Correspondence Regarding William Bateson's Criticism of Walter Weldon's Paper on Crabs

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Bateson's First Attack on Weldon: 1894

St John's College
Cambridge

~~14 Oct. 1896~~

14 Oct. 1896

[revised 20 Oct. 1896]

Dear Mr. Galton,

I am writing to you as Chairman of a Committee of the Royal Society "for conducting Statistical Inquiries into the Measurable Characteristics of Plants and Animals" to ask for information respecting a difficulty I have met with in attempting to follow the Report of your Committee published in Proc. Roy. Soc., 1895. LVII, p. 360.

The material forming the subject of the Report consisted of 7000 young female Crabs (C. maenas) which were chosen "at random except as regards their size." These were divided into 35 groups, each group corresponding to an increase of 0·2 mm. in Carapace-length. The variability of the ratio of the Frontal Breadth to carapace-length was then examined in each group separately. This examination shewed that the variability was greater in the group having a carapace-length of about 11–13 mm. than in the groups of less dimensions, or in ~~adult~~ specimens considered to be adult. It is stated that the law of growth is not yet known and an assumption regarding it is explicitly made (p. 367). The assumption, which I

– daresay may be confirmed by subsequent researches, seems to be not unreasonable, and to it I ~~do~~ am not offering any objection.

That assumption being granted it is then provisionally suggested that "the diminution in the frequency of individuals of given deviation is due to a selective destruction". Upon this hypothesis certain far-reaching conclusions are put forward.

The difficulty that has occurred to me is this. I cannot find in the Report any reference to the condition of the specimens in respect of moults. We are told that the specimens were collected at random except as regards their size, and were then divided into groups, sorted, I presume, solely according to size. What is the evidence that the individuals composing any one groups are in the same moult?

~~As this~~ The importance of this is clear. As the Report shews (p. 362) the ratio of Frontal Breadth to carapace-length changes with growth. But there is I suppose little doubt that after the carapace of a Crab has become hard, increase in carapace-dimensions takes place almost entirely at the moults as a sudden change. This is the view of

older observers and I suppose it is not open to
question.

But if the chief growth occurs at the moults, it
is at the moults that the chief alteration in the
proportions of Frontal Breadth must take place.
Before that deviation of the Frontal Breadth which arises
from Variation can be determined in any sample it is
therefore necessary that the individuals composing
it should be all in the same moult. In the
absence of such determination one sample may
contain individuals in at least two "skins", the one
having different normal proportions from the other.
In the Report no mention of moulting is made, and
I do not know how the homogeneity in respect of moults
is guaranteed for each sample.

It is true that all individuals of each sample have
the same carapace-length within 0.2 mm., but
can it be shewn that individuals of the same size
are always in the same moult?

It appears to me that the question as to the
maturity of the individual in respect of moulting
could be unimportant in one of three
cases only.

(1) If growth were a uniform and continuous process.

(2) If the increase at each moult were so invariable in amount up to a carapace-length of 14 mm. that specimens of the same size are always in the same stage.

(3) If the number of moults and the increase at each moult were ascertainable from a carapace-length of, say, 6 mm. to 14 mm., were known to be wholly irregular in the state of nature [for in this case, as the statistics were taken over a very large area, the growth might be treated as if it were actually continuous].

The first possibility may be disregarded as contrary to fact. Similarly the second possibility need not be considered as being unsupported by the analogy of other forms and contrary to such evidence as there is regarding C. maenas itself.

If the third method of growth had been shewn * or assumed to be the true one, this would no doubt have been stated in the Report, as con-

stituting a ~~vital~~ vital hypothesis. That it (5
is not true is besides almost certain, both from
such direct evidence as already exists, and from
facts given in the Report itself. For in the
larger groups dealt with in Table III I find from
Table I that the number of individuals were
as follows:—

	No. of Specimens.
7·1 — 7·9 mm.	791
8·1 — 8·9 mm.	1078
9·1 — 9·9 mm.	1031
10·1 — 10·9 mm.	1117
11·1 — 11·9 mm.	1106
12·1 — 12·9 mm.	1085
13·1 — 13·9 mm.	803

The fewness of the smallest size might be ascribed
to greater difficulty of collection, but taken in
connexion with the corresponding fewness of individuals
of 13·1 — 13·9, the series of numbers gives support
I think to the view that the progress is not so
irregular as to warrant the treatment even of very
large samples as if the growth of the individuals
were a continuous process. For the numbers seem sufficiently large to give an indication of the comparative frequency of individuals of the various lengths.

I do not know what evidence the Committee
may have had before them regarding the oc-

- currence of the various moults in *C. maenas*, but from such scanty observations as I can find it appears likely that the number of the moults and the increase at the several steps are neither wholly regular nor wholly irregular. This view is in accord with what is known of other animals, especially insects (cp. for example, the results obtained by Chapman in the case of *Arctia caja*. Entom. Record, 1894).

But if the number of the moults and the rate of increase at the moults is not regular, a gathering of animals of the same size may contain specimens in various degrees of maturity in respect of moulting, large specimens belonging to a previous moult and small specimens belonging to a subsequent moult being mixed together.

Since at each moult the Crab enters on a fresh stage in respect of the proportions of its Frontal Breadth, a gathering which contains individuals in various stages in respect of moulting may from this cause alone present a diversity in respect of the proportions of the Frontal Breadth.

(1 Note. See especially Brooks, W. K., On the Rate of Development of the Common Shore-Crab (*C. maenas*) Ann. & Mag. of N. H., 1884, (5) × IV, p. 202.

- Is it not possible that for a carapace-length of 11-13 mm. the heterogeneity proceeding from this cause may have been greater than for the smaller carapace-lengths? If it were so, the greater diversity in respect of maturity might be mistaken for greater variability.

My question then comes to this: how was it known that in the samples of 11-13 mm. carapace-length there was not greater diversity due to mixture of stages in respect of moults than in the smaller samples, and that the especial diversity of proportions observed in these larger samples did not arise from this cause?

An error proceeding from this cause will not be diminished by increasing the number of individuals examined. The whole population, P, of a given length may consist of S individuals in one skin, S^1 in another skin and S^2 in a third skin so that

$P = S + S^1 + S^2$, mixed together in certain ~~difficult~~ percentages.

On the other hand the population, p, having a smaller given length may consist of

– s individuals in one skin and s' in another,

so that

$$p = s + s'.$$

May not the diversity introduced by the mixture be greater in the case of P than in the case of p? This diversity will depend both on the percentages in which the different "skins" are represented, and also on the normal magnitude of the changes in bodily proportions occurring at the mouths separating s from s' and from S from S' and S². But after the samples are numerous enough to represent the percentages truly, subsequent increase in the number of individuals examined will not diminish the error thus introduced.

It seems therefore that the greater deviation of the proportions of the Frontal Breadth in the samples having a carapace-length of 11–13 mm. may be due to greater heterogeneity of these samples in respect of maturity

and number of moults undergone. There is, I think, nothing in the Report which excludes this possibility and it appears to me therefore that the phenomena there discussed are capable of explanation without reference to a Selective Destruction.

It has occurred to me as possible that the slight dimorphism found by Professor Weldon in adult female crabs at Naples, described in the paper (Proc. Roy. Soc. 1893. LIV, p.324) referred to in the Report, may be capable of a similar explanation. Some light might possibly be thrown on this point by a statement of the season at which the samples were collected and of the range of absolute size of the Naples and Plymouth samples respectively.

To return to the samples which formed the subject of the Report it might be suggested that if in any one sample two moults were compounded,

- the measurements of that sample would exhibit a perceptible dimorphism. From the Report I do not feel quite certain that they did not, as the separate curves are not given. But in any case, even if the number of individuals were very large such dimorphism, though existing, might not be very apparent, and the overlapping of the stages may be so irregular as to conceal the definiteness of the dimorphism.

While therefore ~~it~~ on the one hand it is almost certain that the changes in proportions at each moult are so great and so regular as to make it essential that for the purposes of the investigation each sample should be ~~homogeneous~~ homogeneous in respect of maturity, it is on the other hand at least possible that the proportions at each moult is so variable that there is no conspicuous dimorphism even in a sample containing individuals belonging to different moults.

Lastly it might be supposed that if these objections

are
~~were~~ real, the deviations in the case of the
Dentary Margin would have behaved in the same
way as the deviations of the Frontal Breadth.
But from the Report it is likely that the
change of mean in this case is too small and
too irregular to give a clear indication.

The importance of these considerations is so
plain that they must surely have been
examined before the work was undertaken,
and I am prepared to learn that there
is some explanation which I have
overlooked. If there is so I should be
glad to hear what the explanation may be.

Yours truly

W. Bateson.

30A Wimpole Street
22nd October 1896

Dear Mr. Galton,

I quite see the importance of Mr. Bateson's letter. Perhaps it would have been better if I had pointed out the need for paying attention to the question of moults in the first report: but nothing worth saying could have been said until actual observations on growth-rate had been made.

I have now grown some about 1100 crabs, ranging in size from 9 mm. to 13 mm. in initial length, through one moult: and so the data for answering the objection are at hand; but some further computation is necessary before a full answer can be given.

Mr. Bateson is perfectly right, of course, in his statements about growth: namely

(1) Growth does take place entirely (or sensibly so) within forty-eight hours after each moult; and

(2) the amount of growth at each moult does vary considerably in different individuals of the same initial size; so that individuals of the same size, taken at random, have almost certainly not all passed through the same number of moults.

