

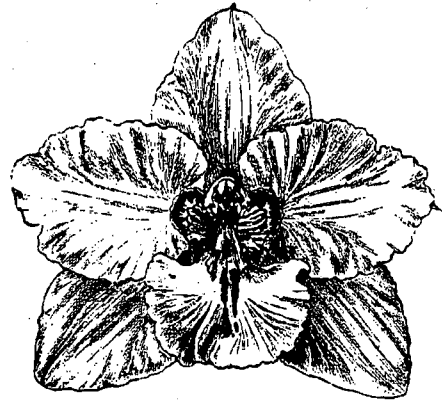
Odontoglossum Alliance Newsletter

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WHEN ONE AND ONE BECOME THREE AT LEAST

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Part one.

When I became interested in growing orchids as a hobby I found that *Odontoglossum* plants were something I could grow. The environment I could offer on a windowsill in Sweden satisfied their needs. This in turn created a desire to grow more and different but yet closely related plants and to learn more about the group as a whole. Field trips to the Andes of Ecuador yielded plants and knowledge about their natural habitats but did not offer any nomenclatural information. To have a plant that does well in cultivation is great. To have a name on it is even better. It may be complicated to explain the psychology for this but somehow it appeals to our sense of order. Unfortunately, *Odontoglossum* literature has always been scarce and can be confusing regarding the names of the species. What appears to be the same species can have different names in various books. In order to understand the reason behind this we have to penetrate the impressive maze of orchid taxonomy and combine it with the reality in the field. The genus *Odontoglossum* is an appropriate study group because the nomenclatural confusion is particularly bad in this case and it is desirable to have valid names for these popular and important plants.

In botany, the International Code of Botanical Nomenclature is the equivalence of what the bible represents to Christianity. It is *the* "law-book" for all botanists, and its purpose is to regulate all botanical nomenclature in a consistent way. When we read the first sentence on the first page of the Preamble in the St. Louis Code of the year 2000, we find that - "Botany requires a precise and simple system of nomenclature used by all botanists in all countries,..." What most orchid botanists and serious hobbyists alike probably would agree on is that this sentence does not reflect the reality at all. In our ambition to organize Mother Nature we seem to favor easy and artificial solutions rather than to accept a complicated reality that is beyond what possibly can be explained in a "precise and simple system".

If we want to know what a true *Odontoglossum* is, then we have to study the type specimen for the genus. This is the original plant on which the genus is based and described. More than two centuries ago the

European adventurers and explorers Alexander von Humboldt and Aimé Bonpland arrived in South America to “discover” things. At one time they crossed the forest covered mountains of northern Peru, near the town of Jaen, where they found a wealth of unknown plants to collect. Many of these grew epiphytically on the trunks and limbs of mossy trees, a habit that must have appeared strange for early European travelers. One particular plant with large yellow flowers, spotted with brown, was later described as *Odontoglossum epidendroides* by the botanist Kunth in *Nova Genera et Species Plantarum*, 1815 (slide 1). The name refers to a plant with “teeth on the tongue, which grows on trees” and the type plant is currently placed in the Museum of Natural History in Paris (P). An illustrator was hired to prepare a drawing of the plant to accompany the description. Unfortunately, the illustrator had little understanding of what the plant and the flowers looked like in a living state, and since an illustrator draws what he sees the illustration clearly depicts a dried and very dead plant (slide 2). After the original collection this species was lost for many years. In 1838, the English botanist John Lindley described another Peruvian plant, collected by Mathews in the Chachapoyas area, as *Odontoglossum lacerum* (slide 3). This was also a plant with yellow flowers, spotted with brown, and a concave lip with a lacerate margin. It did not resemble anything that Lindley had seen before so he concluded that it was a new species. A few years later he received a plant collected by Jean Linden in the forests of Pamplona, Colombia. Finally, there seemed to be a specimen that matched the description as well as the rather stylized illustration of *Odontoglossum epidendroides*, and he cited it in *Folia Orchidacea*, 1852, separate from *O. lacerum*. This was the second documentation of *O. epidendroides* since the original collection. Unfortunately, the “awkward” drawing of *O. epidendroides* misled Lindley in his identification of the plant from Pamplona and this error was discovered by the German taxonomist H. G. Reichenbach. He, in turn, realized that the Pamplona plant represented a new species and named it *O. lindleyanum* after Lindley, in *Bonplandia*, 1854 (slide 4). A single flower from the type of *O. epidendroides* is mounted on a sheet in the Reichenbach herbarium in Vienna

(W), together with a single flower collected in the Pastaza valley, Ecuador, by Wagner and which Reichenbach identified as the same species. At the bottom of the sheet is written in pencil with Reichenbach's handwriting “*Odontoglossum epidendroides* H.B.K.”, followed underneath by “*lacerum* Lindl, brought June 1878 [in German]”. Apparently he concluded at that time that *O. lacerum* was a synonym of *O. epidendroides*. Lindley seems to have been limited to the illustration of the latter species, which demonstrates the importance of examining real specimens when describing new taxa, rather than relying on somebody else's drawings.

