



Hunt Institute for Botanical Documentation
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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.



Baldwin's Shore. Eton College, Windsor

March 11 1947

Dear Dr Arber

I have been meaning, for
far too long now, to send you this mezzo-
tint which I think you may like to
have. It is, as you will see, an earlier
variant (1660) of the one in your book.

I very much enjoyed meeting you,
and was most grateful for your kind-
ness to Professor Robertson's.

Yours sincerely
Wilfrid Blunt

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A 2 1

see A m 1



MINISTRY OF EDUCATION
DEPARTMENT OF ENGRAVING, ILLUSTRATION & DESIGN
VICTORIA & ALBERT MUSEUM
SOUTH KENSINGTON · LONDON · SW 7

Telephone: KENSington 6371

20th December, 1950.

Reference:

*last yr. other cardinals
(3) Inc. 3, A, B
+ fresh label for cuts
Sch. 3, 147*

Dear Madam,

I have received your letter of the 16th instant.

I will let you have a photograph of the Oth cards in due course, and it may be possible to get a satisfactory result by photo-stat, if so, this can be done without much delay.

857 G. 414.1

In my view the best book on playing cards is by W. H. Willshire, 'Playing and other Cards in the British Museum accompanied with a concise history of the subject.' But as it was published in 1876 and is now unobtainable I am afraid I am not being very helpful. Of the more recent books the following give useful information, most of it given by Willshire.

- 5.413.6.93.9* W. G. Benham Playing Cards; History of the Pack. 1931.
- 5.413.6.93* H. T. Morley Old and Curious Playing Cards 1931.
- 12 Nov. v.L* C. P. Hargrave History of Playing Cards, compiled from the old cards in the collection of the United States Playing Card Company of Cincinnati. 1930.

In reply to your last query my colleague in the Department of Textiles informs me that "The embroidered hangings from Oxburgh Hall are illustrated and described in "Embroideries by Mary Stuart and Elizabeth Talbot at Oxburgh Hall, Norfolk," by Francis de Zulueta, Oxford, 1923."

Yours faithfully,

ARTHUR W. RUFFEY.
Asst. Keeper of the Department of Engraving,
Illustration and Design.

Dr. Agnes Arber,
52, Huntingdon Road,
Cambridge.

*Py. F. de Zulueta
The Cottage Adderbury
Banbury Oxon*

9th July 1951.

Dear Mrs. Arber,

Thank you very much for your kindness in bringing in the specimens of Dragon Arum for me to see. Arums have always had a fascination for me, even the everyday wild ones. I am very sorry that I missed seeing you this morning. Thank you too very much for the two offprints, which I shall keep in my copy of your book as supplements. I am often puzzled to know whether the colouring of the woodcuts is contemporary or not, and it is very valuable to have the evidence summarized in this way.

With many thanks,

yours sincerely,

Phyllis M. Giles.

For Mrs. Arber.

Thom Little Bake

An. 1

In this is revised my *Chepe deligite*
as on this *flans* so is thy *life*

Some strokes with *Diethes* sharpist knife

With *conlineas* not leafy *wanda* not that stroke

It *lowe* and *prize* *thee* in the *love* of *Christ*

So *not* *leander* *Bake* is *lost*, *on* *ft* (?) *sent* *hope*

Who *that* *floroid* *Reverence* and *Chepe* *plaz* of *this*

When *the* *saddle* *leaves* and *the* *body* *shall* *remain* (?)

Christ *know* *the* *change* of *his* *and* *will* *hope* *them* *tell*
ye *two* *days*

ffla
Lake

Mrs. Giles found a p. 26 of *Brunfels*, *Hortul.*
and ed. (*Kernick Beggar*)

P. O. Box 1012
THE UNIVERSITY OF GEORGIA
THE UNIVERSITY LIBRARIES
ATHENS, GEORGIA

*H W Lewer F-SA AL4
Piers
Loughman*

*Museu Colebreu Carter
6 Sept. 1951. Museu Gosh
Colebreu Carter*

Dear Mrs. Arber:

and J
In a small way Mrs. DeRenne collect early English garden books, herbals and books on related subjects. We have three of Thomas Hill's early editions, two Parkinson's first editions and a 1633 Gerarde. Of course we have one of your Herbals and also your book on Goethe.

Recently we acquired a copy of A Book of Simples. London. 1908. Introduction by Henry William Lewer. In this introduction Mr. Lewer states that "the original of this littler book was found in the library of a distinguished Essex antiquary; the document unfortunately has no history . . .". I have been unable to get any further information about the manuscript or its author except that he is the author os several other works. The British Museum was unable to supplement this information. Although the work is of no great importance, I imagine, it is even of less importance without some authentication.

Possibly you are familiar with this book. Can you give me any further information on it or its editor? I would be very grateful for any help.

Very sincerely,

Wymberley W. DeRenne

Wymberley W. DeRenne, Archivist.

Mrs. E.A. Newell Arber,
52 Huntingdon Place,
Cambridge.

*2758
E.A. Newell Arber
Smith
Loughman*

*406. b9119
Pm - km collor
9400. d. 723*

*P. 477. c. 278
P. 382. c. 68
P. 382. c. 69*

*Talbot Co 13 Palemona Row
E.C.*

1912.

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works, Books & possibly, also, of

AR 9



UNIVERSITY BOTANIC GARDEN
CAMBRIDGE

Telephone 4101
CAMBRIDGE

21st November 1952.

Mrs. Arber,
52, Huntingdon Road,
Cambridge.

My dear Mrs. Arber,

I have just heard of a copy for sale of Conrad Gesner's Historia Plantarum: Paris, 1541. I cannot find out very much about this work in the books of reference that I have here, although it would appear to be a considerable rarity. If you have any notes about it, I would be most grateful for your opinion as to whether we ought to acquire it for Cambridge if it is not at the University Library or in the Botany School, a point which I have not yet been able to check.^x I hope you do not mind my bothering you with this.

With kindest regards and best wishes to you both,

Yours ever,

J.S.L. Gilmour,
Director.

*It is at the B. School! But I would be
puzzled for your comments on the work!*

Telephone Number: TERminus 3622

Telegrams :
"MINIFORM, LONDON."

In any further communication on this subject the following reference should be quoted :—

Your reference : _____



MINISTRY OF INFORMATION,
RUSSELL SQUARE HOUSE,
RUSSELL SQUARE,
LONDON, W.C.1.

A2 10

7th August 1945.

Dear Madam,

I beg to acknowledge receipt of your letter of 3.8.45, enclosing typescript for despatch to U.S.A. by registered mail and stamped postcard for my reply to you.

Your typescript has been duly passed and sent on.

Press matter intended for publication in North, South and Central America and in the countries of the British Commonwealth of Nations may now be posted in the ordinary way

and need not be sent here for
censorships

The envelope addressed to Dr. Verdoorn
had on it stamps to the value of
 $5\frac{1}{2}$ ^d, but the requisite amount is $6\frac{1}{2}$ ^d.
I have therefore added a penny stamp
to the envelope addressed to Dr. Verdoorn
and am returning to you stamps to
the value of 1d. as no stamp is
required for the envelope containing
my reply to you.

Yours faithfully,

R. Bingham
Senior Press Censor
Postal Section

Dr. Agnes Arber,
52, Huntingdon Road,
Cambridge.

AR 11

ANTHROPOSOPHICAL AGRICULTURAL FOUNDATION

Secretary
DR. C. A. MIER 24/1/49

CLENT COTTAGE,
CLENT,
STOURBRIDGE, WORCS
Tel. Hagley 2618

Mrs Agnes Arber
52, Huntingdon Road
Cambridge.

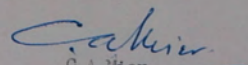
Dear Mrs Arber,

It was very kind to write, as you must be very busy indeed. Yes, this seems the situation everywhere with the publishers: An accumulation of manuscripts awaiting publication.

Thanks also for mentioning Dr Bernard Crow. I shall get in touch with him.

With friendly greetings I am

yours sincerely


C.A. Mier.



EXHIBITION - 19

Dr. A. Arber F.R.S.
52 Huntingdon Rd.
Cambridge

BOTANY SCHOOL,
CAMBRIDGE.

7/10/66.

Many thanks for the suggestion
about Hledwig. Yes, he certainly
would make a most interesting
study, but I fear my Latin is too
weak.

I have just read Goethe's 'Botany'.
May I congratulate you on a
masterpiece? I enjoyed it very much.

Yours,
Paul Richards.

AR 13

Trinity College,
Cambridge.

30. 9. 46

Dear Mrs Arber,

I appreciated
so much the friendliness
of your sending me your
reprint on 'Goethe's Botany'.

When I read Sherington's
lecture that the feeling

more time (for
quate documentation) than
ould afford to give. And
ington is a dear old
with not long to live.
The motive behind the
k intrigued me. I
ot help feeling it was
er like this:

ne (great poet + damn bad scientist)

?
= Sherrington (great physiologist
+ (alas!) damn bad
brisk)

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of someone known to me
personally being insulted
and belittled. Simply ^{speaking} as
a zoologist - and one not
specialised in morphology -
the attack was so palpably
unjust and so mean-
spirited, that I should
dearly have liked to write
a reply. But I realised
that ~~to do so~~ would

and that by belittling Goethe's scientific attainment S. made up for his personal feeling of inadequacy as a poet. My guess may well be wrong; but there was unmistakable personal animus in S.'s lecture, such as can only arise from a conflict of personalities.

Will you please allow me to repay the stamps? I am
estimate. With kind regards,
Yours sincerely,
Norman Picken

AR 14

Oct. 20th,
1946.

Dear Mrs. Arber,

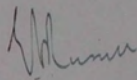
I am delighted to have a copy of your paper on Goethe's Botany, which has interested me very much. Someone ought to do the same for his animal morphology.

There is much need for a synthetic and holistic view of living things such as Goethe's.

I have just come back from Oxford where I attended the inaugural lecture given by Goodrich's successor, A.C.Hardy, an old friend of mine. He has come out strongly as an anti-mechanist, and should be a powerful influence in the right direction.

With best regards,

Yours sincerely,



9 Elmworthy Terrace
 Primrose Hill
 N.W. 5

Dec 14/20

RE ENGRAVING

My dear Mite,

The exact difference between etching and engraving is that in engraving the line is cut out of the metal ^{by a tool called a graver} and in etching the line is etched out by acid.

The polished copper plate requires ^{no further} preparation to fit it for engraving, but in order to transfer a tracing of the design to the copper it is usual to put a slight film of wax on the surface - by heating the plate sufficient to melt the surface of a piece of white wax rubbed on to it.

Then the tracing is transferred by

The design, having been traced on the slightly waxed surface of the copper, is then gone over with the dry point (any needle will do) so scratched on the copper, when the plate can now be engraved - the wax being removed with turpentine.

It is however more usual for all engravings to be begun by being lightly etched, so as to settle the plane of engraving on the copper: - when the engraving is finished there is no evidence of its having been begun by etching.

The difference between an engraved and an etched plate is only to be detected in the quality of the line - the etched line is finer, being done rapidly, sketchily, the lines

means of the back of the tracing being
rubbed over with powdered black lead
as the lines you see with a hard point

There is no difference as to the
plate mark in etching and engraving:
they both must show it, unless the
plate is printed on a piece of paper
smaller than the size of the plate,
but I have never known that to be
done - indeed it would be difficult
to lift the paper from the plate after
it had gone through the heavy pressure
of the great steel rollers.

The strength of the plate mark
depends upon the thickness of the
copper - the thicker it is the more the
have to be ~~worked~~ levelled, to burn out in
the paper

after being scribbled - the direction changed suddenly to an
extent - the engraved line can only be done very slowly
with extreme care -

It is however possible for an etching done very slowly and
deliberately to imitate engraving very closely - but the com-
pense is not possible, for an engraving to successfully imitate
an etching -

I can lend you the tools to draw with the plates, as
at our lecture

Yours
H. M. R.

Ask me any questions that occur to you -

AL 18 42 Lucas's Salt Gardens

Wed Dec. 13 (1950) ^{S. W. 7}

By Sea Acres. I saw
a fresh flower -

knowing ^{an} interest in
them. I've come to conclusion ^{it} is not
a fresh but an accident - ^{it was}
~~the~~ flower into this stream ⁱⁿ or ^{near} ^{the} ^{stream}

I got home from a
round of 4 patients in
Norfolk something after

1:00'clock ^{today} & City

Coop store going (first
fall!) - then - meal

& now I've just come

in ^{from} all the ^{business}
(including ^{academic})
Shipping & am at ^{with}

the back of
the page
is
the
back
of
the
page

Sitting at rest in - (2)
nicely warm room

I'm one from, firstly, two
delightful homes. (The
second I especially ^{enjoy} where)
was already friends &
like everyone) - but
I've very mixed feelings
about my last venture!

(Lack of all the
portraits have gone with a
ease - so I'm not too busy
with complication of work comes
this time)

My last portrait was
at ~~St~~ St Joseph Hall which
is a wonderful ^{old} red brick

Moated Castle - mausoleum (3)
bird - round - quadrangle

One side was originally
the Banqueting hall & is
reported to have been second
of to Westminster Hall
- but this has gone & is
replaced by Adams corner blocks & a
crown piece - but all
the rest is original stonework

1482

The Bedingfelds
have lived there father to
son for 21 generations
(if you count the small

the heir) & have been consistently
Catholic through all persecutions.
They're a lovely set with
crown & banqueting

Do any Queen of Scots (4
& her ladies when she
was in prison. #11
- I should think
you know reproductions?

There are panels of
beasts & birds ^{others} in "high
point" - applique onto
embroidered velvet.

#11 the Sea Lion
(labeled)
"Lion of the Sea" took
my fancy most. One
and ~~the~~ a charming version
of a land lion.
I slept in "The Pastor
Room" in bed "Queen"
Elizabeth might have slept in
the ^{Coats of arms} ~~Coats of arms~~ all about
me -- the present ^{is}

Edmund's grandmother was (5)
the last of the Pastons
(of the Paston Letters)

One sided the
home is now - "finishing
School" for foreign girls
(No doubt by doing this
a lot of their expenditure
is allowed as "expenses"
& not taxed)

My host had great charm
- though I was not captivated
in sympathy - by hostess
I much disliked -
She was, she ^{is} crossing
"pretty" & a supreme
egotist!

There were 8 servants
an unpleasant & despoiling

housekeeper - quite incompetent⁶
- a pleasant butler - W.
I think he slipped off to
the Pub. a good deal
& all the rest were
French Italian (young ones?)
etc & nothing was required.

In my seven sittings
I ~~could~~ sat in Sir
Edmund's dressing room
with unmade bed - etc -
hygiene socks etc
Scattered all over the
room (This was tidier the
night - guest was there!)
I was all over the ~~floor~~

forth by the I succeeded
in getting an electric
fire which would ^{cost} more than
half ^{for the "studio"} ~~work~~ ^{although}, I
had, Lady Bedford
had 3 electric fires
in her bedroom on one
side, & a coal fire
& electric fire in her
Bedroom on the other
side !!!

