare.

Now that the CCC (Cactus Consensus Committee) with the help of DNA testing, has virtually agreed on the grouping of the genus Opuntioideae (K. Schumann), it may be the right time to divide the collection into smaller, more manageable collections. The grouping now established are largely as they had been before unification of the Opuntioideae under one Genus by Gordon Rowley, but also with one or two new ones added. The grouping is now as follows:

Austrocylindropuntia Consolea Cylindropuntia Maihueniopsis Opuntia (most of the Platoid) Pterocactus Tacingia Tunilla (Airampoa) Brasiliopuntia Cumulopuntia Grusonia Miquelliopuntia Pereskia Quiabentia Tephrocactus

It therefore makes sense to use this as a basis for dividing the Nat. Collection into four separate ones: Austrocylindropuntia, Maihueniopsis, Cumulopuntia and Tephrocactus, with a possible addition of Tunilla (the Airampoa). I also hold the Nat. Collection of Pterocactus and I am quite happy to keep that with one other.

My point of raising this now is to ask if there are one or two members of the TSG that would be prepared to take on at least one of the above mentioned group. This will benefit our group a lot because we would then be in a position to give much closer attention to each collection, by extending and increasing the collections and also give more time to study and keep them up-to-date.

I in turn will give every assistance to any one who takes a collection on and naturally pass on the plants for each group. It is however imperative that any prospective member would have the facility to take one group, keep it properly labelled and recorded. The keeper of such a Nat. Collection would apply to the NCCPG (National Council for Conservation of Plants and Gardens, Wisley, for Collection Holder Status. Naturally I will support any such suitable application. In fact, I will advise the Council before the hand-over.

Perhaps I should stress that keeping a Nat. Collection is not an onerous task and the council gives a lot of help to all collection holders on a continuous basis. There is just one stipulation: that the collection be kept in good order and should the need arise the collection should be handed over in tact to a successor

René Geissler.



Fig 1 Part of peel with three main areoles about one week after grafting Magnification x 3



Fig 2 Two of three areoles have just started to come to life.



Fig 3 abnormal appearance of new shoot grafted on H. undatus.

> Fig 1 – 3 Photos by P. Swiatoniowski



Fig 4 Plastic trays under artificial light. Fig 5 Ptero. Kuntzei DJF 186. Fig 6 M. mandragora Gilmer seed Photos by C. Hunkeler



## High In Peru

As long as I have been interested in growing cacti, and it is a lot of years now, my goal has been to go to South America and study them in their natural environment. Why South America? As a kid, countries like Ecuador, Peru and Chile had always been on my mind and for me it was there you could find the real adventure.

Some years ago I travelled in Ecuador studying the flora and fauna on the volcanoes and in May 2000 I had the opportunity to visit Peru. My goal was to search for the high mountain cacti. I have had a long dream to find cacti such as Austrocylindropuntia floccosa and A. malyana in their growing places.

I arrived in Lima in an election chaos. I wanted to go to the mountains as soon as possible. The first challenge for a lonesome, none Spanish speaking boy from Sweden was to find out from where all buses in Lima leave. I spent an unbelievable day in central Lima and managed to find the busline to Cajatambo, the last village before the great alpine area called Huayhuash. I was really tired but satisfied.

Next day we left Lima early in the morning and went north along the Pan-American road. When leaving this big road at Patavilca the bus started to climb the long, narrow and steep road to Cajatambo and then had to stop. While the bus driver and his companion tried to get loose from a rockcrash I could see a lot of cacti. The whole valley was full of *Melocactus, Haageocereus, Espostoas* and other plants. After a couple of landslides and 5-cm marginal from death over the edge of the road into the deep drop we arrived, late in the evening, in the nice village of Cajatambo.

I stayed in the village for a couple of days to acclimatises. I had some problems with my stomach but soon I was on my way climbing the steep hill outside Cajatambo. The reward after 3 hours struggle was a magnificent view over the snow-capped mountains of Cordillera Huayhuash and the beautiful river Rio Pumarmn.

In the thin air I had to rest a lot and almost everywhere I found what I had been longing for: big cushions of *Austrocylindropuntia floccosa*. In the lower part of the valley I found species with almost no hair but as I went higher I the species became more hairy. At about 5000m I found some very beautiful species with both yellow and white hair. I was very happy and collected a lot of seed during my week in Cordillera Huayhuash. This area was a memory for life with it's friendly and helpful people, beautiful landscape, and good weather- sunny days around +20°C daytime and chilly nights down to -15°C. And of course a lot of cacti.

After a couple of days rest in Lima, washing clothes and some good showers I went to Cuzco. I had some nice days stay in the city but soon all people and cars made me feel a strong longing for the really high areas again. I took a bus from Cuzco down to Lake Titicaca and the city of Puno. A chilly town beautiful situated near the water. I made a short travel to Sillustani, the ancient Inca place, and collected some *Tephrocactus* and *Lobivia*.

One of my dreams at home in Sweden was to go to Macusani. This incredibly chilly and remote town, high in the mountains north and slightly west of Lake Titicaca, had long been in my mind mostly because of *Austrocylindropuntia malayana*. Since W. Rausch visited the area in 1971 there have been few collectors there and most of the material circulated in collections today is probably from W. Rausch travels. To find the right bus to Macusani had some comic points. I spent five hours with ten friendly policemen in Juliaca comparing Swedish life and especially Swedish policemen's life against the Peruvian comrades. Meanwhile one of them managed to locate the little bus leaving for Macusani. We left Juliaca at 18.00 when darkness arrived and the ride took about six hours on roads where I felt the kidney change place with the liver several times. Arriving in the town in the middle of the night I walked to a little hostel, awakened the lady and got a room for the night. Good to have the sleepingbag with me!

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In Macusani I spent about a week and made long walks around the town. On two days I got a lift with a car and the driver was very helpful. He stopped at several places and let me walk around and have a look at the flora. It was very beautiful surroundings with snow-capped peaks and night-frozen lakes. Very friendly people and a lot of cacti made this stay a memory for life.

The first day I left the town early in the cold but sunny morning. I had only walked for fifteen minutes when I located the big cushion of *Austrocylindropuntia malyana*. What a happiness! All around I saw big clumps of *A. floccosa*. What a magnificent place! Going down the road towards Ollachea I found a lot of *A. malyana* and also *A. floccosa*. However, when I arrived at the little village Tantamaco the *A. malayna* had disappeared and now I found *A floccosa* growing together with different globular cacti not yet identified. Another day I travelled across the mountains eastwards down to a little village called Escalera. Here the air was warmer and it was much easier to breathe. On the way to Escalera I passed a lot of *A. malyana* and *A. floccosa* and also the Chungora lake which, at near 5000m, was full of ice. Magnificent views. The road down to Escalera was very bad and I saw no cacti there. My memories of Macusani are very strong and positive. I hope that I can return to this place in the future. I am sure that there is much more to discover!

Back in Juliaca again and in the evening I travelled to Lima. I had only two days left in Peru. I called on Carlos Ostolaza, one of the leading profiles studying cacti in Peru. I was welcomed into his house and Carlos and his wife showed a great hospitality. They gave me a chance to see their magnificent cacti garden. It was a very pleasant evening.

One day left and I took a ride to central Lima again. For an hour I searched for a bus to Chancay. Chancay is a dusty place along the Pan-American road, and with a lot of funny meetings and happenings I reached km. 118 and jumped off the bus. Here in the desert I began to search for the nearly extinct *Haageocereus tenuis*. *H. tenuis* was first found by W. Rauh 1954 and was later "found" and described by F. Ritter. In 1988 C. Ostolaza and W. Rauh completed the description when C. Ostolaza managed to study the flower for the first time. Today the threat to the plants is the expansion of the chicken farms in the desert. I found this strange, creeping species and I really hope it can be saved so people in the future can look at it. Sorry to say but many Peruvians dislike cacti and see them as a weed. I met many example of this during my stay in Peru.



Map 1 Travels of Hakan Sonnermo. Map 2. Macusani to Ollachea.

Instead I think it is important that the Peruvian government send signals and tries to build an organisation to support ecoturism in the country. I believe that ecoturism has great possibilities of development in such a country as Peru with it's magnificent flora and fauna.

Back in Lima, late in the evening, I had to start packing for the long journey back to Sweden. For me these weeks had been a memory for life and a dream from childhood had come to reality. I have already started dreaming again.

Hakan Sonnermo

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Kville 4211,450 71 Fjallbacka, Sweden. Extracts from Haken's Plant and seed list. E-mail: hakan.sonnermo~vgregion.se

Extracts from Haken's Plant and seed list.

Austrocylindrica malyanus in Peru / Dpt. Puno / Prov. Carabaya.

Jatacancha, on the way to Ollachea. 13/6-2000

4165m. HNSO 025

Quenkosaya, on the way between Tantomaco and Macusani. 13/6-2000 4131m. HNSO 031. HNSO 032. HNSO 034 Only seeds collected.

Lago Parinacocha, on the way between Macusani and Lago Chungora. 14/6-2000 4465m HNSO 039.

2km. from Macusani, on the other side of the Rio Macusani, on the hills against Hacienda Llulluchani. 15/6-2000

4165m. HNSO041,

4178m. HNSO 042 20cm wide. HNSO 043 20cm wide. HNSO 044.5cm. wide.

4224m. HNSO 045. Many orange flowers.

4227m. HNSO 046.

4276m. HNSO 048. Many seed capsules collected.

Readers might be interested to know that in 1999 the Conservation Committee of the British Cactus and Succulent Society financed an expedition, led by Dr C. Ostolaza, to rescue some of the threatened plants of *H. tenuis* and also the threatened *H. repens.* It was pitifull to see photographs of the plants covered in chicken feathers caught on their spines. The plants were placed in a botanical garden in Peru to safeguard the two species and propagate them. Ed.

