Correspondence W

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UNIVERSITY COLLEGE LONDON
GALTON PAPERS
3/3/22/1

f1r

11th March
1887

5, SALISBURY VILLAS,
CAMBRIDGE.

Dear Mr Galton,

I have a line from Sir Halliday Macartney, the English Secretary of the Chinese Legation, to the effect that Mr "Fêng" has promised to see what he can do in the way either of delivering a lecture on the ethnography of the races inhabiting the

f1v

the Middle Kingdom,
or of making a com-
munication" to you on
the subject. Thus you
see, the affair is
en train. Do not take
the trouble to acknowledge
this note. If you are at
the Council Meeting on
Monday I shall hope to

f2r

see you. Dr Butler,
I rejoice to say, is himself
again. and was dining
at Cairns on Tuesday
last. With my best
respects to Mrs Galton

Believe me

Truly yours

T Wade

You will understand
that "Mr" Feng is Fêng-yi.
or.

or, when one speaks to him,
Fêng tá-jên. He has been
learning and speaking
English over 20 years,
and is as obliging as
intelligent.

Yrs.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

Sir Thomas Wade

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

5, SALISBURY VILLAS,
CAMBRIDGE.

15th March 1887

Dear Mr Galton,

If you meet Fêng-yi may I ask you to bear in mind that he is not the Interpreter, but the Secretary of the Chinese Legation; Sir Halliday Macartney being the English Secretary of the same Legation.

Fêng

f3v

Fêng, I observe, is spoken of as "Mr Fêng," which form, I admit, I find difficult of digestion. In China he would be addressed as Fêng ta-jên, your Excellency Fêng. his claim to be styled ta-jên resting upon his grade, which is indicated by the red button. on his

official cap. The j in
jên is soft as the French
j in jaune or jeune.
The ê is the e in
merchant, the accent
is laid on the ta rather
than the jên. Thus tan
pronounce ta-jên some-
what as a true Breton
would pronounce tázhun

After all this lecture
you will not find our
friend outraged if I now
call him Mr Jêng.

for

f4v

for he is used by this time to be hailed in that way. I can never resist a protest against it, tacit or overt, as the case may be. The French affect my nerves equally when, believing that they are paying us all the honors, they write "Mr Wade Esquire". Pardon such a diatribe.

Yours truly

Wade.

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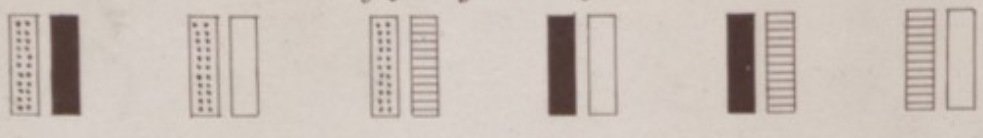
GALTON PAPERS

f.3

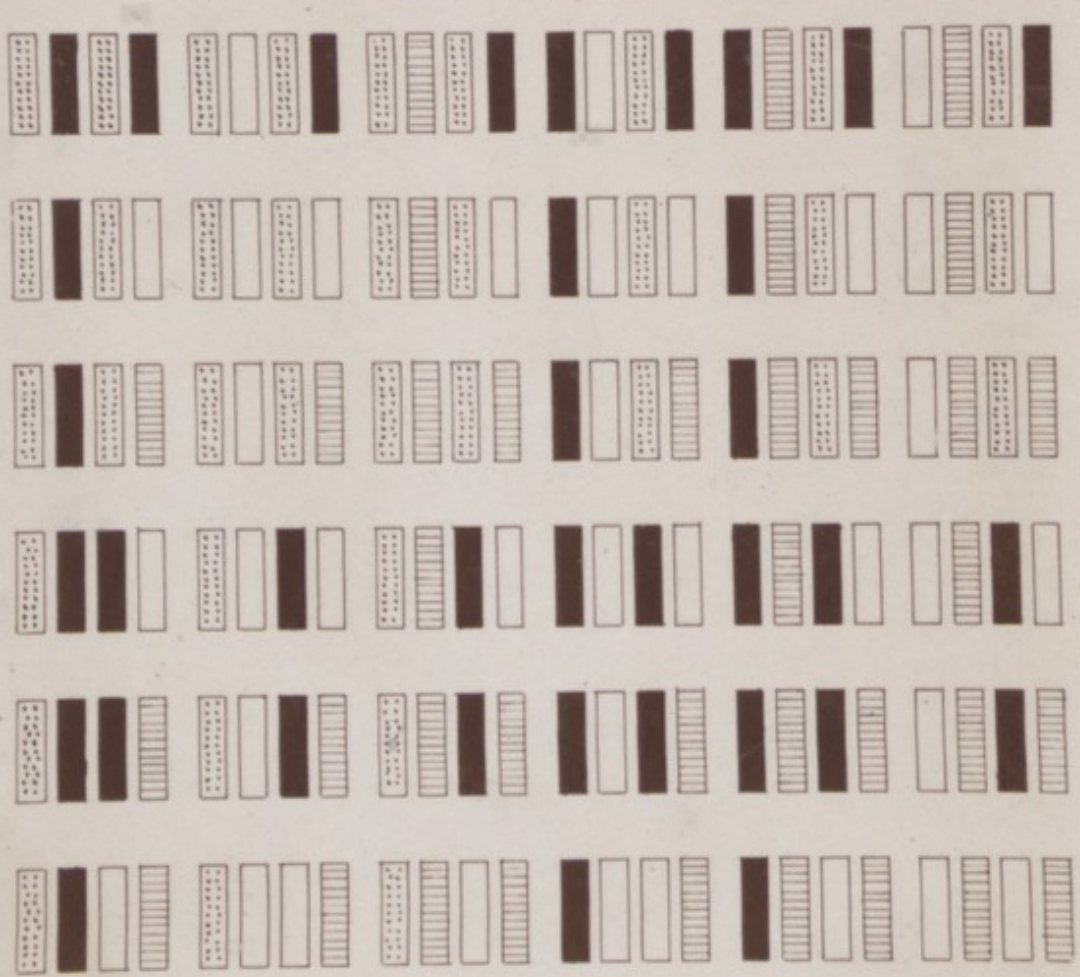
The Possible Combinations Produced by fertilisation in an Organism the Cells of which Contain Four Chromosomes.

The Four Chromosomes are distributed in Pairs to the Gametes, which may therefore

Contain any of the following Combinations.



When fertilisation takes place, two Gametes fuse. The following Combinations are possible, and are probable according to the frequency with which they occur in the diagram.



f 1r

UNIVERSITY OF LIVERPOOL.

DEPARTMENT OF
EXPERIMENTAL & PATHOLOGICAL
CYTOLOGY.

TELEPHONE 19 Y ROYAL.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
3/3/22/2

Prof Weldon

15 Percy Street,
Liverpool.
Jan'y 22nd 1909.

Dear Mr Galton,

My wife tells me that you have very kindly said that you would read the Mss of my book on Heredity. I am sending you herewith five chapters. The rest are in a very unfinished condition. If you are not hopelessly bored with these, I will send the rest later. I have written under two considerable difficulties. The publishers want a book that will be understood by the general reader, and I have felt obliged to deal particularly with Cytology which is highly technical. Without going into con-siderable detail it would be impossible for me to make my interpretation of the Mendelian

phenomena clear. I have found it difficult enough to make Zoological students understand the processes of cell division. It is worse doing it in a book for the general reader.

I am most grateful to you for looking over my MS, and shall be more so for any criticisms you may make, favourable or unfavourable.

With kind regards,

Yours very truly,

Charles Walker.

f2

UNIVERSITY OF LIVERPOOL.

CANCER RESEARCH LABORATORY.

TELEPHONE 19Y ROYAL.

Feb. 3rd 1909.

Dear Sir,

Very many thanks for your letter & for having read my MS. Unfortunately I have dealt with the "steady step by step development of an organism" in a Chapter later than those I sent you. I am doing my best to follow your advice about making the book read more easily. I have felt all along that this was a great difficulty, & am getting two or three friends to read it for me with a view to making suggestions in this direction. I enclose a photograph which may possibly interest you. It is of a diagram showing the possible combinations in an organism with 4 chromosomes upon fertilisation. Again with many thanks.

Yours very truly

Charles Walker.

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PAPERS

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
9/5/22/3

13 Cromwell Road. S.W
26th Dec. 1888.

Dear Galton,

As a new edition of the Geographical Society's Hints to Travellers is being prepared for publication, will you allow me to make a suggestion as regards your section on the determination of heights.

It has recently come to my notice that Pandit K.K's boiling point height of Rima on the Lohit Brahmaputra in Tibet is about 600 feet higher than Needham's aneroid height; as the wider error of the Pandit's thermometer was well known while that of Needham's aneroid was unknown, I at first attributed the difference to aneroid

only.

index error. But subsequently I remembered that it might be largely due to the very considerable monthly variation of the barometric pressure in India - Needham was at Rima - rather I should have said in the neighborhood, for he did not actually reach the place - in December, while the Pandits was there in June. Now to the best of my recollection the barometer stands half an inch higher in December than in June, at Calcutta. Rima is about 4000 feet high in a basin to the South of the great ranges of the Himalayas. It is a long way from Calcutta, 700 miles N.E, and its monthly barometric variation may

be very different from that of Calcutta; still the two probably rise together & fall together; and that a considerable portion of the discrepancy between Needham & the Pandit may be due to this cause. —

I have come across no large discrepancies between barometric results in Afghanistan by different observers each well equipped & thoroughly reliable, & familiar with the best way of treating their instruments; and I believe they were mainly due to the observations having been taken at different seasons.

It has long been a standing order for survey officers in India, when reducing barometric observations, to apply corrections for hourly as well as monthly variations

from the data furnished at the nearest
Meteorological stations. In India
generally the daily rise & fall is so
regular that Col Sykes remarked long
ago that one might (then) tell the hour by
the barometer as well as by the watch.
In Europe the irregularities are con-
siderable, so much so that M. John
Ball seems rather opposed to the appli-
cation of hourly corrections; see Part 3 of
Vol II of the Supplementary Papers of the
R.G.S. — But Europe does not need
exploration, and the regions which are
the happy hunting ground of scientific
explorers of the present day, are more
likely to ~~be~~ be subject to the regular
barometric variations of India than the
irregular of Europe.

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GALTON
PAPERS

f3r

Thus I think the addition of a few curves to Units to Travellers, showing the monthly variations more particularly, and the diurnal also if well marked, for a few centres of reference, would be a valuable addition, affording the means of applying more important corrections to barometric obs^ns. than those which are so happily provided for & facilitated by Parts III, IV & V of your Tables. —

Should you feel inclined to act on this suggestion, you will be able to get full information regarding the barometric curves in different parts of India from H. F. Blanford, late Meteorological Reporter to the Gov. of India, & now residing at 7 Ingles Road

Folkestone.

f3v

Wishing you all the good wishes
of the season

Yours truly,

J. Walker.

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UNIVERSITY COLLEGE LONDON
GALTON PAPERS

f41r

13 Cromwell Road. S.W
2nd January 1889

My dear Galton,

I am glad to find that my suggestions were not unwelcome.

On receiving your reply I wrote to Henry Blanford, the Indian Meteorologist, for information regarding monthly & hourly barometric variations.

I enclose his reply. You will see that it still further widens the horizon; & I must honestly confess that I do not know enough of the subject to conjecture in what manner the aperiodic variations are to be taken account of. Apart from that however the letter gives information of many sources from which knowledge is to be derived of the barometric variations at inland stations as well as on the

Coast lines; — and this information may be useful to you.

It seems as if the Report on the Meteorology of India for 1886 must contain a great deal of the information which is wanted. If you would like to have a presentation copy sent you from the India Office I am sure Strachey would readily have you supplied.

Yours truly

J. Walker.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

7. Ingles Road. Folkestone
Thursday 1869
f51

My dear Walker

You are probably right in your surmise that such discrepancies as you refer to in the determination of elevation by means of the boiling point apparatus, in the Himalaya and Afghanistan, are due to the observations having been made at different seasons, and would be to some extent eliminated if instead of assuming a constant S.L. value of pressure, the observed mean value for the month and hour of the day were taken as the datum for the barometric station of comparison. But whenever possible we should not be content with this average value. The aperiodic variations in pressure distribution should also be taken into account, and this may be done to some extent in the Indian mountain zone, when we have published daily values for several stations on the Himalaya

in Baluchistan &c, and also for the plains
at their foot. In the case of regions
for which no such data are available,
I think the best plan would be to take
the S.L. values from a good isobaric chart
such as those for January and July by
Dr J. Hann in the new edition of Berghaus'
Phys. Atlas. (Charts Nos 33 and 34). I
can easily give you mean monthly values
of barometric pressure for a number of stations
in India, unless you would prefer to deduce
them for yourself from the table that is
given annually in the Reports on the Meteorology
of India. You have them in the
R. Geog Society's library and also in the
Royal Soc. and most probably they could
spare you a copy at the India Office.
The latest is that for 1886. ~~In~~ This Report
Table XIV. p. 67 gives mean monthly values
for 128 stations, including Aden, Bushire, Mauritius, Amini Divi (Laccadives) ~~and~~ Quetta
and Leh, besides several Himalayan
stations, reduced to the values of the Calcutta
standard barometer, which reads .011 inch higher

The capital of Ladakh in the N.W. Himalayas, 11,800 feet above sea

from the New standard.

Unless the aperiodic variations of the barometer can be obtained from a synoptic chart, I doubt whether it is worth while expending trouble on such a further refinement as the correction for the diurnal variation of pressure. I can give you the diurnal variation of Calcutta, Allahabad, Goulpara, Bombay and Leh; and by writing to Calcutta, I could procure that of a number of other Indian stations. That of Bombay you will find in Chambers volume on the Meteorology of the Bombay Presidency, and that of the other four stations in Vol I of the Indian Meteorological Memoirs, which is in the libraries of the Royal and R. Geog. Societies. I can also easily hunt up for you that of some Alpine stations, and some American mountain stations, Arranck &c derived in most cases from a few days or weeks observations, but in most of these cases the correction would make a difference of a few feet only, while the instrumental aperi

of the variation may affect the result by some hundredth part.

For the 'Hints to Travellers' I would suggest giving a reduced copy of Stanni's charts for January and July, and leaving it to the computer to interpolate values for other months. A proof of this, I have, myself, in hand, the Meteorological section. I am however dealing chiefly with climate, and as an element of climate barometric pressure is of subordinate importance, so that I had not intended to say very much about the barometer, which to travellers is of use chiefly for hypsometrical determinations. If Dr Galton decides to give these charts, I would refer to them in my section of the work.

Thank you for kind wishes which I reciprocate, and wish you and Mrs Walker also a happy new year

Believe me

Sincerely yours

Henry F. Blanford

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
335/5

Wallace
Prayer

Frith Hill, Godalming.
July 27th. 1884

3/3/22/4

Dear Mr Galton

Wallace
Prayer

A friend has just informed me that he has been told by one of the Agents for the Sale of the Ordnance Maps that it is not intended to complete the Hill-Shading on the new issue of the one-inch maps. One of the sheets in our district No-255 has been issued with the hills shaded & it is a very beautiful map, and I cannot understand why all should not be done in the same way, as it so greatly adds to the value & interest and even to the intelligibility of the maps, which without the hills are often difficult to make out, especially as the great irregularity with which roads & paths are marked renders mistakes very easy. Footpaths are sometimes marked as roads, while no difference is made

between mere field-roads a tracks f1v
& regular metalled highways. I think
some protest should be made if it
is really true that the hill shading is
ord to be done to the great detriment
of the maps as an educational instrument.
Can you not get some M.P. of your
acquaintance to bring the matter
before Parliament?

I have been just reading your
"Inquiries into Human Faculty" with great
interest and profit. I now however
wish to remark on one point -
your discussion as to the "objective
efficacy of prayer". It seems to
me that the method of general statistical
enquiry you have adopted cannot
possibly lead to true results in this

UNIVERSITY CO
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case because it assumes two things
one of which is certainly not, the other
probably not, true. These are — that
the form of prayer has the same
efficacy as the essence of it, — and,
that all persons can have their
prayers answered in the same way.

It seems to me that there is an
equally good test to be found in
the cases of individuals, whose prayers
have been objectively answered, not
once only but scores of hundreds of
times; not generally, but specifically
and in detail. Two such cases
are those of the Curé d'Ars, and
George Müller of Bristol. I have
pointed out the great importance of
the latter case in my "Miracles and

LLEGE LO
TON

f2v

Modern Spiritualism" pp. 209–211, and if you still feel any interest in the question I strongly recommend you to get his — "Narrative of some of the Lord's dealings with George Muller". 6th. Ed. 1860, — or any later edition if there is one, and I think you will find there, evidence from which there is no escape that his prayers were again & again objectively answered. Of course it rests on his statement only in the book, but it would certainly be possible to get corroborarative testimony from some of the persons who aided him in the early struggles of his work, or of the numerous nurses & attendants at the Orphanages. I really hope you will go into this as a mere question of scientific fact.

Believe me
Yours faithfully
Alfred R. Wallace

NB. The book consists largely of a diary, made day-by-day, recording all the extraordinary events at the very time they occurred.

F. Galton Esq.

Frith Hill, Godalming.
March 7th. 1885

A3r

IX

Dear Mr. Galton

As you have worked so much with averages exhibited diagrammatically you may perhaps have by you some diagrams illustrating the progressive accuracy of the law of deviation, which you could lend me to have copied for a lantern slide for lecture illustration.

What I require is some such diagrams as the following.

See over

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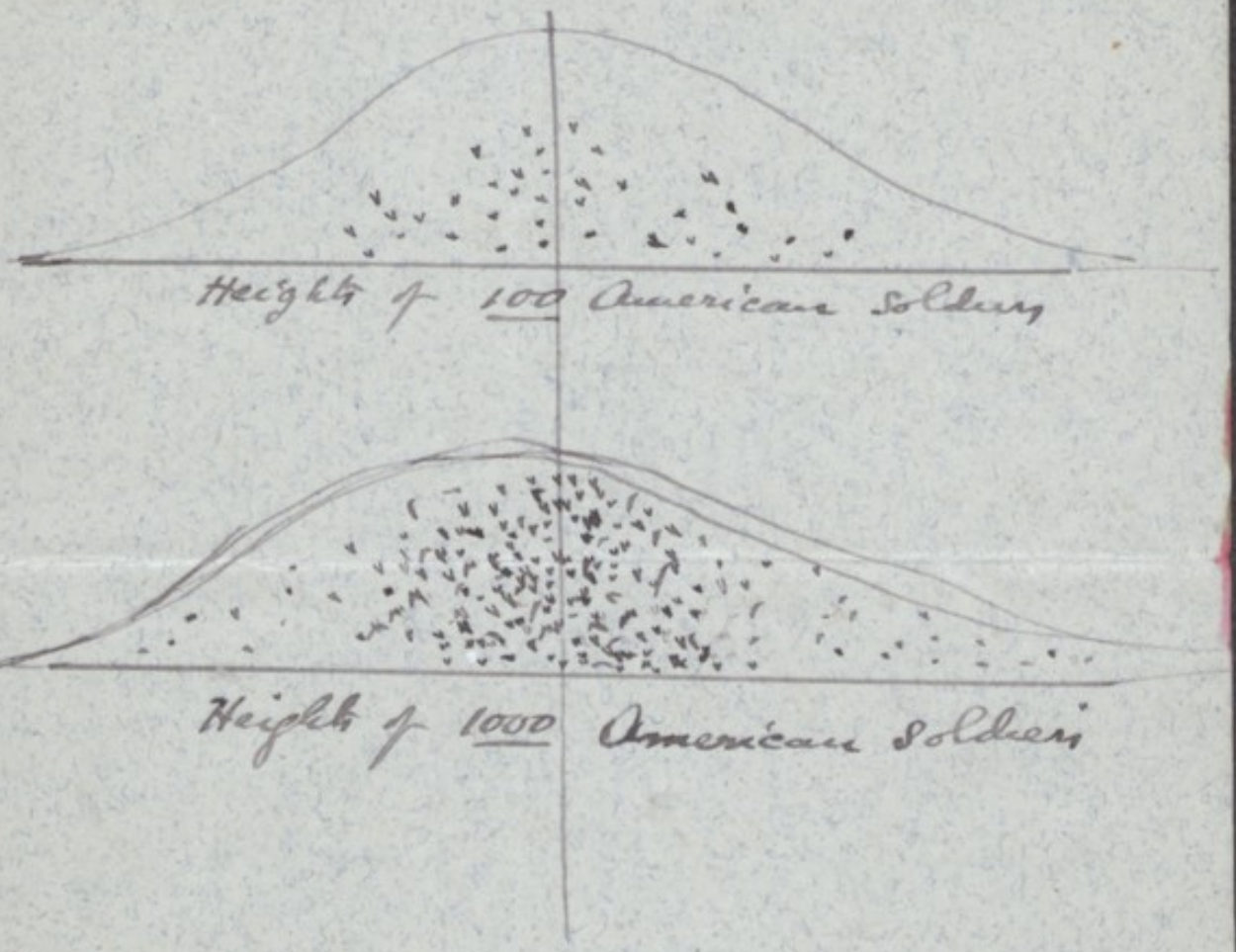
the showing the amounts of deviations
given by a small number of
cases — 50 or 100 for instance
Another giving a large number say
1000 or more, — showing that in
the latter case, there is a nearer
approach to the regular curve
and also that the extremes of
deviation are considerably greater.
I mention it to illustrate the variations
of species, & to show that if we
find a certain amount of irregular
variation whenever we examine
20 or 50 or 100 individuals, we
may confidently assume the
greater extent & greater regularity
of the variation if we could examine



thousands & millions of individuals,

No doubt if you have not such diagrams yourself you can refer me to some work in which I can find them. I was delighted with your address at the Brit. Ass. on Heredity of Stature.

Yours very faithfully

Alfred R. Wallace

F. Galton Esq.

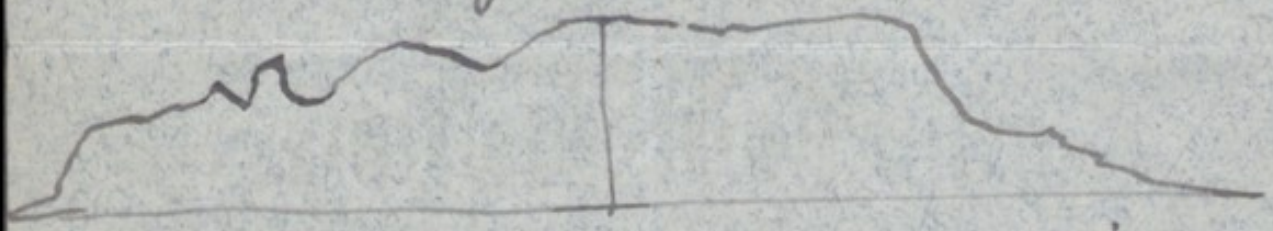
UNIVERSITY COLLEGE LONDON
GALTON PAPERS

Parkstone, Dorset.
Dec. 1st. 1893

My dear Galton

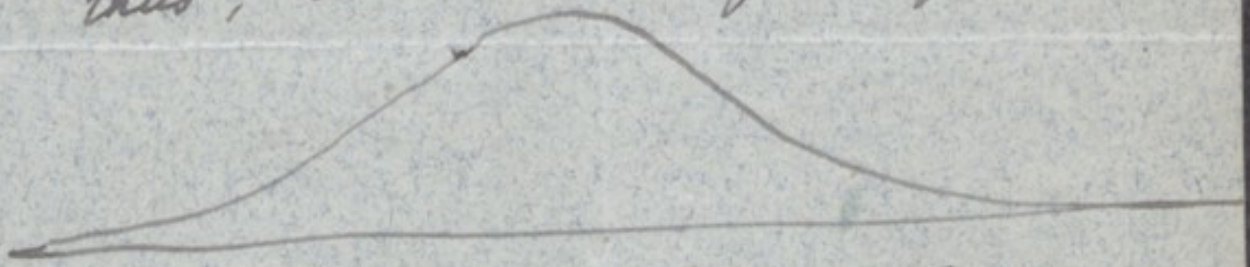
Thanks for the papers which I return. The Committee Programme seems excellent, but I do not think it would be any use my joining it as I could never attend, though I am greatly interested in the proposed work. I am, however, so poor a mathematician that I cannot follow the formulæ & tables in their mathematical form. The only things I can clearly comprehend are the diagrams & curves showing variability in various ways. It seems to me (though I may be

quite wrong) that the mathematical treatment of the subject does not bring out some of the most interesting points as regards evolution by nat. selection. For instance, what may be called irregular deviations from the mean are I think of great importance for nat. select. The variations of some organs for instance will be something like this; showing



that great numbers of individuals in some years or localities, vary considerably both in excess or defect of the mean value. Now if by taking more individuals or in

other years the mean value were (as usual in these cases) considerably in excess - the resulting curve might come out more thus, and the fact of the



great amount of material occurring for modification by natural selection would be obscured. It seems to me important that the measurements of every individual should be represented by a dot in its proper place, leaving the whole mass of dots, even when several thousand, to determine the curve of variation & the amount of divergence in each

direction. This would be understood by the most unmathematical intellect! Another point is the correlative variations of parts in each individual. This is quite a different thing from the law of correlation determined by you! This law may show a relation between the means of several hundred individuals, while in many of these individuals the variations might be in opposite directions. While, for instance, the cubit may increase in definite proportion to stature - as a mean - there may be, & probably are, large numbers of individuals in every generation in which one is stationary while the other either decreases or increases - thus furnishing materials for any combination of variations. I do not see that this is brought out in any of the mathematical results, but rather obscured. Yours very faithfully

Alfred R. Wallace -

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f 7v

POST CARD

THE ADDRESS ONLY TO BE WRITTEN ON THIS SIDE.

PARKS HALF PENNY
FREE
MR 6

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

Alfred Wallace

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

f 7r

My dear Galton

I am very sorry to hear you are still unwell. I cannot see that what you say makes any difference in ~~that~~ my view of your theory. "Mould" I took in the ordinary sense of meaning "shape" or "give shape to". That any "species" ob. be formed without the aid of natural selection, or that any "specific characters" in the true sense of the terms, should be non-adaptive, seems to me quite impossible & even (to me) unthinkable.

A.R.W.

Walters

flr

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
3/3/22/5

60, WELBECK STREET,
CAVENDISH SQUARE. W.

June 6. 95

Dear Sir,

Knowing that you take a great interest in all Anthropometrical subjects, I take the liberty of writing to ask whether you happen to know any publication containing standard measurements of human limbs from

various ranks of life.

I have a very curious case under my care at the North London Consumption Hospl. (41 Fitzroy Square) in which there is hypertrophy of bones and soft parts of the distal ends of both pairs of limbs. I believe there is elongation of forearms & legs as compared with upper arms and thighs, but find

it difficult to prove in the absence of a standard of measurement. Could you help me by indicating one? The case belongs to a disease which has been classicly called Pulmonary hypertrophic osteoarthropathy, of which only 30 a 40 cases have been hitherto published, and its rarity must be my

f2v

excuse for troubling you in
the matter. I hope to show
the man at the coming Annl
Meeting of Brit. Med. Assocn.

Apologizing for the liberty
I have taken

Believe me to remain

Yours truly

F R Walters

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PAPERS

Dear. Galton Esq FRS.

f15

Established 1865.

Memorandum from WALTON & Co.

Fine Art Publishers.

103, SHAFTESBURY AVENUE,
(OPPOSITE THE SHAFTESBURY THEATRE.)

London, W. July 10th 1880

Sir.

We beg respectfully to inform you that we propose to issue (by special permission of the family,) the portrait of The late Sir Rawson William Rawson, K.C.M.G, C.B, as described on enclosed card, but before doing so, we want to see our way to dispose of about fifty copies, so as to cover part of the expense of producing it.

The following have already entered their names as subscribers:

R.G. Rawson Esq,
Lt Col Herbert E. Rawson, R.E,
Sir Wm Robinson G.C.M.G,
Sir George Berkeley, K.C.M.G,
Lt Col L.S. Wills., C.B,
Hon Sir Francis Pakenham K.C.M.G,
Wm Saunderson,
Wm Cazalet,
The Royal Geographical Society.
Rev Fredk Howlett.
Vice Admi Sir Harry H Rawson K.C.B,
H.E. Bateman Esq. C.M.G,
The Royal Statistical Society.
Capt W. Stepney Rawson.
Wm Hall
Mrs H. T. Hall.

P.T.O.

UNIVERSITY COLLEGE LONDON
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f1v

Nathaniel L. Cohen Esq,
Major P.G. Craigie
Thos Lawrence Ward Esq,
R.O Armstrong Esq.

And we shall be obliged if you will please let us know if we may add your name, and if so for how many copies

Awaiting the favor of an early reply

We are Sir

Your most Obedient Servants

Walton & Co

P.S. Should you know of any friends likely to be interested in the portrait, we shall be obliged if you will kindly send us their names & addresses (in confidence) so that we may communicate with them

Francis Galton Esq,

f1

Botanical Laboratory,
Cambridge, Dec 9 1898

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
3/3/22/7

Marshall Ward

Dear Mr. Galton

With reference to your question of yesterday — Hans. sent experiments on changes of form of Bacterium Pasteurianum, due to alterations in the physical environment are the most striking I can find. There is a good summary in Lafar "Technische Mykologie" Chap 37 § 211 p 347. I have not the English Edition at hand.

Yours sincerely
H. Marshall Ward.

UNIVERSITY COLLEGE LONDON
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PAPERS
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J. Ward

f 1 6

TRINITY COLLECE,

CAMBRIDCE.

Feb. 16th 1880

Dear Mr Galton,

I was very glad to find you were not after all unduly bored by my letter. But if I do not mistake the purport of your question about Pythagoras I fear I must inadvertently have led you to suppose that the Pythagoreans "visualized" numbers. My note was written long ago at a time when I knew nothing about

philosophy & when I ~~did not~~
misapprehended the drift of
the Pythagorean speculations.
There is I believe no doubt
that the Pythagoreans - of their
founder nothing certainly is
known - held Numbers to be
the models (παραδείγματα) of
things. To understand this
we have to bear in mind
(1) that they in common with
many other early speculators
ignored & contemned sense-knowledge
(2) that the only exact knowledge

f2r

they had was of Mathematics, coupled with a little astronomy & some ideas about musical harmony, (3) that they had no notions of Logic or Proof but supposed that what commended itself to their reason was true – theirs was the intellectus sibi permissus. In such circumstances, if one take the trouble to think oneself into them it is not hard to see that it is number that has evolved the Cosmos & makes a cosmos of it. Perhaps in 10,000 years time some of the speculations with which we ~~are~~

there days are so satisfied, will appear & fall as far short of adequate correspondence with what may then be expected as the whole truth of things as the speculations of the Pythagoreans now appear & as with all the splendid achievements of modern science spread out before us. It is a secret worth knowing, & all events it cost me a good deal & made it, that the first step in attempting to understand the ancients is to blot completely out of mind modern knowledge. The point of the controversy between the Nominalist & Realists, & all the mystics about Bacon. Pardon haste

Yours very truly J. Ward

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

f3r

Jan. 30th 1884.

TRINITY COLLEGE,
CAMBRIDGE.

Dear Mr Galton,

I regard you as a public benefactor & only wish I could be of more use to you. For some time I have been intending to get together some psychophysical experiments but the difficulty has been to get the money. Just as that difficulty was to some extent surmounted I found myself committed to a tiresome piece of literary work which will take all my time for some months to come.

f 3v

One of the first things I meant to do was to write to you & ask to be allowed to see some of your apparatus: that I shall now be able to do when this exhibition comes off.

I expect you know a great deal more about the whole thing than I do. I may however mention two or three books & papers in which apparatus is described.

As regards Sight – the most complete collection of descriptions of apparatus that I know is that of Snellen and Landolt in Graefe's Handbuch des gesammten Heilkunde Bd. III. pt. 1. In Bd. II, ii

f45

on the physiology of sight by Aubert
there are also several pieces of apparatus
described. But these books are now
seven years old: for what has been
done since I should myself go in
the first instance to the Archiv für
Ophthalmologie & turn over the table
of contents. Cor, & this applies
to the senses generally – look in
the index of papers in Foster's
Journal of Physiology or in Hof-
mann & Schwalbe's Jahresberichten
These only piece of apparatus I
have of this sort is a new perimeter

f4v

an improvement in some little points of what is known as Mr. Hardy's Self registering Perimeter. It is being made by Pickard & Curry of St. Portland St. & was to be finished this week. I will send it to you with pleasure as soon as it is done, if you would care to see it & have not seen it. I shall have no particular use for it before next October.

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Stilling's tables for Stour-Blindness I presume you know: if not I can send his last ones.

Lord Rayleigh has devised an ingenious method of combining pigments so as to enable the operator to vary the size of

9. Board 2 f5r

the sectors while the "experimentee" is looking: he spins an image of his divided disc by ~~means~~ rotating an inverting prism instead of spinning the colour disk itself as is done in the ordinary coloured top. But I think I see how to get over the difficulty another way & am going to have a new colour-mixer made. But I fear it will not be ready in time.

For mixing prismatic colours & making accurate quantitative determinations of variations of colour-sense there are two or three forms of apparatus. Of these you will find accounts in

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337/4

a good paper of Donders. Arch. für Ophthalmen. 1882 "Ueber das Farben-
system: & in a paper by v. Kries in
the Arch. für Physiologie (Phys. Abtheilung)
for 1882 & 1883. I can't say much.
Such a piece of apparatus seems to
me essential but it is villainously
expensive. I enclose a drawing of Donders'
Colour-mixer, for which Schmidt and
Haensch of Berlin ask £20.

As regards Hearing I know nothing -
I should betake myself to the Archiv
für Ohrenheilkunde edited I think
by Tröltsch, to the reference given by
Helmholtz, Hermann's Handbuch der

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f61

Physiologie III, ii, p. 114 ff. Prof. Berger of Jena has worked a good deal at hearing: I have some of his papers somewhere & can send them to you if you like. He has I believe the two biggest tuning-forks in the world. If your exhibition is international he is the sort of man who would like to lend his apparatus.

As regards Touch – there have been several so called Baraesthesiometers: I send you a paper on this subject by Bastelberger on the chance of it's being new. The man who has worked most at Localisation since the classic days of Weber, is Viernodt, but if you

f6v

are not already fully upon his track you can be at once by referring to his text-book of physiology or to Funke's paper in Hermann's Handbuch.

As to Taste & Smell – there is Vintschgau & his disciples

A great deal has been done of late in measuring Reaction-time, but good & simple apparatus is still a desideratum. There is a passable compilation on this whole subject by Buccola just published – La legge del Tempo nei fenomeni del Pensiero. For simple demonstrations there is a good deal to

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TRINITY COLLEGE,
CAMBRIDGE.

be said for what Exner calls his "Neura-

? Ward f75 3

amoebimeter" (Hermann's Handbuch II, ii p. 276) I made one of these once in Prof Stuart's workshop for my own amusement: it is a little sticky in the joints just now but I will have it put right & send it you if you like.

UNIVERSITY COLLEGE LONDON GALTON PAPERS 337/4

Wundt has done a good deal of psycho-physical work of all sorts some of which is detailed in his Philosoph. Studien. So too Buccola, whose papers appear in the Riv. di Filosofia scientifica, which he edits.

Among odds & ends of things which it occurs to me to mention comes the

f7v

attempts at analysing & registering certain emotional expressions by Dr. F. Warner – see Brain vols II & III & a description of some apparatus in Jl. of Physiology a number or two back.

You mention Mosso's Platysmograph, but I fancy that is not a very reliable instrument: I remember being in Leipzig about the time he invented it & seeing some experiments tried with it but with^t any very satisfactory results. My impression is that Marey & F. Franck are people who have some better & later work as regards pulse & circulation –

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PA

f 8r

but it is only an impression.

As regards "higher mental processes" what one wants is rather methods than apparatus. In thinking about your "visualization" inquiries it has sometimes occurred to me that it might be better in any future investigations to ask questions as to definite & well-known objects in [illegible] of trusting to subjective descriptions by persons who have no means of forming a definite standard with which to compare themselves. E.g. to ask a man how many columns there are in the

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ALTON
PERS

Times & to get him to mark off the length or width of a column from memory; or to describe the dinner plates, tea cups or wall-paper which he is continually seeing "at a time when he does not see them" ——— when he has not seen them for a long time, &c. &c.

But I am afraid in saying all this to you I am making myself very offensive. Sending slack to Newcastle. However you must forgive me, if you will & believe that I am only anxious to be of use to you, if I can. Yours truly

James Ward

F. Galton Esq

TRINITY COLLEGE, CAMBRIDGE.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

Ward 1885
for

TRINITY COLLEGE,
CAMBRIDGE.

30 May '85

Dear Mr Galton,

We elected Rayer a week ago & his letter accepting his fellowship under the new conditions was read yesterday & the minute confirmed. So now all is settled for five years. Withers

f 9v

that time he will, I quite believe, make good the promise of work which led the Council & prolong his tenure. I read your letter & the Council & Dr. Jackson read others from Prof Taylor & Robertson Smith. The Master was a good deal amused by an allusion of

f10r

yours & the ingenuity of literary men: perhaps he thought of another Master not a hundred miles off.

I am glad you know Frazer & have been kind enough to encourage him. He is tremendously ardent & yet keeps his head. I

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GALTON PAPERS

f 10v

am only afraid to may lose time in getting on a good trail. When a man begins a new subject at 30 it is a serious thing to have to buy his experience as younger folks can afford & are content to do.

As for psychology — I should like someday to ask you some questions anent your inquiries into imagination, if you are still interested that way.

Yours sincerely J. Ward

J. Ward

f11r

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

SELWYN GARDENS,
CAMBRIDGE.

16. xii '88

Dear Mr. Galton,

I trust you will not be more angry with me for attempting to fulfil my promise to write about your "observations made in church" than you would be if I neglected to act up to my word altogether. No doubt the latter course would spare you an infliction but it would tend through me to damage your good opinion of human nature. So, though I now think it was foolish - in fact presumptuous, to proffer, I feel it is a duty to perform.

The observation, as I understood you, was this:- Without looking at the prayer book, you only heard

J. Ward

f11r

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

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that the clergymen was talking; when you followed the words with your eye, you heard distinctly what he said. The problem, as you put it was:- How can sight supplement hearing?

As regards pure sensations of sight & hearing one would be disposed to say such reinforcement of one sense by another is impossible: on the contrary one would suppose that attention, regarded as a quantity, being finite, the more we attend to the sight the more we must withdraw attention from the sound. And such - other things remaining the same - is certainly the case - & a thousand and one familiar experiences of "abstraction" or "absorption" at once occur as instances in point. But the attention is a finite quantity it is not a fixed quantity & it is conceivable that stimulation through one sense may so arouse & wake up the "apperceptive centre", as it is the fashion

f 12r

to call it, that after diverting what is due to such stimulation, there may still be more available than before for new stimuli.

— — Since I began writing this, it has just occurred to me that I have somewhere made a note of some experiments bearing on this matter & I must look them up before I send my letter off.

However when you described your observations to me, my impression was, & still is, that the explanation is to be found by way of ideation or imagination & not by way of sensation. All perceptions & illusions - psychologically both are in this respect on a par - resolve themselves into complexes of sensations & images, complexes in which it is almost always impossible directly to determine what is "given" (in sense) & what is "thought" (in ideation). To see a landscape in the flat, its distant objects, whatever their ground colour

f 12v

as blue or grey, its deep shadows as purplish &c &c. is almost impossible to the young draughtsman, because what he knows is so mingled with what he sees. Did you ever come across a sort of Xmas. card that amused people a good deal about the time of the Bulgarian atrocities? "Der Bulgarier verlässt Haus & Land, aber wo ist die Katze?" – this was the legend under a picture, in the centre of which was a large & straggling tree. After staring long enough most people at length espied the cat formed by certain lines of the foliage. Up to that minute these lines were quite unmeaning & indistinguishable as a whole from the rest; but once the cat was seen it was impossible to lose sight of her again. The filling in & the grouping of the lines having once been supplied by imagination could not be withdrawn ~~again~~ & one saw the cat as distinctly as one saw the lines before. Experiments have been made – & there is room for more – which throw some little light on this process of recognition.

(5) J. Ward f13r

A series of digits, letters or words, or some more or less complex geometrical figure, is exhibited for an instant by means of an electric spark. The impression so made can if necessary be prevented from persisting, can in fact be wiped off the retina by a repetition of the flash on an empty background. Anyhow what happens is something of this kind: one sees

the first time ⌒ _| ∪ \

second — ∪ \|‾ ⌒ |

third — G ‾ T C |

& so on

But what was seen the first time persists more or less & as representation or image is added wholly or in part to what is seen (i.e. to the sensations had) the second time. So we have really the second time, putting dotted lines for the residue of the first:—

~~CRT~~ GALTON

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The process is like your superposition of photos., in a word. Caldecott's pictures always seem to me the best instances of such suggestion: there is an excellent one in the Jolly Huntsmen, where a single line suggests a whole sheet of field.

Now I should say that there is always a good deal of filling out in what we take to be all matter of ~~direct~~ immediate intuition. We do not take in every stroke nor even every letter as we read - hence we overlook misprints. Nor do we hear every articulation distinctly but piece out so certainly what is familiar that we are not aware of our contribution. Of this we have evidence in the proverbial difficulty of catching proper names especially surnames.

To come now to your experiences in church. I should say that when listening simply, not-

f145

is heard is less than the minimum required as a basis to excite the ideational supplement. Just as in reading you would get GALTON from i + ii above, but not from either singly, so with your clergyman, what you heard was only tantamount to i, but when you read as well, the combined result was =lt to i + ii.

Then comes the question: How can sight thus piece out sound? It was in view of this that I ventured to refer you to Kussmaul. There is, of course, no very close or certain connexion between sounds & forms, but between spoken & written words there is the closest connexion, though no doubt in some persons the connexion is much closer than in others. Many when they read "fancy" they hear their own voices speaking the words, & some, of course, actually speak them. When one is familiar with a voice – as perhaps you are with your clergyman's – it is easy to hear that voice in imagination.

f14v

In such cases then what we have might be symbolised after this fashion:-

1st a very faint reinstatement of the words as spoken by that voice:-

2ndly the interpolation in their proper places - subject to occasional dislocations - of the snatches actually heard

3rdly - in keeping with a general law, turned to much account by Hume - that impressions intensify the ideas with wh. they combine - an intensification of part or all of the faint reinstatement. So that in the end we get, say:- GALTON

(N.B. In this needless rigmarole I have of course used letters to symbolise ~~[illegible]~~ articulations & images of articulations.)

Though there is an especially close connexion between the centre for written symbols & the centres for the production & for the hearing of the articulation symbolised, there is also a close connexion in all

J. Ward

f11r

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

SELWYN GARDENS,
CAMBRIDGE.

16. xii '88

Dear Mr. Galton,

I trust you will not be more angry with me for attempting to fulfil my promise to write about your "observations made in church" than you would be if I neglected to act up to my word altogether. No doubt the latter course would spare you an infliction but it would tend through me to damage your good opinion of human nature. So, though I now think it was foolish - in fact presumptuous, to proffer, I feel it is a duty to perform.

The observation, as I understood you, was this:- Without looking at the prayer book, you only heard

that the clergyman was talking; when you followed the words with your eye, you heard distinctly what he said. The problem, as you put it was:- How can sight supplement hearing?

As regards pure sensations of sight & hearing one would be disposed to say such reinforcement of one sense by another is impossible: on the contrary one would suppose that attention, regarded as a quantity, being finite, the more we attend to the sight the more we must withdraw attention from the sound. And such - other things remaining the same - is certainly the case - & a thousand and one familiar experiences of "distraction" or "absorption" at once occur as instances in point. But the attention is a finite quantity it is not a fixed quantity & it is conceivable that stimulation through one sense may so arouse & wake up the "apperceptive centre", as it is the fashion

f 12r

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— — Since I began writing this, it has just occurred to me that I have somewhere made a note of some experiments bearing on this matter & I must look them up before I send my letter off.

However when you described your observations to me, my impression was, & still is, that the explanation is to be found by way of ideation or imagination & not by way of sensation. All perceptions & illusions - psychologically both are in this respect on a par - resolve themselves into complexes of sensations & images, complexes in which it is almost always impossible directly to determine what is "given" (in sense) & what is "thought" (in ideation). To see a landscape in the flat, its distant objects, whatever their ground colour

f 12v

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f15r

cases between name & thing. I should expect if you were to undertake the kind of inquiry you talked of, that you would find words better heard when the things were seen, i.e. if the speaker for example said Portfolio as he helps one up - & so forth.

The connexion in every case would be reversible. Barely legible print would seem legible if one heard it read & objects that were indistinguishable might be recognised if one heard their names.

On thinking it over, it seems to me that experiments of the kind you proposed would be very difficult to check; but if a number of reliable data of the kind were secured they could hardly fail to throw a welcome light not only on this interconnexion of sense & sense in the individual but still more perhaps on the differences between individual & individual. Still don't you think this personal equation of cautiousness (or what you would call it) would be

be heard & isolate? All the same with your masterly ingenuity as an experimenter I have no doubt you will devise a way of working, if it seems worth while.

I have found the reference mentioned. It is a paper "Ueber den Einfluss einer Sinnesreizung auf die übrigen Sinnesempfindungen" by Urbantschitsch of Vienna. The original paper is in Pflüger's Archiv XXII (1880). There is a summary of it in the American Journal of Psychology May '88. & one still briefer in Nature Dec. 15/87. p. 157

Pardon so much, but it is always a pleasure to me & have a reasonable excuse for writing about these things.

Yours very truly,

James Ward

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GALTON PAPERS

J. Ward

f16r

TRINITY COLLEGE,
CAMBRIDGE.

19. III. 93

Dear Mr Galton,

Many thanks for your paper on "the Just-perceptible Difference". Your "patent" for telegraphing pictures & diagrams is most ingenious. In times of war it might be exceedingly useful.

About "the measure of the imagination" I believe we had some talk the last time I had the pleasure of seeing you. As I think I said to you then, the fact has long been familiar to me: for I am rather fond of hearing & very short-sighted. Even as a youngster I remember being puzzled to find how perfectly I could hear the words

f16v

of a song if I had them in front before me. Also at a French or German play I seemed to hear everyword if I had the text in my hands. To this day I can't be sure what a Frenchman says to me until his words call up mental pictures of their spelling. The proportions in which sight & hearing combine in this interpretation of sounds as words no doubt differs enormously in different people, & in the same individual in different circumstances. But I am very doubtful as to the possibility of ranging these facts under the rubric "measure of imagination". It is not merely that in this you seem to stretch the meaning of imagination beyond existing usage. All perception involves re-presentative "elements"; is more or less re-cognition; but the secondary presentations concerned in this "assimilation" of

f175 ?

primary presentations (or sensations) are not images or ideas. Psychologists are only just waking up to the difference (Cf. recent discussions on Recognition by Höffding, Lehmann, Wundt & others). But I fancy you go too far in saying this peculiar kind of co-operation of past residue & present impressions has been "only partially allowed for under the titles of Expectation & Attention" (p. 3). Lewes invented a useful word Pre-perception (Cf. Problems of L. & M. iii p. 106 f. It was adopted by James (Pples of Ψ? i, 439, where are several useful references).

Apart from this however – I expect you will find time as well as intensity a factor in the case. If the pace of the ~~the~~ memoir-reading diminished as the intensity diminished (i.e. distance from the speaker increased) slowness might prove more or less to compensate for faintness.

f 18r

I mean that you accept Fechner's formulation of the experimental data called

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CAMBRIDGE.

Weber's Law spite of the very severe criticisms that they have received. The statement that you give (p. 1 fin) is certainly not Weber's Law & is strictly denied by many who accept the experimental results. These results amount to this & no more:- Sensibility to difference, relatively to the magnitude of the stimulus, is the same over a wide (medial) range. A human being differs from a balance in not discriminating a constant absolute difference but only a constant relative difference. In this there is no limit of increments in a single sensation, or of a series consisting of units. To get from Weber's Law ($\delta\gamma = k\frac{\delta\beta}{\beta}$) which he called the Fundamental Formula)

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337/5

f4

24, HARLEY STREET,
CAVENDISH SQUARE. W.

11 Jan: 1880

Dear Sir,

Thank you for your letter. I have read the Article on Knee-jak in American Psychological Journal. It is important and I think true that the reaction varies with the nerve-state. We want an Institute of Physiologico-Psychology and Anthropology for the accurate determination of such phenomena.

I delayed a reply to your letter till I could send you syllabus of Cambridge lectures.

f2v

I think if this work is to be of practical use socially - as I desire it should be - that I ought to give lectures to those who want to learn - but how I do not know - I have delivered 7 courses of lectures for the Froebel Society to appreciative audiences -

I have collected the casts, specimens and diagrams for the Cantab course, and if it interests you to see them, shd be very pleased to see you here one morning by appointment to show them to you and receive your criticism. about 11 o'c suits me best -

f2r

Apparatus for the measurement of movements and their combinations is being constructed for me by the Cambridge Scientific Instrument Co

Yours very truly,

Francis Warner

Francis Galton Esq F.R.S.

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GALTON PAPERS

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

F. Warner

f3

24, HARLEY STREET,
CAVENDISH SQUARE. W.

Aug: 1869

Francis Galton Esq

Dear Sir.

Partly acting on a suggestion of yours, our Committee have examined 5.000 children in schools - a printed report I have the pleasure to send you. The Royal Commission on Blind, Deaf and Imbecile children have advised that the latter be separated for educational purposes, so perhaps our work may find practical issue -

f4v

My Lectures for teachers at Cambridge will be published by the University Press - I should be very glad of your suggestions as to forwarding this work and presenting it when it may be of use - Such observations as we made might will be carried on in any Anthropological laboratory in addition to those commonly use, & photographs of postures wd be valuable indications of nerve balance

Yours very truly,

Francis Warner

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

REPORT OF INVESTIGATIONS

IN

FOURTEEN SCHOOLS IN LONDON,

TO BE PRESENTED TO THE COMMITTEE
APPOINTED BY THE PSYCHOLOGICAL SECTION OF THE BRITISH MEDICAL ASSOCIATION.

PREPARED BY

FRANCIS WARNER, M.D.Lond., F.R.C.P., Hon. Secretary to the Committee.

TOGETHER WITH LEADING ARTICLES ON

INVESTIGATIONS AS TO THE
PHYSICAL DEVELOPMENT AND BRAIN STATE OF SCHOOL CHILDREN;

AND ON

THE REPORT OF THE ROYAL COMMISSION ON
THE EDUCATION OF FEEBLE-BRAINED THE EXCEPTIONAL CHILDREN.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

PRINTED AT THE OFFICE OF

THE BRITISH MEDICAL ASSOCIATION, 429, STRAND, W.C.

[*It must be understood that this article is a statement of facts observed in the London Schools only, and that the Report of the Committee appointed in 1888 will be presented to the meeting of the British Medical Association at Leeds (Psychological Section). The present statement is intended to serve the purpose of a Draft Report for the consideration, and if approved, the adoption of the above Committee.*]

(Reprint from BRITISH MEDICAL JOURNAL, July 27th, 1889.)

REPORT OF INVESTIGATIONS IN FOURTEEN SCHOOLS IN LONDON, TO BE PRESENTED TO THE COMMITTEE APPOINTED BY THE PSYCHOLOGICAL SECTION OF THE BRITISH MEDICAL ASSOCIATION.[1]

Prepared by FRANCIS WARNER, M.D., Lond., F.R.C.P., Honorary Secretary to the Committee.

Two applications having been made by the Committee to the School Board for London, and on each occasion the reply having been received that the Board regretted that they must decline our request to be allowed to visit their schools for the purposes of this investigation, the London members of the committee were obliged to confine their observations to other elementary schools.

The following summary of a draft report on fourteen schools in London has been prepared by Dr. Hack Tuke, Dr. G. E. Shuttleworth, Dr. Fletcher Beach, and Dr. Francis Warner.

A form of schedule was drawn up, upon which the facts noted concerning every child who appeared in any way exceptional were recorded; the report of the teacher concerning each child was included.

Ten public elementary schools; two certified industrial schools the district pauper schools, Hanwell; and the Asylum for Deaf and Dumb Children in Old Kent Road, were examined. In every case the members of committee visiting the schools were received with uniform courtesy, and they desire to acknowledge the intelligent assistance received from the managers and teachers. The committee also desire to express their thanks to Dr. Littlejohn, of Hanwell, and to Dr. Martyn, medical officer to the School for Dumb Children. Mr. Rowland Hamilton, a Vice-President of the Statistical Society, has rendered most able and generous assistance in tabulating these results.

Mental Dulness was observed or reported to us in 231 cases.—It is probable that many more children were mentally dull; of those noted, some attracted attention in other ways, and some were presented to us by the teachers. The method of examining children by facts observed without questions asked may indicate the condition of development and state of brain, but it is necessary to question the pupil in order to demonstrate mental aptitude or its absence.

The ten public elementary schools contained at the time of our visits 3,931 children; these are arranged according to standard and sex in Table A.

TABLE A.

	Boys.	Girls.	Totals.
Infants	759	764	1,523
Standard I ...	265	223	488
,, II ...	213	236	449
,, III ...	220	212	432
,, IV ...	180	213	393
,, V ...	152	141	293
,, VI ...	93	97	190
,, VII ...	46	70	116
,, ex VII ...	16	31	47
Totals	1,944	1,987	3,931

[1] Towards the expenses of this inquiry a grant was received from the Scientific Grants Committee of the British Medical Association.

The children seen were distributed as follows in the schools thus:

TABLE B.

—	Seen in the Schools.			Noted on the Schedules.		
	Boys.	Girls.	Totals	Boys.	Girls.	Totals
Fourteen London schools, namely: In 10 public elementary schools:—						
Borough Road School	239	—	239	60	—	60
Stockwell Girls' School	—	451	451	—	82	82
,, Mixed School	103	66	169	11	10	21
,, Kindergarten	42	42	84	5	1	6
St. Andrew's National School, Wells Street, W.	204	189	393	23	19	42
St. Mary's National School, York Street, W.	270	234	504	36	27	63
St. Mary's National School, Whitechapel	274	202	476	31	20	51
Hanwell National School, W.	326	278	604	44	20	64
Mill Hill National School, N.W. ...	76	59	135	11	4	15
Jews' Infant School, Whitechapel ...	410	466	876	33	31	64
Total	1,944	1,987	3,931	254	214	468
In four special schools:—						
District Pauper School, Hanwell ...	637	440	1,077	85	44	129
St. Vincent's Industrial School ...	182	—	182	132	—	132
St. Margaret's Industrial School ...	—	103	103	—	29	29
* School for Deaf-and-Dumb	31	20	51	31	20	51
Total	2,794	2 550	5,344	502	307	809

* See Table L.

In special schools:—Boys, 850; girls, 563; Total, 1413.
Of which on schedules: Boys, 248; Girls, 93; Total, 341.

The following table (C) is given to indicate the principal facts observed. Several special conditions often coexisted in the same case. Further analysis and descriptions of combined conditions are given in succeeding tables.

TABLE C.

—	—	Boys.	Girls.	Totals
	We took notes of 809 cases in the schools ...	502	307	809
See Table E	Cases showing signs of nervousness, nerve-weakness, or defect	207	144	351
,, F	Cases in which nutrition appeared to be defective	100	84	184
,, G	Cases in which mental dulness was reported or observed by us	153	78	231
,, H	Cases presenting cranial abnormalities ...	166	65	231
,, I	Cases with disease or defect of eyes ...	74	75	149

TABLE D.—*Showing the Distribution of the Various Pathological Conditions in the Different Schools, also Relations to Sex.*

School.	Nervousness or Nerve Weakness.			Low Nutrition.			Mental Dulness.			Cranial Abnormalities.			Eye Cases.		
	Boys.	Girls.	Total.	Boys.	Girls.	Total.	Boys.	Girls.	Total.	Boys.	Girls.	Total.	Boys.	Girls.	Total.
Borough Road College Practising School	30	0	30	11	0	11	17	0	17	10	0	10	5	0	5
Stockwell College Practising School *	0	60	60	0	17	17	0	10	10	0	4	4	0	14	14
,, ,, Mixed School	4	4	8	4	4	8	4	2	6	2	1	3	3	3	6
,, ,, Kindergarten	0	0	0	0	0	0	3	1	4	1	1	2	0	0	0
St. Andrew's, Wells Street, W.	7	9	16	10	5	15	11	4	15	8	3	11	4	5	9
St. Mary's, York Street, W....	14	11	25	8	8	16	9	6	15	14	4	18	7	12	19
St. Mary's, Whitechapel	12	7	19	7	6	13	8	1	9	10	7	17	3	6	9
National School, Mill Hill, N.W.	3	2	5	3	3	6	1	2	3	3	1	4	3	1	4
National School, Hanwell, W.	8	8	16	15	4	19	15	6	21	17	8	25	14	3	17
Jews' Infant School, Whitechapel	11	10	21	14	13	27	11	10	21	17	8	25	8	8	16
	89	111	200	72	60	132	79	42	121	82	37	119	47	52	99
District Pauper Schools, Hanwell, W.	37	13	50	6	7	13	38	15	53	32	14	46	15	12	27
School for Deaf and Dumb, Old Kent Road.....	15	6	21	5	4	9	4	2	6	14	4	18	2	3	5
St. Vincent's Certified Industrial School, Dartford	66	0	66	17	0	17	32	0	32	38	0	38	10	0	10
St. Margaret's Certified Industrial School, Mill Hill, N.W.	0	14	14	0	13	13	0	19	19	0	10	10	0	8	8
	118	33	151	28	24	52	74	36	110	84	28	112	27	23	50
As above	89	111	200	72	60	132	79	42	121	82	37	119	47	52	99
Total	207	144	351	100	84	184	153	78	231	166	65	231	74	75	149
	See Table E.			See Table F.			See Table G.			See Table H.			See Table I.		

* This school was visited immediately after the annual inspection by Her Majesty's Inspector. The Standards III to VII, containing 302 girls, were seen by us five months later, when 29 showed nerve-signs.

Cases in which we observed Signs of Nervousness, Nerve-Weakness, or Nerve-Defect are Analysed in Table E.—It is probable that in some of these cases the signs of nervousness or nerve-weakness were only temporary in character.

TABLE E.

Distribution of Nerve Conditions.	Boys.	Girls.	Totals	Low Nutrition.		
				Boys.	Girls.	Totals
Infants	14	18	32	6	8	14
Standard I	20	7	27	10	4	14
,, II	11	9	20	6	2	8
,, III	6	14	20	2	5	7
,, IV	6	17	23	2	4	6
,, V	13	9	22	2	2	4
,, VI	11	14	25	4	4	8
,, VII	6	18	24	0	1	1
,, ex. VII	2	5	7	—	—	—
Ten public elementary schools	89	111	200	32	30	62
Pauper school...	37	13	50			
Two industrial schools	66	14	80			
School for deaf and dumb...	15	6	21			
	118	33	151			
Ten public elementary schools as above	89	111	200			
Fourteen London schools	207	144	351			

——	Boys.	Girls.	Totals.
As coincident conditions, we found in the 200 cases in 10 public elementary schools:			
Defective nutrition	32	30	62
Mental dulness	23	20	43
Cranial abnormalities...	28	16	44
In the 50 cases observed in the pauper school:			
Defective nutrition	2	5	7
Mental dulness...	15	5	20
Cranial abnormalities	12	3	15
In the 80 cases observed in the two industrial schools:			
Defective nutrition	10	4	14
Mental dulness	18	4	22
Cranial abnormalities...	13	4	17
In the 21 cases observed in the school for deaf and dumb children:			
Defective nutrition	2	3	5
Mental dulness	4	1	5
Cranial abnormalities	6	2	8

Signs of Defective Nutrition were seen in 132 cases.—We do not suppose that we noted defective nutrition in all the cases in which it may have been present. The distribution of these cases, according to standards and sex, is shown in Table F.

TABLE F.

—	Boys.	Girls.	Totals.
Infants	21	19	40
Standard I	16	7	23
,, II	12	11	23
,, III	5	4	9
,, IV	6	9	15
,, V	8	3	11
,, VI	2	5	7
,, VII	2	2	4
Ex. VII	—	—	—
Ten public elementary schools	72	60	132
Pauper school	6	7	13
Two industrial schools	17	13	30
School for deaf and dumb	5	4	9
	28	24	52
For 10 public elementary schools (as above) ...	72	60	132
For 14 London schools	100	84	184

As coincident conditions, we found in the 132 cases in 10 public elementary schools:			
Signs of nervousness, nerve-weakness or defect	32	30	62
Mental dulness	30	17	47
Cranial abnormalities...	28	15	43
In the 13 cases observed in the pauper school:			
Signs of nervousness, nerve-weakness or defect	2	5	7
Mental dulness	2	2	4
Cranial abnormalities	3	2	5
In the 30 cases observed in the two industrial schools:			
Signs of nervousness, nerve-weakness or defect	10	4	14
Mental dulness	7	9	16
Cranial abnormalities...	8	5	13
In the 9 cases observed in the school for deaf and dumb children:			
Signs of nervousness, nerve-weakness or defect	2	3	5
Mental dulness	1	1	2
Cranial abnormalities	4	2	6

TABLE G.—*Cases of Mental Dulness.*

—	Boys.	Girls.	Totals.
Infants	26	17	43
Standard I	12	5	17
,, II	6	8	14
,, III	11	2	13
,, IV	6	4	10
,, V	14	1	15
,, VI	1	3	4
,, VII	3	2	5
Ex. VII	—	—	—
In the 10 public elementary schools	79	42	121
Pauper school	38	15	53
Two industrial schools	32	19	51
School for deaf and dumb	4	2	6
	74	36	110
For the 10 public elementary schools (as above)	79	42	121
For the 14 London schools	153	78	231
As coincident conditions, we found in the 121 cases in 10 public elementary schools :			
Signs of nervousness, nerve-weakness, or defect	23	17	40
Defective nutrition	29	17	46
Cranial abnormalities...	21	6	27
In the 53 cases observed in the pauper school :			
Signs of nervousness, nerve-weakness, or defect	15	5	20
Defective nutrition	2	2	4
Cranial abnormalities...	17	5	22
In the 51 cases observed in the 2 industrial schools :			
Signs of nervousness, nerve-weakness, or defect	18	4	22
Defective nutrition	7	9	16
Cranial abnormalities...	10	7	17
In the 6 cases observed in the school for deaf and dumb children :			
Signs of nervousness, nerve-weakness, or defect	4	1	5
Defective nutrition	1	1	2
Cranial abnormalities...	4	1	5

TABLE H.—*Cranial Abnormalities.*

—	Ten Public Elementary Schools.			Pauper School.			Two Industrial Schools.			Deaf and Dumb School.			Totals.
	B.	G.	T.	B.	G.	T.	B.	G.	T.	B.	G.	T.	
Probably rickets	24	5	29	5	3	8	—	—	—	—	—	—	37
Head large, cause not known ...	17	7	24	8	1	9	—	1	1	1	—	1	35
Head small, ,, ,, ,, ...	5	3	8	1	—	1	1	3	4	2	1	3	16
Dolichocephalic or scaphocephalic	7	2	9	3	—	3	1	3	4	1	—	1	17
Forehead narrow or shallow, not included in other groups ...	14	9	23	10	6	16	12	2	14	3	1	4	57
Bosses on frontal bone or at fontanelle ; not included in other groups	12	9	21	3	3	6	18	1	19	5	2	7	53
Other defects of skull not included in other groups	3	2	5	2	1	3	6	—	6	2	—	2	16
	82	37	119	32	14	46	38	10	48	14	4	18	231

—	Boys.	Girls.	Totals
As coincident conditions we found the 82 boys, 37 girls—total, 119 cases, in 10 public elementary schools:			
Signs of nervousness, nerve-weakness, or defect	28	16	44
Defective nutrition	28	15	43
Mental dulness	21	6	27
In the 46 cases observed in the pauper school :			
Signs of nervousness, nerve-weakness, or defect	12	3	15
Defective nutrition	3	2	5
Mental dulness	17	5	22
In the 2 industrial schools :			
Signs of nervousness, nerve-weakness, or defect	13	4	17
Defective nutrition	8	5	13
Mental dulness	10	7	17
In the 17 cases observed in the school for deaf and dumb children :			
Signs of nervousness, nerve-weakness, or defect	6	2	8
Defective nutrition	4	2	6
Mental dulness	4	1	5

TABLE K.

Total of Cases presenting each Nerve-Sign respectively.	Nervous Hand.			Weak Hand.			Lordosis.			Frontals Over-acting.			Orbicular Muscle of Eyelids Toneless.			Finger Twitches.			Number of Nerve-Signs not in Combination.			
Nervous Hand.	B.	G.	T.	B.	G.	T.	B.	G.	T.	B.	G.	T.	B.	G.	T.	B.	G.	T.	Nervous Hand.	B.	G.	T.
In 10 public elementary schools...	44	69	113	×			—			10	28	38	9	1	10	5	4	9	15	16	31	In 10 public elementary schools...	21	28	49
,, 4 special schools...	25	5	30				—			*6*	*2*	*8*	*7*	*0*	*7*		—		*6*	*2*	*8*	,, *4 special schools*	*11*	*3*	*14*
	69	74	143																			
Weak Hand.																						Weak Hand.			
,, 10 public elementary schools...	10	22	32	—			×			1	6	7	3	0	3	1	3	4	3	5	8	,, 10 public elementary schools ...	5	9	14
,, 4 special schools...	16	6	22	—						*3*	*3*	*6*	*6*	*1*	*7*	*2*	*1*	*3*	*3*	*3*	*6*	,, *4 special schools*	*5*	*2*	
	26	28	54																			
Lordosis.																						Lordosis.			
,, 10 public elementary schools...	15	41	56	10	28	38	1	6	7	×			3	1	4	0	2	2	1	10	11	,, 10 public elementary schools ...	4	6	10
,, 4 special schools...	18	5	23	*6*	*2*	*8*	*3*	*3*	*6*				*3*	*0*	*3*	*1*	*0*	*1*	*6*	*1*	*7*	,, *4 special schools*	*6*	*2*	*8*
	33	46	79																			
Frontals Overacting.																						Frontals Overacting.			
,, 10 public elementary schools...	41	2	43	9	1	10	3	0	3	3	1	4	×			2	0	2	3	0	3	,, 10 public elementary schools ...	25	0	25
,, 4 special schools...	68	13	81	*7*	*5*	*12*	*6*	*1*	*7*	*3*	*0*	*3*				—			*8*	*2*	*10*	,, *4 special schools*	*48*	*10*	*58*
	109	15	124																			
Orbicular Muscle of Eyelids Toneless.																						Orbicular Muscle of Eyelids Toneless.			
,, 10 public elementary schools...	14	11	25	5	4	9	1	3	4	0	2	2	2	0	2	×			3	3	6	,, 10 public elementary schools ...	9	3	12
,, 4 special schools...	4	1	5	—			*2*	*1*	*3*	*1*	*0*	*1*	—						—			,, *4 special schools*	*1*	*0*	*1*
	18	12	30																			
Finger Twitches.																						Finger Twitches.			
,, 10 public elementary schools...	20	27	47	15	16	31	3	5	8	1	10	11	3	0	3	3	3	6	×			,, 10 public elementary schools ...	3	3	6
,, 4 special schools...	25	10	35	*6*	*2*	*8*	*3*	*3*	*6*	*6*	*1*	*7*	*8*	*2*	*10*		—					,, *4 special schools*	*8*	*5*	*13*
	45	37	82																			
Total 10 public elementary schools ...	39	49	88	8	14	22	15	47	62	20	2	22	11	12	23	25	34	59					67	49	116
,, *4 special schools*	*19*	*4*	*23*	*14*	*8*	*22*	*19*	*6*	*25*	*24*	*3*	*27*	*3*	*1*	*4*	*23*	*8*	*31*					*79*	*23*	*101*
Final total	58	53	111	22	22	44	34	53	87	44	5	49	14	13	27	48	42	90					*146*	*71*	*217*

Note.—The additions at the foot of the columns include the cases of combinations with one or more of the other defects noted. The numbers in the right-hand margin indicate defects *not* in combination and might be placed in the diagonal square marked ×.

Diseases and Defective Conditions of Eyes.—When a child was seen wearing spectacles, with a squint or other obvious defect, the fact was noted, but many cases must have had errors of refraction not known to us.

TABLE I.

—	Ten Public Elementary Schools.			Pauper School.			Two Industrial Schools.			Deaf and Dumb School.			Totals.
	B.	G.	T.	B.	G.	T.	B.	G.	T.	B.	G.	T.	
Squint	32	26	58	9	3	12	4	4	8	2	1	3	81
Hypermetropia	3	15	18	1	3	4	—	—	—	—	—	—	22
Myopia	3	5	8	2	—	2	—	—	—	—	—	—	10
Disease of Cornea	3	2	5	2	3	5	4	4	8	1	1	2	20
Disease of lids	1	2	3	—	—	—	—	—	—	—	—	—	3
Cataract	—	1	1	—	—	—	—	—	—	—	—	—	1
Eye lost	—	1	1	—	1	1	2	—	2	—	—	—	4
Nystagmus	2	3	5	1	2	3	—	—	—	—	—	—	8
	44	55	99	15	12	27	10	8	18	3	2	5	149

Analysis of 51 Deaf and Dumb Children.—In this school each child was examined separately, and the condition of the palate was noted. Particulars as to head measurements and conditions of general physical heath were most kindly filled in by Dr. Martyn, medical officer to the institution. The secretary of the institution very kindly filled in some particulars of the history of each pupil from the records kept. The ages of these children varied from 7½ to 11 years; average age 8 years. The following table indicates the cause assigned for the deafness:—

TABLE I.

Deaf and Dumb Children.	Boys.	Girls.	Totals.
Congenitally deaf	21	11	32
Sequent to measles	2	1	3
,, ,, whooping cough	1	1	2
,, ,, scarlet fever...	—	1	1
,, ,, sunstroke	1	—	1
Fits or brain disease	3	2	5
Effects of a fall	2	—	2
First dentition	—	3	3
Cause not stated	1	1	2
	31	20	51

In 3 cases the children were the offspring of first cousins. In 5 cases deafness was acquired between the ages of 3 and 6 years; in two instances without cause assigned. The palate was examined in all these cases: in 2 cases it was "vaulted," in 2 "narrow," in 3 "arched." Of these 7 cases 5 occurred among the congenitally deaf pupils, and 3 coincided with other defects of the cranium. In 7 boys and 7 girls the teeth were found ground. The various conditions found in these children are given in preceding tables.

Leading Article reprinted by permission from the BRITISH MEDICAL JOURNAL, *27th July, 1889.*

INVESTIGATIONS AS TO THE PHYSICAL DEVELOPMENT AND BRAIN STATE OF SCHOOL CHILDREN.

AT the meeting of the British Medical Association in Glasgow, the Section of Psychology appointed a Committee to conduct an investigation as to the average development and condition of brain function among the children in primary schools. This Committee has done good work, and has pioneered the way for the medical inspection of schools. Elsewhere in the JOURNAL a draft report is given, drawn up by Dr. Francis Warner, and this presents some of the results of observations made in fourteen London schools; it presents the results of the examination of 5,344 children seen, and contains an able analysis of 809 cases in which notes were taken of the facts observed. The main conditions tabulated are—(1) Cases presenting signs of nervousness, nerve-weakness, or defect; (2) Cases presenting low nutrition; (3) Abnormalities of the skull; (4) Mental dulness; and (5) Obvious eye defects. The distribution of such conditions in the various schools, as also their relation to age and sex, is also indicated; the frequency of the coexistence of these conditions in the same case is also shown in tabular form. We thus see that it is possible to ascertain the distribution of the various types of children, both in a school and among the population. This is a new mode of investigation; and information thus gained is likely to prove a valuable addition to our vital statistics, and to aid the management of each school, thus examined, by indicating in some degree the direction in which the special endeavours of the managers and the teaching staff should be directed. In some schools the number of children with defective development appears to be high, while in others the signs of nerve conditions predominate.

The facts are presented in this draft report very clearly in tabular form; some indications as to causation of the principal defective states are apparent in the statements as to coincident conditions. Thus, in the case of children with obvious indications of low nutrition, want of food appears to be by no means the only cause; a large number of these cases had also badly made heads, suggesting defective inheritance and unfavourable antecedents. Teachers will be interested in the tables on mental dulness, a condition that appears to be very unequally distributed. Here, again, both defects in development and low nutrition appear as marked factors in causation. Again, it is interesting to know that it is among the boys that the largest number of badly made heads and cases of mental dulness are found, while, as might have been presupposed, the largest number of nervous children appears among the girls.

Many other important deductions might be made from the tables before us, which have not yet been completely summarised; but it is not our present purpose to give a full review of this draft report of the London schools, as we understand that other reports will be presented by members of the Committee to the meeting at Leeds in August, when a general discussion will be held on this subject in the Section of Psychology.

We have often urged that the medical inspection of schools is a duty devolving upon the Government, and that a Scientific Commission of Inquiry should undertake an investigation of the condition of school children, as well as the conditions under which they work. An extended knowledge of such facts as those before us might prepare the way for the introduction of special classes or schools for exceptional children, such as exist in many towns in Germany and in Norway, where children who are found unable to work under the ordinary national system are educated in day schools, the classes being small and the teachers specially trained, while the number of subjects of study is limited. Among the 285 children seen in certified industrial schools, the conditions found were very different from those in the public elementary schools; the

antecedents and inheritance of these children vary widely, but in these schools they are under constant control. In truant schools, it seems probable that many children of defective development and low brain-power might be found; here they are removed from parental control, and are liable to severe corporal punishment; this is not the way to remove defective conditions.

Much anxiety has been expressed in many quarters, both in this and in other countries, as to the effects of educational pressure upon the brain stability of children; the report shows that about 1 child in 20 for the public elementary schools presented some indication of nervousness, nerve weakness or defect, and as we follow the distribution of such conditions, the proportion rises to 1 in 10 for the highest class school, or 1 in 7.5 immediately after the Government examination, falling, in the pauper industrial school, to 50 in 1,077; in the latter school the low figure must not be solely attributed to the class of children found there, it is probably in part due to the fact that the school arrangements are in some degree under the supervision of the resident medical officer.

Many results of great importance to society may follow the pursuance of such investigations as those before us. In summarising the census records, certain important conditions in the population are attributed to concurrent causes; it has not been possible thus far to apply any corrections for the circumstances of the school period of life. It would be very satisfactory to be able to compare these observations in elementary schools with others made in the public schools for boys, and the high schools for girls, and also to know the conditions that might be found in other countries, and under different methods of school training.

It is, for many reasons, an urgent matter that we should know the average condition of our school population, the relative proportion of cases of defective development, nerve cases, and children with low nutrition, as well as those with eye defects, both in London and elsewhere, and that we should discover and remove their cause. This kind of medical inspection of schools need not be feared on the ground of expense; it might be instituted gradually in certain districts; and there is reason to believe that it might be carried on at much less cost than the present Government inspection.

Leading Article reprinted by permission from the BRITISH MEDICAL JOURNAL, *Aug. 3rd, 1889.*

REPORT OF THE ROYAL COMMISSION ON THE EDUCATION OF FEEBLE-BRAINED AND EXCEPTIONAL CHILDREN.

LAST week we published a report of the Committee appointed by the Psychological Section of the Association to investigate the brain condition of children in primary schools, and by an unforeseen coincidence the *Times* of the same date published an extract of the Report of the Royal Commission on Blind, Dumb, and Exceptional Children, in which, after citing the evidence given by Dr. Francis Warner, the Commissioners say: "Besides imbeciles and idiots, there is an intermediate class of backward and feeble-minded children who require different treatment to that of ordinary children, and who are at present simply excused from school attendance."

They then make the practical recommendation "That, with regard to feeble-minded children, they should be separated from ordinary scholars in public elementary schools in order that they may receive special instruction, and that the attention of school authorities be particularly directed towards this object." We have for some years directed attention to the social importance of taking special care, in the public elementary schools, of those children whose defects in development and low brain power render them more liable to mental deficiency and social failure, and at the same time the absurdity of supposing that all children, under a system of compulsory national education, can be satisfactorily trained in a similar manner.

To give practical effect to the recommendation of the Royal Commission, it is necessary that the "exceptional and feeble children" in each school should be known to the management, and this can only be done by some kind of medical inspection of schools. It has now been established, by the report published, that children can be classified by scientific observation in schools without undue interference with school routine. To complete the physiologist's work in the matter, more extended observations are needed to establish some definite understanding as to the relative value of the signs observed; a larger number of children in public elementary schools need be noted to determine the average condition of our school population, and the number of exceptional children for whom provision is needed. Besides the condition of children who must be considered as "exceptional" from the educational point of view, we are concerned, as a public matter, to know the causation of the frequent occurrence of "nervousness" and defects of development. To make a further study of "exceptional children," it would be very desirable to have records of the blind, of the truants, and cases summoned for non-attendance of school. It is on these grounds particularly that the action of the London School Board is to be regretted in preventing any inquiries being made in their schools by the Committee.

It is sometimes assumed that defects among a body of children of the poor are due to poverty; it remains to be seen what results would be obtained by an investigation conducted in public schools for boys, and in the high schools for girls. The recommendation of the Royal Commission is that the "exceptional children" should receive special instruction. It will be the duty of the Education Department to find out what modes of special instruction are best suited to the different groups of feeble children. This will probably be best effected by scheduling these children for a certain area, and appointing a competent authority to watch the effects of the methods pursued, and report thereon. When once it is determined that certain social questions are capable of analysis by observation of facts, there is good reason to believe that an increasing body of observers will throw much light upon the matter, and that satisfactory solutions may be found for problems that have long remained visionary owing to a want of analysis by scientific methods.

FRANCIS WARNER,
24, HARLEY STREET, W.

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D. F. Warner

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International Congress of Hygiene and Demography.

LONDON, August 10th to 17th 1891.

PRESIDENT-H.R.H. THE PRINCE OF WALES, K.G.

Section of Demography

5 Prince of Wales Terrace - Kensington - W.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

18 July 1891

Dear Mr Galton,

I delayed answering your letter till the results of investigations of 50,000 children were ready to show you - as soon as ready they had to be printed - proofs are now to hand. I should be very glad to show you my Tables and ask your advice in arranging evidence on certain problems suggested. I am at home every morning till 12. or should be pleased to call on you

f10 ✓

any afternoon or evening -

Monday 5 pm Tuesday 5 pm.

Wed: 2.30 - 3.30 or 5.30 pm

would suit me

I am anxious to extract all possible information from the facts accumulated.

I followed the suggestion you made to me some years ago in tabling "Teacher's report of school work and status" in every case noted i.e. Boys 5,579 Girls 3,607 Total 9,186.

Yours very truly,

Francis Warner

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

8-7. Warner

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UNIVERSITY COLLEGE LONDON
GALTON PAPERS

5 Prince of Wales Terrace,
Kensington Palace, W.

8 Dec: 1891

My dear Sir,

You may remember looking over some Tables giving an analysis of 50,000 children seen in Schools - material wh. was in part presented to the Congress of Hygiene and Demography - You kindly suggested collecting the cases of Binary or combined defects in development and their correlation to Nutrition, Nerve-defects and Mental dulness - this has been

done and the whole material arranged in tabular form.

If you could spare the time it would be a great satisfaction to me to show you this work as it stands now, previous to publication and receive the benefit of your criticisms. I have to give a lecture on the subject at College of Physicians next year and should like to ask your advice as to best methods of preparing scale diagrams for illustrating distribution of conditions and combinations of conditions as found. If you will kindly look at this work again I would gladly pay to have any appointment you may suggest

Yours very truly
Francis Warner

UNIVERSITY COLLEGE LONDON
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5 Prince of Wales Terrace,
Kensington Palace, W.

11 Dec/91

Dear Mr Galton,

I do not return from the London Hospital till late that the hour you mention but Monday 4·30 will suit me very well.

If not convenient to you Tuesday morning at 11·30 would suit me.

Yours very faithfully
Francis Warner

Compton Arms, Stonegrop

Lyndhurst

Magdalen College.

Oxford.

April 12/1907

J Herbert Warren
Vice Chancellor

My dear Sir

My friend Professor Poulton tells me that you hesitate to accept the invitation of this university to come and give the Herbert Spencer Lecture next term, in view of the state of your

f/v

health. Will you let
me plead with you to
consider the matter
further and keep the
question open at any
rate a little longer
I do so the more eagerly
because, speaking in
confidence. it was my
own suggestion and idea,
one which Professor
Poullin however most

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3

strongly endorsed, that you
should be asked to give
the Lecture. If when the
time came you felt it
would be too great an
effort to deliver it, we
would arrange to have
some one to read it for
you or to read part
of it: the - has been
done before now. I do
not wish to importune
or overpersuade you to

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if you feel quite decided
I must accept yours
decision .. but pray think
this over
you might even send it
to be read when
the time came you felt
inclined to coming

May I take the opportunity
of telling you how
much pleasure & profit
I have derived from

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

f3

Magdalen College,
Oxford.

your own writings in
the past. especially
from 'Hereditary Genius'

Believe me to be
Yours very faithfully
T Herbert Warren
Vice Chancellor

f4r

Compton Arms Hotel
Stoneyhoss. Lyndhurst.

Magdalen College,
Oxford.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

April 14 - 1907

Dear Mr Galton

Poulton and I mean what we say. We will take the chance of your not being able to come at all. and of course you must not force yourself or run any undue risk. But what we want is to have

f4v

your pronouncements, from
your brain and pen
if from your lips then
all the better.

I have not myself a
memorandum of this
date. I believe it
was ~~the~~ a Wednesday
Thursday or Friday in
the last week of May

or first two weeks of
June. I must however
in any case write to
Poulton again, and will
ask him to confirm
this statement

Yours sincerely
T Herbert Warren

We shall hope of course
that you will stay
the night if offered
and we will do everything

f5v

to spare gun fatigue.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

f6

From the Vice Chancellor of the University

Dictated.

Magdalen College,
Oxford.

April 29th. 1907.

Francis Galton Esq., D.C.L., F.R.S.,
42 Rutland Gate,
London.
S.W.

Dear Mr. Galton.

Wednesday, June 5th.,
at 2.30. p.m., will do very well indeed for your lecture and I will have it announced for that day. I am very glad to think that at any rate ~~that~~ you will undertake to give it and I hope that you may be able to come. The place will be I think the New Schools which is more convenient than the Sheldonian Theatre if not so showy.

Yours sincerely
Herbert Warren.

f5r

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

Magdalen College,
Oxford.

April 29: 97

Dear Mr Galton

many thanks
for your kind letter.
Please do not trouble about
calling. I shall very
much hope to have
the pleasure of hearing
your Lecture and seeing

f5v

you Then.

It is very good

of you to undertake

The task

Yours sincerely

T Herbert Warren

Watkin

f 1r

2 Nelson Villas
Stoke
Devonport.
21. 7. 78.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
3/3/22/11

My dear Mr Galton

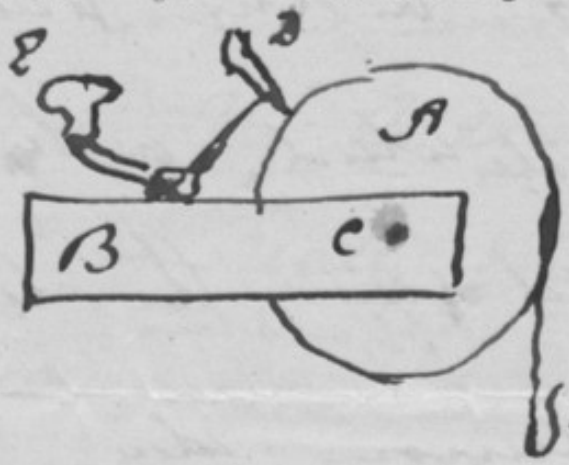
I really must apologise for not answering your letter before, but in changing my home letters so have gone astray. Please send the chronograph to Shoeburyness when done with and the coil here, I was

f 1v

so sorry to hear you had been
laid up with gout in the
knee — most aggravating just
in your instrument was in
working order — I should
think your apparatus nicely
made would give very accurate
results, increasing the weights
would I suppose also increase
the accuracy if at any time
you wanted extreme accuracy.

f25

What do you think of the following as a very portable arrangement. To be attached to a table or mantle piece -



UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

A a coil of telegraph paper pivoted at C, B forked piece of wood - D pencil actuated by handle E - (a second close to one another)

A weight at the end of tube

f2v

would cause the coil to revolve at accelerating speed; the pencil in glass pen would leave lines on the paper the length of which on a flat scale would give the time —

What a curious idea your composite photos is I saw an account of it in the papers some time ago —

Believe me

Yrs very sincerely

G. Hatkin

Watson

f/r

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
2/3/22/12

The Rectory,
Berkeswell,
Coventry.

Dec 29. 1879

My dear Galton

I am afraid you will want propitiating the many explanations & apologies for my treatment of the memoir by poor Mr. Alister so kindly forwarded to me. The first I have been incessantly engaged in examination work since its arrival. For ten days I was at Dartmouth on

f1v

H. M. S. Britannia & since then I have been dealing with papers sent from there - If you will see the file I returned a day or two ago no further explanation will be necessary

Friday was the first night I could give to the paper & now for the results.

First of all let me tell you exactly how the question stood - Some time ago when I sent you a resumé of Glaisher's paper I did this to a certain extent

but very likely you have supplied it—
— Gauss — assuming that the
chance of ~~an error~~ a particular
value x of any observation
is of the form $\phi(x-a)$ where
a is the true value — and
also assuming that the most
probable value as determined
from a given number of observations
is the A. M. of those observations
deduces the law of error to be
$\varepsilon^{-h^2x^2}$ — the frequency and the

facility laws being in this case identical because the independent variable is the same – From this result all the reasoning about "measures of precision" – "probable error" & the like results by straightforward analysis.

– You suggest that the chance of an ~~error of~~ observation x should be of the form $\varphi\left(\frac{x}{a}\right)$ not $\varphi(x-a)$ – at any rate for a certain class of observations – and that the most probable value (a) of a given number of observations

UNIVERSITY COLLEGE LONDON GALTON PAPERS

H. W. Walton f3c

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

Berkswell Rectory
July 30/88

My dear Galton,

I should be much honoured by the reference you prefer to my paper. Would it were worthier –

Here at Berkswell we are just now interested in the social extinction of a family – Poor old Mr Walton

f4

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

f5r

The Rectory.
Berkeswell,
Coventry.

Feb 16. 1869

My dear Galton

I have today received a copy of your most valuable and interesting book & I only regret that you should have taxed yourself to send me one when there must be so many and more deserving objects of your bounty - You have also given me an immortality in it which I

certainly never could have achieved for myself - I am reading it with much pleasure and congratulate you heartily on the successful completion of your labours - For myself I am hoping to produce the second and by far the best Volume of mine work in a month or two - Sometimes I wish that I had not sunk so much thought & labour in this subject of electricity - Not that it is not full of interest but that

The more I read the more convinced I become that with all our labours we have not come within measurable distance of a physical theory, although we have greatly developed our mathematical insight into transcendental functions Laplace Bessel & the rest of them

We are now making a totally new departure with the Hertz experiments published in Wiedemanns but we will the advent of our

f 6v

Electrical Newton -

I trust all is well with you & Mrs Galton & with my friend regards & renewed thanks I am

Ever yours truly

H. W. Watson

Perhaps now you might have leisure to let us see something of you - Is it possible you could ever see your way to another paper at our Philosophical - The Society is vigorous & deserves help Two of the members presided over Sections at Bath -

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

f7r

The Rectory.
Berkeswell,
Coventry.

7 March 1890

My dear Galton.

(1) You say it is clear that the curve of utility can only use (1) same commodity for the same purpose.

You mean this I presume as a confirmation of my view that the identity of commodity apart from its identity in application & surroundings is a merely fictitious identity — E.g. Water as such apart from the consideration

f7v

of its being stored for a special purpose.

(2) Again ~~you~~ starting from my word overlap you say you should think the αbles were most commonly not independent but dependent & entangled –

If I have a function, definitely dependent upon x, y, z &c so that when x, y, z &c are known the function is known it does not matter whether x, y, z &c are dependent or not: it was not this that I had in mind by "overlap" what

I mean was that Jevons's idea seems to be that there is a separate utility function for each conceivable commodity like $f_1(x)$ $f_2(y)$ $f_3(z)$ & that the total utility is of the form

$$f_1(x) + f_2(y) + f_3(z) + \&c$$

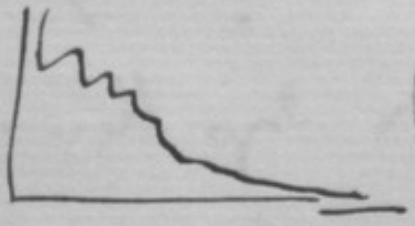
whereas I say the utility function is of the form $f(x, y, z)$ - i.e. you cannot take the utility of x as independent of y & z, &c - of the others. In fact Jevons himself gives an example to the contrary a few pages on where he says there may be two commodities (x, σ_3) interchangeable

f 8v

in use, & in certain given ratios of values (m : n) - this of course is a case where two of the α^{bles} x & y enter in the form mx + ny, or the utility fn is of the form

$$f(\overline{mx+ny}, z \text{ \&c})$$

What you say of the possible abruptness of the utility curve is true but I do not think it affects our conclusions we only want the asymptotic characters of the x, & y axes, thus



UNIVERSITY COLLEGE LONDON
GALTON PAPERS

f9

If you would really like some more of the work & not be bored – I will send it – you want make much of it unless you have the original ferrous at hand –

Ever yours

H. W. Watson

It is interesting to trace his principles – as I think we may – into capital & labour questions

rent

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

Walton

f10r

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

Berkswell 31 Dec/91

My dear Galton

A happy new year to you and Mrs Galton – Of course I should like to come over & lunch with Miss Galton when convenient to her & I trust you may find it possible to come here

The question, as you now put it ceases to be one for the Mathrs. at all – It is the determination of

that I have called the guinea const.
a the measure of irregularity in the
mixing – If this were perfectly uniform
the guinea const. would be zero and every
sample would reproduce the same proportion –
$\frac{b}{a+b}$ (a vulgar thing) but it is only by
observation of a very great number of draws
that you can find the required constant –
In your suggested case $a+b = 100$
& there might be 60 per cent in each
sample –
You take a very great number of samples
& count the number of samples (in suppose)

f11

in which the percentage differs from b. by x

& another no (n) in which it differs from

(b) by y & you say your quincunx constant

$(a^2) = (x^2 - y^2) \log \frac{m}{n}$ a result whose

accuracy increases with number of samples

Ever Ds

H. W. Watson

Jonas Watson

f1r

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
3/3/22/13

The Lodge,
Llandaff.

7th Jan 97

Dear Sir,

It was probably my mistake not Miss Duckworth's. I am only sorry that I have troubled you to read my rigmarole and to write me so fully about it. If the thing I call my memory were worth anything, I should have recalled your purpose from what I have read of your labours.

I was about to ask you

to know what I wrote
to you, but my sister
says "No. If you send
an addressed envelope
it will be one more trouble
to post it than to
burn it" So I obey.

I am as firm a believer
in human heredity as in
Equine. I think I can trace
the paternal & maternal strains
in the virtues & faults of the
eleven of us that reached

f 25

maturity. To specify the latter wd. be invidious. Happily we come from honest God-fearing parents on both sides. My Father's family were deeply reli-gious: his brother Edward (god-son of the Duke of Kent) gave the name of the "Holy Boys" to the 9th Regt. of Fort, in wh. he served in the Peninsula as Sixteenth, by his preaching to the men - a nick-name wh. it still retains. He threw up his commission from conscien-tious motives & became a strong Calvinist. My

f2v

father too joined the
Methodist movement,
as did his ~~Brother-in-law~~ sisters husband
Captn. John Hawtrey, who
afterwards took Holy orders
and became an Enthusiastic
country Parson about the
time his Cousin was Provost
of Eton. But keeping to
my own family; the letters
of my grandfather Jonas,
from Bermuda and Canada
shew him to have been deeply
pious also: of his father Captn.
Longood we know very little,
but the bombardier Jonas
left a record of Puritan sen-
timent in the names that
he gave to his sons taken

Do justly, love mercy, walk humbly
Jan 7/97
f31

from Micah 6-8 a
The eldest was Justly
the second - Love good
for "love-mercy" —
The family does not seem
to have run to the third
precept, tho' we used to be
told there was a Miriam
wh meant Humility -
both untrue.
This religious strain
comes out in this genera-
-tion in the shape of
philanthropy - all my
sisters being great in
district-visiting - & parish
work - some high - some low

f3v

One is a Poor-law Guardian
and for 20 years a constant
visitor at the Workhouse.
The Malady attacked
me as a boy in the
shape of Ragged Schools
still carried on as a
Cert^d. Indust^l. School on
board H.M.S. Havannah
at Cardiff. Also as
Sect^y of various Diocesan
Societies - Treas^r. Bishop's
Fund - Member of the
Canterbury House of
Laymen - &c &c -

For my father being he
a Churchman & John Wesley
persuasion - I naturally
take up defence of the
Establishment or down
with dissent -

I fear all this reads
rather boastful, but I
do cherish the old
"do justly" "traditions" as
a family possession and
responsibility - so I relate
it to be dealt with as you
like. Of physical faculties,
I have little to say - 3 sons
and one daughter were tall
like my Mothers family
tall & lean Except me. My

father was of medium height
and stout; as were nearly all
of his family. I am lean and
5.8½ - some of my sisters are
plumpish - We have all
suffered as my father did
from irritable mucous
membrane - My mothers
father & mother lived to 90
years of age - she died at
73 - her brothers at 70 - and
60 - one young -
Perhaps this group that I
took on the 2nd inst may illus-
trate these remarks - all but
my son, in the middle, & my nephew
standing on left, of one generation,
I am holding up my finger to the dog
Port-time ~~Llandaff~~ saves you
from more - Yours faithfully
Omar Watson

~~The Lodge,~~

f4v

17/3/91

5 The Avenue
Bedford

J. P. Manuel Weale Esq

Dear Sir,

In Sanderson's "Thirteen years among the Wild Beasts of India" chapt. XXIV is a description of a gigantic figure at Shravala Balagola or Sravana Belgola (Fergusson's Indian & Eastern Architecture pp 267-8) in Mysore in which it is stated "On both sides near the feet, the rock is hewn to resemble white-ant hills, with sacred serpents emerging from their galleries". It is not mentioned by Fergusson & two other statues are referred to. A fourth in a cave at Badami is referred to as still older. Are any models or large photographs of these figures to be seen anywhere in England, & are you aware whether white-ant hills form part of any other similar figures or designs in Ceylon or India proper. The form of the figure at Yannur figured in Fergusson, so far as so small an illustration permits one to judge has about the shoulders & chest a conformation remarkably African.

At page 222 the terminal superimposed ornament at summit of Sikra is called an "Amalaka". Can you inform me from what language that term is derived?

I have seen in a translation from an inscription (somewhere R. As: Soc: Journ) the fruit of the neli or nelly tree mentioned in connexion with Buddha.

In chap XII p 140 of Anderson's "Lake Nyami" mention is made of camping in ~~[illegible]~~ a termitarium. Have you ever heard of any tribes - African or otherwise - using them as habitations?

In your "Travels in S. Africa" ch. IV. you say speaking of Damara huts "the shape on the outside was like a snail-shell"

I have an extract from some work on the Chibka Indians of C. America, in which it is said "the interior was divided in the form of a snail". I think from Herrera.

What does this mean?

At p 132 you write of Nangoro's house, as "a perfect labyrinth". Was the interior cone-shaped, as described by some authors as peculiar to those of certain tribes, & did it ever strike you that the idea of such a complicated arrangement might have been suggested by the interior arrange-ments of a "termitarium"?

I hope I am not unduly trespassing on your valuable time by these questions

faithfully yours

T. P. Mansel Weale

F. Galton Esqre. F. R. S. &c &c

P. S. Perhaps you might know some one who could answer them.

Were you ever struck by a similarity between the cadences, as heard at a distance, - a sort of wave-like rising & falling between native chants & the choruses of vley-frogs. I have sometimes been puzzled in Kaffraria to distinguish the one from the other. The peculiar racket-like clack-clack of certain old padde giving the time like the clapping of hands. Vleys or meadow pools seem to be characteristic of highlands of C. Africa

f3r

99 Regents Park, N. W.
26th April. 1891

Weale

Francis Galton Esq.
42 Rutland Gate

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

Dear Sir,

I enclose you some pamphlets I have lately been distributing, in one of which you will see that I have quoted words of yours relative to a visit on the Orange

As a boy I wrote a rather silly poem on those people having read your original work about the same time as I read the voyage of the Beagle

In 1891 I brought out a book, which at the instance of my publishers I misnamed "The truth about the Portuguese in Africa"; in reality the book ought to have been three times the size it was; but I did not feel justified in expending at that time as much money as it would have cost: also I little suspected that any British Government would have sanctioned all the rascality since developed.

More or less however I have been fighting the Company ever since. From the beginning of this year I have ceaselessly been urging the cancellation of the Charter, for I have information as to witnesses I am particularly anxious that the Government should call, & whom, apparently neither the Company nor the Govt are desirous should be heard

f3v

The Aristocracy & the clergy, as in the slave-trade days either keep silent or openly approve of the atrocious mis-conduct of the Company and of its boundless ambitions.

What I have done has been entirely at my own cost without assistance from anyone & I am prepared to continue working so long as I can hope to see any successful result from my action.

At the same time I am a poor man & do not think I should be justified in continuing to expend my capital on a campaign to no effect.

In writing thus frankly to you I feel that at your age I cannot ask much beyond advice. I mean that while I should feel proud were you willing to take the chair at such a meeting I hardly like to ask you to put yourself to that trouble for one who is to you a comparative stranger.

I am Dear Sir

Your obedt. servant

L B Mansell Wade.

P.S. My mode of fighting & strategy may not conform to English political form & custom & consequently be open to critical criticism; but, I have an idea that anyone adopting the fencing irregularities of Joinette or Lisette (I have not a Matière at hand) in the "Bourgeois Gentilhomme" is, if he writes an African Knobberry, more like (to come off conqueror against the finesse of a Colonial Office Politician having the same thread-like pattern on a certain Bizzing ham spiral twisted foil or a Company, whose notions are based on the maxim, that most people can be bought.

By an odd chance I have never met Mr Rhodes in the Colony, although several of the Ministers there I knew.

But I have reason to believe that he was present at a lecture given by me in the Broad Walk Regents Park the day before he last left this country; & also some others of the Directors.

If the person I took for Mr Rhodes were he, & I afterward visited Tussaud's without a calatogue to verify my something more than suspicions. I told that person to his face, that it was the duty of the Government to arrest Mr Rhodes from this returning, as his arrival in Africa would be the cause of fresh troubles for both this & that country.

I have had a considerable amount of correspondence with the Govt, Chartered Co & the Dr of Life. I hired Portman Rooms for £8 0/, but perhaps did not advertise it sufficiently, my reason being that I did not want to have a fingo disturbance, which, owing to what was going on at the time, I feared would perhaps provoke me.

Hence I failed in getting an audience.