I had a good lot of
wildfowl time & had a
couple of nice walks & some sticks
I painted the 3 year old girl
- ^{with} a bear & a
big back smelter of the

Boz (who is - Switzerland) (8
at the moment) - All
mutual friends tell me
the Boz's - complete
^(and?)
chance which makes a
hesitant less did give
age - ! There was
a lot that was very
amusing of course.
How long was letters
grow if we tried to
describe anything. I came
home through Ely & Hayshe
of Merrill.
Hoping to see both fit
not mind the cold
too much for Janet

P-5

I've just been looking
at Herbs, & think it
is the just style of
Ortus Sanitatis illustrations
that are reminiscent of
Mary Queen of Scots
designs.

I'm $\frac{3}{4}$ high Cecil Woodham
Smith "Flower Nightingale"
was it ~~to~~ Sunderland
who was a relative of
Mary Queen of Scots
Mary Queen of Scots - its was

Tuesday morning - I was
tired of day - but I knew
if I don't send this I
was send nothing!

AE 19

Dec 19th 42 Massis file files
S. W. 7

My dear Agnes

May I hope for you letter - this
written - your floods etc
Circumstances - I only referred
with great regret to the
short affair of flood damage
(last letter: I promised myself
how long it was since I had
heard it! - alas it would
have been appropriate!

I did not ask Sir Edmund she
has the cause - its presence of
the May issue of Scott's book.

I did ask him about
monogram panels on it
which were M. A. R. Y.
Superimposed - he said
an expert had come to see
the engraving process & he
sceptical we had found these
marks with private marks
also which had occurred in
the prayer book etc -
(he wanted it out to me)
I went round with the

finish school - but
what they - better, were the
to see the dangers, I stayed
in the King Park where the
bed is so as to see it more
thoroughly.

I wish I will be seeing Martin
& Lucy? It is the first
to Cardiff, the first I was
sorry to hear that stuff or
Methlow had been ready when
they all a fir for Christmas.

The sea gulls are long
and window today &
being so high. I saw (the
simon was fast & there'd not be
a wind) a little white cap
on every ball of my Plane
tree - I looked up
to see if the air was
out to find the invisible
for below.

Love & best wishes for
Xmas to you both
from
Mum



MINISTRY OF EDUCATION
DEPARTMENT OF ENGRAVING, ILLUSTRATION AND DESIGN.
VICTORIA & ALBERT MUSEUM
SOUTH KENSINGTON · LONDON · SW 7
Telephone: KENsington 6371

Reference : 15th December, 1950.

Dear Madam,

In reply to your letter of the 9th December, I beg to inform you that we have no references dealing with the relation of the illustrations on Clas Oth's playing cards to early botanical illustrations.

When visiting the exhibition the writer of the article in The Times mentioned this point to me and suggested consulting your book in order to follow up the theory.

Although there is no exact comparison I feel it highly probable that these 16th century playing card makers did adapt herbal illustrations for the suits of leaves and acorns - these are the two suits in the Clas Oth's cards. But I am not able to go beyond this.

If you would care to have a photograph of the four cards by Clas Oth I can get one done, but it will mean waiting for some time owing to pressure of work in our studio.

Yours faithfully,

ARTHUR W. RUFFY.
Asst. Keeper of the Department of Engraving,
Illustration and Design.

Dr. Agnes Arber,
52, Huntingdon Road,
Cambridge.

A. 21

14 Juli
1927

organt!

sendung

o Spe

bilium

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Wii

to logi-

correw

, in

denis

für

ous the

W. S. Dargatzis

geword

ein

A 221

Dona d 14 Juli
1897

Vorsicht! Mein Lager!

Für die pflanzliche Zellkunde
Meiner Arbeit über Sper-
matogenerie bei Lilium
Mastigon dank ich
Ihnen bestens. Wie
Sie aus den cytologi-
schen Studien ersehen
haben werden, ist
es aus mir nicht
geworden nur für
eine Reduktion der

zung in der Pollenmit-
telzellen zu entstehen,
den. Unsere Zeichnung
wurde nach vertiefter
beim Studium der
Theilungsverläufe im
Embryosack. Sollen
viele Kerne wie in
der Ueberzeugung, wir
müssten aus in der
Pollenmittelzellen
geirrt haben und
nahmen dann an

Neuem unsere Un-

ternehmungen auf. Neues
Material das wir eben
neuer fixierten, mög-
lichte und auch die
Ueberzeugung, das eine
Längsspaltung aus
beim ersten Theil-
schritt in Palladium
tunellen erfolgt. Wir
machten darüber eine
Mittheilung an die
Deutsche Botanische
Gesellschaft. Diese Mit-
theilung ist vor un-

mere wachen geduener
wachen ut sobald wir
die Sonderabzug skal
ta wach wir unser
Lauben thun ein
Dorellen u senden
So feur uns also
Thun einstücken in
Künnen dan das Er
gebnis unserm Unt
suchung uns die Th
rigen über einsteuern

Wir mit Ricken
- Jours No
schere ergebnis

P. Strackburg

Dona 7 Juni
1901

Ar 22

Sehr geehrtes Fräulein!

Mit der Untersuchung
von Iris in der Kör-
nichel im wienigen
Institut beschäftigt.
Die Arbeit ist nun
Abchluss nah, so
dass es wohl besser
wäre wenn Sie die
Untersuchung Ihrer
Pflanze wenn möglich

aufgeben, so doch
hinausschieben mög-
ten.

Ich benutze diese
Gelegenheit um Sie
meiner aufrichtigen
Hochachtung und
Verdankung

Ihr ergebener

P. Frankenberg

Al 23

July 14 1905

THE CAMP,
SUNNINGDALE.

Sir Joseph Hooker presents his
compliments to Miss Parry, and
& thanks her very much for
the copy of her paper on the
subject of Monocotyledon etc.

Sir Joseph had already seen
notices of it & was much
interested in the observations &
views, & looks with confidence
in them yielding important
aid in the classification of
Monocotyledon; Liliaceae especially
what you want. The aid of
the synonymy

52 Huntingdon Road

Cambridge, England

April 14, 1948

Dear Professor Anderson,

Your magnificent parcel arrived this morning; it quite took my breath away, and I want to thank you more than I can say for this generous and heart-warming kindness. Everything will be exceedingly useful. The ivory soap is lovely and worthy to be used with the very nice face flannel. The crisco and rice and prunes will be invaluable, - altogether you have given us so much that we shall pass on some delights to friends. Muriel went off two days ago for a holiday at Lyme Regis in Dorset after a very heavy term of teaching (she has a past to teach geography and English in a boys' school at Ely, and goes over by train every day), and I had a letter from her the same morning as your parcel arrived to say that the food ^{in her hotel} is of the "elegant - scanty" type and, as she is walking and geologising, it is not exactly ideal.

So I have at once posted to her some of your raisins, which are the best possible supplement for inadequate meals. The cheese we particularly appreciate just now, as our ration has just been cut down to $1\frac{1}{2}$ ounces

per week, which does not go far. Also the milk is greatly welcome; Muriel's ration now is 2 milkless days and $\frac{1}{2}$ pint the other 5 days a week (I have extra, in consideration of a gastric haemorrhage I had some years ago), and as she utterly refuses ever to use a drop of mine, I am very glad of these tins, as she wants her allowance in tea and coffee, and it leaves none for cooking. She will be rejoiced to see the chocolate pudding, which is a great weakness of hers. Alas, I only began cooking in my 60th year (when we dispensed with service a year before the outbreak of war, when things were getting critical) and I fear I

shall never be really expert. I remember very well how thoroughly Mrs Anderson evidently understood everything domestic, and her description of her really civilised kitchen, and I often think of her gifts with envy. But the things you send can be used by the tyro.

I wonder how your work goes? Last summer I finished a general book on morphology on which I had been working for many years. The Cambridge Press are publishing it, but it is impossible to say when; printing is now a great difficulty, and they tell one that one can only wait one's turn in the queue for composition. But it is an ill wind that blows no one any good, and at least this delay gave me a chance of having the book back for a month recently, and adding a good deal that had accrued in the months of waiting, and also of revising it (since I had so far forgotten it that I could criticise it severely). I always like the description of Titian's plan of standing a finished picture with its face to the wall for some months and then looking at it again, and scrutinising it "as if it were the face of his worst enemy". I am now putting my tiny lab. in order for microtoming this summer, but I fear the work must be on a small scale, as domestic work devours more time as I get older, and conditions altogether make lab. work difficult.

Forgive this egoistic letter; and thank you again so very much for your most welcome kindness,

with best remembrances to Mrs Anderson,

Yours very sincerely

Agnes Arber

A125

A125

52 Huntingdon Road

Cambridge, England

November 8, 1949

Dr Edgar Anderson
Missouri Botanical Garden

Dear Dr Anderson,

It was very nice to hear from you, and I am delighted that you find anything in my Monocot book useful for students. I am so very sorry that I no longer have a copy of my Palm paper. I have just been looking through my reprints to see if by any chance a stray one was lurking somewhere, but I have not found one. But I regret it less because - though I thought at the time that I had cleared up the matter - I have since felt uncertain whether I had had good enough evidence about the origin of the "coiffe". I had a shot some years ago at further sections of various genera, but I did not get conclusive results. However your letter has spurred me on, and I today secured some date stones, and I mean to try ^{again} whether I can get young plants, from which I might get sections.

My ideas about morphology have changed a good deal in the years since Monocots was published, and I have tried to express them in book now in the press. I shall be sending it to you as soon as it appears - but when that will be I cannot tell. I have corrected the first proof, but everything connected with printing, publishing and binding is indescribably slow now, and I think I shall be lucky if it is ready in 1950.

Somehow the Royal Society publications circulate very badly; indeed after having two little papers there, I decided not

to offer any more to them, as the work seemed to me to be pretty well buried!

With kindest regards and every good wish,
November 8, 1942

yours very sincerely

Agnes Arber

Digitized by Hunt Institute for Botanical Documentation

A 226
Arber, Agnes

A 126

52 Huntingdon Road
Cambridge

England

Dec. 4, 1949

Dr Edgar Anderson
Missouri Botanic Garden

Dear Professor Anderson,

So many thanks for your letter of November 15; how very kind of you to have sent us a parcel for Xmas! That will be lovely - your dessert powders are the greatest boon, and the Christmas baking things will carry your kindness to other hearths as well. We both feel most deeply grateful to you for your thoughtful generosity.

An event this week has probably slowed down the appearance of my little book more than ever. The University has taken over part of a set of war time ^{air, low} hangers outside Cambridge, and the Press had stored there in some 70 or 80 tons of paper. But a few days ago fire swept through them, and naturally found the paper good fuel. We walked out this morning to see the wreck, and found a curious effect. The place was protected in war time by an immensely high wire fence. The gales of the last days had carried such 16-page sheets as were not too scorched to fly, and driven them against the fence, where they had remained. They hung there as if a Giant Housewife had been having an immense field day of laundry of Giant Handkerchiefs.

With again warmest thanks and every good wish for 1950,

yours very sincerely Agnes Arber

A 27

A1 27

52 Huntingdon Road
Cambridge

July 17, 1952

Dear Professor Anderson,

How delightful of you to send me your Plants, Man and Life! It arrived this morning, and as yet I have not had time to do more than read a relatively small part of it. I do indeed congratulate you on being able to make difficult subjects so lucidly interesting. I am more than delighted that you have entered this extremely cogent plea for the taxonomic and other study of weeds and cultivated plants. I have been initiated into this question by W.T. Stearn (formerly Librarian of the Royal Horticultural Society, and now, since April last, transferred to the Department of Botany, of the British Museum (Nat. Hist.), Cromwell Road, London, S.W.7). He is urgent that the national collection should include garden as well as wild plants, and being a most forceful and persistent person, I hope that he may really be able to remove prejudice and get the reform carried. Recently he came to Cambridge and brought your book for me to see, but I could then only glance at it, so I am indeed happy to possess it. We both much like the dust-wraper picture of you, looking with such concentrated and loving attention at your plants. Thank you so much for the charming inscription - yes, indeed, I wish you had been able to look in in person today, especially as Dr J.C. Willis, of "Age and Area", was here this afternoon. He now lives in Switzerland, but is over here to see his daughters - he declares for the last time, as he is 84 and his doctor is most unwilling to let him travel like this again. I showed him your book, and it was fastened upon with special eagerness by his daughter, Margaret Anderson, who was with him, and who teaches in the Geography School here, and who declared it was just what she wanted for her students, and took a note of all particulars about it.

What you say about my grass book gives me of course immense pleasure; you are far too kind in your reference to it - I am indeed proud that it has been of any service to you.

With every good wish to you and Mrs Anderson, and the warmest thanks - Muriel wants also to be remembered to you -

yours very sincerely Agnes Arber

I enclose a little notice of my old friend & former teacher
Frank Oliver.

May 24th 1941 Mount Broom
 (from Lady D. D. & M. G. G.) Guildford, Surrey

Dear Mrs. Arber, It was such a pleasure to receive your two papers. I have already skimmed through both, thereby getting tabs across with pigs' ration + the minute quantity of petrol for mowing such remains of the lawn as *Scirpus granulata*, Garrow, the drought, a couple or more of very active badgers (these destructive elements are placed in ascending order) have allowed to survive. Hittler has pulled inceddaries at the said lawn (or perhaps at the house by it), but these have always fallen a few yards from the lawn i.e. the little wood to the east-south of it. I have done as harm to it! Both your papers were full of interest to me - the one on the interpretation of stem & leaf deals with a subject that is among the most entrancing of controversies. I shall want to read both papers several times; & if I may do so later, I should like to

comment on the leaf-shoot question - not
because anything I can say can be of the
slightest value in this question of
Angiosperm-leaves - but because it helps
to clear my mind. Meanwhile, I will only
mention that there is, perhaps, a more
curious relation in Equisetum (forgive King
Charles' head!) than the hypotheses mentioned
on your page 87 - according to which a partial
shoot has "an inherent urge" to develop
whole-shoot character. There is in Equisetum
a fixed compulsion - at least in normal
cases - for the so-called lateral branches to
produce their own roots - usually, I
think 2 lateral ones - How far this ^{curious} ^{partial}
imitation of two lateral ^{parts} is due to ^{partiality}
- or, is phylogenetically speaking, due to the
retention from a form with rhizomes (in
which haemes develop) roots that pairing
the sheath of the parent-axis became functional,
I cannot say. I should like - if my two types
last out beyond the cones of Equisetum - to
try to find out if there were ^{any} ^{parallel}
cases of upright stems where the branches
produced roots before even leaf-stripping,
if this became, & as it were, lateral

full shoots of a higher order than ³
the parent stem? I may be writing
fearful rubbish; but I have been
puzzling about this queer peak of
(phylogenetically speaking) ultimate
development of what seem to have been
in evolution branches in to full shoots
Perhaps you will think that I am
one of the unwary who base these morpho-
-logical equivalence which they ascribe
to organs on the validity of a translation
into historical (perhaps rather into pre-
-historical) terms - To me it is the
other way round: the morphological
equivalence of two objects ^{organs} is accounted
for by their common origin from another
organ - though their similar appearance
may often be due partly to parallel
development. I've only a very poor
dictionary available at the moment:
my sister's crossword dictionary; its definition
of morphology is "the study of organic form"
If organic form ^{are} to the right - words, surely
morphology ^{is} to the study purely of

form². And don't think that etymology is a safe guide in the definition of words - he, for example, would say that a manufactured article was not made by hand; or that aristocracy was the rule of the best!

