DWARF IRIS SOC.

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DWARF IRIS SOCIETY.,MIDDLEBURY, IND.

PREFACE

As I begin to assemble the various papers which constitute the new edition of the Portfolio I am concious and impressed with the great advances in knowledge of iris that have resulted from publishing much of the available information of the past as well as the addition of new findings as it is learned thrugh the experiences and experiments conducted by various individuals and in the program of the DIS for research.

To one like myself, who started working with dwarfs when they were probably the most neglected and least appreciated of any class of iris, and who was painfully aware of the deficiency of authoritative literature upon the subject, it is gratifying to realize the magnificient contribution to a better condition which prevails today.

Through the medium of this Portfolio have come the writings of most of the real authorities on dwarf iris living today, and from many lands, including some notable scientists, breeders, and amateur growers who have cooperated with experimental crosses.

Although the field of study and research is so vast that it will take a long time to even approach the limits, it is my opinion that even at this time we know more about the inheritance and means of progressive achievement than any other flower society. And certainly the rank and file of our membership is better informed upon the technical aspects of breeding than most societies.

Our contributions to iris knowledge not only has been of value to the dwarf iris people but in many instances is applicable to the work in other fields, particularly the tall bearded iris. In the matter of the spot pattern theory (amoena, variegata, neglecta, Pinnacle, etc) we have arrived at a definite conclusion regarding this inheritance. We have published conclusive evidence regarding the various inhibitors, apparently all originating in the dwarfs. Through our efforts beard color has become a matter of concious recognition, which heretofore had no recognized significance. We have learned how to bring down to the dwarfs certain characteristics of the Talls, which are not naturally inherent in the dwarfs, such as plicata for example. We have solved many and various genetics problems which are particularly applicable to the different species and forms of the dwarfs.

It was the DIS which first published the information on mosaic virus, that brought people to a conciousness of its existence in iris. It was the DIS which brought iris pigments to the attention of growers and published data concerning the pigment tests of Dr. Werckmeister and others.

After all the years of work with talls, apparently no one knows the true nature and inheritance of such factors as the plicatas and tangerine pinks. We are now working on this problem and hope to have the answers before long.

I could go on at great length with further examples of our contributions to knowledge in the iris world, but most informed persoon will readily recognize from the pages of these Portfolios, that they represent a great and worthy addition to the literature of dwarf iris.

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DWARF IRIS SOCIETY SYMPOSIUM 1961

SYM. RATING	VARIETY	TIMES VOTED	NO. OF POINTS	QUALITY RATING
1.	BLUE FROST	62	5920	95.48
2.	ANGEL EYES	49	4620	94.28
3.	HEART'S CONTENT	10	930	93.00
4.	PERKY	11	1000	90.90
5.	BLACK BABY	40	3480	87.00
6.	ABLAZE	70	6080	86.85
7.	CLAIRE	22	1910	86.81
8.	BRIGHT WHITE	60	5190	86.50
9.	CHERRY SPOT	75	6430	85.73
10.	WHITE MITE	57	4880	85.61
11.	VERI-GAY	67	5610	83.73
12.	SPARKLING EYES	57	4760	83.50
13.	FASHION LADY	53	4420	83.33
14.	BURGANDY VELVET	13	1080	83.07
15.	WHITE ELF	30	2480	82.66
16.	FLAXEN	22	1810	82.27
17.	GAY LASSIE	68	5580	82.05
18.	LITTLE JOE	53	4340	81.88
19.	RED GEM	55	4500	81.81
20.	BLUE WHISKERS	25	2040	81.60
21.	GRANDMA'S HAT	24	1950	81.25
22.	APRIL MORN	39	3150	80.76
23.	WEE BLUE	27	2180	80.70
24.	BLAZON	42	3380	80.47
25.	HULLABALU	30	2410	80.33
26.	BUTTERBALL	40	3210	80.25
27.	PASTEL DAWN	31	2470	79.67
28.	VIOLET NIGHT	45	3580	79.55
29.	DREAM CHILD	41	3210	78.29
30.	PROMISE	41	3200	78.02
31.	FLASHLIGHT	13	1010	77.69
32.	PATH OF GOLD	39	3000	76.92
33.	BRIGHT SPOT	33	2530	76.66
34.	GARNETTE	12	920	76.66
35.	TEAR DROPS	11	840	76.36
36.	ROSY CARPET	18	1370	76.11
37.	MOPPETT	12	910	75.83
38.	ORANGE GLINT	28	2120	75.71
39.	LITTLE MOHEE	25	1890	75.60
40.	BUTCH	31	2340	75.48
41.	VIOLET GEM	26	1960	75.38
42.	BUSTER BROWN	29	2180	75.17
43.	DIRTY FACE	36	2700	75.00
44.	BLUE BAND	25	1870	74.80
45.	STYLISH	22	1640	75.54
46.	BLUE SPOT	31	2310	74.51
47.	MUMBO	16	1190	74.37
48.	RED AMETHYST	32	2360	73.75
49.	PRIMUS	25	1800	72.00
50.	HONEY BEAR	13	930	71.53

DWARF IRIS SYMPOSIUM BY COLOR GROUPINGS

WHITE
White Mite
Bright White
White Elf
Crispy
Snow Baby

BLUE
Blue Frost
Claire
Wee Blue
Enamel Blue
Flaxen

LAVENDER
Blue Doll
Lavender Dawn
Bimbo
King of the Rocks

VIOLET
Violet Night
Violet Gem
Mumbo
Sulina
Compacta

BLACK
Little Joe
Black Baby
Black Top
Littel Villain
Jet PEtite

PLICATA
Knick Knack

TAN-BUFF BLEND
Honey Bear
Little Pup
Fior del Mondo

ORANGE
Orange Glint
Fortissimo

YELLOW
Fashion Lady
Bright Spot
Butterball
Path of Gold
Sound Money

CREAM-LEMON
Pastel Dawn
Tear Drops
My Daddy
Inchalong
Hanselmayer

PURPLE
Perky
Blazon
Stylish
At roviolacea
Butch

ROSE-TONES
Cup & Saucer
Rosy Carpet
Mist O'Pink
Tara
Rose Petite

RED
Red Gem
Glow Gleam
Red Amethyst
Garnette
Vindobona

ORCHID-PINK
Orchid Flare
Promise

COPPER-BRONZE BLEND
Copperita
Little Jewel
Rose Mist
Jackie Jean

AMOENA
Angel Eyes
Heart's Content
Cherry Spot
Sparkling Eyes
Cradle Days

VARIEGATA
Ablaze
Veri-Gay
Brownette
Three Coins
Primus

NEGLECTA
Morning Fresh
Grandma's Hat
Spring Joy
Moppett
Blue Spot

PINNACLE
Gay Lassie
Bright Spring
Dew Drop
Little Elsa

GREEN
Dirty Face
Greenie
Little Charmer
Green Sprite

BROWN
Buster Brown
Little Mohee

BLUE-YELLOW BICOLOR
Blue Whiskers
Dream Child
November

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DWARF IRIS SOCIETY AWARDS 1961

WALTER WELCH AWARD MEDAL

Angel Eyes (Bennett Jones)

BLUE RIBBON AWARDS

Perky
Blue Whiskers
Heart's Content
Morning Fresh
Claire

AWARD OF SPECIAL MERIT

Glow Gleam
Atomic Blue
Orchid Flare
Chicken Little
Nancy Marie
Black Top
Brownette
Blue Doll

TEST GARDEN CERTIFICATE

Doriot	28-58
B. Jones	90-7

TEST GARDEN POPULARITY PRIZE

Doriot	28-58
Kavan	12-58
B. Jones	90-7

VOTES CAST AS FOLLOWS:

Angel Eyes	15
White Mite	5
Black Baby	3
Fashion Lady	2
Red Gem	2
Wee Blue	1
Gay Lassie	1
Bright White	1
Flaxen	1

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Perky	17
Blue Whiskers	15
Heart's Content	14
Morning Fresh	10
Claire	10
Little Mohee	9
Pastel Dawn	7
Rosy Carpet	7
Dream Child	6
Bright Spot	6
Burgandy Velvet	6
Honey Bear	5
Ducky Lucky	5
Cradle Days	5
Mumbo	4
Hullabalu	4
Sky Patch	4
Flashlight	3
Dirty Face	3
Tear Drops	3
Cradle Blue	2
Little Blacksmith	2
Garnette	2

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Glow Gleam	11
Atomic Blue	8
Orchid Flare	8
Chicken Little	7
Nancy Marie	7
Black Top	6
Brownette	6
Blue Doll	5

The Blue Ribbon Award is given to the five varieties receiving the most votes, with a minimum requirement of 10 votes. Eligibles require a previous Award of Special Merit.

Any registered and introduced variety is eligible for the Award of Special merit. Five votes required. The varieties not receiving this mimimum of five votes are not tabulated in above list.

Thirty three ballots were cast by the 50 DIS Judges, which is not a very good showing.

SYMPOSIUM COMMENTS

This year we have listed two separate symposiums; one conducted according to our usual procedure, and the other devised under a schedule by color and pattern classes.

In both lists we have followed our previous rules for obtaining a quality rating and then listing them in order according to the ratings.

Both types of Symposium have a distinct benifit; if there could be a preference I would favor the schedule by color classes, as in this way the competition is restricted to colors or patterns of a similar kind. For a blue or yellow to have to compete with a variegata or other type can hardly be considered as fair competition.

Another advantage of segregating varieties under separate categories is that it can at a glance determine the five best varieties within that particular class. This is particularly appreciated by the uninformed gardener when they attempt to plan and buy plants for their garden. A further advantage is that I find some varieties listed in the Color Symposium that has not yet been shown on the regular Symposium. Take for instance by the regular Symposium we would never know that a plicata exists in the dwarfs. But under an individual color or pattern schedule it was brought out of obscurity. I find several varieties listed here that have not as yet reached the regular Symposium.

And yet for comparison of relative quality, both appear to have reached about the same as to comparative merit.

I can assure you that the judges did not find it easy to vote this new Symposium. Formerly they could casually go ahead with listing their favorites, regardless of color or kind, in the order of their preferences and often this did not always indicate the best quality. But in this new Symposium a judge was forced to make detailed comparisons, to know when and why one red was better than the other one.

Another thing which puzzled the judges was where to place certain varieties. You will be surprised if you should try to place a variety under its proper section. There are always the borderline cases that are most difficult. For instance where to draw the line between red and purple, or purple and violet? Where does Rose-tone and orchid-pink divide? Or even blue and lavender, or where does violet end and black begin?

But in spite of the difficulties, I think it is especially good training for the judges to attempt to sort out the various forms into their proper place. I would advise all of our members to at least make an attempt to look at your iris from this standpoint.

You will note that under some sections the representative varieties are very scarce, as in plicata, browns and orchid-pink. This should be an incentive for breeders to work on these scarce groups.

Another problem which must be considered is the difficulty of determining the various color and pattern headings. Some dealers have attempted such a list for their catalogues but no uniform or official schedule has been established, and any such list must necessarily be an arbitrary undertaking which may have to be altered as time and experience warrants.

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PRESIDENT'S REPORT

During the past year the Dwarf Iris Society has made many important strides. As usual our annual meeting was well attended with many new members attending for the first time.

A constitution has been drawn up by our Executive Board and will soon be ready for membership ratification. The writing of this constitution demanded much consideration and thought. It is hoped that it will long be useful to the society and its members. In formulating this set of rules we have tried to keep democratic principles paramount.

Another post has been added to the Executive Board, that of Experimental Supervisor, which will be filled by one of our most capable members, Mrs. Alexia Gerberg. Mrs. Gerberg is most interested in hearing from any our members, who are willing to take part in our experimental hybridizing. This is an excellant opportunity for anyone who is seriously interested in learning by experimentation the answers to many genetic problems. For further details write to Mrs. Alexia Gerberg, Naches, Washington.

The large number of fine new seedlings which are now in the various DIS test gardens is a fine indication of a productive hybridizing program being carried out by our members. Late reports from the various test garden supervisors, inform us that a large number of new seedlings were received this year.

I've received many letters from our various dwarf iris hybridizers protesting the increase in registration fees. Since the increase in fees is to be used to support the TB test gardens, our members are justly disappointed with having to help support a test garden from which they will get no benefit. Perhaps the AIS will appreciate their views and again restore the former fee for registration of dwarf iris.

We are now in the process of preparing a new Dwarf Iris Check list for publication. The first such list, published in 1955, has long been out of print and with the addition of many new seedlings registered, this second edition is much needed.

In addition to the list of all known dwarf iris the check list will also contain valuable information on all species of dwarf iris, a glossary of terms, a list of chromosome counts of the dwarf iris, a section on dwarf iris hybridizers and other valuable information. I suggest that your order be placed early to insure your receiving a copy as a limited number will be published. More information concerning the ordering of your check list will be given elsewhere in the Portfolio (page no. 12).

Our 1962 annual meeting has been tentatively set for the weekend of May 12th and 13th. If anyone needs a stimulus for increased interest in dwarf iris, then attend this meeting at Middlebury. There you will see the finest collection of dwarfs. In addition there are hundreds of excellant seedlings as well as a large experimental planting of various species.

Here's hoping to see you at Middlebury during 1962.

David L. Reath

NEW DIS.AWARDS

Although this report is somewhat delayed,our Executive Board has approved previous plans for fome new awards, which were prepared and the first recipients presented with them during this year. A new medal was designed, now known as the Walter Welch Award, to represent the highest award for a Dwarf Iris, It was presented to Walter Welch in 1960 for his variety Cherry Spot, and to Bennet Jones in 1961 for his variety Angel Eyes.

The Board also indicated its desire to present a new token of recognition of Distinguished Service to the DIS, with a bronze plaque upon a walnut wood base approximately 13 1/2 x 9 inches, with the following engraving: " PRESENTED TO____________ in appreciation of devoted and unselfish service in furthering the progress, aims and ideals of the Dwarf Iris Society."

At our DIS meeting this spring it was presented to Walter Welch, the founder of our society, and to Helen Doriot. For very obvious reasons it is logical that Walter Welch should receive the first of these awards, but probably not all members are familiar with the great contributions that Helen Doriot has offered to the society.

Helen was one of our first members when the Dwarf Iris Club was formed, and was a Charter Member. She was in our first DIS Robin. She was amoung our first group of DIS judges and has served a highly valuable service in an advisory and promotional capacity over many years. In our early days she acquired a pretentions collection of dwarf varieties and species, and prepared and grew them in her extensive garden, which along with her plantings of seedlings provided a special attraction for our members attending our annual spring meetings. All of us will remember those exciting Sunday mornings in her garden, where always a delicious breakfast of hot rolls and coffee was served in the "Poutin House" situated beside a large pool and surrounded by a beautiful rock garden.

But in addition to such activities as hostess and the social embellishments, she was always ready in promoting the societies affairs, in furnishing plants to breeders, reports on the test Garden seedlings, with talks and lectures at meetings, and she has held office as Board Member, Awards Chairman, and Experimental Chairman.

For 1961 this Distinguished Service Plaque was presented to Rudolf Hanselmayer and Leona Mahood.

Rudolf Hanselmayer is known through correspondence to many iris breeders in America, but he first became known to us through his contacts with the DIS. Living in Graz-Puntigam, Austria, he is in close proximity to the native habitat of I.pumila and other dwarf iris species. He habitually goes on collecting trips, searching for the various forms of these species, until he probably has the most extensive collection of species in Europe.

Very early in the life of the DIS he began sending us new species from his trips and his collection, until our own large collections are predominantly gifts from his hand. This alone would suffice to make him eligible for this award, but in addition to this he has contributed much in the way of literature in this field. Almost every issue of our Portfolio contains some valuable article on the classification, description, culture or other aspects pertaining to dwarf iris and particularly concerning the species I.pumila.

Collaborating with Dr. Van New, and other irisarians in that part of Europe he has accomplished much in the promotion of dwarf iris interest. Last spring the German Iris Society held their annual meeting at his garden at dwarf iris blooming time, which is unprecedented for the usual iris societies of the world, which ordinarily prefer the TB iris. We in America are particularly indepted to him for various services which could have been obtained from no other source.

Our other recipient, Leona Mahood, is probably known to most members of the DIS through her various activities. Early in the existence of the Dwarf Iris Club she had organized a local group of dwarf iris enthusiasts into a club. Soon after it was on a working basis she suggested having an official Dwarf Iris Test Garden at her home in Seattle, Washington. This was approved and she became our first Test Garden Supervisor. Ever since that time her ability as a leader and representative of DIS has extended the interest in that area and the results have made this Test Garden and this group a shining example for all others to hope to achieve.

She was a Charter Member of the DIS and a member of our first Dwarf Iris Robin. She was amoung our first group of DIS judges, and has served on our Executive Board from the beginning. It is needless to say that her good judgement which could promote the interest in her own region, was of inestimable help in advice and consultations on DIS problems.

The medal will be awarded annually to a variety receiving the most voted of the DIS judges and which has previously received the awards of Special Merit, and the Blue Ribbon Award.

However, the Distinguished Service Plaque may be given at intervals when the Board deems it advisable, and to persons whom they recognize as worthy of the honor. Therefore there is no precise schedule for presenting this award.

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MIDWEST DWARF IRIS TEST GARDEN

Lucille Kavan

The 1961 Annual Spring meeting climaxed a cold, rainy spring with little sunshine, Many established iris plants were stunted and had little or no bloom. Some of the seedlings bloomed early or after the meeting date. Sunday May 7th our meeting date started out rainy and finally cleared before noon. As usual named and introduced iris plants were set in pots and brough into the basement before our meeting in anticipation of rain. These iris plants were set on tables and ourmembers were able to see and evaluate the different blooms in comfort. We had coffee and rolls and visited with members from Iowa, Nebraska and Kansas. Toward noon it stopped raining and we put on our boots and went thru the test garden to see the seedlings. It was surprising to see how quickly the warm sunshine opened more buds. Zickler's Seedling DK 66-13, a cream self, five inches tall, has good form, 1st year bloom, was given 5 votes for the popularity prize, Roberts B-501 seedling from Flavissima Blazon was in full bloom and it is a honey, about 6 1/2 inches tall, flower an olive-yellow self, flaring falls, one terminal bud on a thin stem well above the neat foliage; it was given 9 notes for the popularity prize. Bennet Jones 93-1 bloomed several days before the meeting and was seen by several members, it also was well-liked. Some of Alta Brown's seedlings and named varieties bloomed before the meeting date and only 2 of Donna Simonson's. Her seedling 907 and 804 bloomed after the meeting and are worthy of awards. Kavan's 12-58 a golden yellow self, 9 inches tall, was given 9 votes but might prove to be a median come next year's bloom.

Of the new named varieties in the test garden, Frost Imp did not bloom, neither did How Now or April Mist. But Curtsy is a lovely dwarf iris white standards, light violet falls, about 7 1/2 inches. Mrs. Reinhardt's 7 seedlings sent to the test garden, only K60-4 an icy blue self 8 1/2 inches put out one bloom stalk.

Some of the newer varieties in bloom were Blue Frost, Fashion Lady, Grape Spot, My Daddy, Angel Eyes, Blue Doll, Glow Gleam, Bee Wing, Cradle Blue, After voting and evaluation of the dwarf iris blooms, our usual auction of surplus plants was held and the meeting was climaxed with a dinner at the Fireside Restaurant.

ANNOUNCEMENT

I wish to announce that the DIS plans to publish a new and revised edition of the Dwarf Iris Check list early in 1962. As the number of copies published will be limited to what we consider necessary to meet the demand, we advise you to notify us if you desire a copy so that we can reserve it for you.

