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Hunt Institute was dedicated in 1961 as the Rachel McMasters Miller Hunt Botanical Library, an international center for bibliographical research and service in the interests of botany and horticulture, as well as a center for the study of all aspects of the history of the plant sciences. By 1971 the Library's activities had so diversified that the name was changed to Hunt Institute for Botanical Documentation. Growth in collections and research projects led to the establishment of four programmatic departments: Archives, Art, Bibliography and the Library.

July 20, 1965

Dr. Richard Evans Schultes
Botanical Museum
Harvard University
Cambridge 38, Mass.

Dear Dick:

You should receive within the next two to three weeks a paper on the florist's cineraria, submitted by Dr. Ted Barkely, Dept. of Botany Kansas State University, Manhattan. I happen to know about this one because Ted sent it to me, asking whether it would be appropriate for EB. I told him that the final answer on such matters rested with you, but I encouraged him to submit it. Ted x is a former student of Art Cronquist and got his PhD here several years ago. His thesis was on a section of the genus Senecio, and his interest in the florist's cineraria is something of an outgrowth of his interest in the genus, with some encouragement on my part to do something with economic plants.

I hope you find the paper as interesting and as useful as I did. Whether it fits a definition of economic botany or not is, of course, dependent on how one looks at the subject. While it does, ~~fit~~ to some extent, fit into a horticultural framework, I think it also fits as well with us. In either situation, it is an acceptable and useful contribution. I made one suggestion on the manuscript as it was being prepared, and that was with the part dealing with the actual economic aspects of the plants. Barkley could not include much because he has had difficulty in getting commercial growers of cineraria to divulge that type of information. He has in his own files some undocumented information on the income, etc., derived from sales of cineraria, but he has been unable to get confirmation of the accuracy of these figures, and has, rightly, excluded this from the paper. Other than in this small part, I have not had anything to say to him about the content of the paper.

Hope you can see fit to accept it.

Sincerely,

David J. Rogers
Curator of Quantitative
Taxonomy

Kansas State University

Manhattan, Kansas 66504

Department of Botany and Plant Pathology
Dickens Hall

July 16, 1965

Dear Dave:

Your letter regarding the Cineraria opus has just arrived. Thanks muchly for looking over the manuscript and giving your comments. The local editorial committee returned it a day or so ago with only grammatical and sentence-structure changes suggested. Usually the stuff I write comes back chicken-tracked in red. I am leaving Monday morning for a Senecio collecting excursion to New Mexico and Arizona with one of the taxonomy grad students, and while I am gone my secretary can make a respectable manuscript out of the marked copy here. Then, should I send it to you for transmittle (is that spelled right?) to R.E.Schultes, or should I send it to him directly?

I certainly do agree that the story could be made much more interesting by adding some of the stuff that I don't know for fact but which related to the horticultural practices used in raising cinerarias. You know though, commercial flower growers and florists who sell the flowers are an extremely tight-lipped and clannish bunch. It has been most difficult to learn anything from them, they seem to have an almost pathogenic fear of having someone find out that they are making any money. Only two commercial growers of any size have ever supplied me with much good information, one was (or is) a fellow named Ziegler in Hatfield, Pennsylvania, and the other is the breeder for Ernst Benary in Germany.

The Geography and Road lay-out of the Great American midwest is such that a sojourn through Manhattan (the Pearl of the Plains) and through Wichita on the way to Colorado is not at all difficult. Manhattan is about 2-3 hours from Wichita.

Cheers, Ted

Kansas State University

Manhattan, Kansas 66504

Department of Botany and Plant Pathology
Dickens Hall

July 8, 1965

Chas
7/14

Dr. David J. Rogers
New York Botanical Garden
Bronx Park
Bronx, New York 10458

Dear Dave:

Here is a trial-run for an insert into the cineraria paper to make it more "economic." The paragraph is intended to go between the first and second paragraphs on page one of the manuscript.

I really don't have much information on the strictly economic aspects of the plant, and what I have was mostly acquired by corresponding with nurserymen and talking with florists. The people in horticulture here at K-State have told me that there are virtually no records and statistics kept in the floricultural businesses. Anyway, the enclosed paragraph just about summarizes what I know for fact about the current status of the business.

An area of interest that I cannot chase down accurately is just who supplies the seed at the present time. Most commercial growers purchase seed annually from one of the big seed firms (e.g. Ball, Vaughan's, etc.) but a few maintain their own seed-producing cultures (e.g. Geo. Didden Greenhouses). In this country, there is only one grower, as far as I know, who makes a business of breeding cinerarias for seed, and that is A. S. Siter & Son. Thus far this company has never replied to any of my correspondence. Whether or not cinerarias are a major interest with them or just small-pickings, I don't know. The impression I get from the people at Geo. J. Ball is that most cineraria seed is supplied by European breeders. There are only a few major breeders, the largest of which (I think) is Ernst Benary, in Hannover. I have corresponded quite a bit with the Benary firm, but have not really learned much about the seed-trade routes. The actual production of seed is a fairly laborious but not a very precise process. The breeders hire bee-keepers to place bee hives outside the greenhouses, then on warm days, the greenhouse windows are opened and the bees do the cross-pollinating. This precludes controlling what crosses with what, but it insures a fairly high seed-set. Although the bees are attracted to the cinerarias, the cineraria pollen is an insufficient diet for them, and they must be fed a supplement.

The above yak-yak is stuff that isn't complete enough in detail for inclusion in the paper. For all I know there might be many more breeders and seed suppliers tucked away in some obscure corner.

When the enclosed paragraph is put into the manuscript, the paragraph that follows it will have to be adjusted by scratching the word "florists" and the stuff in parentheses from the first sentence.

Please let me know how this paragraph "sets." I hope it puts the paper a little more in line with what is expected.

With best regards,

Ted

P.S. Stop by on your way to Marlboro Country

At the present time, the florist's cineraria (hereafter referred to simply as cineraria) enters the florist's trade as a pot-plant in late winter and spring. A potted cineraria usually retails for between two and four dollars, and if reasonably well cared for will persist for at least three weeks. Conversations with florists and nurserymen indicate that cinerarias are currently not as popular in America as they were in the past. There seems to be no particular or overriding reason for this, other than a change in the public's tastes. A related factor may be that the plants are quite sensitive to temperature and will survive best if the temperature does not exceed 65°F., which is somewhat cooler than most Americans prefer to keep their homes. Most growers raise cinerarias in conjunction with other florist-crops, with very few, if any, making cinerarias their prime concern.

July 2, 1965

Dear Dave:

Thank you very much for your letter regarding the Cineraria opus. I am scratching around for a paragraph or two to make the botany a little more economic, but with middling success. I'll send some "additions" as soon as they are scribbled out.

I have all sorts of "general" information regarding horticultural practices and marketing procedures, but statistics are hard to come by. But, have faith! We shall see! Senecio shall prevail!

Again, thanks. I'll get something whipped up very shortly. Come see us —

Ted.

Kansas State University

Manhattan, Kansas 66504

Department of Botany and Plant Pathology
Dickens Hall

June 23, 1965

Dear Dave:

Here is a carbon of the cineraria opus. The original has been sent to the local experiment station editor for review, as per local custom; I thought sending on a carbon to you might expedite matters.

Included is a table, paged 21-22-23, but it should go in where indicated on p. 4. There is one plate, illustrating four plants, but this is being kept here to send to the experiment station editor.

As soon as the local editor and reviewers are through and an experiment station number has been assigned, I'll send a clean-copy original - OK?

Thanks a lot.

Best regards

Ted

answered 6/28/65
need more [illegible]
plant for EB.