I must try to get hold of Troll's

"Morphologie der schilfformigen Blätter" & Kozo Poljan's "third conceptions". The latter I can no doubt get from the Linnaean - the Nat. Hist. Museum has been shut since July - which is most irritating.

In these anxious times it is a help to think of scientific theories - the non-scientific part of the world seems to have gone mad. It is a pity that the Germans, so efficient & intelligent in science, & having such great modern poets as Rilke (I know that he has died)

& such great prose writers as Tuchwanger & Mann, should

Al 28

(1941 paper) (16) 3 to 6 to

In reply A.A. June 8. 1941
 When you say about not production - the latent shoots of
 Equisetum is I guess with some - it seems a particularly
 good example of "ambitious" character of latices! I do hope
 that you have time to write to me how you
 think about the leaf-shoot position - it would be a great
 help. The ideas in the paper will I hope be much
 modified & developed before you turn out a final form
 of a book about myriophyllum I was told it better be

August 6th 1941 Mount Brouns, Guildford, Surrey A1 27 (16) 4 11

Dear Mrs. Arber, you were rash enough to ask me for ~~my~~ further comments on my view of the root-shoot theory. Here they are - but I must premise their expression ~~now~~ by saying again that I really know nothing of Angiosperms & their morphology & by explaining that I am now out of a good scientific library - The Natural History Museum, where it has been my habit to do my general botanical reading, is shut; when I tried going to the Linnaean the books were unobtainable, as, owing to blitzing, they were piled on the floor. Moreover, practical duties here, making ~~it~~ it difficult to go to London, apart from uncertainties of traffic & their being overcrowded. I do hope that you, who are doing really important work, have better facilities in Cambridge.

First for one or two general comments: I still feel that unless morphology has its classification of organs founded on phylogeny it can hardly be useful, except descriptively. Indeed, that is rather an understatement of my views of the shortcomings of morphology ~~divorced~~ divorced from phylogeny. For not only does it seem to me to involve the assumption that in science things are (in their very nature) what they seem, but it also sets up a subjective view of essential nature. Your view of the essence of "root-shoot", so to speak, might be very different from mine; & apart from your greater knowledge & experience, I can't see that there would be any means of weighing the probability of the correctness of our conflicting views. If phylogeny does determine the morphological nature of an organ, there is some basis for forming a judgement, in each particular case, as to which of many interpretations is probably right. For instance in the example of *Ruscus Hypoglossicum*, quite

by you on p. 90, surely the certainty that you feel (or that I imagine, no morphologist ~~not~~ would dispute) ^{that the phyllode is a reduced axis} is founded on the ~~fact~~ knowledge that at some period the ancestors of this plant (whether they were members of the genus *Ruscus*, or even *Angiospermus* at all) had not this leaf-like reduced shoot - or rather portion of the shoot. The presence on some phylloclades of flowers & the disposition of the ^{system in the} appendages do, I agree, strengthen this view; but that, surely, is because they point in the same direction to the probability (I put the matter mildly) that in some ancestor of *Ruscus hypoglossum* the prophyll was a leaf of some sort (a dorsiventral appendage of an axis). Etymologically, your interpretation of morphology is unassailable. But would you also urge that manufactured articles are always made by hand, or that aristocracy is always government by the best citizens? That would be to make the corner of ~~the~~ every word that survives infallible as to the original meaning, usually in some other language, of the word from which the new word is taken ^{shortly put}.

Two ~~short~~ points, that strike me as possible ~~letter~~ criticism ^{follow}. One is that the use of ^{argument drawn from} ~~the root~~ combination of root-stem characters in the hypocotyl ^{loses}, perhaps, some force when one remembers that the seed habit - encapsulation of the young sporophyte - involves ^{many} ~~many~~ ^{incursions} & perhaps a telescoping down & fusion of root-stem regions. I put this very crudely, owing to my ignorance of *Angiosperm* ^{embryology} ~~morphology~~. And, perhaps, all that I really feel is that the hypocotyl is a modified region. The second point is ~~that~~ ^{about} your ^{that} quotation from Spinoza. However well this may cover your theory & however profound Spinoza's insight into philosophy, however incredible his supreme mastery of words, is it not possible that this quotation is due to his lack of knowledge (or if his not having apprehended) the prevalence ^{in nature} of variation? The more I observe plants growing wild

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→ think over the origin of cultivated ones, the more I feel that variation (in plants at least) is universal & not due solely to genes or external conditions. Of course, I shouldn't have become aware of this without having considered ~~the~~ some at any rate of the writings on variation that have appeared since the publication of the Origin of Species. And, for all his great brain, I doubt if Spinoza was quite uninfluenced in formulating the generalization quoted by you by the fact that even if species were not ^{in his day} held to be always fixed (since domestic animals had clearly been bred from variation) yet variation was held to be within ^{the} systematically narrow limits of species. I had made this remark quite tentatively, not having read any of his works.

As to more special comments, I see that on p. 100, in the last paragraph, you claim that certain facts, mentioned a little earlier concerning the ontogeny of the ^{leaf} leaves of Plantago, Musa & the palms, show that the Angiosperms ~~leaf~~ ^{may} sometimes have a diversified origin " & hence (my understanding) cannot always be really homologized from family to family." Surely that accepts the view that phylogeny is at the bottom or morpho-logical equivalence. The general homology - or corresponding morphological category - you accept, I take it. Despite fundamentally different morphology you regard it (as a leaf; ^{make leaves} ^{leaves}) but not as 'really' corresponding to the leaves in related families. I agree that - chiefly on phylogenetic grounds including the evidence from the Psilophytales & large-leaved Devonian & Lower Carboniferous forms - that the leaf is at any rate in the Fucus a partial, branching shoot. I know nothing of the fossils which may or may not have been the fore-runners of the Angiosperms, but it would seem to be natural to accept the view that in their history the leaf had - at any rate in most lines - followed a

parallel course, whether before or after the acquisition of the seed-habit
 or coea of Angiospermy. I am not convinced that all leaves are
 of shoot-nature or ^{are} shoots-transformed - or as you would say of shoot-like character
 Lycopodium-leaves may be enations, such as seem to have
 arisen in *Ptilophyton* & *Rhynia*. I rather incline to the view that
 despite their ^{occasional} small size & similarity the ligulate Lycopods' leaves
 are shoot-like in nature - representing thalloid branches of a high or
 order or ultimate branchlets - & bear enations - the ligula

The shoot-like characters of the leaf seem to me a natural result of
 its pre-leaf lineage. Huxley, on p. 690 of his *Handbuch der Paläobotanik*
 vol 1, after pointing out that large branch-system leaves (he is talking
 of ferns) retain in many cases "leaves of a different kind in the true
 apophysis i.e. apophyses that are not modified pinnae or pinnales & add:
 "Das selbe gilt für die Blätter der *Urogonia* von *Urogonia*"
 That seems to me a satisfactory way of expressing the ^{presence} of shoot-
 like characters

As to the root, is there really no ^{kind} ~~alternative~~ to considering the root
 either as an organ in a primary category commensurable with the shoot
 or to looking upon it as in a sense partaking of the character of
 a pericentral rhizoma, the latter part being root-like & ~~some~~ ^{some} ~~of the~~
 root & shoot are presumably in their ~~form~~ thalloid ancestry descended from
 thalloid branch systems, the former from the subterranean thalloid branches
 the latter from the aerial ones. Presumably the subterranean branches were
 very early modified by their underground habit, perhaps from the first

In making this remark, I quite realize that I must be, in your
 view, ~~suffering~~ under the influence of the phylogenetic twist in
 the morphological thinking ^{to which you allude} on page 27. But I do this with my eyes
 open, because I think that phylogeny is at the bottom of morphology
 I emphatically do not mean that phylogeny is a sort of

biological predestination, deciding all characters.' Speaking crudely, I might say quite ignorantly, it looks to me as if the production of roots were partly due to chemical forces - hormones, witness the rough & ready methods of root production in ^{commercial} hortamones. We have used hortamones - & in two cases successfully to root a cutting of *Berberis Bealei*, which rarely gives good results from ^{ordinary} cuttings; but my cousins, the Richard Burkett-Greene's, who are market-gardeners, have used hortamones on a much larger scale. Elizabeth Greene reported a surprisingly rich crop of roots from various parts of the plant (The *Bealei*, I think, chiefly with *Chrysanthemums*). Is it rash to suggest that perhaps the chemical factor that finally trips out such an astonishing outpouring of "rootiness" on leaves & stems would not work were not these structures lineally descended from thallose-branching sporophytes, which early acquired also subterranean branches? Other factors, such as the heredity of the individual plant (probably itself a composite of very numerous factors) no doubt, is involved in the varied varying characters of plants. But would ~~hormones~~ hortamones + hormones (from which they ^{presumably} are ^{derived} presumably based) work if there were ~~not~~ the common pre-stem-root-~~tree~~ (or shoot root) lineage of organs? [I believe that prothalli do occasionally abnormally produce roots, presumably partly induced by chemical factors; & that might present an obstacle to the very tentative suggestion, I have made, if one believed in ^{an} anthetic nature of prothallus sporophyte in the higher plants. But I think that that view is not now widely held.)

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I haven't left myself much space for other matters! I am more hopeful of the war ending ^{soon} say in 1942 - now that the Russians have fought so well & inflicted such severe casualties on the Germans. I think that the Germans will eventually take Leningrad, Smolensk, Kiev & perhaps Moscow before the very bad weather sets in - But I don't think that that will end the Russian resistance. There must be armies in Siberia; & even if the Western Russian army agreed to surrender all tanks, guns & airplanes, they couldn't deliver the goods unless the ^{Russians} generals in the east agreed to do so. I hope that if the present army is defeated, we shall see ~~an~~ generals, corresponding to Kaledin, Kerimov, Wrangel, ^{in the last war} leading against the enemy armies, which if not equally well mechanized & armed as the present front line troops, are yet organized armies against which Hitler will be unable to leave a mere cordon of police-soldiers. And I can't help thinking that the heavy casualties ~~to~~ now being suffered by the Germans & our bombing will disorganize Germany & bring down Hitler & Co. - What has happened to Goering? ? ?

I am so sorry that your daughter has to give up research for the time being. I do hope that she will be able to carry it on later - in the holidays. I don't know what holidays she will have during the war; but school holidays are quite good in peace-time.

Forgive a very untidy letter -

With many thanks for the pleasure I have had in reading your paper. Yrs very sincerely

Isabel Bone

AR 29 *low*

Aug. 11. 1941

Reply to Lady Isabel Browne's letter, of August 6.

I feel that discussion between us is made very difficult by this fundamental divergence about the relation of morphology and phylog ny. You speak of my view as setting up "a subjective view of essential nature", and you regard this as a shortcoming. But I am coming more and more to feel that in certain senses science is purely subjective - we cannot do anything but impose the framework of the human mind upon the objects of our thought. The fact that, if one rejects the phylogenetic basis, it is more difficult to decide between two views, does not seem to me to be a reason for retaining the phylogenetic basis; it shows that the phylogenetic basis is convenient, but not that it is true. Personally I do not now think that to say that the phylloclade of *Ruscus* is a shoot structure in which the prophyll predominates, means that one supposes it was ancestrally a shoot. It can be convincingly the equivalent of a shoot, without in its ancestry having ever passed through any phase in which it was more obviously a shoot than it is now.

Spinoza said not of course know anything about variation, and he was speaking quite generally about beings of all kinds. But I think that the dictum remains true despite modern knowledge of variation. For what he is talking about is a creature persisting in its own being; this may be widely different from its parent's being; he does not say that

creatures make any effort to be like their parents.

I suppose that what it really comes to is that I am becoming profoundly sceptical about evolution; I feel at any rate that there is something radically wrong in the way we look at the process that goes by that name - but what that something is I do not know and would give much to know.

August 17th 1921 Mount Browne Guildford, Surrey A.L. 30

Dear Mrs. Arber, A tiresome & rather acute attack of rheumatism is keeping me indoors; & the aspirin, which has been prescribed for it prevents my doing ~~much~~ real work on horse-tails - for I am steeper than I need be! But in my comparatively lucid intervals I have been reading Goethe's prose; & in reading Goethe's short & interesting essay on St. Philip Neri, I noticed, a few pages before it, some curious botanical remarks. As they have been published only, I believe, quite apart from his main botanical work, you may have overlooked them & be interested in them. I send a short extract from these remarks, which altogether occupy just under two pages. The part omitted contains an account of Goethe's attention being drawn by a horticultural friend to ^{his} vegetative reproduction of plants. The interest of the second part of the quotation seemed to me to lie in the fact that it is difficult ^{for me} to imagine that if Goethe's insight into the fundamental meaning of metaphors from observing the proliferation of a flowering axis was ever more & more increased, it is difficult ~~to believe~~ ^{not to} believe that he did not regard consider the rudiments of the floral parts as ontogenetically actually metamorphosed during growth. I can't at the moment remember if Goethe ever used that very useful word "Kernanlage" of various German botanists. It has to me a definite meaning, but I can't translate into English words - he seems to me to have had "Kernanlagen" (ekimoid). I was also struck by the word "Uniform" applied to the adult Cacti short - That rather suggests an ideal ground-type to me, though it may well be Goethe's aesthetic sense asserting itself! If he had written uniform one would have been tempted to see a tendency, to the phylogenetic point of view in him! How strange the working of his observations ^{seems} on the

to a twentieth century botanist! A lactus quite innocently developing
two cotyledons but later developing misshapen parts somehow ^{reminds} ~~is~~
of Dizzy's children, born good!

I quite agree that in certain senses science - & every thing else that
we can apprehend - is subjective; for it is only through our minds that we
apprehend the world & ourselves. But, if there be any matter at all, there
is some objective part in it. The fact that the greenness of a leaf, for
instance, does not reside in the chlorophyll - as the text books glibly inform
us - but in the ~~effect~~ combined effect of the rays of light ^{of} falling on our retina
& on the leaf, leaves ^(but!!) so far as I can see, untouched the assertion ~~that~~
~~so far as~~ ^{for} without chlorophyll ^{leaves (noun)} ^{we} wouldn't seem to us green - or if you
peel it shortly be green. I have just finished a very interesting book, Sherrington's
Man on his Nature, which I feel sure you, who are so much interested in
old science, will have read. In his penultimate chapter, the two ways of one
mind the second paragraph ends "To ^{me} what ultimate reality may be is one
of those questions which rise to the mind, that the mind of itself has not
the means to answer." Accepting that doesn't make me reject as
untrue all the conclusions drawn by Sherrington; it does give me
a healthy disbelief in all claims to infallibility.

Also I find it difficult to include a belief in variation with the view
that every creature persists in its own (not ⁱⁿ its parents') being. For, if so,
all variation would be not only founded on ~~its~~ ^{the} inherited genotype, but
would be displayed in its earliest stages - Actually it would seem
that, on the whole, plants at least, show more variation in their later
stages. If one agrees that the word urge, effort, persevere & the like have no
anthropomorphic meaning, then I would be inclined to believe ^{in tendency} that the living
creature is the "material" substance in which the effort to persevere & the
tendency to vary strive in different directions, the former tendency being
nearly always very much the stronger.