The fee will be $2.00 per copy. Please send in your order to Mrs. Lee Armiger, 24300 Edgemont Road, Southfield, Michigan

As with the previous edition, it will contain the usual list of varieties and species, with full description, introducer and date of introduction, awards, etc., and in addition will be articles on the species, a list of chromosome numbers, awards, Symposium, the DIS standards, a glossary of definitions and pronunciations, and a list of the leading breeders and collectors of the past and present with short biography.

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THE SEASON AT MIDDLEBURY

It seems that it is not unusual to gripe about the weather and season for an iris meeting, for it occurs with the TB meetings as well as the dwarf meetings, but this year it appeared to be more general everywhere than in previous years. I know that we missed peak bloom by about a week for our meeting, then for some reason many plants did not bloom, and some showed poorly as abnormal bloom. However in spite of all obstacles the eventual results were quite gratifying.

I shall first try to comment on the seedlings in the test Garden. Although the judges missed bloom on some of them , there were enough in bloom at our meeting for a farily good showing.

Helen Doriot's seedling #23-53 won the Test Garden Certificate and the Popularity Prize. However we ran into some difficulties with the voting, due to an error on my part. I had planted my S-504 blue pumila in the TG plot last year, and as it was later registered and introduced prior to the voting, we considered it ineligible as a seedling for competition. Also it had received the award of Special Merit as a variety and thus could not enter both competitions. With 13 judges voting here, S-504 now named Atomic Blue, received 11 votes, Doriot 23-58 received 7 votes, B.Jones 98-1 received 6 votes, and Zickler's DG-32-19 received 5 votes.

One of my favorites was Simonson's 303 which also was nice last year. It is a nice lavender-blue with a rosy-purple spot on falls, dainty, well proportioned, bloom well above leaves, narrow lêaves and very profuse in bloom.

Another of Simonson's I liked was seedling 1012 with a gray-blue top, purple blend falls, violet beard, excellent shape, 7 inches high.

Bennet Jones 90-10 was a nice clear and bright blue with darker spot around beard, nice horizontal falls but standards somewhat open.

Mahood had an attractive cream with tan halo, yellow beard, #10-60, with nice shape bloom but a little too big bloom for my taste. But her #18-60 a purple with gold beard had excellent shape, smooth color, small bloom and nice proportion, 7 inches.

Ed Zicklet showed another of his dark violet seedlings, #DG-32-19, very dark, violet beard, domed and flaring, which received much favorable comment. Another one, #1E-25-21 was an ivory-white, white beard, with blue blaze below beard, nice shape and good progportion.

Of the newer varieties here on display Greenlee"s Knick Knack received favorable attention because it is our first plicata to come within the dwarf range.

Mumbo put on a good show in deep violet and is among the better varieties in this color range. Grandma's Hat was much better this year, in fact really fine. It has lavender-violet standards and velvety plum-purplefalls, beard yellow tipped white. Nice shape and about 6 inches tall.

Chicken Little is a light yellow standards with cream falls, beard white and a greenish spot on falls, falring, fine shape. Varieties in this color are not unusual in dwarfs but this is a nice one.

Nancy Marie is a darker blue than Cradle Blue, and with good clear color.

In the TG plot was Brown's 54-9 which considering the scarcity of browns is a good addition in this color section. It is a bitone, yellow beard, but shape could be improved.

Brown's M-13-4 is much like Chicken Little but larger on the whole with coarser leaves, but excellent shape and a big fuzzy beard.

Bee Wings is a rather attractive yellow with brownish spot on falls just below the beard and nice shape.

Holly is a brand new one here, which might be called a white with chartreuse-yellow around a white beard, excellent shape.

Chris is a dark purple, violet beard of good shape, but for the fact that we have so many good things in this color it would be outstanding.

Black Baby still seems to get much attention because of its fine shape, but it certainly is not within the black range of color as it blooms here. By actual comparison it is very near to the color of Sass Dark Ruby.

Butterball is still the finest lemon-yellow in the arenaria bybrids, superior shape with no open standards as usually found in the arenaria hybrids.

Amoung the Welch seedlings T-535 appeared to receive general praise from all visitors. It comes from (L-571 x Morining Fresh). L-571 is a Pinnacle type with blue beard and of course Morning Fresh is a blue neglecta. This gave in T-535 lavender standards with deeper rosy-lavender falls, white beard.

In dwarf breeding we expect each year to get some new and outstanding things, it isn't a case of wondering if, but rather what, we will get that is worthy. This spring I found our first Pinnacle type in the dwarfs. We have good Pinnacles in the hybrid dwarfs but so far had none in pure pumila. This new one is U-505, with white standards and yellow falls. Its parentage is (Red-violet Austrian Pumila x L-508 yellow Pumila) x R-509 cream-white pumila).

As yet we have no amoena variety in the pure pumila group, though I have had some amoena seedlings which I did not consider good enough for naming, such as M-507. But this year I had two pumila amoenas which appeared to be much improved over previous seedlings. One was U-501 from (163 pumila x H-501). The other was U-512 from Spring Joy x)-514 a variegata pumila. I have often advised our members to give careful attention to the selection of parents for their particular purposes. If we wish to get a good strong spot on the offspring, both parents should have as strong a spot as possible, and any outcrossing to a self pattern will dilute the dosage of spot, resulting in a weak spot. Another thing to consider is that it appears that blue, violet, and yellow spot are easily interchangable where both colors are in the parentage, and when white is also in the composition we can expect to get amoenas, pinnacles, neglectas and all kinds of variations. It appears that my H-501 has all of these colors in its composition and for this reason has consistently been an excellent parent for the full range of spot pattern forms.

This year I found a pumila that comes the nearest to a black pumila that I have known. It comes from Vindobona a red Pumila x a dark violet Crimean pumila, This is U-506.

Two of the most exicting seedlings came from K-510 x P-502, the latter is White Mite. I have remarked before that K-510 has consistently thrown green color, and thus has been recommended for work in this color range. K-510 comes from (San Francisco x pumila) x (Cook-1546 x Carpathia). It is with chartreuse standards and falls a blend of darker green and yellow, white beard, I have noted previously that when K-510 is crossed with whites it invariable gives greens but when crossed with other colors it gives rather dirty blends, which has caused me to state that white is of necessity to be considered as one of the main ingredients in green color. Green needs a white background for green color to show.

In this progeny were U-508 chartreuse standards and deep green falls of nice clean color, and with blue beard. U-509 was an amoena, white standards, dark green falls, white beard. Both had excellent shape, about 6 inches tall.

I also found a fine shaped white pumila this spring, from R-509 x R-517, both parents near white, Of course as we now have a couple of good white pumilas, as White Mite and Snow Baby, it is not a new find. But from our observations this spring it appears that White Mite contains an intensifier factor which tends to deepen the color rather than dilute it. And we might hope that a different white pumila may lack this intensifier gene so that we can use it for obtaining pastel and tinted tones of anthocyanin colors. This new white is #U-504.

Some of our members have been interested in working for ruffled flowers. In the past we have had some forms which showed a tendency to ruffling, particularly in hybrids involving TB iris, But this spring I found one that undoubtedly is the most ruffling that I have seen in any dwarf so far.

It is U-519 and comes from (M-554 x L0547). M-554 is from (Blue Shimmer x Carpathia) x self). L-547 is from the same parentage, so we can consider them as sister seedlings. It is yellow standards and cream falls, yellow beard. Which brings to mind a suggestion of Bliss, that ruffling may result from a compromise effect from two different size and shape blooms, rather than from some gene factor for ruffling. Here we have such a combination of tall and pumila.

Each year we seem to get a different crop of seedlings, one year pumila seedlings will predominate, another year it may be the later dwarfs, depending on how the pollinations take. This year it happens I had the best results from Lilliputs. I always grow some hybrids within this type of seedlings for further work towards the dwarfs and my Lilliputs are usually the result of such breeding. I was fortunate this year in gettin several outstanding things in this class of iris.

For example I found our first Lilliput in the black range, U-406 came from R-537 x D-536. R-537 is a purple from Lights On x Nana, and this crossed with Orange Glint. K-536 is from a purple chamaeiris form. U-406 is a black of good color, nice shape and with a bronze beard.

There was also one in the true dark violet range, a self with violet beard, 11 inches high. It is U-405, and very distinctive. Its parentage is interesting, (R-401 x White Mite). R-401 is a plicata from J-10 TB plicata z Cretica. The violet self pattern is explainable from our test results of breeding with plicatas, as apparently when the plic gene is absent the blue of the edging reverts to a self pattern. But we would expect White Mite to dilute this anthocyanin, making it lighter tone. However in several crosses which bloomed this spring, we found that White Mite contains an intensifier gene which tends to darken rather than lighten the color. Anyway this appears as what happened in this case.

One of the best in an amoena effect that I have seen was U-413. It had pale cream standards and deep brown falls, yellow beard, good branching and 11 inches high. Border on falls nice and clean cut. This one came from P-524 x Lilli-Bitone. P-524 is Lilli-Var. Thus this was a cross of a vaiegata x amoena.

I had two fine Wabash type amoenas this year, U-409 and U-415. U-409 is the best one, with pure white standards and deep solid spot of violet with nice white border on falls. It has very superior shape and came from P-531 x ?). P-531 is my Pinnacle Lilliput called Lilli-Bright. U-415 came from Lilli-Bitone x ?). I find that intercrossing my Lilliputs is giving some excellent results.

There was a new plicata this year much better than my previous ones, better in shape and in the nice stitching of margin. This was U-411 and came from R-401 x ?). As I mentioned above R-401 is a Lilliput plicata.

One that was really exciting had blue standards, yellow falls, white border and blue beard. Another was a white with blue beard. U-412 came from mixed seed and was a dark antique gold self of fine brilliance and gold beard. Altogether I saved 17 Lilliputs, several of which are worthy of naming.

Sometime back I had found a nice black that came from mixed seed, fine deep color with black beard. It grew to around 8 inches high and I had lined it out for later introduction. This year it grew up to about 11 inches and had two branches on the average among the plants. It is even darker than my Black Top. It will certainly be introduced but in previous years it has shown no branching, and such things make it difficult to know whether to introduce it as a dwarf or as a Lilliput. It is R-536.

While I am considering other than dwarfs here, I wish to mention a seedling of Tell Muhlstein which has bloomed here for two years. It was so beautiful this year that I wrote to Tell advising him to introduce it. It is #1-58, it is white with yellow beard, domed, flaring, wide petals, nicely branched, 18 inches high, but its outstanding quality is for its very beautiful ruffling. It is gorgeous in shape and very appealing. Of course it will probably go into the Intermediate Class because of its height but in character it could very nicely go into the Lilliput class.

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EXPERIMENTAL PROGRAM

It is not an easy task to conduct an experimental program. It has been attempted more than once by the AIS and each time it has failed to receive the expected cooperation of members and breeders and finally failed. We of the DIS have had some better luck with our program but even yet the findings from such work have come mostly from the experiments conducted here at the Misdlebury Test Garden.

However, we are now encouraged by the way our Experimental Chairman, Alexia Gerberg is handling things and we hope for improved cooperation of some of our breeders in the future.

At this time I am handicapped by an unfortunate and unforseen occurrance, the failure of expected bloom in some of the progenies which were scheduled for tests this past spring. Thus the small amount of blooms made it impractical for obtaining ratios of segregation. However enough did bloom for a tentative diagnosis at this time and this can be further checked next spring when sufficient bloom is available for more accurate counts.

In a separate article on plicatas in this issue of the Portfolio I have properly handled the reporting of my plicata experiments, we will not repeat these here. However there are a few other things which can be outlined here.

One of the most important of our studies was concerning the inhibitor for standards only. As we know inhibitors of the past they suppressed the anthocyanin color of the whole flower. Then Paul Cook found an inhibitor in some hybrids of tall crossed with a plant which he states was presumed to be a form of I. reichenbachii. This produced his Progenitor line, with which probably you are all familiar. I have discussed this rather fully in my article on inhibitors in this issue.

But to state it briefly here I had two rather large progenies of purple diploids by I.bosniaca, in which all were yellows, with no anthocyanin colors apparent. Strangely some of these yellow flowers showed some blue in the beards, which I am unable to account for at this time. But the important consideration was that obviously bosniaca has an inhibitor for the whole flower. As yet I have been unable to get reichenbachii to bloom here so as bosniaca is recognized as a form of I. reichenbachii, we cannot say for certain if all forms of reichenbachii carry this inhibitor. Paul Cook also found this same condition to exist in some of his seedlings involving I. bosniaca. Hence we have come to the idea that possibly this inhibitor for standards only is originating from some other source.

With this in view I made crosses of a diploid blue with I. mellita. Only a few of these bloomed this spring but all were with white or near-white standards and blue falls.

In a cross of Mme Chereau x rubromarginata I found a similar result with several showing the bicolor or amoena pattern quite similar to Progenitor.

I might add that in another cross of Mme. Chereau x I. bosniaca I got all yellows or creamy-white selfs, some showing blue beards. So from this evidence I think we may assume that at least this form of reichenbachii has an inhibitor for the whole flower and I. mellita is the source of this inhibitor for standards only.

Since our previous experience of crosses of purples and blues with Hanselmayer, in which it gave suggestions that purple and blue might be separate and individual color factors, we have made some crosses with whites with an idea of confirming this idea. As yet we haven't sufficient evidence to make any conclusions with finality, yet the evidence continues to pile up. In a cross of Cretica x White Mite I had 13 purples, I variegata, 4 yellows, among the few that did bloom. There were no violets, blues or whites showing.

Wee Blue x White Mite had four plants to bloom, 3 blues, I cream. No purples.

J-502 a violet pumila x White Mite gave 8 violets, 6 yellows, 3 whites.

While we wait for further evidence the above will indicate that no contrary data to our previous assumptions were found.

Another matter which has given us some reasons for thought is that I know of no whites coming from strictly yellow breeding in the talls, and I was concerned whether it was possible to get pure whites from yellows. I had grown around 100 seedlings of L. variegata x a different clone of this species and got only one plant which was a creamy-white from the lot. But this spring from a cross of Kinglet x Gold Imperial I counted 13 yellows, I cream, 2 near amoenas with a blue flush on falls and 3 whites. So that satisfies my questions about obtaining whites from yellows.

Another progeny that was quite interesting came from crossing I. bosniaca x arenaria. All were nice dainty plants about 5 inches high, showing the narrow leaves from arenaria, and all were yellows with blue beards. As I mentioned above the fact of getting some blue beards in a cross of diploid TB purples x bosniaca, and again blue beards showing in these arenaria hybrids, makes one wonder how bosniaca accomplishes this characteristic. Both parents in both cases had yellow beards, and particularly in the arenaria cross no anthocyanin is present in either parent. My notes on the bosniaca x arenaria cross state, 7 plants, all greenish yellow, open standards, down-hanging falls, blue beards, 2 buds in spathes, narrow grassy leaves, spathes keeled, 5 inches high and plants sterile.

In a cross of J-537 white chamaeiris x (rubromarginata x mellita) there were 7 plants, all whites with a somewhat greenish cast, white beard, 2 buds in spathes, spathes keeled, narrow hafts, falls tucked, reduced sickle leaves, and apparently sterile. The question arises, what became of the yellow of mellita? I would assume that this is a matter of dilution effect of reduced dosage of mellita yellow. The greenish cast of these whites would suggest some presence of yellow.

Another project which claims some interest here is concerning this mosaic virus. I checked rows of plants for any evidence of it and found the following, though in close proximity to infected plants for several years, show no evidence of infection. Fashion Lady, Gay Lassie, Bright Spring, Bright White, Path of Gold, Sonny, Prairie Gem, Icy Glow, Jean Siret, Crispy, and Lilli-Bright I would be please to have reports of these from other members, so we can hope to obtain a list of varieties immune to this virus.

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LIST OF VARIETIES IN MIDDLEBURY TEST GARDEN

Angel Eyes
Atomic Blue
Ablaze
April Morn
Alinda
Aprilschnee
Albatre
Aramis
Aurea Maculata
Alpin
Azurea
Amber Queen
Aqua Green
Atroviolacea
April Mist
Alba
Blue Whiskers
Black Top
Birght White
Bright Spring
Bricky
Bright Spot
Blue Spot
Butch
Blazon
Black Baby
Blue Frost
Brownette
Barium Gold
Butterball
Blue Lilt

Blue Band
Bees Wings
Black Midget
Blue Doll
Buster Brown
Blue Flash
Blue Mascot
Bronya
Balkana
Betsy Presby
Black Maroon
Bicolor
Burchfield
Buzzer
Burgos
Black Bird
Bouquet
Bride
Blue Jade
Burgundy
Berud
Blarney

Cream & Sugar
Cream Tart
Cradle Blue
Chicken Little
Coerulea
Chica
Curiosity
Contentment
Cyanea
Cuprea
Compacta
Candida
Cretica
Carpathia

Dream Child
Dirty Face
Ducky Lucky
Darmstadt
Die Braut
Dr. Potter
Dainty Miss
Drop O"Lemon
Enamel Blue
Ebony Petite
Easter Holiday
Endymion
Elegance
Elf Queen

Fortissimo
Flashlight
Fashion Lady
Flaxen
Fior del Mondo
Floribunda
Fragrance
Fiancee
Fairy
Florida
Formosa

Greenie
Glow Gleam
Gay Lassie
Green Petals
Gold Flush
Grandma's Hat
Goldhaube
Goldvoget
Goldwerg

Holly
Heart's Content
Hullabalu
Hanselmayer
Heatherbloom
Harbor Lights
Huron Imp

Inchalong
Inky
Ink Spot
Icy Glow
Inge Jordan

Je's Fine
Jean Siret
Judy
J.A. Slote
Joslyn

Knick Knack
King of the Rocks
Keepsake
King's Crown
Lavender Dawn
Little Joe
Little Balkan
Little Charmer
Little Elsa
Little Villain
Little Skipper
Lt. Chavagnac
Leopold
Little Jewel
Libra
La Tulipe
Lutea
Lobelia
Lido
Leander
My Daddy
Mumbo
Miss Matty
Meine Mutter
Moon Gleam
Mauve Mist
Mist O'Pink
Marocain
Moppett
Macrocarpa
Miss H.M. White
Max
Maienreigen

Crispy
Cherry Spot
Cup & Saucer
Chris

Morning Fresh

Nancy Marie
Niobe
Negus
Nugget
Neola
Naomi
Nudicaulis

Olympic Blue
One Fine Day
Olive Eva
Orange Queen
Owaissa
Orango
Orchard Flair
Perky
Pastel Dawn
Primus
Plum Glory
Pannonia
Promise
Prairie Gem
Princess Louise
Papoose
Petite Amie
Petite
Purple Beauty
Pumar Alpha
Pumar Beta
Puck
Pam
Pink Mauve
Patriot
Path of Gold

Red Gem
Red Amethyst
Roxy Carpet
Rose Petite
Rupert
Reichenbachii Yellow
Rose Mist
Reflection

Spring Joy

Green Wings
Graminea
Golden Dream
Grunange
Garnette

Shawn
Sound Money
Sass Dk Ruby
Sonny
Silver Elf
Snow Fairy
Schneeflocke
Statellae
Schneekuppe
Serpolette
Sea Gull
Sweeseri
Sulina
Sambo
Turquoise Gem
Terry Ellen
Tear Drops
Tampy
Tony
Tiny Tony
Tiny Treasure
Tu Tone
Trinket
Titania
Tigerkind

Violet Night
Veri-Gay
Violet Gem
Violet Prince
Viola
Verdun
Villereal
Vindobona
Vandee

White Mite
Wee Blue
Whitone
White Elf
Wee Admiral
Wee Bit
Weserperle
Wee Turque
Wendy
Wee Scot

Mignonette
Mistral
Mandarin
Mirielle
Margaret
Marie Martin

Median Varieties

First call
Paper & Ink
Lemon Flare
Limette
Eisdom
Brownie
Little Rosy WIngs
Small Wonder
Pagan Midget
Helen's Child
Brown Bantam
Pink Elf
Dale Dennis
Laddie Boy
Mogador
Burgundy
Sure Fire
La Perle
Excelsa
Gracilis
Thisbe
Ditton's Purple
Dixmude
Benacensis
Sapphire Night
Lilli-Purple
Lilli-Bitone
Lilli-Blue
Lilli-Bright
Lilli-Var
Lilli-Richtone
Lilli-Green
Lilli-Yellow
Lilli-White
Lilli-Flora
Red-Lilli

Sparkling Eyes
Stylish
Sharp Contrast
Sea O'Blue
Starfrost
Snow Baby
Sun Drop
Schwefelgeiser
Socrates
Yellow Frills
Yellow Bird
Yil

This list is presented for the purpose of indicating the availability of any perhaps old or scarce variety which some collector might wish to locate, and also to indicate the extensive list of all varieties which are on display here at the Test Garden at Middlebury.

