(Bateson)

F. 1

2 Dec. 1896

ST JOHN'S COLLEGE,
CAMBRIDGE.

(W. Bateson)

Dear Mr. Galton,

I am extremely sorry that I cannot attend the meeting on Friday, though I have great sympathy with the objects.

On Fridays I have a meeting at 1.15 which last 2 hours more or less, and on next Friday I am especially bound to be present at it, so I cannot hope to be in London on that day.

I am yours truly

W. Bateson.

3 Dec. 1896 (Bateson) Norwich House ~~St. John's College~~ f. 25

CAMBRIDGE.

Dear Mr. Galton,

After writing to you the other day I met Dr. Foster. We had some talk about the experimental "Farm", and as I cannot be present at the meeting tomorrow he rather urged that I should write to you on the subject, though I do not know that I have anything of value to say.

(W. Bateson)

I understand that there is to be a discussion of the desirability of separating investigations on animals from the botanical side. Surely this should only be done for some very exceptional reason. The great difficulty in breeding experiments is to get continuous observation. That I suppose is the point of getting an institution which shall both have a continuous life, and not go bodily away in vacation (which a single observer must do, if only to get new material in the field). As your circular says, a great deal can be done with simple means if <u>once the continuity of the observations is guaranteed</u>. But to try to do this in duplicate would surely be a mistake. It would more than double the difficulty.

There is besides a practical point of great importance. Among wild animals, insects are especially tolerant of captivity and in many other ways are exceptionally suitable for breeding expts. They can be easily preserved in vast quantities without expense, and their hard parts (nervures, gonapophyses, &c), which show many specific characters, last indefinitely without any special preparation at all. Now in experiments in breeding insects greenhouse accommodation is very desirable – for breeding butterflies it is a necessity. Wherever the station is started there should if possible be some greenhouse space which could be used for both plants and insects. I have no doubt besides that for experimenting on the hybrid finches – a very promising field, as so much is already known – greenhouses would be a great help. One range of houses would serve for all purposes if plants & animals were treated together.

Of course if it is thought best to begin with expts. for the most part relating to domesticated animals

Dec 3/96 F.3r

~~St John's College,~~
Cambridge.

~~alone~~ only, a form in the strict sense might do very well, but I should have thought it better to start with a much wider scope from the beginning.

— You will not tomorrow, I suppose, get to the point of discussing subjects for investigation; but when the time comes I hope you will consider as a subject, the effect of close-breeding on the transmission of parental characters. It is one which could be studied both by expt. and by the co-operative system sketched in your circular. In many ways it is a question of quite exceptional importance, with bearings on the physiology of heredity and on the mode of establishment of new races. It is besides the only way, so far as I see, in which there is any real suggestion of a possibility of controlling the course of inheritance by a definite interference.

— I cannot say how glad I am that something of this kind is to be set going at last.

f. 3v

I am yours truly,

W. Bateson.

f. 4r

I have been very much
occupied the last few days
& have not had an opportun-
ity of answering your letter
I hope to do so shortly.

W. Bateson

14 Dec. 1896

LONDON. S.W.
5.15.AM
DE 15
96
15

F. 4v

POST CARD

THE ADDRESS ONLY TO BE WRITTEN ON THIS SIDE.

HALFPENNY

CAMBRIDGE
9.30 PM
DE 14
96

158

4

Bateson

F. Galton, Esq.

42 Rutland Gate

London

S. W

24 Dec. 1896 (Bateson) Norwich House
~~St John's College~~
CAMBRIDGE.

Bateson Dec 24/96

See Darwin: Domestication Chap XVII (Vol 2 p. 1??) p. 114)

Dear Mr. Galton,

The question I should like to see investigated is whether an animal that is closely – i.e. incestuously – bred has a greater power of transmitting its own peculiarities to its offspring than an animal which is not closely bred. There are, I think, some indications that this may be so, and though there is no real evidence, the possibility would be worth investigating. There is to begin with a very general impression among stock breeders that a "thorough-bred" animal will generally transmit its characters when mated with an animal that is not thorough-bred – that, other things equal, it will be what is often called prepotent (though I believe that is not the proper use of the word). Most breeders will say that the prepotency of the thorough bred is simple enough. Only one kind of strain has gone to the making of the thorough bred – In its back breeding it is unmixed. But in the making of the mixed-

-bred parent many elements have been blended.
When the two breed together therefore, one whole half
is pure bred and the other half is miscellaneous,
so the chances are that the pure bred characters will
show up most in the offspring. This is the view that
for example Sir E. Millais maintains (especially
from his experience of Bassets), and developed in a
pamphlet called "Rational Breeding" that you may
have seen. It is the view most breeders hold.
It practically regards the offspring as a mechanical
mixture of the parental characters.
There are many cases of course in which this view
doesn't agree with the facts at all, and it is possible,
I think, that it misses an essential point.
An animal may be thoroughbred – in the sense
of being pure bred – i.e. from closely similar
parents for generations – without being inbred
at all. Would such a thoroughbred be prepot-
ent? That is the question. ? tests here in Racers & greyhounds
You perhaps know the alleged facts about Canary
mules with Goldfinch. The pair must of course always
be Goldfinch ♂ with Canary ♀ [because the Goldfinch ♀ won't breed in captivity].
It is said that

f. 5v

2. (Bateson) Dec 24/96 f. 6r

ST JOHN'S COLLEGE,
CAMBRIDGE.

light mules – that is, Canary-like mules – are most easily got by breeding from an in-bred hen. The lightest mule* is the best in mule-classes – likest the yellow Canary is the best. (The Goldfinch bill is always inherited to a considerable extent – so the birds can always be recognized as mules.) The great number of Goldfinch mules are Dark, almost exactly like inferior Goldfinches. (Of Bullfinch or Linnet mules none I believe are ever light – but there are no special classes for them). Light mules are great rarities. But it is said that light mules can be got by breeding from "sib" bred – viz – in-bred – hens, the yellow canary character then becoming prepotent.

Now a pure yellow Canary hen would in any case be thoroughbred in the sense of being the progeny of pure yellow Canaries. But it is said that since sib-bred hens came in, the number of light mules has greatly increased.

f. 6v

In most animals
A thoroughbred is as a rule inbred, and the prepotency of thoroughbreds may be due to this rather than to its purity.

In fixing a new var. almost always the first step is to breed in and in, and I suspect that the rapidity of this result may be increased by the inbreeding apart from the mere selection. No one knows how much inbreeding takes place in nature, but generally speaking I incline to believe there is much more inbreeding than is commonly supposed. If it could be shewn that inbreeding had a specific effect in increasing prepotency, the process of establishment of a new race becomes so much the easier to imagine.

By analogy prepotency is rather likely to go with inbreeding, for cessation of variation generally goes with asexual reproduction, & inbreeding is a step in the direction of asexual reproduction, in a sense.

As a subject for expt. inbreeding is attractive also on account of the great practical importance of the question of. For everyone wants to breed as close as he can. Almost nothing is known of the

3) (Bateson) Dec 24/96

F.7

? see Darwin

ST JOHN'S COLLEGE,
CAMBRIDGE.

consequences of inbreeding, beyond the vague knowledge that in certain cases excessive inbreeding leads to "constitutional weakness" (In canaries the number of tail-quills is reduced) and that different animals differ enormously in the amount of inbreeding they can tolerate.

Yours truly

W. Bateson.

(Bateson)

f. 85

Norwich House

~~ST. JOHN'S COLLEGE,~~

CAMBRIDGE.

8 Feb. 1897

Dear Mr. Galton,

Herewith I return the papers recd. from you and from Weldon. They have been shewn to F. Darwin, Heape and Sedgwick. The latter says that he cannot get to the meetings of Committee and would rather not join.

f. 8v

I am rather sorry that the proposal to start some kind of station now falls through; but on the whole I incline to the view that the way is not clear enough to start yet.

There is also a great deal to be said for the proposal that each man is to run his own experiments in his own place. The function of the Committee would then I suppose be to organize the continuation of the expts. if it thought fit in the future.

// I note your correction on my letter — as to Ancona & blk. winged Peacock — Of course I ought to have expressed myself differently — but I had my mind on the other class of cases. The pre-potency of sports at their first appearance plainly has nothing to do with in-breeding, but I fancy any rationale of prepotency has to reckon with

f. 9r

f. 9v

both.

The chief point that occurred to Darwin & to me in criticism of the suggested expts. is that a large number of them are not of a continuous character at all and do not need observations of successive generations. Any of these could be carried out in any good laboratory.

Yrs

Yours truly

W. Bateson.

Several of the remainder might be combined. W.B.

6 Mar. 1897

NORWICH HOUSE,
CAMBRIDGE.

Dear Mr. Galton,

I enclose proofs of the 2 circulars of which I described the purpose in my last letter.

If you do not approve them even in a general way I think it would be well for us to see if we could not in consultation draw up something that would do.

I could come up on Mar. 9

f. 10v

or preferably Mar. 10.

But if you think that with some amendment they will do, will you amend them & return to printer for distribution to Committee.

Until after Sat. next I shall be rather busy but I could attend a meeting on any day in the following week

Yours truly
W. Bateson.

f. 11

Heape & F. Darwin have approved the papers

W.B.

f. 12r

14 Mar. 1897

NORWICH HOUSE,
CAMBRIDGE.

Dear Mr. Galton,

Herewith I return your draft.

I have talked it over with F. Darwin, but Heape is away I believe.

We think that for the present at least such communications as that outlined would be best made by a written letter, but that if it is

f. 12v

found necessary to write many such letters, some circular would be then made more effectually with the experience obtained.

– In the longer printed circular I do not quite like the phrase (over the page) "that they desire to see performed," on the ground that it tends towards committing us to collective ~~unqualified~~ approval of many f 13

proposed schemes of work -
I quite agree with you that
their collective approval
is likely, for some time at least,
to be very exceptional

Yours truly
W. Bateson.

I expect to be abroad for the
first 3 weeks of April, as
egeria (which I am working at)
will be emerging then.
W.B.

f. 145

27 Oct. 1897

NORWICH HOUSE,

CAMBRIDGE.

Dear Mr. Galton,

Thank you for letter received today.

I will write shortly about our plans. I should rather like to see Weldon before doing so, and as I expect to be in London on

f. 14v

Wednesday next (Nov. 3)
perhaps I may wait till
then, unless there is any
reason for haste.

Yours truly
W. Bateson.

G. Bourne

f. 1r

36, BANBURY ROAD,
OXFORD.

Dec 1st 96

Dear Mr Galton

So far as approval of your scheme goes, I can give it my most cordial approval, so far as that is worth anything. It has seemed to me that in all work of the kind proposed the assistance of amateurs is too little to be relied upon, both because their efforts are spasmodic

as a rule, & because they do
not sufficiently appreciate the
importance of the utmost accuracy
attainable.
But when you ask me to undertake
the honour of becoming a
member of the Executive Committee,
should such a Committee be
established, I hardly feel that
I am in a position to accept.
A teacher at Oxford is so much
tied to the place during Terms
that it is nearly impossible to

f. 25

get away without neglecting duties for which he is responsible, & one must necessarily sacrifice many interests in wider matters for the sake of those in which one is professionally concerned. It is not right to become a member of a Committee unless one is ready to be a working member, & I could not promise to be a regular attendant at Committee meetings.

So I must ask you to hold me excused, thanking you the while for the compliment you have paid me in asking me.

I fear that I have done very little

f. 2v

for you of late about terriers. I have some measurements, but not a complete set, and I am suffering from the forgetfulness of those to whom I sent instructions. They have not returned complete measurements. I have been so busy lately that I have not found the time to urge them continually & so the matter has slid somewhat. But I will try to get what is wanted as soon as Term is over

Yours very truly

Gilbert C. Bourne

S.H. Burbury

f. 1r

Dec 4/96

My dear Galton

I will come to your meeting if I can but I am in an attack of lumbago which gets better & sometimes worse and one of the injunctions which I obey or disobey is not to be out late.

I have long been of

f. 1v

opinion that much light might be thrown on evolution by systematic experiments with animals whose generations succeed one another at short intervals - or of course with plants.

Practically I should think you would find

f. 2

a landowner liberal
enough to let you the
land rent free - but if
the thing succeeds as I
think it would you
would soon find it
growing beyond the
modest 20 acres

Ever yours

S. H. Burbury

f. 1

It is particularly requested that all communications may be addressed to "THE SECRETARY."

TELEPHONE Nº 3675.
TELEGRAPHIC ADDRESS.
"PRACTICE, LONDON."

PRACTICE WITH SCIENCE

Royal Agricultural Society of England,
13, Hanover Square,
London, W. January 18th, 1897.

ENCLOSURES

Clarke

My dear Sir,

I am in receipt of your letter, with enclosure, of the 16th instant.

I shall be in Town all this week, and if you could favour me with a call here any morning about 11.30, I would endeavour to be at liberty to discuss the subject about which you write.

If 11.30 a.m. is not convenient, kindly suggest some other time agreeable to yourself, and I will endeavour to make it fit in with my other engagements, if you can give me a day's notice of your intention to call.

Yours very faithfully,

Ernest Clarke

Francis Galton, Esq., F.R.S.,
42 Rutland Gate,
S. W.

Collier

with 2 enclosures which he says [Spencer & A Sedgwick] had (?) I may keep. They are now under their respective name

CHURCHFIELD,
EDGBASTON,
BIRMINGHAM.

Jan 14th
1897.

Dear Mr Galton:

The enclosed will explain themselves. If you think further of the matter you will of course write me, but pray don't trouble unless you think I could be of any good

Yours J. H. C.

Adam Sedgwick
to Collins
see Collins Jan 14/97

f. 3r

Whitefield
Gt. Shelford
Cambs.

12 Jan 1897

Dear Sir

I am much obliged to you for your letter of 10 Jany.

There is at present a Committee of the Royal Society - an informal Committee I believe - wh: is considering a scheme for establishing

f. 3v

an experimental biological station. Mr. Francis Galton is chairman of the Committee, & he would I am sure be glad to hear from you on the subject.

I am not a member of the committee, but I shall doubtless hear if there is any practical outcome of its deliberations

f. 4

& shall be very glad
to communicate with
you again should
you wish it.

Yrs truly
Adam Sedgwick

F. Howard Collins Esqre

H. Spencer to Collins

see Collins Jan 14/97

2 Lewes Crescent
Brighton
Jany 5, 1897

f. 5

~~64, Avenue Road,
Regents Park, N.W.~~

Dear Collins

Mr Adam Sedgwick of Cambridge recently named to me the fact that something like a movement is afoot for establishing an experimental farm or something of the kind, on which this question of the inheritance of acquired characters – or rather functionally-produced modifications, as I prefer to call them – should be tested during a long period.

I expressed my sympathy & promised that if the thing was floated I would make a donation of £10 and an annual subscription of £5 & would make a bequest at my death.

Years ago you were

f. 6

anxious to start some such movement. You may as well communicate with Mr Sedgwick. His address is Great Shelford, near Cambridge

Truly yours

Herbert Spencer

F. H. Collins

f. 7r

CHURCHFIELD,
EDGBASTON,
BIRMINGHAM.

Jan 17 - 05/97

Dear Mr Galton:

It is a long time after to repay an unknown debt, but I discovered only a day or so ago that "registered" letters posted in the ordinary way are charged -/8 for delivery. Some years ago I sent you one on a Sunday when it could not be registered thinking you would receive it without anything to pay.

I have tardily found my mistake & therefore send stamps.

And now to your letter, I should be ~~delighted~~ to assist

f. 7v

with the "farm", but you have really fired such a galaxy of talent at me in your letter that nothing my poor capacities could do would be of any use against so much properly distinguished brain power; & secondly the last two months have more than consumed the amount of which I consider to devote to "hobbies" to give them the lightest title I can. Having cleared the air let us now pass on to your queries.

My letter to Nature suggesting a "farm" appeared on April 17th 1890 (p. 559.) & stated that, during Weismann's crucial experiment of wild ducks, the Zoological Gardens appeared to be a good place

f. 8r

so far as locality went. — The letter is not worth looking up altho about a column in length.

You have of course heard of J. T. Cunningham's attempt a year or two ago to start a similar thing — it died flat probably from the fact that it was known he was to be appointed curator; at least that was what he evidently "played" for.

As to the printed slip you sent containing names, they are all very good, but are they all the very good? I mean that were I taking the subject up the first man to write to would be Weissman himself — his evidence would be more weighty to my

mind, or at least equally, than
any name you already have.
Then why not ask de Varigny.
of Paris. & Haeckel of Jena,
they would be sure to give us some
new light, & that it seems
to me is what is wanted. Again
Gegenbaur, Dohrn (?) of Naples,
Claus of Vienna, & then some
botanists ought to be asked.
Strasburger, etc, but Sir J. Hooker
knows all these. Personally
I certainly have a profound
conviction that the inductions
must be made from a sufficient
area of data – get the thing
<u>thoroughly</u> threshed out before
beginning work <u>at all</u>.

CHURCHFIELD,
EDGBASTON,
BIRMINGHAM.

2.

As to Down I do not know what to say. First of all, are you right in shutting out half the world from the station? in other words ought it not to be on the sea shore, or have a large lake near it. so that aquatic experiments could be carried on pari passu with the land ones?

Now supposing this settled. is there not a much simpler way of getting to work than buying at great cost, which will

f. 9v

not easily be paid for, a separate Establishment?

If 18, or say 20, acres only are needed surely to get this would not be half so difficult near some important place with Laboratories already existing, & curators & research going on.

Could not land be obtained contiguous to Kew, Zoological Gardens - is all their land used - Then again there are all the agricultural colleges which would be only too glad, probably, to let on a 99 years lease a small outlying plot, & the

f. 10r

buildings already there would serve for all that was wanted in the way of workshops. Could not some arrangement be made with Lawes & Gilbert – surely they would agree with the value of the "farm" for heredity.

As to the experiments to be carried out that is altogether too big a subject to enter upon, busy as I am with the Spencer Portrait affair, but have you read in this connexion the last Vol: III – of Darwin's Life & Letters, the last 30 pp. – or so seems to me suggestive.

By all means keep my two letters – I do not want them.

f. 10v

and now may I give a suggestion which has been very useful to me in dealing with some such a correspondence as yours will soon be?

Start sheets of paper with various headings, such as "For Down", "Against Down", "General Locality", "Animal experiments" "Veg. ditto" & so on, then as each letter comes to hand copy in the exact words, what each one has to say under the appropriate heading. or you can marginally note it, & let a copier do the rest.

It is simply invaluable for straightening things up for a Com^ee & was I found invaluable for the mind of — Yours ever

F. Howard Collins.

Collins

f. 11

CHURCHFIELD,
EDGBASTON,
BIRMINGHAM.

Jan 19th
1897.

Dear Mr Galton:

a letter late last night from Mr Spencer makes me think I ought to tell you that I knew nothing whatever absolutely of his views as to the "Form"; when writing my letter to you about it on Sunday.

f. 12r

3

hand side of a 4to. sheet,
leaving plenty of space
for answers to be written
opposite them. Also a place
for signature, & send them
out more or less broadcast
to all likely to be interested.

In my own case — a yachting
matter of great difficulty — the
result was splendid, in giving us
a magnificent basis, on which
we built most successfully &
with little trouble.

Yours F. H. C.

From what I gather, his views & mine are similar in some respects, & hence you ought for the sake of weighing evidence, to know they are quite independent.

May I suggest what has proved most valuable to me in a similar kind of thing? That at your next Comee you should draft out say 10 or 20 leading questions, & have them printed on the left

Collins

f. 13r

CHURCHFIELD,
EDGBASTON,
BIRMINGHAM.

June 27th
1897.

Private

Dear Mr Galton:

Will you excuse my natural, not acquired, stick-to-it-iveness — I can't help its existence, so that you will let me enforce my views with an illustration which knocks me down every time I walk into this Town, & think of the "Farm".

Mason College began with an immense pile of

f. 13v

of architecture, & is now so crippled by all the money being spent on it, that not only are the Professors who are there not well paid, but some who they want to make chairs for, are absent <u>in</u> <u>toto</u>. And the instruments for use in class teaching are simply a per centage of what ought to be there to enable teaching to be carried on at all in accordance with modern ideas. The number of "whips" which go round for this purpose, show it.

now look at Owens College, Man=
commenced in two small rooms
in a little house, in a back street,
& compare it with mature
college.

why not begin gradually,
begin as all successful
things seem to evolve, with
a germ? Give all the money
practically to the brains, & let
the rest look out for itself.
What were Darwin's own Epoch
making experiments, but brains
& make shifts? Mind will
subordinate matter, but get a
pile of matter, with an indifferent
mind inside it, & what results?

f. 14r

f. 14v

I am not writing at random, but you won't get as much to further building, as if you only ask for money to aid brains. I cannot say what I should like to do such an old & valued friend, but you may perhaps read between the lines!!!

Personally I should prefer to ask from the eminent men as to what they think, & then see how finance may fit in.

As I said at the beginning forgive me, but believe me whether or no

Yours ever sincerely

J Howard Collins.

Collins

f. 15

CHURCHFIELD,
EDGBASTON,
BIRMINGHAM.

Jan 29 05
9-30 am.

Dear Mr Galton:

I am glad indeed to learn that our views are not really so dissimilar after all.

If you can spare me the time, I had better think over your letter & reply in __about a week__ – it wants some thought!

On Monday I go to Shop with

3

f. 16r

losing pounds in weight—
flopping up "donators".

In haste for post

Yours ever
K.P.

Spencer at Brighton for a week, & shall be passing thro' London on Saturday to Monday, so that if you wanted to say anything to me a post card at the Savile Club would enable me to see you there or at the Athenaeum, on Saturday afternoon, or Sunday, but let me know time as I may be out should you call.

Do please ask your friends to subscribe to the Spencer Portrait-scheme, as Poor Hooker & I must be losing

Collins

f. 17r

2 Lewes Crescent
Brighton.

Feb. 6 05

Next address
Edgbaston

Dear Mr Galton:

The crossing of our letters was another curious coincidence.

As to the questions in your circular "Subjects for Inquiry" as you will have gathered in the one, & only one, in my opinion (I won't write anyone else's) to be considered for some time, as it will take months probably to decide which of them all is the fittest to survive to the stage of

f. 17v

experimentation. Unless you do farm out largely, you will start with such a heterogeneous lot of elements that you will be smothered in their remains!

Start upon one or at the most two, as the best to try, & then get out costs etc of carrying them out, & then, & not till then, begin to collect money for these one or two specified objects of experiment.

A striking criticism upon your Comm^e. is to be made in the fact that acquired inherit^ce & non-acq. hered. are not equally f 18

represented. And another is how about so large a number of men agreeing about any one single thing? Unless you agree before hand what experiments are crucial, you will never agree after, as the side opposing the result will find faults.

As I am away for holiday please excuse more from

Yours very truly

F. Howard Collins.

CHURCHFIELD,
EDGBASTON,
BIRMINGHAM.

Mar 21st
1897.

Evol. Comm. R.S.

Dear Sir:

I am obliged by the receipt of your circular, but regret that my time is too fully occupied to enable me to undertake any investigations, or to plan any in detail for others.

I should however be pleased to assist

f. 20 v

your Committee, if it lay in my power, with advice upon any plan of experiments which they might care to submit to me.

Yours sincerely

F. Howard Collins.

(F. Darwin) f. 1r

WYCHFIELD,
CAMBRIDGE.

Dec 3/96

My dear Galton

I am coming up to the meeting tomorrow

I fully understand that the last thing you want is to interfere with the Down scheme. But as a matter of fact I think this scheme would be fatal to the Down scheme.

f. 1v

There is neither money nor
enthusiasm for two
schemes. I can see
no advantages and many
disadvantages in separating
animals and plants
and I personally never
meant down to be for
plants only. For insects

f. 2

and many birds you must have green houses which exist at Down and are connected with a small laboratory. I thought I should like to warn you of my views

Yrs sincerely

Francis Darwin

(F. Darwin) f. 35

Dec 8/96

WYCHFIELD,
CAMBRIDGE.

P.S. It is Horace who is worshipful

Dear Galton,

I think Phylometric is a very ingenious form of that type of name, but I feel strongly that some English name will be necessary for the Station, and therefore for the committee. I can think of nothing but Evolutionary Station, or Darwinian Station. I am afraid I think your

f. 3v

postcard title not much
better than our present title
: and any title which accentuat[es]
measurement as the object
is too dull a title for ad=
vertising purposes - It is very
easy to criticise but as you
see I can't suggest any thing
good myself -
As soon as I got back the other
day I wrote to Wm about the
price of Down. He tells me
that £4000 was the valuation

f. 45

for estate duty - He has now
written to the valuer to know
ates) what is the ordinary
market value. Wm doubts
whether it will be much
more than £5000.
I don't quite know what I
had better do next - I will
see whether Wm & George will
write you a letter saying that
they are willing to sell Down
for a Station - I think the
best plan wd be to sell it to
the R-Soc which wd then be
a trustee & might commit
the care of the land to our
Committee. I forgot to say

f. 4v

that I quite approve of your proposal about accessory ordinary members. I suppose we might be allowed later to change the name of the Committee???? I think from what Foster said that he disapproves of any compound Classical name.

Would it be possible to have a long name which would look all right when written & would yet admit of being colloqu^y shortened? Such as Committee for the Continuous Study of Evolution — (Nick name Evolutionary Committee)

But I don't quite like this

Yrs ever

Francis Darwin

(F. Darwin) f. 5r

WYCHFIELD,
CAMBRIDGE.

Jan 8. 97

Dear Galton

I shall make a point of coming on 14th

The £4000 was as I think I said valuation for probe but the sale price to a willing purchaser wd not be more than £4500 or £5000. Both Wm & George (the exors) would be willing to sell for the

f. 5v

Evolutionary Station
tho' they will not sell
to an ordinary purchaser
I think we should all
prefer if possible to
sell to the R. Soc ie not to a possibly temporary body ~~who~~
The R. Soc would hand the estate
over to the Evol^y Com^ee
to manage. It would
have to be arranged so
that if the scheme breaks
down the family could
reacquire the estate

f. 6r

at the price at which
it was sold. I feel
practically sure that
we should each subscribe
£100 so that the price
would be certainly
reduced to £4000
I can only say this on
general impression as
they have not all promised
to give any thing -
I may have something
more definite to say
by 14th.

f. 6v

I am glad to hear that there is so much to consider

Yours sincerely
Francis Darwin

(F. Darwin) f.7

Wyckfield Jan 21/97
Cambridge 97

Dear Galton,
On the bird in the hand principle I think your scheme must be gladly taken up ~~for~~ by us - Though I regret very much that Down cannot be worked, and I am not going to consider that scheme as finally dead.

I should have answered you sooner but I wanted to see Foster as well as (Bateson

Bateson - We should of course like it at Cambridge and it seems to us reasonably accessible from London - I don't know what the Oxford folks will say - I suppose it will have to come before the Committee (by the way Foster will try to come to the next and will keep Thursday Feb 11 clear in case that day suits your views) - We all think that the Station must certainly not be connected with either Phil Soc or the University, but must be under the R. Soc. I don't

see that there can be any
difficulty about getting 2 acres
There is land close to the
station which a land agent
tells me can probably be
rented at £5 an acre -:
and tho' there is no cottage
it would be easy to put
up a galvanised house
Bateson is anxiously consider
ing possible sites & I shall
hear more tomorrow -
Maxwell Masters is glad
to join the Comee - and
Dyer seems interested
in it. I suppose by the way

Weldon will have told him
he was elected?
Bateson is also on the trail
of a man who might
do for caretaker.
I will write again soon
Yours truly
Francis Darwin

(to F. Darwin)

42 Rutland Gate S.W. f. 10r

Jan 23/97

THE ATHENÆUM.
PALL MALL. S.W.

Dear Frank Darwin

Your next letter will ~~be awaited with much interest~~, will enable me to ~~to~~ judge whether ~~the~~ suggested plan should ~~be considered~~ ~~continued~~ still hold good, although ~~not-withstanding~~ that 2 of the main suppositions on which it ~~was based~~ depended, do not ~~do not hold~~; — viz (1) that the Down scheme was not likely to be seriously pursued, and (2) that the small establishment ~~would~~ be controlled by a capable, <u>local</u>

Cttee of the Camb: Phil: Soc:

One must work along the lines of least resistance.

~~We have fixed to~~ ~~A~~ Cttee is fixed for Feb 11, and I should be very glad to hear from you to as to arrange agenda within a week before its meeting; ~~which~~ they will of course much depend on yours a M. Foster's views.

I think I now understand ~~pretty distinctly~~ the chief conditions among which we have to work so far as

~~to be dealt with in respect~~ to the ~~an~~ exper: farm f. 11r

is concerned. I do not speak of the other branch now as the [illegible] has not that well in

(a) a general but not very fervid hand

with the it ~~an experimental farm~~

(b) barely 15 persons ~~who~~ seem to have

a distinct idea of what should

be done at the farm or who would ~~if they had it & who~~ themselves

~~assist the [illegible] of the [illegible]~~

~~would~~ ~~take an active part in carrying out~~ ~~they say~~ experiments.

with the [illegible]

(c) The above persons are scattered

about the country ~~over England~~, and cannot be

expected to come frequently to

London to attend [illegible]

(d) ~~There are~~ More of the really interested persons ~~[illegible]~~

~~distributed~~ resident at Cambridge than

any where else; more even than in London

f. 11v

Encoded by these and ~~such~~ ~~other facts~~ such conditions as conditions,

~~as seems~~ our Cttee ought ~~as soon as may~~ ~~mainly~~ early, for

~~be~~, to ~~now to be in a position~~ formulate what it ~~wants~~ sh^d more properly

and ~~to determine how~~ ~~begin its attempts~~ now to ~~try and~~ to

get ~~the~~ the needed funds. I can fancy something

of this sort:– that ~~we~~ after discussing all the work the Cttee (or some of us)

~~after discussion~~ be deputed & should ~~draw up~~ a report of our

inquiries and ~~suggestions~~ what are wanted to do, (printed "private") round the Cttee [illegible] and then send it to the Council [illegible]

of the R. Soc, asking (1) for such ~~financial~~ money

~~help~~ and as they ~~might~~ would give us, directly or indirectly, and ~~making~~

~~a report~~ (2) that a ~~printed~~ copy, if it be,

~~and~~ if approved, ~~might~~ ~~be sent~~ circulated marked ~~private~~ (together

with written letter) they should authorise its being to a selected list of wealthy persons

who might be thought likely to aid with donations or subscriptions.

On receiving answers from these we could obtain some ~~better~~ idea ~~than~~ ~~now~~ of the support to be hoped for & the next step

wd. have to be considered in that light (the experience

of the Marine Biol: Assoc^n does not encourage

much hope ~~at first~~ of large receipts & subs^n) Yrs s^y

~~Francis Galton~~

Quere Jan 23/97 as to agenda ~~at next meeting~~

(This is a rough draft out of which was sent)

Discuss our views prospects – then

Appoint Sub Cttee to ~~[illegible]~~ draw up a draft Report stating distinctly ~~&~~ what we propose to try to get. That to be sent round in type to several members of the Cttee for remarks. ~~[illegible]~~

At the subsequent meeting to approve the Report as amended and to forward it to R. Soc Council with the request:

(1) for financial aid from the R. Soc (direct or indirect)

(2) for permission to circulate the Report, ~~in the first instance~~ privately among wealthy persons (breeders & others) who are likely to interest themselves in the work, asking for donations, & subs & and other help as may be offered ~~and~~ Also to communicate it ~~in the same way~~ (privately) to leading persons in different societies &c (Lord & Botan)

To report results of this preliminary canvas to R. Soc. Council ~~the answers thus received to form the ground from which our next step should be taken~~ together with our views as to the next step, & asking for their approval of it.

f. 12

f. 13r

Botanical Laboratory, Cambridge, ~~May~~ Jany 26 1897
err

[F. Darwin] Jan 26/97

Dear Galton

I am afraid I haven't anything more definite to add yet – but I can perhaps make one or two things clearer I think your 2 acre scheme ought certainly to have a small Cambridge committee to manage it; all I meant was that it ought not to be connected officially with the Univ. or even the Phil Soc. There is a certain jealousy of Cambridge & the less Cambr idgeness there is about it the better. Also we thought it would appeal to the world as a R Soc commee much more strongly than as a Cambr thing – I don't know whether the local Commee ought to be appointed by our Commee or by the Council of R Soc : I should say by our Commee.

f. 13v

~~As~~ I suppose at the Feb 11 meeting we should have definitely to settle whether we should start your plan of spending capital, or whether we should wait till we can spend mainly income. If that were first settled we should know where we are.

I think it very important to pack our next Commee meeting, ie insist on ~~one~~ people coming by writing privately.

I will write to Dyer.

Yours sincerely

Francis Darwin

(G. H. Darwin)

f. 1r

Jan 12. 97

DOWN,
FARNBOROUGH, R.S.O.
KENT.

Dear Galton

If any of yr. committee wd like to come down to see the place on Sunday, we shd. be very glad to see them. There is a train at 11.55 getting you here for luncheon & you can get back to town for dinner – Please verify trains, as I have only old Bradshaw. I have written to say effect to Weldon

Yours ever

G H Darwin

f. 1v

Charing X	11.10	
Orpington	11.59	
Orpington	2.0	7.42
Char X	2.45	8.23

f. 2r

POST CARD

THE ADDRESS ONLY TO BE WRITTEN ON THIS SIDE.

HALFPENNY

FARNBOROUGH
B
JA 13
97
KENT

116

Francis Galton Esq

42 Rutland Gate

S.W

will you please mention
at Committee that
there are plenty of
trains viâ Bromley
(6 miles) & perhaps
viâ Hayes (4 miles)
Sorry you can't
come yrself

G H Darwin

~~Down~~ Farnboro' R.S.O
Kent

F. 2v

H Darwin

f. 3r

Jan 16 - 97

DOWN,
FARNBOROUGH, R.S.O.
KENT.

My dear Galton

I am sorry none of you can come Down on Sunday. I have written to Wilkins to ask about outgoings — my _conjecture_ is £100 a year.

The only thing I can see in favour of Down Scheme is that it _may_ be easier to get the larger sum for this place than a smaller one for some place with no interest attached to it. I have felt with you that the real difficulty is the

f. 3v

sustentation fund.
I think that if the committee determine to try it the way wd be to have a statement in the Times & Nature as to the nature of the scheme & above all, if you can get any leading Americans to take it up, a similar statement in American papers. Such a statement shd ~~contain~~ be ~~an~~ explicit that the plan depends on financial support with outline of the amount required.

f. 45

It w^d be a difficult paper to write but it might be done.
Probably you will have gathered that the £000, which is the price named, allows a pretty handsome subscription from us. If it were only a matter of purchase once for all I don't doubt the thing might be carried thro' — but the capital required is the important part.
I guess that Lady Derby w^d give something, but not near enu' to carry the thing thro'.
It seems to me just possible

F. 4V

that the matter might be so arranged as to allow of the letting of the house in the summer months, with reservation of certain rooms. If this were possible 150 or 200 a year might be secured.

Yours

G H Darwin

f. 1

RIDGEMOUNT,
BASSET,
SOUTHAMPTON.

Jan:18th /97

My dear Galton,

George has sent me on your letter as to the outgoings on Down.

I think the rates, taxes, water rate, & tithe & schoolrate will together reach about £65, this is too high, & we shall appeal: there would be (say) £4 for insurance, & £60 a year for repairs at the least. The revenue coming in would be about £30 or perhaps £25.

I fear these expences & the size of the house make the proposed station nearly hopeless, apart from the inconvenience of the position of the house so far from a railway.

I have formed no idea of how the establishment is to be supported, unless an American millionaire turns up, which is most improbable.

George has had a tremendous business clearing out the accumulations of so many years, I have been helping for some days.

I hope you are now quite well.

Sara sends her kindest regards to you & to Mrs Galton.

Yours very sincerely,

W E Darwin

Wm E Darwin

Acknowledged 1/3/97.

f. 1

25 Windsor Rd
Forest Gate
~~17 GRACECHURCH STREET,~~
Essex

CHAS. T. DRUERY.

LONDON, 1 . 3 1897
E.C.

W. F. R. Weldon Esq
30^a Wimpole St. W.

Dear Sir

I am in receipt through Dr Maxwell Masters of your circular that of Mr F. Galton relating to the Evolution (animals Plants) Committee of the Royal Society.

My special study for many years has been the varieties of British species of Ferns & in that connection I first discovered the phenomenon of apospory in which as well as in apogamy I am particularly interested. Prof. F. O. Bower subsequently at his instigation Mr W. H. Lang followed up this line of research upon material which I think I mainly supplied & discovered

f. 25

some very unexpected features in the prothallus
phase of Fernlife. If therefore the Committee
in their notices, which I presume they will
publish, would name me as one desirous
of inspecting any observed peculiarities in
the reproductive characters of Ferns (British
or Exotic) I think my experience would
enable me to determine their nature
& I have no doubt that Mr Lang would be
pleased to follow the matter up should I
recommend that further microscopic in-
vestigation which would be more his province
than mine. I think too that if those who
seek for new forms of Ferns in their wild
state were invited to send specimen fronds
to me, together with full particulars as to
habitat, association with or dissociation
from the common ferns of the vicinity, some-
thing might well be learned therefrom.

Further, as I have a considerable

f. 2v

collection of varietal forms, I should be happy to supply material for research to any qualified person recommended by the Committee and be glad to carry out to the best of my ability any suggestion the Committee may see fit to make in the direction above indicated

I am Dear Sir
Yours truly
Chas T. Druery (F.L.S)

3 Dec 1900 (J. Cossar Ewart) f. 1r

The Bungalow,
Penicuik,
Midlothian.

Dear Sir

I very much regret that it is impossible to be present at tomorrow's meeting. I sincerely trust it may be decided to proceed in the direction of obtaining a Zoological farm. Speaking for myself I would like to have the opportunity of making various experi

f. 4v

experimental farms & the Glasgow Technical College have enlisted the active co-operation of some 30 farmers in the west of Scotland. Given an experimental station with an efficient Director or Secretary I believe many would either wish to work at it, have work done at it or have as it were their studs (all of which are more or less experimental) affiliated to it. If I can be of any use at this great distance I shall be very pleased. Yours in great haste

J. C. Ewart

f. 2

ments at a suitable farm in say the South of England which could not very well be undertaken here

I have about 40 acres, at present & accomodation for about 30 horses but if at the outset only small animals are likely to be used 20 acres would be ample. It seems to me ~~that~~ much could be accomplished by organisation

Already various County Council have started

Prof. J. C. Ewart

F. 3

26 Jan 1897

The Bungalow,
Penicuik,
Midlothian.

Dear Mr Galton

I ought to have written weeks since but I have been tempted to delay hoping I might be able to learn more as to the work of County Councils & ~~that~~ I might soon be able to decide about going to London

I hope to be in London

f. 4

for a week or ten days I
& may have the pleasure
of seeing you at the Royal
Society on Thursday
evening. Failing this you
might fix a day when
& where I might see
you. Afterwards I shall
be better able to answer
the query in your
letter of the 16th Jan

My address will be
the Savile Club

Yours in haste
J. C. Ewart

(Prof Cossar Ewart) F.5

Jan 28/97

3, HERTFORD STREET,
MAYFAIR, W.

Cirencester – Russell Swanwick
R. Agr. Coll. Farm

Woburn. R. Ag. Soc

Durham College of Science
(Prof Somerville)

Haggerstone Castle
Mr Leyland

Leeds. Prof. Muir

Downton Salisbury

Aspatria, Agr. Coll. Carlisle

Colonial Coll. Hollesley Bay
Norfolk

over

f.6

possibly also
Bangor. Reading or
Aberystwith

(C. Ewart) f. 7r

Savile Club,
107, Piccadilly. W.

15 Feb. 1897

Dear Mr Galton

Since the meeting on Thursday last of the Plants & Animals Committee I have been considering how the work I am especially interested in might be helped forward. You are aware that I have in hand experiments and I have the limited use of over 1600 pigs. Being so far from London it has occurred to me that much good might result were a Scotch Subcommittee ~~to be~~ appointed. This Committee might consist of say three fellows of the Royal Society and three or four others including e.g. a member of the Midlothian County Council & of the Highland Agricultural Society. If appointed the Scotch Subcommittee

f. 8r

bearing on telegony; hybridiz-
ation experiments and investi-
gations likely to throw
further light on atavism
& on the recapitulation
theory and also on the artificial
fertilization of mares &
other large mammals
Further I am anxious
to start experiments on
the effects of inbreeding.
For the work in hand I
have a stud of forty horses
– including a zebra stallion
– an Indian bull, & a number
of dogs rabbits & pigeons

f. 8v

might first submit a scheme of work for the approval of the Central Committee & they might next obtain what aid they could from County Councils, Societies, Breeders &c for carrying on the work which was considered worth undertaking.

Professor D. J. Cunningham of Dublin tells me various experiments are in hand in the Dublin Zoological Gardens. An Irish Subcommittee might be formed to render what assistance may be possible to work in Ireland. Trusting these suggestions will receive your favourable consideration, I am

yrs truly J. C. Ewart

M Foster

F. 1

Dec. 1. 96.

NINE WELLS,
GREAT SHELFORD,
CAMBRIDGE.

Dear Galton

I fear I cannot get up on Friday – unless you think my presence indispensable – & I don't think you will do that.

I think as I said I should keep the Darwin House scheme separate from this.

Have you asked Heape – & also Cossar

f. 2v

Ewart & Edinburgh - I should strongly urge you to ask the latter supposing that you have not done so

Ever yours
M Foster

What do you think of the Times this morning - it is a pity some one can not tell Buckle what a fool he is making of himself.

[M Foster] f. 3

Shelford
Dec. 3.

Private

Dear Galton.

E. J. Lowe FRS.
is I believe a lunatic
– but he is very hot
on breeding hybridizing
&c – & I understand
wealthy. – – Perhaps he
is worth laying hold of

Ever yours
M. Foster

(M. Foster) f.5

Dec. 11

THE ROYAL SOCIETY,

BURLINGTON HOUSE, LONDON, W.

My dear Galton

We decided to keep the old name for the Ctee — When you have got your programme in a distinct shape write to P.-L. & state your wishes — & the whole matter can then be put on proper footing.

We added Carl Pearson

f. 6v

and gave you power
to add ordinary &
accessory members

Ev yours
M Foster

F. Galton Esq

f. 1

Heape
Correspondance about the
R. Agricult Circular
June - July 1897

~~Draft~~ 42 Rutland Gate SW F.2r

Dec 10/96

Private

Dear Weldon

~~combining~~ (1) About your wishes ~~to~~ to not extend ~~the~~ the original ~~purport~~ work of ~~the~~ our Cttee & the numbers

~~so as to~~ ~~include~~ individual ~~possibly~~ possibly obstructional to original ~~work~~ purposes

members ~~who joined~~ ~~so far as that was considered~~

~~in connection~~ with (2) the power asked

for to day to add to our numbers, ~~chiefly~~

~~on~~ for other ~~work~~ purposes ~~grounds~~ ~~I have raised~~

The opportunity of talking with you has been ~~...~~ neglected

~~as to effecting both.~~

I ~~understood that~~ you approved, & ~~I think agreed with~~ me

~~you were~~ if I understood rightly of

~~about~~ in what I wrote to Bateson ~~asking~~

orig: that ~~that~~ I intended to propose a

resolution at our next meeting

wh: I believed accorded with our

f. 2v

tacit intention hitherto, namely that the responsibility for statements in the papers presented by ~~them~~ the Cttee to the Society, rested with the authors of those papers. In short, that our ~~province was~~ true function was to collect ~~papers~~ (from men of repute,) (and to encourage the writing of ~~such~~ papers about the meas: of plants & animals ~~would be communicated to us~~ ~~Of course no paper that had not been submitted~~

If you agree generally with the ~~that~~ above ~~view~~, no difficulty seems likely to arise from a considerable increase to our numbers ~~for the new class of purposes~~. ~~any member~~ of ~~the Cttee who might wish to communicate~~;

Some such resolution as the above, seems really necessary, because the members of our Cttee ~~being~~ are ~~severally~~ expert in different

F. 3r

~~present there~~ cannot ~~feel themselves~~ all be competent ~~individually~~ to ~~pass~~ give a collective judgement of precisely ~~a trustworthy judgement~~ on even ~~ordinary~~ very ~~the points that are now discussed by outsiders~~

I had much fear that you would ~~might~~ think I had been ~~the~~ forgetful of what I wrote some time back in answer to your letter ~~about~~ bearing on part of the above matter, & so I send this at the last moment

Very faithfully yours

Francis Galton

we shd thus be doing just what most Sc. Socs do, giving publicity to what we think on prima facie grounds to be good.

way,

f. 3v

The opportunity has been neglected of talking with with about combining (17 &c.

with ~~a~~ to extend our ~~the number~~ of ~~X~~ Cttee so as to include men who would possibly be obstructive to its original purposes, with 2, the power asked for today to add to its numbers for other purposes.

F. 4v

~~I was forwarded [illegible] Prof: Weldon to whom your last letter was duly forwarded~~ will probably be held (~~probably~~ ~~Christmas~~ just before or after Xmas)

~~I propose at~~ the next meeting of the Cttee for measurement &c, after, I intend to propose a resolution ~~that~~ ~~the C~~ which I believe ~~to be~~ is in accordance with our tacit intention hitherto; viz that the ~~Cttee are not to and~~ ~~undertake the~~ responsibility for statements in ~~papers~~ papers presented by them to the R. Soc: rests with the authors of those papers. (I am here only giving the general sense of what I mean; ~~the precise words for subject to discussion~~. leaving the precise words to ~~for~~ be determined later)

So I do not think it would be of much use to print & circulate your letters among its members. ~~whom as you know are specialists in different ways & would not feel them but that it would be better~~ especially as the issues between yourself & Prof Weldon are not yet ~~so~~ concisely opposed, & all misunderstandings having been ~~the~~ much ~~time is~~ quite removed. When his next paper ~~appears~~ is read which will presumably be early in 1897, it might be an appropriate time to put forward such objections as you may have to make, before the Cttee, *sion Office*, CHARTERHOUSE, LONDON, E.C.

f. 45

.S.,

Gate, S.W.

Letter from Heape, dated June 18/97, enclosing 3 circulars received on Friday Night June 18th/97 and answered by letter before the 10.30 am post in S. Kensington on Saturday — Draft of answer kept.

Telegram on Monday morn.g from Heape saying he cd. call at 1 h which he did. First he asked to see his, or our letter, wh.: I showed him. He did not appreciate the degree of irregularity. He had been to the R. Agr. S on his way, and told me that the circulars were not printed off and there was plenty of time to alter. Also that he had only glanced at the copies for a few minutes before sending them me. He had had much family trouble (a father-in-law's illness & death). He also spoke of having intended to send me a proof 3 weeks ago, but was not sure that he did. Altogether the explanations seemed wanting in frankness, precision & good taste. Then he said that even if he had ~~done~~ acted as I had supposed he did not think my letter was justified. I at once insisted that it was & that if there were any chance of such a proceeding hereafter, I cd. not consent to serve with him on the Sub Cttee. He was evidently very angry, & contained himself with difficulty, saying he would consider whether or no he would continue to act. He left the corrected paper with me as I ~~had~~ said the time ~~was~~ had been insufficient for me to study it, & that I shd. have liked to consult Weldon (as Secy) who ought to know what was going on. The next day was Jubilee & the day after I returned the corrections with notes of explanation pasted to the side of them, such as Mr Clarke c'd see.

(rough draft)

42 Rutland Gate SW

June 19/97

Dear Mr Heape

Late last night I received copies of the circular (about to be distributed) which, as you say ~~mention~~ in the accompanying letter, ~~you had~~ was drawn up after conference with several practical breeders & with Mr Ernest Clarke. So far as the body of the circular is concerned it seems to have been carefully considered and ~~I have no doubt that it is~~ ~~quite~~ appropriate; I ~~should~~ agree to it with pleasure.

But as regards the preamble ~~first page~~, it is quite ~~[illegible]~~ another matter. You are there represented as ~~having been~~ entrusted by "the Royal Society" to

f. 6v

collect certain data. ~~which~~ This status you
assuredly do not possess; neither
did the Evolution Cttee so far
transgress their own powers as to bestow ~~give~~
~~it to you on their own responsibility~~,
with the certainty of deserving and
the probability of incurring grave
censure from the Council for doing so.
~~All they did was~~ They did no more than to appoint
a Sub: Cttee, consisting of yourself
& myself, to collect the data,
with the understanding that you
shd do the working part and that
I should be chiefly responsible for seeing
that the action of the Sub-Cttee
harmonized with the general wishes of the Cttee.

f. 7r

In ~~this~~ present case, you have however acted entirely ~~on~~ on its yourself ~~own responsibility~~ without ~~giving me any information~~ consulting with me in any way ~~whatever, as to what you had been doing in this matter, with~~ The result is that I must disavow all share in the responsibility and leave you to get out of the extremely awkward irregularity as you best can. // The circular must not go forth ~~in the present state~~, with the objectionable words uncorrected. # Neither do I ~~think~~ think the earlier part of the ~~preamble~~, correct. // I have altered both in the enclosed copy.

f. 7v

I am sure that the Cttee as a whole, will disavow your action. You ~~would~~ do well, if in any doubt, to consult ~~at the next~~ with a Cambridge member of our Cttee who is also a member of the R. Soc: Council, showing them at the same time this letter ~~or~~ (or Mr F. Darwin or Prof. M. Foster) (& any corrections to the circular.)

Pray, let me hear in reply, with as little delay as possible.

Faithfully yours

Francis Galton

Corrections as sent

f. 8r

Royal Agricultural Society of England.

PRACTICE WITH SCIENCE

13, HANOVER SQUARE,
LONDON, W.,
June, 1897.

BARRENNESS AMONGST SHEEP.

DEAR SIR,

The co-operation of the Royal Agricultural Society has been sought by a Committee of the Royal Society in an inquiry now being made upon ~~the~~ question ~~of Sterility in animals~~; and it is proposed, as a first step, to make an investigation as to the causes and extent of barrenness amongst sheep.

s/ d connected with breeding

The collection of the necessary information on this point has been entrusted by the ~~Royal Society~~ to Mr. WALTER HEAPE, M.A., of Heyroun, Chaucer Road, Cambridge; and the Royal Agricultural Society will be obliged if you will favour Mr. Heape with the results of your own experience on such of the matters referred to in the accompanying Schedule as are within your knowledge.

m/ del

Duplicate forms are enclosed, in the hope that you may be willing to hand them to other flockmasters in your district, whose experience will be likely to be of value for the purposes of the investigation.

Thanking you in anticipation for any information with which you may favour Mr. Heape (to whom the Schedule should be returned, and any further correspondence addressed),

I am,
Yours faithfully,

Ernest Clarke

Secretary.

[P.T.O.

$(\frac{2000}{6/97})$.

f. 8v

INQUIRY INTO BARRENNESS AMONGST SHEEP.

NOTE.—It is not expected that every flock master will be able to answer all the following questions; but it is requested that as many as possible may be answered, and this paper then returned to WALTER HEAPE, Esq., M.A., Heyroun, Chaucer Road, Cambridge.

QUESTIONS.	ANSWERS.
RAMS.	
What breed of rams do you keep?	
How many rams were used for tupping last autumn?	age age age
What was the condition of the rams when tupping began?	
Did you give the rams artificial food during tupping time, and if so, what food?	
How long were the rams kept with the ewes? ...	
EWES.	
What breed of ewes do you keep?	
How many breeding ewes did you put with the rams last autumn?	
How many of these were shearling ewes?	
How many of these were two shear ewes? ...	
How many of these were three shear ewes? ...	
How many of these were full mouthed ewes? ...	
How many of these were ewe lambs?	
FEEDING.	
What was the flock fed on from April to May last year?	
What was the flock fed on from May to July last year?	
What was the flock fed on just before tupping time last autumn?	
What was the condition of the ewes at tupping time? Were they poor or fat?	
What was the flock fed on between tupping time and lambing time?	
What was the flock fed on a month before lambing?	
What was the condition of the ewes at the lambing season?	

BREEDING.

What is your practice as to tupping, and how did you divide your flock? ... ...

How many ewes were there in each flock at tupping time? ... ...

How many rams were there in each flock at tupping time? ... ...

Did many ewes go over? ... ...

What was the total number of lambs born this season? ... ...

How many of these were ewe lambs? ... ...

How many of these were ram lambs? ... ...

How many of the ewes had twins? ... ...

How many of the ewes had triplets? ... ...

How many cases of abortion *were seen*? ... ...

How many ewes proved barren? ... ...

What were the ages of the barren ewes? ... ...

Did you notice that all or many of the barren ewes were served by a particular ram? ...

Did you put young rams with old ewes, or old rams with young ewes, or, if not, how did you apportion them? ... ...

Did you find that most of the twins were got by lamb rams or by particular rams? ... ...

Did you notice that most of the twins were born early or late? ... ...

What proportion of barren ewes do you usually get? ... ...

What is the largest proportion of barren ewes you have *known* of? ... ...

Signature ____________________

Address ____________________

____

Date ____________ 1897.

f. 9v

WALTER HEAPE, Esq., M.A.,

HEYROUN,

CHAUCER ROAD,

CAMBRIDGE.

a dated Copy of this was pasted to the corrected proof & sent to Heape today June 23/97 Wed. f. 10

The sentence conveys the idea that the inquiry of the Cttee is limited to the one specified investigation. This inference sh.d be avoided because sterility in domestic animals (however ~~excellent~~ good a subject for a beginning) is hardly a typical and central instance of the ~~inferred work~~ province of inquiry. On the contrary it is ~~but~~ one that may seem to many, as to myself, to lie very close to ~~the~~ its limits ~~of their province~~.

Many alternative phrases would ~~meet~~ this small but important objection, thus

"...now being made upon questions of great ~~interest~~ importance to breeders of stock, and it is proposed..."

Mr. Heape is not entrusted by the Royal Society to collect information. The ~~statement in the draft letter~~ sentence as it stands puts him into an altogether false position (& would give rise to erroneous protests from many quarters, & ~~thus~~ and to disavowals) ~~and would [illegible] the Cttee and most particularly myself as a joint member of his Sub-Cttee into serious difficulties with the Council & the Society. If the sentence appeared in its present form [illegible] they would have to take steps of disavowal~~

The substitute of the word "them" (i.e. the Cttee) for the phrase "Royal Society" w.d remove ~~the~~ the difficulty.

~~P.S.~~ P.S. Mr. Heape ~~has only recently communicated~~ (for the first time) showed to me a copy of this letter ~~to me~~ last Friday, I wrote (pointing out these objections). He has since assured me verbally that (though it is apparently printed off for distribution) it ~~is~~ still exists only in ~~draft~~ proof, subject to revision.

June 30/97

F. 11

Dear Mr. Heape

dated yesterday

Yr. letter ~~received this morning~~ does not construe rightly

my attitude, which was this. I had received from you 3 copies of a circular,

"for consideration", or

that did not bear the word proof ~~or any equivalent~~ ~~but which were~~ luxuriously ~~clearly~~

~~on good paper~~

printed apparently for immediate circulation, a view that ~~seemed~~ was confirmed by

(and therefore was) intended

your accompanying letter, from which it appeared that ~~we had according to that~~ the contents of the circular had been discussed between

You there mentioned that the circular had been discussed between

yourself & Mr. Clarke for some time. ~~In Mr. Clarke's letter~~ ~~[illegible]~~

~~unfortunate statements~~ that I have already pointed out, ~~which~~ which you made

remarks ~~objection~~ whatever about them.

no ~~reference~~ in ~~the accompanying~~ your letter. Everything therefore concurred to show that you

had by yourself in connection with ~~him~~ Mr. Clarke ~~[illegible]~~ sent & passed the letter, in proof ~~and was~~

~~about to circulate it~~ without having let me, as a joint member with

~~yourself~~ of the Sub Ctee, know anything on the matter ~~about it~~. Your verbal explanation

was that ~~I had drawn a false conclusion~~ wrong inference, ~~that~~ the circular was

really a draft, open to ~~correction~~ revision; and that again ~~[illegible]~~

you had sent to me the 3 copies without properly reading them.

~~Of course I withdrew my objections, but you~~ immediately ~~confused~~ put the matter

in a new aspect by ~~saying~~ that even if you had ~~done~~ acted as I supposed, ~~it did not in~~

or that it wd have ~~[illegible]~~ justified my writing ~~to you~~ as I did ~~had done~~, ~~that~~ ~~you did not~~

of the supposed action.

[illegible] see the great irregularity. That view, I protested strongly against, & added that if there was any likelihood of ~~the~~ future action in this sense

that I very much deprecated, I could not continue to serve with you on the

Sub committee.

I thought the explanation was made in a way rather deficient in assurance and clearness, still I frankly accepted it and was withdrawing my remarks when you

Yrs. faithfully

F.G.

The first page contained Mr. Clarke's letter with those unfortunate statements to which I drew attention. You made no remark in your letter about these, so I presumed you had passed them and the rest of the circular, without having even consulted me as the joint member of the Sub-Ctee.