It is quite likely that there is much that is rationally wrong

in our way of looking at evolution. That way is certainly blind to whole sides of the process. And one can see very little chance in imaginable time of reaching the reality of it, undistorted or unaffected by its passage through our minds. But I think that the phylogenetic view is more than convenient; if erroneous it is inconvenient misleading, but it does, ^{if carefully & thoughtfully used} give a means of obtaining an insight into origins. I think that the abstract conception of morphology is also useful; but I think that it increases the amount of distortion from reality likely to occur. And though etymologically it has precedence over the more ordinary view, I think it would be more convenient to give it another name. Need that name ^{suggest} ~~be concerned~~ only with form? What about a word embracing consistency (in the physical sense), the "wholeness" of the part or thing, perhaps including its biochemistry? Would not all that come into morphology in the sense that you suggest?

Please do not trouble to answer my letter, the excuse for which lies in the possibility that the quotation from Goethe is new to you.

yours affly
Isabel Bowne

P.S. Isn't it surprising to see Goethe using Cactus as a masculine proper noun? I should have thought that, like Ficus, it was feminine. I looked up Echinocactus in Willis, hoping to find a specific adjectival name that would indicate ^{the} gender, but could not find any. My German dictionary has not Cactus as a German word; & in the ^{original} ~~abstract~~ it is in heavy black Latin type, as though it were a foreign word. But perhaps Goethe has germanized it; & there can hardly be a higher authority.

than his for the German language!

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TRADE MARK

Fogwills Ltd.
beg to acknowledge with their
compliments and thanks the
receipt of remittance.

Guildford.

Receipt enclosed.

(1)
"Besonders bei der einträtenden Sommerzeit war
es mir wichtig zu beobachten, wie manche [Pflanze
1. B.] davon an das Tageslicht hervortrat. So
wendete ich meine Aufmerksamkeit auf das Können
des während seines Wachstums Cactus Opuntia
unförmlichen Cactus Opuntia, und sah mit
Vergnügen, dass er ganz unschuldig dicke =
= ledonisch sich in zwei zarten Blättern
entwickelte, sodann aber bei fernem ~~Wachstum~~
Wachstum, sich die künftige "Luftform" entwickelte"
p. 363 Vol. 19. Colta complete edition of Goethe's works
Stuttgart & Tübingen ca. 1851 &

^{slightly}
After discussing the seed-dispersal of Acaculia
mollis & the development of ~~the~~ ~~the~~ ~~the~~ "Pirionknie"
(sic. not Samen? Str. Pine nuts?) he proceeds
"Galt das Bis heute in Fortpflanzung durch
Samen, so ward ich auf die Fortpflanzung durch
Augen nicht weniger aufmerksam gemacht.

Am auffallendsten war ^{mir} ~~noch~~ jedoch ein ~~Strach-~~
= artig in die Höhe erwachsener Nelkenstock
Man kennt die gewaltige Lebens- und Vermehrungs-
= kraft dieser Pflanze; Auge ist über Auge an ihrer
Zweigen ^{gebildet} gebildet, Knoten in Knoten hineinge-
= trachtet: dieses wird nun hier durch Samen

2
geleigert, und die Augen aus uns-
= erforschtlicher Enge zur höchst
möglichen Entwickelung ^{zu} getrieben,
so dass selbst die ¹⁸⁵¹ vollendete
Blume wieder ¹⁸⁵¹ vollendete
Blumen aus ihrem Busen hervor-
= brachte.

Zu Aufbewahrung dieser Umben =
gestalt kein Mittel vor mir sehen,
unternahm ich es genau zu
zeichnen, wobei ich immer zu
mehrerer Einsicht in dem
Grundbegriff der Metamorphose φ =
= langte" Ditto p. 363-4.

These extracts appear under the heading
"Störende Naturbetrachtungen" The
observations apparently took place in
the summer of 1787 - I could not make
out when the work quoted was written

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AR 31



THE UNIVERSITY,
ST. ANDREWS.

19. 5. 31.

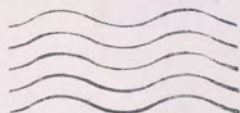
My dear Mrs. Arber.

It is a great pleasure
to hear from you — even in private.
Possibly, I am glad to hear v. with a
cheer-issances, the more because I
can have missed the correspondence in the
L.S. vol. I shall now look up.

I am quite ready to accept v. Doubtless
with a well-flown, for which I have so
doubtful it is my own mind; but how the
can be the more, I have no idea,
and find all my difficulties.

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ANDREWS
FIVE.—
— AM
MAY 31



ST. ANDREWS
— FIVE
II. —
20 MAY 31
+



Mrs. Agnes Arber.

Huntingdon Road.

Cambridge.

See Agnes Arber: "Edmund Spenser and Lytle's
"Niueve Idesthall"; Notes & Queries, May 16, 1909, pp.
345-347, + letter to Times Literary Supplement, April 2, 1931

That has a botanical.

Yes, very briefly.

Diary of W. H. P.

... is not plant. ... in the book
... for a ... name, either in
... a medicine Latin.
... cherisamen I don't know,
it must be quite late. Cheris, a
... is (Phlomis) Arabic or at least
... ; a very late word in the crusades
... I know.
... cherisamen, a cherisamen,
... a puzzle.

One with boys and a scansion;
but I fear that a three-syllabled
cher-y-samen is rather too long
for the line. It might be my date in
script, like 'The public Square'.

I hope all you will with you. It
is but Saturday in the R.B.G.,
... to R. B. G., with

A232

20. 5. 41

Bledlow Ridge
High Wycombe. Bucks

8106

My dear Agnes

Many thanks for your gift of your delightful paper on 'Leaf
& Root in Magnosperms', - & also for that on 'Linnaea & Boraginifera'
which I am v. glad to have, but which I have not read carefully yet.

I am much disappointed to find that it would not be quite convenient for me to attend the Luncheon meeting, - to meet you & to hear

your address on N.B. & M., & to have the social intercourse which one is cut off from here, of course. But when your papers arrive, I shall

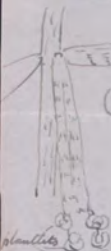
I had your company intimately, in a very privileged way! - It will take me some time to assimilate what is (to me) such an original point of view, - backed as it is by so many of your own observations & thoughts. It is very refreshing the way

you bring in views of so many past worthies, such as Spinoza, Descartes, Philosophy. I remember you were studying years ago. Even Oken you find some gold in. My grandfather, usually a fair-minded man, wrote on a slip inserted in his copy of 'Elements of Physiophilosophy', "Contemptible rubbish": - but he lacked your insight.

Your writing is so lucid, that you make your points perfectly clear. I felt a little shock in reading of 'the leaf as partial & shoot', - & still more of the suggestion that - to put it crudely - 'the root may be a sort of skinned shoot'; - but you throw that out tentatively, & with a delightful sort of protective screen in P 2, p. 102! - Anyway, you make one think, & on

never asserted, How W. B. Bateson would have enjoyed your
 reference to his ^{by his} Chinese sports! I had the privilege of
 being shown his experiments at the 'John Innes' - so many years
 ago. At Clemons, our friend the Kennedy's were much
 pained when after vain attempts to propagate their Paulownia
 shoot-cuttings, they were successful with root-cuttings - Some
 the shoot-skin reappeared there all right. - I after think of E. Sander
 saying to me, 'Do you know what my pet dictum is? - anything can
 grow from anything!' - I hope that her health is better than it was.

We had been growing 2 sp. of *Kalanchoe* in our limestone greenhouse
 & the friend who gave us the plants, & who did not know their
 names, sent me later their flowers. One has large ovate
 leaves (like those of *Dryophyllum* leaflets), which may, I think,
 be *K. daigremontiana*. The other has whole of 3 sub-cylindrical



leaves, with plantlet-bearing teeth only
 near the apex. If you happen to know
 what this sp. is, I know my friend could be
 glad to learn the name - as I should be.

Bryonia flourishes on the chalky slopes of this Ridge.
 I had not noticed before how hairy the tendrils may
 be, the hairs occurring sometimes ^{also} on the spiral part.
 No doubt Hagerup has a reason for regarding the
 whole as a branch with a pseudo-terminal leaf,
 without his evidence one might well take the same
 structure to be a modified shoot. The narrow
 lvs, of course, quite a different arrangement, & there, we
 are told the tendrils sometimes bear flower-buds (in hills)
 as in *Melons*, I think the spec.

- I must not waste your time more with my hesitations,
 but, once more, express my hearty thanks & congratulations.
 I hope that you had an appreciation audience at the
 dinner, & that Muriel was able to attend, & that all
 all is well with you both.

Edith joins me in much love
 Yours G. Hirst

A 233

167

16

THE UNIVERSITY OF LIVERPOOL

TELEPHONE: ROYAL 5460



THE HARTLEY
BOTANICAL LABORATORIES

16th May, 1941.

Dear Dr. Arber,

Please accept my best thanks for your interesting and valued paper. I have read it with great care during the past peaceful night.

Once I had begun on the text, I began to hope that you would venture a little farther into the realm of wholism, giving no suggestion of prominence or priority to any member of a body; but stating frankly that the colonial unit may be membered in varied ways.

It may interest you to know (and to think about) the fact that, whereas the primary root of many land plants is patently a direct continuation of the primary shoot, and may be the one primary member of the body which is defined inter-callary; and whereas some of its characteristics accompany this inter-callary origin, it would not be proper to make this root-formation subservient to shoot-formation. For, although the matter does not seem to have attracted much attention, shoot-formation from root origin is quite common. At the moment, I have in mind particularly the regular formation of leafy shoots endogenously from the roots of some Yuccas and Dracaenas (to mention but two). Still further, as with lateral roots, so commonly, so also with lateral foliar shoots, and with lateral flowering shoots, the origin may dominantly be completely endogenous.

Should you care to follow this matter up in thought, you may be drawn to the conclusion that neither by mode of origin nor locus of initiation is the character of an organ determined; nor can its priority over other organs be established. Rather might it seem that there is potential within the body as a whole an antithetic process of morphological expression: cylindrical growth being restrained, foliar growth being its sequal, so long as the products of growth may be accounted superficial. And, failing that, cylindrical growth by an endogenous process being explored and prosecuted, as in endogenous root-formation, and endogenous ^{shoot} initiation. So that when, as in the former, cylindrical growth may be maintained, members of leafy form need never arise. Whereas as in shoot-formation by

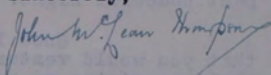
endogenous origin (be it foliar or flowering) limited apical growth finds its counterpart most easily in superficial leaf-formation, and the creation of floral organs.

Despite the interest of the thoughts of ~~Mant~~ and of many of the postulates of Troll, it may seem to you, as it does to me, that here are matters worthy of consideration before the thoughts of others ^{are} husbanded into old grooves made new.

I am glad that you have written on the subject. I hope that more may be expressed about it when peace comes again.

With kindest regards,

Yours very sincerely,



Dr. A. Arber,
52, Huntington Road,
Cambridge.

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A 234

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THE HARTLEY BOTANICAL LABORATORIES
THE UNIVERSITY OF LIVERPOOL

22nd May, 1941.

PROFESSOR J. MCLEAN THOMPSON

TEL. ROYAL 5460, EXT.

Dear Dr. Arber,

On reading your letter of May 18th I was full of regret for expressing, even haltingly, what had been in my mind. And this, particularly, in view of the fact that I had hoped to show my lively interest in your most recent writing.

Far be it from me to fancy that you would be ungracious in reply to what was, indeed, no criticism; but rather a sidelight on the matters you had discussed.

In future, however, I must take to heart what I deem a kindly lesson: attempting to assure that my arguments will be grasped and understood. It would seem to me to be a sad reflection were one to think that lines of thought can never meet.

I trust that your prediction may be falsified. Please do not bother to reply to this note, unless you are so minded that you might think response profitable. It may, however, help you to understand the standpoint of a Holist regarding land plants if I state a simple matter as follows :-

Every land plant with stems and leaves and roots has such organs from its early inception. In no known instance does any one category of such organs precede any other in inception. In no known instance is stem or leaf or root absent from the moment at which the basic body of the creature is defined. To give, in thought, priority to any organ, be it stem or leaf or root, would seem to me to deny the plants behaviour. And even then, collectively these organs would not be the whole plant. The suspensor would still be an integral part of the whole. Its discarding would be a major incident.

41

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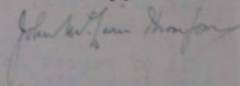
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of my

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I was glad to know that Cambridge has suffered little. Here there has been much damage and loss of life. I regret to say that my Laboratories have suffered somewhat during the most recent troubles. They are, however, fully repairable.

With kind regards,

Yours very sincerely,



Dr. Agnes Arber,
52, Huntington Road,
CAMBRIDGE.

Dr. A. Arber
52, Huntingt
Cambridge

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ered little.
fe. I
red somewhat
however,

Part of reply to Prof. McLean Thompson, May 25, 1941

Your line of argument about the non-priority of any one member of the plant body, is a very interesting one, but I cannot altogether fall in with it. You say that no member should be given priority in thought, because no member has priority development in ~~category~~ ^{but} but this seems to me to be a non-sequitur. One cannot but attempt to see the various members in some sort of relation - that is something which the nature of the human mind compels one to do - and I do not myself feel that there is anything unreasonable in supposing that these different members may represent expressions (in varying degrees of completeness) of a nature which is shared by them all. From this particular standpoint, the order of development - whether simultaneous or successive - does not seem to be relevant.

Returning to your first letter -

I take it that you ^{mean} that cylindrical growth is primary, and that foliar growth only occurs when for any reason cylindrical growth is inhibited. If I am putting the right interpretation on what you say, are not you yourself giving a kind of priority to stem over leaf? But this may be a case of my failing to follow your argument.

Completed
copy

AR35

4th June, 1941.

Dear Dr. Arber,

I had meant to write to you during the week-end ; but it has been tempting to be away.

I did not fancy that you would accept the view of non-priority for any member of the plant body. But I did not think that you would read me as suggesting that "No member should be given priority in thought, because no member has priority in development."

It would never occur to me to put the matter thus. Rather would I accept the fact that neither stem nor leaf nor root has priority in development ; and in doing so, I would hope not to be led into a frame of thought in which "because" would rule a chain of reasoning.

There would be no question of a non-sequitur. But there would be immediately one were to rename a cylindrical body as a whole a stem, particularly if one made this renamed body a prior thing to a root which may itself be cylindrical.

In my mind, the order of development - whether simultaneous or successive - does not in itself justify any priority. But I would venture to cling to the view that any theory which may be advanced does not run counter to facts of development.

You would be right in believing that I have thought of cylindrical growth as ever imminent as one of the first escapes from the inhibition of spherical growth : and you would be right in thinking that I have imagined foliar growth as one of the easy escapes from the inhibition of cylindrical growth. But you would be putting a wrong interpretation upon my thought were you to conclude that I am "giving a kind of priority to stem over leaf." Were I to do that, I would be making a developmental succession a most relevant matter, giving to it a priority in thought which I have never ventured to conceive, despite the many assertions which have been made mistakenly in print on this matter.

And you will see, perhaps, why I raised the point of endogenous development. It is because a cylindrical body as a whole can, and often does, 'evade' the antithetic process of 'leaf -

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formation' (which is so commonly affecting superficial tissues of the portion of the body which is called stem) by forming directly and endogenously other cylindrical members which bear leaves in turn immediately their own cylindrical growth is inhibited. And here again one has what is happening so commonly in roots; the extension of the organ by cylindrical members, each of which may remain cylindrical, and all of which begin life endogenously. And yet, as in such cases as I mentioned in my first letter, these members may become in time leaf-bearing stems, freely exposed above ground for all to see.