Knowing what have been your sentiments in the past, & what I feel assured would have been Charles Darwin's were he alive, I would ask you whether you think that were I again to hire a public hall for the purpose I should secure an audience of scientific persons. To me it is intolerable to think that my countrymen & women should be indifferent to their good name & credit.

Knowing a good many foreigners I can hardly tell you how exasperating it has been to me to hear things said about England on which I felt that truth compelled me to keep silence in very shame.

LETTER OF AUGUST WEISMANN, 1889, PRACTICALLY ADMITTING GALTON'S PRIOITY IN THE DOCTRINE OF THE "CONTINUITY OF THE GERM-PLASM."

f1

To
Francis Galton Esq.
London

Freiburg i. Br.
23 Febr. 1889.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
3/3/22/15

Sir,

You had the kindness to send to me your new book „Natural Inheritance" and a whole series of smaller papers you published before on the same subject. I thank you very much for your kindness and I am indeed very glad, to have now all your memoirs at once at hand for consulting them. Till now I did not know all of them, but some ones, for instance „a theory of heredity" from 1875. It was Mr. Herdman of Liver.

Spoal who – some years ago –
directed my attention to this
paper of yours, after having
read my own papers on here-
dity, on continuity of germ plasm
and others. I regret not
to have known it before,
as you have exposed in yours
paper an idea ** which is
in one essential point nearly
allied to the main idea
contained in my theory of
the continuity of germ plasm.
You will find in the english
translation of my essays
just now appearing, a note
of abt. 2 pages, which
draws the attention of the
reader to your ideas. I
shall profit of the next occa-
sion which offers itself to me,
to give a more extensive
account of your views and
to point out the differences
between our views. Hereby

is a very complicate and difficult matter and I am afraid that none of us, who have thought about it, will have solved the whole mystery of it. Nevertheless it may be, that we have touched upon the right way which leads to the solution of the riddle.

Please excuse my bad and perhaps not always intelligible English! But as you tell me, that you do not read German with fluency I thought it more convenient for you, if I wrote in bad English than in good German.

As soon as my book shall appear You will receive a copy of it, which I beg you to accept as a sign of my high esteem.

Believe me

faithfully your

August Weismann

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
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UNIVERSITY COLLEGE LONDON
GALTON PAPERS
T/3/22/26

Upper Blackwood
Western Australia
22 June 1909

Francis Galton Esq. F.R.S. &c.

Sir,

I believe you are interested in "wideness of heredity in animals" — Having been in charge or possession of live stock for 25 years, I have noticed instances of what seem to me to be hereditarily acquired habits, & also of reflection, or purposeful action. Re the first — In this part of W.A. the bush fires destroy the surface feed every three years or so, but where "Blackboys" "Xanthorrhoea" are plentiful, they provide food for local bred stock. No fire will burn the whole top of the B. boy, but only the greater part, always leaving the upright "grass" growing from the centre or top of the crown. This singeing causes the sugar contained in the core of the stem to rise, making the lower part of the upright grass in centre of crown very sweet, & a little easier to pull out, & also makes it ~~grow~~ start into fresh growth. The long flower stalk also shoots up the following spring but does not do so except after a bush fire. Horses sheep & cattle locally bred from flocks & herds long established in "blackboy" country, know exactly how to siege hold of the centres & pull them out, so as

to get the sweet soft base, but those horses & cattle bred in other states or in districts when there are no Bl. bags will eat the tops quite close, but never learn to pull them properly. Sheep do not learn so much. Sheep in those flocks long established here know how to rear up on their hind legs, and put their heads into the grass untill they get hold of the centre & pull it out, & can fold their fore legs & balance on their hind ones in order to reach one quite five feet high. They will thus keep themselves in good condition in a burnt paddock until new herbage grows. Imported sheep, even if put into the same paddock with them, will not learn from them and will starve if not hand fed, although their locally-bred companions are doing it hourly before their eyes. A neighbour imported bought some fine South Australian ewes, & put them into his paddock with a few local ones. The former would not learn from their mates how to pull or even to eat the Bl.-Bags, but starved, & had to be fed on chaff, while the latter ate the B-bags & throve. There are always plenty of quite low B-bags among the tall ones, but strange sheep do not learn to eat them either. Sheep have been bred here for about 50 years. Many common goats were kept here in the early days, & the sheep ~~they may~~ may have learned from them in the first instance, ~~etc~~. Sheep & cattle will rear up to pull down and eat

the flower stalks of which grow ~~out~~ of the top of the B. bay & all stock will eat them, as they are very sweet, but only local stock will rear up to get at the high ones. I have a number of local bred cattle, and have seen a cow which was 4 years old rear up on its hind feet, with fore ~~feet~~ legs folded down like a dog "sitting up" and seize & break off ~~[illegible]~~ ~~[illegible]~~ the flower stalk of a tall blackbay, I rode up, & found that the crown of the Bl. bay was the same height as my shoulder when sitting on horse and that the stalk had been broken through about 6 in higher, and the cow's saliva was on the stump. I have seen a 4 yr old steer do the same. I have a jersey cow bred in Victoria which bites the tops of Bl. bays quite close down, but does not know how to "pull" them as local bred cattle do. I am informed by a bushman & stockman bred in this district that horses from other parts never learn to pull the Bl. bays properly as the local ones do, although they learn to eat them close - as I have noticed is the case with my own, both local & imported. These observations seem to me to prove that an acquired habit or knowledge may become hereditary.

Re. Purposeful action - In the 1891 drought in the Roebourne (W.A.) district, most of the water-holes in the rivers & creeks dried up, but often water remained for a time a few inches below the surface

of the sand. Many horses as well as multitudes of other stock died - I was informed that a certain horse on a station I was on, saved his own life & the lives of his "mob" by pawing the sand until he had made a hole down to water level & so ~~kept~~ all kept alive until they were found & shifted to a place where water was still on the surface. I had that horse & others out on a journey & camped for night after a hard days ride at a place where the water level was one foot below the surface of the sand in the bed of the creek, and I saw that horse paw the sand & smell it, plainly trying for water, but we gave him & the others some in the damper dish, so I did not actually see him dig a hole & drink - but I was afterwards told by the manager of the station during the drought, that he had done so, as above.

a gelding

A few years ago I bought an old gelding & put him in a paddock with the others, & they were very hostile to him. At one place a big log had fallen at an angle with the fence, & he & a mare were feeding near it - My brother & I, standing on a hill above them. (it was cleared ground) noticed that the mare kept biting at the old horse, & we stood ~~stood~~ to watch. She kept feeding & biting at him at intervals & edging him towards the triangle formed by the log. As soon as she had got him

f5

fairly into it, she whipped round, & lashed several ~~[illegible]~~ kicks into him most viciously & he had to take a standing leap over the log to get out of the way. We have no doubt that the man deliberately planned the trick, as we could see her driving him closer & closer to the place. I trust these observations may interest you. If you are bringing out a new edition of your "Art of Travel" I could send you a few things which I have learnt & which you might think useful enough to put into it. Am sending this through the Secretary of "The Royal Society" as I do not know your address

I am Sir
Yours faithfully
John M. Whistler

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

Brancaster
Upper Blackwood
Western Australia
14 Sept 1909.

Wheeler
Sept 1909

Dear Sir

In reply to yours of 3rd Aug. asking if I can obtain information as to lambs born of ewes (which know how to eat Blackboy tops) in paddocks where there are no Blackboys –

I fear this will be rather difficult, as there are very few "cleared" paddocks in this district, if any. Paddocks having plenty of blackboys are regarded as a useful resource against each season's bush-fires

I will, of course, keep a look out

If the facts stated in my last, do not prove "heredity" of the habit, it seems to me to show that imitation is only possible to young animals

Out of 80 cattle which I had in a paddock here, 79 would only eat blackboys which had been singed, but one cow ate them all the year round & so kept in better condition than the others. The one which reared up to get at tall ones, also

kept in better condition than the others.
These would be a starting points for "natural selection"?
Cattle bred in poison country will feed among poison plants, & very few die; strangers eat & die very soon. There was a strong patch of "Heart-leaf" poison at the watering place at Boyaneys where I used to live - A small herd, local bred, belonging to a neighbour, used to feed there, and none were lost. The owner's daughter bought two cows from another district to run with the others, and these both got poisoned in a few weeks. - This may be of interest to you

I remain Sir
Yours faithfully
John W. N. Whistler

Sir F. Galton F.R.S.

side the secret of our purest aspi-
rations, might not ~~the~~ gain be great?

Is it extravagant to ask whether the value of Genesis & of the Synoptic of not the Four Gospels may not arise from their being so to speak examples of this very process & of the exalting results (instinctively called 'revelation') which it may give? – May 'inspiration' mean as it were that superposition of many thoughts wh. gives us some-thing better than any of them?

If these ideas are far fetched (and they are anyhow difficult to express clearly) may we not at all events ~~anyhow~~ hope that before long we shall have composite photographs of groups of some of the greatest of mankind in ability & character?

f5r

f5v

May we not hope also from groups of representative Smiths & Girls of the highest races, taken at the moment of greatest individual development, — really the high water mark of the race?

Gen would know when this should be — but I should like to plead for the ennobling, the radiant moment of pure 'first love', and again as its complement, the hour of death's transfiguration of even the most homely countenance

I remember seeing in some notice of European Science by a foreign savant the remark that Gen, as would be fully recognised some day, were the one really original genius (I suppose outside pure

purpurin character in the 'Canals' of Mars seems to have gained grnd since recent observations. The article in this week's 'Nature' is indeed suggestive — I wonder what the 'dark spots' at the junction of the 'Canals' can be?

There is also a short summary of Dubois Reymond on Neo-Vitalism by my niece, of the ability of which I feel proud. I fancy there is one misprint or oversight in Dr. Shepherd's 'atom' for 'particle'.

I hope this letter has not teazed you — pray do not answer unless quite conveniently — I am sorry to say the boy you saw has not yet seen Ferrier — He has been over to see me & his intelligence struck me more than ever — both him & Mrs. Galton

Yours most tly

V Welby

ERSITY COLLEGE LO
GALTON

f6v

Mathematics!) Whom England now possesses – Forgive me for repeating this in connection with their thoughts of possible outcomes of P. Work.

Only this morning I came upon Shaftesbury's idea of religion: "a life of increased and enhanced personality, a knowing oneself to be one with the great connected all of reality" – May not such a consciousness, based on fact, replace the more fantastic (albeit predictive) ~~[illegible]~~ conceptions of the mystic & preserve their element of truth? The links which connect our little world with others – or our whole system with other systems – may not be only those of 'gravitation', or the 'waves' wh. Dus are 'light'.

I see that the idea of some

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f7r

June 23d. 1893

DENTON MANOR
GRANTHAM

Dear Mr. Galton

It was certainly worth while to write to Paper, only to get your Criticism! Of course my anxiety to keep it both short and light for an easily bored Brit. Assn. Audience or even for private circulation has given it a certain baldness.

I felt that if I went to the Dictionaries I must do it thoroughly, — not only Johnson but Webster &c & the recent 'Century' Carrell's & so on. — And that

f 7v

Would have entailed an Anthropo-
logical research too, to ascertain,
what is known about 'Savage man's
Queries – That again would have led to an
enquiry as to "animal intelligence"
which must have expanded a brief
suggestive sketch (which was all I
tried to make) into a book.

Of course I am only taking the
various forms of Query – What,
How, Where, When, Why – Who, &c on
the broadest grind & quite see that they
all overlap more or less or rather
intersect – Yet do they not give us valuable
& representative distinctions, without
breaking a continuity which I always

see as unbroken?

With regard to 'when' I was careful to guard myself I thought by explaining that I meant a computation or measurement of time in the human sense.

I suppose to utilise an animal 'when' (like the readiness displayed at the Zoo, &c) in waking at a desired time - But 'when' seems a vaguer query than 'where' & also seems to need discussion?

As to 'why' I shall be satisfied if I can suggest that the line drawn between an ultimate why & an ultimate how, what, &c may at one time be too sharp & at another too much

f8v

ignored -

I am afraid this letter may not be very clear as I am writing in bed just recovering from a sharp bout of headache, &c - But I could not postpone thanking You.

Yours ever most truly

Victoria Welby

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f9r

Febr. 1895

DENTON MANOR
GRANTHAM

Dear Mr. Galton

I was struck the other day by the following words in an interesting summary of recent photographic achievements in the 'Strand Magazine' (Jan.) "I will merely mention such photographic curiosities as F. Galton's Composite System, whereby members of a class of society are photographed singly and then blended to obtain a typical character" –

Thus does the unconscious white dispose of one of the most momentous and fertile discoveries of the age –

It is easy to predict the smile with which our descendants are likely to come upon allusions of

this sort, & still more those abun-
-dant cases in which we read of
the "uglinen of society at large" contrasted
with the "frequent excellence of the social unit;
one writer remarking "None of these
good kind people seem anything like the
monster which they keep to compose"

The italics are mine, for
I want to emphasise the views now
so widely prevalent, -1- That the
Composite Photograph is a mere toy
and of the narrowest application
2. That the group tends to reveal a lower ~~the~~
standard than that attained by the individual.

I am expressing all this
very badly, but happily it will be
well filtered in your mind; & I want

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GALTON PAPERS

f10r

to come to the suggestion which my profound sense of the value of yr. work & of certain unique qualities which it has, gives me courage to make.

I see that the Illustrated L.N. is sending out a new venture called the "Album" — I enclose a notice of it.

Surely here we have an admirable opening for beginning to show that composite Photography has a more serious significance and a richer promise for the 'Study of Man' than we have at all realised as yet? The keen interest taken in portraiture by pen or

f10v

pencil or solar ray; the eagerness
for anything humor-trivial which may throw light
on character is now evident on all
sides - It almost, perhaps sometimes
quite, touches on mania -

But at least it ought to make
the labour of learning more about
human nature easier - I can imagine
e.g. a Moral Essay of great power resulting
from a careful analysis of a
number of representative 'Inter-
-views' (I mean those already recorded
without any such reference).

But such a work - and any
future Psych. or Anthrop. work -
ought to be illustrated with fine
Composite Photographs, in a really
scientifically arranged series -

Meanwhile don't you think
the Editor of the album might be

f 118

very glad to start a more systematic attempt to apply this principle than we have had as yet, and thus perhaps keep to open the public eyes to the important issues involved?

Perhaps known you will feel that it would be rather for the Anthrop. Institute or some Kindred Society to begin: only then there would not be the same machinery for collecting a large number of examples to choose from & the expense would be deterrent, although no doubt that would be met as soon as the value of the enquiry was recognised.

Please forgive me for

f 11v

sending this long rigmarole & on no account feel bound to answer it at all – I just lay the points before you & if you think they will not repay attention or effort I am quite content –

At this moment one is thrilling with the sense of the implications of Lo Rayleigh's discovery: I mean it makes one realise how many more may await the discovering eye.

I hope you & Mrs Galton are now to warm for this severe weather – My husband is just recovering from severe lumbago –

Yours ever sincerely

Victoria Welby

I think the boy's case is now more hopeful

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f12r

May 8 1896

DENTON MANOR
GRANTHAM

Dear Mr Galton

I know you are interested in the questions of pre-natal influence and therefore send you a little document which I asked Col. Maxwell to draw up, after seeing him become quite unwell while the keeper's wife was telling me that her child's hand had been pinched in a door.

Such a case as this

f12v

So well attested is surely of some scientific importance.

He would I am sure be very glad to answer any questions on the subject but though he can himself speak of it with impunity one has to be very guarded in any descriptive allusions or the effect is perceptible at once.

I have been hoping to have

f13r

Something to report about the Composite Photography: but I have been in such wretched health that it has been impossible to go to London at all and Mr Wortley has had to get through some ordered work first - So it must wait for the present I fear. We are going to Scotland almost at once for the Sulphur baths -

Hoping that you and

fBv

Mrs Galton are enjoying this brilliant (though too dry) Spring I am ever gone most truly

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GALTON PAPERS

Victoria Wellesley

I venture to send 2 'Offprints' from Mind of my article in case you would kindly give one to any friend interested in the subject. — Mr. Stout has most kindly done me the honour of mentioning my ('sensific') Notes on his Proofs in his preface to "Analytical Psychology"

My Mother ~~[illegible]~~ told me that before my birth,(shortly) she crushed her finger in the door,and injured one of the nails badly. From my earliest remembrance,and still up to now,any sight of an injured nail,in any other person,even if a total stranger,or any injury,however slight,to one of my own,nails causes me to break into a deadly cold perspiration,with feeling of sick faintness. But still further,if I chance to hear any one else narrating in casual conversation,any injury of this particular sort to themselves or others,it brings on to me exactly the same feeling as I have described above.

So much is this the case,that many years ago,when I was in the prime of life,at a large dinner party,when one of the guests near me persistently chanced to go on talking minutely of some such little accidental injury that had befallen ~~them~~ him,I turned very faint,tried all I knew to shake it off,but could not and presently slid right under the table,quite unconscious for the moment. This is the more singular because on no other point am I in the least squeamish. In old days,I have seen soldiers flogged before breakfast without its affecting me though some of the rank and file would be very much upset, and in cases of death,illness,or wounds,I have never experienced as an onlooker,the sensations I have alluded to as above.

(Signed - G. S. Maxwell -
May 1896

f15r

May 15 1896

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DENTON MANOR
GRANTHAM

Dear Mr. Galton

From Col. Maxwell's letter to me I suspect that his answer to Gen Queen's was somewhat of a retreat from the position he so positively took up to me. Though I am sorry if so to have troubled you with it, it is desirable isn't it that these stories shall be tested before they grow into circumstantial legends after the death of all immediately concerned

I shall be glad

f15

Dose a generalized version of from Queries in other cases.

I am "Weissmannite" enough to be inclined to rejoice at the collapse of cases against him!

Please don't trouble to write again - with warm regards to Mrs. Gallin Terry

Yours sincerely

Victoria Welby

Colonel Maxwell

f165

May 10th 1896.

DENTON MANOR
GRANTHAM.

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PAPERS

Dear Mr. Gallin

Many thanks for your
kind letter - I am sending
on the form for Colonel
Maxwell to fill up and am
sure he will be delighted
to give you any required
details within his knowledge -

I will not on interview
either at your house or his
rooms in the Belgrave
Mansions, Grosvenor Place, be

f16v

desirable? Perhaps however
you can better judge of this on
receiving the filled up Paper
I ought to have mentioned
that he is 64 (having, only
lately, married my husbands
widowed Sister) at which age
you will allow "squeamishness"
of that kind is not common,
especially I suppose in men.

But of course as you
say the presumption of coin

f17r

cidence is so strong that the
evidence wants strict testing:
Still it seemed in this case
worth doing -

I am afraid I expressed
myself stupidly about Mr.
Stout's kind acknowledgement
of what he calls my 'help'
in his Preface. - I only meant
to say that so far as it de-
served the name, that 'help'
was purely due to my self-
-training in "Sensific" -
I am glad indeed you like the bond.

Gt. renewed thanks and to
hope that Sr. "Queries" may
elicit facts of scientific
value I am ever
Mt. truly yours

Victoria Welby

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PAPERS

f17r

Mr 24th 1902

f18r

UNIVERSITY COLLEGE LONDON
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DUNEAVES,
HARROW.

Dear Mr. Galton

Just as I went to Oxford for three weeks to get expert criticism of ~~my~~ some of the points in my book, so I went to Cambridge the other day with the same object; reaping there if possible a still richer harvest —

Among other privileges I had that of seeing Dr. & Mrs.

f18v

Butler who had brought to
boy Gun told me about here.
I was very much im-
-pressed by Dr. Butler & shall
always retain a delightful
impression of him
Since I returned I have been
Gun little (half?) nephew
& we had a pleasant talk
which I hope will be re-
-peated, as I am concocting
some little stories in which

f19r

my Grandson is helping me!
(the proofs of 'What is Meaning'
will soon I hope be in page
now so I shall be less hard
worked)

I told Jim about fine
Composite Photographs be-
-cause one hopes the rising
Generation will discern their
real value - next time he
comes I hope to tell him
of some of my early adven-
-tures in travel through

f19v

bild countries.

I suppose you will be starting for the South, and this year down Tenory.

Yrs - This last cold time has been almost unbearable & we shall it appears have a succession of cold winters now. Yours ever mst by

Victoria Welby

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GALTON PAPERS

April 14 - 1903

f20r

DUNEAVES,
HARROW.

Dear Mr. Gallin

Thank you for your kind and interesting letter.

I have just been studying the second set of Darwin's letters — finding much in them which concerns my work — and was struck by his saying in his first one w/ me, vol 1. 3/60 that Gnus was "excessively difficult to understand" and that (though he characteristically hopes he is wrong) he believes "only a few ~~will~~ would reach your

meaning; while in the second
(vol II. 41.) he is enthusiastic
on the clearness.

And in a letter to Sir J. D. Hooker
(vol I. 213) he says "Suddenly
light-round the subject and viewed
it from an entirely opposite &
new side, & then for the first time
there I was astounded."

Then there is Wallace's critique
of metaphors (vol I. 267) and
Darwin's answer: both illus-
trations of the present uncon-
sciousness of one very deep cause
of our difficulty in assuming
each other's mental attitudes
or modes of working, in virtue

of one common 'descent' and
Nature.

The really amazing thing to me
is that we should take for granted
and be contented with an invo-
luntary isolation, largely the re-
sult of our neglect to develop
linguistic as we have been doing
all the less important, means o
of communication - (Voluntary
isolation is of course indispensable)
We ought to learn the art of
exchanging at will the 'different
ways in which people express
the same idea'! Some day I hope
that will have a prominent place
in early training. 'Put yourself
(intellectually) in his place and

f21v

he will put himself in guns' will then become a recognised Schul maxim!

But as you say, what is wanted is experience in a more than merely linguistic sense. - For it is often our minds which are mutually unintelligible - But this is only repeating on a higher level an experience which man has already passed through - When we have made up our minds that this state of things is not credi-table thus we may find that an improvement is not so hopeless as we think -

But it is inexcusable to give you like this 'more book', though I do plead to excuse that your writings and conversation

han D a great exlēnt helped
D iuspire it –
I am sadly aware however
that I am quite unable D
deal wit the Subject as it
deserves, I can only hope
that it may find better ex
ponents.
Meanwhile I am putting
together ~~notes~~ (as I think
I told you) material for a
book in which some appli
cations of the method which
I call Significs are suggested.
But I do not - whether I shall
have the strength D bring it
D the point of publication

I shall look forward much
to the great pleasure of seeing
you & am very grateful to you
for having spent many of
your precious hours on my
poor work - I am glad you like
its appearance.

Yours most sincerely

Victoria Welby

Excuse mistakes &c. I am
in a constant flutter

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UNIVERSITY COLLEGE LONDON
GALTON PAPERS

Oct 27/1903

DUNEAVES,
HARROW.

Dear W. Gallin

I have read the report in 'Nature' of Pf. Karl Pearson's Lecture with great interest.

How fast we are moving towards the recognition of hitherto ignored needs; and, one hopes, towards their efficient supply!

As for me house cruelty Give me the illness which imprudent visits & the poison-traps most people

f23v

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live in made me suffer
the other day I am much
tempted to ask whether I might
come some day to see you in
your curtainless house!
But it is only a bare hope
Anyhow, as I am seldom
fit for 'London'.
I want to tell you that I
have just had an astonishing
experience.
I had been told by everyone
that it would be useless for

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GALTON PAPERS

me & get Bertrand Russell's
big book as even some of the
mathematical experts found
him difficult to follow, and
he must needs be quite unin-
telligible & the 'outsider'.
Now of course don't know, Co
& speak, even the 'a' much less the
'b' and 'c' of mathematics.
But then (in consequence of
one review) I did send for the
"Principles of Mathematics Vol I."
& found & my amazement that-
here, set forth in intricate
subtle chains of reasoning & in
too technical for me & follow

are some of the very ideas I can
find nowhere else ~~set forth~~;
and which in my crude & feeble
& too often I fear incoherent fashion
I am trying to express.

I am trying to work towards
the three fresh books wh. I am de-
-sired to write. But first I feel
I must somehow get into
touch with W. Russell - and
besides that I am making an effort
towards a really systematic
indexing of my stores of material

Hoping we may meet
I am Yrs. mut truly

Victoria Welby

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PAPERS

f24v

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

Nov 6. 1903

f25r

DUNEAVES,
HARROW.

Dear Mr. Galton

I can't say how much I valued our talk the other day. (And that is accurate. For I could not easily put the ~~reasons~~ value into definite terms now current, and it is not in the end a quantitative question!)

I am thought much of your comment on my lifelong idea of space as curved

(1) Do lines in my 'curved space' always converge? Or should they not diverge?

(2) Am I conscious that a star cannot be where it seems to be? (if the line of light curves)

(3) In the asymptote, the curve is produced in the straight line.

As the first and second I will refer to Prof Mach's interesting article on Space & Geometry translated in the Current 'Monist,' and

find him easier & much less than most writers. He says our imagination is dominated by our modes of visualising, - but that we can represent the facts of Spatial observation either by the Euclidean or the modern geometries, need in the latter cases only to take the parameter K' large enough -

But instead of the infinite straight line he imagines (with the moderns) great circles intersecting - Now I mentally see space as consisting not of great closed circles but as great open spirals

He maintains that the advance made in geometry is a healthy one, is not

becoming intensified and is likely to become very fruitful in result.

He ends by insisting on the truth that "inquirers at the outermost boundaries of knowledge discover my things that do not slip smoothly into all heads but Dr. are not on that account nonsense"

And Sm. are one of those very inquirers - A child may be another.

As to (3) I have looked into the case of the asymptote, & cannot see - perhaps from dulness - that it at all affects the point I tried to express - But that may well be my defect.

Meanwhile thinkers are beginning to complain that we have to strain language in the service of precision. I should say not strain but develop it! Believe me Yrs. most truly

Victoria Welby

f26v

Feb. 8th 1905

f27r

Lady Welby

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

DUNEAVES,
HARROW.

Dear Mr. Galton

I am glad in one sense and sorry in another that you are to have England on the 16th.

But I rejoice in every sense at your giving two Papers to the 'So-So' (as I wickedly call it for short)

I have seen the Proof of the one on 'Restrictions' &c - It ought to do great things.

There is only one little change that, greatly daring, I should venture to plead for.

That is, on the 2d page at the end of Section 2. to omit the words "like those of good stocks of cattle".

I find that 'cattle' used in this Connection is almost invariably understood in a sense you & I (and in fact do else where in this Paper) repudiate. People think of breeding for 'fleshiness' & so on. In short they pounce on a phrase, literalise, misapply and distort that my Cause might be perfectly clear & incontrovertible to the unprejudiced

f28r

I have had a nice letter from
W. Schuster regretting that he
cannot get here next Sunday
but I have asked him to name
another day & look forward
to seeing him -

If I could manage (weather
permitting) to come to your house
some day after Tuesday next
week, should I be able to see
you once before you leave?

You would not I fear have time
to get down here, in any case
and would probably be to
busy to see me.

You know how glad I should

f28v

be if I could be of the least use.

Among my almost unmanageable stores of material I think some items might be found useful by Mr Schuster

Yours most truly

V. Welby

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GALTON
PAPERS

June 25 1906

f29

DUNEAVES,
HARROW.

Dear W. Galton

W. Branford tells me he has had "one of the greatest & certainly most delightful surprises" in his life.

He will always have to tell you how he feels about it — I feel sure that the mere fact of your doing it — and your way of doing it — will itself contribute towards his restoration of health.

I hope you do not suffer from this heat as I do: my best friend just now is an electric fan!

Ever yours truly

L. Selby

August 8. 1905

f30r

TELEGRAMS
FASNACLOICH.

INVERCRERAN,
FASNACLOICH R.S.O.,
ARGYLL N.B.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

Dear Mr Galton

I hoped by now to have returned your proof annotated but unless you wire that you cannot wait a few days I think you would greatly gain by that.

I have been showing your circular in strict confidence to a distinguished friend (name later!) whose own studies and interests have largely been Eugenic and

who is already preaching y^e
Gospel from his point of view.
Then any day we may hear
that our most enthusiastic
supporter, Prof. J. Arthur Thomson
is in the glen - we are going
to ask him soon - them
whether you adopt it - or
no the suggestions thus obtained
cannot but be important.
I feel very small & impo-
tent in the matter & am
mainly clear that any two
the 2 clauses about 'one
in 20' & 'one in 200' had better

f 31r

(at this stage ~~[illegible]~~) be omitted.

The Engineer on the 'Moira' my Cousin's yacht is again an Eugenician of the first water - (He is not only a deeply read man but a thinker - almost Carlylean) -

When I began to speak of your campaign, he began before I could explain to use almost the very words of your addresses altogh he had heard nothing of them! -

So you see you have a Council of war in the glen!

f31v

and these are all people who can judge of the effect on the average mind of the form re of your clauses -

I think you have wonderfully avoided the 'rocks' you letter mentions -

We are here while my Cousin (Sir Walter Spencer Stanhope) is in Yorkshire - till September - Then all the grandchildren whom great joy & the parents follow next week

The beauty is utterly beyond the poor language which at present so lags behind human experience!

Yours ever warmly and faithfully

V. Dilby

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GALTON PAPERS

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GALTON PAPERS

October 2d. 1906

f32r

DUNEAVES,
HARROW.

My dear Mr. Galton

I felt sure you would be interested in the book & the article I left for you — Thanks for their return.

Since my return here I have been carefully studying another book of engrossing interest: Hugo de Vries on the experimental evidence for his Mutation Theory.

It seems to me that the bearing of the facts he brings forward on our ideas of human heredity may prove to be of far-reaching importance:

f32v

I wonder what you think -
His attitude on Pangenesis
is one you will approve?
I am afraid W. Bottomley's
wisdom (except as to its prin
ciple of which I can see the
value) is practically a sealed
book to me. It is curious
how my mind (or what I am
pleased to call that!) seems
to shut up with a snap the
moment I approach any
kind of calculation -
On the other hand Miss

f 33

Certain Cult easily use the Bottomley book!

I see in todays paper that the controversy as to the objective character of the 'Channels' in Mars is at last settled by Photography; and the witness is indeed a Marvellous one. – How long will it be before the inter-planetary ~~sig~~ signalling begins?

Yours mt truly

L. Welby

I am to see the new Secretary of the 'So-So' tomorrow –

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Oct 19 1903

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DUNEAVES,
HARROW.

Dear Mr. Galton

I was glad to hear of the 'Advisory Meetings' but it is sad that your enemy must be active already and in this brilliant dry weather - I hope Pan will be successful but my hand was not allowed to go there because of the absence of sun & frequent damps from the influence of the Mountains.

How doctors change their minds!

The autumn colours & the sunsets have been here like fairyland but the nights are grim

You don't say whether as I hope you received the little German 'Jahrbuch' (re Eugenics) I sent

f34v

Some days ago. If it came all right, don't
Notice this -
Had not seen the plethora of Bishops! My grandson
who is always chaffing Miss Carter as 'The Venerable
Reverend', presents his compliments today as the Bishop
of Eton & the Thames! So they are even then abundant
Take great care of yourself in that hateful
air of London & believe me Yours ever not being
V. Walby

I think you ought to come & live in this fine air

Oct-25. 1905 f35r

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

DUNEAVES,
HARROW.

Dear Mr. Galton

Thank you much for the prompt return, & your kind words. — I see my strongly what you mean. Don't you think one reason for slackness to work is the lack of faith in result?

My correspondence (and conversations with our strangers) reveals a really desperate state of things coming upon us — Take a letter from an able professional man, apparently prosperous & with many

interests: "Once I was full of hope & enthusiasm,
but now of despair..."

Yet I see this as the indispensable prelude
to a great reawakening of mental & moral force
& courage: the birth-pang to the sufferer seems
death and nothing else....

Sympathetically

V. Welby

Lady Welby
May 26 1907

f36r

TELEPHONE
147
P. O. HARROW.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

DUNEAVES,
HARROW.

Dear Mr. Galton

It seems as we say 'ages' since I have heard how you are.

If too much occupied to write (which indeed is usually my case!) would Miss Biggs be so very kind as to send me a report?

I know that you will kindly care to know that my work has lately begun to enter on a much more promising phase than any previously possible.

f36v

Than now three minds practically at work upon it: Mr. Macdonald who is acting as an admirable Editor & Critic and arranging my Papers for hypothetical publication: Dr. Lionel Taylor who is making a thorough study of the Method which I would call Significs, and tentatively writing on the subject; and Dr. Slaughter, who at the times when he can spare some hours from his official work, is engaged in tabulating the mass of references and

illustration references which have
become to me to
deal properly with.

Professor Stout was here the
other day for a fortnight and
greatly encouraged me from the
Critical point of view.

My article in 'Mind' on
'Time as derivative' appears in
July.

I wanted to tell you of a striking
little Paper of Dr. Lester Ward's on
'The Lesson of Mars' in which he
emphasizes the theory, which recent
research seems to have confirmed

f37v

that our world is but in its adolescence while Mars is the elderly world. It seems no longer doubted that the 'Channels' are at least likely to be the product of higher intelligences than ours hard driven to ~~secure~~ preserve life.

At the time when he sent me the lecture I was writing a little paper which I now venture to send you in case you care to look at it. Perhaps in any case I may have it back presently.

Hoping for good news of you somewhere out of stifling London

I am ever yours

V. Wellby

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

f38r

June 12 1907

TELEPHONE
147
P.O. HARROW.

DUNEAVES,
HARROW.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

Dear Mr. Gallin

I hardly need say that your address means much to me, for all your addresses (in their less technical parts!) have done this. But this one makes a special appeal to me.

You have indeed taken a great thought, one which surely wells up again and again, sometimes in noble, sometimes in fantastic or puerile forms, in the groping mind of man.

It is not time that has given its human source the name

f 38v

of Mother. Cause?

Man been thinking & writing much on this subject lately

Every day one realises mm clearly that it is Nature 'herself' whom – or which – is urging us em mm imperative ly to protest practically against a 'pitilen' course which would not hurt but help us if only we fully understood it, as Science is loyally endeavoring to do – For first as I think Gm would say we must learn to interpret 'her' mm fully, we must read 'her' most remote

f39

implications: I must become more sensitive to 'her' indirect monitions

I must not bother you with a longer letter & you must not feel bound to read my short enclosure; (one of a series)

Hoping you are feeling quite right again by now and are taking all possible care I am

Ever yours most truly

V. Delby

f40r

Sept 15 1907

TELEPHONE
147
P. O. HARROW.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

DUNEAVES,
HARROW.

My dear Mr Galton

I have often wanted to write and tell you how very sorry I was to be unable to go and meet you at Helmingham.

I have had some what serious mishaps lately and shall now have to be under permanent surgical & medical care, though I hope this will not prevent my pressing on in work —

I do hope you have

f40v

Not had any drawbacks
and are well & fit for work.
I am glad to watch the
increase of public interest
in Eugenic enquiries &
especially in No. III of the
Memoirs –
You will remember my
somewhat adventurous
hopes for the future of Com-
-posite Photography as
a method for use both
in Sociology at large and
in Eugenics –

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GALTON PAPERS

f41r

At ~~least~~ last I have found a hearty scientific sympathiser in Mr. Caldwell (expert in Chemistry) of Cambridge –

Enclose for a paragraph on the subject which I asked him to write –

If I remember rightly you explained to me that the main difficulty was precisely that of 'over-exposure'. –

It is of great interest to me that Mr. Caldwell should by an invention which must revolutionise

f41v

photography has put the coping stone on the structure of my brother's work –

He has been working at it for two years & more, and I gather that it will be announced at Cambridge this winter – but this of course is for you only. ~~as~~ – Mr. Caldwell is naturally in intimate relations with the Cambridge group surrounding 'J. J. Thomson' & I hope though him to get into touch with them – I am ever warmly & gratefully Yours

L. Welby

Remember me to Miss Bigg

TELEPHONE
147
P.O. HARROW.

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GALTON
PAPERS

DUNEAVES,
HARROW.

Sept 19 1907 f42c

Dear Mr. Galton

To get once more such a letter as that from you was a real joy. And I am glad to think of you at Hindhead out of this deadly London reek brought down by the S.E. wind —

I have sent on your photographs on C. photography to Mr. Caldwell — They have promised to come here on their way up from Cambridge so there may be more to hear then.

Did I tell you that my son has sent me his Master

f42v

for a week or two and I am already discovering that my quite moderate 'spins' (never more than 2 hours at 18 miles an hour) ~~have had~~ are having a wonderfully good effect on sleep and appetite and in regulating organic function –

I can't say, tis, what the change of sky & air is over one lives so near London –

I would give much to see Hindhead again –

I wish we could get across in a motor direct!

f43r

I suppose it is really quite a usual journey – than often tight of trying some Hotel then for a few days: but surgical difficulties w^d. have to be over come.

Yet, as you say, vitality can triumph in unexpected ways; and although there must always be suffering & disablement I feel that the Highland doctor was right when he said, You must never say If I can but always Because I can!

I must lie to gin my work a definite start: I am just the Railway Dobbin who is

f 43v

Again the trucks a shove:
after that they will almost
go of themselves!
At present they block the
human way timibly as Sm
here occasion D Kms.
But then Gms is an Express
Engine & no Dobbin like
Yours ever

V. Welby

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

Sept 29 1907 f44r

TELEPHONE
147
P.O. HARROW.

DUNEAVES,
HARROW.

My dear Mr Gallin
I am afraid I was very incoherent on the subject of the primitive preface & the Number stage (this is badly expressed) and so I enclose copies of my letter & Mr Jourdain's answer –
I do think the matter could be taken in more competent hands than mine –
What is the equivalent of 'decimal' if we adopted 12 instead of 10 as the unit scale?

f44v

Than a young Oxford man here who is greatly stirred by the idea of Significs — I do so long to find worthy workers by its method — I much hope that nothing will interfere with my coming on the 9th.

Believe me, ever yours

V. Welby

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GALTON PAPERS

f45r

Sept 22 1907

TELEPHONE
147
P. O. HARROW.

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GALTON
PAPERS

DUNEAVES,
HARROW.

Dear Mr. Galton

I have been looking out the distance hence to Haslemere & find it entails a journey longer than any I have yet taken.

But I am sorely tempted to carry out your kind wish this week. The motor is roomy and I could lie across the seat during part of the journey, Miss Corbin having a campstool meanwhile.

Would Wednesday 25th suit you? It is the first free day & I fear the last at present.

You are indeed good to wish me to come & stay at Quedley

f45v

later - I can had alas to give up ordinary writing - But I might be able to manage it about Oct 9th for one night. - I hope when I return on the 7th - ~~for one~~ from Devlin to hire a motor for a short while. It could stay the night at Haslemere.

We decided yesterday (the Dr. here & I) on an operation wh. is seldom ventured on at my age, & I am hoping to be ten crippled after that which would I think be early in November.

You will know about Mr. G. Smith's very important studies on Sex in Crustacea & will have seen the notice of them in Nature which is the first I have seen

on the subject. They fix me (as so often happens) precisely at right which I had despaired of: the Scientific right, that is, to tell the Man that my appeal was to the dormant Motherliness in him as well as in us, since in the male also there is an organic capacity for Motherhood -

I have written much lately & venture to send you some little Papers in case sincere & look at them.

But if not I shall quite understand and am yours ever

V. Welby

Please tell Miss Bigg in her letter - the 'Time' Article reprint does not need to be returned, of course

f47r

TELEPHONE 147 P.O. HARROW.
UNIVERSITY COLLEGE LONDON GALTON PAPERS

Nov 12 1908

DUNEAVES,
HARROW.

Dear Mr Galton

It struck me at the time of receiving it, that Mr Caldwell's letter did not express his meaning as I gathered it in conversation and in the results (then incomplete) of experiment which he showed me.

He himself is so conscious of defective parts of experiment (except of course in formula) that it is difficult to get him to write anything!

But what I gathered and what I think he supposes to

be assumed, is that certain mechanical appliances and chemical facilities are now available for the first time, and that this enables him to manipulate light in new ways.

He may of course be mistaken but he has substantially convinced one of the great "Photographic ~~Firms~~ Companies" — This he regrets so far as it entails the "disagreeable consequence" of commercialising the work that he so loves for its own sake.

He is a very simple and unassuming man and has I

f48r

know the greatest admiration for your triumph over difficulties now less acute -

The forward step he has taken seems to depend entirely on the qualities of his newly-invented plate, of which I had heard from others.

I am asking him now to communicate directly with you.

I have heard again from Prof Alexander (Pres. of the Aristotelian Society) today & he suggests my writing to Dr. H. T. Watt (at the Glasgow University) as he is undertaking Questionary work on lines near those you suggested.

f48v

I shall wait a little however, hoping to see Dr. Watt's answer to Prof. Alexander's letter, as it is to be forwarded to me.

I hope this milder weather suits you as it helps me.

I am almost overwhelmed by the work coming in now, but thankful for increased encouragement. Ever most truly yours

V. Welby

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GALTON PAPERS

GALTON PAPERS
LONDON

Lady Welby

f49r

July 13 1909

TELEPHONE
147
P. O. HARROW.

DUNEAVES,
HARROW.

Sir Francis!

Dear ~~Mr Gallin~~

I must (though with much love & thanks to Eva for her) write & tell you how much your little menage touched me - I have been much depressed lately & not least in thinking of you as suffering from this untimely weather - (For that I have one cause of gratitude since it permits what heat forbids, work at the fullest I am still capable of.)

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GALTON PAPERS

Just now also I am getting other messages of 'good cheer'.

One a long letter from a young American genius (even Cambridge men think him that!) who sees almost eye to eye with my poor and little understood efforts for clearing the pathways of thought & action and enhancing the lucidity of life. – Another, Prof. Harald Höffding's visit here fresh from the Cambridge

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GALTON PAPERS

f50r

Centenary & full of the warmest sympathy with my aims.

Another again from the Editor of the Hibbert Journal about a (rather long) Note I am to make in January on a 'Symposium' which appears in October, a Note of course from the point of view of Signific. –

So if only I can keep my mind clear & steady and self criticism at full working order, the work of my life will bear some fruit. – Never can I forget

f50v

that you were one of the very first to welcome and encourage it (e.g. in the Anthropological Paper of the Brit: Ass^{n}:).

But now my longing is to get it into better hands: and that hope seems at least clear on the horizon.

Yours as ever

V. Welby

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GALTON PAPERS

One more thing may amuse you. I have been elected a Member of the Mathematical Ass^{n}: (I suppose because I am the greatest dunce in the Country!) and find myself gazetted in the Gazette - Well, there also appeal is being made to the Primal Sense in us all

f51r

Christmas Day
1909

TELEPHONE
147
P. O. HARROW.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

DUNEAVES,
HARROW.

My dear Mr. Galton

I still have an unfinished letter of mr writing Dec 13th just before this last catastrophe, wh. the Dr. owns is more dangerous than the last & will mean somewhat rigid care.

I wanted to say how glad I was to hear of the Reminiscences.

They ought to be indeed interesting and as you say not least to yourself.

I wonder whether, supposing I were by then fit to lean home at all, you would care to have me on Tuesday Jan

f51v

14th till Friday?

I need hardly say the given the needed strength Wh. I hope will return before the I should greatly look forward to it — and then I could better explain the idea Wh. sounds a little wild, of suggesting carefully conducted experiments in animal breeding for intelligence only

I cannot hear of its ever having been systematically done — Of course for results in animals like horses & dogs it would mean a long process: but it might be

f52r

tried on creatures like rabbits & rats - We should have to persuade someone like the Duke of Bedford with an extension park or domain to undertake it under proper scientific supervision. It is rather provoking that so far, intelligent animals have only been chosen for trick & show work - They do vary curiously. - I never realised it till I kept 3 dogs wh. are astonishingly 'individual'.

With all Christmas wishes to you & also with my love to Miss Biggs if no with you I am ever yours

V. Welby

I have been intensely

f52v

interested in Walker's Essentials of Cytology just published. I had read all I could find on the subject before but the latest advances seem startlingly suggestive. Also Jost's Plant Physiology, a terribly big heavy book.

I know you will kindly congratulate me on being asked to write an article for the new Encyc.a Brit.a on Significs.

I hope I shall accomplish it properly

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f 53

Jan 11 1911

TELEPHONE
147
P.O. HARROW.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

DUNEAVES,
HARROW.

My dear Sir Francis

You are so often in my thoughts that I cannot resist writing to ask him you are and also to tell you of the progress of that work of mine in which you have always taken so kind an interest -

I am sure you will be glad to hear that Statistics is really on its way to complete recognition -

Any able undergraduate

f53v

at Cambridge, Secretary to the Debating Club there is devoting all his energies to its introduction & cultivation among the young men both at Cambridge & Oxford.

He tells me that Symfers answers exactly to an increasingly urgent sense of need among the younger men in both universities.

I have good hopes also of a small conference here on the subject by three or four of the principal Head Masters.

f 54r

I am doubly glad of this as I
am now in the anomalous
position of having lads of ori-
ginal powers at school here sent to me by
their relatives — or friends.

One such lad who un-
doubtedly has the 'seeds' of
genius in him, after having
disastrously failed to pass
here, has now passed triumph-
antly and has even been
warmly received at Balliol.

But the whole credit is
due not to me but to the method
I am hoping now will before
long be openly approved and
introduced, & which as far

f54v

Know is now to be called
Significs —
I can never forget the
practical sympathy you
showed many years ago with
my crude efforts to promote
the 'Significal' mode of
approaching all studies and
promoting all true advance
With love to Eva
I am ever yours warmly
& gratefully
V. Welby

Jan 13
This letter & others got over-
-looked so I send it now
= I have had occasion to write
to Dr. Butler & have got the kind-
-est of letters from him

flr

THE GRAND HOTEL,
TRAFALGAR SQUARE.
LONDON, W.C.
TELEGRAMS FOR HOTEL "GRANOTEL LONDON"
TELEGRAMS FOR VISITORS "GRAND HOTEL LONDON"
TELEPHONE Nº 2723.

7th January 1870

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

My dear Sir,

I have had a long dose of Influenza in Dresden, during which I have seen no letters: so that your condemnation of my paper by the Royal Society has only just reached me.

You will easily understand that I am anxious to learn the nature of my [illegible] misconception as soon as possible: so that if you can allow me to call upon you some time within the next day or two

shall be very grateful.

Let me thank you for the very kind way in which you have tried to make the consequences of my blunders as easy for me as may be.

Yours very truly

W P Worlder

Francis Galton Esq.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

f25

1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

13th January 1890.

Dear Mr. Galton,

I am sorry to hear that you were unwell on Thursday afternoon. I hope that the wet weather will not prevent your speedy recovery.

Your very kind letter gives me courage to trouble you again.

During the past two days I have used your Table 8, and have constructed from it the normal curves for several of my measurements. And in order to convince myself that I was influenced by what you mildly call "bias" in determining my

points a and b, I calculated also the position of a straight line passing through $Q_1 M Q_3$.

Will you be so kind as to look at the tables which I send you, showing the results of this operation?

Take first the length of the cards in the Plymouth Cranyon. This is the curve which you used to show me the character of the whole set. And I quite see that it is a "normal" curve.

But the enclosed table will show you that between the ordinates $M \pm b$ it is as near to a straight line as to the normal curve.

The table is made in this way:

In the first column is the list of centesimal grades. Then comes a column, headed "Observed values", giving the values observed for each grade. Column 3 is headed "Straight line $Q_1 M Q_3$", and gives the ordinate of this straight line at each grade. The next column, headed "differences" gives the deviation of the observed ordinates from those of the straight line.

Then in Column 5 are the values of the Normal curve, and in the last column the differences between the ordinates of this curve and the observed values.

Thus at grade 20, the observed ordinate is 4·0; the ordinate of the straight line $Q_1 M Q_3$ at this

grade is 4·2, differing from the observed ordinate by 0·2; the Normal ordinate (Q = 3·5) is 4·38, = observed ordinate + 0·38.

[The "observed ordinates" have been smoothed ~~out~~ by taking for the value of each grade the mean of the values 5° on either side of it.]

I next took the corresponding curve for St. Andrews' Cranges.

This curve is one of those which from their asymmetry interested you.

The observed Q_1 is in this case +3·5, the observed Q_3 is − 6.

If for the normal curve you take $Q = \frac{1}{2}\{Q_1 - Q_3\}$, you get a curve (shown in column 5 of Table II) which differs throughout very

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GALTON PAPERS

f45

1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

considerably from the observed curve. But if you take for each half of the curve its observed Q, you get 2 half-curves which fit the observed values remarkably well.

Columns 7 and 8 of Table II show the results of a comparison between the observed curve and the + half of a Normal curve whose Q = 3·5°, together with the - half of a curve whose Q = 5°.

But columns 2 and 3 of the same table show that if you

from M
draw straight lines through Q_1
and Q_3 these straight lines also
fit the curve between the ordinates
$M \pm 5.75$.

In the same way, Table III
shows a relation of the same
kind between the corresponding
curve of the South port shrimps,
between the ordinates $M \pm 5.5$.

So that in all these three
cases, the curve of lengths of cara-
pace seems to be nearly a straight
line within certain vertical
limits, which lie between $M \pm 5.5$

and M ± 6.

This is an agreement with my previous result which is so remarkable that I must have felt encouraged to send it to you.

Please do not misunderstand me. I quite see that these curves are really normal curves.

But I do not yet see the fallacy in this method of showing the coincidence between portions of the curves and straight lines: and ~~also~~ if these portions can be shown to be approximately straight, they are so much longer than

I will not trouble you any more just now: but I shall be

f5v

very grateful if you will consider what I have said, and let me know whether it is as absurd as the rest.

Yours very truly

W. F. Weldon

I sent Heincke's paper this morning.

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GALTON PAPERS

f61

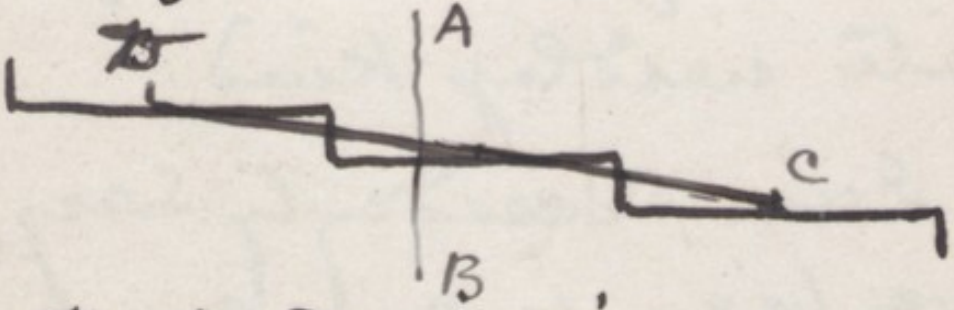
1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

20th Jan: 1890

Dear Mr. Galton

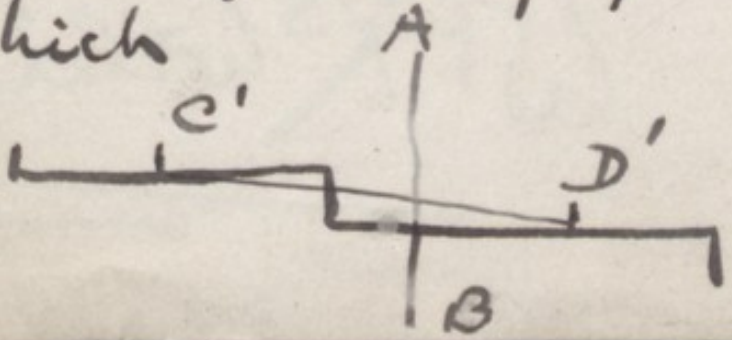
I must tell you of a stupid blunder which occurs in the tables sent to you yesterday.

I interpolated by taking the value of the ordinate at A B



as the intersection of A B and C D.

I send you the proper tables in which



f 61

gives the interpolated ordinate.

The result is not seriously affected: but I fear, if you have looked into the figures at all, my folly will have caused you trouble of a quite needless kind.

I am exceedingly sorry to have been so careless.

Yours very truly

W F R Weldon

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

	Grade	Mean Curve	Difference from Straight Line	Difference from Normal Curve
	5°	2·42	+0·55	−0·02
	10°	1·81	+0·15	−0·09
	15°	1·47	+0·01	−0·07
	20°	1·25	0·00	0·00
Q_1	25°	1·04	0·00	+0·04
	30°	0·85	+0·02	+0·07
	35°	0·61	−0·01	+0·04
	40°	0·39	−0·02	+0·01
	45°	0·21	0·00	+0·02
M	50°	0·00	0·00	0·00
	55°	0·15	−0·04	−0·04
	60°	0·39	+0·01	+0·01
	65°	0·54	−0·03	−0·03
	70°	0·74	−0·02	−0·04
Q_3	75°	0·96	0·00	−0·06
	80°	1·19	+0·04	−0·06
	85°	1·48	+0·14	−0·06
	90°	1·98	+0·45	−0·08
	95°	2·49	+0·77	+0·05

Between 10° and 80°

Mean Devn. from St. Line —— 0·023

—— " —— Normal Curve — 0·039

Between 15° and 80°

Mean Devn. from St. Line — 0·014

—— " —— Normal —— 0·023.

Mean of observed curves of Carapace length.

Crangon.

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GALTON PAPERS

P 81

1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

21st January 1890.

Dear Mr. Galton,

I have an engagement which will prevent me from doing any work for the next two days: but I cannot wait till these are over without thanking you. You have indeed given me new courage.

I will try to do what you suggest as to the partial destruction of a normal curve as soon as possible.

Will you tell me the full title of the book of Tables which

f 8v

you recommend? I am beginning to find multiplication and division fail to be amusing, after a time.

Yours very truly

W. W. Rouse Ball

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GALTON PAPERS

f9r

1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

23rd January 1890

Dear Mr. Galton,

Let me thank you most heartily for your delightful present, which arrived this morning. It makes me long to multiply something, — which is a sensation quite new to me.

I resist, however, and am wrestling with the effect of selection on the Normal curve.

Your arithmetical result seems so hard to understand. In fact I cannot, without trial,

understand how anything but the original curve can be reproduced.

Therefore I will not trouble until one or two generations are worked out.

I have asked Donald Macalister to do the thing generally: but I am afraid he will not be sufficiently interested to make time.

Yours very truly

W F R Weldon

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

F10f

1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

28th. Jan. 1899

Dear Mr. Galton,

I am very sorry I have expressed myself so badly.

What I mean is this:

Let PMQ be a Normal curve, whose Q = 1, and whose axis is MM₁. (see loose figure)

Through a point A in PMQ draw AA₁, parallel to MM₁ and take this as the axis of the curve RAS, whose middle point is therefore A: then, to find the portion of this curve, which

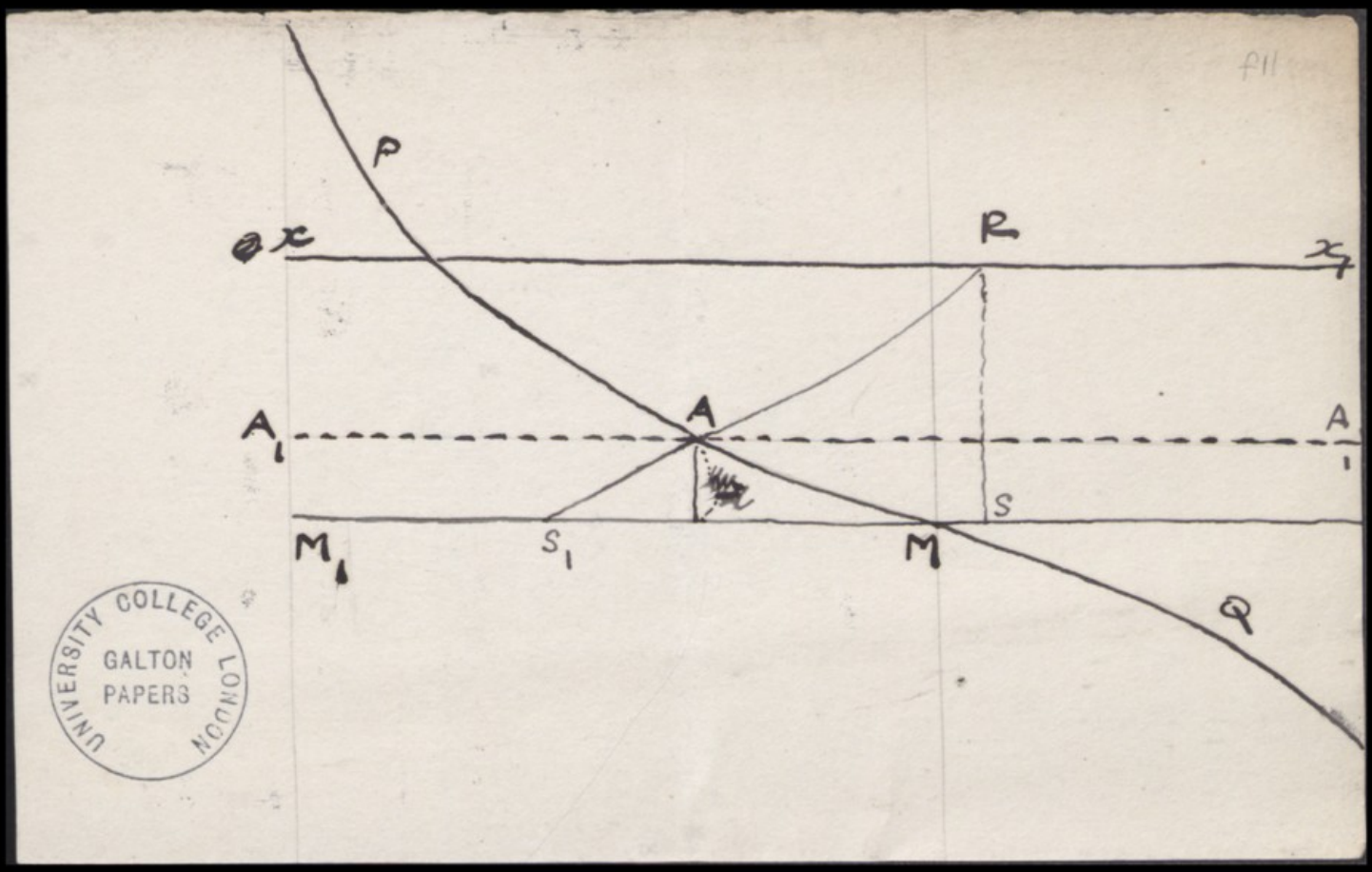
will lie between M M$_1$ and x x$_1$
Take the length of the intercepts
S$_1$ S$_2$.

I have done this in the big figure which you have: taking Q 21.

So that the axis of each curve in the big figure is a line, parallel to the axis of A B, and cutting A B where the secondary curve cuts it.

That is, the M point of each curve lies on A B.

What puzzles me is that this seems to be the same case as your problem (4): so that if



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the proceps were carried on for
the whole of the curve AB, and
if Q = 1 throughout, the Q of
the new curve ought to be $\frac{1}{\sqrt{2}}$.

Yours very truly

W F Sheldon

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

1, Hoe Villas, Elliot Street, Plymouth.
30th January 1890.

Dear Mr. Galton,

The change of the axes of coordinates does not affect my figure.

Let PMP' (in the accompanying figure) represent the deviations of a group of mid parents. Let the Q of PMP' = QN = q.

Let any mid parent, A, of this group, produce offspring. It is required to find the percentage of those offspring whose deviations lie between 0 and q, measured from the axis of PMP'.

By hypothesis, the deviations of the brood of A from their median will form a normal curve; and, neglecting regression, their median will be m. The Q of the curve will also, by hypothesis, be = q.

Therefore the question becomes this: if through a point y = m be drawn a curve of error whose axis is the straight line y = m, and whose Q = q, what percentage of that curve will lie between the ordinates 0 and q measured from a straight line y = 0?

In the figure, through A draw Y'AO', parallel to YO, and QW parallel to YO.

Let YO, OX be the axes of PMP'.

From A draw AP perpendicular to YO.

From YO cut off OV = PM: and through V draw VO, X, perpendicular to YO.

then with Y_1O_1, O_1X_1, as axes, draw the curve $SARX_1$, similar to PMP_1, but with its sign changed. [The change of sign does not affect the result, and makes the figure clearer.]

Through S and R, where the curve $SARX_1$ cuts QW and YO draw SS_1, RC, perpendicular to YO.

The portion of the curve which lies between YO and QW is represented by the fraction

$$\frac{S_1B}{2AO_1}.$$

But $S_1B = SC$;

And $AO_1 = MO$.

$$\therefore \frac{S_1B}{2AO_1} = \frac{SC}{2MO}.$$

Now call $2AO_1 = 2MO = 100$.

$$\therefore \frac{S_1B}{100} = \frac{SC}{100}.$$

Therefore the number of grades through which the curve $SARX_1$ lies between

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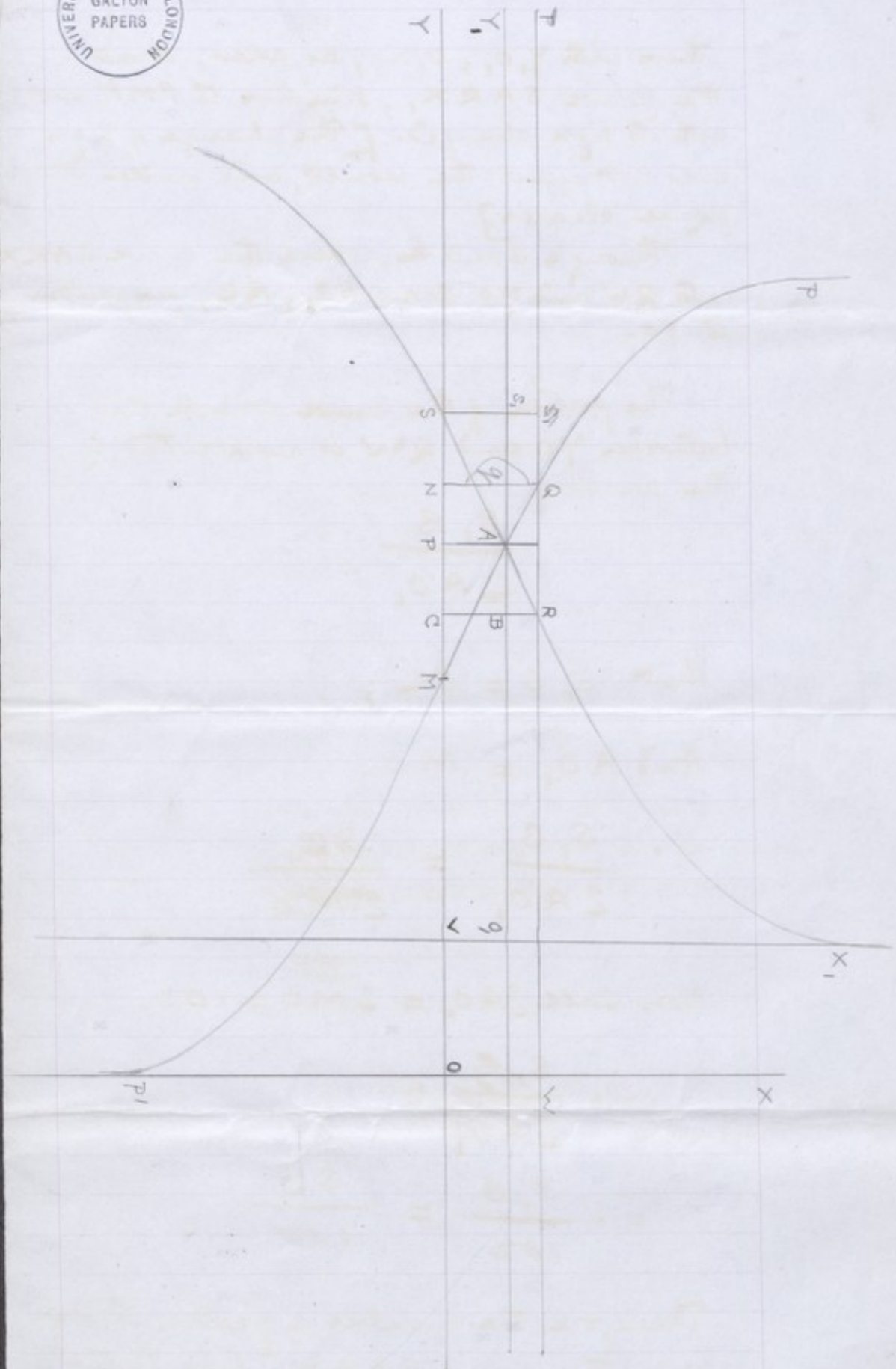

f15r

YO and QW may be measured by the intercept SC on the axis of PMP', measured in grades of that axis.

~~That is, if (as in the case of the curve through A in my original figure) SC = 26% of the axis of PMP', then 26% of the curve SARX' will have a deviation from the M of PMP' which is ~~ not greater than~~ q.~~

That is, if SC = x% of the axis of PMP', then x% of the curve SARX' will lie between O and q, reckoned from the axis of PMP'.

So that in working with the original figure, the changes in the axes, made during its construction, may be neglected.

This is all true except

~~Then I said, the curve through A gives x percent of individuals between O and Q of the original curve. The curve through A second point B gives y percent.~~

~~∴ that these curves together give x+y percent of points between O and Q.~~

In the original figure, 31 ~~each~~ curves are drawn, in the way SARX' is drawn in today's figure.

Each of these contributes a certain percentage, x, of individuals, whose deviation lay between O and Q, reckoned from the axis of the parental curve.

Therefore it seemed to me that the

Total percentage of offspring, lying between O and Q from the parental axis was

$$\frac{x_1 + x_2 + x_3 + x_4 + \ldots\ldots + x_{31}}{31}$$

Now about $\frac{1}{\sqrt{2}}$.

If through all points in PMP' curves be drawn, in in a way similar to that by which SARX' was drawn, a curve derived from all these would, if I understand rightly, be the same as your curve of green shot in Problem (4) — Natural Inheritance p. 69. — So that if Q should be $\frac{bc}{\sqrt{(b^2+c^2)}}$

But in my figure $b = 1 = c$.

$\therefore Q = \frac{1}{\sqrt{2}}$.

Yours very truly

W. A. Weldon

Please forgive the untidiness of this letter. To write it again would delay it. And I want you to get it the first moment I may.

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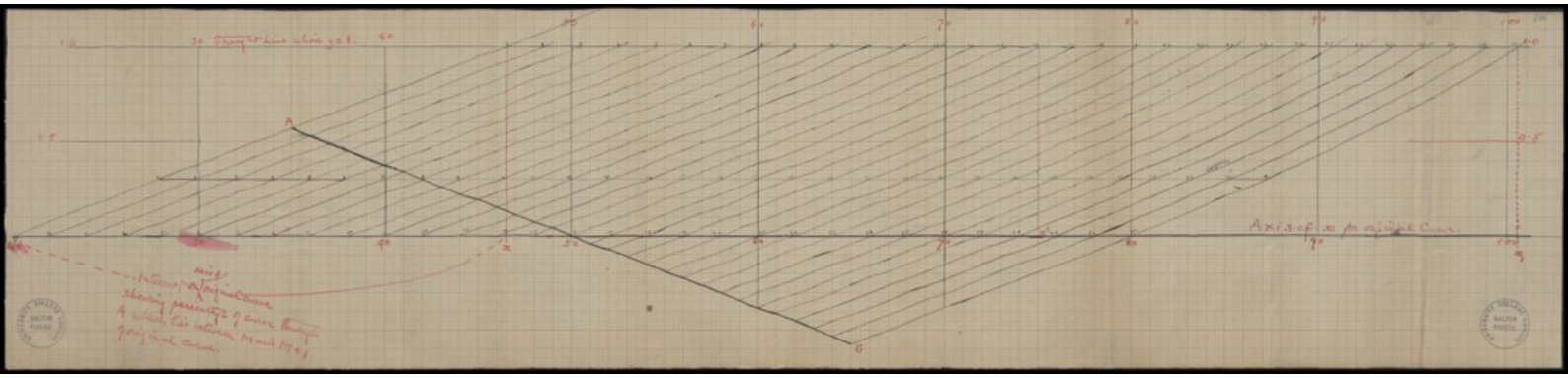

Straight line where y=1.
Axis of x for original curve.
A
B

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f17r

St. John's College
Cambridge
10th February, 1890.

Dear Mr. Galton,

I hope your trip to Hastings is doing you good: and that the influenza is going away and leaving no bad effects behind.

I have been staying here for a week, and MacAlister has been coaching me hard: so that I hope to make fewer mistakes in future.

Under his advice, I have treated all my curves as follows:

I have re-drawn them, ~~drawn~~ and compared them with the curve

$$y = \frac{A}{c\sqrt{\pi}} e^{-\frac{x^2}{c^2}}$$

where A = total no. of observations:

$\underline{c} = \sqrt{\pi} \times$ mean Error (regardless of sign)

$\therefore y = \left(\frac{\text{Total no.}}{\pi \times \text{mean Error}}\right) e^{-\frac{x^2}{c^2}}$

This curve was traced from the Table in Airy's Theory of Errors p. 16.

The curves showed that

(1) – In the majority of cases the area of the actual (observed) curve over the Error curve:

(2) – In nearly all cases a preponderance of the area of

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MacAlister is wrestling with the question of the effect of selection acting on one generation, on the deviations of the next. Therefore my little efforts may probably stop. =

I send you some curves, drawn as I have now drawn them. — Don't you think that for an person who makes blunders this is perhaps safer than your curve? — I see how much ~~better~~ better yours is when it is looked at with your eyes: but my own have not been

f18v

very successful: and even I could hardly make mistakes in reading the enclosed!

Yours very truly

W F R Weldon

the observed curve ~~~~ on the + side over the area on the - side.

[Of course "error" in above means deviation from the median, expressed ~~ in $\frac{1}{1,000}$ ths. of body length]

MacAlister points out that the result (2) indicates that the Geometric Mean ought to be used.

Have you drawn up a table of ordinates of this curve, like ~~the~~ on the table based on the Arithmetic Mean in your book? — If not, I will try to make one.

Result (1) is I suppose the small basis of truth upon which my

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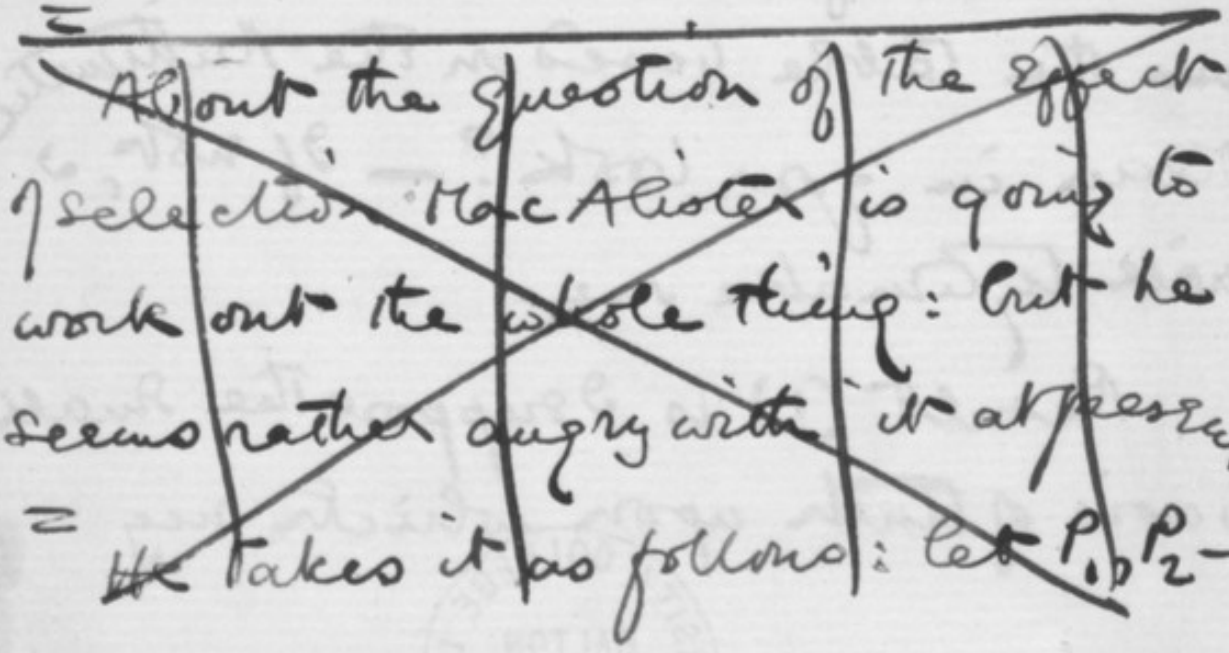
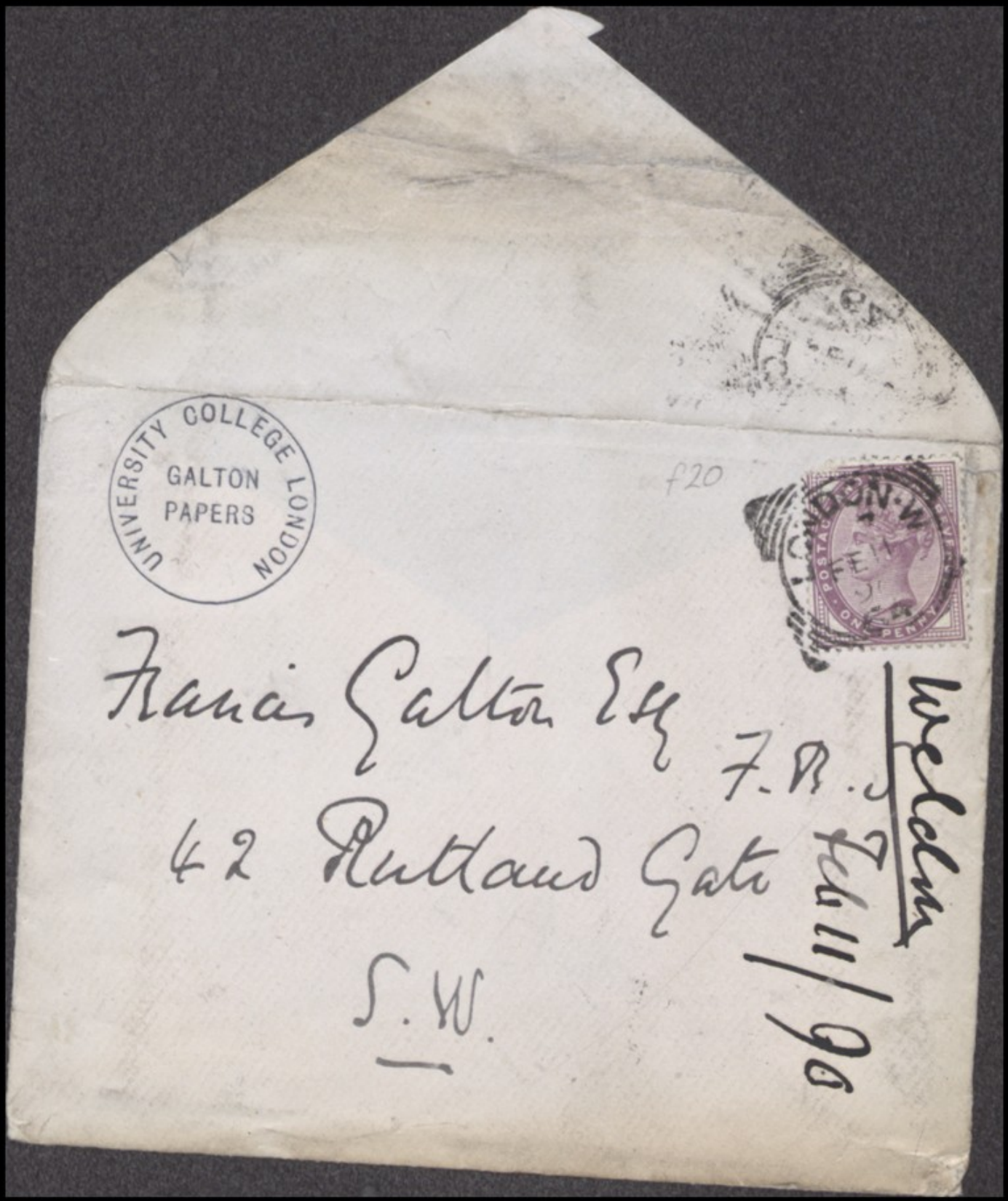
original contention rested.

Tried by this curve, my measures come out so irregularly that MacAlister advises me to double their number; and I am going back to Plymouth tomorrow in order to begin to do so.

Do you not wish that I had come up here some time ago, instead of worrying you with my mistakes?



About the question of the effect of selection MacAlister is going to work out the whole thing: but he seems rather angry with it at present. He takes it as follows: let P_1, P_2 —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f20
Francis Galton Esq F.R.S.
42 Rutland Gate
S.W.
Wellcome
Feb 11/90

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GALTON PAPERS

f21r

1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

12th February 1890.

Dear Mr. Galton,

I am glad to hear that you have got rid of your influenza: and I sincerely hope that you will not have either a relapse or a second attack.

Also I am very sorry for my carelessness in leaving a ~~[illegible]~~ whole phrase out of my letter. You saw what I meant, but that does not excuse my conduct.

About the area of the curves: As I understand the matter, an Excess of the area of the observed over that of the normal curve indicates

the occurrence of some deviations with more than normal frequency: and in this way confirms the existence of the flatness which was apparent in many of my curves when drawn on gm principle.

=

As to the geometric mean.

I have made tables for all the observed curves, ~~showing~~ reduced to Q = 1. — But the interpolations are made by drawing a line from the middle of each step to the middle of the next. — So the mean of all these be of no use

for deciding which mean to use?

2

You suggest that the curves drawn on MacAlister's plan should be ~~drawn~~ compared with theoretical curves in which the mean error ~~varies~~ has slightly different values.

I did not do this in any one case: but I did not determine the mean error in the same way in all cases.

Sometimes it was determined directly from the measurements.

Sometimes it was taken as $= \frac{Q_1 - Q_3}{2} \times 0.8$; and sometimes $\underline{c}$ was determined from

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the Prob. Error thus: $c = \frac{Q_1 - Q_3}{2} \times 0.477$

— So that in some cases the Mean Error used in drawing MacAlister's curve was derived from the Q of your curve, and in others directly from the measurements.

The results were so coincident that I thought it hardly worth while to try all methods on the same curve: but if you think it necessary I will do so.

2

As to the sensitiveness of the two methods:

I did not for a moment wish to suggest that your curve

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1, NOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

was not sensitive in <u>my hands</u>.
– Only the change near the middle part of it is so gradual that to an inexperienced person like myself the other curve seems easier to carry in the eye.

I hope I shall some day learn your by eye also!

Yours very truly
W F R Weldon.

Please keep the curves you have, if you care to do so.

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f241

1, HOE VILLAS,
ELLIOT STREET,
PLYMOUTH.

16th February 1890.

Dear Mr. Galton, The possible meaning of the curves which I sent you matters very little: because they seem to owe their abnormality to the small number of observations on which they are based.

By leaving out the legs (which take a long time to measure, and which give untrustworthy results at best) it is fairly easy to measure a great many shrimps in a day: and since my return from Cumbrae I have measured 200 additional

Shrimps of the Plymouth race, — taking four characters in each.

The four resulting curves are still irregular, but the area of each is nearly exactly equal to that of the Normal Curve.

I enclose one (please keep it if you care to do so). — The others shall be sent to you as soon as I have had time to go over the arithmetic a second time.

This complete normality makes it seem unlikely to trouble you with the Centile Tables of the remaining curves, at until I have made

additional measurements for these also.

Is it not remarkable that the error from insufficient data should have been always in one direction?! MacAlister at least thought this a proof that the individuals with small deviations occurred oftener more often than the Law of Error indicates.

2

I suppose then that you cannot doubt any more the complete normality of the curves of frequency of these variations: and they curves must be taken as a complete demonstration of the truth of your remarks on Natural Selection

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(pp. 119-121 of Natural Inheritance).

But there is a feature about this which troubles me: namely, that I see no way of demonstrating that the curve is a result of Natural Selection. (newly born)

You have a brood of shrimps whose Q = q. These give rise to an adult population whose Q = $\frac{q}{m}$.

This (conceivably) may be done either by the modification of each individual in such a direction that its deviation from the median is reduced, or it may be done by Selection, as shown in your book.

Can you suggest any statistical test which will determine which

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P26r

1, HOE VILLAS,
ELLIOT STREET,
PLYMOUTH.

influence is acting in any given case?

This is a fine old problem which has been suggested by many people, I know: and I hoped that these curves would throw some light upon it: but they throw light upon very little!

Yours very truly

W. A. Forbes

May I venture to ask if you have a spare copy of your paper of 1872, in

which you invented Weinmann?

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

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

1, Hoe Villas, Elliot St., Plymouth.
19th February 1890.

Dear Mr. Galton,

Many thanks for your two papers, and for your letter.

There are now 400 measures in each curve of Plymouth Shrimps: and the curves remain quite normal.

While waiting for material from other places, I have been learning your test of co-relation. It shall be tried upon the Shrimps as soon as possible.

About Selection.

I fear the Shrimp Larva is much too delicate to be touched in any way twice during its life time.

Also, ~~I fear that~~ I cannot at present quite see how an experiment of the kind you suggest would answer all my difficulties.

The Shrimp Zoaea is a fair example of these difficulties.

You probably know that the newly hatched shrimp is nearly transparent, about 0·5 mm. long, and that for some time, perhaps a month, it swims freely on or near the surface of the sea.

Now, consider its enemies. Fish of all, the surface fauna of the sea contains the larvæ of practically every marine animal whatever: ~~so that the initial struggle for food must be very keen~~.

Secondly there is a large population of small adult crustacea, preying upon one another, and upon the floating larvæ.

Thirdly, there are the foraminifera and various Protozoa.

Fourthly the pelagic algæ, the basis of food for the whole surface fauna.

Now, these are all things of commensurable size: they can conceivably struggle against one another: and any change in habit, any protective spine, or colour, or any offensive taste, — any change of this clap will easily be admitted to be useful against these enemies.

But there are other enemies.

First, the whole group of pelagic fishes.

Where the surface fauna is richest, you have flocks of herrings, sprats, mackerel, anchovies, and so on, feeding upon the floating fauna.

Consider the effect of half a million herrings, coming like a solid wall against the tide, and simply breathing. In merely swallowing the water and paping it through their gills, such a body of fish must destroy enormous quantities of larvæ: and their destructive power is so great, as compared with the means of protection at the disposal of a minute larva that this destruction must be <u>quite unselective.</u>

The "protective" spines, and so on, of various larvæ look rather terrible under a microscope, but they do not protect their possessors from destruction of this kind. — At some times of the year, the intestine of the herring is found to be very red, and slightly inflamed: and such a herring will have its whole alimentary

canal loaded with "protected" larvae.

This condition of herrings is quite well known to fishermen: but I cannot learn that any one believes it dangerous to the herring ~~spawn~~

Then consider rain.

A pelagic larva is exceedingly sensitive to changes in the salinity and in the temperature of the water. But a few days of heavy rain in summer can certainly affect both these conditions seriously and suddenly. The salinity is perhaps only altered in bays and creeks which receive rivers: but in these places larvae are often most abundant.

You remember the effect of the smallest river on a Fringing Reef!

How far breaking waves can destroy a very small organism, one can hardly guess.

My difficulty, then, is this: that it seems plausible to argue, in many cases at least, that the great enemies of an animal, at some stage of its life, may be huge climatic or other changes, affecting the population as a whole, with a destructive power which is immeasurably greater than any defensive mechanism possible to the organism. The percentage of the population destroyed in such a case may be so great as to leave no appreciable margin for selective destruction, which must, by the very fact that its power is selective, – that is, that it is commensurable with the resistant power of the organism, – be on a smaller scale than the great

agencies which act irresistibly and unrelaxingly.

I am afraid no experiments on animals in captivity will enable one to estimate the relative efficiency of these two ~~ob~~ classes of forces.

In speaking of experiments, may I say a word about your moths?

So far as I know their history, it is this: Mr. Merrifield established the A, M, and Z races, from individuals bred in his house and garden upon birch trees. He gave some eggs of each race to a lady in Brighton, who fed the larvae also on birch. ~~He gave me~~ These individuals were all slightly larger than those grown by Mr. Merrifield himself. — He then gave some to me: and such of these as survived were grown here in Plymouth, and fed (with Mr. Merrifield's permission) upon ash. — These individuals are larger than any of the whole set, though their relative sizes are as they should be.

This is an example of the sort of difficulty I fear so much in ~~being~~ domestication experiments. Surely you have here evidence that a very slight change in conditions may enormously complicate the result of your experiment?

And in breeding any quantity of animals it seems impossible to preserve the identity of conditions.

For example, a batch of Selenia eggs, if left to itself, will produce larvae who will feed at different rates, and pupate at different times. The moths will then appear

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GALTON PAPERS

at different times: and it will be difficult or impossible to ~~procure~~ obtain all the perfect insects alive at the same time.

Therefore the precocious larvae have to be kept in an ice-box, and the backward ones in a hot-house.

I suppose you and Mr. Merrifield know enough of the effects of temperature on Selenia to allow for this kind of thing. But ~~with~~ in the case of marine larvae no one knows such things: and they would take several life times to learn.

Even with the necessary knowledge, such experiments will not, as it seems to me, show the share of Natural selection in any given case.

For example, your moths can be made to produce races of different sizes by selection. They can also be made to vary in size by the change of conditions implied by the different treatment they receive at the hands of Mr. Merrifield or of his friend in Brighton, or I myself. — There are ~~probably~~ possibly changes of ~~conditions~~ "climate" which can hardly be analysed, ~~but even if~~ — ~~How can one decide~~ In any given case in which Selenia is found to vary in size, in a wild condition, how is one to assign the proper share of responsibility for the change to Selection on the one hand, and to Adaptation on the other?

I have been trying to think ~~what~~ about secondary sexual characters. Suppose a beast with a character (not sexual) which could be measured, and another character which was either a sexual ornament or a clasper, etc., — might one not safely ~~assert~~

assume that the modifying influence of surrounding conditions, whether it acted upon the first character or not, had no appreciable influence upon the second?

When these shrimps are ~~mixed~~ beaten into such shape as I can manage, do you think a series of measurements of an animal such as I suggest, ~~[illegible]~~ would be useful?

With a little thought, a good animal might easily be found.

Yours very truly

W. F. R. Weldon

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f31r

1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

26th February 1890.

Dear Mr. Galton

Were my remarks about the Moths really so impertinent – I did not mean to suggest that? your experiment would not answer your question: and of course I expect from it the result which you have predicted.

A diminution in the Fraternal Q, as the race becomes purer seems not only theoretically to follow from your work, but also to be to a certain extent proved by cattle breeders.

As usual, my difficulty about

Natural Selection was badly expressed.

Suppose, as you say, a brood of 1,000,000 larvae to be born; and suppose 500 of these to reach maturity.

Now, ~~suppose~~ if it can be shown that all the destructive influences acting on the brood are Selective, then 99·95 per cent. of the total population have been killed selectively.

So that, if the most favoured magnitude, in any respect, be P, then 99·95 per cent. of the

population, consisting of individuals whose magnitude is not P_7 have been killed.

~~The~~

Now suppose that 900,000 are killed by unselective agencies.

There remains a sample of 1000 individuals, chosen by chance, of whom 500, or 50 per cent., are killed selectively.

But since the 1000 individuals are chosen by chance, the distribution of variations will be the same in this sample as in the original brood.

So that while in the one case

f32.v

99·95 per cent of the individuals whose magnitude is not P have been destroyed, in the other case only 50% have been so destroyed.

Or surely follows from this that the Q of the resulting population will be greater in the second case than in the first?

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2

I am sorry that I have not used the mean of the ~~Sums~~ Grades 21·2 and 78·8 in finding my M.— And this omission will materially affect my attempts to

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f33r

1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

to work with the corelation formula.
— If you will destroy the tables I sent to you, I will work out the corelations again, using the corrected mean value in each case.

The curves are all slightly a-symmetrical, so that this correction will be important.

=

I hope you will ~~will you~~ never feel obliged to answer my letters till it is convenient for you to do so?

I value very much the privilege

f33v

of coming to you with all my difficulties: but I do not want to become more of a nuisance than ~~my~~ is absolutely necessary.

Yours very truly

W. F. R. Weldon

27th.

Your letter of yesterday has just

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arrived. — Many thanks — I will follow your advice about correlation as well as I can.

The centile tables of each of the 8 shrimp curves are made and shall be sent tonight.

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No. of Cases	Value of (c) = Subject		Mean of (b) = Relative	
	Deviation	Dev. ÷ Q	Deviation	Dev. ÷ Q
40	+5·47	+1·53	+5·95	+1·31
53	+2·47	+0·69	+2·44	+0·78
41	−0·03	−0·008	−0·98	−0·02
46	−2·53	−0·71	−1·39	−0·31
31	−4·53	−1·27	−3·20	−0·71

Relation between (c) and (b)

(c) — M = 178·03 Q = 3·58
(b) — M = 250·52 Q = 4·53

No. of Cases	Value Telson = Subject		Mean of Terminal VI = Relative	
	Deviation	Dev. ÷ Q	Deviation	Deviation ÷ Q
32	+4·02	+0·88	+0·03	+0·009
28	−0·48	−0·11	+3·15	+0·97
43	−4·98	−1·09	−0·03	−0·009

Relation between Telson and Terminal VI.

Telson — M = 192·48 ; Q = 4·56
Terminal VI. — M = 145·57 ; Q = 3·24

Weldon
PLYMOUTH
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ONE PENNY
Francis Galton Esq.re
42, Rutland Gate,
London,
S. W.

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GALTON PAPERS

f37r

1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

26th February 1890.

Dear Mr. Galton,

Enclosed are two tables. — One is the table of the mean of 8 curves, from two sets of Cranger measures. — Four of the curves have 400 measures in each, and four have 300.

You see that the mean curve is nearly normal: only Q_1 is always slightly less than Q_3. — The largest difference was in a curve in which $Q_1 = 0.87$, $Q_3 = 1.13$ — The smallest

ff 37v

difference was when ~~both~~ Q_1 & Q_3 were equal.

This is not a constant character, because ~~it~~ in many of the older curves $Q_1 > Q_3$.

—

The other tables are two attempts to apply your co-relation formula (why do you object to correlation?)

— In each case ~~a "subject" was taken.~~

(1) a magnitude of the subject, which occurred often, was chosen: and its deviation

from its M (found by interpolation) was divided by its (interpolated) Q:

(2) All magnitudes of the relative, which occurred in those individuals in which the subject had the value selected, were collected, and the mean of these was taken.

The deviation of this mean from the M of the relative was divided by the Q of the relative.

The first table [(b) and (c) magnitudes] refers to the relative

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between a portion of the carapace (c) taken as subject, and the whole length of the carapace as relative.

The second table has the length of the last tail-segment as subject, the length of the last but one as relative.

Are these results worth anything?

I should like to ask you also to what extent it is useful to go on increasing the number of these measurements.

As they stand they show:

(1) That in all the organs

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1, NOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

measured the curve of frequency is normal;

(2) That the Q of a given organ varies in the different races of the same species.

Will it be well to simply make more measures of these organs to make the smoothness of the curves greater, or will it not be better to go on, and measure now the younger shrimps?

Yours very truly

W. F. R. Weldon

f40

Mean of 8 curves of organs of Change

	5°	2·52
	10°	1·92
	20°	1·21
+Q1	25°	0·95
	30°	0·72
	40°	0·35
M	50°	0·00
	60°	0·36
	70°	0·79
-Q3	75°	1·05
	80°	1·31
	90°	1·97
	95°	2·50

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f41r

1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

2nd March 1890.

Dear Mr Galton,

Enclosed are the Centile Tables, and two sets of co-relation Tables. — The last seem so irregular, that I shall not make any more until you have expressed an opinion about the present ones. — I am afraid there are still too few observations.

I will read Venn again carefully. — But I have not intentionally assumed that an unselective

destruction is capable of altering the Q of a race.

It is too late to attempt to restate my story today: but I will try to do so soon.

=

A new race of shrimps has been sent from Sheerness: and I have measured the (b) and (c) of 200 individuals. — I shall go on until your opinion of the correlations arrives.

=

Would the curves, without any correlation, be worth publishing

soon?

I do not ask from impatience, but for this reason:

I am sent down here by a joint committee of the Royal Society and the Marine Biological Association, to report upon the Crustacea here, and especially upon the lobster fishery.

It was understood that I should do what I liked, except with regard to the lobsters, provided I did something.

The glorious thing which I did at Christmas must have disgusted this committee rather much: and I am rather anxious to remove

as much of their disgust as may be, — or at least to show that I have not simply been idle.

This is a very paltry reason for wanting to print something, but perhaps you will see that it has some justification.

Yours very truly

W. F. R. Weldon

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Of course you have really done all the work. Will you do such a very small paper the honour of putting your name at the head of it?

f43r

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1, HOE VILLAS,
ELLIOT STREET,
PLYMOUTH.

5th March 1890.

Dear Mr. Galton,

Your letters are always kind: but I shall not easily forget your last. — You may be generous enough to make light of the matter: but you must see that if you had simply rejected my paper of Chronicles and left me to my own devices, I should by no means have ~~cleared~~ ~~up~~ the understood what you have helped me now to understand. And even if the time comes when I am able to "spin away where I please", I hope I shall remember the help which

has enabled me to struggle on now.

I should very much like to ask you to communicate the results to the Royal Society, as you offer to do. But when Dr. Foster wrote to me about the former attempt, he said "I will get this destroyed, and will communicate your revised version". And he repeated this promise when I saw him in Cambridge, a few weeks ago.

Would it not be rather rude if I were to forget this now?

2

Your results ~~on~~ about correlation are very delightful. — I had got

f44r

$\frac{3}{4}$ for the $\underline{r}$ of one set: but I dared not believe that the Southport sample agreed with it so closely as you think.

This was a great trouble: because it seemed as if the correlation must be the same for all races of a species.

The 1000 measures are now finished; and the ~~and~~ percentages of the Sheerness race will be finished tomorrow: as will also (I hope) the correlation of (b) and (c) in this sample.

You shall have the tables of this race, and the final Mean Curve, as soon as possible.

Will it not be well, in ~~[illegible]~~ making

f44v

the Centile Tables, to use the mean of grades 21·2° and 78·8° as the M?

By doing this in the existing tables, the values of Q_1 and Q_3 become much more nearly alike than they appear when the interpolated value of grade 50° is used as M: so that the only appreciable deviation from normality goes out of the whole set of curves.

2

In the Correlation values, would it be worth while, when working with so small a number of cases, to attempt to determine the value of $\sqrt{1-r^2}$? — The mean error of the relative, in each case where more than 30 or so are

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f45

1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

available, might be directly determined? — I suppose it would be quite useless from 30 or 40 cases to draw the curve of relations and determine the Q: but would not the mean error be as likely to be right as the M?

2

Yours very truly

W. F. R. Weldon

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f46r

1, NOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

10th March 1890.

Dear Mr. Galton,

I enclose the final Mean Curve, with the Centile tables of each curve on which it is based.

Also the correlations of (b) and (c) plotted together on one chart. – You see how well your value of $\frac{r}{}$ fits!

But I send also two new correlation Tables (between the length of the Carapace and that of the last segment of the body) which look very bad

indeed.

I have checked the work of these last tables very carefully twice: and I am afraid it is right.

Are you quite sure that it is wise to publish tables of correlation based on so small a number of observations?

Your doctrine that r ought to remain the same through all species of the same type seems to me so fascinating that

unless you are demonstrating it yourself I shall certainly try to do so. Therefore I should like to make the values for Crayford organs so certain that they might be used as a standard in subsequent work: and I shall be anxious to know whether you think values obtained from so small a number of measurements can be trusted in this way.

Even the (b) and (c) table which I send you is very wild: and the others are heart breaking

f47v

would you feel justified in asserting, — merely on the evidence of the three charts, and apart from theoretical reasons, — that the value of r is the same for the (b) and (c) relations of all these shrimps?

Yours very truly

W. F. R. Weldon

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f48r

1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

12th March 1890.

Dear Mr. Galton,

On the table which I sent, the mean curve was the mean of all the others. — The individual curves were simply placed in two sets because the paper would not hold them in any other way.

As to correlations, I will at once begin to make more measures.

Do you think it would be desirable to publish now the results as to the normality of the curves of variation with perhaps a provisional state-

meat as to the correlation of (b) and (c), — or would it be better to postpone publication altogether until more complete correlation tables can be made?

I could measure 1000 Shrimps (taking 4 characters) in a month — including the construction of tables.

To get tables based on 1000 individuals of 3 races would therefore take about three months — because I shall not be able to devote my entire time to this work.

Enclosed are two diagrams

which show the parts I have measured.

Of Shrimps, I have (b) (c), ~~and~~ Telson, Segment VI, and Telson, measured in 400 Plymouth and 300 Southport specimens. (b) and (c) are measured in 300 Sheerness specimens.

Of Pandalus, characters (b) to (g) are measured in 100 from Plymouth, and 100 from Sheerness: as are also Segt. VI & Telson

Of Palaemon characters (b) to (g), and Segt. VI & Telson in 100 from Plymouth.

f49v

By completing all these, a demonstration of the behaviour of I might be made worth something??

Yours very truly

W F R Weldon

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f 50

1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

16th March 1890.

Dear Mr. Galton,

I have nearly put together an account of the curves, with only a reference to the question of correlation.

I shall be in London on Wednesday and shall stay for a few days. — If I should like to take the paper to the Royal Society on Thursday and hand it over to the authorities.

Do you care to look at it first? And if so, may I bring it to you on Thursday? — It is very short.

f 50v

Please send a note to the Savile Club if you can spare me a moment on Thursday. – I shall not be able to receive a reply here before Saturday.

2

About the body segments of the Shrimp.

My measures are all in terms of the body length: so that the sum of the values of the body lengths which I obtain is constant.

The problem which puzzles me is therefore this:

f51r

(1) Let there be a number of bodies of constant length, each made of eight segments.

(2) Let each of these segments vary according to a curve of Error, and let it be possible that the Radian values of all the segments occur associated in one individual, so that $n_1 + n_2 + - + n_8 =$ the body length.

What is then the relation between the variations of the segments?

They cannot surely in this case be independent?

f51v

I quite see the force of what you say when

(1) the segments are assumed to be independent variables, and

(2) the body length is supposed variable.

Yours very truly

W F R Weldon

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f52r

1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

15th April 1890.

Dear Mr. Galton,

I shall be very glad to read your paper, if you will allow me to do so, and to tell you if I understand it.

My paper is, I am sorry to say, down for Thursday next.

I want to ask you about a way of estimating things which cannot be measured.

I have been trying to find a case of an animal in which variation, in a given direction,

is known to be taking place.

Lankester, and other people to whom I have spoken about the Shrimp, seem astonished that variations of equal extent but in opposite directions take place equally often. They say that this may be due

(1) to the insignificance of the variations, — so that selection is practically absent; or

(2) to the fact that the creature has already attained equilibrium in each locality, so that the

local average is the best, and there is no tendency to vary in any ~~[illegible]~~ direction at all.