If we return to northern Peru for a while, we realize that the original areas for *O. epidendroides* and *O. lacerum* basically coincide. A deeper study of the Reichenbach herbarium reveals other collections of what obviously are the same “thing” from nearby areas, but determined as something else. The name “*triumphans polyxanthum*” appears on a drawing of a plant from “Huacabamba” by Dr. Hermann Krause (probably the current city of Huancabamba), and dated 1866 (slide 5). If we visit the cloud forests of southern Ecuador, not far from Huancabamba, it is still possible to find plants that correspond to this drawing perfectly. When we examine the flowers we find that they match the flowers of *O. epidendroides* as well. The name “*triumphans polyxanthum*” refers to plants collected by Edouard Klaboch on the eastern Andes in Ecuador and later shipped to England to be sold at Mr. Stevens Rooms. At least one plant went to a M. F. Kegeljan in Belgium, where it flowered in 1876. Flowers were sent to Edouard Morren at the Botanic Garden in Liège for identification. Morren, in turn, sent a flower accompanied by a letter dated August 2, 1876 (W) to Reichenbach, asking for help with the identification. Morren compared the flower with *O. epidendroides* and *O. lindleyanum*. It is uncertain whether Morren received an answer from Reichenbach or not, but he apparently decided to describe the plant as *Odontoglossum kegeljanii* in *La Belgique Horticole*, 1877. In the Reichenbach herbarium is an envelope with two flowers, which appear identical, together with two labels in Reichenbach's handwriting. One says “*Od. triumphans polyxanthum*,”

Morren with letter -76 [in German]", and the other "Od. lacerum, Williams". Reichenbach described his "O. triumphans polyxanthum" as a good species (*O. polyxanthum*) in the Gardeners Chronicle, 1881, and the description is based on a single flower from Williams (possibly the one in the envelope determined as "O. lacerum"). It is interesting to note that Reichenbach mixed *O. lacerum*, which he synonymized with *O. epidendroides* in 1878, with what became *O. polyxanthum* (which Morren already had published as *O. kegeljanii*). If we keep travel northwards in Ecuador we can find another form of this variable plant in the area east of Cuenca. The flowers are larger and showier with a longer column than the other forms (slide 6) but the shape is similar, and through the intermediate forms in southern Ecuador connects well with *O. epidendroides* from Peru. Recent collections by the author from south of Loja show this species to be highly variable and clear borders between the traditional taxa cannot be detected. When occasional or selected plants are compared they may appear distinct but as more material is examined the borders become increasingly blurry and identification turns into a superficial guesswork.

If we continue our journey in Ecuador northwards, we eventually reach the Pastaza valley where another representative of this orchid can be found. This geographical form seems different enough to justify a separate name at first. It frequently produces a long and branched spike with large yellow flowers, spotted with brown. The edges of the lip are slightly recurved, which creates a convex shape (slide 7). This feature produces a very different appearance at a quick glance, but when we examine the flower closely, and flatten the lip, we realize that the morphological similarities between this form and *O. epidendroides* are convincing. Reichenbach also determined a flower from this area as *O. epidendroides* (the flower is mounted on the same sheet as the type of *O. epidendroides*, mentioned above, and the rest of the plant is in the herbarium of the Botanical Garden in Munich). Bockemühl reached the same conclusion in her monograph of the genus in 1989. As we travel further to the north we keep finding new and slightly different forms of this orchid. There is a clear pattern of changing the shape and, to a lesser degree, the col-

oration with the geographical distribution. The various forms, many originally described as species, do not seem to occur together as different, but rather lock into each other gradually as the links in a chain. This is very frustrating for both taxonomists and horticulturists who can see "different things" but have great problems in defining them. In Colombia and Venezuela we find other forms of this complex, described as *O. spectatissimum* by Lindley in Folia Orchidacea, 1852, and as *O. triumphans* by Reichenbach in Bonplandia two years later (slide 8), (which is the origin of the name "triumphans polyxanthum"). Garay included two plates of this complex in his and Dunsterville's illustrated field guide to the orchids of Venezuela, 1979. One is labeled *O. kegeljanii* and the other *O. spectatissimum*. The illustrations show morphological differences but an examination of the preserved specimens (at SEL) show that they are similar. It is clear that Lindley, Reichenbach, Morren, Garay, Bockemühl and others have recognized the differences between the here mentioned taxa, well enough to describe them as distinct species. On the other hand, they have also managed to merge and confuse them in the same process.