And so you may perceive how the conception of a chimera, such as you advanced in explanation of a root, made me think that somehow your line of thought necessitated a stem to the formation of a leaf, and also to the conception that a stem and a leaf were jointly necessary to the interpretation of a root. One can think of instances in which it would be as easy as it would be fallacious to suggest that a root takes priority over all other organs; even to the point of suggesting that a stem is a chimera of root and leaf. I met this recently in a statement by Willdenow which ran as follows: "The root is the plant indeed: all other members ~~are~~ but its mirrorings."

With kindest regards, and best wishes.

I am,

Yours sincerely,

P.S.

Developmentally, a plant has ^{two} apices. The one is the basal cell of the suspensor; the other is the apex which gives rise to the plumule. What is called, later, the root apex is defined in the intercalary manner. The apex which gives rise to the plumule is not a thing apart from the cotyledons. Neither does it bear stem nor shoot or branch. ^{It is the apex which gives rise to the shoot (stem & leaf) through the life.} Commonly we name the cylindrical portion of the protuberance a stem, just as we name the expanded portion of its axis a leaf. So an intercalary portion of a body, a primary root embedded in a plumule (together with the organs named, in time, leaves & stems). The only "leaves" which could enter into discussion on the chimera nature of a primary root could then be cotyledons.

Dr. Agnes Arber,
52, Huntington Road,
Cambridge.

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So I see this matter, any line of approach which involves a mental assembling of organs which are categorized ^{in the end} is a false presentation for life cases: and in so doing gives a false impression of a race. Even if no leaves (other than cotyledons) were required,

the cylindrical portion of the apex referred to above would still be called a stem. What necessity would there then be to suggest that the root is a stem?

A general conclusion in uniformity with fact of development & of maturity: what is called stem is gradually created between a free apex and an intercalary meristem (that of a primary root: the stem is neither primary in conception nor in location); not in the leaf of the root, or any other organ.

reply to
Prof McLean Thompson

52 Huntington Road
Cambridge
June 7. 41

(16) 7

Dear Professor McLean Thompson,
Thank you so much for
your letter. I am very sorry that
you had to write a second time to
correct the first copy.

I see the force of your
"non-priority" view, & I feel as though
it would be possible to incorporate it
into the idea I have of the
plane body - but this will take
much pondering. It seems to me
possible to look at the plane body

formation' (which is so commonly affecting superficial tissues of the
portion of the body which is called stem) by forming directly and
immediately their own cylindrical members which bear leaves in turn
again one has what is happening so commonly in roots; the extension
of the organ by cylindrical growth is inhibited. And here
cylindrical, and all of which begin in life endogenously
as in such cases as I mentioned in my first paper.
may become in time leaf-bearing
for all to see.

in varying ways, that these varying
ways are not necessarily "right" or
"wrong".

I feel most grateful to you
if the time & care you have taken
to give me your views on these
debatable questions.

- 2 -

formation' (which is so commonly affecting superficial tissues of the
portion of the body which is called stem) by forming directly and
endogenously other cylindrical members which bear leaves in turn
again one has what is happening so commonly in roots. And here
of the organ by cylindrical members, each of which may remain
cylindrical, and all of which begin life endogenously. And yet
as in such cases as I mentioned in my first letter, these
may become in time leaf-bearing stems, freely even, these
for all to see.

- leaf. to see.

L (91) ^{msl}

July 5. 1928

Dear Mr. Fisher

Many thanks for paper on the Tree Habit & distribution! I am always interested in these things. quite apart from the fact that I have just done a course of 70 lectures on the Biology of Insects, only touch the fringe of the subject. I will begin a memoir on the 'making of the Tree'. but found it up a too difficult. This is what one of the biggest things left in Botany. I was but

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hopelessly, & after all the Problem of how to make a 'tree' for a research in water along me. I always like your method of extracting the roots of wood of y and I ignore those who venture etc in the subject. But you will have another shot yet.

11

OXFORD
27, PARKER ROAD

(since I would have asked you or anybody else
otherwise the origin of giving a definition of a cell
or saying what exactly is a 'cell'. Hence I
enclose a few old notes which if you like you can
incorporate in the next. By the way did you see
the remains in 'cell wall' in the Amn. & protoplasmic
of hobby membr. (with the) re-

Yours sincerely

Arthur W. Lawson

By a tree is understood the general form of plant based on the land as the result of 4 structural factors.

(1) The indefinite growing apex.

eg. if the apex grows 1 inch a year, it will be 100 ft in 100 years.
There is nothing like an indefinite branch.

(2) legative perennation, which enables it to grow erect.

(3) Mechanical tissue branch - wind action & heavy the weight.
but in a lignified cell walls.

(eg. a Palm on Crete rises 150 ft. trunk, with only
these 3 factors: is it a tree?)

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(4) The evolution of a climbing vine, which grows by climbing
(Panicum, the fig, etc. any others).

Such tree forms arise in all parts of Phan. sylvatica. It is understood
cells do not carry to top. All such forms would be perennating.

ii. The wind / wave form association.

These form association arise in all parts of Phan. sylvatica. These
were wind / wave forms of Phan. sylvatica at the table Decon. sylv.

See Lecture 100
(1907)

- Hence it follows that the tree is not so much the primitive form
as the generalized result of all land forms.

- Invariably living in the N. Hemisphere, on the verge of the
limitation of tree life by cold: also being almost
dependent for food on cereals & herbaceous life: also
- the human race is responsible for the preservation of all
forms. I have known 4000 years in tree root tree life
still dominant. - We get our my. roots in the relation
of trees to the vegetation. The subject has been
abstracted in a different way. Any student who
talks rubbish about these & thinks he only knows in
a Botanic garden, or in the carriage of the broad day

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herbarium. The form is distinct class. The form is
the generalized form of land life. I always have
been (at least we know's back the Oceanic).
pines the visible feature the land surface was
covered with form form from the first.

of the 4 factors. The primary point. The origin of metabolism can be followed in a large of the present day. So far present notice especially. But the origin, utilization, determination, loss, or replacement of key enzyme groups, is a different physiological subject. I hope to leave it alone. Also the evolution of utilization of the physical behavior of a ligand itself. Introduction even to the Chemical Physiology. But not also find it too involved. I also too much of a case, since ^{can} we have no evidence of the beginning, whatever. The 2nd factor called the tree enables us a bit to look at the present time. so I leave that point.

But the great point is the thing that is lost. In such a point the combination give the best tree forms. viewing the kind of their tissue equipment. But this will always be failures. Hence every forest contains a set of what I call forest failures. which I suppose fact have been down, with the result that to blank many old records of taking in special biological states. and situations or alterations of getting them living.

Each forest fulfills its own brain. It is the labor of the
Vegetation. When I think that with the foolish heading of
Quint's. I should not call the Rhizomata plants
which have the vegetative growth - but I know it is not!
I think they are Forest or Tree plants. I like to
call them the Forest from its growth. They found their
rest; even in the woodland retreat and in the low land limit. (even if not, 1 of each).

But once you get the idea of the forest, all are trees.
You see that the brain are just a tree again.
No change. It is the labor of the Rhizomata plants
having got the best state of vegetation survival. can even
go back to a tree form. I regain its actual being. The
closer I get to back.

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how we come to the extra forest vegetation, which is
commonly called a habocean. This vegetation form where
trees will not. Why: because some necessity of tree life
fails usually with it.

The habocean extra forest plants are usually called
Habocean Perennials, or Humoral plants in the limit
annals. i.e. all vegetate for a short time. I presume
the rest of the time.

There are equally biological states, referring to the short
recesses. The recesses have been developed from normal to the
the old recesses again (which I am not wanting). An annual
may water quite good timber in no recess, only being
in recess. It does not live large.

(A good class of the timber annuals cut down by
bill hook, has no timber in it then a 5 year
and Pinus.)

Next it is seen that all these 'Hebecum Plants' are also
biological states: that they are plants which grow where
Trees do not. (e.g. in forest clearings, living at the gaps
where trees fall). It is spreading beyond the forest canopy.

It is also seen that like the Rhizomatous ferns, which
also travel out of the forest. All extra forest plants are
equally tree ferns: and also equally in to both

of both giving to the gap. The fact that such plants
by the unusually observing of the growth of
a tree in flowers, implies of the system. It may indicate
the effect of their extensive conclusion is to diffuse of
getting a normal living. In the present world they are
still fortunate and occasional, just as much - no decide

1910
1910

from as complete a tropical rain forest in evolution to
 come the can only go on getting more steadily a
 'tropical' since now the forest. But when the future
 will bring the same life in its further evolution
 nobody can say. The addition of the human factor
 which in every country since all the forest of 3-5 years
 it can, quickly cut out all chance of development
 of the forest along side trees. By clearing the forest
 & encroaching tobacco farms, no way helps to
 change the direction of the forest, but it will be abnormal
 since human assistance. To understand the forest and
 will be to understand the big forest of S. America in
 which the forest is, I have been there all the time.

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some of these 5000 sq. miles of more or less
inland forest, and its furnishings and
 clearings. If the world were still covered by dominant
 forest, the forest life would be the only vegetation on land
 as we know it, and all 'failures' would be secondary. The
 better tendency of man's art, in to say the least the influence of
 the failures as the expense of the big trees. (Failure to kill the
 big animals.)

al 37

MUSÉE ROYAL
D'HISTOIRE NATURELLE
DE BELGIQUE
—
31, RUE VAUTIER, 31
BRUXELLES

Bruxelles, le 25 Nov. 1919.

N°

Année

Madame,

— J'ai eu le vif plaisir de recevoir votre aimable lettre et les deux importantes publications que vous avez bien voulu m'envoyer sur le sujet qui nous intéresse tant tous deux et qui, j'ose le dire, doit intéresser tous les biologistes.

Dans cette matière, la difficulté est que les objections peuvent arriver des directions les plus variées, qu'on ne peut connaître tous les groupes d'Organismes d'une manière approfondie et qu'enfin, comme disent bien vos compatriotes, on ne peut être un spécialiste général!

A ce point de vue, je suis particulièrement heureux que vous ayez bien voulu vous charger des Végétaux, domaine dans lequel je manque absolument de compétence.

Votre proverbe : « The baked bread can never go back to the dough » est excellent, et vous l'avez appliqué d'une manière juste et piquante !

— Je regrette beaucoup que votre frère n'ait pas eu l'occasion de passer par Bruxelles : je me serais fait un plaisir de lui montrer notre Musée.

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Mais peut-être viendra-t-il une autre fois, et, ce qui serait encore mieux, en votre compagnie !

— Je regrette également de n'avoir pas de nouveaux travaux à vous offrir en ce moment, mais j'en prépare et, dès qu'ils auront paru, je vous les enverrai.

Veillez agréer, Madame, avec mes remerciements réitérés, l'hommage de mon respect,

L. Dollo.

A 238

Linnean Society,
Burlington House, London, W. 1
4th Octr 1918

Dear Mrs Arber,

In accordance with your
letter, I have altered the sentence in
your abstract thus:-

"... if the organism subsequently
^{has occasion}
~~experiences the need~~ to replace it..."

Yours faithfully

Raymond Jackson
Gen Sec.

Mrs Arber, Esq.,
52 Kentingdon Road,
Cambridge.

If I should have 5 again
 the endosperm - the tetra-nucleate
 sac of another is diploid,
 the miltlets again - this
 suggests that it is a
 theory. It would be necessary

This is a 5 nucleate. Then it is a 4
 nucleate. The four of 3 nuclei
 is a 4 nucleate.

To go as far as possible
 into the origin of the endosperm
 in all the second cases of
 4-nucleate cases
 No instance that is at
 least 3 tetra-nucleate cases
 Plumbagella, Cypripedium
 Gastrodia, in parts of the
 being 4 nuclei, two, then
 first 4 nuclei, endosperm nuclei
 is a 4 - Cypripedium
 second. In Cypripedium
 Gastrodia is a 4-nucleate
 when they are four
 appropriate ones

Nov 12

Oct 39. 58

Department of Comparative Anatomy,
The Museum,
Oxford.

Dear Mr. Arbut

Your question is a little
difficult to answer. I don't
know of any work in which you
will find the subject of Insect
respiration treated from your
point of view. The mere facts
as to the respiratory organs in
terrestrial & aquatic insects
you will find described in any
good treat book such as Parker &
Haswell's ^{Zoology} or Lang's Comp. Anatomy
Also about Crustacea & Trilobites
primitive aquatic arthropods
(the Trilobites in quite modern books
such as the new American edition of
Zittel) — but the impression that
that the original aquatic gill (ventral

& attached to the limbs) has been
lost & later replaced in aquatic
insects by a new tracheal gill
(dorsal & having nothing to do with
the limbs) may not have been
definitely expressed before - I am
not sure. Of course there is
an element of doubt in all phylogenetic
speculations of this kind - but I
think it is very small in this
case. I shall keep your question
in mind & if I can find the time
try to answer it.

Yours sincerely
E. S. Goodrich

Al 40

Nov 13. 18

Danehurst,
Branksome Wood Road,
Bournemouth.

Dear Mr Asby

Unless I misunderstood

the very epitome of your
paper & the Report of the
Meeting of Lin. Soc. does
not "stagnation" and "reversion"
mean title afforded to "Goulden"
~~and~~ ^{of} a structure or organ
once lost & the cause of polytomy
can never be regained?"

Would not the following contradict
this "Law of loss"? Let us take
Dianthus aeneus Brakham
(in Flora & "Sketches of its
several "varieties" as well as
the series called "the direct
action of Change Condition of life".
(See Variation under Cult. 45

65

of (Shadokh Flora) regard
him as genuine species
a description of.

1. P. aequi³ has distinct
veined leaves an almond
herbaceous due to self-mosses:
It when it grows in ground
the leaves "revert" to
complete form.
Many plants have the 2
forms - asexual and sexual.
The leaf is reticulate in
the (Colubridae) the the
vening may occasionally
in discuss the acquired
abnormal character are
flected by the aerial and
inverted ones. ¹² revert to them.
It would be the "law of loss"
of these "evolutions"?

standing in accidents: but
the the remain may be
incomplete & the organ may
a few miles.

Then Hepparis had 3 toes
to each foot: ~~but the middle toe~~
~~like the other toes~~ & but
its disced out, the long,
for only one (the middle toe).
It could be from occasionally
with 3 toes: but ^{2 are} rudimentary
like those of a foot.

Botan. occasionally have
gill-slits: but of course
only a "hint" of piscine
evolution.
So the "Loss" may
be only partially extant:
or perfectly, as in asexual

plants.

Please forgive me if I
have misunderstood your
short account, for I
presume you are already
quite aware of the
few cases I mention.

But I do not know
how they would fall under
any Law of Laws: forced
or actual.

Yours faithfully

Jos. Huxford

8/12/13

A 241

62

Dear Mr. [unclear]

I was much interested
in hearing of paper of the
happy way it was put
together. It is always well
found out in some other
branch what has already
been found in another.