Now a "Royal Native" oyster is generally an oyster hatched at Arcachon, or somewhere near, and sent to Whitstable at a year old to "fatten" for market.

I send you a sketch of such an oyster — it is very rough, because it is only a proof of an unfinished etching: but it shows the points.

You see that the piece of shell round the under, though

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it is ragged from the attacks of parasites, has no radial ridges and furrows upon it. — It is not "filled." | After this portion was formed, the oyster came to white table, & was filled

"Filling" is a character which may be certainly produced in an unfilled oyster by transplanting it to certain localities.

I have a friend here, — Mr. E. H. Fowler, of Oxford, — who became as enthusiastic after reading your book as I did. He wants to estimate the frequency of occurrence of various degrees of filling in oysters (a) which

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f 54r

1, NOE VILLAS.
ELLIOT STREET,
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have been born and bred in a place producing pilled peas, and (b) those which have been for a known time subject to the influence of such a locality, though born of unpilled parents.

Of course any measurement of the depth, extent, and even number of such intangible things is out of the question.

We have tried to devise a means of "marking" the oysters as one marks Tripos candidates:

but the only Tripos lists I have ever opened in manufacturing have not inspired me with confidence in this method.

What can you advise?

2

The new measurements of shrimps, for the correlation tables, are getting on slowly.

I have now races from two places in Holland: and I hope soon to get Mediterranean and Baltic races.

f55r

I hope you have by this time recovered, and that the East winds are not undoing the effects of your rest.

Yours very truly

W. F. K. Welder

Influence of Whitetable
in changing character of
an Oyster Shell.

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f5th

PLYMOUTH
S
AP 15
90

POSTAGE AND REVENUE

Francis Galton Esq.,
5, Bertie Terrace,
Leamington.

f58r

1, Hoe Villas, Elliot Street, Plymouth.
21st. April 1890.

Dear Mr. Galton,

Many thanks for your letters, and for your two papers.

I return the paper on Sexual Selection.

I am not quite sure that I understand exactly your last two terms: as I understand you, you mean as follows:

~~Somatic cell~~ Somatic cell = any cell which by division gives rise to a Soma, — whether this cell be the result of fusion of ♂ and ♀ cells, or a parthenogenetic ovum, or a bud-cell.

Germ-cell = ~~the~~ a cell which by division gives rise to reproductive cells, sexual or a-sexual.

If this be the case, may I suggest the introduction of "~~[illegible]~~ some new term instead of "Somatic cell", — which to my mind at least is associated more with the idea of any tissue-unit? — I think any ordinary histologist would be accustomed to speak of ordinary tissue-elements as "somatic cells", using the term perhaps as an antithesis to "generative cells".

This of course a mere detail, and is probably due to my want of understanding. — But would not γέννημα look rather well in English, and mean nearly enough what you want?

You seem inclined to attribute to the nuclear Chromatin ~~as much importance~~

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the chief function of carrying germinal material: and everyone seems to agree in this view now.

Therefore, though it is perhaps by the way, I wish for my own sake that you would discuss the bearing upon your theory of the formation of "polar bodies".

It is very nice when people say that the chromatin fibres of a nucleus divide longitudinally so that the two resultant nuclei are precisely similar: but this similarity can hardly be supposed to be absolute; and if it be not absolute, it must increase the chance of the absence, in the mature reproductive cell, of one of your germinal species or varieties. So that immediately before going through a process whose object it is to diminish the chance of loss of any one germinal species, every reproductive cell goes through a process which must involve in itself a very serious chance of such loss. — In this connection it is worth bearing in mind that by no means all observers are agreed as to the longitudinal splitting of the chromatin fibres of the egg nucleus before the extrusion of the polar bodies. — Ed. van Beneden, whose work on the egg of Ascaris is most accurate, and whose conclusions on other points are almost all abundantly confirmed by others, believes that, of the original loops formed by the filaments of the egg nucleus, several pass bodily into the polar body and so out of the egg. In this statement van Beneden is confirmed by Carnoy and others.

So that there is a certain amount of evidence [redacted] which goes to show that before maturation there is a selective extrusion of chromatin (= germinal material ?) from reproductive cells.

You know also that a polar body is now said to be extruded from parthenogenetic eggs, before development begins.

In the face of Weismann's contention that the formation of these polar bodies is a deliberate extrusion (1) of [redacted] the germinal matter which incites the egg to maturate, and which is therefore done with [what is to become of the egg of the next generation being left to Providence] — and (2) of a part [what part ?] of the ancestral germinal matter, which is getting too complex, — In the face of this delightful hypothesis, which people seem to have gone quite mad about, I should be very glad if you would add a discussion of polar bodies to your paper.

[redacted]

Are you still interested in finger marking?
After reading your lecture today I made some impressions in a way which might easily and cheaply be used in prisons and so on. — I took a copper plate, covered it thinly with ordinary etching ground, and then painted the surface with turpentine. The ground became sticky : and my thumb left an excellent impression in clean copper, — the ridges removing the ground. Ten minutes

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in nitric acid would of course produce from a plate of this kind a plate from which at least 100 copies could be printed.

Would you care for some prints taken in this way?

=

My little paper excited very little interest in your absence.

Lankester said kind things, for the sake of being kind: no one else said anything

Yours very truly

W. F. R. Weldon

Could you easily tell me how to make a model of a "Surface of Frequency"? – It is a thing for which my reverence grows daily, and I should like to look at it!

Sex & Cross Fertil

M.S. of "SEXUAL GENERATION & CROSS FERTILISATION" by F.G.

together with a letter of Weldon's returning same to F.G. after reading (21.4.90).

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Francis Galton Esq.,
42, Rutland Gate,
London,
S.W.

Sex & Cross Fertil

M.S. of "SEXUAL GENERATION & CROSS FERTILISATION" by F.G.

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Francis Galton,
42, Rutland Gate,
London,
S.W.

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Please return to F. Galton 42 Rutland Gate London SW

A61

I

Sexual Generation x Cross Fertilisation

♀ ♂ ♀

I propose to discuss more fully, ~~and~~ in the light of more recent knowledge, a cause that I pointed out many years ago ( ) of the advantage both of sexual generation and of cross-fertilisation to plants and animals of complex structure, and to give some numerical notion of the enormous magnitude of the advantage.

The supposition on which the calculations are based, is that the process of heredity is essentially particulate. For inasmuch as the peculiarities in minute portions of bodily (somatic) structure admit of being transmitted independently by inheritance, we are justified in looking upon the body (soma) as an organised structure formed through the orderly development of a large number of germinal particles of diverse qualities.

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It is almost needless to point out that the hypothesis of particulate descent is a basis of Darwin's provisional theory of Pangenetics, though unconnected with its larger and more questionable portion from which the name of pangenetics is derived. That (the theory of particulate descent) is to say, it may be held quite independantly of the idea that each cell in the bodily structure, at various stages in its development, throws off gemmules which are sifted out as it were, by the reproductive organs. The hypothesis on which I am about to proceed, does not depend on the way in which the germinal elements may have arisen, otherwise than that they are supposed to be freely capable of self-propagation, each after its kind. It seems impossible to frame an hypothesis consistent with even the plainest facts of heredity, that should not, in the last analysis, be particulate in its character.

f83 ?

1.

In the following remarks, frequent use will be made of the four phrases—

Germinal Material, Germ cells, Somatic cells, and the Soma (or bodily structure). They are to be understood in the following sense.

Germinal particles, whether dispersed, as Weismann's germ-plasm is supposed to be, or otherwise, consisting of very numerous particles differing in species and in variety, self-reproductive each after its kind, but presumably subject to such small variations in descent as are observed in all the living organisms with which we are familiar.

Germ Cells, each containing a large and fair sample of all the germinal particles, which presumably have drawn together in a self formed and loosely organised aggregation.

Somatic cells, each containing a small but fair sample of the various species of the Germinal particles, though they afford a very imperfect sample of their varieties. They are supposed to have combined together in a much more firmly organised* system than that of the Germ cells

The Soma, or bodily structure will be looked upon as a

* foot note

What is now known about the chromatin fibres and of their longitudinal division is highly suggestive of a self-marshalling of germinal particles, and as it were of a roll-calling of the members at an early stage.

as ~~[illegible]~~ a structure of somatic units, each ~~severally~~ derived from a corresponding germinal particle, ~~and~~ ~~which are~~ the particles being developed in orderly succession.

Let α, β, γ, &c stand for the several species of germinal particles ~~material~~, then a perfect ~~germ~~ Somatic Cell is supposed to contain ~~only~~ a few, perhaps in some cases only ~~one~~ a single specimen of each of them; while a Germ Cell is supposed to contain many specimens of each species ~~species~~, and so to afford a pretty fair sample (not only of them but also of) of their varieties. ~~included in each species.~~ Calling ~~Supposing~~ those ~~particular~~ varieties of the α, β, γ, &c species that happen to be ~~are~~ included in some particular somatic cell by the letters α_n, β_m, γ_r, &c, ~~then~~ it is supposed that ~~the Soma corresponding units~~ ~~(will be developed out of them, which we may call in the Soma) [illegible] A_n, B_m, C_r, &c~~ ~~the~~ somatic units, which we may call A_n, B_m, C_r, &c, will be severally developed out of them, & in orderly sequence, and will ~~be~~ form the Soma. Another ~~Soma from another~~ contemporary Soma might consist of the units A_s, B_t, C_u, &c; & similarly differences of variety might exist in others.

It is easy to assign a superior limit to the size of a somatic unit, for it is familiarly known that such small peculiarities as the shape or set of a particular tooth, of a finger nail, of a portion of the ear, & of a swollen frontal vein, all admit of inheritance, therefore the presumed Somatic units cannot be larger than these, and may be exceedingly smaller. Again, it sometimes happens that two ancestral peculiarities are blended in the offspring, then the feature in which the blend takes place cannot be derived from a single particle of germinal material but must come from at least two particles. Consequently the feature in question must be compounded of at least two somatic units. The inheritance of an ancestral peculiarity in a diminished degree does not seem to throw light on the question, as it may be wholly due to "Regression towards Mediocrity", which according to my view of the descent of the germinal particles, being subject to small variations, under similar conditions & those that prevail in known organisms would sufficiently account for the diminution.

Thes beg premised, it follows that if a Somatic cell does not happen to include a single representative of some one or more of the various species of germinal particles that have been called α, β, γ, &c., then it cannot possibly develop into a perfect Soma. If the missing species are of fundamental importance, it could not develop at all. A particular variety of α which we may call α_1, might possibly but rarely change in a single bound to another variety α_2, but it is supposed impossible for an α to become a β in the course of a single (or even in any moderate number of) generations; therefore if an α is lost, the deficiency is almost certainly perpetuated in the offspring, & the cell is not thereby rendered wholly incapable of development.

It is easy to imagine reasons why a species of the germinal particles in the parent might be absent from its filial somatic cell. The whole species might have died out of the parental germ cell as plants (the whole of some species of) may die out of a garden, or it may have failed to be included in it owing to an accidental hindrance. Or again, if included in the parental germ cell it might fail to appear in the filial somatic cell (for) either of the above causes.

~~The origin of the chance~~ The reason why any given species of germinal particles may chance to be ~~being~~ absent from a given somatic cell, does not concern our reasoning; it is only the value of the chance that is important. Let this value on the average be $\frac{1}{r}$, estimating it in the case of a cell of asexual origin at the moment before those consecutive fissions begin in the cell with which the life of a new individual commences.

To facilitate the preliminary calculation, two arbitrary suppositions will be temporarily made. Afterwards, the error they might introduce will be considered. They are, first, that the chance of absence is the same for all the species, ~~the~~ being $\frac{1}{r}$ for α, for β, for γ, &c, severally. Secondly, that the species exist ~~character are~~ so independently ~~that~~ of each other that the absence of any one of them has no sensible effect on the probability of the absence of any other. Neither of these suppositions can be correct, because some of the species, ~~are~~ germinal particles must be hardier or more prolific than others, and therefore more persistent in their appearance, like weeds in a garden, and again there is much evidence of correlation. Still it will be seen later on that the two arbitrary suppositions, and others that will have to be made, cannot lead us into ~~exceedingly~~ a greater error than we may ~~safely~~ lightly dispense with.

Asexual Generation. Starting from this ~~following~~ ~~simplified~~ basis = that there are n species of germinal particles and that the chance of the entire absence of any particular one of them from (of asexual origin, and at the moment before fission commences) a somatic cell is $\frac{1}{r}$, we proceed as follows.

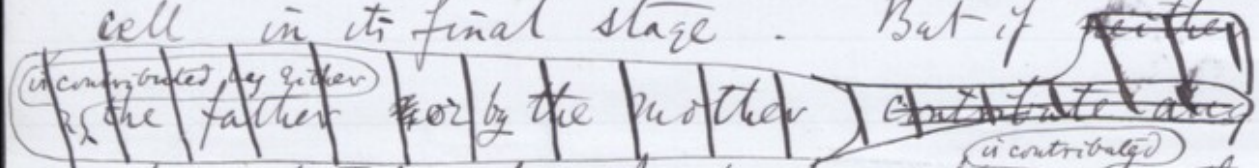
The chance of the non-absence, that is of the presence of one or more germinal particles of that species is $\frac{r-1}{r}$, and the chance of the ~~absence of presence in every one of the n~~ presence of one or more germinal particles of every one of the n species is $\left(\frac{r-1}{r}\right)^n$.

or as 1 to $\left(\frac{r}{r-1}\right)^n$, say as 1 to y

For brevity sake we will ~~hereafter~~ use the letter X to stand for the words "asexual origin"

Sexual Generation. Before the somatic cell that is derived from two parents reaches the stage spoken of when fission commences, and which was in the last paragraph, and which may be called for the moment the final stage of the cell it has passed through an initial stage in which contributions from both the father & the mother were united. Out of their (taken) abundance of duplicated material a more compact system of particles organises itself to form the contents of the same somatic cell at its final stage. If each of the n species of particles is duly represented in both of the parental contributions, the somatic cell in its initial stage will contain n couples of contributions, each couple consisting of (two batches of particles or) at least of two particles of the same species, the one derived from one parent and the other from the other. If either member of the couple is present though the other may be absent, there would still be sufficient variety of material

for the complete equipment of ~~the~~ a somatic ~~cell~~ cell in its final stage. But if ~~neither~~ no representative of ~~any~~ one particular ~~species~~ is contributed by either the father or the mother to the somatic cell, then that cell must be incomplete.



Assuming, as before, that the chance of the entire absence of any particular species from the contribution of the father to the somatic cell in its initial stage, is $\frac{1}{r}$, and similarly as regards the contribution of the mother, it follows that the chance that neither of them will contribute a representative is $\frac{1}{r^2}$. Consequently the chance that either or both will contribute at least one representative of any specified species is $\frac{1-r^2}{r^2}$, and the chance that either or both will contribute a representative of every one of the n species is $\left(\frac{r^2-1}{r^2}\right)^n$.

Thus the chance of an asexually descended germ being perfect may be taken as $\left(\frac{r-1}{r}\right)^n$, and that of a sexually descended germ as $\left(\frac{r^2-1}{r^2}\right)^n$. Or we may write these two chances respectively as 1 to $\left(\frac{r}{r-1}\right)^n$, and as 1 to $\left(\frac{r^2}{r^2-1}\right)^n$.

The value of n must differ greatly in different plants and animals. In so complex a structure as that of man, we may be confident that its value ought to be reckoned in thousands rather than in hundreds, and probably in tens of thousands rather than in thousands. The value of r is ~~probably~~ perhaps to some degree connected with that of n, because the chance of a germinal particle being absent from a system ~~of~~ in which the particles ~~were~~ are very numerous, is likely to be greater than in one where they ~~were~~ are few; for we ~~might~~ would expect ~~in short, it is likely that they would~~ them to be less strictly coordinated. The value of r would no doubt differ in different plants & animals, but it is not unreasonable to suppose that there are many cases in which it would lie between 100 and 1000.

Tables I and II are constructed for values of r and n within the limits indicated above. The upper part of Table I shows the chance of completeness of a cell of Asexual origin, the lower part shows that of one of Sexual origin. Only the two first figures of the result are regarded. Table II gives the number of times that the chance of completeness of a Sexually derived cell exceeds that of one that is of Asexual origin. Only the first figure of the result is here regarded. The values are those of $\left(\frac{r}{r-1}\right)^n$ divided by $\left(\frac{r^2}{r^2-1}\right)^n$.

It will be seen that the advantage of the cell of Sexual origin is enormous, under circumstances that we may reasonably suppose to be of no uncommon occurrence. Thus when $r = 100$ and $n = 10{,}000$ the advantage is so inconceivably large as to require a row of no less than 44 figures to symbolise it. We have the most ample margin in such cases as these, to allow for all the facts that were ignored in the calculation. Corrections on these grounds however large they may be within the bounds of reason, leave the measure of the advantage still so great as to be far beyond the utmost stretch of the powers of distinct imagination.

It will be noticed in the upper division of Table I, that when $r = 100 = n$ the chance of completeness of an Asexually derived somatic cell is as 1 to 2·7; again, when $r = 1000 = n$ the value of the chance is the same. It is always to whenever $r = n$, if they are sufficiently large to allow of the neglect of all values of r that

These are (1) the probable dependence between the absence of one species of particle and the presence of others that crowd it out; (2) the questionable identity of the value of r in the initial stage of a Sexually derived cell $\times$ in the finally stage of the one of Asexual origin; (3) a possible connection between r and n, and lastly, (4) the probability that r is not the same for all species of particles. They are undoubtedly serious omissions, but

are raised to the power of $r-1$, or less, in comparison with those that are raised to the power of r. In this case the expansion of $(r-1)^r$ becomes

$$r^r\left\{1-1+\frac{1}{2}-\frac{1}{6}+\frac{1}{24}-\frac{1}{120}\ \&c\right\} = r^r \times 0.367$$

and $\left(\frac{r}{r-1}\right)^r$ becomes $\frac{1}{0.367} = 2.7$ (omitting the 2nd & subsequent decimals)

As the chances of completeness of the ~~corresponding~~ sexually derived cells having the same values of $r=n$ differs little from certainty, we may say broadly that when $r=n=$ some considerable sum (say greater than 50), out of every 27 of the former cells 10 will succeed, and out of every 27 of the latter cells all will succeed. It follows that in all such cases the relative chance of completeness of the former cell to that of the latter ~~an S cell~~ is practically constant, and ~~[illegible]~~ of the value of 1 to 2·7.

We thus see that no very great advantage is gained by ~~accrues t~~ Sexual Generation unless n is much greater than r; in other words, unless the plant or animal is of complex structure. When $r=100$ and $n=1000$, the advantage has already risen to be at least 2000 times as great (the

~~two~~ first significant figures ~~one~~ being alone given here as elsewhere in this Table), and as n increases still further the advantage of the ♂ (sexually derived) cell increases with vastly greater rapidity in geometric ~~progression~~ ratio.

It is useless in our present ignorance of details to present more elaborate tables. Those that are here given suffice to prove the main thesis, namely that when structure is not complex the advantage of sexual generation is too small to be worth regarding; but that as its complexity increases, the advantage increases with extraordinary rapidity ~~until it reaches~~ to amounts that ~~we~~ require long rows of figures for their expression.

The above remarks apply with no great modification to the benefits of cross-fertilisation. The risk of the absence of one of the α, β, γ, &c species from a Somatic cell was described as being compounded of two risks. First, that the species in question was absent from the Parental Germ Cell; secondly, that, if present there, it failed to be included in the Filial Somatic Cell. It is with the first of these risks that we are now principally concerned. Let the Parental Soma of an asexual plant or animal be called P, and one of the contained Parental Germ cells G. Suppose G to be poorly supplied with, or wholly deficient in, all representatives of the α species, then the Filial Germ Cells G_1' G_2' G_3' will be similarly ill equipped and so, probably, will be the Filial Somatic cell. If the α particles have wholly disappeared from any of these G_1', G_2', G_3', &c, their race cannot be replaced. Consequently their own Filial Somatic cells, and their own Filial Germ cells will be radically imperfect. After awhile the chances will fall against another species, say β, & the β particles will die out, and so on with the rest. So the race would soon become seriously defective and must cease. If the α cells did not wholly disappear but were only very deficient in number, it may be that their deficiency

was caused by the environment; if so by a continuance of the same environment the race would deteriorate, or by a favorable change of the breed of the α particles might expand and the mischief be repaired. Or else the deficiency may have been caused by some passing affection, which when it had passed by, would leave the α particles free to multiply to their normal proportions in respect to the others, and thus ~~repair the mischief~~ to recuperate the loss.

~~I would take this opportunity of again pointing out that~~ It is needless to dwell on the very obvious relation of this reasoning to the asserted tendency of some marked peculiarities, whether they be morbid or otherwise, to skip over one or more generations. The Somatic cell may have been over abundantly supplied, to the prejudice of the contemporary Germ cells. (reference)

I

Chance of completeness of Somatic Cells of Asexual and of Sexual origin, for various values of r and n

The table is to be read thus – when $r = 100$, $n = 500$, the chance of the completeness of an Asexual cell is as 1 to 1.5×10^2, that is as 1 to 150 (all figures after the first two being disregarded). When $r = 100$, $n = 1000$ the chance is as 1 to 23000.

	n	r = 100	r = 200	r = 1000
Asexual Origin, Chances of completeness as 1 to $\left(\frac{r}{r-1}\right)^n$	100	2.7×10^0	1.6×10^0	1.1×10^0
	500	1.5×10^2	1.2×10^1	1.7×10^0
	1000	2.3×10^4	1.5×10^2	2.7×10^0
	5000	6.7×10^{21}	7.7×10^{10}	1.5×10^2
Values of $\left(\frac{r}{r-1}\right)^n$	10 000	4.4×10^{43}	5.9×10^{21}	2.2×10^4
	50 000	1.7×10^{240}	7.0×10^{108}	5.3×10^{21}
	100 000	3.1×10^{436}	5.0×10^{217}	2.8×10^{43}
Sexual Origin, Chances of Completeness as 1 to $\left(\frac{r^2}{r^2-1}\right)^n$	100	1.01×10^0	1.003×10^0	1.0001×10^0
	500	1.05×10^0	1.01×10^0	1.0005×10^0
	1000	1.1×10^0	1.03×10^0	1.001×10^0
	5000	1.7×10^0	1.1×10^0	1.003×10^0
	10000	2.7×10^0	1.3×10^0	1.01×10^0
Values of $\left(\frac{r^2}{r^2-1}\right)^n$	50 000	1.5×10^2	3.5×10^0	1.05×10^0
	100 000	2.2×10^4	1.2×10^1	1.11×10^0

II

Table showing the number of times that the chances of completeness in a cell of Sexual origin, exceed that in one of Asexual origin, for various values of r and n.

n	r					
	100		200		1000	
	First figure of result	Number of subsequent figures	First figure of result	Number of subsequent figures	First figure of result	Number of subsequent figures
100	3	0	1	0	1	0
500	1	2	1	1	2	0
1000	2	4	2	2	3	0
5000	4	21	7	10	2	2
10 000	2	43	5	21	2	4
50 000	1	216	2	108	5	21
100 000	1	432	4	216	3	43

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f79r

1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

14th May 1890.

Dear Mr. Galton. Forgive the long delay in the preparation of the correlation curves. The measures are practically finished, — 1000 Plymouth, 400 Southport, 300 Sheerness.

Unless you feel anxious to see the results very quickly I will not do arithmetic by daylight: because I want very much to do some anatomical work. By the help of Crelle,

f79v

the arithmetic will not take many evenings!

I want to ask you about a possible experiment with the Illustraria, — of which I am allowed to rear another set.

You remember that though the ratio between the races is the same whether the creatures are reared by Miss Pridham, by Mr. Merrifield, or by myself, yet the absolute size of each

race varies. (mean)

Call the size of Mr. Merrifield's A race = Am.

I receive eggs [redacted] whose inherited tendency shall be to vary about Am as a Median.

The resultant moths vary about [redacted] something else, = Aw, as a Median.

It appears that the offspring of my moths, reared at Brighton, vary again about Am.

Therefore the increase of size, causing the median

value of the race to rise from
Am to Aw was not
inherited.

This seems a very typical
instance of an "acquired"
character.

Would it be worth while
to devote a few spare pairs
of one sort to the foundation
of a race which should live
for several generations here
in Plymouth, and should then
be returned to Brighton, — in
order to see through how
many generations the

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[14·5·90] f81r

1, HOE VILLAS,
ELLIOT STREET,
PLYMOUTH.

External conditions can act without producing an inherited change?

I shall be here till the end of this year. = 2 generations: and I can easily find some one at the Laboratory who will take the following generations.

Yours very truly

W. F. R. Weldon

P. T. O.

f 81v

I believe I have not yet
thanked you for your kind
congratulations on the action
of the Royal Society.

f82r

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1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

16th June 1890.

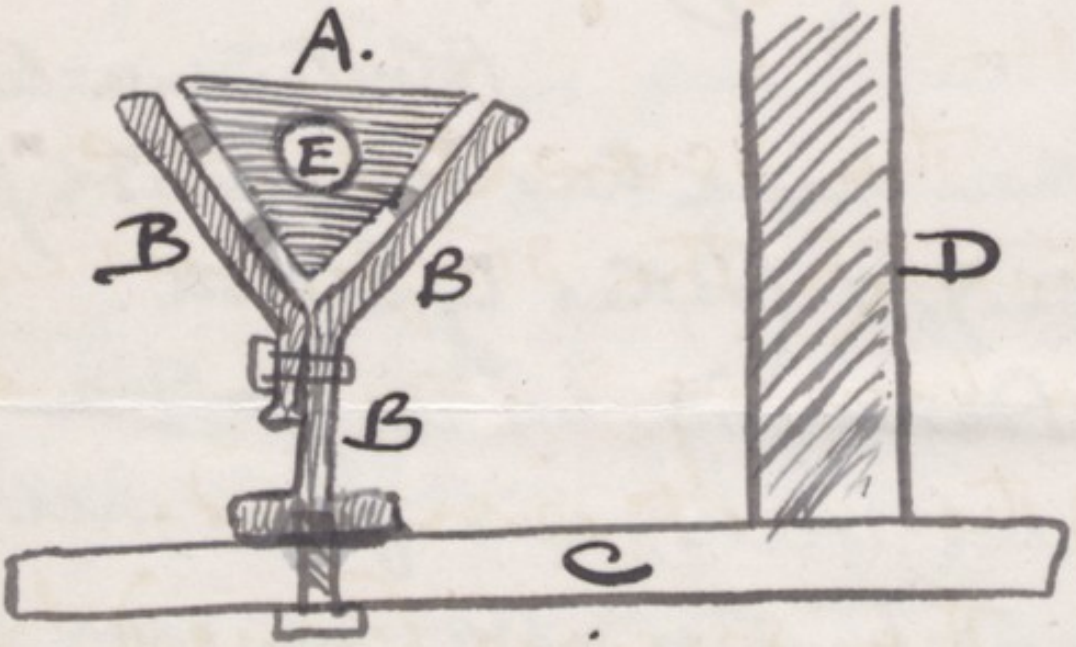
Dear Mr. Galton,

A better machine than mine has been made for Dr. J. H. Baxter by Elliott.

Baxter will be in London next week; and if you will send him a note, he will bring the machine to you, where you like.

In this machine, the microscope is fixed: and a trap stage, travelling on

a screw of 1mm. pitch, carries the object.



the stage (A) is a 3-sided prism of copper, about 3 in. long, and 1½ in. wide on its top side. this prism runs on three parallel rails in a V shape

groove of cast iron (BBB) the groove being contained in a solid casting, Y shaped in section.

The screw (E) runs through the stage, and works in bearings at each end of the cast iron bed.

The screw is turned by a large wheel, whose circumference is graduated into 100 parts (one division is about 5 mm. long).

A small spring pieces

against the wheel, and a notch in the wheel makes a "click" at every revolution. Complete revolutions can therefore be counted by ear, while the eye is occupied with the microscope.

The whole bed of the stage is clamped to a mahogany stand (C), which carries a pillar (P) for a microscope.

The machine works quite well to 0·005 millimetre.

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[Weldon to Galton 16 6 90] f84r

1, HOE VILLAS.
ELLIOT STREET,
PLYMOUTH.

I believe the price of the machine was £9. But of course it was the pistons and if Elliott have kept the models of the castings, a second should be cheaper.

Yours very truly

W F R Weldon

Your caterpillars are doing

much better this year: but
between here and the Colorados,
which are also grazing, there
is no time for arithmetic!

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1, Hoe Villas, Elliot Street, Plymouth.
18th, September, 1890.

My dear Mr. Galton,

I do not know how to thank you for the kind things you say about my paper. You have inspired me with new courage to attack the correlation question. I send you the first results of this new effort: but before beginning, let me ask you to pay no attention to my letter till you have finished your holiday.

The curves of the 1000 shrimps from here agree beautifully with the previous curves, based on 400 individuals.

The (b) and (c) curves came out in the first sample (b) – M = 250·05; Q = 4·53, and in the larger sample (which included the first) M = 499·63; Q = 4·55.

(c) came out at first M = 178·03; Q = 3·58; it is now M = 177·53; Q = 3·50.

This agreement is surely close enough?

The chart of the correlations of (b) and (c) is enclosed. You will see that it extends through nearly 80 grades of the curves, – each determination being based on at least 25 individuals: while those determinations which concern grades between 20° and 80° rest on from 40 to 90 individuals apiece.

For all deviations between 25° and 75°, the straight line r = 0·8 seems to work well enough: which pleases me very much: because, in the first attempt with 400 individuals, you agreed to a determination of r = 0·75.

I do not think that the irregularity beyond 25° from M is due to faulty arithmetic. – You

suggested that such an irregularity might occur.

A thing which is more puzzling than the irregularity in the ends is the tendency of the red crosses to make a line which does not coincide with that of the black. — The mean value of r determined arithmetically is 0·89 for the black, and 0·76 for the red series.

If one takes only deviations within the limits of ± Q in each case, the two arithmetical means agree better.

You shall have the determinations of the other correlations when you come back to Town: and I hope that the whole series will be ready for publication when the Royal Series is ready to hear it.

I think I have the beast you want to do all kinds of things with.

There is a certain prawn, — Palaemonetes varians, — which behaves in a very strange manner.

This prawn occurs from Norway to Asia Minor (I believe). In the South of Europe it lives always in fresh water, being found in most of the Italian lakes: but especially in Trasimene. In the North of Europe it lives always in salt or brackish water.

A German, one Paul Mayer, declares that the Palaemonetes in Naples will not live in salt water: but he is not likely to be right.

Anyhow, there is a certain pond here, whose specific gravity is 1·010 (seawater =

1·025 - 1·029), — which swarms with Palæmonetes. This pond forms the nucleus of a system of canals, making ornamental arrangements round Lord Morley's house: its waters are screened from those of the estuary of the Ilyn by a fine grating: and they are, naturally, protected from poachers and other meddling folk. The only disturbance to which the prawns are subject is that due to one of the bailiffs, who likes to rat them.

Here you have a definite supply of animals of known parentage.

I, during the summer, took gravid females of this prawn, and placed them in aquaria, with water of sp. gr. 1·010. I added fresh water daily, in such quantity as to reduce the sp. gr. by 0·0012 every day: and before the young were hatched the water became quite fresh. The young however hatched, fed, and became ready adults. At this point I unhappily placed some corrosive sublimate solution in the tanks, instead of water, and so the experiment ended. But I have no doubt, from the behaviour of these creatures, that they could be made to breed in water of any density below 1·01. — Adults lived and fed in water whose sp. gr. had been gradually raised to that of the sea.

The change of condition of life in these creatures would be very interesting, of course, in many ways: but there is one especial reason.

You probably know that the ordinary Prawn hatches in a form like that of the Shrimp Larva,

of which I enclose a drawing. You see that behind the mouth parts there are two large branchious legs on each side. These are the first two of the eight pairs of thoracic legs.

The Northern (salt. or brackish-water) races of P. variaus hatch in a condition nearly like that of the shrimps. — they have a third pair of thoracic limbs fully formed at birth. — But the Southern fresh water forms hatch always with six pairs of legs fully formed!

The eggs of the fresh water race are also larger than those of the salt. water forms, and the mother carries a smaller number at a time.

The larvae which I reared in fresh water agreed entirely with those normally hatched in salt water.

I should say that no one has yet established any constant difference between the adults of the Northern & Southern varieties, — perhaps because no one has examined more than a dozen specimens of either race!

Here is a basis for experiments without end: but of course I do not yet know what conditions will increase or diminish the already infinite variability of the creature.

— Like all prawns, it has a rostrum sticking out in front, between its eyes. — I send a drawing of half a dozen, among a sample from Lord Morley's pond.

Can one do nothing with variations

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f79r

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ELLIOT STREET,
PLYMOUTH.

14th May 1890.

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I receive eggs ~~from parents~~ whose inherited tendency must be to vary about Am as a Median.

The resultant moths vary about ~~Am~~ something else, = Aw, as a Median.

It appears that the offspring of my moths, reared at Brighton, vary again about Am.

Therefore the increase of size, causing the median

F80v

value of the race to rise from Am to Aw was not inherited.

This seems a very typical instance of an "acquired" character.

Would it be worth while to devote a few spare pairs of one sort to the foundation of a race which should live for several generations here in Plymouth, and should then be returned to Brighton, — in order to see through how many generations the

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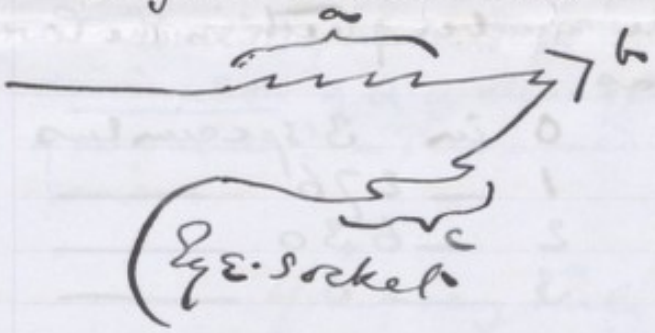
which seem to move by large integers, such as the variations in the teeth of this rostrum?

Of course, the variation does not really move by jumps of whole teeth: but I can't think of no way of expressing it in smaller units.

The variations in 915 specimens are here: —

They are reckoned in this way:



The series (a) are called upper teeth; and (c) are lower. — The tip may be bifid ~~as at (b)~~ as at (b), or simple. But if it is bifid, the upper apical tooth is not reckoned as a dorsal tooth.

So one might make a dental formula for the rostrum drawn, — $\frac{4+1}{2}$, while if the apex were simple, the formula would become $\frac{4+0}{2}$.

Of the individuals examined, the apex was

simple in 432
divided — 483

The teeth on the dorsal surface, counted as above explained, were

f87v

No. of teeth	No. of specimens
1	2
2	18
3	123
4	372
5	349
6	50
7	1

The number of teeth on the lower surface was

0 in 3 specimens
1 — 276 ———
2 — 630 ———
3 — 6 ———

These numbers occurred in all possible combinations, which I will send you if you care to see them.

The results above given, such as they are, seem such as would be obtained by uniting two curves of Error with different M values, — the two curves belonging one to each sex. But I do not see how to take the thing any further.

As soon as the Shrimp correlations are finished, I will measure Palaemonetes, and determine a great many M and Q values for the individuals living at Saltram Park. At the same time, large samples can, if you like, be acclimatised to fresh water, or to salt water, so as to give a chance of broods next

year (broods hatch in June - August).
The creatures are very abundant, so that a supply from Saltram could probably be certainly obtained at any time for control experiments. Each female carries about 100 eggs, — of which half would probably reach maturity. — There may possibly be a spring brood; but if so it is not recorded.

Would experiments on the selective diminution of Q by changed conditions be of no value? — They would be so much easier!

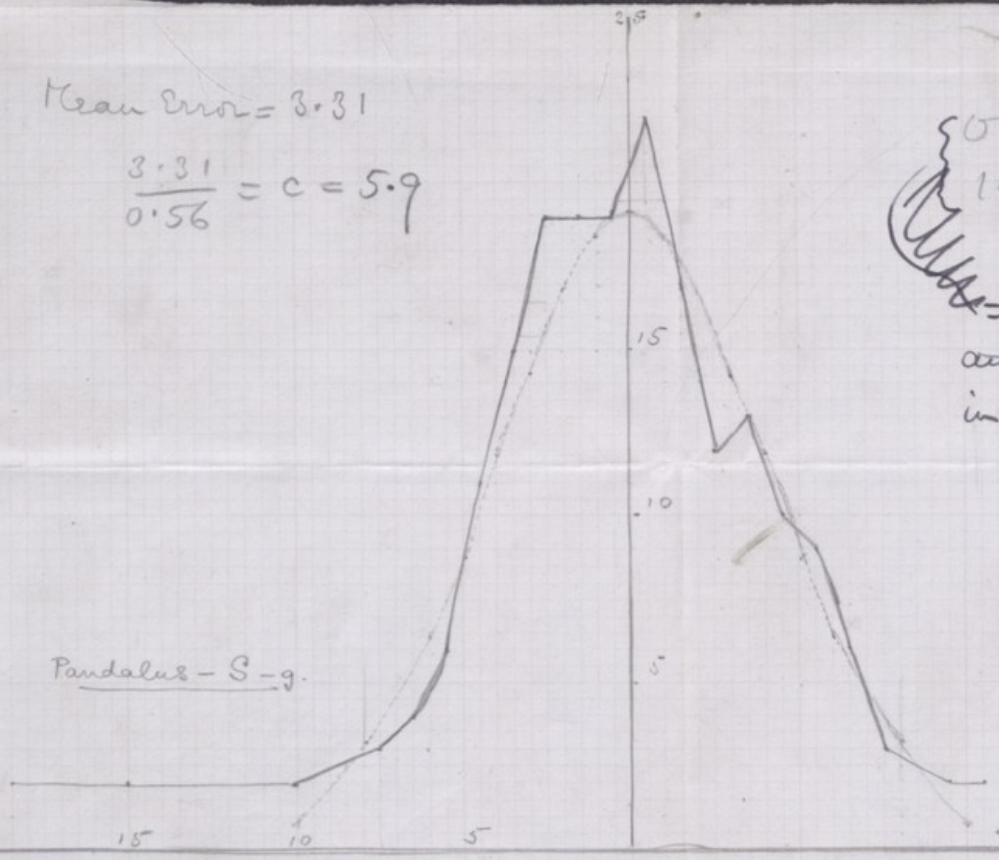
Yours very truly
W. F. R. Weldon

I have sent a copy of my paper to Dr. Bordzilch. — I met him last summer in London for a moment, and found him raising about your Natural Inheritance.

f89

Mean Error = 3·31

$\frac{3 \cdot 31}{0 \cdot 56} = c = 5 \cdot 9$



Outside — 27
Inside — 15

~~= Area between Normal and observed curve, expressed in squares of scale on paper.~~

Pandalus – S – g.

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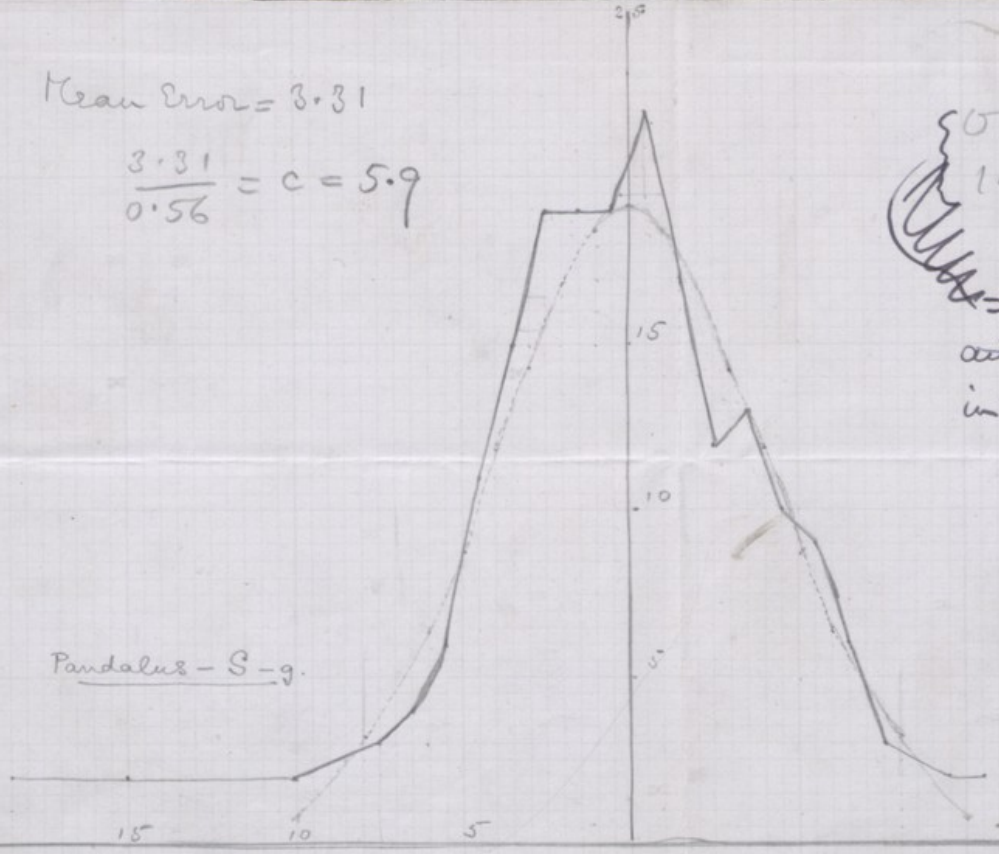

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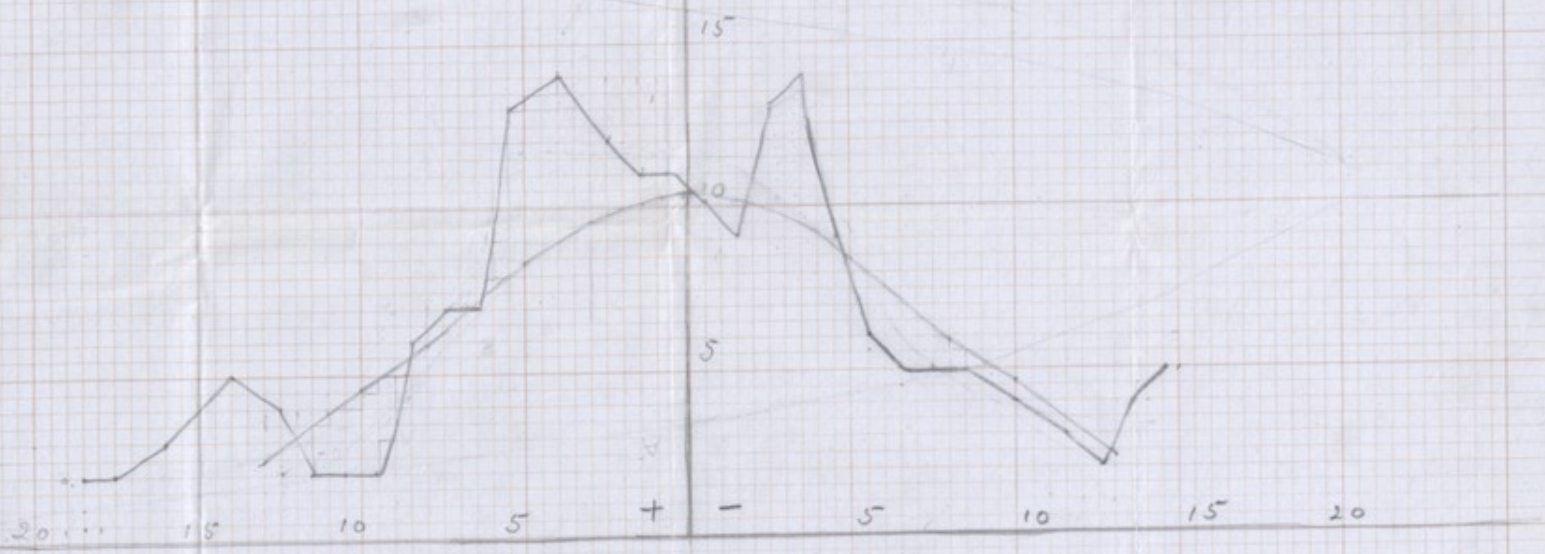
f 30

Mean Inn = 6·01

$\frac{6·01}{0·56} = c = 10·73$

Area within — 53
— outside — 78



Pandalus. Sheerness — (6) 100.

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f 91r

30A Wimpole Street. 1st June 1891.

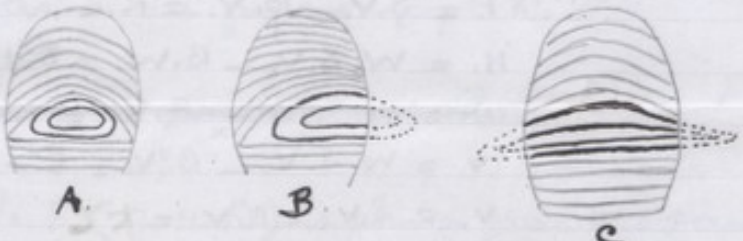
Dear Mr. Galton,

You know how your paper of November on Thumb-marks has troubled one's mind. Your last paper stirred me to think of the matter again. The establishment of isolated "genera" and "species" without sexual selection, and with no apparent selection of other kinds was of course the puzzle: and I have been trying to find a loophole, by which to ~~be~~ creep out of your intrenchments. — You probably have an answer to the following suggestions: but, as you do not answer them in your published paper, I should be very glad if you would let me know what you think of them.

You distinguish your species (a) by pattern, and (b) by the median value of the ratios between the axes.

(a) The Pattern:

In trying to conceive of forms intermediate between loops, whorls, and straight lines, the simplest way seemed to be to consider, a priori, which pattern could, by simple elongation and compression, give rise to the others. The idea which occurred to me was this:



Suppose the general parallel ridges of the thumb to be deranged by an elliptical arrangement, as at A; and suppose this arrangement capable of ~~in~~ elongation in any direction. If the ellipse became elongated to the right, a portion of it (dotted in B) would creep round the thumb, towards the dorsal side, where it might be partially destroyed (by influences to be considered directly) and a loop would be formed. Elongation to the left would produce a reversed loop: and elongation in both

directions would produce the parallel arrangement shown in C.

With regard to the hypothetical destruction or modification of that portion of the ellipse which creeps round the side of the thumb or finger, this seems possible (a) from a simple struggle for room between the smooth, hairy skin of the dorsal side of the digit and the ridged, naked skin of the palmar side, both of which try to grow as much as possible, — compare the struggle between teeth for room in the jaw, etc., etc., — and (b) from the friction between adjacent digits, which in many persons causes continual irritation of the irregular and deformed papillæ which form the transition between the dorsal and palmar systems. Just so are the hairs on the inner surface of one's limbs often broken by clothes.

May I submit the ~~follow~~ enclosed prints, as evidence of the occurrence of something of the kind?

No. 1. is a print of all my right fingers, with very little rolling.

I think you will agree that they fall under your specific heads as follows:—

I. = S.V. — B.V. = k.2. v.

II. = W.S.V. — B.W. = ~~c~~ y

III. = W.S.V. — B.V. = n'(?) = ~~[illegible]~~

IV. = W.S.V. — B.V. = c^3 — m.

V. = S.V. — B.V. = k 2.

(On the fingers, V = thumb side, W. = little finger side)

No. 2 is a better rolled print of my R. thumb. In this the V ~~[illegible]~~ angle is easy: but in points (a) and (b) I have marked what appear to me to be distinct indications of a W. angle, among the disconnected papillæ of the transitional region. In (c) I send an untouched print, by which you can check the pencil marks.

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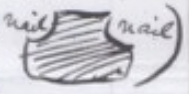
No. 3 contains rolled prints of all my R. fingers (No. IV. twice) in which I have pencilled over the lines which seem to me to correspond to the boundaries of rudimentary angles. I would especially point out ~~that~~ No. II, where the V-angle is very obscure. This obscurity is accompanied by a prolongation of the ridges so far round the finger that they appear almost under the nail in the print (the nail is indicated of course by the excavation on each side 

while on the W side, where the angle is evident, the ridges are not prolonged so far dorsal-wards. In No. III, on the other hand, the V-angle is well-marked, and is well on the ventral side of the finger, while the ill-defined rudimentary W-angle is right under the nail. On ~~the~~ the 5th. finger, where no amount of rolling will produce a trace of a W. angle, the ridges run right onto the dorsum of the finger, and appear under the nail.

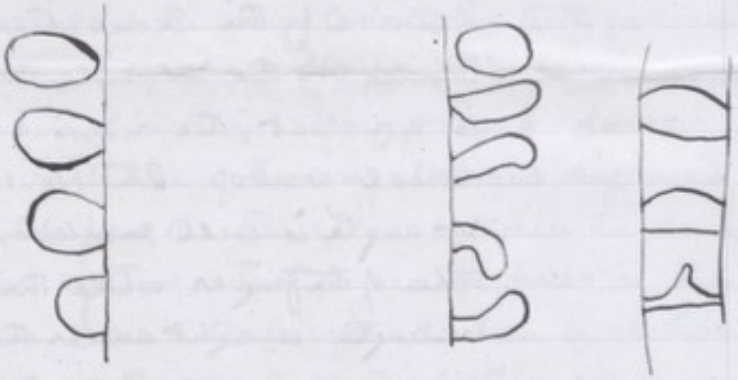
Nos. 4–6 give my left fingers, ~~treated~~ treated in the same way, and exhibiting, as I think, traces of the rudimentary angles in all fingers except No. 5.

No. 7 is from my wife's R. thumb.

No. 8 (G. H. F.) is from a man who seems very nearly to approach the straightened condition which you regard as primary: in these I would point out (1) the marked way in which the pencilled lines converge towards something in the dorsal region, and (2) the position of the disappearing ridges on the dorsum of the finger, well under the nail. This of course is ~~one~~ in accordance with the suggestion of an ellipse pulled out at both ends, as in ⊆ on page 1.

I send these few patterns to illustrate what I mean. You will perhaps know whether such a view is tenable, and will be able to illustrate it, or to confute it, from your own collection, better than from a small number of examples collected by me in a hurry.

You see the importance of this view, if there is any truth in it at all: for a series of intermediate stages would on this view be —



where the vertical line represents the point of union between smooth dorsal and papillary palmar skin: and so your genera run easily into one another.

May I venture to add that the prevalence of your primary type among so highly specialised a people as the Ainos is perhaps in favour of the view suggested?

---

(6) The Axes:

I have not been able to understand how your measurements of axes affect your position as to the separation of the genera.

You find, as I understand you, that the values $\frac{TU}{VW}$ (Fig. 2 of your paper) form a Curve of Error: and that in the same way the values of $\frac{YV}{OI}$ form a Curve of Error: and so on.

Quite so. You seem however to infer from this that each of your generic

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GALTON PAPERS

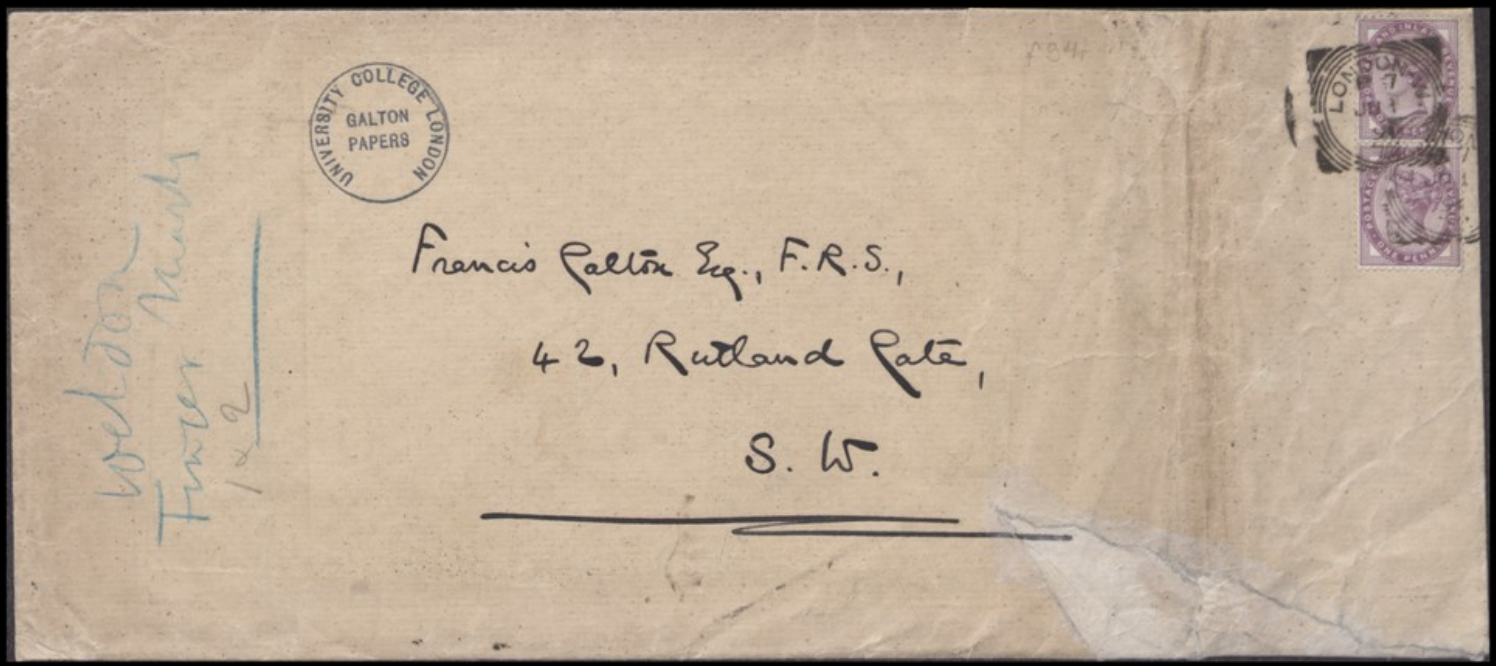
systems varies independently of the others about its own Median. And this I do not see: because there is no evidence that the axes which you measure are homologous throughout: and there is therefore no reason why they should vary about the same Median.

If you take a population of ellipses, and which are subject to accidents causing deformation, and measure a pair of homologous axes through the whole population, the values of the ratio of these axes would of course form a Curve of Error. But if you take several large samples of the population, and measure a different pair of axes in each, you will obtain, as you have done, different curves of error for each group.

I shall be very glad if you will add to mind the trouble of computing these two sets of objections to your views. I have put them forward because I do not want to believe your conclusions if I can help it: but they are so important that I want most sincerely to get a clear opinion about them into my mind: and the only way of getting round your results seems that which I have here suggested. If, from your infinitely greater knowledge of the patterns, you show me that I am simply foolish, I will give up my belief in Normal Selection, and make the best of it. But this process will be so painful that I have felt obliged to plead before you as well as might be, rather than give up at once so important a belief.

Yours very truly

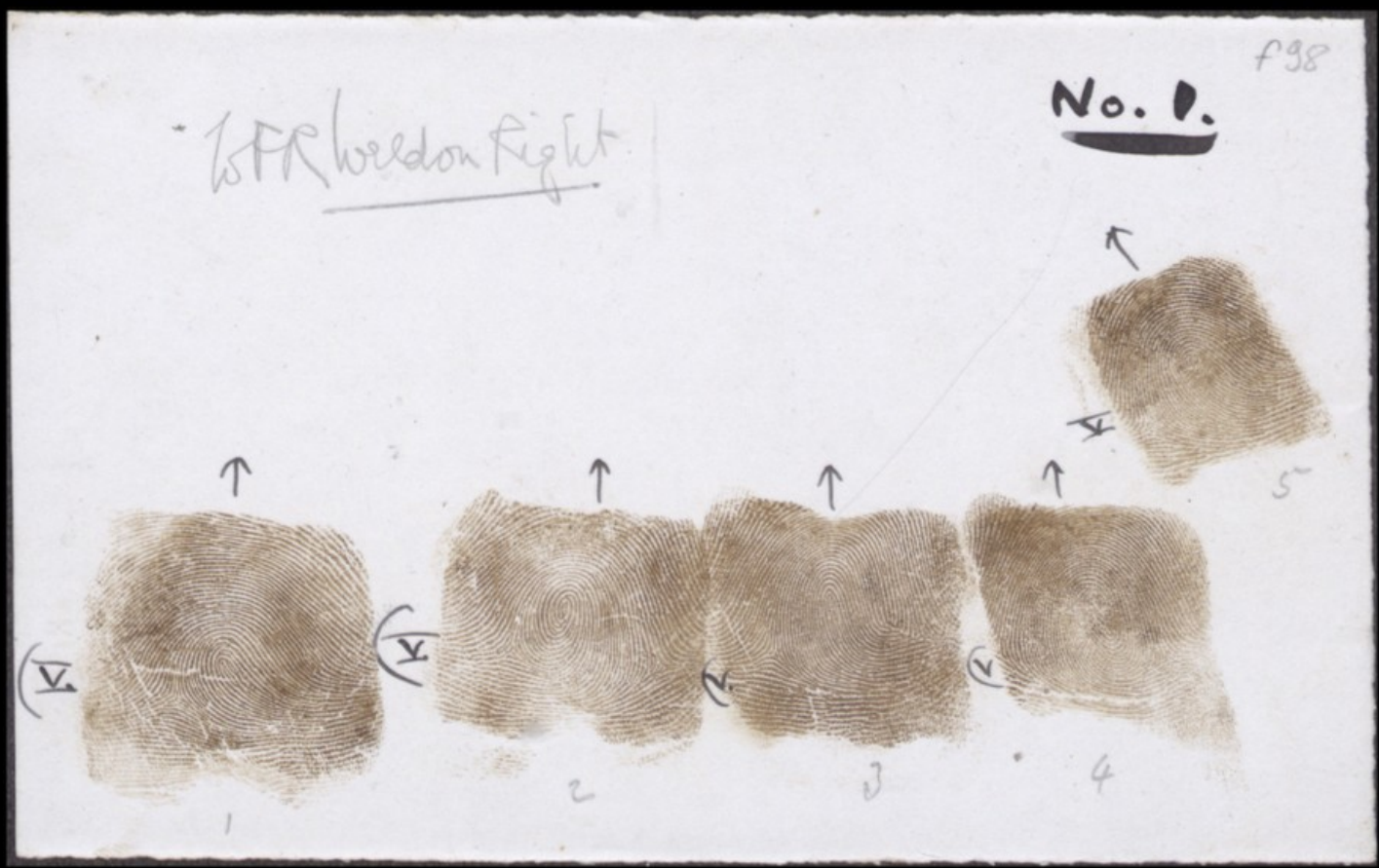
W. F. R. Weldon


UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
Francis Galton Esq., F.R.S.,
42, Rutland Gate,
S. W.

No. 6.

f 96
No. 7.
F. J. W. – Thumb. R.

No. 8.
f 97
Nail
Nail
Nail
W.
V.
III. R.
W.
V.
III. R.


f98
No. 1.
WFR Weldon Right
V.
V.
V.
V.
V.
1
2
3
4
5

f99
No. 2.
Thumb R.
(c)
(a)

No. 3.
f100
IV. R.
W
V
V
W
V
V
W
W
V
V
W
W
V
V
W

f101

WFR Weldon No. 4.

Kft.

f102

No. 5.

WFR &c. Thumb L.

f103
No. 2.

f 104
II R.
I R.
I R.
V R.
III R
IV R. IV
No. 3.

f105

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GALTON
PAPERS
340B

Right Palm.

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f106

30A Grimpole Street.
3rd June 1891.

Dear Mr. Galton, — It is very kind of you to have answered my misunderstandings so fully and so quickly. — I quite understood that the measurements published referred only to loops: but gathered ~~that you had~~ from your abstract in the Proceedings that you had obtained similar results with all patterns.

About the ellipses, — I will not further pretend to question your conclusions: but your letter suggests one or two points.

1. — You, I imagine, regard the patterns as formed by a nuclear disturbance, which pushes apart, as it were, a series of primitively parallel lines. If so, then apart from the whorled or spiral character of the nucleus, potential angles must exist, where the disturbance of the parallel systems ceases, thus:—

I presume that the rudimentary angle under the nail which I have tried to bring out belongs often to this category.

I enclose a print of the palm of my right hand, in which a looped (?) centre of disturbance occurs: and at a and b are angles independent of the nuclear ridges.

2. — In the print of my palm the ridges cross several "folds" in the skin. It seems to me very easy to believe that such a system as that which is interrupted between c and d was primitively continuous across the wrinkle whose position is

indicated by the white streak: yet the floor of that wrinkle, examined with a strong lens, is now quite smooth. Therefore some kinds of constant action can modify these ridges ~~in~~ after ~~they~~ their formation.

3. — You say that the ridges are formed ~~about~~ during the 5th. month. Have you impressions enough to enable you to say that they form at this time patterns like those of adults?

I have made application for all the unborn children obtainable from the U.C. Hospital physicians, and will send you what prints I can get. — For you have of course realised ~~that~~ the possibility that these patterns may undergo a series of changes during development which will indicate their descent.

4. — I am delighted to hear that you propose to discuss and illustrate fully ~~on~~ each of your types and the intermediate stages: because these same intermediate stages are to me, and I fancy to many others, the most fascinating part of the whole interesting mystery. I am especially anxious to learn why you trace your chain of patterns in one direction rather than in the opposite. And I am terribly anxious to know why you feel able to neglect the peripheral parts of the patterns.

The series I enclose is ~~not~~ another attempt to set up a system of the origin of patterns, but a humble exposition of the points which are dark to me.

Nos. 2 and 3 are the right and left hands of a lady, whose fingers cheered me, till I received your letter. For on the 3d. left finger is a distinct nucleus, closed on each side, and very small: and it seemed

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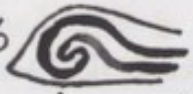
GALTON PAPERS

to me that by elongation of this the pattern on her 2nd. Right finger was obtainable: and so by enlargement a large loop might be formed. This is however now no longer of interest. But the following is still to me difficult.

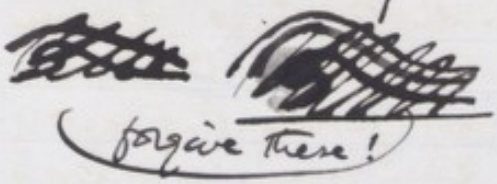
My R. thumb is like this:

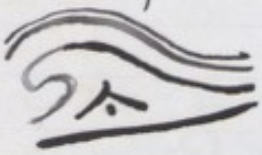
My L. thumb is like this:

Now, what do you call the L. thumb?

If you print it so as to get the whole impression drawn, it is conceivably a loop bent upon itself  ; and the whole nuclear figure is derived conceivably from a loop like that of the R thumb.

But the same pattern may be printed





In which case there is no obvious relation between the simple loop of the R. thumb and the crozier-shaped loop (not ridge simply) of the L. thumb: but on the other hand, there is apparent evidence of a relation between this pattern and your species g, while the rolled print belongs to w.

I only suggest this as a difficulty which occurs to me. — Is it not just possible that intermediate stages may be overlooked by taking only prints of part of a pattern?

This question especially occurs with

reference to crozers. For if a crozier is simply a complication of my left thumb, - i.e., of a twisted loop, its descent from a simple loop is easy: but the origin of a simple spiral ridge seems to me harder.

I only suggest these things as points which appear hard to an outsider, and which I am waiting for your book to clear up.

Yours very truly

W. P. Pycraft

I am glad you liked the prints. They were made with ordinary brown printing ink: and the fingers were inked with a "dabber" which had itself been dabbed on a thinly inked plate.

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GALTON PAPERS

Weldon

f108

30A, WIMPOLE STREET, W.

29th October 1891.

Dear Mr. Galton,

I hope to send you, in a little while, detailed tables of the correlations of which I spoke to you this morning in the Senate House.

The organs are the four which I had previously used in the Shrimp: and the rough figures for the relation a:b = b:a are at present

No. of individuals

Plymouth race (1000) — 0·84
Roscoff (Finistère) (500) — 0·88
Southport — (400) — 0·83
Sheerness — (380) — 0·85

The values obtained for each deviation clustered about the line $r = 0.8580$ well that I thought it worth while to determine the second place of decimals by taking the arithmetic mean of all observed ratios in each case.

Between characters (6) and "Telson Length" the ratio is

Plymouth —— 0.25

Roscoff —— 0.29

Southport —— 0.30

All the other values are, in the Plymouth race, so small, that

I have not thought it worth while to determine them in the other races at present, because of the small number of individuals in each sample.

But I have just obtained, and nearly measured, 400 additional shrimps from Southport: so that I hope soon to have a set of 800 measures of this race, which will give a fair basis of comparison with the 1000 from Plymouth.

When these preliminary determinations are finished, I hope to determine a reasonably numerous set of constants for homologous organs in one or two species. — An exhaustive

f 109v

student, to whom I have preached you, has already undertaken to measure 20 organs in each of 1000 Persons.

Yours very truly

W. F. R. Weldon

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(Prof Weldon) f 1105

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

30A, WIMPOLE STREET, W.

9th November 1892.

Dear Mr. Galton,

If I have not before now thanked you for your kindness in sending me a copy of your "Finger Prints" it is because I have spent all my spare time since it arrived in reading it.

One of my pupils is conducting an experiment, which will interest you, on the effect of breeding from one portion only of a race of variable individuals.

He is taking an infusorian, which reproduces normally about once a day, and which is large enough to be easily dealt with, although a population of

several thousands will live comfortably in a test tube.

Such a population is suddenly submitted to a poison, or other harmful influence, which kills a large percentage. The percentage of survivors is counted, and these are then replaced in normal surroundings, and allowed to multiply for several generations. When the population is again large, the same harmful influence is again applied: the survivors are counted, and replaced in healthy surroundings, as before.

The nice point about the attempt is that the population normally doubles itself every day: so that one can in a

f111

reasonably short time deal with large numbers of individuals and of generations.

Another point is that within certain limits it is possible to ensure the reproduction of the population either by purely asexual budding, or by a regularly recurring sexual union of individuals.

Don't you think that there are possibilities about this?

Yours very truly

W. A. Weldon

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Prof Weldon f 112 5

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

30A, WIMPOLE STREET, W.

27th November 1892

Dear Mr. Galton, I enclose a diagram which may interest you, because it may arise from observation of a "sport."

The inked curve was obtained by plotting 1000 measures of an organ of certain crabs from Naples. (The measures are in units each = $\frac{1}{1000}$ of the length of the body.)

This curve was evidently not approximately symmetrical: so apart from any arithmetical analysis, I tried to draw inside it two "Curves of Error" whose sum might represent the observed distribution fairly well.

The result is shown in the diagram,

and I think you will agree that it fits the observations fairly well.

Having drawn these curves, I calculated from the tables the population represented by each.

The larger curve, whose mean is at 653·5° of the scale, represents a population of 691·9, with a "modulus" of 4·5 units of the scale.

The smaller curve represents 309·7, with a "modulus" of 5·0 units of the scale.

So that these curves, fitted by eye to the observations, represent ~~1001·6~~ 1001·6 individuals, which is surely the same as 1000?

Again, the arithmetic mean (length) of the organ, as distributed among the

population represented by these two curves is 644·8 thousandths of the body length. The A.M. of all the measurements is 644·7.

Therefore either Naples is the meeting point of two distinct races of crabs, or a "sport" is in process of establishment.

You have so often spoken of this kind of curve as certain to occur, that I am glad to send you the first case which I have found.

Yours very truly

W. F. R. Weldon

Weldon

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f 114c

30a, WIMPOLE STREET, W.

14th July 1893

Dear Mr. Galton,

I enclose a list of 23 n values in each of two races of Crabs, - one from Plymouth, the other from Naples.

I should very much like to know how they strike you. — It seems true that the corresponding values in the two races are so nearly alike that one cannot, in view of the relatively large errors of each determination, say that the inequalities are real: - at the same time, one can hardly say that the equality is real.

Such as they are, I propose to

f114v

send them to the Royal Society as soon as may be: and in writing about them it will be convenient to give it a name. — May I call it "Galton's function", so as to emphasize my indebtedness to you?

Yours very truly

R A Fisher

f115r

Marine Biological Laboratory
Plymouth

30a, WIMPOLE STREET, W.

14th July /93

Dear Mr. Galton

As I told you, I hoped to measure the selective death-rate in Crabs during this summer. I have begun to do so: and the results are extraordinary.

In Organ No. 5, the "mean error" is as follows:

"adult" females, (over 35 mm. long) } —— 2·74
1080 individuals }

females 8–9 mm. long } — 2·58
(324 individuals) }

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f115

females 7–8 mm. long
(204 individuals) — 2·44

And apparently, from a few measures, the mean error in animals between 6 and 7 mm. long will be still less.

In 110 females between 9 and 10 mm. long already measured, the mean error is 2·86.

Of course, these numbers are not yet reliable, and many more measures will have to be made. — I have to measure the little crabs at random, as they are brought to me, and to sort them afterwards: that is why so many imperfect

determinations are here instead of one reasonably good one.

Still, there seems distinct evidence that the error of distribution about the mean is at first small: that it increases during growth; and, if the 110 individuals between 9 and 10 mm. are to be trusted, there will be a period at which the error of distribution is greater than it is at maturity.

Is not this a very revolutionary result? It seems to me so extraordinary that I send it to you at once, in its crude condition.

Of course, more measures shall be added as quickly as may be, and you shall know the result; but the final result can hardly be the continuous decrease of error which

I had hoped to find!

f116v

The other organ which I am measuring gives a quite similar result!

Yours very truly

W. F. Weldon

UNIVERSITY COLLEGE LONDON
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Fechner's law cannot affect my own error of measurement, so far at least as I can see, because I use a microscope with cross wires, and make the crab (always of course much larger than the field) travel by means of a screw till its proper points come successively under the wires. Then I read the screw head.

f 117r

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30a, WIMPOLE STREET, W.

26th June 1893

Dear Mr. Galton

Many thanks for the very interesting supplement to "Finger Prints." Surely even a British jury would be convinced by your beautiful pictures! Surely the slight imperfections in the original prints, which you deplore, add really to the value of the records, because they show, in a way which seems true wonderful, how well even unskilful printers

f117v

may collect evidence of this kind?

Yours very truly

W F R Weldon

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GALTON PAPERS

f118

30a, WIMPOLE STREET, W.

7th November 1893

Dear Mr. Galton

Many thanks for your kind letter. — I have made the correction which you suggest in my proof.

I have not answered you before, because I have had to spend every spare moment, during the past three weeks, in looking after a sick relative in the country. — To do this, and to talk to my students for an hour every day has ~~left~~ been enough to make me neglect all my other duties, I fear.

If you will send me your

f 118v

correlation machine I shall be very grateful. — I shall not count it for three weeks, because I am trying to explain the method of arriving at the simple curves of error, and the meaning of the constants in it, before doing anything else. — It is probably very useless for the listeners, but it is very good for me!

Yours very truly

W. F. R. Weldon

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f119c

30a, WIMPOLE STREET, W.

4th December 1893

Dear Mr. Galton

I am sorry I was in a condition of such abject folly when you called yesterday. — Now that I can think a little, I remember that the herring is the very coast of which you are in search.

It is said to be divided in Kiel into two races, varying about the types shown in the enclosed outline.

The two races are found in varying proportions through the Baltic, the North Sea, and the Channel.

A similar dimorphism is said to

be visible in the Sprat.

=

I am writing to ask people to meet on Saturday at 3·0, as you suggested: but at the Savile Club, 107 Piccadilly — Because I have several times asked for a room at the Royal Society on Saturday, and Rix has pointed out that it deprives the attendants of their half-holiday.

There is a room in the Savile which is quite quiet, and where we can talk without disturbance.

=

I return Mr. Wallace's letter. —

=

Will you do what is necessary in

f 120

the matter of writing to the Council of the Royal Society before Thursday. if it is necessary to do anything?

Yours very truly

W F R Weldon

I sincerely hope that I did not infect you with my abominable cold yesterday!!

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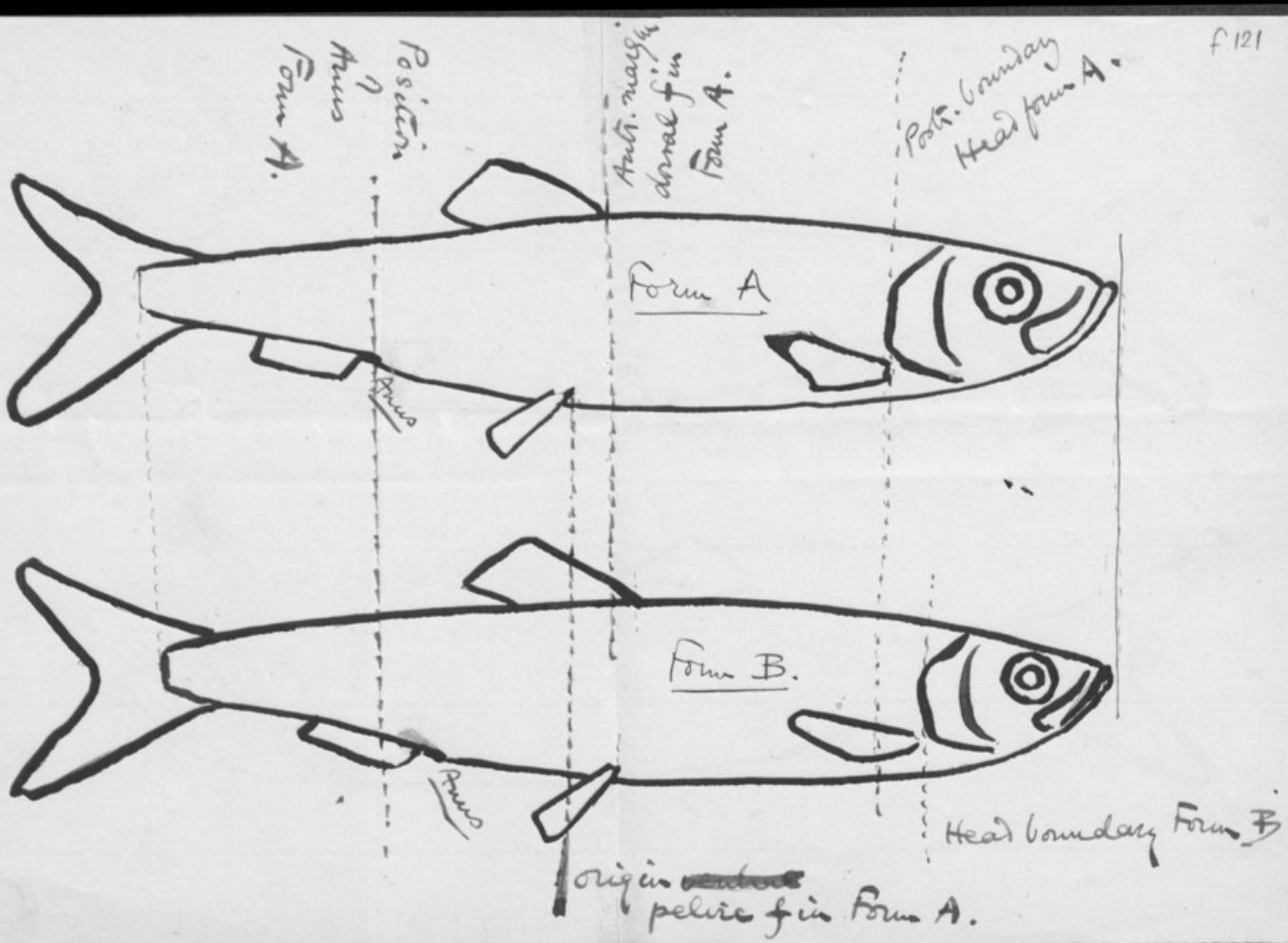
f 121


Position of Anus Form A.
Ant. margin dorsal fin Form A.
Post. boundary Head Form A.
Form A
Anus
Form B.
Anus
Head boundary Form B
origin pelvic fin Form A.

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f 122 5

30a, WIMPOLE STREET, W.

(Monday)
18th December 1893

Dear Mr. Galton,

On Saturday or Friday (16th) I received a postcard from Darwin, agreeing to the proposals of a Committee, and making no suggestion as to addition or alteration.

This morning I telegraphed to MacAlister and Poulton, who have just telegraphed back their assent.

I hope these things will be in time!

Enclosed is a copy of the letter sent to each of the three men.

Is there anything which I can do during the next two weeks? — I

have very short interval between this term and next, and am anxious to go to Munich. — If there is nothing which I can do, I shall leave here on Wednesday evening: so that after Wednesday my address for two weeks will be Baierischer Hof — Munich.

If it is necessary in any way to stay, of course I will do so.

2

Many thanks for Bowditch's work, which I return to you. — The different rate of growth at different grades will be an indefinitely big nuisance in dealing with Selection!

2

f123

The Herring are ordered, and will, I believe, be at University College in time for us to begin work next term.

Yours very truly

W. F. R. Weldon

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Put with letter of Dec 18

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f124r

30A, WIMPOLE STREET, W.

At an informal meeting held on Saturday Dec 9th. present Reps. Galton, Weldon & Weldon it was resolved

That application be made to the Council of the Royal Society to form a Committee, consisting of the undermentioned members.

"For the Purpose of conducting Statistical Enquiry into the Variability of Organisms."

It was also resolved to ask for a grant of money to enable the Committee to obtain material and assistance in measurement and computation.

the members suggested are Messrs.

F. Darwin
F. Galton
~~R. [illegible]~~ A. Macalister
R. Meldola
E. B. Poulton
W. F. R. Weldon

(whom it may afterwards be desirable to add a Statistician.)

It is suggested that one of the first subjects of investigation should be the dimorphism of the Herring, which an adequate supply

f125

can Easily be obtained.

The cost of the Enquiry is Estimated at £120 for which it is proposed to make application Either to the Donations Fund of the Government Grant Committee of the Royal Society.

Please inform me whether you approve of these resolutions or if you have any thing to suggest.

This was sent to Darwin, Macalister + Poulton who were not present but who telegraphed assent.

It seems the origin of the original plants + animals Committee

K. P.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

f126r

30a, WIMPOLE STREET, W.

21st December 1893

Dear Mr. Galton,

I enclose such statement as I have been able to make. — Is it too long? — Please cut it up if you think fit.

I say one or two things about Heincke which are rather necessary for the Council, but hardly fair for "Nature". Also, "Nature" will hardly spare space for outlines on the scale of the enclosed, while the differences will not be very dramatic on a smaller scale?

The Scotch fishery folk, because

one race is predominant at Peterhead, are inclined to laugh at Heincke; and publication of this statement would probably involve one or two replies from them. — This does not matter, of course; but you should be told about it beforehand.

It is some time since I read the Scotchman's paper, and during the last few days I have not been able to find it. — I know that it is insufficient and irrelevant, but I am writing to a man in Plymouth, asking him to send you, for your sake, the exact

f127r

title and reference.

2

By the way, I have some troublesome private business, which prevents me going more than a few hours from home: so I go tonight to Brussels (Hôtel Belle Vue), where any letters will find me during the next fortnight.

Yours very truly

W. R. Wilde

Forgive me for mentioning that

f 127v

you have given me a redundant
l lately. — Please do not do
so in "Nature", because of the
Harrowman.

f128

Hôtel Belle Vue, Bruges

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GALTON PAPERS

30a, WIMPOLE STREET, W.

24th December 1893

Dear Mr. Galton

Your letter is very charming: but it is most unfairly kind to me. — I suppose that as you are writing the letter you can hardly point out that such work as I have done is the logical outcome of your own? ~~but~~ Anyhow, you might cut me out of the first paragraph, so that the sentence stops at "has been recently set forth"?

Yours very truly

W. F. R. Weldon

f 129

POST OFFICE TELEGRAPHS.

No. of Telegram.....

Office Stamp.

SOUTH KENSINGTON B.O. A JY28 94 S.W.

Eyre & Spottiswoode, London.

If the accuracy of an Inland Telegram be doubted, the telegram will be repeated on payment of half the amount originally paid for its transmission, any fraction of 1d. less than ½d. being reckoned as ½d.; and if it be found that there was any inaccuracy, the amount paid for repetition will be refunded. Special conditions are applicable to the repetition of Foreign Telegrams.

Charges to pay } £ s. d.

Handed in at the Western Dist Office at 5 13 p.m. Received here at 5 26 p.m.

TO { Galton 42 Rutland Gate SW

Result sent to you was worthless weldon

UNIVERSITY COLLEGE LONDON GALTON PAPERS 340C

N.B.—This Form must accompany any inquiry made respecting this Telegram.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
2/3/22/17

f130r

30a, WIMPOLE STREET, W.

26th January 1894.

Dear Mr. Galton,

I learn today that I have been made a member of Board F. of the Government Grant Committee of the Royal Society.

I have in my possession a large balance (over £30) of a grant of £50 made to me to buy crabs.

I obviously cannot receive a grant from a committee of which I am a member. What I wish to ask you is this: shall I simply pay the balance which I have or retain back to the Royal Society? or shall

f130v

I ask leave to transfer it to you, as Chairman of the new Committee? — Either course would place the money beyond my control, which is all that I care about for the moment.

Yours very truly

W. A. Worlock

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f131

30a, WIMPOLE STREET, W.

3rd February 1894

Dear Mr. Galton,

I am sorry to say that the measuring machine is not finished: so that the measurements are not begun!

Yours very truly

W. F. R. Weldon

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GALTON PAPERS

f132r

30a, WIMPOLE STREET, W.

4th February 1894

Dear Mr. Galton,

Will you be kind enough to give me your opinion on the following point?

I have collected 26,306 tosses of groups of 12 dice, for use at the Royal Institution. In each group the event recorded is the number of dice with 5 or 6 points, so that the chance of success with each die is $\frac{1}{3}$. I enclose the result, which seems to me good.

A certain set of 7006 tosses, forming part of this result, was made for me by a clerk in the

office of University College, whose accuracy in work of another kind I have had occasion to test by asking him to copy 24,000 numbers of 3 figures each, with excellent results.

A day or two ago, Pearson wanted some records of the kind in a hurry, in order to illustrate a lecture, and I gave him the record of the clerk's 7000 tosses, together with some others.

I gave him the 7000 separately from the rest, and on examination he rejects them, because he thinks the deviation from the theoretically most probable result is so great

as to make the record intrinsically incredible.

You will see how serious a matter this is. On the one hand I feel that I have no right to reject an Experimental result for this kind of reason; and on the other, the result itself does not seem to me incredible.

I am anxious, however, not to rely upon my own judgment in so difficult a matter. — I have therefore resolved to consult as many people as possible. Last night I saw Greenhill, whose experience in target practice at Woolwich makes him know this kind of thing statistically as well as mathematically — He is of opinion that the record

f133r

is perfectly credible, and that I have no shadow of reason to disregard it.

Today I am sending it to Yule and to Edgeworth.

Forgive me for troubling you, when I know how busy you are: but my need is very great.

Yours very truly

W F R Weldon

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There is still no measuring machine for the Herring!

The "most probable" result in each of the enclosed tables is obtained from the expansion of $(\frac{1}{3}+\frac{2}{3})^{12}$, and not from any form of approximation.

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GALTON PAPERS

f.134r

30a, WIMPOLE STREET, W.

6th February 1894

(1)

Dear Mr. Galton

You are very kind in taking so much trouble for me!

I have been trying another mode of testing the dice result:

Whatever right one may have to assume that the result of 7000 throws will be regular with material dice, one has no right to assume a priori that with a given set of actual dice the odds against tossing 5 or 6 points in one throw are exactly two to one.

Last night I tested this from the result of the whole 26,000 throws, and found the ratio $\frac{\text{whole no. 5 or 6 points tossed}}{\text{whole number 5 or 6 points possible}}$

to be 0·3377. This seemed ∴ the experimental chance of success in one trial.
I therefore expanded

~~0·3377~~

$$(0\cdot3377 + 0\cdot6623)^{12}$$

And obtained a much closer approximation to the observed results than by the previous assumption.

For the 26,306, the results were

No. of dice per Group of twelve with 5 or 6 points	Observed	Most Probable
0	185	187·3
1	1149	1147
2	3265	3209
3	5475	5463
4	6114	6263

f 135r

5	5194	5113
6	3067	3038
7	1331	1332
8	403	426
9	105	109
10	14	16
11	4	12
12	0	0

The 7000 tested by the same expansion gave in the groups which Pearson attacks

4 ——— 1571 ——— 1666

5 ——— 1404 ——— 1360

The deviation is already slightly less, and the modulus slightly

f 135v

greater: so that the a priori chance of the deviation in 4 which is actually observed is now about $\frac{1}{100}$ instead of $\frac{1}{300}$, as it was before.

But, these 7000 were tossed with a set of dice given to my assistant for the purpose, while others of the 26000 were tossed with other dice. So that even now we have not got the right odds for these dice.

The Experimental chance of success in 1 trial for ~~them~~ the 7000 is 0·3368; and I will expand this tonight, and let you know the result.

Yours very truly

W. F. R. Weldon

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GALTON PAPERS

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UNIVERSITY COLLEGE LONDON
GALTON PAPERS

30a, WIMPOLE STREET, W.

6th February 1894

(2)

Dear Mr. Galton,

The correction which I suggested this morning makes the chance of a divergence so great as that shown by the suspected result more than 10 times as great as before.

Assuming chance of throwing 5 or 6 in one trial = 0·3377, then chance of throwing 4 successes out of 12 = 0·238105, chance of throwing 5 successes = 0·119436.

From these data, the most

probable number of groups with 4 successes is 1666·7 in 7000
Most Probable No. with 5 successes is 1360·5.

Modulus of expectation, for groups with 4 successes.

$$\sqrt{0.2381 \times 0.7619 \times 2 \times 7000}$$

$$= 50.4$$

Modulus for groups with 5 successes = 46·8.

The observed deviation of the 4 group is −95·7, or 1·9 times the modulus. — Chance of so great a deviation = 0·007.

The deficiency in the 4 group ought to be distributed among the other groups according to their chance of success:

∴ 5 group ought to receive $95.7 \times 0.194 = 18.6$ of these.

∴, knowing the deficiency in the 4 group, the most probable number of throws giving 5 successes becomes $1360.5 + 18.6 = 1379$.

The observed number = 1404, a a deviation of + 25.

$\frac{25}{46.8} = 0.53$; ∴ a deviation so great as this ought to occur in 0.454 of a series of trials.

∴ Chance of the combined result
= ·007 × ·454 = 0·0032
or about $\frac{1}{313}$.

By precisely the same reasoning Pearson shows that the chance of the observed result, on the assumption that the ~~odds against~~ chance of success is exactly $\frac{1}{3}$ in one trial, is about $\frac{1}{3650}$.

So that a very slight approximation to ~~an assumption~~ a knowledge of the actual experimental conditions makes the obtained result ten times as intelligible as before.

Of course the real problem is not so simple as this, because the chance of success for each die ought

[6.2.94] f138

30A, WIMPOLE STREET, W.

to be determined separately: but for my part, seeing that one very simple and obvious correction makes the result so much more plausible, I feel inclined to trust it absolutely, in the belief that the assumptions as to the chance of success or failure, based upon an examination of each die, separately, would account for the observed irregularities more fully.

Surely this is reasonable?

Yours very truly

W. F. Weldon

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f139r

30a, WIMPOLE STREET, W.

17th. February 1894

Dear Mr. Galton,

The measuring machine will be ready on Tuesday.

Miss Jeffrey has been measuring crabs for a week. — I asked her to do this, because she was anxious to do something, and it seemed well that she should have a little training. This proved very necessary. For about two days her measures of the hard parts of a crab were only good to about 0.5 mm.; but she soon improved.

Yesterday she began to measure a herring, so as to learn to deal with a soft creature. — She measured the same herring a great many times,

and improved a good deal. The same thing will be done today.

On Monday I propose to ask her to measure a good many fish, and these measures I propose to throw away. Then on Tuesday she can begin seriously.

In the absence of the machine, the measures are made with "beam compasses": and for every thing except the total length this seems a very good way.

Yours very truly

W. F. R. Weldon

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GALTON PAPERS

f140r

30a, WIMPOLE STREET, W.

12th March 1894

Dear Mr. Galton

I hope you are enjoying yourself, and that the Spring is as beautiful with you as it can be at Nice, sometimes.

The herring go on very slowly. The animals in dying have developed little crumples, which the spirit has made rigid: and for this reason it is necessary to hold them very carefully while the measures are made. Only about 200 are finished!

Miss Jeffrey is very enthusiastic,

and very accurate. — I do not think that the want of quickness is at all her fault.

I am finishing my measures of young crabs as quickly as may be: so that next term it will be possible for me to help in the measurement of the herring.

Might a joint report be presented upon the measurements of two organs in crabs during growth?

I shall soon have 4000, — 2000 between 18 and 25 mm. long, and 2000 between 6 and 9 mm. long, — in each of which two organs are

measured. The Q at various ages ought to be interesting & likely, without growth experiments? — If so, a report on this subject could be sent in in May.

I enclose the card of a man who supplies me with lizards, because it occurs to me that you are in the land of lizards, and the common little beast, — Lacerta muralis — might be worth measuring, if it can be had cheaply.

If you think it worth while, and if you are in this man's neighbourhood, would you care to find out the price of one or two thous and in spirit?

f141v

Have you seen Bateson's new book?
— His idea of a statistical proof of discontinuous variation seems to me queer, and quite inadequate.

Also, he assumes, in spite of your work on Eye-colour, that characters which do not mingle in inheritance are not subject to reversion!

Yours very truly

W F R Weldon

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F142c

30a, WIMPOLE STREET, W.

11th April 1894

Dear Mr. Galton,

I will be at the Royal Society tomorrow from four o'clock.

The herring go on very slowly. Mr Jeffrey cannot get more than 10 individuals per day measured, and has now done less than 400.

I want to ask you whether I had better go on with my young crabs, or measure herring.

The 4000 crabs which I have are not enough, because the mean value of the organs measured chang so fast during growth that they have to be sorted into groups whose total

length is the same (in the younger stages) to 0·2 mm.

Treated in this way, the quartile deviation of the two eggs measured seems so far to be about the same with crabs of 7 mm. 8 mm. and 20 mm. long as it is in the original set which averaged 40 mm. long.

Estimating every thing as percentage of the body length, this result is of course the same as egg result for human nature.

It seems to me a very important thing to settle, because if this result is true, I can see no evidence that

any selective destruction affects the two characters measured.

Now the first character, the "frontal breadth" is dimorphic in Naples crabs, and is therefore emphatically a character which is variable and presumably for that reason important.

I should like to finish this business, and present it to the Committee, as soon as possible, allowing other people to measure herring in the mean time: but I will do what you think best in the matter.

Yours very truly

W F R Weldon

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f 1244

30a, WIMPOLE STREET, W.

6th May, 1894

Dear Mr. Galton,

I am delighted to learn that the Chimpanzee prints were of use. — Lankester would surely be glad to publish figures of them in the Quarterly Journal of Microscopical Science.

Can you come to the Royal Society half an hour before the Committee on Wednesday? I should much like to show you

the present state of the crab measurements.

I think I am getting trustworthy evidence that the Prob. Error increases with growth, in each of two organs: but I have not yet shown that it is at any time greater than in adult life.

The figures are complicated, and I have a great deal of routine work which must be done: so that I should like to explain the thing verbally, instead of by means of a long letter.

Yours very truly

W. F. R. Weldon

f145

You will receive a petition about London University in a day or so. — Will you consult Sir Douglas Galton before refusing to sign it? — I think he will convince you that your signature would help University College.

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f 146 c

30a, WIMPOLE STREET, W.

4th June 1894

Dear Mr. Galton,

I have just heard a story with which I want to trouble you.

Two years ago, a man, said to have been of great promise as a mathematician, — a favourite pupil of Clifford's, — shot himself, in consequence of a painful story which need not now be repeated.

This man's relatives have lately given to Pearson a large manuscript on "the Algebra of Animal Evolution."

This begins with a statement of the need for statistical treatment of all problems in inheritance & evolution:

then follows a chapter on the Chance Function and a series of chapters showing how the theory of Chance should be applied, — including, apparently, a full development of your ~~part~~ surface of frequency of 2 variables, and the application of its properties to correlation, including "ataviism".

Pearson and I feel that in all likelihood the work is fit for publication with a little editorial revision. But we hardly like to read it, because we are both working at these things as well as we can: and we might be placed in a difficult position.

The only way in which we see

a chance of acting without possibility of misinterpretation is by at once placing the manuscript in the hands of some man older than ourselves, whose experience and judgment will command the confidence of those concerned.

You are the man whose name occurred to both of us: and I believe that Pearson (who has immediate charge of the matter) will ask you to read the work. Will you do so?

Yours very truly

W F Weldon

Weldon

f148

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GALTON PAPERS

30A, WIMPOLE STREET, W.

28th June 1894

Dear Mr. Galton,

Many thanks for your interesting document, which I return. — I imagine that there will be little difficulty in keeping spermatozoa alive: the chief difficulty will probably arise from their susceptibility to minute changes of temperature; otherwise vaccine tubes will probably preserve them for a short time, without any extraneous fluid.

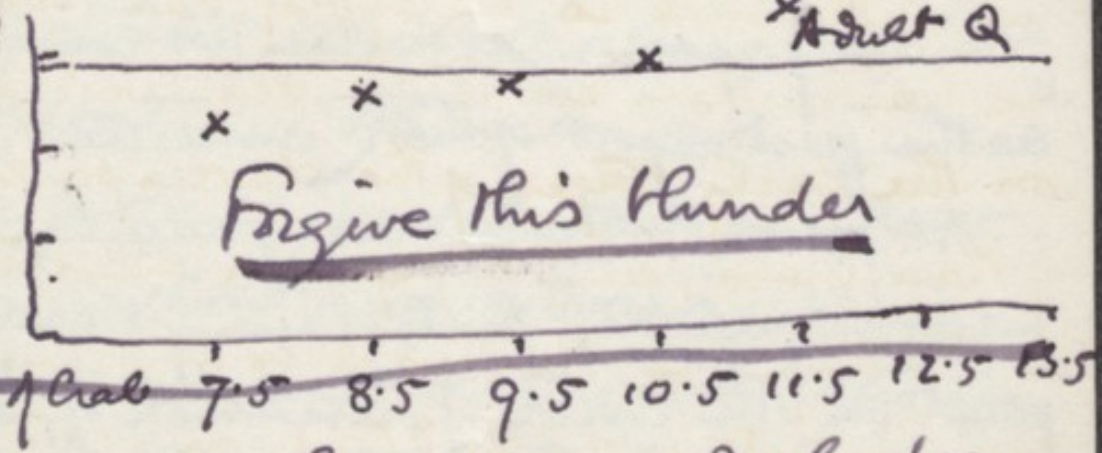
But one must distinguish between keeping them alive and keeping them fertile. So far as I know, there are no observations as to their fertilising power under such artificial conditions.

I am still unable to think of any

one so likely to work at this thing as Walter Heape, of Trinity. — He has married since I left Cambridge: and I do not know his address: but "New Museums, Cambridge" will find him within twelve hours. — He ~~was~~ was a pupil of Balfour's in my year, an enthusiast, with money, who has given his whole time to embryological work. He has written one of the standard papers on the early stages of the development of Mammals, and a paper on menstruation in Monkeys. — He has also performed the experiment, of which I told you yesterday, of transplanting a fertilised ovum from one uterus to another. — He is a very skilful operator, and is likely to be attracted to your problem by the very

fact of the manipulative difficulty.

As to the Crabs: — I have about 5000 individuals, between 7·00 mm. and 14·00 mm. in length: the results are not completely plotted, but I will have them ready by Friday next. For one of the two organs measured, the change in the Prob. Error is certainly of this form:



I enclose the curve, properly drawn: but the figures are only rough approximations.

Any how, there is indication

F149

(1) that for a considerable time the young Q is less than the adult Q and (2) that at a certain period in life the Q is greater than in the adult condition: so that there is evidence of selection.

I propose to show the details to the Committee on Friday, and to ask leave ~~to send you a formal paper~~, after adding more measurements, to send you a formal paper, which can be communicated as the first report of the Committee.

At present, the average number of measurements, determining each point on the curve of variation of Q, is about 700. — I propose to make it up to 1000, at least, before reporting.

Yours very truly

W F R Weldon

f 150

30A, WIMPOLE STREET, W.

7th July 1894

Dear Mr. Galton,

Many thanks for your cheque for £50.

Yours very truly

W F R Weldon

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F151C

30a, WIMPOLE STREET, W.

13th July 1894

Dear Mr. Galton,

Many thanks for your paper.

I should like to talk to you at length about Bateson's book some day. — It is a wonderful collection of facts: but it seems to me vitiated as a statistical argument in favour of discontinuity, by the fact that his animals for investigation were rarely chosen at random. He simply went to museums, and examined selected specimens, in most cases.

His chapter on teeth, I know to

f151v

quite worthless from this point of view. He has overlooked quantities of evidence in favour of the opposite contention.

Thane says the same about his statistics of human ribs, supernumerary digits, etc.

This does not touch your point about sports, or "transiliencies," but it is nevertheless important.

I can't write any more now.—I have measured 160 crabs today; and in measuring by turning a screw one's hand becomes so tired that it is now a great effort to write, or

f152

indeed to use one's hand for any reasonably human purpose, after a long spell of measurement.

Yours very truly

W F R Weldon

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f153r

30A, WIMPOLE STREET, W.

26th July 1894

Dear Mr. Galton,

I have measured 7069 young crabs, and have worked out the ~~inter~~ frontal breadth.

The Mean Errors at various ages remain almost as they were, except in the case of ~~crab~~ crabs from 7·00-7·99 mm. long, – of which there ~~are~~ were very few in my report.

Here are the results of the final trial, compared with those of the first report. I hope you will agree with me in thinking the two (so) nearly

coincident that the result is trustworthy:

Size of Crabs:	M.E. (first trial)	M.E. (2nd. trial)
7·00 - 7·99	2·6 (?)	2·838
8·00 - 8·99	2·781	2·860
9·00 - 9·99	2·799	2·814
10·00 - 10·9	2·795	2·833
11·0 - 11·9	3·051	3·033
12·0 - 12·9	3·172	3·132
13·0 - 13·9	3·048	3·043

(The Mean Error of adults is 2·95)

The curious little rise in crabs

of 8-mm. is due to the presence, among 1078 individuals, of 3 deviations of over four times the modulus, and of 3 others over 2·5 times the modulus.

I am afraid that you will be glad of these six beasts: because they are either clerical errors on my part, or "sports", and presumably transliencies.

In order to test the value of the M. E. determinations, it was necessary to show that each little curve was a real probability curve. I have simply tried to do this, by referring each set of individuals to the same origin, expressing the deviations in terms of the modulus

of each set, and adding the results.

Putting in the 998 adults, there were 8067 individuals available for the purpose.

The curve should, of course, have had its axis at 0, and its Mean Error should have been

$$\frac{\text{modulus}}{\sqrt{\pi}} = 0.5642 \text{ where}$$

modulus = unity.

The sum of the + deviations was 2272.430; the sum of the − deviations 2271.548; so that the Centre was as nearly as possible at 0.

The Mean Error = the sum of these two deviations divided by 8067

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July 26/94

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30A, WIMPOLE STREET, W.

gives 0·5633; and this seems true very nearly = 0·5642. — The modulus from this being 0·9982 instead of 1·000.

