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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.

52 Huntington Road
Cambridge AL 94
March 3, 35

Dear D. Sharman,

Thank you very much
of your letter. I am very glad you
liked the Brit. Rev. paper. I think
your way of expressing the "gestalt"
comparison of inflorescence of flower is very
vivid & I am putting your letter with the
comments & notes on the paper in a cardboard
when I work over it again later (as I
must do) for a book.
I will be returning to "droppers", I will take

advantage of your generous offer &
let me see your slides.

With all good wishes for your
work,
Yours sincerely
Agnes Arber

Botany Department, A 295

Tel. No. 2025L

THE UNIVERSITY,

LEEDS, 2.

Dr A. Arber,
52, Huntingdon Road
Cambridge.

14 June 1939

Dear Mrs. Arber,

Thank you for the re-print of
your paper "Studies in Floral Structure. I".
Although working on leaf & shoot development
I am becoming keenly interested in points of
floral anatomy, since I cannot believe
that floral organs at their initiation can
differ so radically from ordinary vegetative leaves:
whether we agree ^{or not} with Gignac etc in believing
that the two types of bracts are essentially
different in origin, I feel convinced that
the same fundamental laws underly the
space relationships etc of the two types of
bracts.

I hope, in the near future, to return

P.S.O.

to put a few points down for your consideration,
though whether they will be res or of any
use, I can't say.

Yours truly,

B. L. Sharron.

52 Huntingdon Road
Cambridge AL96
June 15.37

Dear Dr. Shorman,

Thank you for your
interesting letter.

Personally I remain unconvinced
by Frejman's arguments. The ontogeny
of foliage leaves & flower parts does not
differ any more than one would
expect, considering the difference of the
end-result; and he seems some of my
too little attention to the fact that the
only fair comparison for the flower
is with a short shoot, not a shoot
of unlimited growth. I suspect that
then; something radically wrong in

one way of looking at ontogeny, &
thinking of primordia & their behavior.
It is easy of course to say this — the
difficulty is to lay one's finger on the
weak spot.

Yours sincerely
Agnes Arber

1
Sep. 4, 1939
my notes for Shamin's
of apex. ~~the~~ short flower

AQ. 97

Department of Botany,
The University,
Leeds 2.
22 July 1939.

Dear Mrs. Arber,

Your papers and letters have stimulated me to thinking about flowers and to seeing how far the ideas one has about the courses of events taking place in the production of vegetative leaves, can be applied to some of the problems of floral anatomy. Although essentially working on leaf development (on monocotyledons mainly and grasses in particular) flowers are always in the back of my mind as possible solutions or more probably stumbling blocks, to many of the questions which arise. With a terrific consciousness of rushing in where angels fear to tread, and realising that a person of limited experience such as mine, cannot really even touch the problems, I am sending you a few thoughts in the hope that you will be good enough to have a look at them and criticise them if you have time. Please be merciless and please don't spend much time on these odd jottings.

I realise that most of the following is highly speculative and probably also extremely dangerous. However I am after a general way of thinking rather than rules and laws.

If in the arguments I seem to be using very common terms like "lump" or "bump" etc, please forgive me - it is

deliberate as I have the feeling that much confusion and slipshod thinking can arise from the use of words like "leaf primordium", leaf initial", carpel rudiment", which automatically lead to a mental begging of the question and to the assumption of a verdict before a discussion of the case.

I must ask you to forgive me if I don't make myself clear in many parts: it is extremely difficult to state clearly the problems themselves, let alone try and give a clear picture of the sort of things which may perhaps underly them. It seems as though one feels the ideas rather than logically thinking them out.

In your Biol. Review paper you mentioned the need for regarding the plant as "a whole" and also for remembering that each plant arises de novo. I feel more and more strongly that this is important: to this I would add that when considering primordia (foliar or floral) one ought to remember that evolutionary, genetic and other influences notwithstanding, the origin, the position and the ~~the~~ subsequent development of a primordium is the resultant of the influences at work in that individual plant at that particular time. Genetical and other factors can only affect the initiation of a primordium by way of food supply, rate of cell division, rate of wall thickening etc., and not by some magic external influence. Hence when one studies the initiation of primordia one should be solely concerned with the factors operating at that instant and on the spot: they are

at play then and there and not back in the dim and distant past.

I am sure that the use of the vascular ground plan, so favoured by some floral anatomists, is not only not valid, but is also highly dangerous and misleading. I am sure that one cannot regard the vascular plan of a flower in the way that Zoologists regard the vertebrate skeleton. It is much more flexible and, I am sure, is often the result of the shape of an organ rather than the cause.

Similarly I feel it may often be rather likely to lead to too fixed a mind to regard the ~~max~~ number of floral parts of a flower of any particular species as conforming to a ground plan. The ground^d plan ought to be remembered as a sort of average case, ~~taken~~^{taken} from the peaks of ^{frequency} distribution curves for each floral part. There seems too much tendency, especially in general textbooks, to think in units and to put in the celebrated X wherever the author thinks the Deity ought to have graced the plant with another stamen etc.!! (I admit stamens do sometimes get aborted: such is entirely different) After all because some establishments have greenhouses one doesn't insert a dotted outline in every plan and say it has been suppressed in the evolution of the design!!! Similarly be^{ca}use Privet normally has leaves in pairs, no one would say that one is suppressed at every node merely on the grounds that tricusssates are easily found.

If one regards the number, distribution etc., of the floral parts as being an expression of the forces at work at the

time of their initiation, it then becomes obvious that the floral pattern of any species is not at all a definite thing, but is merely the expression of the resultant of a number of factors any one of which may in some cases be altered, giving an "anomalous flower".

After this preamble I will leave you to consider some of the points in the following. Most will be taken up with an exposition of the sort of things which I feel underly vegetative growth: I am tending to leave it to you to see how far they can be applied to flower problems. I only hope that here and there an odd point may be useful.

Yours truly,

B.C. Sharman

B.C. Sharman.

52 Huntingdon Road

Cambridge

July 25

1939

Dear Dr Sharman,

Thank you for your most interesting letter; your

enclosure I have not yet looked at, because of a certain hesitation in my mind which I must explain.

