

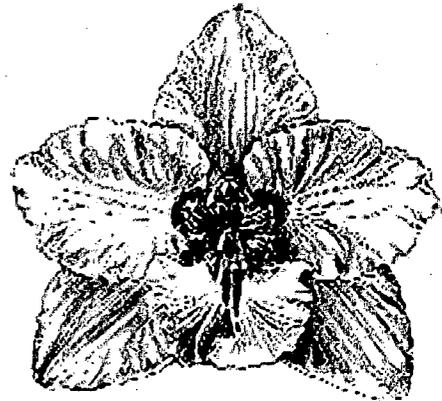
Odontoglossum Alliance Newsletter

Volume 6

November 2013

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ODONTOGLOSSUM SANGUINEUM AND ITS SPLIT PERSONALITY by Stig Dalstrom

There have been some discussions about what *Odontoglossum sanguineum* really looks like and whether there may exist one or more similar species, either under a different name or as undescribed species. In order to learn what the original type collection looks like we have to analyze the type specimen collected by Warscewicz in May 1853, in northern Peru, at the “sources of the Marañón” (at Kew, **Fig. 1**). There is an enigmatic label attached to the herbarium sheet with the following text: “22 *Anachaste sanguinea* (Lindley), a new genus, apparently related to *Cochlioda*, with rich blood-red flowers, like that of a *Comparettia falcata* in size (see drawing No. 16),”... There is also a pencil drawing on this sheet, which looks like it was made by Lindley. A colored drawing with “No. 16”, referring to Peru and Marañón, can be found in Vienna (W45492, **Fig. 2**). I believe this is the drawing that is referred to as “No. 16” on the type specimen. In addition, a collection by Spruce (No. 6004) from “sylvis Yalancay” (forest of ‘Yalancay’), Chanchán [which is near Alausí in central Ecuador], and dated 1859, is also attached to the same sheet as the type: Both of these collections show a plant with a rather short and basically unbranched raceme (there is a short basal branch on the type).

Reichenbach described *Mesospinidium sanguineum* based on the Warscewicz “Marañón” collection (Warsc. 16) in *Ann. Bot. Syst.* 6: 858. 1861 (**Fig. 3**).

Cogniaux described *Cochlioda stricta* in 1897, based on a cultivated specimen he saw in Liege. The plant had been collected by Lehmann and shipped to Europe together with plants of *Odontoglossum cirrhosum*. Cogniaux mentions Colombia as the origin of the plant, but since Lehmann collected lots of *cirrhosum* in Ecuador, much to the consternation of the Klavoch brothers, I assume that Ecuador is the true country of origin. There is also a dried specimen at Kew of a “*Cochlioda*” from El Cisne in southern Ecuador (province of Loja), by Lehmann (No. 6904, **Fig. 4**), from 1876. Lehmann apparently changed the identification later and added a label with “*M. sanguineum*” mounted on the original label. This specimen has a branched panicle.

I have not seen any specimen of the type of "*Cochlioda stricta*", only a poor drawing, which is in the AMES herbarium labeled "*Symphyglossum strictum*" (Fig. 5). It is unknown to me whether this is a copy of a type specimen or a copy of a drawing of the plant that Cogniaux saw in Liege. The drawing looks like it was made by Garay. There is also a copy of Lindley's (?) drawing of the type of *Mesospinidium sanguineum* on the same sheet (AMES 38543). Very little can be concluded based on this drawing of "*Symphyglossum strictum*" other than it looks like a small *sanguineum*.

Symphyglossum ecuadorensis was described by Dodson & Garay in *Icones Plantarum Tropicarum* 3, plate 339. 1980 (Fig. 6). The text says: "Similar to *S. sanguineum* (Rchb.f.) Schltr. but distinguished by the elongate, branched inflorescences, apex of the lip recurved and the large, squarish, paired lamellae on the disc of the lip."

When we look at the illustration of this holotype (Fig. 7), we see that the inflorescence is rather short and simple (a raceme), and not at all branched like it says in the description. The petals are very narrow and the flower has an open posture, which does not look like a typical *sanguineum*. The drawing does not look like it has been prepared from the type specimen at all. After I compared the drawing with a flower from the actual type specimen, this has been confirmed. The flowers on the type specimen are much more like the type of *sanguineum* (Warsc. 16). The lip is curved in both taxa and they both share the large pair of lamellae on the lip. The flower of the type of *S. ecuadorensis* is virtually identical with Guido's photo of "*Odontoglossum sanguineum* 'Rony' (Fig. 8), and also of my drawing SD 69 (Fig. 9). I actually concluded a long time ago that *Symphyglossum ecuadorensis* and *Mesospinidium* (*Odontoglossum*, based on DNA) *sanguineum* are the same species. I asked Cal Dodson once why they had described this *ecuadorensis*, and the answer was basically that "Garay thought they looked different" (I have forgotten the exact sentence). Well, they don't!

To add some information about natural variability, there is a collection by Asplund (No. 18130, Fig. 10), which comes from the Loja area, and shows two plants. One has a simple raceme, much the same as the type of *sanguineum* (and drawing of *ecuadorensis*), and the other has two branched inflorescences, much as in the description of *ecuadorensis*. Both inflorescences show flowers with a curved lip. This Asplund collection was identified by Garay as *Symphyglossum sanguineum*.

So we can conclude that the difference between a short and simple (and likely to be more or less erect) raceme, versus a larger and branched panicle (which is likely to be pendent since the flowers are many and the spike is so thin) means nothing from a taxonomic point of view.

Let's look at the flowers then!

If we begin using the flowers on Guido's excellent composition (Fig. 8) as a typical representation of *Odontoglossum* ("*Mesospinidium*, *Symphyglossum*") *sanguineum* then we can see that the pale rose-colored lip is curved 'halfway'. This corresponds well with what we can see on the various herbarium specimens cited above. Then we look at the front-view photo that Steve Beckendorf sent (Fig. 11), we can see a flower that looks a little different. It seems that the lip is more projected forward than of the typical *sanguineum* lip. But if we look at the side-view of the same flower (Fig. 12) we can see that the lip is actually slightly curved. If we then look at the flowers on Fig. 13, we can see that the curviness varies, from the left flower being more curved, to the right flower, which corresponds well with the flower on Fig. 12. The inflorescence of the flowers on Fig. 13 is paniculate (Fig. 14). The appearance of the lip depends on the viewing angle!

If we then look at the inflorescence on Fig. 15, which was taken in the field by Andreas Kay, we see a short and rather erect looking spike. But the plant carrying these spikes is very small and therefore is likely to produce short and racemose spikes. The flowers (Fig. 16) do not differ from any of the above samples of *sanguineum*. Most of you know that an *Odontoglossum* plant can produce a simple raceme one year and a heavily branched panicle the following year, if the health of the plant has improved.

Finally, if we look at the runt-like flowers on the photo from Lorenz Grubler (originating from Alex Hirtz, Fig. 17), they certainly look different. But why? I assume that the spike was photographed in Alex Hirtz' greenhouse (judging from the diffuse background), or perhaps from plants brought back to and cultivated in South Africa, which is less likely. Alex

Hirtz has very dark greenhouses, at about 3000 meters elevation in dry Quito air. Funny things happen there sometimes and ‘new’ species have been described because of that. *Dracula marsupialis*, as an example, is a rather warm-loving plant with a normal *Dracula*-shaped lip. In Alex’ greenhouse, however, the lip of the type plant did not develop fully, probably due to stress, and therefore looked ‘marsupiate’, hence the name. Growing *Odontoglossum* plants in a very dark and crowded environment is likely to produce less-than-perfect flowers. They are likely to be of poor color and inferior shape, just like the ones on Grubler’s photo. This is not unique for *sanguineum*.