## "AUSTROCYLINDROPUNTIA MALYANA"

I obtained my first specimen of "Tephrocactus malyanus" as a grafted plant in the 1980s. It died. So did a second one. Neither lasted longer than eighteen months. Looking back I realise that it was my faulty cultivation that caused the loss. At the time I told myself that it was because it was a difficult plant. This self-deception was aided by the fact that the grower from whom I obtained my plants told me that the species was difficult to graft because of the dense wool. Another problem, he said, was that the clone he had at that time (the only one in the UK) had black patches inside the stem. He told me that one had to trim back the intended scion until one came to sound bit of stem and then graft it. The existence of black patches inside the stem apparently had no effect on the health of the plant. If one grafted a scion showing a black patch at the cut then apparently the attempted graft would not be a success. I obtained two pieces of stem to graft. I found the black patches, cut back to sound material, grafted the scions and they took. A few months later they died. More recently I have obtained two more grafts. I am told that they are not the same clone as my original plants. One of my plants is grafted on a very short stock. The stock is so short that it is not obvious. The second graft is on a Pterocactus tuberosus tuber. The gravel in the pot hides the top of the tuber and the plant looks to be on its own roots. I have had the plants for about three/four years. I keep them frost-free and water them all the year round, giving less water in winter. My aim is to retain some of the green leaves showing through the wool at the apex of the plants. These leaves are an indication that the plants are growing and I have not had to face the task of coaxing a really dormant plant back to full life.

Attempts to propagation the species appears to have met various problems and the species has not become common. Grafting of the species has been the cultivation method with no reports, until recently, of success with rooting cuttings. One possible explanations for this is that there appears to be a general problem with rooting cuttings from a clone which has, for a long time, been propagated only as a graft. There are comments that such a clone "loses its vigour". (Any comments on this phenomenon?). Seeds from habitat, collected for example by Hakan Sonnermo and Brian Bates have, however, recently reached Europe and so there should be an increase in the number of clones available. Hakan has some plants on their own roots.

Discussions on the naming of a plant can be faced with different types of problems. For example we have a description for Tephrocactus pentlandii but do not know the exact location of the original discovery and cannot be certain what the plant really looked like. Because it is a recent, documented discovery we all know what is meant by "T. malvanus". However, the exact name of the plants is open for discussion. What is the genus of the plants? Do you want to call the plants "Tephrocactus" sensu Backeberg, "Austrocylindropuntia" as Ritter wanted it named or do you favour the more recent trend to drop the subdivisions and simply use the all embracing "Opuntia"? The exact name of the species is also open for discussion. When Rausch found the plants near Macusani, Peru, he named them, in 1971, Tephrocactus malvanus in honour of the Austrian cactus-grower, G.L. Maly. In 1961 J. Marnier-Lapostolle had erected a variety of flocossus, v. cardenasii (after Cardenas), based upon plants found at Achacachi, at 3000m, in Bolivia. (Acacachi is 300km South East of Macusani). According to R. Crook and R. Mottram the publication of this name was invalid (Art. 37.1). However, in 1973 Gordon Rowley wrote an article, "A substitute name for T. malyanus Rausch" and relegated "malyanus" to the name of flocossus v. cardenasii. In the CITES Cactaceae Checklist Second edition 1999 David Hunt lists Opuntia malyana as a synonym of the older established name of Opuntia lagopus Schumann and there is no mention of flocossus v. cardenasii. This is probably because the synonymy was dealt with in 1992 when D. Hunt wrote on Opuntia lagopus (1992:135).

To sum up, therefore, we have a relatively new, desirable form of cactus with which there has been/is problems of propagation and which goes under the names of

Tephrocactus / Austrocylindropuntia / Opuntia

malyanus or malyana / flocossus or flocossa v. cardenasii / lagopus.

Which name do you use? Any comments?

**Bibliography:** 

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C. Backeberg. Cactus Lexicon, with additional text by W. Haage.1977 P482

F. Ritter. Kakteen in Sudamerika. 1981 Vol. 4 P1244

D.Hunt . CITES Cactaceae Checklist Second Edition 1999. P238.

I have not read "A substitute name for *T. malyanus* Rausch" G. Rowley in Repertorium plantarum succulentarum 22:11 1973. Also R. Crook and Mottram, in Bradleya 15/1997 P106, list "Hunt (1992; 135) *Opuntia lagopus* Schumann". Can anyone please provide me with photocopies of these two articles?

It would also help to compare the descriptions of

*Opuntia lagopus* Schumann in Gesamtbeschreibung der Kakteen. Nachtrage 1898 bis 1902 P151 – 152 1903.

T. flocossus v. cardenasii J. Marn.-Lap. in Cactus (Paris) 16 (72) 137

Tephrocactus malyanus Rausch in Kakteen und andere Sukkulentem 1971 22 (3):P43 - 44.

Will anyone please do so or at least provide us with the information?

A. Hill.

Despite the occasional reminder we have never had discussion on where the TSG stands on the questions of Tephrocactus sensu Backeberg, the classification of Ritter and the classification of R. Kiesling nor the lumping of all the plants under Opuntia. This, in my opinion, is a great omission, especially as Dr Kiesling was kind enough to allow us to publish his reclassification of some *Austrocylindropuntia* into *Maihueniopsis* (TSG Vol. 4 No. 1 1998 P237-242.). Will someone please take the plunge and start us off by some comments? Ed.

### FURTHER COMMENTS ON SHOWING OPUNTIAS.

I imagine that we all have to agree with Gareth Darbon's <u>general</u> observations on showing Opuntias. In principle, all judges should know their subject. In practice I suspect that it's a bit of a counsel of perfection. There isn't that many about, Opuntia judges that is. We do have the Judges Course that attempts to set a uniform standard. We should realise how lucky this makes us. A sensible member has told me, that a bonsai society does not have this. Judging is simply down to the individual judge's likes and dislikes. If he doesn't like your style, or you, then give up. So, warts and all, we are probably better served than they are.

I know that Rene has been invited to talk to our Judges Course and that he did say that some of our plants were much easier than average. He gave guidelines, which, from memory, were not so different from God's. Moreover, some judges do know. I have been steward for one who rejected a reasonable 0. rossiana v. fauxiana, on the grounds that he had found it easy to get a big potfull. Clearly he agreed with GD. So do I. But I think that we are a minority, and show managers do have to get a judge. Not everyone within reach will know everything.

I would also like to make a different point. We have the Handbook of Shows, which helps to avoid facing the judge with a choice between a Lithops and a Pachycereus. It is continually reviewed which keeps it up to date. But I do have a tiny niggle, which is related to GD's comments. I think it's daft comparing the big, flat padded Opuntias with the compact varieties. It's like comparing Alpines with Shrubs in a flower show. Think about the word 'Show' for a moment. Shows are the main, if not the only, attempts to put the hobby before the public. The public expects to see Cacti. Their idea of a cactus is an Opuntia. I talk to them and I can tell you that they are disappointed. They are astonished at the enormous variety, which is a good thing. But they wonder why there are none of the things they expected. Big, spiky, Opuntias are showy and we need them in our shows. But who's going to bother when they know that their immaculate 0. pycnantha is going to lose to a mass of 0. glomerata sprawling across the next pot. I don't want to devalue glomerata but some clones are easy and shouldn't be rated too highly. So, as well as better educated judges, I also would like a bit more recognition for the 'Showy' bit in our 'Handbook of Shows'. W.L.Jackson.

### A JUDGE'S REPLY.

In the March 2001 issue of the TSG Gareth Darbon takes a sideways swipe at the judging of plants exhibited in the Opuntia Group.

Last year, together with David Neville, I judged both Opuntia classes in the National Show. David is a long-standing and well-respected Judge. The Shows Committee selected me because of my long association with Opuntias, which dates back to 1963. In the early 1970s I had the most representative collection of Opuntias and related genera in Europe (over 350 different species and varieties). I was also a member of the original TSG. At the National Show last year the *Tephrocactus molinensis* exhibited by Dr Gill Evison was the largest I had seen in my lifetime. Each head was perfect and the plant comfortably sat in a 25cm pot. Although this plant was more recumbent than the normal *Tephrocactus molinensis* one normally sees there was nothing unnatural or artificial about the plant. At the end of the day there was no effective competition.

In the class for two small *Opuntias O. rossianus v. fauxiana* and *O. subterranea* were the most appropriate entry. There was limited competition here and a couple of entries containing padded Opuntias never had a look in. The problem with exhibiting Pterocacti, especially *P. australis* is the problem of stem dieback, which, although mainly natural, does somewhat undermine the genus' competitive edge.

John Betteley.

## **OVER WINTERING TEPHROCACTUS ARTICULATUS.**

I have two greenhouses, one heated and the other not. About eight years ago I transferred all my Tephrocacti to the unheated greenhouse with no detrimental effect. The greenhouse is free standing, has no heating or insulation and the vents are left open at all times to allow for free movement of air. I grow my plants in a mineral soil (John Innes) plus 50% grit. Usually the plants of *Tephrocactus articulatus* only drop the odd segment during winter.

This winter has seen harder frosts than in recent years and a long, cold spring. My Tephrocacti have mostly thrived. However, for *Tephrocactus articulatus* it has been the worst winter ever in my greenhouse. Most plants of *Tephrocactus articulatus* have been reduced to heaps of individual segments. The plants began, in January, to gradually dropped their segments although the bottom segment of each plant has stayed healthy and did not rot. The only exceptions were three plants, which due to a loose pane of glass in the greenhouse roof had been left standing in about three centimetres of water for a few days in late November. In spite of this these plants remained in good condition and lost no segments. A plant grown from a packet of mixed Tephrocacti seeds in 1994 and, by appearance related to the articulatus group also fell apart and all the segments of this plant have succumbed to a black rot.

In the autumn I removed all the top growth from my *Pterocactus tuberosus* plants as advised by Rene Geissler in the September newsletter. The plants have sent up many new branches and now (end of May) flower buds are forming. I anticipate significantly improved flowering. B. Scott.

I live only a few miles away from Brian although at a higher altitude. My outside thermometer has registered -6°C several times this winter. I keep my plants above freezing point so I am not in a position to directly compare my plants under the same conditions as Brian grows. What is the experience of other members in growing *T. articulatus* without heat in winter? I recall, several years ago, being told by a grower that he had lost all his *T. articulatus* because he had moved them out of his heated section of the greenhouse. As a result they had succumbed to the cold. I understand that in habitat *T. articulatus* does not face the type of cold conditions experienced by the Maihueniopsis group of plants. Does the species have any frost in habitat? What are the experiences of our members in over-wintering *T. articulatus*? Do you keep your *T. articulatus* frost free or not? Have you lost plants due to cold? Do the plants require water in order to stop segment drop? If *T. articulatus* does suffer due to cold one would expect a plant that was wet would succumb to cold quicker than a plant, which was dry. If the latter is the case can anyone give a possible explanation of Brian's experience? Ed.