I have been asked repeatedly to read and criticise people's unpublished work, and hitherto I have always refused. In

the different cases there have been all sorts of special reasons for

these refusals, but there is one general reason, which would

also apply to your MS. It is the question of property in what a small child once called "mydeas". If one reads someone else's

unpublished work in a subject bearing on one's own, and finds it

interesting and stimulating, one may unintentionally absorb more

from it than is fair; and also one may find oneself in the difficult

position of discovering in one's friend's MS the same

ideas that one had been pursuing oneself, but carried to a point

which one had not yet reached. No doubt you have in view

memoirs into which your theoretical ideas will be worked, and

considerations such as these which I have been trying, clumsily, to

indicate, make me feel that it might be better for me to wait

until you have published your thoughts on these things and thus

made them common property. Anyway, I will not read your enclosure

until you have had time to think over the question again.

I am very much in sympathy with your general point of view. What you say about "feeling" the ideas I believe is peculiarly relevant in morphology, which demands something over and above what philosophers call "discursive" reasoning. For a long time I have been getting together material for a paper on the leaf which I hope to offer to Biol. Rev., and also for a book in which I want to consider the morphology of flowering plants on general lines. My purpose in these two bits of work is in agreement with yours, for it is a "general way of thinking" that I am after; I believe it is a change in this which is what is wanted in botany. For this reason I have been for some time now struggling with as much philosophy as I can grasp (alas that it is little) to try to clear up my approach to the subject. I think that you might not regard this as waste of time, though most botanists are inclined to use "metaphysical" as a term of reproach. Have you, I wonder, read R. Goldschmidt's "Physiological Genetics" (1938)? So far as I could understand it, it seemed to show that (at least in some cases) identical results might be produced by genic and by environmental action. It seems as if this ought to be borne in mind in analyzing ontogenies. About "suppression" I agree very much with what you say; indeed I have said parallel things about the cotyledons of Dicots and Monocots, and the "threeness" and "fourness" of the leaves of Trillium and Paris on p. 179 of my book on Monocots. My daughter and I were enchanted with your greenhouse comparison, and we began to work out how superbly curvy small "single-fronted" houses might be made to figure, if treated as a mansion with almost everything suppressed!

Yours sincerely
Agnes Arber

Department of Botany,
The University,
Leeds 2.

Thursday, 27 July 1939.

Dear Mrs. Ains,

Thank you very much for your letter which I received today. It was extremely kind of you to write so soon as I am sure you are extremely busy.

About the stuff I sent - I am afraid I may have given you a slightly wrong impression. It was written for you and does not represent a paper in embryo but merely a few scattered thoughts of one who is essentially a "vegetative periodical" writer!! I have not the slightest intention of publishing any of it, even if it should contain anything publishable (which I doubt). It was done with a threefold purpose, (1) to show you what a person concerned with leaf development might think (2) to get a lot of hazy notions off my chest!! (3) a bit of practice in juggling with developmental problems.

So you need have no fear in perusing it or in using any ideas it has (I doubt if any of it is really new to you). Anything in it which should strike up an idea for you, you are most welcome to use. I only hope that

amongst it all there may be an odd thing of use.

For myself, I am still deeply absorbed in grass apices and except for odd notes on things which turn up (such as the stamens - lodicules which appeared in the ear lot. of maize) I intend to try and keep to this aspect of the Gramineae and perhaps move on later to leaf development in the Monocots.

Thank you for the reference to Goldschmidt: I had not come across this but shall get it out and study it. It sounds a most interesting reference.

I look forward to your coming paper on the leaf.

Yours sincerely,

B. C. Sharran.

AE 100

52 Huntingdon Road

Cambridge

July 30

1939

Dear Dr Sharman,

I am returning herewith your MS, and the paper you kindly enclosed. I will jot down below such comments as have occurred to me.

Some thoughts on whorled phylletaxis.

You speak as though there were an antithesis between the factors, such as shape of apex, which presumably determine the number of primordia arising simultaneously, and "genetical considerations", and in support of this you quote the common occurrence of tricusates in Privet, which is generally decussate. But I hardly think that this is a reason for claiming that such characters do not possess a genetical basis. One must suppose that the genic constitution of Privet is such that it produces a apex-type with characters/for one of which the frequency curve has its principal peak at two-leaved whorls, but a secondary peak at three-leaved whorls (You yourself recognise this "graph" way of thinking when you speak of the ground-plan of the flower). We no longer think of the gene as passing through life carrying all sorts of hard-and-fast characters in its handbag, so to speak, and distributing them, like coins of fixed value, at the appropriate moments. Its action is not so simple as that. It can rather be thought of as possessing the "sphere of influence" which you claim for

your young leaf.

I agree with you that the vascular ground-plan of the flower is an utterly different thing from the vertebrate skeleton. At the same time it is an integral part of the whole floral symmetry, and should no more be neglected than should the external shape. I regard it neither as the cause nor the result of the external form, but as correlated with it. I cannot agree with you that "the floral pattern of any species is not a definite thing"; it can scarcely be denied that it conforms to a definite type, with a range of variation which, when studied often proves to have remarkably definite characters.

Have you looked through O. Penzig's "Pflanzen-Teratologie" (2nd ed. 1922, much improved on the first)? It interprets "abnormality" very widely, and includes (under families, genera and species) detailed references to the literature on such cases as your tricussate Privet; with the third volume, which is all bibliography, it is invaluable.

I think also that if you looked systematically through Engler and Prantl's "Pflanzenfamilien", you would probably find a good many examples of whorled leaves to add to your list.

General points about primordia

p. 1

I fully agree that the separation of "leaf" and "axis" is artificial, and I think the Leeds idea of treating (leaf + its sphere of influence) as a unit may have a great value from the point of view of growth physiology. But from the standpoint of morphology it seems to me as artificial as "axis", when one considers that it leads, as you say, to conceiving the construction of the

systems of dicots as sectorial, and of monocots as consisting of hollow inverted cones.

p. 3

I must confess that I cannot get over some scepticism about the possibility of dividing up the leaves at a shoot apex into these three neat classes, and of identifying the numbers in each group with these in the phyllotaxis cycle. If proved, this theory will be of course of the utmost importance, but the paper you enclose is so slight in its dealings with the question, that no doubt it is intended merely as a preliminary note, and one must await a completer treatment of the subject.