Some also have been wondering where this strange looking species actually belongs. The flowers certainly look different from most other odontoglossums. But do they really? The color is shared by a couple of former *Cochlioda* species (= *Odontoglossum roseum* and *vulcanicum*), and the shape is very similar to another quite unexpected species. But let’s look at the DNA cladogram first (Fig. 18). We can see that “*Oncidium strictum*” is a sister species to what is labeled “*Oncidium praestanoides*” (= *Odontoglossum praestans*), which I find highly suspicious. I have not found any voucher material for this sample and doubt that it is correctly identified and/or sequenced. In either case, the sample does not seem to exist anymore and is irrelevant. What’s important though is that this little clade is sister to the clade that consists of *Odontoglossum velleum* and *O. wyattianum*. And these two clades combined are sisters to the *harryanum* clade.

If we compare the flower morphology of *sanguineum* (Fig. 8) with *Odontoglossum velleum* on Fig. 19, we can see some interesting similarities (ignore the color!) in the short and straight, wingless column. Also compare the drawing of *sanguineum* (Fig. 9) with those of *velleum* (Figs. 20 and 21) and you will see some astonishing similarities, particularly in the column shape. But not only that, the two *velleum* samples are also slightly different when compared to each other. The one on Fig. 20 has rather narrow sepals and petals, and the lateral sepals are almost split to the base (similar to Grubler’s *sanguineum*). The flower on Fig. 21, on the other hand, has much broader sepals and petals and the lateral sepals are fused for almost half their length (similar to a healthy *sanguineum*). You can also compare the posture of the two *velleum* flowers on Figs. 19 and 22.

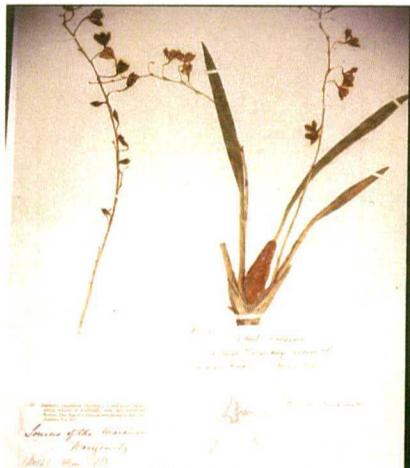


Figure 1

Odm Sanguineum WARC-16-T-K

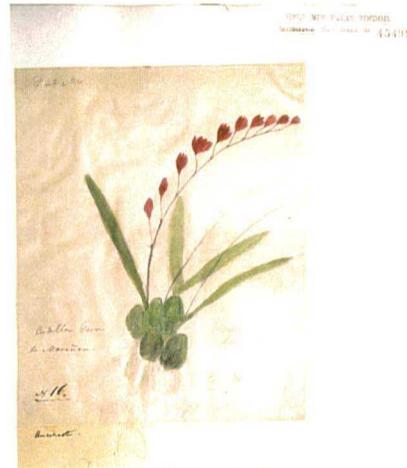


Figure 2

Odm Sanguineum WARC

5. *MESOSPININIDIUM SANGUI NEUM* Rehb, fl. Mss.:

Pseudobulbo ovali compresso, diphylo, folio uno stipante, foliis omnibus cuneato ligulatis acutis, racemo secundo (?semper), bracteis squamaeformibus minutis, sepalis oblongis acutis, lateralibus medium versus bifidis, tepalis cuneato ovato acutis, labello ligulato acuto, lateralibus erectis, carina lineari per disci basin superiorem apice, sc. medio labello divergenti bicruri, androclinio minute lobulato.

Pseudobulbi viriduli punctis atropureis. Pedunculus et ovaria pedicellata violacea. Perigonia sanguinea, illis *Rodrigueziae* secundae aequalia.

Peru: Marañon Warscewicz! Quito, Jamieson!

Figure 3 Odm Sanguineum Ann.Bot.Sys6;858.1



Figure 4
Odm Sanguineum Lehm-6904-K

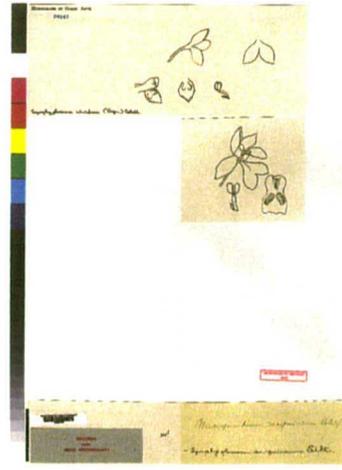


Figure 5
Symphglossum strictum 5944972

SYMPHOGLOSSUM ECUADORENSE Dodson & Garay

Symphoglossum ecuadorensis Dodson & Garay, sp. nov.

Species haec *S. sanguineo* (Rchb. f.) Schltr. similis sed inflorescentia elongata ramosa et lamellis grandibus quadratis labelli recurvati differt.

Epiphytic. Rhizome short; pseudobulbs tightly clustered, ovoid, compressed, gray-green, to 7 x 4 cm, bifoliate at the apex, surrounded at the base by several pairs of distichous, imbricating sheaths with the uppermost developed into leaves. Leaf narrowly elliptic, acute, tapered into a conduplicate petiole-like base, to 20 x 2 cm. Inflorescence from the base of the pseudobulb in the axil of a foliaceous sheath, nodding, branched, to 1.20 m long; flowers produced in succession with several open at a time; floral bracts small, triangular, to 4 mm long. Flowers pink with a lighter colored lip; dorsal sepal broadly elliptic, concave, rounded at the apex, minutely apiculate, erect, to 15 x 7 mm; lateral sepals connate to the middle or above, obovate, concave, acute, spreading, petals elliptical, adnate to the column at the base, erect, hooding the lip and column, to 15 x 9 mm; lip fleshy, shorter than the sepals and petals, adnate to the column down the middle, the margins free, apex ovate, entire, deflexed; disc with a pair of quadrate lamellae under the anther; column dilated upward to form triangular wings near the apex, to 6 mm long; pollinia 2, hard.

Epifítica; rizoma corto. Pseudobulbos aplanados, bifoliados en el ápice, rodeados por hojas angostas bien desarrolladas. Inflorescencia desde la base de los pseudobulbos, ramificada, hasta 1 metro de largo. Flores color rosado; labio apretado contra la columna, recurvado en el ápice.

DISTRIBUTION: Western Ecuador and northwestern Peru.

TYPE: ECUADOR: AZUAY: Between Asunción and San Fernando on road Cuenca-Pasaje, alt. 1200 m, 30 Nov. 1987, Dodson 342 (SEL).

OBSERVATIONS: Similar to *S. sanguineum* (Rchb. f.) Schltr. but distinguished by the elongate, branched inflorescences, apex of the lip recurved and the large, squarish, paired lamellae on the disc of the lip. The illustration in the Botanical Magazine, table 5627, is of this species.

HABITAT: Epiphyte in seasonally dry cloud forest.

FLOWERING SEASON: July to December.

ILLUSTRATION VOUCHER: Dodson 342.