Theoretical taxonomy can certainly become both hilarious and confusing at times so it is important to connect it with reality. Many years of personal observations in the field demonstrate that the *Odontoglossum epidendroides* complex is a variable and very largely distributed single taxon. There are local forms which appear distinct from other geographically restricted forms but the morphology generally changes gradually without clear borders. Occasionally, neighboring forms can appear distinct, but then both forms can merge into a third and common form. For instance, typically *O. kegeljanii* and *O. spectatissimum* look "different" and they occur next to each other geographically. Both of the "species", however, merge into *O. epidendroides*, which unites them. My interpretation is that we are dealing with one variable species, which consists of a number of geographical forms that link into each other, either directly or indirectly. A way to describe this in a proper nomenclatural way is to call the variable and widespread taxon a "superspecies", because it represents a level "above" the traditional species

concept. By definition, a superspecies consists of various forms that create a level beneath the traditional species level, and can be called "subspecies". A "variety" on the other hand can develop in any population and consist of plants that resemble each other without being genetically closely connected (*Cattleya intermedia* var. *aquinii* as an example).

This survey of the *O. epidendroides* complex is what we find when we travel north from the original collection site. Due to a lack of collected material little is documented from the rest of Peru (personal experience). Not until we reach Bolivia can we get an idea of the southern forms of this complex.

Odontoglossum subuligerum (slide 9) was originally collected by Pearce in southern Peru and described by Reichenbach in *Linnaea* (1877). It is a fairly common plant in Bolivia and field studies reveal an interesting story. Bockemühl included this species in her treatment of the genus (1989), but unfortunately misidentified it due to poor type material. She selected a plant from southern Colombia as a probable candidate but which represents another species complex altogether. The author felt comfortable with this inclusion, however, and stated that all known species of *Odontoglossum* were covered in her book. As a result of this statement, Königler concluded that he had found a new species when he examined a Bolivian plant collected by Dieter Hauenstein. The plant, which flowered in cultivation in Germany, did not occur in Bockemühl's treatment. It was described as *O. hauensteinii* in *Arcula*, 1994. Probably because of the rarity of this particular publication, Senghas fell into the same trap and described another Bolivian plant from the same area as *O. vierlingii*, in *Journal für den Orchideenfreund*, 2000. *Odontoglossum subuligerum* is morphologically a stable species but can vary substantially in color from clear yellow to clear green with or without dark brown spots. Different color forms can grow side by side in the same tree. The overall plant habit resembles other forms of the *O. epidendroides* complex but the shape of the column is different. It is shorter and has a definite cavity created by the basal lateral flanks of the column. The anther cap is also quite distinct. The problem is that some forms of *O. epidendroides*, from the scattered

collections that are known throughout Peru, do resemble *O. subuligerum* to a disturbing degree. It is a long leap from northern Peru to central Bolivia and frustratingly little is known about the possible forms that may occur there but it looks like there may exist a chain of connecting links. For the time being, *Odontoglossum subuligerum* is considered a distinct species but little can be said about what the future status will be.

To be continued...

Thanks to Tom Perlite

Tom Perlite of Golden Gate Orchids was most generous in contributing a number of *Odontoglossum* Alliance plants to the Longwood Gardens. The Longwood Gardens had contributed a number of divisions of their very old *Odontoglossum* hybrids to our Alliance. These divisions were auctioned at the Alliance meeting in Illinois last March. I had the opportunity to visit the Longwood Gardens on 26 July. Lee Alyanakian, the orchid grower at the gardens was very pleased with Tom's contribution. I expect to have a more detailed report on my visit in a future newsletter. Needless to say the gardens are beautiful both indoors and outdoors. Tom your plants were a very welcome addition to the collection at Longwood. Many people will enjoy seeing the blooms throughout the years as the flowering plants are assembled in the display gardens.

Oh Say, Can You See...

How Much Of A Species Is In A Hybrid ?

A Model for Inheritance in Orchid Hybridizing

Helmut Rohrl
Part III

Some Observations Based on the Model

By looking at some of the zygote matrices in the preceding section we will come up with some observations.

Observation 1:

Using our model, we can now determine whether a particular plant is a species or a hybrid on the basis of its progeny when crossed with a species. This is a useful tool to distinguish between a species and a natural hybrid, since we can see what splitting occurs in the progeny

In Cross I through Cross VII we analyzed the simplest crosses, namely: species x species; species x simple primary hybrid; and simple primary hybrid x simple primary hybrid. First we looked at the case where $n = 1$ and then for general n . From this list of crosses we can conclude that:

All progeny of a grex will have the same somatic genotype if, and only if, the grex is species x same species, or species x different species.