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SOMETHING TO THINK ABOUT

Most everyone is familiar with our disagreements and contentions regarding this matter of AIS classification, and particularly the title of "Standard Dwarf Iris" as applied to the Section comprising forms of 10 to 15 inches inclusive.

The argument posed in favor of this Section was that it would represent the few Standard Dwarfs which were supposed to grow to this height. We have contended that the term "Standard Dwarfs" should apply to this so called chamaeiris group of iris which are traditionally known and accepted as dwarf iris and which are represented by such common varieties as Sound Money, Rose Mist, Tony, Busser, Sonny, Endymion, and the numerous varieties which have been established and recognized as such down through the past.

We had protested the use of this name for the various new hybrids and miscellaneous iris, on the grounds that they did not represent typical dwarfs and it would make a dumping ground for a motley group of forms.

This summer I received a list of "Median Irises eligible for Awards 1961 ", and under the heading of "Standard Dwarf Bearded" I find the following list of varieties:

Baria
Brite
Fairy Flax
Lilli-White
Little Cottage
Pogo
Blue Denim
Dale Dennis
Lemon Flare
Lilli-Yellow
Littel Dogie
Small Wonder
Brassie
Dancing Bee
Lilli-Green
Lillipinkput
Pagan Midget

Everyone in this list is what is now known as Lilliputs, and not one Standard Dwarf in the lot. I think that any fair minded person will agree that it is not in the best interests of anyone to classify these new type of intermediates under the name of "Standard Dwarfs." And I think that anyone will recognize that we, the Dwarf Iris Society have a just greviance with the AIS for appropriating this name and applying it to an entirely new kind of hybrid which certainly is not a Standard Dwarf by any means of interpretation.

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LIST OF DWARF IRIS SPECIES AT MIDDLEBURY

In addition to the list of named varieties grown here at the Middlebury Test Garden, I am listing here the number of forms of each species, all of which are collected froms sent here from their native habitats. In addition to these we have numerous forms of several species which are seedlings from selfing or intercrossing different forms of a species.

I. pumila	139	I. chamaeiris	7	
I. furcata	1	I. taurica	6	
I. attica	13	I. perrieri	2	
I. illyrica	1	I. kashmiriana	1	24chr.
I. pallida blue	3	I. subvillosa	1	
I. pallida white	1	I. biflora	1	
Macropode	2	I. variegata yel.	10	
I. reginae	1	I. variegata white	1	
I. barthii	3	I. mellita	8	
I. reichenbachii	6	I. aphylla	8	
I. bosniaca	3	I. olbiensis	2	
I. subbiflora	5	Macrocarpa	1	
I. balkana	1	I. croatica	3	
I. pseudopumila	4	I. binata	2	
I. arenaria	2	I. imbricata	1	
I. bloudowii	1	I. cengialti	1	
I. flavissima	1	Reta ?	20	

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In recent letters from Mrs. Beulah Nothstein and from Ben Azer it is announced that the Green Thumb Flower and Garden Club of Lincoln, Michiganl, will hold a spring show for Dwarf Iris, which to our knowledge will be the first show of its kind to be held in this country. Garden displays and our Test Garden meetings of course are in fact outdoor shows or at least serve the purpose for competition and voting by judges, but to date we have not yet had an indoor or "bench show" devoted stirctly to dwarf iris.
We wish to congratulate this group for this pioneering venture and wish them the best of luck.

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NORTHWEST DIS TEST GARDEN REPORT

Leona Mahood

It did seem that this, the tenth anniversary of the Northwest Test Garden should be something special. And it was. Specially cold, windy and disagreable on the meeting day. A few days before snow and hail had powdered the groind white. The dwarfs in full flower were beautiful against the snow, but it melted quickly away leaving the flowers in rags and tatters, and continued cold caused most buds to remain closed. Such trivialities could never discourage a true dwarf iris gardener, and the meeting brought out the biggest attendance ever. It was a pleasure to have Mrs. Don Sturdevant wife of the editor of AIS Region 13 bulletin, and Mr. Robert Schriener with us.

The morning passed quickly as everyone studied the flowers between dashes into the house to warm themselves and tap the coffee pot. Only the cameras were idle on that cold, gray day. Shutterbugs had had their day the previous Sunday, when the garden was studded with figures in strange postures of obeisance before these lovely flowers.

A brief program in the afternoon was devoted to discussions of judging dwarfs, and the colot Symposium, monitored by Bennett Jones and Ron Beattie. Ballots were counted and it was found that Bennett's 90-7 had won the TG Popularity Prize. This fine dwarf, an immaculate white with precise violet dot has been named Polka Dot.

The evening program was highlighted by a showing of the Karl Ajdovic slides of I.Pumila and I arenaria growing in their native homes on the hills of Austria. How we wished that we could be there to see all those many variations nestled in sparse grass on rocky outcroppings. Anyone showing these slides could profit by first reviewing Mr. Ajdovic's article "Austrian Dwarf Bearded Iris Species". See Portfolio number 11 page 47.

With the advancing season other iris took over and it seemed that the story of the Test Garden season was ended. It had only come to the end of the first installment. A second installment filled with exciting surprises was soon to unfold.

It started when shipping time came bringing, almost daily, new boxes of dwarfs. Dwarf iris here, dwarf iris there, here a dwarf, there a dwarf, everywhere a dwarf. What excitement! What fun! What work!

A dozen and more new or recent varieties several of which we had heard of only in the list of registrations. Now their generous owners were sharing them with us. ATOMIC BLUE, LOVELY JEWEL, GREENIE, GREEN ISLAND, AQUA GREEN, EBONY ELF, MODULATION, FANNIE R. DROP O'LEMON, DROP O'BLUE, FROST IMP, POLKA DOT, FAIRY DELL, PLAYTIME. Enough to make one wish away the long months of waiting for spring.

A whole new section of guest seedlings, 57 of 'em. A record smashing lot. Going along the rows we find names of breeders never represented before by their seedlings here: Grace Carlson, Earl Roberts, Lloyd Zurbrigg, Robert Nourse, and Richard Rosenfels. Here too, are new seedlings from Orpha Salsman, Walter Welch, Lucille Kavan, Alta Brown, Helen Tutmark and Ron Beattie. Thank you all, we hope everyone will have some winners.

David Reath sent a large collection of species pumilas, while David Saunders, Walter, Lucille and Alexia Gerberg sent still other pumilas and species. Rudolf Hanselmayer has been a most generous assistant over the years, sending numerous species in an exchange program carried on by Roy Davidson for the mutual benefit of all concerned. The thirty-five plants that Rudolf sent this year arrived in excellent condition after their long journey and

In all near 200 dwarfs were received. Many thanks to all you generous contributors. We hope that next year you will be out for the Seattle World Fair and can find time to visit the test garden.

Only a few years ago that many dwarfs would have been a large collection. Now it became ncecssary to replant and rearrange the entire Test Garden in order to make room and get all in their proper places. A project which took most of the summer. As the planting progressed varieties of like color were grouped together, black with black. variegata with variegata, etc. Surprising what a number of greens are appearing. Greenie, Green Island, Aqua Green, Dirty Face, Green Eyes should form an interesting group.

Nearby a pint sized iris from Hanselmayer proves that nature sometimes produces green iris too. A real green flower with narrow rather twisted falls which support a sure nough blue beard, big, broad and so fuzzy it covers almost a third of the fall.

October days are here. A last dwarf iris (I. balkana x Orange Glint) is lending a grace note with its first bright, brownish-orange flower, as the dwarf iris season has at last come to.

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AN APPRECIATION

As the days passed it became a morning ritual to walk along the iris patch and watch the little fat buds begin to push up on the dwarf irises. Then came the morning the first Atroviolacea bud showed a bit of color. At roviolacea as always is first. One by one more and more tips began showing, and then at last it happened, that first blossom greated me in the early morning. It was March 13th. My little plants are literally crowded with buds. It seems that even the tiniest rhizome is outdoing itself. I find it hard to put into words, but I believe there is not thing that gives quite the same measure of real satisfaction as the thrill of the first bloom on the dwarf iris.

The last few weeks I have been following a time worn ritual of reading through the DIS Portfolios, and that is quite appropriate as preparation for the coming of bloom season. It is amazing how with each rereading you find you learn something new, somtthing you may have missed in earlier readings. It is equally amazing to note the progress in new thinking brought out by long years of research and programs of experimentation.

from a letter by -- Bob Nourse.

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Another step in progress might be mentioned; that a sizable collection of dwarfs has been sent to Mrs. Flaminia G. Specht, of Florence, Italy, which will be used as a display at their Botanical Gardens and also with a view of including them in their International Flower Shows.

In addition to this the firm of Jean Cayeux of France has purchased a large collection of modern dwarfs, for commercial purposes and thus will promote further interest for dwarfs in that county. Gradually the dwarf iris interest is spreading throughout the world. When the big dealers start promoting dwarf interest, it means the dwarfs have achieved favorable recognition.

In England the Orpington Nurseries and Astolat's Nurseries feature dwarf iris. In New Zealand Jean Stevens offers the newest American varieties, and in Australia their largest dealer Royce Spinkston will be furnishings the best in modern dwarfs. Literally the dwarfs are "going places".

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SOMETHING ABOUT OUR DIS ROBINS

The purpose of joining any flower society is obviously to receive some benifits from the society, of which may be named the association with others with mutual interests. The priviledge of attending meetings, having access to the various services offered by the organization, but actually I would say that most members find that their chief benifit comes from receiving the societies bulletins I am sure that such members fail to use many of the facilities and benifits that are due them because they do not take advantage of what is offered. And for this reason many members eventually frop their membership because they feel they are not receiving enough to justify the dues.

For instance let us consider just one small item, which is a service offered by the Dwarf Iris Society to its members. That is our DIS Robins. At the present time I am directing 26 dwarf iris Robins.

What is a Robin? I am sure that most of you are acquainted with Robins but let me explain. A Robin is a group of ten members in a correspondence association. When you join a Robin you will receive a batch of letters in one envelope, one from each member of the group. You put in your own letter and mail all to the next person on the Route List immediately after your name. When the letters have reached all members and returned to you, take out your old letter and put in a new one and mail them on again. This goes on indefinitely or as long as you wish to remain a member.

In the Robin you will make new friends, you will discuss your problems with other members, and you will learn more and quicker than you can by any other method. Most of the experts in the DIS received most of their knowledge through the discussions in these Robins and I may say that practically all of our DIS judges received much of their training through this medium.

The main advantage of a robin is that you are able to ask questions which particularly meets your needs. Ordinarily you can sit by and wait in the hopes that the bulletins will eventually discuss these matters but here you don't waint, you get the answers at once, and besides you personally participate in the discussions. It becomes more of a personal matter.

If you have an idea that a Robin is just a means of exchange of gossip and visiting you are wrong. Naturally some personal exchange of personal matters will occur because these are your new friends, but we try to keep to the subject as much as possible. Also you may think you being a beginner cannot keep up with the more advanced members. We specialize in beginners. But let me say that Robins are for the experts as much as the beginners. You would be surprised at some of the notables who are members of our Robins. Just to name a few I mention, H. Senior Fothergill, Gerald Darby and H. Castle Fletcher of England. Jean Stevens of New Zealand. Rudolf Hanselmayer, Dr. Peter Werckmeister, Dr. Hertha Van Nes, Anita Stross of Italy, May Gould of Australia, and in this country people like Earl Roberts, Wilma Greenlee, Dr. Dave Reath, Leona Mahood, Frances Ellie, Helen Doriot, Alta Brown, Alexia Gerberg, Bonnie Dunbar, in fact most of the leading dwarf iris notables in America.

The truth is that with the robins you are getting the equivalent of several extra bulletins every year, and these at no extra cost.

INHIBITOR FACTORS

Most persons at all familiar with the so called inhibitor action in iris will immediately refer to what is known as the dominant whites in the tetraploid tall bearded iris. Beyond this they know very lettle on the subject. In fact not too much is even known about the nature or inheritance of inhibitors in the talls, for I find some conflicting statement among even the experts.

For example, I find statements such as the following: "The inhibitor of anthocyanin flower color in iris frequently acts as an incomplete dominant. " And certain varieties such as the blue-tinted Snow Flurry is cited as an example of incomplete dominance.

In my experiments here I have found that one dose of inhibitor is sufficient to completely erase the color of a blue parent, except in cases where other than TB anthocyanin pigments are involved. We know that the inhibitor in the TB dominant whites affects only the TB anthocyanin, and not that of such other species as I. aphylla, I. pumila, and certain other dwarf species. We also know that in the past some sphylla derivatives were used extensively in breeding with talls, and thus a different kind of anthocyanin may exist in many of the TB varieties of today. My views on this subject are that the apparent blue effect remaining in these particular dominant whites results from a weak dosage of this different kind of anthocyanin which remains and which is not affected by the inhibitor gene.

As yet no one has fully determined the orgin or nature of this inhibitor gene, and some authorities claim that it derived from the dwarfs. From the records we can trace it back to some of the early tetraploid whites, and presumably may have come through the intermediates up to the talls. This inhibitor is not known in the diploid talls.

However as it is not naturally inherent within the tall bearded species, we conclude that it must have come from some form of the dwarf bearded iris, through hybridization.

Before we were familiar with I. pumila in this country, which is not so far back in years, we had recognized that I. chamaeiris contained this inhibitor. Experiences of our early hybridizers had shown that seldom in the intermediates did the color of the talls show, and when we finally learned that I. chamaeiris was of a hybrid constitution, involving two wets of 8 chromosomes, and two sets of of 12 chromosomes we then realized that probably this inhibitor originated in I. pumila, I. attica, or I. pseudopumila, all of which have a basic number of 8 chromosomes.

We must give Paul Cook credit for discovering and analyzing the inhibitor action of this gene in I. pumila. In a letter from Paul dated as far back as Feb. 1, 1955 he writes as follows:

"It has seemed to me, as a result of my various crosses involving pumila, that all color forms of this iris carry the inhibitor gene that suppresses anthocyanin of the talls. It has also been my observation that none of the anthocyanin colors found in pumila--blue, purple, violet----- has been affected in the slightest degree by this inhibitor gene. As you know of course, my views have come mostly from a rather intensive sutdy of the two progenies from blue tall 10942 x yellow pumila 343, and blue tall 10942 x blue pumila. The fact that the same blue tall was used in both crosses has been a very great help in working out a consistent explanation of the results.

pumila 343 on Fairy Flax, one of the blues that came from 10942 x blue pumila, and in 1953 and 1954 I flowered 107 seedlings from this cross. Inasmuch as the cross was made in the course of practical breeding and was not intended as an experimental cross, I made no actual, individual color counts of the seedlings then in flower. However, I remember that the row showed about equal numbers of blues and non-blues. This was the expectation of course when the cross was made, inasmuch as Fairy Flax, derived on one side from a pumila that carried one dose of pumila blue, would likewise carry a single dose of pumila blue.

I think this cross of Fairy Flaz x yellow pumila 343, together with the two crosses ob blue tall x pumila, demonstrates almost past question that (1) the pumila which I have been using carries an inhibitor gene that suppresses the anthocyanin of tall, and (2) this inhibitor gene has no like effect on anthocyanin of pumila itself. And I might add also (3) pumila blue is dominant (or epistatic) to pumila yellow.

The 1:1 ratio of blues to non-blues (actually 19 blues, 24 non-blues) obtained from the cross of 10942 x blue pumila is the result expected from a backcross of a dominant single-dose heterozygote to the recessive. In the present case, the blue tall, with its anthocyanin suppressed by the action of the inhibitor gene in blue pumila, served as a recessive, permitting the blue color of pumila to appear."

There you have the evidence from the master, himself. At first there was some doubt among some authorities that an inhibitor would work on one kind of anthocyanin and not on another kind. In fact it was questioned that pumila could have a different kind of anthocyanin from the talls. We now have accepted Paul's theory fully and apparently no doubts remain on this matter.

One of our goals still evading us at this time, is to find a form of I. pumila which lacks altogether this inhibitor gene. That it can be achieved is agreed because in some cases pumila may have less than the full four doses of inhibitor gene. It is just a matter of further work and trial and selection.

However we are fairly well satisfied that this inhibitor is absent in the form from Crete, called Cretica, because from a limited number of crosses with talls it appears that no inhibitor action is present.

And in a number of crosses observed, it is apparent that the species I. attica and I. pseudopumila also carry this same inhibitor gene.

Shortly after this pumila inhibitor matter had been settled to our satisfaction, a new and different kind of inhibitor action again appeared in the garden of Paul Cook. This inhibitor was to be the means of originating an entirely new kind of inheritance and establishing a new kind of pattern in iris. I refer to Paul's Progenitor line of seedlings and varieties.

To again let Paul explain this matter I quote from a letter of Feb. 13, 1955, as follows:

"There is a gene that determines blue color for the entire flower, and there is this inhibitor that acts to keep the blue color from appearing in the standards--just the two genes, and no allelism, no linkage. I have had some beautiful ratios in progenies involving this amoena pattern.

Progenitor itself is a first hybrid from a yellow dwarf presumed to be a form of Reichenbachii (which I lost) x shining Waters. It has yellow standards and light blue falls. In 1948 I flowered a long row of seedlings from blue tall 10942 x Progenitor, a backcross progeny to blue tall. 86 seedlings flowered--- 72 amoenas, 14 blue selfs-- see how neatly this falls into a 5:1 ratio--note too that it is the blue selfs that are the recessives.

"I also had in 1948 a small progeny from Progenitor x Shining Waters out of which I saved (and still have) two seedlings--- 6648 and 6748. In 1951 I flowered a progeny of 109 seedlings from Distance x 6648, getting this time 58 amoenas, 51 blue selfs, a 1:1 ratio surely. Presumably the yellow dwarf carried four, or at least three doses of the inhibitor gene, Progenitor two doses, and 6648 one dose --- the results can be accounted for in this way.

"You just can't ingnore ratios like these: your speculation has to start with these facts. And it means that white in the standards (or the absence of blue to be more exact) is dominant, not recessive. It means that there is a specific gene in the yellow Reichenbachii which I used in the cross with Shining Waters that acts to suppress the anthocyanin in the standards of blue talls, though not in the falls." (End of quote)

I want you to note in the above that Paul states the Progenitor came from "a yellow dwarf presumed to be a form of Reichenbachii." I make this notice because it has some connection with some further experiments which I will describe later in connection with this inheritance.

I remember Paul once showed me a plant at his garden which he stated resembled the "Reichenbachii" parent of Progenitor. As I remember it now, it had a resemblence to certain mellita characteristics so much that I wondered it might be a hybrid involving mellita and possibley reichenbackii. Paul stated that he received this plant from seed obtained from Pearce Seed Co. labelled as reichenbachii.