In spite of this very great degree of symmetry, there is a curious irregularity in the very centre of the curve, as you will see from the diagram which I send by book post.

In the course of next week I hope to get the second curve treated in a similar way, and to attack the muscle curves: then

f155v

a week to revise and to write the thing into such English as may be, and I hope to go to Siena and see the just more beautiful than probability curves.

Do you stay in Town long enough to allow me to send you the whole thing before I go?

Yours very truly

W F Sheppard

The revised diagram a letter - f156
is in a square leaf book in my
drawing room

30a, WIMPOLE STREET, W.

28th July 1894

Dear Mr. Galton,

I am sorry that I troubled you with a quite worthless letter. — A stupid mistake in copying has led to a blunder in the crabs of 7mm. long.

Their Mean Error is 2·788, which agrees ~~much~~ better with the former determination. — This of course affects the big curve, and I am now going through that again.

Yours very truly

W. F. R. Weldon

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f157r

30A, WIMPOLE STREET, W.

30th July 1894

Dear Mr. Galton,

It is very kind of you to sympathise, instead of abusing me for wasting your time.

I enclose a table and diagram of what I believe at last to be the true result.

The Centre is at −0·0001.

The Mean Error is 0·56201

(difference from theoretical result $= \frac{217}{56488} = 0{\cdot}38$ per cent.)

The Error of Mean Square is 0·7084 (difference from

F15f

theory = $\frac{13}{7071}$ = 0·18 per cent.)

The number of individuals

between ± 0·09 = 905 instead of 907.

over + 0·09 = 3595 } instead of
under − 0·09 = 3571 } 3581.

Surely one could not expect a much better result than this?

I hope you will have a pleasant time among the Alps. — I shall not bother you with

f158

any more results until you come back.

Yours very truly

W F R Weldon

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[with Weldon to Galton 30.7.94] f159

Table of 8069 (not 8067) Crabs. — Frontal Breadth

Deviation	Observed +	Observed −	Theoretical
(+0.09 to −0.09)	(905)		(907)
0.10 – 0.29	896	814	872
0.30 – 0.49	769	862	774
0.50 – 0.69	649	626	635
0.70 – 0.89	492	514	481
0.90 – 1.09	295	299	336
1.10 – 1.29	194	219	217
1.30 – 1.49	137	122	129
1.50 – 1.69	71	56	71
1.70 – 1.89	46	33	36
1.90 – 2.09	28	16	17
2.10 – 2.29	4	1	
2.30 – 2.49	4	4	
2.50 – 2.69	4	3	12.
2.70 – 2.89	1	1	
2.90 – 3.09	0	0	
3.10 – 3.29	1	0	
over	2	1	
Total	3595	3571	

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1894

[Found with Weldon's Letter
to Galton of 30·7·94

Frequency diagram of
frontal breadth of Crabs]

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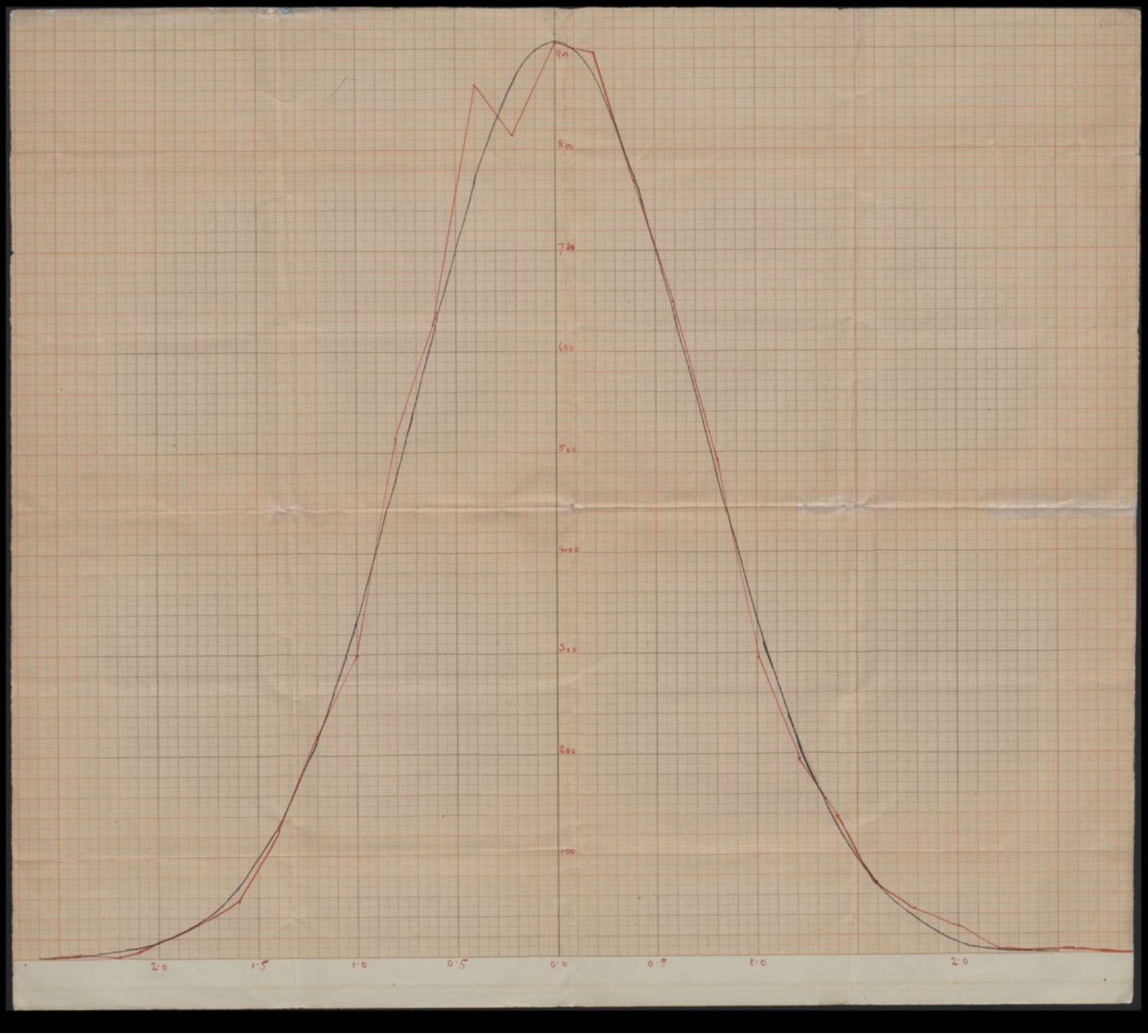
2·0
1·5
1·0
0·5
0·0
0·5
1·0
2·0

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F161

30A, WIMPOLE STREET, W.

18th October 1894

Dear Mr. Galton.

I hope you enjoyed your holiday, and that the bad weather which seems to have prevailed over most of Switzerland did not come near you. — You should have come down on to the good side of the Alps, into the sunshine!

Your note reached me just as I was on the point of sending you the crab paper. As it is, I will not send it until you ask me: because you probably do not want the trouble with it until your final return. — It is very humiliating to discover how one's mistakes increase

with an increased number of cards, in such sorting work as was required at the end of the crab paper. — I found yet another blunder after my last letter to you.

Now, I believe there are no blunders of great importance left; and the result is sensibly the same.

The figures are drawn on a huge scale, so that they may more easily be shown to the Committee. I will reduce them afterwards.

=

I have read de Vries with great interest: but it seems to me that there are lacunae in his argument.

He shows, as I understand him,

that a variation whose frequency is at first represented by a "halbe Galton Curve" can, by selection, in 4 or 5 generations be made to yield a race varying symmetrically about a new mean.

In order to show that such a frequency curve is diagnostic of an "artenbildende" variation, he ought to show that selection from one side of a "ganze Galton-Curve" does not produce this result. — In particular, it must be easy to find dozens of flowers which have recently been doubled in gardens, and are yet single in easily accessible wild places. — If his contention be true,

The curve of frequency of petal-variation should in all these cases be a "halbe Galton-Curve" in the wild state, and it should be impossible to obtain double varieties of the others, whose frequency curve of petal variation is normal.

The statement that an increase in the number of petals of extreme individuals ceases when the curve of frequency becomes approximately normal (Summary, proposition 3) is surely premature, because he has not carried his experiment far enough to prove it.

At least he has proved that we shall have to work with Pearson's

[1·10·94] f163

30a, WIMPOLE STREET, W.

generalised probability function,
so as to include expansions of
$(x+q)^n$ in which x and q are
very unequal indeed!

Yours very truly

W F Sheldon

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GALTON PAPERS

f164r

30a, WIMPOLE STREET, W.

28th October 1894

Dear Mr. Galton,

I do not know whether you get care to look thro' the crab paper. — It shall be sent to you whenever you choose.

I shall be glad to consult you about the herring some day. The measurements made by Miss Jeffrey seem so far trustworthy that they indicate on the whole about the same range of variation as that found by Heincke (so far, at least, as I have yet checked

them) and the curves of distribution, so far as I at present know, look dimorphic. — But she has not yet finished 2000!

Yours very truly

W. F. Sheldon

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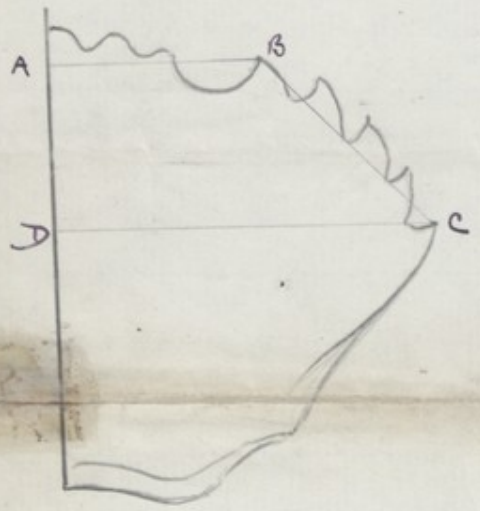
29th. October 1894.

7165

Dear Mr. Galton, — Enclosed is the crab paper. — I have drawn the figures very large, because I wanted you and the Committee to see them on such a scale. — I will draw them on a smaller scale for prep. Fig. 1 is very rough, because it is a failure. It fails for this reason :—



The dimensions considered in the paper are AB, and CD.

Now the position of C depends not only upon these dimensions, but also on the angle ABC, which varies very much. In the paper on correlations in adults, I obtained a measure of this angle, by measuring also the dimension DC: so that ~~in~~ I was able to draw the mean carapace: but ~~this~~ DC was not measured in the young crabs, and it would require some little time to obtain measures on which to base an estimate of its average magnitude at the ages discussed. — Of course, I can do this, if necessary.

I am slowly plotting preliminary herring curves: but as these are only preliminary: I am not wasting daylight over them. By day, I have started another experiment.

Daphnia magna is, when adult, about 3.5 mm. long. It has,

1.

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[29·10·94] f166

on the ~~bo~~ posterior margin of its carapace, a spine. – Leydig, who first accurately diagnosed the species, says that in parthenogenetic females this spine is variable, and may be absent. – I want to try in parthenogenetic generations, springing from one parent, to double its length and to extinguish it. – The material available is a population of such females, in a large bell-jar enclosed in a fern-case in ~~the~~ my laboratory, and warmed. On this bell-jar were placed some hundred individuals taken from another stock in my laboratory; and at the same time, five females, from the same stock, were isolated in beakers, and placed in the same fern-case. To show rate of ~~growth~~ multiplication, I give the history of female No. 5 V.

October 16 – 9·0 A.M. – No. V isolated: by 12 o'clock she had laid eggs in her brood-chamber.

October 19 – 10 A.M. – No. V ~~p~~ has produced 7 young, cast her skin, and laid new eggs in her brood-chamber.

October 22 – 9·30 A.M. – No. V has produced 11 young & a 2nd. brood.

October 25 – No. V has produced another brood. } These two broods contain 19 individuals.

October 29 – No. V _____ }

The first grand children produced October 26th. – Now, the first ~~generation (the~~ brood have all produced young, except one, which was killed.

To show variation of spine, I enclose ~~t~~ outlines, carefully made with a camera lucida, of two individuals taken at random: also the following measurements: In 20 individuals, taken at random out of the large population which serves as stock, and all nearly of the same absolute size, the ratio $\frac{\text{Spine length}}{\text{Body length}}$

[29·10·94] f167

as measured by the ~~da~~ ratio $\frac{AC}{AB}$ in the drawings, varied between 0·232 and 0·395. — In six individuals, grandchildren of isolated ♀ No. ~~II~~ IV., and born of one mother, the body-length and spine-length were as follows:

	Body	Spine
No. 1.	2·52	0·95
No. 2	2·50	0·91
No. 3	2·47	0·90
No. 4	2·43	0·92
No. 5	2·53	0·99
No. 6	2·48	0·91

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In their parent, body length = 3·54, spine-length = 1·31, the ratio $\left(\frac{AC}{AB}\right)$ being 0·370. — The mean ratio in the six offspring is 0·373; and, as you see, there is variation enough for selection.

The creatures can be made drunk with alcohol, and measured in that condition: they then recover. So today I killed, after measurement, Nos. 1–4 above, and kept Nos. 5 and 6, for breeding.

Of course, this Experiment is begun in defiance of Weismann's dictum about asexual reproduction: but if it only results in a proof of ~~Weismann's~~ the truth of Weismann's view, it will be worth having.

What I propose to do, then, is (a) to determine the M and Q values of the ratio ~~AC~~ $\frac{AC}{AB}$ in the stock population, at all ~~ages~~ sizes (the ratio varies very much with growth) and (b) to ~~determine~~ measure every individual produced by

(29-10-14)
f168

one stock-mother: out of these, to breed from the longest spined and from the shortest-spined; and so on.

If the experiment should succeed, the races, thus produced asexually, might be allowed to become sexual (this can be arranged!) and it would be interesting to see the amount of reversion.

I tell you this in detail, because I want you to consider whether the Committee should accept this experiment, together with continued herring, as sufficient work for the winter.

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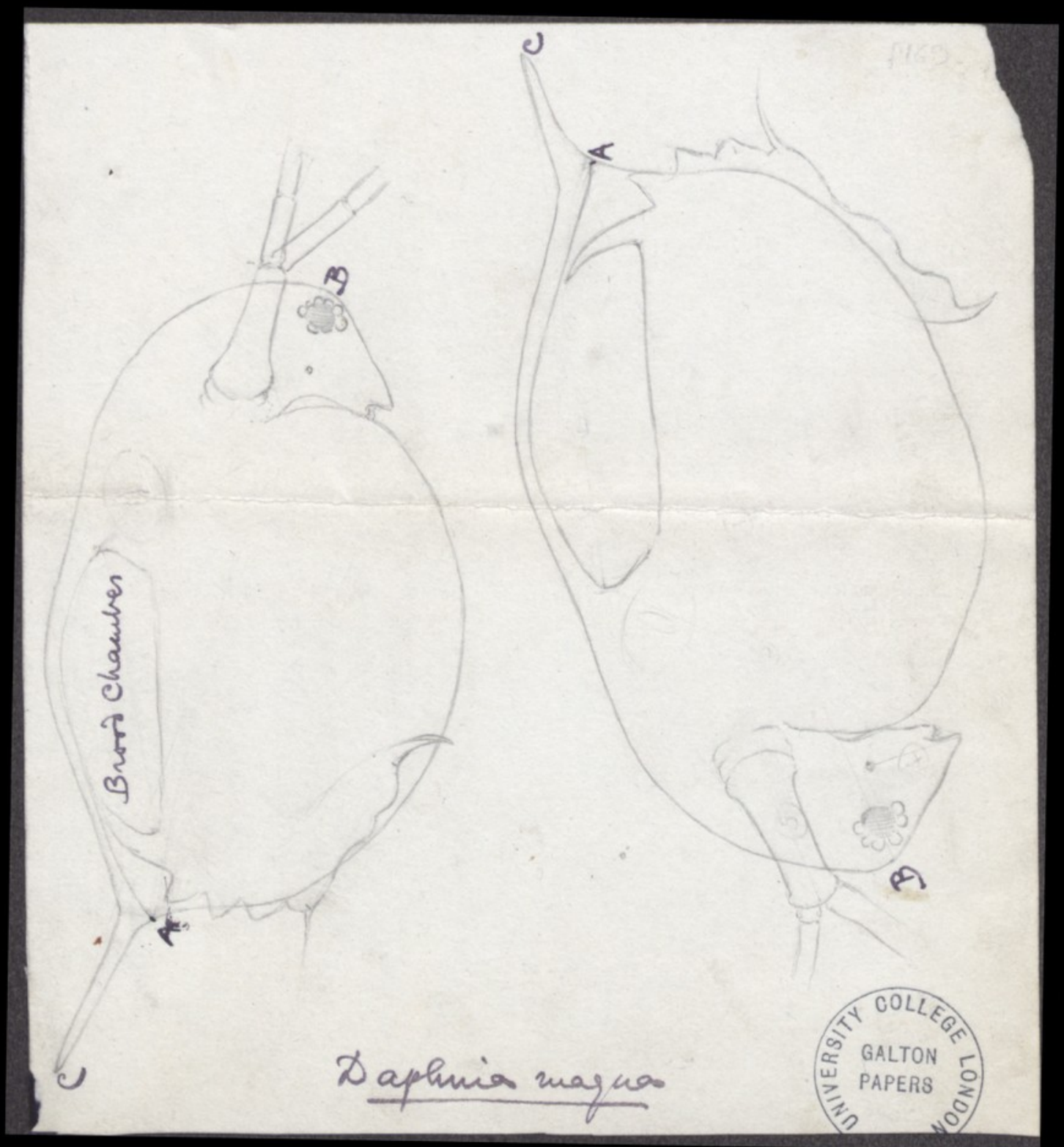
Yours very truly
W F R Weldon

P.S. ~~If the experiment should fail~~ I would point out the following: Bosmina, a closely allied ~~species~~ genus, is known to be asexual for years, — for hundreds of generations, — and has never been seen in a sexual condition. Either, therefore, variation, with possibility of selection, occurs during parthenogenesis, or, a genus exists, and maintains its species for years, and for hundreds of generations, without selection. Now Bosmina is a most abundant and widely distributed species.


C
A
B
Brood Chamber
B
C
Daphnia magna
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f170r

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

30a, WIMPOLE STREET, W.

31st October 1894

Dear Mr. Galton,

I am sorry to have been so careless.

Of course, in Fig. 4, the line BD should be replaced by the strip, of height BD, and of breadth dx; so that $DB.dx = k_2$.

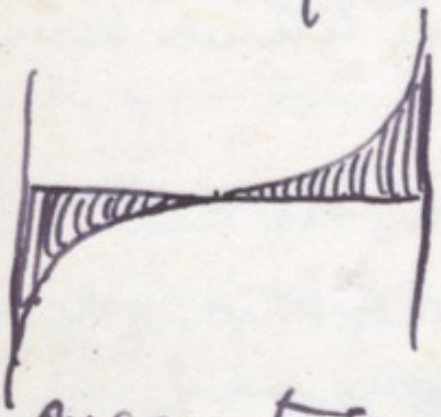
Then, if N be the whole population, the area of the whole curve is $N.dx$; and the ratio

$$\frac{DB.dx}{N.dx} = \frac{1}{c_2\sqrt{\pi}}, \text{ or}$$

$k_2 c_2 \sqrt{\pi} = N dx$ This is the statement which you place first;

f170v

and from your figure, I gather that you have perhaps overlooked my meaning of k. For you say let AB = k = number of population.

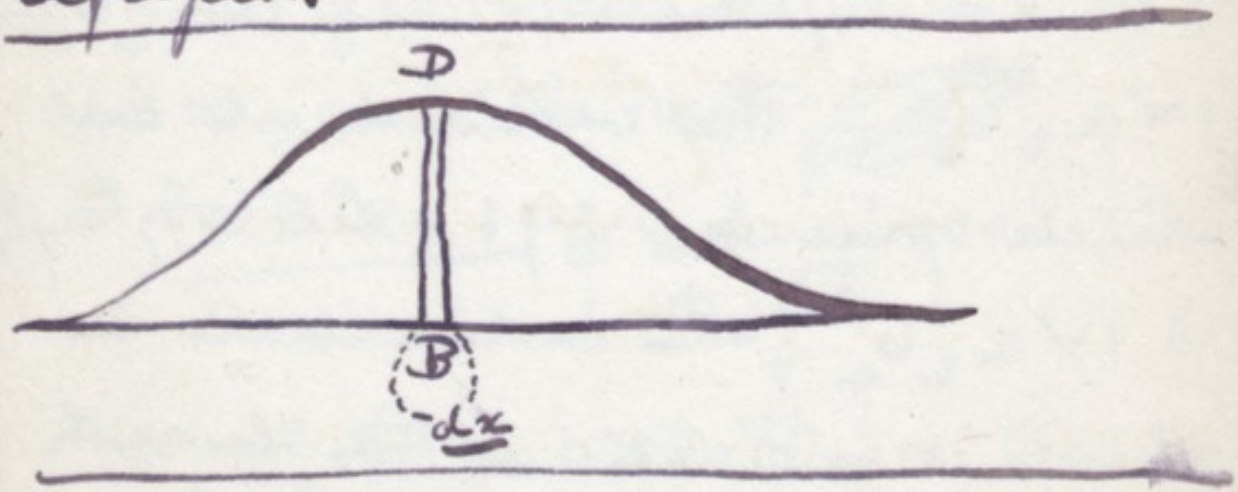


This is not what I mean to say. I mean to say, Let k = the number of individuals whose deviation ~~is negative~~ from the mean is sensibly nothing = DB . dx →

I am sorry that I replaced steps by lines. — I often do so in trying to make diagrams for my own edification: but of course it is wrong to do so in a formal exposition meant for others.

f171r

I have not shown the actual manuscript of the present paper to Pearson: but I have long ago obtained his adhesion to the method there used, granting the postulate that the normal probability equation expresses the distribution of abnormalities. This, however, he will hardly ever admit: and, as you know, he is working hard at a more general expression



f171v

Today I have isolated two Daphnia, both offspring of one parent: in one the ratio $\frac{\text{spine}}{\text{body length}} = 0.320$, in the other it is 0.391. — If you feel inclined for amusing exercise, will you some day devise a nomenclature for parthenogenetic relationships?

For example, Female No. IV. produces 4 children, which I call $IV\acute{a}_1$ — $IV\acute{a}_4$. The child $IV\acute{a}_2$ produces 6 children, which I call IVa_2b_1 — IVa_2b_6. The individual in which spine is 0.391 of the body length is IVa_2b_4; the individual in which it is 0.320 is the Eleventh child out of a second brood produced

Oct 31/94

f172

30a, WIMPOLE STREET, W.

by IV. — She is therefore, in my notation, $IV a^2_{11}$.

But $IV a^2_{11}$ and $IV a'_2$, having the same mother, are sisters. Yet the relationship between $IV a^2_{11}$ and the children of $IV a'_2$ a is much closer than that of aunt and niece, because there is no extraneous male heredity introduced.

And so on.

Yours very truly

W F R Weldon

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f173r

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

30A, WIMPOLE STREET, W.

18th November/94

Dear Mr. Galton,

Pearson admits that some mathematicians use $d\overline{x}$ as I did: but he condemns the practice, and says that one should write $\Delta\overline{x}$ when speaking in my sense.

We have devised the Cachet, which he considers safe.

= He tells me that Flinders Petrie will open this winter a graveyard containing ~~xxx~~ 200

early Egyptian skeletons, of known
lineage and date. — If some one
will undertake to work at them
soon, he will make the great
effort necessary to pack and bring
to England each skeleton in a
numbered lot, with details
from its wrappings, or # from some
source or other, as to its lineage
and date.

But, he will not do this
unless he is assured that some
one will turn his labour to
account. — I at once told

f 174r

Pearson to promise him that the bones should be carefully measured within 2 years of their arrival in England.

Pearson himself wants to work out the results, but he has a true mathematical horror of a real measurement.

If the Committee would care to undertake the business, of course both Pearson & Petrie would be pleased. — If not, I will get some one to measure the things in my laboratory.

The skeletons will probably

f174v

be nearly complete (at least skulls & all long bones) — and Petrie considers the opportunity of getting such specimens of known ancestry and date nearly unique.

What shall be done?

Yours very truly

W F Ridgeway

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f175r

30a, WIMPOLE STREET, W.

1st November 1894

Dear Mr. Galton

I will certainly do as you suggest, and submit the exposition to Pearson.

In the mean time, I do not think that the ratio $\frac{B.D.dx}{N}$ is equal to $\frac{1}{\text{infinity}}$.

I think that dx may be used to mean the smallest difference from x of which account need be taken, — i.e., unit abnormality.

So for example Venn (Logic of Chance, Edition 1888 ~~p 460~~ pp. 462-463) says "we must

say that the number of errors falling into one subdivision dx of our scale is $dx \cdot \frac{h}{\sqrt{\pi}} e^{-h^2x^2}$ where dx is a (small) unit of length": and immediately afterwards, in working out an Example, he says "here $dx = 1$ inch".

So that what I mean by D.B.dx is a column, whose height ~~means a certain number~~ contains a number of linear units, Equal to the number of individuals indicated by the ordinate, and whose base is unity on the scale adopted. So

that the number of individuals is equal either to the number of units of length in B.D, or to the number of units of area in B.D.dx.

Then, if N be the number of individuals in the population, N.dx will, in the sense in which I have here used it, be the number of units of area in the curve, on the scale employed: and ~~either~~ the ratio $\frac{B.D.dx}{N} = \frac{BD}{N}$

$$= \frac{B.D.dx}{N.dx} = \frac{1}{C\sqrt{\pi}}.$$

Of course, I may be wrong in assuming that dx is legitimately used in this way to denote a

finite small quantity: but if so, I do not understand Venn's language in the passage referred to.

In any case, — and this is, after all, the Hauptsache — if you will read my meaning into my dx, I have not written nonsense.

Yours very truly

W F R Weldon

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In saying $\frac{BD.dx}{N} = \frac{1}{c\sqrt{\pi}}$ I assume, of course, that the unit abnormality dx is the same as the unit in terms of which c is expressed: so that $c = n.dx$.

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GALTON PAPERS

f177c

30a, WIMPOLE STREET, W.

5th November 1894

Dear Mr. Galton,

It is very kind of you to say such nice things about the crab paper. — I am only sorry that my careless writing has given you so much useless trouble.

I will go over the whole thing carefully before the Committee meets, and make use of your [redacted] suggestions.

About the date of the Committee. — You will know better than I, but I imagine that the new Council will not assemble until the 30th., after its formal election: and if we

wait for a meeting until that sort of date, we shall not be able to reply to Petrie before he goes to Egypt. — I saw him on Saturday. — He says that the skeletons are in rock tombs, and will probably be very fragile and full of salt. The ~~average~~ cost of collection and transport will be from eighteen pence to two shillings per skeleton.

There will probably be an expense of a serious kind after the arrival of the bones. — He tells me that bones from a ~~cemetery~~ grave place of a similar kind were brought to the College of Surgeons last year, and

that the salt kept the bones in a damp and fragile state. This was remedied by soaking each skeleton in an iron pot full of warm strong size. On cooling, the size set to a jelly and the salt, after a few weeks (?) diffused into this jelly. Then, the jelly was melted, and the bones were taken out — the size which remained in them helping to give strength to them.

Such an operation, on 200 skeletons, would take some time and money: but from the estimated cost of collection and transport, I imagine that £50 would pay for the whole thing.

Could you keep one or two days

f178v

of next week free? — If so, I will write to Darwin & Macalister, on hearing from you, and give them a choice of days.

Yours very truly

W. K. Clifford

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30a, WIMPOLE STREET, W.

9th November 1894

Dear Mr. Galton,

I have only just received reply from Macalister. — He cannot attend on any day next week.

Darwin can attend on Thursday, 15th., at 4-30.

I am writing to Rix, calling the meeting for that time: but I shall ask him to delay the posting of the notices until he has a chance of hearing from you. — So, if the time does not suit you, please write directly to him.

I will prepare (1) some heading

curves; (2) an amended crab paper, with my suggestions inserted, and all reference to "normal" annuals left out — (3) ~~final~~ Statement of Expenditure — (4) Sketch of the daphnia generations — And I will ask Petrie to write a letter about the skeletons.

Yours very truly

W F R Weldon

f 180

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30A, WIMPOLE STREET, W.

12th November 1894

Dear M. Galton,

At the moment of receiving your letter, I received a telegram from Rix, saying that you had suggested (as I read the telegram) "some Thursday" at 2-30. — I telegraphed back, asking for the Thursday next week at 2:30: but I have just received a note saying that, as you suggested the Thursday this week, the meeting is called for that day.

If you did suggest this, I shall

f 180v

be glad: but I seem to have made a great deal of confusion over a simple matter.

Tomorrow evening I will send you an account of the Daphnia experiments. It seems likely to be very complicated: and if you have time to do so, I should like you to consider it before the meeting. — I cannot send tonight, because a generation, which will be old enough to measure tomorrow, is rather important.

Yours very truly

W. F. R. Weldon

F 181c

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30a, WIMPOLE STREET, W.

13th November 1894

Dear Mr. Galton,

I enclose the history of Daphnia No. IV, so far as it goes. The notation used is this: the first generation is called IV a, the second, IV b, and so on.

The individuals are born (almost simultaneously) in broods: these broods are numbered above the letter denoting the generation: so that IV a^2 means the 2nd. brood of the first generation. In each brood, every individual measured is given a catalogue number,

placed below the generation letter: so IV a^2_{11} means the eleventh individual of the second brood of daughters produced by IV.

The decimal, after each record, is the ratio $\frac{\text{spine length}}{\text{carapace length}}$.

One of the tables enclosed gives this ratio for every individual selected for breeding, the other gives the mean ratio in every brood from which such selection was made.

I have, of course, a record of the measurements of every individual.

f182c

The single individuals ~~[illegible]~~ used in breeding seem fairly promising: but I am anxious to know what you think of the average of each brood, and especially of brood $a'_3 b'$, and of $a''_{11} b'$.

Also, it is impossible for the present to express these measures as deviations from the average of a standard population. As I told you, at the beginning of the experiment, a number of individuals were placed in a large jar of water, with weeds, mud, & so on, and left to breed: it seemed likely that the offspring of these would furnish a standard population. They do not

however furnish a population similar to that from which they are derived. The mean ratio has sunk to about two thirds of what it was. (Can

I know the pond from which the original stock was derived, and can obtain a standard from that pond directly. — But the great change undergone by the offspring of my original standard population shows that the feature measured is probably variable under the direct action of conditions of light, warmth, food, etc., which will have to be investigated.

I am starting a new series of broods, with the object of finding

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30A, WIMPOLE STREET, W.

out the chief conditions which directly affect spire-length. — But this is likely to be a very complicated business.

Of course, these considerations only make the problem more interesting than it was before: and I very much want to solve it. But ~~the same time~~ the Committee may say that it requires a problem which is reasonably certain to yield an adequate solution in a fairly short time, and that to risky an attempt as this is not suitable for its present work.

f 183 ✓

Will you consider this point before Thursday?

Yours very truly

W F R Weldon

[with Weldon to Galton 13.11.94]
f184

Table of Generations of Daphnia No. IV showing mean value of spine ratio in each brood:

IV.
·101

- Brood $\alpha' = .344$
  - Brood $\alpha'_2 6' = .374$
    - Brood $\alpha'_2 6'_4 c' = .372$
    - Brood $\alpha'_2 6'_5 c' = .384$
  - Brood $\alpha'_3 6' = .332$
  - Brood $\alpha'_4 6' = .372$
- Brood $\alpha^2 = .334$
  - Brood $\alpha^2_{11} 6' = .369$
  - —— $\alpha^2_{11} 6^2 = .342$ { $a^2_{11} 6^2 c'$ not measured
  - —— $\alpha^2_{11} 6^3 = .349$

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Table of Generations of No. IV., showing only those individuals selected for breeding in each generation:

IV. = ·101

- $a'_2 = ·370$
  - $a'_2 b'_4 = ·391$
    - $a'_2 b'_4 c'_2 = ·388$
      - $a'_2 b'_4 c'_2 d$ not measured.
  - $a'_2 b'_5 = ·367$
    - $a'_2 b'_5 c'_4 = ·394$
      - $a'_2 b'_4 c'_2 d$ not measured.
- $a'_3 = ·339$
  - $a'_3 b'_4 = ·329$
    - $a'_3 b'_4 c$ not measured
- $a'_4 = ·303$
  - $a'_4 b'_1 = ·364$
    - $a'_4 b'_1 c$ not measured
- $a^2_{11} = ·320$
  - $a^2_{11} b^3_{10} = ·341$
  - $a^2_{11} b^2_3 = ·319$

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f186r

30a, WIMPOLE STREET, W.

16th November 1894

Dear Mr. Galton,

Many thanks for your letter, enclosing a cheque for £20.

2

The curves of ray florets in wild Chrysanthemum are horrible things! — Each of them (there are two) is dimorphic, and each component of one is asymmetrical as well.

I began last night to find the various powers of all the deviations, in order to form

f 186v

Pearson's fundamental equation
(see his paper in the Phil. Trans.)
for the resolution of the apparently
symmetrical Lot into two
components. But it will be a
very hard business: and when I have
formed an equation of the 9th. degree
I do not in the least know what
to do with it, except to send it to
some mathematician for solution.

Yours very truly

W. A. Shelford

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f187

30A, WIMPOLE STREET, W.

22nd November 1894

Dear Mr. Galton.

I have sent the real paper to the Royal Society, asking Rix to wait for a note from you before proceeding with it.

Yours very truly

W. F. R. Weldon

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

30a, WIMPOLE STREET, W.

23rd January 1895

Dear Mr. Galton,

I am heartily sorry to hear of your illness. — I hope you are on the way to recovery now, in spite of the very big changes of temperature outside!

You have read Pearson's paper, and I know practically nothing about it: but from the abstract, which he sent this morning, I very much want to know the answer to Darwin's question asked at the discussion of Pearson's last paper, — the Prob. Error of the constants of a curve based on observations. — I have two consecutive

sets of tossings, made with the same dozen dice, which differ very widely.

A set of 7,000 dice-tossings made by a clerk & myself were for a long time rejected by Pearson as impossible. He pays no attention to such abnormal results as that recorded by Pearson in the "Principles of Science", and so on.

Also, I fail to see how a method which treats American school children — the most heterogeneous material on Earth of its kind — as homogeneous can be reliable.

2

As for the Committee's affairs.

The Daphnia experiment has

failed utterly. The spine-length, both absolute and relative, depends on the number of ~~cells~~ individuals grown in a given volume of water. The beast secrete something which I think is a specific poison of the nature of the "Toxin" things secreted by bacteria. — I am trying to get this worked out. — It is a most interesting result, because many cases have been recorded, all pointing to the existence of a relation between the size of animals and the volume of water in which they grow. — I have further shown that a swarm of Daphnia, living

for some time in water containing plants, water worms, etc., etc., renders this water poisonous to Daphnia, but not to closely allied things.

This is nice, but it is not heredity, and it makes that particular form of heredity experiment impossible.

Beyond this, I have been trying to split up the Herring curves into components by Pearson's method. In order to do this, it was necessary to read a book on the theory of Equations because nonic equations were like now quite beyond me.

I have learnt the trick, and obtained solutions for one herring curve. – There

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[23.1.95] f 191

30a, WIMPOLE STREET, W.

are at present three alternative solutions: and I do not yet know which to choose.

The computation is rather long.

There are 10 curves to be resolved, and each one will take at least a week, at the present rate!

=

To atone for this delay, I propose to measure spats in the daytime, so as to compare the herring, ultimately, with a closely allied species — only at present I cannot find a fisherman who will send spats otherwise than through Billingsgate: and there the races are sure to be mixed.

f 191v

Herbert Thompson is slowly going over 7000 or 8000 male crabs, to compare the amount of selection during growth with my results for females.

Once more, I hope you will soon recover!

Yours very truly

W. A. Weldon

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f 192r

30a, WIMPOLE STREET, W.

27th January 1895

Dear Mr. Galton,

It is good news that you are on the way to recovery! I hope today's fog will not do you harm!

I shall look for your article in Nature with anxiety. — Of course, I see the power of Pearson's methods. So far as one can from the mere popular account which he gave on Thursday. Also, I see that there are many cases of "skew" variation: but all cases, which he has given, of variation with an unmistakably skew frequency, are taken from phenomena which are changing with a rapidity much greater

than that of any organs in crabs, or such creatures.

Pauperism, divorces, and the like, have only been invented, in their present form, for a short time, and as he himself shows, the maximum frequency ~~of any trait~~ changes its position at least in ten years.

The point I am anxious about is the theoretical value of slight skewness, when observed in such measures as mine.

Can one, without indecency, ask leave (of course with Pearson's consent) to read the full paper before it is published? — It is now, I suppose, dated, and I cannot see how any

injustice to Pearson could arise from my reading his manuscript. — In his abstract, he mentions the crab curves as examples of a neglect of skewness "of vital importance" in connection with growth. It is a nuisance that this statement should be printed, and that no examination of it, or reply to it, should be possible until the full paper appears, — it may be nearly a year hence!

I tried to answer this statement on Thursday by showing lantern-slides of with diagrams of dice-throwings: it seems true that in cases where one knows that the skewness of the true result is slight, the observed skewness in a small number of observations is

at least as great as that in crabs, or shrimps, or stature of men.

A test of the reality of skewness in such measures would of course be given by a correlation surface. — In skew-surfaces, I suppose the ellipses become some intractable ovoid, with equations of high degree; and the locus of the tangents parallel to one axis ceases to be a straight line.

If you think it well, I should like to try a test of this: and the easiest would be my old friend the Shrimp. — Taking only two dimensions ("carapace length" and "post spinous length" ½ of my old paper) I could collect measures fairly quickly. — There are 1000, of

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30A, WIMPOLE STREET, W.

course, already available; and the straight line plotted on p. 8 of the "R.S. Proceedings" Vol. 57 seems to me experimentally straight. — When Pearson condescends to discuss it at all, he says it is an undulating curve, with a cubic equation. Now, an approximation twice as good as that plotted ought to decide between these two views: and it would not take an impossible time to measure and reduce 3000 more shrimps. — The case is especially good, because the frequency curves of the two organs (see Fig. 2 of the paper) are very suspicious. — If these settle down into

respectable normal curves, on addition of more measures, the result will be worth having.

2

I feel anxious to do this, because Pearson still refuses to answer George Darwin's question about the Prob: Ens: of his constants. — Such an experiment would be a sort of answer.

2

I send you the result of my wrestle with the first sonic, which is at last overthrown. There were three real solutions, but one led to imaginary positions of the component curves. — the two real positions

are worked out in the diagrams.

The solutions are:

Solution 1.

	A	B.
Position of Axis	11.309 —	9.02
Area	693.79 —	284.21
Modulus (or "standard deviation")	2.78 —	3.25

Solution 2.

	A	B
Position of Axis	11.395 —	9.55
Area	579.37 —	398.63
Modulus	2.77 —	3.33

The two solutions are curiously alike: they correspond to

$$p_2 = -1.0802 \quad \text{—— (1)}$$

$$p_2 = -0.8204 \quad \text{—— (2)}$$

The other real value of p_2 was $+0.8681$. — I was anxious that this should lead to a real result, because of course the area of one component would then have been negative, and the result would have been obtained by subtracting the shaded curve from the other. — This is the way in which Poulton, and others men with speculative minds, have often imagined diamorphism to begin. — Further, it would be so

(3) Jan 27 1895 f196

30A, WIMPOLE STREET, W.

nice to test by measuring young individuals. But the meaning of a herring × $\sqrt{-1}$ has yet to be determined: so that the solution is useless.

Forgive such a long letter, and please do not answer it, if it troubles you to do so before your complete recovery.

Yours very truly

W. F. R. Weldon

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f 157

30a, WIMPOLE STREET, W.

7th February 1895

Dear Mr. Galton, It is very sad that you do not get rid of your influenza! – I hope you will not allow any Committees to tempt you during this bitter weather!

Please put my diagrams into the hottest fire you can find. A single sign went wrong in the middle of the arithmetic.

The real ionic gives no real components to the herring curve: so that it is a single skew curve of

Pearson's type. This result is also obtained by Pearson, who has worked the thing independently, and who pointed out my stupid blunder.

2

About the report to the Government Grant Committee:

I enclose a statement of £50 worth of expenditure.

I would suggest in answer to (a), that a first report, dealing with change in the modulus of crabs during growth, is in process of publication in the "Proceedings"; that over 2000 herrings have been

measured, and that the results are being reduced. — Some mention should, I think, here be made of Pearson's work, as the only method by which reduction is possible.

The statistics of floret variation in wild Chrysanthemum from Mr. Strickland are examples of compound skew curves, of which there is as yet no theory.

2

About the crab paper, I think the skewness may be neglected, — for adults at least. — Pearson has one of the adult curves used in the paper, and he chooses this as an example of an almost insensible

skewness. — The frontal breadth curve he has not examined in Plymouth crabs: but in the Naples Cor, in which it was asymmetrical, he prefers the solution which he originally worked out (sum of 2 normal curves) to the results of his skew theory.

By the way, there is going to be a discussion on Animal Variation at the Royal Society on the 28th., and Foster has asked me to open it. — I hope that you will be well enough to come: and I have asked that Pearson should be invited.

May I use the herring curve,

[with 7.2.95] f199

Statement of Expenditure

5000 herring from Plymouth Sound,
preserved in spirit and sent to University College — 22-0-8

Salary and Expenses of Miss Jeffrey,
during a part of the time occupied in
measuring herring ———— 29-5-0

£ 51-5-8

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[7·2·95) f200

30a, WIMPOLE STREET, W.

which he has resolved into a skew result, as an example of the use of this method?

I am anxious to do this, because after his paper the other day I showed some dice results, which led him to think that in my opinion skewness was a mere experimental accident. — Of course, I did not think so generally: though I still think that it may be so in particular cases. — Anyhow, I should like to make some sort of amends.

Yours very truly

W. F. Weldon

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30A, WIMPOLE STREET, W.

11th February 1895

Dear Mr. Galton,

I am sorry to ask you to read the enclosed documents, but I am afraid I must!

As I told you, I have asked that Pearson should be invited to speak on the 28th.: and accordingly I sent him a proof of the crab report. — I am sorry to find that I have not sent you one — here it is!

Pearson replied by sending what is evidently the draft of a magazine article, attacking violently every point in the paper. — He asked

me to return his MS., and so I copied it.

As the Committee is attacked, I think you should have opportunity of reading the attack. I enclose also my answers.

Here, as always when he emerges from his cloud of mathematical symbols, Pearson seems to me to reason loosely, and not to take any care to understand his data. — You will see that he assumes the ratio $\frac{Q}{M}$ to be the real measure of the importance of variation. — He does not see

that this is a matter for experiment, and not for a priori reasoning at all!

Again he says that by expressing frontal breadths in terms of carapace - lengths, my curves of frequency are sections of correlation surfaces — Surely, so is every curve of frequency!

But I will not repeat what I have enclosed.

Can we not get some mathematician on our Committee? — The position now seems to me to be that Pearson, whom I do not trust as a clear thinker when he writes without symbols, has to be trusted implicitly when he hides in a table of Γ-functions, whatever those may be. — Would not George Darwin

have time to judge our reports once a year, or so?

Yours very truly

W F R Weldon

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Of course, we could do more work with another grant: but I should like to consult you before proposing the work.

I have no reason to think Pearson's MS. confidential. — It is clearly written for publication, and he wants to save himself the trouble of copying, I think. — And it is clearly my duty to submit a matter affecting the Committee to its Chairman.

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30a, WIMPOLE STREET, W.

27th February 1895

Dear Mr. Galton

It is delightful to see your handwriting again, and to know that you are convalescent at last. — I shall miss your kind support dreadfully tomorrow. Of course I will tell you about it.

I have quantities of things which I believe to be important to tell you about & new curves, but that must wait.

My photographer has

sent me four useless Lantern slides: so that tonight I have had to redraw the diagrams on a large scale. — It is already Thursday, and I must go to bed without telling you more.

Yours very truly

W. K. Clifford

£2045

[about Feb. 28th written Mar. 1st or 2nd? H.S.H.]

30A, WIMPOLE STREET, W.

Feb/95

Dear Mr. Galton,

I hope you are still going on in the right direction.

I do not know what impression my remarks made: but so far as I can judge only a few physicists and such folk cared about them.

The "discussion" was carried on by ~~Dyer~~ Dyer, Lankester, Agassiz, and Bateson: and each man spoke as it seemed to him fit: so that the statistical

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method was not discussed at all.

I think that a few people will read the report: a few have done so, and seem to care about it: but of real discussion there was absolutely none.

The only real discussion I had was (a) in correspondence with Greenhill, who considers the curves "normal"; and (b) a conversation with George Darwin, who seemed to take the same view: but he was very quiet, and did not say much; naturally, in his present condition he

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did not want to be worried.

About skewness I will write tomorrow. —

Yours very truly

W F R Weldon

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30a, WIMPOLE STREET, W.

3rd March 1895

Dear Mr. Galton,

Please put this letter aside, if you are not stronger, — and read it only when you feel inclined to do so.

The more I think over Pearson's theory of Mean variation, the less evidence I see that it is a real thing: and I want to tell you why.

Consider the enclosed curve No. 1. This is the curve of frequency of ~~[illegible]~~ frontal breadths in 6000 crabs between 8·0 and 14·0 mm. in carapace length.

This curve is asymmetrical. But to treat this curve as a measure of the law of variation of crabs at any stage

of growth is, of course, absurd, because while crabs grow from 8 mm. to 14 mm. in carapace length the mean [redacted] frontal breadth is constantly changing.

As you know, in our report, the mean [redacted] frontal breadth, and the quartile deviation, were determined in every group of crabs having carapace-length the same to within 0·2 mm. - So that the material in curve No. 1 was sorted into 30 groups, each varying about a different mean. Drawing the 30 curves, and summing the corresponding ordinates, the dotted curve in Fig. 1 was constructed. - The 30 means are indicated along the base line.

This is a good example of an

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apparent asymmetry during growth, due to the fact that the mean is changing during the period of observation.

Such a curve is precisely of the form said by Pearson to be characteristic of biological measurements. — I do not know his method: but he explains in his abstract that the difference between this and a normal curve is of the same kind as the difference between the curve of frequency of a given suit in dealing a pack of cards. — The fall of each card changing the chance of a given suit falling next time.

Now consider the chance of

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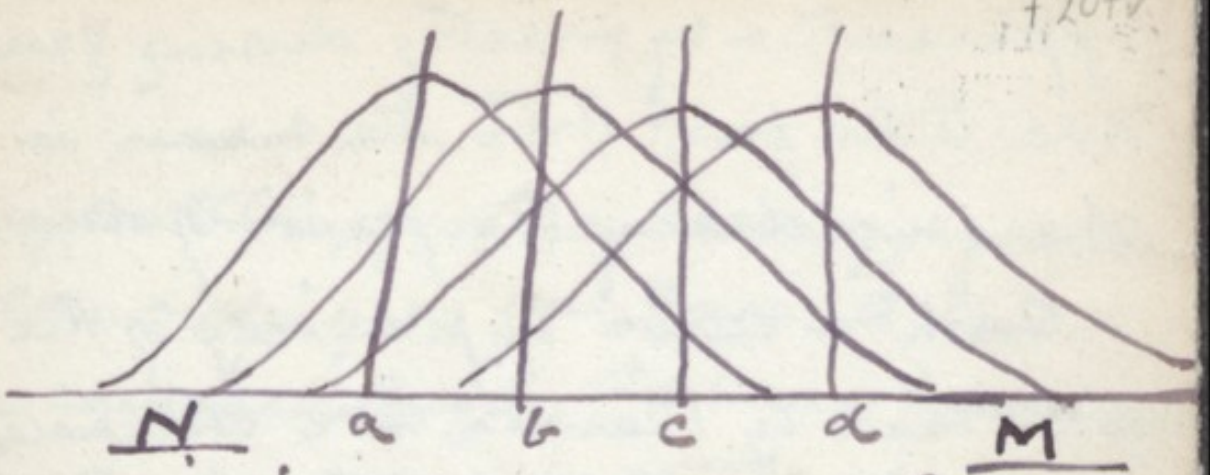

contributions from 4 sets of things, varying about a, b, c, d as mean positions. At N, the chance of a contribution from the ~~red~~ group whose mean is at ~~3~~ d is very small: at M the chance of a contribution from the group with mean at a is small. In the middle, the chance of a contribution from any one of the groups is considerable. Now

1. — Pearson admits that his method does not distinguish between this case and the case of organs varying throughout the period of observation

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(2.)

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about one mean.

2.— He admits that many adult curves are more nearly normal than curves of frequency taken during growth.

3.— He admits that the amount of skewness during growth varies.

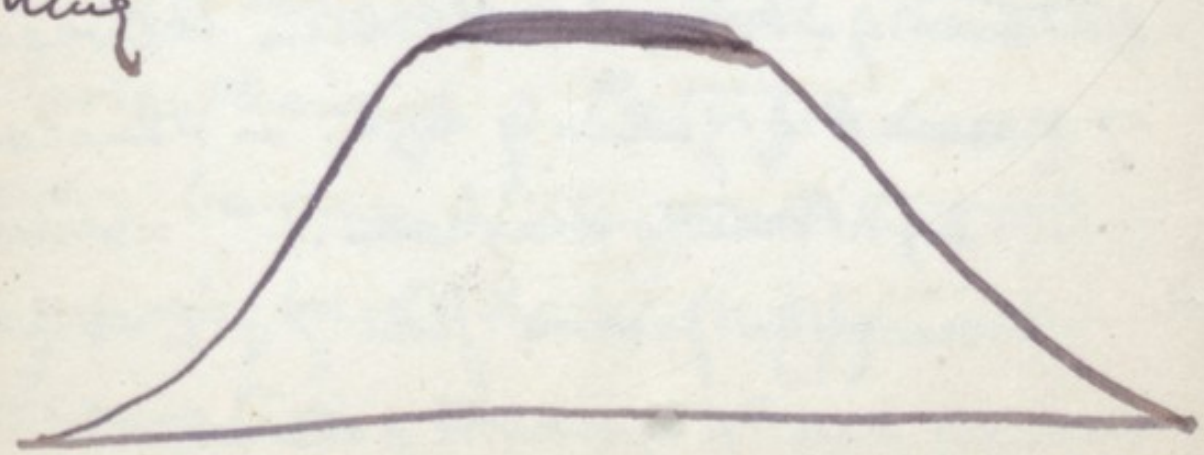
All his examples are examples of cases in which the mean is decidedly changing very fast.

As a type, take Porter's curve of statures of St. Louis children between 7½ and 8½ years of age. — Enclose (Fig. 2) Porter's diagram.

During the period from 7½ to 8½ years of age (an eighth of the whole

~~age~~ magnitude measured is a curious thing to take as ~~dose~~ !)
the mean stature changes 4·8%
of its initial magnitude.

Therefore, if there were the same number of children born in every month, Porter's curve should be the sum of twelve ~~curves~~ equal probability curves. Then, if each curve were normal, the result would be a flat-topped symmetrical thing



But the numbers of births in every month is not the same.

Porter's measures were all taken on 10 days in January: and the age of every child was that of its nearest birth-day.

Therefore, every child, whose birth-day fell between January and June, is reckoned in the table as being too old: while every child, whose birth-day fell between June and December, is reckoned as too young.

I take it we may suppose that in St. Louis the frequency of births obeys the same rough law as elsewhere: that is, the births in February or March will be

some 15 or 16 per cent. greater than in August or September.

So that the majority of birth-days fall between January and June: and the majority of children in Porter's table are therefore reckoned as too old.

Therefore, there will be an excess of children below the average stature in his scheme: and the curve of frequency should be, as it is, not only flat topped, but asymmetrical, with an excess of − deviations.

It seems to me, then, that the whole skewness of Porter's results may be due to the fact that his curve is based on a long period of observation, during which a

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change in the mean, equal to nearly 5 % of its initial magnitude, has occurred: and that the number of observations corresponding to the various instantaneous positions of the mean is not the same.

In any case, the law expressing Porter's curve is so modified by the great size of the unit of age employed that it is no test of the law of variation between small limits of time from t^0 to $t^0 + dt^0$.

Mutatis mutandis, the same remarks apply to Pearson's other

f 210v

cases — House-rents, Divorces, Pauperism, etc., over long periods of years. — These are all things affected by a social evolution more rapid than any change of their kind ever known: and their mean character changes almost from day to day.

I hope you are strong enough now not to be worried by so long a letter. But if not, I hope you will throw it away!

Yours very truly

W F R Weldon

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30a, WIMPOLE STREET, W.

6th March 1895

Dear Mr. Galton,

Let me congratulate you heartily upon your recovery: I shall look forward with great pleasure to seeing you next week.

Pearson does admit that he omits to consider the moving mean in his theory of skew curves, — or he did so nearly a fortnight ago, when I charged him with it. — We had a delightful afternoon, almost each other in a friendly way about this point for some hours: he promised more consideration of it: but since then he has been in bed with influenza. — I hope he will be well enough to take a short

holiday in a day or two: but he cannot work for a little time.

Ten of our men at University College, and in many classes half the students are in bed: so that I, who never get anything worse than a bad cold, feel like the Wandering Jew in time of plague.

About the mathematicians.

I feel the force of what you say, naturally. But I am horribly afraid of pure mathematicians with no experimental training.

Consider Pearson. He speaks of the curve of frontal breadths, tabulated in the report, as being a disgraceful approximation to

importance as any other property of the figure.

For this reason he has fitted a "skew" curve to my "portage breadths". This skew curve fits the dozen observations at the two ends better than a normal curve: it fits the rest of the curve, including more than 90% of the observations, worse.

Now this sort of thing is always being done by Pearson, and by any "pure" mathematician.

Greenhill, to whom I took my troubles, laughs at the whole thing.

You know that his chief business is to teach the properties of probability surfaces to artillery officers in connection with target practice;

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30A, WIMPOLE STREET, W.

and he has a good deal of experience of curves made with gum guiacum & cream of pins.

Greenhill is quite ready to admit the necessity of ignoring the few aberrant observations. George Darwin says that Pearson pays too much attention to the higher moments (which of course depend chiefly on the character of extreme observations).

Now these are the two men working at the applications of probability who know, not only mathematics, but the degree of approximation to be expected from an Experiment.

This sort of instinct as to what

may be expected of an experiment and what may not is a [redacted] quality very rare among young mathematicians, so far as I know them.

But enough of this. — I shall look forward to your proposals next week.

Yours very truly

W F R Weldon

The Herring, which make a skew curve are very heterogeneous. The mean value of the length from snout to anus, on 717 males,

was very widely different from that given by 990 females, — the Extra 270 being obtained by opening another of the cases of herrings. I have not the figures at hand, because I sent them to Pearson, as basis for his curve: but he says that "the material is homogeneous, with skew variation about one mean." I don't believe it!

Many thanks for the cheque.

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30a, WIMPOLE STREET, W.

18th March 1895

Dear Mr. Galton,

Darwin sent me a note this morning, asking that the Committee meeting might be held on Saturday next or on Monday next at four. — He cannot come on Friday.

If Monday will suit you, will you send a note to Rix, asking him to call for that date? — Never mind answering me.

=

I saw Darwin for a little while after the Croonian Lecture, and

asked him whether one could not poison seeds. — He seemed to think the thing quite possible.

The point was, to see how many kinds of external accident could be shown to kill organisms at the rate $= 1 - e^{-hx^2}$. — If there is really any value at all in the estimate of the death-rate among crabs, then the thing should be pretty general.

I would propose to take several samples of (say) mustard seed: to soak each sample for (say) six hours in salt solution, the strength of the solution to be

different for different samples. The seeds could then be sown, and the percentage which germinate determined in each case. — Of course if the expression in the crab report is general, the expression for the number which do not germinate should be

$$y = 1 - e^{-hx^2}$$

where x is the strength of the salt solution, $\frac{1}{\sqrt{h}}$ the modulus, and y the number of seeds killed.

Darwin will tell us the range of salt solution strengths to try, and either he or one of his

pupils will probably help. — 96
one or two people took charge
each of a part of the curve.
— Say one person working with
salt from 0 to 5 per cent., one
from 5 to 10 per cent., and so on,
the thing could be quickly done.
Will you consider this?

Yours very truly
W F R Weldon

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A pupil of mine, now at Plymouth,
is growing crabs, and will send
me their shells as they cast them.
So that the charge of ~~ab~~ deviation
during growth can be determined.

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30A, WIMPOLE STREET, W.

24th March 1895.

Dear Mr. Galton.

Prof. Let me set right a mistake about money.

When you sent me £50 the other day, I cashed your cheque and thought no more of the matter. On preparing a statement for tomorrow's meeting, I find that you have sent me altogether £125, — namely £50 in July, £25 in November, and £50 in March.

As the whole amount belonging to the Committee is £100, I return you £25.

Even so, I have £15 more than I should have, because the herring

have only cost £84-15s.

As to the seeds:

I do not suppose that the curve of selected survivors, after ordeal by salt solution, will be normal.

But it seems to me that the addition of salt solution to seeds is a ~~phenomenon of the~~ factor of the same importance as the addition of length of a piece of carapace to a crab, or of electrical stimulus to a muscle: and I want to find out whether the agreement between the result from crab and the result from muscle is a mere coincidence, or

not. — The experiment is so simple, that a preliminary result will not take long to get: and if you think well, I will get such a result, before troubling the committee.

Yours very truly

W F R Weldon

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30a, WIMPOLE STREET, W.

28th March, 1895.

Dear Mr. Galton,

I have written such account as I could to Mr. Barlow.

Shall I send you the Chrysanthemum curves here or to you, or not? — I want to go away for a little while myself on Tuesday, so I shall be glad to know soon.

Yours very truly

W. F. R. Weldon

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30a, WIMPOLE STREET, W.

23rd April 1895.

Dear Mr. Galton,

Such an analysis as you suggest is much the sort of thing which I supposed I would have to be made of Strickland's papers.

My principal difficulty in attempting such a thing is that I fail to see the obvious connection between the maxima of Strickland's curves and the spiral arrangement of the florets on their stem, of which botanists always speak so much: and until we can understand this, there seems little hope of

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writing an abstract which would be satisfactory to obtain it.

I have just received from Plymouth a little batch of shells newly moulted by crabs, which are being kept in captivity so that their rate of growth may be checked at each successive moult.

Yours very truly

W. F. R. Weldon

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f 221

30a, WIMPOLE STREET, W.

25th April 1895

Dear Mr. Galton,

Please do not think again about the Philosophical Club! Of course I am sorry to have lost an evening's talk with you: but for the rest, it is not worth your kind regrets.

I did not come to the Royal Society, because I was detained by three very interesting dogs. — You remember that Bateson puts the question "what is the least possible size of an extra tooth, when present?" — And he considers that the least possible extra tooth is a considerable calcified mass: so that variation in teeth can only move by steps of this magnitude.

Bateson has not himself examined a very large series of dogs: and both his own results, and those quoted by him, are based upon dried skulls, from which all the soft parts have been macerated away: so that soft teeth, in an "Embryonic" condition would have been lost in these cases, even if they had occurred.

In several dried dog's skulls, which I have to use for teaching purposes, I have noticed slight depressions behind the last cheek tooth, which might be due to the presence of minute, uncalcified tooth rudiments.

I have therefore made an arrangement with Horsley and Schäfer, by which all the dogs from the

physiological Laboratory will be given to me. — Some three or four unhappy dogs are done to death every day in this Laboratory!

Today three dogs gave two cases of such extra teeth: one a quite soft, uncalcified rudiment, the other a very minute calcified particle. One dog of the three had also two teeth absent.

I hope to go through one or two hundred dogs in this way, and to check Baldwin's work by methods which give some chance of finding small rudiments.

The crabs at Plymouth (or rather, some of them) have moulted

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twice: so that there are two successive stages in the growth of the same individual available for measurement. — Twenty crabs are being kept at Plymouth: and as three weeks seem to elapse between ~~[illegible]~~ ~~pair~~ the moults, three more moults will occur this term. — Early in July, I propose to go to Plymouth, and to carry about 100 through two moults at least. — Do you think this will be sufficient material for a determination of the law of growth, to be reported on in October?

2

The new attempt to grow Daphnia has had to wait for warmer weather.

[25.4.95] f223

30a, WIMPOLE STREET, W.

The creatures spend the winter as eggs: but I hope that on Sunday (the only day on which I can get out the country) I shall be able to find some individuals already hatched.

Yours very truly

W F R Weldon

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f224r

30a, WIMPOLE STREET, W.

8th May 1895

Dear Mr. Galton,

I should like you to look at the following figures before tomorrow night, if you do not mind doing so.

I have just received the 2nd. series of cast off shells from 9 crabs, which are kept growing for me at Plymouth by my friend Allen, — the director of the Laboratory there.

Each shell has been measured, and the following table shows the abnormality of each crab at its first and at its second moult. — The abnormalities in thousandths of the

carapace length, ~~and~~ so that it is comparable with the figures in the published paper.

Abnormality in frontal breadth

	At First Notch	At 2nd. Notch
Crab No. 1.	25	18
2	23	15
3	13	11
4	0	4
5	2	3
6	10	8
7	16	18
9	2	2
10	5	10

f225r

Remembering that I only pretend to measure to '004 of the carapace, so that 4 of the above units count as one, I think the results look as well as might have been expected: but there are two things:— the curve showing change of mean value of frontal breadth with increase of carapace length is very irregular (see Table I and Fig. 2 of the crab report) and it will be necessary in order to obtain a good mean value from which to measure abnormality in each case, to correct this by more measurements. — The means from which the numbers I give you are reckoned were got by drawing a free hand curve thro'

a carefully enlarged drawing of Fig: 2 of the crab paper. — But this is, of course, too rough a method for subsequent use.

Also, there is evidently a big, or at least a very appreciable Prob: Error in the Expectation of abnormality in the future, based upon present abnormality: so that many of measures of young must be made.

What I should like to do is to measure a large quantity more little crabs this term, so as to correct Table I: and having done this, to rush down to Plymouth and devote the Long Vacation to rearing at least 250 young through three

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30A, WIMPOLE STREET, W.

moults if time allows, but at least through two.

But I cannot do this and look after Daphnia too. What do you advise?

My own feeling is strongly that I know the rearing of crabs to be possible — while I do not know that I can in any given time find out how to avoid modification of the Daphnias through confinement: and also, the record of a fairly large number of individuals at various periods of growth would be a unique piece of statistical information, directly arising out of the previous work, and having a

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most important bearing on the Darwinian theory. — You see that in the small number of observations given already, there is certainly no marked increase of abnormality during growth!

Will you advise me tomorrow?

Yours very truly

W F R Weldon

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Will you also consider whether the Committee will feel inclined to pay for the bottles necessary (say 300 green bottles, such as cheap sweetmeats are kept in by confectioners)?

These bottles will be placed in large tanks of sea-water, one little crab in each bottle.

I do not know the exact price: but the Challenger paid £70 for about 2 300 of these bottles.

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F 228r

30a, WIMPOLE STREET, W.

24th May 1895

Dear Mr. Galton,

Please forgive me for not having answered your letter, and for having managed to miss you at the Royal Society. — I got there this afternoon just after you had left.

For my part, I see nothing possible with regard to the Chrysanthemum tables, except to publish them as too complex for present analysis, with such remarks as you wish to make. — The difficulty seems to me the want of a method by which a compound of three curves can be resolved.

I wish that we dare leave plants alone for a while, & only consider data brought by some one like Darwin, who could tell us more in detail the conditions of growth!

In ~~collecting~~ considering plants from a large field, & each separate area of the field seems to me to be influenced by one or two dominant conditions, which are few and constant, and perhaps cannot affect the rest of the field at all. — Such are, for example the shade of a hedge; the moisture kept constantly in except along a definite line by a drainage ditch; and so on. — Fixed plants will separate themselves into groups,

each group permanently under the influence of some one or two big dominant causes of this kind, and so the curve of distribution of the whole plants of the field will tend to have several maxima.

~~Again~~ Animals, forced to hunt over the whole fields, will, on the other hand, be on the whole subject to a complex of causes made up of all these separate elements, even in the case of one individual.

Again the primroses are an example of another kind of complexity due to asexual multiplication.

Suppose the original seeds of the primroses examined to have been so distributed by chance that the abnormal flowers were uniformly scattered at first: the

subsequent to growth of the plants would produce just such segregation of clumps of abnormal individuals as appears to occur: and the difficulty of getting a fair sample of the race is largely increased by this fact.

I fancy fixed animals, which had, would lead to precisely such complex curves of distribution.

2.

I have today seen the beginning of a more cheerful and simple result.

My junior demonstrator has measured, in a crab closely allied to mine, but of another genus, all those dimensions which I had measured in order to see how far the coefficient of correlation is the same in two

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30A, WIMPOLE STREET, W.

closely allied genera.

He has measured 1358 individuals. He will make up 1400, for greater case in the subsequent arithmetic.

Today I plotted a beautifully normal curve from one of his sets of measures: and the others look very nice.

This final result will be ready for the Royal Society in October: and when it is ready, I will ask you whether it cannot be communicated in his name & the Committee.

Yours very truly

W F R Weldon

F 231

30a, WIMPOLE STREET, W.

31st May 1895

Dear Mr. Galton,

I have just received the enclosed. — I suppose that although this permission is sent to me it will be proper for you to apply for the money?

Darwin tells me that Strickland will want his material published before a book full can be obtained. — He has already complained to Darwin of the delay in publication.

Can you arrange your summary for publication, with the full table of frequencies, but without any attempt to dissect the frequency curve?

I had some talk today with both

f231v

Darwin & Marshall Ward. Neither could see why any particular number of petals etc. should be more stable than another for a given phyllotactic spiral.

Plants are an unmitigated nuisance to me, since I have mixed myself up with the silly row about the Cineraria: But I should like you to tell me, if you do not mind, whether in your opinion Mrs. London's article (of which I enclose a copy) bears the interpretation which Bateson puts upon it.

Yours very truly

W. F. R. Weldon

 [found after 28·10·95]

Most of the purple Cinerarias are varieties, or hybrids, of C. cruenta, which has bright purple flowers which are very pagrant, and leaves which appear stained with crimson on the under side; and hence the name, cruenta signifying blood. This species is a native of the Canary Isles, etc. etc. ..... About 1827 (fifty years after its first introduction) Mr. Drummond ~~of Cork~~ Curator of the Botanic Garden in Cork, having raised it from seed, and found the seedlings vary considerably, conceived the idea of hybridizing it with C. lanata; C. geifolia; and C. amelloides. These trials, however, do not appear to have succeeded with C. geifolia, which has yellow flowers, and C. amelloides which has blue ones, both being natives of the Cape; but between C. cruenta and C. lanata, the flowers of which were of nearly the same colour, some handsome hybrids were raised. Since that time numerous experiments have been made, and hybrids have been raised from C. cruenta C. aurita (a purple-flowered plant, etc. ...) C. Tupilaginis (a handsome ~~spe~~ species etc. ...) and C. populifolia. Cineraria tupilaginis has two synonyms; viz. Senecio tupilaginis ... and Pericallis tupilaginis.

Some of the most beautiful Cinerarias now in our greenhouses have been raised by Messrs. Henderson, Pine Apple Place; particularly C. Hendersonii and the King, both raised from seeds of C. cruenta. C. Waterhouseana was raised by Mr Tate, ... from seed of C. Tupilaginis,

fertilised by the pollen of C. cruenta. Two new ones have lately been raised, of remarkably clear and brilliant colours, apparently from C. cruenta, named Queen Victoria and Prince Albert, by Mr. Pierce, nurseryman, of Yeovil, Somersetshire.

It will be noted that the "general statement," with which the article commences, is

"Most of our purple Cinerarias are varieties, or hybrids, etc."

So that, in the general statement, it is expressly said that some of our purple Cinerarias are varieties. Of Cinerarias which are not purple (such as the ring) nothing is said.

Then follows a list of hybrids, then a list of varieties.

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f 234r

30A, WIMPOLE STREET, W.

23rd June 1895

Dear Mr. Galton,

I cannot think of any thing needing discussion before you go.

On Friday evening my own work ends, and on Saturday evening I hope to be in Plymouth. — Five hundred bottles, with a stream of sea-water for each, will be ready: and I hope to put a crab in each for observation during the vacation. The average time between two moults is 30 days. — I have arranged to stay 8 weeks, so as to be sure of 2 successive moults from each crab. — This will give 500 pairs of measures, showing abnormality of the same individual at 2 periods of life in 500 cases. — It ought to be enough to determine the laws

of growth, and so to settle the selection question for one case.

While the crabs are growing, I shall measure others, so as to get a better determination of M and Q values at each age than those given in the paper. — I hope to get another 7000 done: but this is rather doubtful. — If I can, it will also settle the not less important question, whether the deviations of more than three times the modulus, on which Pearson lays such stress, are of regular occurrence or not.

Do you know what Poulton is doing about Rockchafers?

I hope you will not think two stages of growth too few for each crab. — I

f235

dare not stay in Plymouth beyond the end of August. — London makes me fairly weak, and Plymouth makes me worse: so that it is my plain duty to University College, as well as a great joy, to spend September somewhere where there is sunshine and exercise, and nothing to measure!

I hope you will enjoy yourself and get strength also.

Yours very truly

W. F. R. Weldon

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

What a pretty thing Pearson's general explanation for heredity is!

f236r

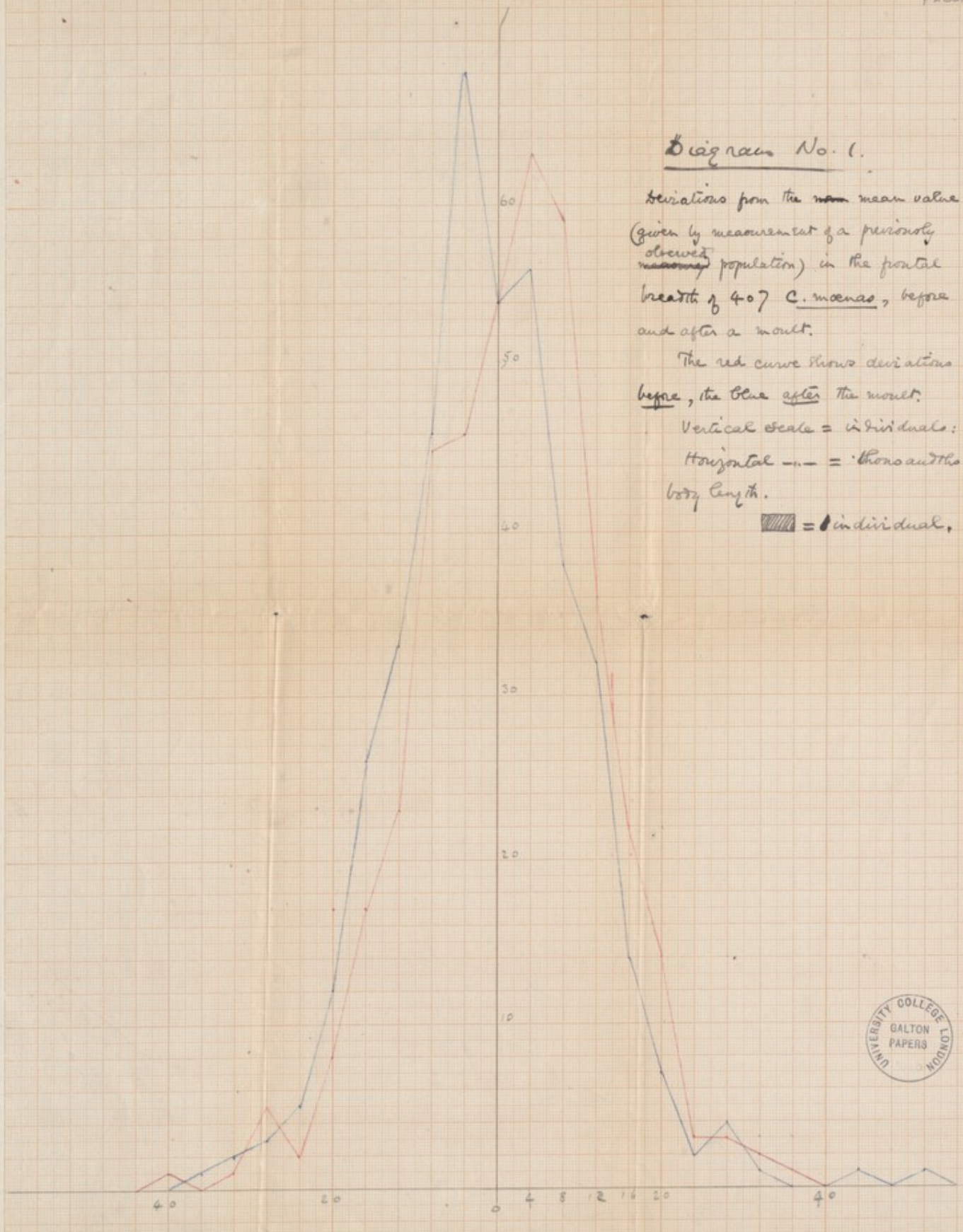

Diagram No. 1.

Deviations from the ~~mean~~ mean value (given by measurement of a previously ~~measured~~ observed population) in the frontal breadth of 407 C. moenas, before and after a moult.

The red curve shows deviations before, the blue after the moult.

Vertical scale = individuals:

Horizontal —"— = thousandths of body length.

▨ = 1 individual.

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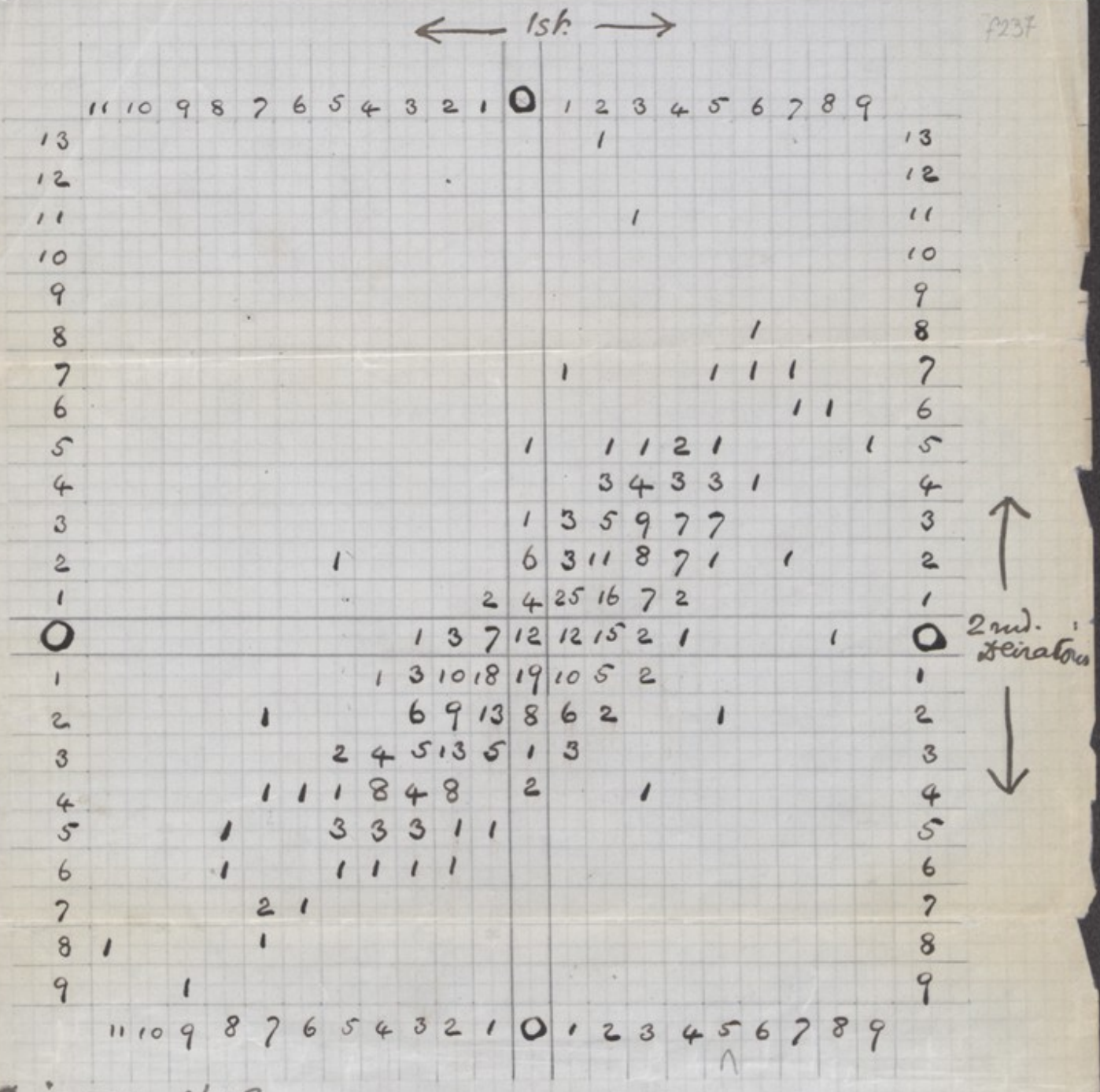
Paper guaranteed one millimeter Cattel & Farey Lens

[with 20.8.95] f236v

[Crabs.
Frequency distributions.
♂ deviations from mean
before & after moulting]

f237

← 1st. →



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	11	10	9	8	7	6	5	4	3	2	1	0	1	2	3	4	5	6	7	8	9	

↑ 2nd. Deviations ↓

Diagram No. 2.

407 C. Moenas ♀ :– Abnormality of 1st. & 2nd. Measures of Frontal Breadths. – ~~1st Abno~~ Deviation at 1st. measure = x; Deviation at 2nd. Measure = y.

Centroid 1st. measures = +0·29

—— 2nd —— = −0·27

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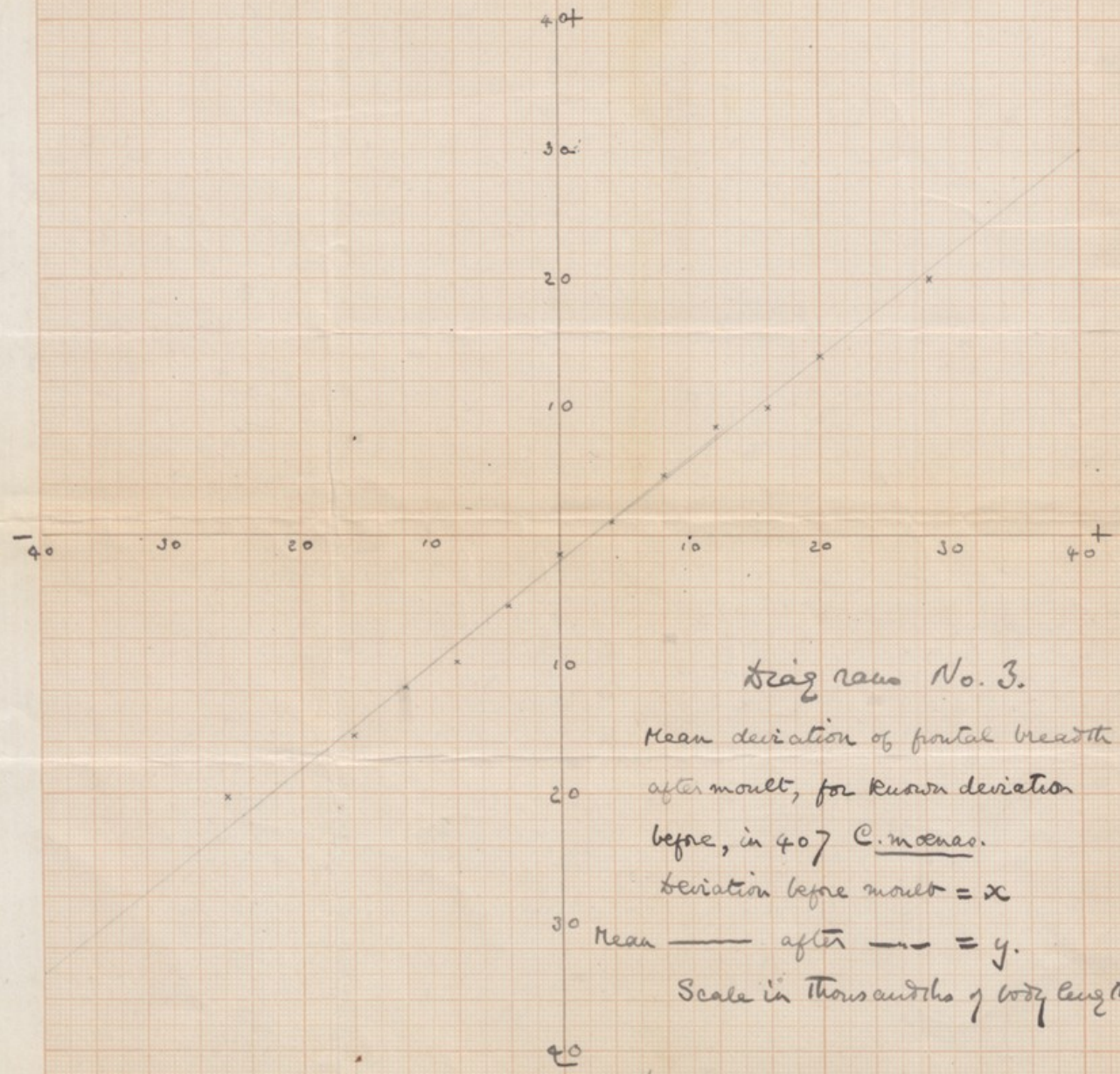

Diagram No. 3.

Mean deviation of frontal breadth after moult, for known deviation before, in 407 C. moenas.

Deviation before moult = x

Mean ——— after ——— = y.

Scale in Thousandths of body length.

The straight line is

$y = 0.8x - 2$

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GALTON PAPERS

f 239

Marine Biological Laboratory,
Citadel Hill, Plymouth.
20th August 1895.

Dear Mr. Galton,

Tomorrow I go South. Before going, I send you a rough sketch of what I have done with the crabs. Please remember that it is only a rough result, and that the arithmetic is unchecked. Therefore, if you like, throw the whole thing away without further thought: only, I believe it shows the general result fairly well. It is based on 40% of the observations only.

1.— Method.— The apparatus used is shown in the photograph. It consists of 460 bottles, each holding about a litre of sea-water, through each of which a current of sea-water (so slow as not to disturb the crabs) was kept flowing by means of initially arranged glass siphons. The bottles were numbered, and each bottle contained a crab. The crabs were supplied every morning with fresh food, more in quantity than they could eat during 24 hours, and the food remaining from the day before was carefully removed. At suitable intervals every bottle was thoroughly cleaned with an india-rubber scrubber. By this means the death-rate was kept at a fairly low limit,— the only serious death-rate having occurred on a certain Sunday, when owing to a gale there was scarcity of fresh food, and two stale mackerel were used.

Crabs, of the proper size, were kept in the bottles until they moulted. The cast skin of every crab which had moulted was removed and measured. Two days after a moult, a crab is about 1·3 times its former length, and has secreted a new hard shell: therefore, two days after moulting, the crab was killed, and its new shell measured[1].

In this way, about 580 sets of observations

[1] The bottle was then re-numbered, and a new crab placed with it.

have been obtained, each set containing the carapace-length & frontal breadth of one crab at two periods of its life.

From these observations, the ratio $\frac{\text{Frontal Breadth}}{\text{Carapace Length}}$ was determined, as in the previous observations.

The eccentricity of this ratio was then determined in each case by reference to the mean values as given in the previous paper. — The curve of mean values for varying size of carapace-length, given in fig. 2 of that paper, was drawn on a large scale, and from it a series of points was determined, each point being the mean between two observations in fig. 2. — Joining these points by straight lines, a curve was obtained, from which the mean value of Frontal Breadth for each carapace-length was found. This method is rough, & will have to be replaced by a better: but the work of looking over the live crabs, & keeping their measurements up to date, has taken about ten hours every day: and there has been no time to measure more dead crabs to increase the value of the means. Some two or three thousand of such measures exist, but it was not worth while to add so few, seeing that $\sqrt{7000}$ and $\sqrt{9000}$ are for this sort of purpose much the same.

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2.— Results.—

It was at first hoped that a direct comparison between the ~~poste~~ Quartile Error of the population dealt with at the first moult, and the corresponding Q afterwards, would give an indication of the result. This was found to be impossible, because neither population had its frontal breadths distributed according to a normal curve. The mean crab, of the population introduced into the bottles, was distributed, with marked a-symmetry, about a

maximum ordinate of deviation +4, so that its Q does not measure the slope of the curve properly.

The same population, after moulting, was distributed about a maximum ordinate −4, also with marked asymmetry. See diagram No. 1, enclosed.

In diagram No. 2, the 407 crabs are plotted with respect both to their first and their second deviations: so that the correlation between the two can be determined. (This diagram is made in the units I always work with, namely 0·004 of the carapace-length: so that the deviation of each crab must be multiplied by 4 to compare with other diagrams.)

The mean 2nd. deviation for every known 1st. deviation is given in diagram No. 3: and I think you will agree that the means lie roughly on the line $y = 0{\cdot}8x - 2$, although a more exact ~~Theory~~ locus must be found later.

The Q of the distribution of 2nd. deviations about the mean indicated by this line is slightly irregular as yet: but we may take it at 4 thousandths of the carapace-length.

So that, calling M_1 the mean, and Q_1 the Quartile deviation, of frontal breadths at the first measurement, and M_2 and Q_2 the corresponding values at the second measurement, we have

$$M_2 = M_1 - 2; \text{ and } Q_2 = \sqrt{0{\cdot}64 Q_1^2 + 16}$$

So that, in order that the Q should increase with growth, it is necessary that

$$0{\cdot}64\, Q_{\equiv}^2 + 16 > Q_{\equiv}^2$$

$$\text{or } 16 > 0{\cdot}36\, Q^2$$

$$\text{or } 4 > 0{\cdot}6\, Q$$

$$\text{or } Q < 6{\cdot}8{\cdot}3$$

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Now, the Q of all the crabs measured by me has been over 9; and therefore, ~~[illegible]~~ this result is curious.

So far, we have not considered (a) the asymmetry of the initial population, (b) the change of mean.

(a) The initial asymmetry is, I hope, due to selection. The crabs were caught for me by small boys: and many died from rough handling before they entered the experiment. — I am preserving all the crabs of a fresh sample collected by the same small boys, in the same way, to check this.

The final asymmetry is of course a result of the initial asymmetry.

(b) The change of mean is due partly to the conditions of life in the bottles, ~~partly to the~~ and is partly correlated with the rate of growth at the moult.

(a) rate of growth at moult:

This has been estimated by dividing the number representing carapace-length at 1st measure into the number representing carapace-length at 2nd. measure. — ~~It is about 1·2~~ The ratio of the two is about 1·27, with a Q of about 0·03; so that of crabs 10 mm. long before 1st moult, half the individuals are between 12·4 and 13·0 mm. long after the moult. — The growth occurs very quickly (in 12 hours, certainly): then the shell hardens, and the dimensions of the crab remain of almost constant for a month, — i.e., until the next moult.

In the following table the ~~deviation of a~~ mean deviation of a crab of known growth rate is given both before and after the moult during which the growth occurs. [The table is made from 417 individuals.]

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[with 20.8.95 (a)]

Growth Rate = Length before Moult / Length after Moult	Mean Deviation at 1st. Moult	Mean Deviation at 2nd. Moult	Amount and Direction of Change of Mean	Number of Individuals
Under 1·22	+1·07	+3·41	+2·36	47
1·22 to 1·23	+2·72	+1·84	−0·88	48
1·24 to 1·25	+0·23	−0·62	−0·85	70
1·26 to 1·27	+0·75	−1·67	−2·42	77
1·28 to 1·29	−0·67	−3·06	−2·39	65
1·30 to 1·31	+1·63	−2·82	−4·45	54
over 1·31	+3·47	−2·65	−6·12	56

So that evidently there is only slight correlation between the rate of growth during a moult and the previous deviation; but there is considerable correlation between rate of growth and subsequent deviation.

(β) The subsequent deviation is also largely influenced by the time spent in ~~captivity~~ captivity: The following table shows the ~~rate~~ amount and direction of change of mean in crabs which had spent a known number of days in the apparatus before moulting.

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Time spent in Bottles before 1st. Moult	Mean Deviation Before 1st. Moult	Mean Deviation after 1st. Moult.	Direction & amount of Change of Mean.
1–5 days	+1·62	+1·61	−0·01
6–10 —"—	+0·36	−1·56	−1·92
11–15 —"—	+4·01	−1·90	−5·91
16–20 —"—	+0·76	−3·16	−3·92
21–25 —"—	+2·75	+1·21	−1·54
26–30 —"—	+4·89	+0·66	−4·23
31–35 —"—	+1·14	+0·88	−0·26

(The last entry rests only on 13 individuals) ※

※ A few Crabs have spent 40 days in the bottles without moulting. These are not included, at present, in the table.

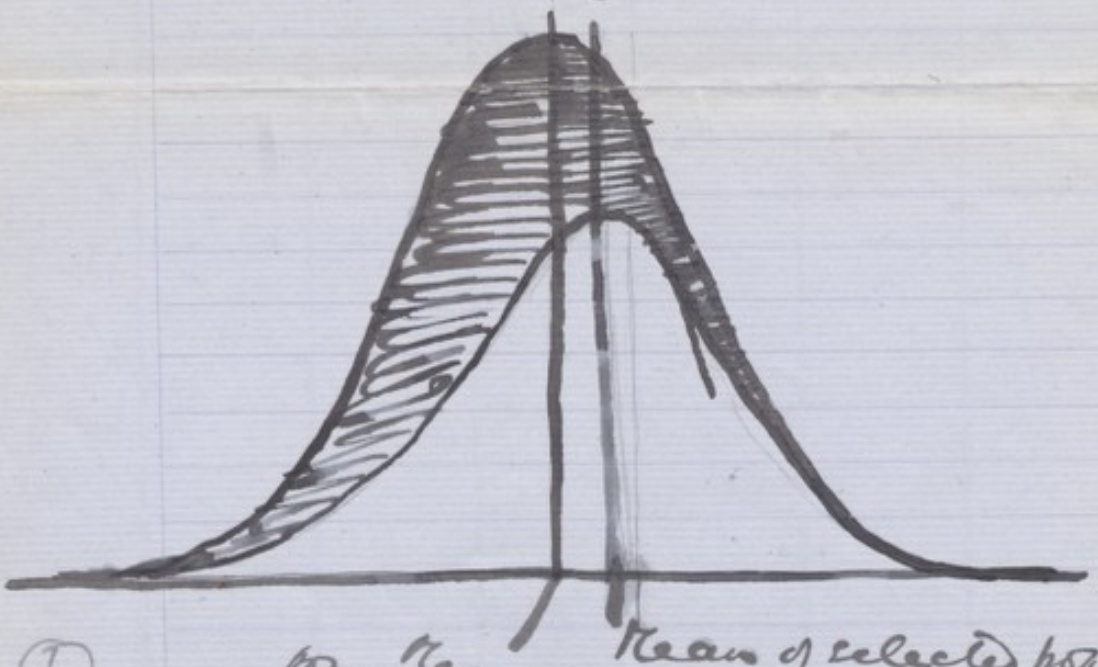
[with 20·8·95.(b)] f244

So that the variable, "Deviation after a given moult," depends measurably upon three others,

1.– Deviation before the moult;
2.– Rate of growth during the moult;
3.– Conditions of environment during the moult and for some time previously.

The problem of measuring Natural Selection depends upon the answer to the question, whether the correlation between 1, 2, and 4 is so much greater than that between (3) and the others, that a change in (3) would not materially affect the result.

If that be so, then a ~~normally distributed~~ population of crabs normally distributed about the mean of the race before a given moult, tends to give rise, after its moult, to a population distributed normally about a mean negative to that of the race: and selection will occur as in the diagram:—



(b.)

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You notice that this involves a greater mortality among crabs of negative deviation than among those of + deviation. — Now, my initial population, which had been subjected to the ordeal of rough treatment by small boys, was a-symmetrical in precisely this way:— it had too few negative deviations, and its mean had ∴ become +,

It may be that the correlation constants from this single set of observations will determine the point, when I get back to Town, and learn to work with 4 variables (if I can ever learn Edgeworth's papers!) — But I am making arrangements to test the thing experimentally, by trying to keep 100 crabs alive at University College next term. They will be under conditions of food, light, temperature, etc., as different as may be from those reared here: but the death-rate will be very great, and the result very long in coming, I fear.

Forgive me for sending you so long a letter in the middle of the Vacation. — And do not trouble about an answer for some time. — I am going to wander in the Apennines for a while, and shall probably not return to Trinity Street till the last week in September. — I have written because I thought, if you were in England, you would care to hear as soon as possible what had come of the experiment: and I think you will agree that it promises a great deal. Do not think me a lazy brute for going away now. — It was not till a week ago that I could plot enough observations to show the need for a repetition under different conditions: and I am not physically fit to start a new seven weeks' experiment now!

Yours very truly W F R Weldon

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(7)

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F.246

30a, WIMPOLE STREET, W.

6th October 1895

Dear Mr. Galton,

Your scheme of measuring horses sounds very interesting, but I fear I cannot help you, at least at once. — I know no one at the Veterinary College: but I will look about, during the next few days, and write to you if anything turns up. Since you mention my College report, let me tell you that I said a good deal more about Pearson than appeared in the "Times." — I was glad of an opportunity of saying how much I really value his work: because in talking of it, both to him and to them, I spend so much time in trying to criticise ~~work~~ points of detail, that I fear I sometimes seem to

f246v

beat it unfairly. — Of course, at such times, one takes the good for granted, and tries to find better ways of treating details, and especially in the assumption that all curves of growing animals are askew. I think he has gone too far. But equally of course, I see the great importance of the work as a whole, and took this opportunity of saying so as heartily as I could.

2 For the crab work, at present I propose to measure some more Plymouth crabs: first, to get more values outside the range of those in the first paper, because some of the crabs grown in the summer grew so much, so that they lie outside that paper, and

f247r

secondly, to make sure whether the mean frontal breadth has sensibly changed during three years.

Until that is done, I do not see the use of a Committee meeting, unless Poulton has measured his Cockchafers. — What do you think?

2

I hope you had a good holiday. — The Apennines were more gorgeous this year than ever. The burnt grass was bright gold colour, and the bilberries went crimson early in August. — In Florence, I bought a nice old folio Giovanni Villani, about which I want to ask if you know the length of the old Florentine braccia?

He says, in his chapter of the

f 247v

Florentine mile, "Questa fu la
"consuetudine e usanza de' Fiorentini,
"el migliaio sie M passini, el
"passino s'intende tre bracia alla
"nostra misura."

Now, from the present length of a Tuscan mile (1800 yards) I feel sure a "passino" is a Roman pace, between two portions of the same foot: but I don't know any measure of a braccio which makes a third of such a distance!

Yours very truly

W. F. Weldon

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GALTON PAPERS

f 248c

30a, WIMPOLE STREET, W.

13th October 1895

Dear Mr. Galton,

It is very kind of you to have taken so much trouble about the braccio. — The point which excited me was the assumed ratio between an arm-length and a pace length. The ratio, as it seemed to me, did not really exist, and I was interested to find that the braccio had varied so much from the length given by a human arm so early as the 13th. century, or before. — But I did not mean to give you so much trouble.

I have written to one or two people about horses, ~~but~~ and if they give any

information worth sending you, you shall have it at once.

For the rest, I have chiefly been giving good advice to students during the past fortnight: but the crab measures go on somehow, nevertheless. — The clay work at University College is now fairly started, and I shall, I hope, measure faster.

An apparatus is being put together, in which to grow some more crabs under conditions different from those at Plymouth. — In this way perhaps the effect of domestication may be got rid of: at least there will be two points at which it can be measured. —. This set of observations must be on a much smaller scale than the other (only 100 individuals at a

time): and it is very likely to fail.

Yours very truly

W. F. R. Weldon

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f 250

30A, WIMPOLE STREET, W.

28th October 1895

Dear Mr. Galton,

It will give me great pleasure to dine with you on Thursday at 6.30.

In the mean time, will you consider the following?

As I told you in August, the Q of the crabs grown in the bottles appears slightly too small, as compared with the Q of previously measured crabs.

I think I know why.

In all these crabs, the mean value of the frontal breadth is changing rapidly: and, as you may remember,

the crabs whose measurements are published in the last report were sorted into groups, so that the carapace-length of no two individuals in any group differed by more than 0·2 mm.

In the table of the report one finds

Carapace Length	Mean Frontal Breadth
8·20 – 8·39	835·41
8·40 – 8·59	830·08

and so on.

For each of these groups, the Q was determined, about the mean of the whole group: so that the grade of a crab whose carapace-length was 8·39 mm. was determined by

the difference between his frontal breadth and 835·41.

In estimating the abnormality of a crab grown in the bottles, I used a different proce[ss]. — In a crab of 8·39 mm. carapace-length, for example, I interpolated between the two adjacent entries in the old table, and so found (say) 832·7 as the mean frontal breadth corresponding to his carapace-length.

So that a crab of frontal breadth = 830, and carapace-length = 8·29, would appear in the results from the bottles as of abnormality — 2·7 units; in the

old tables he would appear as abnormal to the extent of − 5·4 units.

On the other hand, a crab of say frontal breadth = 837, and the same carapace-length, would appear + 1·59 abnormal on the old plan, and + 4·3 abnormal on the plan used in August.

Of course, in trying to compare the Q values of the crabs in the bottles with the corresponding values of outside measurements, one must be careful to treat both sets of measures in precisely the same way.

Therefore, unless you see reason to the contrary, I shall not use interpolated values in going over

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30A, WIMPOLE STREET, W.

the thing again, to get out the final result.

I hope to begin this in about a fortnight: because by that time I hope to have measures enough to show that the mean crab in Plymouth has not changed during the last four years enough to make the change of importance in these measures.

I am very sorry to have no help about the horses. — They must be much more interesting than crabs!

When this growth business is done,

P252v

will it be very dreadful for me, too, to work at a new subject?

Yours very truly

Wolfgang Wilder

3/3/22/17
f253r

Tolland Bay Hotel, Isle of Wight.

30a, WIMPOLE STREET, W.

5th January 1896

Dear Mr. Galton,

A happy New Year to you!

Your letter has been sent on to me here, where I have been quietly wrestling with the cralo.

I shall, of course, be glad to sign Edgeworth's certificate: but I hope he will not prevent Pearson's election. It seems true that both men should already be fellows!

As to the cralo, I think I see the end now. The arithmetic is by no means over: but so far, I have some very interesting results, after

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3/3/22/17

applying Pearson's formulae.

(1) — The adults give a curve of frontal breadths which even Pearson admits, after seeing the constants, to be symmetrical and normal within the limits he allows.

(2) — The curve of grams put into the bottles last summer is very a-symmetrical: it gives

$$\mu_3 = -13.1968366,$$

so that there are too many individuals of negative deviation.