In all other cases, including the many possibilities for crosses more sophisticated than the listed ones, there will be at least two (and in general, many more) cultivars in the cross with mutually distinct genomes. Consequently, to determine whether a particular plant is a species or a hybrid, we can cross it with any known species and check the resulting progeny for uniformity. We also see that whenever one of the parents of a hybrid is a species then half of the hybrid's somatic genome comes from that species.

Observation 2:

Our second observation has to do with "loss" of chromosomes from various parental species over generations of breeding. By checking Cross I through Cross VII when $n = 1$, we see that cultivars of hybrid populations may, or may not, contain chromosomes from a species that is a grand parent, great grand parent, or appears at more removed levels of the genealogy tree. To summarize:

In all $2n$ -diploid crosses with parents that have at most one chromosome from a given species in each of their chromosome pairs, there are cultivars in the hybrid progeny whose somatic genome fails to contain any chromosomes from that species.

On the other hand, in any $2n$ -diploid cross for which one of the parents has more than n chromosomes coming from a given species, every cultivar has at least one chromosome from that species. When there are more than n chromosomes from that species in a parent, there is a whole chromosome pair from that species in the parent, so that each gamete has at least one chromosome from that species. The difference between the number of such chromosomes and n determines the least possible number of chromosomes from that species in every hybrid cultivar.

Even in fairly simple hybrid grexes, randomly selected cultivars may not contain any chromosomes, and hence genes, of a particular species. Still, by selecting cultivars according to certain criteria, such as flower color, flower shape, spike habit, etc., one may pick plants that *seemingly* show genetic influence from a particular species in the background of the grex but whose characteristics come from the mixture through successive hybridizing of certain characteristics of some of the other species in the background of the grex. The "recognition" of genes (by a plant's appearance), or the percentage of genes (also by appearance), of a particular species in such a hybrid is basically unsubstantiated.

Observation 3:

An often invoked, popular concept of an orchid's "blood line" states:

"Since species 'S' is k steps (for example, 2 steps) up in the genealogy tree of a hybrid, every cultivar of this hybrid must have at least $1/2^k \times 100\%$ ($1/2^2 \times 100\% = 1/4 \times 100\% = 25\%$) of the species 'S' in them."

This blood-line concept is not true - **except** when $k = 1$, that is, when the species is only one generation removed from the cultivar. By checking Cross I through Cross VII for $n = 1$, we see that **only** species x species crosses (Cross I) produce progeny with a uniform genotype, and virtually identical phenotype. Crosses III through VII produce cultivars with distinct numbers of chromosomes inherited from species 'S' in their genotype (see **Observation 2**). There is at present, no practical means of determining the genetic composition of a particular cultivar, much less the composition of the entire progeny population, plant by plant. In particular, we can calculate the exact percentages of genetic material that a hybrid inherits only from its parents (which is 50 % from each), and no further back.

For any cultivar, we can say with certainty that its genome consists of $1/2$ of the genome from each parent. We cannot determine the percentage of genetic contribution from any other ancestor, grand-parent to great-grand-parent, and so on.

Yet there is some small truth in the "blood line" hypothesis. Let us examine in which sense. Let's assume that the somatic chromosome number $2n$ is 30, to get into a reasonable range for orchid hybrids. Suppose we make a cross $A \times A$, where A is a species. Then the resulting cultivars will all be identical and hence agree with the statistical average of the somatic genomes of the progeny population. In other words, crossing $A \times A$ results in 100 % species progeny as "predicted" by the "blood line" hypothesis. Now, if we cross a simple primary hybrid with a species, or another simple primary hybrid, then we have to expect up to 4^n , that is, roughly a billion, hybrid cultivars in the zygote matrix that have mutually distinct somatic genomes. The statistical average of these genomes will satisfy the "blood line" hypothesis.

Now suppose we want to make a cross between two such hybrids in a way that the "blood line" hypothesis would hold for our next hybrid. Then we would have to take a sample from the first hybrid as well as the second hybrid to cover statistically this tremendous amount of distinct cultivars, cross all cultivars from the first hybrid with any and all cultivars of the second hybrid, and raise a large number of offspring from each and every seed capsule to obtain the statistical average for the next hybrid. Obviously this is a totally impossible enterprise.

To sum up:

Although the "blood line" hypothesis is statistically correct, it is unusable for all practical purposes.

Observation 4:

The key influence on genotypes and phenotypes of progeny is the actual cultivars selected as parents; the grex of the parents is of much less consequence.

