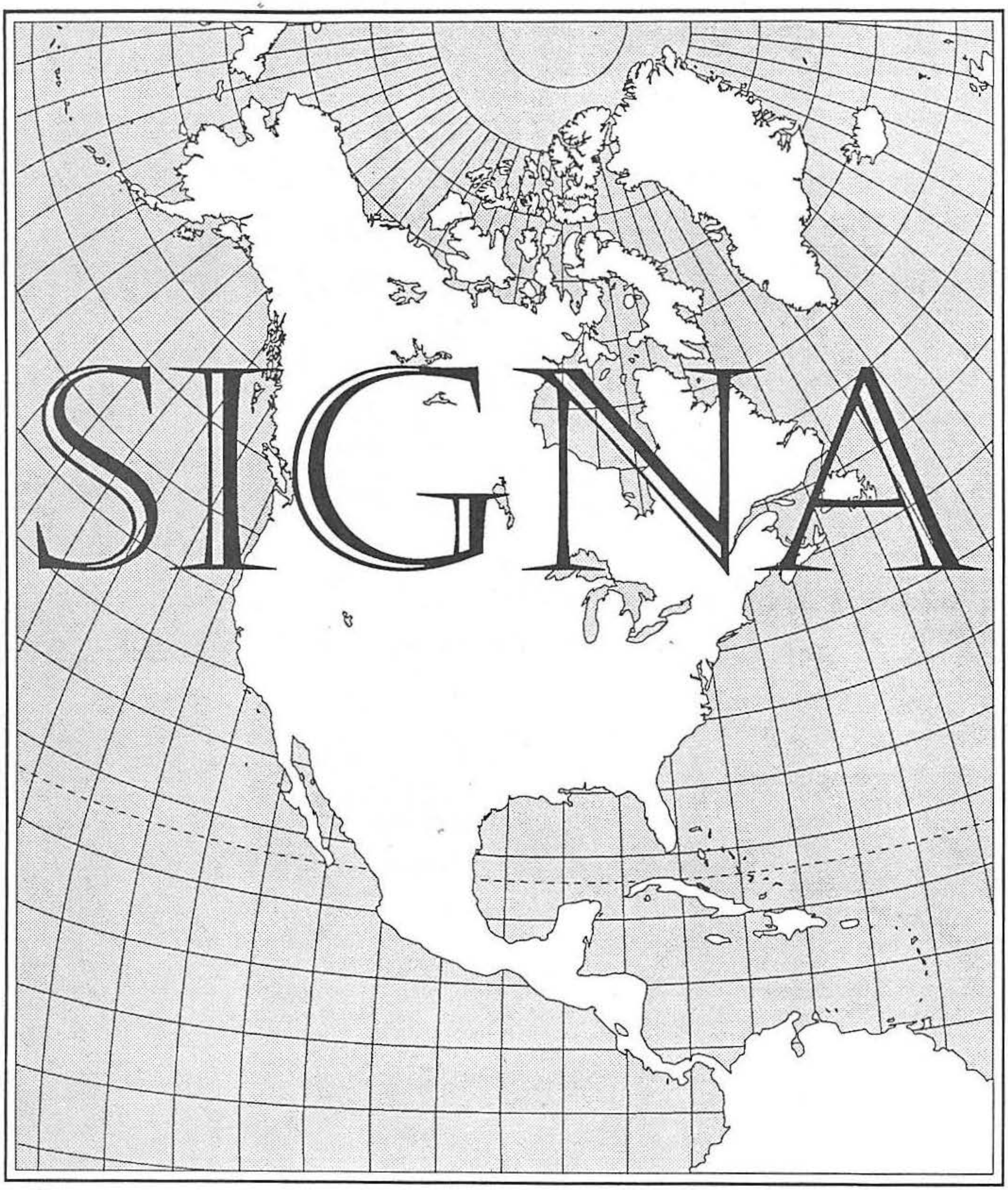
# SIGNA



## Number 62

## Spring, 1999

# Species Iris Group of North America
## Spring, 1998 - Number 60

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Joan Cooper, Robert Pries (1999); Jennifer Hewitt, Dr. Kevin Vaughn (2001)

### *Contents*

# President's Message

Dear SIGNA Members,

As I write this message the check lists are being mailed to those of you who ordered them. As with so many large projects there are always unexpected delays, and this has been no exception. I again wish to thank Jean Witt and Bob Pries and the rest of the committee for a job well done. I think you will be pleased with the end product. There will be some errors, inevitably, so if you find errors or omissions or have comments please send them to us. This will always be a work in progress and we want to make any corrections ASAP so we will improve with each printing.

Our next large project is an index of back issues. Jim Waddick and Jim Murrain have completed an index of the two issues of last year. It is being reviewed by the Board at this time and when we settle on a final format we can begin entering them into the computer. We hope to start by putting each year's index in the following spring issue of SIGNA. Then we will compile these every 5 or 10 years. Meanwhile we will be working backward on the old issues. It will take time but hopefully not as long as the checklists.

There are several changes to the standing committee chairs listed on the inside front cover. Dr. John Taylor of Arizona is our new chair of the research and grants committee. Rodney Barton is now our membership chair; you may know him from his native iris web page. I have appointed Bob Pries as chair of a new committee which will replace the old cultivar registration committee. Jan Sacks asked to be relieved of the old job so she could devote full time to the seed exchange. We are calling this the "Adopt-a-Species" committee. You will hear more about this from Bob Pries in the next issue of SIGNA.

I am sorry to say that the other change to our inside front cover will be the omission of Colin Rigby's name. I first found Colin's name on the inside front cover in 1984 as slide chairman. It has been there ever since. Colin was President for five years starting in 1988. By the fall of 1994 his name appeared as "Temporary" Editor. Except for one year when Jan Sacks took on the editing job before she and Marty became Seed Exchange Chairmen, Colin has remained as our "Temporary" Editor though he hasn't bothered to put the "temporary" in for several years. He has now asked to be relieved of this position so he can, as he put it, "become just a dues-paying member and spend some time puttering with the irises". SIGNA thanks you, Colin, for those 15 years. We still hope to see your name in the bulletin as an author.

This brings me to my next topic. If there is among you out there someone who has a desire to edit this publication, please contact me at my address on the inside front cover or at my email address <signa@bigfoot.com>. I am open to having an editorial committee so even if you don't feel you want to take on the whole job please call me.

There are several exciting events coming up in the next few years, not the least of which is our own AIS Convention in Oklahoma City. It is only a few weeks away, May 4 to 8 1999.

Our SIGNA meeting is May 5th from 11:00 a.m. to 12:30 p.m.. I hope to see many of you there. It is so nice to put a face with the name.

In June of the year 2000 there will be species irises guested at the Siberian Convention in Iowa. In November of the same year there will be another Iris Symposium in New Zealand. The agenda is not firm yet but it will probably be the first week in November. The plans at present are for a five day meeting with papers being presented in the morning and garden tours in the afternoon. They are requesting a list of SIGNA members who would be interested in presenting a paper. If any of you are interested please contact me and I will relay your name and address to the symposium chairman.

In 2001 the Iris Society of Massachusetts is hosting a Median, MDB and Species Convention. Please see the guest request elsewhere in this issue. There are many people in that area interested in species and we should see a good display.

As I was writing this letter I was contacted by e-mail by the chair of the 2003 Siberian Convention in Ontario, Canada. The hope is to make that convention a Siberian and Species Convention much like the 1996 MASS Medley in Massachusetts.

Start making your plans for one or more of these gatherings. There will be many species to be seen in the next few years.

Hope to see you in Oklahoma City,

Carla

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# *SIGNA Dues Increase*

**Because of the increase in postage and printing costs, SIGNA is forced to raise its membership dues. Effective August 1, 1999, dues will be:**

| | | | |
|---|---|---|---|
| **Single Annual:** | **$5.00** | **Dual Annual:** | **$6.00** |
| **Single Triennial:** | **$12.00** | **Dual Triennial:** | **$15.00** |

**Life Member: $125.00** (single)

**Please send membership dues to:**

**Rodney Barton, Membership**
**3 Wolters St.**
**Hickory Creek, TX 75065-3214**

# 1998 Seed Exchange

### Jan Sacks & Marty Schafer

Another seed exchange has been completed and 270 people from around the globe are planting their seed. This year we had 63 donors, including 14 brand new donors. We accept any amount of seed--if you have collected only one item please send it along. **The deadline for mailing seed in 1999 is November 10.** If your seed will not be ripe by that date, please send a list of these late species so they may be included in the seed list. Send to Jan Sacks and Marty Schafer at 337 Acton St., Carlisle, MA 01741-1432, USA. For the 1998 Seed list we took in $3353 with expenses of $668 for packaging and shipping and $612. for the list. All of the work on the seed exchange is done by volunteers. This is an important fund raiser for SIGNA and supports both seed collecting grants and the cost of our semiannual publication. Anyone who is planning a seed collecting trip can apply to the SIGNA Seed Chairs for a grant, these are small but many trips are made up of just such small grants.

This year the contributors were very generous and more of the most popular seeds were in good supply. As usual collected and hand pollinated seed were the most requested. A special thanks to all of you who make this special effort. And to the rest of you. . . PLEASE make a few hand pollinations, especially if you are growing irises in your garden which you know to be pure species. If you have access to irises or irids in the wild, please try to collect some seed. As always, we desperately need seed of arils. To 40 chromosome siberian growers--we really need hand pollinations of known pure species. These irises cross so readily in the garden that the pure species are disappearing. This is also true of the bearded species and the Pacific Coast species. The interspecies crosses--especially the wide crosses--are all extremely popular. The Louisianas were in great demand this year and also the evansias, laevigatas, and the junos. Again, PLEASE, PLEASE, PLEASE take a few minutes this season to add to the SIGNA Exchange.

All seed should be clearly labeled. Anything hand pollinated should be marked **HP**. Anything collected should be marked **coll.** with the location of the collection. On all seed it is very helpful to have a short bit of extra information as to color, or any other specific characteristic of the parent. If you are at all unsure of the true identity of your iris let us know and we will include a question mark with the listing. It is always a good idea to look up a description of a species (Mathew, The Iris is always a good reference) to confirm that your flower, plant, seed look like the description.

Here is a list of the most requested items in 1998:

| | |
|---|---|
| 98B014 | albertii - purple from Terry Varner |
| 98C041 | mesopotamica - ex. plant coll. near Beirut, Lebanon from Maurice Boussard |
| 98D050 | humilis from Jim Waddick |
| 98J063 | chrysographes - black with a trace of yellow on signal from Liselotte Hirsbrunner |
| 98K142 | hartwegii - coll. 1 mi. ESE of Shaver Lake Vil. along Dinkey Rd., 6500', light to medium lavender blue from John Weiler |
| 98Q424 | tridentata HP - from two clones from Marty Schafer and Jan Sacks |

| | |
|---|---|
| 98R444 | unguicularis 'Mary Barnard' from Carla Lankow |
| 98S445 | cristata mixed - from Barbara and David Schmieder |
| 98S449 | cristata 'Navy Blue Gem' from Marty Schafer and Jan Sacks |
| 98S452 | lacustris - from Jill Copeland |
| 98W501 | warleyensis - ex. bulbs coll. in Zeravchan Mts., Ouzbekistan from Maurice Bousard |

We have had some inquiries as to why the SIGNA seeds cannot be sent out earlier. The current timing is about as early as the seeds can be made available. Many people just barely get their seeds dry and in the mail by November 10. If we were to make the deadline earlier, we believe we would have many fewer selections on the list. From that time to January we are compiling, printing and mailing the list; taking and tallying orders, and filling packets with the necessary number of seeds, so that as many as possible can have access to them.

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The following is reprinted from the Proceedings of an International Symposium, *Gardening with Iris Species*, 1995. For the sake of breviy, footnotes to this article have been purposely omitted.

# Louisiana Iris: A Literature Review

**Philip W. Ogilvie--Washington, DC**

**Introduction:** I have done very little field work and reviewed very few herbarium sheets in preparing this paper. It is genuinely a review of the voluminous literature on this interesting group of iris. Past studies of this group have led to more confusion than clarity. I believe that there have been two major factors accounting for this phenomenon: first, an ingrained meme held by taxonomists in a rate of evolution so slow that all change must have taken place before the last ice age (while, on the contrary, I believe this to be a rapidly evolving group where most of the change has taken place in the last 12,000 to 18,000 years - and continues today); and secondly, the bias of the reviewers toward the contemporaneously popular school of systematics that each favored.

The first factor is a historical problem in plant taxonomy. Carl von Linne (Linnaeus, 1707-1778) expressed his concept of fixed species as, "There are as many species as there were originally created diverse forms". Over forty years later the now very senior Linnaeus expressed serious doubts about the stability of species and even proposed a form of evolution through hybridization, but he has been remembered for fixed species and as Adolf Koelsch called him "God's Registrar". Charles Darwin (1809-1882) believed that species changed through natural selection or "descent with modification" of an organism. Darwin's species was also less definite in its boundaries. He wrote in Origin: ". . .I look at the term species as one arbitrarily given, for the sake of convenience, to a set of individuals closely resembling each other, and that it does not essentially differ from the term variety, which is given to less distinct and more fluctuating forms".

In the Darwinian scheme this process occured over such an immensity of time that it was unobservable. He made it expressly clear in Origin: "We see nothing of these slow changes in progress, until the hand of time has marked the lapse of ages, and then so imperfect is our view into long-past geological ages, that we see only that the forms of life are now different from what they formerly were."

Unfortunately the fixity of the Linnaean species and Darwin's extremely long time frame for evolution are the principles that have shaped much of the thinking of post-Darwinian plant taxonomists.

The second problem is one of process; the nomenclature of this group has, over the years, been the victim of fads in systematics. "Splitters" have named every hybrid or variation as a new species, while "lumpers" have ignored what seem to be valid distinctions for the sake of reducing the numbers of names. Two years ago, Jeremy B. Searly expressed some of the problems in reaching a modern definition of species. He stated: "Zoologists, especially vertebrate zoologists, tend to be smug about species definitions. By and large, there is a perception that Mayr's biological species concept works well for the 'higher' forms of animal life, but works badly for many plants and is totally useless for microorganisms. Yet Mayr's definition -- that a species comprises populations which can interbreed among themselves but which are reproductively isolated from other populations of organisms -- is still the only widely familiar modern species concept. The problem for microorganisms is that reproduction tends to be asexual and so every individual can be considered to be reproductively isolated from every other. Plants also often reproduce asexually and, additionally, many forms traditionally viewed as species can interbreed forming fertile hybrids".

It is with this kind of a non-definition of species (which might be expressed as -- a species is what a taxonomist says is a species), not unlike Darwin's working definition quoted above, that we must work with in attempting to bring some kind of order to the chaos of the species of irises in the series *Hexagonae*. The problem is illustrated by two taxonomic treatments. Brian Mathew in 1989 opined, "The Hexagona group consists of five species . . .", while John Kunkel Small in 1933 listed 90 species in the southeastern United States. He stated, "A complete interpretation must await further field work and study". Even this listing failed to list one of the species he had previously named.

### The Louisiana Iris Species:

**Iris hexagona:** The first species in this group to be given a Latin binomial was *Iris hexagona*. Thomas Walter (c.1740--1789), a South Carolina rice planter, in 1788 included this iris in his Flora Caroliniana, the first American flora to be published following the taxonomic system of Linnaeus. Regrettably we know very little about Walter's training in England before coming to the colonies; however, his beautiful classical Latin and awareness of the then modern ideas of classification argue for more than a home education. His lora names the plants he collected in the vicinity of his rice plantation on the Santee River in Berkeley County, South Carolina. After he made a collection of the plants, he listed and sent them to an English herbarium: most of his specimens, including all the representatives of the genus *Iris*, have been lost. Thus we have no type specimen for this species. Further, the habitat in the type locality has changed dramatically in the past two hundred years. The rice fields are gone, replaced by rather open wet piney woods; what was sunny then is today primarily shady. I have not only conducted a thoough search of the type locality, but also organized the county garden club, armed with pictures of *I. hexagona*, to continue my search. They have located numerous colonies of *Iris virginica*, primarily in roadside ditches; a relatively large patch of an ancient white bearded iris, in a former habitation site deep in the piney woods; and the irid *Belamcanda chinensis*, a reliable indicator of eighteenth century habitation sites: but no evidence of our primary quarry. I am convinced that *I. hexagona* is now extinct in this the northern extension of its range. Probably the closest extant populations is in Charleston County, South Carolina.