A Review of the Origin and Development of the Florists' Cineraria, Senecio cruentus DC., et al.[1]

T. M. Barkley[2]

In the latter part of the Eighteenth Century, Francis Masson was sent out from Kew Gardens as a plant collector. While in the Canary Islands, he collected several endemic species of Senecio, some of which were introduced into cultivation at Kew in 1777, and others in 1780. It is from these plants that the complex assemblage of cultivars known as the "florists' cineraria" has been derived (Aiton 1789, L'Héritier 1789, Gilmour, King and Williams 1963).

It is the purpose of this paper to review the history of the origin and development of the florists' cineraria (hereafter referred to simply as cineraria). The attraction for studying the group is two-fold; first, its developmental history has not previously been compiled, and second, the group is nearly unique among well-developed cultivated complexes in that it is derived from a quite restricted group of insular endemics, all of which can be explored and studied experimentally. An investigation of the wild progenitors and the cultivated derivatives is currently in progress, and hopefully it will yield a more complete understanding of how a restricted gene pool can give rise to such a vast array of variation in cultivation.

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[1]Contribution No._____, Department of Botany and Plant Pathology, Kansas Agr. Expt. Station, Manhattan. Botany Serial No. 841. Research supported in part by the Kansas State University Bureau of General Research.

[2]Department of Botany, Kansas State University, Manhattan, Kansas 66504.

This paper is based essentially on the literature and a review of pertinent herbarium specimens (in hbs. NY, MO, US, Goucher College). It also incorporates the numerous subjective impressions gained while dealing with living materials of both the wild progenitors and the cultivated cinerarias in connection with an experimental breeding program. The findings of the experimental studies are to be reported in a later publication.

The Progenitors

The ancestors of our present-day cineraria are members of the section Pericallis of the genus Senecio (Compositae) (Hoffman 1889, Muschler 1909). All members of this section are insular endemics, restricted to the Canary Islands (11 species), Madeira (1 species) and the Azores (1 species). The members of section Pericallis are distinguished from other senecios by possessing palmately-veined basal leaves, by the absence or near-absence of calyculate bracts, by having usually violet-purple to pink or white, or even ochroleucous flowers but never distinctly yellow flowers, and by their restricted distribution. Most members of the section are rather coarse, tough-based erect herbs, with more or less persistent basal parts which annually send up flowering stalks with one or more open racemose to paniculate inflorescences. One species, Senecio heritieri is exceptional in that it is subshrubby and produces flowering heads individually on separate peduncles. Furthermore, all members of the section except S. heritieri have the entire corolla of each ray floret colored uniformly or nearly so throughout. In S. heritieri the outer portion of the ray corolla is colored while the inner portion is white, giving the head a distinctive appearance. This condition is termed "eyed" in the horticultural literature. None

of the Pericalloid Senecios are particularly attractive plants when grown in pots. It is a tribute to some forgotten gardener's insight into the ornamental potentialities of these plants with ~~that caused us to have~~ The cineraria complex of today.

Nomenclaturally, the Pericalloid Senecios have had their woes. The earliest botanical treatment of members of the group was done by Charles-Louis L'Héritier, who in 1786-1787 studied living materials at Kew Gardens, and dried materials in the herbarium of Sir Joseph Banks. L'Héritier described eight species that are currently regarded as Pericalloid Senecios under the generic name Cineraria in his remarkable and fascinating book, Sertum Anglicum (1788). In 1837, DeCandolle transferred the group to Senecio making a few nomenclatural changes at the specific level and adding one species.

The occurrence of the taxa under the generic names Doronicum and Pericallis is a little more involved. Apparently in the preparation of the manuscript for Webb & Berthelot, Phytographie Canariensis, Webb transferred some species of Cineraria (i.e., Senecio) into Pericallis, however, he did not publish these transfers. Eventually, the final treatment of the Compositae for Webb & Berthelot was written by Schultz-Bipontinus, and he transferred some species from Cineraria into Doronicum, while leaving some in Senecio in accord with DeCandolle. However, in synonomy Schultz-Bipontinus published Webb's Pericallis combinations. Regardless of the nomenclatural validity of these names, they have been widely used and indexed.

The question of generic disposition for the Pericalloid senecios must await a taxonomic revision of the group. Until such a time, it is doing the least violence to both the understanding of the biology of the group and to history to maintain DeCandolle's interpretation and

treat them as members of Senecio.

The Pericalloid Senecios have been treated floristically by numerous writers, especially in the mid and late 19th century. Ecological and distributional data are summarized in the works of Webb & Berthelot (1836-1850), Lowe (1858), Masferrer (1881), Christ (1885, 1888), Bornemueller (1904), Pitard & Proust (1908), and most recently, Lems (1956, 1960). It is from these publications as well as herbarium specimens that the accompanying table has been compiled.

Table here

The Modern Cineraria

The cineraria, which is a horticultural derivative of the Pericalloid Senecios, has the morphological marks of its ancestry and quite properly belongs to the section Pericallis. It encompasses much more variation than is shown by the totality of wild Pericalloid Senecios. In stature and gross appearance, cinerarias vary from dwarf (1 foot high), compact, pincushion-like plants with the flowers arranged in a dome shaped "truss" immediately above the leaves, to rather tall (3 or more feet high) open plants with loose, pyramid-shaped inflorescences. The herbage tends to be glabrous or glabrate at maturity, however, the undersides of the leaves are pubescent when the plants are young. The flower heads are typical for Senecio, and except for teratologies, vary mainly in size and corolla color. There is some variation in type and abundance of glandular hairs on the involucral bracts but this remains to be investigated. Size of the heads varies with the cultivar, but the heads of most cultivars are between 3 and 5 cm. across. The corolla color intergrades completely from white to red to magenta-blue and deep purple. Occasional ochroleucous corollas are seen, but no truly yellow cinerarias are known. The disk corollas and the rays may be the same

color, or the disks may be white or ochroleucous while the rays are colored, in which case the head is said to be "gold centered." With ray corollas, the tips may be colored and the inner portion white; such heads are reminiscent of S. heritieri and are said to be "eyed." The converse, i.e., white tips and colored inner portion of the ray, is unknown. Abnormalities which have been maintained in cultivation include double forms with more than one row of ray florets to "double" forms where the entire head consists of ray or abnormal ray-like corollas.

Whether or not the cineraria should be called Senecio cruentus as it is in much of the horticultural literature is a moot point. The wild S. cruentus, although unquestionably involved in the ancestry of the cineraria, is so different in gross aspect that it and the cineraria need never be confused. Furthermore, if the cineraria were to occur in wild, natural populations in the Canary Islands, the presently employed theories of taxonomy would virtually dictate that it be treated as a distinct taxon. It is significant to note that the cineraria is not a wild plant, and it has no populational integrity of its own. Therefore, whether or not it should bear the name of its most conspicuously immediate ancestor, or should be named independently is a theoretical question that cannot be solved either by experimental procedures or by the application of a code of nomenclature. The fundamental implications of this question are seldom aired and are far from settled. But, the fact that the cineraria as a complex is distinctive from any of its progenitors strongly suggests that it should be named independently, in which case the oldest available name is Senecio hybridus (Willd.) Regel, based upon Cineraria hybrida Willd.

The accompanying photographs are of plants grown from seed in the greenhouses at K.S.U. The two wild Canary Island natives (Senecio

echinatus and S. papyraceus) were both sown in October, 1963, and grew as rosettes during the winter of 1963-64. They were kept in pots in a lathhouse during the summer of 1964 and were returned to the greenhouse in early October of 1964. Almost immediately thereafter S. papyraceus began to flower, and it has been flowering continuously up to the present (May 1965). Senecio echinatus, on the other hand, began flowering the first week of May in 1965. Both S. cruentus and S. heritieri were grown in 1963-1964, and both flowered in May of 1964, however S. heritieri did not survive the summer, while S. cruentus survived only as severely debilitated rosettes which have not flowered again. Unfortunately, adequate photographs were made for neither S. heritieri nor S. cruentus.