July 5/97 f. 12

Yr. letter reached me shortly after my card was posted [saying I was about to leave for the Continent] & as the main points are now cleared up, I ~~will~~ do not care to dwell on two details of ~~discrepancy~~ ~~between~~ unsettled your recollections & my own disagree but am quite content to say no more about ~~about~~ that part of the matter. I note however that you do not allude to alterations about to be made in Mr. Clarke's letters, which I shall presume will ~~be the case done~~ follow.

As regards the sterility investigation it seems a very good one to begin with, & was fixed upon accordingly. What I objected to is putting it forward as the one special object of the ~~being not a control~~ ~~but rather a [illegible] subject~~ Committee's inquiry. This I ~~[illegible]~~ stated as clearly as I could, in the Notes of and a particular reason for urging the objection strongly was Explanation -

I take this may conclude the present correspondence. Fg.

f. 13r

POST CARD

THE ADDRESS ONLY TO BE WRITTEN ON THIS SIDE.

HALF PENNY

Walter Heape Esq
Heyroun
Chaucer Road
Cambridge

f. 13v

The final removal of all cause for these difficulties
wh otherwise ~~might~~ ~~that~~ have arisen, ~~gives~~
is quite satisfactory ' I trust
~~very much pleasure~~.
that the forthcoming ~~the~~ material ~~to be derived through the~~
~~the circular~~ will fully serve
~~give~~ the purposes of the inquiry.

I go abroad probably on
Wed. morn.

F. Galton
12 July 1897

(To Dr. Dallinger) f. 14

~~In the earliest stages of development~~ of the ~~cell~~ reproductive cell, whether ~~in asexual~~ (b) sexual or (a) asexual?, ~~does an arrest of growth ever take place during~~ the

Has ~~it a~~ stoppage ~~and~~ contorted abortion (or what we may call) ever been remarked ~~observed~~ during the process of development

~~There are many~~ Cripples ~~grubs are~~ often occur when ~~found in~~ breeding moths ~~is there~~ has any appearance of a crippled development been observed in the very earliest (Karyokinetic) stages of development?

Generation	Persons		Inferences
(0)	A = B	~~Neither below the standard of~~ ordinary physique or health	perfect in soma imperfect in germ plasm but imperfect in different ways
(1)	L = m n = o p = q r = s	are all ordinary health, [illegible] some variability no doubt	perfect in soma imperfect in germ plasm ~~& imperfect~~ in same way ~~in the most part~~
(2)	k = u * = w	are somatically defective	imperfect in soma imperfect in germ plasm as above [illegible] what [illegible] & cannot be [illegible]
(3)	x = y	are still more so / get still more so	
	z	? as to power of both sons [illegible] abstinence to fulfil [illegible]	

* The soma is a constant drain, taking the best of the germs

(Gadman) f. 1r

7, CARLOS PLACE,
W.

Jan. 6th

Dear Sir,

I ought to have answered your letter about the Measurement of Animals & plants some time ago, but I have been a good deal away from home lately hence the delay –

I shall be very glad to do anything I can to assist the work of your Committee

f. 1v

in its investigations -
Pray add my name to the
Committee if you think I
can be of any use to you.

Believe me
Yours truly
F. D. Godman.

Lord Walsingham

f. 1r

TELEGRAPH STATION WATTON.

MERTON HALL, THETFORD.

3. Dec 1896

Dear Mr. Galton,

I am sorry to be unable to attend the meeting tomorrow to discuss the proposal to establish a biological farm - Owing to my absence from home I have scarcely had time to think over your letter There can be no doubt whatever that such an establishment would provide

f. 1v

the means for carrying out many interesting investigations. New lines of enquiry are constantly suggesting themselves which if left solely to private enterprise must wait many years for the solution of such problems as are connected with them. My only doubt would be whether the results likely to be arrived at would be commensurate with the expense involved in

f. 25

maintaining such an establishment. A large crop of negative results must be expected for each positive discovery recorded. I shall certainly not be one to throw cold water upon the scheme and if unable to give it for the present at least any pecuniary assistance I can at least undertake with such facilities as I have here to record facts in relation to the breeding of animals and to gather information which shall be at the service of

f. 2v

any committee appointed to conduct special enquiries on such subjects. I think it would be found in practice by no means impossible to institute special investigations on private estates if only the proper lines of enquiry and the methods by which accuracy could be best ensured were clearly drawn up by a committee and suggested to those who were competent and willing to undertake them.

If funds can be available your more ambitious scheme would of course be preferable

Believe me, Yrs very truly

Walsingham.

(Lt Walsingham)

f. 3r

Merton Hall
Thetford.
Jany 5 - 97.

Dear Mr. Galton

I am not prepared to offer any suggestions at present. The idea which I hurriedly put forward when we met was simply to endeavour to obtain the assistance of breeders of stock in working out some of the many problems which suggest

f. 3v

themselves – E. G. Any habitual breeder of pedigree stock ought to be interested in the question whether a mare's progeny by a second or third stallion show any tendency to reproduce the characteristics of the first stallion to which she has bred. This question would be best settled by using a Zebra first and subsequently

f. 45

a Donkey or pony stallion.
Several questions regarding
the proportion of ♂ or ♀
offspring and the means
if any by which one
or other can be secured
or rendered more probable
would be of great interest
and might attract the
attention of breeders if
a scheme of systematic
observation & recording
were put before them
by a Committee such

f.4v

as yours. The first step should surely be to draw up such schemes in precise language for each particular line of enquiry. When this is done I would endeavour to get them taken up.

I regret that I have no leisure to join your Committee.

Yrs very faithfully

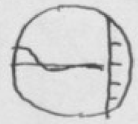
Walsingham.

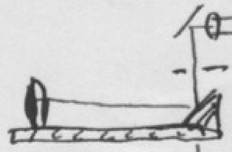
Rough Draft. to Ld. W.

f. 5

Wd. you consider the ~~feasibility~~ advisability of ~~inducing~~ asking the various stud clubs to offer more precise descriptions of the animals ~~on~~ their stud ~~books~~ lists?

Take the case of thorough bred horses as an example. ~~Colour~~
Stud books to contain Colour & certain measures taken by a vet: on a recognised method.*





* Cathetometer is probably the best for use at shows as it is independent of distance and ~~for~~ height of its base above or below the level plot on which the horse stands - it wd. be adjusted to a vertical measure ~~[illegible]~~ beyond or temporary on that plot. It would give easy[illegible] height of withers & of rump. ~~It~~ The depth of chest cd. also be given. It might be that a horizontal apparatus on same principle cd. be used for length of body, supposing that it cd. be ensured that the horse stood squarely.

Photography on bricked path by brick wall — Head cd. not be managed unless strictly in profile

To Ld. Wolverhampton Jan/97

(W. Heape)

f1r

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

1st Dec. 96.
11. pm

Dear Mr Galton,

I have been away for a few days and am only now able to attend to your letter of the 28th Nov. and the printed ~~ ~~ slips you have been kind enough also to send me.

The scheme interests me greatly, and I may say at once I shall be very happy to serve on an Executive committee –

With regard to the meeting on Friday to which you invite my attendance, am I right in concluding that you have done so under the erroneous impression that I am a fellow of the Royal Society?

If that is so I shall be glad to come over on Thursday to see you, it is rather a big matter to discuss in a letter.

I shall be away tomorrow until

evening but if you would be kind enough to send me a wire some time tomorrow I could call upon you any time after 10.30 A.M. on Thursday. I think some preliminary work to be done by a Secretary of the Executive Committee, as you suggest, might ~~for~~ form the nucleus of a very important branch of the scheme of work which I hope will eventually be undertaken.

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE

Very faithfully yours
Walter Heape.

The utilisation of Darwin is not my suggestion. I quite agree with you it will be an error to separate plants and animals too widely.

W. Heape

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

3. Dec. 96.

Dear Mr Galton,

Thank you for your letter, I will attend the meeting tomorrow. —

I should like to add that I asked you to confirm your invitation for Friday's meeting because you addressed me as F.R.S. and I felt it was possible you had made an error in including me!

Believe me

Very faithfully yours

Walter Heape.

f.2

W Heape

f.3

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

Declined

8. Dec. 96.

Dear Mr. Galton,

Can I persuade you to come over and attend the dinner of the Cambridge Philosophical Society as my guest, on Saturday next the 12th Decr. — and to stay with me over the following Sunday? — It would give me very great pleasure if you would do so.

Believe me very truly yours

Walter Heape

(Heape)

f.4r

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

16. Dec. 96.

Dear Mr Galton,

In reply to your note I shall be very happy to serve on the Committee.

With regard to a scheme of experiments to be carried out by persons in the country who might be willing to help — I cannot help feeling that any suggestions should be very carefully considered before being adopted, and for this reason that the practical man before whom they will be placed will be disposed to criticise severely any scheme on such a subject submitted to him by scientific men, and it is most important the practical man should feel that the scientific man, on this occasion, has at heart practical needs.

I would suggest that a member of the Committee should be authorised to confer with certain practical breeders, such as Lord Walsingham, who will know what the practical man wants and what he can carry out, and then

report to the committee the result.
I should very much like to come over and consult you about the whole of this matter which I have much at heart — Should I find you at home and disengaged on Saturday or Monday next & would it be convenient for you to see me at 11 o'cl or after on either of those days.

Yours faithfully
Walter Heape

HEYROUN.
CHAUCER ROAD.
CAMBRIDGE

Very pleased to see you here Saturday eleven o'clock

(Hempe)

1

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

5. Jany. 99

Dear Mr Galton,

I am very sorry I shall be unable to attend the meeting on the 14th, I have a long standing engagement at Lincoln that day.

With regard to the suggestion that I should be made a secretary of the committee, I feel that so long as the aims of the committee are within my ken I shall be very glad to do the work, but that my position as a secretary must depend on that &

2

I don't yet know what the aims of the committee properly are

For instance, I would gladly act as secretary on matters concerned with breeding but could not undertake botanical questions involving special botanical knowledge.

With regard to conferring with breeders, may I suggest that my power to do this shall not be confined to horse-breeders. We shall not find too many practical men willing to help and returns will be small for several years I suspect. It would be well I think to interest breeders of all stock wherever possible.

Besides would it not be a waste of time & energy to visit a horse breeder & neglect his next door neighbour because he may breed sheep?

With regard to possible investigations for breeders to undertake –

It would undoubtedly be well to be in a position to suggest investigations to them, we may find some breeders willing to ~~[illegible]~~ undertake one problem & others another – the main points we have to bear in mind _at first_, seem to me (1) to propose investigations which they can _see_ are of practical importance to them as breeders –

(2) to propose schemes which they can carry out satisfactorily without expending money, and (3) to bear in mind that they are very busy men who do not recognise yet the importance of scientific aid, and who will probably regard any time spent on such matters as wasted. If such men are judiciously handled I think they _may_ become interested, _may_ give us great help and _may_ themselves benefit thereby, but it must take time to bring this about & I should be very careful not to frighten them _at first_.

5 W. Heape Jan 5/97

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

among the "topics" you suggest I do not recognise any which I think will interest breeders generally except infertility - its annual variation they would I think be willing to record, the causes of it they would certainly gladly know, the degree of infertility of in bred stock some would probably be found to record, but the prevalence of malformation they would be quite unable to investigate since they fatten infertile stock & sell it to the butcher - Exceptionally intelligent breeders may

f6r 6

be found who will express opinions, & ~~repeat~~ record cases, of Heredity - Prepotency & Telegony. Variation - fraternal. Sports & their stability; but I fear the value of such evidence will not be great & think these & similar problems can only be satisfactorily attempted in a suitable scientific form.

Investigations on infertility may I think bring to the fore various other questions of importance such as questions concerning "Heat"; the influence of certain pastures (food) meteorological influences, the effect of different stock on pastures & so forth.

7

With regard to the rules of breeding I quite agree with you it would be very interesting & important to discover the rules followed by different breeders & how they work — I hope some scheme can be framed for that end.

I do not know what publications breeders read & write in, beyond the Field, Land & Water, The Farm Journal (I think it is) Stonehenge on the Horse & Dog. Various dog papers like the Fanciers' Gazette, the Dog-owner's annual & so forth.

I am not surprised to hear that Millais lecture amazed you.

f6v 8

HEYROUN.
CHAUCER ROAD.
CAMBRIDGE

He is a bold experimenter, but his ideas regarding epiblastic & mesoblastic prepotency, the fertilising of immature ovarian ova by means of the spermatozoa of a previous sire &c &c are — well — comical.

I am just recovering from a sharp attack of bronchitis, an unusual ailment for me, & cannot get out just now, but if you would like to see me before the meeting I would come up on the 8th & call on you at 12 o'clock —

I leave here for the North after 9th.

Yours faithfully Walter Heape.

(Heape)

f.7r

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

7. Jany. 97.

Dear Mr. Galton,

I have just received your letter of today, and gather that you expected an answer to your letter of the 2nd before mine of the 5th reached you. I did not understand from your letter that any reply was required other than a reply to the query about the title of "Secretary", for wh. there seemed no special hurry. You told me that the meeting had been arranged & I should receive a summons with agenda in a few days. I had no idea you proposed I should say anything either about my own proposal or about Millais' dogs. Your letter of ~~toto~~ today is the first intimation I have had of this.

Again I should like to explain that my suggestion of the advisability of including all breeders in the scheme of preliminary enquiries, was made in consequence of the remark in your letter that I should act as "Emissary to confer with horse-breeders".

with regard to my suggestion it is,- that I should be appointed to act with you for the purpose of conferring with breeders -

a as to problems which they are desirous of having solved &

b as to a scheme or schemes for recording data bearing upon those problems which they will be willing to supply.

Besides this I agree with you it will be very advisable to be prepared with a series of problems the importance of solving which should be urged upon breeders as opportunity offers and their aid obtained whenever possible.

I am very sorry that my delay in answering your letter has been inconvenient to you, but I hope I have explained that in this instance the fault has not lain with me.

Believe me

Yours very faithfully

Walter Heape.

Heape Jan 21/97

58r

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

21. Jany. 97.

Dear Mr Galton,

In reply to your letter of the 15th Jany. May I first of all express my appreciation of your kindly help with regard to travelling expenses, and my personal thanks for your generosity. I can only say I shall do my best to ensure that the money is not wasted.

With regard to the "questions" you ask –

I should be glad to have experiments made (on an experimental farm such as you suggest) ~~on~~ upon.

(1) The effect of different foods and different quantities of these foods, on the breeding capacities of animals and on the offspring produced under such various conditions

f.80

I would suggest consultation with physiologists in order to determine the kinds of foods to be used.

(2) Upon the phenomena attending the occurrence of 'heat' - also, the relation of 'heat' to ovulation investigations on and the relation of insemination to ovulation.

(3) Upon the artificial insemination of ~~females~~ animals, not only of females with the spermatozoa from males of their own species, but with spermatozoa from males of different species

(4) A carefully tabulated record of the effect of in breeding.

(5) experiments to induce Telegony.

I think all these experiments could be performed on small mammals mice, rats, rabbits, guinea-pigs, and that they would bear lines which would prove of interest to

Heape Jan 21/97

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

breeders eventually.

With regard to the experiments to be conducted now by breeders themselves I think we must be careful not to frighten them with too elaborate (and as they may think too visionary) experiments at first, but by the time such experiments as I have suggested begin to bear fruit it is to be hoped that we may have found (or educated) some breeders who will be sufficiently

assured of the benefits to be derived from scientific work to undertake themselves similar experiments on their own stocks.

Believe me

Very faithfully yours

Walter Heape.

Heake Jan 22/97

f.10r

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

22. Jany. 97

Dear Mr Galton,

I got your note this morning just before leaving for London where I have been all day.

I have seen Ernest Clarke & he will attend meeting of Commt. on 11th Feby (it will be well of course to send him a notice as I could not tell him the hour of the meeting)

I will certainly attend —

I have been speaking to Clarke about the best breeds to interview in the first instance, and hope to be able to arrange to see one, ~~of the~~ who will probably be of use, before very long.

— It is very kind of you to take so much trouble about the title of my paper, and I am very sincerely obliged to you.

I am unhappy to find that your judgment, which I value so highly, is opposed to my own.

I wish I could agree with you, but, if I must be honest, I cannot do so. I will ask you to believe it is my inability to do so and not my unwillingness which stands in the way.

At the same time I am fully conscious that I may be wrong.

Believe me

Very faithfully yours

Walter Heape.

Cambridge

10. Feb. 97.

Dear Mr. Galton,

I do not know what view the Committee is likely to take of the position of affairs as they now stand, but I should like to put before you the following in favour of a waiting policy.

My view is that in order to conduct such experiments as are necessary for the solution of problems in breeding (heredity &c) a permanent establishment is necessary, and the requisite income for carrying on such establishment assured.

The value of such experiments may be expected to show itself in about 50 years after they have been initiated, and the importance of a permanent establishment will then be apparent.

One cannot expect annual subscribers to maintain their interest in an institution or in experiments

which, as far as they are likely to see, can produce no results of importance.

In the same way it would hamper, very greatly, the institution if the continuance of experiments should be dependant upon the raising of sufficient money each year.

It appears to me therefore that a sufficient endowment is essential to the scheme; and I should personally greatly regret the establishment of an experimental station on however small a scale unless it was endowed with sufficient income, and unless it occupied land and owned that land on which the experiments were conducted.

I think the ~~fact~~ failure of any such scheme for want of funds would have the effect of seriously

hampering any subsequent efforts to obtain
money for a like purpose; and for these reasons
I am strongly of opinion that it will be wise
not to attempt to begin work on a temporary
basis, but to concentrate our energies ~~to~~ on the
establishment of a permanent institution and
to wait until the requisite capital sum can be
obtained for that purpose.

In the mean time I think valuable work can
be done by collecting information from breeders
and societies and in endeavouring to obtain
their cooperation in the solution of problems
they are interested in, and in the work the
Committee is desirous of eventually undertaking

Yours faithfully

Walter Heape.

Heape
Feb 11 97

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

20. Feb. 97.

Dear Mr Galton,

I see the Plants & animals Sub-committee is to meet again on Friday, to consider documents ~~to~~ about to be circulated to the committee &c. I should very much like to have an opportunity of seeing the scheme you propose to submit, before the meeting of the committee; if it is possible will you let me see it?

Yours faithfully
Walter Heape.

Heape

1

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE. 27. Mch 97.

Dear Mr Galton,

I have been away from home for some days on account of the serious illness of a friend, and have done nothing lately with breeders.

Before long I am going to see a sheep breeder in Essex on the subject of a schedule re. the barrenness of Ewes; and as soon

as I can draw up a satisfactory schedule the Royal Agricultural Society will print it and send it out together with a letter from their Secretary asking flock masters to give me their assistance -

I think this is all I shall be able to do at present with breeders, just now I am busy with my own experiments - and as soon as I possibly can get away I am

3

going away for a change, which I seem to be in need of just now.

Regarding "Period of Gestation" I think you told me you would look up some notes which might be of service to the Enquiry?

About three weeks ago I met, in London, a young man who wants to interest himself in Biological Enquiries. He is a doctor ~~by~~ but does not now practice.

4

KEYROUN, CHAUCER ROAD, CAMBRIDGE

He is at present living on an Island off the West coast of Scotland, which he rents. It is 18.000 acres in extent, the climate is wonderfully mild & equable though wet, as of course you will know, and it is sufficiently isolated for all four footed beasts except deer, and stags periodically swim over to it from Skye I understand.

I was applied to, to advise something

5

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

for this man to work at.

(Pray consider this Private)

I found him to be a sportsman, and I think a sportsman of the best class, a man who would rather watch the habits of an animal than shoot it!! He seemed to be an acute and an intelligent observer, but he has had no training as a naturalist, and I am strongly inclined to think, his habit of

Heape March 27/97 6 f15r

leading an active out of door sporting life, will always prevent him from settling down to sedentary work for any length of time. Under these circumstances it has seemed to me that his tastes and energies could best be directed to problems connected with breeding which can be conducted on his island home! He has an opportunity of taking the place on a long lease and I think he will be inclined to do so especially if

7

some investigation can be suggested to him which he can carry on there and which will be compatible with an out-of-door life.

The ease with which animals can be kept isolated on this island and at the same time have plenty of room to roam about, seems to me to suggest the advisability of trying experiments on inbreeding or variation. but I have found it very difficult to formulate a

f.15v
8

definite experiment —

Can you help me to do so? and if not on the lines I suggest on some other lines.

The man in question is coming up to see me in a few days and if he decides to take a long lease of the island I propose to go up there and see the place.

He is married, and has I understand the control of a good income, he is anxious to have something to do to

9 Heape March 27/97

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

interest himself. He has I think plenty of ability to enable him to work up any subject which may interest him, but I feel that he requires that the subject shall also enable him to live an out of door life.

I think I have told you all I know about him, and shall be very interested to hear what you would suggest for him to turn his attention to.

10 f.16

It would I think be a great pity to let him go away empty – ~~as~~ if you understand what I mean – and I feel sure there must be problems which will both interest him to watch and plan, and ~~interest~~ interest us in their results.

Very faithfully yours

Walter Heape.

Heape

MONKS MANOR,
LINCOLN.

26. May. 97.

Dear M^r Galton,

I am here on account of the serious illness of my Father-in-law, and it is quite impossible to say when I may be able to get away. On that account I cannot be sure that I shall be able to attend the next meeting of the Evolution Committee;

with regard to the distribution of the sum of money at the disposal of the committee, the only use I should have for money would be to supply funds for travelling expenses.—

Yours faithfully
Walter Heape.

F. 17v

I shall however do my best to ~~make~~ be present on whatever day is finally fixed upon. I have practically nothing to report, I have seen a few breeders but have been able to make very little of them. I am neither disappointed nor discouraged on this account, for I anticipate much trouble and scanty returns for a year or two —

An inquiry regarding barrenness in Ewes has been set on foot. The Roy. Agricult. Soc. has undertaken to print and circulate a schedule of questions and in the course of the next six months I hope to receive some replies, I then propose to visit such of my correspondents who show interest and intelligence, and when that is done, to report to you.

f18r

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

18 June. 97.

Dear Mr Galton,

I have been much away for the past two months on account of the serious illness and final death of a relative. I returned last night and now send you copies of a schedule on Bareness amongst sheep, which the Royal Agricultural Society

has printed + undertaken to send out to all their members interested in sheep, as soon as possible.

At present the Roy. Agricul. Soc. is fully engaged with business connected with the show in Manchester, but as soon as they are able to attend to outside matters I shall endeavour to get from them the names of various sheep societies and hope to be able to interest some members of these in the question.

I hope the schedule meets with your approval, it was drawn up after conference with several practical breeders and with Ernest Clarke

Yours faithfully

Walter Heape

WALTER HEAPE, Esq., M.A.,

HEYROUN,

CHAUCER ROAD,

CAMBRIDGE.

f.196

Royal Agricultural Society of England.

13, HANOVER SQUARE,
LONDON, W.,
June, 1897.

BARRENNESS AMONGST SHEEP.

DEAR SIR,

The co-operation of the Royal Agricultural Society has been sought by a Committee of the Royal Society in an inquiry now being made upon the question of Sterility in animals; and it is proposed, as a first step, to make an investigation as to the causes and extent of barrenness amongst sheep.

The collection of the necessary information on this point has been entrusted by the Royal Society to Mr. WALTER HEAPE, M.A., of Heyroun, Chaucer Road, Cambridge; and the Royal Agricultural Society will be obliged if you will favour Mr. Heape with the results of your own experience on such of the matters referred to in the accompanying Schedule as are within your knowledge.

Duplicate forms are enclosed, in the hope that you may be willing to hand them to other flockmasters in your district, whose experience will be likely to be of value for the purposes of the investigation.

Thanking you in anticipation for any information with which you may favour Mr. Heape (to whom the Schedule should be returned, and any further correspondence addressed),

I am,
Yours faithfully,

Ernest Clarke
Secretary.

$\left(\frac{2000}{6/97}\right)$.

[P.T.O.

INQUIRY INTO BARRENNESS AMONGST SHEEP.

NOTE.—It is not expected that every flock master will be able to answer all the following questions; but it is requested that as many as possible may be answered, and this paper then returned to WALTER HEAPE, Esq., M.A., Heyroun, Chaucer Road, Cambridge.

QUESTIONS. ANSWERS.

RAMS.

What breed of rams do you keep? ... ...

How many rams were used for tupping last autumn? ... ... age

age

age

What was the condition of the rams when tupping began? ... ...

Did you give the rams artificial food during tupping time, and if so, what food? ... ...

How long were the rams kept with the ewes? ...

EWES.

What breed of ewes do you keep? ... ...

How many breeding ewes did you put with the rams last autumn? ... ...

How many of these were shearling ewes? ... ...

How many of these were two shear ewes? ...

How many of these were three shear ewes? ...

How many of these were full mouthed ewes? ...

How many of these were ewe lambs? ... ...

FEEDING.

What was the flock fed on from April to May last year? ... ...

What was the flock fed on from May to July last year? ... ...

What was the flock fed on just before tupping time last autumn? ... ...

What was the condition of the ewes at tupping time? Were they poor or fat? ... ...

What was the flock fed on between tupping time and lambing time? ... ...

What was the flock fed on a month before lambing?

What was the condition of the ewes at the lambing season? ... ...

BREEDING.

What is your practice as to tupping, and how did you divide your flock? ... ...

How many ewes were there in each flock at tupping time? ... ...

How many rams were there in each flock at tupping time? ... ...

Did many ewes go over? ... ...

What was the total number of lambs born this season? ... ...

How many of these were ewe lambs? ... ...

How many of these were ram lambs? ... ...

How many of the ewes had twins? ... ...

How many of the ewes had triplets? ... ...

How many cases of abortion *were seen?* ... ...

How many ewes proved barren? ... ...

What were the ages of the barren ewes? ... ...

Did you notice that all or many of the barren ewes were served by a particular ram? ...

Did you put young rams with old ewes, or old rams with young ewes, or, if not, how did you apportion them? ... ...

Did you find that most of the twins were got by lamb rams or by particular rams? ... ...

Did you notice that most of the twins were born early or late? ... ...

What proportion of barren ewes do you usually get? ... ...

What is the largest proportion of barren ewes you have *known* of? ... ...

Signature ______________________

Address ______________________

____

Date ______________ 1897.

WALTER HEAPE, Esq., M.A.,

HEYROUN,

CHAUCER ROAD,

CAMBRIDGE.

Royal Agricultural Society of England.

13, Hanover Square,
London, W.,

June, 1897.

BARRENNESS AMONGST SHEEP.

Dear Sir,

The co-operation of the Royal Agricultural Society has been sought by a Committee of the Royal Society in an inquiry now being made upon the question of Sterility in animals; and it is proposed, as a first step, to make an investigation as to the causes and extent of barrenness amongst sheep.

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I am,

Yours faithfully,

Ernest Clarke

Secretary.

(2000/6.97).

[P.T.O.

f.20v

INQUIRY INTO BARRENNESS AMONGST SHEEP.

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QUESTIONS. ANSWERS.

RAMS.

What breed of rams do you keep? ... ...

How [many rams were used for tupping last autumn? ... ... age

age

age

What was the condition of the rams when tupping began? ... ...

Did you give the rams artificial food during tupping time, and if so, what food? ... ...

How long were the rams kept with the ewes? ...

EWES.

What breed of ewes do you keep? ... ...

How many breeding ewes did you put with the rams last autumn? ... ...

How many of these were shearling ewes? ... ...

How many of these were two shear ewes? ...

How many of these were three shear ewes? ...

How many of these were full mouthed ewes? ...

How many of these were ewe lambs? ... ...

FEEDING.

What was the flock fed on from April to May last year? ... ...

What was the flock fed on from May to July last year? ... ...

What was the flock fed on just before tupping time last autumn? ... ...

What was the condition of the ewes at tupping time? Were they poor or fat? ... ...

What was the flock fed on between tupping time and lambing time? ... ...

What was the flock fed on a month before lambing?

What was the condition of the ewes at the lambing season? ... ...

BREEDING.

What is your practice as to tupping, and how did you divide your flock? ... ...

How many ewes were there in each flock at tupping time? ... ...

How many rams were there in each flock at tupping time? ... ...

Did many ewes go over? ... ...

What was the total number of lambs born this season? ... ...

How many of these were ewe lambs? ... ...

How many of these were ram lambs? ... ...

How many of the ewes had twins? ... ...

How many of the ewes had triplets? ... ...

How many cases of abortion *were seen?* ... ...

How many ewes proved barren? ... ...

What were the ages of the barren ewes? ... ...

Did you notice that all or many of the barren ewes were served by a particular ram? ...

Did you put young rams with old ewes, or old rams with young ewes, or, if not, how did you apportion them? ... ...

Did you find that most of the twins were got by lamb rams or by particular rams? ... ...

Did you notice that most of the twins were born early or late? ... ...

What proportion of barren ewes do you usually get? ... ...

What is the largest proportion of barren ewes you have *known* of? ... ...

Signature ______________________

Address ______________________

____

Date ______________ 1897.

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

28. June 97.

Dear Mr Galton,

I have tonight returned home and wish to acknowledge the receipt of the corrected Schedule together with your "note in Explanation". I will shew both to Mr Clarke on his return from his holiday.

Yours faithfully

Walter Heape.

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE. 29 June 97.

Dear Mr Galton,

With reference to our conversation some days ago, I understood you to say that the Schedule on Sterility in Sheep had evidently been hatching some time, that you knew nothing about it, that the whole proceeding was very irregular and that you were bound to add that if it recurred again you would feel obliged to resign the chairmanship of the committee.

I gather from these remarks that you feel I have, for some reason or other, attempted to do what I had no right to attempt.

If I have misunderstood you I beg you will correct me.

I am anxious clearly to understand what it is you complain of, and

f.22v

I do not understand now.

Yours faithfully

Walter Heape.

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

4. July. 97.

Dear Mr. Galton,

I have been away from home and unable until now to reply to your letter of June 30 – I am glad to learn from it that you frankly accepted my explanation and that you were prepared to with-draw your remarks; I certainly did not understand you were prepared to do so, for, after I had given you the explanation, you said that if it occurred again you would feel obliged to resign the chairmanship of the committee – I think perhaps the explanation of the whole matter may probably be found in the fact that I failed to understand at our interview that you supposed I had designedly attempted to do what I had no right to attempt.

This is not the case, I had no intention whatever of issuing the circular without submitting it to you, if I had done so I should have done wrong, but as it was not so (I thought I had clearly expressed as much, and do not know in what way the explanation was deficient in assurance and clearness) I did not understand why you made the remark quoted above and felt you were not justified in making it – There is another point in your letter which requires mention, namely that I put a new aspect upon the

matter by saying that even if I had acted as supposed I did not see ~~that~~ the great irregularity, or that it would have justified you in writing as you did. I have not a clear remembrance of making the remark nor of what led up to it, though I do not doubt you have quoted me correctly; I would merely point out that the remark referred to my supposed action, and, as I have already pointed out, I did not at

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

the time fully understand what you supposed I had done.

If, as I gather from your letter you rightly interpreted my remark as upholding the view that I was justified in issuing the circular without first submitting it to you, then I was undoubtedly wrong in making it; I can only say that I find it hard to believe the remark was made in such connection for I do not uphold that view.

With regard to the circular itself I see by your "notes in explanation" which accompany the corrected copy, that you think the enquiry is one which lies very close to the limits of the province of enquiry of the Committee of the Royal Soc. I discussed with you the advisability of making this enquiry some time ago, in fact it was one of the first propositions I made, and I understood you fully agreed with me, if however you have

f.24v

changed your views I am quite
willing to withdraw the schedule
altogether,

Yours faithfully

Walter Heape.

f.25

JY 5 '87

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Francis Galton Esq F.R.S.
42. Rutland Gate
London. S.W.

f.26r

AMBRIDGE
7.30PM
JY 6
97

Francis Galton Esq F.R.S.
42. Rutland Gate
London S.W.

f.26v

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

6 Feb. 97.

Clarke is away shall be advised of his return

W. Heape.

12 July 97.

Dear Mr Galton.

I have seen Mr Clarke today & have shown him your suggested alterations in his draft letter to the members of his Society; with your explanatory notes thereon.

He quite approves the alterations made which he regards as distinct improvements, and he proposes therefore now to have copies of the circular as altered struck off & circulated as soon as the business in his office will permit.

yours faithfully

Walter Heape

Heyroun
Chaucer Rd
Cambridge.

(1)

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

7 Oct. 97.

Dear Mr Galton,

In reply to your note — I have, as yet, had no conferences with breeders — you will probably remember I strongly urged the advisability of delaying any efforts in that direction until the results of our efforts with the Sheep Schedules were seen. I have received something over 400 Schedules more or less filled up.

f.28r

(2)

and as soon as I can get the services of a clerk (I have one in my mind) I propose to tabulate the results, and I think several interesting points will be brought out.

~~The~~ Schedules still come in, but slowly now, and there is no reason why the tabulation should not begin at once, as far as they are concerned. I shall be much obliged to you if you will criticise the following scheme for tabulation, and

(3)

advise me of any improvements
which strike you.

I do not see how the number of
columns can be reduced, so many
factors appear to be concerned.

The age of the Rams & of the Ewes
The condition of do. & do.
The food given to both Rams & Ewes
The district in which the farm is situate
and the severity or otherwise of the season
all seem to be important.

~~That~~ Then abortion is a very serious
factor and I fear we shall find it a
serious stumbling block — from what

(4)

NEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

I can learn there is very great
difficulty in determining whether or
not a Ewe which does not give birth
to a lamb, has aborted a lamb or
failed to conceive.

Some of my correspondents are
strongly of opinion that the question
of abortion is a very much more
important question ~~than~~ than
barrenness — and I should not be
surprised if it turned out that the
abortion question leads to some
interesting inferences and to some
interesting work.

f.29r

5

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

The percentage of so called barren Ewes varies from 1% or even under that figure ~~and~~ to 40% or even more, and I do not know at all what the average will be – I have little doubt the average will vary perceptibly for different breeds, and I think also for different districts.

The percentage of barren Ewes returned, is certainly greater than the percentage of Ewes which have aborted, but as f.29v (6

I have pointed out above it is by no means clear that so called barren Ewes have not aborted!

Many correspondents consider the condition of the Rams an important factor influencing barrenness: others think the weather is more important.

It may interest you to know that in the opinion of the majority, twins are born _Early_ in the season. I have not included this point in the enclosed scheme for a summary, it will be easy to work that point out separately.

Yours faithfully

Walter Heape.

WALTER HEAPE, Esq., M.A.,

HEYROUN,

CHAUCER ROAD,

CAMBRIDGE.

f.30r

Royal Agricultural Society of England.

13, Hanover Square,
London, W.,
July, 1897.

BARRENNESS AMONGST SHEEP.

Dear Sir,

The co-operation of the Royal Agricultural Society has been sought by a Committee of the Royal Society in an inquiry now being made upon questions connected with breeding.

It is proposed, as a first step, to make an investigation as to the causes and extent of Barrenness amongst Sheep; and the collection of the necessary information on this point has been entrusted by the Committee to Mr. Walter Heape, M.A., of Heyroun, Chaucer Road, Cambridge.

The Royal Agricultural Society will be obliged if you will favour Mr. Heape with the results of your own experience on such of the matters referred to in the accompanying Schedule as are within your knowledge.

Duplicate forms are enclosed, in the hope that you may be willing to hand them to other flockmasters in your district, whose experience will be likely to be of value for the purposes of the investigation.

Thanking you in anticipation for any information with which you may favour Mr. Heape (to whom the Schedule should be returned, and any further correspondence addressed),

I am,
Yours faithfully,

Ernest Clarke
Secretary.

(500/7/97).

[P.T.O.

f30v

INQUIRY INTO BARRENNESS AMONGST SHEEP.

NOTE.—It is not expected that every flock master will be able to answer all the following questions; but it is requested that as many as possible may be answered, and this paper then returned to WALTER HEAPE, Esq., M.A., Heyroun, Chaucer Road, Cambridge.

QUESTIONS.	ANSWERS.
RAMS.	
What breed of rams do you keep?	
How many rams were used for tupping last autumn?	age age age
What was the condition of the rams when tupping began?	
Did you give the rams artificial food during tupping time, and if so, what food?	
How long were the rams kept with the ewes? ...	
EWES.	
What breed of ewes do you keep?	
How many breeding ewes did you put with the rams last autumn?	
How many of these were shearling ewes?	
How many of these were two shear ewes? ...	
How many of these were three shear ewes? ...	
How many of these were full mouthed ewes? ...	
How many of these were ewe lambs?	
FEEDING.	
What was the flock fed on from April to May last year?	
What was the flock fed on from May to July last year?	
What was the flock fed on just before tupping time last autumn?	
What was the condition of the ewes at tupping time? Were they poor or fat?	
What was the flock fed on between tupping time and lambing time?	
What was the flock fed on a month before lambing?	
What was the condition of the ewes at the lambing season?	

BREEDING.

What is your practice as to tupping, and how did you divide your flock?	
How many ewes were there in each flock at tupping time?	
How many rams were there in each flock at tupping time?	
Did many ewes go over?	
What was the total number of lambs born this season?	
How many of these were ewe lambs?	
How many of these were ram lambs?	
How many of the ewes had twins?	
How many of the ewes had triplets?	
How many cases of abortion *were seen*?	
How many ewes proved barren?	
What were the ages of the barren ewes?	
Did you notice that all or many of the barren ewes were served by a particular ram? ...	
Did you put young rams with old ewes, or old rams with young ewes, or, if not, how did you apportion them?	
Did you find that most of the twins were got by lamb rams or by particular rams?	
Did you notice that most of the twins were born early or late?	
What proportion of barren ewes do you usually get?	
What is the largest proportion of barren ewes you have *known* of?	

Signature ______________________

Address ______________________

____

Date ______________ 1897.

f.31

CAMBRIDGE
9.30 AM
OC 8
97

158

Francis Galton Esq F.R.S.
42. Rutland Gate
London. S.W.

Heape
Oct 8/97

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

13. Oct. 97.

Dear Mr Galton,

Thank you for your note. The Schedules sent to breeders contained no column for information on general health, but that point is largely satisfied by the replies regarding "condition" at tupping & at lambing time —
I don't know what happens to aborted lambs, but I think Early abortions might easily be overlooked even if they are not Eaten.
I propose then to get on with this as soon as I can get the requisite help

Yours faithfully

Walter Heape.

F32

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE. 10. Nov. 97.

Dear Mr. Galton,

I received yesterday a note from Weldon asking me to attend a meeting of the Evolution (animals & Plants) Committee. and asking whether we are prepared to make (1) a statement as to the work done by our sub-committee & (2) a definite request for a grant to cover expenses incurred and

about to be incurred.

I have drawn up a brief account of what has been done and added thereto my view regarding a grant - and enclose the same for your criticism & correction.

Will you please let me hear what you think of it.

I understand the meeting will be held on Tuesday, and as I leave home today until Saturday

I have thought it best to, at once, submit the enclosed to you rather than to write you first for your opinion — as the time is short —

With regard to the application for a grant of £20 — for travelling expenses for the future, I do not forget your generous offer of a like amount for the same purpose, but I estimate that when this part of the work is undertaken £40 will not be more than is required apart from Hotel expenses &c which I propose to find myself.

Yours faithfully

Walter Heape.

Francis Galton Esq

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE. 13. Nov. 97.

Dear Mr Galton,

I am much obliged to you for your suggestion. I did not know there was any idea of providing money for gratuities to herdsmen, and certainly am of opinion that in some cases it will be of advantage to be able to fee them.

Clerical helps I shall be obliged to have from time to time, and it would undoubtedly be convenient to have funds supplied me for that purpose.

With regard to the amount which would be likely to be wanted for these purposes, I find it difficult to estimate.

I should prefer to have a sum granted ~~for~~ "to assist in providing travelling expenses, small gratuities to herdsmen & clerical helps." —

collectively rather than a sum

for each purpose.

I will take an opportunity of speaking to you on the subject before the meeting.

Yours faithfully

Walter Heape.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

Heape

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

2 Jany. 98.

Dear Mr Galton,

It is with much pleasure I hear the winter has been kind to you and I hope the new year will allow you to hold on in London for another month or so.

Thank you for the circulars you send on Pedigree Records, I wish you success with them – if breeders could only be brought to know the

possible benefits to themselves which such work might bring, they would surely do more to help us.

They are difficult to move in this country perhaps in the new world they may be more susceptible.

The Sheep Statistics progress but slowly – I found it impossible to hand the work over to a clerk the schedules are not always accurate themselves, and there is much in some of them to be omitted, or condensed

f35v

BEYROUN,
CHAUCER ROAD,
CAMBRIDGE

I found I must do it myself and it takes longer than I thought it would take, all the more because I am a busy man just now in other directions which demand much of my time and attention; and I cannot settle down for a fortnight and finish it as I wish to do.

Faithfully yours

Walter Heape.

Heape

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE. 28 Jany. 98.

Dear Mr Galton,

I was sorry not to see you yesterday & for the cause which kept you away. I hope you are better.

I have now finished the first stage in tabulating the 400 schedules.

I enclose head lines of the large sheet used for this purpose. Each flock occupies one line

f36r

different sheets are used for different breeds.

I now want to begin summarising the results. and shall be very much obliged if you will tell me how you think this should be done.

Flocks of Ewes of particular breeds are not confined to particular districts or particular counties but there seems generally to be a majority of flocks of a particular breed in a particular county.

I should propose to summarise

(1) The flocks of each breed in each county

(2) The flocks of each breed in all counties.

(3) The flocks of all breeds in each county

(4) The flocks of all breeds in all counties.

do you think this is right to start with.

But with regard to the aborted & barren ewes, should I give the percentage of the total number of barren ewes? or the average percentage for each flock? or both? I mean of course for each of the four summaries noted above.

The usual % barren & the largest % barren being approximate cannot be treated in the same way can they? but the % bearing twins can.

I have been obliged to do all the work myself up to now for each schedule required abstracting to some extent, but now I can put a clerk on to the summarising work as far as figures are concerned and I hope soon to have the job finished.

Yours faithfully

Walter Heape.

Breed of Rams ____________________

Breed of Ewes ____________________

SHEEP SCHEDULES

No. of Schedule	RAMS							EWES						BREEDING															PHYSICAL CONDITIONS	
	Number and Age						1. Condition at tupping time. 2. Artificial food given	Number and Age						Lambs			Ewes								Feeding of Ewes				1. County 2. District 3. &c.	1. Meteorological district 2. Rainfall. Oct. 96. Mar. 97 3. Weather. Oct. 96. Mar. 97
	Lambs	1 Shear	2 Shear	3 Shear	Older	Total		Lambs	1 Shear	2 Shear	3 Shear	Older	Total	Ram Lambs	Ewe Lambs	Total	Ewes per flock	Rams per flock	% short	% barren	usual % known	largest % known	% bearing twins	Time twins born	Food before tupping	Condition at tupping time	Food during gestation	Condition at lambing time		

SHEEP SCHEDULES

		Breeding																Physical Conditions		Remarks
		Lambs			Ewes								Feeding of Ewes							
Older	Total	Ram Lambs	Ewe Lambs	Total	Ewes per flock	Rams per flock	% short	% barren	usual % barren	largest % known	% bearing twins	Time twins born	Food before tupping	Condition at tupping time	Food during gestation	Condition at lambing time	1. County 2. District 3. &c.	1. Meteorological district 2. Rainfall. Oct. 96. Mar. 97 3. Weather. Oct. 96. Mar. 97		REMARKS

Heape (1)

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE. 15. Feb. 98

Dear Mr Galton,

I am sorry to hear you have had such a troublesome attack, and that I cannot have the benefit of your advice and help in the Sheep report.

The point that especially troubles me is the following —

Take the flocks of a special breed in one district or county, the percentage of Ewes which abort or are barren in all these flocks, differs from the average experience of the

2

f.38r

several flock masters —

By "average experience" I mean the percentage of ewes aborting or barren in each flock added together and divided by the number of flocks —

It seems to me that while the actual percentage of one breed will give the best data for comparison with other breeds, the "average experience" will give a truer idea of the loss farmers sustain in this respect.

If you could, without troubling yourself too much, express an opinion on this point I should be much obliged to you.

3

When my report is finished and I can go about armed with results I intend to pay a round or several rounds of visits to flock-masters, and will then make enquiries on the lines you mention in your letter, and let you know the result.

The variety of ailments due to prolonged inter breeding — (or peculiarities) with especial regard to constitution and deformities — is what I gather you want to know.

I anticipate much difficulty in getting

4

useful information on such a matter because, with rare exceptions, any particular breed of sheep has a large range and careful flockmasters are continually importing rams or ewes from other districts to overcome defects as soon as they show any signs of appearing — I mean by that, that they are so clever in discovering any defect that it is overcome before it has established itself —

For instance among flockmasters who breed for the market, the slightest

5

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

indication in a flock that the saddle or the leg is getting reduced in size, is at once overcome by selection of breeding stock from show yards.

Such wide interbreeding as is usual with sheep would, I conclude, not give you such information as would be of use to you - Am I right in this?

The Kent or Romney marsh breed is however practically confined to one district, & so also are the Dorset Horn sheep; they might be worth making enquiry about.

6

Then again the Long Horn cattle are gradually but surely dying out I am told - They mature slowly and breeders for the market prefer to use other breeds.

Clarke tells me that at the coming Birmingham Royal Agricultural show, they are offering special inducements to attract Long Horns, and that one is unlikely ever again to see such a good selection: possibly one might be able to get information there or from some of the exhibitors.

Yours faithfully
Walter Heape.

(over)

7

f.39v

P.S. I shall be glad to know what you wish me to do about the Sheep report, shall I go on with it and present it in your absence, or shall I await your return.

Heape

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

9. June. 98.

Dear Mr Galton,

The report on Sheep has taken me much longer than I expected it would. I have not been in working trim all the time you have been away, in fact I have been laid up a good deal this spring, but apart from that and apart from the fact that I have had a good deal of business to transact, I have not got on with the Report as quickly as I expected. It is developing more than I had any idea it would, and I have hope it will not be altogether useless, but it takes an unconscionable long time – I dont believe I shall finish it before the end of July if I am able to give it undivided attention until then, which is not probable.

It is difficult in a letter to give a résumé of the results, but I may say roughly I find the Suffolk breed gives the best returns of fertility (over 140 lambs per 100 ewes on an average) and nearly the ~~to~~ lowest returns of loss from abortion and barrenness – 4.62% – I have been to see several Suffolk flock masters and have got good information from some of them, one or two being conspicuously intelligent men – I think ~~some~~ some suggestions I have been able to make regarding cause of abortion will be well worth further investigation. The Hampshire have the lowest loss from abortion & barrenness 4.06% but are not satisfactory from a fertility point of view – while the South down returns sent to me, seem to suggest the need for very close attention on the part of flock masters

9 June/98

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

as the average loss per abortion & barrenness is 7.94% and an average of only ~~109~~ 109.35 lambs are got per 100 ewes – there is marked irregularity in South down flocks both as regards fertility, abortion, and barrenness; and I am inclined to think it is quite possible there is a tendency to too close interbreeding in these flocks. But I have not seen any South down breeders yet, (i.e. since I have written the report on that breed,

I must do so.

The Lincolns show the worst returns of all, they have an ~~average~~ average of 12% abortion & barrenness, and I must certainly spend some time amongst Lincoln flockmasters.

My plans are to give all available time to this Report until it is finished and to visit as many flockmasters as I can. At ~~pres~~ present I have money, but if I go to see all the men I want to see I shall be obliged to spend more than the £50 granted

f.41v

of the Royal Society.

I shall hope to see you shortly at an Evolut. Committ. meeting.

Yours faithfully
Walter Heape.

Heape

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE. 8. Nov. 98.

Dear Mr Galton,

I am just finishing my Report on Sheep.—
It is a somewhat diffuse production and is designed for the more intelligent farmer's reading, essentially. Each breed is treated separately (or rather each breed for which I have sufficient data is so treated) the information and figures given me are analysed as well as my ignorance of such things admits, and the many & various opinions of flockmasters, which I have collected, are discussed.—

I propose to publish this in the Royal Agricultural Society Journal —

—

With regard to the Evolution Comm. I enclose draft of a letter, ~~which~~ for your criticism & correction, which I would suggest should be presented by the Sub-committee.

The Report referred to therein and which I suggest should be appended to the letter, I will send you in a few days, if the scheme meets with your approval.

Faithfully yours
Walter Heape.

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE

Heape

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE.

19. Nov. 98.

Dear Mr Galton,

I enclose letter and abstract of Report as promised, the latter is long I am afraid but I see no way of making it shorter without making it at the same time unintelligible; if there is anything in the former with which you do not agree, please alter it as you would wish and return it to me.

I may mention that I shall be in London until the 24th Nov. I expect, up to the morning of which date a letter will find me at The Savile Club – 107. Piccadilly.

Faithfully yours

Walter Heape.

Heape

HEYROUN,
CHAUCER ROAD,
CAMBRIDGE. 26. Mar. 98.

Dear Mr. Galton,

In reply to your letter, the paper I sent you was not written for publication, it is an abstract of about 600 pages of MS. & perhaps 100 tables and was designed to give the committee some idea of what had been done. If the Com: are disposed to victimize themselves for 1/2 an hour someday

I am willing to read it to them & explain what is not clear.

I quite agree with you that the inquiry (at any rate in its present stage) is not one which is appropriate for publication by the Royal Soc: I have never proposed this; and what will be the fate of the MS. I really do not know at present.

With regard to the letter, as you have objection to sign it & prefer to make

f44v

a verbal communication to the committee by all means do so.

Yours faithfully

Walter Heape.

(Sir J Hooker) f.1r

Private

Dec 6/96

The Camp,
Sunningdale.

My dear Galton

Your communication to me introduces two big subjects, neither of which am I prepared to discuss, for want of fuller information.

Theoretically the ~~Down~~ Down project takes my fancy, (I use the word advisedly, because there is unquestionably some sentiment in the proposal.) Two obvious difficulties are apparent; distance of the position from a railway; & far too large a house & offices.

Then with regard both to the Farm & the Down Garden, may it

that day & I am now dependent on the weather for permission to travel in winter.

Ever sincerely yrs

J D Hooker.

f.1v

not be that the whistles are too costly; & could not the object be attained ~~as of either of the~~ by ~~experiments~~ utilizing for either or both purposes existing institutions.

Could not the biological farm be attached to Harpenden, & the experiments of Down be performed at Kew (where an excellent physiological laboratory under a first rate man (Scott) is doing admirable work. — or if preferred at the Horticultural Society's Garden. —

Mr. Foster spoke to me on the subject of Down, & I have promised to see Mr Darwin about it:

The affairs of ~~both~~ each institution in the hands of controlling bodies, & the difficulties in the way of getting persons with sufficient leisure to control increase yearly.

I should doubt the Secretary to either "holding another appointment", consistently with his duties to Garden or Farm; in both cases a large correspondence will soon be developed, & of a nature requiring knowledge & tact to carry it on.

Of the importance of the object there can be no question, nor of the desirability of at once seeking the means of carrying them out.

I much regret that I cannot attend the meeting on Friday, but I have a previous engagement for

(R. Lankester)

f.1r

40 Nottingham Place
W.
Jan. 26th

Dear Mr Galton -

I am thoroughly interested in your plan for an experimental farm - but I can not attend the meetings of a Committee having - as I have - to spend four nights or more a week at Oxford. I am obliged

f. 1v

to undertake a limited number of such duties & I am full as it is. For instance next week I have on Tues. Univ. of London — on Wed. Marine Biolog. Assocn. on Thurs. Royal Society evening meeting — so that I shall have to stay 3 days in London.

Believe me, with best wishes Sincerely yours

E. Ray Lankester.

Sir John Lawes

f.1r

TELEGRAPHS,
HARPENDEN.

Rothamsted,
St. Albans.

December 14 96

Dear Mr Galton

I have always taken a great interest in your work, and shall have much pleasure in assisting you in any way. I had just sent an article on Telegony when your letter reached me. It must be a great many years ago since I answered some questions

for you relating to
my Families. I am
disposed to think that
the Brain is the great
distinctive cause in your
statement in connection
with the human race. People
would not give such
very large sums for the
small animals unless
they could depend (like)
producing like.

Believe me
yours truly
J. B. Lawes

TELEGRAPHS, HARPENDEN.

Rothamsted,
St Albans.

January 31 97

Dear Mr Galton

I am afraid that I cannot offer you any promising assistance as quite independent of 7 grant of £100,000 which has passed into the hands of the Nation I still spend a considerable sum each year upon Scientific work. Although not exactly in your direction still we have carried out very extensive experiments with animals

f.2v

and in some respects
this place would offer
considerable advantage for
carrying out investigations
such as you propose.
How far the Trust Committee
would entertain an idea
of this sort I am unable
to say. I am rather
amused at your Postscript
respecting Rats breeding
freely with out common
sorts, the Hanoverian
Rat has bred so freely
as to exterminate the
old English Rat.
This house which is
probably four or five hundred

years old had when I
was young a certain
number of the old
english Rat in it
but I have not heard of
one being seen for some
years

Yours truly

J. B. Lawes

(E. J. Lowe)

f.15

1897 Jany 9th

SHIRENEWTON HALL,
CHEPSTOW,
MON.

My Dear Sir

You seem not to recognize that I am the same who was the Secretary of the Nottingham meeting of the British association or that I had met you at Kingston Hall. In 1880 I bought the place that I [illegible] now live at from Lord Kenshire & moved from Nottingham.

I am very glad to hear of your proposed investigations & shall be very glad to cooperate. I have been working at these subjects since 1837

communicating with, the best man to help amongst the agriculturalists would be Richard Stratton of the Duffrin near Newport, he is one of the council of the R. Agr. Soc. & a great breeder of cattle. I could get him to give his help. I may mention that I have been crossing cattle, & give you two examples. A black Berkshire boar served an ordinary white sow & the progeny was black & white, it was then served by a red Tamworth Boar & the progeny was red black & white although there was no black either in the sow or the Boar. A friend of mine

f. 1v

but have had difficulty in convincing specialists in 1867 I showed that I had crossed ferns (at the meeting of the British association) but there was only Professor Balfour that would listen to my statement. In 1883 I sent specimens to prove that I had crossed two species of Polystichum to the Linnean Society this paper was returned saying the botanists would not believe it & therefore it could not be printed. Two years later from some foreign observations it became an acknowledged fact. Then in 1890 I showed the fern conference meeting that I could impregnate a fern cell

with a number of spermatozoa & this was fought against by the specialists saying it upset all the notions of botanists which was that only one cell could be impregnated last year I found my case microscopically as the enclosed paper will show. More recently I have devoted my time to the connexion between animals & plants. It would be tooling to enter into this subject which shows me why a man & his wife copy each other, the observations are of a delicate nature & require a good deal of working up they I could tell you what I have done. I do not quite see what you are fixed

a great deal of care is required.
Metamorphists — justified the leafing ~~flowering~~ flowering &c of plants year after year without adopting the same individual plants & quote the oak before the ash one year & vice versa another, but if you picked these say a pair growing in the Keuper & another pair on the Bünter the Bünter plants will always be before those on the Keuper formation, but I shall exhaust your patience, the information thus gained may be of some use to you at all events you can have my assistance

Believe me
Yours sincerely
E. J. Lowe

E J Lowe Jan 19/97 f.2r

SHIRENEWTON HALL,
CHEPSTOW,
MON.

Mr Palmer Jenkins of Beachley has had a breed of Black Dorking Pigs, & for years has always bred black pigs one day he lent his boar to one of his tenants to serve a white sow soon after this this boar served his black sow but the piggery was black & white for the first time, there being no white in either of the parents. I could quote you cows, horses, dogs &c.

I may mention an instance of what I did with Swans, but I could not persuade Professor Huxley that it

f.2v

was correct though I subsequently did the same with a fowl. Previous to this, foxes had destroyed all my swans but one & for two years I had only a female this had made a nest & laid three eggs when I heard of a male in Dolphin I went over at once & brought the male back with me & turned it up with the female, coition took place at once & next morning at 11 am a fourth egg was laid & afterwards two others. I had marked the eggs previously laid & found afterwards they were all sterile whilst the other three produced cygnets. Professor Huxley said that another male must have visited the female but this was an impossibility — there were no males in the neighbourhood & if one had had come it must have been seen & further it would not have left the female, it would surely be a delay for impregnation through the calcareous shell as through it when only membranous. I remember years ago Prof. Gwyn Jeffreys that what he called Limax agrestis could not be of the same genus with Limax marginatus because coition with one instance of lasted 40 seconds whilst with the other they remained fast together for many hours, but I have lived to see the one is called Amalia whilst the other remains Limax of animal & vegetable enquiries.

Believe me

Yours very truly

E. J. Lowe

E J Lowe

(see Stratton over enclosed)

f.3r

1897 Jan 7/13

SHIRENEWTON HALL,
CHEPSTOW,
MON.

My dear Sir

I have had a note from Mr Stratton which I enclose, he is a good authority on breeding & would be invaluable to you.

I ought to have said that I knew Charles Darwin & have always been a firm believer in him & indeed have proved his assertions. I also think that Mr Wallace has quite described Mr Darwins views.