**Iris fulva:** An early ninethenth century plant collector, "Mr. Lyons," sent a very unusual red iris from "the Banks of the Mississippi (sic), low ground not far from the town of New Orleans", to England in 1811. The next year John Bellenden Ker-Gawler (B.1765-1842) named this plant *Iris fulva*. While there are two synonyms for this name, this species is so recognizable and distinct that it represnts the least problem in this otherwise confused and confusing group. Frederick Pursh (1774-1820) published his Flora Americana Septentrionalis in London in 1814 in which he named the same iris, *Iris cuprea*. In 1817, Constantine Samuel Rafinesque (1783-1840) published Claude Cesar Robin's Flore Louisianaise, taken from the latter's report of early nineteenth century travels through the French islands of the Caribbean, Florida, and Louisiana, "Translated, revised and improved from the French". Robin described three iris near New Orleans, likely a part of a hybrid swarm, one of which was probably *I. fulva*. "Rafinesque hints that he had supporting specimens from Robin, but they have not been located. It is folly to dismiss Rafinesque's contention as baseless in view of the numerous unstudied collections in the Museum National d'Histoir Naturelle in Paris."

Since he was in Philadelphia from 1800 to 1805, and then in Sicily from 1805 to 1815 from whence he returned to America, we know that even if the specimens exist, he never examined them. Robin does not mention a plant collection. Rafinesque, without seeing any of these plants, assigned them Linnaean names. He named the "fleurs rouge de brique" or brick-red flowers *Iris rubescens*, but, in the discussion, indicated that it "has some affinity with *Iris cuprea* Pursh".

**Iris vinicolor:** Robin also described two other iris; one *"violet pourpre"* or red-violet, clearly the hybrid later described by Small as *Iris vinicolor:* Rafinesque identified it as *Iris virginica* L. The other one was *"blanc lave de bleu"* (white washed with blue) or *"L'iris blanche, lavee et foitee de violet, a onglet jaune"* (White iris, washed and flecked with violet, with yellow claws). Whatever this iris was (probably another hybrid), it was not the prostrate, *floriferous, leafy iris later described as Iris foliosa*. Rafinesque named Robin's plant *Iris brevicaulis*. Beyond color, washing, streaking, and flecking, a number of other characteristics rule out this identification: "few flowered, leaves shorter . . .the large flowers are half a foot in diameter". To me one of the most compelling reasons for ruling out this identification is that Robin saw all three iris blooming at the same time, March and April, while *I. foliosa* has been consistently indicated to be a late bloomer in Lousiana.

**Iris foliosa:** Rafinesque was appointed professor of botany, natural history, and modern languages at Transylvania University in Lexington, Kentucky, in 1819. There he came to know the iris later named *Iris foliosa* and so when he published the fourth part of his New Flora of North America in 1838, he changed the description of *I. brevicaulis* to correspond to this iris. This practice is precluded by Article 53.1 of the *International Code of Botanical*

*Nomenclature*; thus the name *Iris brevicaulis* is invalid for this iris, since the name was already applied to Robin's white hybrid.

In 1894 a Laura S. La Mance of Pineville, Missouri, sent an iris she had collected in Benton County, Arkansas, to Mr. J. N. Gerard of Elizabeth, New Jersey. The iris was small, prostrate, with large flowers down among the leaves and had been collected on limestone cliffs along the Mississippi River. The next year Mr. Gerard named this specimen *Iris hexagona* var. *La Mance* in honor of Mrs. La Mance. For a variety of nomenclatural reasons, the most important being that this varietal name was not elevated to the species level before the valid species name of *I. foliosa* was published, this name can be considered another synonym of *I. foliosa*. Finally in 1902, K. K. Mackenzie and B. F. Bush published a valid name for this prostrate, *Hexagonae* iris, *Iris foliosa*. Co-types were deposited in the herbaria of the Missouri Boranical Garden and National Arboretum. Five years later Francis Potter Daniels found a population of white to cream-colored iris of this same species which he named *Iris foliosa* var. *boonensis*.

The species concept of the next taxonomic actor on the stage has led to much of the nomenclatural confusion in this group; however, this sense of the dramatic played a lead role in popularizing Louisiana Iris. John Kunkel Small (1869-1938) was born in Pennsylvania, but moved to New York City in 1892. where he spent the rest of his extremely productive life associated with Columbia University and the New York Botanical Garden. The focus of his work was the botany of the southeastern United States and he became "one of Florida's first and most outspoken biologists, photographers, authors and conservationists". His many collection trips to and from the Carolinas, Georgia, and Florida took place from 1891 until shortly before his death. This period included ". . .wholesale elimination of plants and associated animals, drainage programs promoted by Napoleon Bonapart Broward and . . . Henry M. Flagler's railroad down the eastern coast opened up the state in a manner previously unequaled". Small documented what he observed and through his writings made the biological and horticutural world aware of these destructive changes.

**Iris savannarum:** In 1913 he listed only two species of *Hexagonae* iris in his Flora of the Southeastern United States, but, in these trips he encountered several iris in which he found a great deal of variation, and, as he could associate a pattern of morphological characteristics with different localities, he began to name new species of iris. From 1925 to 1929 he published an illustrated series of diagnostic depictions of iris in Addisonia. These plates with descriptions included both previously named iris native to the United States and new species named by Dr. Small. All over the savannas of peninsular Florida, he found a blue/purple, erect iris, certainly related to *I. hexagona*, but distinct, which he named *Iris savannarum*. He also found white forms of this, the commonest iris in the state.

**Iris kimballiae:** From Apalachicola on the gulf coast of the panhandle of Florida, Miss Winifred Kimball sent an iris that occurred in the swamps along the lower Apalachicola River. It too was an erect iris related to *I. hexagona*. Small named it *Iris kimballiae.*

**Iris vinicolor:** That same spring (1925) Dr. Small traveled by train from Florida to west Texas through New Orleans. As the train crossed the delta of the Mississippi River, the native iris were in bloom. After his work with Florida representatives of the *Hexagonae*, all of which were either blue/purple or white, Dr. Small was most impressed with a "winecolored flag" which he named two years later as *Iris vinicolor*. This trip brought together an enthusiastic group of local horticulturists who had been collecting the many natural variations of these iris with a botanist of national reputation. Dr. Small became an enthusiastic participant and announced to the world through the New York press that the New Orleans area was "the iris center of the universe".

**Iris flexicaulis:** A Mr. B. C. Tharp of Texas sent living plants of an iris collected along the "Nueces River". This is probably a confusion with the Neches River just west and south of the Louisiana line. Many *Hexagonae* iris have been collected in this area, but detailed floras of the area south of Houston list no plants in the genus *Iris*. Dr. Small named this plant *Iris flexicaulis* and identified plants that he collected as far east as New Orleans as representing this species.

**Iris rivularis:** As early as 1921 Dr. Small collected a distinctive, erect, blue/purple, *Hexagonae* iris in the northern-most Atlantic coast county of Florida, Nassau County. Small believed the creek where he collected this iris to be part of the St. Marys River drainage to the north, but he had misread the Geological Survey map, and the creek actually flowed into the Nassau River to the south. It was later collected in the St. Marys basin in both Florida and Georgia. Dr. Small named this iris, *Iris rivularis*.

**Iris albispiritus:** In January 1924, Dr. Small wrote to Mr. Walter M. Buswell, a plant collector of Fort Meyers, Florida, saying, "I get reports of white iris from various localities. I suppose the plants or colonies are albinos of the various colored species. Anyhow, I would like to have rootstocks to grow." Finally, in 1927, Mr. Buswell sent Dr. Small rhizomes which he set out in the "iris plantation" of the New Yorlk Botanical Garden. That October "six or eight . . . flowered . . . the flowers showed marked differences from those of *Iris savannarum*, the blades of the sepals and petals are crisped and finely many toothed, the style branches are often toothed along the edges, and the style-appendages are more sharply cut". This "Ghost Iris" was named *Iris albispiritus*.

**Iris giganticaerulea:** On that same trip to New Orleans in 1925, on which Dr. Small for the first time saw the iris fields of Louisiana, he found a very large, blue/purple, erect, *hexagona*-like iris; this "Big Blue Iris" he named *Iris giganticaerulea*. In the 1925 to 1929 series, he named an additional five "species", fairly clearly either variants of already named species or members of the hybrid swarm represented by *I. vinicolor*.

In 1931 Dr. Small together with E.J. Alexander pre-published the iris section of Small's Manual of the Southeastern Flora as "Botanical Interpretation of the Iridaceous Plants of the Gulf States". In this publication J. K. Small named 41 new species (and one orthographic variant) and Alexander named an additional 35, all without designating type specimens or localities. Small stated his concept of iris species as: "Our usual criterion for assigning the status of species is an isolated colony or colonies, the plants persisting through propagation by rootstocks and by an annual accretion of seedlings without showing variation in the characters of the perianth."

The only type specimen of Dr. Small's *Hexagonae* species in the Herbarium of the New York Botanical Garden is an envelope containing the dissected flower-parts of *I. giganticaerulea*. Several other "Type" folders contain plates from the Addisonia series.

The definitive criticism of Small and Alexander's naming of iris on the Mississippi delta was published by Percy Viosca, Jr., in the Bulletin of the American Iris Society in 1935. Viosca also gave his definition of a species: "In this paper my criterion of an Iris species is a large aggregation of plants with reasonably defined similarities of structure, freely interbreeding wherever in sufficiently close proximity, the separate colonies of which have similar ecological requirements, and the aggregation as a whole having a geographic range which can be defined in terms of physiographic features and throughout which colonies are found in all suitable localities." And later, in the same paper, he criticized Small's concept: "A profusion of closely related plants in a limited area with infinitesimal differences not readily classifiable, presenting phenomena not repeated in exactly the same form wherever the same biogeographic conditions recur, certainly do not suggest themselves as distinct species in the ordinarily accepted sense of the term."

Using these criteria, he concluded that there were only four species of iris in Louisiana, only three of which were members of the *Hexagonae* (*I. foliosa, I. fulva*, and *I. giganticaerulea*). Most of the names assigned by Small and Alexander he interpreted as "variants and in part as natural hybrids". Most subsequent reviewers seem to have accepted that his analysis applied to all of Small's species whether from Louisiana or Florida. Viosca, however, indicated that the Florida species would be the subject of a future study, one that, regrettably, was never published.

At the same time Viosca was working in Louisiana, H. Harold Hume was working with the native iris of Florida. He published two papers in the Bulletin of the American Iris Society in 1933 and 1934; between these two studies he was able to identify all of Small's Florida species in the field, at their type localities and elsewhere, establishing rough specific distributions within the state.

Two years later Robert C. Foster published his major revision of North American iris. With regard to the *Hexagonae*, Foster concluded that: *I. fulva* was a valid name of a valid species; *I. hexagona* was the valid name for all of the erect blue/purple *I. hexagonae*; *I. savannarum, I giganticaerulea*, and *I. flexicaulis* were all varieties of . *Hexagonae*, introducing the orthographic variant *I. giganticaerulea; I. foliosa* should be a synonym of *I. brevicaulis* Rafinesque, which should apply to the prostrate blue/purple *Hexagonae*; *I. foliosa* var. *boonensis* was a valid variety for white *I. brevicaulis*; the Small species from Louisiana that he discussed, other than *I. giganticaerulea* and *I. flexicaulis,* should be treated as hybrids of *I. hexagona* var. *giganticaerulea* and *I. fulva;* and with regard to *I. kimballiae*, "More material, whose place of origin is known, should be studied to clear the uncertainty". He was silent on the remaining named and described taxa.

His conclusion on the position of *I. flexicaulis* was: "In certain essential points, mentioned in the description, this plant stands nearer *I. hexagona* than it does to *I. brevicaulis*. Nevertheless, in general appearance it seems quite close to the latter, so much so that it might almost be a transition between the two. Field study and more extensive herbarium material may show that it should be regarded as a variety or form of *I. brevicaulis.*"

Since Foster did not share with us in which "essential points" *I. flexicaulis* agrees with *I. hexagona* and since *I. flexicaulis* is so clearly a prostrate *Hexagonae*, I find it difficult to follow Foster's logic in making it a variety of *I. hexagona*, unless one concludes that he is right that it represents a transition between *I. hexagona* and *I. foliosa* and that all the blue/purple *Hexagonae* are placed in one species, *I. hexagona*. He may have given us a clue to the problem with the statement: "There has been much confusion in herbaria between this species [*I. foliosa / brevicaulis*] and *I. hexagona*. At their most characteristic, there seems to me to be little doubt of their specific difference, but in many cases there are integrades. The character of stem flexuosity, one of the distinguishing marks of I. *brevicaulis*, is almost impossible to determine in dried specimens, and this naturally adds to the difficulty of separating the two in herbarium material."

Since we have no indication that Foster ever saw living material of *I. flexicaulis*, it seems he fell into the same trap he describes above. Caroline Dormon, a trained scientific observer, artist, and horticulturist, not only considered *I. flexicaulis* to be related to *I. foliosa*, but also considered it a recognizable form.

Foster rejected *I. foliosa* in favor of Rafinesque's *I. brevicaulis* on the tenuous grounds that: "It has been necessary to change the name of this species, which has been known since 1902 as *I. foliosa,* since Rafinesque in 1817 described an *Iris brevicaulis*, a description which he amplified in 1837, the two together leaving no doubt as to the identity of this species and *I. foliosa*." I discussed above why I believe that this homonym must be rejected as a violation of Article 53.1 of the *International Code of Botanical Nomenclature* and that the valid name remains *I. foliosa*. Apparently Foster never examined Robin's original description. After publishing the name *I. brevicaulis* in 1817, for Robin's 1807 description, Rafinesque went well beyond "amplifying" that description in 1837, but rather changed to fit a different iris.

One of those keen Louisiana horticullturists, a founder of the Mary Swords DeBaillon Louisiana Iris Society, was a Professor of Horticulture at Southwestern Louisiana Institute in Lafayette, Louisiana, Ira S. Nelson. Nelson interested Dr. L. F. Randolph, Professor of Botany Emeritus at New York State College of Agriculture, Cornell University, in extending his studies of iris karyotypes to the native iris of Louisiana. The two cooperated on a study of introgressive hybridization in a mixed population of *I. fulva, I. foliosa,* and *I. giganticaerulea*. The concept of introgressive hybridization was advanced in 1938 by Edgar Anderson; it asserted that hybridization was an important contributor to evolution through the back crossing of hybrids with plants representing either of their parental lines and thus mixing the genetic material. Randolph, Nelson, and Plaisted concluded that there was "no evidence that introgression had altered significantly the status of the 3 cross-compatible species of Louisiana irises as stable taxonomic units".

The discussion, included in the above paper, of the ecological requirements of the various populations of iris is intriguing in the light of a principle that Anderson felt so strongly about he printed it in capital letters: "The second generation will be made up of individuals each of which will require its own peculiar habitat for optimal development." For example, one of the populations of prostrate iris "was growing in a drier situation than the typical habitat". The plants "were making vigorous growth and appeared well adapted to this atypical habitat." After examining the morphology of plants in this population and comparing them to populations in "typical habitats", it was concluded that while the populations undoubtedly differed physiologically, Randolph, Nelson, and Plaisted could not distinguish them morphologically on the basis of the 13 characteristics selected.

**Iris nelsonii:** The study included large red and yellow forms growing in the Abbeville area of Vermilion Parish discovered by W. B. McMillan in 1937, a year before Dr. Small's death, but never seen by him. Two small populations of "Abbeville Reds and Yellows" were included in this study, one from the Abbeville Swamp and one from "New Iris Heaven". They grew in exceptionally deep water and relatively dense shade, neither the preferred habitat of any of the species nor of the typical hybrids. They appeared to be advanced generation hybrids. After Dr. Ira S. Nelson died (November 14, 1965), Dr. Randolph honored him by naming *Iris nelsonii* for the unusual iris populations previously known as "Abbeville Reds and Yellows". The type specimen was deposited in the Herbarium of the L. H. Bailey Hortorium, Cornell University,Ithaca, New York.

There has been much dispute over the validity of *I. nelsonii* but until the recent work of Dr. Michael L. Arnold of the Department of Genetics in Franklin College of Arts and Sciences at the University of Georgia, Athens, Georgia, and others, there has been little information that might clarify its status. Dr. Arnold has been seeking to resolve long-standing quesions of taxonomic relationships among the *Hexagonae* through the use of modern genetic techniques. The study of molecular evolution ascertains the sequences of nucleotides within the deoxyribonucleic acid (DNA) molecule and then compares the sequences found with the sequences in other organisms. This area of science is quite new. The methods of determining sequences were only developed in 1977. Such identified sequences are then used as indicators of the genetic participation of a species in a tested organism and ultimately of their taxonomic levels and relationships. Dr. Arnold and his colleagues have found evidence of introgression between *I. fulva, I. foliosa,* and *I. giganticaerulea*, contradicting Randolph's 1967 study. He has also found that all three of these species have contributed to the genetic make up of *I. nelsonii*. In another paper he and a coworker concluded that, on the basis of his studies of the molecular structure of the genetic materials and the ecology of the plants, *I. nelsonii* is a valid stable species resulting from hybridization. As a result of extremely preliminary investigations of some of the Florida populations, most of which were probably *I. savannarum*, Arnold and his colleagues see some differences from *I. giganticaerulea*, but, in their opinion, not enough to justify specific distinction.