The two cinerarias are stunted by comparison to commercially raised individuals. The stunting was quite intentional; it was done by supplying a minimal amount of nitrogen to the plants in the rosette stage. As greenhouse space is limited, this allows more plants to be grown in a small area. Experimental evidence demonstrates that withholding nitrogen in the rosette stage has no effect on the plants other than stunting the gross stature.

Plate here

Early History

No definite record exists as to how or exactly when the cineraria was differentiated from its progenitors. In 1788 and 1798, Cineraria lanata and Cineraria cruenta (Senecio cruentus and S. heritieri, respectively) were illustrated in Curtis' Botanical Magazine, and such good advertisement could only have increased interest in the group and helped distribute them throughout the gardens and greenhouses of northern Europe. By 1809, a horticultural complex was sufficiently differentiated so that Willdenow called it Cineraria hybrida to distinguish it from the wild types.

In the early 1800's, progress in horticultural development and differentiation of the cineraria was very rapid. One gains the impression from a survey of the horticultural literature of the period that the cineraria was becoming a prized greenhouse plant. Numerous publications of the time contain descriptions of how to raise the plants; however, scant information was to be had on the origin and development of the cineraria.

A brief article was published by Bouche in 1824 describing the improvement of the cineraria and pointing out that Cineraria hybrida (i.e, the florists' cineraria) and Cineraria cruenta (S. cruentus) are different. In 1826, Drummond published a paper describing the then current horticultural practices regarding the cineraria, and implying that there were numerous varieties of cineraria (cultivars) in existence, some of which were more valuable than others. There can therefore be no doubt that within less than ~~fifty~~ 50 years the cineraria had become a distinctive complex with numerous cultivars.

There is no explicit record of when double-flowered forms were first developed, however, they are mentioned by Burbidge (1877) and are illustrated in the Flore des Serres (1877) and in L'Illustration Horticole (1885). The latter two references include descriptions and imply that their origin was through seed mutations.

A short review of the developmental history of the cineraria is given by Rolfe (1888).

Biological Origin of the Cineraria

Throughout the period of development, many people observed and reported that the Pericalloid Senecios were closely related to each other, and that more than one species may have been involved in the

ancestry of the cineraria. However, there seems to have been a general assumption that S. cruentus was the chief, if not the sole, ancester (Bouche 1824; Journ. d'Hort. 1845; Burbidge 1877).

The earliest definite statement of the origin of the cineraria appears to be that of Focke (1881). He stated that the cineraria originated as a cross between Senecio cruentus and S. populifolius DC. (S. appendiculatus) and that the earliest forms of the cineraria were six named hybrids (bicolor, coelestis, formosa, hendersoni, pulchella, and waterhousiana). Later these were further hybridized with other species from the Canary Islands and from Madeira, namely S. tussilaginis, S. heritieri, S. maderensis, and S. webbii, forming a complex of variation.

Focke's explanation cannot be patently discarded; its simplicity is appealing. It is unfortunate that no further details were given, and no specimens or other documentary evidence was cited. Whether or not Focke's explanation is true in its entirety, or in part, must await experimental studies. Nevertheless, Focke is on record as having regarded the cineraria as a polyspecific complex.

One of the most interesting and informative phases of the history of the cineraria had its beginning in 1895, when W. T. Thiselton-Dyer, the Director of Kew Gardens, attended a meeting of the Royal Society. Apparently there was a discussion of variation and evolution at the meeting, and Thiselton-Dyer was in some manner prevented from making his contribution clear. So he sent a letter to the editor of Nature (Thiselton-Dyer 1895) briefly giving twenty items on variation and evolution which related to the discussion. The eighteenth item dealt with the cineraria. In it, Thiselton-Dyer stated that the wild, Canary Island Senecio cruentus is greatly different from the cultivated florist's

cineraria. He wrote, "I placed upon the table plants of the floral type and of a recent cultivated form of Cineraria cruenta from the Canaries. The difference in the habit and the form and colour of the flowers was enormous. This has undoubtedly been brought about by human selection. As far as is known it has been accomplished by the gradual accumulation of small variations."

This brief statement elicited a response from William Bateson, later to become well-known as a geneticist, giving rise to a debate on the pages of Nature and commentary in the editorial columns of Gardeners' Chronicle. The entire issue is fascinating, for the subject matter bears upon breeding and genetics at a time prior to the rediscovery of Mendelism. Furthermore, it gives an insight into the thinking and personalities of men who have come to be regarded as great in the annals of biology.

Bateson (1895) responded to Thiselton-Dyer with a lengthy letter-to-the-editor, questioning Thiselton-Dyer's assertion that the cineraria was derived from S. cruentus "by the accumulation of small differences." Bateson wondered whether or not the selection of "sports" or seedlings presenting notable or striking variations may not be involved, and whether or not hybridization is a factor. He presented a review of the older literature, and concluded (1) that the modern cinerarias arose as hybrids derived from several distinct species; (2) that the hybrid seedlings were from the first highly variable; (3) that sports (mutants) of an extreme kind appeared after hybridization in early years of the "improvement" of these plants; and (4) that the subsequent perfection of the form, size, and habit has proceeded by a slow process of selection. He closed by noting that Thiselton-Dyer's statement on the origin of the cineraria is misleading, "for this statement neglects two chief

factors in the evolution of the cineraria, namely, hybridization and subsequent sporting."

In essence, the course of the letters which followed established two points; Thiselton-Dyer championed the belief that the cineraria was derived from S. cruentus alone, while Bateson presented a case for the inclusion of several species into the complex. In addition to five letters from Thiselton-Dyer and three from Bateson, the debate also included three letters from W. F. R. Weldon, one from W. Botting Hemsley, and four editorials in Gardeners' Chronicle (cf. references). After dredging up much of the older literature and variously citing it to support different points of view, the debate terminated inconclusively.

Experimental Work

Undoubtedly much experimental breeding had occurred in connection with horticultural attempts to improve the cineraria, however such experimentation did not reach the literature. The earliest purely experimental study to have been reported appears to have been done by Charles Darwin and published in 1877. In this short report, Darwin detailed how he isolated cineraria plants to prevent open pollination, and he noted that the isolated plants produced no seed. If, however, pollen from another plant were introduced to the isolated plant, then seeds were produced. He further observed that some plants are better seed producers than others, stating that "this species shows some tendency to be dioecious."

Seemingly prompted by the Thiselton-Dyer and Bateson debate of 1895, two plantsmen, James and Lynch, initiated experimental studies that have been recorded in the literature.

Rolfe (1898) reported on a series of experiments carried out by a Mr. James, involving the wild S. cruentus and S. heritieri. The

initial cross between these two plants (the f_1) produced hybrids reported as intermediate between the two parents, while the progeny crosses (f_2's) provided a great diversity, some being more like *S. cruentus*, some like *S. heritieri*, and others intermediate. Rolfe reported that in spite of the great variability, there was as yet a considerable gap between these hybrids and the cineraria.

In 1897, R. Irwin Lynch of the Cambridge Botanical Garden wrote a letter to the editor of *Nature*, referring to the discussion of Bateson and Thiselton-Dyer. He said, "It raised in my mind the idea of producing some living evidence on the question," and he requested seeds of all herbaceous species of Senecio from the Canary Islands and Madeira, and appealed to people who may have or be able to obtain such seeds to send them to him. He further stated that the evidence at hand suggests that the species within the group cross readily and are self-sterile.