As I told you before, the crabs were submitted to a severe ordeal before entering the bottles, so that

many died before the experiment began. — Comparing, then, the curve of adults with the curve of young which entered the bottles, there is a suggestion that the animals of + ~~abnormality~~ deviation die more rapidly than those of — ~~abnormality~~ deviation.

But to make sure of this, it was necessary to examine, by Pearson's method, the curves of frequency at different ages given in the old work. This I have been doing: and so far as I have gone at present, I find the value of μ_3 is considerable

and always +.

So that there is in the "wild" young crabs always an asymmetrical distribution with too many + deviations, while in the adults this excess of + deviations vanishes. Again, is the suggestion that selection acts more on the + side than on the − side.

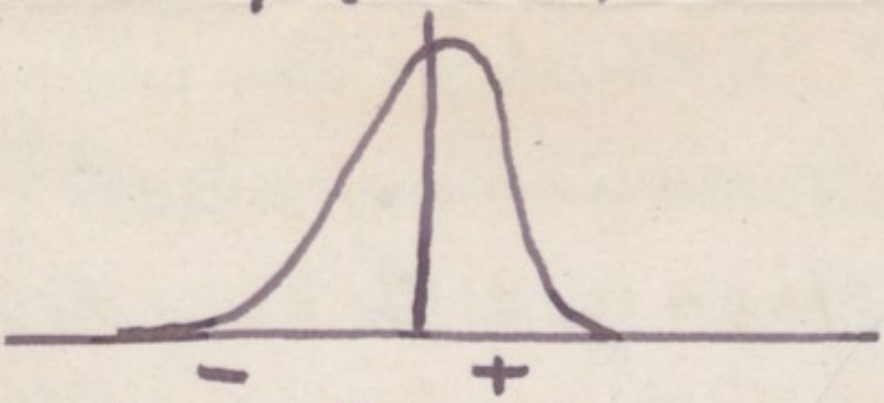
Now, I have not yet found an empirical expression for the very skew correlation between the deviation of young crabs at the beginning of last summer's experiment and the deviations of the same crabs at the end of it. But the

[5·1·96] f255r

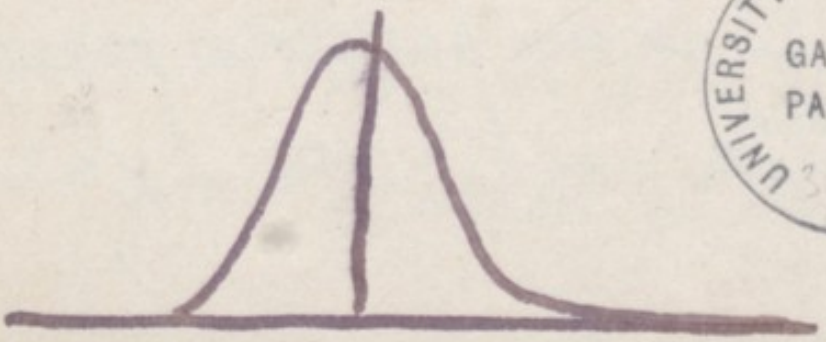
30a, WIMPOLE STREET, W.

Curve of frequency of the whole set of deviations at the end of the experiment has changed so that its μ_3 is positive.

Roughly, the curve at the beginning of the experiment is



with its long "tail" — wards: at the end it is



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GALTON
PAPERS

with its long tail + - wards.

So that the decrease in + deviations during growth is not due to an "adaptive" or other change in the individuals: on the contrary, a protected population tends to develope an excess of those + deviations which disappear from the "wild" crabs with growth.

Therefore, as it seems to me, there is indisputable proof of selection: only, it is not

symmetrically disposed about the mean: but the crabs are like Touchstone's egg, dammed all of one side.

=

It is perhaps interesting, on the palingenetic view of growth, that the youngest crabs have normally the largest frontal breadth ratio: so that one is tempted to regard the ——— tendency to a + deviation as "atavism".

More interesting is an observation of Herbert Thompson's on male crabs from Plymouth. — He is

& was, working out the distribution of frontal breadths in ♂ crabs, at various ages, so as to compare the results with my measures of ♀ crabs.

In 1892-3 we had a quantity of crabs from Plymouth. — He took the males, & the females. — This year, he wanted a fresh stock, to complete his series: and at my suggestion kept the 1892-3 set separate from the new set of measures.

He finds that in crabs of the same carapace length the frontal ratio is always from 1·5 to 2 units less in the 1895 stock

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[5·1·96] P257r

30A, WIMPOLE STREET, W.

than in the others: so that there is a possibility that the ♂ crabs are slowly diminishing their frontal breadth, — i.e., that their mean is being pushed in the direction indicated by the above hypothesis of Selection.

The change is quite slight, of course. — Thompson's unit, like mine, is 0·0001 of the Carapace-length: so that in a crab of 50 mm. the diminution of frontal breadth would only amount to 0·075 mm.; and

f257v

if we assume it to have gone on regularly for a century ~~[illegible]~~ it would not change the crab so that any old pictures would indicate the change. — Therefore it is perhaps fair to hope that the thing may be real. — Anyhow, in two or three years I shall try to get some more ♂ crabs measured!

So far, I do not think the ♀ mean has changed in the

f258

Last 3 years, even to this slight extent.

I shall be in Town on Tuesday & Wednesday, and in a little while now I hope to send you the whole story of the crabs.

Yours very truly

W. F. R. Weldon

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GALTON PAPERS

Weldon

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30A, WIMPOLE STREET, W.

9th January 1896

Dear Mr. Galton,

I will bring Heape's proposals to the Committee.

I return Darwin's letter. Surely £4000 can be raised somehow!

The spurious correlation result is all right. — I sent the whole thing to Pearson, with half a dozen lines, which I asked him to add as a note to his paper, as you suggested. But I also asked him to go through the arithmetic himself. — He has done this, and agrees with me: so that there can, I think, now be no doubt

about it.

The difficulty of getting rid of ratios from the crab work is very ~~great~~ great: because I feel sure the proper statement of the law of increase of size at a moult involves a geometrical progression.

The ratio $\frac{\text{carapace-length after moult}}{\text{carapace-length before moult}}$ is (before moulting)

for crabs 8-9 mm. long — 1·270
—— 9-10 —— 1·277
—— 10-11 —— 1·277
—— 11-12 —— 1·275
—— 12-13 —— 1·270.

Now $\frac{12.5^{mm}}{(1.27)^9} = 1.454$, which is about the carapace-length of the larva when just about to assume the adult shape.

Therefore, if crabs grow in a geometrical progression, an average crab of 12.5 mm. long would have moulted 9 times. Since the close of larval life I can only find one record of measurements of a crab from carapace-length 1.4 mm. to 12.5 (it takes 2 years to grow so much!) — but the one man who has seen it started with a crab 1.43 mm.; and after 9 moults this crab was 12.4

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mrs. Lang.

If one uses an index value, and keeps the "base" of the index nearly constant, the spurious effect of crowd disappears: so that I am trying what comes of splitting the crabs into groups, such that the initial carapace-length in each group is the same within 1 mm.

— The thing is nearly done: I am writing it at length, and I trust it will be ready for Thursday.

Do what I will, I cannot make it appear that there is no selection!

Yours very truly

W. F. R. Weldon

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P 261v

POST CARD

THE ADDRESS ONLY TO BE WRITTEN ON THIS SIDE.

LONDON. W. 12 DE 96 15

W 35

Francis Galton Esq., F.R.S.,

42, Rutland Gate,

S. W.

f 261r

I very much prefer "racial measurements"; for reasons I wrote Cust night.

Yours very truly

W F R Weldon

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

SOUTH KENSINGTON S.W.
1.45PM
DEC 3
1896

f262

30A Wimpole Street — 6th June 1896

Dear Mr. Galton,

If I understand your theory rightly, it involves certain assumptions with regard to the relation between the body of a cell and its nucleus, which seem to me very difficult, — although the same assumptions are made by Weismann, and by many other people. Therefore I want to explain at some length why I feel difficulties.

First, on p. 10, you speak of Karyokinesis "which consists in the "duplication of the elements in the cell and the subsequent "extrusion and waste of about one half of them in the form of a "polar body". — This statement is probably a mere lapsus calami: but you will forgive me if, in order to make everything clear, I remind you of things you already know well enough.

Karyokinetic figures are formed ~~during the~~ before the division of the nucleus in the great majority of cells, whether sexual cells or not; and they are not ~~very~~ followed by the extrusion of polar bodies except in the case of sexual cells (where the number of polar bodies is generally two). When any tissue cell, — say an epithelial cell from the skin — is ~~going~~ in its normal condition, the cell consists of a "protoplasmic" body, with a "nucleus" of different material. The nucleus consists of a network of "fibres", which stain deeply with various colouring matters, suspended in a more fluid substance, which does not stain so deeply. When the cell is about to divide, the nuclear fibres become ~~be~~ disentangled, and a long, coiled thread lies in the equator of the nucleus, in the plane of the future division. During this process, two bodies, the "centrosomata" appear, one on each side of the nucleus*; and all trace of a membrane, separating the nuclear substance from the body of the cell, disappears. Then, clear threads appear, radiating from each centrosoma: and some of these attach themselves to the thread of ~~the~~ deeply-staining material,

(1.)

* A single centrosoma is in some cases at least present in a cell which is not going to divide immediately.

which has in the mean time broken up into a number of isolated "chromosomes" lying in the equator of the original nucleus.

Then, each chromosome splits longitudinally into two halves, ~~longitudinally~~: and one half of each chromosome travels towards each centrosome: so that two mages of chromosomes are formed, at a distance from each other. Each of these mages of chromosomes gives rise to a new tangle of chromatic material; and the cell has two nuclei, instead of its original one. Then, the protoplasmic body of the cell divides, so that you have two cells instead of one.

This procep occurs in the a-sexual ~~mode~~ multiplication of somatic cells during the growth of ~~a complex~~ a complex body, and in the a-sexual division of unicellular animals. So that there is nothing sexually sexual about it.

~~I have made some rough diagrams of the procep.~~

You see, therefore, that "karyokinesis, with its "duplication" of the elements of the nucleus only, is not an sexually sexual procep.

The sexually sexual procep is a karyokinesis which results in the throwing away of ~~a (?)~~ one or more successive portions of the nucleus (generally two): and this is the point on which your theory mainly rests.

~~I see upon what the units which determine~~

You assume, if I understand you rightly, that an egg nucleus contains not one but several sets of bodies, like Weismann's "determinants", each of which ~~determines the character of~~ is capable of regulating the growth and determining the character of some set of organs of the body. During the formation of the "polar bodies", certain of these are lost, and the rest is made up by the set of determinants supplied by the spermatozoon.

In the nucleus of the fertilised egg, these determinants

(2.)

If I've misrepresented your view of polar bodies, please read on, nevertheless. – You will see later why I venture to write at once on the general question of determinants.

are mixed together like the cards in a well-shuffled pack: and the segmentation of the egg is accompanied by a sorting of the determinants into "suits"; one "suit" going to one group of cells, which is thereby rendered capable of becoming say nervous tissue, while another "suit" being dealt to a second group of cells forces these cells to become muscle-cells; and so on.

Your whole contention rests on these suppositions with regard to the nucleus; namely (1) that the character of the nuclear chromosomes "determines" the nature of a cell; and (2) that the separation of chromosomes during embryonic development is always selective.

I do not know whether you realise how completely hypothetical the second proposition is. After fertilisation the nucleus of an egg divides into two halves so exactly similar that no one pretends to detect a difference between them: and before fertilisation the half of the nucleus which is going to form a polar body is in most cases precisely similar to that left behind in the egg. [And about polar bodies let me say (a) that Weismann's distinction between the mode of formation of the first and second does not hold for many animals; (b) that there are ~~quantities~~ many parthenogenetic eggs (Aphis, and the Hive Bee) which produce two polar bodies, like sexual eggs.]

Therefore it is not generally true that the ~~extruded nuclear~~ nuclear substance extruded from an egg before fertilisation is in any demonstrable way different from that which remains behind; and it is ~~never~~ overpraised true that there is visible indication of a difference between the two sets of chromosomes resulting from the division of an ~~Embryo~~ egg-nucleus.

Again, neither in size ~~nor~~ nor in number is there

any relation between the ~~number~~ complexity of ~~a cell~~ the chromosomes and the complexity of ~~its chromosomes~~ the cell containing them. The number of chromosomes formed during nuclear division is remarkably constant in ~~different~~ animals: for example, the fertilised egg of one of the thread-worms forms only two, while in other animals the number may be 64 or more: — but there is no obvious relation between this kind of complexity and the complexity or simplicity of the animal. In a dividing Infusorian the chromosomes are very numerous.

Further, when a cell of an early embryo divides into two, one of the daughter-cells may be going to form a part of the central nervous system, another may give rise to a ~~simple~~ patch of hair, or a group of scales: but when in an adult animal a cell of the liver divides, the result will almost certainly be two liver-cells, each like the parent-cell. But in each of these cases you may have a sensibly identical process of karyokinesis.

~~The~~ ~~evidently a process of karyokinesis may always not be a process of sorting.~~ In the case of a cell which divides, so as to give rise to two offspring precisely similar to itself, I suppose no one will assume a "sorting" of determinants: and since the karyokinesis without such sorting is, so far as we can see, identical with that in which sorting is assumed, the assumption in the second case must be justified by some indirect method.

The majority of writers consider, with you and Weismann, that the assumption of a sorting of determinants during development makes an explanation of heredity easier; and so justify their assumption. But first, no one has attempted any mechanical explanation of how a chromosome or determinant can "determine" the structure of a mass of protoplasm some thousands of times as big as itself; and secondly I want to tell you of some experiments

which seem to me to upset the whole theory of the governance of cells by any kind of substance.

First, I must remind you of the normal development of a sea-urchin.

The egg is small and transparent (less than 0·5 mm. diameter):— it is laid, surrounded by a thin porous shell, in the sea, where it floats.

After giving off 2 polar bodies, the egg is fertilised, and divides into two equal portions: the plane of division being vertical as the egg floats (diagram No. 2.). The egg floats with the polar bodies upwards, and the 1st. division-plane passes through the polar bodies (this is a general rule).

Next, a second division, also vertical, divides each cell into two. The ~~egg~~ embryo has now 4 cells, separated by vertical furrows (Diagram 3). — Then a horizontal furrow divides each of the 4 cells into two; so that there are 8 cells, in two sets of 4; an upper set and a lower set (diagram 4). — The cells of the upper set of 4 now divide unequally, each producing an upper smaller and a lower larger cell: so that the cell $\underline{a}$ fig. 4 produces $\underline{a}_1$ and $\underline{a}_2$ fig. 5. — At the same time each cell of the lower set divides into two equal cells which lie side by side; so that $\underline{b}$ fig. 4 produces $\underline{b}_1$ and $\underline{b}_2$, fig. 5.

Every time any one of the divisions referred to occurs, a precisely similar process of Karyokinesis is gone through.

The divisions described vary wonderfully little, from individual to individual: and it ~~is easy to prove~~ would be easy to follow the divisions of every one of the 16 cells, and to say that in normal development one cell gives rise to offspring which form such an organ, another cell gives rise to such another, and so on. The larva of a sea-urchin is roughly drawn in fig. 6. — And without going into more detail it may be said that in the 16 celled embryo the topmost cells (those like $\underline{a}_2$) give rise only to the skin, ciliated band, and nervous system; the lowest cells, like b_1 and b_2, give rise to the gut, the skeleton, and muscles, etc. — Further, the plane of division between

the first two cells into which the egg divides is the plane dividing the larva into right and left halves; so that normally one of the first two cells gives rise only to left organs, the other, only to right organs (median organs being generally built in two halves!). This relation of the 1st. division plane is not universal.

This sort of definite history has been worked out for a large number of animals. The impression given by such work is naturally that the fate of every cell is from the first rigorously determinate. Upon this impression rests the story about the orderly selection of determinants at every nuclear division. This impression has been tested by Driesch, Chabry, and Roux.

Driesch found that by violently shaking the eggs of Echinus which had divided into 2, 4, or 8 cells, it was possible to separate the cells from one another, and to break the egg shell. Such isolated cells were cultivated.

After an egg has divided into 2 cells, either of these may be isolated. From either it is possible to grow a complete larva, like Fig. 6, perfect in every respect, except that it is only half the normal size: and, so long as the cells composing it are few enough to be counted, they are only $\frac{1}{2}$ the normal number.

If an egg has divided into 4 cells, any one of these, when isolated, will give rise to a perfect larva, like Fig. 6, but of $\frac{1}{4}$ the normal size. — Any three of them, remaining in contact, will give rise to a larva $\frac{3}{4}$ the normal size: — Any two of them form a larva $\frac{1}{2}$ the normal size.

This seems to show that the fate of an embryonic cell is partly, at least, a function of its position: and the next experiment made by Driesch shows this still more clearly.

Consider the 8-celled embryo, Fig. 4. — I have said that the cells of the lower pole (such cells as b) give rise only to Endoderm and Mesoderm: so that all the "determinants" if such exist, which make a cell assume the character of Ectoderm, ought to lie in the upper group of cells, a. —

Now Driesch allowed eggs to segment while subject to considerable pressure. The result of this was, that when 8 cells were formed, they lay, not in two rings of four, one above the other, but in a flat plate: When this plate had been formed, Driesch removed the pressure, and the egg still had the form of a plate. Evidently, if karyokinesis involved the orderly sorting of determinants, 4 of these cells should still be capable of producing ectoderm, and 4 should not: since there was no evidence that the pressure had changed the karyokinetic process. But, the pressure being removed, each of the 8 cells divided into two halves, lying one above the other: so that the embryo consisted of 16 cells, in 2 rings of 8 each. — The 8 cells of the upper ring gave rise to ectoderm, and the 8 cells of the lower ring to endoderm and mesoderm, the result being a quite normal larva like Fig. 6.

Here, by simply changing the position of the cells a and b in Fig. 4, a was made to give rise to an endoderm cell as well as to an ectoderm cell; b was made to give rise to an ectoderm as well as to an endoderm cell.

Chabry has arrived at quite the same results with ascidian eggs. — Wilson has done much the same for Amphioxus. — Haeckel says that the embryos of one of the jelly fish ~~usually~~ often get broken into two pieces while floating in the sea, and that each piece develops.

(Neither of these authors has performed the experiment of segmentation under pressure.)

The effect of position is still better seen in Roux's work. Roux destroyed one cell of a 2-celled frog's egg. Here, it was impossible to separate the two cells: so he simply killed one cell with a red-hot needle, leaving its dead body beside the uninjured cell. Here a live cell developed in the same position with regard to its fellow as in an uninjured egg: and the result was a half embryo, until, late in development, cells of the living half-embryo migrated into the dead half,

and ate it up, by some process like inflammation, whose details I do not know. — Then, the living half-Embryo was no longer in the same position-relation to a dead half-Embryo; and the missing half proceeded to develope.

These experiments seem to me to show conclusively that no theory of the sorting of substances out of an Egg nucleus will completely explain development. — Even granting that the nuclear substance "governs" the character of a cell, it must be assumed that this government is a function alike of the composition of the nuclear substance and of its position in the Embryo. So far as I know, Driesch and I are the only people who habitually preach this: but I do not see any alternative.

Another difficulty about determinants arises from the phenomena of regeneration of lost parts. In an animal which can regenerate a limb, Weismann supposes that the nuclei of certain cells at the base of the limb contain a spare set of determinants, which become active on the removal of the peripheral part of the limb. This is a most complicated story.

For consider a limb as in the diagram.

A finger tip, cut off by the line 1, can be regenerated: therefore just below the line 1 are all the determinants of the portion outside it. At the line 2 the finger can be regenerated: and ∴ below 2 are all the determinants of the distal part again in duplicate. And so for the lines 3 and 4, and as many others as one chooses to draw. And since the same finger can be regenerated more than once, the series of spare determinants in any one piece must be unthinkably large.

The formation of scar-tissues, and the frequent change in the

character of a regenerated organ, are all difficulties: but this letter is so inordinately long already that I will only mention one case.

On page 151 of Bateson's "Materials for the Study of Variation" you will find a figure of a Langouste in which the left eye is produced into an appendage like an antenna.

In the common Prawn, excision of one eye is frequently followed by death: but when the animal lives, the eye-stalk produces a new organ, which is, in a very large percentage of cases, shaped like one branch of an antenna. (This is a statement I have from Lankester. — He told me about it the other day, saying that it was based upon examination of 500 prawns in which one eye had been removed. — He gave me the title of the paper describing the result, but I have unfortunately lost it.)

There are general reasons why any theory of sex, which involves the belief that egg-segmentation is only a sorting out of hereditary substances, seems to me inadequate. You will see that they are general reasons, applying to every form of determinant, idant, pangene, idioplasm, or what not, and by no means specially to yours. I have therefore ventured to write at once. About your special treatment of polar bodies I should like to think for a time: and therefore do not return your essay.

If you would like to see karyokinesis in an egg, I hope to have some live sea-urchins during the next 10 days, in order to show the whole thing to my students. — The day depends on the phantasy of the urchins, and the quality of such sea water as I can get in London. — If I succeed, I will telegraph to you, on the chance that you may be at liberty and may care to come to University College. — It is impossible to make a more definite proposal, from the nature of the experiment.

Yours very truly

W F R Weldon

(9)

I must add an experiment of Boveri, which bears on your theory, though at present I do not quite see how. — You wise!

The Sea-urchin Larva, Fig. 6, has little characters which vary from species to species; so that species can be recognised in this stage of development.

Boveri took eggs of a species A, and shook them to pieces, so that he could pick out fragments of broken eggs with no nucleus at all. He was able to fertilise some of these fragments with spermatozoa of species B, and as a result he obtained larvae of species B.

This experiment has been much criticised: and the general opinion is that Boveri did not completely prove the absence of a nucleus from the fertilised egg-fragments. I believe he repeated the whole thing at Naples this spring; but I do not know the result. Boveri is a very careful and accurate person: and he may, I think, be trusted to have emphasised all the weak points in his evidence: So that people [redacted] who have not his excellent habit, may easily undervalue his statements.

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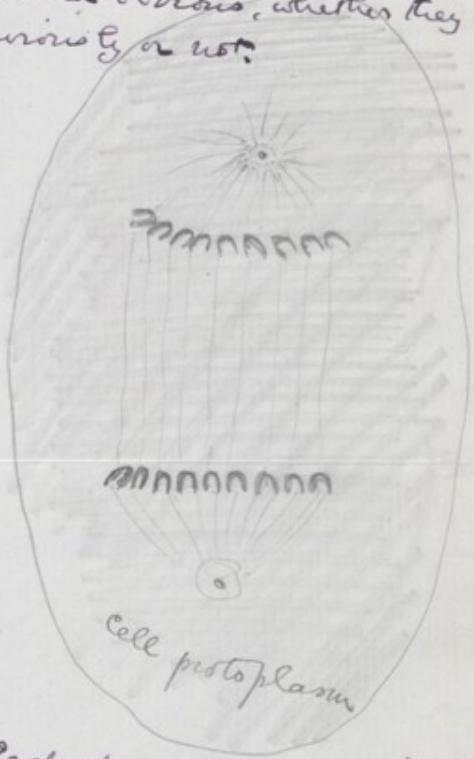
Rough diagrams of Karyokinesis

1.– A "resting" nucleus, with its chromatin tangled, lying in the protoplasm of a cell.
The nucleus seems to be separated by a membrane from the cells substance.

2.– A nucleus preparing for division. – The chromatin threads have thickened, and the tangle is not so complicated. – a–b, the plane of future division. –
Centro somata obvious, whether they were so previously or not.

a b



3.– The chromatin lies as a ring of V shaped (or otherwise shaped) pieces (the shapes differ) in a plane between the 2 centro somata.

4.– Each chromosome has split into 2: and ½ of each has travelled towards one centro soma. – Each ring of chromosomes will now tangle into the condition fig. 1, and the cell protoplasm will divide across a b.

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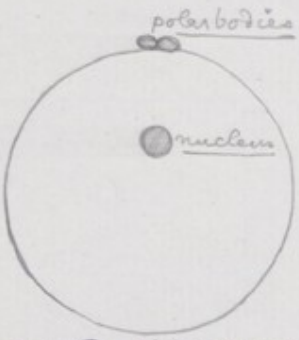
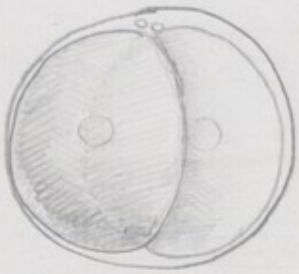
No. 1.



Outline of a star-fish's egg, with the nucleus and the 2 polar bodies: to show relative size of polar bodies, egg, and ~~fecundating~~ nucleus.
× about 80 diameters.

No. 2.



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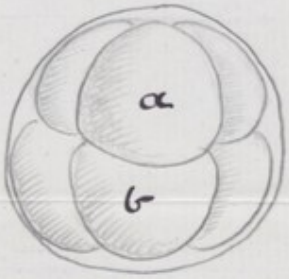

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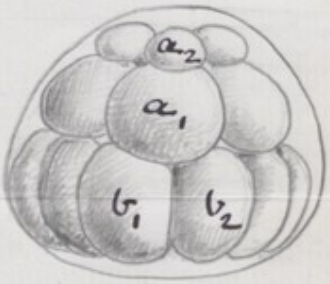

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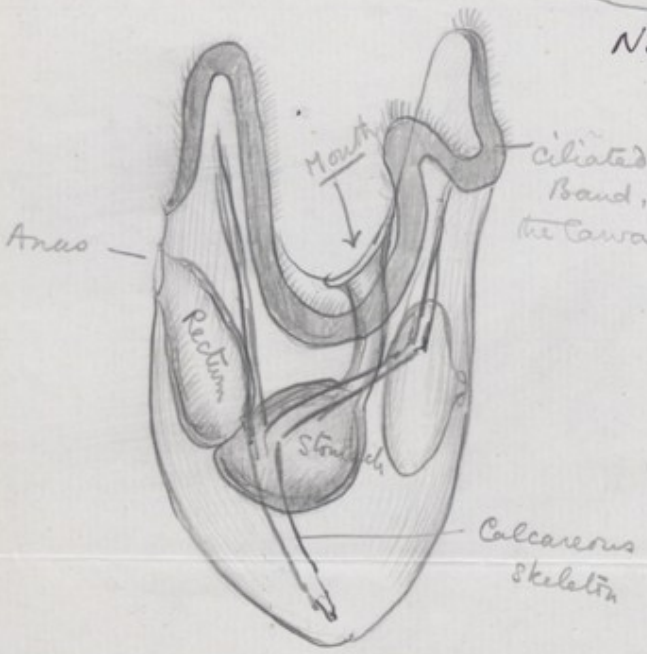

No. 6.

Diagrams of normal development of a sea-urchin.

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30A, WIMPOLE STREET, W.

30th June 1896

Dear Mr. Galton,

I am sincerely sorry to hear of your illness! — I hope your holiday will quickly set you up again.

Enclosed is your Copy. — You will hardly care for more discussion just now: but I should like to write another letter later, when you return. — Lately I have had to examine my students, and wind up the session generally: this has prevented me from doing much

Else. — Also, I have got into a sort of controversy with Lankester and others as to whether or not there is more than the idea of universal correlation in our idea of cause and effect. This is a most futile and annoying subject, but from what these men say, I am afraid it cannot be avoided. — They seem never to have heard of David Hume!

On Thursday I start for Plymouth (on a bicycle, in order to

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get really full of energy before renewing the attack upon the crabs) — And from next Tuesday until September I shall stay there. This time I hope the result will at last be fairly conclusive!

Yours very truly

W F R Weldon

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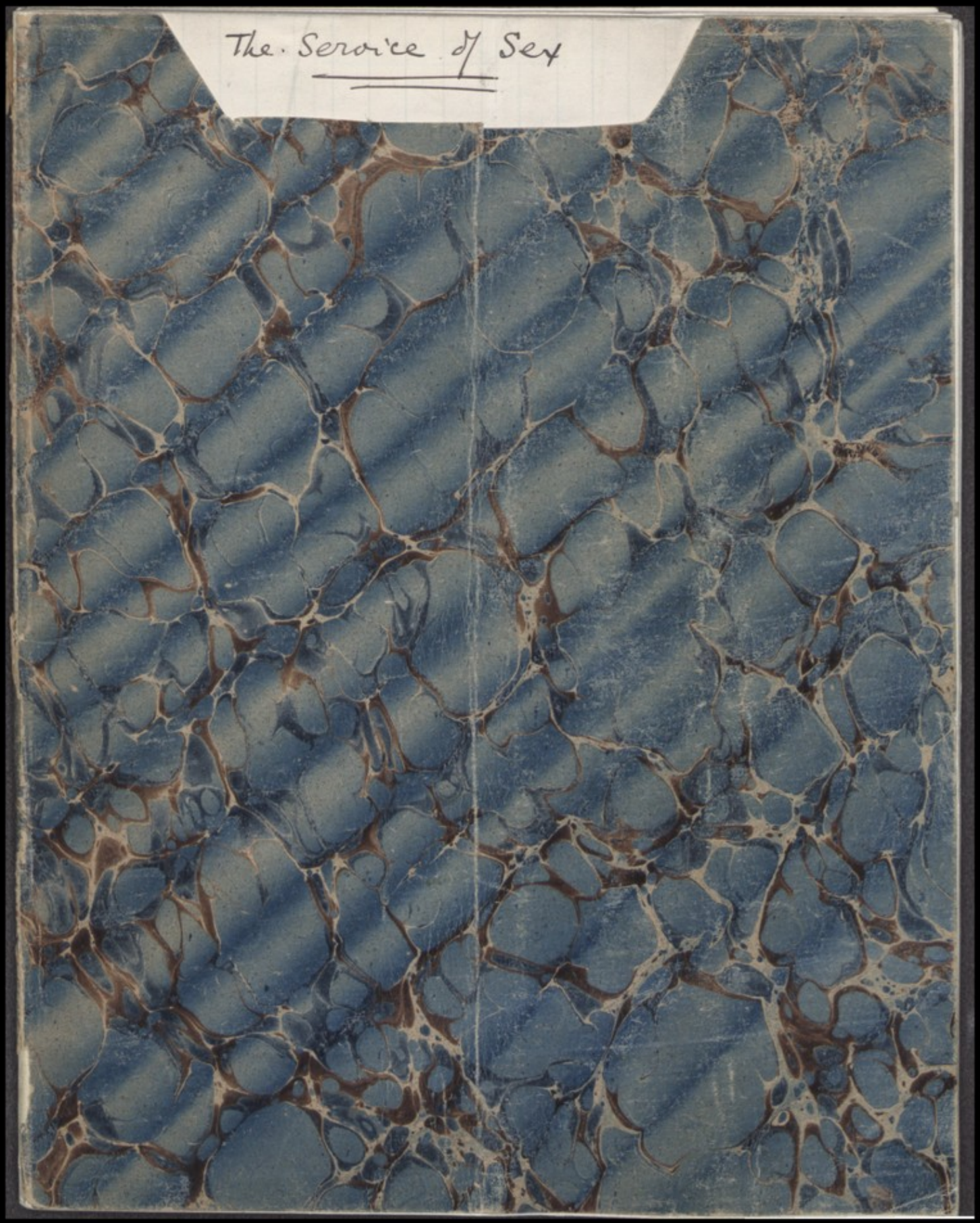
The Service of Sex

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42 Rutland Gate
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The Service of Sex

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The Service of Sex

by 17 Ga

The mischief resulting from close interbreeding is only too familiar to the rearers of every kind of pedigree stock, whether vegetable or animal. Evidence of its existence is met with in every work on that subject and an abundance of those evidences are collected and discussed in Darwin's "Variation of Plants and Animals under domestication" Chap XVIII. The mischief consists in the diminishing of the size of the progeny, in their loss of constitutional vigour, in the tendency to produce malformations, and in a diminution of fertility, often down to sterility. But the loss of fertility is relative rather than absolute, affecting the intermarried couples but not the couples in which one member belongs to the

pedigree stock and the other to a different family.
In the latter case the introduction of the new strain repairs the previous mischief suddenly and completely, the offspring being both numerous and healthy.
Another peculiarity is generally observable, namely that the mischief of interbreeding is at least for a time, progressive. It is rarely conspicuous before the second generation and may not be noticed until the third or even the fourth, but after it has appeared, it steadily increases until the stock comes to an end, though (on the other hand) a favorable turn sometimes occurs & the mischief wears itself gradually away. In short, a stock descended from a single pair and thenceforward kept rigorously pure from alien admixture will, as a general rule, perish.

I propose to discuss the theoretical significance of this most striking fact, which is perhaps the most conspicuous of all the features of heredity, from the points of view of modern biology, first that inheritance is particulate, secondly that the chief if not the sole line of direct descent is from germplasm to germplasm and not from person to person; ~~(from soma to soma~~ that is, to adopt the expressions commonly now employed, it is not from soma to soma. Starting from this basis it will be argued that during the very earliest stages in a reproductive cell the strong organisation of the incipient soma enables it to sequestrate for its own use, the fittest among each class of the available germinal elements, leaving the residue of

them in that cell so much the power. This act of sequestration is held to be a factor of very great importance in the processes of heredity, and by its supposed existence the advantage to a complex organism of bisexual production will be explained and to some degree formulated.

A scheme is annexed to show the outline of the argument, of course in a somewhat bald way after the manner of diagrams, for it will give clearness to the meaning of what follows.

insert scheme
? print it crossways

f 2806 (6)

GENERATIONS	SOMA	Reproductive Cell	GERMINAL ELEMENTS in each of the two parental cells
A First pair, not of same family.	normal }	O	Imperfect, owing to sequestrations for soma, in respect to a few out of very many different classes of elements, so that the chance of the same class of elements being imperfect in both parental cells is small. A perfect set of the classes may usually be made up out of their joint contents.
B Offspring of the pair A.	normal }	O	Imperfect, owing to sequestrations. Since the two parental cells are now fraternally related, the same class of element is often imperfect in both of them, so a perfect set of the classes cannot be usually made up out of their joint contents.
C Offspring of intermarried pairs of B.	imperfect }	O	Imperfect for above reasons, in an increased degree.

Note The word 'imperfect' refers either to quantity, to quality, or to both.

Terms & Symbols

The number of parts or Elements of the body that are observed to vary independently of each other, in different persons, is very large though no trustworthy estimate of their number in ~~different~~ any animals has yet been made. They are the determinates of Weismann, and will be expressed here by Capital letters, taken out of any imaginary alphabet of which the beginning and end is the same as ours but which is extended to the necessary length by supposed interpolations in its middle. The various observed hereditary appearances in different persons of the element A will be distinguished as A_1, A_2 &c, ~~the Ends~~ all there being convertible and belonging to the class A. Similarly as regards B, C, and the rest. Each of

these bodily elements is ~~understood~~ supposed to be developed out of a corresponding germinal element, symbolised by italic letters, as A_1 from a_1, A_2 from a_2, &c. These elements of the germ plasm are the determinants of Weismann. The prefix of S to (a), as S(a), means the whole of the determinants of the class a in the germ plasm of the cell in question; similarly for S(b), &c. Lastly, square brackets signify an organised whole; thus [A ··· Z] represents the soma, and [s(a) ··· s(z)] the ~~corresponding germ plasm~~ original constituents of that portion of the germ-plasm, out of which the above soma was developed.

We may feel assured that the germinal elements a_1, a_2 &c, b_1, b_2, &c, & the rest respectively propagate

their kind with much more certainty than in these ordinary forms of parent and child, with which every one is familiar. Organisms such as these propagate by fission, so that the two halves which stand are respectively in the place of mother & daughter, are almost identical. On the other hand again the relation between either of the two parental persons and that of their child is partial and and is liable to be modified at every stage strangely circuitous. If we denominate the relationship between the soma & its accompanying germ plasm, say roughly, that between $\{A \cdots Z\}$ and $\{s(a) \cdots s(z)\}$, as "quasi-sisterhood", then that between the parental $\{A \cdots Z\}$ and the filial $[A' \cdots Z']$ would be described as the relationship between the ~~half~~ quasi-half-sister of $[s(a) \cdots s(z)]$ and the quasi-sister of $[s(a') \cdots s(z')]$. The reason of the qualifying

word 'roughly' will be understood after reading the following paragraph.

Sequestration The very complicated process of Karyokinesis ~~becomes intelligible~~ which consists in the duplication of the elements in the cell and the subsequent extrusion and waste of about one half of them in the form of a polar body, becomes intelligible if it may be assumed that the fitter half is retained and the residue is extruded. Suppose that there had been originally but a single element of the b class, so that the elements of the cell were at first [S(a), b, ... S(-1)] and that a perfect set of elements were sequestrated on behalf of the soma, the remaining germ plasm would be

deficient in class b. But if duplication had occurred, out of the ensuing [2s(a), 2b, 2r], ~~both soma and germplasm~~ a set of [2s(a), 2b 2r] could be made up and retained, the residue in which b was absent being expelled. The same general idea applies when the imperfection is qualitative, thus let b_1, b_2, differ in fitness, b_1 being the better, After duplication we have [2 s(a), $2b_1$, $2b_2$, ...] which permits both soma & germplasm to be supplied with the b_1 element. The same idea also applies to bisexual generation, where a second halving takes place in each of the two parental cells, reducing the bulk of their joint contents to that of the original contents of one of them. Here also it is to be presumed

that the fitter is kept and the other part extruded. We know next to nothing about the character of the forces in operation, but they are seen to be very active as soon as ~~the visible~~ structure is visible appears in the cell. They [structural details] show marked attractions of a discriminating kind, and there is much in Kargokinesis to suggest that the marshalling of the elements is of the nature of a review, their orderly arrays being not improbably connected with their respective sequence in future development. We may therefore reasonably entertain a provisional belief that the relative fitnesses of the two haloes is an effective factor in determining which of the two shall be kept, and which ~~thrown away~~ shall be cast out.

Accepting generally what has been said, ~~thus far~~, with having much more regard to the idea than to the details, we may formulate the advantage of bisexual over unisexual generation in certain representative cases. It is a very simple problem of chances and in solving it it is much better to ~~begin by~~ considering an analogous case in which all the details are known, just as in those many problems that are treated in the first instance, in the supposition of so many differently colored balls in a bag, from which drawings are made.

Suppose then it is desired to transport a complicated machine to a distant island, for immediate service. Also that the only means of transport is by one of the small and similar

boats in use at the port, and that one of these is just large enough to carry the machine after it has been taken to pieces and packed into n different boxes each labelled with a distinctive number. There are supposed to ~~will~~ be means on arriving at the island, for putting the machine together, but none for supplying any part of it that may be missing or for repairing it if it be much damaged. This corresponds to unisexual generation. If however, to lessen the risk of mischance, a ~~second~~ duplicate ~~set~~ machines, similarly packed and labelled with ~~similar~~ numbers, be sent in a second boat, there is good hope that such parts of the machine as may receive damage in one of the boats will be uninjured and the other, so

that all the parts of one complete machine may be obtained out of the joint cargoes. This corresponds to bisexual generation. Let us now consider the difference of risk in the two cases. Let the risk of any one box being damaged be $\frac{1}{r}$ and let the risk to each box be *independent* of that to every other box. (We must begin with the simplest case). Then the chance of a damage occurring among the n boxes in one boat is $\frac{n}{r}$. So again the chance of any given box being damaged in both boats is $\frac{1}{r^2}$, and that of a damage to a similar box among the n boxes in either boat is $\frac{n}{r^2}$. Therefore in this case the two risks would be as 1 to $\frac{1}{r}$, or as r to 1, and it is independent of n. If for example the

risk to a single element was as 1 to 50, or such that one out of fifty perished then bisexual generation would be fifty times as safe as unisexual.

It seems not impossible if the hypothesis of independent risks is not grossly wrong, to obtain some idea of the values of $\frac{n}{r}$ & $\frac{n}{r_2}$ respectively, from observing the proportion of cases in which karyokinesis becomes aborted. I am no microscopist myself, but learn on inquiry from those who are, that a stoppage, or obliteration, or serious disorder in the course of the process, is by no means rare, also that there may be indirect means of inferring what the proportion of abortion is, without subjecting the developing cell to strained conditions. I am also informed that

their abortion is much more frequent in the ova of the higher animals than ~~there~~ in those of the lower ones. Probably this class of statistical research may commend itself to some competent inquirer. I speak of it merely to show that the results just mentioned, more than will follow, need not be considered as wholly worthless owing to the impossibility of obtaining numerical data.

Having thus far cleared the ground, we can the better appreciate the precise problem before us, still supposing an independance of risks. Let as before, n be the number of classed elements in the offspring of the unisexual parent and $\frac{1}{r}$ the risk to each of them, and let mn be the number in that of the bisexual parentage and $\frac{1}{sr}$ the risk to ~~each~~ in the reproduction cell of either parent.

The two classes of risk are therefore $\frac{n}{r}$ and $\frac{mn}{r^2 s^2}$ or in the ratio of 1 to $\frac{m}{rs^2}$. ~~We may make under this section~~ When that fraction exceeds 1, then there is an advantage in bisexual generation (always supposing the independence of the risks). First suppose $s=1$, that is to say, suppose that the risk to a single class of elements is the same in both cases; then m must be less than r in order that bisexual generation should be the safer; the greater the complexity of the organisation, the less according to this would be the advantage. This is clearly wrong; we must therefore suppose that s is not constant but increases with m; in other words, that the risk to any single class of elements increases with the number of those classes, and necessarily

at the same rate but at one that is somewhat higher one that ~~is in the ratio of~~ the square root of the number of classes. This is a very reasonable view, being supported by much analogy. The larger the number of classes the less strict & stable is the organisation likely to be.

No important difference in the general result is likely to be caused by taking away the ~~restriction~~ hypothesis of independance of risks. Fanciful ~~and far fetched~~ suppositions may be made that would have that effect but they ~~would be~~ are to far-fetched for serious consideration. The conclusion therefore is that the function of sex lies in resisting the ~~miseffects~~ ill effects of what has been called "sequestration" and that ~~it is exerted~~ ~~more favourably~~ the condition of its becoming

increasingly important as the organisation increases in complexity, is that the stability of the organisation should decrease somewhat as the organisation becomes more complex. It is not necessary that it should decrease at a greater or at an equal rate, but if the rate of increase in instability somewhat exceeds the square root of the rate of increase in complexity, it will suffice.

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Passage quoted from p 10
Seems full of faults somehow. In my rough draft it runs.
"The complicated process of karyokinesis ~~ends~~ in uni-sexual reproduction
ends in the duplication ... "
In partial explanation. I must acknowledge that I (wrongly) fancied the
word 'karyokinesis' might be used to comprehend all the "motions of the kernel"
& not only those of the chromosomes.

determinants. I ought to have been more explicit. My
real meaning was that Weismann had given the name of
'determinant' to that definition. not that I accepted all
his views concerning determinants. ~~The fact is that~~
The paper is ~~principally~~ an extention of my old ones ~~& wholly~~
~~undefended of his work, except~~ the main idea of it being there
Segmentation & particulate inheritance but not in those terms

("~~After fertilization the nucleus of an egg~~
No evidence in the eye of any difference between the two halves
of a fertilized egg" — True, the difference is only inferred, not at yet
demonstrable & hardly likely ever to be so.
'No relation between complexity of chromosome & that of
the animal' an important fact but not I think subversive
of my main Conclusion

Karyokinesis is same process in embryo cell and in ~~that~~ some special cell
of an adult — I dont see why it should not be a sorting process in both cases.

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30A, WIMPOLE STREET, W.

22nd October 1896

Dear Mr. Galton,

I return Bateson's letter, with a formal reply. — I am sorry he thinks me such an idiot as to neglect this very obvious point. — It would have been quite useless to discuss it before, because the whole point was the need for experiment on this part, as on other parts, of the Law of Growth. — The simple fact that Q increases in captive crabs, which have all been observed to go through one moult, so that they are no more heterogeneous as to moulting at last

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than they were at first. — is quite sufficient to show that Bateson's objection is not valid. — At the same time, of course it occurred to them, as it would occur to any man who knew about crabs: and in the absence of evidence, he was quite right to urge it. — I have accordingly written you a long, formal letter, which you can use as you think fit. — Perhaps, if you circulate Bateson, you may think it worth while to circulate the answer too: but please do as you like.

I think the next report (when it comes!) must be much longer, — so

that all these subsidiary points can be discussed at length.

There are quantities of objections of a similar kind, which can only be answered by detailed discussion of the laws of growth. — The question of time, as Pearson is always pointing out to me, also has to be considered. —

The measures went on very well during the Summer. — I got 540 more: and these, with 560 last year, make a fair number.

Only, — over 100 of last year's crabs began at q[illegible] 9.0-10.0 mm. length: and I have no sufficient sample of last year's crabs with which to compare them:

So that, if the mean wild crab of a given size this year proves very different from the corresponding mean crab of last year, I may have to throw about 100 away. This is annoying, but I don't think it will happen. — I am now measuring hard at a new standard series for this year: but have not yet measured enough to estimate the difference between this year and last.

Yours very truly

W. F. R. Weldon

Should not Bateson's letter be preserved in the minute-book?

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30A, WIMPOLE STREET, W.

22nd October/96

Dear Mr. Galton,

I hoped to meet you this afternoon. As I did not, I posted a reply to Balesson, which you have probably already received.

On reaching home, I find his postscript.

The explanation of the relative frequency of crabs in the various groups is simple enough. — In ~~1895~~ the case of the 1893 females, and of the 1895 crabs, male and female, a boy was told that I would buy from him crabs of any

(of length

length between two assigned limits/:
and that I would not buy crabs
outside those limits. In 1893, the
limits were 7 and 14 millimetres:
the boy very naturally made a
shot at the middle of the series of
sizes: and as he was always
doubtful whether a crab in the
neighbourhood of 7 or of 14 mm.
would be bought by me, he collected
chiefly those which he knew he
could sell.

In 1895 I thought that the
change in the mean portal
ratio since 1893 would be so small

as to justify me in neglecting it. To make sure of this, I ordered a set of crabs ~~with the Lares~~ above 10 millimetres in length: with the result that the boy chose to collect only those obviously above 10 mm. long: so that in the 1895 series crabs between 10 and 11 mm., — which are the commonest in the 1893 series, — are relatively rare: and as I have already told you, the number of 1895 females between 9 and 10 mm. long is so small as to be useless.

A further reason is that both Thompson and I, in order to avoid ~~running~~ Running what the

result of our measurement in any
given case ought to be, have been
careful to measure our crabs at
random, sorting them into sizes
afterwards. The result of this is
that one chooses, as the boy does,
fair crabs in the middle of the
proposed series, and then fills up
(when our patience was coming to an end)
the two ends. For my own series,
I know that I did not handle
the material very thoroughly in
1893: and I feel inclined to
attribute part of the scarcity of the
largest crabs to my own laziness.

I do not think this will add
any thing to the force of what I have
said before in answer to Bateson:
and you will probably think it

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30A, WIMPOLE STREET, W.

will not to send him this letter. —
If you wish for a formal reply to his
postscript, I will send you another.

Yours very truly

W. F. R. Weldon

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f 303r

THE ZOOLOGICAL LABORATORY,
UNIVERSITY COLLEGE, W.C.

23d March 1896

Dear Mr. Galton,

If you have not already sent my letter on W. Bateson, will you read this before doing so?

On coming here this morning, I found that Thompson had received a long letter from Bateson. suggesting that the whole of Thompson's work and mine might be vitiated because we had neglected the very obvious precaution of taking all crabs used by us from the same place! — Bateson said that he was anxious for this information, because he had seen

to go further criticisms, which he "might possibly publish."

Now the situation seems to me this: an experiment is in progress, but the result of this experiment is not yet arrived at. Bateson thinks himself justified in making the suggestion that those who are conducting it have neglected the most elementary precautions.

Since he took the trouble to write a long letter to the Committee with such suggestions, I felt that decency compelled one to give him a detailed answer: but I think it would be

to go further criticisms, which he "might possibly publish."

Now the situation seems to me this: an experiment is in progress, but the result of this experiment is not yet arrived at. Bateson thinks himself justified in making the suggestion that those who are conducting it have neglected the most elementary precautions.

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to go further criticisms, which he "might possibly publish."

Now the situation seems to me this: an experiment is in progress, but the result of this experiment is not yet arrived at. Bateson thinks himself justified in making the suggestion that those who are conducting it have neglected the most elementary precautions.

Since he took the trouble to write a long letter to the Committee with such suggestions, I felt that decency compelled one to give him a detailed answer: but I think it would be

the merest waste of time to take part in any sort of public discussion until the experiment is complete, and the results are all worked out. — At present, there is nothing really to discuss.

Bateson suggests that I have neglected two obvious considerations, and that neglect of these will vitiate any result. In reply I can only say that I have not neglected these considerations; but whether they affect the result or not depends upon the value of Galton's function in a number of cases which are not yet fully investigated. Such an answer as this is unsatisfactory, and to

give it takes a great deal of time, which ought to be employed in doing the work itself.

I am very anxious not to be in any way rude to Bateson: but do you not think, under these circumstances, that the reply to his letter might simply be the assurance that the points he mentions are being considered by the Committee: that the positive results are not yet ready; and that the whole matter will be fully treated in the next report? This will at least prevent a discussion which at this stage must be futile.

Yours very truly

W F R Weldon

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30A, WIMPOLE STREET, W.

31st October 1896

Dear Mr. Galton,

Many thanks for your Essay.

I have not read it yet, because I have thought it worth while to reply to Cunningham's attack on the whole method of crab measures in yesterday's "Nature".

Of course, the question, whether spirit affected the measures, had to be determined before the growth experiments could be trusted.

Accordingly last year I close

595 crabs, collected during the growth experiments, and ~~compared~~ after leaving them 6 months in spirit, compared their mean frontal breadth with that of the corresponding 1892-3 crabs, used in the first report. — I also compared the mean frontal breadth of the crabs used in the growth experiments (which were measured fresh) with that of the 1892-3 crabs.

The difference (in thousandths of the carapace-length) between

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the mean of 569 fresh crabs of 1895 and the corresponding crabs of 1892·3 was

~~1·487276~~

1·47276;

the difference between the mean of the 595 crabs after six months in spirit, and that of the 1892·3 crabs was

1·58992

So that the difference between a mean, based on fresh material, and that based on spirit material, was only 0·11716 of my units, or 0·001 of a millimetre!

The E.M.S. of the fresh

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specimens was

11·7955;

that of the spirit specimens was

11·9628.

The value of all Thompson's work, and of mine also, of course depends upon this point: and Cunningham's criticism will seem so plausible to many people who have not worked with crabs, that I thought it best to publish these figures at once. — I send them to you at once, in case you may be troubled by Cunningham.

Yours very truly

W F R Weldon

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f 308

30A, WIMPOLE STREET, W.

4th November 1896

Dear Mr. Fallon,

Many thanks for your interesting essay, which I now return, together with Pearson's letter. — Surely your hypothesis is not that used by Pearson in "Nature"? — I understand you to assume that A and B are alike subjects of selection in every generation: and in that case I follow your reasoning.

Pearson's point was that if an animal was selected with respect to A only, and happened to have B large, then continuous selection for the offspring of that animal would

(with respect to A only)

lead to a permanent change in the value of B. — This I quite fail to understand.

2

I have been trying to find a case in which a known amount of selection occurs, so that it may be investigated by the method used for the orals. — In this way, a test of the method can be obtained.

The case I have found is in Livi's Antropometria Militare: and it is illustrated by the enclosed curves.

Fig I is in part an enlarged copy of Livi's wood-cut, fig. 5, p. 32. — The continuous black curve represents the distribution of stature in male Italians

of age for military service, the frequency being expressed in percentages of the total number measured.

The dotted black curve represents the percentage frequency of stature in the persons selected for military service.

These two curves give precisely the information which one hopes to obtain by comparing the curve of crabs bred in captivity with that of wild crabs: and I have tried to treat them as the crabs are going to be treated.

The ratio of the sum of all the ordinates of the unselected stature-curve over 167 centimetres to the sum of the corresponding ordinates of the selected stature curve is roughly 0·8; ~~so that a number of individuals which form 1 per cent of the total selected~~ and if the ordinates of the curve of selected

recruits be multiplied by 0·8, the red curve of Fig. 1. is obtained.

Considering the roughness of the data (mere enlargement from a wood cut) – I think it may be said that for all statures over 163 cm., the red curve coincides with that of the general population: but for all statures below 163 cm., there is a well-marked difference between the two.

Fig. II gives the general population curve "smoothed" by taking for each ordinate the mean of three. And in the same way the red curve of Fig II is the red curve of Fig I smoothed.

I think the coincidence between the two curves from D to F is as close as could be expected. From D towards the mines side of the

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diagrams. The red curve falls below the general population curve, and finally comes to zero at 154 Cms.

Now, evidently, 155 cms. is the least stature of an accepted recruit: so that the area of the general population-curve to the left of AB represents the percentage of men rejected at once because of stature.

But the area BACDE represents the percentage of men who were of stature to serve, but were unfitted by other circumstances — which other circumstances were correlated with stature.

. If this view be correct, there is a very strong correlation between lowness of stature and other causes of physical unfitness.

I want to ask you whether it would not be worth while to get the whole details of Lüri's wood-cut, and to work out from these the law of connection increasing frequency of other physical infirmities with decreasing stature.

Having obtained this from the stature-curves, it will be easy to calculate the percentage of men rejected by the ~~[illegible]~~ recruiting officers, and to check the statement so obtained by direct reference

to the statistics of the Italian Government.

The correlation between smallness of body size and liability to imprisonment (unless it has already been worked out by some one else?) is surely a matter of interest, in itself: and it would be very nice to print, in an appendix to the crabs, the details of an investigation which could be directly tested by independent methods.

Yours very truly

W. F. R. Weldon

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30A, WIMPOLE STREET, W.

8th November 1896

Dear Mr. Galton,

I am glad you like the recruits: they seemed true rather pretty. — I will read up Baxter, and look out for a selection on the + side also. — Livi's data are, I believe, very well sorted into local sections: I have not yet made sure about his general population curve: but the curves of selected recruits are given in great detail for each "Circondario" — which seems to be a quite small recruiting district.

I enclose an answer to Bateson's

last letter. — I waited, hoping to send fuller arithmetic with it: but the arithmetic takes a long time, and it would be too rude to delay any longer.

Yours very truly

W F R Weldon

I have kept a copy of the enclosed for the minute book.

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30A, WIMPOLE STREET, W.

17th November 1896

Dear Mr. Galton,

Many thanks for Bateson's letters. — Some of the points he raises in his last letter are naturally met by the correlation tables already made or making: many of the others (such for example as his statement that a mere determination of the value of r between growth-rate and deviation does not take the sign of the deviation into account) are due to imperfect understanding of the whole method.

Other points are mere verbal criticisms of my hurriedly-written letters.

I should very much like to talk

over the whole position with you and the rest of the Committee (especially with Frank Darwin, if he can at any time manage to come to town!) apart from any question of Bateson's letters.

Only, next Thursday I must attend a Committee in London University, which is revising the Zoological Examinations. I am supposed to be more or less in charge of this business on the Senate, and am so rather bound to it. — The Committee meets at 3; and it may hold only a formal sederunt, and then adjourn. — If it does any work, it may sit till six or seven.

I presume you will go to the Royal Society in any case. May I send you

a note at the last moment, telling you if I can meet you or not?

2

So far as I can see, the whole correspondence with Bateson should be placed in the hands of the Committee somehow. — The members must, I think, either avowedly leave the matter to you and me, or they must read these letters. — Therefore, I would propose to print the whole series myself (since Bateson does not suggest printing my answers!) — and to avoid any possibility of misunderstanding, I would ask you or Maldola to be good enough to correct the proofs by reference to the originals.

When the Committee has read these letters, it may perhaps be well to

have a meeting, at which I can be cross examined. — Or, I would circulate an answer to Bateson's last letter, and to any questions arising out of it which the Committee might put to me by letter.

But I feel very strongly that it will not be profitable to go on discussing the matter with Bateson, until the whole thing is done. The Colour is great: and I would remind you that nearly all my letter-writing has to be done after dinner, — that is, in the only quiet time I have for arithmetic.

Also, under the circumstances, I am sorry to say that I should feel Bateson's presence on the Committee as a serious difficulty.

Yours very truly
W. F. R. Weldon.

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F.315

30a, WIMPOLE STREET, W.

21st. November 1896

Dear Mr. Galton,

Many thanks for sending me your letter to Bateson, which should simplify things very much. — I send it on at once.

About the correlations, I should like to think a little longer before saying more. — At present, I do not quite see the point of your criticism.

Yours very truly

W.F.R. Weldon

f.316

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30a, WIMPOLE STREET, W.

26th November 1896

Dear Mr. Galton,

In calling the meeting & arranging the Committee, about which we were speaking this afternoon, will it not save complications if it is arranged that the present work of the measurements Committee is kept in the hands of the present members for a time?

I say this with reference to possibility of further action on Bateson's part.

I have read his letters over again: and I am sure his first letter, of October 20th., is based on the view

that heterogeneity of moulting is at a maximum [illegible] in crabs between 11 and 13 millimetres long.

I do not mind his changing his hypothesis:— any one may do that. But after writing this at length on October 20, he writes on Nov. 15th. that he does not think he ever made his suggestion; that if he were to hazard a conjecture it would be that heterogeneity is at a maximum at a quite other period, outside the range of my observations altogether.

I do think that under these circumstances his presence on the Committee at which discusses the work

report will lead to a great deal of unpleasantness.

Yours very truly

W. F. R. Weldon

The pure-bred Fox terrier, which, after being covered by a Dachs-hund, and producing young, was covered by a Fox terrier and produced what was told me as an example of telegony is a delusion.

The pup has crooked legs, and was so thought to resemble a Dachs-hund. — It appears that the crookedness is only a kind of

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~~[illegible]~~ club-footedness, which occurs in many animals as an occasional abnormality, and has nothing to do with the crookedness of the Dachshund. But I dared not say this at second hand.

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30A, WIMPOLE STREET, W.

7th December 1896

Dear Mr. Galton,

Your suggestions seem to me admirable, if you do not think that φιλόν has been spoilt by such words as "phylogeny," which means (as a rule) vague speculation of a kind in which I hope our Committee will not indulge.

Of course, otherwise, the word is admirable: and it may be—

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good thing to restore ~~[illegible]~~ the proper meaning to all the group of φῦλον derivatives.

Yours very truly

W F R Weldon

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30A, WIMPOLE STREET, W.

5th January 1897

Dear Mr. Galton,

The r for the mixed shrimp measurements is provisionally = 0.383, which is very big. — I want to go over it again before sending a note to Pearson.

It is between "carapace length" and "post spinous length" of Plymouth shrimps: and the r for measures of the same shrimp is 0.81.

I kept on hoping to be able to send you the crab results also: I hope they will be ready tomorrow!

Ray Lankester is not abroad: he is I believe now in Oxford (the

Museum is his safest address.)

If you have any additions to the enclosed agenda, please let me know tomorrow. — If I do not hear from you then, I will get them copied and sent to Harrison, with a request that the Committee (and Salerio & Heaper) should be summoned for the 14th.

Yours very truly

W. F. R. Weldon

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f.321

30a, WIMPOLE STREET, W.

12th January 1897

Dear Mr. Galton,

The enclosed invitation from George Darwin to the Committee should be read on Thursday? — Will you bring it with you? — Of course I am sending a reply on my own behalf at once.

I also send you a rough draft of the state of the crabs. There is a good deal of important business on Thursday, and I think it would be better that I should not read all this stuff. — I should propose to lay it on the table, and make a short statement, moving that

Gow, Pearson, Balfour, and any one else who chooses, be a sub committee to superintend publication, with power to publish at once.

Unless there are blunders I have not been yet detected, it may well be ready for publication in a fortnight or three weeks at most.

Will you bring the MS with you on Thursday? — I have sent a copy to Pearson, but was too lazy to make a third; so I have none myself.

Will you also bring with you a copy

of the printed report of the informal meeting? — I have stupidly mislaid my own.

Your diagram of growth ratios is very nice: but as I had already worked the thing on the other plan, I did not see need for change: do you?

Yours very truly

W. F. R. Weldon

G. H. Darwin

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DOWN,

FARNBOROUGH, R.S.O.

KENT.

Jan 12. 77

Dear Weldon

We shall be here on Sunday & if you and any of the committee wd like to come down we shall be very glad to see you. There is a train to Orpington at 11.15 getting you here to luncheon & a return train at 6 p.m. (I have an A.B. Bradshaw only) If you give me time I will order a cab, but if no time is given a note to Caper Crowhill, Down, Farnboro' Kent wd procure a cab. If the party is large we shd like to know so as to get luncheon.

Yours truly

C. Darwin

11-15 p.m. Charing Cross.

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30A, WIMPOLE STREET, W.

16th January 1897

Dear Mr. Galton,

You were kind enough to say that you would look at my unhappy manuscript again, so I send it to you, together with some criticisms and remarks from Bateson.

If on a second reading the thing produces on your mind the impression which it has evidently produced upon Bateson, I hope you will say so quite frankly.

Since Pearson has shown the rest of my attempts at statistical work to be merely so much laborious folly, I feel unable to trust my own judgment: but I do not see any escape from the conclusion that selection occurs.

After making every kind of allowance which has been either suggested to me by others or devised by myself I cannot escape from the conclusion that there is a difference between crabs over 13 mm. long, which have been protected through a moult, and crabs of the same length which have not been so protected; and I cannot attribute this difference to the conditions of the experiment. The difference, if it indicates selection at all, indicates selective destruction of big frontal breadth ratios, and hence diminution in the mean: and it occurs precisely at the point where the mean ratio of an 1895 crab is found to be exp

than the mean ratio of an 1892-3 crab.

Any one bit of the evidence may be of little value, because the quantities measured are so small: but it all points in the same direction.

If you agree with Bateson, I shall very probably throw the whole thing into the fire. — If you do not, will you write the minute relating to me for the minute-book of the Committee?

When Bateson voted that I should publish this thing on my own responsibility, he probably felt like Dogberry, and thanked God that he was rid of a knave: but if the Royal Society were to interpret the action of the Committee in this sense, they would almost certainly refuse to publish my paper. They might justly say that a paper, rejected

by a responsible Committee of their own body should not be presented to them for publication.

If you take Bateson's view of the paper, I think the minute should express that view. — If you do not, I should be very glad to have it placed on record that the Committee did not decline to use the paper as a Report because of the inherent futility of the work itself, but for other reasons.

Whichever view is taken should, I think, be clearly expressed in the minute-book.

Have you remembered that we must meet again before the 31st., to report to the Government Grant Committee? — I shall be glad if you will not choose Thursday 28th., because there is a R.S. Council on that day. Yours very truly

W. F. R. Weldon

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30a, WIMPOLE STREET, W.

20th January 1897

Dear Mr. Galton,

Many thanks for both your letters.

I will write to Heape tonight, and try to arrange a day which suits him.

I am very grateful for your "minute," and for the impression, which I gather from your letter, that the crab measurements are possibly worth publishing in some form.

I quite realise that the statement I sent you is not clear. When the whole thing is finished, I will re-write it with much more care: and then I will remember your advice.

It is very kind of you to have

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spent so much time and trouble over the business.

Yours very truly

W. F. R. Weldon

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THE GORDON HOTELS, LIMITED.

THE HÔTEL METROPOLE AND THE WHITEHALL ROOMS, LONDON.
THE GRAND HOTEL, LONDON.
THE FIRST AVENUE HOTEL, LONDON.
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THE HÔTEL METROPOLE, MONTE CARLO.
THE HÔTEL METROPOLE AND THE CLARENCE ROOMS, BRIGHTON.
THE HÔTEL METROPOLE, CANNES.
THE BURLINGTON HOTEL, EASTBOURNE.
THE ROYAL PIER HÔTEL, RYDE, I. of W.
THE CLIFTONVILLE HÔTEL, MARGATE.
THE LORD WARDEN HOTEL, DOVER.

THE CLIFTONVILLE HOTEL,

MARGATE.

TELEGRAMS FOR HOTEL.
"CLIFTONVILLE," MARGATE.
TELEGRAMS FOR VISITORS.
"CLIFTONVILLE HOTEL,"
MARGATE.

14th February 1897

Dear Mr. Galton, I am, as you see, not at home: so that I was not able to answer your letter yesterday.

Do you know that you have changed the day of meeting of the Sub Committee? — We had agreed to meet on Monday at 4.

Balfour at first telegraphed that neither he nor Darwin could attend on Tuesday: but later he telegraphed that he had made arrangements enabling him to come.

Of course, as regards the minutes, I am only anxious to do what is most regular and

convenient. — If the Society prefers to save me the trouble of making notes at the committee-meetings, I am only too glad. — Does this custom apply also to our subcommittee? For the moment its minutes are likely to be more important than those of the larger body?

2

I believe Leonard Hill is now in Oxford: but I am not sure. It is some time since I last saw him. — I will find out about him tomorrow, and tell you on Tuesday. — His chief work with Romanes was on the supposed inheritance of a droop of the eye-lid, in rabbits, in the offspring of parents with certain nerves cut. — The result showed that the eye-lid droop, which did occur in the offspring, was a disease connected with crowding in captivity. — You probably know the experiment. I cannot just now remember the details of it.

2

I am glad you did not think me impertinent on Thursday. — I feel some difficulty in making any suggestions at all

to the Committee, after the maps I have made of my own work for it!

Yours very truly

W F R Weldon

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PAPERS

30A, WIMPOLE STREET, W.

23rd February 97

Dear Mr. Galton,

I shall look forward with interest to your note, added to your previous document.

In the mean time may I say that the more I think of it the more I feel anxious not to formulate any kind of documentary programme or statement at all?

It seems to me that more work could be done, and there would be less risk of finding ourselves in future limited by previous "official" utterances, if we were to authorise a selected number of members of the

Committee to write, each in his own name, but on behalf of the Committee and with the authority of the Committee, to any person he knows of who may be conducting, or willing to conduct, experiments. — Every such member, knowing the circumstances of an individual case, could make the proper arrangements and suggestions: he could, with the help of those to whom he wrote, tabulate a definite scheme of work, pointing out what would be done, what results might be expected, and how the Committee might best give help. — Having arranged this,

he would ask for a meeting of the Committee, lay the whole plan before the meeting, and receive formal authority to apply for money, & to obtain such other help as might be needed.

For example, I think the statement which Mrs Bateson sent in should be seriously considered: and such individual cases could be quickly multiplied. — Bateson himself could provide several, at once.

Have you ever received the slightest help, in devising experiments or in carrying them out, from the celebrated draft programme" be

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Dignitate et Augmentis Scientiarum &c?

Yours very truly

W F R Weldon

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Weldon

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GALTON PAPERS

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30a, WIMPOLE STREET, W.

9th March 1897

Dear Mr. Galton,

I shall be glad to talk over the two circulars with you on Thursday. — I sent them to the printer, without reading them: because I did not feel authorised to take any steps whether I liked them or not. — Now, I have read them.

The shorter circular is quite without the sanction of the Committee, which only authorised the issue of a single circular. Besides this, it contains a quite new undertaking, which has never been discussed, — namely, the promise to "~~provide~~ procure information for any

zealous and competent person who is designing an experiment."

This seems to me a very dangerous promise, and one which ought not to be made without discussion: because in perhaps the majority of cases the information promised cannot be obtained, and in a great number of cases, when obtained it is found to be inaccurate.

Take an Example. — Bateson, in describing to me his broods of butterflies, told me that he fed all the individuals of one series on cabbage. — Cabbage is not the natural food-plant of all the species he mentioned, if it is of any one of them. Bateson scoffed at the

idea that this change of food-plant would affects his results: and as he has probably taken proper care in forming his plans, I have no doubt he is right.

But suppose a similar case were brought before the Committee, as it undoubtedly would be? Aman writes "There is such and such a "problem to be solved by breeding A, B "and C; A and B feed on plants "accepible [sic] to me: C feeds on a plant "I cannot obtain, but will eat "Cabbage. Dare I use cabbage?"

That Bateson individually should report to such a man his opinion would be right and proper. That would not involve action by the Committee. — But I think it would be in the highest degree

improper for the Committee to return
an official answer to such a question.

Remember your S. Illustraria.

Of the few individuals which I
managed to rear, every one (if I remember
rightly) was larger than ~~compared~~
individuals of corresponding groups
fed on Birch in Brighton, — my
broods being fed on Ash in Plymouth.

If the Committee is not prepared to
give official information on minute
points of this kind, — and it cannot
for a moment pretend to do so, —
it cannot fulfil the promise of its
circular.

On the other hand, vague letters
~~will~~ (may) be received in answer to the

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30A, WIMPOLE STREET, W.

circular. Say a man writes asking for an outline of experiments showing whether instincts ~~are~~ of various kinds are inherited in the absence of any teaching by the parent. — In the first place, the attempt to outline such an experiment comes very near to a direct ~~in~~ undertaking of an investigation, and so to a contravention of Foster's resolution: and in the second place there seems to me a grave danger that investigations suggested in this way should be regarded as in some way "official" and that the workers might claim

an official sanction for their work; and so that the Committee should come to be regarded as having a charge de propaganda fide, pushing its own dogma by the aid of its own missionaries.

I realised from Balessa's letters to you about my unfortunate trials how great the danger of this official crew might become. — You have avoided it in my case: but I think there is serious risk of the same thing resulting from the proposed circular.

It would be a very great pity if any one were led to say even for a time that a Committee of the Royal

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Society is or is not officially of opinion that acquired characters are inherited: and although it would not perhaps matter so much, yet it would not tend to the greater dignity of the Committee that it should be known to have given inaccurate information about the breeding of butterflies or of trout.

Yours very truly

W. F. R. Weldon

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30A, WIMPOLE STREET, W.

10th. March 1897

Dear Mr. Galton,

I enclose a letter from Lloyd Morgan, which he wrote in answer to the circular I was instructed to send him. — This letter will show you the kind of thing I fear as the result of circulars, and will make clear to you the reason why I should prefer that Bateson's second circular should not be issued.

Lloyd Morgan's letter simply states a problem, which is known to every one. His letter contains nothing which has not been proposed before.

The difficulty of ~~solving~~ solving his problem arises from the difficulty just of finding a bird which can be certainly reared "by hand" after being hatched in an

incubator, and secondly of making sure that ~~such~~ birds reared under such conditions are capable of pairing and nest-building under some circumstances ~~whatever~~.

The first difficulty Lloyd Morgan cannot overcome at present. He does not know what will be the best bird to use.

The second difficulty is not alluded to in his letter. — As expressed by him, his plan is faulty. For suppose a number of birds to have been hatched in an incubator and reared under the conditions necessary to exclude possible imitation of other birds: suppose further that these birds do not make nests.

Evidently one cannot conclude that the absence of the parent or teacher is the

only reason, or that it is a reason.

Surely he should conduct his Experiment in this way:

Two precisely similar aviaries should be prepared: each so arranged that circulation of birds outside it is impossible. In one, adult birds of the species should be placed, together with some of the young hatched in the incubator. The other aviary should be stocked with young from the incubator only.

If this were done, a conclusion could be drawn from the behaviour of birds in Aviary No. 2 provided that when the proper season arrived the old and young birds in Aviary No. 1 were observed to construct nests, and not otherwise, — unless, of course, the

birds ~~with~~ reared without contact with adults of their own species should proceed to construct normal nests. — Unless this should happen, the experiment, as he proposes it, would prove nothing at all.

Further, what is a normal nest? The shape is of course often characteristic, and the material in many cases; but it would be very hard to make sure in an aviary that the "normal" materials were so placed as to be found with the "normal" ease by a bird looking for them in the normal way. — This could, no doubt, be done for many birds, but it would require some care, and if the ~~the~~ birds reared without the possibility of "imitating" others should be found to build nests at all, the question whether these nests were "normal" or not would be of great importance: and the ~~answer~~ value of the answer given would largely

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30A, WIMPOLE STREET, W.

depends upon a knowledge of details concerning the supply of materials.

Will you consider how the Committee must reply to Lloyd Morgan's letter?

Evidently, we cannot recommend a species of bird for the purpose. — Even if one member of the Committee feels able to do so (which is unlikely) no collective utterance on such a subject can be made.

Evidently also, I think, the Committee cannot officially say that this experiment ought to be conducted in such and such a way and in no other. — No one man can formulate all the in advance necessary steps; and a fortiori no Committee can do so.

I cannot help feeling that the proper

answer is to the effect that we shall be glad to hear from him again when the plan of his experiment is mature.

That reply will offend Lloyd Morgan, and do no good to any one.

Now what I feel about this matter is first that it is a type of what replies to circulars will be: and secondly that a rather awkward complication would have been avoided if some one member of the Committee, knowing Wilds and Morgan, had found out all about this experiment, bringing it before the Committee at a later stage, when the necessary knowledge had been acquired, and when a plan of a really feasible experiment, capable in

any event of giving a conclusive result, could have been laid before us.

= I have as yet received no other reply to the circulars I sent out: but others will no doubt come in. — A circular, sent by Marteis, has brought a letter of quite the same kind from a German — Mr. Druery. — He is willing to supply material for research to any qualified person recommended by the Committee, and to work himself on the basis of any suggestion made by the Committee, relative to Apospory or Apogamy of Ferns. — Here again there is no indication of an experiment.

= Would Wednesday, 17th., at 2.45 suit you for a Sub Committee?

That would probably give time for

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all replies to the circulars to arrive.
There is a R. S. Council on the 18th.,
and Bateson cannot come on Fridays.

Yours very truly

W. F. R. Weldon

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30A, WIMPOLE STREET, W.

24th March 197

Dear Mr. Galton,

Here are the letters. The only additional answer to the circulars is one from Howard Collins, saying that he is too busy to plan definite experiments, but that he will be glad to help us by criticising any plans we may care to submit to him.

I hope you will get rid of your cough at Bournemouth!

Yours very truly

W. F. R. Weldon

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UNIVERSITY COLLEGE LONDON
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30A, WIMPOLE STREET, W.

14th June 1897

Dear Mr. Galton,

Very many thanks for your proof. — I was in Cambridge on Saturday evening, and only returned this morning: so that I have not had time to think about it much. — Certainly, I have no emendations which I could venture to suggest: the thing seems to me quite clear.

I myself regret that you have tied yourself down (by your reference to the log of one half of the several contributions from each of the two parents etc.) to the [redacted] view

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of nuclear division at present in fashion: but that is only my personal fad, and I am probably wrong.

Yours very truly

W F R Weldon

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Marine Biological Laboratory

Plymouth

Weldon

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30a, WIMPOLE STREET, W.

18th July 1897

Dear Mr. Galton,

I hope you will enjoy your holiday. — I am sorry I know nothing about American Stud-books. Pearson is likely to know. He knows something of horses from this point of view.

The crabs are dying slowly.

Change (of some magnitude) in the density of the water makes a very good selective ordeal. For example, if crabs are placed in fresh water, about 20% die in two hours; and another 60% in the next two hours. After 4 hours, 15–20% are still fairly active. I have seen this twice; once on about 60 crabs,

once on 120. — I think there is a measurable difference between the 20% which die first and the 20% alive after 4½ hours, in respect of frontal breadth.

Over 400 crabs, in the Woodhead apparatus, are being kept dirty; but otherwise fed and looked after. These will give an idea of any selective action of bacterial poisons.

Other 200 live in a large basin with nothing to eat but one another. I measure the shells of the victims as they are devoured.

I propose to try more changes of

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THE LABORATORY
CITADEL HILL
PLYMOUTH

4th August 1897

Dear Mr. Galton,

I hope your "cure" is doing you good. — For once I have some news for you which seems to me nicer than all the waters in Auvergne.

First, the mean frontal breadth of the crabs on the beach below here goes on getting smaller. — Just as has been the case before, the means for this year are quite sensibly less than those for previous years.

Secondly the evidence for a selective destruction of frontal breadths is clear and unequivocal: and the greatest amount of such destruction which I have been able to produce affects the broadest crabs most, so that it agrees with all the rest of the evidence.

The most potent reactive agent I have found is China Clay, finely divided as it is when it comes down the rivers into the Sound. The action is apparently merely mechanical, by blocking the gill-surfaces and causing asphyxia.

I enclose a curve of 108 crabs, from 10 to 12 mm. Carp. — The curve (Rack) shows the ~~percen~~ frequency of deviations from the mean frontal breadth among the whole group. — These crabs were placed in China Clay water, and the red curve shows the distribution of the survivors. — The number is small, because I have been anxious to use only quite perfectly caught crabs. — I have, of course, repeated the thing (altogether I have some ~~5000~~ 600 crabs so treated !) — and although all the measures are not complete I have finished enough to feel fairly sure that the result

~~Enclosed~~ enclosed is typical. There can, as you see, be no sort of doubt that the destruction is selective, and in the right direction.

This powerful action of finely divided mud is a very significant thing for a place like this. — Two ~~large~~ rivers, often milky with China clay, and loaded with the sewage of a large town, open into the Sound, and the mouth of the Sound is very largely blocked by a huge breakwater. — The ship channels in the Sound are known to be silting up at a rapid rate, so that dredging is yearly more necessary.

In addition to the mud and sewage from the rivers, the amount of sewage from the big populations of modern ships is not to be forgotten as a source of mud.

The evidence that the one factor in the surrounding conditions of the crabs which one knows to be rapidly increasing is a factor which acts selectively in the right direction is most cheering, I think!

Much of the suspended matter in the Sound is of course putrescent organic matter. — I have tried only dissolved products of putrescence. Enclosed is a curve of frontal breadth deviations of 341 crabs, kept in water containing putrescent squid. — The black curve gives the distribution of frontal breadths among the whole series. — The dotted red curve gives the distribution of survivors after 92 had died, and the continuous red curve the distribution of survivors after 226 had died.

I think there is evidence here also of selective action in the right direction; but it is so small that one should not lay much stress upon it without repetition; and the experiment is so horrible, from the great quantity of decaying matter necessary to kill a healthy crab, that I have not repeated it.

The density of the sea-water here

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is about 1026. — Increasing this to 1032 by the addition of "Tidman's Sea Salt" causes a large death-rate after some 20 hours, and there is about the same kind of evidence for selective destruction of big frontal breadths as there is in the case of putrid water: but this I have repeated, so that I feel certain about it.

Fresh water, on the other hand, has a slight ~~the~~ selective action in the opposite direction, killing the narrowest crabs first. — In one series, the mean deviation of the survivors from the mean of the whole group was about +0·2 unit. — I have repeated the experiment but have not yet made the measures,

ments.

I cannot help being very pleased with the whole thing. — There ~~to~~ seems to be no possibility of mathematical fallacy in it, and the fact that one can so easily and certainly establish a selective death-rate by quite simple and rough ~~in~~dications of conditions obtaining outside seems to open up any number of lines for future work, especially in connection with mud.

Have you gone into John Murray's stories about the change in the character of the fauna of the bottom of the sea in passing from the region where mud ~~is~~ from the land reaches

outwards to really clean water?

Yours very truly

W. H. Welch

I send you this out of the fulness of my heart. Please do not trouble about answering it for a long time. I am nearly dead, and am going to walk about ~~Ethiopia again~~ Italy to recover: so that I shall probably not see any letters until the end of September.

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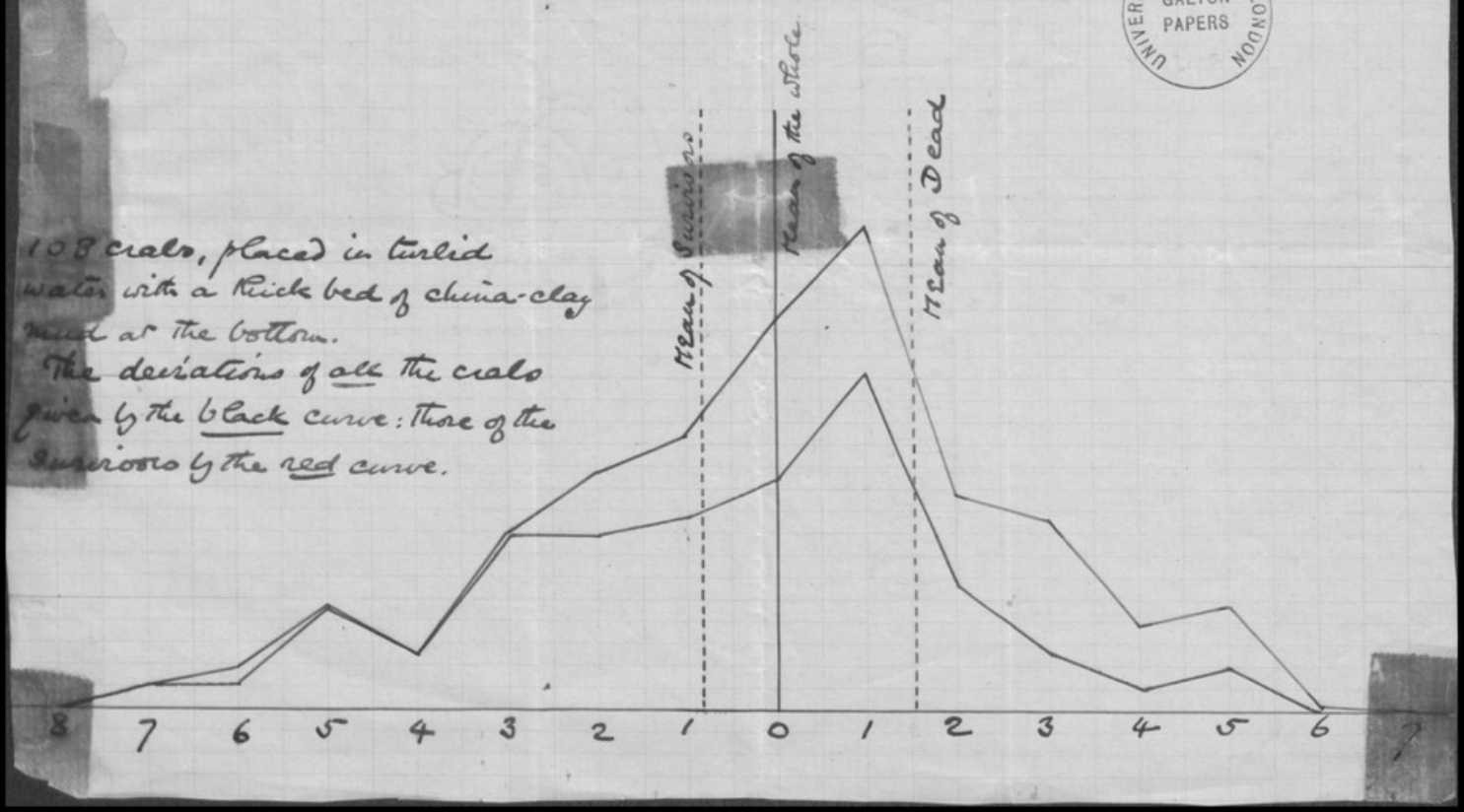
108 crabs, placed in turbid water with a thick bed of china-clay mud at the bottom.
The deviations of all the crabs given by the black curve: those of the survivors by the red curve.



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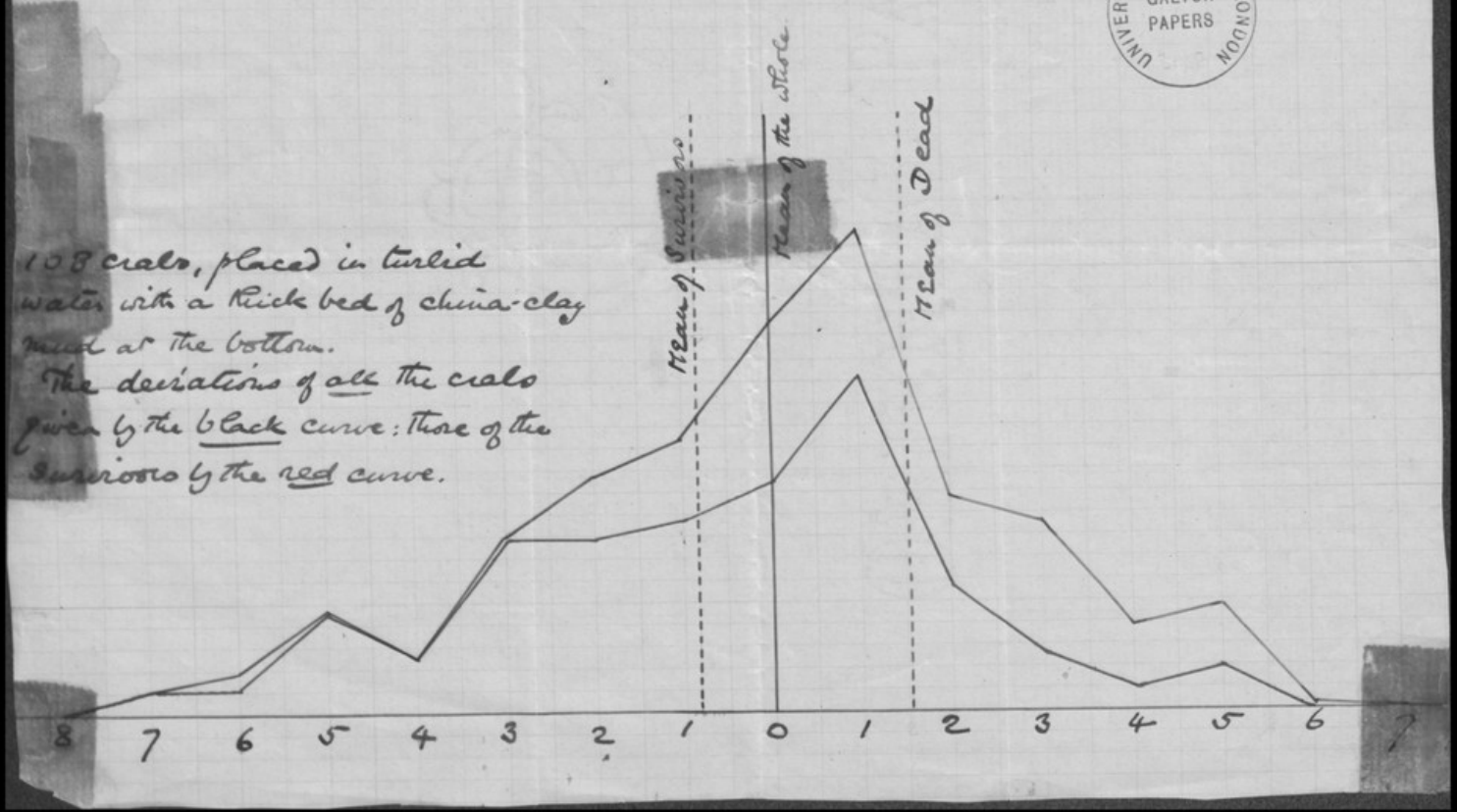
108 crabs, placed in turbid water with a thick bed of china-clay mud at the bottom.
The deviations of all the crabs given by the black curve: those of the survivors by the red curve.



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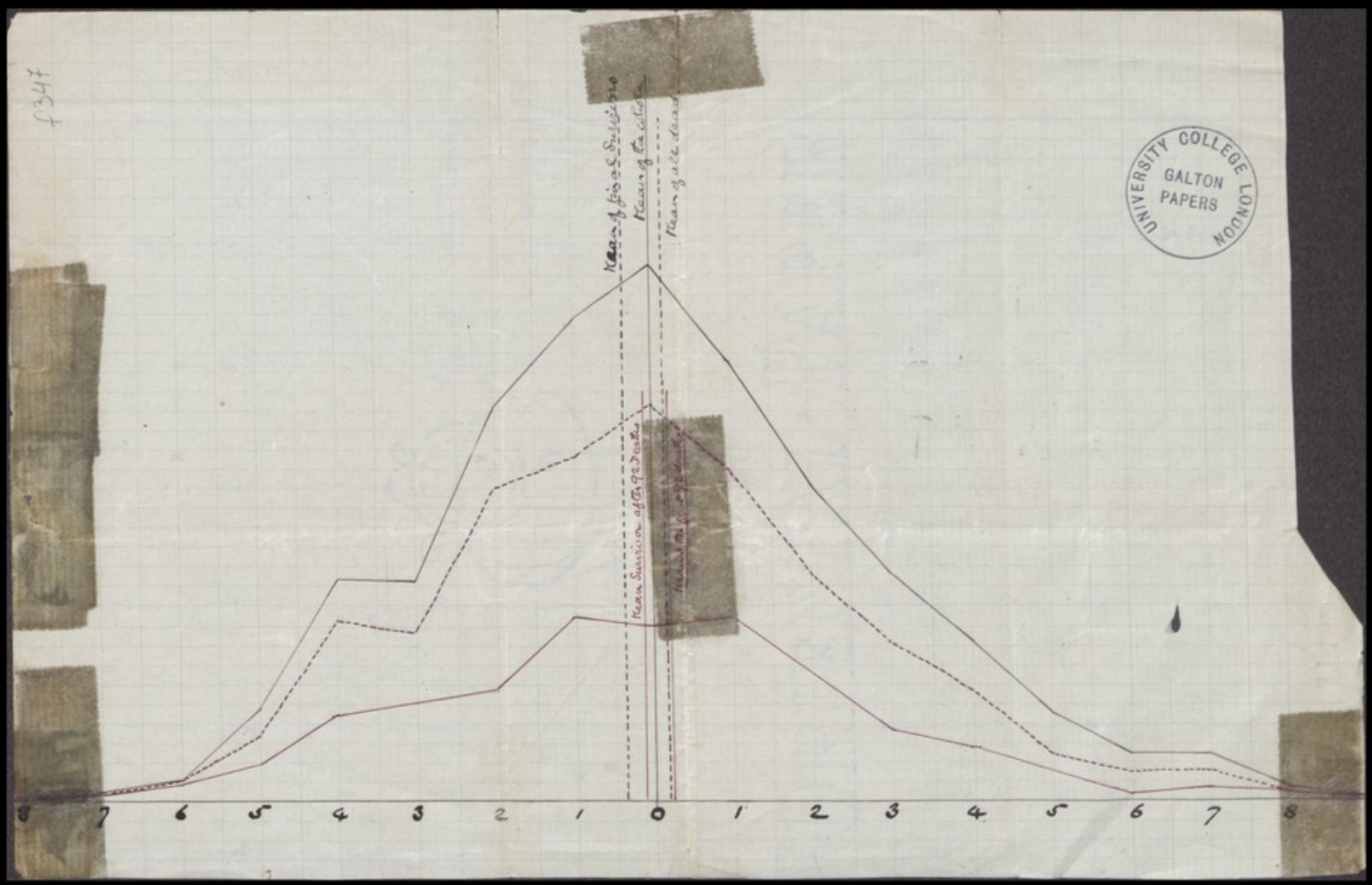

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30A, WIMPOLE STREET, W.

7th November 1897

Dear Mr. Galton,

Would Tuesday or Monday of next week suit you for a Committee meeting? — I have written to people, with the usual result, that the day which suits one man will not suit another.

Bateson (who matters a great deal) cannot come on Fridays: Masters cannot come on Wednesdays and Thursdays.

Bateson came here the other day. He wants to start fowl-breeding, with a view of testing the relative power of inheritance of two apparently equally "thorough bred" ~~[illegible]~~ sires, — one in-bred for ~~[illegible]~~ generations, the other not. He also wants help,

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with his butterflies. — He thinks he could start the fossils and get his help with a grant of some $75. — It seems true that, as no one else (except you and Heape) wants any money, we could not do better than divide the $120 I so in hand between (you and Heape) and Bateson?

Yours very truly

W F R Weldon

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30A, WIMPOLE STREET, W.

9th November 1897

Dear Mr. Galton,

Many thanks. — I will arrange with Bateson and Heape.

Can you help me to devise an experiment?

You may remember that I failed to get any useful result in working at the inheritance of parthenogenetic generations of Daphnia, because the characters I measured were so extremely susceptible to small changes in the water in which they lived. — I have suggested to my assistant Warren (who measured Petrie's bones) that he should work at the inheritance of a physiological character in these creatures, and not at an anatomical character.

Daphnia is very susceptible to change in the amount of salt contained in the water in which it lives. Warren has, by a large series of observations, made curves showing (1) the time necessary to kill all Daphnias of a sample

at constant temperatures and with varying strengths of salt; and (2) the time necessary to kill them all using the same % solution of salt and varying the temperature. By means of these data he can so treat a given sample of Daphnia that he can with certainty kill either the whole or 95%, say.

Having arrived at this result, he has taken 30 Daphnia, and killed all but one, in a 0·2% solution of salt. This took ~~four~~ four days almost exactly. — The survivor was placed in fresh water, and bred. — Three successive broods (some 20 individuals altogether) produced by this survivor were allowed to become full grown in fresh water, and were then placed for 4 days in 0·2% salt solution, at the temperature of the salt through which their parent had passed.

All these individuals survived. They

were then taken out of salt solution, and replaced in fresh water. They bred, and their young were allowed to mature in fresh water. These young of the 3rd. generation are now in salt solution, doing well, and looking as if they also were going to survive.

Here is an apparent case of inheritance with no segregation, — so that $r = 1$.

It is, of course, being repeated on other sets of Daphnia, and with other strengths of salt solution, so as to make certain whether there is some accident about it: but what I am not clear about is the next step, if we find that this result stands.

Evidently, the initial effect of the salt on the random Daphnia taken at the beginning of the experiment is partly a selective effect dependent upon pre-existing differences among the Daphnia themselves. But how far is the subsequent result due to the direct action of the salt upon the ovary of the survivors, impressing an "acquired" character upon (why not killing off all the susceptible-to-salt ova?)

it?

I am most anxious to devise an experiment which shall settle this point, and I do not see at all clearly how to do it. The only thing I can think of is time. If the salt directly changes the eggs, or germ plasm, or what you will, the action may be fairly assumed to be a chemical, and therefore a quantitative action, so that only a certain number of eggs or determinants can be influenced, although this number may be very large. — If this be the case, then by keeping an individual, which has survived the ordeal by salt, (in fresh water) for a long time, and allowing it to produce many broods, there should be in the broods produced a long time after the ordeal a greater "regression" than in those produced immediately after. Again, if $r = 1$ absolutely, then the offspring of the original survivor ought to give rise to offspring immune against salt; and all of these ought in their turn to do the same: so that if one

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30A, WIMPOLE STREET, W.

submitted a set of random Daphnia to salt, removed the survivors from the salt to fresh water, and allowed them to breed, then without any further selection by salt, the 3d. or 4th. generation of descendants of these survivors ought to be immune against salt.*

But I cannot feel sure that these results, even if they were obtained, would be crucial. Can you suggest any better test?

Yours very truly

W F R Weldon

* On the other hand, any quantitative chemical effect of the salt ought, because of the large number of eggs produced, to be exhausted; and therefore an "acquired" character, impressed by the salt on the eggs, ought to disappear?

Please turn over

I ought to add that there is a certain amount of evidence of direct action.

Daphnia will reproduce parthenogenetically for a long time (several years!) under favourable conditions of nutrition, etc. — under other conditions, the parthenogenetic individuals produce eggs which develop into sexual ♂ and ♀ individuals. Now, in our big aquarium, where the stock of Daphnia live, all the individuals are, and have for some time been, parthenogenetic: but two of the individuals bred after salt have produced sexual eggs. This is pleasant in one way, because it leads to a hope of directly comparing the segregation in sexual and in asexual inheritance: but of course it also indicates direct action on the ovary during the experiment.

Parthenogenetic eggs ~~develop~~ hatch after a

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week in water. Sexual eggs take three months, and must be dried. They are very difficult to deal with: so that we are not sure of being able to get the sexual regression.

Are the descendants in the 2nd, 3rd — nth generation of an immune Daphnia all as immune as those in the 1st ?

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sounds charming, and I will try that it is started as soon as may be, if the creatures show the usual sensitiveness to the colours of light!

Yours very truly

W. F. R. Weldon

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GALTON PAPERS

exception in the case of the ten last produced. These are now in 0·2% salt solution, and must still remain ~~there~~ there until Friday morning: but they are at present quite well. — So that an unselected sample of 30 individuals gave 29 which died and one which survived the ordeal. — The survivor has yielded, over 60 offspring of which certainly over 50 ~~probably over 60~~ have survived the ordeal, and none have as yet succumbed.

New parents, from both random samples, are now isolated, and their first born are growing up: so that in less than a fortnight the question, how far the inherited immunity is total, will be answered one way or the other.

=

Your experiment on the effect of light

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GALTON PAPERS

f. 353

30A, WIMPOLE STREET, W.

10th November 1897

Dear Mr. Galton,

In accordance with the news from Bateson and Heape, I have asked Harrison to call the Committee on Tuesday at four o'clock.

Many thanks for your letter about Daphnia. — I also had thought of ~~the~~ watching a part of the race without a second ordeal by salt through some generations.

As to the totality of the immunity, of course I have been as sceptical as yourself: but I have great confidence in Warren, and he tells me that the individual which survived the original ordeal has now produced over sixty young, and that all of these have survived, with possible

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30A, WIMPOLE STREET, W.

11th November 1897

Dear Mr. Galton,

I have not Bateson's letter of Feb. 1st! — I think I have sent you all the letters relating to the Committee. But surely it does not matter?

The Daphnia brood, of which I spoke to you, came out of salt solution today, all having again survived.

How does the following strike you as a test of direct action upon the Eggs?

The experiment is at present performed by putting random individuals suddenly into 0·2% salt solution. By adding salt very gradually to water in which Daphnia are, it is, however, possible to get all the individuals of a random sample to live in 0·2% solution, whereas a great number

die after four days, if they are suddenly placed in it.

Suppose, therefore, a random sample of Daphnia be gradually "acclimatised" to a 0·2% solution, without the destruction of any individual. Suppose that after such "acclimatisation" they are removed from the salt solution to fresh water, and allowed to breed. Then, let their offspring, after growing up, be suddenly submitted to the salt solution. What ought to happen?

I think it is fair to assume that any direct action upon the eggs will occur when the salt solution has reached a certain strength, whether it is added quickly, as at present, or slowly. So that if the result is due to direct action upon the eggs, then the offspring of gradually acclimatised

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individuals ought to be immune against salt to the same extent as the offspring of the present selected individuals. — If, on the other hand, the result of sudden immersion in 0·2% salt solution is mainly in the first instance a selective destruction, without sensible modification of the survivors, then the offspring of acclimatised parents should resist sudden immersion to a less extent than the offspring of selected parents.

I do not feel at all clear about this, but I think it is an experiment worth trying, and a set of random daphnia has been started for this purpose. It is now (after 3 days) in 0·05% salt solution: and we propose to take at least a week over the gradual increase of salt.

Yours very truly

W. F. R. Weldon

Weldon

f.357

30A, WIMPOLE STREET, W.

9th January 1899

Dear Mr. Galton.

Your draft letter seems to me admirable, and I certainly cannot suggest any improvement.

If you think it worth while to put both our names to it, please use mine; but surely yours alone would be enough?

I wish I had a stronger hope that the thing would produce some really useful record!

Yours very truly

W F R Weldon

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UNIVERSITY COLLEGE LONDON
GALTON PAPERS

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30A Grimpole Street W.
8th February 1899.

To the Chairman of the Physiological Committee of the Royal Society

Sir,

I beg to return the paper by Professor Herdman and Boyce, entitled "Observations upon the Normal and Pathological Histology and Bacteriology of the Oyster", which has been referred to me for report.