This follows from the fact that the zygote matrix for any complex cross produces a variety of different genotypes. In turn, we see from the zygote matrices that parents with different genotypes produce different zygote matrices.

Observation 5:

Reciprocal crosses¹ produce the same zygote matrix and hence the same mix of genotypes.

This observation comes about by considering the chromosome pairs AB and BA to be identical. However, in reality, a cross and its reciprocal, even with the same parents, will often produce markedly different progeny.²

The Model and Reality

In setting up our model for mendelian inheritance we had to disregard irregularities that occur naturally during the process of meiosis, zygote formation, and other events. We also had to make assumptions, some tacit, involving the size of progenies that were not in line with reality. So, what corrections are required to bring our model closer to reality? And how will this affect the conclusions we draw from the model? In order to discuss this issue we need to distinguish between the “*actual*” progeny of a grex, that is the progeny population which we can actually see in a nursery, and “*model-based*” progeny, that is, the progeny predicted by our model.

While our model explicitly pictures the crosses between hypothetical plants with somatic chromosome number of $2n = 2$, most diploid orchids

have somatic chromosome numbers of $2n$ between 30 and 40, and more. For instance, most Oncidiinae species as well as hybrids usually have somatic chromosome number $2n = 56$. This means that, *except for species and simple primary hybrids, all other model-based progeny must consist of a tremendously huge number of mutually distinct cultivars*. Although the distribution of the various cultivars in such a population is *discrete* (that is, composed of few uniform and easily distinguishable subpopulations), it *appears* to be *continuous* due to the large number of distinct individuals involved.

The first assumption for our model was that the two chromosomes in each chromosome pair in the somatic genome of a species are *identical*. But in nature they are only *nearly identical*. Otherwise all cultivars of a species, for example, *Laelia anceps*, would be identical, which quite clearly is not the case. Look at flowers from 30 individual plants of *Laelia anceps* in the wild, and variations in size, markings, color, spike length, and floriferousness are quickly apparent. As a consequence, the number of mutually distinct cultivars in a grex will be much larger than the Mendelian model predicts, causing the sharply defined model-based genotypes to become less focused and ‘washed out’. Furthermore, the composition of the cultivars in a given grex will change when different parents are used to produce that grex. This means that the *choice of parents, even when they are species, can have a considerable influence on the progeny of the resulting hybrid*. It also says that *remakes* of a particular grex, using *different* parents, must be expected to produce *different* progeny.

An instance of the latter happened several years ago in Hawaii. W.W.G. Moir made the grex Miltassa Aztec (Miltassa. Cartagena x Miltonia Minas Gerais), and repeated it twice. The original cross and the first remake were carried out with the same parents and in the same order (same pod and pollen parents), and produced white to light yellow flowers with some sporadic patterns. For the second remake, Moir used a

different cultivar of Milt. Minas Gerais. This time the grex produced about 75% whites and light yellows, as well as about 25 % dark purple flowers with extensive and very attractive patterns.

Other issues affecting the relation between model-based and actual progeny will be discussed now.

Limitations Due To The Seed Capsule:

A fruit capsule can hold only a limited number of seeds. Some capsules (of species) have been estimated to contain as many as 3 million seeds. So one can safely say that capsules can hold no more than, say, $10^7 = 10,000,000$ seeds. Ten million is negligibly small when compared with the number of roughly 10^{16} possibilities for different genotypes in the model-based progeny of a complex *Oncidiinae* cross. Thus a seed capsule will contain only a very small and, presumably, *random selection* of individual seeds out of the immense pool of all possible seeds. Moreover, in the case of complex hybrids, a capsule may contain considerably fewer seeds, and on occasion only a few, seeds than the 10^7 seeds proposed above as the upper limit. For example, if a seed pod produces just 10,000 seeds out of 10^{16} possible zygote genotypes, at most only .000,000,000,001% of the possible genetic combinations are actually created as seeds. Due to nature's limits on the number of seeds produced with a cross, as well as other factors described below, the actual progeny population can be quite different from the progeny population predicted by the model.

Man-made reductions in progeny population:

Man-made restrictions further limit the selection of cultivars from this potentially immense pool of model-based progeny. Not all seeds in the capsule will germinate under artificial flasking conditions. The grower may not sow all the seeds in the pod into a flask. Of those that do germinate, not all are raised to maturity. The grower often limits the number of seedlings she/he decides to

cultivate from flask (often fewer than 500), due to space requirements and the expense involved. Seedlings generally make this cut by random selection. Yet another selection, mostly non-random, surfaces when the seedlings are planted out, as the smallest and weakest plantlets generally are culled and discarded. Actual growing conditions in the greenhouse, such as prevailing temperatures, humidity, light, water, and air movement, lead to a further thinning of the progeny population, as those plants unable to adapt to these conditions will fail to thrive.