Also in 1897, Bateson reported on some hybridization experiments which were carried out by Lynch and a Miss Pertz at the Cambridge Botanical Garden. The hybridizations involved *Senecio cruentus*, *S. heritieri*, *S. multiflorus*, and the cineraria, and demonstrated that they would cross with each other, and that the hybrids were highly variable. Bateson concluded that the results were entirely consistent with the view that the cineraria is of hybrid origin, and he stated that *Senecio cruentus*, *S. heritieri*, and probably *S. multiflorus* were among the parental species.

It was in 1901, however, that Lynch himself published the results of his experiments. Altogether he worked with *Senecio cruentus*, *S. heritieri*, *S. multiflorus*, *S. tussilaginis*, and the cultivated cineraria.

He believed that the S. cruentus which he had was not biologically the same as other people's S. cruentus, but rather it was one phase ~~representative~~ of the diverse and complex species. Regarding fertility, Lynch recorded that the group must be cross-fertilized, that some crosses cause sterility, and that crossing with S. heritieri results in "greater or lesser sterility." The cineraria is not very fertile, but some hybrids, notably S. cruentus x S. tussilaginis proved quite fertile. (Pitard & Proust record this as a spontaneous hybrid in the Canary Islands.) Lynch observed that the shrubby character of S. heritieri is lost after crossing "a few times" (he undoubtedly meant in the f_4 or f_5 generation or thereabouts). This is particularly noteworthy, as the shrubbiness had been cited by Thiselton-Dyer as a reason why S. heritieri would be unlikely as a progenitor of the cineraria. To the contrary, Lynch believed that the "eyed" or white-centered appearance of the heads of some cinerarias may have its origin with S. heritieri.

Lynch felt that his evidence, even though circumstantial, was sufficient to state that the cineraria is of hybrid origin. His hybrids indicated that certain crosses, notably S. cruentus x S. tussilaginis provided a "big jump from cruentus in the direction of the florists' cineraria." He observed that the cineraria had the habit of S. cruentus, the inflorescence arrangement of S. tussilaginis, the mode of coloration of S. heritieri, and the reduced fertility which is often characteristic of a hybrid.

Two other areas for which there are reports in the literature are teratologies and flower pigmentation. The former is discussed and

illustrated by Senianinova-Korchagina (1951) in a Russian article that cites some earlier Italian publications. Flower pigmentation has been dealt with briefly in survey papers such as Robinson and Robinson (1931) and Philip-Smith (1933). Coloration has been shown to be due, at least in part, to anthocyanin pigments, and variation in color may relate to internal cellular environment, presumably pH.

History of Cultivation

Reviewing old nursery and seed catalogs provides a wealth of information about the history of a cultivated group. The information presented here was gathered chiefly from the collection of nursery and seed catalogs in the library of the Missouri Botanical Garden. An exhaustive survey of these old catalogs would be a truly formidable task, and consequently, selected firms were followed through from their earliest catalogs (usually the 1850's) to the cessation of catalogs or to the present as the case may be. The firms which seemed to have the broadest offerings and whose catalogs were surveyed in detail are:

Louis Van Houtte (Belgium)
Dillistone & Woodthorpe (England)
(Later Lewis S. Woodthorpe)
Wm. Cutbush & Son (England)
Downe, Laird, & Lang (England)
F. & A. Smith (England)
Rivoire pere et fils (France)
Vilmorin-Andrieux et Cie. (France)
Ernst Benary (Germany)
Burpees (U.S.A.)
Henry A. Dreer (U.S.A.)
Stumpp & Walter (U.S.A.)
Vaughn's (U.S.A.)

In the early 1800's and through the 1860's, named varieties of cinerarias were maintained by cuttings. New varieties were raised

from seeds and were introduced each year. Apparently great prestige was attached to a new variety, as each year the new varieties were introduced with considerable fanfare and were priced high. The 1868 catalog of F. & A. Smith, for example, offers ~~nineteen~~ 19 new varieties, varying in price from five shillings to 10 shillings and sixpence per plant, while offering ~~forty-six~~ 46 older varieties at prices varying from one shilling and sixpence to three shillings and sixpence each.

The catalogs of the 1860's nearly always offer cineraria seed, with a descriptive statement that the seed will produce a number of varieties. Usually only one kind of seed was offered and no attempt was made to distinguish different sorts of cinerarias on the basis of seed. It can therefore be inferred that as late as the 1860's the true-breeding or nearly true-breeding strains had not yet been developed.

In the 1870's and 1880's the practice of maintaining named varieties by cuttings was generally abandoned. Vegetatively reproduced cinerarias are difficult to handle as they tend to lose their vigor after several propagations. Consequently this practice fell into disuse as truer-breeding strains of the cineraria were developed.

The horticultural improvement and the development of truer-breeding strains is readily documented by the seed catalogs. The general catalog of Vilmorin-Andrieux et Cie., lists only one sort of cineraria seed for 1856, "Cineraria hybrida, collection de belles varieties, selon la force et la nouveaute." In 1875 five sorts of seed were offered; in 1884, six sorts; in 1894, 12 sorts, and the apex (at least for Vilmorin-Andrieux et Cie.) was reached with 18 sorts in 1916. The offerings of other firms essentially parallel that of Vilmorin-Andrieux et Cie. Through the early 1900's an increasing number of varieties were

offered with such descriptions as "nearly true from seed," or "60% true from seed," etc. Also at that time, fewer sorts of seed were offered as "mixtures." If the seed catalogs truly reflect the level of development of the cineraria, the "best years" were between 1910 and 1930. During that time the large seed companies (particularly Rivoire pere et fils and Ernst Benary) offered seed selected as to flower color, stature of the plant, etc., and even some teratological forms.

A general decline in the popularity of the cineraria is evidenced by the catalogs of the 1930's and on to the present. Fewer sorts of seeds and more "mixtures" are offered, until at present most seed houses offer only about a half-dozen sorts of seed. As recently however as 1939, the Ohio Experiment Station was able to acquire 26 named varieties for horticultural trials (Alter 1939).

Classification of the Cultivars

A classification of the numerous "varieties" and "strains" is particularly difficult to provide. On one hand is the problem of great variability of a rather transient nature, and on the other is an uncertainty of exactly what is being classified, i.e., whether cultivated plants such as the cineraria should be classified according to gross morphology of the mature plant, or according to genetic potentiality of a group or "strain."

The cineraria is a huge complex encompassing numerous semi-distinct, inter-fertile sub-complexes (groups), each of which in turn is composed of sub-units (cultivars). This is reflected in the taxonomic schemes utilized in most garden compendia (e.g., Bailey, Booth, Bonstedt, Chittenden) and in seed catalogs. Nearly all classification schemes

recognize between three and six groups of cinerarias. The terminology for them varies but in general they are:

(a) compact plants with clusters of heads borne in a truss just above the leaves; often called the multiflora or (if the heads are large) the grandiflora assemblage. Sometimes subdivided into tall or dwarf (nana) groups. Double-flowered forms are sometimes named separately.

(b) tall, open plants with heads borne on long stems well above the basal cluster of leaves; known as the stellata assemblage. Not widely grown at present.

(c) a potpourri of intermediate forms, termed the intermedia assemblage.

A comprehensive classification of the cultivars is all but impossible with the present level of understanding, and to the best of my knowledge has never been attempted. The many subtle differences which distinguish the cultivars appear to vary freely from one generation to the next. How this type of variation should be treated when dealing with a complex maintained by the hand-of-man is yet to be fully explored in the theory of taxonomy.