Dr. Bonpland who talked with
me today on the subject of
who is as well up in all
general botanical questions
as in Reptilian & Piscian
asked me if you referred to
Cecilia an Italian who
discussed Gallo's views
in 250 ago for the botanical

and point, & doubt I who
shy on.
That truth be not the
in mind was the appalling
problem of Oliver & the
return of the "Big Blunder"
to an one man, not to
be always reminded him
a person also selected
older, I said he had no
ration in which to perse-
vere. Notes seem to have
no palaeontological know-
ledge whatever!
You have been positively
anxious to think
that is a great step

forward & you have well
again emphasized the
fact that the passage
cannot be properly studied
by the report of each of
the allied reviews.

As for the little girl I
was going to see, was
being dead while we were
talking; - a newspaper
called me out of the
room being Hayfield's
pater. She was the
sublicranis of the foot
for. & had endeared
herself to all who

came in contact with
her. The star fully
covered of a good
firm unit position
in it. The land made a
considerable mark
just at 26! And so
my racial are allowed
to flourish the earth!
Miserable
Warrior Nelson
J.

AL 42

63
Trumpeter Lodge,
90, Godwinch Road,
Bromleybury,
Surrey.
14. 11. 18.

Dear Dr. Ascher,

I was very sorry to have to leave early last Thursday but hope to make amends by giving a paper on the pappus in the Compositae as a trichome structure & as an example of the Law of Hess.

The President in reply to my apology for running away asked for such a paper and I hope to meet Mr Woodell's criticism very thoroughly.

One point which is not in Chap. V is that most of

the Lower Oligocene Compos.
have a setose pappus and

no fossit Camp. is known
with a typical calyx, even
down to the Lowerocene.
Another point is that
hairiness and pappus or
glabrousness and no pappus
go together very frequently.
I have just received a
revision of Vigueria in which
a great point is made of
this phenomenon, which
seems to indicate Mendelian
inheritance of pappus hairs
and other hairs as a single
factor or character. There are
a number of other points also
but these are the most
conclusive.

Herrington's address is
drawn up & will be circulated
soon. I am sure you will
approve of the reduction of
the original number of
women voters to the points.
Yours sincerely,
Your G. M. S. Wall.

Dallas Hunt to Brewster on road N. S. New York 64

Dear Mrs. Brewster

A 243

Many thanks for your letter. May I venture to advise that "characters which vary with external conditions" are the very same as those which are recognized as found in the course of phylogeny.

There is no other source of the origin of species than that Darwin called "difficult variation" (See Principles of Geology, 2nd ed., II, pp. 270-272).

Compare Brachyotum Brachyotum with Hooker's Storcula Storcula Storcula: 2nd B. calls "varieties". Hooker calls "species". Which acc. to B. are only self-adaptations to various conditions.

I found a form with R. hederacea in the road at the side, but the water had risen and some plank was in it. They produced dissected leaves.

March: N. hederacea, Brachyotum branches up into the air; here stuff with water, air and pollen Spiderweb & Stomata

Why the following are better phyla³
= good cases.

1. Labiate flowers are "theoretically" ^{and} ^{as} ^{usual}
phylogenetically "referred" to ancestral
regular ones. Several of the Scutellaria
Solna, Meast - etc. have been found
with perfectly regular corolla +
5 stamens. So. perfectly regular
leguminous flowers: The central flower
of a fruit of Podagraceae is oft. perfectly
regular in every detail.

2. Flowers are referred to foliage
i.e. homologous with leaves.

A common form of evolution
is for parts of the whole (Green Rose)
to revert to the ancestral condition
of leaves.

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3. Leaves of the vine are homologous
with flowering branches. So that a branch
of the latter will revert to a leaf
or a terminal bear proper

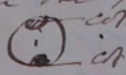
4. Tubers are characteristic of the species
Solanum tuberosum; but if the stem
be not earthed up, and a stagna-
tion of the air, it undoubtedly
grows over as a flowering stem.

5. The stigma never has assumed
"obovate form" for climbing of a wall:
but if in contact with a branch, & roots
to the old method of growing, &

both eyes to ~~the~~ all flowers have stained
the "plant-bone" which shrivel, and die,
and in a tree. This resembles the
2^d and 3^d toe-bone; on the hook is
the heel.

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For fruit stems on ground;
I have not yet seen any fruit. It is at least
plants with long leaves in deep water;
each cross blade at the scapule;
Arrow head and Nymphaeae; both stems have
linear - sagittate - stretch broad leaves.

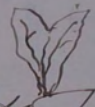
I could not accept Miss Sargant's
 1850: because ~~no~~ other things result
 from the position of the embryo: I then
 1)  } I do not remember that it

ever explained how one or both cot:
 could come close together like cotyledons
 of both moved 90° or one, 180°. If one
 would still be two. Now the two
 cotyledons, being opposite, the distance
 will be $\frac{2}{5} \times \frac{3}{8}$ etc in the Notes.

But all embryos are $\frac{1}{2}$ or $\frac{1}{3}$ as
 the only possible series follows a
single cotyledon (see my paper & Fig.

Trans.). See the ad before the union
 for the cot: they forked

This occurs in the seedling
 See Archibald's book: p. 1. in the position
 of Sirindwona;



Commonly ~~but~~ the seed
 The ad have seed? cot opposite the retone

both regard to the Phyllodes & Midrib of Marsip:- as the base ^{of stem} cord is one scattered, a central part (not central) part of a leaf is the forming the second midrib, by clinging together. They then throw right and left separate and branching off a branch: Consequently, there is no difference between a branch leaf (so called) and a flat phylloide: the latter I often became stems as well as a flange at the top, as in arrow head:

Yours very truly
G. H. R. S. S.

P.S. I have just sent all my collection
to the British Museum Natural History Library Department
of 400 hours the to examine any a Phyllodes. Prize of March to be separate boxes, so can be easy consulted: with pleasure as well.

AL 44

ATAVISM 67

IMPERIAL COLLEGE OF SCIENCE AND TECHNOLOGY.

(ROYAL COLLEGE OF SCIENCE)

SOUTH KENSINGTON,

LONDON, S.W.7.

17.11.18

Dear Mr. Asker
 I should like to have
 heard your paper on the
 "law of law" at the Luncheon -
 The name so as our veins seem
 to run on parallel lines -
 The abstract, which I have just
 seen, is short, but so far as
 it goes it exactly embodies
 what I have myself been
 teaching for a number of years.
 I only ~~think~~ think that I may
 have made something of it
 too, in connection with his wedding

out" hypothesis - I wonder
if you happen to have read
a chapter on relations of plants
to water in a little book on
"Plant life" I wrote some
years ago for the Home Sci. Lib.
series - If you haven't you
will see, on p. 139 how closely
our ideas seem to run in
parallel. These things are
apt to get in the air &
occur perhaps quite independently
to a number of people at
the same time.

I take this opportunity of
sending you a copy of a
recent thing of my own on
wind & water - yours
H. Farman

THE BRITISH ECOLOGICAL SOCIETY.

Founded in 1913.

Honorary Secretary:

E. J. SALISBURY, D.Sc., F.L.S., The Briars, Crosspath, Radlett, Herts.

16/XI/18

Dear Mrs. Fisher

Many thanks for the remarks you so kindly sent. I was sorry we had no opportunity of being a deal after the Linnean meeting the other night as the phenomena with which you dealt have greatly interested me too for some years past. As I said then one has the feeling that the non-reappearance of organs is but one aspect of a broader ~~principle~~ more fundamental cause that for lack of a better term might be called 'momentum' in evolution. The assumption of the completely isolated position in which leaves cannot be explained on a law of loss but obviously is of the same category as the phenomena with which you deal and I think phylloclades come under the same heading. If it is granted that there is a tendency for evolution to go forward in the same direction then the absence of repetition after loss would be explainable on the law of probability. For surely it is true that lost or latent characters often reappear but the reappearance of a complete organ demands the simultaneous reappearance of a whole group of characters. Is it not then

rather a very remote possibility that an organ could reappear
through I grant the objection is theoretical rather than
practical? Of course it is impossible to prove a negative
& therefore a 'law of loss' cannot be demonstrated but if it
is merely one aspect of a broader law of movement in the
latter should be demonstrable by the study of mutations.
In so far as there go they do seem to us to indicate
that mutation is not, as generally assumed, equal
in all directions but tends to be directive. Thus in
the mutations of beetles parallel mutational forms have
been described from a number of parents. I was very much
interested in your paper, as it called attention to a
group of facts that clearly require explanation and
I hope you will forgive these somewhat undigested
criticisms and admirations of an imperfectly
crystallized hypothesis.

With kind regards

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E. J. Salisbury

POSTAGE PAID

POST

A296



CARD

FULL



11. 15 PM

17 NOV 13

Miss Allen

52 Huntington Wood

Cambridge

March: but I don't like
the hand line round the
mouth (on the deck).

If I were you I should
wait for Petrocaric
essence - you've got your
fact out - your full
paper can wait with
the whole pool. & pal.
part, so as to add note
if found necessary

WJ 69

THE BRITISH ECOLOGICAL SOCIETY.

Founded in 1913.

Honorary Secretary :

E. J. SALISBURY, D.Sc., F.L.S., The Briars, Crosspath, Radlett, Herts.

20/XI/18

Dear Mrs Arber

I had not read the passage of DeLo's to which you refer but as you say it expresses the same view as I put forward in my letter.

I am sorry I did not make myself clear regarding the Altit waxes and the point of my reference to cladodes must or consequent have seemed obscure.

It is, on grounds of comparative anatomy & morphology, evident that the Altit waxes are not originally but normally orientated leaves?

(In not sure about this) ~~same species than this condition but~~ must have either a profile position through twisting of the 'leaf' base or a completely inverted one through the ^{twisting of the leaf base} ~~twisting~~ ^{twisting} but quite crudely and shortly

the point is that in the process of evolution it has evidently been easier for the plant to go on turning its leaf rather than to turn it back, despite the fact that the continuation of the original line of evolution (all it 'momentum' predestination - What you will!) involves the differentiation of the morphologically lower side as peltate & the original peltate as spongy tissue

Again to cladodes and ~~perennial~~ ^{also} phylloids there are numerous cases where these structures are so expanded as true leaves yet in every instance it would seem that it has been easier to go on elaborating the clad ~~into~~ ^{into} 'leaf' ~~for the phylloids~~ rather than to increase the development of the leaves themselves, ^{though} these have not been lost but merely reduced. This is all ~~the more striking~~ ^{the more striking} in view of the facility

cf. also
Galium
Malva

with which many scale leaves can be induced to reassume a foliar
character as (e.g. scale ls. of rhizomes)

The leaf like development and enlargement of the stipules in *Alysicarpus*
is a similar case and one could multiply instances.

These all seem to me to support the 'momentum' hypothesis
which if admitted makes the assumption of a 'law of loss' unnecessary.
At least so it appears to me.

Regarding reversion ^{if not very} some think ^{it is} purely pathological and I personally
am for some scepticism the Woodellian view as to their universal
value. But from the frequent occurrence of certain 'abnormalities'
(the term is unfortunate) and the evidence from comparative
morphology of their correspondence to ancestral characters
one cannot I think deny that ~~some~~ reversion does take
place. The lumping of most varieties with variations and
modifications is as much to be regretted as the indiscriminate
assumption that all or nearly all have a phylogenetic
significance.

Trusting I have made my point clearer
~~with kind regards~~
Yours sincerely
E. J. Hellicar

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P.S. Excuse scrawl as I have to write this in the
train.
E. J. H.

Dear Mrs Weber
Many thanks for your interesting
letter. I should like to write you on
two points. 1st: "Abnormalities adjacent
in phylogeny?"

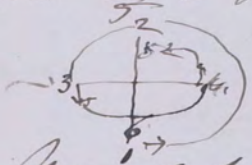
In regard to flowers, when a "regular" flower
becomes "irregular" in the first place, there
is an ab-normality; but when it becomes
permanent and hereditary it then passes
into phylogeny. Similarly leaves which
are oval etc. in the Embryonic, become
spiral and permanently hereditary in
Scrophulariac Apice Species.

Many occasional "spots" are not
hereditary: all such have nothing
to do with phylogeny; in the Embryonic
which first caused the abnormality
must be permanent. Garden varieties are
"ab-normal" but if from Mendel's law
cautions ± 5-6 years become ± "fixed"

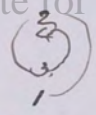
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The re-phyllaxis: In alluding to
 Monocots: I mean the all started
 with one cotyledon: the as it is a
 unimane law that (except $\frac{1}{2}$) that
 is always 3 leaves in a circle, what
 if depressed into a plane = a circle.
 So that if there be one cotyledon the
 leaves may be diakota or triple
 $\frac{2\pi}{3}$, = $\frac{1}{3}$.

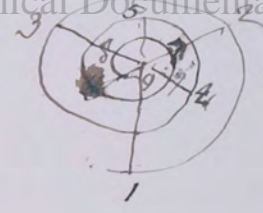
Dicotyledons starting for two ch.
 leaves follow in 4 parts as in Labrotis.
 when they separate the leaves
 arranged along a spiral line - four
 at once into $\frac{2}{2}$ in the following
 order.



Whereas Monocots beginning with
 1 may be



or



= $\frac{2}{2}$

as in the flowers in
 "traces".

75
What other causes can you suggest
for the origination of abnormal forms
which become permanent? I do not
mean anything due to disease or injury

I have a lot of my lectures
printed separately; would they be
of interest to you? If so, I shall
be pleased to send you copies.

The only copy left of 'Self-fertilization'
is found: but had I still the leaves
traces: I have cleared off a large
work now, buy a my Steth eyes.

Yours sincerely
G. H. S.

AL 49 23. VII. 19. 75

Dear Agnes I have long been wanting to say how greatly interested I was in the letter
re: your work that Marguerite sent
me. It seems to me that the law of loss has
very wide bearing indeed and to apply to
function as well as structure. Thus the
beagle Clara seems to have lost her
swallow reflex, if she had to take her
nourishment in that way now it seems
as if she would have to begin anew and
learn how to do it. Then too children
are determined nose breathers Clara suffers
when she has a cold as she insists on
keeping her mouth shut. But all old
children who have become mouth breathers
had to be taught breathing exercises
through the nose after removal of the
obstruction - the operation is only the first
step towards cure. For mouth breathing
seems to be recognized generally now
as a direct cause of ~~the~~ Karyopyc-
tosis & Bromiditis etc. It occurred to me some
time ago with reference to advanced
mental trouble that it was almost
hopeless to attempt to disentangle the
derangement of those portions of the
mind affected in constant use, that it
would be far simpler to try to
develop the side faculties as it were
to make them useful human beings
again. And if so that is surely
that one would attempt when applying
the law of loss, to Psychology?
I remember reading of a case that struck
me as being a good illustration of this.
I had an idea it was in De Quincey's
'Confessions of an Opium Eater' but

I have tried in vain to trace it to
was of a girl who had a breakdown
when in service (I am not sure if she
relapsed on returning to service, as it
is not properly controlled) but who
became perfectly well on returning
to her childhood surroundings - and
remained a useful member in
that community.
I am afraid this is very poor evidence
but I have a vague general impression
from cases that along these lines
fruitful work might be done.
I trust that you will be able to find
in a post Margaret has told me
much of this to make her acquaintance
with very best wishes for the New
Year,
Yours very sincerely
Harold Hills.

Bruxelles, le 23 janvier 1919.

27.

