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About the Institute

The Hunt Institute for Botanical Documentation, a research division of Carnegie Mellon University, specializes in the history of botany and all aspects of plant science and serves the international scientific community through research and documentation. To this end, the Institute acquires and maintains authoritative collections of books, plant images, manuscripts, portraits and data files, and provides publications and other modes of information service. The Institute meets the reference needs of botanists, biologists, historians, conservationists, librarians, bibliographers and the public at large, especially those concerned with any aspect of the North American flora.

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.

My Dear Sir,

Philadelphia (Dec. 19. 31.)

Duly favoured with yours of the 26, & 29
Nov. & also the fine bulb of "*N. moschatum*" from
Boltani, allow me to thank for them, & for
the facilities with which you forwarded them.

The "*N. moschatum*" has I think a larger bulb than
I ever possessed of that plant or its affinities,
& I am very much pleased with it. I guess
it to be *N. ajac. albicans*, of my Monas.

— By the way, Ridgway has just published
a second Ed.ⁿ of the Monas. with many additions,
4 of which may prove to be species. I have done
several fresh copies, & meant to have omitted
at least, no well known copy, & really
took pains to do it; yet alas! the well
known copy *Longueil*, is not mentioned!

— "*humanum est errare*"! — I expect there
are ^{three} ~~two~~ copy *Longueils*, one a var. of the common
large *Longueil*. *Imajor. Monas. Narcis.* often sent
from Holland: the other a var. of *minor. Monas.*
Narcis. — This last M. ^{perhaps} *Nelne*, rec. both single & double!

of from Constantinople a few years
since, & now has them in the herbaceous
collection at one number, & named. From
a bulb of each he gave me in bloom, &
^{certainly} ~~gave~~ the description I published, but I
fear the bulbs have perished. It is smaller
(^{or more}) paler than *cf.* common one last day,
a larger cup; all which the faithful Parkinson
has detailed in his *Paradisus Terrestris*. I am
sorry I had not at the time these plants
flowered, the common double *"longica"*
compositae have much to learn.

"*Multum adhuc restat, multum quæritabit.*"
And such is the motto in my 2.^d Edition.

The American *Pseud-acorus* I shall be able
to furnish you with in Spring. I am sorry no
more of my list will be acceptable to you.

Mr. Sweet joins me in thinking, tho, I confess
(I speak from memory) that your *Tris triflorus* is

J. graminea of B.
was low, not 2
days. Do you confess

The vars. & species
as beautiful as they.

to have them prosper, we
ground when at rest,

lost most of mine by
your "*N. exsertus*,"

will prove *Gomphed*
you corresponds with B.

from ours. You will have
proving much, & the new

new acquisitions, under
asked M. Otto for them

better in your hands; &
to try other foreigners. I

asked the botanic world at the
have not of *Narcissus*
I shall perhaps be able to
bring but as yet I have not

ple a few years
tem in the herbaceous
order, *Rosaceae*. From
me in bloom, I
I published, but I
wishes. It is smaller
common one last day
in the faithful Parkinson
Pradimus Terrestris. I am
the time these plants
a cable "longue" to
ach to learn,

stat, multumque habit.
in my 2^d Edition.
D. acorus I shall be able
bring: I am sorry no
be acceptable to you
in thinking, The, I confess
your *Pradimus* is

I. pruinosa of B.M. — The plant you sent
me was low, not "4-feet" any more, at least better
says. Do you confirm *tripetala*, or *tridentata* with it?

The vars. & species of Dutch bulbous *Ficus* are
as beautiful as they are variable. To suppose
to have them prosper, we must keep them out of
ground when at rest, like the Dutch. I have
lost most of mine by not taking up.