Summary

Considering that the cineraria originated comparatively recently, and under what must be regarded as the best of horticultural conditions, it is rather surprising to realize the paucity of factual historical evidence relevant to the origin and development of the complex. The summarized facts are as follows: The progenitors of the cineraria belong to the section Pericallis of the genus Senecio, a group restricted to the Canary Islands, the Azores, and Madeira. This group was introduced into cultivation in 1777 and 1780. By the

early 1800's the cineraria was distinct from its progenitors, and by the late 1800's it encompassed numerous cultivars and great diversity of form. The biological origin of the complex has been studied fitfully, but the available evidence suggests that the cineraria is of hybrid origin, involving Senecio cruentus and S. heritieri and possibly several other species.

The cineraria is worth reviewing because it is a highly developed complex of cultivars whose possible ancestry is a limited group of readily-measurable insular endemics. Therefore, the cineraria should be a convenient vehicle for studying evolution under the hand of man.

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Table 1. - A Survey of Senecio, section Pericallis

Name	Important Synonomy	Remarks
(Canary Islands) S. cruentus (L'Herit.) DC.	Cineraria cruenta L'Herit. Doronicum cruentum Sch.Bip. in Webb & Berth. Pericallis cruenta (L'Herit.) Webb	Most closely resembles the cineraria in gross aspect. Hybridizes in nature with S. tussilaginis (fide Pitard and Proust, 1908). Coarse herb; hemicryptophyte
S. echinatus (L.f.) DC.	Cineraria ramentosa L'Herit. Caculia echinata L.f. Doronicum echinatum Sch.Bip. in Webb & Berth. Pericallis echinatus (L.f.) Webb	Hemicryptophyte
S. hadrosomus Svent.	----------	Recently described; poorly known. Hemicryptophyte?
S. heriteri D.C.	Cineraria lanata L'Herit. Pericallis lanata (L'Herit.) Webb	A sub-ligneous subshrub, with large heads, "eyed" color distribution in ray flouts.

S. appendiculatus (L.f.) Sch.Bip. in Webb & Berth.	Cacalia appendiculata L.f. Cineraria appendiculata Poir. Cineraria populifolia L'Herit. Senecio populifolius (L'Herit.)DC. Pericallis populifolia (L'Herit.) Webb. Cineraria lactea Schlecht.	Variable; poorly understood. Has complex synonomy including numerous infraspecific taxa. Coarse subshrub.
S. multiflorus (L'Herit.) DC.	Cineraria multiflora L'Herit.	Complex synonomy; included under S. webbii by Lems (1960).
S. murrayi Bornm.	Senecio gomeraeus Bornm. (?)	Poorly known.
S. papyraceus DC.	Doronicum papyraceum (DC.) Sch.Bip. in Webb & Berth. Pericallis papyracea (DC.) Webb	Hemicryptophyte
S. steetzii Bolle	----------	Poorly known
S. tussilaginis (L'Herit.) Lessing	Cineraria tussilaginis L'Herit. Doronicum tussilaginis (L'Herit.) Sch.Bip. in Webb & Berth.	Hybridizes in nature with S. cruentus fide Pitard & Proust (1908). Hemicryptophyte.

S. webbii (Sch.Bip.) Christ	Doronicum webbii Sch.Bip. in Webb & Berth. Pericallis webbii Bolle	Hemicryptophyte
(Azores)		
S. malvaefolius (L'Herit.) DC.	Cineraria malvaefolia L'Herit. Doronicum malvaefolium Sch.Bip. in Webb & Berth.	Reported from the Canary Islands by L'Heritier. The name as now used is restricted to the Azores plant.
(Madeira)		
S. maderensis DC.	Cineraria aurita L'Herit.	Subshrubby.

BOTANICAL MUSEUM OF HARVARD UNIVERSITY

Oxford Street
Cambridge 38, Massachusetts

June 25, 1965

Dr. David J. Rogers
New York Botanical Garden
Bronx Park
New York, N. Y. 10458

Dear Dave:

Thank you for your letter of explanation. My preoccupation is not that editorial matters were discussed by the Council, but that I sensed throughout the Rhode Island Meetings and before them, a kind of lack of liaison.

John Thieret and I have been in correspondence about his wishes to enlarge the scope of the book reviews. Some of the books reviewed in the past have not had direct or indirect relation to Economic Botany, and I think that if we lack space it would be best to sacrifice these. I also suggested to him, after a talk with Robert Breach, that except for very important books he ask reviewers to limit themselves. In this way — reviewing only things of interest to our field and limiting the length of reviews — I think that we can increase our coverage yet not necessarily increase our pagination for reviews.

Thieret, as you know, is very dedicated and I have a feeling that he might well be a good candidate for editor when, as it has every right to do, the Council decides that they wish a change and/or if I feel that I cannot give the time any longer. Men of Thieret's dedication are hard to find and I think,if only for this, we ought to give him every chance to expand up to practical limits, his present nook.

Sincerely yours,

Dick

Richard Evans Schultes
Editor, ECONOMIC BOTANY

RES:mg

June 22, 1965

Dr. Richard Evans Schultes
Botanical Museum
Harvard University
Cambridge, Massachusetts

Dear Dick:

I will try to answer some of the questions in your letter of the 21st to Bob Raffauf because I know that he is now on his way to New Guinea. I too was disappointed that there was no communication to me as to when there would be a Council meeting and only learned of the fact that some of the Council had met on Monday night after I arrived on Tuesday and only learned of the Council meeting to be held on Wednesday night about 4:30 Wednedday afternoon.

As far as the numbers of book reviews, etc., are concerned, both Mr. Breach and I wanted to raise this item because of the number of pages involved, obviously a matter of concern to the management of the financial aspects of the journal. We felt that a discussion would be useful. I only brought the matter up as a possible item for discussion at the Council meeting on Wednesday night and suggested that in your absence nothing could be done. I tried to catch you several times during the meetings but was unsuccessful. Every time I looked up, I found that you weren't where I thought you were, and I couldn't seem to make the right contacts.

There was no proposal made by me at the Council meeting. I felt that certainly it was an editorial question and one that we needed to discuss. It was decided that such proposals should be brought up at a fall Council meeting. It was also suggested to Bob that it would be profitable to list items on an agenda and circulate these to the Council members in advance of the Council meeting.

John Thieret and I have exchanged correspondence on the number of book reviews. He had written asking why we didn't publish more book reviews that he had already sent to us, and our only answer was that there wasn't enough room for them and for the articles submitted so that obviously some decision had to be made to balance out articles versus book reviews. Since we have no specific directive in this connection, it is obvious that some choice had to be made and that choice was made as best as possible between Mr. Breach and me. We suggested to John that the whole matter of book reviews be taken up with you in light of this obvious restriction that we have on the number of pages that we can print. I think you will agree that we had attempted to meet the requirements of the increased number of articles and book reviews, but

obviously there is a limit beyond which the finances will not support the increased papers. It is therefore something for us all to decide as to how to balance out our desire for more papers and more book reviews with the practical business of financing the Journal. Believe me when I say that we had no complaints concerning the quality of the book reviews. It is only that the question of finances raises its ugly head. It would seem reasonable under these circumstances that we do find some way to limit the number of book reviews or to find a way to bring some books to the attention of the readers in a shorter review.

I don't know how it came to your attention about the Council discussion unless Bob Perdue sent you a copy of the Council minutes which he did not send to me; so I don't know your source of information. I hope that this clarifies the thing to some extent.

Sincerely

DJR:MDF

CC: Dr. Raffauf
Mr. Breach
Dr. Thieret

June 21, 1965

Dr. Robert F. Raffauf
Science Information Department
Smith, Kline & French Laboratories
1500 Spring Garden Street
Philadelphia 1, Pennsylvania

Dear Bob:

It has come to my attention that the Council discussed the question of whether or not Economic Botany was publishing too many book reviews. I do not know what, if anything, was decided.