Having these known facts in mind, Mr. Bateson very pertinently asks

(1) whether, since all individuals of the same size are not in the same condition as to moulting, the heterogeneity in this respect may not reach a maximum in individuals of about 12 mm. in carapace-length; and (2) whether changes in the amount of heterogeneity as regards moulting may not afford a sufficient explanation of the observed changes in Quartile deviation.

The first question I have not attempted to answer. I have only attempted to show that heterogeneity as regards moulting will not account for the changes in Quartile

(1.)

Deviation observed during growth in a wild state. Evidently, if this can be shown, the answer to the first question is no longer relevant to this discussion.

I have tried to answer the second question by two independent methods.

Method I. A sample of crabs is collected, and compared with another sample of crabs of the same size, collected at about the same time, from the same place. Each of these samples contains individuals which have moulted n times, others which have moulted $(n+1)$ times (& others which have moulted $(n+2)$, $(n+3)$ ... up to $(n+r)$ times, where neither n nor r is known: but r may with fair certainty, from the observed range of variation in growth-rate, be assumed to be not greater than 10. The theory of chance allows one to assume that if each sample be large, the percentage of individuals which have moulted a given number of times will be approximately the same in each sample (and where the value of r is not more than 10, the Prob. Error of this assumption is small). By measurement it is found that the distribution of frontal breadth ratios in the two samples is sensibly the same: therefore either the constitution of the two samples is the same so far as moulting is concerned, or, the frontal breadth ratio is not a function of the number of moults, but only of the size. In the case of the smallest crabs observed, there is no way of choosing between these alternatives.

But one of the samples alluded to is allowed to grow through one moult, each individual being isolated, and all being kept as far as may be under roughly similar conditions. This sample now consists of individuals who have passed through $(n+1)(n+2)\ldots$ up to $(n+r+1)$ moults: and each individual has increased in size. If this sample be now measured a second time the distribution of frontal breadths in it can be ascertained, and it can be compared with the distribution of frontal:

(2.) The heterogeneity, with respect to moulting, is evidently unchanged: every individual which had at the moment of capture moulted n times has now moulted $n+1$ times: and so on.

breadths in a series of "wild" crabs of the same size. ~~As before, the theory of averages justifies the belief that the percentage of animals, which have moulted a given number of times, is the same in both samples; but~~ this time the curve of distribution of frontal-breadth-~~magnitudes~~ ratios (in the two samples) is no longer the same; the crabs grown in captivity always show a greater frequency of deviations from the mean than is shown by the corresponding "wild" crabs.

Since the frequency of deviation can in this way be shown to increase more rapidly, in a sample of crabs whose heterogeneity with respect to moulting is kept constant, than it does in wild crabs, increase of heterogeneity in this respect cannot account for the observed increase of frequency of deviations.

I am asked to justify an unfinished experiment: and the method of that experiment is what I wish to emphasize:

~~but it is actually less than the observed increase in the Q value of captive crabs in which increased heterogeneity is impossible.~~ [I make this statement provisionally.] ~~The method is that the point I wish to emphasize~~: the result is subject to correction by much computation, which is in part unchecked, and in part unattempted at present.]

It may be said that the whole of this result is due to the effect of captivity, which so often results in increased abnormality. The effect of the captivity may (partly estimated) be ~~checked~~ in this way:

It is quite impossible to tell by inspection when a given crab last moulted; and therefore it is impossible to predict the date of the ~~last moult~~ next moult. In the growth experiments, many crabs moulted within a

few hours of capture; others were kept in captivity for as much as 40 days ~~without~~ before moulting. By determining the value of the coefficient of correlation between time spent in captivity as one variable, and increase of abnormality at a moult as the second, it is possible to measure the effect of all but the first shock of captivity. Speaking as before provisionally, I believe this coefficient to be sensibly zero; so that either the whole observed difference between captive ~~and wild~~ crabs after a moult and the corresponding wild crabs is produced at once, ~~by~~ by mere removal from the shore to a bottle of sea-water, or captivity has no effect upon the result. I ~~regard~~ have some reason to regard the first alternative as improbable.

Method II. — Mr. Bateson's suggestion is, in effect, that the value of the ratio measured, namely $\frac{\text{frontal breadth}}{\text{carapace-length}}$, is a function not only of ~~the~~ actual carapace-length, but also of the number of moults performed by the crab in attaining the given carapace-length.

This can be ~~examined in the~~ tested in the following way:

Crabs, which, after moulting in captivity, have attained a given size, are ~~chosen, and~~ sorted into samples such that the size of every crab in one sample was the same ~~before~~ before its moult; in other words, the rate of growth of all crabs in a sample is the same.

It is ~~probably~~ perhaps true that the crabs which exhibit the most rapid growth-rate at one moult have grown most rapidly in ~~all~~ previous moults: so that upon the whole these crabs will have attained their present size with ~~the~~ a smaller number of moults than the rest. (This is not by any means a certainty: but I think it is fair to assume it as an average.)

Now, if the value of the frontal-breadth-ratio be a

function of the number of moults, as well as of the actual carapace-length; and if the above assumption be granted, then the coefficient of correlation between growth rate after last moult and actual increase of abnormality of frontal breadth after last moult ought to have an appreciable value.

The tables for measuring this correlation are not yet complete: but I have worked it roughly during the progress of the work, and (speaking again subject to correction later) I believe the coefficient in this case also to be sensibly zero.

This method involves an assumption which is not justified by the experiment. But if it be not assumed that there is considerable correlation between growth rate at a given moult and growth rate at previous moults so that the growth rate at the last moult is a rough measure of the growth rate at every previous moult, — if this be not assumed, then the prob: Error of the expectation that crabs of a given size have moulted the same number of times be comes much reduced* and the value of r on page 2 will be only 1 or 2 for the majority of the individuals.

In such a case, if frontal ratio were a function of the number of moults, the curves of distribution for crabs of constant size would, as Mr. Bateson suggests, be dimorphic. In the next report I hope to publish the details of all these curves. They are not dimorphic, judged by any of Pearson's tests: and the appearance of two peaks in the sum of all of them drawn in the last report is due to the fact, already reported to the Committee, that some of them are slightly "skew."

I would once more point out that every statement as to results made in this letter is made without sufficient justification. I have indicated the method, and the result which I believe it will give. More than this I cannot do until the whole series of correlations referred to has been worked out: and, for reasons already reported to the Committee, this involves the measurement of a large series of new crabs, and the treatment of slightly skew curves. These measurements and computations will take some time.

Yours truly

W. F. R. Weldon

*. Because it is only the Prob. Error of the expectation that a considerable number (10–15, say) of fortuitous and independent deviations from the mean growth-rate, occurring successively, will cancel one another.

30A Wimpole Street, W.
22nd October 1896.

Dear Mr. Galton,

I quite see the importance of Mr. Bateson's Letter. –

Perhaps it would have been well if I had pointed out the need for paying attention to the question of moults in the first report: but nothing worth saying could have been said until actual observations on growth-rate had been made.

I have now grown some 1100 crabs, ranging from 9 mm. to 13 mm. in initial length, through one moult; and so the data for meeting the objection are at hand: but some further computation is necessary before a full answer can be given.

Mr. Bateson is, of course, perfectly right in his statements about growth: namely

(1) Growth does take place entirely (or sensibly so) within forty-eight hours after each moult; and

(2) The amount of growth at each moult does vary considerably in different individuals of the same ~~size~~ initial size; so that individuals of the same size, taken at random, have almost certainly not all passed through the same number of moults.

Having these known facts in mind, Mr. Bateson very pertinently asks

(1) whether, since all individuals of the same size are not in the same condition as regards moulting, the heterogeneity in this respect may not reach a maximum in individuals of about 12-13 mm. in carapace-length; and (2) whether changes in the amount of heterogeneity as regards moulting may not afford a sufficient explanation of the observed changes in Quartile Variation.

The first question I have not attempted to answer.

I have only attempted to show that heterogeneity as regards moulting will not account for the changes in Quartile

(1)

Deviation observed during growth in a wild state. Evidently, if this can be shown, the answer to the first question is no longer relevant to this discussion.

I have tried to answer the second question by two independent methods.

Method I. — A sample of crabs is collected, and compared with another sample of crabs of the same size, collected at about the same time from the same place.

Each of these samples contains individuals which have moulted n times; others which have moulted $(n+1)$ times; others which have moulted $(n+2)$ or $(n+3)$ ... up to $(n+r)$ times, where neither n nor r is known: but r may with fair certainty, from the observed range of variation in growth-rate, be assumed to be not greater than 10.

The theory of chance allows us to assume that if each sample be large, the percentage of individuals which have moulted a given number of times will be approximately the same in each: and where the ~~simple~~ value of n is not more than 10, the Prob: Error of this assumption is small.

By measurement it is found that the distribution of frontal breadth ratios in the two samples is sensibly the same. Therefore either the constitution of the two samples is the same, so far as moulting is concerned, or the frontal breadth ratio is not a function of the number of moults, but only of the size. In the case of the smallest crabs observed, there is no way of choosing between these alternatives.