My intimate connexion with ferns has shewn me that climate & circumstance

produce great changes but not
hereditary. In 1878 I moved 3 or 4
large Scolopendriums from Nottingham
all very distinct varieties, in the
heavy they were planted in a
dry windy situation & every one of
them lost their varietal character.
I was however certain that their
stunted normal growth when better
treated would give place to their
original varietal forms, & in
three or four years they had all
become luxuriant & showed their
original characters. In 1842 I
had a Polypodium vulgare var Cambricum
some 4 ft across (in a pot) I

divided this into two kept one
half in the pot & planted the
other half at the base of a new
red sandstone rock where there was
but little soil this gradually
degenerated until it became
a normal species, but I have
~~[illegible]~~ described all this in
my book on Fern Growing. Any
one accustomed to ferns will see
a very sensible difference with the
same species in different localities
of more or less favourable situations
I have had more than 50 years
experience of this & in other branches

f.3v

Stratton

f.4r

The Dufferin

Jany 12th

Dear Mr. Lowe -

If I can be of any use to the Committee of the Royal Society, I am at their service, but I am afraid they are too scientific for my capacity; I have

f.4v

heard yet from Mr. Galton.
This mild season
is welcome to us
farmers who are so
short of keep, a hard
winter would have
been a terrible
calamity - We are
comparatively free
from fog down here
but at my Hill Farm
yesterday it was
very dense

Yours very faithfully
R. Stratton

E J Lowe

f5r

1897 Feby 2

SHIRENEWTON HALL,
CHEPSTOW,
MON.

My dear Sir

I do not quite understand what animals are intended to be experimented upon, if cows & horses & sheep &c 20 acres I fear would be too small a space. I do not also know in what part of England you would propose for your farm, it has occurred to me that if it were anywhere near Lord Tredegar that he would enter thoroughly into it, as he would not

customer than to be means the interest of the Mortgage. I am in hopes of selling without loss but until then my hands are tied.

A good man to help us is The Hon. Charles Ellis who has a house in Piccadilly & an estate at Haslemere. Mr Thiselton Dyer & myself have just proposed him into the Linnean Society, another able man is Dr. Harrison of Fayland Lady Clifton (Chairman of the Clifton Zoological Society) he has been taking notes & making experiments on hybrids at the gardens for years.

Believe me
Yours very sincerely
E J Lowe

f.5v

hesitate to do anything to advance
a knowledge of agricultural animals;
he has a good breed of his own of
cows horses pigs &c & his tenant
Richard Stratton is equally good.

It also occurs to me that,
the study of plants would be a
quite different study though equally
important & I know no one
who would carry out instructions
so well as Miss Micklethwait
who passed first in the Horticultural
College at Swansea & received the
appointment of head gardener there,
she is still studying at the Royal
College of Science & can do far more
work than a man & do it better,

her father is a Monmouthshire
County Magistrate she has done a
lot of microscopical work for me
since she has been at Swanley.

I have a lot of observations
bearing on pangenesis which I
will tell you all about. Your
magnificent subscription compels
me to tell you my own position
at the present moment, though I
hope it will only be temporary. In
1880 I sold Highfield House for
£46000 leaving £36000 on mortgage,
the bad trade of Nottingham two years
ago made the purchaser desert
Highfield House & we had to take
repossession & until I have found another

E. J. Lowe

f.6r

1897 Feb. 6

SHIRENEWTON HALL,
CHEPSTOW,
MON.

My Dear Sir

I have seen Dr Hamin the President of the Clifton Zoological Society & he says he will be most pleased to help you in any manner you may suggest & will devote some of the cages to your enquiries. I am the Honorary Curator there where we have a fine collection of ferns &c. left to the Gardens by the late Colonel James for he

that he can give valuable aid & will be very glad to help and he has expressed to me how much more we require to know as regards the matter in the breeding of pedigree cattle, & how all have the master sterility in such enquiries, however you will see from some of my notes what I mean

Believe me
Yours very sincerely
E. J. Lowe

helped me in my experiments on farms. I have posted to you a copy of this book with my discoveries that was read to the Linnean Society from it you will see how slowly new ideas are acknowledged. For 50 years I could not get any one to believe that ferns could be crossed as there were proved laws that made this impossible nevertheless, I went on trying to convince Botanists that such will have to be altered. The short paper analysed in the book will show you that all I said 50 years ago is

f.6v

now acknowledged to be true & further I have shown explicitly that ~~I have ...~~ multiple parentage has also become a fact. I shall be glad to carry on any inquiries here that may be thought desirable, there are a number of notions that have no truth in them that tend to obscure these enquiries especially as regards animals. I will send you my note book of facts that I have proved during many years of enquiry that you may see what I have been doing. I shall see Mr Stratton this week, I know

[E J Lowe]

f.7

1897 Feb 10th

SHIRENEWTON HALL,
CHEPSTOW,
MON.

My Dear Sir

I send you one of my note books, to look at some curious facts it contains. I have a second smaller book, but have not laid my hands on it.

When you have done with it please return it, but I am in no hurry for it

Believe me
yours very truly
E. J. Lowe

(E. J. Lowe) Feb 7/97 f.8r

marked Private

SHIRENEWTON HALL,
CHEPSTOW,
MON.

Lady Londonderry gave Mrs J. L. Baldwin a large black Donkey (female) which was kept in a field with an alderney Bull she gave me this Donkey & said I might have a curious foal ~~—~~ as the Bull had been in coition with it. Several years ago my Son & Coachman saw this occur in a field near Raglan Castle between a bull & a Donkey.

There has been a ~~cow~~ woman quite recently that has had three retrievers perhaps but I do not get the worth details. Several years ago a woman near Badminton had a baby just like a newfoundland dog. Some years ago a girl in a cottage at Chilwell Nottingham was seen to entice a Newfoundland dog into the house this was watched until they caught the girl & dog in the act.

There is a widespread belief in Monmouthshire that if a woman lies on the left side until she has conceived the baby will be a boy

Please send me [illegible] (E. J. Lowe)

F.9r

1897 March 1st

SHIRENEWTON HALL,
CHEPSTOW,
MON.

My Dear Sir

I quite agree with the views of the committee, but there is one necessary point with regard to those who can & will assist both, they will require it to be stated what would be most useful to the committee for them to do. Stratton (Chairman of the Clifton Zoo) & Dr Harrison are two of these cases, Stratton is making some wonderful crosses with Kerry & Short Horns he has a herd of above a hundred Kerries & quite as many short horns he stands

f.9v

also my story in sheep. Dr Henson (of Clifton Zoo) tells me the Committee will give him unlimited power & place cages at your disposal; but both say what will the Committee want them to do. A friend of Dr Hensons in Derbyshire who he saw the other day as he went to visit an estate of his at Ambergate, would assist in pigeons & canaries, he is a well known exhibitor & has time on his hands. I am sending you another of my books tomorrow in it you will see a printed report of Strattons Cows &c

Believe Me
Yours very sincerely
E. J. Lowe

I cannot learn if anyone has studied the female liquor I mean that from the generative organs, except in differing from the male liquor in being acid nothing further seems to be known. It is rather a difficult subject & not easily obtainable but a better knowledge of it is very desirable for sexual reasons.

E. J. Lowe

f.10r

1897 March 3

SHIRENEWTON HALL,
CHEPSTOW,
MON.

My Dear Sir

Charles Bathurst Junr Esq

of Lydney Park, is also a great experimental farmer, he is quite young & has past at the top of the tree every agricultural branch of engineering at the College of Cirencester ~~science~~. He has to recommend him money & brains, & is devoting himself to experiments, & Colonel Curre tells me he is just the

f.10v

man to carry on careful experiments. Dr Gilbert FRS (or rather Sir Joseph Gilbert of Harpenden) will know all about him. I did not know till today that he had come to live at Lydney Park

Believe me
Yours very sincerely
G. J. Lorne

Lowe

answered Oct 26

F.11c

Octbr 25 1897

Shirenewton Hall,
Chepstow.

My Dear Sir

We propose a new feature at the B.A. meeting at Bristol that shall be interesting to the members & also shall form a fund for Biological research. It is to make the Clifton Zoological Gardens an entertainment for all members of the B.A. during their permanence in Bristol on showing their tickets and at the same time to have an exhibition of animals & plants of interest to Biologists, at the same time the general public will be admitted by payment & the profit divided, one half to the Biological

size but less milk, if the same holds good with animals as I have found in plants. We shall be able to exhibit these crosses as calves, we shall also be able to show experiments of insemination with plants, some of my experiments of this year are very interesting

Hoping you are well

Believe me

Yours very sincerely

E. J. Lowe

f. 11v

Committee of the Royal Society, & the other to the Committee of the Zoological Society for additional structures &c also for Biological enquiry so as to make Bristol one of the centres for these investigations. Garden Fetes were held at the time of the B A meeting at Nottingham in 1866 at Dundee in 1867 & at Bath in 1887 each being great successes. In addition it is suggested that the Clifton College & the Zoological Gardens, (which join) shall be illuminated, together with other attractions. Dr Harrison & myself have consented to manage the Fete & we feel confident that it will be a success. The Local Secretaries have asked me to obtain the moral support of Biologists in order to strengthen their hands with the Local Committee.

I shall be glad to receive a note from you in approval of our scientific scheme. At Nottingham Sir Joseph Hooker opened the Fete, & you will recollect how successful it was, but the present scheme is more scientific as we propose to show animals & plants of interest to Biologists. The Gardens will be open at 9 am each day as recreation for the ladies. I have read your last article with great interest. I am stating a number of experiments in plants. Stratton is crossing Kerry cows with a Shorthorn Bull & I have obtained a Dexter Bull to cross my Alderneys, & $\frac{1}{2}$ breds $\frac{3}{4}$ breds &c. I expect to improve the supply of milk as I feel confident that the increase of milk will come from the Bull although I shall diminish the size of the cows, Stratton will get more any work that I can do to help you will be done with pleasure.

TELEGONY.

SIR,—From many experiments and observations, spread over thirty years, I can confirm the statement of Dr. W. H. Webber in his interesting note in the BRITISH MEDICAL JOURNAL of October 10th. The lasting effects of coition in animals, both with male and female, are to my mind conclusive. I only select about half a dozen experiments, which I think speak for themselves.

1. A white sow was sired by a black Berkshire boar, and produced a litter of black and white pigs; this sow was next sired by a red Tamworth boar, and although there was no black in either of the parents, the progeny were red, black, and white, the patches of black being very conspicuous.

2. A black sow and boar (Duckering breed) had always bred their progeny black. The boar then sired a white sow for the first time; two months later it was sire of the original black sow, which then produced a litter of black and white pigs, although there was no white in either of the parents.

3. An Alderney bull sired a shorthorn cow, the calf being a half-bred Alderney. Afterwards, this the same cow was sired by a shorthorn bull, but still the calf was partly Alderney.

4. A smooth fox terrier was sired by a rough Scotch terrier, and had rough pups; it was then sired by a smooth fox terrier, but the pups were many of them rough coated, and none were like the parents.

5. A Manx tailless tom-cat was sire to an ordinary English cat, and a portion of the kittens had either no tails or very short ones. The tailless tom-cat died some years ago, but up to the present time a few tailless kittens are born.

6. A fair light-haired Englishman married a Brazilian lady, but had no children. Twenty years after he married a light-haired English lady, who subsequently had a dark-haired son that was more a Braziiian in appearance than English.

Numbers of different cases of cows, cats, pigs, rabbits, sheep, etc., might be added; but the few above examples will sufficiently illustrate this phenomenon.—I am, etc.,

Chepstow, Oct. 19th. E. J. LOWE, F.R.S.

British Medical Journal.

f.13r

Old papers of
the Evolution Cttee
of the R. Soc:
Of probably no present
value. Might be useful
if a Darwinian Institute
were ever founded.

f. 13v

I have not as yet found note book
No 2. but am hunting for it

S? J. Lane

see end of book for Stratton cows

(Sir J Lubbock f.1

Station, Orpington.

High Elms
Farnborough, R.S.O.
Kent.

11 Dec 96

My dear Galton

Many thanks for your note. I will read & think on the circular.

Yours very truly

John Lubbock

F. Galton Esq

FRS

Bonn, 13. II. 97.

f. 1

Hochgeehrter Herr College!

Das deutsche Original der „Dohrn"–Adresse habe ich direct von hier aus an alle Herren, auch in England, geschickt, die in der zoologischen Station zu Neapel gearbeitet haben.*) Dazu habe ich benützt 1) das von Friedländer herausgegebene, Ihnen bekannte Adressbuch, 2) das Verzeichniss der Practikanten der Station, welches in verschiedenen Jahrgängen der „Mittheilungen aus der zoologischen Station zu Neapel" publicirt ist. Insbesondere erinnere ich mich den deutschen Text an die umstehend genannten Herren geschickt zu haben. Es ist aber nicht von Bedeutung, wenn die eine oder andere Unterschrift zweimal an mich kommt — denn da die Namen alle in geographischer und alphabetischer Ordnung gedruckt werden sollen, so wird sich bei der Drucklegung jede Doublette eliminiren lassen.

In vorzüglicher Hochachtung

Ihr ganz ergebenster

Hubert Ludwig

*) auch an einige mir sonst näher bekannte Herren.

6 Brunswick Square,
Nov. 29/96

My dear Galton,

The only suggestion I have to make is that it might be unwise to saddle ourselves with a "paid scientific superior". If any worker is using the station for experiments at his own cost he would of course look after his own experiments. If the Committee is carrying on any special set of observations I take it that some

Member or Members of
the Committee would
hold themselves responsible
for that particular set
of observations. The
only reason why I suggest
this modification is on
the ground of expense.

An intelligent care-
-taker is all that is
wanted. A scientific expert
would want a salary of
about £300 – 400 per
annum.

Yours sincerely,

R. Meldola.

CHEMICAL SOCIETY,
BURLINGTON HOUSE,
LONDON.W.

Dec. 3/96

My dear Galton,

I find to my great regret that the meeting of the Council of the British Association has been summoned for to-morrow at the same time as your meeting which I shall therefore be prevented from attending. It is needless to say that the movement has my entire

f.2v

sympathy & that I shall be happy to co-operate as far as possible.

I do hope that whatever decision with respect to locality may be arrived at the botanists & zoologists will work together.

Yours sincerely,

R. Meldola.

f.3r

POST CARD

THE ADDRESS ONLY TO BE WRITTEN ON THIS SIDE.

LONDON W.C.
1 AM
DE 7
96

W.C
6

Francis Galton Esqre F.R.S.
42 Rutland Gate,
S.W.

f.3v

6 Brunswick Square,
Dec. 5/96

What do you think of "Committee for carrying on a Biometric Station"? This would include animals & plants. Or one might say "Zoometric & Phytometric observations" These are mere passing suggestions.

R. M.

Geneometric
Genemetric

f.4r

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HALF PENNY

LONDON W.C.
1. AM
DE 30
96

83

Francis Galton Esqre F.R.S.
42 Rutland Gate,
S. W.

f.4v

6 B. Sq. Dec. 7/96

I am a very poor authority on phil-
-ological matters but I think the
terms "phylometry", "phylometric"
&c. very good. I agree to all
the other suggestions in your letter.

R. M.

LONDON W.C. 4.15 AM DE 8 96 31

f.5r

POST CARD

THE ADDRESS ONLY TO BE WRITTEN ON THIS SIDE

HALF PENNY

LONDON W.C.
7.30PM
JA 13
93
27

Francis Galton Esqre F.R.S.,
42 Rutland Gate,
S. W.

f.5v

6 Brunswick Square,
Jan. 13/96

I am afraid I shall be too busy to attend meeting to-morrow, but will try. My address to Entom. Soc is on 20th at 8 p.m. I hope you will be able to come. "Utility of Specific Characters" is my subject.

R.M.

Meldola

Meldola (enclosure) f.6

6 Brunswick Square,
W.C. Jan. 17/97

My dear Galton,

The enclosed problems are all that suggest themselves just at present, but they are enough to keep an observational establishment going for some years – especially if other experiments are being carried on at the same time. I shall have much to say about No. 3 on Wednesday.

Yours sincerely,
R. Meldola.

57r

3. Physiological Correlation. Take ~~a~~ batches of eggs of a sp. known to be very variable. Bring up under different conditions (food, temperature, moisture). Find percentage of deaths before reaching maturity. (External selecting foes must be excluded) Compare the final mature progeny & see whether any particular variety is favoured by the respective conditions to which each batch has been exposed.

4. The question of sterility of hybrids might very well be tested by breeding experiments with moths. Very little has been done in a systematic way in this direction.

Meldola
Jan 18/97

F.7v

Suggestions for Experiments.

1. Per saltum development. Select well marked "sports" or aberrations of insects (several species could be named) & carry on breeding for many years in succession to ascertain:
   (a) Whether the aberrant form is inherited
   (b) If so whether the aberration persists, i.e. whether it is soon obliterated by intercrossing or whether on the other hand it can be converted into a race by selection.

2. Inheritance of acquired characters. Continuation of Poulton's work. Take a batch of eggs of some "colour sensitive" larva (many could be named) & bring up in groups among differently coloured surroundings. (Two extremes - brown & green would be sufficient). See whether the descendants from the two forms inherit any bias towards the green or brown form & if not whether such bias manifests itself after several years of breeding under same conditions.

Meldola
Feb 1

f8

I have nothing to add to this – it seems to cover the whole ground. I do not notice the names of Dyer, Lankester or Walsingham among Committee so presume they decline to serve.

R. Meldola

for Tues 29th
Athenaeum 3h —

J. G. Millais

f.1r

Dec. 22. 1896

LITTLETON HOUSE,
SHEPPERTON.

My dear Sir:-

I am exceedingly obliged for yours of last evening - Personally I am naturally most interested in the questions you put before me, as I breed about 100 dogs per annum & some 5 or six hundred fowls & keep records on all my breeding transactions -

f.1v

I was extremely sorry that I was unable to meet you at our mutual friend Hrape's last week, but I cannot get away from home just now.

I am in town however every day & would be glad to talk over your letter with you any day after the 28th.

I could meet you at the Athenaeum or any where you care to, as on matters of this question I think a few words are more than many letters.

Yours very truly
Everett Millais

Sir E Millais to
see separate parcel of Stud book; Lectures.

Jan 4. 1897

f.2r

LITTLETON HOUSE,
SHEPPERTON.

Dear Mr Galton –

Please keep both Studbook and Lecture –

I will not forget to send you a record of each hound in the Studbook, but I can only give colour there –

I will call upon you in Feb when I have more time than I have now & go thro' the questions with you.

f.2v

The reason why the names are not put alphabetically is because we go by the years & because it is necessary to keep the book in strict accordance with the Kennel Club Registry –

Yours truly

Everett Millais

UNIVERSITY COLLEGE BRISTOL

3rd Dec. 1896

Dear Mr. Galton,

I am so interested in the proposed Biological Farm for Experimental work and Exact observation that I should have made an effort to run up tomorrow had I not to attend a meeting of the Faculty of Arts and Science here in the afternoon at which my presence is necessary.

Whitman's scheme, when I talked the matter over with him, was still in the embryonic stage; and I have not heard from him as to its further development. I will write to him and send you any information he has to give.

At the Clifton Zoo — tell it not in Gath — they are more interested in fêtes & fireworks than in scientific research. There are however one or two men who might be disposed to cooperate in observational work — especially if it could be brought into touch with a definitely organized scheme.

I should be glad to serve on any executive Committee though my provincial position would I fear preclude frequent attendance at meetings.

I write in haste to catch this evening's post. Anything I can do to further the proposed important piece of Biological work I shall do con amore

Yours very truly

C. Lloyd Morgan

UNIVERSITY COLLEGE BRISTOL

Jan 28th 1897

Dear Mr. Galton,

I must apologize for delay in answering your letter. I partly waited for Prof. Whitman's reply. This I have now received but it gives no details and I gather that his farm is hanging fire for want of funds. He thinks that co-operation between the American and English workers would be most desirable & helpful.

Others will make suggestions as to work on telegony, and the inheritance (or not) of (enforced) acquired characters. For my own line of work I should be glad if experiments were made on the relative ranges of instinctive (congenital)

and acquired ~~&~~ activities (habit) by bringing up young under foster-parents and incubating eggs under alien nurses. The work would, however, require constant supervision and careful observation on the part of the person or persons in charge.

I should suppose that much may be done by utilizing existing establishments which would no doubt cooperate if an association for correlated work were in existence. And I am of opinion that some organization of work at existing centres should precede the renting of any new farm. After some time the felt ~~the~~ needs would afford guidance as to what new departures were called for.

Yours very truly

C. Lloyd Morgan

(Lloyd Morgan)

4th Feb 1897

Dear Mr Galton,

I wrote to the Superintendent of the Clifton Zoo. asking whether they would be willing to assist our Committee by placing say a small mammal cage and a large aviary cage at the disposal of any accredited investigator - at the same time stating that we should not make any vivisection experiments. I have just received his reply in which he says: "the Committee would have much pleasure in doing all they could to further the objects you have in view." They further suggest that I should confer with their Chairman in the matter. I hope if I can find time & a cooperator to utilize (if I can obtain definite permission) an aviary cage for further experiments with a view to determining whether nest-building is true to type in the absence of opportunities of imitation & perhaps on the inheritance of bird-song.

Yours very truly

C. Lloyd Morgan

f.4r

From the PRINCIPAL, UNIVERSITY COLLEGE, BRISTOL.

Feb 8th 1897

Dear Mr. Galton,

I'm afraid I cannot get away on the 11th. If you think well by all means add my name to the Committee: but whether on it or off I shall take great interest in its work and afford any help I can.

I am very proud to have you

f.4v

kind support in the other matter you mention

Yours very truly
C. Lloyd Morgan.

LITERARY & PHILOSOPHIC CLUB BRISTOL

BERKELEY SQUARE,
BRISTOL.

11th March 1897

Dear Prof. Weldon,

I purposely gave only a general outline of the proposed line of work. In an experimental enquiry of this kind one has to start on a provisional basis and look out for new openings and fresh developments as they present themselves in experience.

The observations of one of my students, J. S. Budgett, (now with Kerr in South America) which I reported in Habit & Instinct seem to show that greenfinches and bullfinches will build in confinement.

I propose with the assistance of Herbert Payne, an enthusiastic young ornithologist and one of the junior

f.5v

Master at Clifton College, to transfer Eggs from nests of these birds to cotton-wool 'nests' in my incubator. I shall probably have no difficulty in hatching them, and I hope to rear the young birds. If I succeed, I shall, when they are strong enough and old enough to fend for themselves, transfer them to the aviary cage in the Clifton Zoo. If they survive the winter, then in the Spring appropriate material from torn up nests will be supplied (scattered in the aviary). And if they build, their nests will be carefully compared by Mr. Ployne — who knows his birds' nests and has an admirable eye for such matters — with wild birds' nests.

It appears to me that if the nests are under such circumstances built true to type — as Budgett's observations seem to indicate — the truly instinctive nature of the activity in question will, for the birds observed, be established. If they are not true to type we shall have to consider how far this is due to lack of models, how far to the influence of captivity or other causes.

I shall be glad of any criticisms or suggestions.

Yours very truly

C. Lloyd Morgan.

f.1r

OXFORD
11.15AM
MR 11
97

603

Prof: Weldon FRS
30 A Wimpole St:
London. W.

f.1v

I cannot add anything to my previous note. I think there were sufficient details at this stage, in the letter itself or the paper referred to therein. I shall be able to suggest further lines when I recover some notes I have for the time mislaid. I shall hope to come to the Committee, but have been unlucky on previous occasions in having engagements here on your days. E.B. Poulton

Dec 17/96

f.1r

94, St Aldate's,
Oxford.

Mrs Romanes (Ethel)

Dear Mr Galton

I can't find much. I send a little note book & some of Mr Gulick's papers with a criticism of Mr Moulton & Leonard Hill. University College knows all about George's experiments & would be a good man

f.1v

for your purpose.
This is the first I've heard. Of course
George would wish to help, so where it's
a question of money let me hear.
I have lots of notes but chiefly on ~~things~~
well already made known.

Yours v. sincerely
E. Romanes

Jan 28/97 f.2r

My dear Mr Galton

94, St Aldate's,
Oxford

Please let me have a day or two to think over my finances.

Then don't you think you might apply to my brother in law?

James Romanes Esq
Dunskaith
N.B. Ross-shire

Mrs Romanes

See over

f.2v

Please don't say I suggested your applying to
him asking any particular but rather if I may
say so, say, you heard his interest in science &
or something like that! Also I think you might ask
my sister in law. She is at

Sance hills,

Bournemouth

Yours very sincerely
G. Romanes

See over

(Mrs Romanes) f.3 Feb. 3/97

Galton

94, St Aldate's,
Oxford.

Dear Mr Galton

Since consultation

my lawyer cousin & his

advice I can't promise

any large sum. Will

you despise £25 a

year for at least four

years?

Yours sincerely

E. Romanes

f.4r

POST CARD

THE ADDRESS ONLY TO BE WRITTEN ON THIS SIDE

HALFPENNY

OXFORD
1.15.PM
FE 13
97

603

To

Francis Galton Esq F.RS

42 Rutland Gate

London

S.W.

f.4v

Many thanks

94, St Aldate's,
Oxford.

E.R.

Did you get George's Essays?
Tell Longman to send them.

Mrs Romanes

LONDON-S.W
4.45 PM
FE 13
97

f.lr

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萬國郵便聯合端書

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EMPIRE DU JAPON

G. J. Romanes F.R.S.
Geanies
Ross-shire
Scotland

Great Britain

大日本帝國政府大藏省印刷局製造

Niigata Oct. 26, 1890 f.1v

Dear Dr. Romanes: I send by this mail, in registered package, Mr. Moulton's examination of my tables & my remarks on the same. I have no criticism to make on his calculations except that they are not opposed to my results, & help to emphatize the importance of Positive Segregation.

I am very glad to know the difficulties he has found in understanding my calculations, as it shows at what points I need to be more explicit in stating the conditions of my problem. If Mr. Moulton could help us to a more exact method by which we could determine the minimum degree of Positive & Negative Seg. that will preserve a variety from being entirely swamped it would be a great help. If he should take such data as I have given on pp. 266-7, & assign different values to the different factors — if, for example the vigour of hybrids was less than that of pure-breds by $\frac{1}{100}$ what would the result be?

Yours

J. T. Gulick

~~57 Onslow Gardens S.W. July 30th/1889~~ F2

(1)

Examination of Mr. Gulick's Tables.

I have at last found time to look carefully into the tables in Mr Gulicks paper & I am sorry to say that the comparatively favourable opinion I had formed of them has been displaced. I now think them quite worthless. In the first place the assumptions which he makes are so strangely out of harmony with anything that one can conceive as occurring in nature that the results could have no practical value in assisting anyone to weigh rival hypo-theses, & in the second place even if his assumptions had been legitimate in the case with which he is dealing his results would have no value because the factors whose effects he is considering are not the main factors which are at work & it is needless to say that no reliable approximation can be made if the main factors are left out of con-sideration. For instance it may be very well to neglect the effect of the attraction of Jupiter in our early researches in the motion of the Moon & our doing so will not prevent the results being approximate & having considerable value because we are retaining the two main factors that establish the motion viz: the effect of the Earth & the Sun. But if we exclude the effect of one of these main factors our results would be worthless & would not be rendered substantially less so by the fact that we had taken Jupiter into account in arriving at them.

I will now give you the result of my examination of the tables. They are all based on table 3. Table ~~5~~ 6 is a development of table 3 arrived at by taking into consideration the relative vigour of the breeds. All the other tables are mere deductions from these tables, numerical & otherwise.

If then table 3 is of no value all the others fall with it. To see the fallacy of table 3 it is convenient to take table 2 which is identical with it excepting in its notation, that is to say "R" takes the place of "1 − c".

He takes "R" to represent the ratio of pure breeding, that is to say the proportion of individuals who make pure unions to the total number of individuals in the race. Similarly he takes "c" to be the ratio of those who make cross unions to the total number of individuals in the race. He is therefore right in saying that

$$R + c = 1$$

But R and c will of course vary from time to time with the proportion of the one breed & the other which are found in the race at any time. This seems to me to be so self evident that it needs no demonstration. The proportion of individuals of one breed who will make cross unions will __caeteris paribus__ depend on the proportion of individuals of the other type to the individuals of their own type. Therefore the multipliers R and c in each generation will depend on the constitution of that generation i.e. on the results of crossing etc. in the generation before it. But Mr Gulick has made the extraordinary mistake of taking R and c to be constant, In other words he has assumed that the proportion of pure bred individuals who make pure unions is independent of the proportion which the pure bred individuals bear to the total race. This is of course an impossible supposition on any hypothesis of segregation. It corresponds to no form or circumstance of positive segregation that I can conceive of or that he describes. And yet it is intended by him to represent the results of positive segregation & not of that negative segregation which arises out of your hypothesis of selective fertility. It would be equally inapplicable to the latter

?

Tables 2 & 3 being thus displaced table 1 follows as a matter of course because it is a mere numerical instance of them. Table 4 also goes as it is only the working out of a special case, & table 5 is only a numerical instance of Table 4.

I now come to table 6. This purports to take into consideration the vigour of the various unions. But although it purports so to do it is still based on tables 2 & 3 & has the same radical defect. This defect is not in the least affected by the consideration of the new factor & thus this table is just as erroneous & valueless as those that have preceded it. A similar remark applies to table 7 & table 8 which are only deductions from table 6. I hope I have made clear to you the fundamental error that runs through all these tables. It is quite incurable & as these tables amount merely to calculating the results of this impossible hypothesis it is quite impossible to get any good whatever from them. I will presently give you some calculations of my own relative to your hypothesis which I think will stand criticism & which support it in a very remarkable way. But I wish to deal fully with Mr Gulick's tables first.

And now I come to my second objection to these tables. Even supposing that they were not open to the fundamental objection with which I have dealt it would be very difficult to draw any reliable conclusions from them. For instance take table 1 you will see that it purports to deal with a case in which in 18 generations a race becomes 40.000 times more numerous than it initially was. This of course can scarcely –

correspond to any condition in which natural selection can apply for if the environment was such as to permit so great ~~such~~ a multiplication there is little doubt that all types would be perpetuated & there would be no selection. The more I consider the matter the more I feel that it is impossible to decide as to the sufficiency of selective fertility to explain the formation of species if we merely consider the effect it would have on the numbers of the race as contrasted with ~~to~~ what they would be if no such peculiarity developed itself. Indeed I may say that on pondering over the matter I have come to the conclusion that mere fertility is probably a comparatively unimportant factor in the preservation of the species after a certain sufficient degree of fertility is attained. I do not wish to be misunderstood. To a certain point fertility is not only advantageous but necessary in order to secure survival but I feel that but little reliance can be placed on calculations of the numerical coefficients of fertility (i.e. the ratio of the number of offspring to the number of parents) in determining the relative chance of survival.

Take for instance the oak tree. It produces of course thousands of acorns almost the whole of which die without producing any progeny. Have we any reason to believe that if the number of acorns borne by the tree were diminished

tenth perish?
by one ~~half~~ the race of oaks would ~~be raised~~.
It may of course be said that if all other things are equal the probabilities of survival must be increased by increased fertility of this kind but I feel convinced that when numerical fertility has attained so very high a point in circumstances in which actual increase of the race cannot take place to any substantial extent the numerical value of this fertility sinks down into a factor of the second or third order of importance, that is to say into the position of a factor whose effects are only to be considered when we have duly allowed for the full effects of all the main factors. Until we have done that we gain little or nothing in the way of accuracy of conclusion by taking into consideration these minor factors just as in the case of lunar motion to which I have referred.

You must not imagine however that I think it wholly profitless to see whether there would be any substantial effect on numerical fertility ~~where~~ were selective fertility to manifest itself. But if we want to derive any assistance from calculation it must be by applying it with a good deal more precision & definiteness than anything that Gulick — or still more, Wallace — shows. And in the first place it is useless to confuse the vegetable & animal kingdoms. In the former you have union unaffected by choice; in the latter, so far at all events as the higher animals are concerned you have sexual selection. In order to give you a specimen of what can safely be done by calculation, if you take a problem of sufficient definiteness, I have chosen the case of a flowering plant in which a certain proportion of the race have developed the peculiarity of being sterile with the remainder ~~but possessing~~ while retaining the normal fertility of the race in unions among themselves. In order to give the greatest advantage to your critics, I have assumed that such flowers as possess the peculiarity are sterile to their own pollen; for it is clear that if we suppose that they are fertilisable by the pollen derived from the individual flower itself the fertility would be very slightly affected.

As I have excluded self fertilization it is necessary that one should consider the mode in which fertilization will be produced if we are to get any reliable results. I have taken the case of fertilization by insects, & have assumed that each flower is visited a certain number of times by insects during the period when fertilization is possible; & further, that the insects that visit it have on the average visited a certain number of flowers of the same species before they came there. Of course nothing but observation can fix these latter numbers, but I should not be surprised at finding that they are of considerable magnitude. In order to make the result a little more intelligible, I have grouped it under the numbers which represent the average number of flowers that an insect visits in a journey. This is a little more than twice as great as the number which represents the number of flowers he has on the average visited before coming to the individual whose fertility we are considering.

I send you the formula & the calculation on which it is based in an appendix; but as I think you have a holy horror of algebraical formulae, I give you here a few numerical results.

The cases I have worked out are those in which the number of insects visiting each flower is 5 or 10 or 15; & I have also taken 5, 10 & 15 to represent the number of flowers which an insect visits in a journey. This makes 9 cases in all, & I have applied these to two instances viz: one in which $\frac{1}{5}$ of the whole race have developed the peculiarity, & the other in which $\frac{1}{10}$ only have done so. Taking first the instance where $\frac{1}{5}$ have developed the peculiarity, I find that if on the average five insects visit a flower, & each insect on the average visits five flowers on a journey, the fertility is diminished by about $\frac{1}{10}$. If however the average number of flowers the insect visits is 10, the reduction of fertility is less than ~~$\frac{1}{10}$ &~~ one per cent. (and) it becomes inappreciable if the average number is 15. If on the average 10 insects visit each flower, then, if each insect visits on the average 5 flowers on a journey, the reduction of fertility is a little over ~~1/10~~ one per cent but if it visits 10 or 15, the reduction is inappreciable. If 15 insects visit the flower on an average, then if these insects on the average visit 5 or more flowers on a journey, the reduction of fertility is inappreciable.

By the term inappreciable I mean that it is not substantially greater than $\frac{1}{10}$ of ~~1%~~ one per cent — i.e. not more than one-thousandth.

is $\frac{1}{2}$

Of course if the proportion of individuals acquiring the peculiarity is less, the effect on the fertility under the above hypothesis will be greater; & it will not be counteracted so fully — unless the number of insect visits is larger, or unless the insects visit more flowers on a journey. Thus if only $\frac{1}{10}$ of the race have developed the peculiarity, then, if each flower is visited on the average by 5 insects who visit 5 flowers on each trip, the fertility will be reduced about $\frac{1}{4}$. If, however, the insects visit on the average 10 flowers per trip, it will be only diminished about $\frac{1}{10}$; & if they visit 15 on each trip, it will only be diminished about $\frac{1}{40}$. If in the same case we suppose that each flower receives 10 insect visits, then, if the insects visit on an average 5 flowers per trip, the fertility will be diminished about $\frac{1}{8}$. If they visit 10 on a trip, it will be diminished about $\frac{1}{100}$; & the diminution is inappreciable if they visit 15 on a trip.

Similarly, if a flower receives 15 insect visits, the diminution is about $\frac{1}{35}$; if insects visit on the average 5 flowers on a trip; & is inappreciable if they visit 10 or 15.

These figures will show you that it is exceedingly possible for a peculiarity like this, the effect of which at first sight would seem to be so prejudicial to fertility, may in fact have little or no influence upon it, & if you set against this the overwhelming importance of such a peculiarity in segregating the type so as to give it a chance of becoming a fixed species you will I think feel that ~~his~~ your hypothesis ~~bears examination very well~~ has nothing to fear from a numerical examination.

I have not examined into the case of fertilization by other means, nor have I examined the case of fertilisation in animals, where sexual selection can come in. To obtain any useful results one would have to consider very carefully the circumstances of each case; & at present, at all events, I do not think it would be useful to do so. Nor have I attempted to show the converse of the problem—viz: the effect of swamping where cross fertilization is possible. I shall be very glad to examine any one of these cases if you want me to do so but I should prefer to leave it until I hear from you again.

If you contrast the results that I have given above with those given on pages 181 to 183 of Wallace's book you will see the enormous difference. His calculations ~~could~~ can only apply to the animal kingdom in those cases in which there ~~was~~ is only a union between one individual of each sex; & before you can deal with the question of such animals you will have to take into consideration many elements besides that of mere fertility ~~before~~ if you ~~can~~ wish to get any tolerably accurate result. :X:

Fletcher Moulton

[:X: Here follows the Appendix presenting the ~~of~~ calculations on which the above results are founded; ~~of G. J. R.~~ but it seems unnecessary to reproduce it on the present occasion. — G. J. R.]

(1

Remarks on Mr. Moulton's Reasonings & Calculations.
By John T. Gulick

Mr. Moulton's estimate of the influence of Physiological Selection on the formation of species is quite different from Mr. Wallace's, & in complete accord with results which Prof. Romanes & myself have independently reached by somewhat different roads. I also notice that the conditions & means by which union is secured between compatible forms (in other words the different kinds of Positive Segregation), are regarded by him as among the main factors; while of the chief form of Negative Segregation he says; "mere numerical fertility is probably a comparatively unimportant factor in the preservation of the species, after a certain sufficient degree of fertility is attained (p. 5.)". This is of similar import with my statement that "Divergent evolution always depends on some degree of Positive Segregation, but not always on Negative Segregation". (Divergent Evol. p. 270). This statement, however, needs to be accompanied f.14 (2

by that which I make on the next page, which is that - "We must believe that in the formation of some, if not many, species, the decisive event with which permanent divergence of allied forms commences is the intervention of Segregate Fecandity or Vigour between these forms". (p. 271).

His calculations on the influence of "selective fertility" relate exclusively to success & failure in propagating as affected by complete Segregate Potency & Fertility when associated with different degrees of Efficiency in the free distribution of the ferteligizing element, & not at all to the influence of varying degrees of Cross Infertility & Impotence, or to the principle by which these & other segregative characters are accumulated in successive generations. In the tables of my paper I was dealing especially with this last problem, the right answer to which seems to me to be of special importance for bionomic theory. The importance of Potential & Prepotential Segregation, and its dependence on the free distribution of the fertilizing elements, I had considered in a previous section; but I had not subjected the effects of different degrees of free distribution of compatible elements, when commingled in different proportions with incompatible elements, to mathematical computation. That Moulton's calculations relate to the effects of varing degrees of one of the conditions on which Potential Segregation depends, will be seen by comparing the assumptions with which he starts with what I have said on pages 239–245 of my paper on "Divergent evolution." Especially observe the following passages:—

"Potential Segregation & Prepotential Segregation are caused by more or less free distribution of the fertilizing element together with the greater rapidity & power with which the sexual elements of the same species, race, or individual combine, as contrasted with &c. the rapidity and power with which the elements of different species, races, or individuals combine. xxx When pollen from a contrasted genus has no more effect than inorganic dust, it seems appropriate that we should call the result Potential rather than Prepotential Segregation. xxx For the operation of this principle the fertilizing element from different males must be brought to the same female." (p. 241).

There is one curious fact that needs to be kept in mind. Though the inferior potency of cross

unions is a different principle from the inferior fertility of the same, complete impotence + complete sterility cannot be distinguished. If alien pollen is impotent, (that is, never fertilizes no matter how long the contact), cross unions are sterile; and if cross unions are sterile whatever the opportunity, it is because the alien pollen is impotent.
As Mr. Moulton assumes cross sterility, he also assumes cross impotence.
"When allied species of plants are promiscuously distributed over the same district + flowering at the same time, prepotency of this kind, [that is prepotency of the pollen of each species on the stigma of the same species], is one of the most direct + efficient causes of Segregate Breeding. The same must be true of varieties similarly distributed whenever this character begins to affect them. In the case, however, of dioecious plants, and of plants whose ovules are incapable of being impregnated by pollen from the same plant, no single plant can propagate the species. If, therefore, the individuals so varying as to be prepotent with each other are very few and are evenly distributed amongst a vast number of the original form, they will fail of being

segregated through failing to receive any of the prepotent pollen" (p. 239).

The proportion of failure under different conditions of distribution is the problem considered by Mr. Moulton, & as an answer to Mr. Wallace's calculations concerning the results of Physiological Selection it is very effective; for it shows that Mr. Wallace has overlooked certain important factors that are involved in the physiological incompatibility of organisms whose fertilizing elements are freely distributed. This is exactly in the line of part of my answer to Mr. Wallace.

What I have already said shows that Mr. Moulton's criticism of my tables is not altogether relevant. In further answer to his second point, I would say, it is quite true that the factors whose effects I was considering are not the factors that are ~~not~~ at work in determining the proportion of cross unions, for in my tables the ratio of cross unions & the ratio of cross infertility are the very things that are assumed as having been directly observed, or as having been reached by computations like Mr. Moulton's. Mr. Moulton assumes that complete cross-infertility is a constant character & calculates what the ratios of cross unions will be in a certain class of cases. I take the main factors of Physiological Selection, which are ratios of pure-breeding and ratior of cross-infertility, & calculate what ratio between Proe-breeds and Half-breeds will be produced by any given degree of these factors. In other words, starting with these factors as my data, I calculate the combined efficiency of different degrees of these factors in preserving the race from the swamping effect of free crossing. This is a question of the greatest interest, for if the variations that possess in a superior degree the characters producing pure unions & cross infertility are the ones that are most fully represented by offspring in succeeding generations then we have a principle by which these factors, in so far as they depend upon the endowments of the organism, may be indefinitly increased.

Mr. Houlton's first criticism is that my assumptions are so out of harmony with what occurs in nature, that the results have no practical value. My chief assumptions are that the variable ratios of pure-breeding & of cross infertility will combine in certain degrees long enough to produce their legitimate effects, and

that the forms that are thus segregated are equally fitted to the environment, & are therefore reduced by natural selection in equal ratios.
The theory of Physiological Selection maintains that, through equally fitted to the environment, the two varieties will become increasingly divergent; & in order to test this point, equal fitness is assumed for them in my calculations; for it was only in this way that I could estimate the effects of Segregation exclusive of the influence of different degrees of survival. This assumption may not have been stated with sufficient clearness; but when it is once seen that I am not dealing with the effects of natural selection, the difficulty that Mr. Moulton finds in my table I, (see bottom of p. 4). will be removed. I do not assume that all the individuals produced in each generation will come to maturity & produce, but that, in the case of divergent races that are equally adapted to the environment, and whose hybrids are equally adapted with the pure breeds, the effects of Natural selection will not change the proportions in which they exist. But the greatest misunderstanding on the part of Mr. Moulton is in supposing that I take R & C, that is the ratios of pure-breeding &

of cross-breeding, to be constant. (p. 3). The very center of my argument is that, so far as R (i.e. the ratio of pure-breeding) is dependent on the endowments of the two forms that are commingled, it is inherently variable until it has reached complete segregation, and that, so long as it is associated with cross infertility, both forms of segregative endowment will tend to increase. Table IV is the working out not of a special case as Mr. Moulton thinks, (see p. 3), but of 26 cases in which 11 different values are assumed for C.; & in table V we have 90 cases under 10 values for C. It is equally a mistake when he represents me as not recognizing that "The proportion of individuals of one breed who will make cross unions will depend on the proportion of the other type to the individuals of their own type. (p. 2-3). This is as if one should say of Mr. Moulton's calculations that they do not recognize that the proportion that are fertilized by pollen from the same kind does not depend on the degree of cross infertility. In my calculations I assume that the proportion of pure unions has been ascertained by previous investigation, just as Mr. Moulton assumes in his calculations that the degree of cross infertility has been -

ascertained to amount to complete sterility. As the final purpose of my investigation was to show the self-accumulation of segregative endowments, (see p. 260), I assumed in the numerical statement on which my algebraical statement was framed that the two varieties were commingled in equal numbers. Questions that might have been raised as to what the results would be if three, or four, or five varieties were brought together in equal numbers, or again, what the result if two varieties, one of which was twice as numerous, or 9 times as numerous as the other, were passed by, for their solution did not seem to be necessary for my purpose.

I do not, however, agree with Mr. Moulton that "it is an impossible supposition" that the degree of segregation should be for the most part independent of the proportions in which the segregated races exist, (see p. 8). Indeed Mr. Moulton himself seems to have recognized, (on page 7) that "if we suppose that they are fertilizable by the pollen derived from the individual flower itself, the fertility would be very slightly affected" by the other factors, the effects of which he is calculating.

For the purposes of my investigation is it any more unreasonable for me to assume the ratios of cross breeding, & of cross infertility, as given than it is for Mr. Moulton to assume in his calculations that a <u>certain proportion</u> of the race have acquired the peculiarity of being <u>sterile</u>, (that is <u>completely infertile</u>), with the remainder, & that each flower is visited a <u>certain number</u> of times by insects that have on the average visited a <u>certain number</u> of flowers of the same species before they come. If a student should go to Mr. Moulton's calculations with a view to ascertain the conditions on which the accumulation of cross infertility depends, he would find that complete sterility is assumed as a constant quantity, & would discard the whole as worthless — (for his purpose).
Mr. Moulton may, however, say that his assumed factors are sufficiently constant for a single generation to allow of their being treated as constant, & the computation is complete for that generation if the data have been correctly stated, whereas my assumptions have to extend over many generations in order to avail anything. There is some truth in this contrast, but it is not

near so great as at first appears. The result given by my method in the first or second generation is not the result that would actually be found in any generation, under the conditions of gradual change to which species are usually subjected in a state of nature. The formula for the nth generation is the nearest approximation that my method affords for any generation. In the conditions of natural species there seldom exists any first generation that is strongly contrasted, in the degree of pure breeding + cross-infertility, with the immediately preceding generations. But even under the conditions assumed in my calculation, that is, when two species or varieties are brought together for the first time in equal numbers, it does not require very many generations for the result to approximate very closely to the formula. In my Table I, it will be seen that the figures given under the 10th generation lack less than $\frac{1}{2000}$ of this.

I am aware that my formula is only an approximation, + that, when the ratio of pure breeding is small + the degree of infertility slight, the multiplication of half-breeds + three-quarter-breeds will disturb the values with which the

calculation commences, unless the positive segregation is due to a certain proportion of the ovules being fertilized by pollen from the same plant, or to some other cause that is not disturbed by the multiplication of hybrids.
But even though it is only an approximation, I believe it is sufficient to show that, under some conditions, only a moderate degree of these segregative endowments are sufficient to preserve a race from being swamped, & to ensure the gradual self accumulation of the endowments.

~~Niigata~~, Japan. Feb. 17, 1891

Dear Mr. Romanes:

An explanation seems to be needed of how I can say that— "If cross-infertility, (that is, Segregate Fecundity), is not associated with some form of positive segregation, it will disappear," and at the same time maintain that Physiological Selection may completely segregate varieties occupying the same area, though no other form of segregation is helping in the process. The apparent inconsistency will be entirely removed if it is remembered that segregate fecundity is a narrower term than physiological selection, and that I have used cross-infertility as equivalent to segregate fecundity, while you use it as including not only segregate fecundity, but segregate vigour, and other forms of inferiority of hybrids, and, (what is of ~~still~~ greater importance), the further principle of Segregate Potence and Prepotence. This last principle is an "elective capacity," and, when residing in fertilizing elements that are widely distributed by wind, water, or other agencies, it produces f.25v (2

Potential and Prepotential Segregation, which is as effective in securing positive segregation, in the case of many plants and water animals, as sexual & social instincts are, in the case of birds & mammals.

When I say that Prepotential Segregation is, in some cases, dependent on Local, Germinal, and Floral Segregation, it is parallel with my statement that Sexual Segregation is more or less dependent on Local, Social, and Industrial Segregation. Every species that exists in considerable numbers is more or less affected by Local, (or Regional), segregations that are being continually broken down by migration and crossing; but if a local variety of a mammal becomes recognizably differentiated, sexual and social instincts may arise that will allow of their occupying the same area with other varieties without their crossing. In the same way, the representatives of a plant species occupying a partially isolated position, may become slightly prepotent with each other, and afterwards spread beyond their original home without crossing with the allied forms with which they are commingled.

Please compare what I have said on page 236 of my paper on Divergent Evolution with my statements on pages 270-1.

Not only do instinctive sexual affinities depend for their origin on incompatibilities of location or of industrial and social habits, but the segregative operation of these instincts depend on some degree of local association, bringing those that ~~that~~ are fitted for each other within sight, hearing or smell of each other. So also in the case of plant varieties that are prevented from crossing by Prepotential Segregation, it is necessary not only that the pollen of each should be prepotent with its own kind & that it should be freely distributed, but it must be produced in the same region as the pistils on which it is prepotent and this is usually secured by the ~~Local~~, Germinal, & Floral Segregations on which the breeding of the original stock depended before it divided itself into incompatible varieties. In other words, the new

Physiological Segregations are superimposed upon previously existing Segregations. Segregate Prepotence of even a very slight degree when strengthened by Segregate Fecundity is certainly quite sufficient to prevent swamping without crippling the powers of survival, in varieties occupying the same area, if the pollen of each variety is so freely distributed as to reach most of the germs of the same variety.

A comparison of my definitions with yours leads me to think that your Physiological Selection includes nearly all the principles I have classed under Impregnational Segregation. In my paper on "Divergent Evolution" I say - "In order that Impregnational Segregation should be established and perpetuated, it is necessary, 1st that variation should arise, from which it results that those of one kind are capable of producing vigorous and fertile offspring ‡ in greater numbers when breeding with other kinds; 2nd that mutually compatible forms should be so brought together as to secure propagation through a series of generations." ×

× See Linn. Soc. Jour. - Zool. Vol. XX. p. 238.

‡ The inferiority of hybrids is here referred to, as also in my computations relating to the effects of Segregate Fecundity & Vigour.

The last thirty four pages of the paper are devoted to the influence of impregnational forms of segregation in producing divergent evolution; and on pp 239-242, I point out that: "We must consider it [Potential and Prepotential Segregation] a form of Positive, as well as Negative Segregation; for the free distribution of the fertilizing element, with the superior affinity of the two sexual elements when produced by those that are mutually prepotential, secures the interbreeding of those that are mutually prepotential;" that: "The importance of this principle in producing and preserving the diversities of the vegetable kingdom can hardly be over-stated;" & that: "Amongst water animals that do not pair, the same principle of Segregation is probably of equal importance:" while on pages 259 & 260, I use my computations to show that when moderate degrees of Prepotential Segregation and Segregate Fecundity are possessed by the representatives of a race or variety both characteristics will be preserved & accumulated in successive generations.

As Prepotential Segregation and

Segregate Fecundity are forms of "racial incompatibility in the reproductive system" they are also forms of Physiological Selection as defined by you in your first paper on the subject. It is therefore evident that, from the first, physiological selection was recognized by me as sufficient, under certain conditions to prevent the crossing of two varieties occupying the same area, without destroying either. I also pointed out, that, when negative and positive forms of physiological segregation are both operative in even slight degrees, there will be a cumulative advance in the segregative qualities and showed some of the laws on which this advance depends. I believe no one else has treated of the latter point. I did not at first recognize the very close correspondence of our fundamental theory owing to the fact that with me cross-infertility was equivalent to Segregate Fecundity, while with you it included all physiological incompatibilities of the reproductive system.

John T. Gulick

Concession, Osaka, Japan.

P.S. I have just received "Nature" for Dec. 11, 1890, with your reply to Mr. Wallace. My paper on "The Preservation & Accumulation of Cross Infertility", had already appeared in the Dec. number of the Amer. Jour. of Sci.," and even if it had not appeared, there would have been no harm in your quoting the few sentences you did. I notice that you refer to me as, "the first to conceive, though the last to publish, the theory of physiological selection." My presentation of the subject in my paper on "Diversity of Evolution Under One Set of External Conditions", read before the British Association in 1872, was very brief, but I think about as clear as that by either Mr. Belt or Mr. Catchpool. I there use the following words which show that I recognized both physiological & psychological segregation as causes of continued divergence in varieties occupying the same area.— "If we would account for the difference and for the limited distribution of these allied forms on the hypothesis of Evolution from one original species, it seems to me necessary to suppose two conditions, both of which relate to the state of the species— namely, Separation and Variation. I regard Separation as a condition of the species and not of surrounding nature, because it is a state of division in the stock which does not necessarily imply any external barriers, or even the occupation of separate districts. This may be illustrated by the separation between the castes of India or between different genera occupying the same locality".×

In another passage I say of forms thus separated, "The separation may have been as complete and as long continued in the case of those which now inhabit one valley as in the case of those that are separated by the length of an island"‡

I would not refer to the subject, if I did not suppose that you are writing up the history of the theory for your forth-coming book. I have no means of knowing whether my paper had any influence in starting the minds of Mr. Belt and Mr. Catchpool on this line; but I know that the paper was translated and published in Germany and that the theory of "Separation" as it was called, (using the word which I had introduced), was maintained by Semper and other German naturalists, in

× Linn. Soc. Jour. – Zool. Vol XI. pp. 498-9
‡ Do. p. 505.

opposition to Moritz Wagner's theory of "Geographical Isolation through Migration" as a necessary condition for the divergence of initial species. The theory of Moritz Wagner as first published, required as a condition for the formation of a new species, not only isolation but exposure to a new environment. Semper intimates that, in papers that he published some years later, he expressed more pronounced views concerning the power of isolation; but I have no means of knowing whether this was subsequent to the publication of my paper on "Diversity of Evolution Under One Set of External Conditions" or previous to it. My acquaintance with Moritz Wagner's theory is through an English translation, published, in 1873, by Edward Stanford, 6 & 7, Charing Cross, London with the title "The Darwinian Theory and the Law of the Migration of Organisms" J. T. G.

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5-7 Onslow Square
S.W.
21st Nov. 1891

Dear Romanes

I have read Mr Gulick's criticism on my letter & I do not think that his reply to my objections to his Tables can stand examination. The best method of dealing with it is to express briefly my fundamental objection illustrating it by an instance taken from his tables & then it will be seen that his attempted defence fails.

I say that his Tables are of no value because he assumes that "the proportion of individuals who make pure unions is constant & does not depend on the proportion of the pure bred individuals to the total of the race.

To shew that this is so & that it is an inadmissible supposition one has only to look at Mr Gulick's Table 1. He begins there with 1,000 pure breeds of each of two varieties & assumes that under such circumstances $\frac{9}{10}$ will make pure unions. Accordingly in the second generation he has 200 half breeds & 1800 of each variety of pure breed. Now if you look at the line following the words "Explanation of Table 1." you will find that he assumes that the proportion of the half breeds that do not make mixed unions is $\frac{9}{10}$ which is the same as in the case of the two varieties of pure breeds with which he started. But these 200 half breeds are surrounded by eighteen times their number of individuals with all of whom they can form mixed unions whereas in the previous case the pure breeds of each variety had only an equal number of individuals round them with whom they could form mixed unions. It seems to me to be perfectly absurd to imagine that the proportion of mixed unions can be the same when (if I may use the expression) the temptation to mixed unions is so overwhelmingly greater in the one case than in the other. In other words the proportion of mixed unions (& therefore the correlative proportion of pure unions) must depend on the relative numbers in the several varieties that can intermingle.

If I have made my meaning clear I feel sure that you will agree with me in the conclusion that now I want to point out the fallacy in Mr Gulick's attempted answer. He defends his Tables by saying that they are based on the hypotheses that the proportion of mixed unions has been correctly observed & he seems to think that after saying that I have no right to object to his working out his Tables on that expressed assumption. But after saying this he has tacitly assumed that the proportion of mixed unions is a thing which is capable of being determined by observation i.e. that it is a constant quantity. If a quantity is not a constant no amount of observation will enable you to determine a constant value for it. & therefore the hypothesis that it has been

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observed to be such & such a constant is indmissible. And it must be remembered that in my previous letter I pointed out that my objection to the hypothesis was not based on this proportion not being accurately a constant but upon the fact that the supposition that it was even approximately constant was so little probable that in my opinion no good could be obtained for working out the consequence of so unlikely an assumption.

Yours very sincerely

J. Fletcher Moulton

Prof. Romanes
94 St Aldates
Oxford

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[*Extracted from the* LINNEAN SOCIETY'S JOURNAL—ZOOLOGY, vol. xxiii.]

INTENSIVE SEGREGATION,

OR

DIVERGENCE THROUGH INDEPENDENT TRANSFORMATION.

BY

REV. JOHN THOMAS GULICK.

Intensive Segregation, or Divergence through Independent Transformation. By Rev. JOHN THOMAS GULICK. (Communicated by W. PERCY SLADEN, F.L.S.)

[Read 19th December, 1889.]

IN a previous paper on "Divergent Evolution through Cumulative Segregation"* I have enumerated eighteen classes of natural causes which produce either Separate or Segregate Generation †, and which, in their combined action, tend to produce cumulative Segregation and divergent evolution in every part of the organic world. I have there shown, with sufficient fulness, that cumulative Segregation always produces cumulative divergence or polytypic evolution; but I have not fully shown how Separation from the first involves more or less Segregation, or how Segregation, that at first divides the species into sections with reference to some one endowment, is always tending toward intensified Segregation in which the sections present differences in regard to an increasing number of endowments.