Being curious about the inheritance of the reichenbachiis, I was unable to flower my plants of I. reichenbachii at this time, But I did get takes of Moment Musical and Nocturne by I. bosniaca, pollen. These varieties are diploid varieties, one a red, the other a reddish blenc. I. bosniaca is considered a yellow form of I. reichenbachii and thus I considered it would serve my purposes.

In 1960 I flowered about 40 seedlings from these crosses. All showed as yellow selfs in various depth of color but none of the anthocyanin color of the TB parent in evidence in any of them. This appeared to be ample evidence of inhibitor action for the whole flower. Later I talked with Paul and he found the same results in a similar cross. Thus the question arose whether this would hold true with other froms of reichenbachii, of which we have not determined conclusively to date. This action tended to confuse our previous deductions.

However, from several crosses of talls x L. balkana, I found that I. balkana was producing some bicolors quite similar to the results obtained by Paul in his so called Reichenbachii hybrids. This result was later confirmed from reports of Wilma Greenlee. So this started us on a new speculation.

I.balkana according to Dykes and some others, has been considered as the purple form of L.reichenbachii. However I. balkana is a tetraploid with 48 chromosomes while the ordinary yellow reichenbachii we know has 24 chromosomes. According to the harypotype studies of Mitra, it was found and stated that balkana is a hybrid form, with sets of chromosomes identified or resembling those of L. mellita and I. reichenbachii, and hence it was considered to be a hybrid of these two species. Possibly the mellita chromosomes would account for this inhibitor action for standards only.

With a view of learning whether mellita did actually have this inhibitor, I crossed a blue diploid with yellow mellita. In 1961 only a few of these seedlings flowered, but every one of them had an oyster-white standards and lavender-blue falls. This indicated that the blue of the standards had been suppressed by the inhibitor.

In a cross of Mme. Chereau x Rubromarginata most showed as some tone of light purple, with a few having creamy-white standards and purplish falls. These bicolors were sib crossed, and from these I found 5 purple selfs, 5 bicolors or what Paul calls amoenas, I blue plicata, I yellow plicata, and I yellow bitone. Naturally the purple of rubromarginata is not affected by this inhibitor, but the bicolors certainly were the result of inhibitor action. Because of the poor germination of these seed, and the small number of seedlings, any attempt to determine ratios was impractical. But from the above progenies we can be assured that this inhibitor for standards only originates in I. mellita, and not in I. reichenbachii, as was first thought.

One member when discussing these crosses, asked what became of the yellow of mellita, which should have shown in the standards. The yellow of mellita is not a clean yellow, nor is it a strong yellow. Any outcross would result in further dilution of the yellow to the point where it appears as a creamy or grayed white. I had a progeny from a white chamaeiris x yellow mellita in which all plants were a warm or grayish white.

So to make a summation of the inhibitor situation, we can say that it is quite evident that the inhibitor of the TB dominant whites derived from some dwarf iris ancestor, either from pumila or possibly from some reichenbachii form. Further that there are at least three different inhibitors known to exist in iris, all of them originating in the dwarfs. These are the inhibitor for the entire flower, existing in I. pumila, I. attica and I. pseudopumila. Another inhibiting the whole flower is found in I. bosniaca and probably I. reichenbachii. And the third is the inhibitor for standards only, found in I. mellita.

We must also note that these inhibitors affect only the anthocyanins found in tall bearded iris. They do not affect the anthocyanins of any of the dwarfs, or of I. aphylla, and from several reports it is apparent that they do not affect the anthocyanins of the oncos and regelia forms.

For our practical purposes, our breeding operations, the above knowledge is sufficient for our purposes. But the curious mind will continue to search for the nature of this inhibitor gene and its manner of functioning. To date we have no definite answer to this problem; all we know is the results of its action.

However in our quest for an answer we have had some suggestions which are considered worthy of thought. Dr. Werckmeister has found that the TB dominant whites showed the presence of some pseudobase pigment, wheras the recessive whites indicated an absence of this materila.

We also learn from other sources that anthocyanin pigment is produced in two ways, namely, by the oxidation of the pseudobase, and also by reduction of flavone pigment. Out tests have shown that the anthocyanins of tall bearded iris are of a different nature than those of pumila and other dwarfs; at least the inhibitor action would indicate such a conclusion.

Therefore we have recognized the possibility that the inhibitor gene just may function to stop the sequence involved in the oxidation of this pseudobase, and hence the cycle is not completed. This also suggest that the anthocyanins of pumila may derive from the reduction of flavones, and thus be unaffected by the inhibitor. At present we have no way of knowing for certain about this, but it may offer a lead for further study by the pigment chemists.

To date we know of no inhibitor affecting the carotene or yellow plastid pigments. All of the presently known inhibitors are associated with anthocyanin pigments (blue, purple, red, violet).

After becoming acquainted with these different inhibitors, the next question will be, how to use this factor in our breeding operations. First we recognize that it is a dominant factor,and will segregate out in normal Mendelian ratios.

We know that the TB dominant whites result from this inhibitor. Many of our yellows are yellow because the inhibitor has erased whatever anthocyanins might have been present. We can assume that the patterns with white spot on falls, such as Leading Lady is a case of inhibitor suppressing the spot on falls, leaving the white overlay in the epidermis layer of cells, which is typical and derived from the species I. variegata. We can use this inhibitor for standards only to reproduce in the dwarfs all of the fine things which Paul Cook has developed in his Progenitor line in the talls. We know that most of the TB tangerine pinks of today have this inhibitor gene in their composition.

Some of the disadvantages of these inhibitors are that in crosses involving talls with dwarfs, we have difficulty in bringing down to the dwarfs the TB colors. With mellita we can overcome this difficulty through ordinary segregation, in advanced generation through loss of this inhibitor, because the chromosomes of talls and mellita are homologous. But in pumila and the other dwarfs, we can only hope to find a plant which lacks this inhibitor or which is in a heterozygous condition for this gene. We hope to find such a form of pumila in the near future.

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THE FERTILITY SITUATION

Although fertility is a consideration of some consequence in tall bearded iris breeding it is not of the magnitude or serious consequence that the breeder finds in working with the dwarfs. I find that some of our members complain year after year of their failure to get "takes" from their planned crosses and thus get the impression that the dwarfs are a difficult class of iris with which to work. With a view to clarifying this matter I wish to present some of the problems involved and perhaps suggest means of improving the results.

First we should understand the meaning of the term fertility and sterility. In practice for the breeder it can be reduced to the obtaining of viable seed or the failure to produce such seed, but the situation is not as clear-cut as it might appear. Seldom do we ever experience absolute sterility. Probably at roviolacea is an example of what we might call sterility, yet it has been known to give a seed or two on rare occasions.

Fertility on the other hand assumes full pods of seed. And the gradations range in between these two extremes. We had planned on publishing a compilation of the degrees of fertility of the various species and varieties some time back but unfortunately this failed to materialize, to date at least. But in the absence of such data I hope to offer some suggestions which should serve to enable the breeder to determine when and why certain crosses will produce good seed.

Among the many things which are involved as the reason for so-called infertility the matter of environmental conditions and genetical considerations are the most important. Under the environmental conditions may be considered the climate, season, weather, moisture, heat, etc. This is something we all experience in different degrees and is probably accountable for much of our failure to get "takes."

I do know that I have experienced several days of pollinations where hardly any success was obtained, then perhaps the next several days almost everything produced seed, all with the same plants and same methods. This was undoubtedly an environmental condition.

Much reliance is given to the condition of the pollen by the experienced breeder. It is advisable to save pollen while fresh and store in a cool and dry place for future use. The stigmatic surface should be fresh and "sticky" rather than dried by hot weather or damp from wet weather; there is no doubt that humidity is a contributing factor in our success or failure.

However strictly speaking such conditons are not related to true fertility or sterility, whereas genetical conditions have a direct bearing on fertility. The production of gametes (pollen and Ovules) is dependent upon the normal and regular functioning of meiosis, and any irregularities which disturb this process have an effect upon the results.

Without going into the intricate procedure of events culminating in the production of gametes, I will merely state tht it is a system of sequences involving pairing of chromosomes, cell divisions and finally production of gametes containing half of the contents of the original whole cell. It is important to remember that such germ cells called gametes are in fact "half-cells" which when united in fertillization with the half cell from the other parent, becomes a whole cell again and we know this as the seed.

A term we will use often here is "homologous", meaning corresponding in type of structure and kind, that is related to the degree that one chromosome will pair with another. To explain this further, a diploid such as attica or mellita has two sets of chromosomes, that is each chromosome is duplicated, therefore each chromosome has a partner with which it will paid at one stage in meiosis. After pairing the two chromosomes separate and each retire to opposite ends of the cell, the cell divides and thus each half cell receives one of these chromosomes. Therefore as you can understand a balanced complement of chromosomes is necessary for the normal functioning of gamete formation.

With this brief explanation of the functioning of pollen and ovule formation, we can now proceed to some examples as appled to our practical breeding. As is quite obvious to us, a diploid with two sets of homologous chromosomes is the ideal condition for normal functioning of meiosis, because it has a balanced complement of elements. Therefore we can expect the best of results in working with such a plant. We would expect atticas to cross readily with other atticas or mellita with other mellitas and produce plenty of good, viable seed.

But as soon as we cross outside of a given species type we get into trouble. A cross of I. attica x I. mellita produces a diploid also but a different kind of diploid. The seedling from this will contain a set of attica and a set of mellita chromosomes. That is a set of 8 chromosomes and a set of 12 chromosomes respectively. Not only is there a difference in numbers of chromosomes in each set but they are of a different kind. And as different kinds will not pair, we say they are non-homologous. With this kind of imbalance the process of meiosis is disrupted, and hence no gametes are formed; the result sterility.

At this point we might make a rule, that intercrossing between forms of the same species can be expected to give the optimum in fertility. But this does not necessarily mean that outcrossing to another species implies sterility. The degree of relationship between two species is the important consideration. We can accept the fact that because two species show different characteristics they are to some extent differentiated genetically. At the same time the nearer they are in relationship, the more genes they have in common, and such relationships result in a greater affinity for pairing.

Through experimentation we have found that all of the species having a bisic number of 12 chromosomes apparently are homologous and will pair together. In the same manner we have found all of the iris having a basic number of 8 chromosomes as homologous. Therefore we find that the tall bearded, the mellitas, reichenbachii, balkana, and the aphyllas are homologous, all having a basic number of 12 chromosomes. And likewise pumila, attica and pseudopumilas are homologous and all inter-ferfile.

Although we find no great difficulty in achieving an intial cross between most dwarf iris species, the resultant hybrids sometimes offer problems with fertility. For example we have many hybrids from crosses of I.chamaeiris x I. arenaria, and a few from I. pumila, I. bosniaca, and I. regelia by I. arenaria, which almost invariably are sterile, and as yet we have found no method of overcoming this problem. This is due not only to a differentiation of chromosome type but also to a difference in basic chromosome numbers and sometimes to a difference in ploidy. Any thing which has an unbalanced composition of chromosomes tends to be infertile in proportion to the amount of imbalance, and working with differentiated species is certain to produce many kinds of hybrids with varying amounts of unbalanced compositions.

For a long time we accepted this fact as something to be expected and found varying degrees of infertility for which we had noexplanation. I think that Paul Cook was the first to give us a workable theory which helped us to understand better what to expect from a cross as regards fertility. To quote as near as I can remember it is as follows: When the bivalents exceed the univalents we can expect fairly good fertility; when they are equal the fertility will be decreased to rather poor; but when the univalents exceed the bivalents infertility to sterility is the rule".

Now how does this work out in practive; let us cite an example. I have a hybrid from (tall bearded x pumila) x mellita which gives full pods of seed. This hybrid contains one set each of TB, pumila and mellita chromosomes, that is 12,8, and 12 respectively. This is a triploid and hence is unbalanced in respect to ploidy. In addition with three different species the matter of differentiation of kind is a consideration. But as we know that the set of T B chromosomes are homologous with those of mellita, in pairing they will produce 12 bivalents (paired chromosomes), leaving the 8 chromosomes of pumila as univalents (unpaired chromosomes). Thus we have the 12 bivalents exceeding the 8 univalents, resulting in fertility.

On the other hand a hybrid of pumila x mellita is almost sterile. This kind of hybrid will have two sets of pumila and one set of mellita chromosomes. The two sets of pumila will pair, forming 8 bivalents, leaving the 12 chromosomes of mellita as univalents. Here the univalents exceed the bivalents, result infertility.

In another progeny I crossed a tetraploid TB with mellita, this giving a triploid with three sets of 12 chromosomes. In such a cross we would expect the two sets of TB to pair, forming 12 bivalents, leaving the 12 mellita chromosomes as univalents. Here the bivalents equal the univalents and as expected the fertility was rather poor. But a different condition resulted from crossing Mme Chereau x mellita. Here each parent contributed one set of 12 chromosomes, forming a diploid hybrid with 24 chromosomes. Apparently these were forming 12 bivalents or approximately, as these hybrids were all quite fertile.

Next these hybrids were crossed with I. bosniaca another diploid with 24 chromosomes. This again produced a diploid with two sets of 12 chromosomes which apparently were pairing as all of these seedlings showed fairly good fertility.

I perhaps should make note of certain things which may lead to some confusion regarding the homology of chromosomes of different species. I have indicated above that we find all of the species having a basic number of 12 chromosomes are homologous with each other. I don't want to leave the impression that in all instances pairing is as normal in these diverse hybrids as we might expect amount iris with one kind of chromosomes, such as diploid talls for instance. Chromosomes tend to pair with their own kind and they show a selectivity depending on the relationship or affinity for each other. Where a chromosome has no choice of alternatives, such as in a cross of diploid TB x mellita, it will pair to form bivalents, wheras if two sets of TB chromosomes were available in the composition, they would pair together leaving the mellita chromosomes as univalents, except possibly for some trivalent association on some occasions.

In connection with the matter of triploids, we find them often infertile due to unbalanced complement; in fact iris having odd numbers of sets of chromosomes are ordinarily much more liable to infertitlity than iris with even numbers of sets such as diploids, tetraploids, etc.

It seems that regardless of how much we think we know or in spite of all of the rules, there are always some exceptions for which we find no answers. In such cases we must conclude that there are involved considerations of which we are not aware at this time. One of these cases is concerning the pumila hybrids. I refer to the ordinary hybrids derived from crossing I. chamaeiris x I. pumila, which would have three sets of 8 and one set of 12 chromosomes. We have in this category such things as Violet Gen, Alinda, Green Petals, which seem to be quite fertile, while some such as Atroviolacea, Azurea are almost sterile. We would expect all of them to be quite infertile due to the unbalanced condition, yet each different plant shows a different degree of fertility.

Another group of hybrids show an instability that is unpredictable and considering their particular makeup we should expect them to give good fertility at all times. I refer to the hybrids from crosses of Tall x pumila, then crossed with chamaeris. This is the famous "40 chromosome pot" as it is called sometimes.

Early in my dwarf career I tried crossing the tall-pumila hybrids with chamaeiris varieties and found them somewhat erratic; some showed good fertility while others gave poor fertility. Eut in few cases did I find the high fertility which we have learned to expect from the chamaeiris varieties. It is quite evident to me that regardless of the similarity which is suggested by the amphidiploid constitution and other genetical comparisons, there is considerable divergence from a common gene pool that would insure compatible relationships to the extent of achieving good fertility equal to either parent alone. Yet I would not want to discourage any one from using this kind of breeding for I have found it an excellent source of good breeding materials. Such varieties as Pastel Dawn, Bright White, Fashion Lady, Little Villain, and Lilli-White bear witness to this .

It is when you cross these back to pumila that fertility starts to drop off rather suddenly. Things like Hullabalu, Cherry Spot, Gay Lassie, Red Gem, will give a few seed from several pollinations, enough to carry on the breeding but I would say that in addition to the imbalance of chromosome sets there is also some differentiation between the chromosomes of TE and the 12 type chromosomes in the chamaeiris parent.

With this brief survey of different species combinations and hybrids and the reasons therefor in regards to fertility, I find no reason for condemning the dwarfs for reasons of sterility but I can state that with the help of some knowledge of the principles of genetics and inheritance one can avoid some mistakes and increase the chances of success with pollinations.

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AIS AWARDS FOR 1961

CAPARNE AWARD - Angel Eyes   Bennett Jones   76 votes

Runners-Up
Red Gem - Walter Welch -- 19
Ablaze - Walter Welch -- 18
Promise - Paul Cook ---- 18
Black Baby -H.Sass ----- 17

HONORABLE MENTION-
Bee's Wing -- Alta Brown ----- 3 votes
Black Top - Walter Welch ----- 10
Blue Whiskers -Wlater Welch -- 10
Chicken Little- Bennett Jones - 12
Claire --Alta Brown ----------- 14
Curtsy -- Ruth Stephenson------ 25
Enamel Blue - Wlater Welch ---- 8
Heart's Content-Walter Welch -- 10
Perky -Walter Welch ----------- 9

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Please pay your DIS dues directly to our secretary, Mrs. Lee Armiger, 24300 Edgemont Road, Southfield, Mich. Dues are continuing to come to me at Middlebury and it will save extra work to mail dues direct to Lee Armiger. Thanks.

SPECIES PROBLEMS

I might have titled this paper with the usual question of "What is a species" , but as I am not qualified to answer that question. It is hardly proper to suggest from the heading that it is my intention to answer it. With the title Species Problems I am merely making a statement which does not necessarily infer that I intend to attempt to furnish the answers.

It may be questionable whether it is proper that a person who is not a scientist, should write upon such a highly technical subject, for in fact it would seem that most everybody in this particular field of science has written upon the subject and therefore with such a wealth of material available it is hardly possible that I could add anything to the subject.

I will grant this as true, but I will add that few of our members will ever take the trouble of seaching out the often hard to find literature, and therefore it is practically unavailable for them. But if it is presented here in the simple language of the laymen, they will read it and thus gain knowledge which otherwise would remain beyond their understanding. Therefore I make no pretensions to a contribution to the field of science, nor do I make excuses for presenting my own views and those of recognized authority upon this subject.

When I first conceived the idea of writing on this subject, my first thought was for a book review of "Systematics and the Origin of Species" by Ernst Mayr. But soon I realized that this would require almost another book to do it justice. So don't be surprosed if I refer to Mayr often, for his handling of the subject is as Rh. Dobzhansky states in the introduction to this volume, "It has been written by a general biologist".

In the first chapter of Mayr's book he starts out with the premise that "we have an almost unlimited diversity of opinion in answer to such questions as ; What is a species? How do species originate? Are the systematic categories natural? and so forth. There is no uniform point of view amoung taxonomists; in fact, in regard to many questions there may not even be a majority opinion".

Our normal reaction to this is: What kind of a science is this where there are no definite rules and where everyone may disagree upon the one thing which is the key to the whole system.

Well we have long experienced this sort of thing in Iris. For instance previous to Dykes it seems every individual collector brought home some plants and gave them names, with the result that there were duplications, and various reasons for confusion. Then Dykes grew most of them side by side in his garden and attempted to straighten out the mess by eliminating numerous synonyms and greatly reducing the number of so called species. Dykes has been called a "lumper"wheras some others have nbeen called a splitter.

In the 1953 DIS year book, page 131, Mr. Herrick writes, "-- I met, in the pages of the Median Year Book the names of innumerable wild forms, formerly dismissed in the pages of Dykes as mere local varieties, but here used, as indeed in the new classification of bearded irises propounded by Professors

Randolph and Lawrence, with surprising generosity as specific names. Here we have a problem that needs serious consideration before we are overwhelmed with battalions of names which may have no higher status than those of the Lousiana Irises christened by Small and Alexander"

There my friends, is a concrete example of just one of the problems which prompted my writing of this paper. Up until within the last three or four years the iris world has accepted Dykes as the one dependable authority on this matter. But today we find some of the leading "authorities" in America, reverting to the pre-Dykes practices of splitting up the species into further divisions based on minor variations and giving them the status of individual species.