Your "*N. eximius*," having a "small bulb" I guess
will prove *Ganymedag nutans*. Nob. — I am glad
you correspond with Berlin. Their names of Bulbs vary
from ours: you will have an opportunity of getting many
having much, & be recognized by finding amongst them
new acquisitions, under old names. I have thought of
asking M. Otto for them myself: but I am glad it will
better in your hands; & it will allow me more time
to try other foreigners. Indeed in 2^d Edit of my Monog. I have
asked the botanic world at large for all that. Sweet & his friends
have not of *N. cissinoides* — after another year's cultivation
I shall perhaps be able to do something like a *monog.* on
the *N. cissinoides*. I have not yet got the *N. cissinoides*.

Letter by A. H. Haworth

The Rev. Mr. Edlicombe,
Belton Vicarage
near
Bath

from the Library of
GORDON D. ROWLEY

Sulphur Phoenix (or *Crown*) was ~~omitted~~ ^{omitted} ^{periodical} ^{casually}.
I was not aware of it. The paper was printed and great ^{many} ^{of} ^{the} ^{papers}
Leaving many typographic errors. I had 25 ^{for} ^{copies} ^{for}
the manuscript, (now all given to British & foreign Authors)
save one for myself (2 proofs full of corrections). Before
these said 25 were ~~revised~~, I revised the press & advised
Abneyway to cancel the others, & strike off new, from the
Revised types. But I know not whether he did so.

Dear Sir,

I send you a Catalogue of
some late Shoblands Books, which you will
see are to be sold on the 10th of March and
the following days - They have been valued by
Mr. Sturt a bookseller in Southampton Street
Covent Garden, at £4.50 - I hoped to have
had an offer for them entire, but this not being
the case, I at length determined to send them
to auction - You advised me in your last note
if I had not a price of seven or eight
shillings, I had many applications for a few
books, but I was advised by no means to sell
any, as it would take from the sale of the
rest altogether - If you are inclined to purchase
Mr. Sturt is a very upright and fair dealer.
Man, and of course will be happy to
execute any commission you may propose him
with.

I think I told you in my last letter that
I had sold the Herbarium, Shells, and

Orig. held by
Jordan Rowley

many years, since which I have had an offer for
the *Quercus*, *Salix*, *Abies*, and *Ficus* - which
I hope I shall accept, I have therefore now
only the *Parrot* to dispose of, & yet I have
had no offer for them as a whole, and I am
hoping I should so rare and valuable an
collection - I shall at all events wait a
little longer -

I am sorry to say I still remain
very unwell, but better I hope than when I
last had the pleasure of addressing you -

I shall be glad to hear you and
your little family are well -

And remain Sir

Yours very truly,
E. M. Haworth

Chelsea March 27
1834

from the Library of
GORDON D. ROWLEY

TELEGRAMS - WYTHELEAF.

Sir David
Prain

THE WELL FARM,
WARLINGHAM,
SURREY.

Aug 31, 1831.

Dear Hall,

There comes a time when one thinks of things
that are apt to be overlooked.

Years ago I had occasion to be in rather
intimate correspondence with old Francis Coppin,
who knew more about Rosa, in the old fashioned
way, even than J. G. Baker, and that is saying a
great deal. As a result, I have a lot of his papers
on the subject, into which I know that I shall never
look again.

I know quite well that those who deal with
the genus from the new standpoint are apt to
discount the opinions of those who were tied to
another perspective. But I know too that a
time comes when the new worker thinks it worth
while to compare old records with his own.

Has the John Dimes a complete set of Coppin's
papers? If not I should be glad to send you
the ones I have sent me from time to time, for which
they are worth. Please do not hesitate to say
no if you don't want them, or to say yes,
if I think they would be of the least value
to you.

Yours ever
D. Prain

The National Rose Society of Victoria

(In Affiliation with the N.R.S., England)

Hon. Secretary:
J. WM. FIRTH,
- Lynsway,
89 Flinders Street,
Thornbury, N.17
Victoria, Australia.
Phone: J 3275.

Hon. Treasurer:
W. H. DUNALLAN
- Wiedal,
63 Arbutton Road,
Northcote, N.16
Victoria, Australia
IW 1338

"The Australian Rose Annual"

All communications to the
Editor,

I. A. STEWART,
34 Queen Street,
Melbourne, C.I.,
Victoria, Australia.
Phone: Central 3614.

7th July 1933.