Fig 7. Austrocylindropuntia malyana Photo by Klaus Gilmer

## **TEPHROCACTUS**

Incl. Maihueniopsis, Cumulopuntia and all related genera



A. malyana outside Macusani. Photo by H. Sonnermo

STUDY GROUP Vol.7 No.3 September 2001

## Secretary's Page

Five more members have joined us since the last journal: Dr. Tony Roberts, Alex Hidalgo, Jos Huizer Philip Greswell and Hakan Sönnermo. I wish them every success with the plants and hope they will tell us of their experiences!

•All articles and comment should be send to the Co-Editors:

•Subscriptions and any other correspondence should be sent to the Secretary.

If you write to one of our Officers and expect an answer, please to include a S.A.E.

•Subs. remain at £10.00 per annum for the UK and Europe (European members please note: "no Euro-cheques are accepted by our Banks – Please send £ Notes") Overseas members £14.00 or \$25.- USA (in \$ bills only). <u>Please make all cheques payable in sterling</u> to: "Tephrocactus Study Group (not individuals).

•May I also remind you to let me know any change of address, e-Mail Address, Tel. No. or code.

•Please remember, members may advertise their "WANTS" and "SURPLUS PLANTS" free in the Journal, in no more than 30 words

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## THE AUGUST MEETING.

The meeting began with a short AGM at which all the Officers were re-elected. It was agreed that a Plant Exchange Scheme should be set up and members will see Rene's outline in this Journal of how it will work. This scheme is in addition to the free advertisement available in the Journal to members. It was agreed that we should arrange for small advertisements, about our group, in various cactus and succulent Journals either paying for them or having a reciprocity agreement. It was also agreed that a display and advertising stand at some shows would be an advantage.

On behalf of the members who attended I thank Rene for arranging the meeting and lunch at one of the local pubs. I also thank Rene's wife for providing the welcome refreshments in the hall. I also wish to thank all those who attended the meeting. It was agreed that we should have a meeting at Slimbridge on Sunday August 4<sup>th</sup> 2002.

A. Hill. Chairman

## WHY NO HYBRIDS?

At the August meeting in addition to the two advertised speakers we were shown fourteen slides taken by Hakan Sonnermo on his trip to Peru (See his account in the last Journal p445/6). The slides were very interesting as they showed the plants in habitat. The sight of *A. malyana* and *A. floccosa* growing very near each other prompted the question as to why the two species do not cross breed if they are closely related to each other as is generally assumed. There are no reports of intermediate clones being found in habitat and Piotr Swiatoniowski also makes this point in his article which appears in this issue. Please will members comment on this?

A.Hill

## **COMMENTS ON PRVIOUS JOURNALS**

#### TSG Vol. 6 No 4 December 2000 P.412

It has not been mentioned to Richard Marriott that the seed from Caspana was fresh from Roger Moreton's trip. Also that the soft-skinned lenticular seeds of the Maihueniopsis group are the easiest of all the Tephro-type plants to germinate. That explains why so many people have had such good results with the above seed.

## TSG Vol. 7 No. 2 June 2001 P448.

On Page 448 it is stated that *A. floccosa v. cardenasii* is found at Achacachi at 3,000m as stated in the Lexicon. As Puno on the far shore across Lake Titicaca is at 3,855m this is rather drastically incorrect. With Brian Bates and Peter Down we found a *floccosa* form in two places near Achacachi. On the roadside before entering the town is a hillock where a rock outcrop stands clear of the flat plain along that shore of the lake. Climbing the rear of this hill we found a few small plants of *A. floccosa* with *Lobivia maximiliana*. Not far from Achacachi on the way to Sorata the road passes through a couple of hamlets on the lower slopes before making the steeper climb to the pass between 4,100 and 4,200m. The *floccosa* we found were at Hualte Grande the higher of these hamlets. Most of the clumps of long creamy-white haired stems were 12 to 18 inches across in a line of stones each side of a path, the stones having been cleared from fields within the hamlet. I would have expected to see larger clumps as

we got clear of human activity but along that road it became more barren as we climbed to the pass. The plants were very nice but not markedly distinct from others and certainly not *A. malyana*. If *A. malyana* grew in this area along the well-travelled La Paz/Achacachi/Sorata route it would surely have been found and described before 1971.

A. floccosa has been found over a huge range from A. yanganucensis in the north of the Cordillera Blanca, Peru to the Swaboda plant HS.3a from Mount Tunari north of Cochabamba, Bolivia in the south. Therefore it varies greatly with many different forms and only mammoth field studies could determine if any were varieties. Walter Rausch in 1971 recognised that he had found something different, in the remote area around Macusani, which is now A. malyana. Klaus Gilmer in 1998 found many more sites where it grew in the same remote areas at altitudes from 4,190m to 4,730m. Haken Sonnermo in 2000 found other sites around his base in Macusani at altitudes from 4,13m to 4,465m. Both of these last two state that it grew with floccosa at these higher altitudes whilst at altitudes below this A. floccosa grew alone. I am told that in Ted Anderson's book, "The Cactus family" he equates A. malyanus with A. lagopus.

All those *floccosas* I have seen at Huancayo, Cusco and Achacachi were hummock type plants. Many at 9 to 10 inches tall stems by 2 to 2.5 inches diameter were not far short of the *A. rauhii* description. Some people describe *floccosa* plants with low growing small diameter stems that grow mat-like to carpet the ground. It seems that Britton and Rose tried to separate the mat and hummock plants into *floccosa* and *lagopus*.

**Royston Hughes.** (Liverpool)

I am grateful to Royston for his comments. The height quoted for Achacachi was a mistake in that I relied on the English edition of Backeberg's Cactus Lexicon for the information which is now, as Royston points out, so obviously wrong. The original German edition of the Lexicon also quotes 3,000m so it is not an error in translation. I have not been able to find the correct figure for Achacachi but the surface water of Lake Titicaca is at 3,810m so Achacachi must be above that, otherwise it would be under the water.

One of the reasons I put Achacachi on the map on page 446 was to stress it's position which is so accessible and so far from Macusani. I hoped that members would write in with comments on this. Royston was the only one to write in to make the point, that was in my mind, that if the form of plant found by Rausch, and known as *A. malyana*, was really the same as *T. flocccosus v cardenasii /A. lagopus* from Achacachi then the very distinctive form Rausch found should have been in cultivation very much earlier. It would also appear to me that logically one could also find the form somewhere between Achacachi and Macusani. Can someone please give us the reason *malyana* is considered to be a synonym of *floccosus v. cardenasii* and *lagopus*? Are the *malyana* plants at Macusani considered to be an isolated but identical population from the one at Achacachi or the end of a cline stretching between the two areas? If so where exactly are the plants which appear so well hidden at Achacachi or the intermediate plants in between the two ends of the cline? I agree the two photos which accompanied the protologue of *T. floccosus v. cardenasii* and which are reproduced in The Opuntia Index in Bradleya 15/1997 p106 look like *malyana* but that does not explain the above historical/geographical queries.

A. Hill. Sheffield

## PLANTS IN MY COLLECTION.

This being a relatively new interest on my part I find the plants quite fascinating and of constant admiration which obviously, I hope, we all share and we strive to understand their requirements in cultivation.

Having received as a Christmas present from my wife the new book by Michael Kiesling "Tephrocactus and other Prickly Pears" I decided I would endeavour to make some purchases from German nurseries listed in the book and from Michael Kiesling himself. Many plants came from the Atomic Plant Nursery who also supplied cuttings. Both orders were received in mid April this year.

The sources of supply in England are relatively small, one reason that I bought in Germany. My collection contains many small plants. I really only began collecting in earnest in early 2000 so flowers in many cases are still awaited. However, the fascinating events this year also relate to some of those cuttings that I received from Germany in early April 2001. Four cuttings actually produced roots and a solitary flower within 4/6 weeks (Figs. 1 & 2) The true explanation eludes me. The only conclusion I may have, and this on my part can only be pure speculation, is the plant endeavouring to produce seed as well as new segments to be sure of perpetuating itself? Other more knowledgeable growers may have the true answer. I await Members' comments with interest. (Will members please write in to me with their answers? Ed.)

According to M. Kiesling "Airampoa tilcarensis" (Fig 1) is hardy in an unheated glasshouse down to -18°C without damage.

Malcolm Birkett (Doncaster)

Fig. 1 shows Airampoa tilcarensis (according to Michael Kiesling) in flower. Ritter (Kakteen in Sudamerika P412) refers to it as Platyopuntia soehrensii (Br. & R.) v. tilcarensis (Backbg.) Ritter as a new combination. The CITES Cactaceae Checklist lists O. tilcarensis as a synonym for O. soehrensii (P108) and a recent Bulletin of Cactaceae Systemics Initiatives (No 9 June 2000 p11) has established the name of Tunilla tilcarensis (Backbg) D. Hunt & Iliff. Ed.

## PTEROCACTUS AUSTRALIS FORM WP 51.3

Of the several forms of this species this plant (Fig. 3) is considered to be the most attractive. It was purchased several years ago from a Dutch Nursery at Lexmond. I am advised by TSG member Paul Hoxey that this is one of the clones collected by Wolfgang Papasch who also collected a range of Gymnocalyciums.

John Betteley. Lincoln.

Has any one a copy of a list of WP numbers? Perhaps someone can tell us from where WP 51.3 was collected. Ed.

## "Queen of the Andes" - my outlook on problems with rooting cuttings

One of the Opuntia beauties, *Austrocylindropuntia malyana*, has been known as impossible to root from a cutting. Luckily, it turned out, that this is not a fateful rule. Thanks to such excellent growers and enthusiastic travelers as Hakan Sonnermo, we are able to learn much more about the "Queen of the Andes".

While making inquiries about reasons for this problem, an opinion that 'a clone which has been propagated by grafting for many years loses its rooting potential' appears the most frequently. Michael Kiesling in his book "Tephrocactus and other Prickly Pears" also reported this standpoint. On the other hand, one of the best specialists of South American Opuntias, Klaus Gilmer, is more cautious and doesn't favour this uncertain theory. Perhaps due to his own experiences with clones brought from habitat during his recent expedition.

From the biological point of view this idea, that a clone subject to repeated grafting will lose the ability to form new roots, seems to be wrong. We have to remember that such ability, which gives to a plant the possibility of forming new roots or simply the possibility of vegetative propagation, is recorded in the same place, where all other information about a plant is preserved. Genetic material, in the shape of DNA chains, holds, as well information about the complete appearance of a plant, the instructions for all of its possible functions. What is more, the DNA of a particular clone is the same in every cell that builds its body. Of course, there is possibility of some mutations, that should be called in this case as a tumour (cancer) mutations, but this is another problem. Generally speaking, one clone has got the same DNA set in every cell.