I was rather surprized that this should have come up for discussion without its having been even mentioned to the editor. Furthermore, I assume that it was not mentioned to the book review editor, either, else he would have dropped a line or two on the matter in our correspondence.

Thieret is doing a find job and has hopes of extending our coverage in the review section. As editor, I have learned that many of our readers turn first to the review section and find it helpful. Therefore, I have encouraged Thieret in his hopes. Recently, I wrote him that increased coverage would probably necessitate our limiting the size of the reviews, but I still feel that this section is a vital part of the journal and should not only not be curtailed but should be increased.

Again I find it hard to understand why no one on the Council nor Dave Rogers sought my opinion if this were to be one of the items of business. Perhaps in our Society we ought to strive for a little more liaison — it was noticeably absent in certain phases of the planning for the meeting just past (e.g. failure to send out notices to all members, etc.), and I cannot but feel that it was lacking in this matter of the book review discussion.

Sincerely yours,

Richard Evans Schultes
Editor, ECONOMIC BOTANY

RES:mg
cc: D. J. Rogers
J. W. Thieret
Robert Breach

June 22, 1965

Dr. John W. Thieret
Department of Biology
University of Southwestern Louisiana
Lafayette, Louisiana

Dear John:

Enclosed herewith is a short review by Dr. Eugene Jablonski of a Russian book that you gave us some time back. I am sending the Russian book along to you under separate cover, and you can keep it for your own little file. I am sure you will enjoy it.

You asked me some time back about a $10 stationery bill. There is a question in the mind of Mr. Breach as to whether this is a bill from last July. If so, that one was paid. If not, no bill has been received recently for $10. Send a bill--you get your money.

It was too bad that you weren't at the recent SEB meetings. We would like to have discussed something at the Council meetings about the whole business of editing and other activities of the Journal. It is healthy for us to review these now and again. It seems that a Council meeting will be held this fall somewhere here in the New York area. At that time it would probably be a good idea to gather all concerned together to see how we stand. I hope you can make that one.

Sincerely

DJR:MDF
Enclosure

June 14, 1965

Dr. Frederick G. Meyer
U. S. National Arboretum
Washington, D C. 20250

Dear Fred:

Concerning the reprints of your Coffea article, we had noted in your letter that you wanted 100 copies of the reprints and that a purchase order would be forthcoming. However, when the purchase order came, it was for only 50 reprints and that was the basis for our decision to send only 50.

Although the type for the article has been broken up now, it would be possible to ask the printer to run some offset copies if you are interested in more. This will of course involve another purchase order to do this.

The above explanation is apparently the reason for the mix-up. I hope we can straighten it out.

Sincerely

David J. Rogers
Managing Editor
ECONOMIC BOTANY

DJR:MDF

U. S. National Arboretum
Washington, D. C. 20250
June 4, 1965

Dear Dave:

Re reprints to my coffee article. We have received 50 so far. I thought we had ordered 100, but wonder whether there have been some mixup.

Sincerely,

Fred

Frederick G. Meyer
Research Botanist

May 18, 1965

Dr. Ted Barkley
Department of Botany
Kansas State University
Manhattan, Kansas 66504

Dear Ted:

I am pretty sure that ECONOMIC BOTANY is a good place to put your paper on the flora Cineraria. However, before it is a fait accompli I think it would best be finished and sent to me and I will forward it to Dick Schultes. This will probably be the best technique. As far as I am concerned, the idea is great and we should have more like it in E.B., but I think perhaps I can push it better and get a better reading for it than you can directly.

I expect to be at the AIBS meetings and look forward to seeing you there. As far as coming to see you this summer, I don't know whether we will be able to make it or not, but Marilyn is going to start college at Parsons College, Fairfield, Iowa, in the latter part of June. I think it is about another 150 to 200 miles from there to Manhattan. If the opportunity arises, we might drop in on you toward the end of June, but don't hold your breath.

Sincerely

DJR:MDF

Kansas State University

Manhattan, Kansas 66504

Department of Botany and Plant Pathology
Dickens Hall

May 16, 1965

Dear Dave:

Do you suppose that Economic Botany might consider publication of a 20 ør so page manuscript on the history of the origin and development of the florists' cineraria? The paper is essentially based on herbarium and library work, etc., and summarizes what is known of the wild progenitors and how they were developed into the present-day cineraria. The paper is in the final throes of preparation, i.e. the "re-write" stage.

This August I am giving a squib at AIBS on some experimental work with the cineraria, relating to their breeding habits. The group is not quite as simple as I once thought it was.

Will you be going to AIBS?

We spent Easter week in St. Louis at Carolyn's sister's place; and I spent the days doing library work at the Missouri Botanical Garden. From all outward appearances things there are really on the upswing. Although David Gates hasn't arrived yet, he seems to have the confidence of everyone.

Best regards,

Ted

P.S. Come see us this summer. We would be most pleased to have you come be our guests.

UNITED STATES DEPARTMENT OF AGRICULTURE
AGRICULTURAL RESEARCH SERVICE
CROPS RESEARCH DIVISION
BELTSVILLE, MARYLAND 20705

Send copy to Schultes!

March 26, 1965

sent 4/1/65

Dr. David J. Rogers
The New York Botanical Garden
Bronx Park
New York 58, New York

Dear Dave:

I've been snookered! Actually, I will be happy to see that the participants have the word, etc. The symposium was one of the more successful of my relatively short career of attending such functions, but many other people had the same reaction. Such being the case, I am all in favor of recording it is a symposium number if this is possible.

You realize, of course, that I'm in this Society by osmosis: I have never yet paid my dues, but I'm always willing to make helpful suggestions! Working so closely with the group here which has been active in Society affairs has rather completely embroiled me, particularly when papers have been short for meetings.

Jones is preparing for his trip which leaves him short of time and Perdue had no part in the symposium having been in Ethiopia until 10 days ago. That's one more reason why I should pull the oar on this symposium.

If the remainder of the editorial board has no objections, I'm ready to go to work.

Sincerely yours,

Smitty

C. Earle Smith, Jr.
Botanist
New Crops Research Branch

March 29, 1965

Dr. John W. Thieret
Department of Biology
University of Southwestern Louisiana
Lafayette, Louisiana

Dear John:

After talking over your problems of getting rid of the backlog of reviews, we have decided to have a go at it and get the back log cleaned up by extra pages in the Journal. So now that you have got the reviews, send them in and we will try to get them taken care of.

We hope that from now on, until some definite policy ismade with Dick Schultes about the size of the Journal and the direction that we hope to take with it, you will not be quite so inclusive concerning the book review section.

Again, let me say that I do not intend that this be a criticism of what you are doing. It is merely that there are problems, real ones, that we have concerning the financing of such things. It is easy to say let's publish the best because we are unique, but somebody has to pay for it, as I am sure you are aware, and we cannot keep up with them all.

We will take everything you have now and get it into print. We do have that obligation, and we will meet it.

Sincerely

DJR:MDF

P.S. The letterhead you requested is being mailed today.

UNIVERSITY OF SOUTHWESTERN LOUISIANA

Joel L. Fletcher, President

LAFAYETTE, LOUISIANA

Bob: Your comments - Please

Dr. David Rogers
New York Botanical Garden
Bronx Park
New York 58, New York

Dear Dave:

From here on, in the letters I send to prospective reviewers, I shall state that the review should be a "brief" one and should have an upper limit of 500 words. Until now, I have been telling reviewers that their review may be "any length." Most reviewers, however, have not sent in long reviews-- except Kingsbury at Cornell, whose review of the new book Selenium is about 10 pages long. It will be sent back to him for cutting. you may rest assured.