The most recent review of this group was written by Dr. Norlan Henderson and appeared in No. 291 of the *Bulletin of the American Iris Society* (December, 1994). Since this commentary appeared in a specialized horticultural publication, without the scrutiny of Dr. Henderson's peers in the field of plant taxonomy, it has not been considered in this paper. In my opinion, the conclusions do not add clarity, but rather add to the confusion with new names lacking scientific description.

**Discussion:** To learn how the present condition of this species complex came about we must examine the late-quaternary geography of the United States. During that period the Wisconsin Glacier covered the Great Lakes, all of what is today New England and New York; the northern halves of Ohio, Indiana, Illinois, and Iowa; and the eastern half of Nebraska. The height of the Wisconsin glacial period, about 12,000 to 18,000 years ago, saw the northern shoreline of the Gulf of Mexico near what today represents a bathimetric contour of 60 to 120 meters or 200 to 400 feet. Current thought, reflecting the most recent satallite data, is that the last major low sea-level stand occurred approximately 12,000 years ago and that the apparent sea rise occured in two stages. In the first 8,000 years following the end of this period, the relative sea-level rose to an average rate of 10 millimeters (0.4 inch) per year, while in the next 4,000 years the average slowed to only 0.5 millimeter (0.02 inch) per year. During the past one hundred years the earth has been experiencing a warming trend and today the rate is about 2.4 millimeters (0.1 inch) each year. This relative rise in sea-level is composed of two elements, the actual sea-level rise from melting ice and the sinking of the shore line, primarily from the deposition of alluvial soil. During this period, the coastal plain was much broader, marshy and dissected by many river channels. I suspect that at this time there were the following ancestral species of *Hexagonae* iris on the coastal plain: a red/yellow upright form, a blue/purple upright form, and a blue/purple prostrate form. As the climate warmed, sea-level rose and the glaciers retreated. These iris were isolated in the 'islands' of river deltas along the resulting coastal plain.

While the normal method for the distribution of *Hexagonae* seeds is by floating down stream, it is clear that there must be some other mechanism to carry some up stream; otherwise we would not find them today in areas covered or strongly impacted by the Laurentide Ice Sheet. (Since dabbling-ducks will eat almost anything floating on the water, they may be a mechanism for up-river distribution. It is also possible that mammals such as racoons or opossums may eat the ripening capsules and thus distribute the seeds. These mechanisms should certainly be tested.) The blue/purple upright form may have colonized the Apalachee Bay Area and from there migrated east to the Atlantic Ocean, north to South Carolina, south to the Big Cypress Swamp of Florida, and west to east Texas. As seeds germinated and plants grew in divers environments, natural selection led to populations differing physiologically and morphologically. As the separation became greater, the degree of difference grew more pronounced. A form such as *I. savannarum* adapted to an upland prairie while a form such as *I. giganticaerulea* adapted to ever more alkaline coastal marshes, with major selection events coinciding with each hurricane. In 1938 William T. Penfound and Edward S. Hathaway tested a clolony of *I. giganticaerulea* (located on the brackish Oak Island transect in the Pearl River alluvial plain 40 miles northeast of New Orleans) for salt tolerance. I am sure that if we could test the same populations today for salt tolerance, we would find that the population's tolerance to salt has increased significantly. The Florida members of this group, with populations differing enough for Small to name as species and for Hume to identify and establish at least preliminary distributions, certainly seems like an organism rapidly evolving into a separate species. More work along the lines of that being done by Michael Arnold will be necessary to determine the exact status of these taxa.

The red/yellow erect form and the blue/purple prostrate form seem to have colonizd the delta of the Mississippi. The red/yellow erect species, I. *fulva*, seems to have migrated to the north only, up the Mississippi valley to southern Illinois. The prostrate form appears to have colonized not only the Missssippi valley, but much of that river's basin; as far north as Pelee Point National Park on the northeast shore of Lake Erie in Ontario, Canada; as far northwest as Leavenworth, Kansas; as far southeast as the panhandle of Florida; and as far southwest as the Big Thicket of east Texas. In this process it also seems to have diversified into at least two populations differing primarily in habitat preference: *I. foliosa* preferring dryer upland situations and *I. flexicaulis* remaining in the ancestral marshy habitat. The level of difference is still not clear and again much work remains to be done..

When the three forms came together in the Mississippi delta, the explosion of hybridizing produced a state of panmixia or a hybrid swarm of forms, some of which were named by Small and Alexander as species. The first of these hybrids to be named in accordance with the rules of botanical nomenclature was *Iris vinicolor*. All subsequently named hybrids should be classified under this name. The only known stable populations resulting from this reticular evolution has been named *I. nelsonii.*

**Conclusions:** The evidence from the literature could be interpreted as supporting only three species, conservatively: *I. hexagona, I. fulva,* and *I. nelsonii.* (According to this reasoning *I. hexagona* would in turn be divided into eight subspecies: *I. hexagona* subsp. *hexagona, I. hexagona* subsp. *foliosa, I. hexagona* subsp. *savannarum, I.* hexagona subsp. *kimballiae, I. hexagona* subsp. *flexicaulis, I. hexagona* subsp. *rivularis, I. hexagone* subsp. *albispiritus,* and *I. hexagona* subsp. *giganticaerulea*. The literature may, with equal validity, be understood to be supporting ten species: *I. hexagona,I. fulva, I. foliosa, I. savannarum, I. kimballiae, I. flexicaulis, I. rivularis, I. albispiritus, I. giganticaerulela,* and *I. nelsonii.* Until adequate field collecting and laboratory testing has been done to clearly demonstrate the true relationships and levels of differentiation in this group, I believe the second scheme will lead to less confusion.

**Alan McMurtrie, Willowdale, Ont., Canada writes:**

Exciting News: one of my second generation *Iris mcmurtriei* hybrids is bloomed! Its bud was pale yellow like *winogradowii*! The overall colour when it opened was cream. Its style arms were white with wide dark blue stripes on either side! There were blue veins on the fall, but they weren't on the fall blade. There was a soft yellow highlight around the end of the fall ridge. It's quite nice. I couldn't have asked for a more special first $F^2$ bloom!

Another second generation *mcmurtriei* hybrid from a different cross also bloomed. It was light blue, with small amount of soft yellow influence. These are the only two Iris x *mcmurtriei* clones I expect to bloom this year.

A "Cantab" x *winogradowii* hybrid is currently open (April 8, 1999). It is definitely true! Usually crosses like this turn out to be something else. It is cream overall with a 'masked' blue fall blade (ie. very subdued). The flower has *winogradoii*'s shape. It's bud was pale yellow.

The following article is reprinted from the *Special Publication* of the Society for Lousiana Irises, 1995

# Natural Hybridization--Evolution by Amalgamation

**Michael L. Arnold, PhD.**

The study of natural hybridization between plant species is of fundamental importance of understanding the evolution of plants. This is due to the fact that an estimated 70% of all plant species have arisen through natural crosses between other species. Furthermore, almost all of the food and fiber crops used by mankind reflect the result of natural or manmade crosses resulting in improved productivity and nutritive value. The studies described in this paper highlight the power of state-of-the-art genetic technology when applied to a paradigm of the process of natural hybridization. This model system is made up of species belonging to the plant group known as the "Louisiana Irises". This species group consists nominally of *Iris fulva, Iris hexagona, Iris giganticaerulea, Iris brevicaulis* and *Iris nelsonii*. The findings described in this paper reflect six years of research involving genetic and ecological studies. Results from initial studies were described in a 1989 AIS report by Bennett and Arnold ; (AIS Bulletin No. 273, pp 22-25). The research carried out by Dr. Arnold's group has included, among other things, analyses of pollen tube growth rate, seed abortion and patterns of insect and bird pollinations.

From the above studies we have been able to discern the success of natural crosses between the plant species *I. fulva, I. giganticaerulea* and *I. brevicaulis*. In addition, we have been able to document the formation of a hybrid species *(Iris nelsonii)* from natural crosses between *I. fulva, I. giganticaerulea* and *I. brevicaulis*. These plants hyridize readily under artificial conditions and there are numerous natural hybrid populations formed by crosses between these species. Because of this we have been able to determine what processes are important in limiting and promoting natural and experimental hybridization. Such an understanding results in a better definition of how natural hybridization has come about in numerous plant species. In addition, data from these studies are applicable to plant breeders who need to know what mechanisms might restrict their attempts to cross agriculturally important species and thus limit the efficiency of crossing programs.

Southern Louisiana is a beautifully mysterious place. The bald cypress trees adorned with gray-green strands of Spanish moss strike the senses with a feeling of antiquity. This feeling is heightened by the ever-present alligator, a species that has been present for so long during the earth's history and yet has changed so little in appearance that it qualifies as a "living fossil". As I surveyed this scene once again, I couldn't help but feel that this environment was the place for biological evolution to occur. Of course the rational scientist within me could not be totally dismissed and thus I knew that the musty smell, verdant growth and already oppressive heat and humidity on this spring morning represented no especially suitable place for the origin of new biological forms, no more than for example a desert landscape.

My feeling of childlike expectation did not mainly derive from sensory excitement from the landscape, but rather from my hope that my colleague Bob Bennett and I were on the verge of rediscovering a fascinating biological phenomenon. I say rediscovering because that was exactly what we were attempting to do. As with most scientific endeavors we wished to test a previous conclusion made by an earlier generation of scientists. In this case it was to determine if *Iris nelsonii* had resulted from the intermixing of genes from three other iris species. This species, only found in the confines of extreme southern Louisiana near the city of Abbeville, was formally named in a 1966 publication by Dr. L. F. Randolph.

Although discoveries of new species are of extreme biological interest *per se*, the naming of *Iris nelsonii* carried with it an additional twist. Randolph concluded that not only was this plant type a novel species, but that three previously recognized species of the so-called "Louisiana Irises" had contributed to its genesis. This genesis, according to Randolph, came about from the transfer of pollen and the formation of hybrid seeds between *Iris fulva, Iris giganticaerulea* and *Iris brevicaulis*. The transfer would most likely have been accomplished by a combination of the somewhat ungainly flights and landings of bumblebees and the delicate flitting and probing hummingbirds. Yes, that is right, the birds and the bees, both playing a role in the bringing together of gametes from three species, beginning with an initial hybrid between two of the species, this hybrid mating with the third parent species, this tri-hybrid forming offspring with one of the species, or other hybrid plants, and so forth until a unique blend of genes evolved that produced a unique plant that was discovered and named by a perceptive naturalist.

***I. fulva***

That is how it must have happened if Randolph was right. Yet as we have examined how often iris species are able to form that initial generation of hybrid, we are left with the impression of extreme improbability. In seven years of searching for that elusive $F_1$ (the designation for the first generation hybrid) betwen *I. fulva* and *I. giganticaerulea* or *I. fulva* and I. *brevicaulis* or I. *giganticaerulea* and *I. brevicaulis* we had found exactly zero such plants. How could this be? We go to swamps, marshes, open woodlands and pastures and find hundreds, even thousands, of beautifully colored and uniquly shaped plants. These are hybrids that reflect the work of those same birds and bees that must have led to the creation of *I. nelsoni,* yet all of these hybrids are always generations removed from that first $F_1$. "They must be there!" we keep reiterating as we dissect the DNA molecules from another set of individuals that possess morphologies characteristic of not one, but two or more species.

We scan the results of our genetic analyses, searching longingly at times for the combinations of genes from two species, and we find them, but not in the 50/50 mixture predicted if the plant is that first generation offspring from an interspecies mating. Instead we find an admixture where one species or the other has contributed almost all of the genetic material and one or more of the species have contributed relatively little. It is as if a Hatfield and a McCoy, improbably as it might seem, spawned one progeny many generations in the past and that child then married a Hatfield and their childrlen married Hatfields,and so forth until today. That first Hatfield X McCoy $F_1$ is gone, but we know it was there because the Hatfields of today possess some of the genes from their hated rivals. However, the lack of any present-day Hatfield X McCoy $F_1$s leads us to the conclusion that such "illegitimate" matings are not allowed very often. This is apparently also the case for the nonapocryphal iris example $F_1$s are difficult to form.

Such findings lead us to envision processes that restrict the formation of $F_1$ individuals to a trickle while later generations of hybrids gush forth following the forays of pollinators. One process that may restrict $F_1$ genesis, known since the days of Darwin, involves a competition between pollen tubes from the different species. This competition may resolve itself merely as a sprint. Like sprinters, the pollen grains all line up at the starting line (the stigmatic surface of the flower), placed there by the visitors seeking nectar much as a C.O.D. package is dropped off with payment received. However, in this case the delivery man leaves two packages, one from a family member and one from a stranger. As with all races there can only be a single winner. As Darwin noted, the predicted outcome of such a race is that pollen from flowers belonging to the same species will be the exclusive (or nearly exclusive) winners, resulting in their fathering the majority of offspring. So it could be that pollen tubes from the iris species grow more quickly on flowers belonging to their own species.

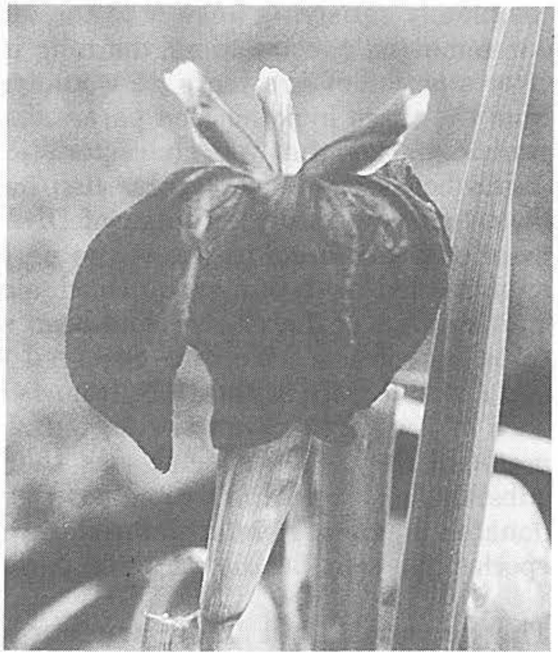
Such is the finding for the irises. Crosses made in the greenhouse involving mixtures of *I. fulva* and *I. giganticaerulea* pollen result in few if any hybrid offspring. "Are the pollen tubes having races?" we wonder. We measure the tubes and find that *I. fulva* tubes grow more quickly than *I. giganticaerulea* whether on an *I. fulva* or an *I. giganticaeulea* flower. A "foot race" could indeed explain the lack of hybrids when the mixtures are placed on the *I. fulva* flowers, the *I. fulva* tubes would reach the finish line (the ovules) first, but such a race can not explain the lack of hybrids when the pollen mixtures are placed on the *I. giganticaerulea* flowers. If it were simply a matter of speed, *I. fulva* tubes would win the races and thus produce 100% hybrid seed. It is also known that even if the pollen tubes of a different species grow as quickly or even more quickly than those tubes orginating from same-species pollen, the majority of the resulting young will demonstrate only genes from one species. In other words, the hybrid seeds must die more frequently than those non-hybrid individuals. Natural selection is once again revealed.

Whatever the cause---a speed race, an endurance race (Do *I. fulva* tubes "tire" more easily than their opponents?) or selected miscarriages of the hybrid progeny, the barrier is real and restricts the possibility of $F_1$ formation, but seemingly not the formation of future generations of hybrids. This leads us to predict that pollen tubes from those rare $F_1$ plants will grow equally as well as the pollen from the parent species on the flowers of the parents thus resulting in the myriad of hybrids present in southern Louisiana.

What does all this discussion of pollen tube races, miscarriages and natural selection have to do with our search for the elusive *I. nelsonii*? "Everything?" we answer. These are the processes that would have determined when and how that initial $F_1$ that acted as the bridge between three species of Louisiana Irises. This bridge eventually, through myriads of further hybrid generations, led to that biological entity now recognized as *I. nelsonii*.

However, as Bob and I drove further back through cultivated fields toward the waiting swamps, we failed to discuss these more erudite topics. but rather we were contemplating what type of habitat had been described as typical for *I. nelsonii*. Indeed, the habitat description presented by Randolph was a key motivating factor for our search. Of course from a purely practical standpoint we were using this information to determine where we should search for *I. nelsonii*. "It's no good looking for *I. nelsonii* in the sunlit marshes typical of *I. giganticaerulea*," mused my partner. I agreed, but we also knew that we couldn't expect to find this species along the bayou margins in shallow water like *I. fulva* or in the dry semi-open hardwood forests where we had seen *I. brevicaulis*. These conclusions were based on Randolph's description of a species that occurred in the darkened confines of cypress swamps with water depths exceeding one meter. The problem with using this information as a predictor of where to find this species was that a suitable habitat did not (as we had repeatedly demonstrated ) guarantee the presence of *I. nelsonii*.

Even more important than the practical aspects of Randolph's information was the tantalizing biological implications, for if *I. nelsonii* were truly a new species derived from crosses among the three Louisiana iris species it must inhabit a distinct niche relative to its progenitors. This was mandated by ecological theory that states that no two species can occupy identical habitats at the same time and place. It thus appeared from Randolph's description that *I. nelsonii* fulfilled one requisite for species status, habitation of a unique environment.