H. R. Darlington Esq.,
6 Stone Buildings,
Lincolns Inn,
LONDON. WC.2.

Dear Mr. Darlington,

I am sending you, with my compliments,
a copy of the 1933 Australian Rose Annual which I think you
will find about up to our usual standard and I hope will
prove helpful and interesting.

At the same time will you allow me on behalf of
a great many Australian Rose lovers, as well as personally,
to convey to you the very deep sympathy we all feel in the
great loss you have suffered in the death of your gifted
wife and life partner.

We have all enjoyed and appreciated the very capable
articles that Mrs. Darlington has contributed from time to time
in the English Annual, and have felt something of the keen mind
and charming personality behind, and as with rose lovers in the
old land we in Australia experienced a deep sense of loss when
the news reached us that one who had contributed so much to the
pleasure and help of rose lovers all over the world, had passed
away.

We trust you will accept this simple and sincere
tribute to her worth and work from rosarians in this land so
far distant but yet near in kinship and sympathy.

Yours faithfully,

I. A. Stewart,
Editor.

TELEPHONE No 50.

TELEGRAMS - GUIDE, ARRBROATH.

HILLSIDE HOUSE,

ARBROATH.

4th May 1937

Col. A.H. Wolley-Dod,
Berkeley Cottage,
Mayfield,
Sussex.

Dear Col. Wolley-Dod,

Yes, you told me about the work you had undertaken to do for the new Student's Flora and I am glad to hear you are to do it alone, although I know what a very difficult thing it will be to tackle. I really do not think, however, that you could possibly have managed to work with Heslop-Harrison - he is such a difficult man and I have heard from more than one source how difficult it is to get him even to answer letters.

I also agree with you and Wilmott in thinking that Keys would probably be the simplest and the most helpful way of condensing the salient descriptive matter of the species and varieties.

As regards the various points you mention I quite think Heslop-Harrison is right in placing X R. perthensis as a var. of Sherardi in spite of Barclay's decision that it was a hybrid.

I am not so pleased with your decision to make R. canina with R. dumetorum and R. Afzeliana with R. coriifolia aggregate species. I think that they are very good and quite distinct species as they stand, and that it seems a great pity to have British nomenclature differing from Continental nomenclature more than is necessary. It seems to me that it would add considerably to the student's difficulties - one so often wishes to consult standard works by continental authors. That, of course, is a difficulty which should not be hard to get over but I cannot help feeling that in the botanical world the more nomenclature is brought into line the better. That, however, is merely my personal view which you must take for what it is worth.

About/

4-V-1937
To Wolley-Dod

TELEPHONE NO 50.
TELEGRAMS GUIDE, ARBROATH.

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HILLSIDE HOUSE,
ARBROATH.

Col. A.H. Wolley-Dod.
4/4/37.

About var. sclerophylla and R. obtusifolia I have no views at all as we never come across them here. Your next point is the Villosae group. I think (in spite of Heslop-Harrison) that you are right in not making pomifera and mollis specifically distinct. The other three species which most continental authors group under tomentosa Sm. seem to me fairly distinct although there is no doubt they merge one into the other on their border lines. Whether I can be of any use to you (which I should very much like to be) in giving you short segregating definitions is doubtful. As you say, my work has been mostly in the field and I have never thought out definitions which would be equally applicable to herbarium specimens. Are you pressed for time? I have been in very bad health all the winter and the doctor says I must get away for a change at once so we are leaving home this week. I feel quite unfit to tackle roses just now and it may be a month or more till we return. If it were not too late I should be very glad to do as you ask me.

I hope you will not make caesia the head of the Coriifoliae. I do not think it is common in Britain - in fact it has always struck me as one of the plants you referred to as being described from an individual bush.

I think you have a very difficult task in front of you but I am sure you will make a success of it and that it will be a very great help to students of Rosa.