When I first took on the task of review editor I stuck pretty closely to books that could logically be considered to be "economic botany." But, as you doubtless have noticed, I have broadened our scope to include botany in general. I aim to have the finest review section for botany books that there is; indeed, I am certain that we have that right now. I would like to continue to carry reviews of botany (including "economic botany") books, but if we can't get the reviews published any faster than we do, then maybe we had better go back to plain "economic botany." This I would do, however, only under protest. Our journal is unique in its field (I know of no other devoted to "economic botany," unless in the past year or two something new has come out). Let's also have it number one so far as reviews of botanical books are concerned.

Why not use a smaller type for the reviews so that we can get more in an issue????? Seems to me that this is one thing seriously to consider.

We still have not heard any suggestion about what to do with all the reviews on hand. Can we have a whopping big review section in, say, part 3 of the present volume? We could increase the size of the review section, we could use smaller type, and I could go once again over all the reviews I have here and cut them with abandon. This way we could get the reviews out before they become ancient history.

PLEASE CONSIDER THE SUGGESTIONS IN THE PRECEDING PARAGRAPH AND LET ME HEAR FROM YOU.

Sincerely yours,

John

Discouraged and Harrassed

Please send another batch of ECONOMIC BOTANY letterhead— but no envelopes. When one has the splendid and inclusive review section that EB has, I assure you that letterhead is consumed like mad.

March 17, 1965

Dr. John W. Thieret
Department of Biology
University of Southwestern Louisiana
Lafayette, Louisiana

Dear John:

This answers two letters of yours to me. One is easy to answer, but the other is going to require a bit more consideration so I won't give you final answers to it.

The only thing you need to do to get your money back for expenses is to submit a semi-formal note to Mr. Robert Breach at the New York Botanical Garden.

On the matter of reading proof, Bob is in charge of all the publications now, and he reads the proof. It is he who has marked the galley that you have noted. He is willing to assume full responsibility for proof reading, and he suggests that all proof be sent to him and that you not be bothered with receiving any galley at all.

Now to the second letter in which you are worried about the numbers of reviews. First of all, let me ask a few questions about your procedures. Do you, for example, when you send out a book for review request that the author confine the number of words requested? I suspect that you have been leaving it open to the reviewer to make up his mind as to how much space is needed for the review. While this is an ideal, I think it is hardly practical to allow a reviewer to keep on going on. Some of the reviews have been extremely lengthy, and perhaps more so than is needed to get an interpretation of the book's meaning and some of the facts about it. It is a practice among many journals--SCIENCE, for example--to give an approximation of the number of words that should be given for a particular book. This varies, of course, with the contents of the book, its size, etc.

Second of all, has it been clearly demonstrated that we should review all botany books in ECONOMIC BOTANY? I do think that we have to think of this restriction--that ECONOMIC BOTANY by its title should think only of those books that have some merit for the readers of this journal. Inasmuch as there is no definition of economic botany as to what it should contain, this is obviously a very difficult question. You and I have had discussions as to what is economic botany, and I think that we

have not defined this field exactly in the same way, and yet you have a working definition which is as good as mine or anyone else's. It is a pertinent point in this discussion to think in terms of whether or not a book really is something that we should concern ouselves with. This question came up, you will recall, when you asked me to review Numerical Taxonomy. It would seem therefore a good idea if we had a discussion between you, Schultes, Breach and myself as to how far we want to go with reviewing. Obviously, anyone can play the numbers game, but I don't believe that we are interested in the number of book reviews that we get out so much as with the quality of the reviewing and the connection of the books with the field of economic botany.

We have a very fine situation where you are doing an outstanding job of getting good book reviews and Dick Schultes is doing a fine job of getting good original papers. Unfortunately, we have to think about the size of the journal because of the costs. Page charges are not coming in as we had hoped they would. Even if we got 100% page charges paid, the amount charged for a page of print to the author is only about 60% of the actual cost of printing and marking. We should not be subsidized by the Garden inasmuch as this is an international journal and not an organ for members of our own staff. If we were able to support ECONOMIC BOTANY in the style which would be pleasing to us all, it is unlikely that we would indeed spend the money just for ECONOMIC BOTANY but would rather spend it for staff salaries which are at a low ebb.

Actually, this problem of book reviews is one that we must take up as an editorial matter, and my answers here are merely considerations which we really should discuss with Dick as the major contributor. I think it is a pertinent point and one that we will have to take up, but these are the things that you may be thinking about in the meantime.

Sincerely

DJR:MDF

Journal: ECONOMIC BOTANY

The Society for
ECONOMIC BOTANY
DEVOTED TO THE PAST, PRESENT, AND FUTURE USES OF PLANTS BY MAN

Box 915
University of Southwestern Louisiana
Lafayette, Louisiana

4 March 1965

Bob:
Care to comment on this?
Dave

Dr. David Rogers
New York Botanical Garden
Bronx Park
New York 58, New York

Dear Dave:

As of the above date, the following statistics apply to the book review business in Lafayette.

29 books are out for review.

27 books are on hand for which I am awaiting a "yes" or "no" from 27 prospective reviewers.

47 books are on hand for which I have yet to send out letters to prospective reviewers or for which I have received one or more "nos" and have not written to yet another prospect.

35 reviews are on hand awaiting my tender editorial touch.

30 reviews are on hand in proof. The proofs are with me right now.

Contrast the above with the following statistics.

in Vol. 19 (1), eleven reviews came out.

in Vol. 18 (4), eleven reviews came out.

in Vol. 18 (3), fifteen reviews came out.

As I think you know, I have stepped up the pace of book reviewing; I want our review section to be the best one for botanical books (it may even be that right now).

But I can see really no good reason for my trying to be relatively efficient down here-- for my spending as much time as I do on the job— when you people can't publish more reviews at one time.

Journal: ECONOMIC BOTANY

The Society for

ECONOMIC BOTANY

DEVOTED TO THE PAST, PRESENT, AND FUTURE USES OF PLANTS BY MAN

I do not know how many reviews you have on hand-- but I am certain that you have some.

WHAT CAN WE DO TO CATCH UP TO THE POINT WHERE WE CAN PUBLISH MORE REVIEWS-- WHERE WE CAN CARRY REVIEWS ONLY OF CURRENT BOOKS.

Within a week I could send 35 reviews to you. And I'm certain that within another month I shall be able to send 20 more. And so on, now that I have stepped up operations.

But what is the use to send you, say, 35 or 40 reviews a quarter when you publish only half that number or fewer!

Shall we slow down? Shall we use a considerably smaller type size for the review section, and so be able to get in more reviews in the same number of pages that we use now? What, pray tell, shall we do?

Sincerely yours,

John

John W. Thieret

UNIVERSITY OF SOUTHWESTERN LOUISIANA

Joel L. Fletcher, President

LAFAYETTE, LOUISIANA

2 March 1965

Dear Dave,

Thanks for your help with the plant formerly known as Stillingia aquatica. (I wish these damn taxonomists would stop changing names!) I suspect that it might be an Amsonia. Is my face red...

Yesterday I took a batch of review books over for mailing-- and spent $2.40 to mail them. How about a check for $10.00 again, which is now the amount I have spent on postage for sending books to reviewers. I think that you already know that our university pays the postage on all the letters I write, but that I foot the bill for the books.

Is there someone at the NYBG who could take care of reading proofs of reviews???? I am so swamped down here that a batch of review proofs that came a few days ago just may be the straw that broke the review editors back. I note that someone up there goes rather carefully over the review manuscripts before they are sent to the printer. Maybe this same person (who catches some things that even I, the great editor, do not catch) would be willing to let me send the proofs to him for going over. Let me know.