I learn of new species names every month or so; just recently the names of I.dresiatensis, and I. timofejevii has been added; and although I am not questioning the possibility that these may be new species which are just now brought to light; from the previous known practice of these collectors it makes one unsure whether to accept them.

For instance we hear of such species as I. talyschi, listed in the AIS check list as a variety of Foster 1901, named TALYSCHI, and the notes state "a tall form of I. imbricata found in Mountain forests". I need not go further with the long list of reinstatements of obsolete names revived as good species and listed regularly in various iris publications which shall be nameless.

I would like to know why prominent botanists and cytologists here, should revert to practices which have long since been rejected asunacceptable to all their fellow taxonomists. Is it because these persons are still laboring under the old Linnean concept of a species?

To again mention Mayr, he states that "It is true that the change from the static concept of Linnaeus to the dynamic species concept of the modern systematist has not entirely escaped the attention of progressive students of genetics and evolution. However, the whole significance of the polytipic species, of the pjenomena of geographic variation, of the differences between geographic and other forms of isolation are by no means as widely appreciated among taxonomists as they deserve".

I remember Dr. Henderson telling me of an example that his botany teacher used for explaining a species. It went something like this: A stool with one leg will not stand up alone. A stool with two legs also will not stand alone. But a stool with three legs will stand up without further support. He applied this to a species; that a good species must have at least three distinct diagnostic characters or traits.

What is a diagnostic character? The International Rules of Nomenclature state that no generic or specific name shall have any status of availability unless and until it is published with a summary of characters which differentiate or distinguish the genus or the species from other genera or species, etc.

It is agreed by botanists that the description whall serve as a diagnosis, and that it should include characters which tend to remain stable. And as Myr states, "The fewer the characters used, the greater is the danger that mistakes in the classification will be made".

In the past only morphological characteristics were considered in the description of a species. Today we can add other criteria such as cytology, genetics, etc.

To attempt to explain what we mean by diagnostic characters, I will list I. attica as having the ovary at base of stalk with a long perianth tube and little if any stem; one teminaal bud with no lateral branching; spathes relatively short, outer valve very slightly keeled, inner valve rounded; beard color is self color or entire one color beard, (which however may be yellow, blue, violet, white, etc); the seed pod has its inner walls opening into each other in bottomhalf. To this may be added the more modern characteristics as having 16 chromosomes.

It is often contended that no character which is common to other or different species should be included in the description, but this is not always a practical matter. Often a closely related species may show one or more of the same characteristics, and a different one from both may show the other charactertics. In which case none of the supposedly diagnostic characters are restricted to any one species.

To illustrate, I. mellita has the ovary at base of stalk but it differs from attica in having two buds rise from the same set of spathe valves. And mellita has 24 chromosomes against 16 of attica. I. pseudopumila has 16 chromosomes but it has the ovary half way up the stalk, with a different shape of ovary, larger plant, later blooming, and has an entire beard like I. attica.

I. pumila has the ovary and perianth tube, and opening in walls of ovary, one terminal bud, and similar spathe valves to I. attica. From these resemblances the old botanists called attica as I. pumila Even today the taxonomists fail to give us a clear cut decision on this matter. However there are definite diagnostic character differences to support the idea of a separate species.

I. pumila has a bicolor beard, the part down inside the throat is always yellow, regardless of the color outside on the falls, therefore as this outside color is all that varies, we should indicate beard color from this part of the beard. It never shows a yellow beard in pumila, although attica has entire yellow beards, as also does I. pseudopumila. Pumila has that pure sky-blue color which so far has not been found in either attica or pseudopumila, which makes it distinct in this character. Pumila is a tetraploid with32 chromosmes, wheras attica and pseudopumila are diploids with 16 chromosomes.

Therefore we can say that pumila differs from attica and pseudopumila in three diagnostic characteristics; that attica differs from pseudopumila in three different ones, and each of these three species have suffices have sufficient differences to give them specific status if we consider them as combination groups. If we must eliminate all characteristics which are common to two or more of this group and insist that each must have exclusive and individual diagnostic characters, then none of these forms can have specific status. But if we can show in a combination effect three characters which differ from another combination, yet is in common with some other species for some characters, we have a basis for determining the status of a group of plants, the combination.

Perhaps the most difficult problem which faces the botanist or taxonomist is to determine the limits between two closely related species. Just for the exercise let us imagine the possible evolution of these three species I. pumila, I. attica and I. pseudopumila. It has been considered among some botanists that I. pumila was merely a tetraploid form of the diploid I. attica, through a doubling of chromosomes. I do not altogether accept this assumption as I find certain things in pumila that are not common to I. attica. For instance where did the blue of pumila come from?

It is also claimed from the Karyotype studies of Dr. Mitra that pumila originated from a cross of I. attica x I. pseudopumila and then a doubling of chromosomes. I cannot accept this story either for some very obvious reasons. But it is possible that all three of these species had a common source and that through individual branches of it developing independently in isolated populations they became eventually so differentiated that each became a different species. That is the way species are originated, by natural selection in separate and isolated populations, by mutations, and various adaptive processes.

I like to think that I. pumila developed from the same source but independently from I. attica. And that I. pseudopumila is a direct desendent of I. attica though it may have acquired some different genes through natural hybridization and eventual reversion to a near approximation to its original chromosome complement. We can study the steady progress in the development of I. pseudopumila from I. attica, stage by stage, by checking the intermediate forms beginning in jugoslavia, proceeding down the east coast of Italy and across the bottom of Italy to Sicily where the representative form seems to reside.

Then the problem exists of determining just at what point I. attica ends and I. pseudopumila begins. This is probably the most difficult problem facing the taxonomist. In Genetics and the Origin of Species by Dobzhansky he cites a case of the Titmouse that originates in Mongolia, spreads westward and southerly to Europe, then circles and wends its way back eastward until it finally meets again its beginning. By this time it had changed to much that the two groups would not mate and in fact could be called separate species. Just where along the line did one species end and the other begin?

We might say that in both cases this is a matter of purely variation, and it is this degree of variation which culminates in the birth of a new species. Which brings us to another problem concerning species.

I think it was Huxley who introduced the term "New Systematics" The old systematics, which is what is meant by taxonomy, was based on strictly morphological characteristics, and as Mayr states, many species were known only from a single or at best a few specimens, the individual therefore was the basic taxonomic unit.

Which reminds me of a recent statement by a noted scientist that as our "chamaeiris varieties" are larger than the type as described by Bertolini, we should not call them chamaeiris but instead as the 40 chromosome complex. Yet it has been generally accepted for many generations that I. italica, I. olbiensis, and other forms under synonyms, are merely forms of I. chamaeiris.

The individual specimen as representing the whole species was the accepted practice of Linnaeus and his followers up until a few years ago, but with the advent of the "new systematics" a different concept is now being gradually accepted. Mayr describes it as "The population or rather an adequate sample of it, the 'series' of the museum worker, has become the basic taxonomic unit. The purely morphological species definition has been replaced by a biological one, which takes ecological, geographical, genetic, and other factors into consideration."

To confute the argument concerning this chamaeiris matter I quote Mayr again, where he says, " It has nothing to do with the validity of a species or with its limits. It is not necessarily the most typical specimen of the species and therefore it should not form the sole basis of the description."

So instead of a particular meuseum specimen being the sole criteria for a species, we now have a great variety of specimen with many variations to be considered and the limits of such variations to be determined before it becomes a separate and individual species.

A fine example of this is in I. pumila. Our old concept of I. pumila was the type we now recognize as the Austrian or Western type. Recently we have obtained a few specimens of populations native to areas further east; in the Balkan areas, the Crimean and Kuban forms of Russia. However as yet we have only a few specimens and not enough for an accurate determination of their status.

We are told that these Russian forms have 30 chromosomes instead of the usual 32 chromosomes. That some have stems of up to two inches, some are much taller and larger forms, they have yellow beards, and in fact are such a mixture that it destroys all of the diagnostic characters wherby we can recognize this species. In such a case how does the ordinary iris person know when he has a true species, or the botanist for that matter. In other words how much variation is allowed within a species, and when does it become a different species. To decide this matter we must find some sound basis for determining what constitutes a species, for until some sort of agreement is reached among the taxonomists it is not a science but merely a group of opinionists with no recognized authority.

But along with the evolution of species we probably must wait for the evolution of the taxonomist. As in the old days the basic unit was the individual and its morphological description, then comes the new concept of the"series" or sample of the populations as the unit. With all of the new divisions contributing their bit to the whole, we find as might be expected that these individual divisions such as cytology, ecology, etc., attempt to determine a species on their own angle of investigation or at least give it undue proportions of importance. As for instance the cytologists will try to determine the status, origin and relationship of a group of plants by their karyotype and cell aspects. The geneticist will put too much dependence on their particular field. All of which reduces the importance of the work of the systematist or cytologist in the morphological field. However we can hope that eventually the relative importance of each section of investigation will be correlated into a whole, which will give a greater clarity to the situation.

But in the meantime how are we as breeders and workers in this field to know when our forms are true species? I am particularly concerned at present with I. pumila. I though I knew this species pretty well from studying and breeding it from my collection of near 200 specimens collected from all known areas where it grows. But every day I grow more confused with the reports that steadily grow in number.

I know pumila as early blooming, as around 4 1/2 inches high normally, as a rhizome that produces several bloom stalks from side growth, that has an acorn shaped seed pod sitting right on the ground, with long perianth tube and no stem, with bicolor beard, the yellow forms always having white beards, the other colors having various blue and violet to white beards. The flower of pumila is rather distinctive, the spathes short and rounded, often scarious at the edges, one terminal bud with no branching, and breeding as an autotetraploid, with four sets of 3 for a total of 32 chromosomes.

But that is no longer the case. Pumila may have 30 or 31 chromosomes, it is an amphidiploid, or allo-tetraploid, it may have a stem up to 2 inches long with shorter perianth tube, it may have entire beards, and even whole yellow beards. It may grow up to 7 or more inches high, and be later blooming, In fact none of the diagnostic characters are representative of this species. Just what does determine the distinguishing characteristics for this species?

It has been said that no two individuals are alike genetically, that they may be differentiated by only a few genes but even these few will result in a different constitution. We can notice this in a large family of people, brothers and sisters are all somewhat different though all may have a general family likeness.

As two individuals may look and be different, so local populations of the same species may differ even more than the individuals. As a species spreads from its original locale, it will form many different local populations, perhaps each a little different from the other. For instance we may see all whites in one area. or all purples in another, or a mixuture of both in some areas. These various groups are called geographic populations. When a polulation becomes isolated through reproductive or other barriers and develops independently, they may eventually evolve into a different species. I think we may call Cretica such a case of independent development through isolation on an island in the Mediterranean.

It is inherent in these series of geographic populations that they should gradually develop a variation more and more as they proceed along their course of expansion. One might compare this course of differentiation, often called evolution, to a moving stream, such as a river, for the process of speciation is going on all of the time and is in a continuous state of change. The problem of the taxonomist is to determine at just what particular point in this moving or changing stream, is the part which can be called a species. Or shall we take the whole stream as representing a species?

Most anyone will agree on what is and what isn't a species except for the borderline forms. The same applies to most kinds of classification. Clear cut divisions can be recognized but when the intermediate or intergrading forms in different populations appear, these cause the trouble.

Therefore what the modern taxonomists are trying to do is to adopt a system which will include all of these variations and borderline forms, and this system is what they call the biological species or the polytypic species. Meaning the elimination of any one particular type, or any individual diagnostic characteristics, of any specific or tangible thing by which we can say this represents the species. The taxonomist has lost his guiding landmarks by which he can pidgeon-hole the species.

As I understand it, the modern systematics is based on relationships. The old concept of a monotype representing the species and this being the lowest level of distinction, as was the theory of Linnaeus, is NOW ABANDONED AND is replaced by a system including all of the neices, cousins, and even distant relations, these subdivisions identified as varieties, subspecies or geographic groups. It is the same tune as the splitters but merely played in a different octave.

For instance suppose we describe and pidgeon-hole each variety or subspecies, giving it a name as representing each geographic population or group. This is the same as "splitting" the species Each of these categories can now have its diagnostic characteristics, just as formerly the species had its identifying characters. Actually I can't see that anything has been gained, but rather it has merely moved down the scale.

We might even go so far as to include I. pumila, I. attica, I. pseudopumila, in fact all of the basic 8 chromosome forms into one species, based on relationship, as all are homologous. Then we could further divide I. pumila as a subspecies, and attica and pseudopumila as others. And finally the local populations as varieties of the respective subspecies groups.

This is no more than shuffling the cards, it is not any more simple or practical than what the previous "splitters" had attained If this is progress I fail to recognize it. To me it is merely confusion.

Let us cite a particular case to illustrate what I mean. A remark was made that " Don't let anyone tell you the reichenbachii are all yellow and the balkanas all purple; they just don't know what they are talking about". I. reichenbachii is the species, I. balkana is considered as the purple form of reichenbachii, as recognized by Dykes and others in the past. In the first place it appears that reichenbachii is a diploid form and described as yellow. The AIS check list gives it as Y4L, a light yellow self. It has 24 chromosomes.

I. bosniaca is called a yellow form of I. reichenbachii, also a diploid with 24 chromosomes. What differentiates bosniaca from reichenbachii?

I. balkana is regarded as the purple form of reichenbachii, yet balkana is a tetraploid form as we know it until recently, until it is reported we have 24 chromosome forms, being diploid. I have learned through tests that balkana gives about half purples and half yellows, so that in fact we can have yellow forms of balkana. So if we can have yellows and purples in both balkana and reichenbachii, and also both diploids and tetraploids, where is the reason for indicating or using the term balkana at all. Why not consider all as varieties or subspecies of I. reichenbachii. Just how does one determine the difference between a purple diploid form of balkana and a purple diploid form of reichenbachii? I would like for the experts to tell me about this and explain his reasoning for the above accusations that I don't know what I am talking about.

In a recent letter from a friend we were discussing some of the new species Dr. Randolph had collected, principally some reichenbachiis a "Holomondes from from Greece and a mellita from Ayazara, near Istanbul. Even Dr. Randolph had stated that this reichenbachii from Chalcadice "are quite like mellita from the Istanbul area --- but this reichenbachii has taller stems (up to 6-8 inches) shorter fatter spathes and shorter perianth tubes than the mellitas" Then " the reichenbachii from Bulgaria is like what we have had before except it has practically no stem--- and quite different from the true bosniaca". So no we have reichenbachii with no stems, and pumilas from Russia with stems up to 3 or more inches, which is a complete reversal of what we had previously recognized as diagnostic characters for these species.

To show how divergent are the opinions among taxonomists, Dobzhansky (1937) defined a species as "that stage of the evolutionary process at which the once actually or potentially interbreeding array of forms becomes segregated into two or more separate arrays which are physiologically incapable of interbreeding" Mayr comments on this " This is an excellent description of the process of speciation but no a species definition. A species is not a stage of a process, but the result of a process.--- it stresses the two basic elements of a biological species definition, the interbreeding of the populations that belong to the species, and the reproductive isolation."

Such a definition may apply to animals but most certainly cannot apply to plants. Most surely we would not call a tetraploid TB iris and I. mellita of the same species just because they are capable of interbreeding. It is apparent that the plant and animal taxonomists disagree beyond any prospect of a uniform concept of a species.

A friend upon reading this article remarked that it makes good reading but can't say that it answeres any problems especially. We all recognize the difficulty of making any kind of classification and especially one so critical and unstable as a botanical classification. But I have always contended that any classification can only be useful if devised for a particular purpose that is intended. It cannot necessarily serve for any other purpose.

Therefore in my humble opinion, it seems that the purposes of a biologist, a student of evolution, a taxonomist, a museum worker or cataloguer, are so different that one system of classification is unable to serve all purposes.

Perhaps it may be necessary to have one for the botanist, another for the zoologist, and each adopt their own concept of a species.

But regardless of how it can be worked out, it needs to be handled in a way that the informed layman can understand. We can't go on indefinitely with the definition which one scientist states, that --- ' a species is whatever a competent systematist or cytologist so determines" And they all differ.

As I view the system in use for identifying man, as complex as this is, it is surprising how competent it works. A man may be classified according to his nationality, race, politics, religion, town, family, and finally Mr. Jones the individual, I see no reason why plants can't be handled in a similar manner. But it can't be a done with one classification attempting to serve different purposes.

And I might add that the greatest confusion exists in the situation which has been called the "lumpers" and the "splitters". This can not be solved until there is some agreement on what represents the lowest taxonomic units in the categories. In other words, we must determine the purpose of the system and then define its sections or categories on this basis.

************* From a Robin from Dave Reath
Persmission taken for granted.

Atomic Blue (S-504) is the best blue pumila and should have seen the show it was putting on for the meeting. It has the wonderful domed standards and flaring falls with the perfect coloring. It will be hard to improve upon this pumila.

The best yellow pumila is No. M-513. It is quite widely distributed so perhaps some of you folks have it. It has very good yellow color with fine shape. The falls are very flared and are shortened and rounded. It has the nicely domed standards and I notice that it played a big part in my pumila breeding this year. It was used with about every other pumila which I used for breeding. Here I'm hoping to transfer the excellant shape to the other colors and also to add the fine color to the other colors to see what combination will produce.

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If you had sent in your contributions we wouldn't have a blank space here.

DWARF IRIS SPECIES

As it has been some time since publication of material upon the dwarf iris species and much has been learned in the meantime, I feel it is advisable to bring us up to date with a more complete report on our findings.

New material is being collected and sent here each year until at the present time we have more extensive collections than had ever been assembled in the past. Also the breeding and study of species has given us much additional knowledge of the inheritance of these species.

It might be proper here to indicate here the most recent reclassification of Pogoniris as presented by G.H.M. Lawrence. Under the subsection Pogoniris are two subdivisions, namely: Series Pumilaw and Series Elatea. Under Series Pumilae is listed the following species: I. biflora, I. Bloudowii, I. chamaeiris, I. Griffithii, I. mandschurica, I. mellita, I. Potannini, I. pseudopumila I. pumila, I. reichenbachii, I. scariosa, I. subbiflora and I. Tigridia.

Under series Elatea is listed: I. aphylla, I. lurida, I. Kochii, all tall bearded, including what we know as the diploids, Tables and ordinary intermediates.

I. biflora is a doubtful name, probably a synonym for I. subbiflora. As yet we have no specimens of I. griffithii, I. mandschurica, I. Potanini or I. Tigridia; evidently they are not found in cultivation. I. scariosa is also a name which we don't find in use today to my knowledge.

I. CHAMAEIRIS.

I chamaeiris was first described by Antonio Bertolini in 1837. The name is derived from a Creek word meaning "on the ground." It is interesting to note that chamaeiris is mentioned by Sir. Grances Bacon (1561-1626) in his essay "Of Gardens". It is native to the coastal areas in Southern France and in northwestern Italy. Chamaeiris has various synonyms or subspecies names, amoung them are I. italica, I. olbiensis, and Dykes includes I. virescens and I. lutescens. It is certain that this complex shows a quite wide variation in size and form; the Bertolini type being the smallest, (around 6 inches hight) wheras I. italica often grows up to 12 inches.

I chamaeiris is the largest as well as the latest blooming of the true dwarfs. Generally speaking, we consider the average normal height of this species and its horticultural derivatives as around 8 inches.