***I. nelsonii***

We were still in the midst of our treasure seeker's conversation (that was exactly what we were doing, seeking biological treasure) when we arrived at the boundary of the swamp. I slipped out of the van and began suiting up for our foray into the native habitat of Cajuns. This meant pulling on chest waders and hoping not to repeat my last performance involving kicking a submerged cypress "knee" and toppling into the stagnant swamp water. The dew wetted our waders as we pushed our way through the vegetation border that marked the entry into the swamp. This dampness guaranteed that the inadvertent kicking over of a fire ant bed would result in both dirt and ants adhering to our boots and, if we were not careful to stamp this off, a subsequent and painful reminder of another inhabitant of this realm.

When the dense plant life was finally penetrated we probe through into the now-familiar environs dominated by the bald cypress. However, in addition to this paradigm of all that is southern Louisiana, we had finally located our quarry. There stood iris flower stalks that were over one meter in height, crowned with flowers that approached in color the brick-red ornaments of *I. fulva*, but were massive in comparison to the petite *I. fulva,* more similar in size to the aptly named *I. giganticaerulea*, but without any hint of the blue that makes up the remainder of the Latin name. The lethargy that always accompanied the struggle for movement in chest waders and the weariness associated with slogging through numerous perfect *I. nelsonii* habitats (perfect that is except for the notable absence of this species) both disappeared.

After having spent so much time and effort in locating *I. nelsonii* one might think that we would immediately begin to collect the necessary leaf samples from which we would later isolate DNA, that would be enzymatically dissected to test for the presence of genetic material from the putative parents. So caught up in the fervor that goes along with any scientific discovery (or rediscovery!), we did nothing for a very long time but scramble betwen clumps of these plants and exclaim "Look at the size of those flowers!" "Look how they stand so upright compared to those of *I. fulva*!" and "Look how broad the leaves are!" After a time, of course, we came to our collective senses and began the methodical sampling that is much more characteristic of how science actually operates. However, even as we clipped portions of leaves from numerous plants we would still ask: "I wonder if Randolph was right?" "Are these plants really a combination of *I. fulva, I. giganticaerulea* and *I. brevicaulis* genes?" A completely satisfying answer to the question of the parantage of *I. nelsonii* depended upon our removing chest waders, donning lab coats and going on a molecular fishing expedition. Over a period of five years we would continue our analyses of more and more pieces of DNA from the three hypothesized parental species and their putative hybrid offspring. Each new genetic marker that was characteristic for either *I. fulva, I. giganticaerulea* or *I. brevicaulis* would be used as a molecular fish hook. We would drop the hook (using state-of-the-art biotechnology) into the milieu of DNA from *I. nelsonii* and determine whether or not that peice of DNA, found in one of the parents, also existed in the genome of *I. nelsonii*. These analyses led to the following conclusion: Randolph, by studying mainly characters such as fruit shape and flower size, had been able to detect the vestiges of hybridization betwen the three iris species. We thus observed the molecular footprints of *I. fulva, I. giganticaerulea* and *I. brevicaulis* in the DNA from *I. nelsonii*. Not all of the pieces of DNA extracted from each of the three parents were found in their improbable offspring; however, some of the material from each species was there. Using the genetic hooks allowed us not only to substantiate Randolph's conclusion of parentage, but also to determine which species had donated the most genetic material during the formnation of *I. nelsonii* and which of the three species had acted as the maternal parent.

The increase in our understanding of the evolutionary history of *I. nelsonii* once again depended upon the use of very recently-developed molecular technology. To attempt to quantify the relative genetic contribution of each of the three parents required a barage of genetic markers. Only by screening for numerous genetic components from each of these three species could we hope to determine whether one of the species had been a main contributor to the genome that now resides in *I. nelsonii*. In contrast, determining what the maternal parent was in the origin of this hybrid species depended not on this brute force approach, but rather on molecular genetic finess. What was required for determining the "direction" (i.e., which parent species contributed the pollen and which received the pollen) of the interspecific hybridization event was a marker that was specific to the maternal (or "receiving") parent. In addition, we needed a genetic molecule that would not have combined material from the multiple parents. Fortuitously (at least for evolutionary biologists) such a molecule does exist. This molecule is called chloroplast DNA because it is found only in those subcellular bodies. Most important, this molecule does not undergo genetic recombination and it is amost exclusively inherited from the mother plant. Also fortuitously (particularly for this evolutionary biologist) *I. fulva, I. giganticaerulea* and *I. brevicaulis* possess chloroplast DNA molecules that have DNA level difference.

An exhaustive survey of genetic characters from *I. fulva, I. giganticaerulea* and *I. brevicaulis* gave a clear picture of the series of events that most likely occurred during the genesis of *I. nelsonii*. The first conclusion was that the maternal parent for this hybrid species was *I. fulva*. This was indicated by the presence of chloroplast DNA in *I. nelsonii* identical to that found in *I. fulva*. In addition, *I. fulva* must also have acted as the predominant parent in the progressive crosses that eventually gave rise to *I. nelsonii*. This latter conclusion was generated by the observation of an immense number of *I. fulva* genetic markers in *I. nelsonii* with only a sporadic occurrence of *I. giganticaerulea* or *I. brevicaulis* genetic material.

What kind of evolutionary scenario would explain both the occurence of an overwhelming majority of the *I. fulva* genetic markers and the chloroplast DNA from this same species? A rather simple evolutionary story can accommodate both of these obvservations, An initial hybrid generation was formed between two of the three parent species. This may have involved birds or bees transferring pollen from either *I. giganticaerulea* or *I. brevicaulis* onto flowers of *I. fulva*, this introducing the chloroplast molecule, ultimately found in *I. nelsonii*, at the earliest stage of this species' formation. Following this initial stage, hybridization would then have proceeded to involve the third parent species. This species must have acted as a pollen donator rather than a pollen receiver as evidenced by the lack of its chloroplast DNA.

To account for the extremely low occurrence of genetic markers from *I. giganticaerulea* and *I. brevicaulis* relative to *I. fulva*, it is now necessary to introduce a crossing bias into our evolutionary tale. This bias must have involved the hybrid plant preferentially crossing with *I. fulva*. How many generations of such crosses occurred we do not know, but it must have been several to almost exclude the genetic material from two of the parents while incorporating a majority of DNA later identified by inquisitive scientists as originating from *I. fulva*.

What can we learn from our analysis of plant species from deepest southern Louisiana? On general, we conclude that highly improbable events do indeed occur given time and the right conditions. In fact, we conclude (as others before us) that improbable events have actually determined the types of life that surround us today. This is no better exemplified than by stands of wild irises growing in darkened cypress swamps in southern Louisiana, stands that reflect not one but multiple improbable events. These events melded together genetic information from three species resulting in a new form able to exist in a novel habitat, a new form oblivious to the capering of chest wader- bedecked evolutionary biologists.

# SIGNA TREASURER'S REPORT
## 1/1/98 - 12/31/98

| | | |
|---|---|---|
| **As of 1/1/98:** | Checking Account Balance | $13941.55 |
| | CD | 5855.93 |
| | Outstanding Officers' Advances | $(27.51) |
| | **TOTAL ASSETS:** | **$19769.97** |

| REGULAR | INCOME |
|---|---|
| Memberships | $2050.00 |
| Past Publications | 96.50 |
| Seed Exchange | 3227.69 |
| Slide Rental Fees | 30.00 |
| Interest on CD | 316.69 |
| Interest on Checking | 126.16 |
| | 5847.04 |

| SPECIAL | INCOME |
|---|---|
| Checklist | $584.50 |
| NE Apogon Auction | 500.00 |

| | | |
|---|---|---|
| **Total Income** | | $6931.54 |

| REGULAR | EXPENSES |
|---|---|
| Membership | 135.61 |
| Current Publications | 3927.49 |
| Past Publications | 19.06 |
| Seed Exchange | 1625.53 |
| Slide Library | 35.00 |
| Miscellaneous | 15.53 |
| **Total Regular Expenses** | 5758.22 |

| SPECIAL | EXPENSES | |
|---|---|---|
| Research Grants | 0.00 | |
| Seed Collecting Grants | 0.00 | |
| Checklist AIS Ad | 46.00 | |
| Medals | 4390.00 | |
| **Total Expenses** | | $10194.22 |
| | | $16507.29 |

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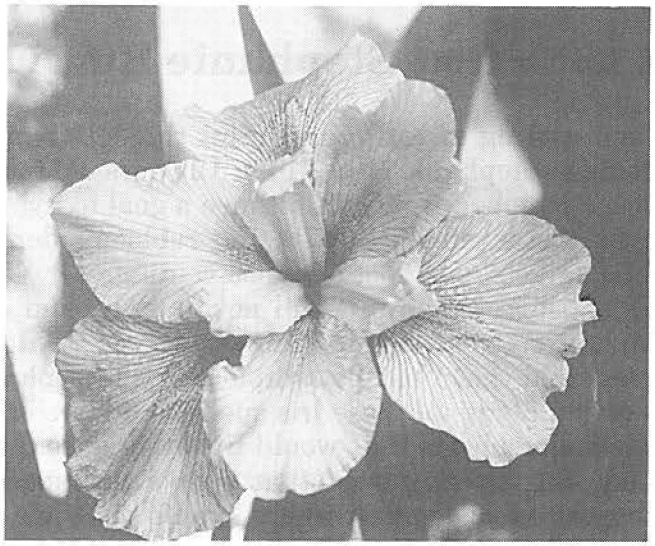
|---|---|---|
| **As of 12/31/98:** | Checking Account Balance | $10077.20 |
| | CD | $6172.62 |
| | Outstanding Officers' Advances | $257.47 |
| | **TOTAL ASSETS:** | **$16507.29** |

**The following about an unusual inter-series cross is based on information supplied by Marie Caillet, Ken Durio and the late Joseph Mertzweiller and is excerpted in part, with permission, from an article "The Story of Tetraploid Louisiana Irises" by Joseph K. Mertzweiller that appeared in the Special Publication of the Society for Louisiana Irises, 1995,**

# "New Hybridizing Opportunities

"Availability of tetraploid Louisiana irises may open hybridizing opportunities which have not been available in the past . What is considered a new hybrid, *I. virginica* X tetraploid Louisiana (BAYOU ROUGE), was produced by Mr. Ken Durio. This is not only an interploidy hybrid (2n X 4n) but also an inter-series hybrid in the Apogon (beardless) classification. *I. virginica* is diploid, 70-72 chromosomes and a member of series *Laevigatae*. Louisiana irises represent the series *Hexagonae* and are mainly diploid, 42-44 chromosomes, with the new tetraploids considered to have 84-88 chromosomes. A factor favoring hybridization at the interploidy level may be related to closer numerical similarity in the chromosome numbers. This may be mere speculation and many other factors are likely to be involved.



"Members of the Society for Louisiana Irises were shown this hybrid during a tour of Mr. Durio's garden in April 1994. . . .The iris is under number and has the parentage: *I. virginica* (light blue, 2n) X BAYOU ROUGE, 4n. It has not been registered, but probably will be in the future. (The iris has now been registered as LITTLE CAILLET in honor of Marie Caillet, long-time SIGNA member and Louisiana iris advocate. Ed.) Falls and standards are twice as broad as (in) the *I. virginica* parent. The bloom is much fuller and about half again larger. Form, texture and substance are much better.

"The scope of this hybridizing breakthrough has not been explored to any great extent, but may be very extensive and could lead to outstanding new hybrids in an entirely new classification of beardless irises. The following preliminary information about 1994 hybridizaing was furnished to the writer by Mr. Durio and is considered very interesting.

1. Selfing set two small pods, but unfortunately contained no seeds. Using V1B87 as pollen parent gave a pod on SAUTERNE which contained six seeds. No germination as of November 1994.
2. *I. virginica* (wide blue X DECOY (Tet. LA) gave 30 seeds and more than half have already germinated.
3. *I. virginica alba* X DECOY gave 18 seeds. Two have germinated and more are expected to germinate.
4. *I. spuria* X Tet LA (several crosses) gave 104 seeds. Seven germinations as of November 1994."

Ken Durio tells us this 'first ever' hybrid between a diploid *I. virginica* and a tetraploid Louisiana iris ". . .is very fertile and sets many pods when pollinated. The pollen will also set pods on tetraploid Louisiana iris. The seedlings are more attractive than that of either parent. The wonderful blooms are midway between the parents in virtually all characteristics."

Ms. Caillet writes: "Experience with growing it has been thus: It was slow to get started, but then took off and grew and bloomed beautifully. I have grown it in regular beds with the Louisianas and kept it well watered in dry months. The foliage stays green and nice most of the year. (Foliage is purple-based and the plant is clump forming, more typical of its pod parent, *I. virginica*. Ed.) Bloom is early -- earlier than that of most La's. The seedlings from it that I have seen at Durio's were much like the original. Thus far, no color breaks from the seedlings."

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## A letter from Stephanie Rust . . .

I am writing to ask you to print this letter in the next SIGNA publication, if you can. My name is Stephanie Rust. I am 12 years old (almost 13) and am an AIS Youth Member. I am also a member of SIGNA. I have a goal to hybridize something very different. I would like to hybridize an in-class MTB that reblooms dependably and has aril-like traits like recurved falls, large signal spot or dense veining. Mr. Clarence Mahan has given me a good plan to go about this. He says it will not be easy, and may take a long time, but I want to try. Mr. Mahan suggested using three species: *I. albertii, I. imbricata* and *I. aphylla*. He also suggested that I use both diploid and tetraploid lines. Could any of your readers help me in locating plants of these iris species? I have ordered what seed I can, but if anyone knows of particular plants that would fit my program, I would be delighted to find them. Also, Mrs. Lucy Burton suggested that I grow aril species in a cold frame. Could anyone out there suggest which species, where I could get them and how to care for them in the cold frame? I live in Zone 5 B. Our weather has a lot of wide swings in temperature in short times and can be dry or very wet for long stretches of time, at any season. It is always very humid in the summer.

I would be very grateful for any help I receive. Thank you.

Sincerely,

Stephanie Rust

Stephanie Rust
Region 18 Youth Member and SIGNA Member

**Stephanie's address is: Ms. Stephanie Rust**
**2016 Meadow Lane**
**Union, MO 63084-4205**

# *Median Odyssey 2001*

## Attention Hybridizers: Guest Iris Request

The Iris Society of Massachusetts will host the MEDIAN ODYSSEY 2001 Convention in eastern Massachusetts, May 18 through 20, 2001.

Hybridizers are invited to submit miniature dwarf, standard dwarf, intermediate, arilmeds, miniature tall, border, and bearded and beardless species and species cross irises recently introduced, or seedlings under consideration for introduction. These irises will be exhibited in eight gardens.

Send guest irises to:
Bill Godfrey, Guest Iris Chairman
3 Pierce St. (Rte 140)
Foxborough, MA 02035
508-543-2711

When sending guest irises, please observe the following guidelines:

1. Up to five rhizomes of each variety or seedling will be accepted.
2. Guest irises will be accepted from July 5, 1999 through August 31, 1999.
3. The name or seedling number must be clearly indicated on each rhizome.
4. A master packing list is to accompany each shipment with the:
    a. Hybridizer's name, address, and phone number;
    b. Name or number of the variety/seedling;
    c. Year of introduction;
    d. Type of iris;
    e. Height and color;
    f. IMPORTANT, SEASON OF BLOOM (Early, Mid, Late, etc.).
5. When guest irises are named after shipment, please notify the guest iris chairman before September 31, 2000 for accurate recording.
6. An acknowledgment of receipt will be mailed to all contributors. Contributors will later be asked for instructions regarding disposition of plants. Failure to reply by May 1, 2001, will be considered as an order to destroy all stock. All returned guest plants will be shipped postpaid.
7. The Convention Committee will exercise all precautions to see that no plant is traded or sold, and that no seed is set, or pollen used.
8. The Convention Committee will adhere to the Code of Ethics as printed in the AIS Convention Handbook. Only rhizomes received through the guest iris chairman will be listed in the convention booklet.

# Conference in Rome, The Evansias

**Bob Pries--Missouri**

In the last issue of SIGNA I presented an overview of the papers presented in Rome, 1998. I alluded to the fact that evidence was presented that completely rearranges our concepts of the crested iris group. In this article I hope to give some explanation.

First I must mention that the term Evansia is an old outdated term, no longer used in modern classification systems. We iris lovers cherish our old familiar jargon. Hence I still use the term Evansias when speaking of the group of rhizomatous Iris that have crests on the falls. Mathew's classification would call these *Lophiris,* a subsection under the section *Limniris,* which of course we dinosaurs still call the old term Apogon or beardless iris as opposed to Pogon Iris the bearded irises.