The first part of this paper (pp. 1-21) contains (a) a historical introduction; (b) an account of the histology of the gills; (c) a description of a new muscle; and (d) an account of the so-called "liver".

The chapters on the histology of the gills and "liver" do not, so far as I can discover, contain any result which is ~~substantially~~ new: they do not advance our knowledge of the subject.

The new muscle is identified as a "protractor pedis" which has changed its function in consequence of the atrophy of the foot. The description of this muscle is new and interesting, but can hardly be said to mark a distinct step in knowledge. The description might well be published in some technical ~~journal~~ journal.

Pages 22-32 of the paper contain an Essay on the greenness of oysters. In the case of "huîtres de Marennes" the authors confirm in detail the results obtained by Lankester in 1885, but do not add anything of importance. They also confirm the old statements that some oysters are green, not from the presence of a colouring matter derived from Navicula ostrearia, as is the case in Marennes oysters, but from the presence of ~~mineral salts~~ some other pigment: and they confirm the statement often made, and lately substantiated by Thorpe, that the pigment in question contains copper.

So far, the paper does not seem to me to contain any statements of importance. The ~~remainder~~ remaining pages are of considerable interest, but they consist very largely of literal

oper

(pp. 46-50)

The quantities of copper dealt with are so minute that analytical details should in my opinion be given.

copies of papers published by the authors in the last four volumes of the Reports of the British Association for the Advancement of Science.

Thus

(1) The chapter "On (Copper and Iron in) Green Oysters" is substantially identical with the appendix to a paper published last year (B. A. Reports, 1898, pp. 562-569). Many paragraphs in the two are literally identical, and all the analytical tables, given in the present paper, are printed in full in the B. A. Report.

(2) The opening section of the chapter on Microchemistry (pp. 42-43) is a verbatim copy of the section "Investigation of the Pigment" in B. A. Reports 1897 pp. 365 and 366.

(3) The presence of copper especially in leucocytes has been aperte ~~referred to~~ already by the authors in Roy. Soc. Proc. LXII p. 30, and B. A. Reports 1897 p. 365, and no additional statements of importance are made in the present paper.

(4) Most of the statements on the growth of oysters in solutions of
50) iron and copper salts have been summarised in the B. A. Reports. The new statement (that oysters grown for a long time in very dilute solutions of copper do ~~afficiently~~ take up that metal) is not accompanied by detailed statements of analytical results. ~~The minute quantities~~ det

(5) Chapter XI "On the Connection between Oysters and Infection" is ~~a literal~~ an almost literal copy of the paper by the authors published in B. A. Reports, 1895 pp. 723-726. The only substantial alteration is the prefix of an unimportant introductory paragraph.

(6.) Chapter XIII, on "The Bacteriology of the Oyster and other Shell-fish" is in great part copied, with only a few verbal alterations of slight importance, from B. A. Reports 1896 pp. 663-665 and 667-668. ~~All the~~ The substance of pp. 60-70 of the present paper, ~~by~~ (including the whole of the tables showing the reactions of Bacteria cultivated) is printed in the report referred to.

The ~~only~~ last few pages (from p. 71 onwards) contain ~~five~~ several tables which have not hitherto been published,

showing the characters of bacteria obtained by cultivation from the alimentary canal of [redacted] oysters, mussels, cockles, whelks, and periwinkles. ~~Taking~~

As announced in B. A. Reports 1898 p. 560, bacilli ~~repeat~~ resembling the Colon bacillus of Rau were frequently isolated, ~~be~~ but nothing absolutely identical with the Typhoid bacillus was found.

These pages contain a further result which ~~is~~ has not been published anywhere, — the identity of some bacilli from the alimentary canal of oysters etc., with the B. Enteridis sporogenes ~~said by~~ of Klein. ~~the agreement with some forms of human diarrhoea~~

I am very strongly of ~~given~~ opinion that the paper in its present form, consisting largely of a mere rescript of published work, should not be published by the Royal Society. The two new points in it, namely the description of the protractor pedis muscle and the ~~identification~~ discovery of Bacillus Enteridis sporogenes in the alimentary canal of molluscs do not seem to me of sufficient importance to justify publication by the Royal Society, even after the removal of extraneous matter from the paper. I am therefore of opinion that the Council should be advised not to publish the paper at all.

Yours.

Etc.

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UNIVERSITY COLLEGE LONDON
GALTON PAPERS

Weldon

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30A, WIMPOLE STREET, W.

Dear Mr. Galton,

I am indeed glad to hear that you are on the right road again. I am looking forward to seeing you tomorrow.

There is no news, so far as I know, of the Committee.

For myself, I have measured through the first of the series of crabs treated with China clay at Christmas, and have again obtained the result that the clay destroys first the broad-fronted crabs, so that the mean of the survivors is reduced.

Of course, Dohrn will be glad to see you at Naples. — If you have time when you are in Naples will you call on my pupil Parkinson, who is measuring some

very variable shells at the aquarium? He would be hugely pleased, and you might be interested. — I do not know exactly what he is doing; he changed his plans since arriving in Naples; but his work sounds interesting, so far as I can gather from his letters.

Yours very truly

W F R Weldon

Have you seen Pearson's paper on heredity of cephalic index?

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Weldon

30A, WIMPOLE STREET, W.

27th May 1898

Dear Mr. Galton,

At the next meeting of the Royal Society Council the medallists will be considered, and among them the Darwin medallist.

I remember Huxley's speech, when the Darwin medal was given to Sir J. Hooker. He said that this medal was founded for the encouragement of younger men. Of the younger men, whose work I know, I can think of no one who has done so much for real Darwinism as Karl Pearson; and I can think of no one whose personal peculiarities would lead him to place a higher value upon recognition of this kind.

I imagine that you will feel as I do in this matter. If you do, will you use

your influence with other members of Council during the next few weeks, so that the award may be accomplished? — If you do not, will you let me know, so that I may not unclerkly propose Pearson?

Yours very truly

W F R Weldon.

If you think Pearson should have this medal, I should be glad of your advice on a further point.

He is, as you know, rather abnormally sensitive. — Should I ask his permission before proposing him? On the one hand, this is a breach of confidence; on the other, I use his name in a way which might

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hurt him. I cannot see clearly.

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HB) [Found, out of place, among letters of Weldon to Galton]

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

30A, WIMPOLE STREET, W.

15th November 1898

Gentlemen.

I beg leave to suggest a slight alteration in the Report of the Council, of which you have sent me a draft.

The section of the Report which deals with the Treasurership refers to the reception by the Council of the resolutions passed by a meeting of Fellows. After stating that this meeting of Fellows had addressed a communication to the President, the Report says "Although it may be a question "how far it is desirable that the Council "should receive extraneous advice in "this manner, the President thought it "best to make the communication known

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to the Council".

These words seem to me to be capable of a construction which I am sure the Officers of the Society did not mean them to bear; and they seem to me likely to convey an erroneous impression of what actually occurred.

The words may, I think, be interpreted to mean that the Council ~~regards such extraneous advice as a common~~ felt doubtful whether it ~~ought to~~ should receive the communication referred to, and that it consented to do so at the request of the President. ~~The words used seem to me further to imply that the Council~~

My recollection of what happened, ~~when the communication was produced~~ is that the President was supported

~~at the last~~

by a large majority of the Council in ~~wishing~~ wishing to receive this communication. If I remember rightly, the matter was put to the vote, and only a very few members of Council voted against the reading of this communication.

The words in the draft Report seem to me to imply ~~that the Council thereby~~ an opinion, on the part of the Council, that a body of Fellows, who met together and drew up an address to the President on a matter affecting the welfare of the Society, were acting in a questionable way. I do not recollect an expression of such opinion by more than one or two members of Council, and the voting seems (to me) to have shown that ~~no~~ no such opinion was held by the majority. ~~I~~

I hope I have made clear to you the ~~reason~~

the reasons why I am anxious ~~think am suggest~~
that these words I have quoted should
be expunged from the Report.

The Report is written in the name of
the Council as a whole; ~~and~~ the words
I have quoted seem to me capable of an
~~interpretation~~ interpretation which is
~~would~~ not in accordance with any
expressed opinion of the Council as a whole; and
this interpretation is likely ~~calculated~~ to
give unnecessary pain to a number
of Fellows ~~of the Society~~, who have been
acting for the common good of the
Society.

Dear
Sir
Yours
etc. etc.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

Weldon

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UNIVERSITY COLLEGE LONDON
GALTON PAPERS

30a, WIMPOLE STREET, W.

25 November 1893

Dear Mr. Galton,

Many thanks for Keek's paper, which I return. – The point is very interesting, and so far as I know altogether new.

2

May I ask you about another matter? I am getting ready an application for the Linacre professorship at Oxford, which I propose to send in as soon as the vacancy is declared. — If you think me a fit person for the post, will you be kind enough to write me a letter, which I may send in with my

application? — I shall be very grateful if you feel able to do this; but I hope you will tell me at once, if you do not think me the right man, or if you prefer not to interfere in the matter.

Yours very truly

W. F. R. Weldon.

f366r

It is not within my capacity and [illegible]
to express a useful opinion
as to your general qualifications for the chair of Zoology
at Oxford, but to the following matter
topics [illegible] lie within my province
and I can speak most heartily in
reference to them. I mean your
to grip ~~[illegible]~~ of the great principle
that zoological species are to be defined
and that the incipient steps in their evolution
are to be discovered, mainly by means of
the higher [illegible] statistics.
I have had the good fortune to be
~~have been~~ acquainted with your principal
and most laborious efforts in these directions, since their

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very beginning, & can testify to your [illegible]
inspiring singular
Zeal and ingenuity as well as to the enthusiasm
that you have excited in others [illegible]
students

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Tuesday 13 or Succeeding Thurs or Friday W. – now bout me

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

8, St. Aldate's, Oxford

~~30A, WIMPOLE STREET, W.~~

3rd June 1899

Dear Mr. Galton,

Will you tell me some days on which you can attend a meeting of the Evolution Committee? — The Government Grant Committee has brought about a crisis in our affairs, by granting us only £50. — As at present arranged, if we cannot distribute money, we are of no use at all; and it seems to me that this will be a good opportunity for considering the whole position. — If it is convenient to you and to others, I shall be glad not to come to Town on a Monday or a Wednesday; otherwise I am at your disposal at any time.

2

I think the problems of race-formation in the little rivers here will be delightful

things to work at. I have been trying to learn something of the distribution of local races in the waters; but of course one's first term in a quite new place does not leave much time for work, - and I have to lecture once a week at University College. — Still, there is a beginning: and as soon as the Long Vacation comes, I think I can start some experiments in the direction of trying to show that the characters of one local race may be to a certain extent ~~initiated~~ produced by selective destruction of another race, by changing the character of the water. The salts dissolved in the waters here differ from river to river so greatly that experiments of this kind are quite possible.

Have you seen Pearson's last piece of work? — It seems to me very beautiful.

Yours very truly

W F R Weldon

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UNIVERSITY COLLEGE LONDON
GALTON PAPERS

8, St. Aldate's,
Oxford
30th July 1899

Dear Mr. Galton,

Many thanks for the Report, which I am sending on today. I have put in Poulton's name.

There is a small point to notice in the proof.

You say "Twenty small marks.... "arranged in 3 horizontal and 5 vertical "rows .... should be made upon the wall" (p. 9)

I do not know whether you use 15 marks, or 4 horizontal rows, so I have not ventured to change anything, although there are 15 marks in your diagram; but it is just worth while to mention it, in case you might like it in the proof.

Yours?

The Oxford rivers have had to rest during the last few weeks, because of the pedigree moths. — These are apparently going on very well indeed. — There are at present about 3500 caterpillars, belonging to 38 families, ~~in each of which~~ forming the 3rd. domesticated generation.

Warren and I are working out the correlation coefficients as fast as we can, but our fastest is very slow, because the work of counting scales on the wings is the most detestable thing I have yet found.

A great point about these moths is that they will live for many days after emergence, — so that there is no need to heat or to cool the pupae, as in the case of S. Illustraria.

This seems to me, after Merrifield's results, of very great importance; because he has shown so well that changing the temperature during pupation introduces "acquired" modification; and unless all the brood are treated in the

some way, the inherited characters of different individuals will be marked by the "acquired" characters in different degrees, and so the coefficient of correlation due to inheritance cannot be accurately determined.

I think one colour spot will be of some interest. — If the results, so far as at present obtained, are reliable, the number of scales on this spot is inherited almost entirely through the male parent, so that the "mid parent" will have to be something which is not exactly the mean of the two parents, — and the male ancestor will have higher correlations throughout than the female.

If this is true, it will give a very nice form of your ancestral series, — because I suppose the mid-parental series will still be $\frac{1}{2} + (\frac{1}{2})^2$, and so on, but neither parental series will be half this.

And if so, what about the nuclei?

If the symmetrical figures made by the ♂ and ♀ nuclei are really indications

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of the hereditary mechanism, then asymmetrical inheritance ought to be associated with asymmetrical nuclear figures; and if we establish our asymmetrical heredity, we will examine the nuclei during fertilisation, and see whether these are asymmetrical or not.

There is some interest about this point, because the eggs of some insects have been said to be exceptional, in as much as each egg normally receives more than one spermatozoon, — so that there is a possibility of excepcive male nuclear ~~selection~~ influence, if our moths should prove to behave in this way.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

Yours very truly
W. F. R. Weldon

The moths will emerge during the British Association meeting, so that I cannot go. — If you want the Committee reappointed, and if you are not going to Dover, will you arrange that some one shall propose reappointment

13715

not Monday 13
July 14
and 21

MERTON LEA,
OXFORD.

5th November 1899

Dear Mr. Galton,

Many thanks for your interesting paper about voting.

Should we not have a meeting of the Evolution Committee some time soon, in order to determine our dissolution? — If you could name a few days some time ahead, I would try to arrange a full meeting. — I can manage any afternoon except Friday, if I can be at Paddington at 4·45; otherwise I may be in a difficulty. — On the 14th., 15th., 21st. and 23rd. I must catch the 4·45, if I come to Town at all.

Would it be too much to ask

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UNIVERSITY COLLEGE LONDON
GALTON
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Shall be in April

whether some time next term you would read my notes of eight or nine lectures?

I am trying to give elementary proofs of the main points about your work and Pearson's. I am rather pleased with my success in showing the properties of Pearson's generalised probability curve in an elementary way, and I wonder whether a textbook of you and Pearson, with an appendix to every chapter containing the amount of elementary algebra necessary to demonstrate the points which arise in the chapter, might be worth publishing.

Pearson will only do one of two things. In the Phil. Trans. he uses only

difficult mathematical processes, and in his popular books he describes results, leaving the processes on one side.

It seems true that many men are deterred from statistical work because they will not blindly use formulae whose raison d'être they cannot understand, while they cannot read Pearson's proofs.

There are plenty of men up here in this position. I find a round dozen come to listen to me: and I think such a book as I describe might be of use to them.

Will you tell me what you think?

Yours very truly

W F Sheldon

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

F373r

Weldon

MERTON LEA,
OXFORD.

28th November, 1899.

Dear Mr. Galton,

I hope you have had a comfortable journey, and that the Mediterranean behaved itself.

We held our meeting yesterday. There were present F. Darwin, Bateson, Heape, E. Clarke, and myself.

After I had read your letter, the others passed a resolution urging the re-appointment of the Committee.

The discussion seemed to me to show more than ever the futility of the Committee. The view taken was that the Ctee. should not attempt to direct, or to discuss the investigations, but that it should exist in order to confer upon its members some sort of authority in their dealings with breeders and such people.

It was distinctly said that in this way the study of "Natural History" might be encouraged, and that those who did not care for "Natural History" ought to leave the Clee.

I do not see that it is possible to pass on passing votes of confidence year after year in work of which one knows practically nothing. That kind of relation between a Clee. and the work of its members is ~~[illegible]~~ absurd.

The "authority" lent by the Clee. to its members is to me detestable. The whole idea of such a thing seems a piece of vulgar snobbishness; and I do not believe that competent people feel the need of such assistance when they seek for information.

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But the most dangerous thing about the discussion seemed to me the deliberate antithesis which these men sought to establish between what they called "Natural History" on the one hand, and any sort of statistical enquiry, leading to numerical results, on the other.

Several members of the Cttee., of whom Bateson is the leader, have the idea of such an antithesis, and are strongly influenced by it. However sincere and able such men may be, I think they do harm, and I think it would be a great misfortune if a Committee of the Royal Society were to officially adopt their views.

I cannot go on acting as secretary to this Committee. Will you advise me what to do?

I should like to write to the Council, asking that at least I may not be re-appointed, and explaining my reasons.

But I could not do this without

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making it evident that in my opinion the whole Committee should not be reappointed: and I am afraid that would be disloyal to Bateson and the others, — who are doing their best for what they believe to be right, whether I agree with them or not.

Will you tell me your views, addressing me at the Hôtel de Russie, Via Babuino, Rome? — We go thither on the 9th. — I hope to write my Lecture notes in reasonable shape while there, and shall take advantage of your patience so far as to send them to you, since you are good enough to allow me.

Yours very truly

W. F. R. Weldon

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Weldon

UNIVERSITY COLLEGE LONDON
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MERTON LEA,
OXFORD.

19th February 1900

Dear Mr. Galton,

It is very good news that your journey is so successful. — I hope that your wise resolve to keep away from England till May will finally vanquish your bronchitis.

You are certainly better away just now. We are painfully getting rid of 10 or 11 inches of snow: most of the telegraph wires seem to be broken, and the South coast is all round the coast.

Pearson's new edition is published; and his new portion about biology is splendid. Such of the Oxford philosophers who condescend to read him are, I believe, very angry; but he will do them (and others) good! I wish he could do himself good. — He is ill with influenza, as usual, and from his letters I am afraid his attack this year has been very severe.

He has gone into the question of the

degree of likeness between repeated parts of one individual, — leaves of a tree, flowers of a plant, and so on, — and the results (which I expect you know) are most interesting.

If he would only take the trouble to explain his methods in a little greater detail, I should not dream of going on with my M.S.

I did not send any M.S. to you. Last term I got on very well, I think, and made the derivations of all Pearson's curves from binomials or from a hypergeometric series fairly plain, without using an integral.

At Christmas, however, one had to consider how to explain his process of fitting curves to observations; and it was clear that this could not be done (at least by me!) without one or two integrations. Therefore I took an Integral Calculus away with me, and during Christmas I wrote an essay on the

nature and use of integrals, so far as it seemed necessary to explain for the purposes of following Larmor. — This was my first lecture this term, and I think people followed it.

But if one has to face the process of integration at all, one had better do so at the beginning, and by so doing avoid long pages of circuitous algebra. So that I have begun to re-write the whole thing. — The great difficulty one had felt before was the coordinate length of some quite simple proofs: and I think one can now get over this.

2

The Evolution Committee still goes on, I believe.

I sent your letter to Foster, and told him that I heartily agreed with your views. I felt that I ought not to resign until I had reported to the Government Grant Committee on the money in my charge; but on Jan. 31st I did resign, having sent in the necessary report. — Dyer, Larmor, Meldola, also resigned. — Bateson was chosen as Secretary, and of course I know nothing more about the thing.

You will probably know, before you read this,

that Foster is now M. P. for London University.
This will have some sort of effect on the Royal Society, I suppose. — One is sorry that Foster thinks it worth while to give the necessary time to the House of Commons!

So far as I know, all the careful people say what you say about the Chameleon's disregard of the surrounding colours. It is a most mysterious beast!

Warren and I are anxiously waiting for the spring brood of our moths to emerge, so that our heredity of coloured scales may give a result of some sort at last!

I am hugely pleased, because I have caught a real pupil. — A man who has taken his degree, and who has lots of money, wants to stay up and work at inheritance! He knows beetles, and took his degree in Botany, so that he ought to do something.

Yours very truly

A. R. Wallace

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Weldon

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

MERTON LEA,
OXFORD.

10th June 1900

Dear Mr. Galton,

Have you read a book on the distribution of "Palaearctic" animals, by Kobelt ("Studien zur Zoogeographie", 2 vols., Wiesbaden, 1897-99)?

If you had not been in the chair the other night, I would have asked leave to sit next you, and to talk about this book, which has puzzled me very much.

Kobelt is a man learned in shells; and he devotes some three quarters of his book to a discussion of the distribution of land snails. — We have a very good collection of shells here, and I have followed most of his points with the aid of the collection.

You remember Darwin's treatment

of the very peculiar snails of Madeira and the adjacent islands, as examples of species which ~~had~~ have differentiated since their isolation in new surroundings, as a consequence of accidental transportation from Europe?

It has been known for some years that one or two snails, extremely like (two) these, occurred fossil in Europe: but Kobelt refers to a series of papers by all sorts of people, which I am ashamed to have overlooked, showing that in the Oligocene and Miocene of Italy, France and Germany ~~the~~ several of the subgenera of Helix and Pupa, which occur now only in ~~the~~ Madeira and the Canaries, are extremely abundant. They are mixed with shells of other genera which are now confined to single islands of the West Indies, or ~~&~~ to the Malay Archipelago,

and with modern European shells.

There is a group of snails put into the genus "Sagda"; now peculiar to Jamaica: this, with another peculiarly West Indian genus Bentellaria, occurs plentifully in the Italian pliocene: and the species of Pupa which occur in these beds are said to belong entirely to two groups, of which one is now confined to the Caucasus and Crimea, the other to the Canaries.

So it is with many other "genera" of Helix, which are now found only in small isolated localities.

Clearly the accepted explanation of insular species, as due to differentiation by selection under new conditions since the formation of modern islands, fails altogether for these land shells: and the worst of it is that molluscs are almost the only group of animals in which one can hope to find a large percentage of species fossilised, — so that precisely where the record is most complete, the pretty theory breaks down most hopelessly.

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I can see no escape from one of two alternatives. Either each species of snail is susceptible to one dominating group of factors in its environment, and is more or less indifferent to others, — or the species are not being preserved by reason of the "adaptive" nature of their characteristic features.

Kobelt further attacks the idea that Alpine animals and plants can be regarded as arctic things which have been forced southwards during a glacial epoch, and persisted in an arctic climate ever since. — He discusses several Swiss deposits, which are said to be preglacial, which contain bones of "arctic" animals. This point I cannot pretend to judge. But he points out a thing which bears upon his argument in a striking way, — namely, that the boundary between the Alpine fauna and the fauna of Italy (so far as snails are concerned) is not a climatic

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MERTON LEA,
OXFORD.

boundary.

He tries to establish the proposition that the areas of distribution, which all the people who write about snails seem to recognise in Europe & Asia, are of preglacial origin. As an example, ~~take his treatment of the Pyrenees~~ Take his treatment of the Pyrenees.

As I understand the matter, the current belief is that the Pyrenees were elevated in Oligocene or Early Miocene time, and that the Gironde was then a great gulf, which was more or less extended up the valley of the Garonne towards the Mediterranean, which was then sea. In the same way the Ebro was an arm of the sea, so that the Pyrenean region, between the Ebro and the Garonne, was more or less an island. Since that time, not only

has land communication with France and Spain been established, but the periods of Glaciation have come and gone. Kobelt does not believe that this had much effect on the snails. He uses the charming phrase "Eiszeit als Episode" — in the history of land molluscs.

Any how, he refers one to Locard, who has published a very full list of all the localities in France where snails are found. It is clear that the valley of the Garonne, and a continuation of that valley into about the department of Hérault, — say, somewhere between Narbonne and Montpellier, — mark a clear boundary between two sets of species.

The big edible snail, H. pomatia, did

not occur South of the Garonne until it was introduced into the country round Bordeaux between 1850 and 1860 (there seems good historical evidence). It does not now occur in far from Bordeaux, within the area between the Garonne and the Pyrenees. Now it does occur down to the sea in the Rhone valley, and it runs along the Riviera, into the Alpes Maritimes, Northwards into the Côte d'Or, which is quite as mountainous as parts of the Pyrénées Orientales — round into Italy — Dalmatia — Greece — and so on.

From the whole of France, Locard records some 300 - 310 species of Helix. From the region between the watershed of the Pyrenees and the Garonne he records 190; and of these, 34 do not occur elsewhere in France: about 30 have their western limit somewhere about the Pyrénées Orientales, and run right on Eastwards,

most of them going well into Italy. About 20 run along the west of France northwards, some of these stopping in the neighbourhood of Poitiers. About a dozen species run over the whole of France; and these are (I think) without exception species which extend into England, Italy, Spain also.

Now, (1) the Rhine is only a boundary between half a dozen species; (2) the snails on both sides of the Ganges and Nipisippi are said to be the same; certainly the snails on both sides of the Rhone are the same. Therefore the mere presence of the Garonne does not account for the 34 peculiar species South of it, and for the large number of Northern species which do not occur South of it.

Again, the suggestion that these species have differentiated after small post glacial migrations will not do. Such migrations are not likely to have affected both slopes of the Pyrenees in

I am shaken in all my foundations by Kobelt's book. Will you read it, and try to show me ~~that~~ how in spite of it one can believe that snail species have been produced and maintained by Natural Selection?

Kobelt impresses me the more because he does not try to draw any inferences. He frankly leaves the thing as a puzzle.

You will perhaps now understand why I have not sent you any M.S. to read. The statistical part is easy, though dull; but I tried hard to write with reference to Natural Selection; and I ~~can't~~ found this abominable thing waiting for me.

Yours very truly

W. F. Weldon

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June 10 1902

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MERTON LEA,
OXFORD.

the same way; and although the Southern slope is little known, 13 of Locard's ~~species~~ 34 peculiar species are known to occur on the South side, and to extend as far as the Ebro: but not a single one of these 34 species is known to occur South of the Ebro.

Here you have a definite set of 34 species, common in part to both sides of a big mountain range, and limited, not by any climatic influence, but by the remains of an old Miocene barrier.

That some of these species have not differentiated after ~~not~~ a post glacial migration across the Ebro & the Garonne is shown by the fact that one of them (Helix constricta, which ranges roughly from Bordeaux and Narbonne to the Ebro) is known fossil in beds which are said to be of Oligocene or Miocene age in the department of Haute Garonne.

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MERTON LEA,
OXFORD.

26th June 1900

Dear Mr. Galton,

I am asked to send the enclosed to you. I think the statements in the memorial are fully justified, and I have been very glad to sign it. — If you are willing to do so, will you return it to Poulton?

I am horribly perplexed by the snails. The idea of "adaptation" to one factor only in the environment seems to me to limit the whole scope of the theory of Natural Selection to a perilous extent.

Are you dining at the R. S. club on Thursday? I should much like to talk to you about these things.

Yours very truly
W. F. R. Weldon

London

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UNIVERSITY COLLEGE LONDON
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MERTON LEA,
OXFORD.

1st July 1902

Dear Mr. Galton,

The series of papers by Pfitzner on variations in the hand and foot is:

I.– Die Variationen im Aufbau d. Handskelets 1895

II.– Die Variationen im Aufbau d. Fussskelets 1896

These in Schwalbe's "Morphologische Arbeiten Vols. IV & VI.

III.– Die Morphologische Elemente des Menschlichen Handskelets

Schwalbe's Zeitschrift für Morphologie und Anthropologie Vol. III. 1900.

I think the first is the only paper with measurements of the bones,– which are those used by Pearson in his work on correlations of long bones of the hand.

(Schwalbe Morphol. Arbeiten Vol II (1892-3) p99 is the paper with measures

It seems to me every day more certain that Mendel's results depend on this sort of thing:

Take any character, varying as in the diagram. — If you do not really measure the intensity of the character, you may easily find that some range say BC looks roughly different from the rest, say AB. Now, suppose blended inheritance. Any parent out of the range AB (especially of course a carefully chosen "typical" parent probably within AM) will produce when mated with a parent out of the range BC, offspring whose mean will lie inside AB: and the S.D. standard

(of one pair of parents)

deviation of the offspring being less than that of the population from which the parents sprang, it may very well be, in particular cases, that the chance of any offspring lying within the range BC is so small as to be experimentally = 0. There can have at once dominance.

That such dominance should occur, of course there must be some definite relation between AB, BC, and the standard deviation of the parental race.

I am trying to work this out for hairy and glabrous plants of Lychnis, which are said to obey Mendel's Laws absolutely. Hairiness is most clearly in this case a character which varies enormously, even in the same plant.

L. diurna has a cluster of root leaves, about 10-12, then a long flowering stem which bears about 6 pairs of leaves.

In one plant, the number of hairs per

Square centimetre of the lower side of leaves was

For each of 2 lower root leaves — 50

For each of 2 upper root leaves — {102 / 133

For upper stem leaves — 148 / 149

The hairs of the root leaves are quite short, and in this plant not glandular. 30 per cent. of the hairs on the stem leaves are glandular (secreting an essential oil which makes the plant sticky).

The upper leaves of the next plant measured had no glandular hairs: the number per square centimetre is from 232 in the lower part of the stem to 323 in the upper part.

These are, of course, both "hairy" plants.

The present observed range of variation is from 148 to 1090 hairs per square centimetre of leaf surface, for the upper leaves: of these hairs, the percentage of

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OXFORD.

glandular hairs varies between over 30 and 0.

It seems to me that if you call a plant with 150 hairs per square cm. "hairy," then the odds that all the offspring of a plant with 1090 hairs per square cm., cropped with a plant having 0 hairs per square cm., will be "hairy" are very great indeed: and I think this is the explanation of much "dominance":

If you take the offspring of such a pair of plants, and pair these offspring at random, you will sometimes pair two "hairy" plants which have very few hairs: and the chance that these will give rise to some glabrous plants is probably large. That the chance of getting a glabrous plant out of such

cropping should be = $\frac{1}{4}$, as Mendel requires, depends entirely on the standard deviation of haininess in the original population?

Does it not seem to you that the majority of "Mendelian" cases can be quite well interpreted in this sense?

Yours very truly

W F R Weldon

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MERTON LEA,
OXFORD.

14th May 1903

Dear Mr. Galton,

I am very anxious to show you Darbishire's mice, and with that or any other excuse it would be a great pleasure to my wife, as well as to me, to see you again.

Can you spare us a night between Tuesday 26th. and Friday 29th., both inclusive?

The mice seem to me to show conclusively how little there is in the theory of pure gametes.

Darbishire has obtained 80 young by crossing an "extracted" albino with a pure-bred fawn-and-white waltzing mouse; and the result

shows how enormously the ancestry of the white mouse matters, whereas of course on the theory of gametic purity the ancestry of a pure recessive individual should not matter at all.

As you know, when fawn-or-white waltzing mice are paired with albinos, the hybrids are black, yellow, or grey, or a patchwork of one of these colours and white.

Any such hybrid, paired with a pure-bred albino, gives some game of various colours, and some albinos — albinos and coloured individuals occurring in about equal numbers. Darbishire has taken 20 of these "extracted" albinos, and paired them

with pure-bred waltzers; the pedigree of ~~each~~ their gang being

Pure Waltzer Pure waltzer Coloured hybrid Pure Albino

Pure waltzer Extracted Albino

Hybrid gang

so that each hybrid of this kind has Grandparents

2 pure fawn-&-white waltzers
1 pure albino
1 coloured hybrid.

The correlation between the colour in the coat of these hybrids, and that in the coat of their hybrid Grandparents, is at present 0.94 (Pearson's arithmetic!)

The correlation between the size of the coloured patches in these hybrids and in their hybrid Grandparents is 0.45.

These correlations are based on 80 game. I think there is no doubt that the 0.94 will fall, for reasons I should like to show you. But anyhow here is a strong correlation between one of the four grandparents and the grandchildren, which can only be transmitted through the pure recepive parent, whose gametes cannot, therefore, be "pure" in the Mendelian sense.

It seems to me that the whole thing must be interpreted as reversion, the remoteness of the ancestor reverted to being some function of the difference between the parents, — the extreme case being, as Darwin said, reversion to a remote ancestor when distinct races are crossed. But I cannot think straight enough to formulate a hypothetical law of the connection between parental character & reversion at present.

Yours very truly W F R Weldon

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UNIVERSITY COLLEGE LONDON
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MERTON LEA,
OXFORD.

22nd May 1903

Dear Mr. Galton,

Will you come here, at any time you like on Monday after Lunch (since you prefer that arrangement)?

I think you will like to see the general result of Darbishire's work before going into the mouse-room itself, which is distinctly odorous on a warm afternoon.

There are now catalogued 317 first hybrids; 303 offspring of hybrids × hybrids; 377 offspring of hybrids × albinos; 80 offspring of

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"Extracted recepive" albino × pure waltzers, and 16 offspring of "Extracted dominants" (the "pink-eyed mice with some colour in the coat" = g'g' of Bateson's Letters in Nature) cropped with pure albinos.

We have now a set of correlation tables which show conclusively, I think, the inheritance of colour characters through the recepive parent (albino) and the consequent effect of using albinos of impure ancestry in making the first crop.

These facts seem to me fatal to any theory of "gametic purity." The

fact that albinos of coloured ancestry, although they produce albino young when ~~p~~ paired together, yet show traces of their coloured ancestors when paired with a coloured mouse, — this kind of fact is surely direct disproof of the purity of albino pairings and is exactly the phenomenon Darwin called atavism ?

Yours very truly

W. F. R. Weldon

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GALTON PAPERS

MERTON LEA,
OXFORD.

22nd May 1903

Dear Mr. Galton,

The enclosed letter from Pearson almost explains itself.

You will remember a notice, in Biometrika, that Bateson proposed before long to reply to Pearson.

Here is his reply. Pearson, rightly enough, urges that it is mere rhetoric, of no serious importance whatever, and that he had better publish it with the rest of his promised attack on Biometry.

But Bateson's ~~rhetoric~~ rhetoric is always clever and specious (I would give a great deal to be able to design a passage like that in the preface to

the Defence of Mendely in which he compares me to Demetrius the Silversmith and himself by implication to St. Paul!)' and I have vainly tried to persuade Pearson that while this stuff will doubtless waste space and money, the refusal to insert it, after what has passed, will give the enemy occasion to blaspheme.

I am not quite sure (I am nearly sure) that an accusation of unfairness might not be plausibly brought, if we refused this.

We both agree to ask you.

Can you find time to read Balestier's portentous stuff in the

train on Monday morning?

What I feel is that as usual I have given the situation into the enemy's hands. If we refuse to publish Bateson, why do we publish the stuff I have written about de Vries and him?

The real reason, — that some one had to provide pages of "copy" for a quarterly journal, and Pearson's time was too valuable to waste, cannot be appealed to.

Will you consider the matter?

Yours very truly

W F R Weldon

Personalities apart, I think the Daily Graphic is the proper journal in which this stuff should be published.

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MERTON LEA,
OXFORD.

29th May 1903

Dear Mr. Galton,

We followed your advice about Bateson's paper, and here is the result. I suppose there will be another book.

We did no good at the Zoological Society on Tuesday. — There was no one who knew anything of Mendel (or of Zoology either for that matter); Bateson spoke for about an hour and three quarters, saying nothing of the way in which he proposed to apply Mendelism to any particular case, but describing results out of the various scientific journals, and saying that these obeyed Mendel's Laws.

At 10.20 we began, and at 10.40

at 10.45 we were stopped by the chairman. Poor Darbishire is demonstrating at Owens College Manchester: he spent Monday night in the train, going back on Tuesday night, in order to attend that meeting. I am very sorry for him!

This sort of thing was said:

Crampe crossed a wild rat with a crop-bred tame albino. He got hybrids of two kinds: grey with black toes, and grey with white toes. This is not Mendelian, to begin with. The grey with white toes, paired together, gave among other things black-and white offspring; and these, paired together, gave 222 black & white, and 6 white descendants. — Bateson, instead of admitting a palpable mosaic, says that one of the components

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of wild rat colour is black and white!

Yours very truly

W F R Weldon

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MERTON LEA,
OXFORD.

5th June 1903

Dear M. Galton,

Many thanks for your kindness in sending me the appreciative Essay, which I greatly enjoyed, and its delightful portrait, which I enjoyed still more.

It is always a joy to us who are your pupils to see how steadily the recognition of your work and of your attitude towards the fundamental problems is being recognised and valued.

I am afraid I tired you horribly in showing you mice and things; I hope you soon get better in Town?

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One has to keep silence about all the things which interest one here, until one has no mercy on any one who will let one talk.

At least I shall try to keep silence before Bateson's criticisms, from this time forth. Controversies of this kind certainly do not seem to help any one else, and they certainly prevent me from even trying to do more useful work.

Yours very truly

W F R Weldon

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MERTON LEA,
OXFORD.

23rd October 1903.

Dear Mr. Galton,

Please let me tell you a rather long story, and then tell me what you decide to do.

Since Turner has been on the Council of the Royal Society, he has often talked to me about biological questions on which he has had to vote, so that I do not feel a difficulty in sometimes talking to him about the affairs of the R.S.

When I saw him for the first time this term, I spoke about the resolution which had been communicated to Pearson, to the effect that the mixture of mathematical methods and biological statements in Pearson's papers was undesirable. This resolution seemed to me an official statement that certain methods of scientific enquiry were undesirable; and

such a statement could be justly made only by a Congregatio de propaganda Fide, not by a scientific society. Turner quite agreed with me, but he had no recollection of any resolution which could bear this interpretation, and thought there was some mistake.

Last night I dined with the Royal Society Club (where I was sorry not to see you!) and Turner brought up this point, when I was talking to Larmor. We got hold of Love (who drew the resolution, as chairman of the mathematical committee) and we all talked a great deal.

I think both Larmor and Love felt that the resolution had been a mistake, expressing far more than they had intended to say, and that it should not have

been communicated to Pearson.

The real difficulty these men feel arises from their knowledge that if Pearson's papers were ~~communicated to~~ referred to the Zoological Committee, they would all be rejected. Lankester, Bateson, and others, would certainly reject them all, as they recommended the rejection of the paper on homoiosis.

I know the attitude of these men, from the way in which Lankester talks when he comes to Oxford, the way in which he tells ~~me~~ my pupils that they are wasting their lives under my ~~false~~ Guidance, and so on: but the attitude of a group of individuals is a thing one can face easily; the official dictum of a big ~~scientific~~ Scientific Society is another matter. I think both Larmor and Love saw this.

Turner and I came back by the same train, and talked yet more. Turner felt sure that Council would willingly make amends for its mistake, if a

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way could be found. — He is anxious to help, but he thought Council would listen with more respect to an independent statement by a senior man; of course we both thought at once of you.

I promised to write to you, and to ask whether you would be willing to obtain from Pearson the actual text of the resolution communicated to him, and to write a letter to Council (next meeting Nov. 5, after which Turner retires.)

It seems to me that the R.S. has made a mistake, which will lead to a regrettable row, unless something of this kind is done soon. — You can do it better than any one else, and I hope you will; but of course I shall do nothing by not writing this letter to you, until I know whether you think my suggestion impertinent.

Yours very truly

W. F. R. Weldon

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MERTON LEA,
OXFORD.

25th October 1903

Dear Mr. Galton,

I hope your "paper ailment" has really passed away by this time?

It is very good news that you are willing to write to the President and Council about this resolution. — I have written to Pearson, as you suggested, asking him at once to send you the exact text of the statement which was made to them, and no doubt you will hear from him tomorrow.

Mrs. Pearson tells my wife this morning that you are good enough to think seriously of coming to see the snails round Palermo. It will be a very great pleasure to us if you do, and I hope your arrangements

f400v

will allers it happen! — Of course, I am not free to choose my time. I give my last lecture here on Friday, December 11, and my first lecture next term on January 25. — We shall leave ~~the~~ Victoria on Saturday 12 December, and spend every available moment in Palermo.

Is it possible that you may be there during at least some of that time?

I shall send you a parcel of shells tomorrow morning, telling you frankly that I am trying to set a snare about your feet; because I think, if you look at the series a little, you will want to see more of it, and I want very much to show it to you. Yours very truly

W. H. R. Weldon

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UNIVERSITY COLLEGE LONDON
GALTON PAPERS

(1)

MERTON LEA,
OXFORD.

8th November 1903

My dear Mr. Galton,

Many thanks for your letter, and for the shells, which arrived last night after all.

I am very glad you are so well, but you should remember the advantage of preventing rather than curing a bronchial attack? Think of seeing your shadow bright blue on a white road once more!

It is certainly not proved that the young of snails from one of the Sicilian hills, transplanted to another, would not change their shape; but it is, I think, unlikely. I think they would die. Snails are among the most difficult animals to transplant from one locality to another, and most careful descriptions of local

F403v

faunas contain accounts of colonies which after being accidentally established disappear.

The permanence of the boundaries which limit the distribution of most snails, even in countries where accidental transport with agricultural produce must often occur, seems to show that they have (as a group) very small power of adaptation to new surroundings. — Consider Helix pisana, whose distribution I think I can give: it runs all round the Mediterranean, through Spain, up the W. coast of France, and touches Cornwall, S. Wales, & Ireland.

In Cornwall, Montagu recorded it more than a century ago as coming about as far E. as Falmouth. It has not got to Plymouth yet.

In the same way, the colonies of the "Roman

snail are very permanently ~~[illegible]~~ limited. ~~It has~~ A colony has been known at Stonesfield for years; but I cannot find a record of a snail caught in Oxford.

These species, and many others like them, are so common where they do occur that they are clearly in a state of rapid reproduction, and the overcrowding which leads to dispersal of most animals must occur: if they do not slowly enlarge the area inhabited by them, the reason must, I think, be that they cannot either survive outside their present area in their present form, or modify themselves sufficiently quickly to meet new conditions?

There are a few cases of snails which can live under a great variety of conditions, but these do not, as a rule, differ remarkably when introduced into a new locality, — or if they do, it is very difficult to correlate the variations with any thing in the environmental conditions.

F4045

I think one of the species which most strongly supports your view of one or two dominant groups of important factors which matter, to the practical exclusion of others, is Helix arbustorum. — I send you a few examples of this species. It lives in rank, wet grass, or in on low herbage in wet hedges. It has colour-varieties, but in shape it is fairly constant, only getting smaller and more conical on high ground. It is on all the pasture lands and low shrubby thickets of W. Europe, from the Arctic circle to the Alps. I send it to you I from Eremsmühlen in Holstein (sea level); II from Risborough (Oxfordshire); III from Bainbridge (Yorkshire); IV from near Andermatt; V from Zell im Zillerthal (1900 feet).

The little pale shell from Andermatt is nearly the "Helix alpicola" of some authors, which seems close to the snow line on Mont Cenis and other alps, — and

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Nov 8/03

MERTON LEA,
OXFORD.

has also been recorded from the banks of the River Lea!

2

About "acquired characters." The thing I want to do more than any thing else is to bring some more Porto Santo rabbits to England. They are the best case on which to base the evidence you want; but two years ago in Madeira I found it impossible to get any alive.

You do not like reading German, but will you look at the enclosed book? I have only turned over the pages myself, but I think it is a summary of a lot of evidence which seems true very important.

It seems true that every character

is at once inherited and acquired. The distinction between the two seems experimentally hopeless: and it seems that at every stage in its development an organism requires not only a certain organic structure, derived from its parents, but a certain ~~stimulus~~ definite environmental stimulus, which shall quicken its structural "mechanisms" into activity.

Dyer will tell you more than I can of the work on Lichens. You know that a Lichen is a symbiotic union between a green alga and a colourless fungus, and that so long as the union endures the alga is peculiarly modified, and reproduces only asexually. You perhaps know also that these green algae, isolated after

long periods of existence in lichen colonies, can be cultivated as pure organisms which quickly assume the characters of existing species of pure green algae?

Here is an environmental effect which has existed for generations, and disappears within the limits of a cultural experiment.

I think a still stronger set of facts is the set I have alluded to in Beiträge Vol. 1 p. 367. Those characters, to which most importance has been attributed as purely "inherited" characters, are undoubtedly the purely embryonic characters, on which "phylogenetic histories" are so freely constructed by Haeckel, Ray Lankester, & Co. — Now the tendency of a whole set of researches, summarised by Herbst in the enclosed pamphlet and badly illustrated by me in Beiträge,

long periods of existence in lichen colonies, can be cultivated as pure organisms which quickly assume the characters of existing species of pure green algae?

Here is an environmental effect which has existed for generations, and disappears within the limits of a cultural experiment.

I think a still stronger set of facts is the set I have alluded to in Biometrika Vol. 1 p. 367. Those characters, to which most importance has been attributed as purely "inherited" characters, are undoubtedly the purely embryonic characters, on which "phylogenetic histories" are so freely constructed by Haeckel, Ray Lankester, & Co. — Now the tendency of a whole set of researches, summarised by Herbst in the enclosed pamphlet and badly illustrated by me in Biometrika,

seems to be shown that every "normal" early mic character, characteristic not of an individual but of a whole class of animals, can be experimentally suppressed by removing a certain definite environmental stimulus at a certain definite time.

~~Nothing~~ If that view of early egg should prove true, — and it looks truer every day, — a prophecy once made in lecture at Cambridge might be verified, and by removal of all the appropriate stimuli a hen's egg might be made to develope into a mass of undifferentiated cells.

Much will be learnt when the work now being done on the development of eggs which have never been fertilised, but only excited to division by "dead" chemical stimuli, has got a little further.

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GALTON PAPERS

[8.11.03] F407r

(3)

Nov 1903

MERTON LEA,
OXFORD.

Will you be good enough to stop me if this sort of thing bores you too much?

I am so glad to be allowed to talk of these things, and you will not do as most men do, and tell me "it is not Zoology" — but one must not weary your kindness!

Yours ever truly

W F R Weldon

Between Dec. 13 and Jan. 25 the mud in Pall Mall will get blacker. The fields round Palermo will get

more and more delightfully purple with irises. That is an environmental stimulus good enough to take a creature like me Southwards.

?

. ~ ~ ~ ! . ~ ~ ~ .

f409r

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

MERTON LEA,
OXFORD.

13th November 1903

My dear Mr. Galton,

Hurrah! On the day you cross the Channel I will gather a big bunch of irises and give them to the Madonna della Grazie.

The only two hotels in Palermo you will care to consider are, I think, the Igea and the della Palma. We stayed last year at the della Palma, and found it very comfortable: it is clean, and has been properly engineered by some civilised northerner, so that one does not get poisoned. The people are all civil, and the cooking is quite good. — I only know the Igea from the outside. It is rather remote from most things. — You probably know the Provost of Oriel? — He often stays at the della Palma, I believe, and could report.

If you have fine weather after your

acclimatisation, you may care to have further railway journey and go down as we do from Genoa. — Every Monday at 12 a Florio big boat leaves for Naples and New York. She takes most of her passengers & her mails at Naples, so one gets excellent cabins, and the saloon nearly empty, with the officers to talk to. One gets to Naples about 2 the next day, and can stay on board till it is time to go in a little boat straight on board the Palermo steamer. — If the "Liguria" should be leaving Genoa at about your time, I know you would be happy in her. I do not know any other boat of the line personally, but they are all made comfortable for the people who have the sense to prefer a slow boat rather than a modern 20 knot affair which is under water all the time.

F410r

I am so glad you liked Herbst. — He and another heterodox friend of mine Hans Driesch run a journal as wicked in its way as Biometrika. They call it Archiv für Entwicklungsmechanik, which is not so pretty as Biometrika?

I have a drawer full of first chapters of books, none of which will probably ever get further. It seems time to useless to summarise other men's work: one ought to go through the whole thing: and the moment one tries to verify any statement so many doubtful points become obvious. — The only useful way is to formulate the old stuff in terms of a new generalisation: and as you say, one wants a new Darwin for that!

Yours very truly

W F R Weldon

I sincerely sympathise with your
feeling at the loss of Brodrick. Of course
I saw very little of him, but I saw enough
to understand what his friendship might
mean to those who really knew him.

I think sometimes the loss of a senior
man means as much to younger men as
to his contemporaries. One rarely knows
the elder man so well, but he is part of
the ideal one makes of what a man should
be, and the world is poorer without him.

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GALTON PAPERS

f411

MERTON LEA,
OXFORD.

24 November 1903

My dear Mr. Galton,

You like puzzles. I have an abominable puzzle, which neither Pearson nor I seem likely to get on with.

Do you know the generations of Salpa? Salpa floats in the sea: the fertilised egg gives rise to an a-sexual individual, which buds a chain of sexual individuals. These are hermaphrodite, but are crosp-fertilised. I set a pupil, who was lately at Naples, to work out the correlation between a character or characters in the a-sexual form, and the corresponding character or characters in the budded hermaphrodites. This he has done. Each hermaphrodite carries a single egg, which develops in its body; so we can get (and this man

has got the data for determining) the correlation between a sexual parent and its asexual young.

The only data yet worked out are correlations between the a-sexual individual, produced from an egg, and the young which is budo.

To my horror, all the correlations are coming out sensibly = 0.

And this suggests a point.

Pearson's way of demonstrating the Law of Ancestral Inheritance (R.S. Proc. 62 pp. 388-389) depends in large measure on a determination of the relation between p_{sn}, the correlation between two mid-parents (s - n)

generations apart, and r_{sn}, the correlation between individuals and their $(s-n)$ th. ancestors.

He shows, by a process depending only on the number of individuals in the mid-parent of each generation, that

$$p_{sn} = 2^{\frac{1}{2}(n-s)} r_{sn}$$

Now if you interpolate, between every two sexual generations, a budding generation, and if you start with a sexual generation as 0, you have in the mid parents

in Generation 1 — 1 asexual individual
2 — 2 sexual
3 — 2 asexual
4 — 4 sexual
5 — 4 asexual

In generation $(2s)$ you have 2^s sexual
in generation $(2s+1)$ you have 2^s a-sexual individuals per midparent.

Now, using exactly Pearson's way of finding the correlation between mid-parents at known distance apart, but giving the proper numbers to each mid-parent, I find

$$r_{2s,2n} = 2^{\frac{1}{2}(n-s)}\, r_{2s.2n}$$

that is, the relation, on which Pearson's proof of the Law depends, holds for generations $2(n-s)$ apart, instead of for generations $(n-s)$ apart, as in ordinary cases. — That may mean that the parental-filial correlation (ordinary cases) exists between successive sexual generations, the intermediate generations ~~being~~ (asexual) having no effect; and that would be a statistical justification of Huxley's old statement that an individual

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Nov 24 1903

MERTON LEA,
OXFORD.

is everything which is produced between one fertilised ovum and the next, whether the various things lead separate existences or not, — the a-sexual Salpa and the several hundred pre sexual buds it produces being all organs of one "individual."

You doubtless know the war between Herbert Spencer and Huxley on this subject better than I.

But the thing seems to me to have a bearing on your way of stating the Law, — as a series of separate contributions from each particular ancestor.

If you would tell me your view of what ought to happen in such a case

I might understand, more clearly than I have yet been able to do, why you make the sum of your series = 1.

=

There is no necessary contradiction, so far as I see, between this result & my own's and Warren's results with Daphnia. — Warren measured correlation between two successive parthenogenetic generations, not between a parthenogenetic generation and its sexual offspring.

Yours very truly

W. E. Castle

F415r

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14th February 1904

MERTON LEA,
OXFORD.

My dear Mr. Galton,

I trust that asthma, and "days of piggishness," and all such things, have finally disappeared by this time? — Also, I am ashamed to have left you without news of your letter, which appeared in "Nature" of the 28th. — I am sending it to you.

It does one good to think of you sitting at the foot of Etna in the sunshine. Is Miss Biggs going up? I believe it is harder than the Monte Cuccio! — Our poor little Shotover puts on a solemn cloud-cap now, and plays at being a mountain, but we have no snow, — only a "record" flood, killing lambs by hundreds and preventing the farmers from sowing wheat in the one year when war is sure to make it fetch a decent price.

The Daily Mail will have told you how last Tuesday we carried the first step in the legislation which will make Greek optional

for every one, and so abolish the boy who gets nothing at school but "an ignorance of two languages and a distaste for all": we won by 2 votes, and I felt proud; because on Monday morning I went to Newcastle, lectured to the Philosophical Society in the evening, and spent the night in the train, in order to vote.

At Newcastle I dined with an old friend of yours, Spence Watson, who spoke much of you.

Please scold me roundly, when you have time: for the careless applause of 300 or 400 folk pleased me, so that the class lecture next day, to half a dozen apathetic undergraduates, who may be made to do something if I behave myself, seemed dull!

Will you, or Miss Bieg's, or both of you, collect some data?

Ancoles (Archivio per l'Antropologia I) has written of Sicilian albinos. — Twenty pairs of pigmented parents had 133 children, of

whom 49 were albino, — by no means a good Mendelian quarter! The only two albino parents known to have married had 5 children, 3 albino + 2 pigmented: so that these parents were not "pure recessives" as they should have been!

Miss Biggs showed me lots of albinos, and of very fair children, round Palermo: and the only children of an albino I know are very fair, though with some pigment.

If such children occur round you now, could you find out whether the brothers of albinos are fairer than other children, or than their pigmented parents? Fair children are so rare in Sicily that a very few families ought to settle the point; and of course such a correlation between fairness of one brother and albinism of another would point to a non-Mendelian "blend".

I much want your views on another point. The R. S. has put me on a committee to consider the form and other features of their publications. They want a Royal 8vo. "Proceedings" (the their chief

organ, — but they want to divide it into A and B, as the Phil. Trans. is divided, so that there will be no journal left in England in which knowledge is dealt with as a whole. I protest as much as I can, urging that the R. S. should above all encourage just those papers which transcend the categories of the narrow index-making specialist, but I fear I am almost alone on the Committee.

You have much better things to think of than these: but if I write of Sicily, the poor little solitary bullfinch on the bare apple tree outside will not seem an adequate herald of Spring any more!

I hope your Spring is already with you, so that there is no risk of any more bronchial attacks. — And if even after your removal from "piggis hueg" you should find "black worms" in your bed-rooms, please consider that they are only harmless little millipeds, and put one or two in a match box for me? They are not snails, but they are very nice.

Yours very truly

W. T. R. Weldon

Ans! Mard 10.

Weldon

f417r

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

MERTON LEA,
OXFORD.

5th. March 1904.

Dear Mr. Galton,

We wanted your encouragement yesterday in Cambridge, when optional Greek was rejected by 1559 to 1052!

But you could not have saved the day, and I hope you are warmer where you are. — You just escaped a bitter week of East wind, which withered all the opening buds and made the land look far more wintry than it did three weeks ago.

In the intervals of shivering, I have been gathering up the evidence about characters, latent in races which apparently breed true, but capable of reappearing and becoming "dominant" when such races are crossed with others. — Such characters may segregate in Mendel's way, and they form a strong body of proof that Mendelian segregation is not due to "gametic purity", but to some process such as that I tried to write about before you left.

Also, the mice are giving clear evidence

that "hybrids" in the later generations can produce less than their original proportion of "recessives": Thus the hybrid grand-children of original hybrids, when paired, give waltzers in the proportion of 13 to 78 normal; and no one can say that $\frac{13}{91} = \frac{1}{4}$?

In the same generation, the black eyed forms which ought to give $\frac{1}{4}$ albinos give $\frac{74}{342}$, whereas the hybrids of the first generation gave $\frac{137}{555}$, a very nearly $\frac{1}{4}$.

This kind of thing, with some work about regeneration and reproduction in unicellular animals, and a fuller account than you have yet seen of the very few eggs in which localisation of function seems perfectly defined, makes up all the stuff I shall collect before trying to put those University College lectures into form for printing. — There seems to be no evidence at all that the localisation of

function in an egg, or in the early embryonic cells, leading to loss of the power to regenerate a whole individual from a part of the embryo, is in any way correlated with loss of power to regenerate lost parts in the adult.

I am cramming all these things into notebooks, and in a fortnight we shall get into some sort of sunshine too, I hope. — We shall go to Ferrara, where one ought to be able to work quietly all the morning, and to see things in the afternoons.

One of the things I want to see is the way in which the lagoons at Comacchio have been developed into nurseries of sea fish; the young fish, floating at the surface of the sea, are caught, and placed in the lagoons, where they become private property; and I believe the way in which they are dealt with during their subsequent growth is very ingenious and interesting. — The system of tenure of such property should also be interesting!

I wonder if there is any chance that you will drift Northwards towards Ravenna or Venice or so at Easter? — If you did such a thing, and if you cared for a few days at Ferrara on your way, it would be a great pleasure

to us, and if you cared to read my manuscript with sharp criticism it would be a very great help to me!

We leave here about the 20th., and term begins on Easter Monday: so I expect we shall leave Ravenna in the neighbourhood of Good Friday.

Wherever you go, I hope the sun will shine on you and the Mistral keep far from you.

Yours very truly

W F R Weldon

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

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W F R Weldon

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ans'd March 12

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GALTON PAPERS

f419r

Merton Lea, Oxford — 6 March 1904

Dear Mr. Galton,

I have tried hard to find some fault in your proof, but however captious I try to be I cannot find any alteration to suggest.

Have you ever got up the "germ-cell theory" of cancer? — It seems to me to have a very important bearing on all theories of inheritance.

It appears that cancer is due to the presence of cells of a peculiar kind, which can be transmitted alive from one body to another, and so spread the disease. There is no evidence of any other organism, unless the views of the very orthodox people at the Institut Pasteur should prove justified, and "ultra-microscopic bacteria" should be shown to exist!

But these large cells of a cancer have been shown to go through a nuclear division, analogous to that gone through by a developing germ-cell, resulting in ~~the~~ a reduction of their nuclear chromatic elements to one half the normal number. They have then been seen to conjugate, and of course, like the great majority of other cells, they divide.

Now a cancer cell gives rise to very remarkable structures, but it never gives rise to a normal embryo: therefore I see no escape from one or other of two beliefs: either these cells are not germ-cells at all, in which case all the peculiar nuclear divisions, regarded by so many people as an essential part of an hereditary "mechanism" have no necessary connection with inheritance at all, — or, as many embryologists believe, a cancer cell is originally a displaced embryonic germ-cell; in that case a germ cell, developing under the extraordinary conditions of environment associated with its presence in a wrong part of the body, is deprived of the ~~external~~ extrinsic stimuli necessary for its development into a normal embryo, and divides without differentiating. In that case a cancer is the practical realisation of an experiment I have told you I ~~often~~ hoped to make, in which an egg, by removal of environmental stimuli, is induced to grow and divide without differentiation.

Either way, the thing seems full of interest?

To my great joy, the R.S. Publications Committee has been induced to recommend that the new and enlarged Roy. Soc. Proc. shall not be divided into A and B. — I feel jubilant, but Larmor wants division,

and Council has been known to prefer the opinion of its officers to that of its Committees?

I think, after my wrangle with Bateson, I see a little light on Mendelism; and it is so obvious, when once it occurs to one, that I think it is true.

Of course, the segregation among the descendants of the hybrids comes out of any theory of reversion among gametes. The difficulty has been dominance in the hybrids immediately resulting from the cross. I think dominance can be shown to follow from the assumptions we make, — that r_{sn} has a constant (average) value for all individuals of a species, — quite easily.

The "gametic purity" folk always get into trouble over the "occasional reversal of dominance". — You know, it may be "reversed, suppressed, or irregular" in special cases, and every such case has a special set of Greek words made to "explain it."

After the visit with Bateson, I spent two days at Reading seeing the Primulas which Bateson had discussed. They are grown by Sutton, and wonderfully beautiful they are!

Sutton have an "albino" race, — with no pigment except chlorophyll, which breeds true, and is "recessive" to coloured races. But most of their "white" primulas are, as they recognise, not really white, but faintly flushed with pink or lilac. These do not breed true; when self-fertilised, they give red, lilac and impure white again, — perhaps other colours. So that the ordinary white primula of trade is a selection from among a variable race, and does not breed true. Now this kind of white primula is dominant.

Here is a clear case, paralleled in mice, in Stocks, in one or two other things, in which a "recessive" character changes its "recessive" nature in a way apparently correlated with change in ancestry.

Therefore I have tried to work out the correlations between the mid-ancestors of a hybrid, and those between hybrids themselves and their mid-ancestors.

Call σ_h, σ_k the standard deviations of the two pure races, and r_{hsn}, r_{ksn} the correlation within each race between an individual and its individual $n-s$ th. ancestor.

then the infallible Pearson has not yet contradicted my result that ρ_{sn}, the correlation between an sth. and an nth. mid parent, is

$$\rho_{sn} = \frac{2^{\frac{1}{2}(n-s)}}{\sigma_h^2 + \sigma_k^2}\left(\sigma_h^2 r_{snh} + \sigma_k^2 r_{hsn}\right)$$

(and $r_{hsn} = r_{ksn}$)

which becomes, if you make $\sigma_h = \sigma_k$, or the 2 races identical, Pearson's

$$\rho_{sn} = 2^{\frac{1}{2}(n-s)} r_{sn}$$

The correlation between a hybrid and its sth. mid-ancestor is in the same way

$$\rho_s = \frac{2^{\frac{1}{2}(s-1)}}{\sqrt{\sigma_h^2 + \sigma_k^2}}\left(\sigma_h r_{hs} + \sigma_k r_{ks}\right)$$

Now if you suppose one of the races more variable than the other, you see that the less variable race contributes less to the mid ancestral correlation, till if you suppose $\sigma_h = 0$, the hybrids are only correlated with the variable race.

Of course, so far, one has said nothing about the <u>mean</u> character of the hybrids. But a great number of recessive characters have a mean and a variability $= 0$; such are albinism in animals; absence of hairs on leaves of plants; absence of spines on Datura-fruit, etc.

In all these cases, the recessive character is of zero mean value, and of zero standard deviation. — This is said. Of course it is not literally true in most cases, but at least A.M. and S.D. are always very small.

Well, now, you take the offspring obtained by crossing a mouse of zero pigmentation, ~~and~~ of race with zero variability, with a mouse of recessive pigment, of race varying in pigmentation.

Since the formulæ shows that the hybrids resulting must be correlated in pigment characters with the variable race, they must be variably pigmented; therefore at least their mean character cannot be pigment $= 0$. I think that is a clear indication that the pigmented condition must be dominant over

the albino condition, when a variable race of pigmented mice is crossed with pure albinos.

When, however, the pigmented race is only slightly variable, and the albinos used are those occasionally found among any crop-bred race of mice, the hybrids may be more strongly correlated with the race to which the albino belongs, because the S.D. of that ancestry may be the greater, — until finally, if one paired such crop-bred albinos with very closely pure-bred mice of one colour, albinism might become dominant.

Darbishire has got as far as piebald hybrids from crop-bred albinos. — Bateson has plenty of cases in which the hybrid has only the character usually "recessive", — this sort of irregularity does exist, and I think in a number of cases it can be clearly correlated with increased ancestral variability in the normally "recessive" form.

If one can once show that dominance depends on the relation between the variability of the 2 pure species, then almost any law of reversion among gametes will bring Mendelian proportions into the subsequent generations.

I expect you will pick holes in this doctrine freely, if you can withdraw your attention from Syracuse, or Naples, or whatever place this may find you in! Else, you will not care to bother with Mendel any more, perhaps. — Please peruse it all. I am rather excited about it. —

We ought to be preparing a good Spring for you, if proverbs are to be trusted: for we are having our full share of East winds, with no sun, and drifting snow flakes filling the air about four days a week.

Our hope of taking Peppard Manor house has rather collapsed: but the Pearsons have taken rooms somewhere near Peppard, and I expect we shall go to their old parson. — We cannot get our cottage, and we shall cheerfully risk phthisis from the patient whose presence frightened Pearson. We shall go about March 25, for three weeks. — I wonder if there is any chance that you and Mrs Bryce will be back, and will care to receive your memories of East Summer, before we scatter? —

Yours very truly

W F R Weldon

UNIVERSITY COLLEGE LONDON GALTON PAPERS

[4215

This abominable paper means it is Sunday night & I have no more stamps. — Please forgive it.

Norton Lea, Oxford March 20 1904

Dear Mr. Ealton, — My first impulse on reading your letter was, to go to Birmingham by the next train, and commit murder on the body of Howard Collins. — I don't believe he will improve your schedule!

Next, I want to apologise for my hurried post card about the fish. — I did not see your note on the paper containing the bones, until after I had written. — There are many fish, of the same family as the Lampish, with green bones; there are a few sharks, too, and I think perhaps the thing you call a Lamprey may have been a Muraena? — The conger-like fish to which one threw one's worn-out slaves in the palmy days of Rome? — So far as I can find out, no one knows what the colour is, no one can guess at its function.

No. Practically no experiments have been made on the transplantation of ~~ovules~~ unfertilised ova. — The manipulative difficulties are, of course, tremendous, and I think in most cases the ova would simply be eaten by the wandering cells of the new body.

I hope Lipari has fulfilled all your hopes?

Beautiful in many ways it must have been, and I am tempted to envy you greatly. — I hope it was not too dirty? — I shall hope to hear from you some day your impressions of the way in which the "domiciliis coatta" system works in such a place, — whether it makes the whole neighbourhood a sort of hell, as its enemies say, or not. — I have only seen it in Ischia, where mild people are, — and even there I do not know much about it. — Capital

f421v

punishment always seems to me a thing infinitely more just, as well as more merciful and cheaper. Surely society has a right to kill a man whose aims are at variance with those of his fellows, but surely it is bad policy, at least, to torment such a man while you tax honest folk to pay for his keep?

My one Italian pupil here tells me that certainly ~~Notobartolo~~ Palizzoli will be acquitted this time (the fourth trial?) — and that as certainly his first act afterwards will be to murder Notobartolo's son, — a harmless naval officer who has nothing to do with the trial!

I am so glad you think the little story about dominant and recessive characters may possibly have some sense in it. — Here is another Mendelian story, which also has some sense in it. Calling W a coloured, waltzing mouse, with pink eyes, and A an albino, Darbishire has made mice with the following pedigrees (H = (~~[illegible]~~ a coloured mouse with black eyes)

1.— $\underbrace{\underbrace{\underbrace{W.A}_{H}\quad\underbrace{W.A}_{H}}_{H}\quad\underbrace{\underbrace{W.A}_{H}\quad\underbrace{W.A}_{H}}_{H}}_{\text{Young.}}$

Now of course, in the light of Mendelian gospel there is nothing in the statement that half the young are like one or other parent, — a quarter like one or other grand-parent, and so on. — That statement, as Bateson has told you, merely shows

your ignorance of Mendel's work: but, if that statement were true, in the progeny of such parents there should be $\frac{4}{64}$ of the same like the 4 albino great grandparents, and another $\frac{4}{64}$ like the albino ancestry of those g.g. parents.

Now Darlishia gets $\frac{10}{93}$ albinos: and $\frac{1}{8} \times 93 = 11.6$.

2.— W.A W.A W.A A A
H H H A
H H
H
Same

This pedigree should I think give on your hypothesis $\frac{5}{16}\frac{7}{32}$

albino same.— Darlishia gets $\frac{20}{107}$ and

$\frac{7}{32}\frac{5}{16} \times 107 = 23.3$!

3.— W.A A.A W.A. A.A.
H A H A
H H
H
Same

This should, I think, give $\frac{5}{16}$ albinos.

Darlishia gets $\frac{30}{121}$, and $\frac{5}{16} \times 121 = 37.7$.

The reason why this is not so close as the other is that the albinos used were ~~probably~~ not all pure-bred: Their back ancestry is therefore wrongly summed. — You notice your series gives always too many albinos, — exactly the error due to neglecting their (unknown) ancestral impurity!

Pearson keeps throwing these things at me, but it seems true that your reverse ordinary series ought to be applied, not to the fertilised ovum, or Embryo, or Gamete, — but, to the reproductive cells of each individual. otherwise, I don't see how to explain results of this form:— One knows that the cross W×A always gives a black-eyed mouse, with coloured coat. Now the offspring of pedigree

~~W A~~ W. W. A ~~A~~ A A A A A
W A A A
H A
Gamete

UNIVERSITY COLLEGE LONDON GALTON PAPERS

are half black-eyed with coloured coats, and half albinos with pink eyes (Albinos observed $\frac{368}{746}$)

Now your series ought, I think, to give $\frac{1}{4}$ black eyed like the H parent: $\frac{5}{8}$ pink eyed albinos; $\frac{1}{8}$ like W. — $\frac{1}{8}\times 746$ is a big number, and no such mice occurred. But if you suppose the reproductive cells of H to revert, you have these half like W and half like A; and since these are paired with albino gametes, the observed result follows.

I shall have, after this week, 3 weeks of wrangle in a friendly way with Pearson over this point at Peppard, varied by rushes to the museum on a bicycle to see that the mice do not die while we talk over them. — After, I hope you will let me tell you the result, — and that you will tell me about domicile coats, and lots of other more interesting things.

Yours very truly
W F R Weldon

F423r

Blount's Court Farm, Kidmore, Reading
2 April 1904.

Dear Mr. Galton, — Welcome home! — I hope your journey will not have tired you much!

As to the mice, indeed I have not neglected the H and W; I only tried to show you how well your series fitted, in the briefest possible manner, by confining your attention (not mine!) to one character.

I have thought of the reversions in this way:

Case I. — $\frac{\frac{W.A}{H}\quad\frac{W.A}{H}}{H}\qquad\frac{\frac{W.A}{H}\quad\frac{W.A}{H}}{H}$

observed result = 10 A + 71 H + 12 W. = 93.

I thought your series should give (supposing A and W pure bred)

$A = \frac{8}{64} = \frac{1}{8}$; $H = \frac{48}{64} = \frac{3}{4}$; $W = \frac{8}{64} = \frac{1}{8}$.

You have so, $\frac{1}{8} \times 93 = 11.6$ against observed 10 A or 12 W.

$\frac{3}{4} \times 93 = 69.8$ against observed 72 = H.

Surely an excellent agreement?

Case II. $\frac{\frac{W.A}{H}\quad\frac{W.A}{H}}{H}\qquad\frac{\frac{W.A}{H}\quad\frac{A.A}{A}}{H}$

observed result 20 A + 73 H + 14 W = 107

I thought one should expect

$A = \frac{8}{64} + \frac{6}{64} = \frac{7}{32}$; $H = \frac{1}{4} + \frac{3}{16} + \frac{1}{4} = \frac{11}{16}$; $W = \frac{6}{64}$

or $A = \frac{7}{32} \times 107 = 23.4$ against observed 20

$H = \frac{11}{16} \times 107 = 73.6$ against observed 73

$W = \frac{6}{64} \times 107 = 10.0$ against observed 14.

Of course, some pink-eyed mice with coloured coats (= W) — in the remote ancestry of the albinos I used, might well account for the discrepancy between the observed & calculated W and A results, although they are not so bad, as they stand?

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Case III W. A A. A W. A A. A
H A H A
H H

Observed Result = 30 A + 72 H + 19 W = 121.

I expected $A = \frac{2}{16} + \frac{12}{64} = \frac{20}{64} = \frac{5}{16}$; $H = \frac{1}{2} + \frac{2}{16} = \frac{5}{8}$; $W = \frac{4}{64} = \frac{1}{16}$

You have therefore $A = \frac{5}{16} \times 121 = 37.8$ against observed 30

$H = \frac{5}{8} \times 121 = 75.6$ against observed 72

$W = \frac{1}{16} \times 121 = 7.6$ against observed 19.

Case IV (which you have not seen !)

W. W. A. A. A. W. A. W
W A H H
H A

Observed result = 24 A + 20 H + 4 W. = 48

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As before, $A = \frac{1}{4} + \frac{1}{16} + \frac{8}{64} = \frac{28}{64} = \frac{7}{16} \times 48 = 21$ against observed 24.

$H = \frac{1}{4} + \frac{2}{16} = \frac{3}{8} \times 48 = 18$ against observed 20

$W = \frac{1}{16} + \frac{8}{64} = \frac{3}{16} \times 48 = 9$ against observed 4.

All these cases seem true to your series as well as the numbers available would lead one to expect, — and of course they utterly smash Mendel; for I, II and III should give, on his view, $\frac{1}{4}$ A + $\frac{1}{2}$ H + $\frac{1}{4}$ W.

Case IV should give on his view $\frac{1}{2}$ A + $\frac{1}{2}$ H, with no W.

Now Case IV leads me to the two sets of phenomena which I cannot see how to bring into accord with your series.

(1) When you pair W (pink-eyed mice with coloured coat) and A (albino) you get mice of variable coat-colour, but with black eyes (340 such young obtained !)

If you treat this black eye as a reversion at all, or as "inherited" in any ordinary sense, it must, from what is known of the pedigrees of the

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mice used, be inherited from ancestors always more remote than grand-parents, to the total exclusion of heritage from grandparents or parents, so far as eye-colour is concerned. — One must consider the possibility of reversion being a function of the difference between individuals mated?

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(2) With pedigree

W W A A A A A A

W A A A

H A

Observed result $368\,A + 378\,H = 746. = 50\%\,A + 50\%\,H.$

Now here I cannot see anything but gametic reversion as a possible theory.

Your series seems to me to predict

$$A = \frac{1}{2} + \frac{1}{16} + \frac{4}{64} = \frac{10}{16} = \frac{5}{8}\ ;\quad H = \frac{1}{4} = \frac{2}{8}\ ;\quad W = \frac{1}{8}.$$

You would have $A = \frac{5}{8} \times 746 = 466{\cdot}25$ against observed 368

$H = \frac{1}{4} \times 746 = 186{\cdot}50$ against observed 378

$W = \frac{1}{8} \times 746 = 93{\cdot}25$ against observed 0

Now if you suppose that each of the parents, H and A, produces generative cells ~~such that they are each generative cell would, when paired with its like, produce a body like that of some ancestor, and suppose your (or any other) geometric series to represent the~~ each conveying especially the properties of some one ancestor; and suppose the proportions of the different sorts of generative cells in each generative organ to be represented by the terms of your series. Then, assuming purity of breed in the individuals used, half the generative cells of H will convey the properties of W ancestors in an active condition, half will convey those of A ancestors. The generative cells of A will all convey properties of A ancestors, because there are none other.

Therefore, when the two individuals are paired, half the gametes of the individual H will give rise to creatures such as result from pairing a W and an A, — that is, as we know from direct experiment, to individuals of character H; the remaining half will give rise to individuals of character A×A, — that is, to albinos.

This will be true, whatever the series which represents the relative

frequency of the different sorts of gametes may in each parent may be, provided the sum of that series = 1. — So that one can play with series, till one finds ~~one a value~~ a set of values such that one can apply a similar gametic hypothesis to cases I. II. III. & IV.

I have some fair fits, but they are not worth showing to you yet.

(3) But there is a further abomination.

(a) There is no trustworthy record that two albino mice, when paired, produce other than albino young, whatever their ancestry. This is what all breeders tell us, — it is of course what Mendel says, — and it agrees with our own laboratory experience, now becoming fairly large.

(b) If you pair two pink-eyed mice, with coloured coats, you always obtain ~~ob~~ pink-eyed young, whatever the ancestry of the parents used

(c) If you pair a pink-eyed waltzing mouse, with coloured coat, and a pink-eyed albino, you always get some black-eyed young; and unless the ~~one~~ albino have waltzing ancestors similar in character to its mate, you never get pink-eyed young.

(d) So far, black-eyed mice, produced by crossing ~~—~~ W and A, and then paired, have not been found to produce a stable black-eyed race, in 3 generations.

So that except under (the) special conditions (c) pink-eyes, when present in both parents, breed true at once, and black-eyes, under the special conditions (d) have not been found to do so.

This seems true again to show that any hypothesis, whether of gametic or of somatic reversion, must treat the ~~the~~ part played by the various ancestors as a function of the difference between the individuals paired, or even in terms of the characters paired.

You notice of course that a "pink-eyed" mouse, showing only the colour of blood through its eye, has zero amount of eye-pigment, though I cannot see the bearing of this remark upon anything in particular!

Yours very truly

W. F. R. Weldon

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15th. August 1904.

MERTON LEA,
OXFORD.

Dear Mr. Galton

The worst of being physically well is that it makes one so stupid. I wanted to say lots of things to you today: but the battle with the good West wind left me so healthily tired that I could only sit like a dog, full of a stupid content.

A letter from Pearson, found on getting back, wakes me up. — I told you, we spent yesterday in seeking for a geometrical series which should express reversion in Cuénot's mice better than Cuénot's own pseudo-mendelian formula. — Pearson has found one, on the assumption that reversion to one parent and its ancestors is not as probable as reversion to the other parent and its ancestors.

Taking for one series (all this parent and its ancestors)

$$\lambda_1 + 2\mu_1 + 4\mu_1\beta + 8\mu_1\beta^2 + \text{etc.}$$

for the other

$$\lambda_2 + 2\mu_2 + 4\mu_2\beta + \text{etc.}$$

where 1 refers to all this parent and

their ancestry, 2 to Yellow parents and their ancestry, Pearson's prediction is

82 Albino
31.5 Yellow
20.5 Black
17 Grey

which is, of course, in excellent accord with the observations.

Now, of course this may be said to be only an illustration of the fact that if you put constants enough into a formula you can fit any given result. But it seems to me more than this. It seems to me the beginning of the real solution of Mendelism, — the exhibition of reversion as a function of the difference between the parents.

If you take two albino mice, — of a ~~shaky~~ caste of with range = 0, so that they

are of zero difference in pigment character, you have no reversion at all. If you cross an albino mouse with any one of several coloured mice, you have what is plausibly a reversion right back to the wild ancestor.

I believe the complete theory of Mendelism is a theory of reversion which will start from these postulates:

(a) The observed correlations between filial and ancestral generations are so and so;

(b) The observed differences between mated parents are such and such;

(c) Given a reversion proportional to the known differences between parents of known ancestry, the law of reversion must be so and so, in order to satisfy (a) and (b).

Only, the algebra of this is too hard for me, and Pearson is naturally too busy with lines he knows to be fruitful to stray aside after my vain imaginings.

My ground for my belief is this. — Taking Darbishire's results, we see that by pairing

a certain piebalded (pink-eyed) mouse with an albino we get wild-coloured offspring, with black eyes. The offspring of such by birds are of various colours. I am gradually getting from the descendants of these, true-breeding mice of any assigned coat & eye-colour, from black with black eyes (which ought, in Mendel's view to be impossible) in either direction. Now, my proceß is the ordinary selective proceß of making the 2 parents of every generation as much like what I want to produce, — and therefore as much like each other, — as possible.

I think you cannot look at Bateson's results, so far as you can disentangle what he really saw from the DD x DR business, without feeling that reversion is a function of parental character. — Once get that for everything, and you have a theory of parental correlation, which involves a theory of filial mean character as well, and therefore solves everything.

Yours very truly W F R Weldon

UNIVERSITY COLLEGE LONDON
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MERTON LEA,
OXFORD.

24th August 1904.

Dear Mr. Galton,

Many thanks. I shall look forward to meeting you tomorrow at Burford, if the weather is not too bad, or I coming over on Friday if it should rain tomorrow.

I have always urged the importance of describing the "pure" races more in details. In the "pure" races themselves there is great variation, and again in the "extracted" pures: until we have a description of individuals, sufficiently close to enable us to know whether an extracted form is "exactly" like its parent or its great-grand parent, I do not see how the Mendel question can be adequately discussed at all.

The combs Bateson showed on Sunday made this very clear. A "rose" comb is a thing like a dish of peas: if you look at the fowl's head from above, you see a sort of rim round the edge of the comb, and the centre is full

of little rounded knobs: so that it looks like

this. — Now, Bateson has only 200 full grown fowls, altogether: most of his work is done by breaking open ~~eggs~~ half-incubated eggs and looking at the chicks, so there is little material for studying the variability of any one "pure" type. But the 2 or 3 individuals he showed differed obviously in the number of these tubercles, and in the shape of the "rim" of the comb. In the same way, the few "extracted" rose-combs he showed us differed.

Now his pupil Punnett, speaking on Friday, said that the pure parental rose-comb was indistinguishable from the extracted pure rose-comb. I asked how many tubercles

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there were in each, but he and Bateson simply sneered, saying they left such unimportant statistical details to us.

Yet, say the pure bred mid-parent had 10 tubercles, the pure-bred mid grandparent 20, and the extracted "pure" form 15, what becomes of gametic purity?

The whole simplicity of Mendelism depends on finding a category so wide that all these minor differences, which can alone decide between your reversionary series and Mendel, are concealed. The moment you get closer descriptions, you find strange stories of the odd behaviour of extracted forms, such as those in the report to the Evolution Committee you have just read.

I am more and more certain that a theory of particulate reversion, such as you indicated in Natural Inheritance, will do the whole thing. I think you want to work out such a theory for units of various sizes, — so that the limit on one side would be reversion by particles so small that the result is a blend, on the other by units so big that

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Large pieces of the individual are affected, as in your Bassett Hounds.

Is not this the theory you have had in your mind always? Is it not the real answer to all the difficulties about fixing the mean of successive generations from correlation tables?

I had a long talk to Pearson about it yesterday: but of course there is the old difficulty. He is too busy to do the algebra, and I know I shall only make a mess of it, if I try.

Such a theory, admitting that the chance of reversion to ancestors of given degree of remoteness is a function of the difference between the parents, would exhibit all forms of inheritance, including Darwin's reversion as result of crossing, which is now called Dominance, as one phenomenon?

Yours very truly

W F R Weldon

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9th October 1904

MERTON LEA,
OXFORD.

Dear Mr. Galton,

I am ashamed to have kept you so long without the figures necessary for my little MS. — Here are a few. I will send some more tomorrow. — I think you can probably follow the whole thing perfectly well, as I could write it, without the diagrams; but I ought not to give you so much trouble.

The next section will begin with the recent work on artificial parthenogenesis, which will, I think, give you a little pleasing excitement. There seems to be no possibility of doubting any more that at least perfect larvae of sea-urchins, starfishes, and several worms can be reared from unfertilised eggs. The behaviour of their nuclei is interesting. Delage, of Paris, says that the number of chromosomes is <u>normal</u>, — that is, double the number in the mature egg nucleus, — after parthenogenetic divisions. So that the doubling of the somatic chromosomes <u>need</u> not be a result of biparental

action. — In a similar way, lots of people have shaken the nucleus out of a Sea-urchin's egg, and have fertilised the protoplasm only with a male cell, getting normal Larvae. — In this case also, of course, there is only one nucleus to start with, having half the ~~num~~ number of somatic chromosomes. — Belage says that this nucleus also doubles its number in subsequent divisions.

The Germans deny this doubling.

The question therefore arises, how many chromosomes are there in a somatic cell of a Sea-urchin? For the same species, the great German authority Boveri and his pupils find 18, 27, 29, 36.

Belage says 18.

This is the sort of result you get when you try to follow the evidence for constancy of chromosomes anywhere. — It is not nearly so good as

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the evidence for Mendel's quarter!

Yours very truly

W F R Weldon

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Oxford — 10th. October 1904.

Dear Mr. Galton,

Very many thanks for your kindness. I am most grateful to you for spending so much time and trouble over my little effort, and for showing me how to make it a little better. Of course, I see the justice of nearly everything you say: but here are some points I should like to speak about.

1. — Your main criticism is perfectly sound, but I cannot help myself. — I see the interest of working at those eggs with intermediate forms of "segregation"; but no one has succeeded in doing so, except in the one or two cases I mentioned. I referred to every animal in which the eggs have been studied from this point of view; the eggs have been chosen simply because work of the kind required seemed experimentally possible, — not from any preference for one result rather than the other, — and the striking thing is that in so many groups of even highly organised animals (Frog, Fish!) the segregation is practically absent. — If one considers the large number of Mammalian double monsters which are associated with malformations of the amnion, such as would probably constrict the young embryo as Spemann constricted a Newt's embryo with a hair, it seems probable that the embryos of Mammals are as little "segregated" in their early stages as the rest. But one cannot get at them to show this directly, so I thought it better to leave them out of consideration. — So far as I can see, the evidence is that segregation may be completely absent in every group ~~of~~ the animals; but it is not time yet to say so?

2. — I wish you would make a definition of "inherited" and "acquired" characters. Weismann says an inherited (blastogenic) character originates solely in the primary constituents of the germ, while an acquired (somatogenic) character is not preformed in the germ, but arises only through special influences affecting the body or special parts of it. Both these definitions, taken literally, are nonsense. Any character can only develope under certain external conditions; but so far as the definition of inherited character means anything, it seems true to mean what I said — that given a few essential conditions, necessary to all life, an inherited character is one which ~~was~~ developes in a definitely predetermined manner, and does not change in response to any environmental stimulus. If you have another definition of inherited characters, I shall be grateful; but as I understand Weismann, such characters as he calls "inherited" have not been shown to exist.

~~I~~ It is absurd to say that an acquired character arises only through influences affecting the body or special parts of it, because the body affected helps to determine the character, as much as the "influences" acting on it. — The same environment affects different animals in different ways. (the term)

I think you cannot restrict ~~the~~ "acquired characters" to those due to

influences which affect the body in post-embryonic life only; and I venture to differ from you in thinking that this restriction is not "conceivably" made. You must have discussed the colour of yellow canaries as an "acquired" character apparently inherited? You know that yellow canaries are fed on Cayenne pepper, and they absorb the colouring matter in a great number of their tissues. One result is, that their feathers "acquire" a deeper orange-yellow colour, which fanciers like. This is post embryonic "acquisition". But the ovary of a hen canary, fed in this way, contains the colour of the pepper, and the coloured substance enters the yolk of her eggs, so that the young embryo is fed upon it. Such an egg may give rise to a bird, whose tissues are charged with the colouring matter of the pepper: and until the young bird has excreted all this colouring matter (which takes a very long time) its feathers are also abnormally deep in colours. Surely this is "acquisition" of a character during embryonic life?

3.— Will you look at Spencer's "Principles of Biology" Vol. I. p. 80?

4.— In one place, you write "pure panegenesis" in the margin; I gladly agree, and I do not feel crushed in the least.

Apart from any question of localising determinants in nucleus or in protoplasm, the weak point of Weismann's attempt to establish a fundamental difference between germ-cells and somatic cells, by reference to embryology, is that in the great majority of cases you cannot distinguish the two till a very late period in development, and that when you trace the origin of a germ-cell, you find it arising by division of a previously existing cell,— one of the two daughter cells being a germ-cell, the other a somatic cell. It is not suggested that the division, which results in this separation, is in any visible way different from the division of ordinary somatic cells.

Now, if you admit, as one must admit, that every cell of an adult body has many properties which normally remain latent throughout its life, but can be rendered active by suitable means,— if you find, as you do find, that in a great many animals you can make any part of the body regenerate any other,— it seems to me legitimate to ask whether any cell of an embryo or perhaps of an adult body, is not capable, under suitable conditions, of regenerating the whole; and whether a germ-cell is not simply any cell which happens to occupy such a position in the embryo, and to be nourished in such a way, that a few of its properties are rendered latent.

That is a sort of panegenesis which de Vries advocated some years ago

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for plants, because he saw in the simpler case of plants what has only lately been shown for animals, — the equivalence of every cell of the Embryo (~~each~~) every other.

The first test of such a view is the possibility of regenerating generative organs after they have been formed and removed. Every one knows that you can produce such a multiplication of generative organs in a flowering plant, by cutting off the flowers as fast as they form, as is required by the view suggested: & there is very little evidence about animals, where the conditions of experiment are so much more complicated, but it is very nearly certain — in Hydroids, and in Planarians.

Curiously, with your letter came a letter from a pupil in Naples, giving promise of better evidence still. The man is the most brilliant person I have ever taught: Dohrn thinks very highly of him, and therefore his statements are worth attention.

The crab Inachus scorpio is normally male or female; no hermaphrodite crabs, or immediate allies of crabs, are known. The sexes are clearly distinguished by a lot of external characters. Inachus is subject to the attack of a parasitic Crustacean Sacculina, and it has been known for some years that individuals attacked by this parasite are externally intermediate between normal males and normal females, while they are sexually impotent, so long as the parasite remains.

To this, my man Smith says he has added two things: (1) while the parasite is on the crab, the atrophy of the generative gland is complete; and (2) when ~~the~~ the parasite dies and drops away, the Inachus may be kept alive; and in such cases it develops a new hermaphrodite generative gland.

If Smith is right, you have a case of regeneration of gonads from somatic cells, such as ought to be quite impossible on any theory of a fundamental difference between the two kinds of cells. Weismann recognises this as a crucial test of his hypothesis. — Smith knows the importance of what he says as well as I do, and he is working in a Laboratory full of competent anatomists who discuss their results together: so I think he is probably right. — Apart from the more important question whether a few undiscovered germ-cells may have survived and served as a basis from which the new gonad regenerates, the result that a male ~~[illegible]~~ or female can be changed into a hermaphrodite by ~~[illegible]~~ environmental action is interesting, — and he cannot possibly make a mistake about that. Any freshman could be sure if he saw such a thing.

It seems to me that such a form of "panmixis" (as I have suggested) avoids all the difficulties you have raised, and at the same time avoids the contradiction between the results of

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experimentally ~~do~~ induced changes in the position of blastomeres, and those which ought to follow on the view that some of these blastomeres contain "germ plasm" while others do not.

I want to regard every cell of an Embryo as something like your "stirp", and to regard the whole process of development as something which renders latent different qualities in the "stirps" of different parts of the body, but does not permanently ~~suppress~~ obliterate any of them. — ~~That a progressive loss of functional [illegible] can be demonstrated in~~ Such a view brings the whole set of phenomena, from fertilisation onwards, under one category, at least.

Fertilisation, a union between two "stirps", leads at once to latency of some characters, and every subsequent ~~[illegible]~~ process leads to the latency of others in some of the Embryonic cells.

Forgive this long letter.

Yours very truly

W F R Weldon

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GALTON PAPERS

Walton about Bidder F433r

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MERTON LEA,
OXFORD.

10th November 1904.

Dear Mr. Galton,

I meant to have thanked you days ago for the first part of your "Index" which you were kind enough to send me. — If you want a full history of a "sport" which has persisted fairly well, you may, I think, get one from G. P. Bidder, Cavendish Corner, Cambridge. He is grandson of the "calculating boy," son of the barrister. His mother is a sister (or a near relative) of Hackman the engineer, who has just died. — I know of five fairly near relatives of his who would get his marks from you. I do not think he wants many marks himself, but he has two brothers, and cousins, all about my age, so that the generation can be judged.

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I do not often feel cause for self-congratulation, but every number of Nature makes me gladder that I am not Bryan!

Yours very truly

W. F. R. Weldon

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GALTON PAPERS

MERTON LEA,
OXFORD.

25th. November 1904.

Dear Mr. Galton,

I have talked to Schuster, but although he is keenly interested, he mistrusts his own ability to do what you would expect him to do if you chose him. — He is reading all your Eugenic statements carefully, and thinking about them: I hope in a day or two he will write to you and ask you to let him call on you.

The memory of all your kindness to us keeps us warm through even this hard frost; but I fear you do not like such bitter cold?

We had quite a series of tiny adventures when we left you yesterday. — Our horse was frightened by the icy road, and refused to stir, so we had to change cabs in the middle of the Park. We fell upon the guard of the 9.50 just as he was waving his flag to start the train, and he

let us in. We thought human drum Oxford could not provide an excitement, but no sooner were we in our homeward cab than down went the horse, grazing his knees and breaking a shaft, so that we had to change again.

Yours very truly

W. H. Wheldon

Tuesday lunch 1437

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GALTON PAPERS

MERTON LEA,
OXFORD.

27th November 1904.

Dear Mr. Galton,

It will be either so cold, or so foggy on Tuesday, that I ought not to hope to see you at University College, where I lecture in a room hardly fit to support human life at the best of times. But I should very much like to see you, and to say a word or two about Schuster. May I call on you about lunch time on Tuesday?

Yours very truly

W. F. R. Weldon

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MERTON LEA,
OXFORD.

15th January 1905.

Dear Mr. Galton.

I am very glad you have chosen Schuster, — for your own sake, because I think he will not disappoint you, and for Oxford's sake, because your choice will do much to encourage the very few others who care to follow the right path.

I am trying to write a tremendous letter to you. Have you time (or inclination!) to hear some more about Mendel before you go South into the sunshine and out of this awful wind? — If you regard dominance as a phenomenon of the same kind as that by which a crab is prevented from growing hundreds of legs, or a worm from growing hundreds of heads, — if you once

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admit the reality of the influence of adjacent parts in inhibiting & each some of the properties of the other, you have at once to make a theory of the manner of putting several sorts of "strips" into the same germ. — And when you consider the equivalence of different sorts of strips, you have to consider how the activity or latency of the properties of each is determined, and how it is influenced (a) by the presence of strips of other kinds, (b) by the nature of the germ from which it is immediately descended.

Taking these two things into account, I believe you can make a working theory of Mendelism, without any "pure gametes"

at all, — a theory which is directly derived from your statements about a "strip".

Only, I know you are very busy, and I will not send you long pages to read unless you tell me I may.

Yours very truly

W F R Weldon

UNIVERSITY COLLEGE LONDON
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"Stella d'Oro" – Ferrara – 25th. March 1905.

Dear Mr. Galton, – We are both delighted that Bordighera is doing you so much good, and that you can stay in your pleasant quarters a little longer. This is a very different country. There is an austerity about it which fits the old town, but it is hardly beautiful. Of course we have no palms, no oranges, no olives, nor any myrtle-covered hills. Far beyond the horizon in every direction is a huge plain of brown mud, with long lines of pollarded willows set beside great dikes full of yellow water. There are no flowers, – hardly a stray daisy – and the only trace of green comes from the thin lines of young wheat here and there among the endless ploughed fields. The town itself is going to be interesting enough. The castle is the most perfect mediaeval fortress I have ever seen in Italy, and the cathedral is magnificent; but one day's experience, complicated by a bad cold and a total absence of any fire places, does not fit one to speak of these things.

I want to tell you of the mice Miss Bigg is kind enough to keep me to get drawn. I have always said that the wild-coloured hybrids produced by crossing yellow-and-white waltzers and ordinary albinos were reversionary forms, and therefore subversive of the doctrine of gametic purity. I think now I have proved this. I have bred as far as the fifth generation of descendants from the hybrids. To start with, I had individuals of the 2nd. generation, and the first thing I did was to select in order to get the different colours to breed true. There was a great difference in the behaviour of the different colours. Yellow (= "fawn" of Darbishire's paper) – always associated with pink eyes, – breed true at once, or sometimes give a small percentage of "lilac" young, also pink-eyed. Lilac (also always pink-eyed) also breed practically true at once. Mice of these colours never give any dark-eyed young, and hardly ever give any albinos (the record is 1 albino in 525 young from such parents).

Black mice, on the other hand, give black, chocolate (with dark eyes), lilac (with pink eyes) and albinos. In the fifth generation I have at last got a fair percentage which seem to breed true, but I am not yet sure of this. Now an albino from such black stock, with its last 6 or even its last 14 ancestors all black, paired with a pink eyed yellow mouse with 6 or 14 pink-eyed yellow-coated ancestors, gives young of the following kinds: – pink-eyed yellows; dark-eyed black; dark-eyed wild coloured, and albino. I cannot see any

Mendelian theory which will describe this result. For, if the wild colour be called recessive to black, so that the black ancestors of the albino may have carried it latent in the Mendelian way, it ought sometimes to appear among the offspring of such black parents themselves: but even black mice directly descended from wild-coloured hybrids did not give any wild-coloured young in the Darbishire's trials, and of generations 3 - 5 I have 961 young, no one of which showed this colour. I think its re-appearance, when you cross such yellow mice and such albinos as I have described, can only be the ~~result~~ result of a definite atavism, produced by a definite cross, just as Bateson and Tschermak show that certain races of white Stocks when crossed give purple-flowered offspring like the wild form. Do you remember Cuénot's ingenious hypothesis to explain the wild-coloured, black-eyed hybrids on "Mendelian principles"? He says that the power of exhibiting any colour in the coat depends on a determinant C, which is dominant over an allelomorph A. This A prevents the appearance of pigment in the skin or eyes: so that an animal of genuine constitution A A is albino: an animal of constitution A C or C C is coloured, but the colour it exhibits depends on special determinants. Of these, Cuénot recognises Y = yellow, with no black pigment and therefore with pink eyes; G = grey = wild coloured, with black eyes; B = black. Now an albino mouse (from its wild ancestors inherits) the determinant G (dominant over Y) but not the determinant C. It is of ~~constitution~~ constitution A G; the yellow mouse being = C Y.

The hybrid A C G Y will form 4 kinds of gametes, A G + A Y + C G + C Y in equal numbers: and if paired with a similar hybrid will give rise to

A A G G + 2 A A G Y + A A Y Y = 4 albinos

2 A C G G + ~~2 A C G Y~~ 4 A C G Y + ~~2 A C Y Y~~ 2 C C G Y + C C G G = 9 dark eyed wild-coloured

2 A C Y Y + C C Y Y = 3 pink eyed yellows.

You get your Mendelian quarter of albinos; but the rest are as 9:3 instead of as 8:4. If you put in lilac as receptive in the yellow mice used, and consider some of the albinos to have been A B, as they certainly were, you can bring the remaining colours in easily enough.

You see this view gets over the difficulties, involved in the appearance of wild-colour & black eyes among the hybrids, well enough: and since some of the wild-grey hybrids are of constitution CC, and can therefore give rise to no albino young when paired together, it allows for the possibility of Durham's result, that by ~~dom~~ increasing the number of wild-coloured hybrids in the ancestry, you diminish the percentage of albinos produced. But it is like all these Mendelian stories. It accounts for all the phenomena Cuénot knew when he made it, but it does not enable one to predict a step beyond. For instance, since 2 out of every 3 pink-eyed mice with yellow coats ought to be of constitution AC, a large proportion of these mice, paired at random, ought to give albino young: but as I have said, in 525 young from yellow (or lilac) parents, I have only 1 albino!

Again, Cuénot, Davenport, etc., declare experimentally that wild grey is dominant over black: so that my black mice cannot have any wild colour in them. Since they give lilac, chocolate, & albino young, they must be of constitution AC. and have 3 colour-determinants, of which black is dominant. Their albino young will then be of constitution ~~AB~~ A B ~~c~~, or A with lilac or chocolate, — and the possibility of wild-coloured young by pairing these with AY or ACY does not appear!

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It is so hard to find you out in an erroneous statement, that I feel a mean sort of joy when I do it. You say you are not going further South, and yet you are going to Grasse, and ultimately I suppose to Marseilles. It is very hard to realise

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the northward slope of the Gulf of Genoa, but I think your latitude is now 43° 46' while we are in the arctic parallel of 44° 50', and surely Marseilles is not far North of Rome?

I hope the Riviera will go on behaving itself. It can be cold enough if it likes, even in April, but I hope it will give you plenty of sunshine, and all the flowers we have to go without.

Yours very truly

W. F. R. Weldon

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Stella d'Oro, Ferrara – 9th April 1905.

MERTON LEA,

OXFORD.

Dear Mr. Ealton,

All sorts of good wishes from both of us to both of you for your stay in your new house. – It seems a long way from this quiet old town to a world in which there are motor boats and all the gay doings you speak of.

You know, while I share your admiration for Rendel very heartily, I cannot feel sorry for him here. – I think, if one lived here, one would quickly be content to work at interesting problems for one's own joy, apart from any wish to convert other people. – He cared to fight about clerical politics, and made himself heard. Don't you think he was happy in keeping his scientific work to himself?