In connection with this a clone of A. malyana collected in 1971 by W.Rausch will have preserved all original features until now. At least, one clone transmits the same DNA set to every new cell that is developed by that clone. If it had been able to form roots in 1971 it would still be able to do it as it is still the same DNA.

According to these facts, it seems that the theory "loss of potential of forming new roots by plants propagated by grafting for many years" is wrong. However, I don't claim it is totally false, but as yet, there is no sensible explanation of this phenomenon. I could imagine that the author of this statement tried to ground it on the simple rule that useless features tend to fade away. But this genetic law refers only to generative propagation, because only in this way that the particular DNA sets of live organisms can be changed, generation by generation. And it is senseless to consider this problem any longer. Especially, since, even if there exist any unknown at present laws of genetics, it is almost impossible, that after only 30 years in cultivation, the W.Rausch's clone of A. malyana has changed its possibility of vegetative propagation. It is certainly very short period of time for any animal, plant or fungus to lose the facility.

After all, there is still the unsolved puzzle; "Why are some clones of A. malyana so difficult to root from cuttings?" I would like to present my own opinion on this theme, that should be certainly be taken as a hypothesis.

It is very likely, that the evolution of *A. malyana* has developed in the direction of more and more hairy plants. We can observe such a phenomenon in the case of *A*.

floccosa, which occurs in habitat as a very variable species on that score. There exist very hairy forms like A. floccosa "udonis", A. floccosa "rauhii", A. floccosa "verticosa" as well as hairless ones like A. floccosa "atroviridis" or A. floccosa "yanganucensis". And, what is very important, someone can always find intermediate clones in habitat. It seems to be quite reasonable, that this special feature is in progress, about which can testify the domination of hairy plants. What is more, this kind of protection in hard, mountain conditions has to be the most effective.

However, I don't want to mention simply advantages, that plants may obtain from the thick hair, because it would have to be a simplification of a problem. At least, if we take hair away, a clone of A. floccosa won't necessarily be more or less sensitive to frost. It depends also on the composition of cell cytoplasm and general structure of a plant.

In comparison with A. floccosa, A. malyana has already attained the climax of hair coating. It is completely overgrown by golden filaments. This may lead to suspicion that the first species is the younger and that A. malyana had to have separated more or less earlier. However, this is a very uncertain statement. There is too little information at present.

There is only one problem. If *A. malyana* is better conformed to mountain habitats, why is A. floccosa more widely diffused taking up many more areas in the wild? The answer could be very interesting, because it is directly related with this feature of some clones that makes them impossible to root...

An attempt at finding the riddle's solution is the second part of this article.

While succeeding generations of *A. malyana* became more and more hairy, there arose a new snag. The very heavy coating started to render photosynthesis difficult or almost impossible. Evolution, as a natural way of development, is a unidirectional process, so the plants couldn't just get rid of too much hair. They had to solve this new problem or simply die.

Looking at cuttings of *A. malyana* it always turns out that the leaves are a deep green colour, but the main stem is very pale. It is usually bright yellow or only slightly greenish. And, what is very important, if one takes the hairs off a plant, the colour of the bare segments will change very little into very pale green. In this, it differs from *A. floccosa*, even very hairy forms, which are always a green, or at least a grey-green, colour.

This observation has led me to believe that *A. malyana*, in the course of evolution, has reduced, almost completely, production of chlorophyll in the stem allowing the weight of photosynthesis to be taken over by the leaves. The chlorophyll in stems became a useless feature. In this simple way, the problem of photosynthesis, trammeled by thick hairs, could be solved successfully. On the other hand, photosynthesis had to take place less effectively and could explain why *A. malyana* wasn't able to expand too much. In connection with this fact, *A. floccosa* could take up much more areas in the habitat.

Fig. 1. Tunilla tilcarensis.

All 3 photos by M. Birkett.

Both cuttings in Fig 2 from M. Kiesling. We know that they are both cultivars from North American species but aren't the flowers lovely?



Fig. 2a. O. polyacantha "pforzheim". Fig. 2b O. macrohiza "Apricot".





Fig. 3. Pterocactus australis WP 51.3. Photo by J. Betteley. Fig. 4. T. geometricus seedlings. Photo by J. Betteley.



Unfortunately, this remission of chlorophyll quantity has probably made *A. malyana* a quite powerless species and here the secret of problems with forming new roots is hidden. A cutting, that someone has cut off for rooting is very weak and loses green leaves quite rapidly. There is, perhaps, too little living vigour inside such a stem and it is simply impossible to generate enough power to form new roots. I would even compare *A. malyana*'s stem to a chlorophyll-less graft that cannot live without its stock. The leaves can be compared with the stock, which secure photosynthesis.

Perhaps a single cutting, which has been cut from a mother plant, has still to realize photosynthesis in order to produce enough energy for starting new roots. Perhaps, too, this isn't possible in case of lack of sun. I have started to prepare a simple experiment that should answer this quite interesting question. It is also important to mention, that *A. malyana* in habitat does not lose all its leaves during summer. The leaves are hidden by the wool but the plants has already a well developed root system, so there is no problem with starting to grow, when there is enough water in the soil.

In my opinion, the clone collected by W.Rausch in 1971, which is the most popular in collections, represents this problem in every respect. In contradiction to this one, clones of *A. malyana*, which were brought recently by Hakan Sonnermo have got certainly more green segments or at least, they become green when someone removes the hair and exposes them to the sun. Such clones probably haven't reduced their stem chlorophyll so much that they cannot achieve vegetative propagation.

If we are talking about the proper name for "Queen of the Andes", I think, that we should try to leave old nomenclature behind. It is not possessing great precision to call Austrocylindropuntia as a Tephrocactus in the light of performances of contemporary botanists and growers. What is more, it is almost certain that *A. malyana* is not a form of *A floccosa*, because there are too many differences in habits of body. As yet, nobody has found intermediate clones in habitat, even if *A. floccosa* and *A. malyana* very often grow together. Also Hakan Sonnermo and Klaus Gilmer confirm this statement. The last name, which could be used for this plant, according to tradition is *A. lagopus*. However, this name is just a synonym of mentioned earlier *A. floccosa*.

Piotr Swiatoniowski (Krakow, Poland.) Many thanks to Hakan Sonnermo whose photographs (Front cover and back cover) help to illustrate this article.

## **TEPHROCACTUS GEOMETRICUS – TSG SEED.**

The March edition of the TSG included the Seed List which included two forms of *T. geometricus*. One packet of each was purchased and sown in April. At the same time, seed of *T. geometricus* obtained from Doug Rowland was also sown. The tray at the top of the photo (Fig. 4) contains TSG seed, where eight germinated after three weeks. In the other tray two have germinated. After the initial germination nothing further has come up.

Significant precautions were taken during and after sowing. Seeds were sown (buried) in a mixture of Seramis (clay granules) and vermiculite with added charcoal. Trays were watered from below with de-ionised water and alternate solutions of the fungicide Chinosol. Even with these precautions I have lost two seedlings.

John Betteley. (Lincoln)

## A REVISION OF MAIHUENIOPSIS.

1 see that Kiesling has at last published his proposed revision of *Maihueniopsis* in which *Austrocylindropuntia* (1938) is now absorbed as a junior synonym. May I make a comment?

Nomenclaturally the proposal is of course unexceptionable: *Maihueniopsis Speg.* (1925) has priority. However it flies in the face of plainly recognisable relationships and I know of no one who is likely to accept it. It is desirable that the nomenclatural units used in a classificatory scheme should not fundamentally conflict with such relationships *if this can be avoided.* In the present case it can be avoided. The following analysis is offered, not as an alternative formal proposal, but as an indication of the lines along which an alternative scheme can be sought. It is also a plea to the author to reconsider.

The story begins in 1984. Kiesling's important and valuable paper, "Estudios en Cactaceae de Argentina: Maihueniopsis. Tephrocactus y generos affines (Opuntioideae)" (Darwiniana 25: 171-215) contains one unfortunate flaw. Under "Maihueniopsis Speg. emend. Ritter", Kiesling, after noting (p. 198) that the distinguishing character of absence of articulation in the stems used by Spegazzini to define the genus appears, from a minute examination of the type material, to be based on a false impression and that the segments are, in fact, distinct, with normal joints between them (a crucially valuable observation), then goes on, por lo tanto, acepto la enmienda efectuada por Ritter (1980) para ese caracter, aunque a los otros de su diagnosis los considero triviales. "Therefore I accept the emendation [of the genus, as surmised along similar lines by Ritter] effected by Ritter (1980) as regards that one character, though I consider the other [characters] of his diagnosis trivial" (my underlining). Following from this judgement, he then proceeds to refer species of two (or, if M. nigrispina is taken into account, three) very different lineages to the one genus Maihueniopsis. This was of course the only single prior segregate available. The two main lineages correspond to the genera Maihueniopsis sens. str. and Cumulopuntia of Ritter. The rank at which we distinguish them is another issue. which can be left for now: the crucial thing is that Ritter's diagnostic details are not trivial and the lineages are distinct.

So far as 1 am aware, no informed opinion, in this country at least, accepts this extended treatment of *Maihueniopsis*. But a reasoned examination is necessary. One has to admit that these high altitude cushion plants can look extraordinarily alike, and on a naive view they might well seem to be all of one relationship. One might have expected a botanist of Kiesling's calibre not to be deceived by lookalikes, but unfortunately, in the case of the cushion opuntioids, flowers and fruits by no means give the same unequivocal cues as they often do elsewhere. Flower structure, in particular, is in fact so remarkably consistent throughout *Opuntia* sens. lat. that, when one remembers that generic distinctions in other parts of the Family are often based on floral characters, insistence on distinct genera in this area begins to look like a case of special pleading! What grounds, then, can be advanced for confidently affirming the presence of "two (or ... three) very different lineages"?

Let us start from square one. Let us take two plants that seem, so far as 1 can gather from other pages of your periodical, to be generally known and agreed upon, and to avoid unnecessary controversy as to their exact classification let us refer them both non-committally to *Opuntia* sens. lat. and call them 0. *glomerata* and 0.*boliviana*. They will represent the two main lineages excellently. Kiesling refers both to Maihueniopsis. Let us now imagine an acute observer new to these plants. He might remark fine differences in presence/absence of tubercles and distribution of areoles and would doubtless notice the differences in the spines. But he might well claim that the two differ little in general habit and that segments of each might be found that had much the same size and shape as those of the other. Flowers and fruits would give little help; the fruit of the first might be more mucilaginous, but both would be fleshy and indehiscent. He might well be led to the conclusion that the various differences, while convincing enough to define two species, were far from enough to suggest that these were widely unrelated.