I also feel a difficulty, both in this paper and in some of the remarks you make in your MS (e.g. the end of p. 4 and beginning of p. 5, and the last paragraph of p. 7) that you use assumptions which may indeed be true, but which so far as I can see, are wholly unproven. For example, on p. 382 of the paper you send, the sentence beginning "When this" (at end of line 26) contains assumptions about the food supply to primordia which can be nothing but pure speculation, but which are stated as if they were matters of known fact. I am not trying to argue against these assumptions; they are ingenious and they may be correct - all I mean is that I think they ought to be treated as working hypotheses which need proof, instead of being simply adopted, and then built upon, as if they had the quality of "givenness", like the postulates in Euclid.

p. 8

Obdiplostemony

I doubt if one need worry much about obdiplostemony. As a leaf and its axillary bud can arise on the same radius of a shoot, there seems to be no difficulty, as far as growth physiology is concerned, in a leaf and another leaf arising on the same radius.

p. 9

Penzig (l.c. vol. 11, p. 456) gives an account of leaf variation in Teasle; leaves in threes appear to have been often recorded. On the same page there is a mention of Merina longifolia having the leaves generally tricussate, but sometimes in fours or fives. pp. 9-10

In comparing the flower and dwarf shoot, I had not thought of the point you make (1st par. of p. 10) - "when there is a change becoming higher". I think this is an excellent idea, and if you will allow me, I will quote you on this point, if and when my book sees the light - but this will not be for years, and I hope before that you will have had occasion to put the idea into print yourself, so that I can quote from that.

Your suggestion (p. 11) as to the part played by the palea in affecting the transition from two orthostichies to three is a very interesting one. But I do not quite see how it ~~can~~ be made to hold water when one thinks of dicots with decussate leaves, which have to "double" prophylls to help them to bridge the change from leaf to flower symmetry, and yet manage to do it. It is true that ^{direct} prophylls are ~~are~~ sometimes not exactly opposite one another; perhaps they may show enough one-sided approximation for your purpose.

How many thanks of your kindness in letting me see your very interesting & stimulating notes, also many thanks of your second letter, so generously given & the me made free with your ideas, yours sincerely Agnes Arber

Dear Dr. Thurn, I am sorry I have not yet had a chance to
reply to you, but I have just seen your
concerns have occurred
Some Thoughts on Whorled Phyllotaxis. *Wm.*

Copy my comments on Thurn's notes with ~~the~~ July 30 39

Posted
Mrs. Thurn
July 31 39
+ wrote for
Aug 16-39
to say
he had received it

You speak as though there were an antithesis between the factors, such as shape of apex, which presumably determine the number of primordia arising simultaneously, and "genetical considerations", and in support of this you quote the common occurrence of tricussates in Privet, which is generally decussate. But I hardly think that this is a reason for claiming that such characters do not possess a genetical basis. One must suppose that the genic constitution of Privet is such that it produces an apex-typet with the characters of which the frequency curve has its principal peak at two-leaved whorls, but a secondary peak at three-leaved whorls (You recognize this "curve" way of thinking when you speak of the ground-plan of the flower). We no longer think of the gene as passing through life carrying all sorts of hard-and-fast characters in its handbag, so to speak, and distributing them, like coins of fixed value, at the appropriate moments. Its action is not so simple as that. *It can rather be type of as passing as*
of the influence of other factors
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often proves to have remarkably definite characters.

Have you looked through O. Penzig's "Pflanzen-Teratologie" (2nd edition, 1922) (much improved from first edition)? It includes (under families, genera and species) detailed references to the literature on such cases as your tricussate privet; ^{was} the third volume, which is all bibliography, ^{is} is invaluable.

I think also that if you looked systematically through Engler and Prantl's "Pflanzenfamilien"; you might find a good many examples of whorled leaves to add to your list

General Points about Primordia

p. 1

I fully agree that the separation of "leaf" and "axis" is artificial, and I think ^{it is} your idea of treating (leaf + its sphere of influence) as a unit may have great value from the point of view of growth physiology. But from the standpoint of morphology it seems to me as artificial as "~~leaf~~" and "axis", when one considers that it leads, as you say to conceiving the construction of the stem of dicots as sectorial, and of monocots as consisting of hollow inverted cones.

p. 3

I must confess that I ^{cannot} have ~~feel~~ some scepticism about the possibility of dividing up the leaves at a shoot apex into these three neat classes, and of identifying the numbers in each group with the phylletaxis cycle. If proved, ^{the theory} it will be of course ^{the most} most important, ^{in terms of} but the paper you enclose is so slight in its ^{is} treatment of the question, that no doubt it is ~~intended~~ intended merely as a preliminary note, and one must wait a completer

treatment of the subject.

I also feel a difficulty both in this paper and in some of the e.g. the sentence at beginning of p. 5 and end of p. 4 remarks you make in your MS, that you state, as if they were matters of fact, assumptions which so far as I can see, are wholly unproven, though they ^{may} be true. For example on p. 382 of the paper you send ^{the} sentence beginning "When this, at end of line 26 contains assumptions about the food supply to primordia which can be nothing but pure speculation, but which are stated as if they were matters of known fact. I am not trying to argue against these assumptions; all I mean is that I think they ought to be treated as working hypotheses which need proof, instead of being simply adopted, and built upon, as if they had the quality of "givenness". ^{but I postulate in Euclid.}

p. 8.

Obdiplostemony.