Its leaves remain quite green during the winter, the stalk usually rises above the leaves supporting s single terminal bud but rarely it will produce two buds in a terminal cluster, due probably to environmental conditions such as climate, season, culture, etc. Chamaeiris always has its ovary at the top of the stalk as contrasted with pumila, which has its ovary at bottom of stalk, with spathe valves more green and rounded and close wrapping, showing only a slight keel on the outer valve.

As to colors in the chamaeiris forms, they are mostly blue-purple and red-purple, or medium to pale yellow, with an occasional near-white form. The patterns run in beautiful selfs and bitones, with occasionally a faint border on the falls. Beards are predominantly yellow, but often beards of violet, blue or white are found on contrasting petal colors, which adds variety and are most effective.

Considered genetically, I. chamaeiris is an amphidiploid or allo-tetraploid, having two sets of 8 and two sets of 12 chromosomes differentiated in pairs and ono-homologous, making a total of 40 chromosomes. In functioning at meiosis the two sets of 8 chromosomes pair normally, as do the two sets of 12 pair interse, thus each differentiated type functioning as a diploid, with regular pairing and exchange and distribution of factors. However as would be expected none or little crossover and exchange of factors between the 8 type and chromosomes occurs, and as each separate group gives individual expression, the final result is a composite effect in most cases, with this exception; that the dominant factors within the 8 type chromosomes tend to suppress the latent and recessive forms in the 12 type chromosomes, and vice versa, which results in an expression of mainly the dominants of each group, accounting for the restricted color range inthis species. However when both groups of factors segregate their recessive white forms simultaneously, we get a white form. That is, it is a double expression of white, and hence a double recessive. There is no dominant white flower in chamaeiris to our knowledge.

Recently there has been some discussion and experimentation as to the origin and genetic composition of I. chamaeiris. Dr. Randolph has stated he has found evidence that one of the parents of I. chamaeiris is I. pseudopumila, a species growing quite near to chamaeiris territory, which has 16 chromosomes and is native to sicily and southern Italy. The other parent is claimed to be a form of tall bearded, possibly I. pallida or similar form. However this is only an gypothesis so far and all we know for sure is that this other parent has a basic number of 12 chromosomes, which might be any one of such species as reichenbachii, aphylla, or possibly others.

I might suggest that most of the older dwarf iris varieties as we know them have been selected forms or derivatives of I. chamaeiris, as suggested by their morphology and numerous chromosome counts showing 40 chromosomes.

Despite the AIS classification indicating that the dwarfs above 10 inches in height, including Lilliputs and varied intermediate gybrids, are "Standard Dwarfs", we know that the true, and traditional standard dwarfs were such varieties as Sound Money, Little Jewel, Rose Mist, Tony, Tiny Tony, Harbor Lights, etc., which average around 8 inches.

I. PUMILA

When we come to I. pumila we are dealing with the recognized "King of the Dwarfs". In the botanical classification I. pumila is distinguished as representing the "type " of the Dwarf Iris group, as this section is titled the "Pumilae Section".

I. pumila was collected and described by Linnaeus in 1753, and the location Austria. It is the only species which has never been questioned or its status disturbed by subsequent botanists. The name is derived frm the Latin pumilas, meaning dwarf, diminutive.

I. pumila has the largest distribution area of any known iris, extending from Austria, down the basin of the Danube thragh Hungary, Roumania, Bulgaria, Down onto the island of Crete, and over into the Ukraine, Donets, and Kuban basin and the Crimea in Russia.

Certainly with so side a range with its possibilities of variation, we could expect a rather interesting diversity of forms as it progressed into distant lands, and different enbironments, and this actually occurred. Up in Austria we find more coarse forms, with wider and longer leaves, with shorter bloom stalks, and over into Russia the daintiness increases, with shorter and more narrow leaves,longer and more slender stalks, and blooms smaller and more narrow in the segments. Then the Serbian and some of the Roumanian forms show a still different overall variation, the whole plant larger in all of its parts and apparently a better grower for some climates. Yet for a species of such widespread populations the general similarity is surprisingly close.

I. pumila is particularly distinguished by its short and almost non-existent stem, with the ovary practically sitting upon the rhizome, and with its long perinth tube rising to support the flower, which is as high or higher than the leaves. This height ranges from about three to five inches though most are about 4 1/2 inches high. Some of the Serbian forms will average to five or six inches. And from reports on a few Russian forms we know, really not enough for a decisive opinion, some have been found having a stem an inch or more in length, which is in contrast to the Western group of pumila. The bloom is in nice proportion to the plant, the whole presenting a dainty ensemble that makes it a veritable bouquet.

The rhizome of pumila is highly prolific of bloom stalks, in that we often find up to eight of these stalks expressed as tiny side fans on a single rhizome, and each with its one terminal bud. A clump thus becomes a solid mass of bloom and eliminates the necessity of and compensates for, the lack of branching. Pumila is shallow rooted, thus requiring frequent replanting and a good garden soil to do its best performance. The spathe valvew are closely wrapping the stam, are rounded with a slight keeling on the outer valve, the valves showing a scarious or onion skin texture at its upper tips, often reddish. But they are neat and rather inconspicuous.

The ovary or seed pod is quite distinctive in pumila. It is rounded and pointed, looking much like an acorn, is variable in size from one half to over an inch in diameter and plainly showing the seams which outline its three compartments. On the inside of the pod are three sections, the walls making a complete division in the upper half of the pod, but in the lower half the walls are open into each other. No other iris is known to have this distinctive character, except its close relative I. attica which some think may be the immediate parent of pumila, and I. chamaeiris which is apparently a hybrid involving either I. pumila or I. pseudo pumila both of which are very close relatives, and hence derived this characteristic from pumila. The walls and outer shell are thin and papery when ripe, of a dull warm grayish color, resembling parchment.

The flower also has a distinctive character. The standards are arched and just short of meeting at the top, the tips reflexed or turned outward, in typical forms. Most of the species tend toward tucking or a reflexed condition. The segments as they rise from the perianth tube show a definite triangle or vase-like form, to where the falls turn abruptly outward, in contrast to the gradual curve that is apparent in other iris.

But it is in color and pattern forms that I.pumila demonstrates that it is the "King of the Dwarfs". No other species can boast of as many variations and combinations of as many colors and patterns as I. pumila. With four basic color factors, violet, purple, yellow and white, we have obtained a range from black through the violets and blue purple, reddish, lavender, orchid pinks, green, browns, blends, yellow to orange, ivory and white. With the spot pattern we get amoena, variegata, neglecta in numerous combinations and variations, in addition to clear self patterns, bitones and bicolors as Pinnacles.

Beards are of special interest in the pumilas. The yellows have only white beards, always. When I write of beards I mean the part which shows out on the falls, for this is the only part which shows any variation; all pumilas have yellow to reddish-orange down the inside of the flower. But on the anthocyanin colored flowers, we have dark violet, purple, lavender, blue, and white beards. No yellow beard is known at present in pure pumila. Pumila is the only species in iris that has a true blue color, and apparently it is one of the components in the violet factor, for in breeding violets we get blues. Another fact which indicates that it is a different anthocyanin from the other irises, is that the anthocyanin inhibitor does not suppress pumila blue but it does suppress the blue of talls.

In view of the wide range over which I. pumila grows and its extensive variation among the different geographic populations, some writers have offered a plan of dividing the pumilas into two groups namely the Western and the Eastern Groups, which compares somewhat to a previous division called pumila Occidentalis and pumila Orientalis.

Rudolf Hanselmayer has outlined these sections as follows: "The Austrian pumilas or Western group includes all pumilas growing in the wild in Austria, Hungary, Czecoslawakia and west or Roumania (Sieben burgen). The Serbian pumilas and the Rumanian pumilas are from Dobrogea, which grows wild on the west coast of the Black Sea, though this locality is also in Dobrogea. The Serbian pumilas were introduced by Dr. Werckmeister; the Rumanian pumilas by myself. Between these two groups are the Rumanian pumilas heliotropii and rozaliae."

"The Eastern Group include the Rumanian pumilas from the west coast of the Black Sea and the Russian pumilas from Crimea, Kuban and from Ukraina. These pumilas are distinguished from the Western Group not only by their narrow leaves and parts but also by their chromosome number which has been counted as 30 instead of 32 chromosomes.

Of course as I mentioned before, we have not had enough of these Eastern forms to really determine their status, but until such time as we can study further, we can view this as a possibility.

Now for the genetics of I. pumila; it is apparently a simple tetraploid, judging from its breeding and inheritance behavior, for it interbreeds readily, giving full pods of seed, it shows no restrictions in producing the full gametic range, and apparently is fully homogeneous. Dr. Mitra has proposed the theory that pumila originated from a cross of I. attica x I. pseudopumila, then followed by a doubling of chromosomes. This finding was based on a study of the Daryotype, meaning the number, shape, size and general morphological character of the chromosomes. This according to Dr. Randolph makes pumila an amphidiploid.

I have not the space here to argue the point but we do know that according to the statements of several reputable geneticists that Karyotype has no genetic significance, and thus is no criterion of the genetic constitution of a plant. Therefore from my own experience and the doubtful nature of these conclusions, I cannot accept this theory.

But we do know that pumila has four sets with a basic number of 8 chromosomes, making a total of 32 chromosomes normally.

I mentioned previously that pumila produced amoenas, Pinnacles, Variegatas, neglectas and bitones through a factor known as the spot factor. Pumila has two basic pattern forms, in addition to the regular color factors. These are the self pattern, or overall flower color, and the spot pattern. This self pattern can be in any color and is independent from the spot pattern factor, which superimposes the spot overlaying the self color, on the falls. Thus these amoenas, variegatas, etc are really the expression of two independent factors in combination. The border on the falls is merely the self pattern showing outside the limits of the spot on falls. Both of these patterns are dominant factors.

As all of the oldtimers considered the amorna pattern as a recessive character and inherited as one unit, this new finding comes as surprize and even now some persons have not accepted it. The fact is that the white self of amoena is recessive white, and the dominant spot overlaying the falls is merely in addition to the recessive white factor. Thus amoena involves two independent factors in combination.

We must not forget that important gene known as the inhibitor factor which is so important in breeding, particularly with the talls. It is believed that this inhibitor which is responsible for the Tall Bearded Dominant whites, originated in the dwarfs, which obviously points to I. pumila. We know that this inhibitor is present in pumila, that it suppresses the blue of talls but does not affect the blue of pumila. Which assumes that the blue factor or type of anthocyanin in pumila is different from that of the talls.

Before I leave this matter of color and pattern inheritance I wish to propose another theory. In my breeding of the pumilas of which I have grown many thousands of seedlings, in every case of crossing violets with Hanselmayer and other yellows I have gotten violets, blues, and yellows but no purples. In crossing purples with Hanselmayer and yellow I get purples, orchids, and yellows but no violets or blues. I am now crossing these violets and purples with recessive white for a final check. From these experiments it would appear that the factors for purple and violet were separate and independent factors.

One other thing should be mentioned here, as it has been the subjuect of many discussions recently. We have one lone plant of a form collected on the island of Crete by Atchley in 1929, that is a rather smoky pinkish-mauve, spot pattern on falls, lavender beard and 4 inches tall. It has 32 chromosomes. But it has some appearances which womewhat resmeble mellita character; in fact Atcheley at first called this a form of mellita but finally decided it was a pumila. The perianth tube is of that slender and curving type we recognize in mellita, rather than the usual somewhat stiffish character to the perianth of pumila. The texture of flower is a flimsy kind and the smoky color, and tendency to sickle shape leaves reminds one of mellita. Yet anyone who knows pumila would assuredly recognize it as pumila. But this is a good example of how an isolated population can develop independently and finally come out as a different variety.

But I didn't mention this alone to show the discrepancy between it an pumila but to discuss another matter. during the past three years several persons have crossed tall plicatas with Cretica and gotten plicatas in the first generation. It was immediately assumed that Cretica has the plicata gene, based on the assumption that plicata is a recessive character in inheritance. At first I was doubtful about this cross but as it was repeated elsewhere and I got plicatas myself from the cross, I had to accept it but not the idea of Cretica having the plicata gene.

The way I have explained this is, that in talls plicata is recessive to blue, but it is dominant over the end recessive which is recessive white. (This providing the theory of multiple alleles is true) Therefore if plicata is dominant over recessive white in talls, it apparently can be dominant over recessive white in pumila. Thus when Cretica threw a recessive white segregate, it allowed the Tall plicata factor to give expression.

This would work out if Cretica does not have the inhibitor factor, or if it were in low dosage wherein a segregate lacking the inhibitor gene is possible. Study of the sister seedlings of this hybrid plicata showed a predominance of purplish tones in low dosage, which indicated the absence of inhibitor gene. This would explain why Cretica can give plicatas where other pumilas do not allow it because of this inhibitor of anthocyanin color. That to me, is the explanation of plicatas coming from Cretica and we have no reason to believe at present that Cretica has the factor for plicata.

There was a time, not far back, when I. pumila was unavailable in this country. We know that Burchfield, and Sass did not have pumila, even though Sass gives pumila as the parent in his varieties of dwarfs and intermediates. Andllet me not forget to say that it has been the custom in the past to call all dwarfs as pumilas and is even a common practice in Europe today.

When I began work with the dwarfs it was almost impossible to find a pumila and the only varieties extant at all were Schreiners varieties called Sulina, Nana, and Carpathia. There were grown from seed collected in the Baltic Sea areas and Schreiner named them from places in that area. Today all species are becoming more plentiful and several named varieties of pumila are available to the breeders and growers, which I will attempt to list here, at least the most important ones, for your information. As for breeding purposes it is important ot know which are selfs and which are with spot on falls, I will indicate these in the list.

April Morn	blue	spot	Carpathia	Yellow	spot
Blue Spot	neglecta	spot	Nana	purple	spot
Cretica	purple	spot	Sulina	violet	spot
Hanselmayer	lemon	self	Little Balkan	purple	spot
Spring Joy	neglecta	spot	Flashlight	yellow	spot
Morning Fresh	neglecta	spot	Red Amethyst	red	spot
Crimea	violet	spot	Flaxen	Blue	self
Dr. Lemperg	violet	spot	Wee Blue	blue	self
Sea O'Blue	blue	self	Arrowhead	blue	self
White Mite	white	self	Bernd	var	spot
My Daddy	ivory	self	Inge Jordan	lilac-bl	spot
Snow Baby	white	self	Greenie	Green	spot
Atomic Blue	Blue	self	Wee Blue	blue	spot

Never before was such a wealth of breeding material available to the breeder. We hope that America never gets in the position it was when I started working the dwarfs, with no material to be had.

I. ATTICA

Here is probably one of the great-grand-daddies in iris history if not in evolution. It is undoubtedly the progenitor of I. pseudopumila and could easily be the daddy of I. pumila though it also could have derived from a common source with pumila. The fact of its diploid composition in relation to its tetraploid relatives, I. pumila and its low number of chromosomes, would indicate its prior existence. In fact it is probable that it covered a much greater area in prehistoric days and was covered and destroyed by the glacial period which pushed all vegatation further south in its path.

I. attica Bois. and Heldr. 1859 is probably the most limited in its native habitat range of any iris species, being found in only a comparatively small area in Greece. Until rather recently it had never been in America. The first specimen known here was given to me by Mr. A.C. Herrick of England. A year later I received two more forms from Mr. Darby of England, then Dr. Randolph sent me four new forms which he had collected in Greece. All of these were counted by Dr. Randolph and found to have the expected number of 16 chromosomes. Attica is a diploid form with a basic number of chromosomes.

As for color and pattern forms it seems to have approximately a similar range, except for our expectation that a tetraploid had the advantage of a larger number of combination effects and thus augments the variety of effects. But with one exception however; this is the blue of pumila. So far we have been unable to find a blue attica.

Another difference in attica is that it has an entire color beard that is one color its entire length, which is in contrast to pumila which always has a bicolor beard. Also attica has entire yellow beards. For instance in pumila a yellow flower always has a white beard, wheras in attica it can be either yellow or white.

Other minor differences between pumila and attica are the sickle shape leaves which are more prounouced than in pumila. Of course attica being a diploid, we expect it to be relatively smaller in all its proportions. Otherwise attica appears to be simply a smaller edition of I. pumila.

For one thing attica is more difficult to grow than pumila. I might state that it is very susceptible to lack of good drainage and as its root system is very shallow, it is subject to winter or spring heaving if not furnished with a light mulching.

The botanists of the past have indicated I. attica as a form of I. pumila, and thus the descriptions applied to pumila will to a great extent also apply to attica, and therefore it leaves little for us to describe concerning I. attica.

It is unfortunate that so far we have had difficulty in growing crops of seedlings, as they are not as easy to pollinate as pumila with fewer seed, and less germination. I find this true of all diploid forms in comparison with tetraploids. But with the less number of seedlings I have grown I find about the same kind of inheritance that we know for I. pumila. Therefore for most breeding purposes, we find pumila much more benificial. Yet for experimental work, the diploid form has all of the advantages of requiring less seedlings for obtaining ratios of inheritance and diagnostic purposes.

At present I have about ten different forms of this species collected in Greece and am now growing some seedlings from seed collected by Dr. Werckmeister. So with the forms which Dr. Randolph has distributed around the country, it is readily available for most breeders.

I. PSEUDOPUMILA

I. pseudopumila was collected by Vincenzo Tineo in 1827 and described as native to Sicily and Southern Italy. It had apparently never been in this country until Dr. Randolph collected it and brought it to America. At that time he sent me two forms, one from the island of Sicily, the other from Southern Italy. Both were so very different in character that it is difficult to conceive of such variability within one taxonomic unit.

My notes from the San Martino form says; Standards greenish yellow, Falls purplish spot with border. Overall height 3 1/2 inches, ovary 4 inches up the stalk, perianth tube 4 inches. S. open, F. tucked, slender stalk, wide pointed leaves, not curved, greenish yellow beard, It bloomed with chamaeiris.

The other form was lowgrowing and prostrate, except for the bloom stalk which was 3 inches high. The leaves were as extremely sickle shaped as I. mellita, and rather wide.

When we think of pseudopumila it is natural that we should compare it with attica and pumila which apparently are very closely related, but in the typical form it is so different from either that one can hardly realize that they could be so different.

For instance the seed pod is half way up the stem, which would suggest that it is a hybrid from some species having the ovary at top of stem, as both attica and pumila have their pods at bottom of stem. The shape is also different, instead of being rather rounded it is longer, up to 3 inches at times, quite large, resembling somewhat the shape of I. chamaeiris, and the pod wall is thick and meaty. It has one terminal bloom, no branching. The former came from San Martino, the latter from Apulia, Italy.

Later I received other plants, in other than the variegata pattern; one a violet form, another in yellow. One of these in particular was interesting, in that it comes from the Adriatic Coast near Zadar, Yugoslavia, which is considered outside of pseudopumila territory. Cytological tests of the chromosomes of this Zadar plant show that it has 16 chromosomes which are very similar to those of the known I. pseudopumila. Dr. Randolph reports that it is much smaller than the type locality form. Thus suggests the possibility of its being a oonnecting link between I. attica and the taller forms of pseudopumila native to Southern Italy.

In looking at this Zadar form I fail to distinguish anything about it that would place it in the pseudopumila group. It has the look of I. attica in everyway. Even its Karyotype to my idea, lacks the evidence to regard it as a pseudopumila. Dr. Mitra states that in both species all chromosomes are subterminally constricted but that in attica the # 1 chromosome is submedian, wheras in pseudopumila it is median. This alone seems to be the sole evidence for calling it a pseudopumila rather than an attica . He further states that in the Zadar form, the satellites occur on chromosomes 2,3, and 7, the same as I. attica, wheras in other pseudopumilas they occur on chromosomes 2,6, and 3. On the basis of the shape of just one chromosome it has been claimed that I. pseudopumila extends its territory over into Yugoslavia.