ANNEXE

Madame,

— J'ai appris avec un vif sentiment de regret la perte considérable que vous avez faite, — que nous avons faite, si j'ose dire.

Car votre mari était mon ami, et ce n'était pas seulement un savant éminent, c'était aussi un homme excellent dont le souvenir restera dans le cœur de tous ceux qui l'ont connu.

— Par contre, j'ai été très heureux de voir que vous aviez retrouvé, chez les Végétaux vivants, par votre Laws of Loss, ma loi de l' 'Irreversibilité' de l' 'Evolution'.

Je n'ai plus de tirages à part de mon petit travail Les Lois de l' 'Evolution', mais j'en ai fait faire une copie, que je vous adresse par ce cour-

rier.

Le volume y annoncé n'a pas encore paru! Mais j'y travaille toujours, ayant accumulé, pour l'écrire, des matériaux depuis plus d'un quart de siècle. Impossible de vous dire que, dès qu'il pourra paraître, je m'empresserai de vous en offrir un exemplaire.

En attendant, voici la liste des principaux mémoires où j'ai traité de l'Irreversibilité de l'Evolution:

1. Les lois de l'Evolution (Bulletin de la Société belge de Géologie, 1893, Vol. VII, p. 164).
2. Sur la Phylogénie des Dipneustes (Bulletin de la Société belge de Géologie, 1895, Vol. IX, pp. 96 & 97).
3. Les Ancêtres des Marsupiaux étaient-ils arboricoles? (Travaux de la Station zoologique de Wimeroux, 1899, Vol. VII, p. 199).
4. Sur l'origine de la Tortue Luth (Dermochelys coriacea) (Bulletin de la Société royale des Sciences médicales et naturelles de Bruxelles, 1901, Vol. LIX, p. 28).
5. *Eochelone brabantica*, Tortue marine nouvelle du Brusellien (Éocène moyen) de la Belgique, et l'Evolution des Chéloniens marins (Bulletin de l'Académie royale de Belgique, 1903, p. 819).

ANNEXE

6. Poissons de l'Expédition Antarctique Belge (Résultats du Voyage du S.Y. Belgica en 1897, 1898, 1899, sous le commandement de A. de Gerlache de Gomery, 1904, pp. 141, 174 & 197).
7. Les Dinosauriens adaptés à la Vie quadrapède secondaire (Bulletin de la Société belge de Géologie, 1905, Vol. XIX, p. 443).
8. La Paléontologie éthologique (Bulletin de la Société belge de Géologie, 1909, Vol. XXIII, pp. 397, 410 & 412).
9. Les Téléostéens à Ventrales abdominales secondaires (Verhandlungen der K.K. zoologisch-botanischen Gesellschaft in Wien, 1909, Vol. LIX, p. 139).
10. Les Céphalopodes adaptés à la Vie Nectique Secondaire et à la Vie Benthique Tertiaire (Zoologische Jahrbücher, 1912, Supplement XV, 1, pp. 107, 109, 111, 115, 130, 138).
11. *Podocnemis congolensis*, Tortue fluviale nouvelle du Montien (Paléocène inférieur) du Congo, et l'Évolution des Cheloniens fluviaux (Annales du Musée du Congo belge, 1913, Vol. I, p. 59).

12. *Globidens Fraasi*, Mosasaurien mylodonte nouveau du Maastrichtien (Crétacé supérieur) du Limbourg, et l'Éthologie de la Nutrition des Mosasauriens (Archives de Biologie, 1913, Vol. XXVIII, p. 625).
13. Les Vertébrés vivants & fossiles (Guide illustré de Bruxelles, Touring Club de Belgique, 1916, Vol. II, p. 123 = 13 du tirage à part, et p. 145 = 35 du tirage à part).

Vous possédez les numéros 8, 12 et 13, puisque je les avais envoyés à votre mari.

Quant aux autres, je n'en ai, malheureusement, plus de tirages à part.

Mais vous les trouverez aisément dans les recueils périodiques où ils ont paru et qui ne peuvent manquer d'exister en votre ville.

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Au surplus, si vous éprouviez quelque difficulté à cet égard, je me permets de vous rappeler que j'ai le très grand plaisir de compter de nombreux amis à Cambridge et que, par conséquent, vous pourriez consulter les tirages à part désirés soit chez les Professeurs Marr, Gardiner ou Seward, soit chez les Docteurs Shipley ou Gadon, soit chez Woods ou chez Forster-Cooper, soit, enfin, chez le Professeur Punnett ou chez Sir William Bateson.

Si j'avais su que vous vous intéressiez parti-

ANNEXE

Particulièrement à l'Irreversibilité, je vous aurais, certainement, offert ces brochures au moment de leur publication. Quoiqu'il en soit, vous pouvez être assurée que je vous adresserai, à l'avenir, tout ce que j'écrirai sur la question.

— Il est très facile de découvrir des exceptions apparentes à l'Irreversibilité de l'Evolution, mais toutes celles que j'ai pu soumettre à une analyse approfondie n'ont pu résister à l'examen, mais se sont montrées être, au contraire, d'éclatantes confirmations!

On voit alors que la structure nouvelle: ou bien, c'est tout autre chose que l'ancienne; ou bien ce n'est pas exactement la même chose.

Bien entendu, il s'agit toujours de l'Irreversibilité morphologique, — et non de l'Irreversibilité physiologique.

Quant à l'Atavisme, — parfois invoqué contre l'Irreversibilité, — il ne prouve rien, car il est toujours partiel, étant, malgré

tout, le résultat d'une lutte entre l'Herédité & la Variation: je suis en mesure de le démontrer de la manière la plus convaincante à l'aide des Chevaux polydactyles, dont aucun des cas connus, si nombreux pourtant aujourd'hui, s'il venait à se fixer comme espèce nouvelle, ne reconstituerait, même partiellement, la forme ancestrale, Tripurion ou Prototippus!

- Vous avez très bien choisi en prenant les Angiospermes aquatiques pour l'étude de votre Law of Loss.

J'en ai moi-même examiné deux cas, au point de vue de l'Irreversibilité de l'Evolution: Zostera et Nelumbium (voir n° 10 de la liste ci-dessus, pp. 106 et 107).

Je y aurait également bien des choses intéressantes à faire, dans la même direction, pour les Fleurs à Zoophilie secondaire, ayant perdu leur Corolle pendant une époque d'Anémophilie secondaire, et l'ayant remplacée soit par un Calice veillé, soit par des Etamines veillées, soit par des Bractées veillées.

Ma foi vous savez tout cela mieux que moi et vous y avez certainement déjà pensé.

Veuillez agréer, Madame, avec toutes mes excuses, pour ce long bavardage, l'hommage de mes sentiments respectueux,

L. Dollo.

A 251

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF PLANT INDUSTRY
WASHINGTON

ACCLIMATIZATION AND ADAPTATION
OF CROP PLANTS.
COTTON BREEDING.

August 1, 1919.

Miss Agnes Arbor,
Balfour Laboratory,
Cambridge, England.

Dear Madam:

I have just read with much interest your article
"On Atavism and the Law of Irreversibility" in the American
Journal of Science.

May I ask whether, in your opinion, I am wrong in
considering the appearance of pod corn Zea mays, var. tunicata
as a reversion.

I suppose there can be no doubt that the ancestors
of maize were perfect flowered and that one of the changes
attendant on following the segregation of the sexes was re-
duction in the size of the glumes of the pistillate inflores-
cences. It seems reasonably certain that pod corn has arisen
independently a number of times and it is certainly an inherit-
ed variation. Furthermore the Mendelian behavior is so regular
that there seems little possibility of explaining the varia-
tion by the synthesis of factors.

The enlarged glumes seem undoubtedly to be the
homologues of the glumes of the staminate inflorescences
though often developed to a monstrous degree in pod corn.

Altogether I fail to see how this variation can be

other than an exception to the "Law of Loss".

Thinking this variation may not have come to your attention I take the liberty of writing, and if not too much trouble would greatly appreciate your opinion on this subject.

Under separate cover I am sending a few pamphlets that give some idea of the nature of pod corn.

Very respectfully yours,

E. W. Collins

Aug 24.19

Zea mays

I am inclined to think that Poulson's is a better evidence of real reversion to an earlier loss of a particular condition than anything I have hitherto met with. But I do not think so far as I understand it, that it can be claimed as exactly an exception to the "Law of Loss". If the glumes in the pistillate flower normally ^{are} ~~are~~ ⁱⁿ a reduced form, then no reason why a variation should not occur in which these structures are hypertrophied. But if the glumes in the normal pistillate flower were reduced to the point of being entirely ~~absent~~, absent, then a variation in which they reappeared ^{might} ~~could~~ be regarded as offering an exception to the Law of Loss. Yet, on the other hand, - even in ^{the latter} ~~the latter~~ we may conceive that the species, since it drops produces glumes in its staminate flowers, etc. (know how to produce them (starting from a female of species!) is true even if they had been lost as far as the species can be said to have "lost" them - hence whether or not it would be a real exception to the L. of L.

A252

TELEPHONE
VICTORIA 2005.

91

74, EATON SQUARE.
S. W.

6 Dec 1919

Dear Mr Arber,

Many thanks for sending me
your papers. I have read them
with much interest & am so
grateful to you for sending them
to me.

I am back at work now, reelling
in the joy of getting back to
Equisetum in the mornings &
to leisure & family life in the

afternoons.

I spent a delightful summer in the country, working up the slides I had cut before the war & just after it began. The peacefulness of the country & the continuity of the work were so pleasant.

My cones of *Equisetum giganteum* interested & puzzled me a good deal, because the annulus was sporangiferous & partially vascularized. I came, regretfully I confess, to the conclusion that this peculiarity

was secondary. It would have been very pleasant to have found an annulus preserving a condition prior to the evolution of a difference between leaves & sporangiophores!

But I feel convinced that I have not found that. Now, however, I am wondering how your theory of the law of loss in evolution would be applied to the vascular strands running to the point of attachment of the annular sporangia. I think it might apply for the 'new' vascular strands are more like sporangiophore-traces than leaf-traces. And yet they are, in a general sense, repetitions of the lost leaf-traces.

It is rather unfair to bother

You then about a paper which
only come out, rather suitably,
on April 1st, in the Annuals
But, of course, I don't look for
a reply!

Hoping your little daughter
is well

Yours very sincerely

Isabel M. P. Brown

apt. Claradox Rd
Dec. 4/21

Dear Mr. Arber

I came on the following footnote in Hooker's
Introd. to the Flora of Tasmania, p. ix. "It is not
meant by this that any character of a species which
may be lost in its variety never reappears in the
descendants of the latter; for some occasionally
do so in great force; what is meant is, that the newly
acquired characters of the variety are never so
entirely obliterated that it has no longer a claim
to be considered a variety". I want to talk to
you one of these days about the law of hors,
but just now I am desperately busy with statistics,
as I want to get my book out shortly. I am
finding out various new results, one of which
will I think put the area completely into the
background as merely a special case of a
wider general law, which seems to apply equally
to animals. With kind regards

Yours very sincerely
Oliver C. Willis

Hooker's MA. 19.18
in library

AL 53

12

Wardell-Teratology

Vd I p 154

Reappearance of lost traits in *Chenanthus Cheri*,
Melthuba & *Arabis*. This he interprets as
reversion to what is the normal state in *Papryodon*,
Dipterygium, *Selenia*.

AL 54

Criticize for Professor
Sahni. ~~the~~ April 13. 1920
(Date) letter

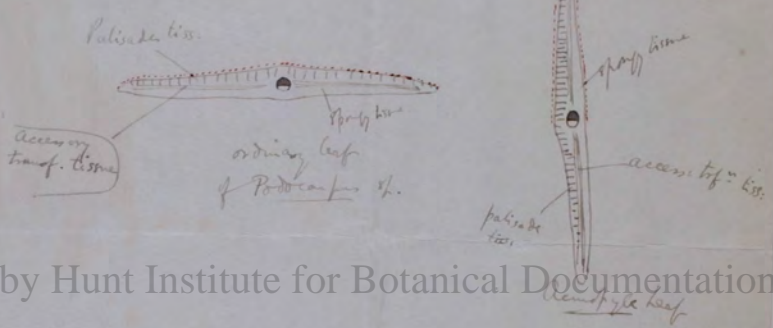
93

Railway Station
Benares Canal

Benares Hindu University.
DEPARTMENT OF BIOLOGY

Benares City

You very kindly asked me for any exceptions to the law of leaf
that may occur to me, so I venture to cite one although it may
be only an apparent exception, not a real one.



Digitized by Hunt Institute for Botanical Documentation

In the leaf of *Acrostyle* which is expanded in the "vertical plane" (morphologically) the accessory transpiration tissue has probably been formed by modification of the parenchyma in a xylem-phloem plane of the leaf (i.e. the cambium derived plane) while in *Podocarpus* for the parenchyma at the right & left sides of the midrib. Yet the two tissues are identical in structure, although the transf. tissue in *Acrostyle* has probably no genetic relation to that in *Podocarpus*, except an indirect one inasmuch as both are developed from leaf parenchyma. A similar argument might hold for the palisade tissue & spiny tissue. I give this example with some diffidence - I am not quite sure it will stand the test of close scrutiny.

With kind regards
Yours very sincerely
Sahni.

Mrs. Anand Mohan



AR546
Anatomical Department,

St. Thomas's Hospital,

Manor House
Cockspur Street, London, S.E.1.
Surrey

Dear Miss Arber

I enclose a short note

on meristic variation in the
nasal conchae.

I had long been puzzled by
this apparent reversal, but your
paper (*Amer. J. Sci.* 48, 2) (1919)
set the matter straight. I hope
we have interpreted our evidence
in a way that would meet with
your approval.

Incidentally we have come across
an instance of true reversal & hope
to publish it someday. The interorbital
septum has been lost as from reptiles
(Mammal-like series of S. African dog team)
to mammals, then regained in some
primates. If you could put me onto

any similar evidence of a true
reversion (not meristic in character)
I should be very grateful.

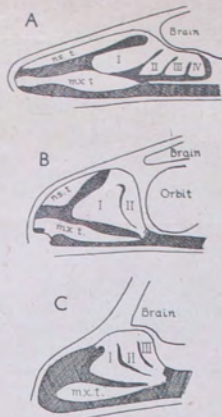
Yours sincerely

R. Whittier Haines

Meristic Variation and Reversibility of Evolution

PROJECTING into the nasal fossa of any generalized metatherian or eutherian mammal is a series of turbinals, namely, a naso-turbinal (Fig. A, *nst.t.*), a maxillo-turbinal (*max.t.*) and four ethmo-turbinals (I-IV), and there can be little doubt that such an arrangement characterized the remote ancestors of the Primates. But in all primitive Primates (from which we exclude the Lemnures), including such forms as *Tarsius* (Fig. B), *Hapale*, *Chrysothrix* (*Saimiri*), *Cebus* and *Logothrix*, there are but two ethmo-turbinals (I, II), for in these forms the orbital cavities have so enlarged as to obliterate by their approximation the posterior part of the ancestral nasal fossa. In the baboon, gibbon (Fig. C), chimpanzee and gorilla, and in man, the nasal fossae are again enlarged, partly at least as a result of the growth in width of the skull-base in support of an enlarged brain, and in all these forms three¹, and in man sometimes four² or even five³, ethmo-turbinals may be developed. The phylogenetic trend in Primates seems, therefore, to have been towards a reduction of the turbinal series in early forms and a secondary expansion thereof in certain of their descendants.