But one of the samples is allowed to grow through one moult, each individual being isolated, and all being kept as far as may be under ~~similar~~ roughly similar conditions. This sample now consists of individuals who have passed through $(n+1)$, $(n+2)$ ... $(n+r+1)$ moults; and each individual has increased in size. The heterogeneity, with respect to moulting, is evidently unchanged: every individual which had at the moment of capture moulted n times, has

now moulted (n+1) times; and so on. If this sample be now measured a second time, the distribution of frontal breadths in it can be compared with the distribution of frontal breadths in a series of "wild" crabs of the same size: and this time the curve of distribution of frontal breadth ratios is no longer the same in the two samples:— the crabs grown in captivity always show a greater frequency of certain deviations from the mean than is shown by the corresponding "wild" crabs.

Since the frequency of deviation can in this way be shown to increase more rapidly, in a sample of crabs whose heterogeneity with respect to moulting is kept constant, than it does in wild crabs, increase of heterogeneity in this respect cannot account for the observed increase of frequency of deviations.

[I make this statement provisionally. I am asked to justify an unfinished experiment; and the method of that experiment is what I wish to emphasise: the result is subject to correction by much computation, which is in part unchecked, and in part unattempted at present.]

It may be said that the whole of this result is due to the effect of captivity, which so often results in increased abnormality. The effect of captivity may partly be estimated in this way:—

It is quite impossible to tell by inspection when a given crab last moulted; and therefore it is impossible to predict the date of the next moult. In the growth experiments many crabs moulted within a few hours of capture; others were kept in captivity ~~long~~ for as much as 40 days before moulting. By determining the coefficient of correlation between time spent in captivity as one variable, and increase of abnormality at a moult as the second, it is possible to measure the effect of all but the first shock of captivity. — Speaking as before provisionally,

I believe this coefficient to be sensibly zero: so that either the whole observed difference between captive crabs and after a moult and the corresponding wild crabs is produced at once by mere removal from the shore into a bottle of sea-water, or captivity has no effect upon the result. — I have some reason to regard the first alternative as improbable.

Method II. — Mr. Bateson's suggestion is in effect that the value of the ratio measured, namely $\frac{\text{frontal breadth}}{\text{carapace-length}}$, is a function not only of actual carapace-length, but also of the number of moults performed by the crab in attaining this carapace-length. This can be tested in the following way:

Crabs which, after moulting in captivity, have attained a given size, are sorted into samples such that the size of every crab in one sample was the same before its moult; in other words, the rate of growth of all crabs in a sample is the same.

It is perhaps true that the crabs which exhibit the most rapid growth-rate at one moult have grown most rapidly in previous moults, so that upon the whole these crabs will have attained their present size with a smaller number of moults than the rest. — This is not by any means certain: but I think it is fair to assume it as an average.

Now, if the value of the frontal breadth ratio be a function of the number of moults, as well as of the actual carapace-length, and if the above assumption be granted, then the coefficient of correlation between growth-rate after last moult and increase of abnormality of frontal breadth after last moult ought to have an appreciable value.

The tables for measuring this correlation are not yet complete: but I have tested it roughly during the progress of the work, and (speaking again subject to

later correction) I believe the coefficient in this case also to be sensibly zero.

This method involves an assumption which is not justified by the experiment. But if it be not assumed that there is considerable correlation between growth-rate at a given moult and growth-rate at previous moults, so that growth-rate at the last moult is a rough measure of the growth rate at every previous moult, — if this be not assumed, then the prob: error of the expectation that crabs of a given size have moulted the same number of times becomes much reduced*: and the value of r on page 2 will be only 1 or 2 for the majority of the individuals. In such a case, if frontal ratio were a function of the number of moults, the curves of distribution for crabs of constant size would, as Mr. Bateson suggests, be dimorphic. In the next report I hope to publish the details of all these curves. They are not dimorphic, judged by any of Pearson's tests: and the appearance of two peaks in the sum of all of them drawn in the last report is due to the fact, already reported to the committee, that some of them are slightly "skew."

I would once more point out that every statement as to results made in this letter is made without sufficient justification. I have indicated the method, and the result which I believe it will give. More than this I cannot do until the whole series of correlations referred to has been worked out: and for reasons already reported to the Committee, this involves the measurement of a large series of new crabs, and the treatment of slightly skew curves. These measures and computations will take some time.

Yours very truly
W. F. R. Weldon

(5.)

* Because it is only the Prob. Error of the expectation that a considerable number (10–15, say) of fortuitous and independent deviations from the mean growth-rate, occurring successively, will cancel one another.

Norwich House
Cambridge. 28 October, 1896

Dear Mr. Galton,

Before replying to Professor Weldon's letter as a whole I should like if possible to have some further explanation of the meaning of two passages in it.

The first passage is that [foot of p. 4] in which it is suggested that on certain hypotheses it is to be expected that there would be an appreciable "correlation between growth-rate after last moult and increase of abnormality of frontal-breadth after last moult." I understand the process previously described, by which it is proposed measure growth-rate after last moult, but I cannot identify the two groups whose frequencies of abnormality are compared in the phrase "increase of abnormality."

The second passage is that [top of p. 5] in which it is suggested that unless growth-rate at the

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last moult is a rough measure of the growth-
– rate at every previous moult, the probable
error of the expectation that crabs of a given
size have moulted the same number of times
becomes much reduced. I mention this
passage because I do not understand ~~it~~ the
suggested argument: it is however of less
importance because I do not question the conclus-
ion, viz. that it is very probable indeed that
all crabs of 12–13 mm. are within one or two
moults of each other.

I am sorry to complicate the discussion by
asking for this further explanation, but
I have been doing my best to understand these
passages without success.

Yours truly,

W. Bateson.

(2)

3 November, 1896

Norwich House
~~ST. JOHN'S COLLEGE.~~
CAMBRIDGE

Dear Mr. Galton,

When I addressed you as Chairman of the Committee I of course put myself in your hands. I therefore accept your suggestion that I should send to Professor Weldon such comment on his letter as I can make in the absence of further explanation, and that for an answer I should await a second publication.

If my request for further

explanation was unreasonable,
I regret it.

The course I took in address-
ing you, instead of making a more
public criticism, was chosen for
two reasons. First it seemed
right that a work which had
entailed severe labour should
be dealt with in a sympathetic
spirit. Secondly it seemed
to me that either the possibility
of error by mixture of moults
must have been dealt with
before the publication of the
Report; or that the absence
of reference to this difficulty
must have been due to an
oversight.

I am yours truly
W. Bateson.

30A Wimpole Street
7th November 1896

Dear Mr. Galton,

The explanation which Mr. Bateson asks for may perhaps be most easily given by taking a numerical example.

I have chosen from the records of crabs grown in 1895 two cases of individuals whose carapace-length, after moulting in captivity, was 13·9 millimetres. The length of the carapace before the moult was

in crab No. 1 — 10·5 mm.
——— No. 2 — 11·6 mm.

So that the amount of growth, during the moult observed, differed greatly in the two cases.

The amount of growth may be measured in either of two obvious ways: we may say that crab No. 1 receives an increment of (13·9 − 10·5) = 3·4 mm. of carapace-length, crab No. 2 receiving (13·9 − 11·6) = 2·3 mm., or we may say that crab No. 1 has increased to $\frac{13\cdot9}{10\cdot5} = 1\cdot32$ times its former length, crab No. 2 having increased only $\frac{13\cdot9}{11\cdot6} = 1\cdot20$ times its original length.

The question, which of these two methods of measuring growth we ought to adopt, is of some importance in estimating the number of moults performed by a crab in reaching a given size: because it involves the question whether the sizes of an average crab, at successive moults, form an arithmetical or a geometrical series.

I cannot pretend to have settled this question: but I cannot express the growth of an average crab as an arithmetical series; while, taking as measure of growth the ratio

$$\frac{\text{carapace-length after moult}}{\text{carapace-length before moult}}$$

I find that this ratio is tolerably constant for crabs whose initial length varies from 8 to 13 millimetres,

although it diminishes slightly, as one would expect, with [mean] increased size. The values I find for 1895 crabs are

Initial length of Crab	Mean Growth Ratio at Moult
8·00 – 8·99	1·270
9·00 – 9·99	1·277
10·0 – 10·9	1·277
11·0 – 11·9	1·275
12·0 – 12·9	1·270

(I do not guarantee any of the arithmetic in this letter! Also the mean for crabs of 8 mm. is based on very few (27) individuals.)

So far as it is worth anything, this result goes to show that the successive sizes of an average crab, at successive moults, are the terms of a Geometrical Series, whose constant multiplier is about 1·275. – Of course, I do not for a moment suppose this to hold throughout life: but it seems a rough working rule, which one may use, for want of a better, during the period of active growth.

Assuming this rule for an average crab, we have to consider a crab observed not to increase at the average rate at one moult. Three limiting hypotheses suggest themselves:

(a) – There may be perfect correlation between the growth-rate at one moult and the growth-rate at others. If this be so, we may safely infer that a crab, observed to grow at a given rate during one moult, has grown at the same rate during previous moults.

(b) – There may be no correlation between the growth-rate at one moult and the growth-rate at others. In this case, whatever the growth-rate at ~~a~~ an observed moult, it is most likely that the

growth-rate at previous moults will have been that of an average crab.