Dykes states that if it were not for its distribution, pseudopumila might be looked upon as a hybrid of chamaeiris x pumila, for it is practically a large chamaeiris with a long perianth tube. This describes it quite well, however it is quite different from both chamaeiris and pumila in most of its characteristics.

Then the question appears, in consideration of it bein of so close relationship to attica and pumila, with chromosomes homologous to both, and the same number and kind of chromosomes, where did it get that chamaeiris look, that big seed pod, that ovary half way up the stem, that late blooming with chamaeiris, etc. We know that aside from possible mutations, it is possible for one species to cross with another to produce a hybrid and perhaps acquire new characteristics, and then by the loss of univalents or chromosomes without partners, these odd elements will become lost and the species will revert to its original number of basic chromosomes but may retain the new acquired genes or factors. This may be the answer to pseudopumila.

I find pseudopumila is probably the least adapted to the conditions here in the Midwest, of any of the species. I have difficulty in growing it, and it appears to be expecially susceptible to rhizome rot. Which explains why I have been unable as yet to grow more than a very few seedlings from it. It is possible that it may be better adapted to some areas where pumila is more difficult to grow, and it should be tested for such environments.

I. MELLITA

Here we have the "delightful iris", from the latin mellitus, meaning delightful. It was collected and described by Victor Janka von Bules, abbreviated Janka, in 1874. I. mellita resides in that area above attica and below pumila, away from the Miditerranean and extending from the Albanian area, through the Balkan zone and over into Turkey along the Black Sea.

Mellita is another species which we have not explored fully, leaving much to be deisred in the way of offering a full description of its inherent capacities. We have however had some collected forms to grow and study. The plants we know here are usually of a smoky red-purple or a smoky blend of purple and yellow, although Dykes says that clear yellows are found in the Turkish area.

Mellita is a little plant, seldom growing over four or five inches, and its most conspicuous character is its sickle-shaped leaves. The leaves are rather coarse and deep green, the spathe valves are long, all green, sharply keeled and coarse, resembling some additional leaves. From these spathe valves rise two buds on slender wiry perianth tubes, which extend well above the half prostrate leaves. As with pumila, the ovary is at the base of the stem down among the leaves. Shape of overy resembles that of pumila.

The flower of mellita is highly distincitive, easily recognizable by anyone who has once seen it, and it is quite strong in dominance, being apparent in its hybrid progenies. The standards are about twice the size of the falls, more cupped shaped than arched, leaving them quite open at the top, and they are wide. The falls are always abruptly tucked, even folding back to the perianth tube. The texture and substance is noticeably thin and papery, but in spite of this appearance the flower is able to stand unusual wear and handling. The beard on the purple form is an electric violet or blue, really brilliant, while on the so-called yellow forms it is always white. Mellita always has what we call an entire beard, that is one color its entire length. It also is distinctive in that the beard is soft and meager, like cat fur that has matted down. Mellita always has purplish veins radiating from the base of standards and rather conspicuous on the falls, particularly on the haft which shows over a white area around the beard.

There is another form called I. rubromarginata, which is evidently just a varietal form of I. mellita. It gets its name from a red margin or edging to its leaves and on the tip of the spathe valves. Strangely this red margin is most clearly visible during heavy growth in spring and in the fall, wheras during the hot summer months it is seldom very noticeable. The leaves as a whole seem to be darker than on the forms without the red edging.

Due to the fact that rubromarginate is most always offered to the customer in the purple form, we had the impression that rubromarginata was the purple form while mellita was the yellow form. But after growing seedlings I found that this red edging was not in any way associated with flower color, which I should have known, but that we can have rubromarginatas in yellow color that is with yellow flowers and vice versa. I also found that this red edging is apparently a recessive character, judging by the few specimens I found among seedling of mellita x rubromarginata) x sib).

I rubromarginata was collected by Baker in 1875 from the locality near Thrace. The fact that it grows right along with the mellita forms in the same populations would indicate that it is merely a normal segregate in the mellita composition.

Genetically mellita is a diploid with 24 chromosomes. Hybrids of diploid talls x mellita have been studied and found to produce 12 bivalents at meiosis, indicating that the chromosomes are homologous with those of talls. Which makes it a useful plant for bringing down some of the TB characters into the dwarfs.

Anyone who has seen that line of new TB iris that Paul Cook developed from a cross he has indicated as tall x I. reichenbachii, will appreciate the value of any iris which contains this inhibitor. To explain this, Paul claims that reichenbachii has an inhibitor for standards only, and that it will erase tall blue from the standards leaving them white but retain the blue in the falls. Thus a new bicolor is created, which is different genetically from the usual spot pattern on falls.

Well I have found that I. mellita apparently has this same or a similar inhibitor, for in several progenies of seedlings from tall blue by mellita I botained several forms almost identical to Progenitor, which is Paul's hybrid of tall x reichenbachii.

As the color range is restricted to purple and yellow in the mellitas, I had hoped to get a white form through interbreeding the different forms, but out of around 200 seedlings no further variations have appeared to date.

A work about culture is not amiss here, for many people have difficutly in growing mellita. To do well it whould be separated and transplanted every year in sufficient time to become well established before winter. It requires a rich soil with good drainage to do its best, the soil should be a sandy loam, mixed with peat moss and compost, and kept moist until the plants are well established. After seeing a natural planting in a woods. on a steep slope and half shade doing so very well, I have concluded that such a condition is the ideal situation for growing this species.

I. Reichenbachii

I. reichenbachii, (Heuffel- 1858 -Balkana) is the name applied to a group of iris that covers a large area in south-east Europe and which Dykes called the Balkan representative of I. chamaeiris because of its similarity in form. He further states it is known under various names, as bosniaca and serbica for the yellow forms, and balkana for the brownish-purple forms.

This however is not in complete conformity with the modern views, Today we hear "Don't let anyone tell you the reichenbachiis are all yellow and the balkanas all purple; they just don't know what they are talking about". Which brings up a problem which deserves some attention, for of all the species in the dwarf iris group this one seems to be the least known and the most confused. Little is known of this group except for the description of Dykes and that of the collectors.

I have received at least 8 or 10 forms which were allegedly collected in the wild, and some have been tested cytologically, and found to have variable numbers of 24,32,36,40, and 48 chromosomes. The true forms are supposed to have 24 and 48 chromosomes, for the diploid and tetraploid forms respectively. As yet I have found few of mine to be true species, although some have not yet bloomed, except one plant which I received from Paul Cook called I. bosniaca.

This one has all of the appearance of being true, though with an entire white beard, whereas I had thought yellow reichenbachii had a yellow beard. One that Grace Guenther grows has an orange beard.

From all records I have searched it seems that reichenbachii is regarded as the yellow form and I. balkana as the purple form. Yet I. bosniaca as it is called, is a yellow form, and both it and reichenbachii are diploids with 24 chromosomes. Why do we retain the name bosniaca in view of this, what is their difference?

A similar case is I. balkana, the purple form, which is a tetraploid with 48 chromosomes. But now we find report of both 24 and 48 chromosome balkanas as well as both purple and yellow forms. So apparently we must revise our estimate of balkana.

Dykes stated that reichenbachii is readily distinguished by its rounded, sharply-keeled spathes. He also states the flowers are either a clear yellow or more delicate texture than the flowers of I. chamaeiris, or of a brownish-purple like those of I. melita.

The plant I received from Gerald Darby of England appears to meet the proper descriptions. My notes on it are as follows: Height 9 inches, standards 2 inches high, 1 1/2 inches wide. Falls 1 1/2 inch long, 1 inch wide, as you may note the standards are much larger than falls. Standards arched, falls tucked abruptly. Beard violet-purple with no yellow down inside. Small white flash at haft, standards smoky-violet with deeper veining, Falls same but one shade lighter, style arms as standards. No spot on falls, white pollen. Ovary at top of stalk, both valves sharply keeled, herbaceous. Spathes are distinctive for their shape, being flattened rather than rounded as Dykes describes, very short and compact, oblong or elliptical. Perianth tube 1/2 inch, two blooms in the terminal. Tallest leaf 5 inch x 1/2 inch, pointed and tends to sickle shape. The stalk is stiff and almost woody, with strong bracht showing.

I have attempted several times to self balkana but without success. But I did the next best thing in crossing it with a white chamaeiris and with a yellow pumila and with TB pinks. In all cases the segregation showed about half purples and half yellows. The purples always had purple beards, the yellow always yellow beards. No spot pattern was evident in any of them.

This indicates that balkana is heterozygous for yellow, which would support the view that balkana is not necessarily exclusively purple, and also that by selfing we can have a tetraploid yellow form as well as a yellow diploid form.

In the hybrids of balkana x white chamaeiris of course all ovaries were at top of stem. In the balkana x pumila seedlings the ovary was at varying distances up the stalk, as we would expect this kind of hybrid to show. Also the spathe valves resembled chamaeiris in the balkana x pumila forms, with the inner valve round and outer valve partly keeled. So apparently the position of pod and the keeling of spathe valves is a mixed dominant character.

In view of the claim of Paul Cook that his Progenitor line of new talls came from I. reichenbachii, I was most interested in testing out this fact. I crossed tall blues with bosniaca and in no case did I find any indication of this inhibitor.

Instead I found it had an inhibitor for the whole flower, But I did use Balkana with blue falls and find it has this inhibitor. Which brings up the question of why balkana should have it and not the yellow reichenbachii?

It may be interesting to know that Dr. Mitra found that six of the 12 basic chromosomes of reichen bachii were similar to those of I. mellita. And we have found that mellita has this inhibitor for standards only. And the Dr. concludes by saying it appears that the tetraploid I. balkana may have had an allo-tetraploid origin, combining the genomes of I. mellita and the diploid form of I. balkana.

Therefore it may be quite possible that balkana is not exactly a true reichen bachii, yet is undoubtedly a very close relative. We shall know much more about this complex after we have had the time to study it further.

I. SUBBIFLORA

I subbiflora, Brotero, 1804 is probably of the least interest to us and may be the least known of all the dwarf species. It is native to the coastal region of Portugal and Spain and apparently a close relative to I. chamaeiris and has 40chromosomes.

I received some seed from Jean Stevens of Australia and from these I grew 8 plants, all of which have bloomed here for about four years. All are almost identical in color, which is a very dark violet self,with bright yellow beard. The height varies considerably, ranging from around 7 inches to about 12 inches. Dykes says it comes in a dark black-blue, a deeper color than he has seen in any other iris. He also says it may come in purple or a yellowish-white, but I have noted no such colors amoung my seedlings.

Actually I see little difference between subbiflora and I. chamaeiris except possibly in size, the former a little larger, and otherwise it comes very close to chamaeiris type. There is no doubt, at least in my mind, that I. subbiflora is simply a different stage or we might say a different geopgraphical group or population in the stream of evolution of chamaeiris. It is not nearly as differentiated from I. chamaeiris as is the Russian pumilas from the Austrian forms, or the different variations of I. pseudopumila.

There existed quite a bit of misunderstandings about this species among the earlier collectors. It has been called, biflora, I Lisbonensis and I. subbiflora rather carelessly until Dykes finally helped to clarify the matter.

It appears that subbiflora performs wonderfully in southern California and Mr. John Tearington is highly enthusiastic about its fine qualities. Thus far we know far too little about it, But for its dark color alone it has potentialties for our breeding program.

I. ARENARIA

I arenaria, Waldst, und Kit. 1802, comes under the general heading of I. flavissima which is recognized as the representative type, but both much alike in general character except for size. I. flavissima is the largest, with I. arenaria the samllest. I. bloudowii and I. mandschuria are intermediate in size.

A peculiarity of this group is that the different forms jump vast areas of space to appear in widely separated populations. Arenaria appears in Hungary, in Transylvania, and then over in the southern Russian territory as the form I. Bloudowii, then into Manchuria as I. Mandschuria. I have never seen Mandschuria but have grown the other forms.

Arenaria is among the tiniest of the dwarfs, growing to around 4 inches high, with narrow grassy leaves, slender stalk, with ovary at top of stem, The spathe valves neat and rounded, looking inflated and often scarious at the tips. Usually two buds arise from the spathes in a terminal cluster, which bloom at different periods a few days apart, resulting in waves of blooming, but the flower is short lived, lasting only one day and on hot days it is over by early afternoon. On a cloudy day blooms will not appear, as it only opens in sunlight. As the flower declines it gradually twists into a screw-like formation.

It comes in only one color, a bright buttercup-yellow, with yellow or orange beard. This is a plastid or carotene pigment, with no anthocyanins or flavonnols, and it is apparently pure breeding for yellow, This suggests arenaria is a homozygous species for color.

It seed itself regularly, so that in all cases if you save open pollinated pods you will get true arenaria seedlings. In fact I know of only one case in which arenaria is known to have produced a hybrid as the pod parent. This was an arenaria x aphylla hybrid grown by Dr. Hertha Van Nes of Germanh. This hybrid is a rosy-lavender self with blue beard, and showing the characteristics from both parents.

A strong characteristic of the flower is its open standards, with wide and rounded, horizontal falls. The bud is bronzy-brown before opening, which shows on the undersides of the falls after opening. The pod is pointed at both ends, opening on the side when ripe and is of a papery substance when dry.

Even as a pollen parent it is difficult with most species excepting I. chamaeiris. It will cross readily with chamaeiris to produce a hybrid which is sterile. We have two seedlings of pumila x arenia, the first and only ones of their king, called Pumar Alpha and Pumar Beta, by Jay Ackerman. These are nice small plants, very robust, with tiny yellow flowers, but which are sterile.

Yet we find quite a large number of chamaeiris-arenaria hybrids such as Tampa, Keepsake, Tiny treasure, Bronya, Mist O'Pink, Promise, Bricky, Cup & Saucer, Glow Gleam, and a very old one called Ylo By Miss Sturtevant.

This habit of self pollination has evidently been a factor in keeping its gene pool pure. And therefore considering the handicaps involved. It is advisable to open the bud before the flower opens, take off the stamens, and pollinate the flower then, and close the flower and cover to avoid contamination.

I have just recently received from Karl Ajdovic of Austria some plants and also seed of a population of arenaria in Austria. He also sent several color slides showing them growing in the wild. They were in open fields, among low grass and other vegetation, with one bloom showing in plants which appeared to be separated approximately two to four feet apart, no big clumps as we know in other dwarfs. The field was spotted rather evenly all over with single blooms.

Bloudowii although considered as a form of I. flavissima, is of an entirely different character than arenaria or flavissima. Its bloom is more thick and stiff, more coarse in every way, as is the stalk and leaves. It lacks the finish or daintiness of arenaria. It appears to be harder to grow, and I have difficulty keepint it alive.

All ot this group are diploids with a complement of 22 chromosomes or two sets of 11 chromosomes. It has been claimed by most authorities that arenaria is a form of I. regelia, this conclusion based on the fact of its creeping stoloniferous roots and the aril or that white spot on the seed. Yet on the other hand we find more characteristics that resemble the dwarf section than favoring the regelia group. For example its early blooming, its samll size, its wide, rounded horizontal falls, it clear yellow color and lack of anthocyanin colors, its absence of dark beard and brown signal on falls, its lack of crossability with other iris groups, and finally its chromosomal differences.

Simonet found the chromosomes of arenaria and flavissima so different from the regelias that he removed them from the Regelia Section and put them into the Psammiris of Spach, a group in between the Pogoniris and Regelia of Dykes. However in spite of any possible argument as to classification, we do know that Bryant Fitch crossed arenaria with the regelia Korolkowii and obtained a small hybrid about 6 inches tall showing the brown beard and brown signal spot over a near-white base color that is very beautiful. However it is entirely sterile, which it should not be if it is a Regelia form.

One thing I should mention here is regarding germination. The seed of Arenaria should be planted immediately upon ripening, for if they are allowed to get hard and dried out, they will not germinate the following spring but will wait for two years to appear. For this reason handling arenaria seed are rather difficult. For that matter I find many reports of growers finding it hard to keep arenaria alive. Here I have no difficulty, it grows like a weed.

I grow it in open rows in the field, in a rich sandy loam, in full sunlight and give it no thought. But each year I lift it, separate and transplant it, early enough that it gets established before winter. It tends to die down in late fall so that it appears to be almost dying, but each spring it grows and blooms profusely and is entirely hardy. A bit of compost mixed with the soil will do wonders for performance.

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BRINGING DOWN TB COLORS TO THE DWARFS

Due to the fact that the tall bearded iris have been so fully developed to where their wide color and pattern range is regarded as the standard for such matters, it is understandable that our first thought in a comprehensive breeding program should be to duplicate everything now found in these talls. Such an achievement is naturally very desirable. At the same time we have certain things in the dwarfs which could greatly expand the range in the tall bearded iris.

For instance we have a clear blue in pumila that is presently unknown in the talls. The dominant whites of talls originated from an inhibitor originating in the dwarfs. The fine things derived from Paul Cook's Progenitor line derived from an inhibitor for standards only, now known to come from I. mellita. No doubt some other contributions have added much to that extensive array of colors and patterns now known in the talls. And perhaps the dwarfs have some other as yet unkown factors to contribute to the talls.

But we have of the dwarf iris interest also concede that the talls have something to contribute to our field if or when we learn how to transfer these genes into the composition of the dwarfs. So the matter of ways and means is our chief problem.

Due to the differentiation of numbers and kinds of chromosomes in the talls and dwarf, at least some of them, it has produced a barrier to some of our plans. However we have hopes that this is only a temporary situation and that with improved methods we can overcome this barrier. Sometimes when you can't win by a direct attack, you can achieve a victory by detouring around the obstacles. We do not recognize anything as impossible.

Today there are only a very few things which we lack in the dwarfs which we hope to gain from the talls. Until very recently the plicata pattern was one of these objectives, but at present we have finally solved this problem to our satisfaction, at least to the point of knowing how to get dwarf plics, though as yet the full rewards of our guest are just beginning to show.

Right now what appears as our most difficult problem is getting a tangerine pink dwarf. Several of us are working on this project from different angles, and wondering who will be the first to succeed.

I mentioned the lack of homology between the tall and dwarf chromosomes as the major deterrant in transferring colors and patterns in either direction. Another obstacle lies with the different inhibitors existing in the dwarfs.

Not long ago we were suggesting the use of I. bosniaca as a medium of exchange of factors on the premise that homology existed with talls and the reichenbachii forms. Just recently we have learned that bosniaca contains an inhibitor for the whole flower and thus it is incapable of use where TB anthocyanin pigments are concerned. Perhaps I should say it is incapable if use with TB anthocyanins when this inhibitor is present in the hybrid. For in advanced generations of such hybrids one can expect the loss of this inhibitor through secregation, where certain proportions of the seedlings will be unaffected.

Naturally this general rule will hold for I. mellita also, as it contains an inhibitor for standards only. So that leaves a choice of either of these, for our work in bringing down to dwarfs any of the anthocyanin colors of the talls.

As yellow pigments are unaffected by these inhibitors one can use either mellita or the reichenbachii forms for this work.

Therefore from our present knowledge we are recommending the use of I. mellita or I. reichenbachii for bringing down the tall bearded colors to the dwarfs.

With diploid talls the procedure is very simple, merely cross the tall parent with the dwarf, then sib cross these seedlings.

But with tetraploid talls it is not such a simple matter. The FI generation of seedlings will contain two sets of TB chromosomes and one set of mellita for example. As the two sets of tall will have a greater affinity we can expect them to pair, leaving the mellita chromosomes as univalents or unpaired elements. Thus these mellita chromosomes will eventually become lost and it reverts to predominantly tall bearded again.

So we have devised a method whereby it should be practical to transorm this TB tetraploid into a diploid form, then we can proceed to carry on by crossing with mellita and finally by sib crossing these seedlings.