3. Genetic exchange during cell division:

Meiosis, while it is occurring, is a source for deviations from the model-based progeny population. An uneven exchange of genes, or small groups of genes, between homologous chromosomes has been shown to sometimes occur during meiosis in species orchids. It is reasonable to assume that this also occurs in hybrids. Obviously it leads to a more diversified progeny population. It also means that, even in simple primary hybrids, individual chromosomes in the genome of a cultivar may contain parts that come from different species in the genealogy tree. As a consequence, the genetic code coming from a particular parent species may gradually die out during successive hybridizing.

Unreduced gametes and chromosome doubling:

As stated above, meiosis produces the gametes which fuse to make zygotes. However, even in 2n-diploids, meiosis does not always result in only n-gametes. Occasionally 2n-gametes will be produced from the 2n somatic cells, that is, gametes whose gametic chromosome number is the same as the somatic chromosome number of the parent plant. These are referred to as *unreduced gametes*. Obviously the progeny produced by such unreduced gametes does not follow our model³. When two unreduced gametes of two diploids combine and fuse, the results are allotetraploids in the progeny. Still another process leads to autotetraploidy respectively

allotetraploidy: the fusing of two n-gametes, followed by a doubling of the chromosomes. This process occurs in nature and can be achieved in the laboratory. Although both of these processes (unreduced gametes and doubling of chromosomes) occur outside the laboratory, they only occasionally contribute to differences between the actual progeny and the model-based progeny. To some extent, they are responsible for the random appearance of exceptional cultivars in a progeny population. It should also be mentioned that not only polyploid gametes can appear during and after meiosis, but aneuploid gametes as well. These will mostly result in non-functional zygote and embryos, and hence have little effect on the progeny population.

Other changes in the structure of the genome of a zygote seem to occur during the cell fusion leading to the zygote. These are chromosomal aberrations caused by deletion and duplication of sections, and by the translocation of segments. They all involve the breaking and recombining of homologous as well as non-homologous chromosomes. Experiments seem to indicate that the joining chromosomes of the gametes may adjust themselves by deletion when they are physically different, e.g. have too different lengths, so they are able to form a stable pair of chromosomes.

Mutations:

Another deviation from the model-based progeny picture is caused by genetic mutations. These very rare events lead to the modification of some, or parts of some, chromosomes. They often have disastrous consequences for the gamete respectively zygote, but can also create some surprisingly different and attractive cultivars. Due to their rarity, their influence on the progeny population is negligible.

Proliferations:

Proliferation is an instance of 'mericlone' that occurs spontaneously in certain seed flasks. For some reasons, perhaps caused by the chemical composition of the flask medium, protocorms

begin to subdivide into genetically identical protocorms. When this process keeps repeating itself, the result is called proliferation. It leads to a large number of genetically identical cultivars which, if not culled when seedlings are planted out, can produce a biased progeny. If just one replant flask containing proliferation is planted out, an unsuspecting person may be led to believe that some dominance⁴ may have struck the phenotype of the cross. But, if several flasks are raised, proliferation cannot be mistaken as an instance of dominance as the some flasks may lack proliferation, and proliferation in different flasks comes from different protocorms and hence produce distinct progeny phenotypes.

All the events described in this section can generate considerable differences between model-based progeny and actual progeny. However, understanding the model-based progeny indicates to us what the general expectations are for the actual progeny.

Dominance and

Recessiveness

Individual genes or a combination of several genes are responsible for certain characteristics or features of the plant. Superficial reasoning could imply that the characteristics seen in the progeny of a cross are "intermediate" between the corresponding characteristics of the parents. As explained in the previous section, there is an immense number of distinct cultivars in the progeny of a cross - other than a simple primary cross. So this "intermediate proposition" is unlikely to be valid for a randomly selected progeny population of a complex hybrid - unless the definition of "intermediate" is stretched to become meaningless. Moreover, this proposition often fails even when the statistical average is considered, not individual cultivars. The reason is genetic dominance, which is generally observed with respect to a characteristic, e.g. lip color, or floriferousness of a particular parent

over the other parent in a cross. A dominant trait (lip color or floriferousness) will appear in a large proportion of progeny who have inherited the characteristic of that parent.

As pointed out in paragraph 6 of the preceding section, there can be occurrences of "false dominance". However, they are easily recognized as such, as they do not affect the progeny quite uniformly.