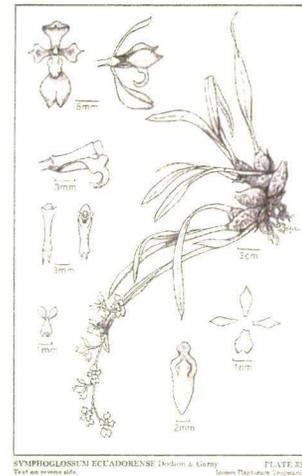


Figure 7
ecoadorensis-D342-T-SEL-b

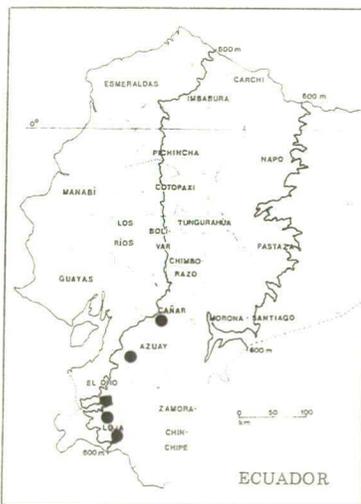


Figure 6



Figure 8
Odontoglossum Sanguineum-Rony

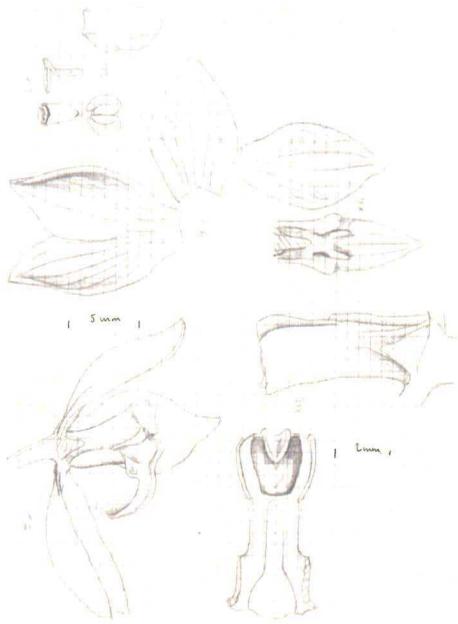


Figure 9
sanguineum-SD69-b



Figure 10
sanguineum-Asplund18130-K



Figure 11
IMG_5993

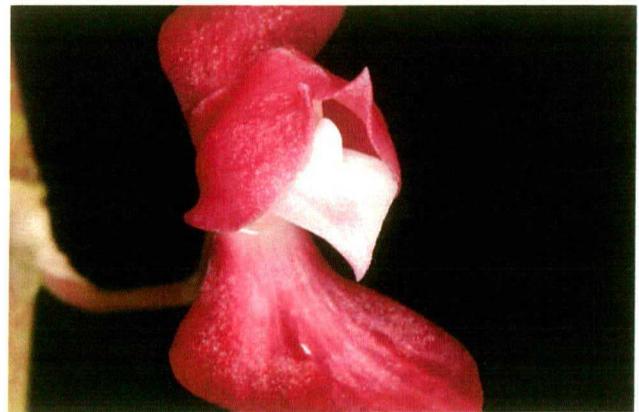


Figure 12
IMG_5998



Figure 13
sanguineum-3-a



Figure 14
sanguineum



Figure 15
Odontoglossum strictum AndreasKay



Figure 16
Odontoglossum strictum Kay2



Figure 17
sanguineum Grubler

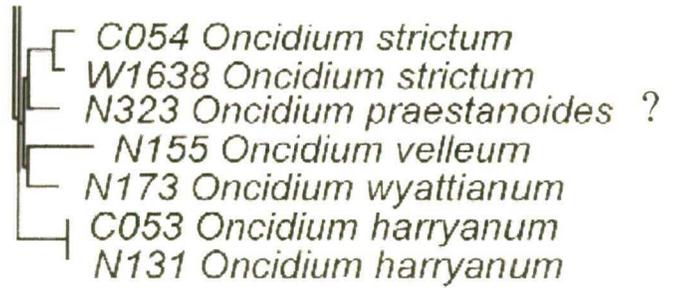


Figure 18
sanguineum clade

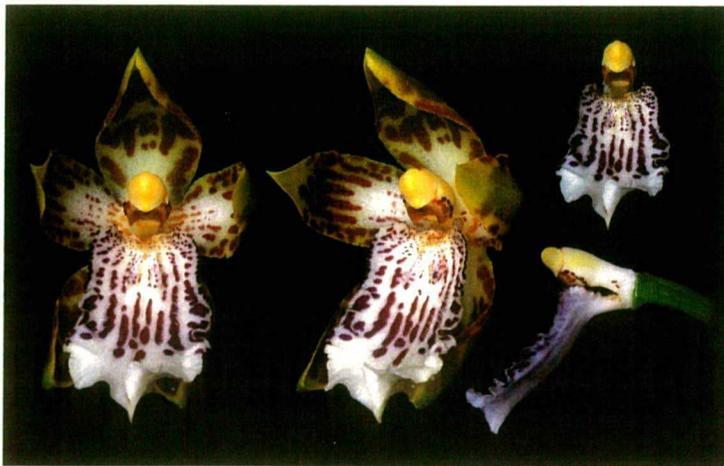


Figure 19
Odontoglossum vellum



Figure 20
vellum SD608-b

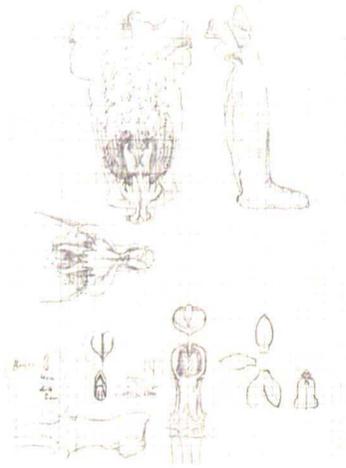


Figure 21
vellum-SD 2048



Figure 22
vellum

Report on Membership

We mailed out 71 newsletters in August 2013. This mailing will be 65 newsletters because 6 members have yet to pay their dues for 2013-2014. For those people I will send out a letter informing them that we are sorry to lose them as members and hope they will have a change of heart and continue with us. I recall that the largest number of newsletters I ever mailed out was about 125. This included a number of libraries and organizations that were not paying dues. Now I only send out the newsletter to those that have paid. There are no more free ones. When you think of the membership in the AOS which has shrunk from its highest number and today where they are only 25% of the number, we are doing pretty well. However our numbers are very small. We need to encourage our orchid growing friends to take up the *Odontoglossum* Alliance plants. If any member has any suggestions on how we can encourage this and membership, please let me know.

John E. Miller

Jemiller49@aol.com

SCATTERED THOUGHTS.....

By Andy Easton

I used to write a column by this name many years ago. Maybe time to revive the concept, certainly the scattering is now more pronounced!

One of the major problems holding back interest in the Odontoglossum Alliance is the woeful approach to their evaluation by American Orchid Society “judges”. Incompetence would be one of the milder terms that could be applied to their efforts of late. What is the concomitant result? Discouragement among the shrinking number of exhibitors and fewer plants submitted for judging.

In August, a plant now called *Cyrtochilum Juliann* ‘Colombo’ was exhibited in Medellin, Colombia and judged by AOS judges. There was but one team, of five credentialed judges. I have seen the plant on a prior bloom and have discussed the award with one of the team involved. The plant, which I would evaluate in the high AM range, scored a pathetic 79 point HCC/AOS. Now both the judges I know who were on the team are very knowledgeable about *Cyrtochilums*, one would be probably the pre-eminent *Cyrtochilum* expert in the United States. I was told both scored the plant strongly in the AM point range. Now, mathematically, I have a little problem with this..... if scores are within a six point spread and say the two experts gave the plant in question 83 points, the other three would have all had to score 77 to preclude an Award of Merit. But one must ask, why would three Florida area judges not listen to the more informed comments of the California-based judges and indeed, why would the Californians not try to explain to the Floridians that they were making a bad mistake?

If at times I seem despondent about the level of knowledge for and the interest in, the *Odontoglossum*, then indeed let me confirm I am! Worldwide, this beautiful and challenging group seem to have been passed by and generally under-appreciated. When you think that many orchid areas like Cape Town, the New South Wales and Victorian coast, New Zealand, coastal California, Oregon and Washington, not to mention Colombia and Ecuador, provide simple, even optimal environments for *Odontoglossum* Alliance culture, the present situation is disappointing to the point of almost being depressing.

But let’s look at a few lines of breeding and see if we can learn and maybe become enthused about new avenues in this amazingly diverse group.