I doubt if one need worry very much about obdiplostemony. As a leaf and its axillary bud can arise on the same radius of a shoot, ^{there seems to be no difficulty} why should there be any difficulty, as far as growth physiology is concerned, in a leaf and another leaf arising on the same radius at slightly dif

p. 9

Penzig (l.c.) Vol. 11, p. 456, gives an account of leaf variation in Teasle; leaves in threes appear to have often been recorded. On the same page there is a mention of Morina longifolia having the leaves generally tricussate, but sometimes in fours or fives.

opp. 9-10

In comparing the flower and a dwarf shoot, I had not thought of the

point you make (1st. par. of p. 10) - "when there is a change, it is usually so that the number of floral leaves in each successive whorl is decreasing, giving the idea that the ~~system~~ ^{system} is either petering out or is being overshadowed by the development of the primordia it has produced. In vegetative shoots, the phylletaxis usually becomes more and more complex as the plant is continually building up an excess of food ^{over} its absolute requirements: more and more primordia can be maintained in young stages - in other words the phylletaxis is becoming higher". I think this is an excellent idea, and if you will allow me, I will quote you on this point, if and when my book ~~comes out~~ ^{see 5/2/20} - but this will not be for years and I hope before then you will have ^{had occasion to} put the idea into print yourself so that I can quote from that.*

I also think that your suggestion (p. 11) as to the part played by the palea in affecting the transition from the two orthostichies to three is a very interesting one "It seems ~~that~~ ^{that} one could regard the palea of an ordinary grass (as illustrated by your type drawing on p. 140 of your Gramineae) as functioning during the development of the flower in much the same way as do the bifid leaves with two blades sometimes found in dicots when a decussate changes to a tricussate in vegetative shoots." But I do not ^{quite} see how it can be made to hold water when one thinks of the Dicots with decussate leaves, which have no "double" prophylls to ^{help them to} bridge the change from leaf to flower symmetry. ^{It may be to do it. Perhaps a smaller set of opposite prophylls: perhaps lying on the opposite side of the palea.}

I hope you will extend your work to the flower; a comparison on broad lines of a ^{development in cereals I have} great many types would be a good test of your theory of phylletaxis change

W. H. H. ^{W. H. H. your studies in botany are very interesting & helpful}

* May 18.35 I, then really agree in this case of *Scirpus ustulatus* Linn. exsp. - in center palea & stamens a complex habit in fruit with 2 (May 18.38) for *Polygonum* ^{under 3 whorls} ~~leafy~~ ^{the small number of whorls are}

52 Huntingdon Road

Cambridge

July 25

1939

Dear Dr Sharman,

Thank you so much for your most interesting letter. Your enclosure I have not yet looked at, because of a certain hesitation in my mind which I must explain.

I have been asked repeatedly to read and criticise people's unpublished work, and hitherto I have always refused. In the different cases there have been all sorts of special reasons for these refusals, but there is one general reason, which would also apply to your MS. It is the question of property in what a small child once called "mydeas".

~~I am working at a paper which I hope to send to Biol. Rev. on the leaf, (intended as a pendant to the one on the flower) and I am also getting together material for a general book on morphology (flowering plants). Now if I read your notes, though I shall not of course intend to appropriate anything ydcb, if yours, I may perhaps unconsciously do so~~

~~I am rather a subject bearing on one's own~~
If one reads someone else's unpublished work and finds it interesting and stimulating, one may ^{unintentionally} absorb more from it than is fair; and also one may find oneself in a difficult position of ^{discussing in my MS} one finds the same ideas that one ^{has} had been pursuing ^{in one's} ~~unconsciously~~ to a point ^{one} had not ^{reached} already worked out.

Pyrusidyl fenestras (1938).
 I am very much in sympathy with your general point of view. What you say about "feeling" the ideas I believe is particularly relevant to morphology which demand something over and above what philosophers call "discursive" reasoning. For a long time I have been getting together material for a paper on the leaf which I hope to offer to Biol. Rev., and also on a book in which I want to consider the morphology of flowering plants on general lines; here again I am in agreement with yours for it is "a general way of thinking" that I am after; I believe it is a change in this which is what is wanted. For this reason I have been for some time now struggling with such as much philosophy as I can grasp (alas that it is little) to try to clear up my approach to the subject. I think that you may not regard this as a waste of time. I have not yet really understood Schleicher's metaphysics with this, though most botanists use "metaphysics" as a term of reproach.

I should very likely find your ideas very helpful, but would it not be better for me that I should wait until you have published them and they become common property?

No, but you will have your ideas in common property. I should very likely find your ideas very helpful, but would it not be better for me that I should wait until you have published them and they become common property?

Anyway, I will not read your enclosure until you have thought over the question again.

I am very much in sympathy with your general point of view. What you say about "feeling" the ideas I believe is particularly relevant to morphology which demand something over and above what philosophers call "discursive" reasoning. For a long time I have been getting together material for a paper on the leaf which I hope to offer to Biol. Rev., and also on a book in which I want to consider the morphology of flowering plants on general lines; here again I am in agreement with yours for it is "a general way of thinking" that I am after; I believe it is a change in this which is what is wanted. For this reason I have been for some time now struggling with such as much philosophy as I can grasp (alas that it is little) to try to clear up my approach to the subject. I think that you may not regard this as a waste of time. I have not yet really understood Schleicher's metaphysics with this, though most botanists use "metaphysics" as a term of reproach.

About "suppressin", I agree completely; indeed I have said parallel things about the cotyledons of Picots and Mono and the leaves of Trillium and Paris, on p. 179 of my

as from Department of Botany,

Tel. No. 2025L

THE UNIVERSITY,

LEEDS, 2.

A2101

Thursday 16 Aug. 1939

Dear Mr. Arber,

I am afraid I have only a few minutes to spare, but I would like to write to thank you for your most useful comments on the "jottings" I sent about flowers as seen from one dealing purely with problems of vegetative primordia. It was very kind of you to criticize them so carefully - your remarks were most helpful. With regard to your remarks about the possibility of food relations etc. as being largely the determiners of the position etc. of emergence of a new primordium - I

am only too willing to admit that it
is purely an assumption and
possibly a dangerous one at that! I
don't know about the position in flower
providia, but it gives a very useful
"working hypothesis" for many of the
~~events~~ tendencies not and course of
events followed by the ^{repeated} providia.

Thanking you very much for
all your trouble (and also for
suggesting Penzig).

Yours sincerely,

B. C. Shapran.

do from Department of Botany

Tel. No. 20251

THE UNIVERSITY,
LEEDS, 2.

20 August 1939

Dear Mrs Arkar,

Thank you for your P.C. which came on the 19th. I am sorry to have caused so much trouble. I received the M.S. safely as you will have gathered from my note which will have crossed your P.C. My delay in notifying you about the M.S. was caused by my being in Ireland doing a little combined holidaying and botanizing.