(c) – There may be some kind of compensating arrangement: so that a crab which grows rapidly during one moult grows slowly during the next; and vice versâ. Such a law would imply that crabs which had moulted a great number of times were more likely to have moulted for the same number of times than crabs which had moulted two or three times only: but I cannot easily picture such a law giving an increased heterogeneity up to ~~say seven eight~~ any assigned size and then causing uniformity.

The two hypotheses (a) and (b) are easily considered: and the tables given show the number of moults which a crab of 13·9 mm. may be expected to have passed through on each of these hypotheses, if it has at the observed moult

(α) – the smallest growth rate ~~seen~~ (except one) namely 1·15;
(β) – the growth-rate of crab No. 2 above, namely 1·20;
(γ) – The mean growth-rate = 1·275;
(δ) – The growth-rate of Crab No. 1 = 1·32
(ε) The largest observed rate = 1·35.

1. – Hypothesis of Complete Correlation: Rate ∴ constant through successive moults:

Growth-rate:	1·15	1·20	1·275	1·32	1·35
Sizes at successive Moults	13·9	13·9	13·9	13·9	13·9
	12·1	11·6	10·9	10·5	10·0
	10·5	9·67	8·55	7·98	7·19
	9·14	8·06	6·71	6·04	5·18
	7·95	6·71	5·26	4·58	(3·72)
	6·91	5·59	4·13	(3·47)	
	6·01	4·66	(3·24)		
	5·23	(3·89)			
	4·54				
	(3·95)				

II. Hypothesis of no correlation:

Growth-rate at observed Moult	1·15	1·20	1·275	1·32	1·35
	13·9	13·9	13·9	13·9	13·9
Sizes at	12·1	11·6	10·9	10·5	10·0
successive	9·49	9·10	8·55	8·24	7·84
Moults	7·44	7·14	6·71	6·46	6·15
	5·84	5·60	5·26	5·07	4·82
	4·58	4·39	4·13	(3·98)	(3·78)
	(3·59)	(3·44)	(3·24)		

It will be seen that on the hypothesis of no correlation between growth-rate at successive moults, the most probable expectation is that crabs of 13·9 mm. long will differ by not more than one moult: and the Prob. Error of this expectation is small. The possible range of variation on the hypothesis of complete correlation between successive growth-rates is evidently much greater. On any compensation hypothesis, the crabs would, I think be generally alike as regards moulting.

The truth, as existing observations, already referred to by Mr. Bateson, show, lies between these hypotheses: and my object in making these tables is to point out that in any case a crab which is known to have grown much at an observed moult is likely to have moulted less frequently than a crab which is known to have grown little at one moult: so that the value of the growth-ratio here described is a measure, however feeble, of the chance of heterogeneity as regards moulting.

Now to show how I propose to use this in estimating the effect of heterogeneity of moulting on frontal breadth, we will go back to Crabs No. 1 and 2.

Crab No. 1, before moulting, was 10·5 mm. long: and its frontal breadth, in terms of the carapace-length taken as 1000, was 809. The mean frontal breadth of "wild" crabs whose carapace length is 10·5 was found in 1895 to be 804·55; so that Crab No. 1 was abnormal, to the extent of + 4·45 units, compared with its fellows before moulting. After moulting, its carapace was 13·9 mm. long, and its frontal breadth was 776; that of the mean wild crab 13·9 mm. long being 761·20. Crab No. 1 was therefore +14·80 units abnormal as compared with the mean of its fellows after moulting. Its abnormality had changed +10·35 units during the moult.

Crab No. 2 was 11·6 mm. long before moulting, and its frontal breadth was − 5·42 units abnormal, compared with the mean wild crab of corresponding length. After moulting, its carapace-length was 755, or − 6·20 units abnormal, compared with the mean wild crab of 13·9 mm. The abnormality of Crab No. 2 had therefore changed − 0·78 units only during its moult.

Now, the growth-ratio of Crab No. 1 was 1·32; and the growth-ratio of Crab No. 2 was 1·20: so that these crabs differ, not only in "change of abnormality during moult," but also in growth-ratio. The question is, whether these two differences are generally associated or not. If they are, then the change mean change in abnormality of frontal breadth, observed in a group of crabs whose growth-ratio is 1·20, will not be the same as that in a group of crabs whose growth-ratio is 1·32; if there is no correlation, then the mean

change of abnormality during a moult will be the same in the two groups. — The change in the "mean change of abnormality" as the growth-rate changes is the change referred to in my last letter as the best available measure of possible effect of heterogeneity in moulting: — [illegible].

[illegible]

Yours very truly

W. F. R. Weldon

30A Wimpole Street, W.
7th November 1896.

Dear Mr. Galton,

The explanation which Mr. Bateson asks for may perhaps be most easily given by taking a numerical example.

I have chosen from the records of crabs grown in 1895 two cases of individuals whose carapace-length, after moulting in captivity, was 13·9 millimetres. The length of the carapace before the moult was

in crab No. 1 — 10·5 mm.
——— No. 2 — 11·6 mm.

So that the amount of growth, during the moult observed, differed greatly in the two cases.

The amount of growth may be measured in either of two obvious ways: we may say that crab No. 1 receives an increment of (13·9 − 10·5) = 3·4 mm. of carapace-length, crab No. 2 receiving (13·9 − 11·6) = 2·3 mm., or we may say that crab No. 1 has increased to $\frac{13·9}{10·5}$ = 1·32 times its former length, crab No. 2 having increased only $\frac{13·9}{11·6}$ = 1·20 times its original length.

The question, which of these two methods of measuring growth we ought to adopt, is of some importance in estimating the number of moults performed by a crab in reaching a given size: because it involves the question whether the sizes of an average crab, at successive moults, form an arithmetical or a geometrical series.

I cannot pretend to have settled this question: but I cannot express the growth of an average crab as an arithmetical series; while, taking as measure of growth the ratio

$$\frac{\text{carapace-length after moult}}{\text{carapace-length before moult}}$$

I find that this ratio is tolerably constant for crabs whose initial length varies from 8 to 13 millimetres, although it diminishes slightly, as one would expect, with increased size. The mean values I find for 1895 crabs are

Initial length of Crab	Mean Growth Ratio at Moult
8·00 – 8·99	1·270
9·00 – 9·99	1·277
10·0 – 10·9	1·277
11·0 – 11·9	1·275
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(I do not guarantee any of the arithmetic in this letter! Also the mean for crabs of 8 mm. is based on very few (27) individuals.)

So far as it is worth anything, this result goes to show that the successive sizes of an average crab, at successive moults, are the terms of a Geometrical Series, whose constant multiplier is about 1·275. – Of course, I do not for a moment suppose this to hold throughout life: but it seems a rough working rule, which one may use, for want of a better, during the period of active growth.

Assuming this rule for an average crab, we have to consider a crab observed not to increase at the average rate at one moult. Three limiting hypotheses suggest themselves:

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(b) – There may be no correlation between the growth-rate at one moult and the growth-rate at ~~an observed moult~~ others. In this case, whatever the

growth-rate at an observed moult, it is most likely that the growth-rate at previous moults will have been that of an average crab.

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(3)

II. Hypothesis of no Correlation:

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two groups. — The change in the "mean change of abnormality" as the growth-rate changes is the change referred to in my last letter as the best available measure of possible effect of heterogeneity in moulting: ~~and, as I said in my last letter, it is very small indeed.~~

Yours very truly,
W. F. R. Weldon.

St. John's College
Cambridge.

15 November 1896.

Dear Mr. Galton,

My letter of Oct. 20 related of course solely to the published Report of the Committee; for subsequent proceedings I knew nothing beyond the statement of intention on p. 367 of the Report. I had no reason to regard my criticism as premature.

From Professor Weldon's letter, for which I of course understand the Committee have no responsibility, I learn

(1) That at the time of the publication of the Report the suggested error due to mixture of moults had not been excluded.

(2) That it was not thought worth while to allude to this possibility of error, because no actual observations on growth-rate had been made.

The assumption however that error had not been introduced by mixture of moults was at least as important as the other and somewhat cognate assumption (that as to the Law of Growth), which, though no actual observations on growth-rate had been made, was nevertheless explicitly stated in the Report (p. 367.)

(3) That since the publication of the Report

attempts have been made to shew by two Methods that error was not as a matter of fact introduced by mixture of moults.

(4) That the experiments and calculations upon which these methods are based are still incomplete, but that the results so far are thought to indicate that serious error was ~~introduced~~ not introduced by mixture of moults; that nevertheless the possibility that serious error was thus introduced still remains.

In the following remarks I shall try to shew why, as it seems to me, these results, as at present described, are inconclusive.

Method I. The applicability of this Method rests, I think, on a logical error. Reduced to its elements the argument is not misrepresented by the following syllogism.

> The frequency of deviation is [normally] increased in crabs having a carapace-length of 12–13 mm.
> An [abnormal and even greater] increase in the frequency of deviation may occur without increase of heterogeneity in respect of moults.
> Therefore the [normal] increase occurring at 12–13 mm. is not due to increase of heterogeneity in respect of moults.

? mean

This is an example of "illicit process".

It may also be put thus: —

> Crabs of a captive sample shew greater increase in frequency of deviation than a wild sample.
> But the captive sample is less heterogeneous in respect of moults than the

wild sample
Therefore the normally increased frequency of Deviation in the wild samples [of 12–13 mm] is not due to heterogeneity in respect of moults.