Whatever the terms, we can imagine (on the evolutionary family tree) the rhizomatous iris as dividing into three large branches: the two large limbs of bearded and beardless, with the crested coming off the beardless limb near its base. Somehow this scenario always bothered me because of the history of hybrids that have been produced. There are no substantiated hybrids between bearded and beardless iris. 'Palverna', an old registration, seems to have been a misunderstood weakling *I. pallida* seedling, and not a cross of *pallida* with the beardless *verna*. Since we are viewing these as two independent branches, the otherwise lack of hybrids seems consistent. It is when we look at crested iris hybrids with bearded or beardless that their branching from the beardless gives us problems. There is only one reported hybrid from a beardless and a crested iris, 'Toltec'. This was supposedly an *Iris missouriensis* Nuttall X *Iris tectorum* Maxim. On the other hand, there have been several crosses of *tectorum* with bearded iris including the well-known 'Paltec'. 'Paltec' gives the unusual characteristic of having a beard that sits on top of a crest, leaving little doubt as to it authenticity. The evidence exhibited by known hybrids suggests the Evansias should branch off the bearded and not the beardless.

As a group the evansias show a rather unusual geographic distribution. In the eastern United States we have *Iris cristata* Solander, widespread through the Appalachians and into the Ozark Mts. In very limited areas around the shores of Lake Michigan and Lake Superior is the sand dune iris, *Iris lacustris* Nuttall. Both *cristata* and *lacustris* have three parallel crests on each fall. They are often confused with each other, and are undoubtedly closely related. But *lacustris* and *cristata* are quite different from other evansias. *Iris tenuis* Watson has been compared to *cristata* in the look of its plant and flower, but has almost no crest. The presumed crest is a rise in the central midrib of the fall. It was not until relatively recently that it was even considered an Evansia. Previously it was considered a *Californicae* because of its distribution in the Pacific Northwest. The remaining evansia species are all Old World. Japan has another dwarf crested in *Iris gracilipes* A. Gray but it is very slender in its plant form and much more delicate appearing than *cristata*. In Korea from Mount Odaesa we have a virtual unknown to the Western gardening world *Iris odaesanensis* Lee. I understand recent collections will bring this to us in the near future. Undoubtedly one of the best known of the evansias is *Iris tectorum* Maxim., the Japanese roof Iris, which of course is from China. Another similar species coming from the Himalayas is *Iris milesii* Baker. These two are said by a number of authors to be very similar. I can attest that both are very easy from seed but unfortunately I have never seen *milesii* in flower. Both have a prominent central crest, which is divided on its edge with many tiny fingers much like a cock's comb. Botanists use the term fimbriate. And this type of crest is what most people picture as the archetype of crested iris. Another group of crested irises are houseplants in the north. These are from Asia and include *Iris japonica* Thunberg, *I. wattii* Baker, *I. formosana* Ohwi, and *I. confusa* Sealy. These iris produce a cane with a fan surmounted by sprays of scores of gorgeous flowers distinguished by lacy, fimbriate crests. They vary from the tall, sometimes six-foot *wattii* to the two to three-foot *japonica*. They share so many common traits some have suggested they could be all one species.

For those new to evansias that is an overview of the prominently grown species. Many have been compared to orchids and represent a very exotic group of iris. But the story gets more interesting. Beards, crests, or the lack thereof would be an easy way of distingishing the types of rhizome-bearing iris. Unfortunately one of the bearded groups, the *Pseudoregelias*, have a beard but also can have a crest under it much like the aforementioned hybrid 'Paltec'. Indeed three species which have not been well distributed, and therefore little known, have been placed at one time or another by some with the crested iris and by others in the *Pseudoregelias*. These are *I. leptophylla* Lingelsheim, *I. sichaunensis* Zhao, and *Iris latistyla* Zhao.

Other disputed crested iris are *I. proantha* Chien, *Iris speculatrix* Hance and *Iris rossii* Baker which are sometimes placed in the beardless series Chinensis (Diels) Lawrence. They have a 'crest' that is not fimbriate and is more or less simply an elevation of tissue over the central midrib much like *Iris tenuis*.Watson. Finally we have I*ris subdichotoma* Zhao, said to have a crest but also placed by some with *Iris dichotoma* Pallas in the genus Pardanthopsis. Obviously much more research is needed.

One can see how anxious I was to hear the latest research in Rome. What I saw and heard, had me on the edge of my chair. A cladistic tree was shown to us with scores of species branching off at various levels. I strained my eyes to try to find each species. At the very base of the tree was *Iris tectorum*. This would indicate relationships with both bearded and beardless species. Hybridizers might consider this a potential bridge between these two large groups and it certainly would suggest that crosses with any of the rhizomatous species might prove worthwhile. I then looked for *Iris milesii*. My impression from the literature was this was very closely related to *tectorum*. Both species have been said to have produced a hybrid with the evansia hybrid 'Question Mark', a *wattii/confusa* cross. Rodionenko placed *milesii* and *tectorum* together in his series *Tectores*.

Imagine my disbelief and astonishment when I saw that *milesii* was not even close to the rhizomatous iris but with the bizarre subgenus Nepalensis. Most gardeners know this by way of the species *I. decora* Wallich. *Decora* is separated from species having rhizomes by having none, but rather a small bud at the top of a group of fleshy roots, often compared to the bud and roots of daylilies. *I. decora* with *I. milesii* just didn't seem logical. In discussing this afterward with the famous plantsman Tony Hall, who is curator of the Alpine yard at Kew, I was amazed that he seemed in total agreement with this part of the presentation. Perhaps the plant I know as *milesii* is not the true plant? I looked at the picture of *milesii* in Kohlein's Iris. I have included a sketch of this picture. I recently noted a photo from Kew in the BIS Yearbook. I sketched this also. The two pictures do appear very different. But individuals of a species can vary dramatically. Then I thought about the fact that *decora* does have a crest. It also carries its standards flaring outward somewhat like *tectorum*. (See sketch of *decora*.) Even with considering these similarities I am not ready to accept *milesii* as a Nepalensis. Unfortunately I am also not prepared to say that we are even discussing the same plant. I would love to see more pictures and descriptions from anyone who has grown *milesii*. Of course this is not a matter that can be decided by a vote. Ultimately herbarium specimens of the original collections may need to be consulted before we can assume we know the real *milesii*.

My next shock was the position of *Iris speculatrix* Hance (2n=44). The cladistic analysis placed this basal to the Spuria Iris. Although this did not seem so extraordinary once I thought about it, it was certainly a different relationship than I had considered before. *I. speculatrix* is fairly rare in cultivation. It would be interesting to note if anyone has tried crossing it with *I. spuria* L. (2n=44), *I. foetidissima* L. (2n=40) or *I. graminea* L. (2n=34).

My last surprise was the position of *Iris tenuis* Watson. This was allied on the tree to *Iris missouriensis* Nuttall. *Tenuis* has always been somewhat of a mystery. It was assumed for many years to be allied to the 40 chromosome Pacific Coast Natives. This view was reconsidered when the chromosome number was counted as 28. My Pacific Coast friends tell me that they would argue for a strong connection to *Iris cristata* (2n=24 or 32). Although the plants do have a lot in common these characters might be responses to life in similar habitats. *Missouriensis* is a plant of full sun near rivers where there is perhaps some access to the water table belows, but often scorching hot and dry in the summer at ground level. Close inspection of *missouriensis* also shows a crest-like rise of the central midrib with an indentation extending linearly to either side. This is similar to *tenuis*' "crest". It seems incredible that no one has suggestd a relationship before. *Iris missouriensis* has been counted as 2n=86 and 88. I had always wondered whether *tenuis* might have some affinity to series Chinensis. *Iris minutoaurea* Makino (2n=22) and *I. koreana* Nakai show some similarities to *tenuis,* I thought. It would be interesting to perform crosses of *tenuis* with all of the above.

The conclusions presented are my interpretation of information presented at the symposium. I had hoped that the proceedings would be in print by this time so I could give a better presentation. Unfortunately I must rely on my memory and in doing so undoubtedly have overlooked many fine points. The cladistic diagrams which were presented were in some ways preliminary data. My understanding was that there was an intention to double the observed data base and then to reanalyze the data. Even though the presented information was based on one thousand parameters, some of these branches had only limited confidence levels while others were very sound. For example the indication that *pardanthopsis* should be returned to the genus *Iris* had overwhelming statistical support, yet some of the above mentioned positioning of crested species on the evolutionary tree had only weak statistical support. Therefore with an increase in the data base very different conclusions could result. I therefore offer these thoughts only as a starting point for Iris discussions and your contemplation. My apologies to the Jodrell Laboratories if I have misrepresented the evidence in any way.

| | |
|---|---|
| Nos. 1-3 | *Iris japonica*: plant, flower and seed capsule |
| Nos. 4-10 | *Iris confusa*: stem, plant, flower, stamen, style arm and seed capsule |
| Nos. 11-15 | *Iris wattii*: leaf, flower, fall, standard and seed capsule |

**Illustrations reproduced from <u>Iris of China</u> by Waddick & Zhao, Timber Press, 1992**

# NOMINATING COMMITTEE REPORT

The Nominating Committee for SIGNA would like to present the following slate of officers, effective January 1, 2000.

| | |
|---|---|
| President: | Carla Lankow |
| Vice President: | Will Plotner |
| Secretary: | Paul Martin |
| Treasurer: | Janet Sacks |

For Director, term expiring 2003: Darrell Probst and John J. Taylor

We are very pleased to be able to offer SIGNA members what we believe to be an outstanding slate. Paul Martin is the designer of SIGNA's medals. Darrell Probst is a former SIGNA Seed Exchange Director and has collected many species iris plants and seeds on his Asian collection trips. John Taylor is the Editor of the Dwarf Iris Newsletter and Checklist. There are many other accomplishments that could be included here but these are the ones that come quickly to mind. The other officers have all agreed to repeat their terms for two more years and we are happy to have them back.

Respectfully submitted,

Barbara Schmieder, Chairman
Clarence Mahan
Jean Witt

**Note:** Anyone wishing to nominate another person for office may do so by following the outline in the SIGNA By-Laws, Article VII, Nominations and Elections. The complete By-Laws are in SIGNA pp. 3130--3133. The By-Laws state that the petition must be returned to the President no later than June 1st; however, because of the short time period, that date has been amended to July 1.

# e-mail Address Directory

New SIGNA Membership Chairman, Rodney Barton, has volunteered to put an e-mail directory together. The directory would be available to SIGNA members upon request. Send your e-mail address (via e-mail, of course) to Rodney at <RBarton@hsc.unt.edu>

This article first appeared in the semi-annual publication of the Iris Society of Massachusetts, ***IRIDISM***, Vol. 18, No. 1, Spring, 1998. It has been revised by the author to include observations from the '98 season and other information of possible interest to SIGNA readers.

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# MUSINGS OF A MAD LILY-GILDER

**Lynn Markham--Masssachusetts**

It's been sixteen years since I re-began to hybridize irises after a ten year "dormant" period. Being older and wiser (hmmmm) by then, and considering the limited arable land we have and the even more limited capacity of an aging (gracefully of course) gardener to care for vast plantings even if there were somewhere to put them, I resolved not to "mount my horse and ride off in all directions" as I did in the '60s, but instead to focus on the BB class to the exclusion of pretty much everything else. But anyone who's played this game for very long knows that each new crop of seedlings raises the MOST interesting questions, and some of them have nothing at all to do with the putative subject at hand. And so I muddle forth, trying to work the side-issues into the BB line so I won't have to apologize to myself for playing with them - I wonder who I'm kidding.

The core program has certainly progressed since 1989, when I bloomed the first crosses containing more than one or two candidates for inclusion in the border bearded class. Now there are so many appropriately proportioned seedlings in the patch that it's almost possible to forget that the "right" type of plant is hard to get. **ANGEL FEATHERS** (t-factored dominant white BB - 1973) is a key factor, though she's hardly done it alone. In dog breeding, top sires get most of the press but wise breeders recognize that the sine qua non for a solid line is a terrific "foundation bitch" - that's '**Feathers**', who by happy chance took from her tiny pod parent, **LITTLE LYNN** (pink BB - Sheaff '62) the ability to reduce both flower and plant size, and from her pollen parent, a beautifully branched small white TB from ((Hall pink x **CELESTIAL SNOW**) X **CLIFTS OF DOVER**), the even rarer ability to produce optimum branch placement and bud sequence on a regular basis. Barry Blyth says he rarely back-crosses, moving ahead instead through improved progeny. In principle I agree and over time lean harder on her extended family than on the matriarch, but I'm not sure when I'll retire '**Feathers**' for good and all - when you're working in a class that's seriously limited in numbers and you have one plant that can be depended upon to control certain elusive but necessary features . . .

The other key factor is *Iris aphylla* in its various forms, including not only the direct approaches that are going on here, but also the many advanced generation hybrids from tetraploid MTB lines Ben Hager, Jim Craig Terry Varner and others were developing while my own breeding program was fast asleep. These plants have been invaluable in providing slimmer stems and foliage, more dramatic branching, and plant qualities well suited to this climate, e.g., amazingly long roots to resist drought damage and anchor the plants against winter heaving. Unusual color effects are also emerging - these I've barely begun to explore but they are endlessly fascinating. I think that ultimately, every BB born here will involve this nifty species to one degree or another, and many already do. But the *aphylla* work seriously threatens to become an end in itself as I continue to collect disparate clones and

*I. aphylla* B66-2 - "glaciata" recessive white

play with them. My excuse is to improve my breeding tools. The real reason is that I've fallen in love with the species.

The *aphylla* bloom has been excellent the past two years. I hadn't grown or seen *polonica* for some time and had forgotten that it's white-bearded. A few years ago I had a part-*aphylla* seedling which was a deep, deep purple with large white beards and it was a lovely thing - unfortunately I lost it but it should be easy to re-create, given white-beard-on-purple available at the species level. Another attractive clone I hadn't seen for years is *bohemica*, a softer purple than most but warm and glowing, with fine form, branching and plant habit. B66-2, of course, is an "albino" - or rather a true "glaciata" (plicata recessive) white. Mind-boggling possibilities from that one. And H-17, a tiny but powerful plant, is the sole example of true yellow in the species (well, parchment yellow - maybe even dirty yellow but so what?). Again, lots of possibilities, although H-17 is a little reluctant in both pollen production and seed sets. The more I collect, compare and use various clones, the more I feel that it's absurd to think of "*aphylla*" as a single entity, as some people tend to do. Even those that physically resemble each other do not, repeat NOT, breed the same.

Among pure *aphylla* seedlings, hand-pollinated or otherwise, there is enormous variation, not only in flower type and color value, but in vigor and other plant qualities. After all, the clones collected and distributed in the '60s and '70s have a wide assortment of attributes and defects, suggesting that the former might be capitalized upon and the latter eliminated without too much difficulty. For example, 'Dark Violet' has wonderfully saturated color and a suggestion of ruffling, but the flowers tend to expand unattractively as the blossoms mature, to the detriment of form. S-2 Geneva has quite stable form and lovely branching (also double-budded spathes) but nary a ruffle, and the plant tends to spread itself out rather than forming a tight clump. **Aphylla Wine-Red** is a rich, clear color and has nice, compact form, but it isn't a particularly aggressive plant and its branching (often short clusters of stalks emerging from one rhizome) tends to be crowded and inelegant. Polonica, for all the intrigue of its white beards on deeply saturated petal color, is a narrow, sloppy flower, but the branch habit is excellent and so is the plant. All of this

Seedling #94S-3B2 - Aphylla Wine-Red X *I. aphylla* 61-56A argues strongly for intercrossing clones and making deliberate selections, whether the goal is marketing improved garden *aphyllas*, developing better breeding material, or both of the above.

I've bloomed quite a number of interclonal crosses recently and found that vast improvements can happen immediately when unlike clones are crossed. Two selections from 6l-56A X **Aphylla Wine-Red** are better than either parent, combining the smooth hafts of '*Wine-Red*' with the extraordinary branching and vigor of 61-56A. Even more striking, at least in color variation, was the assortment obtained from APH-1A2 (small flowered, multi-branched dark purple from SIGNA seed) X H-17. Selections include a wonderfully smooth red, a near-blue with each blended beard tucked into a V-shaped white blaze, and a bright violet with intense yellow beards. No yellow flowers in this progeny but it was a small lot, so I haven't attempted to draw any conclusions about the color potential of H-17. Wish it were not so hard to get progeny from it - I think the cross B66-2 X H-17 could tell us a lot and I haven't been able to accomplish this. Still trying. I've been trying (also unsuccessfully so far) for glaciata cultivar X H-17 as well, since that would provide some of the same information, at least with regard to color.