Our Spring has come at last. We have blackthorn, and pear-blossom, and even

an almond in flower here and there, so we are quite gorgeous. It all came in one night. There was a day of black scirocco, then a wicked dry electric storm, with lots of lightning but no thunder, which made one's hair crackle and one's temper go to pieces, — and then bright sunshine and a big West wind. I bicycled before that wind 35 miles to a Pompoosa, — a place you and Nip Briggs ought to see some day. It is on the edge of the Lagoons, — an XI century abbey, as simple and impressive in its way as a Greek temple, standing quite alone in a plain, as Paestum does, only the plain is infinitely more dreary. The buildings are all of brick, ~~with~~ covered outside with the very beautiful ornament

f4448

of terra cotta and brickwork which one finds all over this country where building stone is hard to get, — and covered inside with early frescoes, childish and crude enough but very charming, the latest going down to Giotto's time. The Campanile used to be a lighthouse, and the place a port, until the sea left it. The view from the top is magnificent, stretching from the white alps North of Venice right down to the purple mountains beyond Bologna.

2

When you have finished the "Measurement of Resemblance", will you care to tell me if the enclosed pages, which are meant as a sort of summary of the first few chapters of the book I am trying to write, are intelligible, and whether they seem to you more or less reasonable?

Do not let me interrupt your work, but

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if you have time and inclination to tear this statement to pieces, I shall be very grateful.

Yours very truly

W. F. R. Weldon.

You have no doubt seen what a sensation Pearson's theory of the strength of ~~Dams~~ Dams has been making lately?

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ans. April 27

Ferrara

19 April 1905

MERTON LEA,
OXFORD.

Dear Mr. Paton,

I am most grateful to you for your kind advice and criticism of the pages I sent you. I wish I could use my mother tongue a little better! It is the finest language in the world if one can use it properly, but it is very hard, — and hearing another language through the window while one writes does not make it easier.

Somewhere in this book there must naturally be an essay on you. I have tried to write it, and I fear I must ask you to be good enough to read this also. — But please do not take any trouble about it till some day when you are at leisure. — I send you only a copy of what I have written, that you may not think I want it back until you are quite ready to give it me. — If you will bring it with you to England,

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I shall get it quite soon enough.

We leave on Saturday, hoping to get North without too much delay. – You are lucky to be so near a frontier, for if the strike bothers you, you can drive to Villa franca and get out of it: but it is a long way from here to Chicago!

This place has been full of interest, which makes up for want of sunshine and for cold winds. – Just as we are going comes a blaze of sunshine, with blue skies and lilacs: but in spite of it all the country is not half so beautiful as Kent will be when we pass through it next week, or as Oxfordshire when we get there.

It has been pleasant to see a part of Italy where no one looks starved. Every

f4465

peasant here is well draped, and the richness of the town is shown by the presence of over 3000 Jews! — It is the comfortable richness of English agricultural county, with no one enormously wealthy, and no one very poor; and the chief crop being wheat makes it look like Cambridgeshire, — though they make very good "b.b.b. butter" too.

If only this pope, who seems to have some sense, could bring the landlords back to public life, so that they cared to stay on their estates and look after their tenants and their crops, the whole country might be rich like this, on agriculture, without trying to compete with manufacturing countries by using coal at a guinea a ton!

I suppose your holiday will soon come to an end too, now. — I hope it has made you quite strong again, and ready for another dose of London? — I am looking forward to hearing your paper on the

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Measurement of Likeness very soon.

Yours very truly

W F R Weldon

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MERTON LEA,

OXFORD.

5th May 1905.

Dear Mr. Galton,

I hope it will soon be time to bid you welcome home again; and it is certainly time to thank you for your kindness to my manuscript. I am very grateful for all your notes, which make the thing far better than it was: but I am especially glad to see what you say about a slight power of variability, at least, in the determinants themselves. — I want to believe in such a power, but I was trying to write what I imagined you believed, — and I had not realised your view.

I shall alter the passage affected by that note with gladness, — and as soon as lectures here are fairly going I shall write another chapter, which I hope you will some day look over, about the point. — If one admits no variability in the determinants, clearly some form of intermittent "sporting" or "mutation" is a logical necessity, and I am delighted to get rid of that! — But if the determinants do vary, I cannot yet see quite

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clearly that the sum of the regression coefficients need necessarily = 1 ?

So far as I can see, as yet, any variability of the determinants in passing from one generation to another ought to diminish the ancestral correlations and regressions: any part of a filial character, due to variation in such elements, would be something not exactly represented in its ancestry at all, so that in such a case the sum of the regression coefficients should be a little less than 1 ?

Variation as likely as not to cancel one another

But Pearson, as you know, declares that the series which best fits his things has a sum quite sensibly greater than 1.

It seems to me that the theoretical need for assuming "sports" or "mutations" as new starting points of varieties depends very largely or entirely on this question of the sum of a series one can never observe far enough to be quite sure of the more remote

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terms, and at present I am in a hopeless fog about it, which is not made clearer by my having to lecture several times a week on the anatomy of insects, about which I care not at all just now.

Ferrara when we left was full of men selling silk-worms' eggs: and the cocoons they showed were so delightfully variable, so beautifully distinguished into races, that I had to bring some eggs home, in the hope of starting another set of observations. Did you realise that ½ Oz. of eggs requires 700 pounds of food before they become moths, and yield only 50 pounds of silk?

We were delighted to see your account of yourself, and of the good you had gained from Bordighera. — Men who had been in your neighbourhood at Easter came back here showing evident traces of sunshine. I showered and cuffed them, and hoped your face was as brown as theirs.

Yours very truly

W. F. R. Weldon

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Do you know that several journals have misunderstood the sarcasm of Pearson's letter about Arthur Thomson, and quoted it as serious praise of Oxford work?

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MERTON LEA,
OXFORD.

12th May 1905.

Dear Mr. Galton,

You seem to have brought the summer with you. This is our first week of sun, and I hope it keeps you warm enough, even after the South.

I am sincerely sorry you feel unable to accept the nomination to the Presidency of the British Association. It would have been a great pleasure to many of us to come and hear you: but no doubt you are wise. These things must be a great strain.

I see your point about the constancy of regression. — but you would terribly diminish the correlation coefficients?

There has been something to do here lately, for once, in bringing the mouse catalogue up to date after my absence. The apparent reversion to wild-grey colour in the offspring of crosses between albinos whose ancestors have been black for four generations and yellow a

Lilac mice whose ancestors have been yellow or lilac for four generations, has been repeated several times. Further, yellow mice, whose ancestors have never produced albino since the segregation from the original hybrids, 5 generations ago, have produced albino young when mated with albinos, - showing, I think, clearly that they have some latent albinism inherited from the original cross which has never "segregated" out as Mendel says it should.

Yours very truly

W. F. R. Weldon

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MERTON LEA,
OXFORD.

6th June 1905.

Dear Mr. Galton,

If you think it possible that you may come into Pearson's neighbourhood for a time during the summer, we shall benefit as well as he; so it is not only for your sake and his, but for ours also, that I send you a little map, and try to praise the Lambourn valley.

The valley is too good for any words of mine, so I will simply say it is like the first two verses of The Lady of Shalott, and is not yet spoilt by the light railway which runs up it. Somewhere between Lambourn and Newbury I am sure there are places to be found in plenty where "four grey walls and four grey towers Overlook a space of flowers"; and if one could find such a place, don't you think it might very well embower the Lady and Gentleman of Rutland Gate?

From Lambourn you can drive to all sorts of nice places. You may go up onto the ridge of the downs, along the Ickneild Way, and look out over the Thames Valley. — Thomas Hardy has tried a dozen

times to describe the great stretch of wooded valley, with Oxford in the distance, which you see from anywhere between the White Horse and Ilsley.

On the other direction, the loop of the Kennet from Hungerford to Marlborough is charming. You probably know Savernake Forest. If you do, you want to see it again. If you do not, surely you want to make its acquaintance?

Kingsclere, near Hungerford, has a big and beautiful almshouse.

For longer excursions, you have the Vale country from Newbury to Bradfield, through Coldash and Bucklebury. — Aldermaston is interesting, and of course you know Silchester, close to it.

All down the Lambourn Valley [illegible] I am sure every other yard is paintable and worth painting. So is Kingsclere.

If I have succeeded in tempting you to

think of it, I shall be only too glad to look up the local house-agents. It would be much easier for me here than for you in Town.

I was much interested in Mr. Moritz last night. As I read his statement that even the lowest Englishmen knew their classical authors so well, I looked round the carriage full of undergraduates, reading the sporting papers and talking cricket; I wondered what he would have said?

Yours very truly

W. F. R. Weldon

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MERTON LEA,
OXFORD.

9th June 1905.

Dear Mr. Galton,

Your delightful invitation only got to me at 9 o'clock this evening. I hope you understood that if I had seen it in time I should have answered and accepted very gladly.

Till I saw your telegram, I thought you had given me the most delightful day possible, because I went to look for a house which might tempt you. I could not find one in the Lambourn Valley, and that valley is only beautiful in places, which I had remembered, separated by miles I had forgotten.

But I found a charming house at Chieveley, 4½ miles from Newbury, 6 miles from Racon, some 20 from Oxford. It is the vicarage. I have not seen the inside, because I was so dirty through much patching of bicycle tyres that I dared not call: but I prowled and pried, and saw that it was good.

It is an 18th. century house, square-pontiled with that curious triangular metope pierced by

a round windows like a cyclops eye. It stands in 12 acres of old world garden and little park, with big roses and trim hot hedges and them great blue-green Elm trees. I could see you revelling in the peace of that garden.

Indeed, if you have not yet chosen or made definite plans, I think this place worth considering. The agents say, 12 bedrooms, 3 reception rooms and a big hall: stabling and so on. — Rent 10 guineas a week.

The agents are Draweatt and Watson, of Newbury. — The man is the Rev. R. A. Faussett, Chieveley, Berks.

I do not want to be impertinent, but if you care to consider the place, I will gladly go over again and see if inside, or if Miss Biggs cared to bicycle from Newbury some day, I should be very glad to pilot her. — She could do the whole thing with less than 10 miles of riding.

I am rather much engaged till next Friday, but after that I am free practically any day

F4545

up to and including the 28th.

Chieveley itself is a little village nestling in a wooded hollow on the downs, and a short drive gives you the great spacious views you knows in country of that sort. — From the big ridge this morning, I saw the Chilterns, then Oxford, with Warwickshire hills showing dimly behind it, then Gloucestershire all blue behind Farringdon, and the Swindon country away to my left: and between me and all this lay the broad fertile Thames valley, studded with rounded trees.

I hope my journey, which has cost me so great a pleasure, may lead to what I know would give you pleasure through the summer, as it would make Parson and me glad.

Yours very truly

W. F. Stoddon

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Pad dep 9.40 Newbur arr 10.50

Newb: dep 2.30 Pad arr 4.25

Nil 3.10 4.50

4.10 5.50

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MERTON LEA,
OXFORD.

16th June 1905.

Dear Mr. Galton,

In case you have not seen the enclosed advertisement, – or the thing it refers to, – I send it on to you. Please do not trouble to return it.

It has set me wondering why you do not work with transparent photographs and projection lanterns?

If you had three small lanterns, each lighted by a fairly feeble incandescent lamp, say of 20 candle power, and provided with an iris diaphragm, I think it would be easy to obtain three fields, each of sensibly equal illumination, and these being superimposed on a screen, you could place your three photographs in the lanterns, and observe your standard signs, and the differences between the two images whose similarity is to be measured.

While you did this, the room in which you worked would of course be dark: but it would be provided with a number of electric lamps, each of small candle-power, and each capable

of being turned on independently of the rest.
Having compared your images in a dark room, you could turn these lights on one by one, until the differences between the images, and at the same time some of your standard signs, disappeared.

I think that if you made six sets of observations with your first three photographs, distributing them in the lanterns in every possible way, you could "calibrate" your lanterns once for all.

The arrangements for lighting the room would want a little care, and of course the lighting power available should be so great as to render almost the whole lantern image indistinguishable: but with small lamps in the lanterns I think this would not be very hard?

Such a plan suggests all sorts of nice things to try. You arrange your photographs, and light the room gradually until certain differences disappear. You know the illumination roughly in terms of unit electric lamps.

Now you light the room brilliantly, and turn light

after light out, until the differences reappear.

What will be the relation between the 'illuminations' just after re-appearance in this case and before dis-appearance in the previous case.

Again, differences having just disappeared, you go and sit in the dark for half an hour, ~~before~~ and see if they can be perceived on your return.

I have no doubt you have thought of this, and rejected it for reasons I am too stupid to see: but I venture to suggest it, in case you may have overlooked the possibility.

Yours very truly

W. F. R. Weldon

You probably know that you can get chloride of silver lantern plates, which are printed slowly like slow silver paper, and toned with chloride of gold. I think with these it would be as easy to get prints of equal intensity as in the case of paper prints.

W

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MERTON LEA,

OXFORD.

26th July 1905.

Dear Mr. Galton,

Will you help me to solve a puzzle? It arose in this wise. I am in charge of certain skulls, including those of some supposed Saints. Pearson was over there with me the other day, and said in joke that you would like to know the mean skull of an English Saint.

After, the vicar of Blewbury, near Dorchester, showed Pearson a barrow, in which he said were the bones of "Berenius", who "first baptised an Englishman". And Pearson and I plotted privily how we might despoil that barrow.

But in the meanwhile, I have read a lot of chroniclers, - Bede, William of Malmesbury, the anonymous monks of Winchester, Gloucester, Abingdon, and other places, with a few more folk. These all agree that S. Birinus was sent to Britain by S. Gregory the Great: that he baptised Kyneisilo, King of the West Saxons, in Dorchester: that he was made Bishop of Dorchester, and that with Kyneisilo's help he built a church

in Dorchester, where he was decently buried, as a Bishop should be. Then, when the Bishopric was merged in that of Winchester, he was taken to the Church there, built by King Cnut's son: and William of Malmesbury says that "next to God, Birinus is Patron of the royal city of Winchester."

Bits of Birinus were everywhere in the 11th. century. Abingdon had a rib: Gloucester (I think) had another bone: but Winchester had most of him, because among the last things Knut did was to give the monks of Winchester a "feretrum" to carry his relics about in, and a big silver-gilt candlestick to carry in front of him.

Clearly, he was not buried in a barrow at all. Clearly, in the 11th. century, he was believed (by everyone except possibly the monks of Dorchester) to be in Winchester.

But the Vicar of Blewbury relies, as he says, on "extant local tradition"; — and that is my puzzle. Clearly the Vicar is ignorant

beyond the limits of the pardonable; but that only makes it more likely that he is quoting the local tradition as it was told him. – Can you conceive that this is really a genuine persistence of a local memory for 1300 years? The barrow is said to be known as the "Hill of Berenius" even now!

I am the more interested because one of my skulles came from a ~~[illegible]~~ barrow at Goodmanham, where S. Paulinus, first Bishop of York, is said by Bede to have destroyed a heathen temple. Canon Greenwell, who gave this skull to Oxford in Rolleston's time, has labelled it, in quotation marks, "Paulinus the Missionary."

Can you suggest a way of getting at the real origin of such tales, as they exist now?

Near here is a church, whose pre-reformation name was the Church of the Assumption. It has the Assumption carved all over it. The dean of Ch: Ch: told me that he was once in the village on the day of the feast of the Assumption, and found the people celebrating a great festival "for the day of the village Duck"; – no man

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knowing why! In less than 400 years, this tradition was forgotten, though an effect of it has persisted. Is it conceivable that men like Birinus are really remembered by the peasantry to this day?

Yours very truly

W. F. R. Weldon

If I have said any thing which seems to detract from the merits of S. Augustin, I apologise!

I cannot find a printed chronicle of Dorchester: if an M.S. exists, it is probably here, but I have not found any one who knows it, and I have too much reverence for old manuscripts to lay profane hands on them myself!

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MERTON LEA,

OXFORD.

27 August 1905.

Dear Mr. Galton,

Many thanks for the schedules. I will try to make good use of these, and to ask for others later on.

I hope Ockham is all you expected. It is a glorious bit of country. I remember bicycling about it many years ago on one of the old big-wheeled machines. I have spent unconscious hours on many of the roads near you, after tumbles.

Pearson has a far better toy than mine against next Wednesday, — a "telephoto" lens of 30 feet focal length, giving a sun 2 inches across. — Since he is pleased to think I can develope photographic plates, I have had to try. Between us the other day we saw three sun-spots on two successive photographs, which is a great joy to us, because Abney says there are reasons why it is impossible to see sunspots at all, even in enlargements, unless the original image is at least 4 inches across. — Beck, who does not believe Abney, made Pearson's lens, and will probably give it to University College if we can show him clearly that it

photographs sunspots.

Today I went out to Foley for a drill. We got out the whole thing, and focused a grey cloud. Then it rained, and we covered the thing up with waterproof sheets. Then it rained worse, and we pretended we did not care about eclipses. Then it rained worse, and I had 18 miles of it on a bicycle to get home.

What is our duty in this case? — Under a sheep fold, 6 inches below the ground, a human skeleton has been found. The local doctor, a surviving relic of the Bob Sawyer type, says the bones are those of an old man who died "hundreds of years" ago. On this statement, the police and the coroner decline to take any steps. The parson does nothing, and the bones lie in a workman's shed. — There are many pieces of skull, half a pelvis, and one unbroken femur, besides lots of other bones. — Pearson and I, independently, agree that these are the bones of a young woman

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photographs sunspots.

Today I went over to Haley for a drill. We got out the whole thing, and focused a grey cloud. Then it rained, and we covered the thing up with waterproof sheets. Then it rained worse, and we pretended we did not care about eclipses. Then it rained worse, and I had 18 miles of it on a bicycle to get home.

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who has not been buried so very very long.

I enclose the best I can do with one of the negatives you were kind enough to let me make. Please forgive me for caricaturing you in this way. - You know enough about the lower forms of man to know that respect and affection show themselves in strange ways: - look upon this as one of them, and pardon it.

Yours very truly

W. F. R. Weldon

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MERTON LEA,
OXFORD.

12 September 1905.

Dear Mr. Galton,

My little holiday ended as the moon rose: but before I begin to use the energy it has given me for the further revision of Chapter II, I want to thank you for the kind deep which gave me the happiest part of these few days.

And, as people always do, when they are grateful, I want to ask another favour. If, when the booklet is finished, you do not think it altogether too feeble, and if I can find a decent line of English for once, will you let me dedicate it to you? — I will not say you wrote it, because that would be too great a slander: but if there is any good in it, you inspired it, and I should like to be allowed to say so.

Yours very truly

W. F. R. Weldon

f463r

Merton Lea, Oxford. — 8th. October 1905.

Dear Mr. Galton, — Bateson's experiments are full of interest, but I am sorry I cannot see that one of his inferences has yet been justified. The very first result he obtains seems true to overthrow the whole hypothesis of gametic purity, and with it the value of his gametic formulae. He takes two white-flowered races, each of which breeds true under normal conditions, and finds that when these are crossed the flower-colour of the hybrids is what he himself describes as a reversion to that of the common ancestor of both races (Reports to R.S. II pp. 84 etc.). Among the offspring of such hybrids, a second reversionary form, the red, belonging to ancestors probably less remote than the purple, appears (cf. Report II, C.C.). This seems to show that two forms of ancestral flower-colour character have been transmitted through many generations in a latent condition, and only excited to dominance by the exceptional circumstances connected with the crossing. It is exactly a case of Darwinian reversion.

Tschermak, who was one of the first, if not quite the first to follow Mendel, has described a number of similar cases in which ataristic characters become dominant when two true-breeding races are crossed, though neither race normally exhibits the character in question. He points out that such facts make it necessary to revise the whole hypothesis on which Mendelian ratios are founded, although the ratios themselves may still express certain cases.

You say, the prediction of visible characters is a great achievement, and Bateson has accomplished this in a wonderful way. To this I cannot help answering two things, however ungracious they may sound. First of all, Bateson's numbers, taken as a whole, seem to me capable of another interpretation; Secondly, they are admirable enough as they are, but they are in no sense a prediction: on the other hand, the results were unexpected, and have only been brought within the scope of Mendelian formulae at all by adopting from Cuénot a view of the relation between ~~chalcone~~ white and coloured flowers inconsistent with that adopted by Mendel and approved by Bateson himself in the case of white and red "Green Peas"; by Tschermak in many cases, by Bateson in Snapdragons and Datura, and by ~~others~~ de Vries. My great difficulty is precisely that nearly every fresh case involves a new hypothesis as to the gametic constitution which represents any visible character, and as to the relation between similar groups of visible characters.

Now, as to the numbers. The larger experiment quoted in the Reports to the R.S. II pp. 88-89 agrees so well with that described in the

Letter to you that I copy the numbers obtained in each, with those indicated for each by Bateson's present theory:

	The Letter.		The Report II. pp. 88-89.	
	Observed	Theory	Observed	Theory of Letter
Purple + Picotee	27	27	306	274
Red + Tinged White	9	9	76	92
White	28	28	269	285
Long pollen	3072	3072	500	487
Round pollen	1024	1024	149	162

[Please believe I mean only what I say, when I point out the colossal good luck in the agreement between theory of a random mating and observed result in the Letter. He deserved good luck, and indeed he got it!]

These two experiments agree so well, we may add them together.

In pollen, we have: parents (F_1) long. Remoter ancestry, $\frac{1}{2}$ long $\frac{1}{2}$ round, so far as we know. Offspring, (F_2) $\frac{1}{2}$ long like parent, $\frac{1}{4}$ long like one line of ancestry, $\frac{1}{4}$ round like the other. Here is the old story; the result is not crucial between you and Mendel.

In flower colours, we have a greater complication, because we have not sufficient evidence yet given. But in the experiment described in the Report, it is pointed out that the white races used [redacted] produce purple or red "rogues" (p. 81), which are neglected in the account (p. 96): I think this may be of importance, for the following reason:

The purple F_1 parents produced in F_2

	Totals of both lots.		Bateson's Theory		Your Series	
Purple	333		301		357·5	= parents
Red	85	382	101	414	357·5	= remoter ancestors
White	297		313			
	715		715			

Now take your series: you have $\frac{1}{2}$ like the parental generation, purple: $\frac{715}{2} = 357$ or 358, and the Prob: Error of the experiment is $0.67\sqrt{\frac{715}{4}} = 9$ say. The observed deviation is 24 or 25, or less than 3 times the Prob: Error. In the more careful or more fortunate experiment of the Letter, $\frac{64}{2} = 32$, and the Prob: Error = 2·7. $32 - 27 = 5$, or a difference of less than twice the Prob: Error.

Therefore the proportion of purples is not inconsistent with your series, either in the 2nd. experiment, or in the whole result. In the first experiment, your proportion of purples = 325 is better than Bateson's theory gives, so that on the total of both experiments, your value is also better.

F4645

Now for the reds. It is admitted that these are atavistic, and that they represent nearer ancestors than the purples. But the time when the race-character was red-flowered must be so remote that your series would not give any sensible number of reds: but when we see that pure seed set in the open 1 or 2 per cent. of red or purple "rogues" normally occurs (Report p. 96) and that at least some such appearance of "rogues" occurred in some of Bateson's original round-seeded stock (Report p. 81), it seems quite possible that even in carefully-selected stock there may be a near red ancestor of an apparently pure white parent.

That there is something irregular about these white "pure" peas is shown in the Report p. 85, where two exceptions are given to the uniformity of F_1. The white form Emily Henderson has long and round pollen: ~~a Long~~ long pollened forms of this crossed with round pollened forms gave in F_1

29 purple plants from 9 crosses
1 red plant from 1 cross.

Then the long pollened form Blanche Burpee, crossed with round pollened Emily Henderson (called "red" instead of "round" by an obvious misprint in the table, as shown by the heading of the table, as well as by the fact that Emily ~~Hendero~~ Henderson is white) gave

3 Purple plants from 2 crosses
14 Red ——— 3 ———

Again, it is distinctly stated on p. 84 that a white round × white long "sometimes gives purple, sometimes P.L." (P.L. = Painted Lady = the red of the latter: cf. Report p. 84, 2nd. and 3rd. paragraphs, and p. 85 top paragraph.)

When we are told in the Report *first* that 15 plants of F_1 out of 47 were red instead of purple, and *secondly* that the whole account neglects red or purple rogues which may amount to 1 or 2 per cent. of any generation, unless the flowers be carefully covered, as was no doubt done in the second experiment; & it seems to me there is abundant room for the odd red ancestor who would bring all these numbers into good accord with your series.

Therefore it seems to me, with regard to my first point, the value of the numbers themselves, one must conclude that the regularity in F_1 has not been shown to hold, ~~and~~ and that there is a very considerable ~~margin~~ margin of uncertainty about the pedigree of the whites used. This would not be of importance to Bateson in 1901 or 1902, when he so eagerly embraced the doctrine that ancestry did not matter: but I think it is sufficiently important to make

any *prima facie* case against the application of your series to these numbers out of the question.

If you will let me do so, I will presently write at even greater length about the hypotheses involved in Bateson's beautifully capricious numbers: but I dare not trouble you with more than this tonight.

Yours very truly

W F R Weldon

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14651

Merton Lea, Oxford — 9th. October, 1905.

Dear Mr. Galton, — The test of Bateson's hypothesis of whitenaps, adapted from mice to Sweet Peas, will come in the generation F_3, which he has not yet decided to grow. I have tried to predict the results of cropping his white-flowered F_2 among themselves, and I cannot get his result. I venture to send you all the calculation, because combinations are tricky things, and I do not want to do Bateson injustice through a slip in my own arithmetic.

Bateson gets $\frac{28}{64}$ white-flowered individuals in F_2. These are of 15 distinct kinds, on his theory; but there are 5 groups of kinds, each containing sub-groups which differ only in the Latent ~~pigmenting~~ elements B and b; and since these elements do not affect the distinction between coloured flowers and white flowers, but only between reds and purples among the coloured flowers, I shall neglect them, and deal only with the gametic elements C and W, R and r, which affect the difference between white and coloured flowers. Regarding only these two pairs of elements, Bateson says his white flowers in F_2 are of composition and relative frequency

(a)	CC + r r	1
(b)	CW + r r	2
(c)	WW + r r	1
(d)	WW + R r	2
(e)	WW + R R	1
		7

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In order that a plant may be coloured, it must contain both C and R; and the only white-flowered plants whose gametes can yield this combination are unions between either (a) or (b) and either (d) or (e).

unions of 1

Now all these plants are indistinguishable; and the chance that a family, produced by white-flowered plants crossed, will contain a coloured member, is the chance that with random cropping you will crop either (a) or (b) with either (d) or (e).

I take it that if you have 7 plants of the constitution above written in one series, and 7 similar plants in another series, the number of ways in which you can pair them is $7^2 = 49$; and I think the number of ways in which you can pair either (a) or (b) with either (d) or (e) is

* ~~The~~ "of course" here means that when he wrote he still held Mendel's view of the nature of white flowers, and had not applied Cuénot's mice formulae.

(a) × (d) ——— 4 (b) × (d) ——— 8
(a) × (e) ——— 2 (b) × (e) ——— 4

or 18 times out of 49. That is, I think the chance that a batch of seeds giving the F_3 plants from a cross between two white-flowered F_2 plants, should contain a coloured plant, is $\frac{18}{49}$ or rather less than ~~1/2~~. 1 : 2.

Bateson's statements on this point are a little difficult. In the Report to the R.S., in a note added last December from the result of work on F_3 last year, he says "All whites ..... and round-pollened F_2 plants, of course breed true to those characters" (p. 98); and on p. 95, speaking of whites descended from purple "rogues", he says that although the observations on these are insufficient "All the facts observed point to the conclusion that extracted whites" (i.e., F_2 whites) "give only white when bred together".

In his letter to us, he says that "the odds against any family produced by crossing white flowered F_2 containing a coloured member are about 1·5 : 1."

You have here the experience of two seasons: one gives the odds in favour of a coloured family = 0, the other gives it as 1 : 1·5.

If I am right, the theory gives 1 : 2.

I think the two years taken together are strong evidence, until the full figures are published, that the behaviour of these "extracted whites" is not in accordance with Bateson's theory, which will fail for F_3, just as it failed for the mice for which Cuénot invented it.

You may say, Bateson did not cross at random, but crossed flowers with long pollen and flowers with round pollen only. This does not matter, because the ratio between flowers with long and flowers with round pollen in each of the 5 groups I have considered is said by Bateson's table to be exactly 3 : 1, so that his ~~choice~~ choice of gametic constitution, so far as the difference between white and coloured flowers is concerned, was quite unaffected. His correlation is between pollen character and purple: but that only affects the distribution of purple and red inside the category "coloured": it has no effect on the ratio between "coloured" and white.

Yours very truly
W. F. R. Weldon

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F467/

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MERTON LEA,
OXFORD.

13th. October 1905.

Dear Mr. Galton,

It is very sad to hear such news of you tonight. I was afraid on Wednesday that the cold and damp had hurt you, but I hoped it would not be so bad as this! I trust the "new moan" will remain a jest. It would be far from a jest if you began the winter in that way.

It was kind of you indeed to take so much trouble for me when you were ill and had your own things to do!

I have several hundred more hicks for Cuénot, but you will not care to be bothered with them till you are better. In the mean time, since the theory of subsidiary determinants was invented to get rid of difficulties in the mice, I send you some very bad photographs of a dozen individuals from Darbishire's F_2. Since his conversion to Mendelism, Darbishire himself speaks of these as "with some exceptions indistinguishable from wild mice". — They

are the "picotee forms" of wild mouse. There were 340 individuals in Barlow's F_1, and according to his catalogue in Biometrika, 146 fell within the range of these photographs.

I am making a series of photographs to show the whiteness in individuals of F_4 & F_5, with all their ancestors and collateral relations. — It is the only way to show the folly of this "picotee" hypothesis, but it will take a little time, and before it is done, I hope you will be well enough to care to look at it.

The numbers in rings (2) etc. are Barlow's numbers indicating amount of whiteness and colour. — The other numbers are only the catalogue numbers of the skins used, so that you may compare these photographs with the account in Biometrika, if you care to do so.

I hope you will be able to get into the country next week, as you spoke of trying to do. There is not so much bacterial dust outside

London, and though it is cold it is very beautiful. The leaves are gorgeous, but they are falling fast, especially from the elm-trees. If Miss Biggs really wants to ride over to Bibury and see the elm-trees covered with gold, she should come in the next fortnight, at least.

Yours very truly

W. F. R. Weldon

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f469r

MERTON LEA,
OXFORD.

24th. October 1905.

Dear Mr. Galton,

It is very good news that you are well again in spite of the cold! — On Saturday night, when the thermometers here went down to 16°, I began to hope you could go South into the sunshine at once.

These categories of the Mendelians are strange things. I have been trying to learn the combs of fowls, to follow Bateson's account of poultry. In the ordinary (presumably crop-bred) cocks, whose heads I get from the poulterers, the forms of comb he describes certainly exist, as one knew they did: but there is a fair proportion of plausibly intermediate forms, and the range of variation within any one category is very great. — I hope to show you photographs of these also presently. — Do you not think the study of ordinary mongrel individuals, such as one finds in a poulterer's shop, a perfectly fair

test of Mendelism? — The theory, as applied to pure races and their hybrids, says that each form of comb depends upon a definite determinant, dominant over some comb-determinants, recessive to others. If this is a statement capable of any general application, crop-breeding should not produce intermediate forms of comb, and a crop bred fowl should be a mixture of pure bits, so to speak. It should have the comb of one pure race, with perhaps the tail feathers of another, and so on. If the theory of determinants as developed by the Mendelians does not lead to this, the alleged sharp distinction between the comb-forms in hybrids between pure races and in the offspring ~~[illegible]~~ of such hybrids must depend not only on the nature of the embryonic determinants but on the ancestry of the forms mated, — which is, on the Mendelian view, absurd!

You are most kind to think of me in connection with your advisory meetings. Of course

I should be greatly interested in them; but whether I could help or not is quite another question. May I come to the first meeting at least, since you are kind enough to suggest it, and see whether I am quite useless? — I am very anxious that some one who is trying to work at inheritance in other forms besides man should keep the knowledge of what you are doing in mind. Whether I can help the meetings or not, I am sure they can help me!

You are never willing to suggest, so I must point out to you how you have wronged the Medici. They bore not three pills only, but <u>six</u>; perhaps the truncation of their arms by the pawnbroker is a sign that they did not administer all their pills at once?

Do you know how to blazon their arms? — The best I can do is "<u>Gules</u>, six pellets <u>or</u>, in orle, the uppermost charged with three fleurs-de-lys of the field." — But on the one hand, a pellet, though ~~round~~ spherical, is always <u>sable</u>, and on the other any circular charge on an English or French shield, if <u>or</u>, is flat, and called a bezant. — But the Medici bore "palle d'oro," which were certainly (spherical) ~~round~~ and golden. — It is a puzzle of a quite Mendelean kind.

As there is no history of a physician among the early members of the family, don't you prefer the story given in Roscoe's Life of ~~Lorenzo~~ Lorenzo, that the palle are the marks left by six balls, which hung from the battle-mace of the magician Ruggello, and were flung at Averardo de' Medici so hard that each one of them dinted his shield?

Yours very truly

W. R. Waldron

F4717

Oxford — 19th November 1905

Dear Mr. Galton, — I wish for my own sake you were back here, because I never wanted your advice so badly as I do just now. A fortnight ago, as Chairman of the R.S. Sectional Committee on Zoology, I received the MS. of a paper by Bateson's pupil Hurst, on the colours of race horses. This seemed the most perfect demonstration of Mendelism I had ever heard of, and I sent it back at once, ordering it to be published without the delay caused by referees, because of its great importance. Then I considered how I ought to most publicly and quickly retract everything I had said against Mendel. Then it occurred to me that in justice to Hurst's careful work, I ought to convince myself, by actual reference to the Stud-book, that he had stated his case properly. So I got a stud-book, — a the last 8 volumes, — and worked.

Hurst says there are 2 categories of coat-colour in race horses, — one, "bay or brown", dominant to the other, "chestnut": so that a bay or brown horse may be either DD or DR in Mendel's phrase, while a chestnut horse must be always RR. It follows that if you pair two chestnuts, you must always get chestnuts: and Hurst gives over 1100 foals from such unions, of which only 9 are not chestnut. He suggests that these are due to printer's errors or some such source. He gives six "Bay or Brown" horses, which he calls DD, because when mated with horses of any colour these gave only Bay or Brown foals, — the six together giving 370 foals, a a very fair test of the absence of Chestnuts.

He gives 12 Bay or Brown horses which he calls DR, because when mated with Chestnuts they

give 50% Chesnut and 50% Bay a Brown, as indicated by Kendel's formula DR x RR = RR + DR

There 12 horses gave foals such unions

Bay a Brown	355
Chesnut	347
	702

or an ideal 50% of each.

The figures for the individual horses are given, and they fluctuate very little. I did not copy them.

There seemed no answer to this at all, and I had a couple of days when all the world seemed crumbling to bits.

Then I took up the Stud Book. It is a long business. Every volume treats of four years, and is from 800 to over 1000 pages long, arranged under names of mares. There is no index to breeding horses at all. The colours of horses are only given in the early of their birth, not in the entries of the birth of their foals, and except in the last two volumes, the colours of mares are only given in the years in which they were born. When you begin to track a horse through this wilderness, you do not even know how many foals he has had: so you do not know whether he is worth tracking.

There is, however, an annual list of foals, under the names of their horses. This has only been published during the last few years: it does not go back far enough to cover the 15–20 years which form the breeding time of a horse. However, I took this annual list as a guide to those horses which had

foals enough to be worth tracking, and began to pick out the Bay or Brown horses, ~~etc~~ in alphabetical order, then to trace their unions with Chestnut mares. I fully expected to get Hurst's 50% Chestnut and 50% Bay or Brown among the foals: but when I had done a very few, I saw that my results were far more irregular than his. Taking only those with more than 20 foals, I have now got

Bay or Brown Horse	Foals by Ch. mares	
	B. or Br.	Ch.
Bushey Park	23	7
Common	38	21 *
Hackler	40	2
Bread Knife	22	10
Wolf's Crag	51	3
Buccaneer	11	23
Adieu	15	25
	200	91

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Wolf's Crag is the only one of these I picked. When I had done the others, I wondered whether such an abnormal beast as Hackler could be paralleled, and from the few foals recorded I for Wolf's Crag in 1904 I resolved to try him.

But even if you leave Wolf's Crag out, it is clear

* and I classified as Br or Ch.

that the chance of getting a Ch. foal with one of these horses and a chestnut mare is not ~~always~~ the same as with another. Seeing this, I resolved to take Chestnut mares, and to follow all their matings with B or Br. horses. This involves (1) finding the colours of every mare: (2) getting together all her foals (she may run through 5 vols. of the Studbook!) and the names of their sires; (3) getting the colours of the sires; (4) indexing such a record, when made, under names of sires. So far, I have done all the Ch. mares whose names begin with A, B or C, and I have a very large number of foals recorded: but of these only about 350 – 400 are of the type required. The B or Br. horses in such a list are, as you see, chosen quite at random so far as I am concerned: but there is still the same preponderance of B or Br. among the foals. I do not send you the figures yet; because the index grows slowly but still a little every day (I have put in 9 or 10 hours every day at it lately!) and more because like I have carefully scrutinised the record of every horse which gave only B or Br., I cannot safely leave ~~him~~ its foals in my total. I must be sure that it has produced a Ch., or they may call it DD.

I think I may feel free from any need to apologise for opposing Mendelism so far: but here is a difficult point to settle. This paper will be read before the R. S. directly, and I must go and discuss it. What on earth am I to say? — Hurst has had the same marvelous luck that Bateson had with his Peas. He looks at the studbook, and finds Mendel. I look at it, and find what I have told you. I know I am sober, and

[19.11.05] H473r

I think I am about as sane as I ever am: what can I conclude?

Hurst must have noticed these things. He has hunted through the Studbook for horses which gave no Ch. when mated with Ch mares. Is it conceivable that he found the six he quotes at once, without finding some like Wolf's Crag, with 51 Br or Br. to 3 Ch., on the way?

It is horrible to think that a man has picked his evidence in a case of this kind. I feel a thorough blackguard for entertaining the idea: but I do not see any alternative, unless there really is a devil somewhere about Bletchley, who makes it possible to see the same thing in one way here and otherwise in Cambridge!

I wish you were in Rutland Gate. I would come and bother you, and you would laugh at me kindly, finding some wise way out of it all. Without you, I sit and fret, feeling overworked and losing my sense of perspective, so that this thing seems more difficult than it really is: but it really is hard enough!

One thing is clear. I must carefully index one whole volume of the Studbook, following the mares recorded back to their first foal, and I must index the record so named under names of sires, with their colours. Then I shall have about 5,000 foals of known colour, with colours of their parents.

Then, by tracing the pedigree of the parents through the mares of the early volumes, one will

be able to see how far the idea I have been trying to think out is true, — that your dominance of elements in the strip is determined by the condition and relative numbers of elements in each germ cell at the moment of fertilisation.

This letter is far too long and egotistical. Please forgive it.

I am really glad you are far away from all these squalid riots, and I hope by this time that both you and Mrs Biggs are quite well. — Was her "set back" serious? I hope not?

Will you think me a brute if I take the studbook to Rome? I really want a holiday, but I dare not leave this thing unsettled.

Yours very truly

W F R Weldon

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You will understand that this letter is "confidential". I have no right to talk about Hurst's paper till it is published: but I know you will not say anything, and I have surely a right to verify his statements, — or otherwise!

Do you think $D = n$ is a Mendelian formula?

F4744

Merton Lea, Oxford – 24 Nov 05.

Dear Mr. Galton, – Many and sincere thanks for your advice. – I dare not follow it just yet, for I differ so utterly from Hurst in every single thing. I can hardly trust my eyes when I read the stud book. – Horses such as those I sent you abound. – To test the average result of pairing Ch and the Br Br these people call DR, I took all the Ch mares in a certain volume of the Studbook whose names began with A, B, or C; I followed these through the various volumes from their 1st foal till their death or till 1903, recording every foal, its colour, with the name, colour and parentage of the father. I had then over 200 mares, chosen so far as I can see at random. I had 240 B. or Br. sires which I knew must be called DR by a Mendelian. These sires gave, from the Ch. mares

Ch. foals — 329
B. or Br. — 408.

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This is a deviation from 50% of more than 4 Q. But the truth is almost certainly worse than this. For I have 41 Br Br. horses who gave (70) ~~only~~ Br Br. foals from these (and no Ch) mares. These horses have B. or Br. parents, so that to a Mendelian they may be either DD or DR. To show that they are DR, one must show that they produced a Ch. foal from some mare. But most of them are young horses, which have as yet produced so few foals from Ch. mares that this test is worthless. – When the first foal produced by a B. horse is Ch., I have of course included him. But a B. horse whose first dozen foals by Ch. mares are B., is left out. – I did this on purpose, because it gave

Mendel every possible advantage.

I have now begun to work at the reciprocal cross, — DR mare and Ch. horse.

DR mares should be easy to find, because there is a list of breeding mares under the names of their Sires. I took this, and the first Ch Sire I knew was Ben Battle. Among over 100 breeding mares he produced 44 Bay Br. — Since he was Ch., it ought to follow that the dams of these mares were not. For Ch. × Ch., both being recessive, should breed true. — The stud book says that 12 of these B. or Br. mares are by Ben Battle from Ch mares.

Among the comparatively small number of fillies produced by Ben Battle I have more exceptions to the true breeding of Ch. × Ch. than Hurst finds among the 1100 foals he catalogues from such matings!!

I have been through Ben Battle and Bend Or from this point of view. Ben Battle × Ch. mare gives 12 cases in which Chestnuts do not breed true, Bend Or × Ch. mare gives one. I do not yet know any other Ch. thoroughly. — But these "mis-prints" are inherited: for Bend Or's "Roan or Ch." colt from a Ch. mare was mated with a Ch. mare and gave 2 Ch. + 1 B., while 9 of the 12 B. mares from Ben Battle × Ch. mare gave to Ch. horses

10 Ch. + 17 B.

The need to look out the colour of the dam has made the whole thing very slow: but I have so far got from "DR" mare × Ch. horse

f475r

52 Ch. + 81 Ba Br.

There three sets of numbers, — the DR horse × Ch. mare set on p. 1, the limited DR mare × Ch. horse pony mares due to Ch × Ch. unions, and the set just written, are each a possible $\frac{1}{2} + \frac{1}{2}$; but the deviation is always in the same direction, and each result agrees with its fellows far more closely than with $\frac{1}{2} + \frac{1}{2}$?

I am very sorry you are getting on so badly at Pau. I have rather feared that the wet winds from the Atlantic must make rain and an enervating climate round the first mountains they meet, there as they do in Cornwall & Britanny. — If you decide to wander, I hope you will go E., away from all that. — The country round the E. end of the Pyrenees on the French side is very beautiful, though 20 years ago, when I walked a little there, the inns were detestable. They have probably changed all that, and if you could find a decent inn at Banyuls, on the Mediterranean, about a dozen miles N. of the Spanish frontier, you might like it. — There is nothing but a Zoological Laboratory and a lovely country, but the country is good, and if you went (through Toulouse) you would delight in Carcassonne, Narbonne, Perpignan, and other little old world towns I have forgotten. In Perpignan they hang blankets and carpets from house to house across the streets to keep the sun out. I have never seen this elsewhere, though of course you have. I, who have never seen the East, think the whole town looks "Oriental"!

If you cared to gravitate slowly that way towards Montpelier, to worship Rabelais's grave for a moment,

— far more worthy than Our Lady of Lourdes! — and then to see Aigues Mortes and the Camargue on your way up to Arles and the P.L.M. to Paris, I think you would wander through some splendid country, and have more bracing air than if you went Westwards.

Will you tell me if you think of it? You know, in Camargue I have seen wild horses caught with a lasso; I believe you can see it still. Twenty years ago, I knew the man who "ran" about half of it, growing wine in a way you would like to see, so that when Phylloxera went down to hurt the roots in autumn he could flood the whole county and drown the beasts: making salt, growing horses, & having (unhappily) a chemical works. — If he is still alive, he will put you up and show you things with pleasure: and I will gladly find out, if you tell me. There are flocks of flamingoes in Camargue, and every one's father & brother has seen a live beaver. I saw a dam made by beavers, but it was deserted. — Try it! Provençal is an easier tongue than Basque, anyhow. — Work out the reason why the men who come to Camargue in summer live in beehive shaped huts with little tunnels leading to the doors, like the things Eskimo build of ice?

But you must both get well and be happier, wherever you go!

We shall go to Rome (Hôtel de Russie) on the 9th. or 14th., according to the date of this wretched paper. Tell me if it is not my duty to take the stud books? I go to bed whispering Brown Ox is Chestnut: I wake up declaring St. Simon is Bay, — and I don't want to do that in Rome!

Yours very truly W. F. R. Weldon

Ent. 1 Dec 12

Weldon

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SAVILE CLUB
SODALITAS CONVIVIVM
107, PICCADILLY. W

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f476r

10 December 1905.

Dear Mr. Galton,

We had our little fight over the horses on Thursday, and now in an hour I shall go down to Folkestone in front of this letter, on the way to Rome.

I sent you Hurst's paper yesterday. – It is my only copy. When you have done with it, will you be so kind as to send it to me (Hôtel de Russie) in Rome? – I am taking the studbook, so as to verify all my statements and to have a little note ready for the R.S. as soon as may be.

I have found "DR" horses which give when put to Ch. mares

50	Bay or Brown to	1	Chesnut
38	— " —	1	— " —
20	— " —	1	— " —
40	— " —	2	—
53	— " —	3	— " —

and so on. Taking the Ch. mares in alphabetical order, and looking out their foals by DR horses, I find 920 Ch. to 1140 B.Br. (I quote from memory: the last digit is not 0 in either result: but the truth is at the bottom of my portmanteau)

To dispose of the view that exceptions to the

rule of Chesnut × Chesnut giving Chesnut only are all due to mis prints & faults of judgment, I have collected a number of cases in which these "mistakes" are inherited.

I think these things produced some effect on Thursday. I am told that Bateson himself admits that his case broke down. He did not admit it at the R.S! He set his face like a millstone, and produced three pigeons, two black and one white, out of a bag. He said, it was all very well to talk about the tangle of records in the Stud-book, but here were three actual pigeons, which the Society could see for itself. By means of these pigeons, he proposed to demonstrate the folly of all biometric methods. — But Rayleigh ruled that this was a new point, which could not be raised after his opponents had exhausted their right of speech: so he sat down.

It is all very well to have shown that Mendelism does not account for these results, but I am anxious to get rid of controversy for a while, and to see what hypothesis will express them. They seem to me ideal material on which to try hypotheses involving

change of dominance in the determinant elements of the strip according to the constitution of the two germ cells which meet in fertilisation, — on the lines of the card-game I have spoken to you about.

The excep of Bay ~~elements~~ foals when you pair Bay and Chestnut horses is beautiful, when you remember how recent the fashion for Chestnut horses is. Three generations back, nearly all the ancestors were Bay. Four & Five generations back, Chestnuts were quite rare.

z

Your Eugenics committee on Wednesday was not very successful: few of the men, who had promised to do things, were able to be present. I was glad to go, because Schuster told me how well the family histories were going on.

z

I hope your own history is going on well? Kip Chapy told me sad tales of you and of Kip Bègs when I met her the other day. — The Times map tells me you have had very little sunshine. I hope that will come. And at least I hope the sea winds will do you both good.

I wish the train I must now go and catch would help us to find you in Rome. You would be better there, as we should be the better for seeing you!

Yours very truly

W F R Weldon

F478

46) [Found, out of place among letters of Weldon to Galton]

[1905]

At the meeting of the R.S., held on Dec. 7, a discussion took place as to the possibility of representing the inheritance of coat colours among English thoroughbred race-horses by an simple Mendelian formula. The object of the present note is to comprise certain statements, which I made on that occasion, and to record certain facts which ~~seem to me to deserve~~ must be borne in mind by any one who attempts to form a theory of the gametic "mechanism" by which the inheritance of coat-colours may be ~~imagined to~~ effected.

The data here dealt with are given in Vols. XVIII and XIX of Weatherby's Stud book. I have extracted what I believe to be a fairly complete (list) ~~record~~ of all the foals produced by Chestnut mares ~~that~~ in England during the eight years covered by these volumes. ~~In these cases~~ I have not ~~only~~ tried to ~~follow~~ ascertain by other means the colours of mares, sires, or foals, ~~which are given~~ in the small number of cases in which these data are not recorded in the Stud-book. From the somewhat laborious nature of the work, and the large amount of copying involved in it, it is probable that some clerical errors have ~~crept~~ crept into my tables: I have, however, been as careful as I could, and I believe the statements which follow are substantially correct.

(1) The first thing to be looked for, in attempting to ~~find~~ construct a Mendelian formula, ~~for~~ is a character which is

"purely recepive"; ~~the~~ the ~~only~~ individuals which exhibit such a character breeding true to it when paired. Of the commonly recorded coat-colours, Ch., B., Br., ~~Bl~~ and the intermediates between these, not one exhibits the characters of a Mendelian recepive. ~~A~~ Bay ~~a Brown~~ individuals, when mated together, ~~give~~ ~~such a large proportion of Ch~~ give such a large proportion of Ch., Br. and Bl. foals that ~~as~~ they cannot be regarded as recepive: the same is true of Browns. With the possible exception of Grey, which occurs so seldom that I have not yet sufficient evidence on which to discuss the progeny ~~of grays~~ obtained when two greys are mated, the only colours which ~~appear~~ can plausibly ~~as~~ be regarded as recepive is Ch.; the 202 Ch. sires which occur in my records have produced 1500 foals, of which 23 are

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GALTON PAPERS

Galton f480r

Hôtel de Rufie
Rome.

22 December 1905.

MERTON LEA,
OXFORD.

Dear Fr. Galton, — I have not yet thanked you for your kind letter, because I waited to send you some more figures. When I came here, I had, of four matings between "DR" horses and Ch. mares, the following foals:

	Ch.	B. or Br.
1st. set ____	331	399
2nd. set ____	369	445

From the reciprocal crop, Ch. horses and "DR" mares, I had

Ch. 225 B. or Br. 296.

I am now trying to take all the horses in a four-year volume of the stud-book, and to follow all their matings with Ch. mares through the 3 or 4 volumes which record their lives. — I have had first to make a slip catalogue of the horses, then to look out the colours of the mares and enter these against

their names in the stud-book; then to pick out the Ch. mares, and enter their foals on the slips bearing the names of the appropriate sires. So far I have only done 3000 mares, and the Ch. among these gave

371 Ch. + 439 B. or Br.

by 167 "DR" horses: but they also gave 206 B. or Br. foals by (45) B. or Br. horses which I have not yet shown to be "DR", though some will certainly prove to be of this character.

You see, the excess of Bay or Brown foals, corresponding to the known preponderance of Bay or Brown in the remoter ancestry, continues.

Pearson tells me that Hurst's paper has been withdrawn "pending further examination of the records". I am very glad, because this quarrel about a simple question of fact need not now get into print. At the same time, it is not easy to see what I ought to do. — At the R.S. the other day, I said that the figures I quoted were hurriedly collected, and

promised to make a statement of the revised result at the earliest possible date. I must, of course, be ready to keep that promise, so I must see nothing of Rome, but work away at these things (the next meeting of the R. S. is on Jan. 18. I propose to leave here on Jan. 8, to put the figures into shape in Oxford between the 10 and the 18, so as to be ready): but do you think, since there is no need to go on attacking Hurst, that I may delay publication till I can try to treat the offspring of Ch. mares properly, by reference to their own ancestry and to the colour and ancestry of the horses with which they are mated?

It would be so much more satisfactory to do that than to write another controversial paper full of more destructive criticism!

I think the Ch. mares alone, although they only give one line of the correlation table which ought to be made between parental and filial coat-colours, will give some results of value, if one takes so many of them that one can have reasonable confidence even about the extreme cases? — And I think it will be worth while to deal for once with a whole population, not with a small random sample of it. — Only,

I could find it in my heart to wish one need not do it in Rome! To sit here 8 hours a day or so, doing mere clerk's work, seems rather waste of life?

I hope you are warmer and much stronger by this time? — I fancy your Atlantic winds are not so cold, for all your want of sunshine, as the Tramontana which keeps the sky blue here!

It is a shame to hail the story of these horses over your Christmas, but I am so brutalised by it I cannot think of anything else, except to send all sorts of good wishes to both of you.

Yours very truly

W. F. R. Weldon

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Rome 28. December 1905.

* – i.e., the English mares. The Irish are separately recorded, and I have not indexed these yet.

Dear Fr. Galton. – First, let me say how glad I am that you are warm at last. Your warmth means a black sky, pouring rain, and a hot Sciocco here: but the thought of your shaw hat made blue sky and sunshine between the torrents that fell from my umbrella as I walked along the Corso just now! – I shall certainly follow your kind advice, and get a note ready for the R. S. by the 18th., if they will take it. I have now done all the Ch. mares* in a four-year volume, except a few whose colour is only recorded in volumes I have left at home. From these I expect about 200 more foals: but taking the results at present fairly certain, I have from Ch. mares

(1) by 107 Ch. sires (Hurst's "RR × RR") I have

744 Ch. + 12 Not Ch.

These 12 are from 11 sires, so that about 1 sire in 10, of the "pure recessive" type, has given "dominant" offspring, though the total proportion of such offspring is small. I know that in the next volume at least 5 more of these same sires will give B. or Bn. foals by Ch. mares. The approximation to true breeding when Chestnuts are paired is therefore just what I had hoped for: – near enough to justify a rough practical statement, but not enough to justify a theoretical statement of "gametic purity". – In the attempts to work out "pure recessives" on the basis of your rein of changeable dominance, with the dominant or recessive character of each determinant depending on the relative numbers of each kind present in the strip at the

moment of fertilisation, Pearson and I in the summer found always from 1 to 2% of the progeny with dominant characters, — a result which does not fit the Mendelian assertions, but fits these horses splendidly, — and these are the first things of which one has been able to examine numerous unions between "pure recessives", tabulated by impartial persons (I mean the stud book folk: not myself!!)

(2) From Ch. mares by B. or Br. horses which Mendelians would call "DR" I have

Ch. 581 B. Br. Bl. 701.

If the expectation were really 50% of each, this would be a deviation 60, with an S.D. of say 18 or a Q of say 12. — A deviation of 5 Q, in the same direction as before.

On the other hand, taking the rough ratio 4 Ch: 5 B. which seemed to fit the numbers of foals in Oxford from other volumes, the numbers should be Ch. 570 B. Br. Bl. 712
or the observations differ by 11 only: say by their Q.

(3) From 50 Br. or B. sires, both of whose parents were B. or Br. (I have not yet found their grand parents in all cases) I have by Ch. mares

B. or Br. 303 Ch. 0.

(4) In the whole volume, there are only 6 Black horses mated with Ch. mares: these have produced by such matings

8 Ch: + 6 Br. + 12 B. + 2 Bl.

So that no simple Mendelian dominance of black

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over Chestnut is possible.

(4) There are 3 grey horses mated with Ch. mares: these give by such unions

4 Ch + 5 Gr. + 4 B. + Br.

so that no simple Mendelian theory fits Grey!

I am sure your view of dominance will fit all these results like a glove. For reasons due to the adoption of better methods of recording, the next volume of the Stud-book will be much easier to index than that I have just finished: half of it was indexed before I left Oxford: I hope to revise that, and to finish the volume, before Monday week, and then, after a couple of days asleep in the train, to write up such a note as you suggest.

I wish there were time to ask you to read it before sending it in, — but perhaps you will console yourself for being spared so much trouble more easily than I for sparing you!

You may imagine, I have not seen much of Rome. We had a delightful evening with Mr. & Mrs. Cameron Salton on Christmas Day, and they were good enough to plan a little festa at Tivoli tomorrow, to celebrate the end of my volume: but the Scirocco has put an end to that: we are in for the traditional three days; the colour has gone out of everything, and it is raining as it can rain in Italy. We cannot complain. We had a fortnight of bright, cold weather without a single cloud, which would have been perfectly enjoyable but for the horses. Now, we must put up our umbrellas patiently and be glad that

you have sunshine. — May it last through all your stay at St Jean de Luz!

Yours very truly

W. F. R. Weldon

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Oxford – 4th. February 1906.

Dear Mr. Galton, – It was a great pleasure to hear good news of you yesterday. I hope you will enjoy your stay among the hills. – Spring must indeed be coming, if you can venture so high already! It was delightful to think of the flowers that must be all about you, as the snow flakes whirled about yesterday. We do not feel the Spring here yet!

We had great fun with the horses on the 18th. – I send you the proof of what I said. There is no doubt about two things: (1) The excess of Bays and Browns over ~~the expected~~ the proportion predicted by a simple Mendelian theory is a real thing: and (2) those Bay or Brown horses which do give Ch. foals by Ch. mares do so in different proportions. – In the proof are given 3 or 4 extreme cases, in which the proportion of Ch. foals is small. These are connected through such sires as Bushey Park, which gave 6 Ch. + 28 B. or Br. a Bread Knife (12 Ch. + 26 B. Br.) with horses like those quoted by Hurst, giving 50% Ch. + 50% B. Br., and through these with such horses as Queen's Birthday (= 25 Ch. + 13 B. Br.) = Simon Tadd (15 Ch. + 5 B. Br.) Buccaneer (30 Ch + 13 Br.) and so on. I do not trouble you with a long list: but I have now got all the records for 3 volumes, giving over 9000 foals from Ch. mares by about 1100 sires; and the proportion of sires which give these erratic percentages of Bays is wildly in excess of what it should be if the expectation for each were 50% Ch. + 50% B.

Bateson's answer is simply that the chestnuts attributed to such horses as those quoted in the paper are all due to misprints or misdescriptions, and that these horses are really all "DD", giving no Chestnut foals at all. In the one case he specially quoted (Veracity, which gave 2 Ch. + 29 B. Br.) he could not throw doubt on the description, so he tried to show that the Ch. foals were attributed to the wrong sire! His view is, that in describing the foals of "DD" horses, stud-grooms and printers say Ch. for B.; on the other hand, when describing the offspring of Chestnuts, they wrongly say B. for Ch., once in 60 times. This is a strange theory of error, and if it could be shown to be true, the only legitimate conclusion would surely be that the evidences, given by the stud book, is of no scientific value?

It is certainly true that the colour of a foal, as recorded in the stud-book just after birth, is not always the same as the colour given in the Racing Calendar, where the thing is redescribed when it races: but in the few cases in which a change ~~occurs~~ is recorded, there is no special reason for believing that it is due to misdescription. It is a real change of dominance during growth. There will be perhaps 80 or 100 such cases in my lists, and I shall have to get into communication with the breeders about these, when my tables are finished. – I hope to spend tomorrow at the Jockey Club, getting preliminary information.

So much for the proportion of Ch. to B. or Br. among the whole progeny of Ch. mares. I have tabulated the colour of the parents of each sire recorded, and am now making a table of the grand-parents of each. It takes a very long time, because

the colours are only given with the record of the birth of the horse or mare; not in the subsequent records of breeding; and if you do not know the date at which a horse was born, you may have to look through 3 or 4 volumes at least before you find him.

For the offspring of Bay "DR" horses, mentioned in the proof, there is certainly a strong correlation between grandparental and filial colours, which ought not to exist on Mendel's view.

A "DR" Bay horse may be of pedigree $\underbrace{\underbrace{\text{Ch.}\quad\text{B.}}_{\text{B}}\quad\underbrace{\text{B.}\quad\text{B.}}_{\text{B}}}$ and a Chestnut mare may have two Chestnut parents, or none, or one. A horse mates with so many mares that I think it is fair for the moment to regard the Ch. mares as of constant ancestry, namely $\underbrace{\text{Ch.}\quad\text{B.}}_{\text{Ch.}}$, which is about the average of all of them. Allowing this, we have in these lists two kinds of foals by "DR" Bay horses: those of pedigree

$$\underbrace{\underbrace{\text{Ch.}\quad\text{B.}}_{\text{B}}\quad\underbrace{\text{Ch.}\quad\text{B.}}_{\text{Ch.}}}_{\text{Foal}}$$

giving on your view $\frac{1}{4}+\frac{1}{8}+\ldots$ Ch. or probably $\frac{1}{2}$

and those of pedigree

$$\underbrace{\underbrace{\text{B.}\quad\text{B.}}_{\text{B.}}\quad\underbrace{\text{Ch.}\quad\text{B.}}_{\text{Ch.}}}_{\text{Foal.}}$$

giving on your view $\frac{1}{4}+\frac{1}{16}+\ldots$ Ch. or less than $\frac{1}{2}$.

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Subject to revision, I find among the foals given in the proof

	Ch.	Not Ch.	
Sire with 1 Ch. parent	~~77~~ 700	677	exactly what you would have predicted, and Mendel would not.
Sire with no Ch. parent	418	635	

So that the two kinds of "DR" horses, which ought to give the same proportion of Chestnuts, clearly do not. It is clear that there are high ancestral correlations, which ~~must~~ must be worked out.

It is also clear that the law of reversion is not the same for matings between B and Ch. as for matings between Ch. and Ch. You have for Chestnuts

$$\underbrace{\underbrace{\text{Ch.}\quad\text{B.}}_{\text{B.}}\quad\underbrace{\text{B.}\quad\text{Ba.}}_{\text{B.}}}_{\text{Enthusiast (Ch)}}$$

gave by Ch. mares 48 Ch. + 1 "misprint."

$$\underbrace{\underbrace{\text{B.}\quad\text{B.}}_{\text{B}}\quad\underbrace{\text{Ch.}\quad\text{B}}_{\text{B}}}_{\text{Ocean Wave (Ch)}}$$

41 Ch. + 1 "misprint"

f4 P5r

while for Bays with similar ancestry you have typically

Ch. B. B. B.
B. B.
Avington (B) = 21 Ch. + 23 B. or Br.

And there are a few pedigrees among the "DD" series which make me fear that when a Ch. and a B. are mated, reversion to fairly remote ancestors occurs with very sensible frequency. This will be interesting if true; it will parallel the reversion right back to the ancestral wild mouse when one crosses albino mice with Japanese waltzers, — a Bateson's reversion to the wild sweet Pea when 2 white races are crossed: not to speak of Darwin's cases! — But it will involve no end of work. This pedigree will show you what I mean.

Galopin (B) 1872 St. Angela (B) 1865 Hampton (B) 1872 Hermione (B) 1875
St. Simon (B) 1881 Perdita II (B) 1881

1.— Persimmon (B)
2.— Florizel II (Br.)
3— Diamond Jubilee (Br.)

St. Simon was a "pure dominant"! In my 3 volumes, he has 86 B. or Br. foals by Ch. mares, and no Ch.

Perdita II was never mated with a Ch.; but she bore no Ch. foals to horses which were "DR".

Galopin is one of Hurst's "pure dominants": in my period he had 38 B. Br., +1 called "B. or Ch." by Ch. mares.

St. Angela, Hampton, and Hermione, all produced Ch. foals.

You see that the 3 brothers, Persimmon, Florizel II, and Diamond Jubilee, had parents and grand parents all B. or Br. — From the full pedigrees of their grand parents which I enclose, you will see that their great grand parents also were all B. or Br. Now pour Ch. mares we have

	Ch.	Not
by Diamond Jubilee	5	4
Persimmon	23	23
Florizel II	26	32
	54 + 59	

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This is the kind of thing Bateson has in his mind when he scoffs at ancestry: and if (as is likely) he has not troubled to make out these pedigrees beyond grand parents,

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one can hardly blame him. But if you look at the whole pedigrees, you see that ~~St. Simon had far less Ch. in his back ancestry than Hampton~~ the back ancestry, especially of Hampton and Hermione, contains many Ch. individuals (I have underlined these in red): and the only explanation I can see of the Chestnuts in the progeny of Persimmon and his brothers is, that union with Ch. mares caused reversion right back to these Ch. ancestors in a sensible percentage of the foals.

Luckily Hampton, Galopin, and St. Simon are very popular sires: so I can get a good lot of evidence about the effect of putting these and their ancestry in different parts of the pedigree of other horses: and by comparing these, I hope to get a hint as to what the law of reversion really is, and to confirm this by making a table of correlation between proportion of Ch. foals produced and proportion of Ch. individuals in the ancestral generation which seems most important.

If I can manage to do this, it will then be possible to deduce what the constitution of the present generation would have been, if the ~~horses~~ ancestors had been mated at random: and if that deduction leads to representation of ancestry in the proportions of your series $\frac{1}{2} + \frac{1}{4} +$ etc., as it very well may, from the look of the actual proportions of Ch. and B., then I think there will be a little step in the direction of learning how the apparent discrepancies between you and Mendel arose, — and also a little step towards a theory of the determination of dominance in this case, at least. — But if one is going to look up 30 ancestors for each of 1100 sires, I don't know when the work will be done! They are so inbred that it will be easier than it seems (you see how many names are common to these four pedigrees!) — but it will be very long: and this place is not as University College used to be. There, one's pupils were pleased to spend a little time in helping one: but here no one will do anything: and there are so many tricks, due to the number of horses of the same name, that an ordinary clerk would be a danger rather than a help.

You are too kind to my poor little book. — It must lie in the drawer for the present. — I must see the way through these horses, now. One could not write the chapter of the book without being a little clearer about this matter?

I am so glad of what you say about Saleby. — I have long felt as you do!

I wish I could have one day with you in your hills! — Four days in a railway carriage, and a couple of afternoon's walk, with one concert last week, are all the holiday I have had since this riot began: and you cannot imagine how brutalising it is to read nothing but horse-pedigrees all day long! I fully expect to find myself hissing when I brush my hair!

Yours very truly

W. F. R. Weldon

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ROYAL SOCIETY.

Uncorrected Proof, issued for the convenience of Fellows at the Meeting, not to be communicated to any Journal for Publication.

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Note on the Offspring of Thoroughbred Chestnut Mares.

By W. F. R. Weldon, F.R.S.

(Received January 15,—Read January 18, 1906.)

The colours of English thoroughbred race-horses, as recorded in Weatherby's General Stud-Book, are grouped under the six main categories—Grey, Roan, Chestnut, Bay, Brown, Black; but each category includes a considerable range of colour, and intermediates occur with quite sensible frequency. At the meeting of the Royal Society, held on December 7 last, the suggestion was made that the relation between the most important colours, Chestnut, Bay, and Brown, might be expressed by a simple Mendelian formula—Bay or Brown being regarded as determined by one Mendelian unit, which was "dominant" to the "recessive" Chestnut. In discussing this suggestion, I made certain statements, which I promised to justify as soon as possible, by offering the data on which they were based, for publication. The object of the present note is partly to fulfil my promise, and partly to call attention to certain facts which must be considered in the attempt to apply any Mendelian formula whatever to the inheritance of coat colour in race-horses.

I have taken from Weatherby's Stud-Book a fairly complete record of all the foals produced in England by Chestnut mares during the period (rather more than eight years) covered by vols. 18 and 19. All cases of doubtful paternity have been excluded, and I have not tried to obtain external evidence in the few cases where the colour of sire or of foal is omitted. With these limitations, I believe the following statements are substantially complete and accurate.

The Chestnut mares recorded produced, in the period dealt with, 5643 foals by 726 sires, the colours of the sires being as follows:—

Colour of sire.	No. of sires.	Colour of sire.	No. of sires.
Ch.	214	Br. or Bl.	10
Ch. or Ro.	2	Bl.	8
Ch. or Bl.	1	Gr.	3
B. or Ch.	1	Ro. or Gr.	1
B.	340	Br. or Gr.	1
B. or Br.	24	Gr. or Bl.	1
Br.	120		
			726

It will be seen that 41 of these are intermediate between some two of the accepted colour-categories.

Omitting the Roan or Grey sires, the foals produced are shown in the following table:—

Offspring of Chestnut Mares.

Colour of sire.	Colour and frequency of foals.							
	Ch.	Int. Ch.	B.	B. or Br.	Br.	Br. or Bl.	Bl.	Totals.
Ch.	1568	—	18	2	3	1	2	1594
B.	1118	7	1449	89	132	4	8	2807
B. or Br.	44	1	85	9	21	2	1	163
Br.	233	2	439	44	146	13	14	891
Br. or Bl.	22	—	5	1	5	1	3	37
Bl.	13	—	28	5	11	2	8	67
	2996	10	2024	150	318	23	36	5559

1. *Chestnuts Mated Together.*—It is clear that Chestnut cannot be regarded as a Mendelian recessive, since Bay, Brown, Black, and their intermediates, may be produced by the union of two Chestnut individuals. The number of foals not Chestnut, resulting from such unions, is 26, or about 1 in every 60 of the whole number observed. They were produced by 21 out of the 214 sires recorded, or about 1 sire in every 10 produced at least 1 foal which was not Chestnut. Since the mean number of foals per sire is only $1594/214 = 7{\cdot}4$ through the period observed, the most probable number of sires giving foals of colour other than Chestnut, if each sire were capable of

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giving 1 such foal in 60, would be $7{\cdot}4/60 \times 214$, or about 26, the standard deviation of the expectation being nearly 4·6; so that the result obtained is not inconsistent with the view that any Chestnut horse is capable of producing, by union with a Chestnut mare, a small proportion of foals of some colour not Chestnut. If each sire had produced but a single foal, we should clearly have to expect that only 1 sire in 60 would produce a foal of abnormal colour. Among the sires recorded, 56 have produced only a single foal each, and of these foals one is Bay or Brown.

On the other hand the Chestnut sires, *Amphion* and *Despair*, have produced 115 foals, all Chestnut, while *Amalfi*, *Brag*, and *Dog Rose* have together produced 36 Chestnuts and seven of other colours during the period examined.

We may conclude: (1) That the mean chance of obtaining a foal which is not Chestnut from two Chestnut parents is about 1/60; and (2) that this chance is not the same for all sires; it is probably less than 1/60 for *Amphion*, *Despair*, and their like, and much greater for such sires as *Amalfi*, *Brag*, and *Dog Rose*. This variability in the power of transmitting colours other than their own, possessed by Chestnut sires, is precisely what Galton's theory of dominance would lead us to expect; it is, however, a difficulty in the way of any Mendelian theory involving a reasonably small number of elements.

2. *Chestnuts Mated with Bays and Browns.*—On the view that Bay is a Mendelian "dominant" to Chestnut, Bay sires should be sharply divided into two series: those which are "hybrid," or of constitution "DR," giving 50 per cent. of Chestnut foals and 50 per cent. of Bays by the unions considered, and those which are "pure dominants," giving only Bay offspring. Any Bay horse with one Chestnut parent must be "hybrid" on this view; but it does not follow because both parents are Bay that a Bay colt is a pure dominant. In the table below, those Bay sires which had *either* one Chestnut parent, *or* were known to have produced a Chestnut foal, are classed as "DR"; while a Bay whose parents were both of some colour other than Chestnut was classed as DD unless I could find a record that it had produced a Chestnut foal.

Chestnut Mares × Bay Horses.

Sire.	Colour and frequency of foals.									
	Ch.	Ch. Ro.	Ch. Bl.	Ch. B.	B.	B. Br.	Br.	Br. Bl.	Bl.	Totals.
"DR"	1118	1	1	4	1441	83	98	4	5	2430
"DD"	—	—	—	1	308	6	49	—	3	370
	1118	1	1	5	1449	89	132	4	8	2807

Among the 64 sires classed as DD, no less than 37 have produced only one foal each during the period dealt with. Some of the sires are so young that their whole progeny from Chestnut mares is too small to afford a test of their real nature; others are so old that the labour of testing them by reference to previous volumes of the Stud-Book is great, and involves risk of error. For such reasons as these, the number of sires classed as DD is certainly rather above than below the truth. In spite of this, however, the proportion of Chestnut foals produced by the sires called DR is too small for any simple Mendelian theory. The simple Mendelian expectation is half, or 1235 Chestnuts, the standard deviation being $\sqrt{2470/4}$, or about 24·8. The observed deviation is — or more than times the standard deviation, and a reference to the Tables of the Probability Integral shows that the chance of getting a deviation so great as this is only about 1 in 1000.

The matings with *Brown* horses, treated in the same way, give similar results.

Chestnut Mares × Brown Horses.

Sire.	Colour and frequency of foals.								
	Ch.	Ch. B.	Ch. Br.	B.	B. Br.	Br.	Br. Bl.	Bl.	Totals.
"DR"	233	—	—	246	25	72	9	9	594
"DD"	—	1	1	193	19	74	4	5	297
	233	1	1	439	44	146	13	14	891

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Here the simple Mendelian expectation is 297 Chestnuts, so that the observed deviation from the required result is — 64, the standard deviation of the expected result being $\sqrt{594/4} = 12{\cdot}2$, or the observed result is even more improbable than that given by Bay sires.

If we attempt to consider some more complex Mendelian theory of the relation between Chestnut and Bay or Brown, we must base our hypothesis of the number of gametic determinants involved upon some separation of sires into groups, those of each group giving a definite proportion of Chestnut foals when mated with Chestnut mares. My observations do not at present decide this point; but through the period I have dealt with no such separation of sires into groups seems possible. Taking only the Bay or Brown sires which have given at least 20 foals in the period covered by my records, I find—

Sire.	Foals.	
	Ch.	Not.
Oberon	1	42
Blue Green	1	28
Minting	2	31
Veracity	2	29

at one end of the series, and at the other such sires as *Rightaway* (21 Ch. + 10 Not), or *Queen's Birthday* (13 Ch. + 3 Not), which offer a transition to such Chestnut sires as *Amalfi*.

The detailed discussion of individual sires, and of the proportion of Chestnuts yielded by each, must be postponed until the work of extracting the records from the Stud-Book has been completed. In the meantime, however the following points seem clear:—

1. No simple Mendelian view of the relation between Chestnut, Bay, and Brown, regarding Chestnut as a simple recessive, can be maintained.

2. The chance of getting a Chestnut foal from a Chestnut mare is not constant for sires of any colour whatever, and there is no indication that sires of any colour can be sorted into groups such that those in each group will give Chestnut foals in a Mendelian proportion when mated with Chestnut mares.

3. These points, together with the values for parental and grand-parental correlations already given by Professor Pearson* make it probable that the facts of inheritance of coat colour in horses can be expressed in terms of the hypothesis outlined by Mr. Galton in 1872,† and developed by him in his subsequent writings.

I hope to discuss the application of Mr. Galton's hypothesis to these data before long; but the extraction of pedigrees from the Stud-Book is an operation which takes time, and I have felt bound to offer this imperfect statement in justification of what I said in the discussion to which I have referred, without waiting until my work should be completed.

* 'Phil. Trans.,' vol. , A.

† 'Roy. Soc. Proc.,' No. 136, pp. 394—402.

14895

Oxford – 15 February 1906

Dear Mr. Galton, – I think you will forgive another long letter, because I have sign of something you will care to hear. I think the result of crossing "DR" x "RR" in these horses is going to be reversion to ancestors of which the nearest are the great grand parents, and in order to show the relation between these and the foals, you must consider the ancestry of the Chestnut "recessive" parent as well as that of the other, so that gametic purity goes at once.

I have as yet very few figures, but I think they are enough to show what the result will be. I have so far dealt only with B. or Br. horses, ~~those parents~~ with one Chestnut parent. – I have tabled the parents and grand-parents of half the horses on my list, and I have enough of the kind chosen to furnish 1041 foals by Ch. mares.

The fixed part of the pedigree of every foal I shall speak of is therefore

B. or Br. Ch.

B. or Br. (sire) Ch. (dam)

Foal

Of the 4 great grand-parents on the sire's side, one is always B. or Br., but the other three vary. Classifying the foals into groups, according to the number of Chestnut great grand-parents on the male side, I find

No. of Ch. G.G.P. on Sire's Side	Foal Colour: Ch.	Intermediate	Not
0	80	0	90
1	210	0	210
2	196	0	188
3	31	3	33
	517	+ 3 +	521

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Clearly, such correlation as there may be here is well within the limits of error.

Next, I started to calculate the ancestry of the Chestnut "recessive" mares; and after doing this for a few cases, I found what I want to show you. A Chestnut mare may have both parents Ch., or one, or neither. I therefore classified the few foals I have so far tabulated in terms of the grand parents on the dam's side (those on the sire's side being constant). I found

No. of Ch. G.P. on Dam's side	Ch. Foals	Foals not Ch.	Totals
0	18	24	42
1	64	69	133
2	26	35	61
	108	128	236

There is very little correlation here! — Pearson's various tricks for such cases give values for r between 0·05 and 0·09, — all possibly 0, considering the small numbers and the big error.

But taking all the ~~gran~~ great-grand-parents of these foals, I find

Foals

		Ch.	Not	Totals
Number of Ch. Great grand parents	0 – 3	63	95	158
	4 – 7	45	33	78
	Totals	108	128	236

The provisional value for this correlation is $r = 0{\cdot}24$, — or very close to the average value for grand parents!

I realise the tentative nature of this result, but it is very cheering to have got it. I shall ultimately have some 2000 foals of this class, on which to test it more thoroughly: then I can make 8 groups, changing only one great grandparent from group to group. But, without laying any stress at all on the value which r at present has, I think it is nearly certain (1) that the grand-parental correlation is nearly = 0, and (2) that the correlations are increasing instead of decreasing as you go upwards through the ancestry.

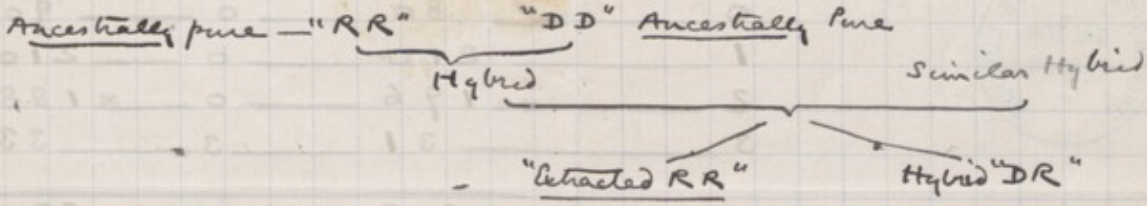
Already, one sees that the $\frac{1}{2}$ "RR" + $\frac{1}{2}$ "DR" produced when you mate the offspring of Mendelian hybrids are in the right proportions: for in the generation in which these experiments are usually made, the pedigree is

Ancestrally pure — "RR" "DD" Ancestrally Pure

Hybrid

Similar Hybrid

"Extracted RR" Hybrid "DR"



When such an "extracted recessive" and "hybrid" are mated, their great-grand parents, and remoter ancestry, are from the usual conditions of the experiment 50% pure "RR" and 50% pure "DD". — When the same experiment is performed with later generations descended from the same cross, you get the "peculiarities in the behaviour of extracted individuals", the "abnormal heterozygosis", etc., etc., which are ways of saying that recessives no longer do in fact breed true.

But the great point is the fact of reversion. If this result stands, there can be no doubt about it: there is reversion through the pure recessive. From this result to Darwin's wild pigeon there is only a short step, — and the whole subject will be back again where you and Darwin put it, cleared from the muddle of "gametic purity."

f489v

I have done this kind of thing before paying attention to possible mis prints, &c., because such sources of error will not affect more than ½ % of the entries, at the outside, and I do not think that ½ % will matter. The racing colours are given in the Racing Calendar for those foals which race: but when these differ from the Stud-book colors, what is one to do? Mr Weatherly, the son of the man who founded the Stud book, is in charge at the Jockey Club record office. He is an Oxford man, and made his people show me the whole thing. It is done so carefully, by such a staff of earnest, well-educated men, that extensive mis prints are quite out of the question. Error must arise, when there is error, from false judgment of young foals by breeders; and I don't see how to deal with that. — Only, if the sort of result I have shown you is really the truth, one can neglect this narrow margin. — It could not affect these correlations seriously!

My own belief is that the foals change sometimes, from change of dominance during growth, as your friend the Boar has changed, & will change. — Has he quite lost his stripes yet?

I am sorry you are still so cold. — We oscillate between sharp frosts and warm, summer-like days in a rather trying way. — Last week we had a dia grammatic little hurricane, with thunder, lightning, and such hail as I have never seen before. It came up the Thames Valley: it was only a few miles wide. The wind unroofed houses, the lightning killed horses & cattle and at least one man. It took about 15 minutes to pass this house, where it only blew one window in: but it was at its worst over & on the valley.

You like to get near a great personality. — Have you ever read Napoleon's autobiography? — I read a little every day, when I can't read any more stud-book. — It is the most fascinating book I have read for years. If you read Wellington & Napier down there, read Napoleon himself between whiles. You will enjoy the contrasts.

Yours very truly
W. F. R. Weldon

Please do not trouble to return the pedigrees, or any other figures you will let me send you. — I always copy things before trusting them to the post.

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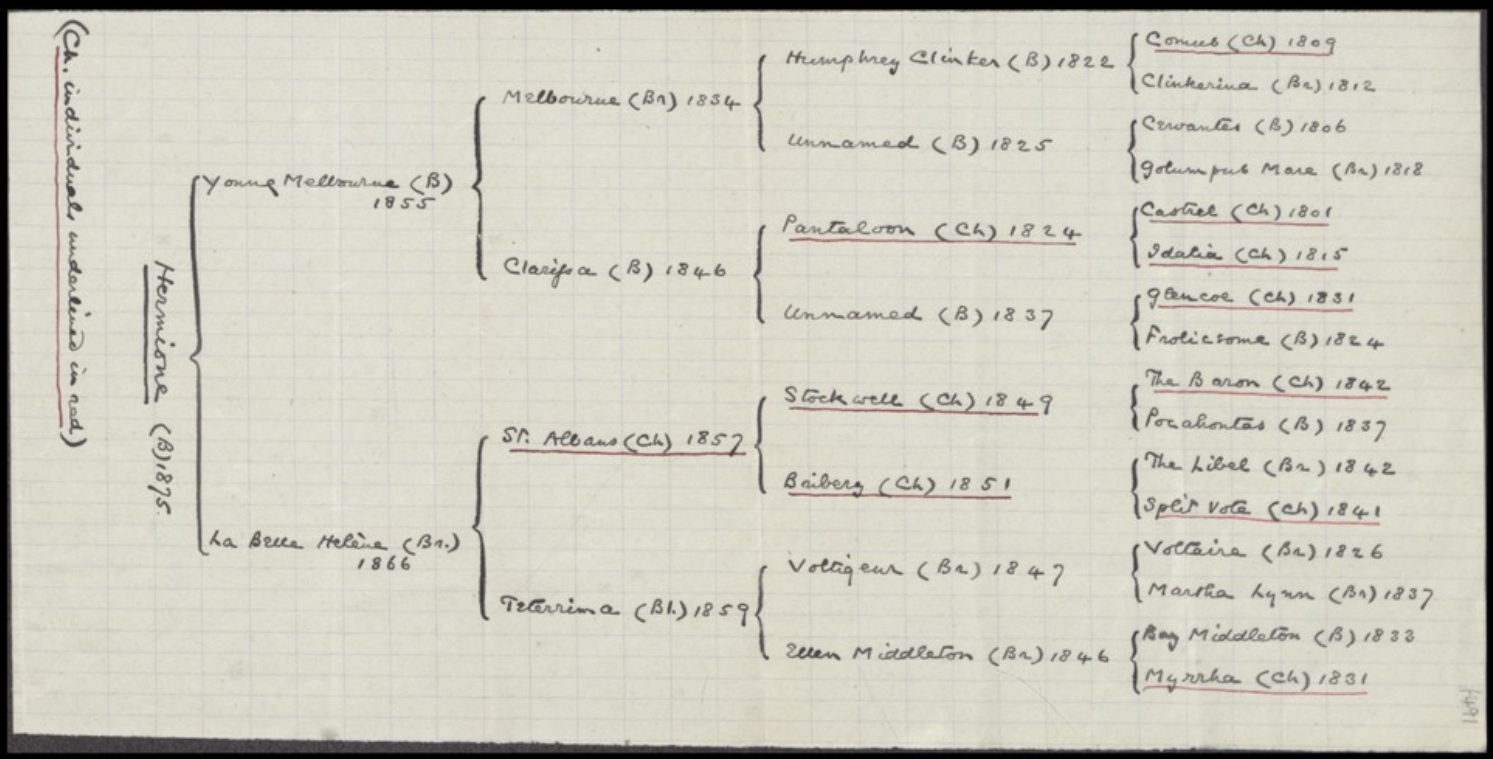
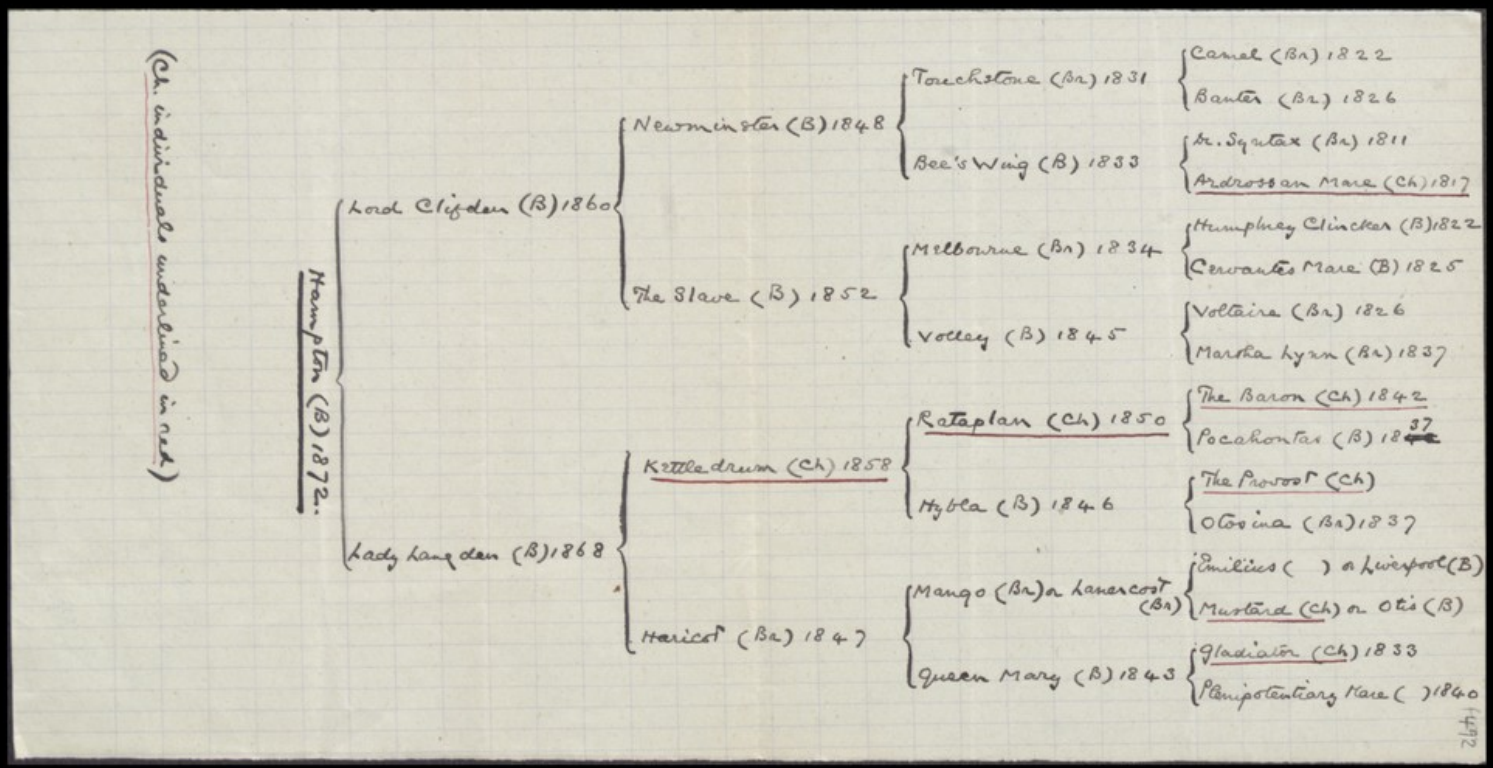
GALTON PAPERS


(Ch. individuals underlined in red)
Hermione (B) 1875
Yonne Melbourne (B) 1855
La Belle Hélène (Br.) 1866
Melbourne (Br) 1834
Clarissa (B) 1846
St. Albans (Ch) 1857
Tetterina (Bl.) 1859
Humphrey Clinker (B) 1822
Unnamed (B) 1825
Pantaloon (Ch) 1824
Unnamed (B) 1837
Stockwell (Ch) 1849
Baiberg (Ch) 1851
Voltigeur (Br) 1847
Ellen Middleton (Br) 1846
Comus (Ch) 1809
Clinkerina (Br) 1812
Cervantes (B) 1806
Columpus Mare (Br) 1818
Castrel (Ch) 1801
Idalia (Ch) 1815
Glencoe (Ch) 1831
Frolicsome (B) 1824
The Baron (Ch) 1842
Pocahontas (B) 1837
The Libel (Br) 1842
Split Vote (Ch) 1841
Voltaire (Br) 1826
Martha Lynn (Br) 1837
Ray Middleton (B) 1833
Myrrha (Ch) 1831


(Ch. individuals underlined in red.)
Hampton (B) 1872.
Lord Clifden (B) 1860
Newminster (B) 1848
Touchstone (Br) 1831
Camel (Br) 1822
Banter (Br) 1826
Bee's Wing (B) 1833
Br. Squatax (Br) 1811
Androssan Mare (Ch) 1817
The Slave (B) 1852
Melbourne (Br) 1834
Humphrey Clinker (B) 1822
Cervantes Mare (B) 1825
Volley (B) 1845
Voltaire (Br) 1826
Martha Lynn (Br) 1837
Lady Langden (B) 1868
Kettledrum (Ch) 1858
Rataplan (Ch) 1850
The Baron (Ch) 1842
Pocahontas (B) 1837
Hybla (B) 1846
The Provost (Ch)
Octavia (Br) 1837
Haricot (Br) 1847
Mango (Br) a Lanercost (Br)
Emilius ( ) a Liverpool (B)
Mustard (Ch) a Otis (B)
Queen Mary (B) 1843
Gladiator (Ch) 1833
Plenipotentiary Mare ( ) 1840

(Ch. individuals underlined in red.)

St. Angela (B) 1865.

- King Tom (B) 1851
  - Hark away (Ch) 1834
    - Economist (B) 1825
      - Whisker (B) 1812
      - Florantha (B) 1818
    - Unnamed (Ch) 1823
      - Nabocklish (Ch) 1810
      - Mip Tooley (B) 1808
  - Pocahontas (B) 1837
    - Glencoe (Ch) 1831
      - Sultan (B) 1816
      - Trampoline (Ch) 1825
    - Marpessa (B) 1830
      - Muley (B) 1810
      - Clara (B) 1824
- Adaline (B) 1851
  - Ion (Br) 1835
    - Cain (B) 1822
      - Paulowitz (Br) 1813
      - Paynator Mara ( ) 1810
    - Margaret (Br) 1831
      - Edmund (B) 1824
      - Medora (Ch) 1811
  - Little Fairy (Ch) 1841
    - Hornbea (Ch) 1832
      - Velocipede (Ch) 1825
      - Cerberus Mara (B) 1820
    - Lacerta (B) 1816
      - Zodiac (Ch) 1801
      - Torboa (B) 1803

(Ch. individuals underlined in red.)

Galopin (B) 1872.

- Vedetta (Br) 1854
  - Voltigeur (Br) 1847
    - Voltaire (Br.) 1826
      - Black Cock (B) 1814
      - Phantom Mare (B) 1816
    - Martha Lynn (Br) 1837
      - Mulatto (B) 1823
      - Leda (B) 1824
  - Mrs. Ridgeway (B. or Ro.) 1849
    - Birdcatcher (Ch) 1833
      - Sir Hercules (Bl.) 1826
      - Guiccioli (Ch) 1823
    - Nun Darrell (gr) 1844
      - Inheritor (Bl.) 1831
      - Nell (gr) 1831
- Flying Duchess (B) 1853
  - Flying Dutchman (Br.) 1846
    - Bay Middleton (B) 1833
      - Sultan (B) 1816
      - Cobweb (B) 1821
    - Barbelle (B) 1836
      - Sandbeck (B) 1818
      - Dariolette (Br) 1822
  - Meropa (B) 1841
    - Voltaire (Br.) 1826
      - Black Cock (B) 1814
      - Phantom Mare (B) 1816
    - Unnamed ( ) 1817
      - Juniper (Ch) 1805
      - Sneezer Mare (Br. or Bl.) 1810 or 1811

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Weldon

f495r

28 February 1906

MERTON LEA,
OXFORD.

Dear Mr. Galton,

Miss Biggs has told me how narrowly you escaped being involved in a terrible accident. The loss of your things will be a great nuisance to you, but I am heartily glad to hear you were not in this unlucky hotel yourselves! — I hope you have only lost clothes and such things, which may be replaced after some trouble, — not papers which contain results of your work?

After doubling the number of foals examined, I think my results will stand: but I must have explained myself even worse than usual, or you would not be surprised at what I find.

For horses in general, Pearson has already found that the parental, grand-parental, and great-grand-parental correlations are about $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, as they should be. — It did not seem worth while to repeat his work, which could well stand without help from me. — What I have tried to do, is to see how the correlations between special arrays of foals and their ancestors are

modified by such selection of parents as occurs in Mendelian experiments.

Call the 1, 2, 3 the filial, parental, and grand-parental generations.

For horses in general, you have

$$r_{12} = r_{23} = \tfrac{1}{2}$$

$$r_{13} = \tfrac{1}{4}$$

Now if you fix the character of parents, as I do by taking always a Ch. mare and a B. horse, Pearson's formula for ρ_{13}, the correlation between the array of foals from such unions and their grand-parents, is

$$\rho_{13} = \frac{r_{13} - r_{12} r_{23}}{\sqrt{(1 - r_{12}^2)(1 - r_{13}^2)}}$$

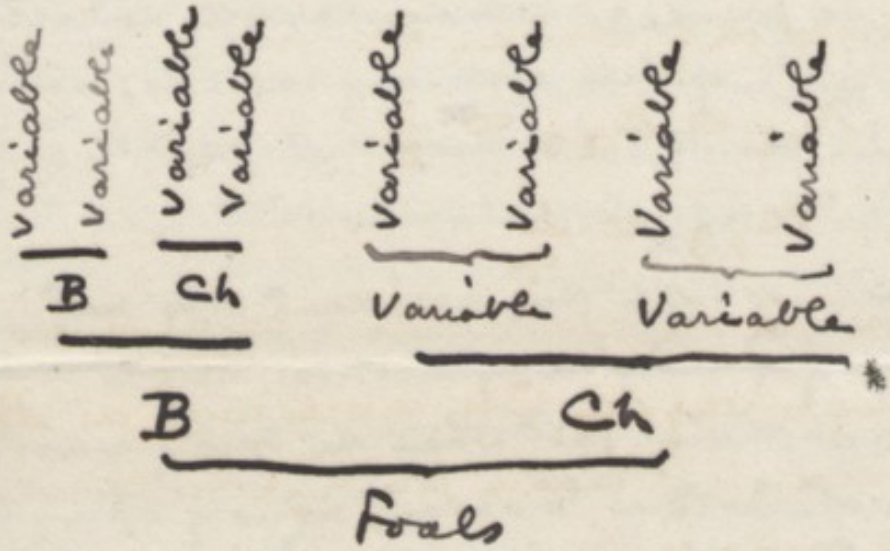
This might be greater or less than r_{13}, with particular values of r_{12}, r_{23}; but taking your values, it becomes

$$\frac{\frac{1}{4} - \frac{1}{2} \times \frac{1}{2}}{1 - \frac{1}{4}} = 0$$

F496r

So that my result is only a direct arithmetical consequence of your laws, — so far as zero grand-parental correlation goes.

For great-grandparents, Pearson's formula becomes very complicated, and involves the 9.9. parental correlations. You see I have always fixed both parents, and the paternal grand-parents: so that my pedigrees are



and it is quite possible that with such highly artificial grouping the apparent correlation between great grand parents and ~~his~~ the filial generation may be spuriously increased. — Considering the great complication of Pearson's formula, and the way in which small changes in the value of quantities like $r_{1.4}$ would affect it, I do not think it is worth while to use the formula yet. I think it is wiser to get the result of observation settled, and then to show, as no doubt one will

F496V

be able to show, that when Pearson's formula is made to give the observed result, it does so by giving to R_{14} the value you would assign to it.

If this comes to pass, — and it looks as if it would, — then the want of correlation between filials and grandparentals generations, which is the reason why Bateson and Co. scoff at "ancestry", will be shown to be a necessary consequence of the Law of Ancestral Inheritance, and the explanation of their other results will be found in a more remote ancestry, outside the range of their ordinary records; while their "gametic purity" will drop once for all into the category of "terminological inexactitudes."

I sincerely hope this accident will not cause you very great inconvenience, and so spoil the rest of your holiday, just when the Spring must be coming to us at last. — There are signs of Spring even here today. The first daffodil in our little garden is nearly in flower, and the sun shines as it has not shone for a long time.

Yours very truly

W. F. R. Weldon

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Welton

H971

MERTON LEA,
OXFORD.

28 February 1906

Dear Nip Biggs,

Indeed I congratulate you both on your escape. — Tomorrow I shall try to be sorry for your frocks: but today I can only be glad for yourself.

Yet even today I can be very sorry for the way in which this horrible accident will change your plans and spoil your holiday just as the Spring is coming to our country in earnest. — Today is the first day for a long time that you have been labelled "Fine" in the Times.

Have you gathered much from your man about Bonaparte and Joséphine? — I have had a shock. — I had always thought her rather dull: the de Beauharnais interested me as little as the other ci-devants who tried to use the Revolution for their own ends: but I knew nothing of her. Napoleon himself says nothing: but that in asking her to marry him he was "actuated by the feelings which actuate any

honest man in such a position": that seems to me all he ought to say.

But for my shame I have read again the Mémoires of Mme. de Rémusat; and as usual, when one reads [illegible], I have suffered.

I always liked to think of the saintly Joséphine, adoring Napoleon, who is described by Seeley and the English historians who like to belittle our national foe. But the Lady Mme Rémusat describes is a sort of second-rate Mme. Tanqueray, for whom no decent man could care.

I am glad, in a way, because I feel I can worship Napoleon himself even more than before: and yet I am sorry, because I have looked at a painful story which does not concern me, and which I have no business to have learnt. — I feel just as I

f498

did when I read Froude's Carlyle, and swore I would never again read gossip about any one.

Get some new books, and do not read Mr. de Rémusat.

Yours very truly

W. F. R. Weldon.

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GALTON PAPERS
3/3/22/18

Tregerthen
St ~~Ives~~ Ives
Cornwall

20 Sep: 1909

My dear Galton,

Mrs Spencer Butler has shown me your letter, in which you write of rockingstones, or ~~logan~~ logan rocks, as they are more commonly called in Cornwall to log being to rock. They occur in great plenty wherever the hills are composed of coarse granite, every hill having several on it, and I will not say that some may not be found on the finer-grained granite which composes some of this country. When the fine point on which the logan rock is balanced gets worn away, the rock settles down on a broader base and ceases to log. On the other hand, when the base of a rock gets worn to a fine point, without its balance being so far disturbed as to cause the fall of the rock, it begins to log though it did not do so before. I have several times noticed rocks logging which, though they were familiar to me, I had not observed to log before. The converse ~~case~~ I do not remember to

Westlake

have occurred within my personal experience, but there are certainly rocks known as logan rocks which now require so great an exertion to move them that I think their points of support must have got worn less fine than they were within memory not very long past.