Thanking you for the trouble

Yours truly,

B. C. Sharran.

Al 102

52 Huntingdon Road

Cambridge

August 14

1941

Dear Dr. Sharman,

I went to the Gardens today to see your
teasle which was indeed a magnificent object. ^{The Counter} ~~They~~ might not
want to part with it now, but if you want to work it out, shall
I ask them to give it to me when they are cutting down the bed
for the autumn, and then I would send it to you? It is so
huge that it would have to be cut up to send you, which I
fear would rather spoil it. ^(I think you could still see the essential) All the various abnormalities in
the way of split leaves, double buds etc. are most interesting.
Penzance quotes a certain amount of literature on these abnormal-
ities in *Dipsacus*, but like most teratological things, I
expect it has not been well worked out.

I hope you had a pleasant journey home ^{vacate} - also
that your return to the middle of the town on Tuesday night
was not too trying in the raid. We had a good deal of bombing
after you left, and I regretted that I had not persuaded you
to stay longer in shelter.

It was a great pleasure to get to know you
personally, and to hear about your work.

With all good wishes,

yours very sincerely

Agnes Arber

A 2103

52 Huntingdon Road

Cambridge

Oct. 13

1941

Dear Dr Sharman,

I am so sorry not to have been able to get you the teasle before. As I told you, the Superintendent promised to send me a post card when it would be all right to take it, but I think he must have forgotten. As it was getting so late, I went to the Garden today, and though I failed to find him, the teasle had so obviously ceased to be an ornamental object, that I appropriated it. I had meant to send you parts of the (?) laterals as well, as they showed forked leaves, etc., but I found they were hopelessly shrivelled.

I should have gone ~~down~~ sooner, but we have been rather put out of things by a tiresome little Blitz in this road at the end of September, but no casualties except most minor ones, I am glad to say. It was a Sunday evening - very dark and pouring wet, and there had been no siren, when we have ^{eight} ~~nine~~ bombs along this road finishing up with a bread basket of incendiaries a little nearer the centre of the town. The bomb nearest to us made a crater in the road near enough to us to throw up a chunk of the road surface on to our roof smashing a hole through which the rain poured. Our windows and ceilings stood, but people on the town side

of us had a great deal of damage. One bomb burst in the basement of a school for small children and also destroyed their air raid shelter, but as it was at night the children were safely away. It was a little unlucky for Muffel that it was the night before she began her practice teaching at the Training College, and one is not on the top of one's form after such a night. It made it worse that there was no means until dawn of learning exactly what had happened.

With kind regards and hoping that your going into the Navy will be deferred - (I suppose you have never considered the work which various botanists are doing under Hamshaw Thomas (as Air Force) in interpreting aerial photographs? I imagine that one might apply direct to him about how to get into ~~it is one, certainly to it.~~)

it if you felt drawn to it

Please forgive the (small typing)

Agnes Arber

Agnes Arber

A2104

52 Huntingdon Road
Cambridge

Oct. 27

1941

Dear Dr Sharman,

Thank you very much for your letter, and for sending me such a variety of macerated material, so beautifully put up. I am very glad too to have the directions for doing it. I have enjoyed examining it this morning - it must carry conviction splendidly to students to see the elements in this three-dimensional way.

I do hope you will have a good term and that the calling up may be deferred indefinitely - it is really of such extreme importance that people like you should be left to keep intellectual life going in these bad days.

With kindest regards to you from us both,
yours very sincerely

Agnes Arber

A 2105

52 Huntington Road

Cambridge

rec. 11

1941

Dear Dr Sharman,

I am indeed most grateful to you for having taken so much trouble to let me see your remarkable macerated material. At present I have not had time for more than a cursory examination, but I am very much struck especially by Helianthus with its superb slime strings and plugs. I will look up the reference in Priestley and Scott at the first opportunity. I am also much interested to see the wood of Primys of which one has heard so much. Thank you very much indeed.

We have had a glorious time of peaceful nights lately - varied by an unnecessary night up a week ago when an over-zealous local A.R.P. official insisted on our sitting up all night during a practice raid, because there might be real incendiaries among the practice duds.

I fear the new conscription regulations will affect the universities very severely. Muriel hopes that she will be allowed to teach next year, but at present there has been no clear ^{about Training colleges} statement about whether women/who are intending to teach, but have not yet actually entered the teaching profession, are to be reserved.

20 2A
What a curious December this is! On Dec. 9 we saw snowdrops,
primulas, pansies etc flowering in a garden near here

With every good wish for as much happiness as is
possible under the present condition at Christmas and in
the New Year,

yours very sincerely

Agnes Arber

AQ106

TEL. 1493.

56. BATEMAN STREET,
CAMBRIDGE.
Jan: 4 1927

Dear Agness

I hope you will like these
plates particularly as there are some
of grasses.

A humble gift is this,
Not worth a kiss
And if you give the same,
You are to blame,
If these you don't desire
Don't ~~use~~ burn with fire
And do not throw away
My gift, I pray
For if you do you'll ~~lose~~ lose
Some curious view
Of plants alive or ~~dead~~ dead
Pressed, or ~~red~~
And it would pity be
Such waste to see
And so adieu dear friend
This is the end
Love From

~~H. H.~~ Giles Robert Sob

Dept. of Botany.

A2157

BRITISH MUSEUM (NATURAL HISTORY), CROMWELL ROAD, LONDON, S.W.7.

Date 7/2/1927

MEMORANDUM.

From

To

J. Ardagh

Dr. A. Arber.

The plate of *Carex alpina* Swartz. (L. Vahlia)
was drawn by R. K. GREVILLE, see "Notes
on the Drawings for English Botany" (Garny)
in Journ. Bot. Aug. 1904. In this week's Notes
and Quere's (5 Feb. p. 107) ^{Vol. 42 Suppl.} Archibald Sparke states
that "The Wild Garland" and "The Life of Linnaeus"
are by Miss S. Haring.



ISTITUTO - ORTO
DELLA
R. UNIVERSITÀ DI PADOVA



Dr. Agnes Arber
52 Huntingdon Road

Inghilterra

C A M B R I D G E

A 2109
L'ISTITUTO BOTANICO DELLA R. UNIVERSITÀ

~~via Garibaldi 10 - 35100 Padova - Tel. 049/809111~~

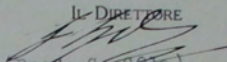
14 IX 1945

Gentile Signora, rispondo alla Sua del 8 Agosto.