Yours sincerely,

Mrs J. A. C. Coriifoliae
Coriifoliae

3.5.1948

"Fourwents",
17 Melrose Road,
Merton Park,
LONDON, S.W. 19

From AT Wilmott

Dear Mr Watson,

I have agreed to edit the B.S.B.I. Report on the Conference, and am glad to have your contribution on Rubus, forwarded to me by Mr Price. I am just going through it, and have a few questions arising to which I should be glad to have your answers.

(1). "Eurasian arctic species". I am not sure just what this means, and would be glad if you would make it a little clearer. Do you mean "Eurasian-Arctic", i.e. species inhabiting either Eurasia or the Arctic regions? I do not know any species truly Arctic except *R. ~~arcticus~~* arcticus and *R. Chamaemorus*, but you may have them in mind. Or do you mean "boreal" rather than "Arctic". I have had a good deal of experience with the Arctic Flora, and am a little puzzled, so I think that other readers may also be puzzled.

(2). "autonomous apomicts". I do not remember this expression and wonder if you mean "automatic apomicts", i.e. those which are always apomict (or should it be "obligate apomicts", I cannot remember just what the normal term is, ~~and~~ but somehow "autonomous" does not seem right to me. Autonomous should mean "under its own laws", or something like that, and I do not see how the idea fits in. Perhaps you can make it clear. Is it your own term, or is it generally used? Reading on I see that it means apomictic of their own accord, or something like that, so perhaps it is a reasonable use. I must enquire of some geneticist, unless you can give me a reference to its use in literature, as you possibly can do. Perhaps I just happen never to have met the term.

(3) "Morifer". This also is something I have not met, and perhaps needs a word of elucidation for others in similar state of ignorance! Turning over the pages, I see that "Rubi Moriferi" comes later, so I think the addition of "(see below)" would meet the case, if you agree.

(4) I had forgotten *R. saxatilis* as a subarctic species, but I am not sure that it is really arctic, and the same, but more so, of *R. Idaeus*, I should have thought. I write at home, being convalescent from Streptococcal poisoning, but can look these up when I get back. Arctic is so often written when subarctic would be more correct. Arctic does not mean "within the Arctic Circle", but part of the real Arctic Flora, which begins about ten miles north of the tree limit, according to Polunin and other specialists on the Arctic Flora.

(5) I do not think one should say that "*R. saxatilis* is perhaps *R. arcticus* x *Idaeus*". It is clearly a species and not a hybrid. Do you agree to saying "may have arisen from *R. arcticus* x *Idaeus*"? That would, I think, be better. Or, shorter: "is from *R. arcticus* x *Idaeus*". Similarly with *R. caesius*, but in accordance with Rules of Nomenclature one must say "*R. Idaeus* x *saxatilis*", and not "*R. saxatilis* x *Idaeus* (alphabetical order by Rule)".


(6) "Blackberries". In Cumberland the "blackberry" is *Ribes nigrum*! I have therefore always preferred the unambiguous name "bramble". I wonder if you would agree to the change? I have had some experience with "English names" in connexion with the Museum series of post-cards, and have always endeavoured to use an unambiguous name when a common one - like "bramble" - actually exists.

Having come to the end, I must say that I think it is exactly what was wanted by British botanists, and am very glad you have put it together so clearly. It will make a valuable contribution to the Conference Report.

I do not know that I entirely agree with your last paragraph, much as I welcome evidence indicating that the Glacial Period did not extinguish our Flora. I think the multiplication of forms is a product of the Glacial Period, and that in Pliocene times the genus was probably more simple. But that is a paper I have to write, and a subject on which I contemplated speaking at the Conference, so my views - not yet available - need not affect what you say, unless you wish to hedge a little. Probably it is best to leave it as you have written: it is the conclusion to which you have been led, although I may hope to be able to make you modify your opinion when I write up my ideas.

Anyway, many thanks for putting it together so nicely. If you will let me have your comments on my questions, it will be ready to send to the printer when I get the rest.

Yours sincerely,


A.J. Wilmott.