Well Bob isn’t down for long! The same day the *Caucopsis* opened, this little cutie also showed its face. Blooming on a tiny single-bulbed plant it carried three perky flowers. *Odtna Moliere* ‘Etoile Polaire’ is a fine old Vacherot & Lecoufle breeder that makes beautiful *Vuylstekearas*. The future for potted blooming plant sales in the *Odontoglossum* Alliance lies with compact, floriferous plants that will finish at a saleable size in around 24 months. Only then will the type be competitive with genera like *Phalaenopsis* and nobile type *Dendrobiums*. In every crossing to date with *Oda Prince Vultan*, we see fast growers, precocious flowering and an overall ease of growth that greatly pleases. Some plants sent to Keith Andrew in England have bloomed spectacularly and he too is testing the waters with some PV hybridizing.



**Odtna Moliere X
Oda Prince Vultan**

From a Hamilton remake of a few years ago. I notice it differs in many aspects from some of the supposed older forms of Bradshawiae around. Maybe some of the English got their wires crossed in days gone by? Bob of course treated the seed so he has both tetraploids and diploids. I put up a diploid here to remind members it is still a very handsome flower and also that you need quality diploids when you are working in a group where new diploid forms of species appear regularly. The new arrivals can be used to make diploid embryos that can then be treated with oryzalin to give a very useful mix of tetraploids and diploids. This primary is quite easy to grow although when I use the tetraploid form in hybridizing, I have found it is a shy parent. Hopefully the diploids will be OK

Oda. Bradshawiae.



Oda Devossiana 'Gerardus'

This plant, bred by Howard Liebman, is everything a Vuylsteckeara should be: showy, floriferous and fertile! From a very serendipitous crossing of a modern Oda Harrods Forever X a modern Miltoniopsis, Leo Holguin. It's been a long time since Cambria and this is a new color and a plant more influenced by the Mtps than the Oda. I have until this year only had a small piece so one crossing per blooming is all I would dare attempt. But this year the plant is strong so I'm trying three pollinations, one of which is with Oda Prince Vultan 4n.... naturally!

Gerardus Staal can always be relied upon to be breeding something interesting. This is one of his seedlings of Oda Devossiana blooming for the first time. What the RHS "Taxidiots" may be calling it now is of little matter. I find it interesting to see how the relative pelargonidin/delphinidin levels will influence the coloring of this primary hybrid. Some are quite purplish red whereas others tend more to the caramel shades. In the Hawk Hill collection, there is a fine tetraploid form of edwardii and I am starting to experiment with this species again. We recently sleuthed out the real parentage of the orchid known (and awarded by AOS) as Oda Chanticleer 'Lyoth Garnet' and it is almost certainly an edwardii hybrid from Stuart Low all those years ago. There are **SOME** beautiful species in the genus now called *Cyrtorchilum* and some interest in its hybrids of late. Many of them grow like weeds and if you are willing to take the time to train their unruly inflorescences, they can create quite a show.



Vuyls Scott Binder 'Super'

If you think I like these alba whites, you're right! This a Hamilton sib-crossing that has plenty of vigor and excellent flower substance, unlike many of the alba Odont. lines. We are using a diploid hybrid of (Onc Illustre X Odm Hallio-crispum album) with a range of things like Victoria Village to give an alba percentage that is strong-growing and useful for both cut flowers and blooming potted plants. Some see the decline of traditional marriages as affecting the wedding flower market. I'm more inclined to see the arrival of gay weddings as a potentially large growth area. It's not easy to survive in the orchid business, one has to be open to all selling opportunities!

Oda Victoria Village 'Bob's Sib'



Well, just to prove that even Bob Hamilton doesn't always hit the jackpot, here is one of his recent efforts that hopefully will continue on its journey to the dump pile! We may register it as *Caucopsis Yap Yap* or *Caucopsis Squogre* (the flowers are ugly enough for either name!) just to give a warning to others. *Caucaea* is an interesting miniature genus in the Alliance that has some affinity to *Miltoniopsis*. Bob and I discussed a possible hybrid and he went ahead. There were few plants and they were slow growers, requiring some extra work in the flask. Sum total of the plants were enough for one medium-sized compot which Bob passed on to me this past Spring. I planned to pot them individually until I noticed the strongest plant of the group had a small spike. It took forever to open and last week finally it unfolded. Ergh! Dull, unattractively conformed and upon close inspection, the whole cross has heavy and possible aneuploid foliage. Oh well, many promising pathways end up at insurmountable ravines!

More on Orchyata Bark by Robert Hamilton

Recently, I received e-mail from Russ Vernon asking me to update my experience with Orchyata bark. Russ heard from a prominent Southern California grower his collection had suffered root damage when grown in Orchyata. The bark's pH had dipped too low to sustain roots. In the appended note I'll share my experience which does not corroborate this.

My partner John Leathers and I began growing in Orchyata after we were invited to share the cost of a shipment with Golden Gate Orchids. This was a generous offer and an act of kindness and good will on the part of Golden Gate. At Hawk Hill John grows *masdevallias* and I grow odonts. Our initial order was for 100 bags of bark. We chose bags of two sizes, half the order was medium and half was fine (Orchyata has a confusing and bizarre way of branding bark size which even today I cannot fathom). We had heard from growers we respect, namely Andy Easton of New Horizon Orchids, Kevin Hipkins of Royale Orchids, Australia and other "Anzac" growers the product was excellent. I am guessing that first order was about four years ago. The reason for turning to this offshore product was simple. The bark we were buying, which came from a producer in Oregon, was as best "iffy" in quality - poor is probably the better word.

Some shipments were fair while others were poorly graded. The shape of the bark was not conducive to keeping open space in the mix. Our plants were suffering. This fir bark was lasting less than a season. While on the roof one day I noted a dead-zone in the area I was dumping spent bark. The situation had gotten bad enough that we got in our car and drove to Redding, on the Northern California border, to inspect the bark of a different producer, a distance of 211 miles. While better, we were not impressed.

We initially used Orchiata straight out of the bag with no amendments. Because the cost of this premium bark was higher than local products we began amending the bark with lava-rock and charcoal, eventually abandoning charcoal as messy and useless. Initially, no buffer was added (a buffer is typically some compound of calcium that dissolves in proportion to the acidity of a substrate thus keeping the pH somewhat constant. Dolomitic limestone and oyster shell are two such products).

Our immediate effect of changing to Orchiata bark was dramatic – excellent plant and root growth. We can also share experience with the lifetime as this product. I am ashamed to admit there are still a few plants that remain potted in mix from that initial trial. Note, this four-year-old bark; it has begun to breakdown which is something you'd expect from a mix this old. It is holding more moisture than it should and the tops of the pots are showing algae and moss growth – an indicator the bark has exceeded its lifetime.

In response to Russ' inquiry I performed a "soil paste extract", a standardized protocol for measuring substrate pH, on my oldest mix to see if there is a pH issue. Indeed, there is. This old bark measures a pH of ~4 which is quite acid - too low for good growth. Some roots in this old media have suffered. Having said this, four years is an outrageously long time for an orchid to remain potted in a mix. This is a testament to my laziness.

As experience with Orchiata bark progressed I began amending it by adding oyster shell as a buffer. Living on the California coast this is an easy material to get. It is sold by agricultural suppliers and in pet stores for bird owners and tropical fish raisers. With time, after inspecting excellent growing done in our greenhouse which is shared with Tim Brydon, I followed Tim's lead and began adding red lava-rock, about the same size as the bark, to the mix. Thus, I now had a buffered mix more likely to maintain a stable pH and the lava-rock, being cheaper than the bark, is a cost savings. Up to 50% does not seem to interfere with excellent growth.

I am currently repotting my collection en masse. The current bark/lava-rock mix is holding up very well. I should say excellent. The roots look great (having said this, there are some odonts that, probably because of their genetics, never hold good roots). There is nothing that makes motivates a change from this excellent mix. We've just purchased more Orchiata. We'll continue to add a calcium buffer as well as lava-rock as the Lava-rock filler as it is cheap and, as an added benefit, lava-rock adds weight to the pots.