- The nature of the fallacy would be sufficiently indicated if (on p. 2 of 1st letter) after the words "cannot account for the observed increase of frequency of deviations", Professor Weldon had inserted the implied qualification, "in the captive sample."

- There are several subordinate points in the statement of Method I which call for remark. Of these I shall refer to four only: 3 and 4 are I think only matters of wording.

1. After the crabs have moulted once in captivity it is said that the "heterogeneity with respect to moulting is evidently unchanged." I question whether this statement is not misleading except as denoting a merely arithmetical conception. In dealing with live animals allowance must be made for differences in individual constitution and conditions of body, such that the effect of captivity may in one case cause the moult to be hastened and in another to be delayed. There is nothing improbable in this; and apart from organic Variation, such a cause of difference between individuals may easily be found in the states that different individuals must at the time of capture, be in as regards preparation for their next moult.
It would not be surprising if for instance those that at the time of capture are nearly ready to moult, moulted too soon owing to captivity;

while the same disturbance might cause those which were not nearly ready, to be behind hand in their next moult (or vice versa). The result would then be that though all would have moulted once more in fact, yet that one more moult would mean a very different amount of physiological progress in different cases. The difficulty is this: that since an animal at a given moult leaves the last skin behind by a discontinuous change, arithmetically each moult can be counted only as an integral conception; but its consequence to the crab's development may be capable of subdivision by alteration in conditions. Apart from this it is scarcely doubtful, on analogy, that there may be real [? seasonal] Variation in respect of number of moults, and that some individuals may wholly or partially skip a stage which others pass through. [Even in the few observations made by Brooke there is some indication of such a phenomenon. His specimens ♀ D, ♂ C and ♂ Y are most easily brought into line with the rest by supposing that at the transition from 13 × 16 mm. to 17 × 22 mm. from 13 × 16 mm. to 17·5 × 23 mm, and from 12 × 14·5 mm. to 17 × 20 mm. respectively, something of this kind occurred.]

If this is admitted, it follows that though all may have only moulted once more arithmetically, yet as a result of that moult some may have passed into a stage physiologically more advanced than that of the others.

On the hypotheses upon which my objection

rests it is indeed almost certain that by causing a sample all to pass through one moult, the "heterogeneity in respect of moults", that is to say heterogeneity in respect of stages in physiological or developmental progress, would from this cause alone be increased.

2. [on page 3] "It may be said that the whole of this result is due to the effect of captivity, &c."

That captivity has directly or indirectly produced an effect is sufficiently shewn by the fact that the captive crabs after their moult have a greater frequency of deviation ·X· than wild crabs of the same size.

The reasoning which follows therefore, by which it is sought to shew that captivity has had no effect, must be taken as intended to shew that captivity has had no direct effect.

This reasoning I submit is inconclusive. The expectation suggested by Professor Weldon is scarcely one which can be entertained. Apart from the fact alluded to above (§2) that it is

·X· Professor Weldon's letter has ... "show a greater frequency of certain deviations." The word "certain" is in this argument superfluous. For if the deviations shewn by the moulted crabs are of a kind different from those shewn by the wild crabs of 12–13 mm. which formed the subject of the Report, the comparison would have no force.

not hard to conceive "captivity" as operating in contrary ways on different individuals, there is the further difficulty that "length of time spent in captivity" may also produce contrary effects. For on the one hand we may imagine that those crabs which have been longest confined will be the most habituated to confinement and so be in the most normal state of health for their moult; but on the other hand, having been for a longer time exposed to abnormal conditions in respect of food and habits, they might be expected to shew the effects of confinement more than those which moulted when comparatively fresh from their natural surroundings. It would not be hard to extend this reasoning. I can see no reason for imagining that in the case of a short period of captivity, the length of time spent in captivity would be in any practical sense a measure of the effect of captivity -

3. It is assumed that r is not greater than 10. Is this 10 reckoned from the Megalopa, or otherwise? If from the Megalopa, I suppose Brooks's view is accepted and Spence Bate's rejected.
I only ask this to get the point clear: it has no direct bearing on the argument; for I do not doubt the conclusion, that the heterogeneity of the two wild samples in respect of moults may be safely assumed to be the same -

4. [1st letter on page 2] Professor Weldon remarks incidentally that "---- or the frontal breadth is not a function of the number of moults, but only of

of the size". This expression lacks precision; for in any case frontal breadth is not a function of size only. This is sufficiently shewn by the fact that it is the variability of the frontal breadth in crabs of the same size, which forms the subject of discussion.

Method II.

Though the crab's development in regard to carapace-length is undoubtedly broken by the moults into a series of stages in size, succeeding each other with some definiteness, and though a proportional diminution in frontal breadth is a part of the crab's developmental progress, and this progress is similarly broken by the moults into what must be regarded as stages in maturity, Professor Weldon's view demands that these maturity-stages have none of the value of normal and physiologically distinct epochs in the progressive change of the frontal breadth; and that, apart from individual Variation, the progress as regards frontal breadth depends solely on the progress in length made at the size-stages. This I submit is contrary to the analogy of other animals whose developmental progress involves an interrupted passage through a series of size-stages and of Maturity-stages. The view that the crab's development is peculiar in this respect should only be admitted on very direct and convincing evidence.

The method by which Professor Weldon proposes to test this view is an indirect one. He says that if it is true that each moult constitutes a more or less definite stage in maturity of frontal breadth proportions, then, on experiment, a certain correlation should be found where, as far as he can yet tell, does not occur.

It is suggested that the growth-ratio as determined by observation on one moult in captivity, should then be correlated with the difference between the amount of abnormality presented by the specimen before the moult (as compared with the mean for wild crabs of the same size), and the amount of abnormality presented by the same crab after the moult (as compared with the mean for wild crabs of its new size).

For several reasons it may be doubted whether the experiments here described would give ~~sufficient~~ data sufficient for testing the validity of my objection; but admitting provisionally that they do, it is nevertheless evident that in order

to use them for this purpose an operation must be performed which in Professor Weldon's letters is not indicated.

In his second letter Professor Weldon defines one of the variables as "mean change of abnormality" at last moult in a group of crabs having the same growth-ratio. The "group of crabs" are so far only defined as those that have the same growth-ratio.

Now such a group may contain individuals that began and ended the moult at various lengths, some being small and some large, consequently in different stages of physiological development. [I mention this matter here without suggesting that it has any important effect on the result. On this point Professor Weldon's letters are not quite clear to me. The first letter speaks of the individuals in a group as having all begun and ended ~~that~~ moult at the same size. ~~The~~ The second letter however speaks of the groups as sorted according to the growth-ratios only.]

But secondly the groups may contain individuals which before the moult presented abnormalities of contrary signs. I find nothing in either letter which excludes this possibility. Let us consider the effect on the calculation of the mean amount of abnormality.

My hypothesis is that frontal breadth depends partly on the maturity-stage as indicated by the number of moults undergone. Take now the case of a crab showing much + abnormality before the moult and having a high growth-ratio. Before the moult her frontal breadth was wider than the mean for crabs of her size; in other words she presented an appearance of physiological immaturity. At the moult she grows more than the normal amount. She will therefore have to enter into comparison with crabs larger than those with which she would have been compared if her growth-ratio had been normal. It is therefore to be expected that the + abnormality (appearance of physiological immaturity) will have been increased. On the contrary, if the growth-ratio had been less than the

mean, it would be expected that the + abnormality would have been diminished.

On the contrary, if the original abnormality had been negative, a high growth-ratio would after the month diminish the abnormality; for there would be less appearance of physiological precocity.

For + abnormality has not been distinguished from developmental backwardness, nor − abnormality from developmental precocity.

In a tabular form the expectation is as follows:—

	growth-ratio	expectation
+ abnormality before moult.	high	increase of abnormality
ditto	low	diminution - - - -
− abnormality before moult	high	diminution - - -
ditto	low	increase - - - -

Put intellectual development for reduction of frontal breadth and consider the problem in the case of a school of boys, who are sorted into forms according to their height without regard to their ages. Other things equal, boys who are small for their age will appear intellectually precocious, because they are being compared with their juniors and vice versa. Let the boys grow a year and examine the consequences of slow and rapid growth-rates respectively. A boy who last year seemed abnormally precocious will if he has grown more than the average seem less precocious now, because owing to his quick growth he will have to be put into a form of taller (i.e. proportionally senior) boys; but if he had grown less than the average, his appearance of precocity would have been increased.

As this illustration shows, therefore, the same growth-ratio may lead to contrary effects according as the original abnormality was positive or negative. The individuals in each group must therefore

be sorted according to the sign of the original ab-
normality as well as according to growth-ratio,
~~before~~ and the results must be separately compared
within the positive and negative categories.

That if this is done, a rough test of the importance
of my objection might be provided, I admit;
but even then the result would be subject to the
assumptions (1) that no considerable allowance
need be made for special forms of individual
Variation; (2) that no important corrections
need be made for the effects of captivity;
(3) that the assumption as to Law of Growth made
in the Report (p. 367) is approximately correct.

A preliminary though extremely rough test would
be provided by a simple comparison of the sign
of the original abnormality before the moult with
the growth ratio determined at the moult under-
gone in captivity. Adopting Professor Weldon's
assumption that the growth-ratio observed at
one moult is a rough indication of the number

of moults undergone, it would on my hypothesis be expected that initial + abnormality would on the whole (and especially in extreme cases) be associated with a high growth-ratio, and — abnormality with a low growth-ratio.