My desires to make the EB review section the best for botanical books may be the end of me some day. I've about 20 or 25 reviews on hand for editing; I've about 35 books waiting for reviewers; and that huge batch of proof takes this lousy time to appear on the scene. What is essentially a thankless job sure takes a lot of time.

Mardi gras today in Lafayette. What a big bunch of b.s.

Somewhat discouraged,

John

John W. Thieret

UNITED STATES DEPARTMENT OF AGRICULTURE
AGRICULTURAL RESEARCH SERVICE
CROPS RESEARCH DIVISION
BELTSVILLE, MARYLAND 20705

March 2, 1965

Dr. David J. Rogers
The New York Botanical
Garden
Bronx Park
New York 58, New York

Dear Dave:

The symposium papers will be published in Economic Botany, apparently, so you might as well finish your contribution. Dick wrote me that he would contact the various authors concerning their contributions and I presume that everyone outside of the Economic Botany inner circle was contacted. Obviously, he knew that you knew things were stirring. So far as I know, no official decision has yet been made as to whether a whole number of Economic Botany will be devoted to the symposium papers.

Sincerely yours,

C. Earle Smith, Jr.
Botanist
New Crops Research Branch

February 16, 1965

Dr. John W. Thieret
Department of Biology
University of Southwestern Louisiana
Lafayette, Louisiana

Dear Harrassed Review-Editor:

Why do you want to review Sokal and Sneath Principles of Numerical Taxonomy in B.B.? It is not economic--it costs lots of money and doesn't sell anything in the way of plants. It is a garbage-type book.

Inasmuch as Sokal and Sneath are more or less competitors to Rogers et al., it would not do for me to write the kind of review that the book needs. There are errors both in terms of botany and in terms of mathematics. If I say this, it is likely that it would be thought about as sour grapes; so I am afraid you will have to go somewhere else if you insist on reviewing this book in B.B. which I hope not.

Sincerely

DJR:MDF

UNIVERSITY OF SOUTHWESTERN LOUISIANA

Joel L. Fletcher, President

LAFAYETTE, LOUISIANA

10 February 1965

Dear Dave,

If I sent you a copy of Sokal and Sneath Principles of Numerical Taxonomy, will you prepare a most masterful and literate review for our favorite journal?????

Sincerely yours,

John

Harrassed Review-Editor

February 16, 1965

Dr. Harriette V. Bartoo
Department of Biology
Western Michigan University
Kalamazoo, Michigan

Dear Hamiette:

I have your letter of February 9. In response to your question about the bill for your reprints, I have learned that the bill will very shortly be sent to you directly by our Publications Department. The bill has not gone astray as you feared.

This year there will be meetings in the late spring or early summer, probably about the 15th of June, at the University of Rhode Island. It is to be a joint meeting with the American Society of Pharmocognocists. Dr. Heber Youngken is the chairman of the committee this time, and you should correspond with him if you wish to participate. His address is College of Pharmacy, University of Rhode Island, Kingston.

It should be an interesting meeting. The symposium this year is jointly sponsored with the Pharmacognosy Society and will be on the general subject of chemo-taxonomy.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

WESTERN MICHIGAN UNIVERSITY

KALAMAZOO, MICHIGAN

Feb. 9, 1965

David J. Rogers, Managing Editor
Economic Botany
The New York Botanical Garden
Bronx New York 10458

Dear Dr. Rogers:

This may seem a little out of the ordinary, to ask for a "bill" but I have not yet received one for the reprints of my article which appeared in the last issue of the journal and since I wrote along with the order that the bill was to be sent directly to me, I was beginning to be a little fearful that the directions had not gone to the proper source and that possibly it might have been sent instead to some office here at school.

As I explained, the faculty fund was given directly to me and I put it into a special account at the bank as soon as it was received. I was quite pleased with the set up of the article and have had quite a few requests for reprints which I am in the process of sending out.

I was a little disappointed that I was not able to attend the meeting at Montreal but it came at a bad time for me where ordinarily I would have been able to attend the Xmas meetings. I presume that this will take the place of the heretofore meeting scheduled at Easter or thereabouts?

If you will see to getting me the bill for the reprints I will see that it is promptly paid.

Sincerely yours,

Harriette V. Bartoo

Harriette V. Bartoo
Biology Dept. W.M.U.

PS Should this instead have been sent to Hans Schmidt? Would you kindly pass it along to the proper person. Thx. HVB

UNITED STATES DEPARTMENT OF AGRICULTURE
AGRICULTURAL RESEARCH SERVICE
CROPS RESEARCH DIVISION
BELTSVILLE, MARYLAND 20705

February 2, 1965

Dr. David J. Rogers
The New York Botanical
Garden
Bronx Park
New York 58, New York

Dear Dave:

Seems I have accumulated a couple of your letters to answer. First, the matter of the page charges has gone forward for approval. Your business manager is indeed optimistic if he really expects this to clear within a day or two. It will probably take a week to ten days before I get the approval. An order must then be written and forwarded. Because you did not give me an estimate on the number of pages, I measured it up with a ruler and estimated nine pages.

Next, thank you very much for the suggestion on the paper by Betty and Cliff. I have had a copy for some time. The references which are given in this paper are only for the publications mentioned by the individual authors of contributions and frequently do not cover the field at all completely. I think I shall go ahead with the accumulation of titles in hopes that I can build up a usable file.

Richard Evans certainly should be in on the symposium volume bit and I shall write him today concerning this. In further conversations with Quent, he feels that the major stumbling block is going to be the conservative members of the Council who like to reserve a financial cushion for the Society and who may be unwilling to pay for the publication of the symposium volume. If they can be persuaded, apparently the rest of the Council would not be difficult to convince. I feel that it is definitely worth a good try.

Sincerely yours,

C. Earle Smith, Jr.
Botanist
New Crops Research Branch

cc: Dr. Quentin Jones

The New York Botanical Garden, Bronx Park, Bronx, N. Y. 10458

Dr. C. P. Wilsie
Department of Agronomy
Iowa State University
Ames, Iowa 50010

Dear Dr. Wilsie:

Thank you for your letter of the 26th of January.

We are attempting to round up the papers for the symposium on ethnobotany at the moment. Dr. C. Earle Smith, Jr., is more or less in charge with Dr. Quentin Jones working with him.

We appreciate your encouragement to print these papers.

Sincerely yours

David J. Rogers
Managing Editor
ECONOMIC BOTANY

DJR:MDF

IOWA STATE UNIVERSITY
of Science and Technology
AMES, IOWA 50010

Department of Agronomy

January 26, 1965

Dr. David J. Rogers, Managing Editor
Economic Botany
New York Botanical Garden
Bronx, New York

Dear Dr. Rogers:

I have had some correspondence with Quentin Jones regarding the papers given in the Symposium on Ethnobotany of Some New World Cultures at the Montreal Meeting of the A.A.A.S.

In teaching material on the origin and adaptation of crop plants I find such papers extremely useful and wonder whether it may be possible to bring these together, publishing them perhaps as a special symposium issue of Economic Botany? This would be a very worthwhile thing to do if the financing is not too difficult.

Sincerely yours,

C. P. Wilsie

C. P. Wilsie
Professor
Agronomy

CPW:jf

December 9, 1964

Dr. Ivan A. Wolff
U.S.D.A., A.R.S.
Northern Research and Development Div.
Peoria, Illinois

Dear Ivan:

I have checked with our publications manager and with the printer and find that the costs to you will be $35 per page. That is our full cost for publication, and payment of this charge is the only way in which you will be able to achieve publication of the materials requested. at an early date

Sincerely

David J. Rogers
Managing Editor
ECONOMIC BOTANY

DJR:MDF