So, what's up with the warning Russ received from the Southern part of my state? I can't say. Was it "el toro poo-poo"? There is fact in the issue that aging bark substrates experience a pH shift toward acid. This holds true no matter what source bark is used. From my experience Orchiata is by far the most stable and best bark I have used. Is four years a long time to keep an orchid in a mix? Yup, four years is too long.

Having said these things I cannot account for another grower's culture. Incorporating a calcium buffer will extend the lifetime of a mix. I suspect such buffers should even be applied annually but do not have experience with this (perhaps a grower with more experience can elaborate)? Lava-rock is cutting costs and keeping the pots more stable because of its weight. It does not seem to hurt anything at 50% volume by volume.

In noting my continued endorsements of Orchiata please bear in mind we've received no considerations from this firm. We do not resell bark or for that matter orchid sundries. I remain an advocate for Orchiata bark. Try it, you'll like it!

Bob Hamilton 9/13



An orchid plant in Orchiata bark after two (2) years in the same medium.

Introduction to Henry Wallbrunn's Article which Follows by Robert Hamilton

“Never theorize before you have data. Invariably, you end up twisting facts to suit theories, instead of theories to suit facts.” - Sherlock Holmes [to Watson]

Professor Henry M. Wallbrunn's paper, The Art of and Science of Orchid Hybridizing was given to me a couple of decades ago by Alek Koomanoff. Alek is one of the best informed and intelligent odont growers I know. He's gifted with a near-photographic memory.

After my initial read of Wallbrunn's paper I sought out Steve Beckendorf and Don Wimber, like Wallbrunn respected Professors of genetics. From Don and Steve I gained a better understanding of the implications of this paper.

I had begun an ardent breeding program of odonts. It began as a “roll of the dice”. Eventually I developed protocols to change the ploidy of my crosses and from this I have learned a lot.

Most readers will probably not read this paper. Many will find it confusing, particularly on a first-read. I did. With some help with the lingo and a couple of re-reads and it now makes perfect sense. I longer consider myself a gambler. I've learned to “count the cards”.

Sincerely,

Bob Hamilton

The Art and Science of Orchid Hybridizing

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At the outset I feel that the distinction that I make between art and science should be clarified. It is sometimes said that the practice of medicine in the past was an art and that today, in part at least, it is a science. What we mean is that at first it was largely intuitive but it is now based on a reasoned way of proceeding. The important connection between the two is that after a successful intuitive approach to a problem which we call an artistic solution, careful study often reveals a logical reason for the result. This has become science; what is more, similar problems and their solution are no longer designated art because they cannot be said to have been solved intuitively.

A trend in orchid breeding is the unrestrained attempts to produce larger and larger flowers with wider and wider segments. It can be argued that closing the gap between a petal and the dorsal sepal is aesthetically pleasing and a laudable goal. But it can just as easily be argued that when the 2 petals overlap one another with the consequent disappearance of the dorsal sepal, the cause of beauty has not been served. This, however, is unusual and difficult to produce. Everyone following trends in orchid breeding knows that these monsters are more likely to win awards and therefore bring higher prices than their more beautiful ancestors or siblings. Consequently the hybridizer's goal is to produce still more monstrous and therefore commercially rewarding crosses.

In science we distinguish between goal-oriented or practical research and what we call basic research. A breeding program initiated with a particular end in view is an example of practical research. This may result in the very achievement that was desired and it often is a fairly direct and relatively short pathway. On the other hand, the goal may be unattainable with any rational breeding program and in this case a long and arduous pathway leads only to frustration or self-deception. The case I have in mind is that of trying to produce blue cattleyas.

I suppose that the blue one would want is of the nature of a *Zygopetalum* or *Rhynchostylis coelestis* lip color. Some who have embarked on this quest point to the grey-blue flowers that have been produced and proclaim success or at least partial success. Above I referred to this as self-deception. Others have been realistic and called it a frustrating failure.

It may be that the clear blue that is desired is unattainable in the genera *Cattleya*, *Laelia* and *Sophranitis* or any combinations thereof without one or perhaps several new mutations. Now, some mutations are certainly not impossible, but no long-term breeding program should be based on a mutation that has not yet occurred.

Mutations are not uncommon since each cell has many genes so that perhaps 5 or 10% of the cells will have its own new mutation. But we are looking for a particular change at a particular locus or maybe worse, several mutations at several specific loci. In this situation the probability is that of finding the proverbial needle in the haystack.

The genus *Epidendrum* including *Encyclia* is so large that I cannot say for certain that the true blue is not to be found therein. (Perhaps *E. schumanianum* will do.) If the blue is found and can be used to make an F₁, with a large *Cattleya* or *Brassocattleya* there may be a methodical way of achieving the desired result although it would take several more generations. Because of anticipated sterility of the F₁, shortly after the F₁ seeds have produced protocorms, they should be treated with colchicine to double the chromosomes, thereby assuring pairing partners for a normal meiosis. The generations that follow, although time consuming, are fairly obvious.

With the new technique of genetic engineering or somatic cell hybridization, genes or sections of chromosomes from an unrelated species or hybrid such as a *Zygopetalum* or *Ascocenda* might be incorporated into a *Cattleya* but that is something for the future. Even that might not give a clear blue unless certain genes already in the *Cattleya* are eliminated. Obviously prior to any such attempt, careful analyses of the pigments, pigment precursors, inhibitors, and pH of variously colored forms must be obtained so that we know what we have, what we need, and how we change from one to the other.

The advantage of basic research, as compared with applied, is that it has greater potential because it leads in unexpected or serendipitous directions besides acting as a basis for applied research. Examples in orchid hybridizing of this dichotomy are given by deciding to produce a large yellow *Phalaenopsis* rather than making a cross of small-flowered, highly colored species to other small or large forms, then inbreeding to find out what will develop and letting the results give both information and determine the new direction or directions for the next round of hybridizing.

The inbreeding of the first generation can be back-crossing or producing an F₂. Both are regularly used in most genetic research and even in practical plant and animal breeding but they have been largely avoided by orchid breeders (Lenz & Wimber, 1959). Often the reason for eschewing the techniques that have been invaluable elsewhere is that inbreeding leads to inferior forms or a depression in vigor. Undoubtedly individual seedlings may be poor but in the long run for every inferior specimen there is one that is superior. If one

is content with a large number of mediocre or intermediate progeny then one should refrain from inbreeding, but if one is willing to discard a number of undesirable specimens in order to obtain some which are much better and with new complexes of traits unavailable by any other means, then self the F_1 and cross one's fingers.

Actually a distinction should be made at this time between a simple back-cross of the F_1 to one of the parent species or varieties and the true inbreeding of the F_1 to a parent clone. The same is true of selling an F_1 rather than sib-mating although the latter may be all that is available if self-sterility turns up. If we are merely trying to recover some trait such as alba that is lost in the F_1 crossing back to any of the parent strain should be enough to accomplish this. What is more, any inbreeding depression in vigor will, in general, be avoided. But we may unfortunately be throwing away some potentially desirable traits that have never been given a chance to make themselves known because they are dependent upon homozygosity of recessive alleles that have mutated within the last few generations and have not yet spread in the population. In order to rake advantage of this potential that is hidden away in recessives, actual selling or crossing back to the parent clone is necessary. Of course, the F_2 rather than a back-cross gives a chance for making recessives from both parents become homozygous; it does not, however, increase the probability of finding new homozygotes. This is due to the fact that any particular new recessive in either parent has a probability of only one half of being in the F_1 . All of this discussion is based on diploids. If tetraploids are involved, the problem becomes considerably more complicated not just because of more genes that could have mutated and more ways of segregating but also different degrees of dominance with various doses of alleles.