Dominance can manifest itself from "somewhat" to "total", and often depends on the particular cultivar of the grex involved. Hence, *Laeliocattleya* XYZ cultivar "A" may be dominant for lip color when crossed with *Sophrolaelia* UVW cultivar "1" and not be dominant when crossed with *Sophrolaelia* UVW cultivar "2"; similarly *Laeliocattleya* XYZ cultivar "B" may not be dominant when crossed with *Sophrolaelia* UVW cultivar "1". The opposite of dominance is recessiveness. Dominance for a certain characteristic is carried by one or several genes, possibly located on different chromosomes. So, only a small part of the parental genome has to be transmitted to the progeny to have a particular dominant behavior show up. In other words, dominance with respect to a certain characteristic does not necessarily imply the presence of a large part of the parent's genome in the hybrid cultivars. The converse is also true: if a hybrid population is recessive with respect to a certain characteristic, and the recessiveness comes from one parent, it does not follow that the cultivars of the progeny contain only very little of the parent's genome.

According to Mendelian laws of inheritance, the investigation of dominance in orchid species requires several steps. First, the growing and blooming of many - say, a few hundred - simple primary hybrid cultivars. Then, the crossing among themselves of a fairly large number of these hybrid cultivars and, again, the growing and flowering of fairly large numbers of these secondary crosses (see [A], p.574 a.s.o.). In

addition it requires the statistical evaluation of the appearance of the investigated characteristics. Obviously these efforts are cumbersome and time consuming, considering that five or more years may pass between the pollination of a plant and the first flowering of the progeny. As a consequence, few such investigations have ever been undertaken. The same procedure applies to searching for dominance in hybrids. But the latter includes the additional difficult task, even in relatively unsophisticated complex hybrids, of determining from which of the possibly many ancestral species the characteristic has evolved. The reason for this is that in the presence of more than two species in the genealogy tree, several different groupings of genes may produce the same characteristic.

¹ One cross is the reciprocal of another cross if the same actual parents are used, but in reverse order. For example, *C. warneri* 'Crispin' x *C. trianae* 'Brenda' is reciprocal to *C. trianae* 'Brenda' x *C. warneri* 'Crispin'. Note that the same cultivars, not just the same grex, must be used for a reciprocal cross.

² See section on Non-Mendelian Inheritance

³ See section on *The Model and Polyploidy*

⁴ See section on Dominance and Recessiveness

Final Dues Reminder

This is the final notice to those of you who have not as yet paid your membership fee for the year August 02 thru May 03. If there is a reminder slip in with this newsletter, you are delinquent. If you have not paid before the November newsletter publication, you will be dropped from the mailing list. With the increase in postal rates, the Alliance cannot afford further notices and mailings.

John E. Miller

Treasurer

Election of Directors

All three directors were re-elected with a counting of ballots. Their new terms are as follows:

Helmut Rohrl, Chairman

Term Expiring 2005

Howard Liebman

Term Expiring 2005

Robert Hamilton

Term Expiring 2005

Odontoglossum Alliance Meeting

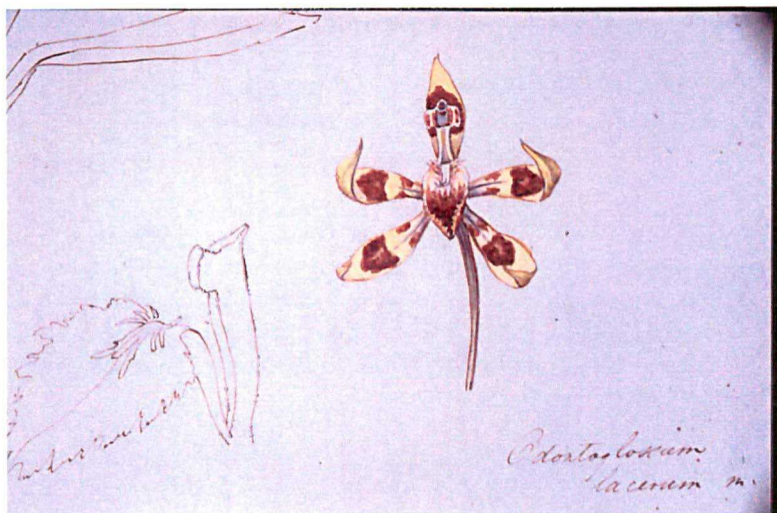
The Odontoglossum Alliance meeting will be held in conjunction with the Hilo Orchid Society show and AOS Trustees meeting in Hilo, Hawaii 19-23 March 2003. Our meeting will be on Friday afternoon 21 March 2003. We will have four speakers for our session. The program is currently being organized. Details will be announced in the November 2002 newsletter. We are also planning a dinner for the evening of 21 March. There will be an auction of donated Odontoglossum Alliance material to include some of the RHS watercolors the Alliance purchased from Bruce Cobbledick and a set of the 16th WOC wine glasses engraved with Odontoglossum Alliance. These glasses were distributed to the attendees at the dinner in Vancouver and we have a few left. It is also planned to have the release of the complete book "Odontoglossums" by Leon Duval, originally published in 1900. The book will have both the original French and the English versions. Our entire membership list will receive the mailing from the Hilo Orchid Society as they announce the details of the show and meeting. They have made some very favorable room rate arrangements. We hope to have a good attendance at the meeting.