If such an association were found, the suggestion that frontal breadth depends partly on the number of moults, could not, I suppose, be longer resisted. As Professor Weldon does not refer to such an association, I conclude that it was found not to occur.

With reference to deductions from observation of growth-ratio some subordinate points require consideration, but I do not think they are essential at this stage.

~~Before~~ ~~[illegible]~~ Professor Weldon remarks that on certain hypotheses the crabs of a given size must for the most part be within one or two moults of each other. This I have no difficulty in believing. Indeed, if it were not so, it might be contended with some

plausibility that increase at the several moults was so irregular that a very large sample of crabs might be treated as if the growth of the individuals were really continuous (as indicated in my first letter).

But I would again observe that dimorphism, definite and recognizable, would only be found if the distinction between the proportions at successive moults were fairly sharp. That the gradation from moult to moult, as regards carapace-length definiteness is fairly clear from the statistics which Professor Weldon's and Mr. H. Thompson's collections provide (see my first letter and postscript). The possibility that crabs as they pass from moult to moult may similarly pass through stages in regard to the proportions of their portal breadth, distinct enough to affect the Quartile Deviation, but so irregularly ~~+~~ overlapping as not to produce a manifest dimorphism, is not, I think, excluded by any evidence ~~Before~~ ~~[illegible]~~ at present offered. Pending a statement as

to the details of the curves, and especially as to the "skew" curves, ~~detailed~~ further discussion of this point is impossible.

The Report seems to me to have failed to prove the points which it claims to have proved, for the reason that whereas it is almost certain that crabs like many other Arthropods, in their development pass, by a discontinuous progress through maturity - stages more or less distinct from each other and more or less determinate, yet in the Report this phenomenon has been disregarded, and the crabs have been treated as if their growth were a uniform and continuous process.

It will be clearly understood that in addition to the objection I have urged, there is the further objection that since the frontal breadth measurement is unfortunately a character which undergoes a progressive change, it is necessary that permanent ~~the~~ individuate Variation

delete this paragraph; see letter next page. FG

Norwich House
~~ST. JOHN'S COLLEGE,~~
CAMBRIDGE.

16 Nov. 1896

Dear Mr. Galton,

I see that in the last paragraph of the letter I sent yesterday I fell into a confusion. It arose through my changing parts of the paragraph and leaving the rest. The assumption in the Report was of course not introduced to meet the objection as to precocity and backwardness,

f55v

but the other objection that individual Variation might attain a maximum which at a later age might be diminished.

If the paper is still in your hands I will ask for to delete the last paragraph "It will be clearly understood---.... discussed".

Very yours truly
W. Bateson.

[delete this paragraph; see letter. 7.]

in it should be distinguished from manifestations of precocity or backwardness, which may disappear when full maturity is reached. Until this has been done there is no evidence of Selective Destruction. It was to meet this difficulty, of course, that the assumption as to the Law of Growth was explicitly made (Report, p. 367); pending further evidence, therefore, that question cannot ~~be~~ properly be discussed. (1 Note)

I am,
Yours truly,
W. Bateson.

(1 Note)
Professor Weldon attributes to me the question "whether .... the heterogeneity in this respect [moulting] may not reach a maximum in individuals of about 12 – 13 mm. in carapace-length". I do not think I asked this question. But if I were to hazard a conjecture on the subject, it would be that the maximum heterogeneity in this respect, and in respect of appearances of precocity, would probably be reached at whichever stage signs of sexual maturity

(Note ctd.

begin. From Proc. Roy. Soc., 1896, LX,

p. 224, Note, I learn that sexual maturity may occur at 20 mm. [? the eggs being then already extruded.]. Turning to Professor Weldon's calculations I find that a crab 20 mm. long may be expected to have moulted about twice since it was 13 mm. long.

42 Rutland Gate Sw
Nov 17/96 Private

Dear Weldon

Herewith is another paper from Bateson & I enclose with this his accompanying letter to myself. We must talk over what is the fairest course to adopt when we meet (as we probably shall) before the meeting of the R. Soc on Thursday.

You see that he offers to print his 4 letters for circulation among members of the Club.

My greatest difficulty is

thinking what should be done arises from the lengthiness of the papers. I wish the gone could be stated in much more condensed language.

It would in many ways be helpful, if Balfour were made a member of our Cttee but I know you feel that in other ways it might not be advisable. The other members of it, besides yourself, hardly do enough.

Very faithfully yrs

Francis Galton

f59r

15 Nov. 1896 Norwich House

~~ST JOHN'S COLLEGE~~
CAMBRIDGE.

Dear Mr. Galton,

In the light of the explanation Professor Weldon has kindly sent I think I understand the methods he proposes to employ. It has seemed therefore more convenient to make my comments in the form of a letter to you as Chairman, so that if the matter comes eventually before the Committee all the four letters may be in the same form.

I do not know quite what course to propose to take. I shall not of course expect any answer pending the completion of the calculations, but I suppose that as the Committee is responsible for the Report, they should have an opportunity of considering the objection, of which the seriousness has not I think been diminished.

As in such a matter no opinion can be formed without rather close attention, I am willing to put the four letters into print for distribution to the Committee.

I shall not do this without your leave and in the first instance I should of course submit a proof to you.

In my copy of Professor Weldon's second letter there is a slip on p. 5 – "carapace-length was 755" for "frontal breadth was 755". I do not know if it occurred in the original. Anyhow it is obviously a slip of the pen.

Ever yours truly
W. Bateson.

F.60r

3 January. 1897 (I) Norwich House /
~~ST JOHN'S COLLEGE,~~
CAMBRIDGE.

All over

6.2.97
This was not written with a view to circulation W.F.

Dear Mr. Galton,

I very much appreciate the suggestion that you and Weldon so kindly make, that I should join the Measurement Committee. On the whole however I think I had better not. I am not convinced that the present lines of inquiry of the Committee are fruitful and I do not think it is likely that the results will be at all proportionate to the labour expended. But while I would rather not be a member of the Measurements Committee I shall of course be glad to be of any use to them if I can, and it would be a great pleasure to me to be associated with the work of the experimental "Farm" in any possible way.

As you say, it is very difficult to draw up
draw up
questions which shall elicit anything of value
from the breeders respecting the conditions of
prepotency, but I am afraid that side of the
investigation is likely to be very difficult
for a long time to come, whatever subject is
chosen, and I am disposed to think that
the influence of in-breeding is as manage-
able a subject as any. From the stud-
herd-books, without the actual animals for
examination, there is little to be got I feel
sure for many reasons: especially because
(to the untrained eye at all events) there is no
between breeds
surer more to be learnt from crosses than from
the mixing of strains, and the crosses of course
don't get into the pedigree books, &c. for the
most part. For this reason and others
I think horses not likely to be a good subject.

ST JOHN'S COLLEGE,
CAMBRIDGE.

1.

But ~~if I can~~ perhaps something could be got from the cattle breeders on the subject of crossing Angus and Galloways with Shorthorns & W. Highlands. These are crosses made every year in great numbers for the meat trade and the characters of these 4 breeds are exceedingly definite & the inheritance often very precise. Moreover as between horned & polled there is a fairly sharp distinction that can be put into words (or even into measurements if preferred) without question of personal judgment being introduced. It does not seem impossible that by sticking to the first instance closely to one simple point like this, something might be done by a collective inquiry. Possibly the Smithfield Club might be induced to give some facilities & to put us in the way of making inquiries. The advantage of this subject is that

(2

~~in-bred~~ 4) between thoroughbreds

the crosses are made anyhow in the course
of business, and we have simply to see the
results. Moreover as regards the breeding of
the parents we shall generally have the herd-book & vouchers
(which we can't get in pigeons or poultry).
Then as the results are not bred pure, there is
not likely to be any reluctance to give ~~the~~ information.

Darwin speaks of the cross below. polled & horned as "not invariably" producing horned offspring. as far as I have seen it is ~~nearly always~~ very generally polled. In some notes I have of the cross-breds at Smithfields in two years shows. this comes out strongly. (2nd Ed.

Of the points given in An. & Pls Chap. XIV several
are very suitable, I quite think, especially the
Trumpeters & Silkies - but they of course are for expt.
not for inquiry. ~~at least not~~ It is to be remembered
that in both cases presumably we should be concerned
with breeds originally sports in all probability
(By the way, though silkiness may not be commonly
inherited, the purple colour of the flesh curiously
enough often is - I saw two pullets lately bred
out of Wh. Leghorn × Silky cock, both totally purple
with very peculiar ~~feathers~~ plumage & exactly like each other though
fowls I ever saw - though not Silky at all) quite unlike any

142/3 f62r 5

1.

ST JOHN'S COLLEGE,
CAMBRIDGE.

Fowls are in several respects more manageable for the particular purpose I have in view (and I think for inheritance - expts. generally) than Pigeons, because you can use the same cockerel to diff. pullets & so make control expts. (& vice versa) whereas in the case of Pigeons this is not so easy. Also you can get so many brothers & sisters from the same laying while with Pigeons you can only get the pair.