Before proceeding to other aspects of inbreeding, however, some evidence should be presented to show that selling or back-crossing generation after generation does not necessarily lead to depression of vigor or deterioration of desirable traits. The long continued inbreeding of *Phal. sanderana* started off by Burgeff is a famous example of long continued upgrading leading eventually to superior clones, at least one of which in the seventh generation received an F.C.C. in *Cymbidiums* because of the very few tetraploids in the early days, *C. Alexanderi* 'Westonbirt' F.C.C. was both selfed and used for back-crossing over and over. In both cases flowers far superior to the original developed. Also, *P. Hellas* 'Westonbirt' F.C.C. was selfed and this produced better clones than the awarded parent.

There have been innumerable cases in which flowers with different desirable traits are crossed where the F_1 , turns out to have neither of those for which the parent were chosen, and it is summarily discarded. This is hybridizing entirely according to phenotype without regard for the genotype and its potential. Certainly an F_2 should be made if the original goal was worthy of raising an F_1 . So that this will not seem pure fantasy let me give a recent concrete example although in this case the desired trait was the same in the 2 parent species. *Paphiopedilum fairieanum* 'alba' was crossed to *P. bellatulum* 'alba' in the expectation of producing a very desirable 'alba' *P. Iona*. Since a typical colored form of *Iona* developed instead this was considered a failure and the end of the breeding program. From the above information it is obvious that one of the parents was a C alba and the other an R alba. (Wallbrunn, submitted for publication). This means that *P. Iona* was doubly heterozygous since there is little doubt that the parents and F_1 were diploids. Inbreeding the F_1 I should have given an F_2 of which 7/16 or almost 1/2 would be expected to be alba. Of course the forms of the second generation *P. Iona* would be quite variable but that gives more choices which I assume is a desirable situation. If the F_1 had been back-crossed to either alba parent, one half of the progeny should have no anthocyanin. The F_1 was never used for either type of inbreeding. The same type of missed opportunity occurred some years ago when *Laelia purpurata* 'Werkauseri' was used, in the hopes of obtaining blue-purple labella in the *Cattleya* hybrids.

Sometimes the biochemistries that lead to the 2 desirable traits in the two parent forms are mutually exclusive so that the combination of traits in the hybrid is not possible. From such a commercial failure we have at least learned something about their breeding characteristics that may be useful for future work. Sometimes, however, the very combination that was originally being sought and not found in the first generation turns up in some small fraction in the second. How small the fraction is depends upon the following:—the number of segregating allele pairs or sets of 4 in tetraploids; whether or not loci are linked, and if linked, the distance apart on the chromosome; the amount of synapsis if the parent species' chromosomes have diverged during evolution; the ploidy, and if polyploid the distance of the loci from the centromere; too many variables to be determined by one datum i.e. the size of the fraction. But along with other information such as a chromosome count this can be whittled away.

Let us return to those early attempts to produce a large round yellow *Phalaenopsis*. One parent had to be a large, well-formed white, a tetraploid. The larger the white the better since it had to be mated to a small species and this invariably drastically decreases the size of the offspring. The first 2 small species used were *P. mannii* and a yellow *P. lueddemanniana*.

The yellows obtained from *P. mannii* were hailed as a great achievement but by today's standards they are poor in color and shape. Worse yet, they held little hope for further development, being triploids with almost complete sterility. With the recent development of a polyploid *P. mannii* through the use of colchicine, the sterility problem should be overcome. I now have seedlings of a tetraploid white crossed to the polyploid *P. mannii*. How this will affect color and segment shape will be most interesting. Usually as the ploidy increases so does the width of petals. But in this case we are adding a set of chromosomes that make for very narrow petals.

The polyploid *P. mannii*, whose chromosomes have not been counted for fear of losing the plant upon cutting the few good roots, has

a very fine form as compared with a normal diploid *P. mannii*. This, however, is very different from a comparison of it with the overlapping petals of the whites now available.

If the polyploid *P. mannii* is a tetraploid and if the width of petals of the hybrids follows the rules of calculating quantitative traits by using the geometric mean of the parents' measurements (Mehlquist 1946, 1947), this F₁ tetraploid will actually have a somewhat poorer form than the original triploid *P. mannii* hybrids but the sterility problem will have been overcome.

If we are merely trying to get a fine yellow color then the early attempts ought to be forgotten since by using *P. fasciata* that goal has been achieved a few times. There are, however, various yellows from other species, each somewhat different, and with other traits that make them all worth using, although not always with large whites for the other parent.

Some forms of *P. cornu-cervi* have excellent color but it is with other members of the *Stauroglottis* subgenus that this is best utilized. *P. venosa*, *P. fuscata*, *P. cochlearis*, *P. amboinensis* all have potential but once again the choice of the second parent is very important if one is not to conclude that these are no value (Compare with Moir & Moir 1980 p. 49). One must not believe that traits, even if they seem to come from one parent, are not also dependent on what the other parent contributes.

Both *P. cornu-cervi* and *P. cochlearis* when crossed to large whites give washed-out, poor flowers. Since *P. sumatrana*, for example, crossed to the same whites will produce superior hybrids, the common belief has been that *P. cornu-cervi* and *P. cochlearis* have nothing to offer the hybridizer. *P. cochlearis* x *amboinensis* is superior to either parent and *P. cornu-cervi* crossed to *P. sumatrana*, *P. Samba*, and *P. mariae* have all been excellent.

The important principle that I am trying to establish is that traits as such are not necessarily passed on from parent to offspring. It is genes not traits that are parts of chromosomes. We can follow a gene from one generation to the next but the phenotypes are the results of interactions of many genes and the environment. We can usually rule out consideration of the environment's contribution because we keep it constant, but as we produce new gene combinations we are often astonished and delighted by entirely new traits. In this case the hybridizer is considered an artist.

If the new gene combination results in something undesirable the hybridizer really has 2 choices, one is to follow Brahm's example of discarding work that he did not feel was up to his standards and the other is to carefully analyze and describe the results so that others may avoid making the same type of poor flowers and so that the mechanism leading to undesirable traits can be understood. The former choice is that of an artist; the latter a scientist.

Let us look at several of these unexpected gene interactions. Many years ago I crossed *V. cristata* to *P. Dos Pueblos* and produced *Vandaenopsis* Mem. Mari De Costa. *V. cristata* has longitudinal maroon lines on a light background-color in the labellum. *P. Dos Pueblos* has a typical white lip with a few tiny dark red marks. The hybrid much to my amazement had a large solid maroon lip. No one could have predicted that the lines would be replaced by a solid color that extended part-way on to the underside of the lip.

If we had known exactly how the various genes controlling pigment production and distribution function, and this includes the promoters, operators, and repressors, we might have been able to predict the outcome of this cross. When we have learnt that much, production of such a spectacular hybrid will be commonplace and the term artist will no longer be applicable to the breeder.

The point is, of course, that with enough basic knowledge, prediction is child's play but obtaining the necessary information and analyzing it is very difficult and time consuming and we are far from having completed this task for most genes in most organisms. There are, however, a few logical procedures that lead to an occasional unexpected but gratifying result and these are the ones that I have been using and that I want to present at this time.

The hybrid between *P. fuscata* and *P. violacea* is a deep rhubarb color with segments somewhat curled along their long axes. This is *P. Bee Ridge*. I crossed this back to *P. violacea* and was astonished by the great range of phenotypes in the offspring. Many of the clones had what is generally considered a better form than any parent or grandparent but the most striking trait was the pure green in the first seedling to flower. Because of this color, I named the hybrid *P. Bornean Emerald*. A second one has flowered with this same green. Of course the Bornean form of *P. violacea* has green in the distal part of sepals and petals but it is nowhere near as intense and it is not found in the basal part of these segments.