In a cross of B66-2 X **MAROON CAPER** (Warburton '64 - an *aphylla* half-bred itself), all plants were unbelievably vigorous and all approximately *aphylla* in stature (10" to 15") - the one I kept is a smooth red with great branching which produced three bloomstalks and 10 increase on a first year plant. It's fertile both ways and of course has the potential to produce the glaciata pattern. This is exciting for the promise of getting some ruggedness into glaciata lines, most of which trace to the Gibson plicatas and many of which have difficulty surviving in this climate. But they are SO beautiful - the glaciata, whether white, yellow or pink, has great clarity of color because of the complete absence of anthocyanin - it's almost luminous. In 1998 we bloomed a lot from B66-2 X *I. aphylla* 'Dark Violet'. This was primarily a test-cross to see if the glaciata recessive is hiding in any of the fully pigmented *aphyllas* and 'Dark Violet' doesn't seem to harbor it, but of course these babies all should be carriers. Few of them had impressive flowers but like the **MAROON CAPER** kids, they were aggressive plants (one rather nice one managed four stalks and 11 increase on its maiden bloom) and selections were made for the best combinations of vigor and flower quality. Oddly, wimpy little B66-2 seems to produce uncommonly vigorous offspring.

Now that the SPEC class is available, *aphylla* enthusiasts are faced with the age-old dilemma as to what is sufficient improvement to warrant introduction. Excellent garden performance is a must; unusual breeding potential for various qualities is added incentive. I think most of us (*aphylla* nuts that is) are proceeding with caution, lest we duplicate what's already "out there". Our 1997 *aphylla* introduction, **BRIGHT WATER**, from open-pollinated SIGNA seed, was selected for a combination of exceptional vigor, attractive foliage and a lovely branch habit. Its slight tendency to ruffle and its ability to produce interesting color blends in cultivar crosses are bonuses. Jim Craig has recently registered one called **MINNOW**, after hesitating for some time because, although it's an excellent clone, he wasn't sure it was really "different". The deciding factor in that case was climate-tolerance; the plant is viable in southern California, where the vast majority of aphyllas sulk and die out because of the lack of true winter. That alone, I think, is "different" enough!

In our *aphylla*/cultivar combinations, there's much of interest and more to come. One deep violet on white plicata selection from a 1993 cross of TB **BLUE PETTICOATS** (Schreiners '65) X B66-2 is teasing me to introduce it. IB in size but decidedly SPEC-X in style, it's a total non-conformist, producing dozens of ruffled but narrow flowers which take two days to expand the tightly closed standards and spread the falls. The effect in a clump is both amusing and confusing! Actually, I think I like it partly for its idiosyncrasies, especially since they come with great branching, killer-aggressive plant habit and stunning red-purple leaf bases and spathes. Whether this gets introduced or not, it and a similar sib are making interesting seedlings including, from a cross with TB **BALTIC STAR** (Stahly '94), a nearly black-on-white plicata with tiny TB-shaped flowers on 14" branched stems, as well as several really pretty IB-size luminatas.

Actual results of initial *aphylla*/cultivar crosses vary enormously, depending in part on the *aphylla* clone selected, but even more on the choice of cultivar. Some progenies segregate wildly for stature and others don't, probably reflecting the diversity or uniformity of genetic background of the cultivar more than its own phenotype. Flower qualities seem very dependent on the cultivar parent, the best choices proving to be those with not only attractive but very stable form, and excellent substance. A cross of TB **CARRARA MARBLE** (Sheaff '70) with *I. aphylla* 'Dark Violet' produced almost exclusively IB-type plants, about 23" to 28" in height, with nicely shaped mid-size flowers, most ruffled to some degree. **'Carrara'** is a crisp, wide-petaled, precisely ruffled mid-size TB flower with extraordinary substance.


**Seedling #9395-64A - BLACKBEARD X *I. aphylla* 'Dark Violet'**

A cross of BB **BLACKBEARD** (Weiler '89) X 'Dark Violet' produced markedly smaller flowers and a height range from 14" to 30", most widely branched and carrying many buds. The flowers, as expected, were much simpler in form than the '**Carrara**' kids but for the most part shapely and well-substanced, as the cultivar parent is. Unfortunately they varied greatly in vigor and winter-hardiness. **BLACKBEARD** is extremely striking with its inky beards on pale blue, but the plant has problems in this climate. I think I'm actually grateful for the assortment of difficult winter/spring conditions we've had here the past two years, since those which would succumb did, and those that have survived and bloomed well should be New-England-proof. There were gorgeous rich purples, pale blues and whites, but the most striking (and one of the better plants as well) is a soft blue with black-purple beards like **BLACKBEARD** and very decent sculpted form. It has fertile pollen, which of course has been spread all over the yard. Except for the size, this flower reminds me of a similarly colored TB seedling that was much admired around here for a while, from **BLACKBEARD** X **SECRET WEAPON** (our '94 BB), but that one croaked in the brutal winter of '96/7, as my gut had told me it would do. May the new baby do better! Several plants have been saved from this interesting lot, including one very aggressive individual which in 1998 continued to bloom through July 1 on late stalks produced on the side-fans, and was still a respectable clump when the party was over. If this is a true "sequence-bloomer" it could be a breeder for extended season or even true rebloom.

Visitors to the garden in '97 were much taken with a starchy white flower with gold hafts and beards on modified *aphylla*-type stems, from the small dark *aphylla* seedling APH-1A2 X my 92-1A, a ruffled cold white BB which is itself about 1/4 *aphylla* (the other 3/4 consisting of liberal doses of '**Feathers**' and '*Carrara*' with some black x pink TB combinations I perpetrated some 30 years ago). This seedling is so "cultivar-looking" that one person even suggested the cross might be wrong, but since the pure *aphylla* was the pod, no chance. Segregations in that cross were really wild - the other item I retained is an extremely smooth dark purple with what you might call "tame *aphylla*" form and branching - it looks absolutely nothing like the "finished" white sib, but should it have to? The color is intense, the finish very slick and shiny, the form simple but controlled. Ideal stalk positioning in the clump, flower placement on the stalks, and sequencing of bud maturity result in an elegant display of individual flowers right through the blooming season. The garden effect is stunning, and if I had to discard one of these two sibs (which fortunately I don't) it would not be this one.

**Seedling #94-61C - *I. aphylla* seedling #APH-1A2 X BB seedling #92-1A**

These almost-wild things sing a song of their own, and I was extremely pleased to catch our flower-arranger pal Kathy Marble, who doesn't even like most bearded irises, actually coveting one from **BRIGHT WATER** X Barry Blyth's bronze-bearded pale blue TB, **TOUCH OF BRONZE** ('83/4). This seedling is smooth parchment in color with just a hint of anthocyanin wash (in some lights, slightly pinkish) on the petals, with sultry blended beards. The flower is very plain but neatly formed. Clump habit, branching and bud placement are superb, and perhaps the prettiest thing of all is the very pointed, exotically colored buds. The great thing about having people like Kathy look at your plants is that, never having bought into the latest rage in cultivar color, form, etc., they view them open-minded. Amazing what happens when you do that!

My love affair with these last two plants has me wondering whether I should, in addition to crossing them back into the '**Feathers**' line for better

**Seedling #94-62A - BRIGHT WATER X TOUCH OF BRONZE**

proportioned and balanced BBs, figure out how to use them so as to preserve their own special character while further expanding the color palette. I already know I can do the former and it's in the works. The latter, I suspect, will require a delicate genetic balancing act and a great deal of luck, which of course renders the project very, very tempting. Our space problems certainly haven't gone away and I shouldn't but . . . . oh well.

Next to *Iris aphylla*, the greatest potential for serious digression from the core (BB) program is in my personal passion for color and pattern. I simply go wherever these promise to be and damn the torpedoes! The **Floridor** project (those revolting dog-eared diploids that raised visitors' eyebrows in the back patch in '97) is one of the wilder quests - that probably deserves its own article and I'm not ready to write it yet. Tamer ventures abound - for the most part they involve crossing out to great TB stuff like screaming-red-bearded slate blue **TWILIGHT BLAZE** (Keppel '92) or near-black lumi-plic **BALTIC STAR** and taking the long way home. The fringe benefit from this approach is that there are now some pretty nice TALL BEARDEDS segregating out of BORDER BEARDED lines instead of the other way 'round - and yes, I do rather like the irony!

# *BOOK NOTES . . .*

### Anne Blanco White from the BIS Species Group Bulletin, Spring, 1999:

Many years ago I came across the special painting, for irisarians, of that bit of trouble in the Garden of Eden and promptly forgot to make a note of it. Now it has turned up again in The Story of Eve by Pamela Norris (Picador, 1998): Adam, Eve & the Serpent by Hugo van der Goes painted around the early 1470's. The point is that Eve has a blue, bearded iris in a stratigic position, Adam just has to use a hand.

But another picture of The Garden of Paradise by the Uberrhemischer Meister naturally includes amoungst the clumps of flowers some blue, bearded irises and the author comments that these recall 'Jesus's descent from the royal house of David'. This is new to me. Can any of you offer more information?

### . . .from Jean Witt

This information comes from The Secular Spirit: Life and Art at the End of the Middle Ages. The Metropolitan Museum of Art, E.P. Dutton & Co., Inc., New York. p. 132, 138. 1979 (No author--a compilation)

Although it is referred in the book to as a 'lily', the florin, the first gold coin produced in bulk during the Middle Ages, bears the image of the fleur de lis. It was authorized in 1252, in Florence, Italy, weighed 3.51 grams, and became one of the great international currencies of its time.

The badge of the Florentine Wool Merchants, the Arte della Lana, also has a row of four fleur de lis behind a banner and a sheep. In the fourteenth century this wool merchants' guild was one of the most powerful in the city. The badge, in copper and enamel, is about 12 cm long, and was probably worn on a chain around the neck on ceremonial occasions.

Encyclopaedia Britanica, 1963 ed. vol. 17, in the section on Persian Art, in the plate opposite p. 594, a painting entitled Humay and Humayan in the Imperial Garden at Peking, Herat School, about 1450, shows four people in a flower garden. Toward the rear, under fruit trees, are a number of obvious bearded iris with branched stems, as tall as the first crotch of the tree. This points to tall bearded irises as garden plants in Persia, and possibly also in Peking.

Opposite p. 595, a plate entitled Caligraphy in Nastaliq (a style of calligraphy), by Imad al-Husni, 16th century has lifelike irises and tulips as decorations under the calligraphy; one iris could be an Onco.

From the new book, The Tulip by Anna Pavard:

p. 42. In a Mogul Miniature of Shah Jehan's time (1627-1658), there is a pale yellow dwarf bearded iris.
p. 82. In the painting, Allegory of Spring, 1616, by Jan Brueghhel the Elder (1568-1625), with other flowers, are a very recognizable *I. sibirica* and a large, well-shaped light blue bearded iris which could be *I. pallida*.
p. In Flower Still Life by Ambrosius Bosschaert (1573-1621) painted around 1621, *Iris variegata* and *Iris sibirica* are clearly recognizable.

Please, any member who has access to a large metropolitan museum with a collection of Persian paintings, look for irises in them. Bearded irises have obviously been a prized garden flower for several centuries.

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# Yellow-leaved *Iris pseudacorus*

**Jennifer Hewitt - England**

It is generally said that seed from *Iris pseudacorus* 'Variegata' is unlikely to give anything other than green-leaved plants and many of us would verify this. But in England, in recent years, some with yellow leaves have been raised, by different people. All of them have yellow leaves in spring but, like the parent, become green sooner or later during the summer.

First on the scene was **Blondie**, registered by Roger Broomfield in 1994. This was exhibited at a show held by the West & Midlands Group of the BIS but has not been distributed as far as I know. It was a fine plant, grown in a pot, but was said not to increase quickly. To the best of my recollection, the leaves in June, when it was in flower, were not entirely yellow but had some faint green striping. The flowers were the usual yellow.

A couple of years later another BIS member, John Fielding, asked me to trial a yellow-leaved seedling he had raised from *I. p.* 'Variegata' seed. Like **Blondie,** this does not seem to be a very strong grower and has not flowered here, though John says it does very well in a friend's pond. I have been growing it in permanently moist soil but perhaps in more shade than is ideal, and it has now been moved to a lighter, but still damp, spot. The leaves are a soft, slightly greenish, yellow in spring and light green in summer.

I believe that both these were single occurrences in groups of green-leaved seedlings, but the third raiser had a whole batch of yellow-leaved seedlings. Bob Brown, not a BIS member, got his seed through the Hardy Plant Society which specialises in hardy perennials including irises. He picked out the one he has registered as **Lime Sorbet** (198) because of its vigour, important to him as he is a nurseryman and has been able to build up stock fast enough to introduce it in 1999. It also seems to differ in that the leaves become green more slowly, changing gradually over the whole summer. The flowers are typically yellow, the height only 30 inches (76cm); **Blondie** is given as 24 inches (60cm) tall, I got **Lime Sorbet** in 1998 and so far it is growing well.

Looking through a draft copy of the Cultivar List of Species, it seems that these are the only plants which are recorded as having inherited coloured leaves from their pod (and probably pollen) parent, though they are all-yellow rather than striped with green. Many variants of flower colour are recorded, especially in the USA. Why have the foliage variants appeared only, it seems, in Britain?

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***Help wanted . . .***

# SIGNA needs an Editor

**If you would be willing to take the job of Editing SIGNA, or become part of an Editorial Staff, please write or call Carla Lankow (address inside front cover---phone: 425-235-7065) or e-Mail her at <SIGNA@bigfoot.com>. e-Mail is helpful but not necessary.**

The following is reprinted from the Proceedings of an International Symposiun, *Gardening with Iris Species,* St. Louis MO, 1995

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# Hardy Irids of South Africa

**Maurice M. Boussard -- France**

South Africa is undoubtedly a paradise for the Iridaceae, harboring circa 900 species in some forty genera out of the nearly 1650 species of 70 genera now inventoried. South Africa boasts, for example, the major portion of species of the second largest genus of the family, *Gladiolus*, which would even rank first if the bulbous Iris: *Iridodictyon* (Reticulatas, 10 ssp.), *Xiphium* (10 ssp.) and *Scorpiris* (Juno ca. 60 ssp.) were raised to generic status and thus subtracted from the true *Iris* (ca 250 ssp.). This is a sustainable hypothesis since *Scorpiris* is probably more distantly related to a rhizomatous Iris than, say, a *Dietes*.

The Republic of South Africa roughtly includes three climatic zones:

Zone 1 - The West and Southwest Cape enjoy a Mediterranean climate with mild and more or less wet winters, but long, hot, dry summers and a decreased rainfall from South to North and from coast to the inland.

Zone 2 - The eastern part of the country (East and North Cape, Orange Free State, Natal and Transvaal) is subjected to a monsoon climate with cool (even cold in the mountains) and dryish winters, but hot, wet summers. Rainfall is scarcer from East to West.

Zone 3 - The coastal South Cape, a small third part, received rainfall all year round and so often accommodates evergreen cormophytes (*Dierama, Morea, Watsonia* etc.)

Such variations of temperature and humidity explain such a wealth of geophytes including our cormaceous Iridaceae. There are no true bulbous irids in S. Africa. These are only found in the boreal Old World and the Neotropics. The resting period is either summer (Zone 1) or winter (Zone 2) and that has to be taken into account under cultivation. There are also some rhizomatous genera in the family.

These South African Iridaceae belong to the following tribes:

Iridoideae: One rhizomatous, evergreen genus, *Dietes* (5 ssp.) and nine other genera (*Barnardiella, Ferraria, Galaxia, Gynandriris, Hexaglottis, Homeria, Moraea, Rheome and Roggeveldia*) which are all cormaceous and usually deciduous. It includes Sisyrinchioideae with only one genus, *Bobartia*, resembling *Aristea* in habit. The bulk of this tribe is American with some outlying members in Australia and New Zealand.

Nivenioideae: Four genera rhizomatous and evergreen, *Aristea* (40 ssp.), *Klattia* (3 ssp.) *Nivenia* (9 ssp.), *Witsenia* (mono-specific). The three latter are the so called "shrubby genera" with woody stems, a feature shared with the Australian genus *Patersonia*.

Ixioideae: The main tribe of the whole family and its South African representatives. All are cormaceous with at times a stoloniferous and/or barely defined true corm (as in *Gladiolus sempervirens* and *Schizostylis coccinea*). A tribe occurring only in the Old World with a greatest cencentration in South Africa, albeit some genera are also found elsewhere (i.e. Tropical Africa, Madagasacar): *Babiana, Crocosmia, Dierama, Hesperantha, Lapeirousia, Radinosiphon, Tritonia* - two of them (*Gladiolus* and *Romulea*) extend up to Maghreb and Eurasia, as does *Gynandriris*.

As for hardiness, it is quite a relative notion involving either the *locus natalis* of a given plant or the place where it is being cultivated. The coastal belt of the North Cape is frost free and has low rainfall (*Ferraria foliosa* & *F. schaeferi* occur there). It is a world apart from the Sutherland plateau which is swept by bitterly cold winds and often capped by snow in winter (a farmer there told me the temperature may drop to -10° C. and the minimum recorded was -18° C.) but harbors such gems as *Moraea ciliata* and *Romulea komsbergensis*.