Sheep in some respects are as suitable as cattle, as there too crosses are constantly made, but the records of the breeding of the ewes wd. not generally be so certain as in the case of the cattle. At the same time some fine results could be got as to the properties of particular lines, for the reason that one sire is used at the same time & place with such a large number of mothers, though very often these dams would not all be bred the same way & would very frequently be cross-bred in quite uncertain ways.

(3

~~It does~~ From what I have learnt of J. Hillais' views it has ~~seem~~ seemed to me that he can have no notion of the great irregularity of inheritance – Perhaps in dogs the characters blend much more evenly than in other animals. I heard him, for instance, at the Lost. Soc. lately state that a wild race is prepotent always against a domesticated race, which is certainly not the case – witness the 'light' mules betw. Canary & Goldfinch, &c. (I saw a striking case to the contrary yesterday. We have here a lot of hybrids raised by Lynch from seed of a wild Cineraria (C. multiflorus) straight from the Canary Islands fert.d with pollen of several Garden kinds. In the great majority of seedlings the flowers are practically the garden form – in a great number, curiously enough, they are blue or purple according as the male parent was blue or purple. I don't say they are the same shades at all, but they might be described

f.63r

1.

ST JOHN'S COLLEGE,
CAMBRIDGE.

as being on the 'acid' side or the 'alkaline' side according to the paternal influence)

I expect to be in London for a few days this week, beginning Tues. (St. Pancras Hotel) and if there ~~would~~ is anything I could make clearer I would at any time meet you.

Yours truly
W. Bateson.

(4

Bateson

Bateson
Jan 3/97

f 63v

[This was clipped behind Bateson to Galton, 3.1.97] f64

Space

*1

F. Galton.

(mammals, birds, fish, insects)

Wanted a stock of various hardy plants and small animals, of well known pedigrees, suitable for future experiments. Each sort

1) to be divided into groups to be severally placed in charge of Zoo: Gardens, Botan: Institutions, or private persons in all cases under conditions that will ensure their multiplication without accidental crossing with other varieties

2) Variation. Arrange with horticulturists to preserve for us to rear & propagate all markedly aberrant forms, such as gardeners call "rogues", and pluck up and now throw away.

3) Reversion Try to breed back domestic varieties to, or rather towards, their original or supposed feral forms. Record carefully the results in the successive generations (Statistics of 1) percentage ~~th~~ [illegible] in which the form was derived, 2) ditto as to return

4) Telegony – At laboratory, pair together marked varieties; keep parents, and kill the hybrid offspring (if not wanted for other purposes). Send the parents away to correspondents to panmix and to watch for telegony. Repeat in successive generations always first 'soiling' the females before breeding them pure forms

5) Fraternal and Confraternal coeffts of variation. (Frat: means offspring of same sex, of same parents. Confrat: means those of similar pairs of parents, in respect to the characteristic in view)

1. offspring of panmixed parentage.

2. " of well bred and long established varieties, descended originally and many generations back, from a single pair and subsequently panmixed inter se (Quere. with or without destruction of aberrant forms in the mean time).

3. " of specimens of above race, closely interbred for successive generations (memo: in an ideally pure and stable breed, there wd. be no fratl or even confratl variation).

6) Parento-filial coefficients of variation similarly to the above

7) Mental influences on mother affecting child

If inquiries about this are made of farmers, corresponding expts could be devised with small mammals (as with Jacob & his flocks)? mainly by amateurs.

8) Period of gestation (like Lord Spencer's tables) Journ R. Agr Soc. Vol I. Using only the produce of mothers (mares) who had taken the horse once.

9) Records should be begun of all persons who now make experiments in breeding & ~~those~~ [illegible] interesting varieties, – of all public Zoo & Botan: Instits and of others likely to help.

10) Efforts to be made, to cause more information (as color, stature) of pedigree stock to appear in stud books and especially with guarantee of accuracy of prize winners at shows. – Vertical measures of pedigree stock at cattle shows, so far as my cathetometer could do it

cathetometer

W. Heape.

1.) Effect of different foods & different qualities of these foods, on the breeding capacities of animals and on their offspring produced under such various conditions
(Consult physiologists as to kinds of food)

2.) Phenomena attending the occurrence of 'heat'. Investigation in relation of 'heat' to ovulation. Relation of insemination to ovulation

3.) Artificial insemination of animals, not only of females with spermatozoa from males of their own species, but from those of different species

4.) Carefully tabulated record of effects of in-breeding

5.) Experiments to induce telegony.
All these c'd be performed on mice rats rabbits guinea-pigs & wd be of interest to breeders eventually

Prof Meldola

1. Per saltum development :– Select well marked sports or aberrations of insects (several species cd be named) and breed 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 crossing, 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 'color-sensitive' larva (many cd be named) and bring up in groups among differently colored surroundings. (Two extremes,- brown and green, wd suffice). See whether the descendants from the two former inherit any bias towards the green or brown form and if not, whether such bias manifests itself after years of breeding under same conditions
3. Physiological correlation :– Take batches of eggs of a species known to be very variable. Bring up under different conditions (food, temp:, moisture). Find percentage of deaths before reaching maturity (external selecting foes must be excluded). Compare the final mature progeny, and see whether any particular variety is favored 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.

A. Sedgwick

1. Extensive and abtard hybridising both in plants and animals from the point of view of obtaining new hybrids
(2). In the case of failure in hybridising, to ascertain at what stage the failure came in, whether after entrance of the spermatozoa or before
3. Thorough investigation of the phenomena of telegony in birds and mammals and, if possible in plants
3a. Amount of change wh: can be produced by continued action of changed conditions on one organism, and on organisms following one another by asexual transmission.
4. Transmission of modifications produced by changed external conditions a by asexual reproduction, b by sexual reproduction both parents having been modified, and the external conditions being maintained
5. The same expts, the external conditions not being maintained
6. The same made after the race had been subjected to the changed conditions for a variable time and 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 is produced, modified in any way the subsequent continued inbreeding
9. A renewed examination of inbreeding in the ciliate Infusoria.
10. a confirmation of Watase's experts in producing male and female Rotifera at will.
11. Thorough investigation of parthenogenesis in Insects and ~~in~~ any other forms wh: might present themselves
12. Effect upon a race ~~of allowing~~ apparently well established of allowing all individuals born, or produced, to breed; particularly in plants

13 (I can see no advantage in large animals over small (except in their importance to practical men) with regard to these Expts)

Norwich House
Cambridge

18 January, 1897

— f68

Dear Weldon,

I am obliged to you for your letter.
I had supposed that the reference to myself was made for the reason you give. It is not in any way my wish that it should remain in what you may send for publication; though of course you are at liberty to make reference to the correspondence with Galton if you think fit to do so.

The reference as it stands however, is not precise and I doubt whether it is sufficient. In my former letter (20 Oct. 1896) I was not criticising the Law of Growth assumed in the "Report", but I was pointing out the very different and unspecified assumption that the frontal ratio was not closely dependent on the number of moults undergone,

B

apart from Carapace-length. If you do refer to that correspondence you will of course remember that it is not in the hands of the reader. So far as I know, indeed, it has ~~only~~ been read only by yourself – and perhaps Galton.

To what extent the facts you now record provide an answer to my objection. I am not sure. Before coming to a conclusion it would I think be necessary to see at-all events some of the original figures before they are grouped or ~~averaged~~ averaged. The statement of observations should give the following particulars:

Initial Carapace Length	Initial Frontal ratio	Initial Devn.	Frontal ratio	2nd Carap. length	2nd Frontal Ratio	2nd Devn

– and perhaps the no. of days in captivity before moulting – arranged in order of carapace-lengths.

All these no doubt you have already prepared. Several other quantities could be calculated by the reader

from the rest, but as you must have them ready they should be given. To these particulars ~~should~~ should be added a statement of

(1) the mortality during the experiment.

(2) the season of collection of the 1892-3 samples.

(3) in Table I the number of individuals of each Carapace-length in 1895

and in any case

(4) a Table shewing the relation between initial frontal deviation, growth-ratio, and final deviation.

There are points in the new paper on which I would say something if I could write at length, but for some time to come that is out of the question. I doubt besides whether much detailed criticism would be profitable in the absence of some of the particulars mentioned above.

(3)

In addition to the verbal slips I mentioned in returning the MS. there is surely an error of statement on p. 15. In the sentence "If there is any considerable" you again disregard the question of sign of the initial deviation. As it stands, this statement ~~is wrong~~ can hardly be correct, and I suspect there is an error of method.

As to this new work as a whole I scarcely know how to say what I feel; yet if I say nothing I may hereafter be reproached for my silence. You have given vast labour to it, and I cannot doubt that you have seriously thought yourself justified in the conclusions you put forward. Perhaps too you have thought a little of the responsibility you take in using your authority in publishing such work — for, as

You must know, though many will accept your conclusions, few will read a paper like this and fewer still will even make an attempt to follow it or to judge the matter for themselves. Nevertheless to me the evidence seems so inadequate and superficial that I do not understand how a responsible person can entertain the question of accepting it: I very truly regret that you should give your countenance to such a production.

Yours truly
W. Bateson.

Norwich House Cambridge

1 Feb. 1897

[Posted 8th Feb in envelope]

Dear Mr. Galton,

From what passed at the last Committee meeting and from conversation with Prof. Weldon I have gathered that it is wished that those who desire to make use of the facilities which the action of the committee may create should give some detailed account of the investigations they want to carry out. As I do not know what suggestions may come from others or how far they may be compatible with my own I can only speak for myself, and say how I should individually set about the investigation of the heredity problem if I had the opportunity.