After expounding the principles on which these laws of divergence rest, I will give a few examples of divergence, calling attention to the complete correspondence between the facts of nature and the principles expounded in this and the previous paper.

Separation always involves more or less Segregation, for no two portions of a species possess exactly the same average character. When a homogeneous species is divided into two large sections,

* Journ. Linn. Soc., Zool. vol. xx. pp. 189–274.

† Separate Generation, or Separation, is the indiscriminate division of a species into sections that do not intergenerate. Segregate Generation, or Segregation, is the Independent Generation of different sections of a species when the sections are composed of somewhat divergent classes of variations. Segregation differs from Selection in that the latter denotes the exclusion of certain kinds from opportunity to propagate, while the former denotes the division of those that propagate into classes that are prevented from intergenerating. I use intergenerate rather than interbreed that I may have a term equally applicable to plants and to animals. Independent Generation, or the prevention of intergeneration, whether it be through Separation or Segregation, I sometimes call Segeneration. Darwin used Isolation as equivalent to geographical separation, while later writers have sometimes used it as equivalent to Independent Generation. Following Darwin, I use it for distribution in different areas, especially when barriers intervene.

it may be difficult to prove by measurement that there is any difference in their average character; but on general principles we may assume that, at least in some points, there is a slight difference. It is evident that when the separated sections are small there is more likely to be *diversity* in the average character of the sections, and that, roughly stated, the probability of divergence from this cause will be in direct proportion to the variableness of the species, and in inverse proportion to the size of the different sections. When a few stragglers form a small colony in an isolated position there is the strongest reason to expect that they will not be able to propagate the characters of the species in exactly the same proportions in which they are produced by the main body of the species, or by any other small colony that is propagating independently; and when the original stock has been rendered highly variable by the crossing of somewhat divergent varieties, the degree of difference that will probably be presented by any two independent colonies will be correspondingly increased. We must bear in mind that, while specimens possessing an average character in any one respect are always abundant, those perfectly representing the average in every respect are rarely, if ever, found. Now, is it to be supposed that any one, or any small number of these imperfect representatives of a species will, if separated from the rest, transmit all the characteristics of that species in the exact proportions presented by the average character of the original stock?

Mr. Francis Galton has conclusively shown* that in the children of parents whose heights deviate from the average of the race to which they belong there will be a similar deviation amounting on the average to a certain fixed proportion of that presented by what he calls the mid parentage. The mid-filial deviation in the groups investigated by him was about two thirds of the mid-parental deviation. There is therefore a regression in the average character of the offspring toward the typical character of the group. It must be observed, however, that this law can hold in full force only where there is free crossing, otherwise no divergent race could ever be formed by any amount of selection and independent breeding.

* See "Types and their Inheritance," an address before the Section of Anthropology of the British Association in 1885; also 'Natural Inheritance,' p. 97.

Eight Principles of Monotypic Evolution.

Let us now consider how this initial Segregation, which is always present where migration or geological subsidence produces indiscriminate Separation, is enhanced and intensified by the cooperation of other principles, and how forms, segregated through possessing different characters in some one respect, come to diverge in other respects. For example, when differences of colour become the occasion for sexual and social Segregation, how does this open the way for divergent transformation in habits of feeding and in a thousand other respects? The principles co-operating with Independent Generation in producing this enhanced divergence are all causes of simple transformation, or monotypic evolution when there is free intergeneration. Divergent breeds of domestic animals have always been produced when the different sections of a species in the care of different races of men have been prevented from interbreeding, thus securing their Independent Transformation during the process of domestication. So in nature, when any form of Independent Generation has been established, any cause of transformation that may afterwards arise will always produce more or less divergent evolution, and never that which is in every respect parallel. But we must defer the discussion of this subject till we have enumerated the more manifest of the principles of monotypic evolution:—

1. *Assimilational Transformation*, or modification due to deficiency with economy, or redundance with profusion, of growth, resulting from different degrees of assimilative power. "Economy of growth" is a term already in use, but a term is needed that shall include both this and its opposite.

2. *Stimulational Transformation*, or modification produced by changed motions in the fluids of an organism responsive to changed molecular influences in the environment. Under this principle we may place the direct influences of light, heat, electricity, the dampness of the air or the saltness of the water in which the organism is bathed, the quality of the food, and all stimulation from physical and chemical causes, exclusive of those resulting in muscular activity or the movement of organs.

3. *Suetudinal Transformation*, or modification due to the effects of use, disuse, and habitual effort in producing motions, and in resisting the strain of gravity and other forces tending to produce motion. Suetude is not found in the dictionary, but I venture

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to use it as including both assuetude, which is being accustomed to, being practised in, habitual use,—and desuetude, which is disuse, discontinuance of practice. This principle has been recognized by most biologists, though it has recently been called in question by Weismann.

4. *Emotional Transformation.*—Dr. C. V. Riley, of the National Museum, Washington, has called attention to the influence of parental emotions, especially maternal emotions during the term of pregnancy, as a factor in evolution (Address "On the Causes of Variation," before the Section of Biology, American Association, August 1888; also in 'Popular Science Monthly,' vol. xxxiv. pp. 811–816).

5. The cumulative development of adaptations through "the survival of the fittest" when the fittest are other than average forms. This is the principle of *Unbalanced Selection* or of *Selectional Transformation.*

6. Transformation produced by the indiscriminate destruction of a portion of a species, with the accompanying probability that the remaining portion will not possess all the characters possessed by the species previous to the elimination. This principle I call *Unbalanced Elimination*, or *Eliminational Transformation.*

7. Transformation produced by different degrees of amalgamation of the varieties and races which have resulted from previous Segregations. In most species there is a constant process of amalgamation by which thousands of minor varieties are absorbed; but when the process proceeds beyond ordinary limits, and the barriers that have divided well-marked races give way, transformation must follow. This principle I call *Diversity of Amalgamation*, or *Amalgamational Transformation.*

8. The cumulative development of the more fertile of the forms that are equally adapted. In other words, transformation produced by diversity in the relative fertility of varieties that are equally adapted to the environment and the constitution of the species, or by change in the degrees of fertility possessed by the same variety at different times and in different places. This principle I call *Unbalanced Fecundity*, or *Fecundal Transformation.*

Of these principles, all, except the 6th, 7th, and 8th, have been more or less discussed by writers on biology, though some of the forms of Selection depending on the relations in which the members of a species stand to each other have never been

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pointed out, and many writers have failed to observe that natural selection often produces fixity of type instead of transformation, and that divergence in the kinds of natural selection depends on Segregation, and not necessarily on exposure to different environments.

Assimilational, Stimulational, Suetudinal, and Emotional Transformation belong to a class of principles that have sometimes been grouped under the term Variation, while Selectional, Eliminational, Amalgamational, and Fecundal Transformation may be classed as *principles of Unbalanced Propagation*. It should, however, be carefully noted that Variation usually indicates deviation from the average, an entirely different factor from those which relate to the change of the average itself. It may therefore be well to group these first four principles as *principles of Involution*. The principles of Unbalanced Propagation are abundantly established as genuine methods of change in the average inheritable characters of species, not only by experience derived from the domestication of plants and animals, but by observation of similar effects produced by natural processes. On the other hand, the principles of Involution, though very marked in their influence on individual character, cannot be easily tested as to their effects on the inheritable characters of species. Weismann maintains that acquired characters cannot be inherited. If this is so, there can be no involution of specific characters, and the only factors in monotypic evolution are the causes whose laws of action are expressed in the principles of Unbalanced Propagation.

I have not mentioned "Acceleration and Retardation" as principles of transformation, for they seem to be but phases of the law of Suetude; for, as explained by Cope, the former is the effect of Use or Effort in the parents, producing in the offspring accelerated inheritance, while the latter is due to Disuse or Cessation from Effort, producing in the offspring retarded inheritance *. So also Hyatt's "law of Concentration" (or "Acceleration," as he often calls it) seems to be a general law of inheritance relating to the transmission of characters originating under any and every principle, the effects, whether progressive or retrogressive, being inherited at earlier and earlier ages in each successive generation †. It is also doubtful whether Correlated

* 'Origin of the Fittest,' pp. 203-7, 228.

† Proceedings of the American Association,' vol. xxxii. pp. 352-361.

Transformation should be considered a separate principle, for it seems to be simply the inheritance by offspring of characters that have for many generations been united in the endowments of at least a portion of their ancestry, and the correlation of these endowments must have been produced through the action of other principles.

The prevalence of males in times of pressure, with the prevalence of females in times of plenty, is regarded by Dr. W. K. Brooks, of Johns Hopkins University, as a characteristic established by natural selection, by which the organism acquires variability or fixity of type according as either character is most needed; for according to his observations the males represent the former, and the females the latter element. There can be no doubt that in many species the males are more variable than the females, and that in some of the same species the proportion of males increases with the degree of adversity; but this does not seem to be sufficient ground for maintaining that the increase in the proportion of males will increase the variability of the offspring. Increase in the number or amount of the variable element does not necessarily involve increase in the variability of either element, or in the offspring of both. I find need of additional factors in order to bring these facts into any relation to the increase of variability. Granting that the sperm-cell is the source of variation and the germ-cell the source of fixity, and that increased tendency to variation in the offspring will be secured by an increased range of variation in the sperm-cells, it does not follow that increase in the relative number of males will increase the range of variation in the sperm-cells, and therefore in the offspring. But if conflict with the environment and the winnowing process of natural selection falls most heavily upon the males, there must be some advantage in having their relative numbers increased in times of adversity; and if the exposure of parents to hardships increases the variability of either male or female offspring, and especially if it increases the variability of both, plasticity will be increased.

Prof. Cope's "Doctrine of the Unspecialized" ('Origin of the Fittest,' pp. 232–5) rests on the fact that the most highly specialized types, as well as individuals, are most likely to be exterminated by extraordinary changes in the environment; and Mr. Hyatt's "Geratology" ('Proceedings of the American Association,' vol. xxxii. pp. 349, 360) teaches that types that are being slowly exterminated usually assume forms resembling those produced by

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old age and disease in the individual. These and other parallel laws in the growth and decay of types and individuals are of great interest, as they afford organic conditions under which the principles of transformation must act.

After considering certain general propositions that apply equally to all of the eight principles above enumerated, I shall consider more particularly what the effect of some of these principles is when cooperating with Independent Generation. The only principles I shall treat in this special way are the four principles of Unbalanced Propagation.

The Transformation of freely Intergenerating Organisms never Divergent.

I mention these eight principles of transformation, not with the purpose of entering upon a full discussion of the same, but simply to point out the relation in which they all stand to divergent, or polytypic, evolution. It is evident that whether acting separately or together, they can never be the cause of divergent evolution in organisms that are freely intergenerating; for in such a group of organisms whatever modifies one part of the group in characters that are inheritable will ere many generations modify the whole. If the group is exposed to a variety of inharmonious conditions, which with Independent Generation would produce divergent character, with free Intergeneration the only result will be variation. Without Segregation there can be no permanent divergence; and with Segregation there must be divergence; and with cumulative Segregation there must be cumulative divergence. This principle, which I call *Divergence through Segregation*, was the subject of my previous paper.

Independent Transformation never Parallel, but always Divergent.

If any species is divided into two or more sections that do not intergenerate and that are severally subject to highly complex transforming influences, it can only be by a series of coincidences which the reason refuses to receive as in the slightest degree probable that any two sections will be modified in exactly the same way. This high degree of probability, amounting to a certainty, that when causes of transformation cooperate with causes producing Separation or Segregation, the result in successive generations will be increasing degrees of Segregation and of divergence, is what I call the law of *Intensive Segregation*. The different forms of this principle, resting on the certainty that the cooperation of any one of the principles of transformation with any one of the principles of independent generation will produce increasing Segregation with increasing divergence, are the following:—

1. *Assimilational Intension*, or Segregation and Divergence through Independent Assimilation.
2. *Stimulational Intension*, or Segregation and Divergence through Independent Stimulation.
3. *Suetudinal Intension*, or Segregation and Divergence through Independent Suetude.
4. *Emotional Intension*, or Segregation and Divergence through Independent Emotional Transformation.
5. *Selectional Intension*, or Segregation and Divergence through Independent Selection.
6. *Eliminational Intension*, or Segregation and Divergence through Independent and indiscriminate Elimination.
7. *Amalgamational Intension*, or Segregation and Divergence through Independent Amalgamation.
8. *Fecundal Intension*, or Segregation and Divergence through Independent Fecundal Transformation.

In groups that do not intergenerate, divergent forces reveal themselves whenever transformation is introduced. If it were possible to believe that in any case the effects of Independent Selection or of Independent Suetude had been completely parallel, it would still be impossible to believe that both of these, together with the remaining six principles of transformation, would ever so combine as to produce completely parallel effects. It is a familiar fact that no two persons are exactly alike; and it is probably true that no two groups of any organism are exactly alike. Though we cannot fully explain the fact *we accept as a certainty the non-equivalence of biological quantities*; and consequently we assume with confidence that there cannot be completely parallel transformation in isolated sections of a species, even if all are surrounded by the same environment. This principle is not inconsistent with the production of what Prof. Hyatt calls "representative or parallel characteristics" in two or more divergent series of forms. What he points out is that, under the influence of heredity, similar organisms exposed to

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similar environments undergo similar transformation ('Anniversary Memoirs of the Boston Society of Natural History,' 1880; "The Genesis of the Tertiary Species of *Planorbis* at Steinheim," pp. 24–29).

In the description of these principles I have used the adjective "Independent" to signify that the principle is operating in sections of the species that are prevented from intergenerating. If Isolated Selection were used instead of Independent Selection, it would be constantly liable to be understood as meaning Selection acting upon sections produced simply by geographical separation; for Darwin never used Isolation to designate the prevention of free crossing in other ways. In the term "Independent Variation" Mr. Romanes has already used the adjective "*Independent*" as meaning "*when accompanied with the prevention of intercrossing*;" and as it is less likely to be misunderstood, I prefer it. Part of what Romanes indicates by "Independent Variation" is, I think, in my scheme distributed between the four principles of Assimilational, Eliminational, Amalgamational, and Fecundal Transformation when acting on independent groups. As these principles are quite distinct, the separate names will be a convenience. If there are other forms of transformation, the causes of which cannot be given, I would prefer to class them as due to unknown causes rather than attribute them to Variation, which, as there used, is only a name for unexplained transformation. I would not turn Variation from its usual meaning, which is deviation from the average character of an intergenerating group.

The Pervasive Influence of the Causes of Transformation, and the Law of Intension.

In my paper on "Divergent Evolution through Cumulative Segregation," p. 215, I made the statement that, "When Separate Generation is long continued, we have reason to believe, it always passes into Segregate Generation with divergent evolution." The same had been expressed in a previous paper by the statement that "Variation is so strong, that all that is necessary to secure a divergence of types is to prevent their intermingling"*. The certainty that Independent Generation with transformation will never produce parallel, but always more or

* 'Diversity of Evolution under one Set of External Conditions," Journ. Linn. Soc., Zool. vol. xi. p. 499.

less divergent evolution is *the law of Intensive Segregation* already referred to; but in addition to this certainty there is a very strong probability that where Independent Generation is long continued, transformation of some kind will supervene. If there are any species in which the power of cumulative variation has been entirely lost, this latter law cannot hold in their case; but it is doubtful whether among species that reproduce sexually there are many such. The variability of some species is so small, and the conditions of the environment are so constant, that comparatively long periods of Independent Generation pass before perceptible transformation arises. This seems to be the case with the 13- and 17-year races of *Cicada septemdecim*, to which I shall refer when giving examples from nature. From the high probability that long-continued Independent Generation will be followed by Independent Transformation, and the certainty that Independent Transformation will be divergent, there follows the corollary that long-continued Independent Generation will probably be attended by divergence. In other words, Independent Generation long continued is almost always attended by Independent Transformation; and Independent Transformation inevitably produces Divergence. This double principle I call *the law of Intension*. This law rests on the ubiquity of transforming influence, and on the impossibility that in a species possessing any plasticity the inherited effects in any section independently generating should be exactly the same as in any other section.

We cannot doubt that, when a diversity of powers and susceptibilities in the different sections is acted upon by a great variety of influences, the responses of the different sections will be unlike; and the result will be increasing segregation and increasing divergence. Now it is impossible to doubt that in species propagating sexually, and possessing some degree of plasticity, these are exactly the conditions whenever the species is divided into sections that do not intergenerate.

It should be observed that, in accordance with the principle of Intension, not only is indiscriminate Separate Generation when long continued transformed into more and more strongly Segregate Generation, but any form of Segregate Generation, resting on some one principle that causes the division of the species into sections differing in regard to some one form of endowment, will, if long continued, be inevitably reinforced and intensified by transformations, which, being independently combined and transmitted, will multiply the number of characteristics in regard to which divergence takes place. If, for example, the pollen of a given variety, when falling upon the stigma of the same variety or race, is impotent over the pollen of any other variety or race that falls upon the same stigma at the same time, or at a somewhat earlier time, what I call Prepotential Segregation will divide the species into two groups that are prevented, for the most part, from intergenerating; and these separate groups, gradually coming under the influence of different degrees, forms, and combinations of the transforming principles, will in time become strongly characterized species. It is not, however, necessary that all or any of these forms of transformation should cooperate with Segregation in order to produce a distinct species. The accumulated effects of Segregation, unaided by these principles of transformation, would be sufficient to produce well-defined species; but it is impossible that they should often remain unaided.

As the law of Intension is one of the most general of the laws relating to divergent evolution, it is not strange that the principles through which it is made evident are of a general nature. The marvel is that concerning so wide a law the evidence is so complete.

Utilitarian and Non-Utilitarian Divergence.

The principles of Suetude and Selection are directly related to the development of utilitarian characters; but the effects of the other six principles are often not only wanting in, but opposed to, utility. Assimilational Transformation includes redundance of growth, which is not always, as well as economy of growth, which is always, utilitarian. Some of the inherited effects of Stimulation and Emotion fortify the constitution against the destructive influences of the environment, while others leave the offspring more exposed than the parent. Unbalanced Elimination, Amalgamation, and Fecundity may be advantageous, useless, or disadvantageous. We have, therefore, in these six principles of transformation abundant cause for the introduction of non-utilitarian characters; and, when accompanied by Independent Generation, they must be the source of multitudes of non-utilitarian divergences. In the earlier stages of divergent evolution the non-utilitarian distinctions are more abundant; for in the later stages multitudes of them are weeded out by economy of growth (as has been clearly pointed out by Mr. Romanes *); and still others, through coming under new conditions in the environment or through some new habit of intelligence, become useful endowments, and are brought under the preserving and accumulating influence of Natural Selection or of Suetude. It should, however, be noted that the development of useful specific differences is as much due to Independent Generation as is the development of useless specific differences. Diversity of Suetude or of Selection does not produce divergent evolution unless it cooperates with Independent Generation.

Selectional Intension, or *Segregation and Divergence produced by Independent Selection.*

That we may gain a clear apprehension of the nature and influence of this principle, certain discriminations, which have not always been recognized by writers on the subject, are absolutely necessary; and, for the sake of avoiding misunderstandings, it is desirable that these distinctions should be represented by clearly defined terms. I am fully aware that many will be opposed to the introduction of new terms into the treatment of a subject that has been so long and so ably discussed. If these discriminations were not found necessary by the author of the 'Origin of Species,' or if the distinctions, so far as recognized by himself and others, have been expressed in the language of ordinary description, why should a more accurate terminology be needed now? In reply, it may be said that the freedom from technical language which is a great advantage in a work which for the first time calls attention of the world to a vast subject, is a serious defect when the exact relations of the subject come under discussion.

In order to secure clear thinking on the subject, I have found it necessary to keep the following distinctions constantly in mind:—

(1) The Selection that results in the transformation of species is not the selection of one species to the exclusion of another. The breeding of the horse to the exclusion of the ass modifies neither the one nor the other. It is the exclusive generation of certain variations of a single intergenerating group that gradually

* Physiological Selection," Journ. Linn. Soc., Zool. vol. xix. p. 383.

transforms the group. When, therefore, we speak of Selection as a cause of transformation, we refer to the Selection of the variations that are to interbreed and keep up the race, to the exclusion of other variations. In order to maintain the same distinction in the nomenclature of natural processes, what I call *Selection* is caused by the failure of certain forms of a species to perpetuate their kind as contrasted with the success of other forms. If the failure includes all the forms of a species, I call it the *Extinction* of that species, and class it as a cause of transformation in the remaining species only so far as it makes a change in their environment.

(2) The exclusive generation of certain forms of an intergenerating group does not necessarily result in transformation. Experiments in artificial breeding show that if we select only the typical representatives of a race, the general character of the race is not changed, though any tendency to fluctuating variation may be gradually diminished, and the stability of the type increased. When, however, one form of deviation from the mean is constantly selected without a counterbalancing selection of the opposite deviation, the transformation of the race is always the result. In other words, *Balanced Selection produces Stability of Type, and Unbalanced Selection produces Transformation of Type.*

In the light of this twofold law we see how there may be stringent Natural Selection without transforming effect. It has sometimes been maintained that the transformation of species through the Natural Selection of favoured races is a necessary process which must be operating in nearly every species; for in nearly every species there is a constant struggle between the different forms of variation; and as it never happens that all the forms are equally successful, the process of Natural Selection is always bearing in full force upon the species. If it could be shown that Natural Selection, wherever it exists, must necessarily produce transformation, it would be impossible to resist the conclusion that nearly every species is undergoing transformation through this cause. But it is Unbalanced, and never Balanced, Selection that produces transformation. We also see that heredity tends to make the most successful form the average form, and thus to convert Unbalanced into Balanced Selection. From this it follows that in order that Selection should pro-

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duce continuous transformation, it is necessary that the form of variation selected should from time to time be changed. This may be expressed as the law of *Continuous Transformation through Successive Changes in the Character of the Selection.*

Though Selection produces transformation only when it involves the survival of other than typical forms, it is still very possible that there are but few species in which completely Balanced Selection prevails for very many generations in succession. It is still certain that long-continued Independent Selection gradually passes into diversity of Selection producing divergent evolution.

(3) Though in more than one passage Darwin maintains that uniformity of external conditions involves uniformity of Natural Selection, and that isolation can have no effect in transforming a species if physical conditions and surrounding organisms remain the same, still, I think, that if the question had been distinctly brought before him, he would have admitted that exposure to a new or changed environment was not a necessary condition for change in the character of Sexual Selection. Now I think it can be shown that, besides Sexual Selection, there are several forms of Selection that depend upon the relations of the members of one species to each other, and that may undergo change without the organism being exposed to either a changed or a different environment.

Selection depending on the relations of the organism to the environment I call *Environal Selection*, of which I find two kinds, namely:—Natural Selection and Artificial Selection. Selection depending on the relations of the members of a species to each other I call *Reflexive Selection*, the chief forms of which I call Conjunctional, Dominational, and Institutional Selection.

(4) It must be carefully noted that Diversity of Selection depending on diversity in the relations of the organism to the environment, does not necessarily involve the exposure of the organism to different environments. In other words, change even in Environal Selection does not necessarily involve either change in the environment or the entrance of the species into a new environment. It may be due to a change in the methods of appropriating the resources of the environment, introduced by the organism without any change in the environment. Darwin's teaching seems, at times, to be in conflict with this statement, but there are passages in his writings which distinctly state that

variations in instinct may lead to different habits of sustentation, and it is evident that, as soon as the qualities that win success in the different sections differ, the Natural Selection must differ.

It should be remembered, however, that the meaning of anyone's statements on this subject will depend on his definitions of the words used. What is meant by environment, external conditions, and other similar terms? Until we define we shall only beat the air, however exact our statements may be. I therefore repeat what I have elsewhere stated, that, according to my definition, change in the environment is always change in activities that lie outside of the species, or of the segregated group of individuals that is under consideration. In Darwin's usage, the phrase "Change in external conditions" seems to carry the same meaning; but in some places this can hardly be the case, and accordingly great obscurity hangs over some of his statements on the most important subjects.

Diversity in the uses to which different sections of one species put their powers, when appropriating resources from the same environment, must produce diversity in the forms of variation that are most successful in the different sections. This I call *Active Natural Selection* as contrasted with *Passive Natural Selection*, which varies according to differences in the environment. All diversities of Natural Selection that do not vary according to differences in the environment must be classed as diversities of Active Natural Selection, for they must have originated in some variation in the powers of the organism, or in the diversity of uses to which it has put its powers. Diversity in the successful use of the powers of the species, whether initiated by diversity in the action of the species in its different sections, or by diversity in the activities of the different environments, necessarily introduces diversity of Natural Selection. This principle may be expressed as the *Dependence of Diversity of Adaptational Selection on Diversity of Successful Use.*

(5) Now diversity in the successful use of its powers in the different sections of a species cannot be maintained and accumulated without some degree of Segregation between the different sections, for within one intergenerating group every initial divergence is speedily merged in the general character of the group. This law may be briefly defined as the *Dependence of*

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Increasing Difference in the kinds of Adaptational Selection on the Continuance of Segeneration. As was shown in my paper on "Divergent Evolution through Cumulative Segregation," without the aid of causes preventing intercrossing the selection of other than average forms will produce *transformation*, but never *divergence*,—will produce Monotypic, but never Polytypic Evolution.

(6) Diversity in the character of the Selection may be introduced, not only by the intervention of new forms, but also by the cessation of old forms of Selection. We shall find that important differences of this kind may arise, resulting in considerable transformation before any new form of Selection has distinctly supervened. A good illustration of the *Cessation of Selection* is found in the increasing frequency with which human mothers, notwithstanding their failure to give suck, succeed in raising their children. The power to give suck is through this process being diminished in the more civilized races, though there is no reason to believe that those who do not give suck have, on the whole, any advantage over those who do. The new result is therefore being produced, not by the introduction of a new form of Filio-parental Selection, but by the cessation, or the weakening, of an old form. Romanes was, I believe, the first to point out the effects that must often be produced by the cessation of Natural Selection *, but he has not considered the cessation of other forms of Selection.

(7) It is often convenient to distinguish between Selection resulting from rational devices and that resulting from the superior success of organisms better adapted than their rivals of the same intergenerant to the natural laws and conditions of the environment, or to the natural constitution of the species to which they belong. The former I call *Rational Selection*, and the latter *Adaptational Selection*. Under the former I place Artificial and Institutional Selection, and under the latter I place processes that are as unlike as Natural and Sexual Selection. This classification does not, however, seem to me so important, or so fundamental and clearly definable, as that which

* See an article on "The Factors of Organic Evolution" in 'Nature, vol. xxxvi. pp. 402–404, in which reference is made to previous papers in which the Cessation of Natural Selection is discussed.

rests on the fact that some forms of Selection depend on the relations in which organisms stand to the environment, while others depend on the relations in which the members of the same species stand to each other. It may here be noted that Artificial Selection is the exclusive generation of those that are better fitted to the rational environment, through the failure to propagate of those that are less fitted. The effect is the same whether the failure to propagate is through lack of adaptation to humau purposes, or through lack of adaptation to the unreasoning environment. Natural Selection is propagation according to adaptation to the Natural environment, and Artificial Selection is propagation according to adaptation to the Rational environment.

(8) Another discrimination which I have found it convenient to make, is that between Comparative and Superlative Selection. *Comparative Natural Selection* is the direct result of varying degrees of adaptation to the environment, without the additional influence of rivalry between the members of the same species. It is propagation of the fitted, according to the degrees of their fitness, controlling the expansion of a species before its members crowd and supplant one another. *Superlative Natural Selection* arises from the competition of members of the same species for the possession of identical resources, and results in the survival of those only that are most perfectly fitted to the environment. Comparative Selection is the Survival of the Fitted—of all the fitted, according to their degrees of fitness; Superlative Selection is the Survival of the Fittest—of only those who through superlative fitness can, in a crowded community, find the sustenance and other conditions necessary for perpetuating their kind.

The following classification (p. 329) of the forms of Selection will, I think, be a help in maintaining these and other distinctions.

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FORMS OF SELECTION.

	ADAPTATIONAL SELECTION.	RATIONAL SELECTION.
ENVIRONAL SELECTION.	*Natural Selection.* Balanced. Unbalanced. Active. Passive. Comparative. Superlative.	*Artificial Selection.* Balanced. Unbalanced. Active. Passive. Comparative. Superlative.
REFLEXIVE SELECTION.	*Conjunctional Selection.* Balanced. Unbalanced. Sexual. Social. Filio-parental. Comparative. Superlative. *Dominational Selection.* Balanced. Unbalanced. Sustentational Domination. Protectional Domination. Nidificational Domination. Nuptial Domination.	*Institutional Selection.* Balanced. Unbalanced. Ecclesiastical. Military. Sanitary. Penal. Comparative. Superlative.

Natural Selection.—As Natural Selection involves not only the superior propagation of the better fitted, but the inferior propagation of the less fitted, and the non-propagation of the least fitted, it may be described as the *Exclusive propagation of those better fitted to the natural environment, through the failure to propagate of the less fitted.* Transformation by means of Natural Selection depends on varying degrees of adaptation to the environment in creatures that are intergenerating, the higher degrees being possessed by other than average forms. Divergence is produced by Natural Selection only when to the above conditions producing transformation are added causes that prevent intercrossing between the sections that are being independently transformed. In other words, *Independent Natural Selection produces Divergence.*

Sexual Selection is the exclusive propagation of those better fitted to the sexual constitution of the species through the failure to propagate of the less fitted. In the words of Darwin, "It depends on the advantage which certain individuals have over others of the same sex and species solely in respect of reproduction." * It is the form of Reflexive Selection which has received Darwin's attention, and is consequently familiar to all. There are, however, certain points that need to be emphasized.

This is the principle in accordance with which correspondence is secured between the external characteristics and the sexual instincts of a species, and also between the instincts of the two sexes, in as far as they relate to reproduction. This result is secured partly by the failure to propagate of those whose powers of attraction and conquest do not reach the standard demanded by the instincts of the other sex, and partly by the failure of those whose instincts diverge too widely from the typical characteristics of the other sex. For example, on the highlands of North China I have observed a species of creeping cricket of the genus *Bradyphorus*, the male of which calls the female by a sharp stridulation, to which the female responds by approaching the male and finally climbing upon his back. Now we can well understand that the call of the male has been brought to its present shrill, penetrating perfection through the failure to attract mates in the case of males that were but feebly endowed; but it is equally certain that those females whose sluggish instincts have been capable of responding only to an unusually intense call have, for the most part, failed of leaving offspring, and, if any have been so unreasonable as to wait for the male to seek them out, they have, doubtless, perished without perpetuating their perverted instincts. If my view is correct, the change producing divergent sexual characteristics may be either in the instinct, or in the characters with which the instinct is correlated. It seems probable that in the vast majority of cases the more strongly divergent forms have been reached by a multitude of deviations alternating between the psychical and the physiolc-

* 'Descent of Man,' 3rd page of Chap. VIII.

gical and morphological characters of the species, the chief, indispensable condition being the prevention of interbreeding between the diverging sections of the species.

Sexual Selection is sometimes referred to as if it were the influence of sexual instincts in giving character to the organs of a given sex, first by the instincts of the same sex rousing the organs to successful activity in securing propagation, the degree of success depending on the degree of adaptation of the organ to the purpose of the activity (as in the case of barnyard cocks winning partners by the use of their spurs), and, second, by the instincts of the opposite sex being roused to successful action according as the endowments of the given sex are fitted to the end (as in the case of peacocks winning partners by the display of ornamentation). Starting, however, with this conception of the nature of Sexual Selection, we shall find great difficulty in obtaining from the principle any explanation of the origin of species, or of divergent evolution of any kind. If divergent instincts are the causes of divergent forms, colours, and qualities, what are the causes of the transformation of the instincts in lines that are persistently divergent? The problems of transformation and divergence are as far from solution after the application of the theory as before.

If, on the other hand, we recognize Sexual Selection as the harmonizing of the forms, colours, and qualities of a species with its sexual instincts, and of the sexual instincts with its forms, colours, and qualities, we shall not claim that either set of characters is directly and continuously the cause of transformation in the other; but rather that the two sets play upon each other in such a way as to produce a state of unstable equilibrium in both sets, the result of which is indefinite transformation in the secondary sexual characters of each section of a species that constitutes a separate intergenerant; and that the Independent Transformation inevitably results in Divergence. In Darwin's presentation of the principle of Sexual Selection, the chief endeavour is to show that differences in voice and ornamentation between the males and females of the same species are probably, in a large degree, due to diversity in the action of Sexual Selection upon the different sexes; but this is a very different result from differences in the same respects between those of the same sex in closely allied varieties and species; and no clear

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understanding of the subject will ever be reached till those who study and discuss the subject discriminate between these two classes of phenomena. The formation of differences of the former kind is simple transformation without divergence, while the entrance of differences of the latter kind is divergent evolution tending to the production of separate species.

If a species deficient in secondary sexual distinctions, after being divided into segregated sections, attains a high development of such distinctions, it is easy to believe that they will be developed in different ways in the different sections, and that thus they will become specific distinctions; but it is not so easy to see why a species in which sexual distinctions have already been fully developed should undergo divergent changes in the different sections into which it may be divided. It is in such cases that we discover the important influence of what I have called unstable equilibrium. It seems probable that in some cases small differences originating through indefinite variation in only a few isolated individuals are seized upon by the exaggerating fancies of the other sex, and are thus first preserved through isolation and then exaggerated by Sexual Selection. In other words, *Independent Sexual Selection produces Segregation and Divergence.*

Social Selection is the exclusive breeding of those better fitted to the social constitution and instincts of the race through the failure to breed of those less fitted. Social organization has reference chiefly to co-operation in securing sustentation and defence. If for each species there were but one possible form of social organization through which sustentation could be secured, there would be no need of considering Social Selection, for the form of social organization would be rigorously determined by Natural Selection, and the success of the individual through conformity to that organization would be sufficiently explained by the principle of Natural Selection. But different forms of social organization are often exhibited by the same or closely allied species; and we find that, in such cases as elsewhere, the prosperity of the individual is largely dependent on his conformity to the social organization to which he belongs. Social Selection must, therefore, in some cases have been an important factor in maintaining a correspondence between the capacities and the social organization of a race or species. When a species

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or a section of a species is undergoing a change of social habits, there will be individuals that fail through reverting to the old instincts and methods which put them out of accord with the rest of the community. But through the failure of these the inherited instincts of the race are brought into increasing accord with the new habits till, in the case of most species, there are but few individuals that fail through lack of appropriate social instincts. Nevertheless in the branches of the human species that have attained the highest civilization the process is still far from complete, for the instincts of many individuals are in conflict with civilized habits.

We find that the natural faculties that are best fitted to secure individual success, and a numerous and long-continued descent, are different under different forms of civilization. Social habits in a great measure determine the food and clothing of a community, and thus deeply affect the qualities of the race. The exposure to which the young are subjected is also largely determined by social custom, and so the quality of the constitution that is permitted to survive. In other words, the form of Parental Selection that prevails in any community is often determined by Social Selection, as the form of Social Selection is sometimes determined by Natural Selection. Many matters, which amongst irrational animals are determined by instincts guiding the individual directly to the needed resources and showing what provision must be made, are with man determined by social instincts leading the individual to follow the general experience or traditional habits of his clan.

As in countries where there are no beasts of prey the gregarious instinct of cattle ceased to be a necessity for the preservation of life, it is no longer maintained by Natural Selection, but it may be preserved by Social Selection; for though occasional stragglers appear, they are, through lack of adaptation to the social organization, specially liable to fail of finding mates, and therefore to fail of propagating their kind. Between the capacities of a community and its social organization there is a constant action and reaction which tends with more or less rapidity toward transformation; and this tendency is increased when a small community, during a long separation from other communities, gradually increases in strength, independently constructing a civilization of its own. In other words, *Independent*

Social Selection tends toward divergent evolution of capacities and of social organization.

Filio-parental Selection is the exclusive breeding of those better adapted to the relations in which parents and offspring stand to each other, through the failure to live and propagate of those less adapted. How the power of giving suck and the corresponding instinct for sucking were first developed it may be impossible to tell; but it is evident that having once been established as the method of sustentation for the young of mammals, any young lacking the instinct would perish without leaving descent. There is every reason to believe that, with the exception of man, it may be truly said of every individual mammal that all its ancestry, through all its generations that have elapsed since they became fairly mammalian, have had this instinct in full force; and yet it sometimes fails, and the line of descent is cut short. Till comparatively recent times the same was true of man; but we now find some cases in which the young survive in spite of their inability to suck, and the constancy of this mammalian characteristic is being gradually impaired. There is also in some races an increasing tendency to shorter periods of lactation, or to the entire suppression of the function; so that it seems not improbable that there may yet arise a variety of the human species in which the power will be comparatively obsolete. Under such conditions the instinct for sucking would cease to be of any advantage, while special advantage would accrue to those best able to thrive on the artificial food habitually provided by the parents. In some countries this would be the milk of ruminating animals, while in other countries it would be some vegetable preparation. In the islands of Micronesia it is the sap that exudes from the cut end of the immature fruit-stalk of the cocoa-nut tree. In Japan it is a sweet extract of malt. Through this diversity in the food provided by parents for their infants and small children, there is even now a constant diversity in the Parental Selection prevailing in different countries. Diversity in the forms of Parental Selection is also produced by diversity in the clothing and artificial heat provided by parents, in the protection, on the one hand, of children from the wind and rain and direct rays of the sun, and, on the other hand, their exposure to the same with shaven heads or naked bodies, and in the methods of binding, cramping, and mutilating the head, feet, waist, and other parts of the body. From this point of view we see how largely the form of Parental Selection is determined by social custom, and how it is sometimes enforced by Social Selection, which excludes from the benefits of the caste or tribe all who have not been through the ordeal.

As Filio-parental Selection is due to different degrees of adaptation between the parent and offspring, it may be characterized not only by fatal departures in offspring from the characters required in their relations to their parents, but by fatal departures in parents from the characters required in parents in their relations to their offspring. As an example of the former, we may refer to the death at birth of children with excessively large heads; and as an example of the latter, to the death at birth of all the children of a mother with a contracted pelvis.

Dominational Selection.—Variations that are equally fitted to cope with the environment may be divided into two classes—those better able, and those less able, to cope with other members of the species in appropriating resources. Increase of population and the consequent competition between members of the same species condemns the latter to premature death, or at least to failure in propagating, unless they find new resources by migrating or by changing their habits. Competition between kindred for the possession of identical resources we find directly connected with three quite distinct principles of evolution:—(1) With the principle of *Superlative Selection* tending to the destruction of all forms except those most fully adapted to the environment; (2) With the principle of *Dominational Selection* tending to discriminate between those equally adapted to the environment, through the success and consequent propagation of those only that are best able to cope with their kindred in appropriating advantages; (3) With the principle of *Competitive Disruption,* tending to break up old relations and old habits, and so preparing the way for the formation of new habits producing segregation and divergence. Of these three principles, the last was referred to in the second chapter of my paper on "Divergent Evolution through Cumulative Segregation," p. 221, and the first has already been mentioned in this paper. The remaining one I shall here briefly describe, without attempting to show its important influence on the transformation and divergence of species.

Dominational Selection is the exclusive breeding of those better able to appropriate natural resources, or mates, or the provision made by parents or society, not through being better fitted to the environment or to the organized methods of co-operation and assistance, but through being better able to overcome or outdo their rivals of the same species. It results from the contest or rivalry with each other of members of the same species that are equally fitted to the environment and to the constitution of the species, and the consequent failure of all that are not able to cope with their kindred. "The law of battle" is a form of Dominational Selection which Darwin emphasizes as having great influence in determining what males shall have the best success in procuring mates. But there is a similar law determining what individuals shall obtain the resources furnished by nature, or elaborated by parents and society. We may have Dominational Selection relating to sustentation, protection, and nidification, as well as to the possession of females. And in gaining a single end there may be a great variety of dominating methods. Combat between males for the possession of females is not found in the vegetable kingdom; but the prepotence of the pollen of certain flowers over that of other flowers of the same race may play a similar rôle.

Dominational Selection differs from Natural Selection in that it does not depend on degrees of adaptation to the environment, and from other forms of Reflexive Selection in that it depends on a quite distinct form of the relationship in which members of the same species stand to each other. It seems desirable that this form of selection, which depends on adaptation for overcoming, outdoing, or supplanting others of the same species, should be clearly distinguished and named. We further note that there can be no doubt that Dominational Selection acting for many generations on sections of a species that are prevented from intercrossing will in all probability follow somewhat different lines. In other words, *Independent Dominational Selection will produce divergent evolution.*

Institutional Selection is a form of exclusive breeding closely related to Social Selection, but differing from it very much as Artificial Selection differs from Natural Selection. Institutional Selection is the influence of institutions, customs, and laws in determining what classes of individuals have an opportunity to marry and raise children. In most civilized countries criminals convicted of important offences are so confined as to prevent their adding to the population of the community during the time of their confinement. This is a method of improving the race that might be carried further than it has been. In some countries the insane are confined in asylums and not allowed to marry; and in other countries ecclesiastical and military restrictions prevent certain portions of the community from raising families.

Result of the foregoing Survey of Selectional Intension.

The analysis which we have now completed enables us to see how far changes in the form of Selection are due to changes in the environment, and how far to changes in the organism. We find:—First, that all the forms of Reflexive Selection are due to the relations of members of the same species to each other, and are liable to change without any change in the environments. Second, that Active Natural Selection is due to change in the successful use of the powers of the organism in dealing with the environment, and is not dependent on change in the environment. Third, that Passive Natural Selection, which is due to the exposure of the organism to a different environment, is often produced by the organism's entering a new environment without there being any change in either the new or the old environment. Fourth, that when Passive Natural Selection is produced by change in the environment, the more effective forms of Selection do not appear till the organism has so multiplied as to produce what I call Superlative Natural Selection through intense competition between rival individuals of the same species in gaining possession of limited resources. And, fifth, that Passive Comparative Natural Selection, which depends on change in the environment, without special rivalry between the members of one species, also depends on variation in the adaptations of the organism, many of which variations do not depend on that change in the environment which has produced the change in the Natural Selection, nor, indeed, on any change in the environment except those fundamental physical changes by which the world has passed from its primitive gaseous to its present partially liquid and solid state, rendering it a fit abode for organisms.

Eliminational Intension.

Eliminational Intension is Segregation and divergence produced by the indiscriminate destruction or failure to propagate of part of the individuals of similar sections of a species. Though indiscriminate destruction cannot be classed as a form of Natural Selection, it may nevertheless be the cause of transformation; and when a species is distributed in sections that are prevented from intergenerating, divergent evolution will often be hastened by the indiscriminate destruction of part of the members of one or more of the sections. If a species inhabiting a large island is divided by geological subsidence into two equal sections, there may be a very close resemblance in the average character of the two sections; but if a subsequent eruption of hot ashes destroys a large portion of the individuals of one section, or of both, the probability of a close correspondence in the average character of the two sections will be very much less than before the eruption.

Again, when an area occupied by a species is divided into two or more equal districts, the occupants of which can have little or no opportunity for crossing, divergent evolution will arise in the different districts unless there is some constantly operating cause that ensures all the varieties that survive and propagate in any district shall survive and propagate in all the districts. No such cause has ever been pointed out; but, on the contrary, it can easily be shown that the probability is very small that such a correspondence would occur, even if at the time of the division of the area every individual in each district was represented by a completely similar individual in each of the other districts. Let us suppose a case:—

1. Suppose the creatures under consideration to be a species of mollusk, the sexual instincts of which act without any segregative tendency between the varieties of the same species, there being no aversion or other impediment that interferes with the free crossing of all the variations occurring within the limits of one district.

2. Suppose that the number of individuals in each district is 10,000,000.

3. Suppose that one in a thousand of these had a tongue strong enough to feed on the bark of the tree, the leaves of which are the ordinary food of the species, and that one in a thousand is capable of digesting the same, so that, in each district alike, one in a million could survive in this way though the crop of leaves should fail.

4. Suppose that there are, through diversity of adaptations of this kind to the products of the environment, ten different kinds

of accessible forms of food, on each kind of which one in a million of the individuals of each district might feed if driven by necessity.

5. Now suppose the same necessity should occur in each district through the destruction of the leaves on which they habitually feed; and that there are accordingly in each district a hundred survivors able to maintain themselves on other kinds of food.

Under such circumstances (the correspondences of which we have in our supposition made much more exact than the actual deviations from a mean ever present)—but even under such circumstances of completely parallel variation—what is the probability that in each of the separate districts the few that would meet with other individuals and have an opportunity to propagate the species would be similarly endowed and similarly related to the environment?

In order to still further simplify the problem, let us assume that in the case of each kind in each district the probability that it will succeed in propagating is exactly balanced by the probability that it will fail. The probability, then, that any given number of the ten kinds in a given district will succeed is found by estimating the number of combinations that can be secured by taking that number of things out of ten things in different ways. This is completely parallel to the number of ways in which ten pennies can be arranged as to head and tail, each penny representing one form of variation, and its lying head-up indicating success in propagating. In 1024 experiments the probability is

That 0 will succeed	....	1 time
„ 1 „ „	$\frac{10}{1}$	= 10 times
„ 2 „ „	$\frac{10\times9}{1\times2}$	= 45 „
„ 3 „ „	$\frac{10\times9\times8}{1\times2\times3}$	= 120 „
„ 4 „ „	$\frac{10\times9\times8\times7}{1\times2\times3\times4}$	= 210 „
„ 5 „ „	....	252 „
„ 6 „ „	....	210 „
„ 7 „ „	....	120 „
„ 8 „ „	....	45 „
„ 9 „ „	....	10 „
„ 10 „ „	....	1 „

These figures are found in the eleventh line of what is known as the "Table of the Binomial Coefficients," or the "Arithmetical Triangle"*. And so in the case of any number of objects, the number of combinations that may be made with n objects is found in the $n+1$th line of the Arithmetical Triangle classified according as there are 0, 1, 2, 3, or more objects in each combination. The whole number of combinations may also be found by calculating the nth power of 2.

The possible combinations of the ten varieties in question are 1024, which is equal to 2 raised to the 10th power; the probability, therefore, that the combination that succeeds in one district will also succeed in the other district is $\frac{1}{1024}$, or 1 in 1024; while the probability that those that succeed in the one district will not be all the same as in the other will be $\frac{1023}{1024}$, or 1023 in 1024, which is more than a thousand times greater than the reverse probability.

These 1024 different results, any one of which may occur in one section, are calculated on the supposition that all the representatives of the species in one section that succeed in propagating will in time coalesce by intercrossing; but, as we shall presently see, the number of divergences in the two sections may be vastly increased by the diversity of ways in which the same varieties may be combined through the greater or less influence of minor segregations within the bounds of each district.

Amalgamational Intension.

In my paper on "Divergent Evolution though Cumulative Segregation," p. 233, I have referred to the fact that the vast majority of divergent forms produced by Segregation, after existing for a time, are interfused with competing forms of the same species. Now it is evident that when a permanent Segregation arises, if in the separate sections there is a diversity of amalgamations between the slightly divergent forms produced by partial segregations, the results will be divergent in these separate sections. That there will be diversity in this respect, we may argue: first, from the improbability that all the varieties in one section will occur in each of the other sections; second, from the improbability that if the same varieties occur in each section, they will occur in the same proportions; and, third, from the improbability that if they are the same and in the same

* See 'Principles of Science,' by W. S. Jevons.

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proportions, they will break over their barriers and interfere with each other in precisely the same way in each section. Amalgamational Intension relates only to the last point. The other two points have been discussed under the principle that Separation always involves more or less Segregation (see the third paragraph of this paper).

Taking up again the supposed case considered under Eliminational Intension, if the different kinds of new food were so situated as to make it more or less difficult for those feeding on one kind to cross with those feeding on other kinds, the representatives of the species in each of the completely separated districts would be divided into minor segregations of a partial kind; and the different degrees of intercrossing between the minor segregations in the separate districts would be an additional cause of divergence, which we may appropriately class as a form of Amalgamational Intension. Occasional interchange of stations by the varieties in one district would produce a degree of homogeneity in the forms of one district that would not be found when comparing those of different districts; but as the degrees of intercrossing between any two or more identical varieties that might happen to be preserved in both districts would, in all probability, differ in the different districts, the correspondence that at first existed between certain portions of the two sections would gradually disappear. We shall find that in order to ascertain with facility the number of different sets of combinations in which any given number of varieties may be combined while all are propagating, and the probability that any given degree of correspondence will present itself in any two sets of combinations that may be taken at random, we need a table by which the number of permutations that may be made with given numbers of things may be analyzed. I have constructed such a table, which I call the Permutational Triangle *, with the aid of which the solutions of problems that would otherwise require much time are easily reached.

Returning to the above calculation, we observe that in 1024 experiments, under the circumstances there assumed, there would probably be but one occasion in which, out of the ten identical varieties which were assumed to occur in each district, the same varieties would succeed in propagating in each district. We

* I give in an Appendix this Permutational Triangle, calculated to the tenth line, with an explanation of how it was formed.

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have now to consider the degree of probability that these identical varieties will make the same combinations with each other in the different districts. I shall not attempt to give a complete answer; but by carrying the computation through several steps, I shall sufficiently exhibit the extreme improbability that, even when identical varieties succeed in propagating in the different districts, they will combine with each other in the same way and in the same proportions.

As in the case of the 10 varieties that have been under consideration, 5 is more likely to be the number of varieties that succeed than any other number, 5 is most likely to be the number of successful varieties in each district when the varieties happen to be the same in each district; and we will therefore begin with that number. If, now, we suppose that there are 5 varieties in each district, and that there is the same chance in the case of each variety that it will breed with any one of the other varieties, as there is that it will be segregated and breed by itself, we shall find that in 120 experiments there will probably be 1 occasion in which all the varieties of one of the districts will be segregated from each other, and 10 occasions in which three of the varieties will be segregated, and 20 occasions in which two will be segregated, and 45 occasions in which one will be segregated, and 44 occasions in which none will be segregated *. These probabilities are expressed by the fractions $\frac{1}{120}$, $\frac{10}{120}$, $\frac{20}{120}$, $\frac{45}{120}$, and $\frac{44}{120}$. And the probability that the same varieties will be intercrossed and the same ones segregated in each district is $\frac{1}{120}$; while the probability that some one particular set of segregations and intercrossings that is designated in advance will occur in both districts is $(\frac{1}{120})^2$. For example, the probability that all the 5 varieties in one district will be segregated is $\frac{1}{120}$; and the probability that all in both districts will at the same time be segregated is $(\frac{1}{120})^2$.

But the two districts may correspond by the complete failure of all varieties to propagate, in which case they will continue to correspond. Again, there may be but one variety in each district that succeeds in propagating, and that the same, in which case there will be no chance for diversity of Amalgamation in the different districts, at least not before a diversity of subordinate segregations has first arisen. Again, if the same two varieties

* These figures are found in the 5th line of the Permutational Triangle. See Appendix.

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succeed in propagating in each district, the probability of complete correspondence in integration will be as 1 to the factorial of 2, or as $\frac{1}{1\times2}=\frac{1}{2}$;

if the same 3 varieties, the probability $=\frac{1}{1\times2\times3}=\frac{1}{6}$;

" " 4, the probability $=\frac{1}{1\times2\times3\times4}=\frac{1}{24}$;

" " 5, " $=\frac{1}{1\times2\times3\times4\times5}=\frac{1}{120}$;

" " 6, " $=\frac{1}{720}$;

" " 7, " $=\frac{1}{5,040}$;

" " 8, " $=\frac{1}{40,320}$;

" " 9, " $=\frac{1}{362,880}$;

" " 10, " $=\frac{1}{3,628,800}$.

These fractions represent the probability of complete correspondence as to the varieties that intercross and those that remain segregated in the different districts when the same varieties occur in each district; and the squares of these fractions represent the probability that any special combination that may be indicated will occur in both districts at the same time. If there are, for example, the same ten varieties in each district, the probability that they will combine in the same way is $\frac{1}{3,628,800}$, and the probability that this way will be the breeding of each variety with its own kind, without any intercrossing, will be $\left(\frac{1}{3,628,800}\right)^2$. But there may be degrees of correspondence in the combinations of different districts. As we have just seen, the probability that there will be correspondence in ten points is $\frac{1}{3,628,800}$, that there will be in eight points is $\frac{45}{3,628,800}$, that there will be in but one point is $\frac{1,334,960}{3,628,800}$, while the probability that there will be no correspondence is $\frac{1,334,961}{3,628,800}$ *.

We have thus far considered only the divergences that come

* The denominator of these fractions is the factorial of ten, that is $1\times2\times3\times4\times5\times6\times7\times8\times9\times10$, and the numerators are found in the tenth line of the Permutational Triangle. See Appendix.

from a diversity of binary combinations coexisting with segregated varieties; but it is evident that the number of divergent arrangements that may be produced by any given number of varieties exceeding two will be much larger if to the above arrangements are added all that may be produced by arranging single with trinary, and binary with trinary; and if more complex combinations are introduced, the number may be still further increased.

Of the five suppositions with which we started *, the second and third assume a uniformity in the contingencies relating to the number and character of the individuals never realized in the different sections of a species that is divided by natural barriers; and the fifth assumes a uniformity in the changes affecting the environment which, though not often realized, is here assumed for the sake of showing that divergence of character is not dependent on the organism being exposed to different environments. In connection with the fourth supposition, it would have been in accordance with the usual conditions of nature to have assumed that, besides the many kinds of food of which only a very small fraction of the species could avail themselves, there would be a few kinds on which much larger numbers could feed; and that when the numbers that could partake of one kind of food were sufficient to ensure the propagation of those thus adapted, that variety would survive in both districts. But such certainty relating to the propagation of some of the varieties would not prevent the contingencies and the divergences that would arise in the propagation of the much rarer or less favoured varieties. It is also evident that similar contingencies would arise whenever the pressure of population on the supply of food should render it necessary for large numbers to seek new resources. The divergent tendency of such pressure, from whatever cause the pressure arises, is in no respect an arbitrary supposition; and the arbitrary assumptions which I have introduced in order to simplify the problem remove from consideration some of the contingencies that must produce still greater divergence.

Fecundal Intension,

or *Segregation and Divergence produced by Independent Fecundal Transformation*, that is by different relative degrees of fertility

* See page 338 of this paper.

possessed by the same forms of variation in separate sections of the species.—Relative Fecundity is propagation according to degrees of fertility. As it involves not only the superior propagation of the more fertile, but the inferior propagation of the less fertile and the non-propagation of the least fertile, it may be described as the exclusive propagation of the more fertile, through the failure to propagate of the less fertile. It would avail nothing in determining the form that is to prevail in succeeding generations if it did not in some degree preclude the crossing of the less fertile with the more; but, as it is evident that, so long as increased fertility is not a disadvantage, the more fertile half of the species will leave a larger number of offspring than the less fertile half, it follows that when the offspring have come to maturity a larger portion of the fertile will consort with the fertile than in the previous generation, and so the fertility of the following generation will be still further increased. The chief check to this law of *Cumulative Fertility* is found in the antagonistic law of Cumulative Adaptation through Adaptational Selection. The combined action of these two laws results in the triumphant development of the most fertile of the best fitted, or the best fitted of the most fertile.

Another result from the combined action of these two laws is that in species that are well adjusted to the environment the typical, that is the average, form of the species is not only the best adapted, but it is the most fertile; and this correlation between fertility and adaptation in the average form of the species or race is a strongly conservative principle, tending to prevent the rapid transformation of the race or species. Giants, dwarfs, and extreme departures from the type of other kinds are more likely to be sterile than the typical form of the species; and therefore if, through change in the environment or in the social conditions, some extreme form has an advantage in gaining subsistence, it will usually fail of propagating its kind with the relative rapidity of the less-favoured average form. This is at present true of highly intellectual variations of civilized man. Those of moderate capacities are more prolific, and accordingly persist, though less successful in other respects than the intellectual. But so long as the most successful individuals are those surpassing the average in intellectual endowment, so long will the average endowment be more or less steadily advancing; for, of intellectual families, those that are fairly fertile will leave more impress on

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succeeding generations than those that are sterile; and of fertile families, those that are above the average in intellect will have the best success in leaving descendants to inherit their endowments.

Combined Influence of these Principles.

We have not at present sufficient knowledge of the influence of each of the principles of transformation to enable us to estimate their comparative importance; but we know enough of their combined action to anticipate with confidence that wherever Separate or Segregate Generation arises, producing more or less divergence, there these principles will in time intensify the result. The transformations and divergences of nature are produced by the interplay of numerous factors most intimately combined, and though for the purpose of comprehending the process we are compelled to study each principle by itself, we must remember that in nature they not only combine, but combine in a vast variety of ways. There is, however, reason to believe that species sometimes become so devoid of plasticity that all transformation is precluded, and, if the environment is changed, even in the most gradual manner, extinction is the result.

Divergent Evolution in the Land-Mollusks of Oahu.