For the scope of this paper, "hardiness" refers to the rather harsh climate of the northeast part of France:

- average rainfall 700 mm, mainly from autumn until spring with some summer showers. That means the summer-resting items are best cultivated under glass to keep the ground dry.
- some 100 days (for the most part, night) of frost, from October onwards to early May. Lowest templerature -15° C. 5°F
- average summer temp.: 25-32° C. but the night remain cool, rarely above 15° C. The extremes recorded were -22° C. January, 1968) and +41° C (July, 1976).
  77-90 59°F -8 106

The plants are cultivated in pots kept dry during dormancy or in frames with glass to protect them from the climatic hazards (rain, snow, frost). Pot culture is avoided when possible since plants do not always thrive in them due to sudden changes of temperature that may warm up the compost too much, inducing too early dying down, and pots do not allow a full spreading of roots.

Here is a choice of fairly hardy South African Iridaceae, checked as being amenable to cultivation over at least 5 years:

- *Anomatheca laxa* and *A. viridis* are freely blooming and self-sowing almost as weeds. *A. grandiflora* is a bit more fussy.
- *Aristea*: not easy as a rule, *A. ecklonii* being the easiest. A pity since it is one of the few iridaceous genera to have true blue flowers, at times of very good size (e.g. *A. biplora, A. lugens. A. monticola,* and *A. spiralis*).
- *Babiana scariosa* (mauve/yellow), *B. spathacea* (cream, streaked purple) and *B. virginea* (pure white, scented) are the most hardy, taking hard frost without any injury.
- *Bobartia* is another temperamental genus, like *Aristea*. *B. gladiata* is still thriving in a pot where it once stood -10° C. Large starry yellow, flushed brown flowers on a flattened, very stiff stalk.
- *Crocosmia* is as easy as it is handsome. *C. masonorum, paniculata,* and *pottii* are fully hardy, *C. aura* needs some winter protection.
- *Dierama:* several ssp. *(D. ambigum, D. cooperi, D. dracomontanum, D. medium, D. pictum, D. pulcherrimum, D. robustum*, and *D. trichorhizum*) are fairly hardy and moisture loving, as is *Schiozostylis coccinea*.
- *Dietes bicolor* (the showiest to my taste) and *D. iridioides* do well against a south facing wall, just protected from snow and heavy frost by a plastic sheet.
- *Ferraria*: Their half-succulent foliage makes them unfortunately somewhat tender; *F. crispa* is the hardiest (-8° C).
- *Freesia* are all definitely tender and for the cold house.
- *Geissorhiza asplera* (bright blue) and *inaequalis* (lilac) behave well and are self-sowing. I did not check the hardiness of the tiny beauties of that genus (*G. matthewsii,*

*G. monanthos, G. radians,* or *G. splendidissima*) yet.

- *Gladiolus:* this large and diverse genus should deserve a paper on its own. Among the winter-growing ssp., *G. angustus, G. buckerveldii, G. cardinalis, G. carmineus, G. carneus, G. cunonius, G. liliaceus, G. orchidiflorus, G. splendens, G. tristis, G. undulatus,* and *G. uysiae* are good chaps as are the summer-flowering *G. crassifolius, G. dallenii, G. flanaganii (G. cruentus), G. microcarpus, G. ochroleucus, G. oppositiflorus, G. papilio,* and *G. sericeovillosus*, though some late bloomers are best grown in pots as to enjoy their flowers which otherwise might be cut by autumn frost.

- *Gynandriris cedarbergensis* (white) and *G. setifolia* (mauve) are more than easy; *G. australis* and *G. pritzellians (torta)* a bit tender.

- *Hesperantha: H. bachmannii, H. falcata* (both white), *H. pauciflora* (beautiful purple) and *H. vaginata* (bright yellow tipped black) are frost-tolerant winter-growers. The summer flowering *H. baurii* and *H. huttonii* (both pink, the latter having peculiar *Gladiolus*-like winged seed) are easy too.

- *Homeria: marlothii* despite its very local distribution is a tall, striking and adaptable plant, as is the orange form of *ochroleuca*. The two glittering ssp. *H. comptonii* and *H. elegans* are unfortunately less frost proof.

- *Ixia:* no problem with *I. flexuosa, I. latifolia,, I. maculata, I. rapunculoides* and *I. trifolia*.

- *Lapeirousia:* none of the winter-growing species are hardy enough to be grown in the open, though the shiny *L. oreogena* is standing in a frame for a couple of seasons. Conversly the summer-flowering *L. erythrantha* does well and multiplies.

- *Melasphaerulea graminea:* a noxious weed.

- *Moraea* is another large genus worthy of a special report. The winter-growing *M. bellendenii, M. bipartita, M. ciliata, M. papilionacea, M. polyanthos, M. tripetala,* and *M. vegata* as well as the summer-flowering *M. alpina, M. alticola, M. elliottii, M. huttonii, M. polystachya, M. robusta* and *M. spathulata* are all easy rewarding subjects.

- *Pillansia templemannii* is surprisingly hardy in spite of the mild climate of its wild occurrence, but very reluctant to flower. It has to be kept not too dry in summer.

- *Romulea* is one of my preferred genera: small, freely growing and flowering plants with comparatively large and shiny, variously colored blooms. The most striking ssp., including the "great reds", are luckily also the hardiest ones (*R. amoena, R. atrandra, R. flava, R. hantamensis, R. hirta, R. monadelpha, R. sabulosa, R. subfistulosa, R. syringodeoflora, R. tortuosa* etc.). Curiously *R. viridibracteata* also grows well but it is utterly refractory to flowering over the some 12 years I have had it--any clue?

- *Sparaxis* (including *Synnotia*) are fairly hardy as are some *Tritonia (T. crocata, T. deusta, T. lilacina, T. rosea, T. securigera,* and *T. squalida)*. All are fast multiplying.

- *Watsonia* finally, encompasses some hardy species either deciduous (*W. aletroides, W. laccata, W. meriana, W. schlechteri, W. vanderspuyae, W. versfeldii* in spring, and *W. densiflora, W. latiflora,* and *W. transvaalensis* in summer/fall) and evergreen ones (*W. angusta, pillansii, tabularis,* and *W. willmaniae*).

It may be advisable to point out that some species thrive so well as they behave almost as noxious weeds, whether by seed or "spat". Examples are *Anomatheca viridis, Geissorhiza aspera, G. inaequalis, Gladiolus orchidiflorus, G. tristis, G. undulatus* (the worst), *Gyrandriris setifolia, Homeria ssp., Melasphaerulea sp., Morea bipartita, M. polyanthos, M. polystachya, M.vegeta,* and *Romulea pratensis*. So it would be wise to accomodate such invaders in pots as to avoid unwanted and endless spreading.

This is just a skimming through of the South African wealth of the hardy Iridaceae, but I still emphasize the fact I am holding as "hardy" any plant not needing the protection of a greenhouse (even if some rarities are such grown to minimize any chance of loss) yet requiring some care (pot or frame cultivations, protection from untimely rainfall) allowing them to withstand any weather hazard and achieve a good dormancy. Ony five species, *Crocosmia masonorum, C. paniculata, C. pottsii, Gladiolus dallenii,* and *Schizostylis coccinea*, have proven to be fully hardy with me. i.e. cultivated in the open without any special care.

Last but not least, I'm pleased to acknowledge the endless and appreciated help of Dr. Peter Goldblatt, who is always willing to share his intimate knowledge of this enticing family. He has also been for some twenty years and is still the faithful supplier of rarities (at least outside South Africa) such as *Babiana virginea, Hesperantha latifolia, Homeria marlothii, Lapeirousia neglecta, L. oreogena,* and *Moraea falcifolia* and so on. . . Many thanks Peter!

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Reprinted from the British Iris Society Species Group Bulletin, November, 1998

# *Iris foetidissima* and leaf miner

**Anne Blanco White--London**

Earlier this summer I was sent some specimen leaves by a member who thought they might be infested with rust. Casual inspection indicated that was probably not the case and as I ran my fingers down the surfaces I was sure. The leaves had a multitude of small bubbles halfway in size between measles and chicken-pox and rather like the blobs used in Braille. Now rust has been quite common in irises over the last two or three years, probably because of the hot and humid conditions over the summers, and a good magnifying glass is probably the best tool for identifying it. It really does look like the early stages of oxidisation of iron bars or whatever. At the same time,you need to remember that a number of the iridaceae have an orangish tinge to the leaves which can be taken for rust if you are looking for trouble. By and large,though, the irises proper don't have this coloration. Anyway, the treatment for rust is to split up your plants at the appropriate time so that they get better ventilation and fertilisation.

Now I haven't seen leaf miners in foetidissimas for something like 25 years and certainly not since I banished the aquilegias from the London garden. If there are only a few of these little bulges down the leaves they can probably be dealt with by running your finger and thumb firmly down the leaves and squashing the little bugs. If the outbreak is severe--and where it suddenly appears out of the blue it probably will be--then it really is a case for a systemic insecticide. One of the odd things about irises is that their leaves tend to have a waxy surface so a spray-on compound just runs down to ground level without doing any good. You need to mix the systemic insecticide to the appropriate concentration and pour it down the leaves to the heart of the fan so that any surplus goes on down to the roots. The tender leaf growth at the bases will absorb some of the insecticide and the roots will circulate the rest. This should be done as soon as the infestation is recognised, again in the autumn about early September and early October when the new leaves are growing fast, and in the spring when all the other plants are growing fast. Granted that we don't want to have too many fancy chemicals around the place if we can avoid it, nonetheless if a sudden major outbreak of anything can be ruthlessly dealt with at the time this is better than letting it get a good grip on any sort of crop. All the same, have a good look round for any other plants which may carry leaf miners and act as a permanent host.

# Questions, Mine and Yours

**Bob Pries--Missouri**

Although the response was not huge, I was delighted by what I received and I urge everyone to consider ideas they would like discussed in SIGNA. If you can write your own article by all means do so, but if you just have a question please send it to me and I will try to find an answer. As to my question about *Iris missouriensis* not flowering several people wrote saying they had experienced the same things. I would be interested in hearing from more of you since I don't think I have the problem licked just yet. Several people suggested they required a dry rest in summer. Although I did not mention that earlier, it is something I already do. I suspect my problem may be not enough water in spring. And another factor could be the variety of missouriensis I am growing or perhaps its provenance which I no longer remember. I did get the following definitive answer on the other beardless iris I asked about.

**Tony Huber** writes about my question concerning the origin of 'Blue Light' .

Dear Bob, I am writing you concerning the last SIGNA publication, regarding your column Questions, Mine and Yours. I am the person who may help to clear up the iris 'Blue Light' mystery.

At the time that I was in charge of research and development, we had an official 'All America Section' test garden. Tests are made to identify and judge new breeds of ornamental annuals under local conditions with the purpose of choosing Medal winners. For the tests of 1986 some special perennials had been allowed to compete. One of the members, a well known breeder and seed company (Ernest Benary), sent us seeds identified as *Iris versicolor* 'Blue Light'.

Seedlings were planted into the test-ground, and as expected they didn't bloom the same year, so we left them on the spot. I identified them as being of the *Iris setosa* type. The following year (1987) they flowered at the first week of June. The plants were identified as *Iris hookeri* syn. *setosa* var. *canadense*. They looked just like the plant I had collected several years before on the coast of northwest Newfoundland. The falls of hookeri are larger and bluer than *Iris setosa* from Alaska and the Yukon forms.

After the flowering season, I wrote Mr. Ernest Benary a letter to inform him of the misidentification of the iris species. I didn't get an answer, but in mid-September Mr. Benary came to visit the test-ground and we discusseed the matter. First he was surprised that nobody else had reported the wrong identification . He apologized and felt very sorry about it. I asked him about the origin of the iris. He admitted that somebody had identified and collected the iris on the east coast of Avalon Peninsula in Newfoundland. Mr. Benary was unable to correct the mistake because his catalogue for 1988 had already been printed. But he agreed to include an erratum in his catalogue and that is what he did.

During our discussion he mentioned that Jelitto Perennial Seeds had the right of distribution and sales of iris 'Blue Light' for Europe during 1988 and later for America. I ordered a catalogue from Jelitto and that was the beginning of new problems. Jelitto's catalogue indicated clearly *Iris versicolor* 'Blaulight' ht. 100 cm (3.28 feet) while Benary indicates ht. 60 cm (24 inches).

Could it be that Jelitto really thought that this was an *Iris versicolor* and has never seen the plant in bloom, or that he got the corrected information too late. I can not understand why he translated the trade name to 'Blaulicht'? The worst thing happened in his 1989 catalog, where he offered *Iris versicolor* 'Blaulicht' and *Iris setosa* 'Blue Light' with description of different species and trade mark. Either they forgot to take *Iris versicolor* off the list, or they did it to boost the business. Whatever, the whole procedure, instead of solving the problem, complicated it.

Today most annual flowers are imported and have a trade mark or commercial name, in spite of the fact that they are propagated by seeds. Improved shrubs, trees and perennials (iris included), have cultivar names, and are propagated asexually, while collected plants and seedlings of species should be sold under species names because the stability of seedlings is questionable or extremely variable.

What can we do now? If *Iris hookeri* 'Blue Light' has been sold and become well known, the name is here to stay and should be registered. Could it be that some perennial growers have already selected some special breed and multiplied them asexually. If they are raised from seed they should be sold as 'Blue Light' seedlings.,

Now let's talk about the good thing. Finally *Iris hookeri* has been planted in many American gardens, without our knowing it. I hope Irisarians are finding a good spot and enjoying them.

Some growing tips for *Iris hookeri*. The success in cultivating or buying *Iris hookeri* is to grow them in pots, and pots that are not bigger than one gallon nursery size. My experience is that the plant should be left in the pot. Dig the ground deep enough to place the top of the pot at ground level, mulch the surounding ground with pine-needles or cedar shavings to keep the ground clean, and cool and humid. Keep pots and ground moist but not wet during the growing season. *Iris hookeri* does not like to stand in water as many *Iris setosa* do, and can stand dry conditions during the dormant season. East or west exposure with 4 hours of sunlight is enough to get plenty of flowers. I have had pots in different locations and have not lost a single plant in seven years. They have never been moved or transplanted.

Many thanks to Tony for the above answer to my question. I will point out one thought on which we may disagree but only slightly. Seed strains as in other groups of plants can be registered cultivars. Unfortunately there is little understanding that if they are registered as cultivars, not every seedling will be that cultivar. Only those that exhibit the special trait for which the cultivar is named can be called by that name. This means that someone registering a seed strain as a cultivar has a special obligation to describe the special trait such that one can recognize the character without having the original parent present. For example 'Kermesina' was a name for red-purple variants of *Iris versicolor*. 'Claret Cut' is a vegetatively propagated color form selected from the seed strain 'Kermesina'. Cultivar-seed strains may be useful for seed exchanges but the typical vegetatively propagated clone is most rewarding. Tony's discussion points to the fact that registration can be a valuable tool in keeping straight our communication in catalogs.

I will close with another controversial issue. When the SIGNA medals were accepted by AIS there was no attempt to grandfather existing registrations into the award system. Yet without these older cultivars, especially those that fit no other category, the reason for creating the classes of species and species cross would have been moot. When the Pacific Coast Iris was recognized as a class, all the previous registrations that had been introduced were given a chance to compete. This would probably be unwieldy in our case, yet I just discovered a mechanism which might allow some of these to begin on the ballot. There is no time limit for an iris to win entry into awards competition with an honorable mention. It is true they no longer are automatically listed for a vote on the ballot, but they may be written in at any time. If they were to receive enought write-in votes they would receive an honorable mention and begin competing for a higher award. For example, 'Roy Davidson' was introduced prior to the classification SPECX. It could be nothing else. But since it was introduced prior to SPECX it never has competed by being listed as a choice. The Species Medals have been delayed in their voting because there were not enough plants competing. What if I were to list two or three cultivars that are in this situation. If you as a judge thought them worthy garden plants then you could write them in on your ballot. If enough judges did then they would go into the system to compete. I would like to hear our members' comments. Tell me about cultivars you would select. My bias is that several cultivars, which have never had their chance, are still plants worth recommending to the beginning gardener.

## An addendum to . . .

# A Guide to Species Irises,

### Their Identification and Cultivation
### Edited by The Species Group of the British Iris Society

A GUIDE TO SPECIES IRISES

Series Tenuifoliae - *Iris qinghainica* Y.T. Zhao

*Distribution:*
Qinghai province, China.

*Description:*
**Rhizome** lumpy, dark colour, forming compact tufts (see photograph no.79), old leaf fibres persisting. **Leaves** 5-15cm long, 0.2-0.3cm wide, grey-green, linear, apex acuminate, no marked mid-rib. **Stem** subterranean, very short, sheathing, lanceolate, membranous basal leaves; 3 bracts 6-10cm long, 0.6-1.8cm wide, green with paler membranous margins, 1-2 flowers. **Flower** 4.5-5cm diameter, blue/violet; falls 3-3.5cm long, 0.5cm wide, blade flared; standards 3cm long, 0.4cm wide, upright; styles 2.5cm long, 0.3cm wide, crests narrow, deltoid; tube 4-6cm long, slender; ovary about 1.5cm long, fusiform. **Capsule** ? **Seeds** ? **Flowering** June/July.