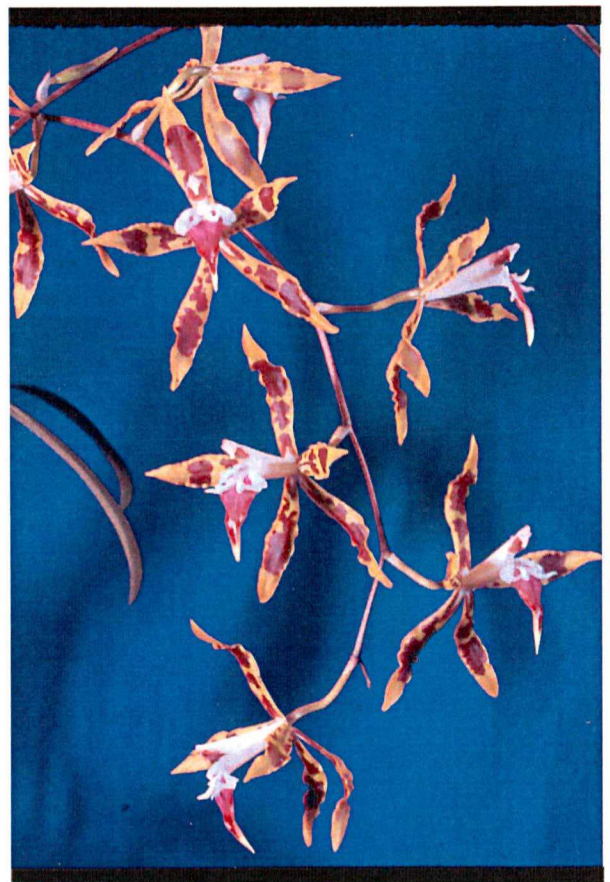
Odontoglossum epidendroides Slide 1



Dried specimen Slide 2



Odontoglossum lacerum Slide 3



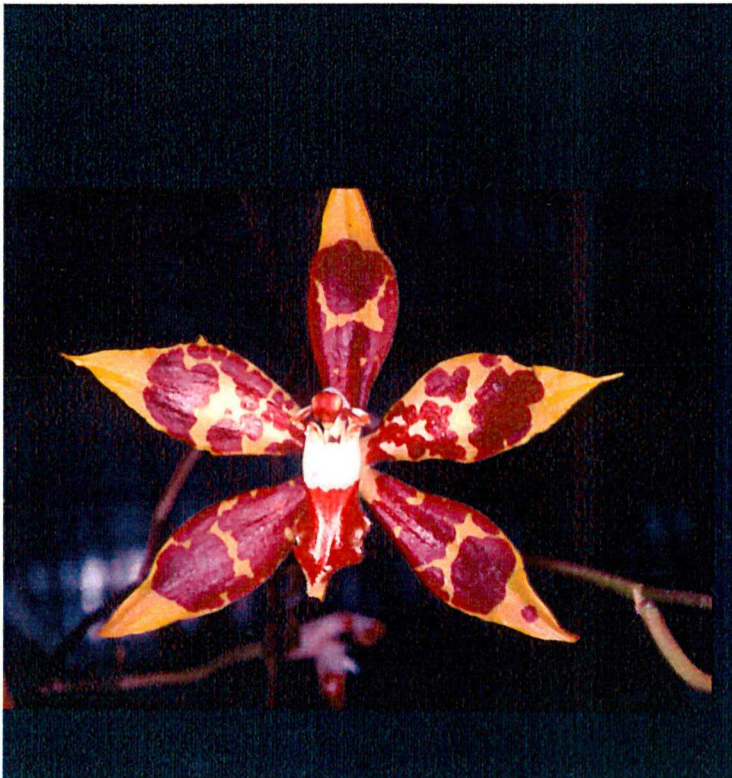
Odontoglossum lindleyanum Slide 4



"triumphans polyxanthum" Slide 5



Odontoglossum polyxanthum Slide 6



Odontoglossum epidendroides Slide 7



Odontoglossum spectatissimum Slide 8