L'individuo di *Chamaecrops humilis* piantato in questo Orto nel 1584 è in ottime condizioni e ha fiorito anche nella decorsa primavera. Tutto è intatto in questo Istituto. Con rispettosi saluti

PADOVA (ITALIA)

IL DIRETTORE


(Prof. G. Galini)

when considered in itself, and now - as a further test of the efficiency of the theory - we will apply it to the comparison of Papaver and Platystemon. In the section of the base of the ovary of Papaver Rhoas (Fig. 4, A 4), the carpels, which at their first appearance have closed ^{loculi} ~~loculi~~ (cf. also Fig. 1, B 1) surround the vestigial apex of the floral axis. Such sections may be compared closely with that of Platystemon californicus, cut at a corresponding level, which is drawn in Fig. 7, A 1. If the marginal bundles of adjacent carpels were united, and the ovules were borne on a placental outgrowth from the fused margins, we should arrive at a scheme of structure recalling Papaver. Fig. 7, A 2, with its open loculi, if it were so modified, might be compared with the section of Papaver Argemone drawn in Fig. 2, C 1. Fig. 7, B, is cut through the upper part of the a very young ovary of Platystemon, and we see that, just before becoming stylar, the loculi of the carpels repeat the closure which we noticed in the base. This closure of the loculi at the tip of the ovary can be paralleled in Papaver. The stigmatic members of Platystemon, like those of Papaver, are each formed from a single carpel only; but those of Platystemon differ from those of Papaver in incorporating the median as well as the lateral regions of the carpel. We thus see that, though at first glance the gynaecium of Papaver seems remote from that of Platystemon, the fundamental relation of these superficially dissimilar types becomes clear when they are considered in the light of the theory here adopted.

IV. Summary

This study is based upon serial sections of the gynaecium of Papaver Argemone L., P. Rhoas L., P. hybridum L., P. nudicaule L., Argemone mexicana L., Meconopsis aculeata Royle, Platystemon californicus Benth., and other Papveraceae. It is shown that the minute structure does not support the hypothesis that the gynaecium of Papaver includes two types of carpel - sterile and fertile (see also Arber (1937)). On the contrary, the evidence favours the view that the gynaecium consists of a single whorl of carpels, which are identical in type and are equal in number of the placentae, and to the duplex

7
Puff Aug 26. 29

I was indeed ~~delighted~~ delighted to receive
you by hand letter; was much gratified to
me that you should recognize & thank
your work by word. I have expected been startled,
in word, for very little knowledge, as the
frequency in which I have seen in discussions
your study & by you on which I have unpolished
notes & drawings; even to some aspects of your own
both the response, although in my own steps take a
desire to see, then.

The present will you & closed inquiries
is more desirable, but I feel sure that a
reaction is coming, to wait, the work?
yourself & you the world, reveal with constant
to the eye - is true spirit.

Digitized by Hunt Institute for Botanical Documentation

Thank you for reference of Richard's
columns & Malfe's work;
interest & greenness.

I am venting to send you copies, three
of my books shown as state in print (Manuscripts:
Gummea; & Herbar (ed: 2)). I shall give you
much pleasure if you will accept them - a letter
replied. I should be as pleased (to you go, &
in my ways was not shown) share with me.
I think to a great pleasure; I could mean
you on Rhode, but few times is unlikely to
I shall be all & due to Congress.

Direktion
des Botanischen Gartens und Institutes
der Universität

Professor Dr. Wilhelm Troll

Al 110
Halle (Saale), den 23. Mai 1939
Am Richter 1

Verehrte Frau Arber!

Die freundliche Aufnahme, welche meine und meiner Schüler Arbeiten bei Ihnen finden, freut mich außerordentlich. Die meisten Morphologen sind ja der klassischen Tradition unserer Wissenschaft so sehr entfremdet, daß ihnen schon alle Voraussetzungen des Verständnisses fehlen. Aus Ihren Arbeiten dagegen spricht mich stets ein verwandter Geist an. Besonders weiß ich Ihre mit tiefer Einsicht in das Wesen morphologischen Denkens gepaarte kritische Haltung zu schätzen, die man sonst weithin in einem fast erschreckenden Umfang vermißt.

So begrüße ich auch Ihre Stellungnahme zu Mattfelds neuer Theorie von der Herkunft der Petala auf das lebhafteste. Erstaunlich, daß man bisher die Hohlschuppennatur der Blumenbaltnähängsel bei den Silenoideen übersehen hat! Bei dieser Gelegenheit mache ich Sie auch auf die Kritik Eckardts in der Zeitschrift f. Bot. aufmerksam, die ebenfalls zu einer Ablehnung des Mattfeldschen Standpunktes kommt.

In der Hoffnung, Sie im nächsten Jahr in Stockholm zu sehen und persönlich kennen zu lernen, bin ich mit den besten Grüßen

Ihr sehr ergebener

W. Troll

Veröffentlichungen
aus dem Botanischen Institut
der Universität Halle
1938/39

Anschrift: Am Kirchtor 1, Halle (Saale)