However if he met ripe fruit containing fully developed seeds he might be expected to be struck at once by pronounced differences in the latter. The seeds of the first would be flattened or lenticular; those of the second rounded out so as to approach a spherical shape and in all probability with a protecting thick thread or girdle (Ritter calls it "Reifen" - "hoop") around the circumference. If he correlated these characters with the small vegetative differences already noted and then looked at other specimens, he would find the correlations repeated. Further, he would find that the seeds of other plants described as distinct species related to these two exemplars would fall without difficulty into one or other of these two categories. The significance of these differences is further confirmed by Stuppy's microscopic investigation into the cellular structure of the outer integument ("aril"), which is found to be quite different in the two cases. It may seem surprising to attach such importance to the seed. But seed characters must be rated as among the most highly conservative of all, giving valuable indications of relationship, and here they must be taken to indicate a long-standing divergence of descent. Stuppy has no hesitation in seeing two genera. Confirmation of the divide comes from an unexpected quarter: years ago, Royston Hughes observed that the seedling cotyledons are totally different in these two species. And, quite apart from its other distinct characters, the seeds of O. nigrispina are markedly reniform, and different again.

How is it that Kiesling could disregard these characters and even at that date (let alone with his latest proposal) arrive at such a misconceived circumscription of *Maihueniopsis'*? His 1984 key to the genera treated in the paper suggests one possible reason. He was clearly preoccupied with the aerenchymatous (corky, with empty airfilled cells) nature of the seeds of *Tephrocacuis* sens. str., with their striking lateral "auricles" (a feature he was the first to describe). He used this character to separate off *Maihueniopsis and Austrocylindropuntia* by the lead "... Seeds with a compact aril, soft or hard, but never aerenchymatous, without auricles ... " The two latter are then separated by a couplet depending primarily on segment shape and leaf length without reference to seed form at all. His generic diagnosis of *Maihueniopsis* skates over the question): "... Seeds globose to pyriform, regular; aril compact ... " Most baffling of all however is the fact that the individual specific descriptions clearly indicate the different kinds of seed form without showing any recognition of their possible significance.

Through its disregard of seed characters, the 1984 circumscription of *Maihueniopsis* opens the way for this present unfortunate extension which likewise mixes seed types, since that of *Austrocylindropuntia* is also globose. I see Kiesling is careful to call it a

"draft", though since its publication puts it in the public domain the new combinations in it may well start to circulate. It may be that Art. 34(b) of the Code ["a name is not validly published ... when it is merely proposed in anticipation of the future acceptance ... of a particular circumscription"] will discourage this. But invalidity has never prevented circulation of names (sometimes of the most bizarre kind, as I see your pages testify!). Clearly there is a need to consider what kind of rescue package can be formulated.

Perhaps the first thing to note is that Wallace & Dicke's cladogram shows conclusively that Austrocylindropuntia is absolutely basal. The brief note in Kiesling's latest draft, "According to Wallace & Dicke Austrocylindropuntia occupies a place somewhere between the Opuntioideae, while Maihueniopsis, is, according to these authors, far from the first" gives a very incomplete picture. Certainly their researches provide only one kind of evidence, and in the finer details this may be questionable. But the isolation of Austrocylindropuntia is so strikingly shown as to be absolutely compelling. It branches off at the lowest point of the Opuntioideae, long before any of the N. American groups, and long before Opuntia sens. str. (i.e. the flat-padded plants). If any group has a claim to be considered as a segregate from Opuntia sens. lat., it is this. If any classification fails to show its distinction it is gravely misleading. This should be the starting point.

The course is then clear. Only let *Maihueniopsis* be restricted to the circumscription adopted by Ritter (without its odd inclusion of  $\theta$ . molinensis) and we see that it is not connected with *Austrocylindropuntia* at all. the present proposed transfers of austrocylindropuntioid plants are of course unacceptable. The remainder then falls into place. Once plants of the  $\theta$ . boliviana alliance have been disentangled from the enlarged *Maihueniopsis* that Kiesling had previously envisaged, it is easy to see these as scaled-down versions of the austrocylindropuntioid line (as indeed the Wallace-Dicke results indicate): the globose seeds, the spines, even the form of the minute leaves all correspond. Doubtless the  $\theta$ . floccosa group is also connected, as Kiesling suggests. Maihueniopsis sensu Ritt. itself is thought to be related to *Tephrocactus* sens. str. as indeed the flattened seeds and often flattened spines would suggest. There could be various ways of implementing the revision. This is not the place to argue whether *Cumulopuntia* (1980) should be recognised at generic level or referred as a subgenus to *Austrocylindropuntia* - to which there is no obstacle. The important thing is that the disentangling should be done.

The best person to do it would be the author. Spegazzini once handsomely acknowledged that details of one of his earlier publications had been incorrect. I am quite sure that the warmest of welcomes would await Roberto from all of us his colleagues if he were to take a corresponding step. So far as 1 know, all of us have for years been doing our best to dissuade him from proceeding with this present scheme, so fatally tied to his earlier misconception. I have, as I am quite sure all of us his colleagues have, the most profound respect for Roberto. His knowledge of the individual plants in their native habitat is unrivalled; his neotypes an invaluable asset. My hope is that he himself will obviate my sad need to feel such deep regret at being obliged to write this comment.

James Iliff.

The original article to which J. Iliff refers appears in TSG Vol. 4 No. 1 March 1998 p237 – 242. R. Kiesling has written a reply to the above article. However, this will appear in our next issue.

Ed.

## Plant Exchange Scheme

Over the past few years many members have been advertising and asking for certain plants they have been looking for without success. The problem has been discussed at the last A.G.M. and it was agreed to set up a scheme for all members. Many other "Specialist Groups" have such a scheme that has proofed very successful.

It is important that plants are distributed among members as widely as possible to get good feedback about successful growing of the more difficult plants, or when a plant is lost, it can be easily replaced. For such a scheme to work properly it is of the utmost importance to have enough members willing to pass on cuttings or plants to others. Plants will often be strengthened by the careful removal of a cutting. Now we have close on one hundred members, this should not be a problem.

• So firstly, will all members willing to pass on cuttings please let me have a list of their plants from which cuttings may be passed on! Plants from *Tephrocactus, Maihueniopsis, Cumulopuntia, Puna, Austrocylindropuntia, Pterocactus and Tunilla (Airampoa)* should be included. (This list will not be passed on to any one by me!) Remember, you too may ask for plants missing in your collection!!!

• In the next issue, I will let you know if I have sufficient lists of suppliers to start the scheme. Please do not send requests before then! Then members may write in to me, asking to be put in touch with the member who can supply a certain plant, bearing in mind that it would be unreasonable to ask for more than one or two. Always enclosing a SAE (stamped addressed envelope), or an IRC (International Reply Coupon)!

It is then up to the recipient and the supplier to negotiate the transaction. Please also bear in mind that it would be reasonable for the supplier of the plant/s to ask for a small sum for supplying, plus refund of postage.

I will try and spread requests for certain plants so that one supplier is not inundated with requests for the same plant. Remembers that this is a service FREE to members and it will be up to you to make it work!!!

E-mail or write to René Geissler with SAE or IRC (address on Secretary's page).

## **SERAMIS**

Several members have expressed views t o me about using Seramis, which appears to be a form of clay granules. If you have used the product please will write to me to give details of how you used it and your evaluation of its use? Ed.

## COMPOST

In TSG Vol. 6 No. 1 March 2000 P372 Bill Jackson asked all members of the TSG to write to him saying what you use as a potting compost. Just a list of ingredients will do although if you feel like adding a few notes about anything, that would be welcome. Any notes of improved success/disastrous results would be a bonus. The intention is to produce an article giving statistics about the popularity of various ingredients and their proportions, including fertilisers as well as the base materials. The bulk of the review will be anonymous with prior permission sought before publication of any personal reference. It is anticipated that many members will be using one of a few basic mixtures but there will be a few who have a rather unusual mix. In order to make the statistics be of representative numbers it is important that we have as many replies as possible. Please will you send Bill the information requested? As he said in the original request, the details need not be great (although the more information you wish to give will be welcome). If you use what you regard as the standard mix that is used by most people then that information is important for the purposes of the study. Please do take part. Ed.

## A SEEDY QUESTION.

Having purchased four, very nice, small plants of *O. invicta* over a year ago, all with seed pods, I would like to know if any member knows why these pods haven't ripened. They remain firmly attached to the plants and show no signs of drying. I assume that the plants were rooted cuttings from a larger plant which had flowered. They are all growing well and producing new offsets. Yet the seed pods show no sign of drying or falling off.

I know that, in the UK, *O. invicta* has to be very large to flower and that flowering is a very rare event. But these pods are already formed and I would like to harvest them and sow the seed.

Any help would be useful. Will they eventually ripen and fall off or do they need to be removed?

D. Hutchinson (Leicestershire.)

We are aware that *O. invicta* is not South American but this is a query to help a member which might be of help also to other members. An answer might also be in relation to all Opuntias. Ed.

### **CLONES IN CULTIVATION.**

I would like to glean information about clones of particular species or genera that are in cultivation. The main idea would also bring to light as many synonyms as possible of particular species. In connection with this project I would like to ask you please to send me a few pictures of properly developed segments. Although the segments need not be perfect please do not send pictures of segments that are too young or are unnatural due to the young age of the plant or due to lack of light. I promise that all pictures will be sent back if you request this. All authors of the pictures will be mentioned in the planned article.

We start with the Chilean group of Maihueniopsis. You may find them in your, or your friend's, collection under the following names:

archiconoidea, atacamensis, camachoi, colorea, conoidea, domeykoensis, grandiflora, leoncito, rahmeri, taratacana, wagenknechtii.

Please do not disappoint me or our Society! This will have to be teamwork. Piotr Swiatoniowski, ul. Krolowej Jadwigi 143, PL-30-212 Krakow, Poland.



Fig. 5. A.floccosa, just outside Macusani on the hills near Hacienda Llulluchan. Photo by H. Sonnermo.



Note the black seed pod. Photo by H. Sonnermo.

## **TEPHROCACTUS**

Incl. Maihueniopsis, Cumulopuntia and all related genera



Two km north of Macusani, Peru, on the road toTantamaco. In the background Allicap mountains and, to the right of the biggest mountain, Lago Chungora. Photograph by H. Sonnermo.