Dollo's 'law of irreversibility', even in its modern phrasing: "Evolution is reversible in that structures or functions once gained may be lost, but irreversible in that structures or functions once lost can never be regained"⁴—does not apply here, for ethmo-turbinal III has been lost in primitive Primates and has reappeared in certain recent forms. Arber⁵, quite independently of Dollo, formulated from botanical evidence a 'law of loss', the "general rule that a structure or organ once lost in the course of phylogeny can never be regained; if the organism subsequently has occasion to replace it, it cannot be reproduced, but must be reconstructed afresh in some different mode", and later collected such data as had been submitted from time to time as evidence contrary to Dollo's law. Such evidence included the re-acquisition of a lost toe in a laboratory race of cavy, the re-development of lateral digits in some horses, the occasional presence of a fifth stamen in anomalous *Iris* specimens. Arber pointed out that these were all cases of meristic variation and that Dollo's law did not apply to them.



The present instance of the nasal turbinals is also meristic in nature, but is derived from normal anatomy and not from teratology: it is bound up with the fundamentals of Primate evolution.

The ambiguity of Dollo's law depends upon the interpretation of the words 'structure' or 'organ'. Presumably if the entire Primate ethmo-turbinal series had been phylogenetically lost, no single turbinal could ever have been regained: but so long as even one member of the series persisted in primitive Primate forms, the redevelopment in descendant recent forms of the full turbinal complement remained a possibility. It seems advisable, therefore, to add to the law a rider to the effect that, in the case of structures constituting a series, the law applies to the series as a whole, and not to the individual members thereof.

A. J. E. CAVE.

Royal College of Surgeons Museum,
 37, WINDYBELL PLACE,
 St. Thomas's Hospital Medical School,
 London.

¹ Cave, A. J. E., and Haines, R. W., *J. Anat.*, **74**, 493 (1940).

² Schaeffer, J. P., *J. Morph.*, **21**, 613 (1910).

³ Paull, S., *Morph. Jk.*, **28**, 483 (1890).

⁴ Needham, J., *Biol. Rev.*, **13**, 225 (1938).

⁵ Arber, A., *Amer. J. Sci.*, **48**, 27 (1919).

Dear Dr. Wheeler Harris

Thank you very much for

sending me your most interesting paper - My eye with I have for a while been reading it and I had not seen it. I am delighted

to hear that you are interested in the same. I am delighted to hear that you are interested in the same. I am delighted to hear that you are interested in the same.

do not think I can offer you any of either. I am sure you will find it very interesting. I am sure you will find it very interesting.

It is a very recent discovery. I am sure you will find it very interesting. I am sure you will find it very interesting.

I am sure you will find it very interesting. I am sure you will find it very interesting.

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It is a very recent discovery. I am sure you will find it very interesting. I am sure you will find it very interesting.

Digitized by Hunt Institute for Botanical Documentation

VA IX, 1817 p 303

the branched inflorescences, are a variety of an ancient form. Plantains are found in the same region. I am sure you will find it very interesting.

first order, there is a capital / min varies
 interpretas, there is also no final consonant / open
 as to what ^{type} ~~type~~ ^{of things} ~~type~~ ^{is held}, on comparative grounds, to
 represent the primitive ~~of~~ type. I feel that
 animals in a small more helpful field. You
 compare ancient have a clear a family
 any w scheme of 6 rows lines ~~the~~ evolved by
~~the~~ ^{any} ^{of} ^{principle} ~~the~~ ^{more} ^{more} ^{you} ^{diff} ^{due} ^{to} ^{detail}, ~~the~~
~~can~~ ~~any~~ ~~way~~ ~~as~~ ~~less~~ ~~difficult~~ ~~to~~ ~~see~~ ~~so~~ ~~you~~ ~~can~~ ~~see~~
~~the~~ ~~evolution~~ ~~of~~ ~~the~~ ~~principles~~ ~~on~~ ~~the~~ ~~the~~
 evolution as proceed.
 I am sorry to see I no longer have any copies
 of Alston pp - I order to be one.

23, Abercrombie Street
Battersea
London S.W. 11.

November 20 1950.

Dear Mrs Annet,

You may recall that during the very pleasant hour I spent with you and your daughter the other evening you mentioned Nehemiah Grew, and Christchurch Newgate St. I have collected a little information which may interest you. The enclosed photo, shows the church as it is today. You might like to keep it, as she can just see the monument.

I turned up the notes on the church in the volume of The Royal Commission works on London which gives a brief note.

"North Wall Mary (Huetson) wife of Nehemiah Grew M.O. 1685. Marble cartouche with scrolls, bust of woman and shield of arms."

Yesterday I visited the church, and found that it was the only wall memorial not completely destroyed. The bust has gone, and only the

cartouche is left. The inscription is by no means clear, but I found a ladder which proved very useful. As far as I can make out the inscription in Latin, is as on the enclosed sheet. (I have a duplicate) But I am no Latin scholar and several of the words I am very dubious about. But I hope it will do for the moment. I have also written to the incumbent, for any information he can give, also I have an idea there is a detailed history of the church which should give the inscription. I am trying to trace a copy, and if possible I am hoping to get a close-up photograph for you.

I will let you know as soon as any further information comes to hand.

yours sincerely

W. Norman Lawfield

In Commetario

Un prop jacet

MARIA

Buli Hughson MGRGD: 6c Grifellae

Filia St. Harredum altem

Niemiae Grew Medicinae Doctoris

Vror defiderarifima

Quipper Mulict, Corporis Ingenii, morning

Venus Fate pulcherrima

Cemmam, quam Natura genuit.

Erpolivir Religio:

Donec, Radiis indies auctis, in Stellam,

Confortium alumere tur,

Nempo 9^o Apr die, A.D.

1685

Alfatis snae 27.



Christchurch Newgate St London. 1950

The monument to Mary Grew can just be seen
in the centre of the photograph, at the right hand
side of the third window from the left. (a light patch
in the corner)

Leam End
West Bloathly
East Grimstead
Surrey

April 8. 1942.

Dear Mrs. Baker.

It is interesting - I had
some duplicate plates sent me,
in 1927, & we - I am sure don't
own any them. I am sure Mrs.
Clarke would lend you his
personal copy of Walpole XV
if you told her the circum-
stances & ^{said that} I
Next time you are in Chichester
Road, my
to Cousin Jane

Alvin Edith: he was my
mother's first cousin, I am
devoted to them. I do like
to think you know them.
Just off to San Jo. (Punther)
saying excuse great haste.
Yours sincerely
t. A. S. daule.

at "Little Woodland"
Wormley Godalming
Surrey

No. 8

Dr Agnes Fikes

Dear Madame

I regret that your letter
to me has only just come
into my hands, as my letters
are not being revisited from
47 Oakley Street, Warrington
thru this I am able to call for
them. I am much interested
in hearing that you propose to

write a life of Nikolaich Presn, as
years ago I thought of doing so
myself - the last war intervened
and since then the opportunity has
never occurred. I was told
many years ago by a Mr. Jarvis,
who was ^{an} Assistant-Secretary of
the Royal Society, that there
were a great many papers
relating to him in those
Archives, and I was going to
do my work on them if
they could be made available.

information. You may already have
consulted ~~these~~, my husband
has a Miss Julia Greig 1891-
some papers relating to
Archieval Greig, but they
seemed to be largely conjectural
I think there were allusions
to or letters from a Mr. ³ Elizabeth
Greig among them?

I hope to be at home for
a few hours on Wednesday
Nov. 25th and will then
bring along - for the day

These letters I shall be
delighted to send you any
thing likely to be useful to
you that emerges from them.

My husband was always
told as a child that he
was descended from
O. Schenck's feet?

~~Yours are~~
~~for faithful,~~

Mr. Z. I. ^{4th}

Dear Mr. Fisher

I have been through the papers left by Mrs. Julia Fisher & the results are so very meagre that I am afraid you will not think them worth sending. You will notice that any in my own handwriting (too fragmentary to send you) are all original from copies & that their accuracy is not vouchered for. I send also a document signed "E. G." presumably the "E. G. G."

Green whom you mention. I know
nothing of her & as far as I know my
husband has no relations living on his
father's side. Mr Potts might
have been useful, but his letter
is dated 1882. I wish I could
have done more.

Now I must thank you for
your great kindness in sending
me the delightful volume of
"Makers of British Botany" - it has
just arrived & I am taking
forward with much interest
& reading it. The Editor Frank
Oliver is a very old friend of mine

and I was deeply attached to this
 charming wife Billie. We used
 to meet Dr. John Field Scott at
 their charming home in The Vale
 Chelsea - long since demolished.
 Dr. Brown, was with us in Australia
 in 1914 I sat next him at a
 Peasant Government-house luncheon
 in Melbourne. - The Sewards cell
 that I think at the same time
 was one more link - a modern
 one - at home done. I believe
 the two ^{of} Billie's staying in some ^{of} the
~~reservations here at~~
 hotel is ^{an} cousin -
 Indeed I wish I could have been more
 nos to Dr. - I shall never do anything
 myself. Between ^{myself} ^{and} ^{the} ^{crew}

1 Frere Street,
Battersea,
London S.W.11.

April 21 1951.

Dear Mrs. Arber,

For some long time now I have been intending to write to you, and return your very interesting paper that you were kind enough to lend me.

I think I told you that I wrote to Mr Mine-Haycock about Nehemiah Grew, but he was unable to tell me any more than he had told you, at that time I did not know that you had already written to him.

It is unfortunate that I have not been able to glean any more information concerning the monument. Things being much as they were. As far as I know there has been no move to yet to collect the monuments from the various city churches. At Christ Church there is a small brick building, such as you describe in your letter, and it is possible that the bust is still in there, I cannot find who holds the key. It is well and truly locked. I will however keep a look out for information, and will let you know of anything I come across

Work keeps me very busy these days, but of course it is most interesting, and that makes all the difference. I get out quite a lot as well, visiting nurseries, shows, and gardens. I also do quite an amount of editing and writing.

I spent Easter in Cambridge, and went through some of my Father's papers, among one bundle I was very delighted to find the obituary notice of Dr. Arber together with a photograph, reprinted from the Geological Magazine. I of course never knew Dr. Arber, but frequently heard my Father talk about him, and his work.

(2)

I am enclosing with this letter a small catalogue of an exhibition of flower prints being held this month in Battersea Library, it is much the same as that held last year, but on a smaller scale. I intend going this afternoon.

Finally I must ask you to be kind enough to overlook my typing errors. I now have my Father's machine and am teaching myself.

yours very sincerely,

W. Norman Lawfield

A260

1 Frere Street,
Battersea,
London S.W.11.

May 11 1951.

Dear Mrs. Arber,

I am sorry to trouble you but it has just occurred to me that you may not have seen all the material in the British Museum, with reference to Nehemiah Grew. I think it may be of interest to you. In case you have not I am enclosing a list that I have taken from the Sloane MSS. catalogue. I have the complete Sloane catalogue. If you would like, I should be very pleased to make copies of any MSS. you require. At present all my spare time is devoted to work on Hans Sloane, and I find the Museum authorities most helpful.

A week ago I was at the Natural History Museum, and had occasion to buy some cards, and I am sending two sets to you, they may interest you, if not you can always use them for correspondence.

As I have said be sure and let me know if you would like copies of any items on the enclosed list.

Yours very sincerely,

W. Norman Lawfield

BRITISH MUSEUM SLOANE MSS.

- Grew (Alexander). Letter to his brother, n.d. Copy. 1926, f. 190 b.
- Grew (Mary). 1st wife of Dr. N. Grew. Letters to, from her husband, 1678, and n.d. 4062 f. 192; 4066, f. 359.
- Grew (Nehemiah), M.D.; Secretary to the Royal Society. Miscellaneous papers 17th-18th centt. 1941; 1942; 1950.
- Collections relative to the history of Egypt, 17th cent. 1963.
- Observations medicae, 17th cent. 1949.
- Catalogus plantarum, 17th cent. 2145.
- Latin translation of his "Anatomy and vegetation of Roots", 17th cent. Hologr. 1926, ff. 144-179, 191-201.
- Greek and Latin synonyms, 1656, 1945.
- Musaei Regalis Societatis in Collegio Greshamensi descriptio, 17th cent. 1927.
- Catalogue of the chiefest rarities in the Gresham College Museum, (16)81. 2346, f. 21 b.
- Animadversions of M. Lister on "the Anatomy of Vegetables", with Dr. Grew's answers, 1673. 1929, ff. 1-11.
- Letter to L. Le Vasseur on the translation of the treatise on plants into French, 1674. Lat. 1926, f. 198.
- Letters to Foreign Correspondents on the election of a new President of the Royal Society, 1677. Lat. 1942, ff. 1-3.
- Letters to his 1st wife, Mary, 1678. 4062, f. 192; 4066, f. 359.
- Letters to Sir H. Sloane, 1692-1711, and n.d. 4036, ff. 113, 309; 4037, ff. 77, 116; 4039, ff. 299, 328; 4042, ff. 30, 275; 4059, ff. 83, 84.

Nehemiah Grew M.D. (cont).

Letter to a French savant, n.d. Draft.
4066, f. 357.

Letter to his mistress, n.d. 4066, f. 358.

Letter to F. Nazari, n.d. 4066, f. 360.

Letter to, from W. Mathews, 168 2/3 4076, f. 133.

Letter to, from S. Lee, 1690. 4062, f. 235.

Letter to, from his sister, M. Willes, 1697.
4062, f. 275.

Letter to, from R. Thoresby, n.d. 4025, f. 286.

Letter to, from S. Sewall, n.d. 4067, f. 140.

Certificate of his marriage, 1678. 1941, f. 17

The Mausl. A 261

Franklinham

March 18th 1921.

Dear Dr. Alder,

Thank you for your letter.

I am interested in your research work, although unable to give you very much information about Henry Sampson other than that Henry Sampson M.A. was Rector of Franklinham 1650-1660 founding the Congregation which now meets in the Old Meeting House

He became a physician died July 1700

I have a list of names and dates of Ministers of the Old Meeting House since the time of Henry Sampson. If they are of any value to you I should be pleased to let you have them

Yours Sincerely

W. Broadbent

Suggestion anyway - & get
it preserved, in case of
my departure. I carry on
at S. Abbott's Aldeneyate on
Sundays at 11 - by myself &
can alternate for my old
stock, have enough to come.

~~Depart last night. 7.15 PM~~
April 30 Also Friday. 2.15 PM W
boat so I had got for [unclear] &
address - then found [unclear] & [unclear]
I put up in error. I write the
card Feb 19
(Confounding)



Agnes Arber
52 Huntingdon Rd
Cambridge

Ac 62

Hill House, Bedford ^{Feb 20} ~~19~~. 20. Thank you very
warmly for the Tercentenary of N.Y. paper, which
came today. I see on p 219 a very kind reference!
It makes me sad that the queer changing of
padlock prevented your seeing both tablet & Bust.
They are still there, the Bust in a little room
which had not burned. I have kept your letter
with suggestion as to ultimate resting place for
the Bust. The fall of collecting relics of Wren
Churches to be put into a Room. I'm going to
tell my husband Col: Ursther of yours.