Inserting genes from one species to the background genome of another species is what Edgar Anderson called introgressive hybridization which is also the title of his short book (Anderson, 1949) on the subject. To accomplish this transfer of a few genes into a different genome requires repeated back- crosses but the case above and the 2 to follow are examples of only the start of this series of crosses. Anderson was looking at the process from the other end of the series. That is, he found traits from one species in populations of another closely related species and realized that they had been displaced by hybridization and repeated back- crosses. Had a trait been so different from those of either original parent he wouldn't have discovered its origin. In the above case, the very green *P. Bornean Emerald* would have looked like a mutant *P. violacea* since it greatly resembles that species but no one would have been able to guess that rather than a mutation, the incorporation of some genetic material from the poorly shaped yellow and brown *P. fuscata* had generated the change.

P. Princess Kaiulani is the hybrid first made and named by Oscar Kirsch, of *P. violacea* and *P. amboinensis*. When this is crossed back to *P. violacea* it makes the very variable *P. Princess Violet*. A look at a few of these confirms the assertion that gene combinations give traits outside of the range of the 2 parent species. One of the first of this cross to flower is shaped very much like a Bornean *P. violacea* but the purple is not only darker than that of the species but it covers the sepals, thus it is much more widespread than that found in even the Malayan form of *P. violacea* which in turn is much greater than that in the Bornean form that happened to be the actual parent.

Perhaps at the other extreme is the very light *P. Princess Violet 'Lace'* which from a distance looks like a very much improved Bornean *P. violacea*. The form is excellent and this might be traced back to the *P. amboinensis* grandparent. Closer examination shows that the purple area is made up of many small dashed lines of color. This design is not to be found in either species or the hybrid in its background.

One last example of the variability in back-cross progeny that exceeds the extremes in the 2 original species is found in *P. Frank McClain* which is *P. Amblearis* x *P. amboinensis*. At one extreme is an overall mahogany flower that has a waxy sheen. Neither parent is shiny and although *P. cochlearis* is almost all one color, it is a washed-out yellowish. *P. amboinensis* has large dark blotches but the color is certainly not solid.

What I am advocating, therefore, is backcrossing F_1 hybrids to parents and perhaps second and third generation back-crosses, as well as selfing the F_1 . This system was used extensively with *V. sanderana* and its first generation hybrids. That was done not in the hope of finding new traits but rather in the hope of regaining the very fine form and size already to be found in *V. sanderana* with perhaps a few other colors that came from the other species. Elsewhere in orchid hybridizing repeated back crossing and selfing has been sparingly used.

The more genetically dissimilar 2 parents of a hybrid are, the more infertile that hybrid is expected to be; but the progeny of such a hybrid are less variable as the parents of the hybrid are more different. At first sight this seems to contradict common sense. The more difficult it is for chromosomes to synapse in the hybrid, the fewer viable spores will be produced. This means that only certain combinations of chromosomes are going to survive and as that number decreases so does the variability of the survivors.

Perhaps the most extreme example of this lack of variability in the progeny of a hybrid whose parents were genetically poles apart is given by the various sibships traceable to *Ascps*. Irene Dobkin 'Rayna' A.M. as the female parent (Wallbrunn, 1981). *A. Irene Dobkin* is the triploid offspring of a tetraploid white *Phalaeopsis* and a diploid *Ascocentrum miniatum*. Crossing it to *P. Dos Pueblos* produced *A. Rayna Wallbrunn*, over 20 plants of which have flowered and revealed great uniformity in color, size and shape. The same is true for the 5 plants of *A. Feetz Corriwell* which is *A. Irene Dobkin* x *P. Iuedde, nammiana*. It is also true for plants of *Beardara* Henry Wallbrunn, the hybrid of *A. Irene Dobkin* and *Doritis pu/cherrima*. Just compare this uniformity within sibships with the overwhelming variability in those back-crosses that I have been citing.

If there is some degree of infertility in primary hybrids and a back-cross is desired, it usually is more successful when the species rather than the hybrid is the pollen parent since the tube nucleus that controls synthesis and growth of the pollen tube has a normal haploid set of chromosomes whereas the F_1 pollen may have a nucleus that has chromosome deficiencies or duplications that interfere with synthesis of certain substances.

When this cannot be accomplished because the chromosomes from the two parents will not synapse properly, the only way around this difficulty is to double the chromosomes by using colchicine or a similar drug. Unfortunately, the chemical treatment should be given to protocorms and we find out about the sterility only after the seedling has flowered. If the F_2 is desirable enough the simplest way I see is to start the original cross over again and treat the F_1 protocorms. This sets one back anywhere from 3 to 5 years, but it is better than abandoning a very promising direction and the F_1 plants that develop may in themselves be far superior to those obtained the first time with no treatment.

Almost all the genetics that most learn in school or that has been written about orchids is Mendelian in nature and that is due to the fact that it is orderly and predictable because of the marvelous precision that characterizes meiosis. This orderly aspect of the process that eventually leads to gametes made it possible for Mendel and his successors to devise laws of inheritance that are simple and that predict the frequencies of various kinds of offspring with great accuracy.

It is interesting to learn that many other scientists of Mendel's day were also attempting to discover the laws of heredity and they all failed. Their failures and Mendel's success were the results of the difference in the plants they were crossing. Mendel used different forms of one species and most of the others were crossing different species. Usually crosses of different species lead to sterility of the F_1 so the laws that Mendel found from the second generation (F_2) could not be obtained when the F_1 had no offspring. What is more, crosses between species commonly lead to an intermediate between the 2 parent species whereas often crosses between different forms of one species exhibit dominance, that is, the F_1 will be similar to one parent with respect to certain traits and perhaps the other parent concerning other traits but there may be no intermediacy. Since the prevailing idea of inheritance was that of blending of parental

traits, an F_1 that was intermediate seemed to uphold this ancient belief, and with no F_2 available, it could not be shown to be false.

However, today we are often crossing different species, rather than using different forms of one species as Mendel did and so have a much greater array of possible results than simple Mendelian genetics would lead us to expect.

If we cross 2 forms of the same species, simple Mendelian laws including those related to linkage may apply very nicely. The frequencies are somewhat different if tetraploids rather than diploids are involved but that is not difficult to understand and the proper calculations to predict frequencies can be made. Even when different species are used, Mendelian laws may apply if the 2 species have not diverged in their evolution. In some cases another taxonomist might even call them 2 subspecies of the same species. This is the case with different labiate cattleyas.

However, when 2 species have diverged to the point that some of their chromosomes will not synapse properly in the hybrid they produce, what viable gametes may develop are in no way constrained to follow the Mendelian arithmetic.

In an autotetraploid with a pair of alleles that exhibit complete dominance-recessive behavior, the fractions that correspond to the $3/4 : 1/4$ of the diploid are $35/36 : 1/36$ (if the locus is fairly close to the centromere) (Burnham, 1962). If the tetraploid is of the allotetraploid (also called amphidiploid) form we may again obtain the $3/4$ and $1/4$ fractions provided the chromosomes of the 2 parent species are dissimilar throughout the genomes. But what if some of the chromosomes of species A still recognize some of those of species B as homologues and hence synapse while others have diverged to the point of no synapsis and an allotetraploid has been formed? The F_2 from this allotetraploid could conceivably have some traits that follow the 35:1 pattern and others the 3:1. Two and 3 factor differences could conceivably give a very confusing statistical picture.

If the locus of a gene being followed is far enough from the centromere so that there is significant crossing-over between it and the centromere, an autotetraploid will give values between $35/36 : 1/36$ and $187/196 : 9/196$ (Burnham, 1962).

What all this means is that those simple genetic laws we teach to classes often have little meaning to the orchid hybridizer for several reasons.

1. Commonly we are crossing species, not forms of one species.
2. Tetraploids rather than diploids are the desirable parents.
3. The strength of linkage of a gene to its centromere has no effect on frequencies in a diploid but has in autotetraploids and even in allotetraploids if some of the chromosomes of the 2 species are still alike enough to behave as homologues or partial homologues.