> STUDY GROUP Vol.7 No.4 December 2001

## Secretary's Page

Another new member has joined us since the last journal: John Bodle. I wish him every success with the plants and hope he will tell us of his experiences with his plants.

•All articles and comment should be send to the Co-Editors:

•Subscriptions and any other correspondence should be sent to the Secretary. May I remind you please to let me know any changes to address telephone number and E-mail address.

If you write to one of our Officers and expect an answer, please to include a S.A.E.

•<u>Subs. are now due</u> and remain at £10.00 per annum for the UK and Europe (<u>European</u> members please note: "no <u>Euro-cheques are accepted by our Banks – Please send £</u> <u>Notes</u>") Overseas members £14.00 or \$25.- USA (in \$ bills only). <u>Please make all cheques</u> payable in sterling to : "Tephrocactus Study Group (not individuals).

•Please remember, members may advertise their "WANTS" and "SURPLUS PLANTS" free in the Journal, in no more than 30 words

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## COMPOST

Very few members responded to the request for information about ingredients for use in compost. Alan Hill's repeat request called forth a few more but they are far from reaching a significant sample size. Thank you to those who did, but I think the original idea of producing statistics is now dead in the water. Down the years I have talked to many cactophiles about this. Most seem to prefer a mixture of John Innes and grit, sometimes with sand. The few returns from our members echoed this preference. So it does seem that the statistics would have been pretty boring; rather like "Ask the Audience" on that telly show\* when the audience thinks it knows the answer. However I have received a letter from a new member that makes some interesting points and reproduce parts of it below. This new member is David Parker. New member is not quite right. He's new to TSG but arrives fresh from 20 years maintaining the Echinocereus Reference Collection, (1500 stock plants) which has now moved to Bolton.

He says "On compost, N. Taylor at Kew and I had many talks but always came back to the 'basic loam' being of a reasonable standard. We have used many of the JI composts found in stores and garden centres and found them poor; plants did not do well in them. We found a yellow 'Kettering Loam' and used this from 1975 to 1978 with great success. This came from a lady farmer who found the sale of topsoil to be much simpler, and more profitable, than farming. Unfortunately she did eventually sell it all and then retired. After two years trying to find a good replacement I have been fairly happy with this mixture.

| 10 parts | Arthur Bowers JI no.2                     |
|----------|---|
| 7 parts  | Small (3-6mm) lime free agricultural grit |
| 4 parts  | Horticultural sand (Washed)               |

I use a steel, pint jug as a measure and do the mixing in a large, old baby bath. Added also:

| 2 Trowels   | Gypsum           |
|-------------|------------------|
| 1.5 Trowels | Steamed bonemeal |
| 0.5 Trowels | Superphosphate   |

I am now using this same compost, with some success, on new Opuntia cuttings and will see how things go. This mixture also worked well for Echinocereus seed. They came up like weeds. However, from what I read in the Journal, I will need to find some special mixture to grow Tephros from seed. I will write to John Betteley in Lincoln next year for advice.

During the season I use 'Malathion 60' chemical spray twice a year and an LOC. Soap several times in my water butts. For feed I use Chempak No. 4, not No.8." Malathion 60 is still available from agricultural farm shops but it has all the hazards of 'Sheep Dip'. So it is advisable to use an "R60" respirator fitted with British standard RF624, A1,P3 Chemical filters". I (WLJ) have to echo this rather stern warning. I have always assumed that these dangers were a problem for serious growers who were exposed for long periods many times a year. Hobbyists like me, I thought, surely needn't get too excited. But lately I have come across several examples where relatively light exposure has produced symptoms. Symptoms which don't go away. It seems we should <u>all</u> take proper care.

W.L.Jackson Sutton Coldfield.

\* Who wants to be a Millionaire. (Editor's Note: What a daft question.)

## IS POOR VEGETATIVE PROPAGATION CAUSED BY SILENT VIRUSES?

In a recent issue of the JOURNAL OF THE TEPHROCACTUS STUDY GROUP, Piotr Swiatoniowski comments on the apparent inability of *Austrocylindropuntia malyana* to root from cuttings after several generations of cultivation by vegetative propagation [1]. In reading this article, I was reminded of when I was a horticultural student at the University of Alberta (Edmonton, Canada) in the early 1970s. One of my professors, Dr. Bill Andrews, was a specialist in potatoes, which of course are vegetatively propagated. I well recall him mentioning that potatoes are susceptible to viruses in home and commercial cultivation because growers do not use sanitary practices in planting, and use the largest tubers for eating and the stunted ones for next year's crop. The stunted ones were those which had the viruses, and thus the problem was compounded.

In considering why *Tephrocacti* and related *opuntioids* may be slow in rooting after several generations of grafting or rooting cuttings, it seems to me that the problem may be due to silent viruses. These are viruses which do not cause visible signs of infection such as chlorosis, spotting, corky patches, or browning off. I suggest that the problem of slow-to-root cuttings may be virus infection spread by contaminated grafting knives and tainted growing media.

Most of the research on cactus viruses has been done on various *Opuntia* species, *Zygocactus* hybrids, and the saguaro *Carnegia gigantea*. There are a number of viruses known, such as Sammons' Opuntia virus [9], Cactus X, Cactus virus 2, and Zygocactus virus, to name a few. Cactus viruses are generally latent, that is, no external symptoms are visible. Early workers who screened wild plants for viruses may have missed some that did have external symptoms because the viruses had long incubation periods, up to two years [2]. A virus found in *saguaro*, for example, had an incubation period of 395 days between inoculation of a clean plant and positive identification of virus [8]. A variety of viruses are found in wild cacti, and there maybe a mixture of virus species in one individual plant. Viruses spread slowly in habitat due to vegetative propagation [5].

With many cactus viruses, the greatest rate of infection in wild plants is found near urban areas [3]. It appears that urban pollution may stress the plants and predispose them to infection. Tobacco mosaic virus has been found in *Opuntia basilaris* growing wild in Arizona [10]. A 1977 study on cultivated opuntias in Europe and the Americas showed that 73% of them were infected [7]. 3% had Sammons' opuntia virus, 37% had Cactus X virus, 34%had Cactus 2, and 2% had both Sammons' opuntia virus and Cactus 2.

Viruses found in *Opuntia engelmanii, O. phaeacantha and O. macrocentra* in Arizona sometimes had visible signs such as chlorotic rings, but often no visible symptoms were seen [4]. The effects of the viruses can be subtle. For example, it was shown in a study on *Nopalea cochenillifera* that Cactus X virus reduces the Crassulacean acid metabolism (CAM) activity of the plant [6]. CAM is a method of photosynthesis used by desert plants to photosynthesize while minimising water losses. A reduced ability to properly photosynthesise will obviously slow down growth and place wild plants at a competitive disadvantage. The same cactus viruses may have different effects on

different species. A study in southern Arizona showed that *saguaro* infected with SOV and saguaro virus had no visible symptoms, but infected *platyopuntias* showed chlorotic rings on their pads [3].

Clean sanitation is important in propagating cacti. Grafting knives should be rinsed in a dilute disinfectant solution between each plant. Hands should be washed when handling plants, especially if you smoke. Tobacco mosaic virus is very long-lived and has been known to survive 50 years on dried plant material. It is found in tobacco products used by smokers, who spread it by handling plants. There are no vaccines to prevent cactus virus infection. It has been reported that acetylsalicylic acid (aspirin) can induce resistance to Tobacco mosaic virus [11] but I have no practical experience in this.

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Dale Speirs. Calgary, Alberta, Canada.

## **OPUNTIA FROM MENDOZA**

Figures 1 & 2 shows an illustation of Rh 666. Ralf Hillman photogaphed the plant 30km northwest of Mendoza city, Argentina, at 2,000m. At the time I spoke to him no name had been assigned to the plant. Will members please inform me what name they would attribute to the plant? Ed

## PERU WEATHER - SOME HINTS WHEN GROWING A. FLOCCOSA.

I would like to comment upon some articles in recent TSG Journals and also give some more information from my travels in May 2000 in the Cordillera Huayhuash and Macusani area of Peru.

First something about the weather. During my stay in the Cordillera Huayhuash, about two weeks, I had dry and sunny weather the whole of the time. Only late one afternoon did some snowflakes fall. Also in Macusani I had two weeks of sunny weather with some clouds in the middle of the day. There was no precipitation at all except for very strong dew in the mornings. A strong memory is the travel from Lima, in haze, up into the mountains and suddenly all the clouds disappeared and the fantastic blue sky became visible.

On the contrary, I have talked to some trekkers who did the Huayhuash circuit, I think, two or three years ago and they had terrible weather with a lot of snow and rain in July. This exceptional weather was surely depending on the very strong El Nino at this time.

I believe Rauh, when he talks of precipitation, also had a meeting with El Nino. Normally it is very dry from May until September in the Peruvian Andes and if you study some data from some meteorological stations in the high Andes they confirm this. Cuzco, with data from 1931, receives only 2mm during June and July and Puno, with data from 1961, receives in the same period 7mm. So I cannot believe the *floccosa* group plants receive precipitation the whole year round! Wherever I saw the earth it was dust dry.

From November until March, however, I believe that even England has nothing to compete with when talking about rain. I talked to some people in Macusani and saw some pictures and it was like looking at some beautiful north Scandinavian Christmas cards. Down in the little village of Tantamaco I spoke to an Indian who told of 50cm. of snow in wintertime. This snow slowly melts away due to the influence of the sun.

So at home I give my A. malyana and floccosa plants a rainy autumn, but as much light as possible, and from January only some occasional watering. From March no water at all except for the summer dew. In winter in Sweden the outside temperature can go down to  $-20^{\circ}$ C so I put my plants inside the greenhouse. My thermostat is set so the plants have a cold winter but do not freeze. I also provide extra light. From spring until late autumn I grow my plants outside in direct sun but protect them from rain. In the high Andes I have never measured a maximum temperature of more than +18 so plants in habitat never have the plants inside in the greenhouse be careful and watch out for the red spider mite!

It is very interesting to see in August, after four months of dryness, the green foliage coming out from the hair of *A. malyana* and the plants start to grow. Also the *floccosas* show signs of a new start and later in March the hair again covers the plant as a protection during the summer months. I have also seen that the plants can endure both water and cold without any harm.

I took some small samples of soil home with me and I measured the pH. It gave a clear signal that it is a bit higher than our normal cactus soil. I found it to be pH 7. I gave some soil to Piotr Swiatoniowski. Perhaps he later can do some more analysis.