Oahu is one of the Sandwich Islands, or Hawaiian Islands as they are now usually called. It is of volcanic origin, but the two mountain-ranges, which lie one on the north-east and the other on the south-west side of the island, show no signs of recent volcanic action. Unlike the mountains of Hawaii and East Maui, their sides are very deeply furrowed by the action of water, and their forests are not broken by flows of lava. The forests of the island cover these two ranges, forming two disconnected strips, the one about 36 and the other about 18 miles in length. In these forests are found 600 or 700 varieties, representing over 200 species, belonging to 7 subgenera, of the subfamily *Achatinellinæ.*

Two of these subgenera, *Amastra* and *Leptachatina*, are, for the most part, found under the dead leaves of trees in damp places; and one, *Laminella*, is found chiefly on low shrubs, while the remaining four are always found on trees or shrubs. Now it must be remembered that the climate is tropical, and that the rainfall is so distributed through the year that in the shady groves there is nothing to drive the arboreal species from their haunts on the leaves or branches of the trees. Still further, as this branch of the Helicidæ, unlike most other branches, produces its young, not from eggs, but in a living active form, there is no occasion in its life-history that requires it to leave the tree in which it lives from generation to generation. In the distribution and divergences of these varieties and species we learn the following lessons :—

1. *Varieties are incipient species, and species are strongly pronounced varieties.*

A full collection of the varieties and species of any polymorphic genus produces an oppressive sense of confusion on the mind of any one who examines it for the first time. This is preeminently true of a full collection of the *Achatinellinæ* of the island of Oahu. Seven genera or subgenera are represented by a multitude of varieties and species, which, within the limits of each genus, are, for the most part, completely intergraded with each other. As natural selection has not removed the intermediate forms, it is impossible to say where a species begins and where it ends. Having selected a given form as the type of a given arboreal species, we soon find that it inhabits perhaps only one or two valleys, say half a mile in width, and only one, two, or three miles in length. Beyond these limits it is represented by varieties that become more divergent as the distance from the home of the type increases ; and, in the case of *Achatinella* and *Bulimella*, this divergence is so rapid that at the distance of 8 or 10 miles every one will admit that the forms all belong to different species. Indeed, in many cases, though the same vegetation is present, the habits of feeding have changed, while in other cases the form has changed while the habits remain essentially the same.

Though it is easy to find degrees of divergence which most naturalists will agree in calling specific, but which in a full collection are shown to be completely intergraded, yet if a full collection of the different forms should be submitted in succession to a hundred different naturalists to classify, it would be found that no two would agree as to the number of species ; and a still greater diversity of opinion would be revealed as to where the limits of the different species should be placed. This is exactly what we might expect if varieties are incipient species, and species are simply more strongly developed varieties. Such being the case, it is folly to ask that the nomenclature should be based on some fundamental distinction between species and varieties.

The best nomenclature is the one in which the specific distinctions correspond in degree with those that are recognized as specific in other families, and in which a degree of divergence that is considered specific in one part of a genus is considered specific in every part. If the distinctions on which Reeve, Pfeiffer, and Newcomb have founded the species in Makiki and Manoa are received as specific distinctions, then similar distinctions occurring in the forms of other valleys must be recognized as belonging to different species. I by no means contend that these differences should be regarded as specific; but having received the three or four forms of *Achatinella* found in Manoa as good species, it will not do to say that the three forms of *Achatinella* found in Waialei, differing from each other in the same way, are but one species.

Notwithstanding the diversity of opinion that will always exist as to how many species should be made of the forms occurring in any one valley, every one will agree that the forms of *Bulimella* and *Achatinella* found in any one valley are quite distinct species from those found in valleys that are ten or twenty miles distant. The lessons we are drawing from the divergences in this family are therefore not dependent on any special views concerning the number of species that ought to be received.

As examples of intergrading species, examine first the types of *Achatinella producta*, *A. adusta*, and *A. Buddii* from Makiki; then all the forms of these and the other species of *Achatinella* found in Makiki; and then the forms found in the successive valleys of the whole mountain-range.

If freedom from intergrading is received as the necessary and sufficient test of good species, then a multitude of forms that are now only varieties may be turned into good species by burning the forests in alternate valleys on either side of this mountain-range. Moreover, if this is the true test of species, the species-maker who throws intergade forms into the fire is quite consistent, even if not quite frank.

Whether we call these divergent forms species or varieties, the process by which the divergence has been produced is a matter of equal interest. Indeed, some evolutionists maintain that one of the chief desiderata in the theory of evolution is an explanation

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of the origin of varieties *. Variations are deviations from the average, but varieties are groups of individuals in which the averages differ, and in which the inheritable characters differ. Still further, it is usually admitted that the divergences presented by varieties are not always essential to the well-being of the forms that possess them, and that in many cases the forms that are confined to separate localities might exchange positions without suffering disadvantage. Divergence in these initial stages has seemed to many to be an obscurer problem than the advancing usefulness which sometimes entirely remodels an organ. For, as Prof. Le Conte has said, "Natural selection does not make an organ useful, but only more useful."

I believe that the theory of divergent evolution, presented in this and the preceding paper, is applicable to the formation of divergences during the stage when some of the differences, if not all, bring neither advantage nor disadvantage to those that possess them. Whatever we call these divergent forms, can we give any explanation of the causes that have produced them?

2. *Divergent Evolution does not necessarily depend on either change in or change of the environment.*

In other words, it does not necessarily depend on change in the conditions surrounding the organism, or on the organism being brought into a district presenting a different set of conditions.

Darwin maintains that isolation (that is geographical separation), without any differences in the surrounding organisms or in the physical conditions, presents no occasion for divergence of character. He says, "If a number of species, after having long competed with each other in their old home, were to migrate in a body into a new and afterwards isolated country, they would be little liable to modification" ('Origin of Species,' 6th ed. p. 319).

Spencer expresses the same idea by saying that "Vital actions remain constant so long as the external actions to which they correspond remain constant" †. "There must be maintained a

* See 'Evolution and its Relations to Religious Thought,' by Joseph Le Conte, published by Appleton & Co., page 252.

† Though apparently opposed to his theory of "the production of certain local forms by amixia," this same idea is found in Weismann's 'Studies in the Theory of Descent,' pp. 109–115 (English edition).

tolerably uniform species so long as there continues a tolerably uniform set of conditions in which it may exist." (See Spencer's 'Principles of Biology,' §§ 91, 156, 169, 170.) In other words, divergence of character in the descendants of one stock occupying different districts does not arise except as it is preceded by difference in the physical conditions, or in the surrounding organisms, of the different districts. After moulding this thought in many forms, Spencer makes it the fundamental principle on which he builds not a small portion of his philosophy. Darwin is more guarded in his statements; still, as we have already shown, he sometimes seems to reason from an assumption quite in accord with what Spencer would have us receive as essential to the very idea of causation in vital processes. For example, his explanation of the fact that on the different islands of the Galapagos Archipelago one genus is, in many cases, represented by several closely allied species which are undoubtedly modified forms of one continental species, seems to rest on the assumption that if every species that gained access to any island had at the same time gained access to the other islands of the archipelago, there would then have been no occasion or opportunity for the divergences we now find (see 'Origin of Species,' 6th ed. p. 355).

It seems to me that the divergences presented by the varieties and species of the subfamily *Achatinellinæ* of the Sandwich Islands are at variance with this assumption. Not only are islands in sight of each other occupied by divergent species, but different parts of the same mountain-range, exposed to the same winds and rains and clothed by the same vegetation, are the homes of divergent forms.

Turning to the map of the island of Oahu, we find a mountain-range extending 36 miles from north-west to south-east nearly parallel with the north-east coast. The north-east side of this range is exposed to the trade-winds fresh from the ocean, and accordingly receives a heavier rainfall than the other side; but there is not much difference in the amount of rain received by the different valleys on one side of the mountain. In nearly all these valleys on either side of the range are found shady groves of what the natives call the "kukui" (*Aleurites triloba*). Many species of the subgenera *Achatinella* and *Bulimella* have their haunts in these groves, some species clinging to the leaves and young branches, and others to the old branches and trunks. Most

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of the species thrive only where the shade is dense and the atmosphere laden with dampness a large portion of each month.

The student who starts with the assumption that divergent varieties and species arise only through exposure to different environments, will expect that these groves, at least those on the same side of the mountain-range, will be occupied by the same species. Having found one set of species in a given valley, when he comes to a valley ten miles distant possessing the same conditions of soil, rainfall, vegetation, and shade, where the birds, reptiles, and insects are the same, where the mice and ants, their only known enemies, are the same, he naturally looks on the leaves and branches of the familiar trees for the snails he has found in similar stations not far distant; but what is his surprise to find only different species, all allied to, but quite distinct from, those he has previously known! Twenty miles from the first valley he renews his investigations, finding the forms of all the different groups still more divergent, though all the conditions of the environment are, so far as he can observe, the same.

He finally perceives that he must either assume that there are occult influences in the environment varying with progressive force with each successive mile, or he must give up the theory that the cause of this divergence is exposure to different environments.

3. *When the environment is the same in two districts occupied by allied species or varieties, it is evident that the differences that distinguish the latter cannot be advantageous, even though their differences include strongly contrasted habits.*

For in order that these differences should be advantageous, it is necessary not only that they should relate to the performance of vital functions, and therefore be differences of adaptation, but it is necessary that these differences of adaptation should relate to differences in the environment, so that the forms would be at some disadvantage if they should exchange districts. Adaptational specific differences are not always advantageous, and in such cases the divergence cannot be primarily attributed to diversity in the action of natural selection in the different districts. Under the protection of Isolation, diversity of natural selection may arise which helps in producing divergence; but when the environments are the same, the divergence is in no sense advantageous, and, in some cases, may even be disadvantageous.

A familiar example will perhaps put the distinction between the causes of existence and transformation and the causes of divergent existence and transformation in a clearer light. The forms of language are growths that are governed by the laws of utility as fully as the forms of varieties and species. Each language and each part of a language exists and persists only as it is found to be of use. The "Survival of the Fittest" is a law that is perhaps as conspicuous in the domain of language as it is in the organic world. Again, every language, like every organic species, is in many respects determined by the environment. A language, for example, developed in Java will present names for many plants and animals that will not be represented in a language developed in Greenland. But, granting all this, does it follow that linguistic differences are necessarily advantageous? The Polynesian system of counting by fours, and the Eskimo system that proceeds by scores, are undoubtedly useful systems; but is there anything advantageous in the difference? I think not, for each system is as well adapted to the environment of the other as to its own environment. We may look upon the more important parts of a language as persisting through their usefulness, the survival of the fittest being the law; but the divergent evolution which brings several languages out of one seems to be principally due to other principles which are closely akin to the principles that produce divergences in the organic world. The fundamental condition in both organic and linguistic divergence is Segregation; and, this being secured, diversity of habits, bringing diversity of aptitudes and diversity in the forms of survival, is sure to arise even when the environment is the same.

4. *Specific differences are not always differences of adaptation to the environment; and those that are not should not be attributed to the action of natural selection.*

It is admitted by every one that a distinction relating to a character that is of no use in the economy of the organism cannot have arisen under the influence of natural selection. Those who maintain that all specific distinctions are due to natural selection maintain at the same time that these distinctions are both adaptational and advantageous. There are naturalists who

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maintain that the very essence of the Darwinian theory is "that specific differences must be advantageous," and therefore adaptational; while they do not claim the same for generic, family, and ordinate distinctions, or indeed for varietal distinctions, if I rightly understand *. I have never seen any attempt to explain this supposed exception in the midst of the taxonomic series; and it seems to me that the break in the continuity of nature which this interpretation of the Darwinian theory supposes, should lead us to a very careful investigation of the facts before we accept it as a true interpretation of nature.

I shall content myself with pointing out one distinction, occasionally occurring between allied species, for which no use has ever been, or is likely to be, found. I refer to the distinction between what are known as dextral and sinistral forms. This distinction relates to the tortion of the animal and its shell upon itself. It is most easily recognized by placing the shell on its back with the aperture upward, and observing whether the aperture lies on the right side of the central columella of the shell or on the left. In the first case it is described as dextral, in the second as sinistral. In most families and genera of water-mollusks the sinistral form occurs only as a sport (amongst Mammals the heart is sometimes found on the right side), and even amongst air-breathing mollusks the dextral form vastly predominates. Amongst the *Achatinellinæ*, *Amastra* and *Leptachatina*, which are genera of terrestrial habits, are (with perhaps the exception of one or two species) dextral in form; while the other genera, which are plant-feeders and constantly hanging to branches or leaves, present many species that are constantly sinistral, and many others that are both dextral and sinistral. Why should *Achatinella adusta* in Panoa and Makiki be constantly sinistral, when its nearest allies found in the same valleys are both dextral and sinistral? Why should *Achatinella bacca* and *A. abbreviata* in Pałolo and Waialae be constantly dextral when the other species of *Achatinella* in the same valleys are for the most part sinistral? Is there any adaptation to the environment possessed by a dextral form which would be lost if the form was reversed? If not, natural selection could not have anything to do with that part of its character. *Bulimella rosea* is sinistral, while *B. bulimoides* is dextral. If in this respect they should exchange forms, would

* See letter from Mr. W. T. Thiselton Dyer in 'Nature,' vol. xxxix. p. 8.

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any disadvantage be experienced by either species? It is impossible to conceive of any disadvantage that would follow, and therefore I cannot believe that this difference in the two species is primarily due to natural, sexual, or any other form of selection.

There are many other specific distinctions presented in this family which seem to be of no advantage, though they are not so far removed from all suggestion of the possibility of use as the character we have just been considering. The brilliant colours and varied patterns presented by many of the arboreal species would be of advantage to themselves, if they served as warning of nauseous qualities to creatures that are liable to prey upon them; but no such creatures exist. The birds of the forest-region are exclusively fruit- and nectar-feeding, and the mice which in recent years have made sad havoc with the mountain snails, unfortunately do not spare the highly-coloured species.

There can be no doubt that when representatives of different groups or subgenera occupy the same trees they remain segregated through the influence of sexual instincts, which must be associated with some means of recognizing those of their own group; but it is not at all probable that the colours and patterns of any species are recognized by their mates, or have been developed under the influence of sexual selection. There is, therefore, strong reason to doubt whether selection of any kind has been concerned in the production of the beautiful colours and patterns of these species, unless possibly correspondences in colour within the limits of a genus are, in some cases, due to the inheritance of tendencies produced by selection when conditions were very different from what we now find. But the divergences in colour and pattern in the species of one genus cannot be thus explained.

5. *The average radius of distribution for species of the same value in different groups of closely-allied species varies in the different groups directly as the power and opportunity for migrating, and inversely as the plasticity and variability of the several groups.*

Comparing the distribution of the *Helices* of Europe with that of the *Achatinellinæ* of Oahu, the most striking contrast is found in the size of the areas occupied. *Helix pomatia* is distributed from England to Turkey, over an area two thousand miles in length, while of the seven genera of *Achatinellinæ* on Oahu

I know of but one species that seems to be distributed over the whole 36 miles of the main mountain-range, and this one is represented by three varieties belonging to different parts of the range and perhaps worthy to be regarded as different species. The species to which I refer is *Auriculella auricula* (Fér.), the typical forms of which are found on the eastern half of the mountain-range. On the other half of the range we find the closely allied forms to which I have given the manuscript names *solida* and *pellucida*. This great contrast in the size of the areas occupied must be due either to the greater plasticity of the Sandwich-Island species, or to their having inferior opportunities for migrating, or to both causes. As I become better acquainted with the great difference in the habits and circumstances of the contrasted species, I give increasing weight to the difference in the opportunities for migrating. With the continental species, floods must be one great means of distribution; but in the case of the insular species, the floods would carry floating individuals upon the grass-land or into the sea, in either case to perish. Again, the habit of travelling upon the ground, which belongs to most of the Helices of Europe and America, gives incalculable opportunities for migration which are not enjoyed by species that are strictly arboreal, as are many of the Sandwich-Island species. Most of the Sandwich-Island species are still further restricted in their opportunities by their inability to resist a dry atmosphere or exposure to the sun, which renders it necessary that they should remain in the isolated areas that are favoured with shade in the different valleys.

The habits of the different subgenera occupying Oahu are also instructive as throwing light upon the relative areas occupied by the species of the different genera. *Achatinella* and *Bulimella* seem to be the most restricted in their opportunities for migrating: first, because they are entirely arboreal in their habits, clinging to the trunks and branches of trees through their whole life-history; and, second, because, for the most part, they occupy the shady and damp thickets and groves, the shade in each valley being separated from similar shades in adjoining valleys by lofty and sparsely wooded mountain-ridges at each side of the valley and by open grass-land at the mouth of the valley. On the other hand, *Apex*, which for the most part occupies trees and shrubs on the ridges which are connected with each other through the central ridge of the mountain-range, and *Amastra* and *Lepta-*

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chatina, which are for the most part found on the ground under dead and decaying leaves, seem to possess better opportunities for migration than either *Achatinella* or *Bulimella*. Corresponding with these facts we find the species of *Achatinella* and *Bulimella* especially limited in the areas they occupy, while the species of *Apex*, *Amastra*, and *Leptachatina* are less so. For example, the area occupied by *Amastra turritella*, *A. tristis*, and *A. ventulus* includes the areas occupied by many species of *Achatinella* and *Bulimella*; and *Apex loratus* and *A. pallidus*, occupying the mountain-ridges, range from Makiki to Halawa, exceeding the range attained by any arboreal species occupying the valleys of the same region.

6. *When a group of divergent forms that are fertile with each other are being developed through the influence of local or geographical segregation, other conditions remaining constant, the number of forms that will be produced within a given area will vary inversely as the square of the average radius of distribution for the different forms.*

As this average radius of distribution may be taken as the measure of the power and opportunities for migration, we may say that other powers and opportunities remaining constant, *the number of species developed within a given area will vary inversely as the square of the power and opportunity for migration.*

Though migration is in one sense a cause of isolation, it is evident that the number of isolated groups of individuals does not increase with the increase of migration. Isolation is produced by the great contrast between ordinary and extraordinary combinations of opportunities for migration; and this contrast is as great in the case of species that have limited powers and opportunities, as in the case of those that have very great powers and opportunities. The number of isolations thus produced that can exist within the limits of a given area must vary inversely as the square of the power and opportunity for migration.

The facts of distribution we have been considering seem to correspond to this law.

7. *Forms that are most nearly related, and are therefore the least subject to sexual and impregnational segregation, are distributed in such a manner that their divergence is directly proportional to their distance from each other, which is also the measure of the time and degree of their geographical*

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segregation; while those that are most manifestly held apart by sexual instincts and impregnational incompatabilities do not follow this law.

Bulimella is represented by two groups of species, one of ovate form, the other elongated and with the outlines of the spire less rounded. The widest divergence between these groups is presented by species occupying the same districts and valleys, but the widest divergences in the species of either of these groups are found in valleys widely separated. In the latter case, the degree of geographical separation is probably an approximate measure of the time and degree of segregation, and therefore the measure of the degree of divergence; while, in the former case, the segregation is probably as complete between forms occupying the same valley as between those of widely separated valleys. There is reason to believe that in the eastern part of the island these two groups are not held apart by sexual segregation or segregate fecundity and vigour, for there is complete intergrading, and the divergence between the groups in any one valley is much less than is found in the north-west portion of the island, where sexual incompatibility seems to hold them apart.

Achatinella bacca and *A. abbreviata* completely intergrade with each other, but they are associated with a number of other species of *Achatinella* with which they do not intergrade, prevented it seems to me by mutual antipathy and sterility. We have, therefore, in the eastern valleys two groups of *Achatinella* completely segregated from each other, though occupying the same districts and in some measure the same stations; while in the other valleys the two groups coalesce, the different species occupying any one valley being only partially segregated by divergent habits of feeding

The different subgenera, which are undoubtedly segregated by divergent sexual instincts, as well as by physiological incompatibilities, are equally divergent, whether we compare forms from the same, or from distant valleys.

8. *The distribution of the varieties, species, and genera of* Achatinella *on this island is just such as would be produced by divergent evolution, which depends on segregation as a necessary condition even when the environments are different, and which always follows long-continued segregation even when the environment surrounding the different sections is the same.*

Increasing difference in the forms of natural selection does not necessarily depend on exposure to different environments, but does depend on some form of Independent Generation. It may be safely said of the multitude of varieties which inhabit the island of Oahu, that every one is more or less segregated from all other varieties. And I believe this will be found true concerning varieties in every part of the world. This fundamental fact would probably never have been denied, except for the delusive idea that the advantage of divergence would lead to the accumulation of divergence even if segregation were entirely wanting. What could be a greater mistake for the breeder of animals than to imagine that by selecting extreme variations and breeding them together he would in time secure well-marked races? It must be equally at variance with fact to suppose that any advantage secured by divergent variations can be preserved and accumulated while the different forms are freely intergenerating.

In the family we are considering, the chief forms of segregation are probably what I have called local, geographical, industrial, and sexual segregation, strengthened in many cases by segregate fecundity and vigour. As illustrating local segregation I would mention varieties and species of *Apex*, for the most part occupying the mountain-ridges which are all connected with each other, without the intervention of geographical barriers. Geographical segregation is illustrated in the forms of *Achatinella* and *Bulimella*, which for the most part occupy the deep valleys, the ridges forming barriers that are very rarely surmounted. Industrial segregation is illustrated by the closely-allied varieties of one group of species that occupy one valley, but are prevented from freely crossing by different habits of feeding. It is probable that sexual or seasonal segregation prevents the pairing of *Achatinella* with *Bulimella* when both occupy the same trees. Moreover, cross sterility would undoubtedly prevent the multiplication of the hybrids, if cross-unions ever do occur between forms so widely divergent. There can be no doubt that the same principles prevent the strongly marked groups of either genus from intergenerating; as for example, in the case of *Achatinella bacca* and *A. abbreviata*, which are intergraded with each other, but not with the surrounding species of *Achatinella*.

Again, divergent forms of natural selection do not necessarily depend on exposure to different environments. Industrial Segregation is produced by different methods of using the environ-

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ments; and the same cause will often produce diversity in the forms of natural selection affecting the segregated sections. Cumulative divergence in the methods of using the environment in the different branches of the species depends upon their segregation, and, therefore, increasing divergence in the forms of natural selection affecting the different branches depends on their segregation. But Industrial Segregation is not the only form of Independent Generation that opens the way for increasing diversity of natural selection. Geographical Segregation under the same environment, though it does not of itself produce divergent forms of selection, opens the way for change in the habits of feeding with diversity of natural selection in the different sections of the species. Take, for example, the species of *Achatinella*: in Manoa and Mikiki they chiefly occupy the Kukui (*Aleurites triloba*) and other trees, while in Kawailoa and that region they neglect the larger trees and take to the Lobelia and other shrubs and herbaceous plants.

But why should the degree of divergence increase with the continuance of the Segregation? The answer seems to be that the combined effects of the different principles of transformation in the segregated groups increase with the time of segregation; and, as independent transformation is never parallel, the divergence increases in the same ratio. Diversity of natural selection is undoubtedly one of the principles producing this divergence, even when the vegetation and physical conditions of the different districts are the same, for when the habits of feeding change, the natural selection must usually change. But there are cases of divergence accompanying Segregation in which the habits of feeding seem to have remained unchanged; and in such cases I explain the divergence in part by the principle that separation always involves more or less segregation, and in part by the influence of the four principles which I have called Assimilational, Eliminational, Amalgamational, and Fecundal Transformation. Of these, Eliminational and Amalgamational Transformation are perhaps the most constantly operative. The principle of unbalanced Elimination is closely allied to the principle that separation involves Segregation; for both represent phases of the fact that any small fragment of a species is incapable of propagating all the qualities of the species in the exact proportion presented by the average of the species.

Similar Facts in other Fields.

Many of the facts embodied in these eight propositions must have been observed wherever naturalists have studied the geographical distribution of the varieties and species of polymorphic genera; but in the distribution of the *Achatinellinæ* there are features of peculiar interest arising from the fact that the powers of migration possessed by the species of the surrounding environment are very much greater than those possessed by the *Achatinellæ*. Through this circumstance a comparatively uniform environment is produced in which the effects of Independent Generation unmodified by the effects of changed environment may be observed. The remarkable facts of distribution which we have on the island of Oahu are found in other parts of the Sandwich Islands, wherever this family occurs. I am also fully convinced that, in other parts of the world, wherever one genus or family of very low powers of migration is surrounded by a body of plant and animal forms possessing much higher powers of migration, these similar facts will present themselves whenever investigation is made.

The distribution of land-mollusks belonging to the genus *Partula* found on the Society Islands presents similar features. The island of Reiatea, which is but 14 miles in length and 3 or 4 miles in breadth, is the home of about 30 species and varieties, most of which are confined to areas only a few square miles in extent. I am not informed as to the distribution of the vegetation on which these species feed, but there is no reason to suppose they occupy limited districts corresponding to those occupied by the different species of *Partula*.

DIVERGENCE IN INSECTS.

The dependence of divergence on some form of Segregation is most clearly exemplified in insects, and though my studies are but limited in that field, I shall refer to a few cases, which may serve to direct attention to a class of facts of the highest interest not only to Entomology but to general Biology.

DIVERGENCE IN THE SPECIES OF THE LEPIDOPTEROUS GENERA *Erynnis* (*Pamphila*) AND *Thanaos* (*Nisoniades*).

These two genera of small North-American butterflies are worthy of the special attention of those who are studying the

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problems of divergent evolution; for they furnish strong indications that organisms which are with difficulty distinguished from each other by external form or colour, may, nevertheless, be well established species—segregated presumably by sexual instincts corresponding to sexual characters by which those of opposite sexes of the same species readily recognize each other, and probably cut off from the possibility of producing hybrids through incompatibility of physiological endowments. In the origin of some of these species Geographical Segregation may have had an important influence; but concerning others there can hardly be a doubt that the segregative influences, holding apart species that occupy the same districts, were, from the first, peculiarities of their sexual instincts and constitution. The reason for accepting this view of their origin is found in the fact that, though slightly divergent in other points, the characters by which they are clearly distinguished are found in the forms of the male genitalia; and in the characters of these organs we find clearly marked species, for the most part free from the intergrading forms which would certainly be presented if the different species were not prevented from crossing by sexual instincts or constitution.

A full description of these genera, with observations on the asymmetrical development of the right and left sides of the genital armature in *Thanaos*, will be found in Scudder's 'Butterflies of New England;' see also Mem. of the Boston Soc. Nat. Hist. ii. (1874), and Proceedings of the same Society for April 27, 1870, vol. xiii. p. 282 (1871).

Divergent Species of *Basilarchia*.

Basilarchia (Scudder) is an attractive genus of butterflies peculiar to North America, where it is represented by four or five species. Three of these are found in New England, and are minutely described in Scudder's 'Butterflies of New England,' from which I draw my information (pp. 250–305).

The distribution of these three species is of great interest, as it illustrates divergence both with and without Local Segregation. *B. Archippus* ranges over nearly the whole of the United States and over the southern portion of Canada. *B. Astyanax* occupies the valley of the Mississippi and eastward to the Atlantic from the Gulf of Mexico on the south to the lakes on the north. *B. Arthemis* is distributed from Newfoundland

and Nova Scotia on the east, over New England, Canada, the region of the lakes, away to the north-west, toward the confines of Alaska. It will be observed that the area of distribution of *B. Archippus* includes the whole of that of *B. Astyanax* and a large portion of that of *B. Arthemis*; while the areas of *B. Astyanax* and *B. Arthemis* overlap along the whole northern border of the territory occupied by *B. Astyanax*. This area of overlapping distribution in which the three species are associated is about a thousand miles in length, and from one hundred to one hundred and fifty miles in width.

Forms of Segregation that separate B. Archippus *from* B. Astyanax *and* B. Arthemis.

It is evident that, in the present condition of distribution, geographical barriers and territorial separation have nothing to do with the integrity of *B. Archippus* as a separate species. In other words, it is not under the influence of Geographical or Local Segregation. Whatever may have been its past history, these certainly are not the causes that at present prevent it from interfusing with other species of *Basilarchia* with which it is associated.

Again, Seasonal Segregation seems to have but little influence; for, though *B. Archippus* seems to appear 15 or 20 days earlier than the other species, the remainder of the breeding-season, which extends over many weeks, is coincident.

The habits and feeding instincts of this species must tend to separate it somewhat from *B. Arthemis*, for this latter species frequents forest-regions, especially when elevated and hilly, while *B. Archippus* is found in the open country in fields and meadows, especially in low levels. The eggs of *B. Arthemis* are chiefly deposited on the species of birch and willow that are found on the highlands; while the eggs of *B. Archippus* are chiefly deposited on the willows and poplars found on the low-lands, though on the White Mountains it occasionally extends its range to as high levels as *B. Arthemis*. There is therefore between these species a slight degree of Industrial Segregation; but this partial segregation does not prevent their being often found in the same fields, and unless held apart by sexual instincts and by partial infertility, hybrids, which are now very rare, would be very common.

We are therefore lead to believe that diversity of sexual instincts, accompanied by a considerable degree of cross-sterility, is the chief cause preserving the independent character of this species. Except for the sexual Segregation and Segregate Fecundity there is every reason to believe that this species could never have arisen, or, if it had arisen as a variety in some isolated locality, would have been submerged in the allied forms when its wider distribution was reached. This conclusion, which has been reached by observing the general relations of the species, is confirmed by a minute examination of the structure of the three species. We find that while the male genitalia of *B. Astyanax* and *B. Arthemis* differ but slightly, those of *B. Archippus* are considerably divergent. This is an index of the psychological and physiological relations of varieties and species of no small importance; for a comparison of many species shows that differences of this kind are usually accompanied by corresponding degrees of segregation in sexual instincts and of cross-sterility. In other words, we find that difference in the male genitalia, which is a form of segregate structure, is an index of Sexual Segregation and Segregate Fecundity.

The partial Segregation of B. Astyanax *and* B. Arthemis.

In the relations of these two species we find examples of segregative influences differing somewhat from those that have just been found in the case of *B. Archippus.* Regional Segregation, with exposure to different climates and adaptations to different food-plants, has undoubtedly had an important influence in the formation of these species; but, in the part of the country where they co-exist, their life-histories correspond completely, and cross-unions seem to be frequent. The hybrid form has been described as a separate species, and some entomologists have classed it as a dimorphic form of *B. Arthemis*, but Scudder gives several reasons for believing that it is the result of cross-unions between these two species. There are, however, several reasons for believing that partial Segregate Fecundity exists between the two species; for, in the strip of territory where the two are associated they do not completely coalesce, as would be the case if they were completely cross-fertile. In Scudder's 'Butterflies of New England,' pp. 159–160, we find mention of two species (*Cercyonis Alope* and *C. Nephele*), in which the cross-sterility must be considerably weaker than between the two species we are now considering; for, in the intermediate region

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in which their areas overlap, the intergrade forms are comparatively abundant. Moreover, the difference in the male genitalia of *B. Astyanax* and *B. Arthemis*, though much less than that which appears when either of these is compared with *B. Archippus*, is such as indicates a considerable degree of infertility.

In these two species we have then a good example of partial Segregation through distribution over areas, which, though overlapping, are for the most part distinct, reinforced by partial Segregate Fecundity which may or may not be accompanied by slightly divergent sexual instincts. There is also some Segregation resulting from the fact that the plants on which *B. Arthemis* seeks to deposit its eggs are chiefly the birches and willows of the hilly country, while *B. Astyanax* prefers fruit-trees of the Rosaceæ family, and other plants that are found in the more open country. These are, as I have shown in my paper on "Divergent Evolution through Cumulative Segregation," exactly the conditions that produce, in successive generations, increasing degrees of Segregate Fecundity.

Cumulative Segregation in the Formation of the above Species.

I judge that in the relations to each other of these three species we have the results of divergent evolution through cumulative segregation very clearly illustrated. In the earlier stages of divergence in this genus, *Basilarchia Archippus* with its fondness for the open fields must have become partially separated from the parent form from which both *B. Astyanax* and *B. Arthemis* have since sprung. The separation may have been in some measure due to what I have called Protectional Segregation; for we find that the form that has kept to the open country has through protective selection gained a very close resemblance to the colouring of *Anosia plexippus*, which is protected by its disagreeable qualities. The other form has probably gained compensative advantages by keeping closer to the woodlands. But the partial Segregation thus produced would never have resulted in constant specific differences if Segregate Fecundity had not arisen between the two forms. We may believe that some form of Impregnational Segregation (either Segregate Structure, Segregate Fecundity, or Segregate Vigour) was early introduced, and that under the protection of this barrier the specific distinctions of the two forms became fully established, though even now the barrier is not so complete as to entirely preclude hybrids between *B. Archippus* and each of the other species. Examples of both these hybrids are described by Scudder.

While this Segregation was being completed, one of the two forms thus created must have become subject to a new set of segregative influences, arising from wider distribution with diversity of climate and of habits of feeding, reinforced by a slight degree of Segregate Fecundity. *B. Astyanax* and *B. Arthemis* are the two species resulting from this last Segregation, and the process is so far from being complete, that wherever the areas of these two species overlap a hybrid form, which has been known as *B. Proserpina*, appears. That it is a hybrid is proved by the fact that it "varies most toward *Astyanax* where this prevails, and most towards *Arthemis* where that prevails," that it is found only in the narrow belt where the two species are brought into contact, and that it has been reported from so many points in this narrow belt that there is reason to believe that it occurs wherever the two species are brought into contact. If our exposition of the Segregations to which these species have been subjected is correct, they are cumulative in two respects—first because after one Segregation has been established another is superimposed, and second because a partial segregation established in one generation tends to become more complete in subsequent generations.

The primary causes in the whole process are the activities of the organisms acting upon each other and upon the environments in such a way as to produce, in the first place, Independent Generation with some degree of divergence, and then Unbalanced Natural Selection and other forms and transformation, which, acting upon selections of the species that are prevented from crossing, result in ever increasing divergence.

Divergent Evolution in the Periodical Cicada (*Cicada septemdecim*)*.

In this species we have examples of two quite distinct divergences, each depending on its own forms of Segregation, which are easily recognized.

The life-history of this insect covers 17 years and one or two

* My information is chiefly derived from the U.S. Department of Agriculture (Division of Entomology), Bulletin No. 8, by Dr. C. V. Riley.

months. The imago appears late in May, and for a little more than a month the males make the woods ring with their shrill stridulations. The eggs, which are deposited in the green twigs of trees, mature during the latter part of July; and each newly-hatched larva dropping to the ground, takes up a solitary subterranean life, which it follows till its period of 17 years is nearly complete. It then appears above the ground, passes into its winged stage, and enters on a few weeks of social life which closes its career. This species is widely distributed in that part of the United States that lies between the Atlantic shore and the Rocky Mountains. It does not, however, occur in Minnesota, Northern Michigan, or Northern New England. It is, however, represented by two races in every respect the same, except that one has a life-history of thirteen and the other of seventeen years. The 13-year race prevails in the Gulf States, while in New England and the Middle States the 17-year race is alone found. In Illinois, Missouri, Kansas, and in several of the Southern States the two races occur in the same localities; but it is evident that even in such localities it is only once in 221 years that there will be any opportunity for crossing between them, and we are informed by those who have made a special study of the subject that they do not cross when these opportunities occur.

These two races are therefore protected by partial Local Segregation; by Cyclical Segregation rendering it impossible that a brood of each occupying the same locality should have opportunity for crossing more than once in 17 generations of the shorter-lived race, or once in 13 generations of the longer-lived race; and by Sexual Segregation that shows itself in diversity of instincts preventing them from pairing when other conditions favour.

Whether devices have been tried to induce cross-unions, and whether such unions are unfruitful, I have never heard; but the simple fact that 15-year forms do not appear in localities where the two races are found, indicates that in nature they do not cross. Several such localities have been reported, but in none of them has an intermediate form been found. It seems, therefore, that we may safely draw the conclusion that we have here a case of complete Sexual Segregation between forms which to the human eye are undistinguishable, and which call their mates with stridulations which to the human ear are the same. Now I claim that in such races as these we have the beginning of divergent species—a beginning that lies in the segregative influences of constitutional and instinctive qualities persistently inherited by the two races, though the naturalist who examines specimens of the two races cannot distinguish them. All that is necessary to convert these two races into good species is the transformation of one or both of them while they are thus prevented from crossing; for we may be assured that the results of transformation under such circumstances will never be completely parallel.

Each of these races is again subdivided; for accompanying each is a diminutive form, differing somewhat in colour, not so early by eight or ten days in its first appearance, producing a quite distinct stridulation, and showing no disposition to associate with the larger form. This small form was described in 1851 by Dr. Fisher as a new species under the name *Cicada Cassinii*. Dr. Riley, however, hesitates to receive it as a separate species because the differences presented by the male genitalia are not constant. He says "there are sufficient differences to separate the two forms as distinct; but while the hooks of the large kind (*septemdecim*) are quite constant in their appearances, those of the smaller kind (*Cassinii*) are variable, and in some few specimens are indistinguishable from those of the large kind. This circumstance, coupled with the fact that the small kind regularly occurs with both the 17- and 13-year broods, would indicate it to be a dimorphic form of the larger, and only entitled to varietal rank"*.

I consider this case as of equal interest with the previous one; for it is an example of complete segregation between the forms of one species through diversity in their instincts. Whether these divergent instincts are sexual or social may be a matter of question; but in either case they are effectual in preventing crossing.

If future investigation shows that the small form is often produced directly from the eggs of the large form, it will have but little claim to be regarded as a separate race; but even then, if the small form breeds only with its own kind, as has been reported by several observers, and if the offspring persistently reproduce the characters of the parents, it will have to be considered something more than a dimorphic form of the large one. It would, in that case, be a dimorphic form that is assuming the

* United-States Department of Agriculture (Division of Entomology), Bulletin No. 8, p. 7.

character of a species. If the two forms were without segregative sexual and social instincts, then, with cross-fertility, the small form would be rapidly absorbed by the large form, which greatly preponderates in numbers; and with cross-sterility the small form would rapidly become extinct; for, through the comparative scarcity of their numbers, the representatives of the small form would have but little chance of mating with each other.

On the other hand, if the Sexual and Social Segregation is complete, it matters but little whether the forms are mutually sterile, for the separate races or species will be protected by the Positive Segregation produced by the divergent instincts, even if the Negative Segregation, depending on structural incompatibility and lack of physiological adaptation, is entirely wanting. It is only when associated with Positive Segregation that is partial in its results, that Negative forms of Segregation become important factors in the preservation of diverging forms.

In animals that pair, Segregation through sexual and social instincts plays a similar rôle in giving pre-emptive power to the males of a given species over the females of the same species, that is played by Potential and Prepotential Segregation in organisms whose fertilizing elements are distributed by wind or water. In the one case Instinctive, and in the other Potential Segregation, arising between varieties of the same species, marks these varieties as being the initial forms of divergent species.

This species presents another form of Segregation which is of much interest, though it has not yet resulted in forms that can be ranked as different races. I refer to the complete Cyclical Segregation that exists between the different broods of a given race appearing in different years. Of the 13-year race there are seven broods, and of the 17-year race fourteen. As an example of different broods occurring in the same region I would mention the two broods in the district of Columbia, one appearing in 1885 and at intervals of seventeen years thereafter, and another appearing in 1894 and at intervals of seventeen years thereafter. We have no means of testing the sexual or social instincts of these different broods, for they never appear in the same year. No one can say whether if they could be brought together they would be found as indisposed to breed with each other as are the 13-year and 17-year races. But, be that as it may, the two forms are as completely segregated as they can be, and the opportunity for independent, and therefore divergent, transformation is much

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the same as that which exists between the 13- and 17-year races. Two or three of the States have but one brood each; but in Ohio seven 17-year broods are reported, and in North Carolina one 13-year and six 17-year broods. I judge, however, from the reports that, even in these last-mentioned States, there are but few places, if any, where more than three broods overlap.

I have not seen any discussion of the causes that have produced these broods, but if we may believe that they have existed for a thousand generations, a possible if not probable cause is found in the unsettled conditions of climate that must have attended the breaking-up of the great ice-period. During years of diminished cold, colonies may have taken possession of regions which were too cold for their development at the return of the 17-year period when the offspring should have appeared; and still some of the benumbed and delayed pupæ may have survived, making their appearance one, two, three or more years later, when conditions were more favourable. The following observation referred to by Dr. Riley, in explanation of the accelerated or retarded appearance of sporadic individuals, throws some light on the origin of the different broods:—"That circumstances favourable or otherwise may accelerate or retard their development was accidentally proven in 1868 by Dr. E. S. Hull, of Alton, Ill., as by constructing underground flues for the purpose of forcing vegetables, he also caused the Cicadas to issue as early as the 20th of March, and at consecutive periods afterwards till May, though, strange to say, these premature individuals did not sing. They frequently appear in small numbers, and more rarely in large numbers, the year before or the year after their proper period. This is more especially the case with the 13-year broods"*. That climate has been an important factor in the development of the 13- and 17-year races is indicated by the fact that most of the districts occupied by the 17-year race lie north of lat. 38°, and most of those occupied by the 13-year race lie south of that line, though in Illinois there is a 13-year brood as far north as lat. 40°. Dr. Riley has not referred to the coincidence, but it seems to me a fact of some interest in this connection, that the southern limit of the great ice-cap which covered Canada and the northern part of the United States during the Glacial epoch extended along

* U.S. Department of Agriculture (Division of Entomology), Bulletin No. 8, p. 8, by C. V. Riley.

an irregular line between the parallels of lat. 38° and 40°. Lying south of the ice-region there was probably a considerable belt of country covered with pines and other conifers not adapted to the breeding of this species, so that both races, if they then existed, must have been crowded into the southern portion of the region now occupied by the 13-year race.

Instinctive and Cyclical forms of Segregation, such as cause the independent generation of the races and broods of this species, are usually associated with clearly developed specific distinctions relating to form, colour, and function. This does not, however, prove that the segregative divergence was subsequent to the general divergence in other respects; for if complete segregation continues for many generations it is likely to be followed by other divergences, and the divergent forms are then ranked as separate species. Moreover, the number of generations covered by the initial stage in which the different sections are only races is very small compared with those that are likely to be covered by the stages when they are separate species and genera. It is only, therefore, by rare chance that we find two forms that are still in the earliest stage of divergence and are, at the same time, completely segregated by constitutional differences. Again, segregative endowments are usually developed somewhat gradually; and while the segregation is advancing other transformations take place, so that by the time all crossing has come to an end the different sections have become well-marked species. Sometimes, as in the case of the three species of Butterflies already considered, there is more or less crossing after the sections have become quite distinct species. Such cases, however, as are presented by the 13- and 17-year races and by the different broods of this species of *Cicada*, show that complete segregation may be produced by the psychological and physiological constitution of different races, while distinctions of form, colour, and manner of call are entirely wanting so far as we can observe. This has seemed impossible to some naturalists, especially since Darwin has admitted that cross-sterility cannot be attributed to natural selection, and has therefore attributed it to the indirect effects of other qualities which have been produced by natural selection.

The great contrast in this respect between the species of *Basilarchia* and the 13- and 17-year races of *Cicada septemdecim* may perhaps be partially explained by the fact that the latter spend the greater part of their existence under ground, where the conditions have not been seriously changed since the close of the last glacial period. Again, one generation of the 17-year race of *Cicada* covers a period equal to that of thirty or forty generations of the *Basilarchia*, bringing thirty or forty fluctuations of climate, food, &c. to the latter, while the former is, for the most part, protected from serious fluctuations.

It is of course equally impossible to prove by all-inclusive observations, either that transformation is never completely parallel in sections of a species that are prevented from crossing, or that independent generation long continued is sure to result in independent transformation, and therefore in divergence; but it is of no small interest that we find in the 13-year and 17-year races of this species the strongest proof that there are sometimes divergences which our senses do not perceive. If our senses were a sufficient test, it might be maintained that between these races a high degree of local and Cyclical Segregation has existed for many generations, without any other form of transformation having arisen to increase the divergence; but if our informants are correct when they tell us that these races do not cross when appearing in the same district and at the same time, we need not hesitate to affirm that there must be some distinguishing characteristics by which those of one race are able to find each other, as well as segregative instincts which lead them to choose each other's society; and, even if our informants are mistaken in supposing that cross-unions do not occur, there must be some form of incompatibility between the two races, resting on divergent endowments; for otherwise we should find hybrid descendants with periods of more than 13 and less than 17 years' duration.

Concluding Remarks.

Outline of the Argument in support of the Theory of Divergent Evolution through Cumulative Segregation.

(1) The invariable experience of mankind in producing domestic races shows that Segregation is a controlling factor. The Segregation that produces domestic breeds and races is found to be of two kinds: first, that which is produced by men who designedly preserve the different styles of variation presented by one species, while at the same time they prevent them from crossing; and, second, that which commences in the indiscri-

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minate division of the species into sections that are prevented from freely crossing through their being under the care of separate tribes of men, and which is changed into decided Segregation through the diversity of selection, or of some other transforming principle, to which the different sections are sure to be exposed; for it is found that these principles when brought to bear on separated sections never produce completely parallel effects.

(2) The paramount effects of Independent Generation having been shown in the broad fields of biological experiment presented by the domestication of plants and animals, the question is next raised whether species in a state of nature are subjected to influences dividing the individuals of one species into sections that are prevented from crossing; and if they are, how far this Independent Generation involves Segregate Generation.

In my paper entitled "Divergent Evolution through Cumulative Segregation" it was shown that there are many classes of activities by which the individuals of a species are thus divided, and that, in the majority of cases, the very process that separates them assorts them into classes with reference to one or more points of character; thus producing segregation that is completely parallel in its character to the segregation that is designedly produced by the pigeon-fancier between his various breeds of pigeons.

In the earlier half of the present paper I have shown that the indiscriminate division of the species, which often results from migration or geological changes, and sometimes from other causes, inevitably involves some Segregation; and whenever the transforming influences of the other factors of evolution begin to operate in the different sections, this initial Segregation is inevitably intensified and the divergence increased; for it is in the last degree improbable that change produced by these principles of transformation in sections that are prevented from crossing should be completely parallel in the different sections, even when exposed to the same environments. Having shown that the forms of Segregation produced in nature are analogous to those produced in artificial breeding,—

(3) The last step is to show, as has been attempted in the latter half of the present paper, that the relations to each other of varieties, species, genera, and the higher groups are such as would necessarily be presented if all such differences were the result of evolution that is always dependent on some form of

Segregation, but not always on diversity of natural selection, nor always on exposure to different environments.

We have found that persistent differences, whether varietal, specific, or generic, are not all adaptational, for some of them have no relation to utility; and that adaptational differences are not all advantageous, for some of them relate to adaptations that would meet with equal success if the organisms should exchange habitats; but that in every case divergence, whether utilitarian or non-utilitarian, whether advantageous or disadvantageous, is not maintained without Independent Generation.

Reply to Criticism.

In view of the examples of divergence that have been discussed in this paper, I think I may state, as in my previous paper, "It is therefore evident that the simple fact of divergence in any case is not sufficient ground for assuming that the divergent form has an advantage over the type from which it diverges" *. Mr. Wallace has criticised this statement, using the following words †:—"It seems to me that throughout his paper Mr. Gulick omits the consideration of the inevitable agency of natural selection, arising from the fact of only a very small proportion of the offspring produced each year possibly surviving. .... He omits from all consideration the fact that at each step of the divergence there was necessarily selection of the fit and less fit to survive; and that if, as a fact, the two extremes have survived, and not the intermediate steps that led to one or both of them, it is a proof that *both* had an advantage over the original less specialized form." But what if the type from which the new form diverges is surviving at the same time that the new form survives? And what if both the forms are surrounded by the same environment which they use in different ways? Where then is the proof that the newer form has an advantage over the older form? This was the class of facts I had been considering in the preceding paragraphs, which led to the conclusion criticised by Mr. Wallace; and instead of omitting "the consideration of the inevitable agency of natural selection," it was the very thing I was considering, as will be seen by referring to p. 213. I had pointed out, that when a segregated portion of a species exposed to the

* Linnean Society's Journal, Zoology, vol. xx. p. 214.

† Nature, vol. xxxviii. p. 491.

same environment changes its habits, learning to appropriate resources that had not been previously used, it becomes a new intergenerating group "*in which a new and divergent form of natural selection is established*;" but that the result of the divergence thus produced is not necessarily advantageous, and may for many generations be somewhat disadvantageous. As I was aware that many naturalists would consider it absurd to suppose that disadvantageous, or even non-advantageous instincts, ever persist and become the occasion of divergent selection, I referred to Darwin's opinion that such might be the case with sexual instincts, and that the progenitors of man were deprived of their hairy coat by sexual selection that was, in its earlier stages, disadvantageous. I am not aware that Darwin has ever attempted to show how divergent sexual instincts arise and become permanently fixed as distinguishing characters of varieties and species. "The Advantage of Divergence," the principle on which he relied to account for divergent habits, producing divergent natural selection, he never attempted to apply here; and, above all, when he believed the newer instinct to be either non-advantageous or disadvantageous, as contrasted with the older instinct, he certainly could not have attributed advantage to the resulting divergence. As I have pointed out on previous occasions, Darwin assumed a psychological divergence in the sexual instincts of a species in order to account for the divergence in their secondary sexual characters relating to form, colour, &c.; and as there is no reason given why the psychological divergence should take place, or why it should precede the change in form and colour, the theory of Sexual Selection, as presented by Darwin, is incomplete and unsatisfactory, especially in its relations to divergent evolution. If he had thrown light on the causes of divergence in sexual instincts, he would have found the same or similar principles applicable to the explanation of divergence of all kinds. But my object in referring to his opinion here is to point out that he was free to admit that permanent divergence in sexual instincts may be non-advantageous, or even somewhat disadvantageous; and if this is true of sexual instincts, I do not see why it may not be equally true of industrial instincts. I think there is ample evidence that, when segregation has been established, divergence which is neither advantageous nor disadvantageous often arises in industrial as well as other instincts, and that these instincts may introduce new forms of natural, sexual, or social selection. The relations which exist between habits and their objects are in many species constantly varying in such a way as to constitute a series of experiments; and when independent generation exists between different sections of a species, there is nothing to prevent divergence in the results of those experiments in the different sections, even when exposed to the same environment.

In Darwin's 'Posthumous Essay on Instinct,' published as an Appendix to Romanes's 'Mental Evolution in Animals,' on pages 378–384 mention is made of certain "imperfections and mistakes of instinct," and of certain instincts "that are carried to an injurious excess," and of others that are "small and trifling." Of the last-named he says:—"I have not rarely felt that small and trifling instincts were a greater difficulty in our theory than those which have so justly excited the wonder of mankind; for an instinct, if really of no considerable importance in the struggle for life, could not be modified or formed through natural selection." After mentioning several which might perhaps be considered trifling but are really of great importance to the species, he alludes to a few that seem to be "mere tricks" or "habits without use to the animals." Mr. Romanes, referring to these cases, offers the following explanation on p. 275 of the same work (I quote from the New York edition, Appleton & Co., 1884):—"We have seen abundant evidence that non-adaptive habits occur in individuals, and may be inherited in the race. Therefore, if from play, affection, curiosity, or even mere caprice, the animal should perform any useless kind of action habitually...., and if this habit were to become hereditary in the similarly constituted progeny, we should have a trivial or useless instinct." As an example of a strongly inherited non-adaptive instinct in a wild creature may be mentioned the cackling of the wild hen of India after having laid an egg. This habit is referred to by Darwin as one that may be slightly detrimental; but all that is necessary to put it beyond the developing influence of natural selection is that it should fail of bringing advantage to the species; and that it is of no advantage will, I think, be generally admitted. If, then, species differ in regard to instincts that are non-advantageous, they are liable to present non-advantageous differences in form and colour, resulting either from the same causes that have produced the divergent instincts, or from divergent forms of natural, sexual, and social selection produced by these instincts; it will, however, be found that Segregation is the cause, or at least the necessary condition, on which the divergence depends.

In the present paper I have mentioned cases, representative of multitudes of others, in which there is divergence between two varieties or species occupying different districts, but surrounded by the same environments. In such cases, the differences presented by the separate forms and the divergence by which the differences have been produced, cannot be regarded as advantageous; for if the forms should exchange districts, the environment being the same, no disadvantage would be experienced; and this is equally true whether the differences relate to industrial adaptations, or to adaptations between the sexual instincts and the other secondary sexual characters of the group, or to characters that are absolutely non-utilitarian.

Mr. Wallace says that, in my previous paper, he looks in vain for any proof that cumulative segregation produces cumulative divergence; but at the same time, he claims that the segregation of which I speak, and which I have illustrated by a supposed case in the breeding of pigeons, is a form of selection which he calls "selection by separation." Adopting his phrase for the moment, I understand that he fully admits that in domestication "selection by separation" will produce divergence. Does he then doubt that the same process produced by natural causes will result in divergence? Or does he deny that "selection by separation" ever takes place in nature? He will probably grant that wherever natural causes act upon the representatives of a species in such a way that in each generation those presenting one style of variation are led to breed together and are prevented from breeding with other kinds, there divergence will certainly follow. This is what I call Segregation. That without it there is no cumulative divergence, and that with it there is always divergence, is amply proved by the universal experience of man in the domestication of plants and animals. All that is lacking is the consistent application of our knowledge to the theory of evolution.

Segregation is a process of much deeper significance than indiscriminate isolation, with which he seems to confound it; and one which in nature arises from a wide range of causes, some of which I have pointed out. But isolation without assortment of the forms according to any principle by which those of a kind are brought together, is often transformed into Segregation by the operation of the principles of transformation in the isolated sections of the species. This change is often brought about by the difference of the environments to which the organism is exposed in the isolated areas. This one form of Segregation has been clearly pointed out by Darwin, though he did not recognize segregation as a necessary condition for divergence. There are, however, many other ways in which nature produces a similar result. Some of these are operative when the organism is distributed in isolated districts but surrounded by the same environment, and some of them have to do with the development of non-adaptative divergences, which cannot come under the cumulative influence of natural selection.

It thus appears that Independent Generation co-operating with Natural Selection is one form of the wider principle of Segregation, which, in its many forms, is the ever present condition preceding cumulative divergence. Whatever divides the representatives of a species in such a way that those of a kind are made to intergenerate while prevented from intergenerating with other kinds is a cause of Segregation. This is my definition of Segregation; and my theory is that whatever causes Segregation causes divergence, and without Segregation there is no cumulative divergence. Now, in order to refute the theory it is necessary to show either that Segregation does not take place in nature, or that it is not accompanied by divergence, or that divergence takes place without Segregation. As Mr. Wallace has not attempted to prove any one of these counter propositions, I think his criticism is aside from the main issue. Even if my paper presents "a body of theoretical statements" with "no additional facts," this does not show that the theory is incorrect or the new use of the old facts unimportant in the explanation of divergent evolution. 'The Origin of Species' was filled with new theories applied to old facts. The importance of Cumulative Divergence through Cumulative Segregation, if a fact, is admitted. Is it a fact? is then the question that needs to be discussed; but, if Segregation is supposed to be no more than Isolation, the discussion will be of little avail.

In the Journal of the Royal Microscopical Society, 1889, part i. pages 33–4, will be found an appreciative, though a very brief review of my theory, closing with the suggestion that fuller elucidation is needed of the alleged tendency in nature to transform separation, when long continued, into increasing segregation

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and divergence. Want of space in my first essay made it necessary to postpone the full discussion of this part of the theory; but in the present paper I have sought to point out some of the more manifest principles on which this general law of *Intension* rests. There are undoubtedly other principles of transformation, which, when combined with separate breeding, inevitably produce divergent instead of parallel evolution; but the principles pointed out in this paper are sufficient to establish the general tendency, and to show that natural selection is by no means the only principle on which the law rests. If we could obtain sections of a species presenting exactly the same average character, and if we could prevent all the principles of transformation from coming in to aid in the process, separate breeding under such conditions would perhaps never produce divergence; but, as separation never produces exactly equivalent sections, it always tends to introduce transformation, through changed or unbalanced action in principles that would otherwise be unchanged and balanced in their action and therefore without transforming influence, and transformation in the separated sections inevitably becomes divergence. We thus gain an explanation of the fact that Isolation, even when accompanied by exposure to the same environments, usually introduces divergent forms of Selection, natural, sexual, social, or dominational, and often new effects from the action of other principles. Independent Generation precedes and determines the possibility of the divergence, and if it is segregative, it also determines in a measure the form of the divergence; but if it is simply separative, the form of the divergence depends on some other principle or principles.

APPENDIX.

Construction of the Permutational Triangle.

In the last chapter of my paper on "Divergent Evolution through Cumulative Segregation" (p. 250) I referred to the Permutational Triangle, which I had constructed to facilitate the solution of a problem there raised in regard to the degree of probability of extinction that would, under certain conditions, result from Segregate Fecundity. The first four lines of the table were obtained by direct observation on the permutations of letters arranged to represent the pairing of animals entirely

f.25r

THE PERMUTATIONAL TRIANGLE.