- ECKARDT, TH.: Das pseudomonomere Gynoeceum. *Chronica Botanica* 4, 206—208 (1938).
- FÖCKLER, H.: Über den Einfluß des Lichtes auf die Atmung farbloser assimilierender Gewebe und seine Rolle beim „funktionellen Sonnenstich“. *Diss. Jb. wiss. Bot.* 87, 45—92 (1938).
- HAECKEL, I. und W. TROLL: Botanische Ergebnisse der Deutschen Hindukusch-Expedition 1935, 1. Lieferung. *Repert. spec. nov. regni veget.*, Beihefte 108, Nr. 1 (Dahlem bei Berlin 1938).
- MEUSEL, H.: Pflanzengeographische Kartierung in Mitteldeutschland. *Natur und Heimat, Sudetendeutsche Vierteljahrschrift für Pflanzen- und Tierkunde* 9, 97—102 (1938).
- Pflanzengeographische Gliederung des mitteldeutschen Raumes. *Mitt. d. Sächs.-Thür. Vereins f. Erdkunde zu Halle* 61/62, 1—87 (1938).
- Pflanzensoziologische Systematik. Eine Betrachtung anläßlich des Erscheinens von R. Tüxens Abhandlung über „Die Pflanzengesellschaften Nordwestdeutschlands“. *Zeitschr. f. d. ges. Naturw.* 4, 399—405 (1939).
- MONTFORT, C.: Funktionstypen der Lichtverwertung bei Pflanzen und das Problem der gelben Blattfarbstoffe. *Nova Acta Leopoldina, N. F.* 6, 1—6 (1938).
- Funktionstypen des Assimilationsapparates und das Problem der gelben Blattfarbstoffe. I. Aufdeckung und vergleichende Analyse der Carotinoidwirkungen. *Kieler Meeresforschungen* 2, 301—344 (1938).
- Funktionstypen des Assimilationsapparates und das Problem der gelben Blattfarbstoffe. II. Die Bedeutung meereskundlicher Forschung für Grundfragen der Pflanzenphysiologie und der Vegetationskunde. *Kieler Meeresforschungen* 3, 17—60 (1938).
- und H. FÖCKLER: Licht und Atmung bei Licht- und Dunkelgeweben, grünen und farblosen Organen. *Planta* 28, 515—534 (1938).

- POST, E.: Zur Oekonomie des *Bostrychietum*. *Planta* 28, 743—744 (1938).
- Weitere Daten zur Verbreitung des *Bostrychietum*, II. *Hedwigia* 78, 202—215.
 - *Bostrychia tangatensis* spec. nov., eine neue *Bostrychia* der ostafrikanischen Mangrove. *Archiv f. Protistenkunde* 92, 152—156 (1939).
- RAUH, W.: *Gentiana terglouensis* HACQUET, ein neuer Fall von schiefer Wirtelbildung. *Ber. D. bot. Ges.* 56, 267—274 (1938).
- Dendrologische Beobachtungen auf einer Sommerreise in Korsika. *Mitt. D. Dendrolog. Ges.* Nr. 51, 172—189 (1938).
- SCHMID, G.: Aus der Frühgeschichte des botanischen Gartens in München. *Sudhoffs Archiv f. Gesch. d. Medizin u. Naturw.* 31, 148—164 (1938).
- Rosensorten zur Goethezeit in Weimar. *Rosenjahrbuch* 1938, 107—114.
 - Blumistik im klassischen Weimar. *Gartenflora*, N. F. 1, 57—60 (1938).
 - Knaut und die Knautie (*Knautia*). *Thür.-Sächs. Zeitschr. f. Gesch. u. Kunst* 26, 101—112 (1939).
 - Caspar Friedrich Wolff. *Der Biologie* 8, 33—35 (1939).
 - Literaturbericht über Pflanzenwelt der Provinz Sachsen. *Mitteil. Sächs.-Thür. Vereins f. Erdkunde zu Halle* 61/62, 121—137 (1938/39).
- STRACK-WEYNAND, B.: Die Photosynthese bei Süßwasseralgen in Abhängigkeit von der plasmatischen Lichteinstellung und vom physiologischen Zustand der Plastiden. *Protoplasma* 30, 216—253 (1938).
- TROLL, W.: Vergleichende Morphologie der höheren Pflanzen, Bd. 1, H. Teil, 1. Liefg. Berlin 1938.
- Über die Dickenperiode der Sprossachse. *Chronica Botanica* 4, 211—213 (1938).
 - Die morphologische Natur der Karpelle. *Chronica Botanica* 5, 38—41 (1939).
 - Über die morphologische Natur der Blütenorgane. *Der Biologie* 8, 39—44 (1939).
 - Vergleichende Morphologie der höheren Pflanzen, Bd. 1, II. Teil, 2. Liefg. Berlin 1939.

Veröffentlichungen
aus dem Botanischen Institut
der Universität Halle

1936/37

- EDLICH, F.: Einwirkung von Temperatur und Wasser auf aerophile Algen. *Archiv f. Mikrobiologie* 7, 62—109 (1936).
- HELM, J.: Das Erstarkungswachstum der Palmen und einiger anderer Monokotylen, zugleich ein Beitrag zur Frage des Erstarkungswachstums der Monokotylen überhaupt. *Planta* 26, 319—364 (1936).
- Primäres Meristem oder Prodesmogen? Kritisches zur Terminologie dieser Begriffe. *Planta* 26, 523—528 (1937).
- MONTFORT, C.: Carotinoide, Photosynthese und Quantentheorie. *J. wiss. Bot.* 83, 725—772 (1936).
- Umwelt, Erbgut und physiologische Gestalt. I. Lichttod und Starklichtresistenz. *J. wiss. Bot.* 84, 1—57 (1936).
- Umwelt, Erbgut und physiologische Gestalt. II. Die Bedeutung der natürlichen Lichtfelder für die spezifische photochemische Wirkung bestimmter Strahlen. *J. wiss. Bot.* 84, 483—516 (1937).
- Erzeugt das Zusammenwirken aller Spektralfarben im weißen Licht eine Steigerung ihrer photosynthetischen Einzelwirkung? *Ber. Deutsch. bot. Ges.* 55, 142—156 (1937).
- RAUH, W.: Die Bildung von Hypokotyl- und Wurzelsprossen und ihre Bedeutung für die Wuchsformen der Pflanzen. *Nova Acta Leopoldina, N. F.* 4, 395—553 (1937).
- SCHAEFFEL, H.: Zur Morphologie des Gynocecum der Phytolaccaceen. *Flora* 131, 41—59 (1936).
- Vergleichend-morphologische Untersuchungen am Gynocecum der Resedaceen. *Planta* 26, 470—490 (1937).
- SCHMID, G.: Des Bauern in Ziegenhain Adam Dietrich Beziehungen zu Linné. *Thüringer Fährlein, Monatshefte*, 5, 351—359 (1936).
- Geschichte der Botanisierbüchse. *Österr. bot. Z.* 85, 140—150 (1936).
- TROLL, W.: Morphologie, einschließlich Anatomie. *Fortschritte der Botanik*, herausg. von F. v. Wettstein, 5, 13—26 (1936).
- Vergleichende Morphologie der höheren Pflanzen. Bd. 1, 2. Liefg. Berlin 1937.
- WEBER, H.: Vergleichend-morphologische Studien über die sproßbürtige Bewurzelung. *Nova Acta Leopoldina, N. F.* 4, 229—298 (1936).