Some comments about *Tephrocactus viridis* (See TSG Vol. 5. No.1 March 1999. p306). I have one plant labelled with this name and it is surely an *A. floccosa* with very

yellow spines. I obtained it from a Garden centre in the USA. I think that it is only a name from Knize. The problem is that if you order this plant or something else from Knize you cannot be sure that you get what you have ordered!

Hakan Sonnermo. Sweden

This article gives some very interesting information that should help members to grow their plants. Also see the report by Klaus Gilmer, TSG Journal Vol. 5. No. 2. June 1999 p325 – 328 and the following issue p 336, on experiments he carried out on *A. floccosa*. One of his conclusions was that *A. floccosa* in cultivation could not survive moisture and actual freezing. Ed.

## A SUFFERING AUSTROCYLINDROPUNTIA FLOCCOSA.

In the last issue of the Journal, Vol. 7 No. 3 September 2001 Fig. 5 on page 466 showed a habitat plant of *A. floccosa*. Hakan Sonnermo took the photograph. An intriguing aspect of the picture was that there were two colours depicted on the plant: white and yellow. Either there were two plants growing together with different colours of hair, one plant with different coloured hair on the stems or parts of the one plant were dead. Hakan has confirmed that the last alternative is the correct one. It was the only *A. floccosa* he saw with signs of dead stems. He saw no dead material of *A. malyanus* Ed.

VARIATION IN FORMS OF AUSTROCYLINDROPUNTIA FLOCCOSA. Figs 5 & 6 show two plants growing very near each other in habitat. However, the amount of hair is so different that a "splitter" such as Backeberg might try to argue there are two species! Ed.

## **TEPHROCACTUS PLATYACNATHUS**

Figs 3 & 4 show the features discussed below. The one in the flat pan, Fig 3, is a refugee from Mary's collection. It has had a difficult time, including one winter lying on its side, outdoors in a puddle. It was labelled *O.diademata* but never grew enough to look like that or anything else. This is the plant mentioned for its extraordinary juvenile growth on page 343, Vol. 5. No.3 of the Journal. Long, thin black segments with weak, adpressed spines. Once it got going it produced wide, flat spines, which grew away from the areoles tangentially to the segment surface. Alan Hill saw it at this stage and thought it looked like *T. platyacanthus*. Although slightly marked with transverse, light brown bands the spines are mainly white and have a bright, clean appearance. The segments are now dark green and short. They would be almost spherical but for the tubercles which are quite pronounced. The end result is a very compact, spiny and business like plant.

The other Fig 4, in a taller pot, was bought in Germany last year, or was it the year before? On the Scottish trip anyway. The spines are longer, narrower, browner and twist and straggle in all directions. The segments, too, are up to three times longer. They droop in winter and fall off if you cough. Because they are longer the plant is more open and the whole effect is straggly and dingy. Well, it's good to have more than one clone, but I bet you can guess which one I like best. It will be interesting to see how they develop. Let's hope they don't gradually converge.

W. L. Jackson. Birmingham

It is planned to discuss the status of *Tephrocactus platyacanthus* in the next issue. Ed.



Fig. 1 (Top) & Fig 2 (Bottom). An Opuntia from Mendoza RH 666. Photos by R. Hillman.





Fig. 4. "Tephrocactus platyacanthus". Photos by W. L. Jackson.

### ERRATA

Mr James Iliff has kindly pointed out that Michael Kiessling's name, which appeared in our last issue, is spelled with a double "s" and thus differs from Roberto Kiesling. Michael Kiessling lives in Germany and wrote the book "Tephrocactus and other prickly pears". Dr Roberto Kiesling lives in Argentina and has published several scientific papers including the one in Darwiniana 1984, which is a very important paper on the plants we study.

## **SERAMIS**

You get these irrational convictions. Well, I do. Sometimes they arise from forgotten knowledge, lurking somewhere in the subconscious, and can be brilliantly accurate and relevant. Sometimes they're junk. I have this thing about CLAY GRANULES. I am sure that they are going to be more use than grit. I am still searching my memory for some rational basis for this and have found some ideas. But really it is just a feeling. Eventually Warren Withers offered some gray stuff for sale. I tried it. All my plants are potted in a mixture containing this stuff. Now his suppliers have refused to sell anything less than industrial quantities which he can't store. So I'm stuck. Searching for an alternative I tried Seramis.

I dislike chemistry. All that slopping solutions from one container to another, getting the right dilution and that sort of thing. Seramis comes with ready mixed solutions. One is high potash. I thought I might try a bit of hydroponics. That is the design purpose for this product. I had this plant cramped in a ridiculously small pot. I repotted it in pure Seramis and poured in a little water just to get things started. Seramis floats. So top heavy plants with a small root ball turn over and fall on their choicer neighbours. So, if you know anybody who wants the remains of a small, and expensive, bag of Seramis complete with liquid fertilizer just send them round here.

There are lots of these clay granule products; Hortag, Cat Litter, Warren's Gray Stuff and many more. If I can find an inexpensive version of about the right particle size I will be able to continue. Since I intend to use this as one component of a mixture I am, perhaps, being a bit unfair to Seramis which I have only used neat. It is fair to say, as well, that it looks good. But it is expensive.

W.L.Jackson. Sutton Coldfield.

## SERAMIS GRANULES.

From my article in the September Journal members will realise that growing Tephrocacti and Allied genera is a relatively new experience on my part. Speaking with John Henshaw of Croston Cacti I mentioned that my experience with rooting cuttings was not, in my opinion, as successful as I that thought it should be. John suggested using seramis if I could find it. It was quite true; the product is not readily available. I have in fact only ever located Seramis in Garden centres and then only a few. However, on the plus side the product is clean, light and easy to handle and, above all, it is reusable.

A policy I have always adopted when growing Alpines, Cyclamen, Saxifraga and many other genera is to provide rapid drainage and obviously I make no exception with Cacti. This applies to seed, cuttings and to established plants. I only take cuttings between mid April and late July. After that date I find that they just sit around waiting for spring so why not leave well alone?

The attached sketch will, I hope, illustrate what to me is a well-proven and totally reliable system for rooting cuttings. I hope that others will be encouraged to try it.

I water generously with a houseplant narrow spouted watering can until the water flows freely from the drainage hole and then stand the pot(s) in a shady spot. The surface granules will be seen to dry out fairly rapidly but this is not an indication to start wateringagain, quite the reverse.

I wait at least seven to ten days, then test for rooting and then water as before. At the height of the growing season I have found, in some instances, certain species will put new roots even down to the drainage grit in two weeks.

When it is considered that sufficient new roots have been formed to allow potting on then proceed with Extreme Caution. When removing the cutting from the rooting medium Seramis granules will readily adhere to the roots and no attempt should be made to remove these granules because this spells certain

disaster. The granules, anyway, are a distinct



benefit. Start watering as normal after about seven to ten days and keep slightly shaded. Seramis granules can also be used in seed raising but this is untried, so far, on my part. Malcolm Birkett. Doncaster.

## SERAMIS.

I first encountered this product about six years ago when it was being sold off cheaply in a local Garden Centre. Two and a half litre packs were originally priced up at £2.99, prior to discounting, so I purchased ten packs.

Seramis comprises absorbent clay granules, particle size 3-5 mm, and it is light brown in colour when it is dry. It has the ability to absorb and store more water than its own weight, making water available at the root system as required. Seramis is a German product and was, until two years ago, distributed in the UK by Pedigree Petfoods at Melton Mowbray, Leicestershire. It has been used extensively in Germany for hydroponic plant cultivation, for which it was originally intended. The pH of the granules is between 6.2 and 7.5 although this can alter using an acid feed, for example one containing pentoxide. With the starter packs of this product a feed was included, together with a watering indicator.

I have used neat Seramis for rooting down cuttings and for growing difficult cacti and succulents with delicate root systems and have found it ideal. Because it retains its granular structure a greater amount of oxygen is available at the root. On repotting, the product can be sterilised and recycled.

Since the product distribution has been taken over by an agency it seems to be less available. As an alternative I now use Tesco's lightweight Premium Cat Litter (in a blue bag). The clay granules are smaller but equally absorbent and the deodoriser appears to be harmless to plants. A sixteen-litre bag retails about £4.50 so it is much cheaper than Seramis and the structure of the granules appears equally stable. I use this product for rooting cuttings and as an ingredient in general compost mixes.

John Betteley. Lincoln.

Seramis has a website: www.seramis.co.uk Ed.

#### A REVISION OF MAIHUENIOPSIS

In TSG Vol. 4 No. 1 March 1998 p 237 - 242 there appeared an article headed "Revision and Transfer of some Austrocylindropuntia by Dr Roberto Kiesling. (Submitted in Spanish to the Tephrocactus Study Group in the UK by Dr Roberto Kiesling with permission to publish it in our journal as a draft document)." In fact Dr Kiesling's letter stated that the paper could be translated and "circulated in your study group but please mention that it is a draft"sic. Unfortunately the Editor at the time was unaware that there are rules governing the publication of such papers and could not therefore forsee any pitfalls. Thus the intended informal circulation of a draft paper was presented in our Journal as one "with permission to publish". Dr Kiesling had already sent a copy of the draft to Mr Iliff and there was some correspondence between them. It was only this year that Mr Iliff became aware of the article in our Journal. He then wrote the article that appeared in the last issue, Vol. 7 No. 3 P461 - 463. Mr Iliff sent a copy of his article to Dr Kiesling before it was published. Dr Kiesling then sent a letter in reply to Mr Iliff and that response by Dr Kiesling to Mr Iliff forms the basis of the answer below. There are now two areas for discussion. One area is whether the concepts Dr Kiesling was considering are valid and could be developed into a final paper for publication. The other area is whether the appearance of the article in March 1998 constitutes "publication".

At this stage I would like to apologise on behalf of the TSG to Dr Kiesling for the confusion, and problems, we have caused due to the way we presented his draft to members. The problem is that some people have accepted the article in our Journal as an official publication of a revision. Dr Kiesling's answer is that "the combinations in the paper (really a draft) are not published, and the code of nomenclature is clear in this aspect: Art. 34.1: A name is not validy published 9a).... (b) when it is merely proposed in anticipation of the future acceptance... (c) ... In this case, as the translation clearly indicated (at the beginning) it is only a draft and it is not a proper publication, not a finished research".