Factorials.	*Factors.*	*Occurrents.* (0)	*Concurrents.* Of the first degree. (1)	Second degree. (2)	Third degree. (3)	Fourth degree. (4)	Fifth degree. (5)	Sixth degree. (6)	Seventh degree. (7)	Eighth degree. (8)	Ninth degree. (9)	Tenth degree. (10)
		1						*The*	*Ini*	*tial*	*Num*	*ber.*
$\lfloor 1=$ 1= no. of *occasions.* in 1st line.	(1)	0	1					*The*	*firs*	*t li*	*ne.*	
$\lfloor 2=$ 2= „ in 2nd	(2)	1	0	1				*The*	*seco*	*nd li*	*ne.*	
$\lfloor 3=$ 6= „ „ 3rd	(3)	2	3	0	1			*The*	*thir*	*d li*	*ne.*	
$\lfloor 4=$ 24= „ „ 4th	(4)	9	8	6	0	1						
$\lfloor 5=$ 120= „ „ 5th	(5)	44	45	20	10	0	1					
$\lfloor 6=$ 720= „ „ 6th	(6)	265	264	135	40	15	0	1				
$\lfloor 7=$ 5,040= „ „ 7th	(7)	1,854	1,855	924	315	70	21	0	1			
$\lfloor 8=$ 40,320= „ „ 8th	(8)	14,833	14,832	7,420	2,464	630	112	28	0	1		
$\lfloor 9=$ 362,880= „ „ 9th	(9)	133,496	133,497	66,744	22,260	5,544	1,134	168	36	0	1	
$\lfloor 10=$ 3,628,800= „ „ 10th	(10)	1,334,961	1,334,960	67,485	222,480	55,650	11,088	1,890	240	45	0	1

lacking in instincts or qualities that secure the pairing together of those of one kind.

For example, let A, B, C represent three females of three varieties of pigeons, and a, b, c three males of the same varieties, all occupying one aviary. Now supposing they are devoid of Segregating instincts, and that they all pair, what are the probabilities concerning the pairing of the males with their own kind? These will be clearly shown by arranging the letters representing one of the sexes in one fixed order, placing the letters representing the other sex underneath in every possible permutation of order. If we make six experiments the probability is that in 2 cases none, in 3 cases one, and in one case 3, will pair with their own kind. These numbers constitute the four terms of the third line. The first, second, and fourth lines were constructed in the same way, but for the construction of the tenth line in this way I estimated that several years of constant writing would be required. The remaining lines here given were therefore constructed according to the following rules, which were discovered by studying the first four lines. The discussion of different methods of constructing the Permutational Triangle, and the interesting properties of the same when constructed, must be deferred; but I may say here that I believe it will be found an important instrument for estimating a large class of probabilities.

A	B	C
a	*b*	*c*
a	c	b
c	a	b
b	a	*c*
b	c	a
c	*b*	a

One method of constructing any line of the Permutational Triangle from the preceding line.

(1) Of any given line, any desired number, except the first, may be obtained by multiplying the preceding number of the preceding line by the factor of the given line and dividing the result by the figure marking the degree of correspondence of the column of the desired number. (2) The first number of any line is one less or one more than the second number of the same line, according as the factor of the line is an odd or an even number.

f.1r

[FROM THE AMERICAN JOURNAL OF SCIENCE, VOL. XL, JULY, 1890.]

ART. I.—*The Inconsistencies of Utilitarianism as the Exclusive Theory of Organic Evolution;* by Rev. JOHN T. GULICK.

Natural Selection an Exclusive Theory with some Biologists.

IN a previous article, entitled "Divergent Evolution and the Darwinian Theory,"* I dwelt chiefly on the need of a bionomic theory that should explain polytypic, as well as monotypic, evolution. One of the chief deficiencies in Darwin's discussion of the "Origin of Species," is that he does not distinguish with sufficient clearness the conditions that are necessary for the transformation of an original species into a new species, when the former disappears in the process, leaving the latter to occupy its place, and the conditions that are necessary for the production of two or more species from one original species. In this paper it may be instructive to examine a vigorous attempt that has been made so to expound the theory of natural selection (which Darwin considered as inadequate to cover all the forms of monotypic evolution,) that it shall serve as the full explanation of both monotypic and polytypic evolution in all organisms lower than man. By confining our attention to Mr. Wallace's very interesting and suggestive volume on "Darwinism," we shall be better able to judge of the possibility of producing a self-consistent theory on this basis; but we should bear in mind that the same view is maintained by many naturalists, and that parallel statements abound in their writings. Mr. Wallace's volume not only embodies the mature reflections of one of the joint authors of

* This Journal, vol. xxxix, pp. 21–30.

the theory of natural selection, but it fairly represents that phase of biological theory which considers diversity of natural selection through exposure to different environments the only cause of divergence. The following passage will show the exclusive nature of his theory: "A great body of facts on the one hand, and some weighty arguments on the other, alike prove that specific characters have been, and could only have been, developed and fixed by natural selection because of their utility. We may admit that among the great number of variations and sports which continually arise many are altogether useless without being hurtful; but no cause or influence has been adduced adequate to render such characters fixed and constant throughout the vast number of individuals which constitute any of the more dominant species."—*Darwinism*, p. 142. This is in strong contrast with the following passage from the close of the Introduction of the sixth edition of the "Origin of Species," which is the last one that received the revision of the author: "I am fully convinced that species are not immutable, but those belonging to what are called the same genera are lineal descendants of some other and generally extinct species, in the same manner as the acknowledged varieties of any one species are the descendants of that species. Furthermore, I am convinced that Natural Selection has been the most important, but not the exclusive, means of modification." On page 421 of the same edition, Darwin calls attention to the fact that this passage has "been placed in a most conspicuous position" in the different editions of his work, and complains of the writers who misrepresent his conclusions on this point.

Facts that are Neglected or Denied.

Though Darwin maintains that besides the inherited effects of use and disuse and the direct action of the external conditions there are other forms of variation leading to permanent modifications of structure independently of natural selection (*Origin of Species*, 6th London ed., p. 421), he does not attempt to explain how these divergences arise. Neither Darwin nor Wallace appears to have observed, that, as in domestication, the isolated breeding of other than average forms, in whatever way it is secured, is the one necessary, and always effective, cause of divergence, so, in nature, wherever there arises the isolated breeding of other than average forms, there divergence will be produced; or that, as exposure to different environments is only one of the causes that lead isolated bands of men to desire and select different types of variation in the same species of animal, so exposure of wild species to different environments is only one of several classes of causes that may subject isolated portions of one of these species to different forms of selection, producing divergence; or, again, that as differences in the uses to which men put an animal are not necessarily useful differences, so the differences in the uses which isolated portions of a species make of the environment, though they produce diversity of natural selection, leading to permanent divergence, are not necessarily useful differences. These, with other allied doctrines, which were presented in my paper on "Divergent Evolution Through Cumulative Segregation," have received adverse criticism from Mr. Wallace in the work mentioned above. He says: "In Mr. Gulick's last paper (*Jour. of Linn. Soc.*, Zoology, vol. xx, pp. 189–274), he discusses the various forms of isolation above referred to, under no less than thirty-eight different divisions, with an elaborate terminology, and he argues that these will frequently bring about divergent evolution without any change in the environment or any action of natural selection. The discussion of the problem here given will, I believe, sufficiently expose the fallacy of his contention, but his illustrations of the varied and often recondite modes by which practical isolation may be brought about, may help to remove one of the popular difficulties in the way of the action of natural selection in the origination of species." (Note on p. 150).

In this passage Mr. Wallace seems to take issue with each and all of my propositions; but after a careful study of his whole discussion, one cannot but be in doubt whether he fully dissents from any of them. This uncertainty arises either from his failing to recognize distinctions which I have made, or from ambiguities and inconsistencies in his own statements.

Extending the meaning of Natural Selection does not save the Theory.

He represents me as contending that divergent groups are frequently found in which the action of natural selection is wanting. He here fails to distinguish between the absence of diversity in the action of natural selection and the absence of any action of the same principle. I have never maintained that any species can long escape the action of natural selection; but I have that natural selection cannot produce transformation of a race unless it secures the propagation of other than average forms of that race; that it cannot be a cause of divergence unless to this condition is added the independent generation (i. e., isolation) of groups that are subjected to some diversity in its action; and, that, in isolated groups, some of the divergent characters may be due to other causes of trans-

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formation. In the passage I have quoted from p. 142, he expresses great confidence in the proof that all specific characters are developed and fixed by natural selection; but in the discussion that follows concerning the influence of natural selection, he claims as belonging to this principle sets of influences which are usually included under sexual selection, and which he cannot regard as due to the reactions between the species and its environment. (See *Darwinism*, pp. 282–5), and, even then, it is found too narrow to cover all the facts of specific divergence; for, when he comes to consider the origin and development of accessory plumes, he has to abandon the theory to which he has clung through the greater part of the book. Speaking of the enormously lengthened plumes of the "bird of paradise and of the peacock," he says, on page 293, "The fact that they have been developed to so great an extent in a few species is an indication of such perfect adaptation to the conditions of existence, such complete success in the battle of life, that there is, in the adult male at all events, a *surplus of strength, vitality and growth power, which is able to expand itself in this way without injury.* That such is the case is shown by the great abundance of most of the species which possess *these wonderful superfluities* of plumage. * * * *Why, in allied species, the development of accessory plumes has taken different forms, we are unable to say, except that it may be due to* that *individual variability* which has served as the starting point for so much of what seems to us to be strange in form or fantastic in color, both in the animal and vegetable world." (The italics are mine.) According to the theory he has elsewhere maintained, *these superfluities* of form and color which are not controlled by natural selection should present, "a series of inconstant varieties mingled together, not a distinct segregation of forms" (p. 148); but in this passage he teaches that they have assumed different forms in allied species. On p. 141 he maintains that characters which are neither beneficial nor injurious are from their very nature unstable and cannot become specific, while here he offers a suggestion as to how they have become specific. There is then a problem that presses for solution, namely, the explanation of permanent divergence in characters that are useless without being hurtful (p. 142), unless he considers his suggestion "that it may be due to individual variability" an adequate explanation; and I presume he does not. On page 142, he says of characters that are "useless without being hurtful:" "No cause or influence has been adduced adequate to render such characters fixed and constant; but in speaking of "the delicate tints of spring foliage, and the intense hues of autumn," he says: "As colors they are unadaptive, and appear to have no more relation to the well-being of the plants themselves than do the colors of gems and minerals. We may also include in the same category those algæ and fungi which have bright colors—the red snow of the Arctic regions, the red, green, or purple seaweeds, the brilliant scarlet, yellow, white, or black agarics, and other fungi. All these colors are probably the direct results of chemical composition or molecular structure, and being thus normal products of the vegetable organism, need no special explanation from our present point of view; and the same remark will apply to the varied tints of the bark of trunks, branches and twigs, which are often of various shades of brown and green, or even vivid reds or yellows" (p. 302). He here seems to admit that instead of useless specific characters being unknown, they are so common and so easily explained by "the chemical constitution of the organism" that they claim no special attention.

Inconsistency in extending the meaning of Environment.

If Mr. Wallace accepts the definition of natural selection which makes it the survival of those members of a species which are best fitted to its environment (and this is the scope he seems to assign to it in the earlier half of Chapter V where the matter is under special discussion), then he ought to admit that changes in a species produced by the action of the members of the species on each other although they are adaptive are not due to natural selection. If, on the other hand, natural selection is made to include the actions and reactions of the species on itself (and this he does on pages 282–5), then certainly he ought to admit that there may be changes in the action of natural selection without any change in the relations of the species to the environment. One way to escape this dilemma is to extend the definition of the environment so as to include every influence that affects the species, whether it is within the species, or external to it; but this reduces his doctrine, that without change in the environment there is no change in the organism, to the fruitless truism that without some cause there is no change in the organism. An example of Mr. Wallace's extending the meaning of the environment so as to include the action of the members of a species on each other, is found on page 149. After mentioning several arguments intended to show the impossibility that isolated portions of a species should diverge while exposed to the same environment, he remarks, "It is impossible that the environment of the isolated portion can be exactly like that of the bulk of the species. It cannot be so physically, since no two separated areas can be exactly alike in climate and soil; and, even if

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they are the same, the geographical features, size, contour, and relation to winds, seas and rivers would certainly differ. Biologically, the differences are sure to be considerable. The isolated portion of a species will almost always be in a much smaller area than that occupied by the species as a whole, hence it is *at once in a different position as regards its own kind.*" He then enumerates several differences in the biological environment that are liable to occur; but the point I wish now to note is, that he mentions as one of the differences in the environment the "*different position as regards its own kind.*" This is exactly the difference which, in so far as it is the prevention of intercrossing and the consequent unification of endowments and habits, constitutes isolation; and unless he is able to show that this difference is incapable of producing any divergence, his contention is unsustained. But he here yields the point at issue, by mentioning this amongst the effective differences. The only way to escape the force of his concession is to claim, as he virtually does here, that isolation, being the separation of the isolated fragment from the influence of the original stock, is in itself a difference in the environment. By taking this position, however, he involves himself in another contradiction; for, if isolation is a difference in the environment, why does he deny that it has a direct influence in producing change in the organism?

Diversity of Natural Selection during exposure to the same Environment.

Another discrepancy in Mr. Wallace's theory is that, while he rightly assigns great importance to diversity of natural selection arising from divergent habits in appropriating the resources of the same environment, exhibited by different sections of the same species occupying the same area, he, nevertheless, insists that the representatives of a species, isolated in different areas of the same environment, will be neccessarily subjected to the same influences from natural selection, and will inevitably maintain the same characters and of course, the same habits. That he believes divergent habits may arise, when the divergent groups are occupying the *same area*, and are prevented from crossing simply by the divergence of habits, will be seen by the case of the varieties of wolves mentioned on p. 105, and by some of the cases mentioned on pp. 108 and 117; also by the statement, on p. 119, that—"When one portion of a terrestrial species takes to a more arboreal or a more aquatic mode of life, the change of habits itself leads to the isolation of each portion," and by a similar statement at the bottom of p. 145. That he believes there can be no change, either of habits or structure, when portions of the same species are isolated in *different areas* under the same environment, appears from the statement on p. 149, that—"If the average characters of the species are the expression of its exact adaptation to its whole environment, then, given a precisely similar environment, and the isolated portion will inevitably be brought back to the same average of characters." And this he maintains will be the case even "if we admit, that, when one portion of a species is separated from the rest there will necessarily be a slight difference in the average character of the two portions."

Does the Difference in the Environment increase with each successive Mile?

If the divergences presented by the Sandwich Island land molluscs are wholly due to exposure to different environments, as Mr. Wallace argues on pages 147–150, then, there must be completely occult influences in the environment that vary progressively with each successive mile. This is so violent an assumption that it throws doubt on any theory that requires such support. Of all the suggestions made by Mr. Wallace concerning possible and inevitable differences in the environments presented in the successive valleys, it seems to me not one meets the requirements of the case, or throws any light on the subject. The one suggestion which is quite applicable as an explanation is the one already quoted that "the isolated portion is at once in a different position as regards its own kind." This is, I believe, a most potent difference, which (as Mr. Wallace's language seems to indicate), is directly introduced by isolation, and (adhering to the meaning usually given to environment,) is not at all due to difference in the environments presented in the different areas.

Unstable Adjustments disturbed by Isolation.

There is a sentence in another chapter of Mr. Wallace's book which attributes to isolation (though without recognizing the important results that must follow) just that kind of influence in introducing a certain class of physiological divergences, which I claim for it in introducing, not only physiological, but also psychological and morphological divergences. I claim that there is, in many species, more or less variation with unstable adjustment, in the habits which determine what forms of food it shall appropriate, and that, when a few individuals of such a species (the offspring perhaps of a single female) are isolated, this adjustment is often so disturbed by

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the failure of the few individuals to completely represent the average character of the species and by their being freed from competition, and wide interbreeding with those of their own kind, that divergent habits of feeding are formed. I further claim that for the production of this result it is not at all necessary that the environments presented in the isolated districts should differ in any respect. Indeed if all but one pair of a variable species should be destroyed, the descendants of that pair, remaining in the same area and under the same environment, would probably differ more or less from the original stock. Those that breed together must have habits that enable them to do so; and the offspring of those that interbreed widely will for the most part, inherit the powers and habits that enabled their ancestors to interbreed widely; but if the offspring of a single family are carried to an isolated area presenting the same environment, there will be nothing to ensure the perpetuation of exactly the original powers and habits, unless the power of heredity is such that each pair is sure to transmit the complete average character of the whole species; and this is not the condition of all species that pair, if of any. Within the limits of each freely interbreeding portion of a species a mutual harmony and adjustment of habits is preserved, because it is the condition of propagation within those limits; but between portions that are prevented from interbreeding there is nothing but heredity to prevent divergence in the kinds of adjustment; and in variable species, the probability is that divergence will in time show itself more or less distinctly. Though Mr. Wallace considers this reasoning fallacious when applied to divergence in habits he uses an exactly parallel reasoning in the portion of the following passage which I designate by italics. "*It appears as if fertility depended on such a delicate adjustment of the male and female elements to each other, that, unless constantly kept up by the preservation of the most fertile individuals, sterility is always liable to arise. . . . So long as a species remains undivided, and in occupation of a continuous area, its fertility is kept up by natural selection; but the moment it becomes separated, either by geographical or selective isolation, or by diversity of station or of habits, while each portion must be kept fertile inter se, there is nothing to prevent infertility arising between the two separated portions.* As the two portions will necessarily exist under somewhat different conditions of life, and will usually have acquired some diversity of form and color—both which circumstances we know to be either the cause of infertility or to be corelated with it—the fact of some degree of infertility usually appearing between closely allied but locally or physiologically segregated species is exactly

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what we should expect" (p. 184–5). Notwithstanding this statement he does not seem to have grasped the idea, that in the geographically isolated portions as well as in the others, the "different conditions of life" of which he speaks, may be the different relations to the environment into which the separated portions are brought by their divergent habits, without any reference to inevitable differences in the size and contours of the different areas or in any other features of the environments; and that the divergence in the habits may be directly due to the prevention of interbreeding between separated portions which inevitably differ in average character, especially if they are very small portions.

Isolated portions differ in varying degrees from the average character of the Species.

The italicized portion of the passage last quoted attributes to isolation, in stronger language than I should be willing to use, a direct influence in producing divergence in the adjustments on which fertility in the different portions of the species depend. I should prefer to say that in *some species* the adjustments on which fertility depends are so delicate that, adjustments producing perfect fertility within one intergenerating portion of the species, will not produce fertility in another portion that has been long isolated. I do not make my statements so sweeping as his concerning the divergent influence of isolation on any one class of characters, but I include all classes of inheritable characters, in sexually producing organisms, as coming under its influence. I also insist that the direct influence of isolation in producing divergence is in proportion to the degree of segregation, which varies immensely in different forms of isolation which are equally complete as preventives of intercrossing. A very stable and homogeneous species may be divided by geological subsidence into two large sections, each represented by a vast number of individuals. In such a case the difference in the average character, and consequently the degree of segregation, of the two sections will be infinitesimally small, and the influence of the isolation thus produced will chiefly consist in its preserving in the different sections any diversities that may arise in the effects of natural selection, or of other principles of transformation. The isolation between the land animals of Ireland and Britain, which Mr. Wallace cites as adverse to my theory, is of this kind. Again, there may be transportation and isolation of very small fragments of a very variable species. In such a case separation may involve a degree of segregation that from the first produces perceptible divergence. Again, the process by which

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the isolation is produced may be in itself segregative, in that it brings together those endowed in some special way, causing them to breed together, and preventing them from breeding with others. This is especially the case with Sexual, Social, and Prepotential, Segregation, and in some degree with Industrial Segregation. Isolation thus produced is in its very nature segregative, and would result in divergence if diversity of natural selection did not arise in the different sections of the species. Segregation with divergence may also be produced by natural selection or some other principle of transformation co-operating with some form of isolation that of itself is not perceptibly segregative. As segregation of other than average forms always produces divergence, and without it there is no divergence, I claim that it is the fundamental principle of divergent or polytypic evolution. Natural selection, which is the exclusive propagation of those better adapted to the environment, when it results in the preservation of other than average forms, produces confluent or monotypic evolution; but it is never the cause of divergence, except when co-operating with some principle of isolation in such a way that the two principles produce segregation. Failure to recognize these distinctions, prevents Mr. Wallace from understanding my theory, and leads him to represent me as claiming for isolation all that I claim for segregation.

Incompatibilities arise during Positive Segregation.

On pages 173–186, Mr. Wallace maintains that "Natural selection is, in some probable cases at all events, able to accumulate variations in infertility between incipient species" (p. 174); but his reasoning does not seem to me conclusive. Even if we grant that the increase of this character occurs by the steps which he describes, it is not a process of accumulation by natural selection. In order to be a means of cumulative modification of varieties, races or species, selection, whether artificial or adaptational, must preserve certain forms of an intergenerating stock, to the exclusion of other forms of the same stock. Progressive change in the size of the occupants of a poultry-yard may be secured by raising only bantams the first, only common fowls the second, and only Shanghai fowls the third year; but this is not the form of selection that has produced the different races of fowls. So in nature rats may drive out and supplant mice; but this kind of selection modifies neither rats nor mice. On the other hand, if certain variations of mice prevail over others through their superior success in escaping their pursuers, then modification begins. Now, turning to p. 175, we find that in the illustrative case introduced by Mr. Wallace, the commencement of infertility between the incipient species is in relations to each other of two portions of a species that are locally segregated from the rest of the species, and partially segregated from each other by different modes of life. These two local varieties, by the terms of his supposition, being better adapted to the environment than the freely interbreeding forms in other parts of the general area, increase till they supplant these original forms. Then, in some limited portion of the general area, there arise two still more divergent forms, with greater mutual infertility and with increased adaptation to the environment, enabling them to prevail throughout the whole area. The process here described, if it takes place, is not modification by natural selection. The natural selection of which he speaks does not arise till, with each advancing step, a new and complicated adjustment (which introduces the two new forms, each with unabated fertility with its own kind but with diminished fertility with the other kind) has been attained by some other process. That other process is the one described in the passage I have already quoted from pp. 184–5, where, according to my apprehension, the cause of divergence is more correctly stated than it is in the passage now under consideration. In the latter part of my paper on *Divergent Evolution through Cumulative Segregation* I have shown that the different kinds of incompatibility, preventing complete fertility between incipient species (and there called forms of Negative Segregation), cannot arise except as accompaniments of Positive Segregation in some form; but that, having once arisen in connection with partial Positive Segregation, they increase from generation to generation by a law that is quite distinct from natural selection. It was also shown that endowments only partially segregative (as, for example, somewhat divergent habits of feeding), when not concurrent with any forms of cross incompatibility, are liable to be obliterated by crossing; but, when associated with segregate fertility and cross infertility, will increase from generation to generation, even if the mongrels are as well adapted to the environment as the pure forms. I at the same time called attention to the fact that, when associated with some form of partial positive segregation (as divergent habits of feeding, or segregative sexual and social instincts), greater vigor, of pure forms, as contrasted with the mongrels, would have the same effect as their greater fertility. In other words, Segregate Vigor would preserve a partially segregated variety as effectual as Segregate Fecundity.

Incompatibilities will disappear unless preserved by Positive Segregation.

Mr. Wallace has given a very instructive computation on pages 181–4; but it does not seem to me to prove, as he supposes, that infertility between the individuals of a species cannot increase "unless correlated with some useful variation," but that it cannot arise, except as a transitory variation, unless associated with some positively segregative principle, causing those to pair together which are fertile with each other. My contention is that, without some positive form of segregation fecundity and cross sterility can never arise; and that after it has arisen under segregation, no amount of correlation with useful variation will preserve it, if the positive segregation is removed. If, for example, all the species of humming birds were brought together in one country, and were deprived of all segregative habits and instincts, it certainly would not require many generations to reduce them to one species. If equally adapted to the environment, the species that would succeed in perpetuating itself would be the one represented by the largest number of individuals; or if several species were entirely cross fertile and were in the aggregate represented by a larger number of individuals than any other similar group of species or than any single species, then, the resulting species would be the hybrid descendants of this most numerous group. All the other species would become extinct through failing to mate with "physiological complements."

Why any need of distinctive Recognition Marks for those whose Ancestors had but one set of Marks.

An example of one of the effects of divergence being treated as if it were the primary cause of divergence is found on pages 217–228 and 284, where the need of distinctive characters for easy recognition is given as the chief cause of divergence in calls, odors, and colors. The importance of distinctive characters by which the members of a species may distinguish their mates from those of other species cannot be exaggerated; but how does it happen that the descendants of one stock which had originally but one set of such characters, have become segregated into groups, needing distinctive marks. By confounding the problem of successive, monotypic adaptation with that of coexistent, polytypic adaptation the real causes of divergence have been obscured and misapprehended. The diversity of Sexual and Social Selection, which Mr. Wallace in these passages speaks of as natural selection, is due to diversity of sexual and social instincts which in their turn have been produced by different forms of segregation. For a fuller exposition of this subject I would refer to my paper on "*Divergent Evolution through Cumulative Segregation*" (Linn. Soc. Jour. Zoology, vol. xx, pp. 234–8). The principles which I have called Sexual and Social Segregation, Mr. Wallace has mentioned in several places under the name "selective association," or "selective isolation," but he does not recognize the fact that, whenever this principle segregates forms whose immediate ancestors were not segregated, it must be the direct cause of divergence; and that, when divergent forms that have arisen under Industrial and Local Segregation are brought together through increase of numbers, this principle is often the one cause preserving varieties that would otherwise be obliterated. With plants whose pollen is distributed by the wind, and probably with both vegetable and animal forms whose fertilizing elements are distributed by water, Prepotential Segregation plays the same role as the segregative instincts of higher animals. As this principle depends on the greater rapidity with which the male and female elements of the same variety or species combine, as contrasted with the elements of different varieties and species, we might call it isolation through selective impregnation, just as Mr. Wallace has called the instinctive segregation, "isolation through selective association." Whatever names we give these two principles, they must be important factors in divergent evolution.

Segregation produces Domestic Races, why not Species?

Mr. Wallace seems to be opposed to the idea that some form of isolation is essential to divergence; but in his argument he yields so much that I cannot but think his opposition is largely due to his misinterpreting the theory. Mr. Romanes has mentioned eight or ten forms of isolation; and Mr. Wallace says I have discussed thirty-eight forms; but neither of us claim that these are the only possible forms; nor do we claim that any form of this principle is essential to the transformation of one species into another when the original one disappears in the process. The phrase "new species" as used by Mr. Wallace in the following passage is ambiguous; but the second sentence seems to indicate that he is here discussing divergence as well as simple transformation. He says: "Most writers consider the isolation of a portion of a species a very important factor in the formation of new species, while others maintain it to be absolutely essential. This latter view has arisen from an exaggerated opinion as to the power of intercrossing to keep down any variety or incipient species and merge it in the parent stock. But it is evident that this can only occur with varieties that are not useful, or which, if

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useful, occur in very small numbers." . . . (p. 144). Near the end of the same chapter, after presenting arguments in favor of this position, and after reviewing some of the facts which I have presented concerning the divergences of Sandwich Island land molluscs, he remarks—"We have, however, seen reason to believe that geographical or local isolation is by no means essential to the differentiation of species, because the same result is brought about by the incipient species acquiring different habits or frequently a different station; and also by the fact that different varieties of the same species are known to prefer to pair with their like and thus to bring about a physiological isolation of the most effective kind" (p. 150). Except that he has used "physiological isolation" where I should have used psychological segregation, this last passage is as completely in accord with what I have presented in my paper on "*Divergent Evolution*" as it could have been if he had copied my statements. But how is this passage, and one of similar import on page 185, to be reconciled with his own statement just quoted from page 144. On pages 217, 218 and 226, he bases his argument for the importance of different coloration in closely allied species on the obvious necessity for means "to secure the pairing together of individuals of the same species," if a new species is to be kept "separate from its nearest allies." He here assumes the fundamental fact on which the theory of segregation rests. All that is wanting is its recognition as a universal principle on which all permanent divergences, whether varietal or specific necessarily depend. In the formation of domestic variations it is fully recognized; for he says, "It is only by isolation and pure breeding that any specially desired qualities can be increased by selection" (p. 99). If experimental biology shows this to be a constant law, is there any good reason for not applying it in the general theory of organic evolution? Seeing it is admitted that artificial selection, unaided by isolation, is of no avail in producing divergent races, how can it be claimed that natural selection, unaided by isolation, is of any avail in producing varieties and species. Again, as in domestication, the segregate breeding of other than average forms always produces divergence, have we any reason to doubt that, when the same process takes place in the grouping of organisms in a natural state, the result will also be divergence?

The discrepancies to which I have referred are it seems to me due to deficiencies in the theory which Mr. Wallace maintains in common with many others. These problems that drive the exclusive utilitarian into various inconsistencies, can, I am convinced, be consistently explained by the theory of Divergence through Segregation.

26 Concession, Osaka, Japan.

[From the American Journal of Science, Vol. XL, December, 1890.]

Art. LV.—*The Preservation and Accumulation of Cross-infertility;* by John T. Gulick.

In his work on "Darwinism" in a section entitled "The Influence of Natural Selection upon Sterility and Fertility," Mr. Wallace reaches the conclusion that "If it [the cross-infertility] was so closely correlated with physical variations or diverse modes of life as to affect, even in a small degree, a considerable proportion of the individuals of the two forms in definite areas, it would be preserved by natural selection." (p. 178). That the infertility of an incipient species with its nearest allies is often preserved and accumulated, no one can doubt; but there are, it seems to me, very strong reasons for believing that this can never be due to natural selection. Natural selection is the *exclusive* breeding of those best adapted to the environment of the species, through the failure to propagate of those that are less adapted; and the *separate* breeding of those that are equally adapted introduces a wholly different principle. In order to produce the cumulative modification of a variety, selection, whether natural or artificial, must preserve certain forms of an intergenerating stock to the exclusion of other forms of the *same* stock I may select bantams as the object of my attention for a few years, and then excluding them, raise only Shanghai fowls; but this is not the form of selection by which these divergent races were produced. Again, if rats should supplant mice in any country, some persons might call it natural selection, but such natural selection would modify neither rats nor mice. On the other hand if certain variations of mice are better able than the rest to escape their pursuers, they will leave the most numerous offspring, and modification of species will commence. Now if we turn to page 175 of Mr. Wallace's book, we find, that in

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the illustrative case introduced by him, the commencement of the cross-infertility is in the relations to each other of two portions of the species partially segregated from the rest by occupying a definite part of the general area, and partially segregated from each other by different modes of life. These two physiologically segregated local varieties, being, by the terms of his supposition, better adapted to the environment than the more freely interbreeding forms in the other parts of the general area, increase till they supplant these original forms. Then, in some limited portion of the general area, there arise two still more divergent varieties, with greater mutual infertility, and therefore with still less commingling of the two, and with power to prevail throughout the whole area.*

The process here described, if it takes place, is not modification by natural selection, but a supplanting which does not produce modification, and which does not take place till a new and complicated adjustment has arisen in a portion of the species that is partially segregated, by occupying a definite portion of the area. This new adjustment introduces two new varieties, each with unabated fertility with the other variety; and the process, or principle, by which it is reached receives no explanation in the section we are now considering; but from what he says on page 184, we may judge that his only explanation is an application of the principle of Intensive Segregation, more especially that form of this principle which I have described as the effect of isolation on unstable adjustments, but which Mr. Wallace has rejected as untenable. Moreover, in the supposed case pictured by Mr. Wallace, the principle, by which the two forms are kept from crossing and are preserved as permanently distinct forms, is no other than that which Mr. Romanes and myself have discussed under the terms Physiological Selection and Segregate Fecundity. Not only is Mr. Wallace's exposition of the divergence and the continuance of the same in accord with these principles which he has elsewhere rejected, but his whole exposition is at variance with his own principle, which, in the previous chapter, he vigorously maintains in opposition to my statement that many varieties and species of Sandwich Island land molluscs have arisen while exposed to the same environment in the isolated groves of the successive valleys of the same mountain range. If he adhered to his own theory "The greater infertility between the two forms in one portion of the area"

* This brief outline of the method by which Mr. Wallace thinks cross-infertility has been produced and accumulated, though given, in another connection, in my article on *Utilitarianism as the Exclusive Theory of Organic Evolution* (this Journal, July, 1890), is here repeated, that the correspondences and divergences in the different theories we are here discussing may be better apprehended.

would be attributed to a difference between the environment presented in that portion and that presented in the other portions; and the difficulty would be to consistently show how this greater infertility could continue unabated when the varieties thus characterized spread beyond the environment on which the character depends. But, without power to continue, the process which he describes would not take place. In order to solve the problem of the origin and increase of infertility between species he gives up his own theory and adopts not only the theory of Physiological Selection but that of Intensive Segregation through Isolation, though he still insists on calling the process natural selection; for on page 183 he says, "No form of infertility or sterility between the individuals of a species can be increased by natural selection unless correlated with some useful variation, while all infertility not so correlated has a constant tendency to effect its own elimination." Even this claim he seems to unwittingly abandon when on page 184 he says: "The moment it [a species] becomes separated either by geographical or selective isolation, or by diversity of station or of habits, then, while each portion must be kept fertile *inter se*, there is nothing to prevent infertility arising between the two separated portions."

Mr. Wallace adopts these two fundamental doctrines of the theory of Divergent Evolution through Segregation, but he does not apply them exactly as I would. Why, for example, should he resort to the supposition that when the two divergent varieties occupying the extensive area are everywhere somewhat infertile with each other the increase of that character is gained *only in a limited portion of the area*, and then spreads by conquest? Would it not be simpler, and at the same time truer to the facts of nature, to assume, that the divergent variety can not arise except as it is aided by some form of positive segregation, preventing free crossing with the parent stock; and that in cases where this prevention is only partial, any cross-infertility once introduced will diminish the swamping effect of the crossing that occurs, and will *everywhere* tend to increase, because the majority of each generation of the pure form will be the descendants of those whose cross-infertility was above the average. There are, moreover, other forms of negative segregation equally effective with cross-infertility and Segregate Fecundity. It often occurs that when segregation with divergence has once begun to show itself, the variations that are most fully endowed with the new character, (though less adapted to the new mode of life than the old form is to the old mode of life) will best escape both the severe competition with the rest of the species and the swamping effect of crossing. In time the pressure for food becomes as

f9v

great with the new form as with the old, and escape from competition ceases. Under these circumstances either the mal-adaptation or the infertility of the hybrids will be of the highest importance in preventing swamping. But, is it necessary to suppose, as Mr. Wallace does, that the infertility will disappear in cases where the hybrids are as well adapted as the pure forms? In such cases, during the preliminary period when escape from competition is gained most fully by those most fully segregated, there will naturally be a rapid accumulation of all segregative endowments; but, when that condition ceases, there will still be a sufficient reason for the continuance, or even increase, of the cross-infertility in the fact that more than half of each generation of the pure form will be the descendants of those whose cross-infertility and other segregative endowments are above the average. This principle is one form of what I have called Self-Cumulative Segregation.

This law of self-accumulation does not seem to apply to cross-infertility (or to any other form of negative segregation) that is not associated with positive segregation. Nor is it quite clear that, when unassociated with negative segregation it applies to positively segregating characters (such as social and industrial instincts that lead animals of one kind to pair together, and the prepotency of the pollen of a given kind on the stigma of the same kind securing a similar result for plants). When, however, characters producing positive but incomplete segregation are associated with those producing negative segregation, both classes of characters must tend to increase till the segregation becomes pronounced. As soon as this point is reached, the Reflex Selection, by which the different portions of the species have been kept in harmonious relations with each other, is suspended, and there is nothing but the force of heredity to hold them in correspondence; but the force of heredity, securing this correspondence, has itself been created by the long continued Reflex Selection, and when this is removed, it gradually fails, and divergences of all kinds multiply, increasing the incompatibility of the two forms. Thus arises diversity of habits, diversity of sexual and social instincts, and diversity in the affinities of the male and female elements; and in each respect this diversity tends toward the point of complete incompatibility.

Positive segregation diminishes the amount of crossing, and negative segregation diminishes the swamping effect of crossing when it occurs. Negative segregation may be of the following forms: (1) lack of fertility of first crosses and of the hybrids, which I call Segregate Fecundity; (2) lack of Vigor in hybrids, which I call Segregate Vigor; (3) lack of adaptation in hybrids, which I call Segregate Adaptation; (4) lack of escape from competition in hybrids, as compared with pure forms, which I call Segregate Escape from Competition. Of these all but the 4th were considered in my paper on "Divergent Evolution through Cumulative Segregation," where I endeavored to show that in their coöperation with positive segregation they were parallel factors producing similar results. Now in his supposed case (pp. 173-9), Mr. Wallace has treated the Segregate Adaptation, or hybrid maladaptation, as if it were the effective factor by which the hybrid infertility is alone enabled to increase or even continue. I see no reason why this should be so. The effectiveness of these negative factors in preserving a species must depend on their being associated with positive segregation; but the effectiveness of any one of the negative factors is not destroyed by the absence of the others; though Segregate Escape from competition is, under ordinary conditions, confined to the preliminary stages of divergence; and may, in certain cases, be the necessary condition leading to the other forms of negative segregation.

Mr. Wallace's criticism of the theory of physiological Selection (pp. 180-3) is unsatisfactory; (1) because he has adopted the fundamental principle of that theory, on pages 173-9, in that he maintains that without the cross-infertility the incipient species there considered would be swamped; (2) because he assumes that physiological selection pertains simply to the infertility of first crosses, and has nothing to do with the infertility of mongrels and hybrids; (3) because he assumes that infertility between first crosses is of rare occurrence between the species of the same genus, ignoring the fact that, in many species of plants the pollen of the species is prepotent on the stigma of the same species when it has to compete with the pollen of other species of the same genus; (4) because he not only controverts Mr. Romanes' statement that cross-infertility often affects "a whole race or strain," but he gratuitously assumes that the theory of Physiological Selection excludes this "racial incompatibility" which Mr. Romanes maintains is the more probable form, and bases his computation on the assumption that the cross-infertility is not associated with any form of positive segregation; (5) because he claims to show that "all infertility not correlated with some useful variation has a constant tendency to effect its own elimination, while his computation only shows that, if the cross-infertility is not associated with some form of positive segregation, it will disappear; and (6) because he does not observe that the positive segregation may be secured by the very form of the physiological incompatibility. Many species of plants may be promiscuously distributed over the same area, and still be com-

f. 10v

pletely segregated by what I have called Potential Segregation. In other words, if the pollen of each species is potent only when falling on the stigma of the same species then the species are completely segregated though growing in the same area. Still further two species may be fairly fertile when artificially crossed, and yet be completely segregated while growing together, through the fact that the pollen of either species when falling on its own stigma will be prepotent over the pollen of the other species even though the alien pollen has fallen upon the stigma considerable earlier. This I have called Prepotential Segregation. Now a variety that is segregated from the parent form by prepotential Segregation and cross-infertility, will neither fail of propagating nor be swamped by crossing though it is indiscriminately mingled with the parent form. In my paper on "Divergent Evolution" I have referred to this special combination of positive and negative segregation, produced by the incompatibility of the male and female elements, and have endeavored to show that when these characters occur together, they tend to increase in intensity according to a law of self-accumulation. (Linn. Soc. Jour. Zool., vol. xx, pp. 239–40, 259–60). Without here entering into any computation, it is evident that the prepotency of the pollen of each kind with its own kind, if only very slight, will prevent cross-fertilization as effectually as a moderate degree of instinctive preference in the case of an animal, and if segregate fecundity, (i. e. cross-infertility) is added it will tend to keep the variety from the swamping effect of the little crossing that occurs, and the variations that are above the average in these characters will have the largest influence on the pure form in each successive generation.

I regard Physiological Segregation as including all kinds of incompatibility between the male and female elements of different groups, whether these groups are varieties of one species, or species of one genus, or species of different genera, or species representing still more divergent groups; and I maintain that the importance of this principle in the origin and continuance of divergent groups, cannot be exaggerated in the case of organisms whose fertilizing elements are freely distributed by wind or water; for in these cases the segregate compatibility and cross incompatibility of the male and female elements may be the means by which the prevention of free crossing is secured, as well as the means by which the swamping effect of the crossing that occurs is prevented. There is, it seems to me, strong reason to believe that this principle is a leading factor in the segregation of multitudes of water animals, as well as in the segregation of species of plants, whether terrestrial or aquatic; and Mr. Romanes has rightly emphasized the importance of investigations in this line.

26 Concession, Osaka, Japan.

[From the American Journal of Science, Vol. XL, December, 1890.]

Art. LV.—*The Preservation and Accumulation of Cross-infertility*; by John T. Gulick.

In his work on "Darwinism" in a section entitled "The Influence of Natural Selection upon Sterility and Fertility," Mr. Wallace reaches the conclusion that "If it [the cross-infertility] was so closely correlated with physical variations or diverse modes of life as to affect, even in a small degree, a considerable proportion of the individuals of the two forms in definite areas, it would be preserved by natural selection." (p. 178). That the infertility of an incipient species with its nearest allies is often preserved and accumulated, no one can doubt; but there are, it seems to me, very strong reasons for believing that this can never be due to natural selection. Natural selection is the *exclusive* breeding of those best adapted to the environment of the species, through the failure to propagate of those that are less adapted; and the *separate* breeding of those that are equally adapted introduces a wholly different principle. In order to produce the cumulative modification of a variety, selection, whether natural or artificial, must preserve certain forms of an intergenerating stock to the exclusion of other forms of the *same* stock I may select bantams as the object of my attention for a few years, and then excluding them, raise only Shanghai fowls; but this is not the form of selection by which these divergent races were produced. Again, if rats should supplant mice in any country, some persons might call it natural selection, but such natural selection would modify neither rats nor mice. On the other hand if certain variations of mice are better able than the rest to escape their pursuers, they will leave the most numerous offspring, and modification of species will commence. Now if we turn to page 175 of Mr. Wallace's book, we find, that in

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the illustrative case introduced by him, the commencement of the cross-infertility is in the relations to each other of two portions of the species partially segregated from the rest by occupying a definite part of the general area, and partially segregated from each other by different modes of life. These two physiologically segregated local varieties, being, by the terms of his supposition, better adapted to the environment than the more freely interbreeding forms in the other parts of the general area, increase till they supplant these original forms. Then, in some limited portion of the general area, there arise two still more divergent varieties, with greater mutual infertility, and therefore with still less commingling of the two, and with power to prevail throughout the whole area.*

The process here described, if it takes place, is not modification by natural selection, but a supplanting which does not produce modification, and which does not take place till a new and complicated adjustment has arisen in a portion of the species that is partially segregated, by occupying a definite portion of the area. This new adjustment introduces two new varieties, each with unabated fertility with the other variety; and the process, or principle, by which it is reached receives no explanation in the section we are now considering; but from what he says on page 184, we may judge that his only explanation is an application of the principle of Intensive Segregation, more especially that form of this principle which I have described as the effect of isolation on unstable adjustments, but which Mr. Wallace has rejected as untenable. Moreover, in the supposed case pictured by Mr. Wallace, the principle, by which the two forms are kept from crossing and are preserved as permanently distinct forms, is no other than that which Mr. Romanes and myself have discussed under the terms Physiological Selection and Segregate Fecundity. Not only is Mr. Wallace's exposition of the divergence and the continuance of the same in accord with these principles which he has elsewhere rejected, but his whole exposition is at variance with his own principle, which, in the previous chapter, he vigorously maintains in opposition to my statement that many varieties and species of Sandwich Island land molluscs have arisen while exposed to the same environment in the isolated groves of the successive valleys of the same mountain range. If he adhered to his own theory "The greater infertility between the two forms in one portion of the area"

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would be attributed to a difference between the environment presented in that portion and that presented in the other portions; and the difficulty would be to consistently show how this greater infertility could continue unabated when the varieties thus characterized spread beyond the environment on which the character depends. But, without power to continue, the process which he describes would not take place. In order to solve the problem of the origin and increase of infertility between species he gives up his own theory and adopts not only the theory of Physiological Selection but that of Intensive Segregation through Isolation, though he still insists on calling the process natural selection; for on page 183 he says, "No form of infertility or sterility between the individuals of a species can be increased by natural selection unless correlated with some useful variation, while all infertility not so correlated has a constant tendency to effect its own elimination." Even this claim he seems to unwittingly abandon when on page 184 he says: "The moment it [a species] becomes separated either by geographical or selective isolation, or by diversity of station or of habits, then, while each portion must be kept fertile *inter se*, there is nothing to prevent infertility arising between the two separated portions."

Mr. Wallace adopts these two fundamental doctrines of the theory of Divergent Evolution through Segregation, but he does not apply them exactly as I would. Why, for example, should he resort to the supposition that when the two divergent varieties occupying the extensive area are everywhere somewhat infertile with each other the increase of that character is gained *only in a limited portion of the area*, and then spreads by conquest? Would it not be simpler, and at the same time truer to the facts of nature, to assume, that the divergent variety can not arise except as it is aided by some form of positive segregation, preventing free crossing with the parent stock; and that in cases where this prevention is only partial, any cross-infertility once introduced will diminish the swamping effect of the crossing that occurs, and will *everywhere* tend to increase, because the majority of each generation of the pure form will be the descendants of those whose cross-infertility was above the average. There are, moreover, other forms of negative segregation equally effective with cross-infertility and Segregate Fecundity. It often occurs that when segregation with divergence has once begun to show itself, the variations that are most fully endowed with the new character, (though less adapted to the new mode of life than the old form is to the old mode of life) will best escape both the severe competition with the rest of the species and the swamping effect of crossing. In time the pressure for food becomes as

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great with the new form as with the old, and escape from competition ceases Under these circumstances either the mal-adaptation or the infertility of the hybrids will be of the highest importance in preventing swamping. But, is it necessary to suppose, as Mr. Wallace does, that the infertility will disappear in cases where the hybrids are as well adapted as the pure forms? In such cases, during the preliminary period when escape from competition is gained most fully by those most fully segregated, there will naturally be a rapid accumulation of all segregative endowments; but, when that condition ceases, there will still be a sufficient reason for the continuance, or even increase, of the cross-infertility in the fact that more than half of each generation of the pure form will be the descendants of those whose cross-infertility and other segregative endowments are above the average. This principle is one form of what I have called Self-Cumulative Segregation.

This law of self-accumulation does not seem to apply to cross-infertility (or to any other form of negative segregation) that is not associated with positive segregation. Nor is it quite clear that, when unassociated with negative segregation it applies to positively segregating characters (such as social and industrial instincts that lead animals of one kind to pair together, and the prepotency of the pollen of a given kind on the stigma of the same kind securing a similar result for plants). When, however, characters producing positive but incomplete segregation are associated with those producing negative segregation, both classes of characters must tend to increase till the segregation becomes pronounced. As soon as this point is reached, the Reflex Selection, by which the different portions of the species have been kept in harmonious relations with each other, is suspended, and there is nothing but the force of heredity to hold them in correspondence; but the force of heredity, securing this correspondence, has itself been created by the long continued Reflex Selection, and when this is removed, it gradually fails, and divergences of all kinds multiply, increasing the incompatibility of the two forms. Thus arises diversity of habits, diversity of sexual and social instincts, and diversity in the affinities of the male and female elements; and in each respect this diversity tends toward the point of complete incompatibility.

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pletely segregated by what I have called Potential Segregation. In other words, if the pollen of each species is potent only when falling on the stigma of the same species then the species are completely segregated though growing in the same area. Still further two species may be fairly fertile when artificially crossed, and yet be completely segregated while growing together, through the fact that the pollen of either species when falling on its own stigma will be prepotent over the pollen of the other species even though the alien pollen has fallen upon the stigma considerable earlier. This I have called Prepotential Segregation. Now a variety that is segregated from the parent form by prepotential Segregation and cross-infertility, will neither fail of propagating nor be swamped by crossing though it is indiscriminately mingled with the parent form. In my paper on "Divergent Evolution" I have referred to this special combination of positive and negative segregation, produced by the incompatibility of the male and female elements, and have endeavored to show that when these characters occur together, they tend to increase in intensity according to a law of self-accumulation. (Linn. Soc. Jour. Zool., vol. xx, pp. 239–40, 259–60). Without here entering into any computation, it is evident that the prepotency of the pollen of each kind with its own kind, if only very slight, will prevent cross-fertilization as effectually as a moderate degree of instinctive preference in the case of an animal, and if segregate fecundity, (i. e. cross-infertility) is added it will tend to keep the variety from the swamping effect of the little crossing that occurs, and the variations that are above the average in these characters will have the largest influence on the pure form in each successive generation.

I regard Physiological Segregation as including all kinds of incompatibility between the male and female elements of different groups, whether these groups are varieties of one species, or species of one genus, or species of different genera, or species representing still more divergent groups; and I maintain that the importance of this principle in the origin and continuance of divergent groups, cannot be exaggerated in the case of organisms whose fertilizing elements are freely distributed by wind or water; for in these cases the segregate compatibility and cross incompatibility of the male and female elements may be the means by which the prevention of free crossing is secured, as well as the means by which the swamping effect of the crossing that occurs is prevented. There is, it seems to me, strong reason to believe that this principle is a leading factor in the segregation of multitudes of water animals, as well as in the segregation of species of plants, whether terrestrial or aquatic; and Mr. Romanes has rightly emphasized the importance of investigations in this line.

26 Concession, Osaka, Japan.

f1

THE ROYAL SOCIETY,

BURLINGTON HOUSE, LONDON, W.

July 1, 1897.

The Assistant Secretary, by desire of Prof. M. Foster, begs to enclose herewith a letter from Mr. de B. Crawshay. The transmission of this letter has been delayed by the events of the past week.

f.2r

Evolution Cttee (Galton)

June 19. 1897

Dear Sir.

Dr. Maxwell Masters has sent me your Circular and in reply thereto I think ~~that~~ with regard to my particular study it would be well if I could be elected a Member of the Evolution Committee but I do not know what reservations or qualifying for it may need.

My study is so very unique and peculiar that as far as I know no other person studies and records it. it is the development and origin of spots in genus Odonto-glossum

f2v

Order Actidaceae.

If I were on the Committee I could do the thing easier in regard to the subject and meet much the arrangements that will be needful in my case.

Yours faithfully

de B Crawshay

Salvin

f.1r

RAILWAY STATION
HASLEMERE
POST AND TELEGRAPH OFFICE
FERNHURST, HASLEMERE.
½ MILE

HAWKSFOLD,
FERNHURST,
HASLEMERE.

4 Jan 1897

Dear Mr Galton,

Godman has just lost a near relation & this may account for his silence. I shall see him tomorrow & explain matters.

I shall be happy to join

f.1v

your committee if you
think I shall be of any
use. I can attend on
the 14th the day you name

Very truly Yrs

Albert Salomons.

Salvin

f.2

RAILWAY STATION
HASLEMERE
POST AND TELEGRAPH OFFICE
FERNHURST, HASLEMERE.
¼ MILE

HAWKSFOLD.
FERNHURST.
HASLEMERE.

13 Jan 1897

Dear Mr Galton,

I much regret to find I am unable to go to town tomorrow so shall not be at the Royal Society's Rooms in the after-noon.

Very truly yrs
Osbert Salvin.

old & more or less exacting task. —

I hope to be at the Royal Society on Thursday evening & to meet you when perhaps we may have an opportunity of talking over the subject more fully. —

Very truly Yrs

Osbert Salvin.

(Salvin)

RAILWAY STATION HASLEMERE
POST AND TELEGRAPH OFFICE FERNHURST, HASLEMERE.
½ MILE

HAWKSFOLD,
FERNHURST,
HASLEMERE.

25 Jan 1897.

Dear Mr Galton,

You will think that I have taken an unreasonable time to answer your last letter, but your questions required a good deal of consideration & I had to see Godman on the subject, as he & I are practically in the same boat & spend

f.3v

most of our working time
together.
We are still so much engaged,
& shall be for some considerable
time, in working out our
preserved materials with
reference to their bearing
on the Geographical Distribution
of Plants & Animals that
it will not be possible

for either of us to undertake
ourselves
any experiments such as
you suggest. It seems to me
that the essence of the proposed
Experiments is that they should
be continuous over a prolonged
period & that involves close
attention on the part of the
Experimentors. Neither Godman
nor I am free to undertake
such work being tied to our

TELEGRAPHIC ADDRESS,
"ZOOLOGICAL, LONDON"

Zoological Society of London,
3, Hanover Square, London, W.

5th Decr. 1894

Dear Galton

I am sorry I did not succeed in getting to your meeting yesterday. There was a Council of the Brit. Assn. at the same hour which I specially wished to attend. I should like

f.1v

to have fuller particulars
of the proposed "Biological
Farm" which seems
to be an excellent
idea –

Yours very faithfully
P. L. Sclater –

Adam Sedgewick

f.1r

Whitefield
Gt. Shelford
Cambs.
Dec. 17, 1896

Dear Mr Galton

I am very much obliged to you for your interesting letter of 12 Dec. The subject referred to is a most important one & I am glad that you have taken it in hand. But your task will not be an easy one, it behoves

mention mankind in general, it ought to be easier.

With regard to (2), if you are able to establish an experimental station, it ought to be near one of the Universities if possible - for this reason. You have here and at Oxford a considerable number of men with the

f.1v

everyone interested in such matters — who would not be interested if the bearing of the facts to be investigated were properly put before them — to assist you to the utmost of his powers. For my own part, I feel that what I can do is very little, because what you chiefly want is money & that in large quantities, & this I cannot give you or get for you.

It seems to me that an establishment for the investigation of the subjects you mention has the same kind of importance to those who are concerned with the production of living organisms that a research Laboratory has to the practical chemist; no doubt it will be more difficult to get, though, considering the large number of rich men practically interested in the subject, not to were granted a proportionate share in the management of the institution, & if too much stress were not laid at first upon purely speculative investigations.

I very much doubt if you can have research, the only immediate object of wh: is the discovery of truth, to order - such research must it seems to me come spontaneously & can only be produced by private

2 A. Sedgwick Dec 17/96
f.2r

knowledge & zeal required for investigation who would give their work for nothing & be only too glad of the chance to work at such subjects. London would be equally suitable - but there is the difficulty about distance - you could not have a farm within a walk of the place where people

f.2v

live; & you might be troubled by roughs & bank holiday people. even if you went some distance out.

But you cannot have such an establishment without money, & to get money you must interest people like Lord Rosebery, Lord Rothschild, the Duke of Devonshire, Mr Gilbey, McAlmont & such like millionaires. Surely these men could be as much

interested by watching for the result of a properly conducted series of experiments in breeding as they are in watching the result of a New Market Race. They ~~think of~~ nothing of spending £10,000 & more to gratify their interest in the latter subject, could they not be got to give a similar sum to forward the first named object? I believe they could, especially if they

3. A. Sedgewick Dec 17/96

f3r

would lend their establishments for the purpose of solving questions interesting to us & to them. (4) the recording of experiments & observations under (3). Such a man would report to & be directed by your Committee & he would endeavour to interest the great practical men especially the moneyed ones in the subject. I regret that I cannot suggest experiments, for I

The tabulation & examination of the views of these men on the questions referred to

individuals who have been led by their circumstances, constitution, temperament, inspiration if you like, to do the work & get the results. The work done in a great establishment such as you require must have a stronger stimulus than the mere desire to discover the truth, & must have a definite practical object wh: practical people can understand.

If you once get an
establishment of the kind
started, the purely
scientific work will soon
come, & justify itself
by its results.

However these are
dreams, & to you
commonplaces. Why should
I take up your time
by writing them?

With regard to (1), I think
that much might be done
especially to make a
beginning for (2) by appointing
a man keen & interested
in these subjects, & one
who would be likely to
hit it off with the people
concerned, to confer
with great breeders,
farmers, & other
practical men with
the object of ascertaining
(1) the questions wh: interest
them & wh: they cannot ~~carry~~ enquire
into ~~out~~ for lack of opportunity.
(2) the questions on wh: they
differ. (3) how far they

4. A Sedgewick Dec 17/96

f. 4r

am ignorant of the possibilities of the situation. But your offer would soon throw light upon this, & would largely add to the list of experiments wh: you already see your way to undertake.

Again thanking you for your letter, & wishing you every success in the great project

f. 4v

you have undertaken

Believe me

yours very truly

Adam Sedgwick

I am ashamed to send this letter, but as you have been kind enough to ask my opinion, I feel bound to send it though I know that I can say nothing on the subject that will not already have occurred to & been considered by you.

17 Jan/97 A Sedgwick f.5r (1)

Whitefield
Gt. Shelford
Cambridge
17 Jan. 97

Dear Mr Galton

Thank you for your letter of the 15th. I should like to be a member of your Committee, if I could see my way to attending its meetings, but unfortunately my work in term time renders it at present very difficult for me to

experimental work in breeding to cultivate practical breeders both of plants & animals & find out from them what questions they wish to see settled, paying special attention to those points in wh: there were differences of opinion amongst them. At the same time the staff of scientific workers & ~~of~~ the amateur workers attracted by the opportunities of the

f.5v

get away from Cambridge even for a few hours. At the same I much appreciate your suggestion that you should propose my name at the next meeting.

My own experience (in connection with the M.B.A) marine Biological Association has convinced me of the importance in undertakings of this kind of making most prominent the practical, immediately useful, side, & of allowing the purely scientific; speculative

sides to ~~come~~ come in where they can. There are two obvious reasons for taking this line (1) the path of work is more clearly marked out (2) by working out questions which seem to have a practical value you enlist the sympathy of the practical man & so of a wider & richer public. Therefore my plan would be if I could afford to run a farm of

had been subjected to
the changed conditions
for a variable ~~amount~~ time & any
number of generations.
(7) a sustained series of
expts in inbreeding.
(8) The same but occasionally
allowing the female to
be fertilised by a foreign
male to see if the change
in her constitution so
produced, modified
in any way the subsequent
continued in breeding.