*Cultivation:*
A plant of high mountains and open grasslands around 2500m while the type specimen came from the shore of Lake Qinghai so it probably has a taste for damp land in the growing season. Only recently discovered, it is not in general cultivation yet.

*Observations:*
Mathew remarks that it is believed to be related to *I. loczyi*, but differs conspicuously in that *I. loczyi* has a far longer perianth tube and larger flowers.

I apologise to all buyers of The Guide for this oversight. I only discovered it the other day when I was filing some slides and needed to check the series of this one; interesting that no one else seems to have noticed! It does feature among the plates, but there is no trace of it in either the body of the text or the index. There are several blank pages at the back into which this can be copied.

**---Anne Blanco White**

# *I. ruthenica*

**Michio Cozuca - Japan**

It is sad that the iris sent by Mr. Edmundas Kondratas of Kaunas, Lithuania last year did not grow well here. It is maybe because of our hot and wet climate in is growing period. They must have a cooler climate in Lithuania, suitable for the iris, I think. In fact, according to James Waddick & Zhao Yu-tang's book Irises of China, this iris mainly grows in the Northeastern regions of China from where the iris seems to be introduced into Japan in an ancient time, maybe 150 to 200 years ago. And I am sure today by my study, the Somoku Zusetsu by Iinama Yokusai, 1856 (See SIGNA Bulletin No. 58, Spring 1997 p. 3061) is the first Japanese book that contains an article on the *Iris ruthenica*.

However, the problem has not yet been resolved now. It is still unknown by whom and how it had been brought into Japan. It is same thing as about the *Iris sanguinea* var. *pumila* that has been introduced into Europe and then into the U.S.

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This article has appeared in the BIS Yearbook and in The Review, the publication of the Japanese Iris Society.

# Nagai Type of Japanese Iris

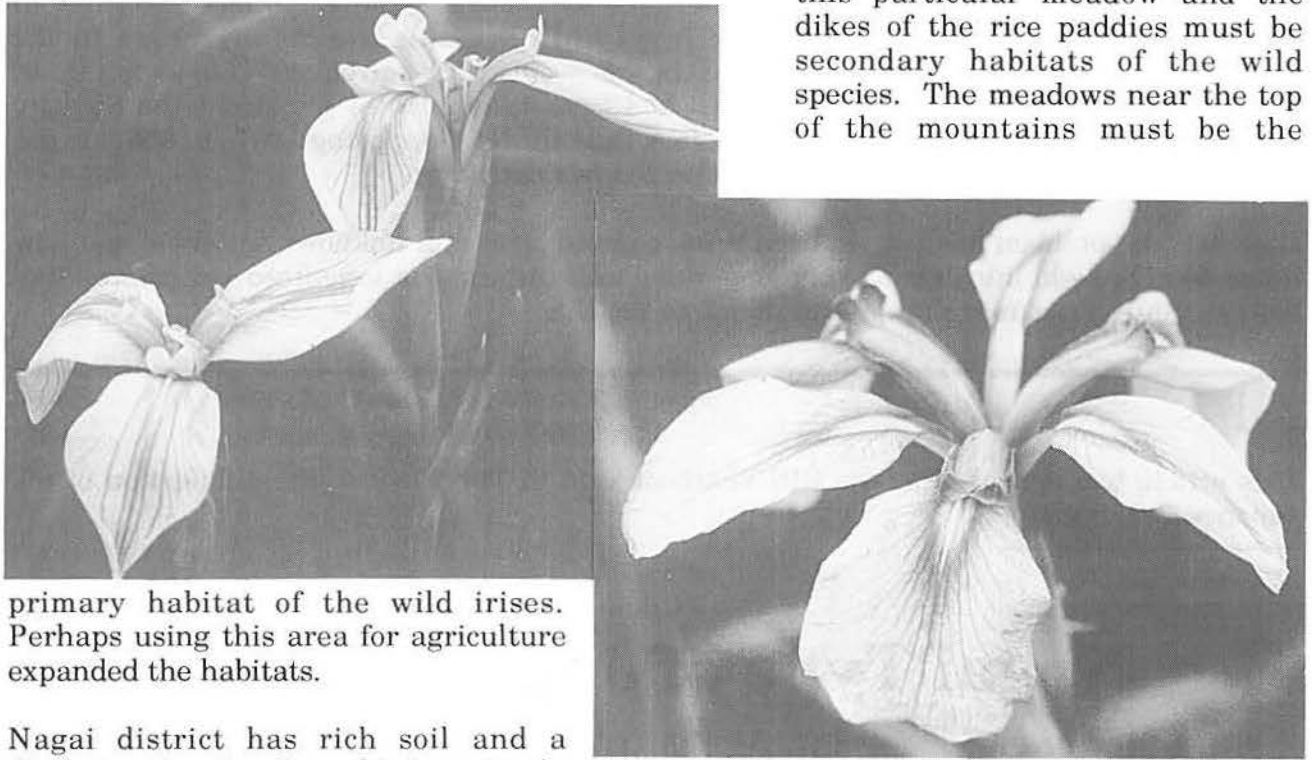
**Hiroshi Shimizu--Japan**

The Nagai type of Japanese iris is older than the other three types, i.e. Edo, Ise and Higo. Last year I visited the city of Nagai in Yamagata Prefecture for the first time. My purpose was to investigate the Nagai varieties. The Nagai district is a very important place for Japanese iris lovers, especially for those who are interested in the origin of garden cultivars. That is because the Nagai type is very old and cultivated only in the Nagai district.

Mr. Toshihiro Nagata, who grows Japanese irises for Kamo nursery, and I visited Nagai city in June 1997. Nagai is located in northwestern Honshu. We went by train and were welcomed at Nagai station by Mr. Kakima, who was a key person for our planned investigation. Mr. Kakima is an enthusiast of Nagai varieties and has tended them as a volunteer at the Nagai Ayame Garden for a long time.

We first went by car to Hagyu village on Nagai plain. It is a quiet farming village where there are many rice paddy fields. Upon taking a short walk, we found many flowers of wild *Iris ensata* growing along the dikes of the rice fields. These irises were not cultivated froms that had been naturalized, but truly wild plants. This was evident by their simple flower form with three narrow falls and almost no color variation.

After resting, we followed Mr. Kakima to a stream on a hill near the border of the Ide mountain range and the Nagai plain. We found many of the wild species in bloom in a meadow on the hill. We felt fortunate to find two plants of the white species. Mr. Nagata and I were excited at this finding but Mr. Kakima remained calm. He told us that there were many mutant forms of the wild species in the meadows of the Ide mointains. I concluded that this particular meadow and the dikes of the rice paddies must be secondary habitats of the wild species. The meadows near the top of the mountains must be the primary habitat of the wild irises. Perhaps using this area for agriculture expanded the habitats.



Nagai district has rich soil and a diversity of native flora, birds, animals and insects. Here many natural variants of *Iris ensata* have survived. The people who live in northern Honshu, and especially in Nagai district, protect nature. So wild species have great potential for developing cultivars in the future.

We visited Nagai Ayame Garden the next day. This garden has cultivars of Ise, Higho, Edo, American and Nagai types of Japanese irises. We concentrated on the Nagai varieties. I made a number of observations about these irises:

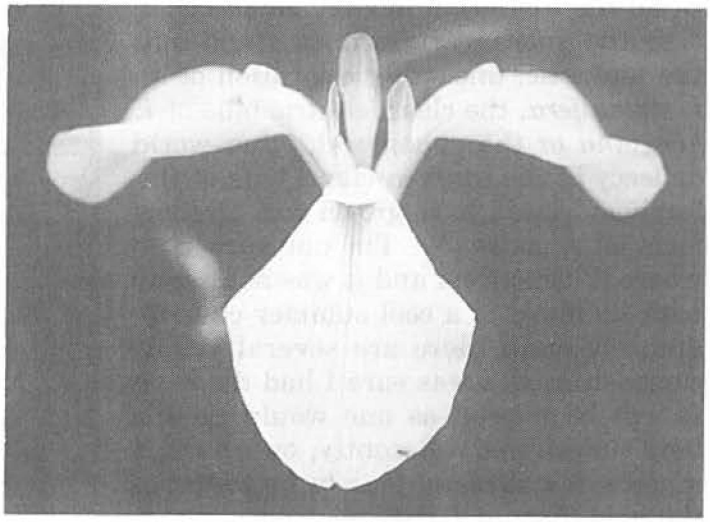
1. About half of the cultivars had simple forms with narrow falls and standards, but they showed a very wide range of color and pattern variations.
2. The other half of the cultivars had color and patterns similar to old Edo cultivars, but flower shapes and sizes were intermediate between the wild species and the old Edo varieties.
3. Some of the Nagai cultivars had flowers with a form different from any other type.

The old records of this gatden reveal that it was established in 1919, by collecting Japanese iris plants from many private gardens in Nagai and Hagyu villiages. These varied widely in colors and patterns. Mr. Kakima explained that Hagyu villagers had collected many mutant forms from the nearby mountains and plains for their gardens. They enjoyed the blooms, and often drank sake while viewing them. The bloom season coincided with the time when villagers took a vacation from their agricultural work.

Hagyu village was a castle town in olden times, and very crowded. It had long been the home of cultured people who enjoyed collecting, growing and appreciating clones of *Iris ensata*. This activity at Hagyu might be called "the birth of Japanese iris cultivars". I believe that the following factors were necessary for this "birth":

1. Gene pool: Many mutants were brought into a limited area from the wild.
2. Open pollinatin by bees. The resultant seeds increased the flower's variation.
3. Devotees: Many people enjoyed the flowers, selected the most beautiful forms, and possibly they exchanged the best flowers.

The iris breeding activity of Japanese gardeners has always differed from that practiced in the West. European and American gardeners obtain new cultivars by crossing different species, but the Japanese gardener pursues variation by gathering mutants from the countryside.

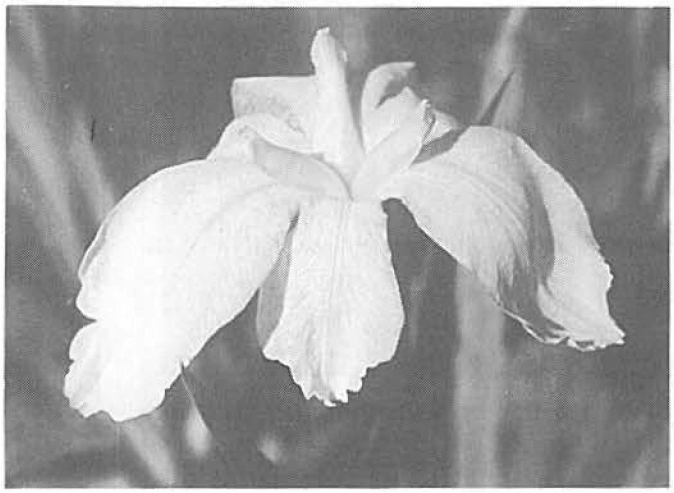


As an aside, I believe that the addition to the gene pool of white forms was probably critical to delvelopment of a wide variety of colors in cultivars of *Iris ensata*. The wide range of color and patterns would not be possible without genes for the white color. If there were not a pool of recessive genes for white, other mutant color variations would be masked with purple or violet, which are the dominant genes in the species. There is no sign of any species other than *Iris ensata* having played a role in the development of the Japanese iris. All cultivars and wild forms of the Japanese iris have the same chromosome count (2n=24). The only exceptions are aneuploids of Ise varieties (2n=25), and these cross readily with natural diploids. Therefore they are regarded botanically as belonging to a single species. The Nagai type is the oldest of all the Japanese iris groups which survive today. Its existence gives us hints as to how the modern cultivar was born, and provides material for future hybridizing.

# *This and that . . .*

### Colin Rigby

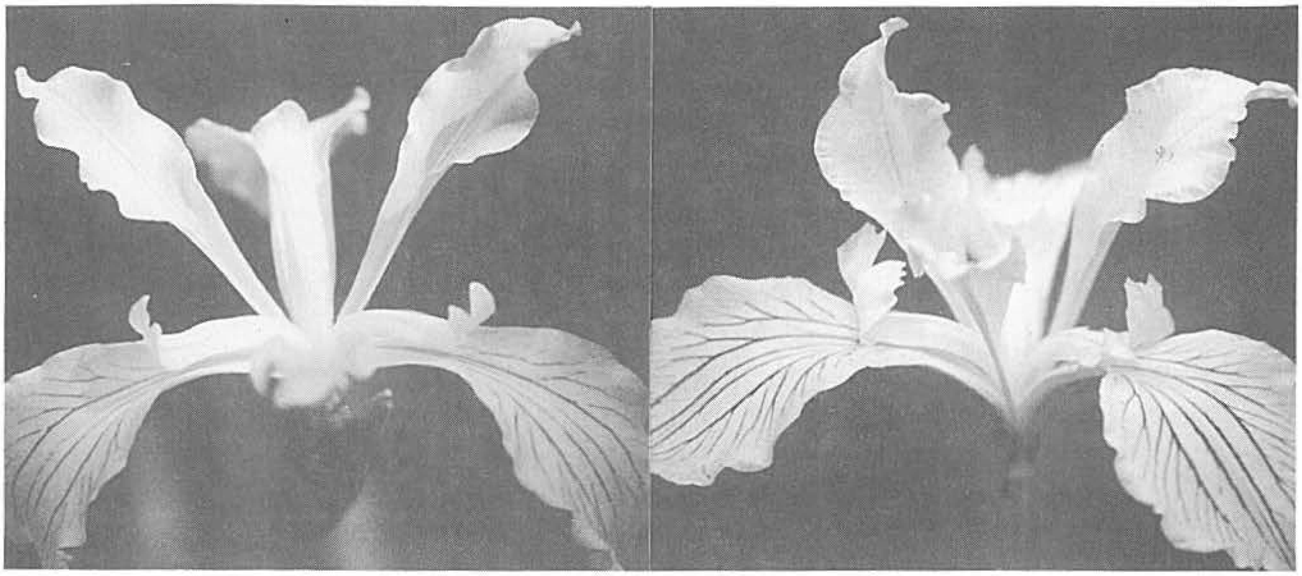
It is with great sadness that I report that the two *I. setosa* var. *interior* sent by Lawrence Duffy, Fairbanks, AK have left this world. Although they seemed to be growing well last fall, they have not emerged this spring. Too much water? Planted in the wrong place? It's disappointing and leaves a big question with me as to why plants do well in one place and not another. In four years, *I. tectorum,* for example, has sent up one bloom stock while it grows and blooms for others just a few miles away. I know that registered iris clones are supposed to come from one initial plant, yet I can vividly recall three instances where a plant grew miserably and failed to bloom, although a rhizome of that same iris from another garden source grew and bloomed well. These were all tall bearded hybrids. Species and some beardless iris that did not perform well for me were chalked up as not liking the weather, soil conditions or my fumbling gardening ways. I've no answer, certainly, but apparently others have also experienced this phenomenon. Do you have a comment?



***I. nelsonii***

Of all the irises that have come and gone over the years, certain ones stand out: the fantastic, unearthly coloration of an *I. stolonifera*, the clear, electric blue of *I. hoogiana* or the ephemeral, other-world delicacy of the *confusa* clan. One of the loveliest irises I have grown was a yellow form of *I. nelsonii.* I'm not sure now where it came from and it was not happy with its move to a cool summer climate; and, although there are several yellow clones around, I was sure I had the best. It will be missed as one would miss a long time friend. Recently, on a lark, I made a few crosses, four to be exact, of the wild form of *I. ensata* to some three-fall modern hybrids--the form I prefer. I was going to set the Japanese iris group on its ear with flowers displaying the grace of the species on tall branched stems. And, of course, there would be some color variation. The seedlings bloomed for the first time last summer and I was amazed. All of the flowers were variations in color and form of the species, and oddly, the better looking flowers were without a branch. In talking with a well known Japanese iris hybridizer, she said that she had quit using **ROSE QUEEN** in her breeding program because all the seedlings looked like **ROSE QUEEN**. I had often wondered how such big and varied flowers could all come from one iris species until I recently heard that the early variants were selected from clones growing in the wild. The article on the Nagai type elsewhere in this issue gives credence to the amazing role of natural hybridization. I now look at the wild form of *I. ensata* with a new admiration. What a strong constitution it has!

Several years ago when we were looking at Pacific Coast Native irises in the wild, we came upon two plants of *I. bracteata* growing several inches apart from each other. The marked difference between the two plants was most apparent and since I had no film in my camera (it happens frequently), I asked Carla Lankow to take photos of the plants. They are shown here. The flower with the better form and wider petals also had a group of ants gathering the nectar at the base of the petals, proof again of the wonders of nature's hybridizing techniques. Perhaps we should not try so hard to turn apples into pears.



This is my last issue as editor of the SIGNA publication and there is no way to express my appreciation and gratitude for the help and support given. Jean Witt spoke of the many doors that botany had opened for her. To count the doors that SIGNA and my association with it have opened for me would be impossible.

Thank you all and Happy Gardening,

Colin