A. Hill. Chairman and present Editor of the TSG.

## Dr R. Mottram has given his views, as printed below, on "publication".

This case is certainly unique, but the word "draft" is not so significant as the words "with permission to publish." I don't think that there is any escaping the fact that any final version published will simply repeat the validations made in the `draft', thereby confirming that Kiesling fully intended to publish the new combinations at the time that the `draft' was published. While Kiesling may wish to revise some of the text of his article, the new combinations will presumably remain intact as already published. If Kiesling writes that he no longer accepts the names already published in the `draft', then they will be invalidated, but otherwise, underthe priority rule, Art. 11.3, validation occurred in March 1998 with the publication of the `draft'.

Art. 34.1(b) was never intended to cover examples like this. It was added to the Code to cover the incidental use of names prior to publication that had insufficient data for validation. Wherever names are published with all requirements, then that is the place of publication. Prior to 1935, when there were no requirements other than a description in any language, inadvertent validations are common. To take just one example: Opuntia rhodantha first appeared in print as a provisonal name in an article describing plants that that been brought to a meeting of the German Society. The name was attributed to Schumann, who proposed to publish it later. However, since a very brief circumscription had been published with the provisional name, it is validly published in that place. Four years later, Schumann presented his full desciption as a `spec. nov.' in his book, and indeed, in the meantime another author, Rehder, had published another description and an illustration of it, but Schumann's provisional place of publication has priority. There are innumerable other examples where provisional publication had priority. Principle III of the Code puts it succinctly: "The nomenclature of a taxonomic group is based upon priority of publication". Use of the word `draft' does not alter this convention.

I note that in the Repertorium 49(1998), Eggli has accepted these new names (in bold print), then adds "English translation of a Spanish text submitted "with permission to publish it...as a draft document" and thus probably invalid under Art. 34.1b." - thus contradicting himself.

The names published in the TSG Bulletin in Kiesling's article stand as validly published at the moment, because all the conditions for valid publication of the names have been fulfilled. The appearance of the word `draft' does not constitute grounds for rejection under Art. 34.1b, in my opinion. `Permission to publish' overides any such reservation, even if the word draft is significant - which I doubt anyway.

If Kiesling now revises the article, but still accepts the new combinations, then their place of valid publication is still that in the TSG Bulletin. Acceptance of the names now means that he accepted them then - he would be just confirming what he said in the draft! Art 34.1b only deals with cases where the author does not accept the names (rather than competence of the article), as is made clear in the examples given under that Article. This is not `provisional publication' in the normally accepted sense.

If Kiesling wishes to revoke the new combinations made in the TSG Bulletin, he can still do so, by simply publishing a statement - anywhere as long as it is effective (but not in unpublished correspondence), to the effect that he does not accept the name combinations in that article. However, that also means that he will not be able to republish the name combinations elsewhere - or if he does, they won't have any priority. R. Mottram.

## Dr R. Kiesling's reply to the article by Mr J. Iliff in the last issue.

A few years ago I sent Mr Iliff the draft of my paper and later I sent a copy to Mr. Geissler who had previously written to me about *Pterocactus* and other items. He asked me if he could communicate my ideas to TSG members through the bulletin. I agreed to this mentioning that it was only an informal communication, not a real publication. I did not give this "publication", more importance than a personal letter. It was intended to be just a little more diffusion of my thoughts thus giving the opportunity to other people to comment before the paper was properly published (if it ever was).

After some letters from Mr Iliff and the announcement by Hunt that there was to be a new number about Opuntioids, where more papers could be published which might perhaps also mentioning about the affinities and differences of *Maihueniopsis* and *Cumulopuntia*, and *Austrocylindropuntia*, I stopped my work. In the mean time Stuppy kindly sent me his paper on anatomy of the seeds (also I seen his lecture at St. Louis, July 1999). This is indeed an excellent work although I do not accept all the conclusions without considering also other characters (please see below the comment on *Puna*) but it is a real deep investigation of the subject.

There are another two reasons for not yet finishing the paper. On one hand I want to do field study on some species, at least some of the *floccosa* group, and also the ones of the Chilean and Peruvian low areas which are not compact cushions, but look like *Tephrocactus*. Perhaps I will never have the opportunity to study that in the field, but

I am keeping the intention. The other reason is Mr Iliff, whilst not completely convincing me, had an influence on my thoughts and now I doubt if *Cumulopuntia* is or not a different genus and if it must be united to *Austrocylindropuntia* or not. I take Mr Iliff's opinion into complete consideration and stop my paper that was in, but "unpublished", in the Tephrocactus Study Group bulletin.

Concerning the study of Wallace. I know a couple of preliminary reports but not the final paper on Opuntioids. I have talked to him and his student at St.Louis at 1999. I understand they made only made one *Cumulopuntia* analysis with one of the Chilean species of low Northern Chile (tephro-like), but this can be a misunderstanding on my part. According to Kattermann they studied several specimens he gave to them. I don't remember the answer of Wallace's student about analysis of *Maihueniopsis* (sensu strictu).

The embryo differences observed by Hughes are important, as are also the differences in the fruit. According to Kattermann, who recently showed a couple of impressive slides at the Los Angeles US-Convention, plants of *Maihueniopsis* have a juicy "ovarium" (the seeds are immersed in a juicy mass produced by the funicles) and a thin pericarpel, but plants of *Cumulopuntia* have dry funicles at the fruit (it means the arils are in contact with each other) and its wall is thick. Next season I will try to see as many specimens as possible, of different species, to corroborate if this is general or if there are many exceptions. I have some slides of *M. glomerata* fruits and will observe in detail the pericarpel of the fruits. I am also wondering if *Cumulopuntia* and *Austrocylindropuntia* can be parts of the same genus as in some *Austrocylindropuntia* the fruit is like that of *Maihueniopsis* in this character (juicy funicles, thin wall in *A. shaferi* and *vestita*, at least, but not in *A. subulata*, the type species).

*M. nigrispina* is exceptional in several senses. I remember at 1983, when trying to define *Maihueniopsis*, several details of this species appeared different than the others (like *T. molinensis* in the *Tephrocactus*). Does Mr Iliff suggest it corresponds to another genus? I sent Stuppy *O. nigrispina* seeds, which are indeed not corresponding exactly to *Maihueniopsis* (lenticular, soft aril), or to *Cumulopuntia* (globose or pyriform, very hard).

I thank Mr Iliff for his nice comments about the importance of my paper of 1984. I know it was a pioneer research in the groups considered, and now most of the specialists are more or less following those concepts (with the differences we are talking about). Also I know it is not a final work, only an improvement. Many times I have mentioned that I consider that paper good about the generic concepts and about the systematic of Tephrocactus (1), but deficient about the specific systematic of Maihueniopsis. I knew from the beginning some limits in between the species were not completely understood by me; for instance the "T. rossianus" and "T. ferocior" but also the T. curvispinus (now re-described by Gilmer and Thomas), and perhaps other names. I remember the case of a Backeberg species (T. catacanthus) from Jujuy collected by Muhr west of Purmamarca. She said to me that it is very common there but I tried to find it without success. According to the photograph it looks a completely different species, but perhaps Muhr remembers wrongly that it was in abundance and it was in fact an abnormal specimen? Also the fact that there are other species growing in Chile and Bolivia (Peru also), stopped my possibilities to cover the entire group. I then decided to publish the 1984 paper. If I had waited to cover all the species and areas the paper would have never been published.

The paper of Gilmer and Thomas on *Tephrocactus* in Schumannia, is basically coincident with mine, except they consider *T. geometricus* and *T. halophyllus* as variations of *T. alexanderi*.

To be sincere, I never appreciate Ritter's work very much and perhaps this influenced me to underestimate his considerations on *Cumulopuntia*. I know he discovered many new species, some ones extraordinary, but I assign most of his Argentinean new species to the synonyms of previous ones. His way of thinking was not that of of a student. His conclusions were based in his own logic, thinking isolated from others and without accepting reasons or Nomenclature Codes. His book is more a permanent fitting than an exposition of reasons. It was publish as he wrote it because he did not accept any critical revision,. His types are aberrations, only fragments to compliment the required conditions, or perhaps fragments for his own use to remember the species.

Preparing my 1984 paper I suspected, and it is reflected in the text and the comparative table, that *Austrocylindropuntia* is close to some *Maihueniopsis* (read *Cumulopuntia*), but as mentioned, I want to study the intermediate species (*floccosa* group), the tephrocactus-like species, and also the type species (*A. subulata*). Last year I obtained some fruit material of *subulata* and also of *exaltata*, but the material from other species is necessary.

About *Puna*. The results of Wallace were taken by the late Anderson to combine them in *Maihueniopsis*, but according to Stuppy (in press in this moment), at least the seeds of *P. bonieae* correspond to *Tephrocactus*, and he is making the new combination in the Kew Bulletin. I don't pretend to have at all the times the reasons (I express sincerely my doubts on my concept of *Maihueniopsis*), but the several characters of *Puna* (some unknown in the subfamily), gave me the security that it is a good and different genus. When people are guided by only one character, as Wallace by DNA or Stuppy with seed anatomy, they "lose the woodland looking the tree". Stuppy kindly invite me to share the authorship of *"Tephrocactus bonnieae* nov. comb.", and my answer was to ask him to consider all the other characters before make this combination, but he is sure about his conclusions and is making the combination. In my impression anatomical characters –as all the others- can be also variable.

Mr Iliff does not dissuade me completely (images of the absolutely similar wild plants are too strong in my mind), but, accepting Mr Iliff's reasons, I do not go ahead with my proposition. As mentioned above, I accept that I should wait. R. Kiesling.

## **IMPORTANT NOTICE**

Next year we are joining Oxford Branch of the BCSS for their Open Show on the 14<sup>th</sup> July 2002. We will have twenty-one classes of Opuntia to represent all groups but mainly those of South America including Tephrocactus, Cumulopuntia, Maihueniopsis, Puna, Austrocylindropuntia, Pterocactus and Tunilla (Airampoa).

UK members will receive a copy of the schedule in good time beforehand so that you can prepare your entries. If any other member wishes to receive a copy please infprm the Editor.

This is an excellent opportunity to show off your plants! So please will you make an effort to enter in this Show and support the TSG?

Two A. floccosa growing very near each other in the Cordillera Huayhuah, Cajatambo, Tinco, close to Fig. 5 HNSO 002a. 30 cm wide cushion at 3698m Rio Pumariniri, Peru. Photos by Hakan Sonnermo. Fig. 6 HNSO 002b at 3700m