Our method is as follows. First we cross the TB with I. attica, the resultant seedling will have two sets of TB and one set of attica. As the attica chromosomes are non-homologous with those of talls there is no pairing or exchange of factors with talls and hence the attica chromosomes will remain as univalents.

At gamete formation , the pollen and ovules will receive half of composition of this cell, namely one set of tall and about half of the set of attica chromosomes. Thus it will lose about four attica chromosomes, and leaving about four in the composition. Different seedlings will get different groups of chromosomes, so that in some of these seedlings the factor forflower color will be lost entirely. That will leave the gamete with only TB factors for flower color. This it will breed as a diploid TB for flower color.

Having changed the tetraploid to a diploid for the factor of flower color, we now cross this seedling with mellita. The tall parent will give one set of chromosomes which we indicate as (T), and mellita gives one set which we indicate as (M). So our new seedling has a composition of TM. This is now a normal diploid with the addition of about 3 or 4 chromosmoes of attica, but for flower color, the factor which concerns us at this time it is a regular diploid.

Tall and mellita being homologous, the segregation will produce the usual Mendelian ratios for diploids. Therefore when we sib cross these TM seedlings, the following generation should produce one TT, two TM, and one MM. Naturally this TT seedling is pure TB for color, having lost all of the color factors of mellita and attica.

At the same time the factors for flowers color are segregating in this manner, other factors for size, season, height, etc. are functioning in the same manner. In this way we can expect to obtain some seedlings having a composition of TT for flower color but MM for size or season of bloom and perhaps TM for certain other factors perhaps for sickle shape leaves or beard color or shape of spathe valves. In fact each seedling will show a different combination of factors, so that we can go up the row and select just about any type or combination that we wish if enough seedlings are grown.

In this way one can be certain of bringing down to the dwarfs any color or pattern known in the talls. This will apply to the tangerine pinks as well as to dominant colors such as the blacks, reds, blues, etc.

What we have described above will apply to such dwarfs as are homologous with talls, but in regard to I. pumila we run into serious trouble which is not so easy to explain.

The chromosomes of I. pumila are so differentiated from those of talls that they do not pair regularly and hence the matter of exchange of factors is the critical consideration.

We sometimes hear remarks about the number of chromosomes of pumila that will pair with those of talls, but so far our only apparent proof has been a test by Dr. Randolph, using tetraploid, tall x attica, in which it was found that one chromosome of attica was occasionally producing a trivalent, thus could be considered as homologous with talls.

Fortunately for us, I. pumila carries most of the colors and patterns known in talls, except plicata and tangerine pinks. Therefore it is not so important that we need to use pumila for bringing down tall colors to dwarfs. But due to the true blue color found in I. pumila, it has been the goal of some breeders to bring this blue up into the talls. Paul Cook has worked on this project for a long time but with no success to date. After a couple of back crosses to talls, the blue factors of pumila are lost.

However, if one has the will to work over a long period, and with a systematic method of selection and breeding, I see no reason why this goal connot be achieved. It means chance juggling of chromosomes over which one does not have complete control but by the laws of averages can be expected to culminate in success.

We will suppose that the particular chromosome upon which this blue factor of pumila exists is not homologous with any of the TB chromosomes. In our work our purpose will be to incorporage within the regular TB gene pool a pair of this blue gene from pumila. Only by having a pair of this gene can it be expected to remain premanently.

As selection is a main consideration in this program, we must so arrange it that we can detect and determine when this blue gene is present in a seedling. Therefore we must use a TB recessive white for crossing with a blue pumila. Whenever blue color is present in a seedling we know this blue gene is present.

So we cross pumila blue x recessive white TB and this first generation progeny should give probably blues and yellows. Next we select a blue seedling and cross this back to the recessive white TB parent. This will give us a plant with three sets of TB chromosomes and one set of pumila chromosomes. In this progeny you should get blues and whites.

You have now arrived at the point where selection is of prime importance. You now select a blue and cross with a blue sister seedling. In this cross each parent will contribute only about a half set of pumila chromosomes, that is about 4 chromosomes, thus losing about the same number. But which of the four that are retained will vary with each seedling. If one parent should retain A-B-C-D and the other parent retain E-F-G-H then this seedling would again contain a full set of pumila chromosomes, but we would not expect this to occur very often. All that we are interested in at this time is to retain the chromosome B, which contains that blue gene. So suppose we get A-B-E-F from one parent and A-B G-H from the other parent. That gives us a composition of A-A-B-B-E-F-G-H in the pumila set. Already we have AA and BB in diploid state where they have pairing partners and can remain within the composition premanently along with the TB sets of chromosomes. The univalents EFGH having no partners will soon become lost.

We would hope that this pair of AA chromosomes would not have any factor to detract from our ultimate goal of a TB with pumila blue color, but it may. In which case further crossing to either of the immediate parents gives another chance of a different combination.

The point to be observed here is that through a process of proper selection and line breeding, we can hope to reach our objective. We cannot say exactly which generation or how many generations it will take because just like dice or cards, it depends on how they fall.

This is what I meant at the start when I said one way to win was to bypass the barrier. If we can't incorporate it into the TB sets, we can add them to the normal complement as additions.

One can even back cross one of these blue seedlings to the original recessive white TB parent and then sib cross again and further purify your blood lines. In fact, although the chances diminish the further we stray from this point, it is possible to get this B chromosome into a tetraploid state along with the other TB sets, through proper selection and breeding.

For you members who like to work on cross-word puzzles or tackle challenges, try this out for a real workout. You will find it most exciting and worth your efforts.

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WHAT IS A PLICATA?

To the question, what is a plicata, we can readily give an answer as to its visual expression, for it is described as a white with a border or edging of anthocyanin veins, stitiching or stipples aroung the standards and falls. It may also come in yellows with anthocyanin edgins. But of the genetical aspects of poicata, we know very little for a certainty.

Several of the experts of the past have given their views on the subject, but as yet no one has arrived at a difinite conclusion as to the nature and inheritance of this pattern factor.

In AIS Bulletin #43, April 1932, a series of discussions on plicata inheritance was presented by Miss Peckham and Mr. Bliss. which is interesting. In Bulletin 106, July 1947 we have an entire number dedicated to plicatas. In Bulletin #123, October 1951 Dr. Sturtevant proposes some theories concerning the plicata factor, offering alternate theories of an independent factor and multiple-alleles. However, he does appear to favor the idea of plicata being one of a series of multiple alleles.

According to the multiple allele theory there are three alternate genes in one series, namely blue self, plicata, and recessive white. The blue is top dominant over the other two genes and white is the end recessive. That leaves plicata as recessive to blueself but in the absence of blueself becomes dominant over the recessive white.

To understand the merits of the two different theories we must first establish a working basis of the possible genetical foundations. As tetraploids are much more difficult for handling ratios and the necessary number of seedlings, it simplifies the matter greatly to work with diploid forms, and also makes the diagnosis and explanations more simple.

Before we start theorizing we whall make some assumptions which I think are quite obvious to anyone. First a plicata must have the factor for anthocyanin formation, as certainly it is present in the plicata stitiching or edging. Hence we can assume that the gene or factor for blue color is present.

The next fact is that the design or arrangement of the plicata edging or stitiching is the result of waht we may call a pattern factor.

And finally it is generally agreed that the plicata expression is a recessive character, and if this is true, then the controlling gene must be in a homozygous condition.

With these three assumptions as a basis we can not begin to analyze and reason out the possibilities of the situation.

Our first conclusion would be that the production of blue pigment is controlled by genes or factors in a strictly color series of alleles. And as white is merely the absence of color pigments, it is quite obvious that this color series comprises the colors blue and white as alternate genes.

The distribution of this color is another matter. The normal expression of color is an all-over pattern we have found, covering both standards and falls. Thus any deviation from this norm must be the result of a different gene or factor, either acting in conjunction with the gene for self pattern or as a separate gene functioning in the absence of the self pattern gene. Hence this plicata gene could be an allele in the same pattern series with self pattern, or it could be an independent gene in a different series, acting as a distributor of anthocyanin pigment.

However in either case, we can be sure that the factor for color and the factor for pattern, are in different series, and hence plicata expression is in fact a combination effect of two separate and individual sets of genes, namely pigment or color and pattern genes.

To summarise these conclusions we will recognize the color series as a pair of alternate genes, indicated as (B) for blue color and (b) for its recessive allele which is white. Then we will identify the pattern series as a capital (P) for the self or all-over pattern and (p) for the plicata pattern.

Here we have a case of two independent series of alleles, each with a pair of alternate genes, For some reason I am unable to conceive of how one can arrive at an explanation of a series of multiple alleles in this case. Unless one takes the position that plicata is merely an allele in the color series, thus explaining that blue self, plicata, and recessive white are alternates in the one color series, and ignoring entirely the matter of distribution of color by a pattern factor.

One of the first things which caused me to feel doubts about our present theory of plicatas, was the usual assurance that the white part of a plicata is recessive white. If this were true then crossing a plicata with recessive white should give whites but instead it gave blue selfs.

So I began to figure out hypothetical crosses and what to expect. If blue selfs can come from plicatas then it must be inherent in the plicatas. Hence if blue pigment and plicata pattern are both necessary constituents, it is obvious that both genes must be present in plicata.

Then I tried to work out things on the theory that plicata is an independent gene outside the blue series of alleles. By this theory a plicata is simply a blue iris basically, with a recessive pattern gene acting as a modifier or distributor of anthocyanin pigment, placing it around the edges rather than all over the flower. When the pattern gene is absent, or in less than a homozygous state, it reverts to blue. Therefore a blue may have one dose of p and still remain a blue self. Or a white may have p or pp and because no blue is present it remains white .

Thus the possible gene compositions may be BBPp-BBpp -BBpp- BbPP-BbPp-BbPp- Bbpp-bbpp-bbPp-bbpp. Naturally anthing lacking B will be white. Anything with BB or Bb but with less than two doses of p will revert to blue self. A plicata may be BBpp or Bbpp.

Only if the blue factor were in a heterozygous condition could we expect to obtain whites in breeding. This has been borne out in some of my progenies which I will indicate further on. It must also be recognized that a recessive white may also contain one or two doses of p, but because no blue is present, it connot give expression.

Although I will admit that the progenies of experimental crosses I have grown are not yet sufficient for any conclusive evidence, I do think they may help to support the above assumtions. Unfortunately only a few of each progeny bloomed this spring, I hope to obtain some more satisfactory rations by next blooming season. But anyway I will offer what is now available.

Crossing plicatas with plicatas gave only plicatas, no whites or blues. Intercrossing the recessive white parents gave all whites. I. pallida x Columbine a recessive white, gave all blues, indicating that this form of pallida is homozygous for blue. I might add that the recessive whites used in these expreiments came from (Widget x Daystar) x ?) and thus apparently may contain the plicata gene. Where I use the term plicata gene I have reference to the recessive pattern gene p.

One cross of Mme. Chereau x diploid recessive white gave 4 plicatas, 5 blue bitones, no whites. Another cross of diploid plicata x recessive white gave 5 blues, 2 plicatas, no whites. Whence came the blues? It would appear that when a seedling lacked sufficient dosage of plic gene, it reverted to blue. As one dose of blue is sufficient for the anthocyanin expression, and as reduction of dosage of plicata gene would be expected to occur in an outcross to recessive white, I can only assume that perhaps this recessive white must also contain one gene for plicata, which combined with the plic gene from Mme. Chereau to give the required two doses of plicata.

An unexpected segregation occurred in a cross of diploid blue x recessive white. Here I obtained 4 blues, 2 plicatas, no whites. If the blue had been heterozygous for white, that is one dose of blue with one dose of white, this crossed with recessive white should have given some whites. If the blue parent were homozygous for blue, that is two doses, crossed with recessive white should have given all blues, no whites, no plicatas. But the fact that it gave no whites but did give two plicatas, would certainly seem to indicate that some independent gene outside this allelic series was contributing to the whole. My explanation of this would be that both the blue and the white parent each contained at least one dose of plicata gene. Yet each patent could still be homozygous for blue or white respectively.

It has been claimed in some cases with with tetraplid forms that plicatas have been obtained from crossing two recessive whites. This apparently has not occurred with diploid recessive whites. But we also know that occasionally blue slefs have been obtained from crossing two tetraploid recessive whites, which has not occurred in the diploid whites. So isn't it quite possibe that if or when these recessive whites may throw a blue segregate, and they may also contain the plicata gene, a plicata can then occur. I can understand that it is entirely possible for the tetraploids to have a somewhat different and more extensive combination of genes than the diploids, and thus may give a wider range of variations of the characters, but for basic study of the plic factor the diploids can more easily furnish the necessary evidence.

We might suppose that in tetraploids this is a case of complementary genes for blue color; one white having one of the genes, the other white containing its complementary gene. When they are combined blue color is produced. I have had reports that Matterhorn x Jake has produced an occasional blue.

Another progeny bloomed this spring which may add some help on this plicata problem. For some time I have recommended to our robin members the use of I. mellita in bringing down plicatas from the talls to the dwarfs. This was proposed on the assumption that tall and mellita chromosomes are sufficiently homologous for regular exchange and segregation.

I first crissed Mme Chereau x I. mellita, and also with rubromarginata. In both progenies I obtained a variation of purples and rather muddy blends, and a few showing the bicolor pattern with creamy-white standards and purplish falls. This latter was obviously due to the inhibitor for standards, now known to exist in mellita.

I next sib-crossed these seedlings and they bloomed this spring. It was a small progeny of about 18 seedlings but among them I found one blue plicata and one yellow plicata, the remainder being blue and white bicolors and purplish selfs. This would seem to indicate normal segregation of recessive character for a dihybrid.

Another problem which has been confusing to many persons is the fact of getting a plicata from crossed of tetraploid plicatas x Cretica, the latter presumed to be a form of I. pumila. Some of our authorities immediately concluded that Cretica must contain the factor for plicata.

In this cross we would naturally assume that the usual pumila inhibitor would erase the anthocyanin color of the TB parent but evidently it didn't, so we have now concluded that Cretica lacks this inhibitor. But we still had to explain getting a plicata in the first generation seedlings.

In a homogeneous plant it is presumed that a recessive character must be in a homozygous state to give expression. I think this is where the experts were confused. In this hybrid of TB x Cretica we have an amphidiploid, or what we call a couble diploid. The two sets of TB chromosomes are functioning independently as a regular diploid, and thus would have the required factors for color and pattern of plicata. At the same time the two sets of Cretica chromosomes are also functioning on an independent basis. We know that Cretica will occasionally throw a recessive white segregate, and thus is giving expression as a white. When the double expression of plicata from one half of the plant, and recessive white from the other half, gives expression simultaneously, The white being neutral will premit the plicata pattern to show as a visible expression. To me it seems that simple, as an explanation of this confusing problem.

It is interesting to note that Bliss in an article in Garden Chronicle, Feb. 14 & 21, 1920, states, "Plicata froms when crossed with certain neglectas and squalens carrying plicata, the expected Mendellian ratio of one-half plicata form is obtaines" He goes on at great length discussing the possible origin of plicata and its distinctive characteristics deviating from normal pallida types, but finally settles for an assumption that plicata type has arisen as a mutation from pallida by dropping out of a single factor or set of linked factors. He also poses the possibility of plicata being perhaps two or more cumulative factors. And he particularly points out what he calls the plicata beard, which apparently is always associated with the plicata pattern.

To quote Bliss: "The typical beard, which is very different from either pallida or the variegata beard, and which is clearly recognizable even in squalens-plicata crosses, is carried, practically intact, through generations of transition seedling, in which it has disappeared with the other colour characters of plicata, reappearing again unaltered in the following generation in all the seedlings of the plicata (colour) type. In more than 100 seedlings from one cross, all of which were plicatas, all had the typical plicata beard. Yet four of the six parents in the ancestry (so far as known) of this cross were not plicatas."

As we have no tangible evidence as yet to the origin of the plicata factor, other possibilities than the mutation theory may be considered. We know of no form of pallida which has ever given plicatas, only that they appeared in hybrids involving I. pallida and I. variegata. A study of the variations of the veining in I. variegata in advanced generations of its hybrids might suggest that plicata could originate as a modification of the self pattern of pallida when the two were combined. In other words it would act as a compromise effect between the two opposing expressions. But I think we may be certain, that plicatas appear only in hybrids which originally derived from crosses of I. pallida and I. variegata.

Then we have the problem of the yellow ground plicatas. It is apparent that this plicata gene is always associated with anthocyanin pigment, or we might say it is acting upon anthocyanin only, as we know of no such pattern in yellow. It is also obvious that it is not necessarily an allele of self pattern as a general manifestation, but rather suggests that self pattern is the normal expression of any color and plicata factor is a separate and independent gene acting upon the anthocyanin pigment only, as a modifier or distributor. Ans as blue pigment is a necessary component in the plicata expression, a yellow groung plicata is therefore basically a blend, and will revert to a blend when the plicata factor is absent.

The above attempt to analyze the inheritance of plicatas is not meant to be a conclusive or final decision. Until our experiments are further developed this serves merely as a tentative report and some suggestive thoughts upon the problem. I hope it will be the means of encouraging other tests by our members, and make it possible to finally settle this matter. I will appreciate any comments by anyone who may have other experiences to contribute.

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DWARF IRIS REGISTRATIONS 1961

Due to publication of the Portfolio before the end of the year some registrations of 1960 were unable to be included in the last issue, therefore these late ones are shown here along with the 1961 list. Thus the following are 1960 registrations:

CHAMAEIRIS CAMPIGLIA (Randolph) 1-22H (Campiglia, Italy) 7 in. Medium purple self, tan beard, 40 chromosomes.
CHAMAEIRIS LE LUC (Randolph) F-2G (Le Luc, France) yellow self, 40 chromosomes.
PUMILA MUNICH (Randolph) M-10B (Munich Bot. Garden, Germany) 6 inch, greenish-yellow self, 32 chromosomes.
ATOMIC BLUE Correction; name changed from Little Bit which was not available.

REGISTRATIONS FROM JAN.1 to NOV. 13th 1961.

ALREADY (Warburton) C-539 (Welch G-504 x G-509) x unknown x Red Dandy). 6 inches. Deep purple-red self, beard same.
DAINTY BLUE (Berndt) (Fairy Flax x Lilliput) 4 inches. Light blue self, white beard.
DIZZY DAME (Welch) K-510 (San Francisco X pumila) x (Cook-1546 x Carpathia) 6 in. S. Chartreuse Green, F. Olice-green blend, white beard.
FAIRY DELL B. Jones) 93-1 (Doriot A25-25 x (Sulina x Welch L-545) 6 in. Heliotrope self, blue beard.
FIFTH WHEEL (Berndt) 113-F (Fairy Flax x Lilliput) 5 in. Sky-Blue slef, white beard.
GAY FLIRT(E.Roberts) B-501 (I.flavissima x Blazon) 9 inches. Golden yellow self, yellow-orange beard.
GOLDEN CUPID (Alta Brown) M-67B (Baria x Orange Glint) 9 in. Buttercup yellow self, orange beard.
ORCHID SHEEN (Welch) M-503 (I. pumila x I. pumila) 5 inches. S. Orchid pink, F. deeper orchid falls.
POLKA DOT (B.Jones) 90-7 (Fairy Flax x (Sulina x Welch L-545). S. white, F. white with navy blue spot.
TRIOLET (Soper-Eva) 1-61 (From two pumila sdls) 6 in. S. cream, F. cream, purple-madder spot.

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NEW DIS MEMBERS FOR 1961

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E. Harold Blackner, 2633 Seneca St., Flint 4, Michigan
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Please pay your DIS dues directly to our secretary, Mrs. Lee Armiger, 24300 Edgemont Road, Southfield, Mich. Dues are continuing to come to me at Middlebury and it will save extra work to mail dues direct to Lee Armiger. Thanks

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