Every so often we find an article that attributes the occasional inability to produce viable hybrids between 2 species of the same genus to differences in chromosome number. For the first generation the number of chromosomes is immaterial since each haploid set has the complete directions for the production of all essential biochemical pathways of one of the parent species. The 2 sets of chromosomes may be incompatible with each other or one with the cytoplasm of the other and the more unlike the chromosome numbers of the 2 haploid sets are, the more likely incompatibility will be found. But this is a reflection of the greater length of time the 2 species have had to diverge from a common ancestor resulting in greater biochemical differences, thus chromosome number, *per se*, is not involved.

Now when we come to the first generation hybrids of 2 species with different chromosome numbers, sterility is to be expected especially if the difference is large. The greater the lack of synapsis of homologues, the greater the loss of fertility. In most animals and plants it would be 100% but with the hundreds of thousands of ovules in a developing orchid capsule, chance segregation of combinations of chromosomes that contain all the essential genes can occur every now and then. This undoubtedly accounts for some of the cases of partial fertility that we find.

We are left to explain however, a number of examples of great fertility in which the first generation hybrid is between 2 species with different chromosome numbers. These are commonly found in *Paphiopedilum* and *Oncidium* and probably other genera as well. It seemed to me that to explain this, one should first look at the different chromosome numbers and suggest a mechanism whereby the differences arose.

In plants, chromosome numbers of various species within a genus are commonly multiples of some basic number for example 28 and 56 within the genus *Oncidium* or 38 and 76 within the genus *Vanda* or even 38 and 76 within the same wild species *Doritis pulcherrima*. It is obvious this increase in the diploid number started as an autotetraploid resulting from duplication of the chromosomes without the accompanying division of the nucleus. Normally the triploids that result from a cross of diploid by tetraploid whether auto or allotriploids are nearly 100% sterile because of the "imbalance" of the genetic material that ends up in a nucleus of a gamete or zygote.

In those cases in which we find a series of numbers such as 26, 22, 30, 32, 34, 36, 38, 40, 42, in *Paphiopedilum* or 40 and 42 in the equitant *oncidiums*, the changes from the ancestral condition must be due to a fusion of 2 telocentric chromosomes at the centromere for a reduction in number or a separation of a metacentric into 2 telocentrics with a resulting increase in number (Duncan & MacLeod 1949, Tanaka and Aoyama 1974). An increase can also have its beginnings in a trisomic as a result of nondisjunction of one pair of sister chromatids. This is a slower, more hazardous route but may well be the mode of increase occasionally.

After separation of the 2 arms of a metacentric chromosome, homology of each new chromosome to one arm of the metacentric would still allow synapses to occur in a hybrid between the original and the derived form and thus a hexad rather than a tetrad would be produced. The separation of the hexad would be into two chromatids attached to 1 centromere and 4 chromatids containing 2 centromeres and these 2 combinations would be genetically equivalent. Of course, crossing over will complicate the picture but this type of synapsis and separation with or without crossover exchange accounts for those hybrids with fertility that otherwise would seem to defy our understanding.

That such separation of fusion at centromeres has actually taken place seems to be shown in *Paphiopedilum* by Karasawa and Tanaka (1980) using the new highly selective staining method that allows one to observe banding patterns on chromosomes other than those of the famous dipteran larval salivary glands. Unfortunately, most other orchids have chromosomes much smaller than those of the *Diandrae* and so identification of a particular arm in other forms would be very difficult and to my knowledge has not been done.

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Odontoglossum Alliance Meeting to be Held in San Francisco 20-23 February 2013

The next meeting of the Odontoglossum Alliance will be held in San Francisco at the time of the San Francisco Orchid Show 20-23 February 2013. Economic conditions have shortened this show to 3 days with the Preview Party on Thursday night, 20 February 2013. We are having a joint meeting with the Pluerothalid Alliance on Friday 21 February.

The meeting will be held in the Firehouse at the Fort Mason Center. I have included some material on

the location in this newsletter. Featured wines will be served with dinner following. The menu will include choices of roast beef, turkey and vegetarian lasagna. Members of both Alliances living in the area will contribute by providing a variety of specialty dishes. All in recognition of the economic climate to make it as attractive as possible for members to attend. Two talks are planned: One by each of the Alliances (OA and PA). The Odontoglossum Alliance talk will be about recent trends in hybridizing. As usual there will be an auction of fine material from both alliances. I expect to see some premium Odont divisions available in the auction.

We will see if several local greenhouses can be available for touring on either or both Saturday and Sunday. Later in the newsletter is some material on local motels close to Fort Mason.

Several venues were considered and the overriding factor was the current economic climate. It is hoped this decision will be attractive to many of our members and that we will have a good turnout.

Tickets to the Preview Party and the show can be obtained over the internet. The address for the web site where these can be ordered is found is:

<http://www.orchidsanfrancisco.org/poe.html>

We expect the cost of the dinner at the meeting to be reasonable. In the November 2013 newsletter we will have firmed up on those costs. In addition we will have information on how to make a reservation.

We look forward to a good crowd. In this November newsletter are some details on the meeting. This includes suggestions as to hotel locations close to the show. More details on the meeting will be in the November newsletter.

The San Francisco Orchid Show is the best show in North America to see Odontoglossum alliance material in the show. The sales area is huge with many opportunities to acquire high quality material.

A good web site to look for hotels is: www.sfravel.com. The specific page is <http://www.sanfranciscovisitor.com/bgt.html>. A selection of hotels picked from the web site follows.

Travelodge by the Bay (415) 673-0691

1450 Lombard St. San Francisco, CA 94123

Lombard Motor Inn (415) 441-6000

1475 Lombard St.

Francisco Bay Motel (415) 474-3030

1501 Lombard St.

Redwood Inn (415) 776-3800

1530 Lombard St.

Town House Motel (415) 885-5163

1650 Lombard St.

Star Motel (415) 346-8250

1727 Lombard St.

Cow Hollow Motor Inn* (415)-921-5800

Lombard Street

S F Motor Inn (415) 921-1842

1750 Lombard St.

Coventry Motor Inn (415) 567-1200

1901 Lombard St.

Ramada Limited (415) 775-8116

1940 Lombard St.

Buena Vista Motor Inn* (415) 923-9600

PO Box 475517 San Francisco, CA 94147

Chelsea Motor Inn (415) 563-5600

2095 Lombard St San Francisco, CA 94123

Motel Capri (415) 346-4667

2015 Greenwich St.

Hotel Del Sol (415) 921-5520

3100 Webster St.

Best Inn (415) 776-3220

2850 Van Ness Ave San Francisco, CA 94109

These hotels are within a couple of blocks of Fort Mason. These appear to be clean and comfortable, but not elegant. The web site offers reviews of the hotels. The ones marked with an * I have stayed at for previous meetings and shows. They are clean, neat, not elegant, reasonably priced and with parking. I often walked to the show from these hotels.

The meeting to be held on Friday evening will be in the three story building which is the second one down from the show in the Fort Mason Complex. The address is:

Fort Mason Center

Landmark Building A

San Francisco, CA 94123

Phone 415-345-7500

Request for Auction Material

One of the more interesting and entertaining events at our Odontoglossum Alliance meeting is the auction of fine odontoglossum material. We have had many donators who have brought in fine material. Much of this material has been of plants that are awarded, hard to find species or well know hybrids. Occasionally we have some of the Nellie Roberts watercolors or other fine old orchid illustrations. The results of our OA auction have been used to keep our dues down and provide resources that allow us to increase the size and color content of our newsletters. This newsletter is typical of what can and is being done. I urge all our members whether you plan on attending or not to donate to the auction. If you are not coming so you could bring the material to the dinner, you can mail it to Steve Beckendorf, Steve will get it to the meeting and auction.

So look over your material and find something or if possible a couple of things and get them to the auction and meeting.

Mailing address

Steve Beckendorf

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