Veröffentlichungen
aus dem Botanischen Institut
der Universität Halle

1937/38

Anschrift: Am Kirchtor 1, Halle (Saale)

- ECKARDT, Th.: Untersuchungen über Morphologie, Entwicklungs-
geschichte und systematische Bedeutung des pseudo-
monomeren Gynoeceums. Diss. Nova Acta Leopoldina,
N.F. 5, 1—112 (1937). RM. 22.1)
- Über Blütenbau und Verwandtschaft des Strandlings,
Litorea lacustris L. Hercynia, Abhandl. d. Bot. Vereinigung
Mitteldeutschlands I, Heft 1, 154—165 (1937).
- HELM, J.: Anlage und Entwicklung der Blattstieldornen von
Quisqualis indica. Flora 131, 425—437 (1937).
- KALTWASSER, J.: Assimilation und Atmung von Submersen als
Ausdruck ihrer Entquellungsresistenz. Diss. Protoplasma 29,
498—535 (1938).
- KERSTAN, G.: Die Waldverteilung und Verbreitung der Baum-
arten in Ost-Afghanistan und in Chitral. In „Deutsche im
Hindukusch“, S. 141—167 (Berlin 1937).
- KOSSWIG, W.: Zur Soziologie und Ökologie des mitteleuropäischen
Auenwaldes. Diss. Halle (Saale) 1937.
- MEUSEL, H.: Mitteleuropäische Vegetationsbilder. I. Die Steinklöbe
bei Nebra und der Ziegelrodaer Forst. Hercynia, Abhandl. d.
Bot. Vereinigung Mitteldeutschlands I, Heft 1, 8—98 (1937).
- Verbreitungskarten mitteleuropäischer Leitpflanzen. I. Reihe.
I. *Dictamnus albus*, II. *Stipa capillata*, III. *Dentaria bulbifera*.
Ebenda, 115—120.
- Aufgaben pflanzengeographischer Heimatforschung. Er-
zieher im Braunschweig 5, 512—513 (1937).
- Natürliche Waldgebiete und Verbreitung der Steppen-
heiden. Karte mit Erläuterungen. Mitteleuropäischer Heimat-
atlas, Blatt 2b.
- Über das Vorkommen des Schmalblättrigen Federgrases,
Stipa stenophylla Čern., im nördlichen Harzvorland. Her-
cynia, Abhandl. d. Bot. Vereinigung Mitteldeutschlands I,
Heft 2, 285—308 (1938).
- Verbreitungskarten mitteleuropäischer Leitpflanzen, 2. Reihe.
I. Die verschiedenen Arten von „*Stipa pennata*“, II. *Jurinea*

1) Zu beziehen durch den Buchhandel und das Sekretariat der Deutschen
Akademie der Naturforscher, Friedrichstr. 50a, Halle (Saale).

cyanooides (L.) Rchb., III. *Aruncus silvestris* Kost., IV. *Digitalis purpurea* L., V. *Digitalis ambigua* Murr. Ebenda, 309—326.

- MONTFORT, C.: Die Trockenresistenz der Gezeitenpflanzen und die Frage der Übereinstimmung von Standort und Vegetation. Ber. Deutsch. bot. Ges. 55, (85)—(95) (1937).
- POST, E.: Systematische und pflanzengeographische Notizen zur *Bostrychia-Caloglossa*-Assoziation. Revue algologique 9, 1—84 (1936).
- Zur Verbreitung von *Catenella* in Neuseeland. Hedwigia 76, 189—190 (1936).
- Weitere Daten zur Verbreitung des *Bostrychietum*. Ebenda 77, 11—14 (1937).
- RAUH, W.: Beiträge zur Morphologie und Biologie der Holzgewächse. I. Entwicklungsgeschichte und Verzweigungsverhältnisse arktisch-alpiner Spaliersträucher. Nova Acta Leopoldina, N.F. 5, 289—348 (1937). RM. 8.¹⁾
- Über die Verzweigung ausläuferbildender Sträucher, mit besonderer Berücksichtigung ihrer Beziehungen zu den Stauden. Hercynia, Abhandl. d. Bot. Vereinigung Mitteldeutschlands I, Heft 2, 187—231 (1938).
- SCHMID, G.: Linné im Urteil Johann Beckmanns, mit besonderer Beziehung auf F. C. Medikus. Sv. Linné-Sällsk. Årsskrift 20, 47—70 (1937).
- Ein bisher unbekanntes Bildnis von Mattioli (1500—1577). Sudhoffs Archiv f. Gesch. d. Med. u. Naturw. 30, 133—151 (1937).
- Das ehemalige Vorkommen von *Gentiana lutea* in Thüringen. Mitt. Thür. Bot. Ver. 44, 9—34 (1937).
- SCHMIDT, G.: Die Wirkung der Lichtqualität auf den Assimilationsapparat verschieden gefärbter Gewebe. Diss. Jb. wiss. Bot. 85, 554—591 (1937).
- TROLL, W.: Morphologie, einschließlich Anatomie. Fortschritte der Botanik, herausg. von F. v. Wettstein, 6, 17—30 (1937).
- Vergleichende Morphologie der höheren Pflanzen. Bd. 1, 3. Liefg. Berlin 1937.
- Geleitwort. Hercynia, Abhandl. d. Bot. Vereinigung Mitteldeutschlands I, Heft 1, 1—2 (1937).
- Über Verzweigung, insbesondere die Bildung von Hypokotyl- und Wurzelsprossen. Nova Acta Leopoldina, N.F. 5, 499—504 (1938).
- Über die zerstreute Blattstellung bei den Dikotylen. Chronica Botanica 4, 39—40 (1938).

1) Zu beziehen durch den Buchhandel und das Sekretariat der Deutschen Akademie der Naturforscher, Friedrichstr. 50a, Halle (Saale).