17 Jan/97 A Sedgwick (2) f.6r

place would be able to
follow out their own
ideas, particularly
taking advantage of
the ~~by~~ scientific bye-
products of the
more official work.
Speaking purely as a
non-practical, knowledge-
-for-its-own-sake, man
I should direct my attention
(1) to extensive & absurd
hybridising both in plants
& animals, & to the results

f.6v

~~as~~ from the point ~~of~~ of
view of obtaining new
types.
(2) In the case of failure
in hybridising should
endeavour to ascertain
at what stage the failure
came in, whether after
entrance of the spermatozoon
or before.
(3) A thorough investigation
of the phenomena of
Telegony in birds &
mammals, & if possible in
plants.
3a. The amount of change which can be produced by continued action of changed conditions on one organism, & on organisms following one another by asexual reproduction
(4) the transmission of
modifications produced by
changed external conditions
(a) by asexual reproduction
(b) by sexual reproduction
both parents having
been modified
& the external condition
being maintained.
(5) the same experiments
the external conditions
not being maintained
(6) the same experiments
made after the race

my attention has been turned away from these matters of late, but I am sure that anyone who has considered as you have will look upon these suggestions as meagre & possibly puerile

Yours very truly
Adam Sedgwick

Francis Galton Esq.re L.L.D.

17 Jan 97 A Sedgwick (3) f7c

? reviewing

9 A reviewing & examination of inbreeding in the ciliate Infusoria (vide Maupas)

10 A continuation of Watase's experiments in producing male & female Rotifera at will

11 A thorough investigation of parthenogenesis in Insecta & in any other forms wh: might present themselves.

f7v

(12) The effect upon a race apparently well established of allowing all individuals born or produced to breed; — particularly in plants.

With regard to your question (6), I can see no advantage in large animals over small (except in their importance to practical men) with regard to these experiments. To be able to expt on large

mammals you require far more money, & to engage the attention of rich men you must expt on large mammals. Money is wanted & public interest is wanted; I should therefore go to the great breeders — the more intelligent of them — & find out what they want, & by suggestions increase their wants.—

f1r

(H. Spencer)

2 Lewes Crescent
Brighton
~~64, Avenue Road,~~
~~Regent's Park, N.W.~~
January 16, 1897

Dear Galton,

I knew nothing about all which your letter tells me. Sedgwick's intimation that something was being done constituted my sole knowledge.

The courses suggested seem to me impolitic. Everything is on too large a scale.

The purchase of Darwin's house seems appropriate as a matter of sentiment but as a matter of business very inappropriate. The whole undertaking would

house I should distinctly say No.

I cannot say more now. I dictate this from bed where I am passing much of my time of late.

Truly yours,

Herbert Spencer

f. 1v

be handicapped at the outset by heavy expenditure to little purpose. I should be disinclined to cooperate were any such imprudent step taken.

The thing should be commenced on a small scale by the few who have already interested themselves in it – say three or four acres with some cheap wooden buildings. Undertakings <u>of kinds not before tried</u> must begin in small ways and grow by success. If they begin in large ways they are almost certain to fail.

Cooperation with breeders would, I believe, be futile. You could never get them to fulfil the requisite conditions, and Selection would be certain to come in and vitiate the results.

Your last question respecting my contribution & its applicability to the "Committee & the Royal Society" I do not understand. I do not know what you mean as to any action of the Royal Society. If it refers to the purchase of Darwin's

f.1r

CHIEF VETERINARY INSPECTOR'S OFFICE:
SANDYFORD VILLA,
NEWCASTLE-ON-TYNE,

CLEMENT STEPHENSON,
CHIEF VETERINARY INSPECTOR
TO THE
COUNTY COUNCIL OF NORTHUMBERLAND.

3rd Nov 1895.

Dear Sir,

I duly rec^d yours of the first inst. & I have carefully read & noted its contents.

Let me say here that "Heredity" & "Atavism" have occupied a good deal of my attention, the two together are inseparably connected with breeding problems –

I take it that the

of the measurements you require.

The cattle I keep are pedigreed Aberdeen-Angus –

I enclose a copy of last years catalogue. this years is in the press.

Yours truly

C Stephenson

To
Francis Galton Esq F.R.S.

f. 1v

measurements must be made upon mature animals - that too in ordinary store condition. not fed up.

Let me know how to proceed - the kind of measure to use - & I will gladly do what I can to assist you in the matter.

I have two photos of my last years double Champion "Barton Bride" if you think that they will be of any use for measurement. I will send you copies - of course. I cannot say that they have been taken under your standard conditions.

Are you likely to be down this way at any time soon. If so, I would show you my herd. & you could give me a practical demonstration

1894.

Private Catalogue.

Balliol College Herd

OF

Aberdeen-Angus Cattle,

THE PROPERTY OF

Mr Clement Stephenson,

BALLIOL COLLEGE FARM, LONG BENTON,

AND

SANDYFORD VILLA, NEWCASTLE-ON-TYNE.

1894.

f.1

1894.

SIXTH CATALOGUE

OF THE

BALLIOL COLLEGE HERD

OF

ABERDEEN-ANGUS CATTLE,

THE PROPERTY OF

MR CLEMENT STEPHENSON,

BALLIOL COLLEGE FARM, LONG BENTON, AND SANDYFORD VILLA, NEWCASTLE-ON-TYNE.

Balliol College Farm is about twenty minutes' drive from Newcastle. The herd may be seen at any time on application to Mr Stephenson, Sandyford Villa, Newcastle-on-Tyne.

OCTOBER 1894.

f.2

INDEX.

COWS AND HEIFERS.

BULLS.

YOUNG BULLS.

PREFACE TO SIXTH EDITION.

I FIND that a Catalogue that is only issued occasionally is, owing to births, sales, and purchases, no true index to the constituents of a Herd. To remedy this, I have decided to issue a Catalogue from time to time as required, so that it may be as nearly as possible a clear and correct list of the animals in the Herd.

Upon inspection, I believe that it will be admitted that the cattle are all individually good representatives of the breed. They are healthy, robust, and full of lean flesh; they are on short legs, and have the lightest of bone; and the females are all regular breeders and good milkers.

To the frequenters of the great National Shows, the Herd is well known. It is impossible to give an account of the many prizes that members of the Herd have won since 1878. I may, however, mention that representatives of the Herd, in addition to winning first and champion honours at the Summer Shows for Breeding Stock, have also secured champion honours (competing against all breeds and all ages) at the great Fat Stock Shows—namely, **at York in 1881; at Leeds in 1883; at Norwich in 1881, 1883, and 1893; at Birmingham in 1883, 1884, 1885, 1887, and 1893; and at Smithfield in 1885 and 1887**. These victories speak for the high character of the Herd and the capabilities of the Breed.

The Herd may be seen at any time, and full information will be given upon application to the Owner, CLEMENT STEPHENSON.

SANDYFORD VILLA,
NEWCASTLE-ON-TYNE, October 1894.

f.4r

CATALOGUE.

COWS AND HEIFERS,

ARRANGED ALPHABETICALLY.

THE FIGURES REFER TO THE POLLED HERD BOOK.

ACHIEVEMENT 2ND 18539,

Bred by Clement Stephenson, calved June 3, 1890;

		sire Jovial Souter -	7634
dam Abbess 3rd	-	3616 by Bluebeard - -	648
2 d. Abbess 2nd	-	1969 by Cavalier - -	411
3 d. Amelia of E. Tulloch		1900 by Pure Angus Bull	
4 d. Ashentilly	-	1029 by Colonel of E. Tulloch	391
5 d. Agnes, bred at Easter Tulloch			

Jovial Souter 7634, sire Souter Johnny 1615, dam Jovial 7054 by Young Viscount 736. He was sold for 300 guineas, and is now stud bull in the herd belonging to Sir James Duke, Bart. of Laughton, Sussex.

Abbess 3rd was first prize cow at the Northumberland County Shows, 1882 and 1883; and second at the Royal (York) Show, 1883.

Bluebeard 648 gained the third prize at the Highland Society's Show at Stirling in 1873; and in 1874 the first prize at the Kincardine Show at Laurencekirk, the first prize at the Royal Northern, and the first prize at the Highland Society's Show at Inverness.

Abbess 3rd is the dam of the champion bull Albion 6525, and his full brother Albert Edward, now in Tasmania. She bred many prize-winners, and was the grand-dam of Abbess of Turlington, the champion cow in America. Owing to an accident, the old cow was shot and buried in 1892.

AGATHA OF SEAHAM 20705,

Bred by the Marquis of Londonderry, K.G., calved March 3, 1893;

	sire Acorn	-	8034
dam Astræa -	3863 by Juryman	-	404
2 d. Ariadne 2nd -	3862 by Westertown	-	421
3 d. Ariadne 1st -	1956		

Acorn 8034, the sire of Agatha, was by Evander 3717, dam Abbess of Benton 7774 by Englishman 2076, g. dam Abbess 3rd. See notes to Achievement 2nd, page 7.

Astræa, the dam of Agatha, was calved in 1876, and is still a remarkably good looking cow, with scarcely a sign of old age.

ASHLEAF OF SEAHAM (Vol. XIX.),

Bred by the Marquis of Londonderry, K.G., calved December 13, 1893;

	sire Prince of Kyle	-	9538
dam Agnes of Seaham	18120 by Rosannus	-	4249
2 d. Abbess of Benton	7774 by Englishman	-	2076
3 d. Abbess 3rd -	3616 by Bluebeard	-	648
4 d. Abbess 2nd -	1969 by Cavalier -	-	411
5 d. Amelia of Easter Tulloch	1900 by an Aberdeen-Angus Bull		
6 d. Ashentilly -	1029 by Colonel of East Tulloch		391
7 d. Agnes, bred at Easter Tulloch			

BRIDE 13343,

Bred by A. Beddie, Strichen, calved December 7, 1886;

	sire Sir Peter	-	5020
dam Craigo Queen -	8141 by Baron of Corse	-	1966
2 d. Craigo Jewel -	6484 by Moraystown	-	1439
3 d. Craigo Gem -	5258 by Colonel Gordon 2nd		1465
4 d. Polly of Strichen -	5265 by Oscar -	-	484
5 d. Craigo 2nd of Strichen	5544 by Craigo -	-	260
6 d. Young Craigo, bred by the late Capt. Carnegy of Craigo			
7 d. Lady Craigo -	99		

Bride 13343 was the dam of Bridesmaid of Benton 18540 that as a yearling, 1892, was first at the Northumberland, Hexham, Durham, and Yorkshire shows, and, although only a yearling, was first in the class for Aberdeen-Angus cows and heifers, and was awarded the Scotch cup as the best in the Scotch classes at the Smithfield Show, December 1892. In 1893 she was champion at Norwich, winning £100; at Birmingham the following week she was first in her class, £15, won the Scotch cup, £30; the President's prize of £25; the Elkington challenge cup, £105; and the Thorley challenge cup of £105. At Smithfield she was first in the extra class, the only prize she could compete for.

BENTON BRIDE 19843,

Bred by Clement Stephenson, calved January 11, 1892;

	sire Albion -	-	6525
dam Bride - -	13343 by Sir Peter	-	5020
2 d. Craigo Queen -	8141 by Baron of Corse	-	1966
3 d. Craigo Jewel -	6484 by Moraystown	-	1439
4 d. Craigo Gem -	5258 by Colonel Gordon 2nd		1465
5 d. Polly of Strichen -	5265 by Oscar -	-	484
6 d. Craigo 2nd of Strichen	5544 by Craigo -	-	260
7 d. Young Craigo, bred by the late Capt. Carnegy of Craigo			
8 d. Lady Craigo -	99		

Benton Bride, in 1893, was first at the Northumberland and Tyneside Shows, and second at the Yorkshire.

BLACK DUCHESS 13944,

Bred by P. Mackie, Easter Skene, calved March 9, 1887;

		sire Rosannus - -	4249
dam Matchless Duchess	5205	by Knight of St Patrick	2194
2 d. Duchess 3rd -	943	by March - -	355
3 d. Duchess 1st -	930	by President of Westertown	354
4 d. Duchess of Westertown	927	by Rob Roy Macgregor	267
5 d. Favourite of Tillyfour	1237	by Hanton - -	228
6 d. Lola Montes -	208	by Monarch - -	44
7 d. Queen Mother -	348	by Panmure - -	51
8 d. Queen of Ardovie	29	by Captain - -	97
9 d. Black Meg -	766		

Black Duchess, when the property of Mr Todd, Crathes, won eight first prizes, a challenge cup at Torphins, and was also one of a family prize. In 1894 she was first at the Northumberland show, and second at the Hexham and the Yorkshire Shows.

DUCHESS OF BENTON (Vol. XIX.),

Bred by Clement Stephenson, calved February 21, 1894;

		sire Birse Mannie - -	8105
dam Black Duchess -	13944	by Rosannus - -	4249
2 d. Matchless Duchess	5205	by Knight of St Patrick	2194
3 d. Duchess 3rd -	943	by March - -	355
4 d. Duchess 1st -	930	by President of Westertown	354
5 d. Duchess of Westertown	927	by Rob Roy Macgregor	267
6 d. Favourite of Tillyfour	1237	by Hanton - -	228
7 d. Lola Montes -	208	by Monarch - -	44
8 d. Queen Mother -	348	by Panmure - -	51
9 d. Queen of Ardovie	29	by Captain - -	97
10 d. Black Meg -	766		

EIRA 10728,

Bred by Owen C. Wallis, calved April 4, 1885;

		sire The Proud Knight -	1922
dam Estella -	5676	by Juval - - -	1880
2 d. Easter -	4540	by Challenger - -	1260
3 d. Erica 6th -	3023	by Major of Bognie -	444
4 d. Erica 4th -	1697	by Trojan - -	402
5 d. Erica 2nd -	1284	by Chieftain - -	318
6 d. Erica -	843	by Cupbearer - -	59
7 d. Emily -	332	by Old Jock - -	1
8 d. Beauty, bred by Hugh Watson			

EXACTLY-SO 14314,

Bred by Clement Stephenson, calved March 24, 1888 ;

		sire Souter Johnny	-	1615
dam Exact	-	11768 by Elmar	-	3704
2 d. Elissa	-	7934 by Editor	-	1460
3 d. Easter	-	4540 by Challenger	-	1260
4 d. Erica 6th	-	3023 by Major of Bognie	-	444
5 d. Erica 4th	-	1697 by Trojan	-	402
6 d. Erica 2nd	-	1284 by Chieftain	-	318
7 d. Erica	-	843 by Cupbearer	-	59
8 d. Emily	-	332 by Old Jock	-	1
9 d. Beauty, bred by Hugh Watson				

Exactly-so was the dam of the yearling bull "Earl Benton" 9099, sold at the 1892 Perth sale for 175 guineas.

ELISSANNA 17197,

Bred by Clement Stephenson, calved January 18, 1890 ;

		sire Souter Johnny	-	1615
dam Elissann	-	13082 by Evander	-	3717
2 d. Elissa	-	7934 by Editor	-	1460
3 d. Easter	-	4540 by Challenger	-	1260
4 d. Erica 6th	-	3023 by Major of Bognie	-	444
5 d. Erica 4th	-	1697 by Trojan	-	402
6 d. Erica 2nd	-	1284 by Chieftain	-	318
7 d. Erica	-	843 by Cupbearer	-	59
8 d. Emily	-	332 by Old Jock	-	1
9 d. Beauty, bred by Hugh Watson				

Souter Johnny 1615, calved in 1877, sire Adrian 2nd 622, dam Moonlight 1479 by Clansman 398, 2 d. Georgina 3rd 1231, by Damascus 495, 3 d. Georgina of Rothiemay 532 by Fintray 125, 4 d. Old Lady Jean 187, calved in 1845. From 1879 to 1887 he was a frequent prize winner. He was wonderfully thick fleshed, upon short legs, and was a gentle, placid-tempered bull. He was fruitful up to his thirteenth year.

EXACTLY RIGHT (Vol. XIX.),

Bred by Clement Stephenson, calved January 15, 1894;

		sire Cerberus	8181
dam Exactly-So	14314	by Souter Johnny	1615
2 d. Exact	11768	by Elmar	3704
3 d. Elissa	7934	by Editor	1460
4 d. Easter	4540	by Challenger	1260
5 d. Erica 6th	3023	by Major of Bognie	444
6 d. Erica 4th	1697	by Trojan	402
7 d. Erica 2nd	1284	by Chieftain	318
8 d. Erica	843	by Cupbearer	59
9 d. Emily	332	by Old Jock	1
10 d. Beauty, bred by Hugh Watson			

EFFULGENCE (Vol. XX.),

Bred by Clement Stephenson, calved June 4, 1894;

		sire Cerberus	8181
dam Elissanna	17197	by Souter Johnny	1615
2 d. Elissann	13082	by Evander	3717
3 d. Elissa	7934	by Editor	1460
4 d. Easter	4540	by Challenger	1260
5 d. Erica 6th	3023	by Major of Bognie	444
6 d. Erica 4th	1697	by Trojan	402
7 d. Erica 2nd	1284	by Chieftain	318
8 d. Erica	843	by Cupbearer	59
9 d. Emily	332	by Old Jock	1
10 d. Beauty, bred by Hugh Watson			

GOLDEN GARTER 19846,

Bred by Clement Stephenson, calved March 17, 1892;

		sire Spokesman	8795
dam Gravity	4864	by Nicholas	1210
2 d. Duchess Marie	3410	by Hampton	492
3 d. Daisy of Montbletton	1025	by Victor of Ballindalloch	403
4 d. Lady Ida	1021	by Black Diamond	464
5 d. Mayflower 2nd	1020	by The Earl	291
6 d. Mayflower of Montbletton	614	by Craigo	260
7 d. Lady Craigo	99		

Spokesman 8795, sire Jovial Souter 7634, dam Southesk 5th 4420.

JOVIALITY 14319,

Bred by Clement Stephenson, calved June 3, 1887;

	sire Evander - -	3717
dam Jovial -	7054 by Young Viscount -	736
2 d. Judy -	2996 by Ballimore - -	741
3 d. Jilt - -	973 by Black Prince of Tillyfour	366

4 d. Beauty of Tillyfour 2nd 1180, bred by Hugh Watson, Keillor

The above pedigree needs no comment.

Juryman 404, Judge 1150, and Justice 1462 were sons of Jilt 973.

Evander, bred at Ballindalloch, sire Julius 1819, dam Evening 4187.

JILT OF BENTON 21105,

Bred by Clement Stephenson, calved April 15, 1893;

	sire Albion - -	6525
dam Joviality -	14319 by Evander - -	3717
2 d. Jovial -	7054 by Young Viscount -	736
3 d. Judy -	2996 by Ballimore - -	741
4 d. Jilt - -	973 by Black Prince of Tillyfour	366

5 d. Beauty of Tillyfour 2nd 1180, bred by Hugh Watson, Keillor

JIPSEY 11TH 17367,

Bred by W. Whyte, Spott, calved January 24, 1890;

	sire Rover of Powrie -	4991
dam Juddy 2nd -	7960 by Dreadnought -	1844
2 d. Juddy - -	4717 by Khan - -	1262
3 d. Jipsey - -	1767 by Engineer - -	571
4 d. Old Jip - -	965 by Othello - -	319

Rover of Powrie, bred at Powrie in 1883. He is still in use at Hatton of Eassie.

JUST JEANNIE 15799,

Bred by W. Whyte, Spott, calved February 20, 1889;

	sire Brennus 3rd -	5907
dam Jennie Deans of Spott	11941 by Dagon - -	2040
2 d. Jenny of Spott -	3546 by Juror - -	908
3 d. Jipsey - -	1767 by Engineer -	571
4 d. Old Jip - -	965 by Othello - -	319

JIPSEY OF BENTON (Vol. XIX.),

Bred by Clement Stephenson, calved January 21, 1894;

		sire Albion - -	6525
dam Jipsey 11th	-	17367 by Rover of Powrie -	4991
2 d. Juddy 2nd	-	7960 by Dreadnought -	1844
3 d. Juddy -	-	4717 by Khan - -	1262
4 d. Jipsey -	-	1767 by Engineer - -	571
5 d. Old Jip -	-	965 by Othello - -	319

LUXURY 3RD 14323,

Bred by Clement Stephenson, calved March 17, 1888;

		sire Souter Johnny -	1615
dam Lemon 2nd	-	2264 by Bacchus - -	607
2 d. Lemon -	-	854 by Rifleman - -	325
3 d. Lizzie -	-	250 by Young Andrew -	9
4 d. Lively -	-	256 by Earl o' Buchan -	57
5 d. Bell of Ardovie -		116	

Lemon 2nd was the dam of Luxury 7783, the Birmingham and Smithfield champion of 1885.

LAVENDER 5TH OF PORTLETHEN 17313,

Bred by G. J. Walker, Portlethen, calved March 9, 1890;

		sire Nicholas of Aboyne	4908
dam Lavender 3d of Portlethen	9422	by Gight 2nd -	2128
2 d. Lavender 2d of Portlethen	4425	by Wallace of Kelly 2d	1339
3 d. Lavender - -	886	by Palmerston -	374
4 d. Lemon - -	854	by Rifleman -	325
5 d. Lizzie - -	250	by Young Andrew	9
6 d. Lively - -	256	by Earl o' Buchan	57
7 d. Bell of Ardovie -	116		

LADY LAMINA OF GLENBARRY 9360,

Bred by W. J. Tayler, Glenbarry, calved December 8, 1883;

		sire Sir Maurice -	1319
dam Lady o' Buchan -	4578	by Logie o' Buchan -	1506
2 d. Stumpie 2nd -	3150	by Fyvie - -	737
3 d. Bk. Bess of Burnshangie	1943	by Prince of Leochel	753
4 d. Bell of Bogfern -	1942	by Bk. Prince of Ennenteer	512
5 d. Jane of Bogfern -	540	by Grey-breasted Jock	2

LADYLIKE 12379,

Bred by A. Geddes, Blairmore, calved November 16, 1886;

		sire Merryman -	4050
dam Lady Lamina of Glen-barry - -	9360	by Sir Maurice -	1319
2 d. Lady o' Buchan -	4578	by Logie o' Buchan -	1506
3 d. Stumpie 2nd -	3150	by Fyvie - -	737
4 d. Bk. Bess of Burnshangie	1943	by Prince of Leochel	753
5 d. Bell of Bogfern -	1942	by Bk. Prince of Ennenteer	512
6 d. Jane of Bogfern -	540	by Grey-breasted Jock	2

The two-year-old heifer Lady Barry 19205, that has been a frequent prize-winner this year, is a daughter of Ladylike.

LADY HOPE 14967,

Bred by A. Geddes, Blairmore, calved December 2, 1888;

		sire Escape - -	6024
dam Lady Lamina of Glen-barry - -	9360	by Sir Maurice -	1319
2 d. Lady o' Buchan -	4578	by Logie o' Buchan	1506
3 d. Stumpie 2nd -	3150	by Fyvie - -	737
4 d. Bk. Bess of Burnshangie	1943	by Prince of Leochel	753
5 d. Bell of Bogfern -	1942	by Bk. Prince of Ennenteer	512
6 d. Jane of Bogfern -	540	by Grey-breasted Jock	2

LADY BANK 16416,

Bred by A. Geddes, Blairmore, calved April 10, 1890;

		sire Estrup - -	5348
dam Lady Lamina of Glen-barry - -	9360	by Sir Maurice	1319
2 d. Lady o' Buchan -	4578	by Logie o' Buchan -	1506
3 d. Stumpie 2nd -	3150	by Fyvie - -	737
4 d. Bk. Bess of Burnshangie	1943	by Prince of Leochel	753
5 d. Bell of Bogfern -	1942	by Bk. Prince of Ennenteer	512
6 d. Jane of Bogfern -	540	by Grey-breasted Jock	2

LUNA OF SEAHAM 19455,

Bred by the Marquis of Londonderry, K.G., calved January 22, 1892;

		sire Acorn - -	8034
dam Lily of Seaham	11371	by Englishman - -	2076
2 d. Lucy of Burncastle	10114	by St Clair - -	1160
3 d. Maggie of Westert'n	935	by Success - -	469
4 d. Rose 3rd -	925	by Prince Albert of Westert'n	237
5 d. Rose of Westertown	387	by Earl Spencer 3rd -	26
6 d. Marion -	308	by Uncle Tom - -	90
7 d. Blinkbonny -	315		

LADY OF BENTON (Vol. XIX.),

Bred by Clement Stephenson, calved January 22, 1894;

		sire Mayor of Windsor	8566
dam Lady Lamina of Glenbarry - -	9360	by Sir Maurice -	1319
2 d. Lady o' Buchan -	4578	by Logie o' Buchan	1506
3 d. Stumpie 2nd -	3150	by Fyvie - -	737
4 d. Bk. Bess of Burnshangie	1943	by Prince of Leochel	753
5 d. Bell of Bogfern -	1942	by Bk. Prince of Ennenteer	512
6 d. Jane of Bogfern -	540	by Grey-breasted Jock	2

LADY HOPEWELL (Vol. XIX.),

Bred by Clement Stephenson, calved March 4, 1894;

		sire Mayor of Windsor	8566
dam Lady Hope -	14967	by Escape - -	6024
2 d. Lady Lamina of Glenbarry - -	9360	by Sir Maurice -	1319
3 d. Lady o' Buchan -	4578	by Logie o' Buchan -	1506
4 d. Stumpie 2nd -	3150	by Fyvie - -	737
5 d. Bk. Bess of Burnshangie	1943	by Prince of Leochel	753
6 d. Bell of Bogfern -	1942	by Bk. Prince of Ennenteer	512
7 d. Jane of Bogfern -	540	by Grey-breasted Jock	2

f.9r

LOVELY OF BLAIRMORE 3RD 16417,

Bred by A. Geddes, calved April 28, 1890;

		sire Estrup - -	5348
dam Beauty of Clova -	11162	by Dancer - -	2656
2 d. Lovely of Clova 8th	8844	by Hero of Haughton	1697
3 d. Lovely of Clova 6th	5729	by Heir of Clova -	981
4 d. Lovely of Clova -	2026	by Black Prince of Clova	518

LADY SEAHAM 20708,

Bred by the Marquis of Londonderry, K.G., calved January 9, 1893;

		sire Acorn - -	8034
dam Lily of Seaham	11371	by Englishman -	2076
2 d. Lucy of Burncastle	10114	by St Clair - -	1160
3 d. Maggie of Westert'n	935	by Success - -	469
4 d. Rose 3rd -	925	by Prince Albert of Westert'n	237
5 d. Rose of Westertown	387	by Earl Spencer 3rd -	26
6 d. Marion -	308	by Uncle Tom - -	90
7 d. Blinkbonny -	315		

MAY MOON 15784,

Bred by Owen C. Wallis, calved June 2, 1887;

		sire Englishman -	2076
dam May Queen of Advie	7939	by Highland Chief -	1590
2 d. Mayflower of Advie 4th	4439	by Elcho - -	595
3 d. Mayflower of Advie	3108	by Conqueror -	1190
4 d. Dandy of Advie -	3106	by Trojan - -	402
5 d. Rose of Advie -	3105	by King Charles -	236
6 d. Old Rose of Advie	3104	by Craigo - -	260

Englishman, sire Young Viscount 736, dam Edith 2973. He was a frequent prize winner and a noted heifer-getter, and was eventually sold to Mr O. C. Wallis for 300 guineas.

MAY 24TH 21080,

Bred by T. Smith, Powrie, calved April 12, 1893;

		sire Provost of Powrie	9561
dam May 22nd - -	18516	by Monarch 6th -	5580
2 d. May 5th - -	6086	by Norman of Powrie	1257
3 d. May 2nd - -	3727	by Porty - -	649
4 d. May of E. Tulloch -	3732	by Theodore -	393
5 d. Mayflower of E. Tulloch	3519	by King Henry -	390
6 d. Bamba - -	1200	by Duke of Wellington	219
7 d. Bengie - -	276	by Stanley of Portlethen	14
8 d. Young Duchess 2nd	32	by Porty - -	50
9 d. Old Maggie -	681		

NIGHTINGALE 20TH 10458,

Bred by J. Scott, Easter Tulloch, calved April 1, 1883;

		sire Marnoch -	2237
dam Nightingale 7th -	6009	by Jock of Er. Tulloch	992
2 d. Nightingale 2nd -	6006	by Harry of Fasque	440
3 d. Nightingale of E. Tulloch	1742	by Theodore -	393
4 d. Nina - -	1815	by Colonel of E. Tulloch	391
5 d. Mary, bred by J. Strachan, Wester Fowlis			

Marnoch 2237 was by Warrior 1291, from the Ruth cow Madge of Portlethen 1217.

Nightingale 20th was dam of the first prize yearling steers at Smithfield, 1891 and 1892.

PRIDE OF ENGLISHMAN 10580,

Bred by Clement Stephenson, calved January 21, 1885;

		sire Englishman -	2076
dam Pride of Aberdeen 16th	3302	by Gainsborough 3rd	598
2 d. Pride of Mulben 2nd	2359	by Lochiel -	723
3 d. Pride of Mulben -	1919	by Jim Crow 4th -	352
4 d. Pride of Aberdeen 5th	1174	by Bright - -	454
5 d. Pride of Aberdeen	581	by Hanton - -	228
6 d. Charlotte - -	203	by Angus - -	45
7 d. Lola Montes -	208	by Monarch -	44
8 d. Queen Mother -	348	by Panmure -	51
9 d. Queen of Ardovie -	29	by Captain -	97
10 d. Black Meg -	766		

Pride of Englishman 10580 as a yearling was second prize-winner at the Royal (Norwich) Meeting, second at the Highland and Yorkshire Societies, and first at the Northumberland and Durham County Shows. As a two-year-old, she won first prize at the Royal (Newcastle) Meeting, and as a three-year-old she was second to Abbess Royal 10572 at the Northumberland and Yorkshire Shows.

PRIDE'S BRACELET 17202,

Bred by Clement Stephenson, calved January 1, 1890;

		sire Souter Johnny -	1615
dam Pride of Englishman	10580	by Englishman -	2076
2 d. Pride of Aberdeen 16th	3302	by Gainsborough 3rd	598
3 d. Pride of Mulben 2nd	2359	by Lochiel -	723
4 d. Pride of Mulben -	1919	by Jim Crow 4th -	352
5 d. Pride of Aberdeen 5th	1174	by Bright - -	454
6 d. Pride of Aberdeen	581	by Hanton -	228
7 d. Charlotte - -	203	by Angus - -	45
8 d. Lola Montes -	208	by Monarch -	44
9 d. Queen Mother -	348	by Panmure -	51
10 d. Queen of Ardovie -	29	by Captain -	97
11 d. Black Meg -	766		

PRIDE'S FLOWER (Vol. XIX),

Bred by Clement Stephenson, calved January 25, 1894;

		sire Cerberus -	8181
dam Pride's Bracelet -	17202	by Souter Johnny -	1615
2 d. Pride of Englishman	10580	by Englishman -	2076
3 d. Pride of Aberdeen 16th	3302	by Gainsborough 3rd	598
4 d. Pride of Mulben 2nd	2359	by Lochiel - -	723
5 d. Pride of Mulben -	1919	by Jim Crow 4th -	352
6 d. Pride of Aberdeen 5th	1174	by Bright - -	454
7 d. Pride of Aberdeen	581	by Hanton -	228
8 d. Charlotte - -	203	by Angus - -	45
9 d. Lola Montes -	208	by Monarch -	44
10 d. Queen Mother -	348	by Panmure -	51
11 d. Queen of Ardovie -	29	by Captain -	97
12 d. Black Meg -	766		

QUEEN OF SPAIN 11179,

Bred by Col. Godman, Smeaton, calved September 26, 1885;

		sire Erin - -	3713
dam May Queen 5th -	4499	by Porty - -	649
2 d. May Queen 3rd -	4498	by Prince of Wales 2nd	394
3 d. May Queen -	2504	by Colonel of E. Tulloch	391
4 d. Bamba - -	1200	by Duke of Wellington	219
5 d. Bengie - -	276	by Stanley of Portlethen	14
6 d. Young Duchess 2nd	32	by Porty - -	50
7 d. Old Maggie -	681		

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QUEEN OF BALLIOL (Vol. XIX.)

Bred by Clement Stephenson, calved October 3, 1893;

		sire Albion - -	6525
dam Queen of Spain -	11179	by Erin - -	3713
2 d. May Queen 5th -	4499	by Porty - -	649
3 d. May Queen 3rd -	4498	by Prince of Wales 2nd	394
4 d. May Queen -	2504	by Colonel of E. Tulloch	391
5 d. Bamba - -	1200	by Duke of Wellington	219
6 d. Bengie - -	276	by Stanley of Portlethen	14
7 d. Young Duchess 2nd	32	by Porty - -	50
8 d. Old Maggie -	681		

QUEEN OF BALLIOL 2ND (Vol. XX.),

Bred by Clement Stephenson, calved October 17, 1894;

		sire Albion - -	6525
dam Queen of Spain -	11179	by Erin - -	3713
2 d. May Queen 5th -	4499	by Porty - -	649
3 d. May Queen 3rd -	4498	by Prince of Wales 2nd	394
4 d. May Queen -	2504	by Colonel of East Tulloch	391
5 d. Bamba - -	1200	by Duke of Wellington	219
6 d. Bengie - -	276	by Stanley of Portlethen	14
7 d. Young Duchess 2nd	32	by Porty - -	50
8 d. Old Maggie -	681		

RETICENT 21108,

Bred by Clement Stephenson, calved February 6, 1893;

		sire Cerberus -	8181
dam Reticence 2nd -	18548	by Greatheart -	6078
2 d. Ruth's Darling -	8106	by Mercury of Wellhouse	2247
3 d. Ruth of Wellhouse	2390	by Bob Lowe -	633
4 d. Ruth of Tillyfour -	1169	by Bk. Prince of Tillyfour	366
5 d. Beauty of Tillyfour 2nd	1180,	bred by Hugh Watson	

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RUFFLES 5167,

Bred by W. Anderson, Wellhouse, calved April 23, 1882;

		sire Knight of St Patrick	2194
dam Ruth of Wellhouse 2nd	4588	by Victor of Kelly 3rd	854
2 d. Ruth of Wellhouse	2390	by Bob Lowe -	633
3 d. Ruth of Tillyfour -	1169	by Bk. Prince of Tillyfour	366
4 d Beauty of Tillyfour 2nd	1180,	bred by Hugh Watson, Keillor	

Ruffles is the dam of the bull "Rustler" 8761, sold at the Perth (1891) sale for 120 guineas, and now stud bull at Advie.

RUTH'S WELCOME 15667,

Bred by Clement Stephenson, calved December 18, 1888;

		sire Greatheart -	6078
dam Waterside Ruth -	10779	by Waterside Sir -	2408
2 d. Royal Favourite -	4592	by Duke of Fife -	1592
3 d. Ruth of Tillyfour -	1169	by Bk. Prince of Tillyfour	366
4 d. Beauty of Tillyfour 2nd	1180,	bred by Hugh Watson, Keillor	

RADIANT 19853,

Bred by Clement Stephenson, calved January 6, 1892;

		sire Albion - -	6525
dam Ruffles - -	5167	by Knight of St Patrick	2194
2 d. Ruth of Wellhouse 2nd	4588	by Victor of Kelly 3rd	854
3 d. Ruth of Wellhouse	2390	by Bob Lowe -	633
4 d. Ruth of Tillyfour -	1169	by Bk. Prince of Tillyfour	366
5 d. Beauty of Tillyfour 2nd	1180,	bred by Hugh Watson, Keillor	

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ROVING ROSE 15806,

Bred by W. Whyte, Spott, calved February 12, 1889;

		sire Rover of Powrie -	4991
dam Rosebush of Spott	11944	by Dagon - -	2040
2 d. Rosemary of Spott	10765	by Dreadnought -	1844
3 d. Rose Queen -	4716	by Man o' the Mearns	1843
4 d. Rosebud of Spott	3540	by Tulloch - -	675
5 d. Rose of Spott -	1763	by Othello - -	319
6 d. Rosette - -	964	by Pioneer - -	326
7 d. Dreish - -	797	by Heather Jock of Shielhill	278
8 d. Flakey - -	795	by Deuchar - -	78

RUBY 33RD OF POWRIE 21082,

Bred by T. Smith, Powrie, calved January 16, 1893;

		sire Norfolk 5th -	7022
dam Ruby 6th of Powrie	7752	by Monarch - -	1182
2 d. Ruby 2nd -	3520	by Emperor of E. Tulloch	396
3 d. Ruby of E. Tulloch	1723	by Prince of Wales 2nd	394
4 d. Ruth of Melville	1408	by Theodore - -	393
5 d. Reubena -	1033	by Jupiter - -	471
6 d. Rebecca -	340	by a son of Adam -	39
7 d. Bell of Kinnaird	328	by Colin - -	35
8 d. Old Bell -	98		
9 d. Old Lady Ann	743		

RUBY WINE (Vol. XX.),

Bred by Clement Stephenson, calved July 9, 1894;

		sire Provost of Powrie -	9561
dam Ruby 33rd of Powrie	21082	by Norfolk 5th -	7022
2 d. Ruby 6th of Powrie	7752	by Monarch - -	1182
3 d. Ruby 2nd -	3520	by Emperor of E. Tulloch	396
4 d. Ruby of E. Tulloch	1723	by Prince of Wales 2nd	394
5 d. Ruth of Melville	1408	by Theodore - -	393
6 d. Reubena -	1033	by Jupiter - -	471
7 d. Rebecca -	340	by a son of Adam -	39
8 d. Bell of Kinnaird	328	by Colin - -	35
9 d. Old Bell -	98		
10 d. Old Lady Ann -	743		

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ROSALIE OF SEAHAM (Vol. XIX.),

Bred by the Marquis of Londonderry, K.G., calved January 13, 1894

		sire Zantho	9734
dam Ruby 3rd of Kippendavie	15708	by El Moro	2714
2 d. Ruby 9th of Powrie	9242	by Rosemount	3202
3 d. Ruby 6th of Powrie	7752	by Monarch	1182
4 d. Ruby 2d of Easter Tulloch	3520	by Emperor of E. Tulloch	396
5 d. Ruby of Easter Tulloch	1723	by Prince of Wales 2nd	394
6 d. Ruth of Melville	1408	by Theodore	393
7 d. Reubena	1033	by Jupiter	471
8 d. Rebecca	340	by a son of Adam	39
9 d. Bell of Kinnaird	328	by Colin	35
10 d. Old Bell	98		
11 d. Old Lady Ann	743		

SOUTHESK 5TH 4420,

Bred by the late Mrs Morison, Mountblairy, calved June 19, 1878;

		sire Royal Hope	1207
dam Southesk 4th	3604	by Captain of Bognie	579
2 d. Southesk 2nd	1051	by Odin 1st	498

3 d. a cow bred at Mountblairy, see H.B., Vol. II., p. 42

SPINK 7516,

Bred by G. Reid, Clinterty, calved March 20, 1883;

		sire Lord Chancellor	1782
dam Spott of Baads	4378	by Royal Charlie of Baads	1335
2 d. Lark	2462	by Prince Albert 2nd	745
3 d. Livie of Baads	2463	by Leo	349

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WITCH OF ENDOR 23RD 21086,

Bred by T. Smith, Powrie, calved February 21, 1893;

		sire Norfolk 5th -	7022
dam Witch of Endor 12th	17171	by Asphalt -	5847
2 d. Witch of Endor 8th	13071	by Monarch 2d of Powrie	3045
3 d. Witch of Endor 2nd	6092	by Norman of Powrie	1257
4 d. Mayflower 2d of E. Tulloch	3521	by Emperor of E. Tulloch	396
5 d. Mayflower of E. Tulloch	3519	by King Henry -	390
6 d. Bamba - -	1200	by Duke of Wellington	219
7 d. Bengie - -	276	by Stanley of Portlethen	14
8 d. Young Duchess 2nd	32	by Porty - -	50
9 d. Old Maggie - -	681		

This heifer is own sister to Mr Sykes's Witch of Endor 19th, that has won several prizes.

WITCH OF ENDOR 24TH 21087,

Bred by T. Smith, Powrie, calved May 6, 1893;

		sire Provost of Powrie	9561
dam Witch of Endor 15th	18522	by Norfolk 5th -	7022
2 d. Witch of Endor 10th	14294	by Norfolk -	3082
3 d. Witch of Endor 5th	10562	by Monarch -	1182
4 d. Witch of Endor 2nd	6092	by Norman of Powrie	1257
5 d. Mayflow'r 2d of E. Tulloch	3521	by Emperor of E. Tulloch	396
6 d. Mayflower of E. Tulloch	3519	by King Henry -	390
7 d. Bamba - -	1200	by Duke of Wellington	219
8 d. Bengie - -	276	by Stanley of Portlethen	14
9 d. Young Duchess 2nd	32	by Porty - -	50
10 d. Old Maggie -	681		

Witch of Endor 15th was a prize winner at the Highland Society's Show, 1893.

f.13r

BULLS.

ALBION 6525,

Bred by Clement Stephenson, calved February 3, 1888;

	sire Souter Johnny -	1615
dam Abbess 3rd -	3616 by Bluebeard -	648
2 d. Abbess 2nd -	1969 by Cavalier - -	411
3 d. Amelia of E. Tulloch	1900 by a pure Angus bull	
4 d. Ashentilly -	1029 by Colonel of East Tulloch	391
5 d. Agnes, bred at Easter Tulloch		

In 1889 Albion was first prize yearling bull at the Royal Agricultural Society (Windsor Meeting), first at the Durham County Show, first at the Northumberland County Show in the all-age class, and first at the Great Yorkshire Show (Hull Meeting).

In 1890 he was first in his class and reserve for champion at the Durham County Show, first at the Northumberland County Show, third at the Highland Society Show, and first at the Great Yorkshire Show (Harrowgate Meeting). In 1891 he was only shown once, namely, at the Royal Agricultural Society (Doncaster Meeting), when he was placed second.

In 1892 he was first at the Northumberland, the Tyneside, and the Durham County Shows. In 1893 he was first in his class, and won the champion challenge cup, value 50 guineas, for the best breeding animal in the cattle classes. He has not been shown since.

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CERBERUS 8181,

Bred by J. T. Cathcart, Pitcairlie, calved March 20, 1890;

		sire Norfolk - -	3082
dam Lady Jane Grey -	10065	by Monarch -	1182
2 d. Jennet 6th -	3508	by Gainsborough 3rd	598
3 d. Jennet 3rd -	1494	by Major - -	351
4 d. Jennet 2nd -	909	by Leo - -	349
5 d. Jennet - -	904	by Alford - -	231
6 d. Jenny of Tillyfour	353	by Hanton - -	228
7 d. Young Jenny Lind	207	by Angus - -	45
8 d. Jenny Lind -	27	bred by J. Pirie	
9 d. Old Jenny Lind -	34	bred by J. Pirie	

In 1892 Cerberus was second to Albion at the Northumberland, Tyneside, and Durham County Shows. In 1893 he was third at the Royal, first and special prize at the Northumberland and Yorkshire Shows, first at Tyneside, and second at Durham. In 1894 he was first at the Northumberland, Tyneside, and Durham County Shows, and second at the Yorkshire.

TERMS FOR SERVICE OF BULLS.

Approved Cows and Heifers will be received for service at fees ranging from Two to Ten Guineas.

No Cow or Heifer to be sent which has cast her previous calf, or that has been served by any other Bull since she produced her last calf.

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YOUNG BULLS.

SEARCH LIGHT (Vol. XIX.),

Calved December 25, 1893; sire Albion 6525, dam Southesk 5th 4420.

See page 23.

LIGHT HEART (Vol. XIX.),

Calved January 5, 1894; sire Cerberus 8181, dam Luxury 3rd 14323.

See page 14.

BRIDESMAN OF BENTON (Vol. XIX.),

Calved January 7, 1894; sire Albion 6525, dam Bride 13343.

See page 9.

ALL RIGHT (Vol. XIX.),

Calved February 3, 1894; sire Cerberus 8181, dam Achievement 2nd 18539.

See page 7.

PRIMATE OF BENTON (Vol. XIX.),

Calved February 5, 1894; sire Albion 6525, dam Pride of Englishman 10580.

See page 18.

NAWAB (Vol. XIX.),

Calved March 14, 1894; sire Albion 6525, dam Nightingale 20th 10458.

See page 18.

LANCET (Vol. XIX.),

Calved April 16, 1894; sire Mayor of Windsor 8566, dam Lovely of Blairmore 3rd 16417.

See page 17.

1895.

Private Catalogue.

Balliol College Herd

OF

Aberdeen-Angus Cattle,

THE PROPERTY OF

Mr Clement Stephenson,

BALLIOL COLLEGE FARM, LONG BENTON,

AND

SANDYFORD VILLA, NEWCASTLE-ON-TYNE.

1895.

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1895.

SEVENTH CATALOGUE

OF THE

BALLIOL COLLEGE HERD

OF

ABERDEEN-ANGUS CATTLE,

THE PROPERTY OF

MR CLEMENT STEPHENSON,

BALLIOL COLLEGE FARM, LONG BENTON, AND SANDYFORD VILLA, NEWCASTLE-ON-TYNE.

Balliol College Farm is about twenty minutes' drive from Newcastle. The herd may be seen at any time on application to Mr Stephenson, Sandyford Villa, Newcastle-on-Tyne.

OCTOBER 1895.

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COWS AND HEIFERS.

BULLS.

YOUNG BULLS.

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PREFACE TO SEVENTH EDITION.

I BEGAN farming in 1871, but it was not until 1880 that, as the result of breeding and feeding experiments, I determined to establish a Breeding Herd of Aberdeen-Angus cattle. Since then, I have endeavoured to get together, and keep only animals that are true to type, faultless in pedigree, sound in constitution, regular breeders, and good milkers.

It is impossible to enumerate all the prizes that have been awarded to members of my Herd. It is sufficient to say that, in addition to winning first prizes and champion honours at the Summer Shows for Breeding Animals, they have also secured champion honours (competing against all breeds and all ages) at the great Fat Stock Shows—namely, **at York in 1881, at Leeds in 1883, at Norwich in 1881, 1883, and 1893, at Birmingham in 1883, 1884, 1885, 1887, 1893, and 1894, and at Smithfield in 1885 (Luxury 7783), 1887 (Young Bellona 5630), and 1894 with Benton Bride 19843.**

In addition to the above record, other exhibits from the Herd have been six times reserve for champion.

The Herd may be seen at any time, and full information respecting it may be obtained upon application to the owner, CLEMENT STEPHENSON.

SANDYFORD VILLA,
NEWCASTLE-ON-TYNE, October 1895.

CATALOGUE.

COWS AND HEIFERS

ARRANGED ALPHABETICALLY.

THE FIGURES REFER TO THE POLLED HERD BOOK.

ACHIEVEMENT 2ND 18539.

Bred by Clement Stephenson, calved June 3, 1890:

		sire Jovial Souter -	7634
dam Abbess 3rd -	3616	by Bluebeard -	648
2 d. Abbess 2nd -	1969	by Cavalier - -	411
3 d. Amelia of E. Tulloch	1900	by Pure Angus Bull	
4 d. Ashentilly -	1029	by Colonel of E. Tulloch	391
5 d. Agnes, bred at Easter Tulloch			

Jovial Souter 7634, sire Souter Johnny 1615, dam Jovial 7054 by Young Viscount 736. He was sold for 300 guineas, and is now stud bull in the herd belonging to Sir James Duke, Bart. of Laughton, Sussex.

Abbess 3rd was first prize cow at the Northumberland County Shows, 1882 and 1883; and second at the Royal (York) Show, 1883.

Bluebeard 648 gained the third prize at the Highland Society's Show at Stirling in 1873; and in 1874 the first prize at the Kincardine Show at Laurencekirk, the first prize at the Royal Northern, and the first prize at the Highland Society's Show at Inverness.

Abbess 3rd is the dam of the champion bull Albion 6525, and his full brother Albert Edward, now in Tasmania. She bred many prize-winners, including Achievement 13080, that was reserve for champion at Birmingham and Smithfield in 1889; and she was the grand-dam of Abbess of Turlington, the champion cow in America. Owing to an accident, the old cow was shot and buried in 1892.

f.4v

ALLOW ME (Vol. XX.)

Bred by Clement Stephenson, calved January 9, 1895;

		sire Cerberus - -	8181
dam Achievement 2nd	18539	by Jovial Souter -	7634
2 d. Abbess 3rd -	3616	by Bluebeard -	648
3 d. Abbess 2nd -	1969	by Cavalier - -	411
4 d. Amelia of E. Tulloch	1900	by Pure Angus Bull	
5 d. Ashentilly -	1029	by Colonel of E. Tulloch	391
6 d. Agnes, bred at Easter Tulloch			

AGATHA OF SEAHAM 20705,

Bred by the Marquis of Londonderry, K.G., calved March 3, 1893;

		sire Acorn - -	8034
dam Astræa - -	3863	by Juryman -	404
2 d. Ariadne 2nd -	3862	by Westertown -	421
3 d. Ariadne 1st -	1956		

Acorn 8034, the sire of Agatha, was by Evander 3717, dam Abbess of Benton 7774 by Englishman 2076, g. dam Abbess 3rd. See notes to Achievement 2nd, page 7.

Astræa, the dam of Agatha, was calved in 1876, and is still a remarkably good-looking cow, with scarcely a sign of old age.

ASHLEAF OF SEAHAM 21994,

Bred by the Marquis of Londonderry, K.G., calved December 13, 1893;

		sire Prince of Kyle	9538
dam Agnes of Seaham	18120	by Rosannus	4249
2 d. Abbess of Benton	7774	by Englishman	2076
3 d. Abbess 3rd	3616	by Bluebeard	648
4 d. Abbess 2nd	1969	by Cavalier	411
5 d. Amelia of Easter Tulloch	1900	by an Aberdeen-Angus bull	
6 d. Ashentilly	1029	by Colonel of East Tulloch	391
7 d. Agnes, bred at Easter Tulloch			

ASTRÆA 8TH 19873,

Bred by Colonel Stirling, Kippendavie, calved December 12, 1891;

		sire Diamond of Guisachan	5290
dam Astræa 5th	14367	by El Moro	2714
2 d. Astræa	3863	by Juryman	404
3 d. Ariadne 2nd	3862	by Westertown	421
4 d. Ariadne 1st	1956		

Astræa 8th won 2nd prize at Stirling in 1893.

ALINE OF LUDDICK (VOL. XX.),

Bred by J. B. Adam, Luddick, calved December 10, 1894;

		sire Beau Benedict	10636
dam Panacea	18437	by Pride of Morning	5641
2 d. Pinkeyes 3rd	10709	by Hatton	3813
3 d. Pinkeyes 2nd	4034	by Just Out	896
4 d. Pinkeyes	892	by Sir James 2nd	378
5 d. Prima Donna	851	by Moses	360
6 d. Perdita	848	by Druid	225
7 d. Princess Philomel	269	by Rory	48
8 d. Nightingale	262	by Strathmore	5
9 d. Mary of Wester Fintray	21	by Strathmore of Fintray	317

f.5v

BRIDE 13343,

Bred by A. Beddie, Strichen, calved December 7, 1886;

			sire Sir Peter	-	5020
dam Craigo Queen	-	8141	by Baron of Corse	-	1966
2 d. Craigo Jewel	-	6484	by Moraystown	-	1439
3 d. Craigo Gem	-	5258	by Colonel Gordon 2nd		1465
4 d. Polly of Strichen	-	5265	by Oscar	-	484
5 d. Craigo 2nd of Strichen		5544	by Craigo	-	260
6 d. Young Craigo, bred by the late Capt. Carnegy of Craigo					
7 d. Lady Craigo	-	99			

Bride 13343 was the dam of Bridesmaid of Benton 18540 that as a yearling, 1892, was first at the Northumberland, Hexham, Durham, and Yorkshire shows, and, although only a yearling, was first in the class for Aberdeen-Angus cows and heifers, and was awarded the Scotch cup as the best in the Scotch classes at the Smithfield show, December 1892. In 1893 she was champion at Norwich, winning £100; at Birmingham the following week she was first in her class, £15; won the Scotch cup, £30; the President's prize of £25; the Elkington challenge cup, £105; and the Thorley challenge cup of £105. At Smithfield she was first in the extra class, the only prize she could compete for.

She was also the dam of Benton Bride 19843, that was champion at Birmingham and Smithfield, 1894, and the first animal to win Her Majesty the Queen's Challenge Cup, value £150, given for the best beast bred by exhibitor. At the two shows named she won, in cash and plate, £660, and was sold to Harrod's stores to slaughter for £130 net cash.

BENTON BRIDE 2ND (VOL. XX.),

Bred by Clement Stephenson, calved December 31, 1894;

			sire Albion	-	6525
dam Bride	- -	13343	by Sir Peter	-	5020
2 d. Craigo Queen	-	8141	by Baron of Corse	-	1966
3 d. Craigo Jewel	-	6484	by Moraystown	-	1439
4 d. Craigo Gem	-	5258	by Colonel Gordon 2nd		1465
5 d. Polly of Strichen	-	5265	by Oscar	-	484
6 d. Craigo 2nd of Strichen		5544	by Craigo	-	260
7 d. Young Craigo, bred by the late Capt. Carnegy of Craigo					
8 d. Lady Craigo	-	99			

This heifer is full sister of the double champion Benton Bride 19843.

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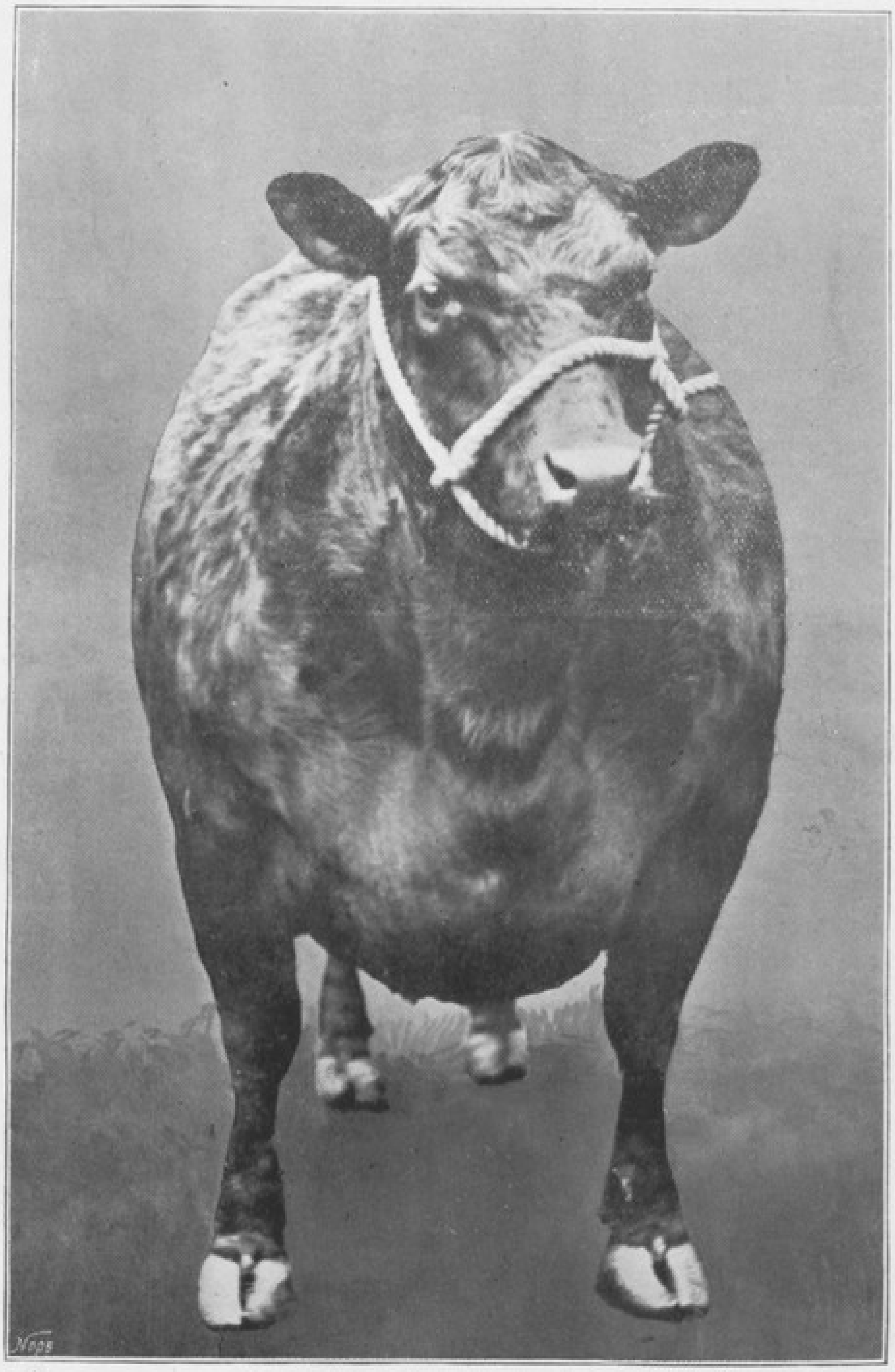

BENTON BRIDE 19843.