8.5.1948

"Fourwents",
17 Melrose Road,
Merton Park, S.W. 19

Dear Watson, (if you agree that Mr is not required between us),

I am very glad to have such a nice reply from you. I am rather horrified to find that I was wrong about "Idaeus". I have always been able to trust my memory, and cannot think how that error crept in.

*To be a
a character
standing up tall*
Bramble. You evidently have gone further than I. So far as I can now see, bramble is really --Brier, the thing that gets hold of you, and therefore is the turio. I must look further into it. Yet, as I said, blackberry does not everywhere mean Rubus fruit, as you say, for years ago, staying in Cumberland with two school teachers who were themselves locals, and knew their plants pretty well (also were intelligent), I was told that in that part of the world Blackberry and Wineberry meant Black-currant and Red-currant respectively, and our blackberry was universally bramble. Hence my effort to find an unambiguous name, and I thought that bramble would do. We certainly find bramble-jelly in many parts of the country. It was a minor point, on which I must, in view of your arguments, give way, but I feel the need of more investigation. I doubt if Rubus Chamaemorus would be reckoned as a bramble by any countryfolk.

Autonomous apomict. Thanks for the reference. It does not seem to me a very wisely made term, but as it is in use it will no doubt continue. So many terms are not wisely made, probably because it is really difficult to coin satisfactory new terms as well as because so many "scientists" are not linguistically inclined. I am glad to have that cleared up. I must look into the matter further when I get back to the Museum. I would still like you to reconsider adding the word "from", i.e. "R. saxatilis is perhaps from R. arcticus x idaeus". It is scarcely a mere "hybrid" in the usual sense of the word in systematic, even in it is "of hybrid origin". It seems to me that one confuses things by introducing another meaning to the word "hybrid" when one can so easily avoid it. To my mind it is misleading to say that it is "R. arcticus x idaeus" (even with perhaps) when its behaviour as a species, with habitat, distribution etc. different from the two "parents", surely indicates that something besides mere hybridisation has gone into its production, whatever that something is. And "from" seems to me to remove the objection so completely without any other consequence to your argument. We can perhaps discuss this when I get back. It is scarcely a "microspecies" surely? I should have considered it a macrospecies, but perhaps I do not know enough about it.

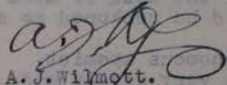
Arctic. Thank you. I think the general use of arctic as merely "inside the arctic circle" does cause much confusion in many ways and is best avoided, and I am glad you agree to remove the possibility of misunderstanding.

Glacial Period effect. I have not properly investigated the distributions yet, but I believe that the apomict genera - if I may thus put it - Hieracium, Taraxacum, Alchemilla, Rosa, Rubus, Crataegus in N. Am. - are critical within the area of glaciation in general, and simple outside that area, and I am inclined to believe that they are the result of the consequent vegetation swirls bringing together allied previously separated species which, crossing, could not cross normally but produced the cytological abnormalities. I would like

to have your opinion as to how far the general distributions in Rubus would agree with this or not. In Asia Minor and really southern (warm) Europe, or North Africa, these apomicts are not in evidence to any extent, I believe. It is so in Hieracium and, I think Alchemilla. Taraxacum may be affected by distribution as a weed of cultivation, but even that is simple comparatively, I think, outside the glaciated area. If this is so, it must mean that ~~in~~ ^{some way} the production of these critical genera occurred in relation to the glacial period in some way. That is the general idea to be worked out in detail. I should therefore expect that Rubus in the Pleistocene was as simple as other simple (more or less) genera. I wonder what evidence you have against this?

Again thanks for your reply. The only outstanding point is, I think, my dislike to calling R. saxatilis a "hybrid" between arcticus and ideo, and caesus "R. ~~sax~~ ideo x saxatilis". Somehow these two statements together mutually destroy saxatilis as merely a "hybrid"?
 (or at any rate, "caesus" - a hybrid of arcticus).

Yours sincerely,


A. J. Wilmott.