The subjects, I take it, on which we want evidence are these:—

1. The facts as to the transmission of parental characters and the conditions determining the transmission (including the question of the blending of characters, the fixation of characters &c)

(1

2. the conditions (other than those included in (1))
determining a. number of offspring: viz.
fertility or sterility.
b. sex of offspring.

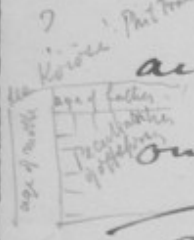
A good deal was said at the meeting as to the importance of discovering the points in which breeders are especially interested. These I think are for the most part included in the above, though for the breeder the questions also take more practical and particular forms, as for example the effects of in-breeding and the extent to which it can be carried without detriment, quantitative evidence as to the effects of crossing, the conditions determining what is called "early maturity" (meaning of course fitness for market), the effects of the age of the parent and so forth, the precise form of the question differing in the case of different animals and different breeds. In fact the questions that the breeder

(2)

are interested in do not differ essentially from those which the naturalist want answered.

Upon any of these points, information relating scattered to special breeds of domestic animals can be easily collected in any amount. A vast quantity is already in print. The difficulty is to find the point at which a line of systematic experimental inquiry can be begun.

With regard to the subject numbered (2), though a good deal has been written, no one I think has yet shewn how to start a fruitful investigation and I should at first leave them altogether out of consideration.



But with regard to the transmission of characters it has for a long time seemed to me that the point at which the beginning must be made is perfectly clear. There is, as I stated in my letter of December, a considerable amount of evidence that the degree to which parental characters are

transmitted may be in great measure dependent on the degree to which the transmitting parent was closely bred. I know no other indication of a positive kind which seems even to lead to any general principle, and so far as I am aware no one has yet made a plausible suggestion as to any other circumstance determining prepotency of transmission. I should therefore begin the inquiry with a statistical examination of the relation between the degree to which a parent was closely bred and the degree to which it possessed the power of transmitting its various characters. Any attempt to determine laws of Regression, or to investigate fixation of characters by experimental breeding must, I think, begin at this point.

I do not think that in what I wrote before I succeeded in making clear what I meant; but if you take the cases given by Darwin in V. & Pl., 2nd ed. chap XIV and consider its application, you will see how important this

(4

preliminary question is. In those of his cases
of extreme prepotency does he call attention to
the backbreeding of the parent (in this connexion,
I suppose to have a different sense but
I mean) which might nevertheless bring several
of his cases under our rule. He speaks of the
whose prepotency of improved Shorthorns – they were
course be about the closest bred animals ever known.
separately considered. "The ball Favourite is believed to have had a pre-
W.B. potent influence on the Shorthorn race". If to
6.2.97 look at the pedigree given by Hutch (Marriage
of Near Kin, 2nd ed. p. 262) you will see that
(1st ed p 252)
Favourite himself came of incestuous breeding
and was bred into most of his descendants, so
that the resulting animals were nearly all
"Favourite". That such a race should be pre-
potent [is just what one would expect on my
over common cattle
hypothesis.
The Austrian "Emperors" are quoted as prepotent.
and his family are
Emperor Charles V is meant especially. He was
the son of 1st cousins – married his 1st cousin.
had son Philip II who married his 1st cousin
and daughter Mary who married her 1st cousin.

(5)

"Some Roman families" are also mentioned -
compare Hath., pp. 56 & 57. ~~[illegible]~~

The Ptolemys are the only royal family known

In the other cases mentioned by Darwin one can no longer trace the pedigrees, but I suspect that in some at least the prepotent animals may have been in-bred.

Without particulars of the back-breeding the statement of their prepotency in respect of certain ~~[illegible]~~ characters has little value as evidence of a principle.

You will see too that if there is any truth at all in the belief that by breeding near relations together prepotency of transmission is obtained, then the problem of the fixation of characters and the establishment of a new race is greatly simplified. The question of the "swamping effects of intercrossing" indeed loses importance beside the other question how much in-breeding occurs in nature and what are its precise effects.

(6)

To investigate the question I suggest that the following subjects are suitable :—

For a <u>collective statistical inquiry</u> I suggest-ed the crosses between Ab. Angus black polled cattle (or Galloway ditto) and other breeds, especially Shorthorns. The results to be collected on a form something like this —
I assume the case of a first cross.

Herd book

Calf – sex. whether heifer or steer. age or date of birth	Sire's name and breed	Sire's H.B. No. <u>or</u> statement of his breeding	Dam's name & breed	Dam's H.B. No. <u>or</u> statement of her breeding	Description of Calf	
					Horns – whether 1. long 2. Short. 3. loose "scurs" 4. polled	Colour whether 1. black 2. blue grey 3. brokenf & if so, what colours.

I should choose a length & kind a classes 1 + 2.

When the statistics were got together they would be classified according to the degrees of close-breeding of either parent and the results compared. It seems to me this would give several elements we want and I can think of no other breeding experiments now made by others on a large scale likely to be so useful for the purpose, though I have no doubt that by special arrangement with some sheep-breeders a similar investigation could be started in regard to sheep. There would however be great difficulty in comparing the results obtained in different flocks.

For experiment I should start if possible three lines of investigation.

I. Fowls. Choose two (preferably three) breeds which are common and hardy, differing greatly from each other — for example black-breasted Game and white Leghorn. The one having long dark legs, a pea-comb, peculiar plumage and "Game" shape & carriage — the other having

short yellow legs, single comb, white plumage and "American" shape and carriage.
Of each I should keep one strain bred in each year as closely as I could, keeping the numbers fairly high if possible.
Of each breed I should also raise birds by crossing strains as remote from each other as possible.
There would then be of each breed two sets of birds differently bred – thoroughbred and shew the characters of their breed – A. closely bred. B. the result of mixing strains.
In each year I should then make as many crosses as I could between

(1) a Game cockerel of strain A run with Leghorn pullets of strain A and Leghorn pullets of strain B – running him with each pen of pullets on alternate days.

(2) a Game cockerel of strain B similarly run with Leghorn A pullets & Leghorn B pullets.

(9)

And a similar pair of experiments would be done with Leghorn cockerels of A strain and B strain.

In each pair of experiments 2 hens are as far as possible brother-cockerels & sister-pullets.

— the results would then be tabulated in respect of each character.

If it were essential that numerical measures of inheritance should be made something might be done in regard to length of leg-bones. Or by using a Bantam breed preferably Black Bantams — something might be made out by examining inheritance in respect of stature and weight, but there might be objection to evidence from Bantams on the ground that they are mostly closely bred already. This would apply less to Black Bantams. Were it not for this the crossing of Game Fowls & Game Bantams would be an extraordinarily good subject for a quantitative experiment. Perhaps the difficulty could be overcome.

II. Finches

(10) If a man were got who had experience

I should certainly start an investigation of the hybrid finches of which I spoke in my letter. arranging the expt. somewhat as for Fowls; but of course in this case only the breeding of the Canaries would be known, for the cock Goldfinches or linnets have to be caught wild. Nevertheless the same Goldfinch cock could in successive years be tried on canary hens belonging to an A and B strain as before.

In many ways this would be a very important expt. but there are so many difficulties in breeding Canaries that I do not think I would attempt it myself without the help of an experienced man. I have never done any.

III. The same thing could be tested simultaneously in some Lebiidæ Coleoptera. For instance Amphidasys betularia (the Peppered moth) has a totally black var. doubledayaria. (Crossed with the type (which is white speckled with black) there is good evidence that intermediates are quite exceptional. The cross-bred brood dividing pretty sharply into type and var.

(P.T.O.)

I think an experiment could be made much as described above, and the numbers of individuals which in the respective crosses were type or var. tabulated.

The cream-coloured Spilosoma lubricipeda and its black var. radiata could be dealt with in the same way. Some particularly good expts. can be made in this case. The Heligoland form of radiata is almost totally black. When crossed with the type intermediate forms are produced, varying between the black form and a kind of half-way form called intermedia - but between intermedia and the type (which is cream-coloured) it is said that no intermediates are produced.

Now in England a race has been reared by selection which is nearly as black as the Heligoland radiata. When this artificial race is crossed with the type, all intermediates are produced - shewing that the selected race has a different centre of stability. I can shew you specimens of these forms at any time.

(12

Perhaps some of these details may show you more clearly what was in my mind when I first wrote. Of course all these expts. would bear on the question of blending or non-blending of characters and would contribute to the solution of the problem (which is a very practical one to all breeders) respecting the degree to which in-breeding can be carried

/

If there is any one at the Station competent to attend to butterflies I should like to transfer to it — for at least a part of the year — some experiments I have now in hand as to the blending or non-blending of a northern and a southern race (Pararge egeria and egeriades) which can breed freely together and of a lowland and an alpine race (Pieris napi & bryoniae) which meet each other at the intermediate height, are capable of breeding together, but yet appear to regress to the two forms. I have to go away for fresh material in the summer and it would be a great convenience to me to be able to leave

(12)

them in charge of some one. The particulars as to them expts. I could shew you easily with the Specimens some day.

I did wish I was going to write to you. Perhaps you would kindly shew him this letter.

Yours truly

W. Bateson.