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BLACK DUCHESS 13944,

Bred by P. Mackie, Easter Skene, calved March 9, 1887 ;

	sire	Rosannus -	4249
dam Matchless Duchess	5205 by	Knight of St Patrick	2194
2 d. Duchess 3rd -	943 by	March -	355
3 d. Duchess 1st -	930 by	President of Westertown	354
4 d. Duchess of Westertown	927 by	Rob Roy Macgregor	267
5 d. Favourite of Tillyfour	1237 by	Hanton - -	228
6 d. Lola Montes -	208 by	Monarch - -	44
7 d. Queen Mother -	348 by	Panmure - -	51
8 d. Queen of Ardovie	29 by	Captain - -	97
9 d. Black Meg -	766		

Black Duchess, when the property of Mr Todd, Crathes, won eight first prizes, a challenge cup at Torphins, and was also one of a family prize. In 1894 she was first at the Northumberland show, and second at the Hexham and the Yorkshire shows.

EXACTLY-SO 14314,

Bred by Clement Stephenson, calved March 24, 1888 ;

	sire	Souter Johnny -	1615
dam Exact - -	11768 by	Elmar - -	3704
2 d. Elissa - -	7934 by	Editor - -	1460
3 d. Easter - -	4540 by	Challenger -	1260
4 d. Erica 6th - -	3023 by	Major of Bognie	444
5 d. Erica 4th - -	1697 by	Trojan - -	402
6 d. Erica 2nd -	1284 by	Chieftain -	318
7 d. Erica - -	843 by	Cupbearer -	59
8 d. Emily - -	332 by	Old Jock -	1
9 d. Beauty, bred by Hugh Watson			

Exactly-So was the dam of the yearling bull "Earl Benton" 9099, sold at the 1892 Perth sale for 175 guineas.

Souter Johnny 1615, calved in 1877, sire Adrian 2nd 622, dam Moonlight 1479 by Clansman 398, 2 d. Georgina 3rd 1231 by Damascus 495, 3 d. Georgina of Rothiemay 532 by Fintray 125, 4 d. Old Lady Jean 187, calved in 1845. From 1879 to 1887 he was a frequent prize winner. He was wonderfully thick-fleshed, upon short legs, and was a gentle, placid-tempered bull. He was fruitful up to his thirteenth year.

ELISSANNA 17197,

Bred by Clement Stephenson, calved January 18, 1890;

		sire Souter Johnny	1615
dam Elissann	13082	by Evander	3717
2 d. Elissa	7934	by Editor	1460
3 d. Easter	4540	by Challenger	1260
4 d. Erica 6th	3023	by Major of Bognie	444
5 d. Erica 4th	1697	by Trojan	402
6 d. Erica 2nd	1284	by Chieftain	318
7 d. Erica	843	by Cupbearer	59
8 d. Emily	332	by Old Jock	1
9 d. Beauty, bred by Hugh Watson			

EXACTLY RIGHT 22370,

Bred by Clement Stephenson, calved January 15, 1894;

		sire Cerberus	8181
dam Exactly-So	14314	by Souter Johnny	1615
2 d. Exact	11768	by Elmar	3704
3 d. Elissa	7934	by Editor	1460
4 d. Easter	4540	by Challenger	1260
5 d. Erica 6th	3023	by Major of Bognie	444
6 d. Erica 4th	1697	by Trojan	402
7 d. Erica 2nd	1284	by Chieftain	318
8 d. Erica	843	by Cupbearer	59
9 d. Emily	332	by Old Jock	1
10 d. Beauty, bred by Hugh Watson			

EFFULGENCE (Vol. XX.),

Bred by Clement Stephenson, calved June 4, 1894;

		sire Cerberus	8181
dam Elissanna	17197	by Souter Johnny	1615
2 d. Elissann	13082	by Evander	3717
3 d. Elissa	7934	by Editor	1460
4 d. Easter	4540	by Challenger	1260
5 d. Erica 6th	3023	by Major of Bognie	444
6 d. Erica 4th	1697	by Trojan	402
7 d. Erica 2nd	1284	by Chieftain	318
8 d. Erica	843	by Cupbearer	59
9 d. Emily	332	by Old Jock	1
10 d. Beauty, bred by Hugh Watson			

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EXTRACT (Vol. XX.),

Bred by Clement Stephenson, calved January 14, 1895;

		sire Cerberus - -	8181
dam Exactly-So	-	14314 by Souter Johnny -	1615
2 d. Exact	-	11768 by Elmar - -	3704
3 d. Elissa	-	7934 by Editor - -	1460
4 d. Easter	-	4540 by Challenger -	1260
5 d. Erica 6th	-	3023 by Major of Bognie -	444
6 d. Erica 4th	-	1697 by Trojan - -	402
7 d. Erica 2nd	-	1284 by Chieftain - -	318
8 d. Erica	-	843 by Cupbearer -	59
9 d. Emily	-	332 by Old Jock - -	1
10 d. Beauty, bred by Hugh Watson			

EFFULGENT (Vol. XX.),

Bred by Clement Stephenson, calved May 28, 1895;

		sire Albion - -	6525
dam Elissanna	-	17197 by Souter Johnny -	1615
2 d. Elissann	-	13082 by Evander - -	3717
3 d. Elissa	-	7934 by Editor - -	1460
4 d. Easter	-	4540 by Challenger -	1260
5 d. Erica 6th	-	3023 by Major of Bognie -	444
6 d. Erica 4th	-	1697 by Trojan -	402
7 d. Erica 2nd	-	1284 by Chieftain - -	318
8 d. Erica	-	843 by Cupbearer -	59
9 d. Emily	-	332 by Old Jock - -	1
10 d. Beauty, bred by Hugh Watson			

JOVIALITY 14319,

Bred by Clement Stephenson, calved June 3, 1887;

		sire Evander - -	3717
dam Jovial	-	7054 by Young Viscount -	736
2 d. Judy	-	2996 by Ballimore - -	741
3 d. Jilt	-	973 by Black Prince of Tillyfour	366
4 d. Beauty of Tillyfour 2nd 1180, bred by Hugh Watson, Keillor			

The above pedigree needs no comment.

Juryman 404, Judge 1150, and Justice 1462 were sons of Jilt 973.

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JILT OF BENTON 21105,

Bred by Clement Stephenson, calved April 15, 1893;

		sire Albion - -	6525
dam Joviality	-	14319 by Evander - -	3717
2 d. Jovial	-	7054 by Young Viscount -	736
3 d. Judy	-	2996 by Ballimore - -	741
4 d. Jilt	-	973 by Black Prince of Tillyfour	366

5 d. Beauty of Tillyfour 2nd 1180, bred by Hugh Watson, Keillor

JIPSEY 11TH 17367,

Bred by W. Whyte, Spott, calved January 24, 1890;

		sire Rover of Powrie -	4991
dam Juddy 2nd	-	7960 by Dreadnought -	1844
2 d. Juddy -	-	4717 by Khan - -	1262
3 d. Jipsey -	-	1767 by Engineer - -	571
4 d. Old Jip -	-	965 by Othello - -	319

JIPSEY OF BENTON 22371,

Bred by Clement Stephenson, calved January 21, 1894;

		sire Albion - -	6525
dam Jipsey 11th	-	17367 by Rover of Powrie -	4991
2 d. Juddy 2nd	-	7960 by Dreadnought -	1844
3 d. Juddy	-	4717 by Khan - -	1262
4 d. Jipsey	-	1767 by Engineer - -	571
5 d. Old Jip	-	965 by Othello - -	319

Jipsey of Benton was 1st prize yearling at the Royal (Darlington), the Northumberland County Show, and the Tyneside, and second at the Yorkshire (Halifax).

JENNY OF BENTON (VOL. XX.),

Bred by Clement Stephenson, calved January 27, 1895;

		sire Albion - -	6525
dam Jipsey 11th	-	17367 by Rover of Powrie -	4991
2 d. Juddy 2nd	-	7960 by Dreadnought -	1844
3 d. Juddy -	-	4717 by Khan - -	1262
4 d. Jipsey -	-	1767 by Engineer - -	571
5 d. Old Jip -	-	965 by Othello - -	319

LUXURY 3RD 14323,

Bred by Clement Stephenson, calved March 17, 1888 ;

		sire Souter Johnny	1615
dam Lemon 2nd	2264	by Bacchus	607
2 d. Lemon	854	by Rifleman	325
3 d. Lizzie	250	by Young Andrew	9
4 d. Lively	256	by Earl o' Buchan	57
5 d. Bell of Ardovie	116		

Lemon 2nd was the dam of Luxury 7783, the Birmingham and Smithfield champion of 1885.

Luxury 3rd is the dam of Lightheart 11834, first prize yearling bull at the Royal, Northumberland, Tyneside, and Yorkshire shows 1895.

LUXURY OF KELMARSH (VOL. XX.),

Bred by R. C. Naylor, Kelmarsh, calved December 18, 1894 ;

		sire Ethelbert of Glamis	8309
dam Luxury 6th	19850	by Greatheart	6078
2 d. Luxury 3rd	14323	by Souter Johnny	1615
3 d. Lemon 2nd	2264	by Bacchus	607
4 d. Lemon	854	by Rifleman	325
5 d. Lizzie	250	by Young Andrew	9
6 d. Lively	256	by Earl o' Buchan	57
7 d. Bell of Ardovie	116		

Ethelbert of Glamis 8309, sire Alister 1939, dam Evangeline of Glamis 13136 by Alister 1939.

LAVENDER 5TH OF PORTLETHEN 17313,

Bred by G. J. Walker, Portlethen, calved March 9, 1890 ;

		sire Nicholas of Aboyne	4908
dam Lavender 3rd of Portlethen	9422	by Gight 2nd	2128
2 d. Lavender 2nd of Portlethen	4425	by Wallace of Kelly 2d	1339
3 d. Lavender	886	by Palmerston	374
4 d. Lemon	854	by Rifleman	325
5 d. Lizzie	250	by Young Andrew	9
6 d. Lively	256	by Earl o' Buchan	57
7 d. Bell of Ardovie	116		

LADY LAMINA OF GLENBARRY 9360,

Bred by W. J. Tayler, Glenbarry, calved December 8, 1883 ;

		sire Sir Maurice -	1319
dam Lady o' Buchan -	4578	by Logie o' Buchan -	1506
2 d. Stumpie 2nd -	3150	by Fyvie - -	737
3 d. Bk. Bess of Burnshangie	1943	by Prince of Leochel	753
4 d. Bell of Bogfern -	1942	by Bk. Pc. of Ennenteer	512
5 d. Jane of Bogfern -	540	by Grey-breasted Jock	2

LADY LOVE OF BENTON (Vol. XX.),

Bred by Clement Stephenson, calved February 8, 1895 ;

		sire Cerberus -	8181
dam Lady Lamina of Glenbarry - -	9360	by Sir Maurice -	1319
2 d. Lady o' Buchan -	4578	by Logie o' Buchan	1506
3 d. Stumpie 2nd -	3150	by Fyvie - -	737
4 d. Bk. Bess of Burnshangie	1943	by Prince of Leochel	753
5 d. Bell of Bogfern -	1942	by Bk. Prince of Ennenteer	512
6 d. Jane of Bogfern -	540	by Grey-breasted Jock	2

LADYLIKE 12379,

Bred by A. Geddes, Blairmore, calved November 16, 1886 ;

		sire Merryman -	4050
dam Lady Lamina of Glenbarry - -	9360	by Sir Maurice -	1319
2 d. Lady o' Buchan -	4578	by Logie o' Buchan	1506
3 d. Stumpie 2nd -	3150	by Fyvie - -	737
4 d Bk. Bess of Burnshangie	1943	by Brince of Leochel	753
5 d. Bell of Bogfern -	1942	by Bk. Prince of Ennenteer	512
6 d. Jane of Bogfern -	540	by Grey-breasted Jock	2

LOVELY OF BLAIRMORE 3RD 16417,

Bred by A. Geddes, calved April 28, 1890 ;

		sire Estrup - -	5348
dam Beauty of Clova -	11162	by Dancer - -	2656
2 d. Lovely of Clova 8th	8844	by Hero of Haughton	1697
3 d. Lovely of Clova 6th	5729	by Heir of Clova -	981
4 d. Lovely of Clova	2026	by Black Prince of Clova	518

LOVELY OF BENTON (Vol. XX.),

Bred by Clement Stephenson, calved April 17, 1895 ;

		sire Albion -	6525
dam Lovely of Blairmore 3rd	16417	by Estrup -	5348
2 d. Beauty of Clova -	11162	by Dancer -	2656
3 d. Lovely of Clova 8th -	8844	by Hero of Haughton	1697
4 d. Lovely of Clova 6th -	5729	by Heir of Clova -	981
5 d. Lovely of Clova -	2026	by Bk. Prince of Clova	518

LADY SEAHAM 20708,

Bred by the Marquis of Londonderry, K.G., calved January 9, 1893 ;

		sire Acorn - -	8034
dam Lily of Seaham	11371	by Englishman - -	2076
2 d. Lucy of Burncastle	10114	by St Clair - -	1160
3 d. Maggie of Westert'n	935	by Success - -	469
4 d. Rose 3rd -	925	by Prince Albert of Westert'n	237
5 d. Rose of Westertown	387	by Earl Spencer 3rd -	26
6 d. Marion -	308	by Uncle Tom - -	90
7 d. Blinkbonny -	315		

Acorn 8034 by Evander 3717, dam Abbess of Benton 7774.

MAY MOON 15784,

Bred by Owen C. Wallis, calved June 2, 1887 ;

		sire Englishman -	2076
dam May Queen of Advie	7939	by Highland Chief -	1590
2 d. Mayflower of Advie 4th	4439	by Elcho - -	595
3 d. Mayflower of Advie	3108	by Conqueror -	1190
4 d. Dandy of Advie -	3106	by Trojan - -	402
5 d. Rose of Advie -	3105	by King Charles -	236
6 d. Old Rose of Advie	3104	by Craigo - -	260

Englishman, sire Young Viscount 736, dam Edith 2973. He was a frequent prize-winner and a noted heifer-getter, and was eventually sold to Mr O. C. Wallis for 300 guineas.

MAY 24TH 21080,

Bred by T. Smith, Powrie, calved April 12, 1893;

		sire Provost of Powrie	9561
dam May 22nd - -	18516	by Monarch 6th -	5580
2 d. May 5th - -	6086	by Norman of Powrie	1257
3 d. May 2nd - -	3727	by Porty - -	649
4 d. May of E. Tulloch -	3732	by Theodore -	393
5 d. Mayflower of E. Tulloch	3519	by King Henry -	390
6 d. Bamba - -	1200	by Duke of Wellington	219
7 d. Bengie - -	276	by Stanley of Portlethen	14
8 d. Young Duchess 2nd	32	by Porty - -	50
9 d. Old Maggie -	681		

NIGHTINGALE 20TH 10458,

Bred by J. Scott, Easter Tulloch, calved April 1, 1883;

		sire Marnoch -	2237
dam Nightingale 7th -	6009	by Jock of Er. Tulloch	992
2 d. Nightingale 2nd -	6006	by Harry of Fasque	440
3 d. Nightingale of E. Tulloch	1742	by Theodore -	393
4 d. Nina - -	1815	by Colonel of E. Tulloch	391
5 d. Mary, bred by J. Strachan, Wester Fowlis			

Marnoch 2237 was by Warrior 1291, from the Ruth cow Madge of Portlethen 1217.

Nightingale 20th was dam of the first prize yearling steers at Smithfield, 1891 and 1892.

NANCY OF BENTON (VOL. XX.),

Bred by Clement Stephenson, calved February 4, 1895;

		sire Albion - -	6525
dam Nightingale 20th -	10458	by Marnoch -	2237
2 d. Nightingale 7th -	6009	by Jock of Er. Tulloch	992
3 d. Nightingale 2nd -	6006	by Harry of Fasque	440
4 d. Nightingale of E. Tulloch	1742	by Theodore -	393
5 d. Nina - -	1815	by Colonel of E. Tulloch	391
6 d. Mary, bred by J. Strachan, Wester Fowlis			

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PRIDE OF ENGLISHMAN 10580,

Bred by Clement Stephenson, calved January 21, 1885;

		sire Englishman -	2076
dam	Pride of Aberdeen 16th	3302 by Gainsborough 3rd	598
2 d.	Pride of Mulben 2nd	2359 by Lochiel -	723
3 d.	Pride of Mulben	1919 by Jim Crow 4th -	352
4 d.	Pride of Aberdeen 5th	1174 by Bright -	454
5 d.	Pride of Aberdeen	581 by Hanton -	228
6 d.	Charlotte -	203 by Angus -	45
7 d.	Lola Montes -	208 by Monarch -	44
8 d.	Queen Mother -	348 by Panmure -	51
9 d.	Queen of Ardovie	29 by Captain -	97
10 d.	Black Meg -	766	

Pride of Englishman 10580 as a yearling was second prize winner at the Royal (Norwich) Meeting, second at the Highland and Yorkshire Societies, and first at the Northumberland and Durham County Shows. As a two-year-old she won first prize at the Royal (Newcastle) Meeting, and as a three-year-old she was second to Abbess Royal 10572 at the Northumberland and Yorkshire Shows.

PRIDE'S BRACELET 17202,

Bred by Clement Stephenson, calved January 1, 1890;

		sire Souter Johnny -	1615
dam	Pride of Englishman	10580 by Englishman -	2076
2 d.	Pride of Aberdeen 16th	3302 by Gainsborough 3rd	598
3 d.	Pride of Mulben 2nd	2359 by Lochiel -	723
4 d.	Pride of Mulben -	1919 by Jim Crow 4th -	352
5 d.	Pride of Aberdeen 5th	1174 by Bright -	454
6 d.	Pride of Aberdeen	581 by Hanton -	228
7 d.	Charlotte - -	203 by Angus -	45
8 d.	Lola Montes -	208 by Monarch -	44
9 d.	Queen Mother -	348 by Panmure -	51
10 d.	Queen of Ardovie	29 by Captain -	97
11 d.	Black Meg -	766	

PRIDE'S FLOWER 22374,

Bred by Clement Stephenson, calved January 25, 1894;

		sire Cerberus -	8181
dam Pride's Bracelet -	17202	by Souter Johnny -	1615
2 d. Pride of Englishman	10580	by Englishman -	2076
3 d. Pride of Aberdeen 16th	3302	by Gainsborough 3rd	598
4 d. Pride of Mulben 2nd	2359	by Lochiel -	723
5 d. Pride of Mulben -	1919	by Jim Crow 4th -	352
6 d. Pride of Aberdeen 5th	1174	by Bright - -	454
7 d. Pride of Aberdeen	581	by Hanton - -	228
8 d. Charlotte -	203	by Angus - -	45
9 d. Lola Montes -	208	by Monarch -	44
10 d. Queen Mother -	348	by Panmure -	51
11 d. Queen of Ardovie	29	by Captain -	97
12 d. Black Meg -	766		

PRIDE OF ALBION 2ND (VOL. XX.),

Bred by Clement Stephenson, calved January 26, 1895;

		sire Albion - -	6525
dam Pride of Englishman	10580	by Englishman -	2076
2 d. Pride of Aberdeen 16th	3302	by Gainsborough 3rd	598
3 d. Pride of Mulben 2nd	2359	by Lochiel -	723
4 d. Pride of Mulben -	1919	by Jim Crow 4th -	352
5 d. Pride of Aberdeen 5th	1174	by Bright - -	454
6 d. Pride of Aberdeen	581	by Hanton -	228
7 d. Charlotte - -	203	by Angus - -	45
8 d. Lola Montes -	208	by Monarch -	44
9 d. Queen Mother -	348	by Panmure -	51
10 d. Queen of Ardovie -	29	by Captain -	97
11 d. Black Meg -	766		

QUEEN OF SPAIN 11179,

Bred by Colonel Godman, Smeaton, calved September 26, 1885;

		sire Erin - -	3713
dam May Queen 5th -	4499	by Porty - -	649
2 d. May Queen 3rd -	4498	by Prince of Wales 2nd	394
3 d. May Queen -	2504	by Colonel of E. Tulloch	391
4 d. Bamba - -	1200	by Duke of Wellington	219
5 d. Bengie - -	276	by Stanley of Portlethen	14
6 d. Young Duchess 2nd	32	by Porty - -	50
7 d. Old Maggie -	681		

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QUEEN OF BALLIOL 22375,

Bred by Clement Stephenson, calved October 3, 1893;

		sire Albion - -	6525
dam Queen of Spain -	11179	by Erin - -	3713
2 d. May Queen 5th -	4499	by Porty - -	649
3 d. May Queen 3rd -	4498	by Prince of Wales 2nd	394
4 d. May Queen -	2504	by Colonel of E. Tulloch	391
5 d. Bamba - -	1200	by Duke of Wellington	219
6 d. Bengie - -	267	by Stanley of Portlethen	14
7 d. Young Duchess 2nd	32	by Porty - -	50
8 d. Old Maggie -	681		

QUEEN OF BALLIOL 2ND (VOL. XX.),

Bred by Clement Stephenson, calved October 17, 1894;

		sire Albion - -	6525
dam Queen of Spain -	11179	by Erin - -	3713
2 d. May Queen 5th -	4499	by Porty - -	649
3 d. May Queen 3rd -	4498	by Prince of Wales 2nd	394
4 d. May Queen -	2504	by Colonel of E. Tulloch	391
5 d. Bamba - -	1200	by Duke of Wellington	219
6 d. Bengie - -	276	by Stanley of Portlethen	14
7 d. Young Duchess 2nd	32	by Porty - -	50
8 d. Old Maggie -	681		

RETICENT 21108,

Bred by Clement Stephenson, calved February 6, 1893;

		sire Cerberus -	8181
dam Reticence 2nd -	18548	by Greatheart -	6078
2 d. Ruth's Darling -	8106	by Mercury of Wellhouse	2247
3 d. Ruth of Wellhouse	2390	by Bob Lowe -	633
4 d. Ruth of Tillyfour -	1169	by Bk. Prince of Tillyfour	366
5 d. Beauty of Tillyfour 2nd	1180,	bred by Hugh Watson	

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RADIANT 19853,

Bred by Clement Stephenson, calved January 6, 1892;

	sire Albion - -	6525
dam Ruffles - -	5167 by Knight of St Patrick	2194
2 d. Ruth of Wellhouse 2nd	4588 by Victor of Kelly 3rd	854
3 d. Ruth of Wellhouse	2390 by Bob Lowe -	633
4 d. Ruth of Tillyfour -	1169 by Bk. Prince of Tillyfour	366
5 d. Beauty of Tillyfour 2nd	1180, bred by Hugh Watson, Keillor	

In 1895, Radiant 19853 was second in the three-year-old cow class at the Royal (Darlington), second at the Yorkshire (Halifax), and 1st at the Tyneside show.

RUTH'S WELCOME 15667,

Bred by Clement Stephenson, calved December 18, 1888;

	sire Greatheart - -	6078
dam Waterside Ruth	10779 by Waterside Sir -	2408
2 d. Royal Favourite	4592 by Duke of Fife -	1592
3 d. Ruth of Tillyfour	1169 by Black Prince of Tillyfour	366
4 d. Beauty of Tillyfour 2nd	1180, bred by Hugh Watson, Keillor	

RUTH OF BENTON 7TH (VOL. XX.),

Bred by Clement Stephenson, calved January 27, 1895;

	sire Cerberus - -	8181
dam Ruth's Welcome	15667 by Greatheart - -	6078
2 d. Waterside Ruth	10779 by Waterside Sir -	2408
3 d. Royal Favourite	4592 by Duke of Fife -	1592
4 d. Ruth of Tillyfour	1169 by Black Prince of Tillyfour	366
5 d. Beauty of Tillyfour 2nd	1180, bred by Hugh Watson, Keillor	

ROVING ROSE 15806,

Bred by W. Whyte, Spott, calved February 12, 1889 :

		sire Rover of Powrie -	4991
dam Rosebush of Spott	11944	by Dagon - -	2040
2 d. Rosemary of Spott	10765	by Dreadnought -	1844
3 d. Rose Queen -	4716	by Man o' the Mearns	1843
4 d. Rosebud of Spott	3540	by Tulloch - -	675
5 d. Rose of Spott -	1763	by Othello - -	319
6 d. Rosette - -	964	by Pioneer - -	326
7 d. Dreish - -	797	by Heather Jock of Shielhill	278
8 d. Flakey - -	795	by Deuchar - -	78

Roving Rose 15806 is the dam of Mr Nimmo's prize-winning cow Rose of Benton 19854.

RUBY WINE (Vol. XX.),

Bred by Clement Stephenson, calved July 9, 1894 ;

		sire Provost of Powrie	9561
dam Ruby 33rd of Powrie	21082	by Norfolk 5th -	7022
2 d. Ruby 6th of Powrie	7752	by Monarch -	1182
3 d. Ruby 2nd -	3520	by Emperor of E. Tulloch	396
4 d. Ruby of E. Tulloch	1723	by Prince of Wales 2nd	394
5 d. Ruth of Melville	1408	by Theodore -	393
6 d. Reubena -	1033	by Jupiter -	471
7 d. Rebecca -	340	by a son of Adam -	39
8 d. Bell of Kinnaird	328	by Colin - -	35
9 d. Old Bell -	98		
10 d. Old Lady Ann -	743		

SOUTHESK 5TH 4420,

Bred by the late Mrs Morison, Mountblairy, calved June 19, 1878 ;

		sire Royal Hope -	1207
dam Southesk 4th -	3604	by Captain of Bognie -	579
2 d. Southesk 2nd -	1051	by Odin 1st - -	498
3 d. a cow bred at Mountblairy, see H.B., Vol. II., p. 42			

Southesk 5th 4420 was bought at the Mountblairy Sale, October 1883. Since then she has produced eleven calves, all good ones. She is a great milker.

f.13v

SPINK 7516,

Bred by G. Reid, Clinterty, calved March 20, 1883;

		sire Lord Chancellor -	1782
dam Spott of Baads	4378	by Royal Charlie of Baads	1335
2 d. Lark - -	2462	by Prince Albert 2nd -	745
3 d. Livie of Baads	2463	by Leo - - -	349

SPINK OF BENTON (VOL. XX.),

Bred by Clement Stephenson, calved December 27, 1894;

		sire Prior of Balliol -	12022
dam Spink -	7516	by Lord Chancellor -	1782
2 d. Spott of Baads	4378	by Royal Charlie of Baads	1335
3 d. Lark - -	2462	by Prince Albert 2nd -	745
4 d. Livie of Baads	2463	by Leo - - -	349

SCOT OF KELMARSH (VOL. XX.),

Bred by R. C. Naylor, Kelmarsh, calved December 10, 1894;

		sire Ethelbert of Glamis -	8309
dam Scota of Kelmarsh	20897	by Columbus 2nd of Whittlebury -	6642
2 d. Scota of Horsted 2nd	10879	by Knight of the Favourites	3920
3 d. Scota of Horsted	8120	by His Worship 2nd -	1889
4 d. Scota - -	5222	by Paris - -	1473
5 d. Waterside Daisy 3rd	4023	by Waterside Major -	964
6 d. Waterside Daisy 2nd		by Major 3rd - -	662

TIP OF WYNYARD (VOL. XXI.),

Bred by the Marquis of Londonderry, K.G., calved April 15, 1893;

		sire Ebro - -	8263
dam Tip 3rd -		by Oor Jock - -	4919
2 d. Tip of Gellan	6027	by Warrior - -	1291
3 d. A 1 - -	5005	by Monarch - -	1182
4 d. Ann of Culsh	2534	by Prince of East-town -	435
5 d. Lucy of Culsh	2287	by Geordie of Culsh -	910

Ebro 8263, sire Malay 6938, dam Elyne 11920 by Englishman 2076.

f.14r

. WITCH OF ENDOR 23RD 21086,

Bred by T. Smith, Powrie, calved February 21, 1893;

		sire Norfolk 5th -	7022
dam Witch of Endor 12th	17171	by Asphalt -	5847
2 d. Witch of Endor 8th	13071	by Monarch 2d of Powrie	3045
3 d. Witch of Endor 2nd	6092	by Norman of Powrie	1257
4 d. Mayflower 2d of E. Tulloch	3521	by Emperor of E. Tulloch	396
5 d. Mayflower of E. Tulloch	3519	by King Henry -	390
6 d. Bamba - -	1200	by Duke of Wellington	219
7 d. Bengie - -	276	by Stanley of Portlethen	14
8 d. Young Duchess 2nd	32	by Porty - -	50
9 d. Old Maggie -	681		

This heifer is own sister to Mr Sykes's Witch of Endor 19th, that has won several prizes.

WITCH OF ENDOR 24TH 21087,

Bred by T. Smith, Powrie, calved May 6, 1893;

		sire Provost of Powrie	9561
dam Witch of Endor 15th	18522	by Norfolk 5th -	7022
2 d. Witch of Endor 10th	14294	by Norfolk -	3082
3 d. Witch of Endor 5th	10562	by Monarch -	1182
4 d. Witch of Endor 2nd	6092	by Norman of Powrie	1257
5 d. Mayflow'r 2d of E. Tulloch	3521	by Emperor of E. Tulloch	396
6 d. Mayflower of E. Tulloch	3519	by King Henry -	390
7 d. Bamba - -	1200	by Duke of Wellington	219
8 d. Bengie - -	276	by Stanley of Portlethen	14
9 d. Young Duchess 2nd	32	by Porty - -	50
10 d. Old Maggie -	681		

Witch of Endor 15th was third (as three-year-old) at Highland and Agricultural Society's Show at Edinburgh in 1893, and was first prize aged cow and winner of Ballindalloch Challenge Cup as best cow at same Society's Show at Dumfries in 1895.

In 1895, Witch of Endor 24th was highly commended at the Royal (Darlington), and first at the Northumberland County Show.

f.14v

BULLS.

ALBION 6525,

Bred by Clement Stephenson, calved February 3, 1888;

	sire Souter Johnny -	1615
dam Abbess 3rd -	3616 by Bluebeard - -	648
2 d. Abbess 2nd -	1969 by Cavalier - -	411
3 d. Amelia of E. Tulloch	1900 by a pure Angus bull	
4 d. Ashentilly -	1029 by Colonel of East Tulloch	391
5 d. Agnes, bred at Easter Tulloch		

In 1889 Albion was first prize yearling bull at the Royal Agricultural Society (Windsor Meeting), first at the Durham County Show, first at the Northumberland County Show in the all-age class, and first at the Great Yorkshire Show (Hull Meeting).

In 1890 he was first in his class and reserve for champion at the Durham County Show, first at the Northumberland County Show, third at the Highland Society Show, and first at the Great Yorkshire Show (Harrowgate Meeting). In 1891 he was only shown once, namely, at the Royal Agricultural Society (Doncaster Meeting), when he was placed second.

In 1892 he was first at the Northumberland, the Tyneside, and the Durham County Shows. In 1893 he was first in his class, and won the champion challenge cup, value 50 guineas, for the best breeding animal in the cattle classes at the Durham County Show. He has not been shown since.

f.15r

CERBERUS 8181,

Bred by J. T. Cathcart, Pitcairlie, calved March 20, 1890;

		sire Norfolk - -	3082
dam Lady Jane Grey -	10065	by Monarch - -	1182
2 d. Jennet 6th -	3508	by Gainsborough 3rd	598
3 d. Jennet 3rd -	1494	by Major - -	351
4 d. Jennet 2nd -	909	by Leo - -	349
5 d. Jennet -	904	by Alford - -	231
6 d. Jenny of Tillyfour	353	by Hanton - -	228
7 d. Young Jenny Lind	207	by Angus - -	45
8 d. Jenny Lind -	27	bred by J. Pirie	
9 d. Old Jenny Lind -	34	bred by J. Pirie	

In 1892, Cerberus was second to Albion at the Northumberland, Tyneside, and Durham County Shows. In 1893, he was third at the Royal, first and special prize at the Northumberland and Yorkshire Shows, first at Tyneside, and second at Durham. In 1894, he was first at the Northumberland, Tyneside, and Durham County Shows, and second at the Yorkshire.

TERMS FOR SERVICE OF BULLS.

Approved Cows and Heifers will be received for Service at fees ranging from Two to Ten Guineas.

No Cow or Heifer to be sent which has cast her previous calf, or that has been served by any other Bull since she produced her last calf.

f.15v

YOUNG BULLS.

JOVIAL ALBION (Vol. XX.),

Calved December 27, 1894; sire Albion 6525, dam Joviality 14319.

RADIATOR (Vol. XX.),

Calved January 17, 1895; sire Cerberus 8181, dam Radiant 19853.

BLACK DUKE OF BENTON (Vol. XX.),

Calved January 21, 1895; sire Albion 6525, dam Black Duchess 13944.

LATCH KEY (Vol. XX.),

Calved January 29, 1895; sire Cerberus 8181, dam Luxury 3rd 14323.

Latch Key is full brother to Light Heart (11834).

RUFFLER (Vol. XX.),

Calved February 5, 1895; sire Prior of Balliol 12022, dam Ruffles 5167.

STORM KING (Vol. XX.),

Calved February 5, 1895; sire Albion 6525, dam Southesk 5th 4420.

ROVER OF BENTON (Vol. XX.),

Calved March 14, 1895; sire Prior of Balliol 12022, dam Roving Rose 15806.

LORD HOPEWELL (Vol. XX.),

Calved April 8, 1895; sire Albion 6525, dam Lady Hope 14967.

MAGNATE OF BENTON (Vol. XXI.),

Calved May 21, 1895; sire Rabbi 11228, dam May-Moon 15784.

ROYAL GARDENS KEW
DIRECTOR

Kew, May 10. 95

Dear Mr Galton,

In our necessarily hurried conversation yesterday, I am not at sure that I completely grasped the nature of your enquiry.

If I understood it rightly the enclosed memorandum embodies all that occurs to me.

Schwendener is I believe

f.1v

naughty in Germany but I must confess that I have not followed his handling of his work

Yours sincerely

W. T. Thiselton-Dyer

If I understood you rightly the subject about which you mentioned to me you were investigating was the distribution of the florets on the receptacle in the Ox-Eye daisy, (Chrysanthemum leucanthemum). These exhibited recurrent "maxima of frequency". I gathered that the space order was what was in question & that particular types occurred more often than might be expected.

Now Hubert Airy has I think successfully shown (Proc. R.S. 21, p. 176 & 22, p. 298) that the passage from one arrangement to another in the case of uniformly smooth spheres packed together by a suitable mechanical arrange=ment is effected by what he calls 'condensation'.

I should expect that if such a state of things existed in nature and 'variation' came

Thiselton Dyer

1895

f.24

into play, ~~then~~ the results would follow the ordinary frequency curve.

But they apparently do not. What then is the cause of the discrepancy?

I can only tentatively suggest that the explanation is to be sought in the want of homogeneousness in the substance of the structures subjected to 'condensation'. They would fall into some arrangements more readily than into others, or some arrangements would be more stable than others. It would be in some degree analogous to what would happen at roulette if the path of the ball to some holes ~~[illegible]~~ had been made easier than to others.

I think you said that these recurrent numbers were widely distributed in the vegetable Kingdom. But this is intelligible when we remember that all lateral organs have been packed in the bud and subject to 'condensation'.

W. T. T. D.

Kew, May 10. 95

Thiselton Dyer

f3r

Kew, Dec. 9. 96

Dear Galton,

I have just returned from a rather belated holiday abroad. I am not yet quite in touch again with "the resources of civilization". I am sometimes alarmed at thinking how easily, & with what pleasure, one could lapse into a barely feral existence!

But Salvin has told me about the meeting at the R.S. and Sir Joseph Hooker something of a scheme about Down.

at Oxford when he followed Lord Salisbury. It then appeared to me that he virtually threw over Darwin. I cannot doubt then that it wd. be of immense importance to establish a body of fact & I believe it might be easily done, which would be secure from the attacks (not always I am afraid disingenuous) of men like Bateson.

The difficulty lies in the 'continuity'. That must be absolutely unimpeachable. In a brief conversa-

f.3v

of Nature matter, so far,
have I any distinct gift.

I have no doubt, & have
long thought, that most
valuable results might be
obtained with regard to matters
commented on in your first
paragraph, especially amongst
plants, by continuous observation.
The facts that come under my
notice in ordinary Horticulture
are simply astounding. But it is
<u>generally</u> impossible to ~~[illegible]~~
make use of them for any
scientific purpose on account

of the practical impossibility
of getting any accurate
record of their history. I
selected the garden Cineraria
as a case on which I feel
myself on secure ground. But
as you know even that was
vehemently contested by Mr
Bateson.

Nothing astonishes me so much
as the general ignorance
which obtains amongst scientific
men: — (i) as to the enormous
~~possib~~ possibilities of variation;
(ii) as to the potency of
natural selection. The last
public utterance I heard of
Huxley's was at the B. Ass^n

to the average mind.

The only chance I see is to get hold of some man of means like Sir John Lawes or Mr Darwin who will be content to settle down and peg away year after year, carefully recording his results. Any-thing like an Institution requires hired assistants and I see the difficulty of either getting them to stick to the work or of their being capable of being thoroughly relied on.

There is the further difficulty

Thiselton Dyer Dec 9/96 f.48

tion which I had on the subject with Sir J. D. Hooker, if I understood him rightly, he was impressed with the same [redacted] thought that had occurred to me. It w^d. be easy enough to set up an Institute and start experiments. But unless an observer could be secured who would go on for a lengthened period, say 25 years, I am not very confident that the result would be of any value. I could point to some experiments by Decaisne which extended I think over some twenty years

only to prove a
fiasco – In astronomy
the phenomena observed can't
be meddled with. The
variable elements are (1) the
personal equation of the observer,
(2) the instruments. Both
can be verified. But in
observations such as is
proposed, the least carelessness
on the part of an ~~[illegible]~~
assistant, such was the
change of observer, may
vitiate the whole ~~[illegible]~~ matter of observation.
My experience as the head
of a large technical staff

shows me how difficult it is
to attain accuracy in the
most trifling matters. In
fact I am not sure that
if experiments such as you
contemplate were carried on
here I should have much
confidence in them unless
I did them myself, & even
then I should fear that
some source of error might
creep in. In a scientific
experiment I don't think ~~you~~ one
can trust or believe anybody;
not because they are dishonest,
but because the canons of scientific
rectitude are not intelligible

Thiselton Dyer Dec 9/96 f.5

ROYAL GARDENS KEW DIRECTOR

of deciding on experiments the significance of which will be generally accepted. I am very much struck with the discordance which has become apparent as to the bearing of Prof. Weldon's investigations.

Yours very faithfully

W. T. Thiselton-Dyer

Lead of expts taken out Feb/97

f.1

Wallace
1891

Sent to Mrs Romanes 1894
· & returned
I had previously in 1891 ~~or~~/92
lent them to Romanes who was
occupied with a similar project

Private

(1) Parkstone, Dorset.
Feby 3rd. 1891

My dear Mr. Galton

Don't you think the time has come for some combined and systematic effort to carry out experiments for the purpose of deciding the two great fundamental but disputed points in organic evolution,—

(1) Whether individually acquired external characters are inherited, and thus form an important factor in the evolution of species,— or whether as you & Weismann argue, and as many of us now believe, they are not so, & we are thus left

series of experiments at the same time,— and if the Zool. Soc. would allow some of the experiments to be made with their animals in their gardens much ~~experiment~~ expense would be saved. To be really good however the hybridity experiments (and the others too) would have to be carried out with large numbers of animals, and thus some sort of small experimental farm would be required. Surely some wealthy landlord may be found to give a small tenantless farm for such a purpose. Then, using small animals such as _Lepus_ and _Mus_ among mammalia, some gallinaceous birds and ducks, and also insects, a good deal could be done even on

f.2v

to depend almost wholly on variation & natural selection.

(2) What is the amount and character of the sterility that arises when closely allied but permanently distinct species are crossed, and their hybrid offspring bred together. Whether the amount of infertility differs between the hybrids of species that have presumably arisen in the same area, & those which seem to have arisen in very distinct or distant areas — as oceanic & other islands.

Both these questions can be settled by experiments systematically carried on for ten or twenty years. The question is how is it to be done. Talking over the matter with Mr. Theo. D. A. Cockerell, a very acute & thoughtful young naturalist we came to the conclusion that a Committee of the British Association would probably be the best mode of carrying out the experiments, by the aid of a B. Assn. grant & a Royal Society grant, aided perhaps by subscriptions from wealthy naturalists. It seems to me that one paid observer giving his whole time to the work could carry out a number of distinct

P.S. It would of course be better still if a fund could be raised sufficient to establish an Institute for experimental enquiry into the fundamental data of biology. This is surely of far higher importance than the anatomical, embryological, & other work for which the Plymouth Biological Station was founded.

A.R.W.

Feb 3rd 1891

P.3r

a large scale, at a small cost. On the same farm a corresponding set of plant-experiments could be carried out; and an intelligent well educated gardener or bailiff, with a couple of men or even one, under him, could superintend the whole operations under the written directions and constant supervision of the Committee.

Would you move for such a Committee at the next B. Ass. meeting? You are

f.3v

the man to do it both as the original starter of the theory of non-inheritance of acquired variations, the only experimenter on pan-genesis, & the man who has done most in experiment and resulting theory on allied subjects.

We thought first of a separate Society, but I doubt if a new society could be established & supported, whereas a Committee either of the B. Ass. or of the Royal Society could do the work quite as effectively & would probably receive as much support from persons interested in these problems. It seems to me a sad thing that years should pass away & nothing of this kind be systematically done. I feel sure you would meet with general support if you would propose the enquiry.

Believe me
Yours very faithfully
Alfred R. Wallace

Francis Galton F.R.S.

P.T.O

Parkstone, Dorset.
Feb. 6th. 1895

My dear Galton

If I remember rightly my letter, to which you refer, was mainly a statement of some suggested experiments which, in my opinion, *would* serve to test the question of the inheritance of acquired variations, and the reason I marked it *private* was, because it was written hastily and without that careful and leisurely consideration which would be given to a paper for *publication*. I had, & have, no objection to its being shown to any one interested, but only to its being in any way made public by *being printed*. As to Cunningham's

Bateson's & your views has been cut in two, but it has been in the Ed. of Fortnightly's hands — & proofs corrected — since last August, & only now got in! I told you at the time you published your finger-print articles that I thought your deductions from them as to Nat. Selection & species &c. *all wrong*, & you will see I still hold that view & give my reasons —

I would rather you did *not* send my letter to Cunningham. Wait till something definite is being proposed or doing.

Yours very truly

Alfred R. Wallace —

Francis Galton Esq. F.R.S.

f.4v

suggestion, of course the thing ought
to be done somehow; but
Francis Darwin called on me
the other day, & he said that there
was a feeling at Cambridge that
Cunningham was not a man
of sufficient weight to carry it
through, & that many would
have nothing to say to it from him, — which
seems to be the case.

I think your plan would be a
good one, and why should not
you, Lankester, Poulton, & one or
two others start it in earnest?
Would Flower support it & get
the Zool. Soc. to allow experiments
in the Gardens? The Jodrell Laboratory
at Kew ought to be the very place
& means of experiments with
plants. How I wish Darwin
had left money for such
experiment instead of for that
huge Kew Catalogue of Plants
which is being made so expensive
and bulky that it will be
almost useless for the purpose
he intended it for, & really
money wasted!

Surely if the thing were made
sufficiently known & its importance
dwelt on, there are some few
rich men who would endow a
modest institution of the kind needed.

I am sorry my article on

of individuals to be kept healthy and to be largely increased by breeding, — and they would all have to be continued during several years depending on the duration of life of the various species experimented with.

My wife and I are in pretty good health & beg to be kindly remembered to Mrs. Galton. As everybody seems to come to Bournemouth we shall hope some day to have a call from you.

Yours very faithfully

Alfred R. Wallace.

F. Galton Esq. F.R.S.

(2) f5r

Parkstone, Dorset.

Feb. 7th. 1891

My dear Mr. Galton

On receipt of your interesting letter I sat down & jotted the enclosed notes of the kind of experiments that it seems to me would test the theory of heredity or non-heredity of individually-acquired characters. Also a few as to fertility or sterility of hybrids, and as to the real nature of some of the supposed instincts of the higher animals. I do not myself see much difficulty in carrying

f.5v

out any of these, but then I am not an experimenter as you are. Still, I shall be glad to know exactly where the difficulty or insufficiency lies. If these, or any modifications of them, would be valuable & to the point, it seems to me that the mere keeping the plants and animals in health & properly isolated would fully occupy the keeper or keepers of the farm, - while the actual experiments - the deciding on the separation without selection of the various lots to experiment with, - which should be crossed & when, and other such matters, would recur only at considerable intervals & could be supervised by the members of the Committee, or some of them, by means of, say, a weekly inspection.

I have limited my notes to three points in which I feel most interest, but of course experiments on variation such as Mr. Merrifield is carrying on for you, could be added to any extent if there were any danger of the keepers having too little to do!

All the experiments I suggest would require considerable numbers

4

Wallace
7/7/91
with letter

Experiments for testing heredity of acquired characters

1

f.6r

With Plants.

1. Effect of wind.

Two identical sets of perennial plants - one set grown for successive generations fully exposed to wind, both natural and artificial - the other set grown in as still an atmosphere as possible consistent with health.

When a decided effect of the different conditions is perceptible, let seeds of both sets be grown under identical conditions, and it will be seen whether the individual difference is transmitted.

2. The effects of drought and moisture, could be tested in the same way.

3. The effects of diversity of soil - chalky, sandy, clayey, - peaty, - might be similarly tried.

4. The effects of light and shade, might be easily tried

2) <u>Remarks</u>.

f.6v (3)

Experiments made with a variety of species as here suggested would test the theory in two ways; - firstly, by showing whether any increased effect was produced in successive generations, and, secondly, by ascertaining whether the effects produced, leading to a diversity in the two sets, continued to any degree when the seeds of both were grown under identical conditions.

Of course, selection of any kind must be rigourously excluded. The seed for each successive sowing must be taken by some mechanical method that would give a fair sample of the <u>total amount</u> of seed produced. Plants must be chosen which are vigorous enough to grow under the varied conditions without injury to their health or powers of reproduction, and considerable numbers must be grown so as to eliminate the effects of accident and to render a fair sampling of the whole crop possible.

If <u>any</u> decided effect on the individual plants is produced by the diverse conditions of growth here suggested, acting on individuals for several years, there would not appear to be any great difficulty in deciding whether these effects were or were not transmitted to succeeding generations.

8

6. A more expensive but a good and practicable experiment would be with, say, three or half-a-dozen half-wild horses such as the Mexican mustangs (or better still some really wild horses.) These should be bred till a lot of, say, twenty were produced. These divided into two lots of equal quality should thenceforth be systematically trained, the one lot as hunters or racers the other lot as carthorses these being daily used in drawing weights at a walking pace and never allowed to run. After five or six years these should be bred from to see if the offspring exhibited any differences in the direction of cart-horse or racer respectively.

f75 15

Experiments for testing heredity of acquired Characters

With Animals.

1. Two sets of common dovecot pigeons might be exposed to different conditions, one set being allowed freely to fly about, the other set being confined in a spacious yard netted over at two or three feet from the ground so as to prevent flight. Birds reared and kept under these different conditions for several years would probably exhibit differences in the development of the bones and muscles of the wings and legs respectively. After these differences became distinctly perceptible and capable of measurement, the offspring of the two sets should be reared under identical conditions when it would be seen whether the effects produced on the individuals were transmitted to their offspring.

2. Ducks might be reared and kept for several years without access to water except for drinking & washing, another identical set having ample swimming space. Some acquired individual differences might arise between the

two sets as regards the webbed feet or the development of the muscles used in swimming. If any such change could be detected heredity could be tested as in the case of the pigeons.

3. Poultry, pigeons, ducks, pigs, rabbits &c. could be fed on two kinds of food, the one very nutritious the other comparatively innutritious, but so arranged as both to be compatible with health. After several years of this treatment, beginning with very young animals, a considerable difference would probably be found in the average size & weight of the two sets. When this was clearly established the offspring of the two sets, if exposed to identical treatment, would show whether the effects produced on their parents were hereditary.

4. Similar experiments to the last could be tried with Lepidoptera of various species. The particular kinds or quantity of food of the larvae being regulated so as to produce broods larger or smaller than the average.

5. In other cases the larvae might be exposed to low temperatures so as to produce melanism, ~~and both these~~ By breeding from considerable numbers of insects thus treated and a corresponding set bred normally and subjecting these to identical conditions it would be seen whether the modification produced in the individuals was transmitted to the offspring.

6. (see over)

In these experiments with animals, as in those with plants, considerable numbers of individuals must be employed, and at every stage of the process selection must be rigidly excluded.

12

Experiments on Instinct

F8r (9)

~~E~~ An important series of Experiments might be carried on as to the alleged instincts of the higher animals.

1. The supposed nest-building instinct of birds could be tested by means of a wired enclosure of considerable extent with bushes and shrubbery, into which young birds could be turned loose, after having been reared in such a way that they could not have seen the proper nest of their species. Half a dozen experiments of this kind, with the nest built by the birds wholly from their "inner consciousness" placed by the side of the typical nest of the species, would do more towards the settlement of this vexed question than all that has yet been written on it.

2. The alleged "direction instinct" of dogs, cats, and other animals could be tested by a few experiments which have never yet been made. Dogs or cats whose knowledge of the locality has been restricted to a mile or less from their place of birth, should

be taken thoroughly blindfolded, and by a very circuitous route, to a spot from 3 to 10 miles distant according to the nature of the country, and their proceedings carefully watched and recorded by some person not known to them till they either found their way home or gave up the attempt to go home. We should thus learn their mode of action, what senses or faculties were used, and what proportion of the species were able to find their way home across a completely unknown intervening country. We should learn whether any special instinct was involved, or whether, (as I think more probable) all the recorded cases are the result of the use of the ordinary ~~senses~~ faculties of sight, smell, and memory, combined with a certain amount of good luck.

3. Another important set of experiments would be the repetition and extension of those made by the late Mr. Spalding on newly-born animals. It might thus be ascertained whether the knowledge or experience of the parent is inherited, or merely those nervous and muscular co-ordinations which render some sensations pleasurable others disagreeable or alarming, and which produce corresponding reflex actions which simulate the result of experience.

16

isolated areas.

Experiments on the fertility of Hybrids. f9r 13

Animals.

In order to arrive at any definite and trustworthy results it will necessary (a) – to choose the species of such genera as are known to breed freely in confinement. (b) Several closely allied species should be chosen and all the possible crosses made between them. (c) The hybrid offspring produced by each cross should be segregated, and so treated as to give them every opportunity of breeding. (d) A considerable number of individuals – 5 to 10 at least – of each sex should be obtained before experimenting with any of the species.

Among the animals that might probably serve for these experiments are the following: –

Mammalia. – Species of Dasyprocta (Agoutis) and Cavia (Guinea-pigs); Lepus (Hare and Rabbit); Gazella, (Gazelles); Capra (Goats, Ibexes &c.) Ovis (Wild Sheep); Cervus (Deer). In the three last groups there are many allied but distinct species which offer excellent materials for experiment in hybridisation.

Birds. — The families Anatidæ (Ducks and Geese) and Phasianidæ (Partridges, Pheasants, Quails and Jungle fowl) offer perhaps the best material for experiment among birds, as they breed rapidly, and many breed well in confinement, and some are known to hybridise.

Insects. — Experiments might perhaps succeed in the hybridisation of some of the allied species of Bombycidæ ~~the~~ a family which includes the silk-worm moths.

Plants.

A very wide field is open for experiment as to the fertility of hybrids among plants. The numerous experiments hitherto made have not had this special object in view. In some cases the experimenters have held that the fertility of hybrids proved the non-specific distinctness

of the parents. Others have been more interested in showing that very unlike plants, often even when belonging to distinct genera, could be hybridised. Others again have sought only to improve the race for horticultural purposes.

A systematic series of experiments in crossing the various species of genera which are usually easy of cultivation, and such as have ~~usually~~ numerous well-marked though closely allied species, would lead to most interesting results. The results of crossing representative species from different continents or from opposite sides of a continent, and of those which inhabit a continent and a continental or oceanic island, would be of the greatest value, as indicating whether some difficulty in producing hybrids and considerable or complete infertility of the hybrid offspring is a constant character of species, or is in any way dependent on their having been differentiated in the same or in

Parkstone, Dorset.
Feby. 13th. 1891

My dear Mr. Galton

It will be I am afraid impossible to discuss the difficulties of experiment you urge, by correspondence, and I will therefore confine myself to a short reply to the objections you have actually made, which seem to me very easily done.

(1) Plants in windy & still air. You say, "it might be said" there had been selection. But this is very easily obviated, & is the very point on which experiment is superior to observation of nature. In an ordinary open garden or field plants properly cultivated are

a difference of the same nature & fairly comparable in amount it would be a decided fact in favour of inheritance. No doubt it might be urged that the effect would be minute but cumulative, & that might be admitted, & the experiment continued under exactly the same conditions for say ten generations. If then no differential effect were produced in the offspring, the evidence would be strong against inheritance. Of course the fairest way would be for the advocates of inheritance to formulate the experiments they would admit to have weight, and the opponents of inheritance to do the same.

Then you say "nature affords an abundance of examples far superior to

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not killed or prevented from flowering & seeding by wind. They grow healthily under it, and I feel sure that not one in a hundred plants would so suffer. The contrast w. be produced not by the violence of the wind in the one case but by its absence in the other set, they being grown in a glass-covered (or glass-sided) garden. If a common perennial plant was grown — a mallow or a wall-flower — for example — a set of 50 or 100 plants might be grown on for 3 or 4 years so as fully to establish whatever change could be produced in the individuals by the diverse conditions.

Then at the end of that time take the whole of the seed produced by each lot, — take two samples of say the 100 largest or heaviest ~~&~~ 100 smallest or lightest, or better perhaps 100 of the average, of each, and cultivate them side by side under identical conditions. It would not matter to me, or I think to you, what anybody said, — but, if there were (a) a decided & measurable difference in the two lots of plants from which the seeds were taken, and (b) there was no measurable or decided difference between the plants grown from these seeds under identical conditions, this would be one definite fact against inheritance, — while if there was mode of procedure; & I regret that I cannot have the advantage of discussing the question with yourself & others who are well acquainted with the subject & with the special difficulties of experimentation.

Believe me
Yours very truly
Alfred R. Wallace

P.S. Pray do not trouble to reply to this unless you think anything further from me may be of any use.
A.R.W

Of course I have referred to the one experiment of [illegible] previous as an example, not by any means considering it one of the best experiments. A.R.W.

Feb 13/91

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artificial use". This I altogether demur to. In nature we always & inevitably have selection of various kinds, due to soil, aspect, winds enemies, overcrowding, &c. &c. &c. & we cannot possibly separate the effects of these from any possible inherited effects due to diversity of conditions. But this is what we can & do do in cultivation. We save plants from overcrowding & therefore from the struggle with other plants, we can give all the same soil & aspect, protect all alike from enemies, give both the same selection or the same absence of selection of seeds. In nature you cannot possibly tell whether any peculiarity in individuals is due to conditions or to genetic

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variation, while if you take those cases where the difference is clearly an adaptation to conditions — as the dwarfer plants at higher altitudes — you have the probability, almost certainty, of a considerable amount of nat. selectⁿ. By experiment you are able to avoid all these uncertainties & determine the effects of certain definite modifications of environment on individuals, — & ~~?~~ then ascertain whether the modifications thus produced are inherited.

Of course the experiments with animals would involve expense but, with the smaller animals not very much, — & I understood you to say that this would not be an obstacle.

If you or any one else will point out the difficulties or uncertainties in the other experiments I suggested I will be glad to answer them, as I think I have done in the one case you have referred to. It is only in this way that we can arrive at a satisfactory

In Nature, too, you have the uncertainty introduced by double parentage; each parent in all cross-fertilis.d plants, may have (a) different characters & have grown under different conditions. In experiment you eliminate this cause of uncertainty.

Parkstone, Dorset.
Decr. 2nd. 1896

Dear Mr. Galton

I was very pleased to have your private circular & letter. I had just written to Poulton, hoping that Oxford would yet do something of the kind. But the R. Soc. will be better still. There are many great landlords & rich men who are Fellows of the R.S. Will none of them give a farm for the purpose? Surely many must have farms of very little letting value. And if

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a great Landlord gave a farm, perhaps some Millionaire might devote a fragment of his wealth to endowing it. If they will not do this they are not very worthy fellows of the R. S.

It would be rather a pity to have the botanical separate from the Zool. experiments, as one resident Manager could as easily look after both as over one. Even with both his time would be barely occupied. Could not additional land be got near Darwin's house & the whole Biological Farm established there? The easy accessibility from London would be a great advantage, and I fancy the adjoining land is rather poor, but dry & healthy. I hope now something may be done.

Yours very sincerely

Alfred R. Wallace.

F. Galton Esq. F.R.S.