The facts are, I think, matter of common knowledge in Cornwall, although one is sometimes led to regard as common knowledge what has been long known to him. As to the cause of the facts, they are attributed to what we call the "weathering" of the stone, and that again is attributed to rain and frost. Our granite moors are so well covered with turf, heath and furse (= Gorse) that there is not now much opportunity for the wind to erode by driving sand against the rocks. But ~~[illegible]~~ geologists have pointed out that this was a powerful cause of rock erosion in times when the land was bare.

Sir Arthur Evans's ~~[illegible]~~ utterance on the subject

and this experiment, which you mention, was
never before known to me, and I was equally
unaware of any suggestion that ants may
have played a part in the production and
destruction of Rock-mortars. But the suggestion
that acids, arising from the decay of minute
vegetation, have been the principal agents
in the erosion of the rock-basins which
the older antiquaries used to attribute
to the Druids, has been made to me by
the Rev. W. K. Andrews, F.G.S. my brother-
in-law, with whom I have been in the habit
of walking over our granite moors for a
great part of my life. That the rock-basins
are not artificial is proved by their occurring
indiscriminately in the coarser granite, although
hills of the fine granite are capped by
rocks which would have presented equal
artificial attractions. At least, such is
sometimes the case in this neighbourhood, and I think that it is so also on
Dartmoor. The explanation of the basins
commonly given by those who have never
seen the Druids is that they were enlarged
and moulded to the regular forms in which
they are often found, by the wind rotating
the crystals which first got detached in

slight natural cavities. This is to me incredible, because the water and fragments in the basins may be seen to be quite still even in very high winds, so soon are they protected by the sinking of the floor. Also Mr Andrews measured carefully a great number of the rock-basins in this neighbourhood, but was unable to discover that the directions of their major axes had any relation to that of the prevailing winds or to any thing else. He then struck out the idea (at least, if others have had the idea, it was unknown to us) that, as soon as the cavities had become deep enough for water to settle in them, microscopic vegetation ~~decaying~~ decaying in the water set free acids the action of which on the rock carried the erosion further. This may be so, and a similar cause may have cooperated with rain and frost in balancing loose arrocks and in turn dislodging them. But I await some proof of the existence of the vegetation and the acids before I can count it as a vera causa.

Believe me to be yours sincerely

J. Westlake

Rutland Gate crossings

The Board of Works for the Westminster District.

CAXTON ST.
VICTORIA ST.
S.W.

Town Hall, Westminster, S.W.

13th January, 1887.

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PAPERS
25/3/22/12

Sir,

I beg to acknowledge the receipt of your letter of yesterday's date as to the crossings at Rutland Gate, and, in reply, to inform you that the Surveyor, to whom the matter has been referred, has ordered that crossings ~~to~~ be laid as suggested by you.

I am, Sir,
Your obedient Servant,

J. E. Smith.
Clerk to the Board

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

TELEPHONE No. 5263 GERRARD.

Guardians of the Poor of the Westminster Union.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
22/2

Fortnightly Meeting Friday at 4.30 p.m.
Next Meeting ____ day of ____

William Lee,
Clerk to the Guardians
and Superintendent Registrar.

OFFICES:
Poland Street, Oxford Street, W.

All communications are
to be addressed to
The Clerk to the Guardians.

16th May 1905.

Sir,

Referring to your letter of the 14th instant I herewith enclose a brief list of children with some particulars as to their parents. If you wish to interview those in the workhouse this could be easily arranged.

You are at liberty to inspect any of the books and records in this office and should you require any further information I shall be very glad to supply it upon hearing from you.

Your obedient Servant

W. Lee

Clerk.

Francis Galton Esq
42 Rutland Gate
S.W.

Westminster Union

Name of child changeable	At which establishment	Parents address and occupation
Brougham Adelaide	St Mary's Walthamstow	mother 20 Gloucester St Bloomsbury a Charwoman
Sharp Ada	"	mother 19 St Annes Court W. a Charwoman
Warner Mary	"	mother in Workhouse, a Servant
Lelevre Louise	"	Father ditto a Tailor
Fleming Catherine " Elizth	"	mother 4 Marshall Street, a Charwoman
Crowley Elizth	"	" 12 Clarendon Square Somers Town a Tailoress
Coffield Fanny	"	Father in Workhouse, a Labourer
Light Henry	Southend	mother St Pelagias Limehouse a Laundrywoman
McAllister D " Hy " Bruce	" " "	mother 10 Broderick Road Tooting a Genl. Servant
Hayes Arthur	Mill Hill	Father in Workhouse a Stonemason
White George " Walter	"	Father 13 Poland Street a Carpenter
Sullivan Annie	Salisbury	mother 8 Greens Court a Charwoman
Haunstler Maud " Hy	Margate	do 26 Delhi Street Kings Cross. a Tailoress
Hubbard Fredk " John	Bonner Road	mother 11 Noel Street a Scrubber at the Workhouse
Haskey Margt	Ilford	mother in Workhouse a Clerk.
Small Violet	"	" 6 Angostic Place a Charwoman
Hawkins Julia " Theresa	Ilkley	Father in Workhouse Blind a Sailor
Shave David	Exmouth	mother in Workhouse a Genl Servant.
Lygo Agnes	Hillingdon	Father in Workhouse a Journeyman Butcher
Walters Ethel	Greenwich	" in Workhouse a Journeyman Grocer
Kook Auguste " Ida " Alfred	Hatfield	mother at Hanwell Lunatic Asylum. a Charwoman
Bissett Ethel " Rose	Standon	mother 121 Wardour Street a Charwoman

H. T. Wharton 41

39. St. George's-road,
Kilburn. N.W.
March 24. 1888.

Dear Sir—

In "Nature" for December the 1st, 1887, p. 112, I read a note enquiring whether sufficient interest really exists to justify a serious attempt to found an Anthropometric Laboratory and Family Record Office.

Pray allow me, for one, to assure you that such an Establishment would be a great boon, in my opinion, and one of which I should regularly

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regularly avail myself.

At the "Healtheries" moreover, the crowds always waiting to be measured must have shown much of the interest of the public, but their very vastness made many go Empty away on account of the weariness of waiting their turn.

Although you know this, of course, you may not further know that there is, or used to be, a tobacconist (named Ward, I think) in the Burlington Arcade — on the left hand, a few doors

from Piccadilly — who for years has kept a weighing-chair, and a book in which his customers (his charge was sixpence) registered their weight from time to time.

I shall be exceedingly pleased if your proposed foundation takes place. Besides the interest of the observations, if you had an accurate test of the focal length of each eye, you would save many people much trouble and expense in helping them to find the right strength of the spectacles which they ought to wear — a matter now chiefly in

the hands of unscientific opticians
— save in the case of the wise or the
wealthy.

Will the highest Esteem. believe me,
Dear Sir, Very faithfully yours,
Henry T. Wharton.

(M. A. Oxon., Sussex.)

Francis Galton, Esq., F.R.S.,
&c. &c. &c.

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Wharton

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UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
3/3/22/22

Hydrographic Department,

8 Oct/88

Dear Mr Galton

I owe ya many thanks
for ya letter, pray accept them. It
will give me an excellent starting
point in trying to manage matters

Yours truly
W J L Wharton

F1

6.XI.08

PARCELS, CLAVERDON, G.W.R.
STRATFORD ON AVON BRANCH.
TELEGRAMS, CLAVERDON.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
3/3/22/23

Claverdon Leys,
Warwick.

I have only seen one Wolfschild
near Nuneaton about the year 1901
age about 9 or 10, rather hairy on
body, average height, wrinkly and
hard knees and elbows, idiotic and
wild expression, shifty eyes —
seemed very timid and usually sat
shrunk up in the corner of his cage —
occasionally made as if he would
spring out showing his teeth

with a guttural sound —

He was exhibited by natives of India, who said they had caught him in the jungles — I should doubt them treating him kindly — They told me that they fed him on unleaven bread and vegetables (no meat) Water and milk occasionally to

f3

drink — I was told he lived for a few months only —

Have heard of 2 or 3 other wolf children during my 30 years in India, but have not seen them — I was told they only lived a few months after capture, but this may be due to mismanagement and injudicious

Feeding —

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

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GALTON PAPERS
3/3/22/24
344/7

9 Octr,

FT 114

1905

My dear Edward

I send you enclosed very much what you wrote only I dont mention Pruet. You can do that in your will & I dont want to hamper you,

Fanny left me suddenly on Saty, she got a letter begging her to come home for her little dog was ill & they did not know what to do. The Dog was her Mother's great pet & Fanny's constant Companion so I begged her to

go at once. The Dog knew her & tried to get to her. The Doctor said nothing could be done. It died soon after.

I have sent a cheque to Fred Powlett. The Lady has not a penny at present. I had a nice letter from Horatio who says the Wedding was hurried on by Mrs Chaplin! & that the Armands feel great anxiety about their future. Frances talks of coming down for a day soon. I am getting quite clever with my chair & get about very well.

Lucey talks of coming soon
for a few days,
What I want to impress upon
you all is, that I am always
delighted to see you, but that
my health & spirits do not
require you. I am never
dull, I can always employ
myself, so dont think I
require some one with me,
Mrs Maids are most careful
of me so I am quite comfortable
My dear love to you both
Ever yr. Affectt. Mother
E A Wheeler

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Alnwick

TELEGRAPH OFFICE, MARCAM.

TWYN-YR-HŶDD,
MARCAM,
PORT TALBOT.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
3/3/22/25

May 17th
1903

Dear Uncle Frank,

Are you interested in
"telegony" as a matter
affecting heredity?

I know it is altogether
denied by scientists, & I
know nothing about it,
but have just come across
what appears to be a bona

fide case. My facts are not
quite accurate, but I have
been promised them in detail.

A friend of J. Aspinall's, (a
very old friend of mine), living
near Bath, tells me that his
Curly Retriever bitch, which
black
he showed me, was bitten some
8 years ago by a large White
Borzoi dog which died directly
after the fernie by an accident.

The bitch had two or three more
- by a retriever dog
litters, dropping pure bred
retrievers. She was then bred
by one of her own sons (a pure
retriever) & had several
pure black puppies, & one
white one, having the
characteristics of the Borzoi
dog, & some of his habits in
marked degree. She was kept
during autumn
inside both & Key, & there
was no dog at all resembling
the ~~Dogs~~ Borzoi around
about. The owner of the

f4

bitch is a man named Heathcott, who married Lady Eva Campbell, & he assured me he had no doubt of the facts. He has promised to lend me photos. of the Borzoi & of the white pup, which also died.

All the rest were pure bred black retrievers, a breed I have never known to throw a white pup, though browns are or were common. He wrote to Tegetmeier of the Field newspaper at the time, but no notice was taken of his letter. Ever yours affect S. G. Whele

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
T/3/22/26

Upper Blackwood
Western Australia
22 June 1909

Francis Galton Esq. F.R.S. &c.

Sir,

I believe you are interested in "wideness of heredity in animals" — Having been in charge or possession of live stock for 25 years, I have noticed instances of what seem to me to be hereditarily acquired habits, & also of reflection, or purposeful action. Re the first — In this part of W.A. the bush fires destroy the surface feed every three years or so, but where "Blackboys" "Zanthorrhoea" are plentiful, they provide food for local bred stock. No fire will burn the whole top of the B. boy, but only the greater part, always leaving the upright "grass" growing from the centre or top of the crown. This singeing causes the sugar contained in the core of the stem to rise, making the lower part of the upright grass in centre of crown very sweet, & a little easier to pull out, & also makes it ~~grow~~ start into fresh growth. The long flower stalk also shoots up the following spring but does not do so except after a bush fire. Horses sheep & cattle locally bred from flocks & herds long established in "blackboy" country, know exactly how to siege hold of the centres & pull them out, so as

to get the sweet soft base, but those horses & cattle bred in other states or in districts when there are no Bl. bags will eat the tops quite close, but never learn to pull them properly. Sheep do not learn so much. Sheep in those flocks long established here know how to rear up on their hind legs, and put their heads into the grass untill they get hold of the centre & pull it out, & can fold their fore legs & balance on their hind ones in order to reach one quite five feet high. They will thus keep themselves in good condition in a burnt paddock until new herbage grows. Imported sheep, even if put into the same paddock with them, will not learn from them and will starve if not hand fed, although their locally-bred companions are doing it hourly before their eyes. A neighbour imported bought some fine South Australian ewes, & put them into his paddock with a few local ones. The former would not learn from their mates how to pull or even to eat the Bl.-Bags, but starved, & had to be fed on chaff, while the latter ate the B-bags & throve. There are always plenty of quite low B-bags among the tall ones, but strange sheep do not learn to eat them either. Sheep have been bred here for about 50 years. Many common goats were kept here in the early days, & the sheep ~~they may~~ may have learned from them in the first instance, ~~etc~~. Sheep & cattle will rear up to pull down and eat

the flower stalks of which grow ~~out~~ of the top of the B. bay & all stock will eat them, as they are very sweet, but only local stock will rear up to get at the high ones. I have a number of local bred cattle, and have seen a cow which was 4 years old rear up on its hind feet, with fore ~~feet~~ legs folded down like a dog "sitting up" and seize & break off ~~[illegible]~~ ~~[illegible]~~ the flower stalk of a tall blackbay, I rode up, & found that the crown of the Bl. bay was the same height as my shoulder when sitting on horse and that the stalk had been broken through about 6 in higher, and the cow's saliva was on the stump. I have seen a 4 yr old steer do the same. I have a jersey cow bred in Victoria which bites the tops of Bl. bays quite close down, but does not know how to "pull" them as local bred cattle do. I am informed by a bushman & stockman bred in this district that horses from other parts never learn to pull the Bl. bays properly as the local ones do, although they learn to eat them close - as I have noticed is the case with my own, both local & imported. These observations seem to me to prove that an acquired habit or knowledge may become hereditary.

Re. Purposeful action - In the 1891 drought in the Roebourne (W.A.) district, most of the water-holes in the rivers & creeks dried up, but often water remained for a time a few inches below the surface

of the sand. Many horses as well as multitudes of other stock died - I was informed that a certain horse on a station I was on, saved his own life & the lives of his "mob" by pawing the sand until he had made a hole down to water level & so ~~kept~~ all kept alive until they were found & shifted to a place where water was still on the surface. I had that horse & others out on a journey & camped for night after a hard days ride at a place where the water level was one foot below the surface of the sand in the bed of the creek, and I saw that horse paw the sand & smell it, plainly trying for water, but we gave him & the others some in the damper dish, so I did not actually see him dig a hole & drink - but I was afterwards told by the manager of the station during the drought, that he had done so, as above.

a gelding

A few years ago I bought an old gelding & put him in a paddock with the others, & they were very hostile to him. At one place a big log had fallen at an angle with the fence, & he & a mare were feeding near it - My brother & I, standing on a hill above them. (it was cleared ground) noticed that the mare kept biting at the old horse, & we stood ~~stood~~ to watch. She kept feeding & biting at him at intervals & edging him towards the triangle formed by the log. As soon as she had got him

f5

fairly into it, she whipped round, & lashed several ~~[illegible]~~ kicks into him most viciously & he had to take a standing leap over the log to get out of the way. We have no doubt that the man deliberately planned the trick, as we could see her driving him closer & closer to the place. I trust these observations may interest you. If you are bringing out a new edition of your "Art of Travel" I could send you a few things which I have learnt & which you might think useful enough to put into it. Am sending this through the Secretary of "The Royal Society" as I do not know your address

I am Sir
Yours faithfully
John M. Whistler

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

Brancaster
Upper Blackwood
Western Australia
14 Sept 1909.

Wheeler
Sept 1909

Dear Sir

In reply to yours of 3rd Aug. asking if I can obtain information as to lambs born of ewes (which know how to eat Blackboy tops) in paddocks where there are no Blackboys –

I fear this will be rather difficult, as there are very few "cleared" paddocks in this district, if any. Paddocks having plenty of blackboys are regarded as a useful resource against each season's bush-fires

I will, of course, keep a look out

If the facts stated in my last, do not prove "heredity" of the habit, it seems to me to show that imitation is only possible to young animals

Out of 80 cattle which I had in a paddock here, 79 would only eat blackboys which had been singed, but one cow ate them all the year round & so kept in better condition than the others. The one which reared up to get at tall ones, also

kept in better condition than the others.
These would be a starting points for "natural selection"?
Cattle bred in poison country will feed among poison plants, & very few die; strangers eat & die very soon. There was a strong patch of "Heart-leaf" poison at the watering place at Boyaneys where I used to live - A small herd, local bred, belonging to a neighbour, used to feed there, and none were lost. The owner's daughter bought two cows from another district to run with the others, and these both got poisoned in a few weeks. - This may be of interest to you

I remain Sir
Yours faithfully
John W. N. Whistler

Sir F. Galton F.R.S.

Whetnall

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UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
3/3/22/23

18 Park Place
Cardiff
24th. May. 90

Dear Sir, (in a Card from Mentone)
In April 1889 — I/ you
kindly informed me that
your new Instrument" for
testing delicacy of perception
of difference of tint" was described
in "Journal of Anthropological
Institute" for April 1889.
I have not yet been able
to see this Journal, as I
live in a part of the country where
reference to scientific papers is
far from easy.
I find on enquiring that the
price is 5/-

This is rather a large sum to pay for seeing a single paper. I therefore write to ask you if it is possible to buy the paper separately. Otherwise I fear I must wait till I am in London & able to visit some library where the Journal is kept.

I suppose the investment is not described in Nature, or in Brit. Assoc. Reports, & or in Phil Mags. I could refer to them here.

I am engaged in a work
on Colour & hence am
wish to see anything bearing
on the subject.
To day I have received an
interesting letter from Sir G. Stokes
in reference to the effect of
Steam upon ~~sunlight~~ light.

Yours very faithfully
Chas. T. Whitmell.

F. Galton Esq. FRS

P.S. Perhaps I may mention
that Capt. Abney has helped me a
good deal. I mention these
names merely to remove the
hesitation I feel as a stranger in again
troubling you.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

F47-11

18, Park Place,
Cardiff.

27 May 1890

Dear Sir, I beg to thank
you very much for
your letter with its clear
and interesting account of
your apparatus for testing
"hint-sensitiveness".
If I have the opportunity I
shall gladly avail myself
of your kind invitation; and
shall visit your Laboratory.
I was one of those tested at
it, when it formed part
of the Exhibition - the Coincidences -
I think

I must try & cancel
your original article for
the results, unless they
are embodied in any
reports on your Anthropological
Labs.
Lord Rayleigh's experiments upon
matching the compound yellow
are very interesting, and are
doubtless well known to you.
But since 1881, see 'Nature' 17 Nov 81.

Again thanking you for the
great trouble you have taken.

Yours faithfully
C. Wheatstone

F. Galton Esq. FRS &c &c

Whyte

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UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
3/22/28

20 Christchurch Road
Hampstead. N.W.
17 Nov 1908

Dear Sir.

I have just been reading your volume of "Memories" with intense interest and enjoyment, and with no little satisfaction in the thought that in my capacity as literary adviser to Methuen & Co I had something to do with its production. Evidently from your opening paragraph my letter to you arrived at a very opportune moment!

W Methuen sent me a copy of the book on the

day of issue, but I postponed
reading it while I could set
aside two entire evenings
for the purpose. Except
for certain pages which
are rather beyond my grasp
owing to my lamentable
ignorance of science, I read
it through yesterday and
today, and I am now
venturing to write to you
not merely to express the
pleasure I have derived
from it but more particularly
to make a suggestion.

Among the most interesting
pages in the book are
those in which you give

your impressions of Lord Brougham, Professor Tyndall and Carlyle, and of other famous men whom you have met, but you could naturally find room in such a volume for only a few pen-portraits of the kind — only for those, in fact, which were strictly relevant and almost essential to your narrative.

Would it not be an interesting, and not too laborious, task for you to undertake an elaborate series of such analytical

studies from memory of all the other really memorable men & women whom you have met? Such a volume would be a veritable treasure-house to future historians & biographers, and there is no one living who has anything like your qualifications for undertaking it. How few people, for instance, have given any thought or study to physiognomy! One would like to hear more from you about the personal appearance of Tyndall & Carlyle & Browning & Stanley & all those

others. One would like
also to see you trace the
comparative effects of
"Nature and Nurture" in
such cases as these.

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Box 2

I am nurturing to embrace
his idea to you on the spur
of the moment. I have
not discussed it with
Dr Mahaun. I am quite
certain however that he
would agree with me
that such a volume from
your pen would be of
extraordinary interest
and value.

Yours faithfully
Frederic Wyatt

Francis Galton Esq
F.R.S.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

f7

12 Summer Place
London S.W.
16 Sep.

Dear Sir

Having read recently with interest Dr Alfred Russel Wallace's plea for the serious study of Phrenology in his book "The Wonderful Century", I have induced the Editor of The Daily Graphic to open the columns of that journal to a discussion of the subject, not merely by men of science but also by other writers and observers whose views will be of interest and value. Among the first contributors, for instance, will be my friends Dr C. W. Saleeby and Mr M. H. Spielmann.

As Dr Oliver Wendell Holmes says in "The Professor at the Breakfast-Table", sensible people are agreed that there is Something in Phrenology.

The question at issue is How much?

I take it that whatever the intelligent "Phrenologists" may claim, Dr Wallace

II

does not pretend that in the studies of Gall and Combe and Spürzheim there are the foundations of an exact Science." On the other hand, his scientific opponents talk almost as though the study of a man's head and face were negligible as a means of noting his character.

Your opinion upon this subject would be particularly interesting and I shall be greatly obliged if you will favour me with it. The discussion may begin in the course of the next week.

Yours faithfully
Frederic Myers

Francis Galton Esq. FRS. &c &c.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

From the chapter on Phrenology
in Dr A Russel Wallace's book
"The Wonderful Century"

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GALTON
PAPERS

Let us now consider, briefly, what phrenology is, what is the evidence on which it is founded, and what are its practical results. In the first place, it is a purely inductive science founded, step by step on the observation and comparison of facts, confirmed and checked in every conceivable way, and subjected to the most rigid tests. By means of large collections of skulls, and casts of the heads of men and women remarkable for any mental faculty or propensity, and by observations and measurements of thousands of living persons, the correspondence of form with function was first suspected, then confirmed, and finally demonstrated by the comparison of the heads of individuals of every age, both in health and disease, and under the most varied conditions of education and environment. Three men of exceptional talents and acuteness of observation devoted their lives to the collection of these facts. They studied also the brain itself and discovered many details of its structure before unknown. They studied the skull, its varying thicknesses in different parts and at different ages as well as under the influence of disease; and it was only after making allowance for every source of uncertainty or error that they announced the possibility of determining character with a considerable amount of certainty and often with marvellous exactness. Surely this was a scientific method of procedure and the only sound method of ascertaining the relations that exist between the development of the brain and the mental faculties and powers.

N.B.

Dr Wallace, while feeling that his chapter should be read in its entirety for its exact position in the matter to be realized, suggests to me that if any one point from it should be put to the ordeal of controversy, the above extract could best be thus utilized: the question being added: Why is this not a truly scientific method of enquiry.

Everyone says this experiment must fail. It is as yet a splendid success — Well furnished as a Gentleman's club. no bawdy songs, floor clean, men polite. The first attempt of the kind in England. Don't spend more than £100. 415

THE WICK CLUB.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
23/22/29

WICK is a village in Gloucestershire, about six miles from the cities of Bristol and Bath: the number of people in the parish is nearly one thousand.

The first experiment of a Club was made in December, 1903, when two small rooms were opened. The object of the experiment was to determine, as closely as might be, whether a Club were wanted or not. As, during the first six months, 167 paid for entrance to and use of these two rooms, it was assumed that a Club would fulfil at least some useful purpose.

Thereon an additional room or hall—forty feet by twenty-five—was built. In this room are a good full-sized billiard table, a good piano, seemly pictures, and various furniture for various games. This room is also capable of being speedily arranged for concerts and public entertainments. Without touching on personality, which were abhorrent, it must be stated that this new room could not have been built but for a generous exchange of land.

The Club was opened on Thursday, the 21st September.

Should the first year's working show that these three rooms can be financially supported, they will, with the furniture, be granted to the village as a Club in perpetuity for the rent of £10 a year. It is to be noted that what (it is believed) are termed in public auctions "natural amenities" are also provided.

In order that the clergy of all denominations, and Tories, Conservatives, Liberals, Radicals, and the innumerable and diverse factions of Socialists may be all free, without offence, to support this experiment, it has been determined that the Club shall be associated with no political party, and shall be marked by no declared preference for any distinctive form of dogmatic religion. This self-denying ordinance is also necessary in order that the constitutional use of the provided schoolroom may not be interfered with.

There is the authority of the Duke of Devonshire and Lord Grey in support of the contention that it must be greatly to the benefit of any English village to possess, under its own management, a Club for reasonable amusements, and as a centre for social gatherings. And it is assumed that all who hear of the present experiment will give it sympathetic support.

But, unfortunately, money, though the root of all evil, is also the one thing necessary for the success of even the most righteous material enterprise, and—also unfortunately—the inhabitants of Wick are not, as a body, capitalists. Indeed, their aggregate income is smaller than the personal income of more than one of those to whom an appeal is now made. The subscription, therefore, that each can pay is small. This subscription has been fixed at three shillings for the year, or one shilling a quarter: even this sum may be prohibitive to some. The charge for a fifty game of billiards is twopence, for a hundred game fourpence: a game at billiards can be played by four. By this means a certain though comparatively small income will be assured, which there is definite hope will be supplemented by a few honorary members personally interested in the welfare of the village.

So, before the handing over to the village of the Club, with all its financial liabilities, can be reasonably entertained, there should certainly be some money in hand as a nest-egg. This is not required as security against rent, for all the charges of management will be met before rent is claimed; it is required in order to make the Club a financial success. Rent is made a charge under the belief that the necessity for such payment must increase the sense of responsibility in the members, and especially in their elected committee: a sense of responsibility which many in our country will agree it is advisable to encourage.

A general appeal for financial assistance is now made. Those who are of opinion that such a scheme as the present should be supported locally will not—and quite rightly—assist. But there is reason to believe there are many throughout our country who regard the establishment of village clubs as in great measure a national concern. There would appear to be no doubt that, during the last hundred years, though the country working-men have advanced greatly in physical comfort, their social life has grown more sombre and dull from the dying out of old forms of village amusement, so that now for reasonable, or unreasonable, distraction they have to crowd into towns.

That the people are desirous of finding amusement on the spot is suggested by the fact that since the commencement of the building of our new Club a scheme for a local band has been formulated, a glee club has been mooted, and it has been contemplated that Saturday evenings may be well filled with "sing-songs," tournaments, or interesting, not dry-as-dust, lectures. All this would have been impossible if the villagers were without a place belonging to themselves. A village schoolroom can be used for many and admirable purposes, but its use is necessarily restricted, and, too, the people cannot feel that sense of personal proprietorship in a provided or non-provided schoolroom which, some of us believe, is a necessary basis for moral and intellectual advance. In this present world, "romping towards self-government by the masses," there are not only some, but thousands of us who believe that the salvation of our country and empire lies in quickening the moral and intellectual character of our people.

If it be admitted that a village club is a good thing for a village then the present questions are: When a good clubhouse is handed over to the people of a village practically as their own property, and is left to their own independent management, can they manage it successfully? Will any moral and intellectual benefit inure to them from the sense of proprietorship and from the incumbent responsibilities? Will their lives be made happier and better, and residence in the country be rendered more attractive?

Those who think this experiment may be of value are asked to assist—with money; no contributions made in reply to this appeal will be credited towards rent, except as has been already stated.

Subscriptions or donations may be sent to the Treasurer, F. C. CONSTABLE, M.A., Wick Court, Near Bristol.

Donations—no matter how large—are asked for: honorary membership subscriptions of One Guinea are hoped for, and ordinary membership subscriptions of the ridiculously small sum of *Three Shillings* are clamoured for.

F.C.C.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
3/3/22/31

3 Gray's Inn Square
W.C.
June 3rd/87

Dear Sir

I was under the impression that you were going to bring up certain appliances to the schools: Mr Eve will get a ~~scale~~ weighing machine. Other things being equal to you Tuesday or Friday afternoon after 2.15 suits me best.

The Council unanimously accepted your kind offer with regard to the "Mental Fatigue". I hope to send you in a few days a fully revised copy with the words the Council wish inserted.

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Two of the members of Council have already sent me their forms filled up.

I am Sir

Yours faithfully

W H Widgery

Francis Galton Esqre

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
3/3/22/30

3 Gray's Inn Sq:
W.C.

Sep 22/87

Dear Sir

I should have written to you on Sunday had I not just receided your note. The fatigue forms I am happy to say have been filtering steadily in during the holidays and now number over 100.

I looked over the anthropometric apparatus and found that the mouth piece to the spirometer was broken inside the packing. I still have however the two pieces you gave me at first.

f5

P.S. I will bring the Fatigue soon with me to School.

UNIVERSITY COLLEGE
GALTON
PAPERS

I explained the instruments to one class this afternoon but we hadn't time to get to the actual measurements. At present I am in want of a tape band to make the head measurements and a foot rule to show inches and tenths to subtract from the height: if you actually possess either I should be glad but I am pretty sure I can borrow the first and could easily get the second made.

One small point: do you wish the Christian names in full and do they come after the surname?

I hope the colder air of England will soon free you from the Italian

fever. I shall be at school to-morrow (Friday) from 2.15 to 4 and am quite willing to meet you there at any time you please on Saturday: the boys will not be there then.

Has the date for your Kensington lectures been fixed yet? I am looking forward to them. The catalogue to the Guild Library has been finished: the days after the final proof had been sent to the printer 380 books were presented then! The last part of my holidays was peculiarly happy as I got engaged to a lady who was for some time an assistant mistress at the High School for Girls at Leamington.

I am Sir
Yours faithfully
W H Widgery

F. Fulton Esq

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GALTON PAPERS
2/3/22/31

(Wilder) 1902 11

ZOÖLOGICAL LABORATORY
SMITH COLLEGE
NORTHAMPTON, MASS.
U. S. America -

Francis Galton Esq. F.R.S.
~~South~~ Kensington Museum.

My dear Sir.

I have taken the liberty of sending you a recent paper of mine entitled "Palms and Soles" and which I have intended as a continuation of the subject in which you have for so long interested yourself. In this paper I have endeavored to express the feeling that any work on this subject can be no more than an extension of the principles suggested by Sir Wm Herschell and so thoroughly formulated by yourself, but I hope that in time I may make a not unworthy addition in this field.

I have at present under preparation a second paper which deals more minutely

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ZOÖLOGICAL LABORATORY
SMITH COLLEGE
NORTHAMPTON, MASS.

with the morphology of the separate lines and areas, as well as with the problems of hereditary and ethnic influence in the epidermic markings of the palms and soles; basing my work upon a fairly extensive collection of family and racial prints, to which I am adding constantly.

As it has been my endeavor to come in contact but nowhere to overlap the field of your own investigation, I hope that my labors will assist and interest you. I am glad that I here may take the opportunity of expressing my deep interest in and appreciation of your work.

Very respectfully Yours.

Harris H Wilder M.D.

Northampton Mass.
Oct 14. 1902.

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GALTON PAPERS

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ZOÖLOGICAL LABORATORY
SMITH COLLEGE
NORTHAMPTON, MASS.

November 1902

Francis Galton Esq. F.R.S.

Dear Sir.

I am sending you several papers, &c. which may be of more or less interest to you, including: (1) a Popular Science paper of mine upon palms and palms, giving a few suggestions concerning classification, although I have worked it out far more completely since that time; (2) a set of directions for printing, with photographs illustrating same- (3) a copy of "Pearsons Magazine", containing a very absurd story of "Finger-Tips", the point of which hangs upon your method — (This is a certain second rate magazine and I hope you will not consider it a fair type of American literature) — and (4), enclosed in this letter, a fragment of the margin of a play-bill with explanation written upon it —

I have also sent you a complete set of prints

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GALTON PAPERS

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ZOÖLOGICAL LABORATORY
SMITH COLLEGE
NORTHAMPTON, MASS.

which may be considered of average quality, though containing not of the best. As for finger-tips, you see that the field I have taken does not include them, as there is certainly no need of duplicating your work, and therefore it is only by chance that my palm and sole prints include them, and when they do, as they are not rolled, the triangular pads may be wanting. I send you, however, the set taken from a pair of twins, not identical, which may be of some value.

Should I ever get another good chance at identical twins I will try and obtain rolled prints of the patterns of both fingers and toe tips for you. You will see by my papers that my fundamental inquiry in all this has been dialogical, i.e. tracing the homologies between patterns & pads, studying the amount of

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GALTON PAPERS

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ZOÖLOGICAL LABORATORY
SMITH COLLEGE
NORTHAMPTON, MASS.

individual variation &c, but at the same time I am urging, as best I may, the value of these epidermic markings in recording individuals for purposes of establishing identity, only, it seems to me that the large and obvious markings of the palms and soles furnish a plainer and more practical set of characters than do the patterns of the finger tips, my "apical patterns" — Of course in this regard, the idea and the results have been wholly yours, as you have established the two necessary points (1) of individual variation, and (2) of the permanence of the markings - and the use of palms and soles similar to the finger tips is merely a secondary matter. I have a much longer and more detailed paper nearly completed, dealing with methods of formulation

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

Wilder 46

ZOÖLOGICAL LABORATORY
SMITH COLLEGE
NORTHAMPTON, MASS.

and classification of palms and ferns, and will certainly send you a copy as soon as issued – I am confident that scientifically in the civilized world the legally recognized method of identification will be by means of the marking of a part or the whole of the papillary volar surface of hands and feet, though whether the method employed deals with a portion or with the whole surface is a mere detail. Perhaps I might have some opportunity to collect information of interest to you, especially while working among the negroes and other non-caucasian races, although I do this only by proxy, and I could not always demand more than the regular returns from my assistants in the matter. Should you suggest, however, something which you would like, I will try and obtain it for you.

Yours very truly
Harris H. Wilder

Nov/15/1902.

3/3/22/31. 47

72 Dryads Green
Northampton, Mass.

Dec. 26. 1904.

Mr. Francis Galton.

My dear Sir.

I am glad you found the prints of interest. Regarding the racial value of the markings I am after all not very sanguine in spite of my paper. I think the best indication is my tabulation of 4 sets of foot prints of adults. 13 individuals in each set, in which the results were practically the same, and all unlike the same number of Maya Indians. One would naturally suppose, of course, that a large number would be necessary for any conclusion, yet in these four cases the small number gave a like result.

I shall let that matter rest for the present as I have many other things on hand, and am more interested in the work that Miss Wheeler is doing, and of which she has, I believe, given you some account. She is attempting, among other things, a

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Northampton, Mass.

morphological classification of the types of patterns, both apical and palmar (plantar), and is intending to send you a tabulation of her scheme as soon as she gets it into definite shape.

Regarding the paper of Féré, I know it quite well and have been looking it over since receiving your letter. As I see absolutely no indication of any connection between the various types of markings and mental calibre, the idea strikes me as too absurd to be seriously entertained and does not merit a careful, practical examination. In the first place, the simple arch is not primitive, and the somewhat similar form that exists in the Lemurs is due to a very specialized flattening out of the pads, the result of a special adaptation.*

On the other hand the simple arch represents

---

x. Cf Miss Whipples paper. p. 355.

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Northampton, Mass.

the last stage in the reduction and loss of the pattern, where the core has "degenerated through both triradii" as I believe Miss Whipple would put it, and every step in the process may be found and followed in various human hands.

Perhaps the statistical method such as you suggest, would appeal to many as necessary, but to me, as a comparative morphologist, it seems as though the entire story were clear as daylight.

At present the human toe patterns seem to us of especial interest, as they have assumed strange shapes and are spread over both sides and often a part of the dorsum as well as the ventral surface. I wish that I could have gotten some of these from Pygmies and Ainos, but they are exceedingly hard to manage as each toe has to be wrapped about in a strip of paper cut just the right size; in fact two

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stripes have to be used, the first could take ink, the second clean. An ordinary foot print gives absolutely no indication of this, since the simple loops which sometimes appear, is but a very small part of the pattern, and by no means the core, as would seem at first to be the case.

If I may now say a word or two concerning Systems of Identification. I hope you may not get the impression from what I have said or published, that I am making any serious attempts to found a rival system, for I certainly am not. Of course, like you, I am an enthusiast upon the employment of some part of the ridged area for this purpose and believe that ultimately it will become universal. My main interest in the entire

6.
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Northampton, Mass.

Subject has been that of a morphologist, and I feel that the only result of interposing a rival system, even if successful, would be to distract the public and postpone the success of either.

It is true that I do see certain advantages in making use of the longer and more conspicuous markings of the palm, and perhaps of the sole also, and if there is anything to be gained by this employment, the ultimate development of the system may be in that direction. The hypothenar pattern, for instance, in cases in which it presents some special feature, brands the possessor with an indelible mark that could be seen as a clear print across a court room, and the addition of such features might be at times a great convenience. I rejoice at the rapid

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F12

Instead of the finger-print system, which appears to be used more and more in our cities. as is seen by frequent notices in the daily papers, although there is a curious popular tendency to attribute this and all other means of investigation to Bertillon! Such matters correct themselves in time, and I am surprised to see how rapid seem thus far has been the popular education in the matter.

As a final word concerning Péré's ardues, the best I have yet found adorn the hands of one of our best of the younger American Artists, Robert Van Vnet Sewell!

Yours very sincerely
Harris H. Wilder

Sir Samuel Wilks

A1 [illegible]

Answered Aug 3

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
3/3/22/32

8, Prince Arthur Road,
Hampstead, N.W.

July 31. 1903

Dear Mr Galton

I was much interested in your letter of the day before yesterday in the "Daily Chronicle" & more especially as to there being no typical Englishman the different classes of society having apparently their own peculiarities — for Jay Sandow & other Athletes however strong & muscular failed in symmetry of form. I was reminded that some years ago an artist told me that he was requested

by another artist friend in Paris
to procure him Phtos. of the model
Englishman — of course nude —
which he could compare with those of
other Nations — My friend could
only procure pictures of artists models,
the same men who attend the
medical examination classes to be
used for indicating the usual
parts for operation — None of
these men were well proportioned
& he therefore failed to help his
French friend — At the same time

he knew remarkably well formed
men amongst the better classes &
aristocracy but he could not
prevail upon any of them to be
photo^d — naked.

An artist's widow ~~[illegible]~~ tells me exactly
the same thing. She failed to procure
any pictures of well formed women;
but she knew one model who had no
scruples presenting her taking about her
photo in various postures & now & then
~~the~~ his women procured similar ones from other models
for the purpose of private sale — none of them
my friend says — were beautiful refined
specimens of the female figure but were

a vulgar low type class of women
the odds that most artists take her
that to get a fine classical type they
twist up a model from two or three
but if I remember rightly Phidias did the same.
The subject of degeneration seems well
before the public & the Med. Colleges have
been applied to by the Government. A Member
of the County Council spoke to me yesterday
on the subject & suggested that his Body
or the State should introduce exercises in
for the young people. I told him, as
far as I had written, it is not yet clear
which is the best age to promote these
developmental changes.

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July 31 1903 45

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

8, Prince Arthur Road,
Hampstead, N.W.

In considering such questions as physique I have long thought that the increased height of women must have some deep & important physiological cause. I don't know whether you have made measurements of the last generation of women compared with previous ones but there can be no doubt of the fact that a very large number of girls are taller than their mothers. Within this we have the increased mental culture whereby women compete with men

on the deepest subjects & above all undertake
employments which were once slight to
be wholly masculine — I ask myself
is the woman becoming more masculine?
For the first time the Census has found
births below the average —
Men I think have not shown any
increase in stature —
The Births I think still show the superiority
in numbers of male over female — if this
had been reversed there assuredly would
have been some altered physiological law
at work

Every old idea was that
the male & female were much
the same in conformation until a
certain period when the development
into man took place — the mere
continuation of the growth meant female.
For many reasons this does not hold.
It is commented on by Tennyson who
says — 'woman is not undeveloped
man but diverse' — ~~the~~ Although
the points I indicate may not be
well pronounced — I can conceive
that there may be a tendency in

the deviation I speak of — that
woman is becoming more manly
as them mentally — by her physique —
& bearing fewer children — If there
be any truth in these surmises — it
is very important for the race.
The usual answer to the question — why
are ~~more~~ present girls so tall? — is. that
it is due to exercise — This is inadequate
& not true in fact. ~~At~~ At present the
cause is perfectly inexplicable —
Hoping I am not troubling you I am

Yrs faithfully
Samuel Willes

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

3/3/22/32
F9a

8, Prince Arthur Road,
Hampstead, N.W.

Aug 5. 1907

Dear Mr Galton.

Thanks for your letter.

I have long retired from practice & being old & decrepit can do very little work - I shall not therefore be able to assist you very much in the matter of finger prints but I have asked Jarvis to give a lecture at the Hampstead Scientific Society

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GALTON PAPERS

F10

& he has consented to do so early
in the Autumn. This may arouse
interest in the subject of which I may
avail myself — I am President
& the subjects are generally interesting but
have great trouble in not admitting lecturers
who are too ready to come & discourse
on thought reading — astrology &
the universal existence of life in inorganic nature
&c — & on anything which their
'idle brains' can conceive instead of
bringing us facts & results of observation.

As regards the subject on which I wrote. I am under the impression that, although very reading is limited, no great change takes place in a nation or race within a generation so as to enable people to discern any change from their own observation. What I deem to be impressed with is the continuance of the character as that of the French described by Cæsar. It deemed therefore extraordinary that during our war in Africa, not only did the population of many European Countries attack us as having turned Tyrants but scholars & many well informed men

f12

I said the same - A leading philosophic Society in Switzerland discussed the Boer War & passed a resolution lamenting that the English had lost their love of independence & liberty which had been their characteristic. Apart from their opinion - was it not ignorance which made them believe that a nation could change its character in a few years - ? - It seems to me that a little knowledge of History would have shewn them that Chamberlain Rhodes & Milner were the same type of men who had made England what it is - especially in the Elizabethan period -

I was regarded as the Patron of Mahomed as he was poor & I arranged to keep him at Gorge. His enthusiasm was unbounded - Was this due to his Eastern blood? - I hope I have not written anything suggesting a reply as I don't want to trouble you further.

Yrs faithly
Samuel Wilks

Willey

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
3/3/22/33

17 Acton Lane,
Harlesden.
N.W.
July 23. 1894.

Dear Sir,

Please accept my best thanks for so kindly sending me the apparatus for obtaining finger-prints of the Melanesians. I can only assure you that I shall do my best to obtain a satisfactory series for you, as I am, as a biologist, of course very much interested in your work; and also I am very anxious to get as many biological results (in the widest

sense) as possible from my
expedition which is undertaken
as Prof. Haddon may have
told you, as Balfour Student
of the University of Cambridge.
I suppose one error that must
be guarded against is that of
getting finger-prints from the
same individual twice over.
Very likely however that will not
be a real difficulty. I al-
ready possess the two small
manuals mentioned by you.

I remain, Dear Sir,
Yours very truly,
Arthur Willey.

Francis Galton Esq., M.A., F.R.S.,

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
3/3/22/34

Wm G. Willis FB

Grittleton.
Chippenham. Wilts.
13th April 1890.

Dear Mr. Galton,

A long and severe illness prevented me from doing anything about physical tests for candidates for the Army until last week, when I sent a long letter to the Director General of Military Education, at the War Office. My proposals were founded on your system of physical measurement, which I consider should be made imperative, and on my own views of physical tests for

Gymnastics, Running + Jumping, Swordsmanship, Riding, and when possible for Swimming + Diving - Skill should be optional - + I submitted a scheme for carrying out these tests - What the result will be, I don't know, but, judging by former Experience + by the "Times" Article on your Paper, I fear we shall not be successful -

Articles in Newspapers are very misleading, +

to judge of their value,
it would be necessary to
know by whom they are
written - The writer of
the article I refer to,
acknowledges that it is the
total of mental, moral, &
physical qualities that
really tells, & yet he
writes against affixing
any tests except for
the first named quality,
which is not logical -
So also does "a Head Master"
in the "Times" of 9th Inst. who
says that the training acquired
in the play fields and in
athletic games is valuable
for training young men for
the duties of after life,
& yet he objects to their

receiving any credit for
what will do them so much
good – I will send you
a copy of my letter, when
I have time to write it
out. I expect to return
to London about the
End of this month, when
I will call on you.

Believe me

Yours very truly

J. D. Hooker

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

C. L. Wills

GRAND HÔTEL DU NORD & CONTINENTAL

MONTUY-DUMINY. Propriétaire

POSTE & TÉLÉGRAPHE dans l'Hôtel.

Boulogne-s-mer, le 1 August 1886

Dear Sir

In reply to yr & Mrs F. Greenwood. I should tell you that I am a believer in the ill effects of marriages of first cousins in England & have seen its effects But I have failed to see any bad results, strange to say, in Persia. Perhaps because generally the marriages of cousins there, are the only real love matches in the upper classes? I am aware that it seems illogical but it is so. I a medical man failed to see the usual results.

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GALTON PAPERS

the only visible effect of the marriage of cousins so usual in the east – ("Daughter of my uncle" is alarm of endearment equivt to "darling".) is to perpetuate certain features in families and to exaggerate types: thus the royal family the Kajars are remarkably alike. Many families in Shiraz are easily recognizeible from the features alone, even by a European

Eg 1. That the marriage of first cousins, on the fathers side particularly i.e. the daughter of the amū or paternal uncle, in Persia are the rule and follow if they can be arranged.

Eg 2. There are no such proverbs as the Persian (and most Orientals) consider these marriages as particularly lucky – and so they are in one way, as the young people see a good deal of each

F3 342/9

other, whilst other ~~matters~~ marriages are as a rule quite a lottery—
A merchant wedding a merchants daughter
a servant ——— servants —
A coppersmith —— coppersmith —
and so on in the various trades.

I much regret that it is so but I can only say I am of my old (& the usual) opinion as to these marriages of cousins. Like most medical men I cd give shoals of cases of lunacy deafness dumbness &c &c in these cases in England.

If you are the author of "the hand book of travel", as I suppose, I wd draw yr attention to an appendix on Travelling in Persia (where it is an art) in my "Land of the Lion & Sun" (out of print but to be got at Mudie's &c, as a thing likely to interest you.

I shall be very happy to give you
any information I can (I
lived there 16 years) on Persia
My address is.

Broadhurst House
4 Gardens
S Hampstead NW
or Oriental Club.

Yours
Sincerely
C. J. Wills.

F. Galton Esq.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
3/3/22/36
f1

Mr Dog
Wave no
Primroses
on the 19th, but
when Gladstone
dies I will
spend 1st on a
bunch of his
flower if he
has one as
"jolly innocent"
& cheap as
Primroses -

My dear Bessy

I thank you much for the nice Pencil, a most useful present for I use a pencil so constantly & have generally 2 or 3 in my pockets - Emma Nixon continues to come down stairs every day & for many hours tho' she is always thankful to undress & lie down in bed again. She can only come down stairs backwards but does it very safely. I shall go & see her to day but not till after Post time as I have an aftn tea party &c. I want

to tell her all about Charles Darwin's funeral which I went to! When I saw it was to be in the Abbey (& Oh! how glad I am) I telegraphed to the undertaker for a ticket. & rec'd back a kind note from George D. asking me to join the other mourners in the Jerusalem Chamber at 11. So I went & slept at St Pancras the night before & was in good time. I knew no one, till, because of the Galton face & wonderful likeness to Emma, I seized upon your Frank & introduced myself & he presented me to 2 or 3 others & to his wife, & she & I kept close together for the rest of the morning. I like her much, & how very nice & taking Frank is! ——

f2

It was presently arranged that as the Procession would be very large, long, & slow some of us should be conducted at once to the mourners seats in the choir & wait there. & Mrs F. & I went accordingly with others - One effect among all the grand & beautiful arrangement struck me very much - The first so faint & distant sound of singing that one doubted if it was singing at all, & its extremely slow & delicate increase & development & gradual fulness until one saw thro' arches gleams of white dresses, & presently under the organ the Coffin appeared covered with flowers & carried up the chancel - The Daily News (I'm sorry for you my Tory but so it is) gives a most excellent & exact account of all - The Coffin was set in the chancel on a remarkably high Bier & so stood well above the crowd of heads. 63

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
2/3/22/36

Southampton.
Jan 11th 1884

Dear Mr Galton,

I have been away for the last few days or should have sent you the enclosed before. I hope it may be of service to you. I am sending you the two samples of ink: and enclose three specimens of the paper we use showing the amount

of Expansion when damped.

I hope you saw all you wished to the other day, and am sorry we could not ask anyone to meet you at luncheon. I am taking my wife up to Scotland for change of air but hope to be back by the middle of Jul. Believe me

Sincerely Yours

C.W. Wilson

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GALTON PAPERS

Ink for stamping

Sample N°1

Grind 2 lbs of ordinary Lithographic printing ink with 1/2 lb of ordinary "Middle" Lithographic varnish —

This can be kept for a considerable time even exposed to the air — A months supply can be made at a time.

Sample N°2.

Each day grind up about 1/2 oz of the above with 6/16 oz of ordinary "driers" as used by painters. —

This must be kept under water. —

The grinding is done on an iron plate with a stone muller and takes about quarter hour

This ink dries rather rapidly in dry weather and it is well to then mix a little Letter-press printing ink with it — in small quantities at a time as required for use

Expansion of paper

3 samples herewith showing amount of expansion. —

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GALTON PAPERS

F10

"Rives" paper used for photo purposes —

Pearson

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GALTON PAPERS

Original 4" × 4"

Wet. $4\frac{11}{80}$" × $4\frac{2}{80}$"

Dried. $3\frac{78}{80}$" × $3\frac{76}{80}$"

8. 6. 89.

fll. 343/2

Ordinary drawing paper

Expansion.

Original. 4". × 4"

Wet. $4\frac{6}{80}$" × $4\frac{1}{80}$"

Dried. $3\frac{79}{80}$" × $3\frac{78}{80}$"

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
343/2

8-6-89.

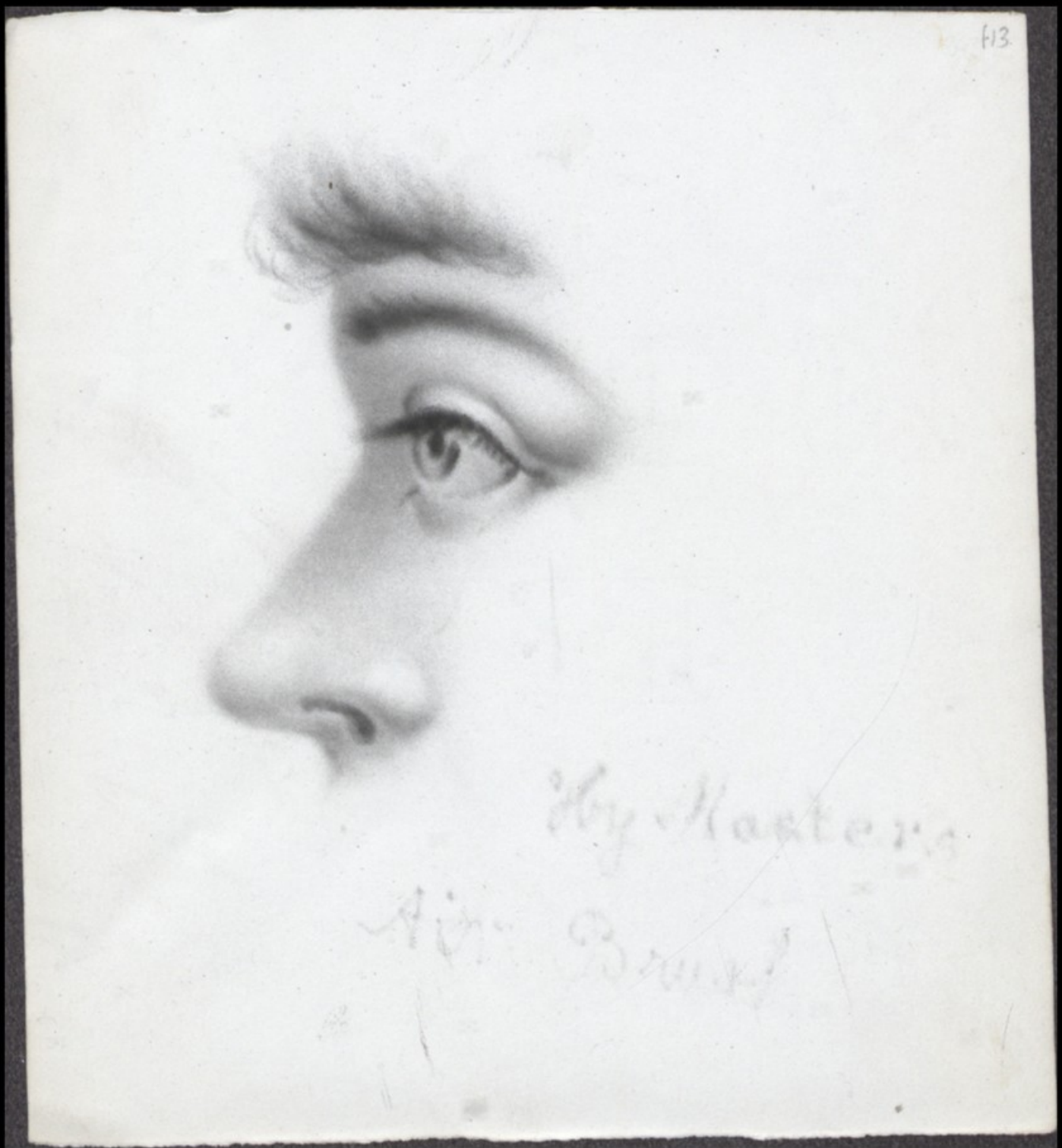
F12

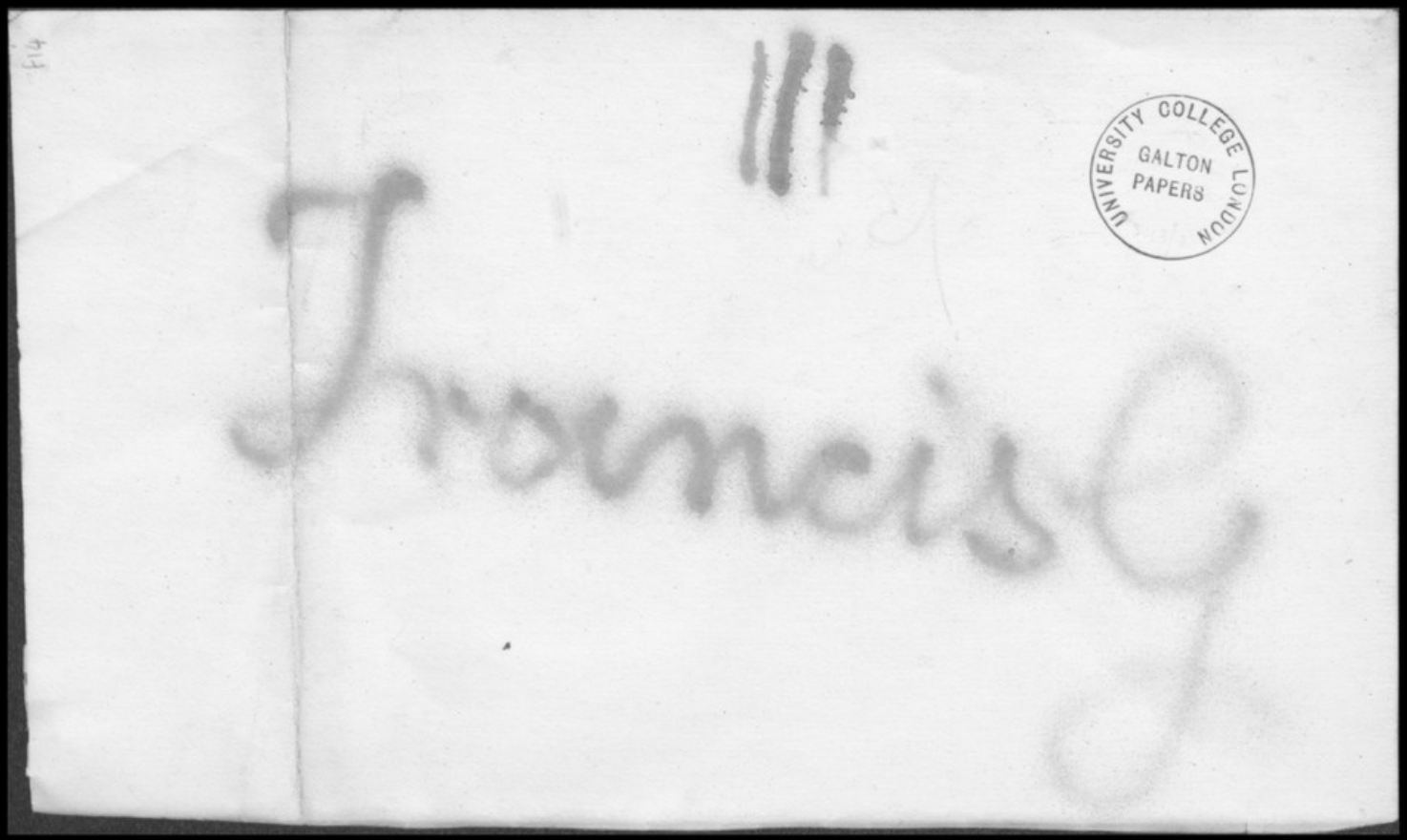
UNIVERSITY COLLEGE LONDON
GALTON PAPERS

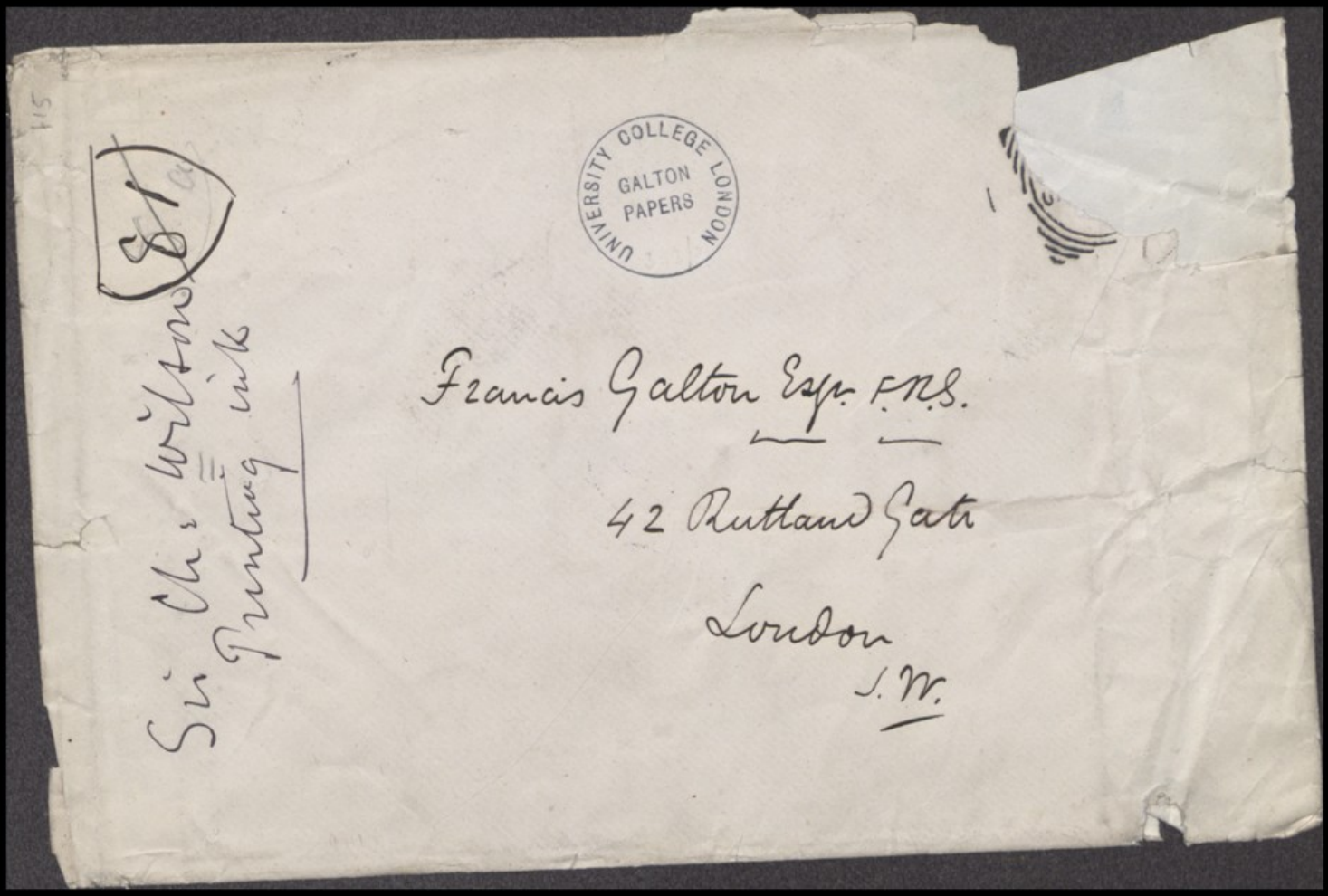
"Loaves tough"

Original 4" × 4"
Wet. $4\frac{7}{80}$" × $4\frac{1}{80}$"
Dried. $3\frac{78}{80}$" × $3\frac{75}{80}$"

8.6.89.


F13
Hey Masters
Air Brush


Francis G
UNIVERSITY COLLEGE LONDON
GALTON PAPERS


UNIVERSITY COLLEGE LONDON
GALTON PAPERS
Francis Galton Esqr. F.R.S.
42 Rutland Gate
London
S. W.

Sir Charles W Wilson

Declined on plea of Deafness

PALESTINE EXPLORATION FUND,

OFFICE AND MUSEUM
HOURS: 10 TO 5.
CLOSES AT 2 P.M.
ON
SATURDAY.

38, CONDUIT STREET,

LONDON, W.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
3|3|22|37

June 7th 1902

Dear W. Galton,

If you are not otherwise Engaged on Tuesday the 17th would you kindly honour the Fund by taking the chair at the Annual Meeting at the Royal Institution. The Meeting commences at 4 p.m., and the business will be, the reading of the Annual Report; a short address by myself (with slides) on the results obtained

under the Last Heads, and on the work which we hope to carry out during the next two years; additions to the General Committee; and election of the Executive Committee. The Chairman usually says a few words of encouragement and advice. The proceedings generally last about 1¼ hours, sometimes less. The report and address will I hope be in type the day before the Meeting.

The members of the Committee would esteem it a great favour if you could take the chair this year.

Believe me
Yours Sincerely
C. W. Wilson

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
343/2

T Wilson

Rivers Lodge
Harpenden
S O Herts

Tuesday ~~12 Nov.~~ 3 Dec. 1895

Dear Sir

I am at present engaged in preparing an article on Weights ~~&~~ and Measures.

I come across in the late Mr Chisholm's manual a table of proportions between human measurements

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GALTON PAPERS
3/3/22/38

Which I suppose is very
old & very well known

		4 digits =	1 palm
	1 span =	12 —	3 —
	1 foot =	16 —	4
1 cubit =	1½ ft =	24 al. 20. al. 32	~~24~~ 6
1 step	2½ =	40	10
1 pace	5 =	80	20
1 fathom	6 =	96	24

	in.
The digit being estim^{d} at all	0-76
∴ the foot at all	12-16
cubit	18-24
pace	60-80
fathom	nearly 73 in

As you, I know, take great
interest in Anthropometry
I venture to ask if you

can tell me in what books or papers I can compare their dimensions with the most authoritative measurements of modern and ancient races, & of the Greek Egyptian & ~~But~~ Assyrian statues

Mr Flinders Petrie, you are probably aware, thinks the digit & the cubit of the old Egyptians were 2 distinct & incommensurable units. but I am not sure

that the results of the method used in his book, Induction Meteorology, are to verify all.

The craniological measurements I see in books on Ethnology seem very confused. It is the arrangement & complexity of the contents of scales that we should like to know.

My friend Dr Pye Smith tells me it is undoubted that the brain of the ordinary Chinese weighs more than that of an European.

Yours very truly

J Wilson BA Lond.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

F. Wilson Tu 10 Dec '95

Rivers Lodge, Harpenden, S.O., Herts.

LLEGE LONDON
TON
ERS

Dear Sir

Accept my best thanks for your kind reply to my letter

I have found Prof. J. Marshall's book in the British Museum. He seems to take the height of the head from the crown to the chin as unit. the total height of the body being I think 4 4/9 of this.

The only reference I find

UNIVERSITY

το ~~πυγ~~ πυγμη is in
Liddell + Scott from Pollux
(archaeologus c AD 183) = 18
δακτυλοι. i.e. taking 1 δακτ-
-υλος = 0·75 in = 13·5 in
πυγων is said to be length
from elbow to first joint of
finger + = 20 δακτυλοι = 15 in
Thus 24 δακτυλοι or 18 in might
be one cubit the natural cubit
+ 28 δακτυλοι 21 in may mean
the cubit of the Pyramid builders
estimated at 20.67 or 20·63 in

Yours very truly
J Wilson

THE QUEENS COLLEGE BIRMINGHAM
UNIVERSITY COLLEGE LONDON GALTON PAPERS 3/3/22/59

HF

Jan. 21st., 1891.

Dear Mr. Galton,

I hope that the interest which you take in Anthropometry, & the kind assistance which you have already given me, may act as excuse for again troubling you. I have now in hand a very large number of measurements from the King Edward's Schools, Birmingham, & also from Rugby, which have never been published, & I am promised further figures from other schools (Bromsgrove &c.) Of course those which are of chief interest to me are the Birmingham boys, but the others will serve as a useful means of comparison. My object in writing now, is to ask you whether you could give me any references to papers or articles on schoolbaj's hgt. wgt rc. more recent than Roberts' book (1878). Of course I know the text-books, & I have Rowditch's papers, on which, I think, I cannot do better than model my own. Further, would it be asking too much, if I asked you to look over the paper when completed, & advise me as to where to publish it? If you would do so you would confer a great favour upon me. You will be glad to hear that we have found a habitation for our Laboratory in the Athletic Institute, a place where there is a gymnasium, & where the physical culture of the city, especially of the Board Schools is supervised. It is centrally placed, & in every way a most suitable place for the purpose. We opened the Labor-

Windle

PROFESSOR WINDLE, M.D.,

DEAN OF THE MEDICAL FACULTY.

TELEPHONE 575.

Nov. 18th. 1891.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

Dear Sir,

Knowing the interest which you take in Anthropometric matters I venture to write to you for a little assistance. The Philosophical Society in this City is at present instituting an Anthropometric Laboratory in which I hope we shall be able to do some useful work, especially in the direction of making systematic measurements of children in the various schools at regular intervals. I am now anxious to learn where the most suitable & unused

of meetings of the British
Association may be obtained &
am in hopes that you will be
able to assist in this
matter - Any hints also,
which you could give me
with regard to the manage-
ment of such a laboratory
would be much appreciated.

I am,
Dear Sir,
Yours very faithfully
Bertram C. A. Windle.

Prof: Wm Ogle MD
FS

Nov. 13. 89.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

My dear Sir,

I am exceedingly obliged to you for your kind reply to my letter - The chief part of the Laboratory work will be undertaken by myself & I shall most gratefully & hope hereafter avail myself of your kind permission to visit your Laboratory - I have

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two schemes on hand to secure continuous records of growth - on which I should like your opinion - Perhaps you would favour me with an interview when I come up to London if convenient to you. I am.

Yours faithfully

Bertram C. A. Windle.

Sat^a. at 2¼ at laboratory

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THE QUEENS COLLEGE BIRMINGHAM

PROFESSOR WINDLE, M.D.,

DEAN OF THE MEDICAL FACULTY.

TELEPHONE 575.

Decr 2. 1891.

UNIVERSITY COLLEGE LONDON GALTON PAPERS

Dear Sir,

I purpose, unless prevented coming up to London on Saturday. If I do so could I see your laboratory earlier the afternoon? I scarcely like to ask it, knowing how busy you are, but if you could make an appointment by which I could have a quarter of an hour's talk with you at your own house or elsewhere I should be very much obliged as I have one or two things which

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I particularly want to ask you about & writing is so tedious a method for both sides - If the afternoon is unsuitable for you or for the Laboratory I could come in the morning - I shall arrive in town about 10-15 A.M.

I am,
yours faithfully
Bertram C. A. Windle

Fras. Galton Esq.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

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MASON COLLEGE, BIRMINGHAM

Queen's Faculty of Medicine.

TELEPHONE No 68.

Prof. Windle. M.D. D.Sc.
Dean.

Jan. 20th. 1893.

Dear Mr Galton,

I am drawing near the conclusion of a paper dealing with the stature & growth of schoolboys from certain Public Schools - I am dividing it into -

1. Stature - in which I deal with about 40,000 measurements of boys from King Edward's Schools in this City, Rugby & Giggleswick - giving Distribution tables, percentiles at the different ages & percentile - grade tables of height, weight & chest girth -
2. comparison of the same with other schools, former tables & similar results in America

& other countries -

3. rate of growth of King Edward's boys in the three categories with comparisons -

My object in writing to you is to ask your advice as to where it would be best to publish it. When Prof. Macalister was stopping with me some little time ago, I asked his advice she seemed to think the Journal of the Anthropological Society would be the best place & he kindly offered to stand for further to it there -

It is, however, as you may guess from what I have said, of considerable length & I fear it may be too long for them. If you would kindly advise me on this point I should feel very grateful to you -

Our Laboratory is now working - I am measuring the pupil teachers, who attend for four years at the Athletic Institute, the public is admitted on certain days & by degrees I hope we shall have a useful

Laboratory.

Wish many apologies for troubling you at such length

Yours,

Yours very faithfully

B. Ortrayn C. A. Windle

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

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MASON COLLEGE, BIRMINGHAM · PROGRESS THROUGH KNOWLEDGE ·

Queen's Faculty of Medicine.

TELEPHONE No 68.

Prof. Windle. M.D. D.Sc.
Dean.

Jan 25. 1893.

Dear Mr Galton,

I am very much obliged to your postcard letter. Your views very much coincides with my own. I will work the percentiles & comparisons into a paper of results for the anthropological Soc. I can at our local Philosophical Soc. to publish the tables of distribution & percentages as they relate to a local object.

I shall hope to send you copies when they appear & wish many thanks for

your kind advice & encouragement.

Yours faithfully,

Bertrand A. W. Russell.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

UNIVERSITY COLLEGE LONDON
GALTON PAPERS
3/3/22/40

24a Nürnbergerstr.
Berlin, Jan. 17th 03.

Dear Sir,

Having read your book "Inquiries into Human Faculty & its Development" with the greatest interest, I should be very much obliged if you would kindly let me know if & under which conditions I could obtain the rights of translation. (Into

f2

German.)

Yours most truly,

G. P. Wolff,

stud. rer. nat.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

20. Town Mead Rd
S.W.
Fulham
Nov 22nd 1892

To Francis Galton Esq F.R.S
&c &c &c

Dear Sir,

Some years ago I met you at Walvish Bay when I was 2nd Mate of the Brig "Coquette" when you arrived from Lake N'Gami where you with your servant had been travelling and you boarded the Coquette in an Indian Rubber Canoe which yourself paddled off to us and I was the first to receive you since then I have written you about the Congo which has now become important since my relation Capt Tuckey has explored it

I trust this will find
you well and prosperous
and Remain Dear Sir
Faithfully Yours
Edwyn Archer Wood
Master Mariner
late 2nd Mate of the
Brig "Coquette"
Capt Owen
in 1851 at Falmouth Bay

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
3/3/22/4

I am in great distress at
present having lost through
a Solicitor a fortune and
estate in the Isle of Wight
Hill House "Brading is
my Freehold but he during
my absence from England
made away with my deeds
and I have thought I might
ask you to assist me I
have read your work just
Published at Mac Millans
and other matters on the
Anthropological study
if I had £10. I can get
a syndicate to help me
about the Ivory and Guano
matter and perhaps work
the Gold Reefs I must deal
with the Portugese in
that for a Concession
— — — — —

and perished with his crew
at the first falls from Fever
I have also written you about
a discovery of mine in 1851
at Cape St Mary after you
left us at Walwich Bay
there exists a petrified
Guano Island in the Bay
and at Elephant Bay only
20. miles from Cape St Mary
there is vast deposits of
Ivory and Quartz Reefs
Gold Bearing the Ivory
I believe is from so old
Fresh water holes that have
become brackish with the
encroach of the Sea on the
Coast and the Animals have
died about them years ago
but still the Ivory is in
good preservation, and
I write to ask you if you
could advance me a sum
of £10 to get about so that
I might get help to get out

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OLLEGE LONDON
ALTON
APERS

F3

C. 2. Ward
"St. Marys Lambeth Infirmary"
Brooke Street
Kennington S.E.

To Francis Galton Esqr. F.R.S.

Very Dear Sir,

You will remember me when I tell you I was supernumerary Second Mate of the Brig "Coquette" which received you on board at Walwich Bay. I received you at the Gangway when you had paddled yourself off in your India Rubber Canoe from the Mission Station there. and took you down to the Cabin to Capt Owen her Commander when you astonished us all by your telling us you had come down from the Lake N'Gami with your Servant and had been the last 7 days of your travel over the Awful Kalahari Desert on one Bottle of Water between you until you saw our Mast heads lying at Anchor in Walfish Bay. I am as I told you related to

to the late Capt. Tuckey who pe=
=rished with all his Crew at the
First Falls of the Congo. through
my late sisters Marriage with
Mr. Richard Tuckey of Wiltshire
Grandson of Capt. Tuckey. who gave
the name of Tuckie Land in the
Elephant Country up the Bight of
Benin.

I saw your Portrait in the
"Daily Graphic" last Friday re=
counting your Travels in South Africa
and enclose this one I had previous
ly of you, but they dont agree in
your likeness to me and should
like one from yourself of Photo
if I could be so gratified at
this time of your age as I am
myself 77 years old. and your
self 86.

I must regretfully tell
you I have been so defrauded
by a Solicitor of my Estate
"Hill House" Brading I. of W.
whilst I was at Sea some years
ago. that I am now in my

old days a Pauper Inmate of this Infirmary and Asthma my illness. and should you be able to spare me a little money for some fruit and cakes I may get in here if I pay for them I shall be thankful. and may God Bless your last days.

I am Dear Sir
Yours Faithfully
Edwin Archer Wood
late Master Mariner
and D.S.O of Italy
"for saving Life at Sea"
1850
belonging to Genoa

No 16.278. Certificate London

P.S. I have invented a perfect Airship but have no finance to bring it out. it would easily fly from Walfisch Bay to Lake N'Gami over the Kalihari with or without the Trade Winds but not for War purposes E.A.W.

ALL LETTERS TO BE ADDRESSED
TO THE SECRETARY.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
3/3/22/42

SOCIETY OF ARTS,
JOHN ST. ADELPHI.
LONDON.W.C.

Dec 21. 1892

Dear Mr. Galton

At a Meeting of the Royal Commission the other day a strong desire was expressed that a Set of your anthropometrical apparatus should be included in the British Section at Chicago. I stated that I had had some communication with you on the subject and that you were not able to do anything in the matter,

Francis Galton Esqre.,

Ont

but it was suggested that the Cambridge Scientific Instrument Co. might be inclined to lend a set of your approved pit.

Will you kindly let me know what you think of the suggestion; and if you approve, perhaps you would not mind yourself mentioning the matter to the Cambridge Company.

If the apparatus could be provided the Commission would undertake the cost of transport,

and

13 343/7

and I have no doubt Professor Putnam, the head of the Ethnological department, would be glad to make arrangements for showing the apparatus.

yours faithfully

H. J. Ward

Secretary

see back for title of proposed exhibit

ALL LETTERS TO BE ADDRESSED
TO THE SECRETARY.

ROYAL COMMISSION CHICAGO EXHIBITION 1893

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
3/3/22/42

SOCIETY OF ARTS,
JOHN ST, ADELPHI,
LONDON.W.C.

January 28/1892.

Dear Mr. Galton

I have received from Prof. Putnam, the head of the Department of Ethnology at Chicago, a letter in which the following passage occurs in reference to your Anthropological laboratory: viz. "I should feel specially pleased "and consider it a great honour "if Mr Galton would make an "Exhibit of at least a portion of "his special apparatus in our

"Anthropological Laboratory. As his
"name is so identified with work in
"this Department, the Exhibition of
"any ~~apparatus~~ apparatus from his
"laboratory would prove of particular
"interest.
"Dr Boas, Dr Bowditch, and Professor
"Jastrow are the gentlemen to whom
"I look for carrying out the details
"of the work of the Physical laboratory.
"I have placed the matter in the
"immediate charge of Dr Boas who will
"receive assistance from the others. I
"shall therefore probably have all the
"apparatus necessary for the work in
"hand, and of course we do not expect
"to enter into many of the delicate tests
"which can only be carried on in the
"quiet of a special laboratory".

Yours faithfully,

Francis Galton Esq. H. T. Wood
F.R.S.

Secretary

10. Pembridge Gardens.
December 21st. 1881.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
3/22/43

Mr James T. Wood.
Presents his
compliments to Mr
Galton.

Mr Wood lost his sight
by an accident seven years
ago. and understanding
that Mr Galton is interested
in the subject of dreams.

upon a horse, with
reference to riding and
driving. Personally I
never had. and only
realized what the
expression meant after my
accident. when I discovered
that the touch alone.
I mean without a firm
hold is sufficient to

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indicate the movements
of the person one is
walking with.

Wishing you the
compliments of the
season,

I remain,

Your's truly,

J J Wood

to —
F. Galton Esq.
Rutland Gate,

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

10. Pembridge Gardens.
December 21st. 1881.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
3/3/22/43

Mr. James. T. Wood.
Presents his
compliments to Mr.
Galton.

Mr. Wood lost his sight
by an accident seven years
ago. and understanding
that Mr. Galton. is interested
in the subject of dreams.

would be glad to receive one of his circulars.

Mr. Wood has since his accident, observed two marked Types in his dreams, one that he is never conscious of being blind when he is dreaming the other that he has frequently dreamed that he has lost his way among

the ruins of buildings
destroyed by fire and that
a policeman or workman
with all his flesh dried-up
comes to assist him out. —
Then he awakes, and
becomes aware that he has
been dreaming.

to
Francis. Galton. Esq.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

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10. Pembridge Gardens.

December 23rd 1881.

Dear Sir,

Thanks for your letter of the 21st inst. It will give me much pleasure to receive a call from you here.

I am as a general rule either at home or within reach of a morning, but If you will give me

notice, I will unless
unavoidably prevented
keep an appointment
either for the Morning or
afternoon,

Carrying apparatus
about may be inconvenient.
If therefore you prefer
it. I can come over to
Rutland Gate at any
time you may

f6

appoint, wishing

you the compliments of the

season I am yours truly

James Wood

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

to

F. Galton. Esq.

Rutland Gate.

f7

10. Pembridge Gardens.
January 4th. 82.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

Muscular Sensibility

Dear Sir.

My neighbour Captain Ross who many years ago served in India tells me that the Native Throfs chequed the contents of the bags

containing the Land
Revenue & divided the
coins into three heaps
by the touch alone. viz:-
Full weight. Light-weight
and Intermediate.

How would it be
to ask the subjects of
your experiments if
they had a right hand

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
3/3/22/44

October 17, '92—

Wellesley College.

Wellesley, Mass.

Dear Sir:—

I inclose to you an anthropometric table which I compiled last winter after your own percentile method, and also a comparative table showing some results of the work which was done in the Wellesley College Gymnasium during the winter of 1891-'92—

I write to ask if you will give me your advice. For the past six years I

have had charge of
the Anthropometrical
work in this institution
during which time I
have been about from
one place to another
and from man to
man to try to learn
all I could about
the science. Also I
have studied Anatomy
and Physiology, and
I am a member of the
American Statistical
Society. I tell you all
this that you may know
what I have done,

and I wish to ask if
you will advise me
as to the next step
which I should take.
Is there a course of
reading or study
which would help me?
Of course I am chiefly
interested in that part
of Anthropometry which
may be of practical
use in connection with
the Gymnasium here
at the College.
I feel very strongly that
I need the help of added
knowledge in order to

do the best with my work; and Knowing of your interest in Anthropometry I have ventured to write this letter. Hoping I am not asking too great a favor I am.

Very Sincerely.

M. Arnold Wood.

Sir Francis Galton,
42 Rutgate Gate,
Kensington, London, England.

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS

ANTHROPOMETRIC TABLE

DEPARTMENT OF PHYSICAL TRAINING, WELLESLEY COLLEGE.

Arranged according to the percentages at the left, from the measurements of fifteen hundred Wellesley students. The black figures indicate millimeters and kilograms; the red, inches and pounds. "Lung Capacity" is given in litres and cubic inches.

MEASUREMENTS TAKEN AND COMPILED BY M. ANNA WOOD, Wellesley College, Wellesley, Mass.

PER CENT.	AGE.	WEIGHT.	HEIGHT.						GIRTH.																				DEPTH.	BREADTH.				LENGTH.								STRENGTH.					
			Height	Sitting.	Knee.	Pubes.	Navel.	Sternum.	Head.	Neck.	Chest.	Chest full.	Ninth Rib.	Ninth Rib full.	Waist.	Hips.	Thigh R.	Thigh L.	Knee R.	Knee L.	Calf R.	Calf L.	Ankle R.	Ankle L.	Instep R.	Instep L.	Uparm R.	Uparm L.	Elbow R.	Elbow L.	Forearm R.	Forearm L.	Wrist R.	Wrist L.	Chest.	Abdomen.	Head.	Neck.	Shoulders.	Waist.	Hips.	Nipples.	Shoulder R.	Shoulder L.	Elbow R.	Elbow L.	Wrist R.	Wrist L.	Foot R.	Foot L.	Stretch of Arms.	Back.	Legs.	Chest.	Forearm R.	Forearm L.	Capacity of Lungs.
1	17-.5	35-3	1505	902	458	970	1060	1415	555	340	849	904	755	807	740	1060	650	650	420	410	360	370	256	243	307	300	250	250	256	254	170	160	214	226	166	119	404	261	364	249	327	328	400	400	265	264	1760	101	170	40	36	35	3-65
		166.	79-0	36.1	18.	34.3	41.7	55.7	21.9	13.7	33.4	35.6	29.7	31.8	29.2	41.7	25.6	25.6	16.6	16.4	14.2	14.6	10.1	9.6	12.1	11.8	9.8	9.8	10.1	10.	6.7	6.3	8.4	8.9	6.6	4.6	15.9	10.3	14.3	9.8	12.9	12.9	15.7	15.7	10.4	10.3	69.3	220.6	374.7	88.4	79.8	77.1	201.
2.5	17-.9	37-6	1710	915	491	1014	1109	1404	586	339	870	883	761	816	764	1094	657	654	426	405	362	363	250	240	320	315	269	254	260	245	168	168	208	217	165	118	398	264	370	243	331	320	430	427	290	299	1746	95	155	38	36	33	3-16
		177.2	67.1	35.6	17.8	36.8	41.3	55.3	23.1	13.4	34.3	34.8	30.1	32.1	30.1	43.1	25.8	25.8	16.8	16.1	14.3	14.4	9.9	9.5	12.6	12.4	10.6	10.	10.2	9.6	6.6	6.6	8.2	8.5	6.5	4.6	15.7	10.4	14.6	9.6	12.7	12.6	16.1	16.1	10.2	10.2	68.7	209.4	341.6	91.8	79.3	78.7	200.8
5	19-.3	37-8	1694	895	444	841	1038	1389	579	335	813	867	700	764	698	998	623	622	396	390	352	355	240	233	297	295	255	246	300	285	165	165	199	207	160	114	396	258	358	238	326	306	426	427	295	296	1726	90	140	36	35	31	3-12
		168.6	66.7	35.2	17.5	33.1	40.8	54.7	22.8	13.2	32.	34.1	27.6	30.	27.5	39.3	24.5	24.5	15.6	15.4	13.9	14.	9.4	9.2	11.7	11.6	10.1	9.7	11.8	11.2	6.5	6.5	7.8	8.1	6.3	4.4	15.6	10.2	14.1	9.4	12.8	12.	16.8	16.8	10.1	10.1	68.	198.4	321.8	79.4	77.1	68.3	190.7
10	20-.7	40-3	1624	885	435	826	1021	1324	574	329	796	851	680	743	670	951	606	603	384	380	342	346	232	225	286	290	245	240	295	277	160	160	195	197	159	109	386	246	346	226	320	300	426	425	292	291	1726	81	135	33	33	29	2-96
		182.6	63.9	34.8	17.1	32.6	40.2	52.1	22.6	13.	31.3	33.5	26.8	29.3	26.4	37.4	23.8	23.7	15.1	15.	13.5	13.6	9.2	8.9	11.3	11.4	9.6	9.4	11.6	10.9	6.3	6.3	7.7	7.8	6.3	4.3	15.	9.7	13.6	8.9	12.6	11.8	16.5	16.5	9.9	9.9	67.9	178.6	297.2	78.2	74.7	63.9	180.6
15	21-.10	41-.5	1661	878	489	819	1011	1312	570	326	773	840	666	713	666	936	595	595	392	377	333	329	229	223	283	290	250	250	290	270	150	150	189	190	158	106	382	238	338	218	305	288	418	418	285	285	1685	72	122	32	32	28	2-82
		182.5	65.4	34.6	16.9	32.2	39.8	51.6	22.4	12.8	30.4	33.1	26.2	28.1	26.2	36.8	23.4	23.4	14.9	14.8	13.1	13.0	9.0	8.8	11.1	11.4	9.8	9.8	11.4	10.6	5.9	5.9	7.4	7.5	6.2	4.2	15.	9.4	13.3	8.6	12.	11.3	16.5	16.5	9.3	9.3	66.1	169.2	273.5	70.3	68.	61.7	172.3
20	21-.4	90.	1652	870	486	800	1003	1302	566	322	773	832	659	706	657	943	586	585	374	370	326	325	217	217	277	280	240	240	290	270	150	150	188	188	152	108	378	236	334	211	300	278	416	414	280	282	1677	70	117	31	30	27	2-79
		198.4	65.	34.3	16.8	31.7	39.5	51.3	22.3	12.7	30.4	32.8	25.9	27.8	25.9	37.1	23.1	23.1	14.8	14.6	12.9	12.8	8.6	8.6	10.9	11.1	9.5	9.5	11.4	10.6	5.9	5.9	7.4	7.4	6.1	4.3	14.9	9.3	13.2	8.3	11.8	11.	16.4	16.4	11.	11.1	66.1	166.1	267.9	68.3	66.1	60.5	170.8
25	20-11	56.8	1642	867	492	804	997	1302	560	320	765	824	650	708	650	933	576	577	368	366	324	323	214	215	275	284	235	235	289	267	153	156	185	185	157	107	375	233	330	209	297	276	412	412	280	285	1660	71	111	30	29	26	2-72
		125.6	64.6	34.1	16.6	31.7	39.2	51.3	22.1	12.6	30.1	32.4	25.6	27.9	25.6	36.7	22.7	22.7	14.5	14.4	12.8	12.7	8.4	8.5	10.8	11.2	9.3	9.3	11.4	10.5	6.1	6.2	7.3	7.3	6.2	4.2	14.8	9.2	13.	8.2	11.7	10.9	16.2	16.2	11.	11.2	65.4	154.1	244.6	66.1	64.2	57.3	168.6
30	20.8	57.8	1634	860	499	796	990	1297	563	318	757	818	644	702	643	923	565	564	361	361	320	319	213	213	274	277	230	230	285	264	152	152	180	180	156	106	374	230	327	205	295	272	408	408	277	280	1652	69	105	29	29	25	2-63
		127.4	64.3	33.9	16.6	31.3	39.	51.1	22.2	12.5	29.8	32.2	25.3	27.6	25.3	36.3	22.2	22.2	14.2	14.2	12.6	12.6	8.4	8.4	10.8	10.9	9.1	9.1	11.2	10.4	6.	6.	7.1	7.1	6.1	4.2	14.7	9.1	12.9	8.1	11.6	10.7	16.1	16.1	10.9	11.	64.8	152.1	240.4	65.4	63.6	56.7	166.6
35	20-.5	58.8	1642	858	477	795	985	1330	562	316	750	812	639	708	630	904	562	562	352	347	316	311	211	211	282	281	230	230	285	264	152	152	180	179	155	105	370	228	333	204	300	276	408	408	282	282	1650	66	101	28	28	24	2-62
		129.1	64.7	33.8	16.9	31.3	38.8	52.4	22.1	12.4	29.5	32.	25.2	27.9	24.8	35.6	22.1	22.1	13.9	13.7	12.4	12.2	8.3	8.3	11.1	11.1	9.1	9.1	11.2	10.4	6.	6.	7.	7.	6.1	4.1	14.6	9.	13.1	8.	11.8	10.9	16.	16.	11.1	11.1	64.9	149.6	231.6	64.2	62.4	55.7	163.8
40	20.2	59.9	1620	854	463	787	980	1324	559	314	745	805	634	702	624	907	560	559	357	355	314	313	209	209	290	293	228	228	290	268	153	153	179	177	154	104	368	226	329	201	302	280	412	412	285	285	1630	65	98	28	27	24	2-53
		133.1	63.8	33.6	16.3	31.	38.6	52.1	22.	12.4	29.3	31.7	25.	27.6	24.	35.7	22.1	22.1	14.1	14.	12.4	12.3	8.2	8.2	11.4	11.5	9.	9.	11.4	10.6	6.	6.	7.	7.	6.1	4.1	14.5	8.9	13.	7.9	11.9	11.	16.2	16.2	11.2	11.2	64.3	148.6	227.	63.2	61.2	54.8	160.8
45	20.	61-9	1612	850	463	783	974	1318	556	312	737	802	629	699	620	899	555	553	354	352	310	307	207	207	279	278	225	225	290	270	152	152	177	175	153	104	366	224	326	197	297	276	410	410	285	285	1620	62	95	27	26	23	2-47
		139.1	63.5	33.3	16.3	30.8	38.3	51.9	22.	12.3	29.	31.6	24.8	27.5	24.4	35.4	21.9	21.8	13.9	13.8	12.2	12.1	8.1	8.1	11.	10.9	8.9	8.9	11.4	10.6	6.	6.	7.	6.9	6.	4.1	14.4	8.8	12.8	7.8	11.7	10.9	16.1	16.1	11.1	11.1	63.8	130.6	200.6	62.2	57.1	52.7	151.8
50	20.10	64.9	1605	846	470	778	968	1312	556	310	732	796	624	695	624	893	549	548	351	349	309	305	205	205	279	278	225	225	290	270	153	153	175	173	152	103	364	222	325	195	295	274	410	410	285	285	1610	59	90	26	26	22	2-46
		143.1	63.2	33.2	16.1	30.6	38.1	51.7	21.9	12.2	28.8	31.3	24.6	27.4	24.6	35.2	21.6	21.6	13.7	13.6	12.1	12.1	8.1	8.1	11.	10.9	8.9	8.9	11.4	10.6	6.	6.	6.9	6.8	6.	4.	14.3	8.7	12.8	7.7	11.6	10.8	16.	16.	11.	11.	63.4	128.6	190.6	61.2	56.1	52.1	149.3
55	21-.3	67.3	1598	842	476	772	963	1306	554	308	730	791	619	692	619	886	544	542	349	347	307	303	204	204	278	277	224	224	290	269	153	153	174	171	151	102	362	220	324	193	294	272	408	408	284	284	1600	56	86	25	25	21	2-40
		148.3	62.9	33.1	16.1	30.4	37.9	51.4	21.8	12.1	28.7	31.1	24.4	27.2	24.4	34.9	21.4	21.3	13.5	13.4	12.	11.9	8.	8.	10.9	10.9	8.8	8.8	11.4	10.6	6.	6.	6.8	6.7	5.9	4.	14.2	8.6	12.7	7.6	11.4	10.7	16.	16.	11.1	11.1	63.	123.6	186.	59.1	55.1	50.7	146.5
60	21-.8	70.8	1592	838	476	769	957	1300	553	307	729	788	614	689	614	878	537	537	345	344	303	301	202	202	277	276	222	222	290	268	154	154	173	169	150	102	360	218	322	191	292	270	406	406	284	284	1590	54	82	25	25	21	2-34
		156.2	62.7	33.	16.1	30.3	37.7	51.2	21.8	12.1	28.7	31.	24.2	27.1	24.2	34.6	21.1	21.1	13.6	13.5	11.9	11.8	8.	8.	10.9	10.9	8.7	8.7	11.4	10.6	6.1	6.1	6.8	6.6	5.9	4.	14.2	8.6	12.7	7.5	11.5	10.6	16.	16.	11.1	11.1	62.9	121.6	182.6	58.1	54.1	49.8	143.8
65	22-.3	73.2	1586	834	476	765	951	1296	552	306	727	786	609	687	609	870	532	532	342	340	299	297	200	200	276	275	220	220	290	268	154	154	171	167	149	101	358	216	320	189	290	268	404	404	284	284	1580	52	78	24	24	20	2-28
		161.4	62.4	32.8	16.1	30.1	37.4	51.1	21.7	12.	28.6	30.9	24.	27.	24.	34.3	20.9	20.9	13.4	13.4	11.8	11.7	7.9	7.9	10.9	10.8	8.7	8.7	11.4	10.6	6.1	6.1	6.7	6.6	5.9	4.	14.1	8.5	12.6	7.4	11.4	10.6	15.9	15.9	11.1	11.1	62.2	118.6	174.	57.1	53.1	48.7	140.8
70	22-.8	75.6	1574	830	476	761	945	1290	550	305	724	784	604	686	604	862	527	527	339	337	295	293	198	198	275	274	218	218	290	268	154	154	169	165	148	100	356	214	318	187	288	266	402	402	284	284	1570	50	74	24	24	20	2-22
		166.6	62.1	32.7	16.1	30.	37.2	50.8	21.7	11.9	28.5	30.9	23.8	27.	23.8	33.9	20.7	20.7	13.3	13.3	11.6	11.5	7.8	7.8	10.8	10.8	8.6	8.6	11.4	10.6	6.1	6.1	6.7	6.5	5.8	3.9	14.	8.4	12.5	7.4	11.3	10.5	15.8	15.8	11.1	11.1	61.8	116.6	170.6	56.1	52.1	47.6	137.8
75	23-.3	78.4	1562	826	476	757	939	1284	547	304	721	781	599	685	599	854	522	522	335	333	291	289	196	196	274	273	216	216	290	268	154	154	167	163	146	99	354	212	316	185	286	264	400	400	284	284	1560	48	70	23	23	19	2-16
		172.8	61.8	32.5	16.1	29.8	37.	50.6	21.5	11.9	28.4	30.8	23.6	27.	23.6	33.6	20.5	20.5	13.2	13.1	11.5	11.4	7.7	7.7	10.8	10.7	8.5	8.5	11.4	10.6	6.1	6.1	6.6	6.4	5.7	3.9	13.9	8.3	12.4	7.3	11.1	10.4	15.8	15.8	11.1	11.1	61.4	113.6	165.3	55.1	51.1	46.5	135.8
80	23-.8	81.7	1550	822	476	753	933	1278	544	303	718	778	594	683	594	846	517	517	332	330	287	285	194	194	273	272	214	214	290	268	154	154	165	161	144	98	352	210	314	183	284	262	398	398	284	284	1550	46	66	22	22	18	2-10
		180.1	61.4	32.4	16.1	29.6	36.7	50.3	21.4	11.8	28.3	30.7	23.4	26.9	23.4	33.3	20.3	20.3	13.1	13.	11.3	11.2	7.6	7.6	10.8	10.7	8.4	8.4	11.4	10.6	6.1	6.1	6.5	6.3	5.7	3.9	13.8	8.2	12.4	7.2	11.1	10.3	15.7	15.7	11.1	11.1	61.	110.6	160.1	54.1	50.1	45.7	132.8
85	24-.3	85.9	1538	818	476	749	927	1272	541	301	715	775	589	680	589	838	512	512	329	327	283	281	192	192	272	271	212	212	290	268	154	154	163	159	142	97	350	208	312	181	282	260	396	396	284	284	1540	44	62	21	21	17	1-98
		189.4	60.5	32.2	16.1	29.5	36.5	50.1	21.3	11.8	28.2	30.5	23.2	26.8	23.2	33.	20.2	20.2	13.	12.9	11.2	11.1	7.5	7.5	10.8	10.7	8.3	8.3	11.4	10.6	6.1	6.1	6.4	6.3	5.6	3.8	13.8	8.2	12.3	7.1	11.1	10.2	15.6	15.6	11.1	11.1	60.6	107.6	154.6	53.1	49.1	44.8	128.8
90	25-.1	90.6	1521	813	476	745	921	1266	538	299	712	772	584	677	584	830	507	507	326	324	279	277	190	190	271	270	210	210	290	268	154	154	161	157	140	96	348	206	310	179	280	258	394	394	284	284	1530	42	58	20	20	16	1-86
		199.6	60.1	32.1	16.1	29.3	36.3	49.8	21.2	11.8	28.1	30.4	23.	26.7	23.	32.7	20.	20.	12.9	12.8	11.1	11.	7.4	7.4	10.8	10.7	8.3	8.3	11.4	10.6	6.1	6.1	6.3	6.2	5.5	3.8	13.7	8.1	12.2	7.	11.	10.1	15.5	15.5	11.1	11.1	60.2	103.6	148.6	52.1	48.1	43.9	123.8
95	26-.6	96.3	1510	807	476	737	915	1260	534	298	709	769	579	674	579	822	502	502	323	321	275	273	188	188	269	268	208	208	290	268	154	154	159	155	138	95	346	204	308	177	278	256	392	392	284	284	1520	40	54	19	19	15	1-78
		212.4	59.4	32.	16.1	29.1	36.2	49.6	21.1	11.8	28.	30.4	22.8	26.6	22.8	32.4	19.8	19.8	12.7	12.6	10.8	10.8	7.4	7.4	10.6	10.6	8.2	8.2	11.4	10.6	6.1	6.1	6.3	6.1	5.4	3.7	13.6	8.	12.1	7.	10.9	10.1	15.4	15.4	11.1	11.1	59.8	101.6	146.6	51.1	47.1	43.1	121.8
97.5	27-.5	101.3	1502	797	474	730	909	1252	531	294	705	766	574	671	574	814	497	497	320	318	272	270	186	186	267	266	206	206	290	268	154	154	157	153	136	94	344	202	306	175	276	254	390	390	284	284	1510	38	52	18	18	14	1-68
		223.2	59.1	31.8	16.1	28.8	35.8	49.3	20.9	11.8	27.8	30.2	22.6	26.4	22.6	32.1	19.6	19.6	12.6	12.5	10.7	10.6	7.3	7.3	10.5	10.5	8.1	8.1	11.4	10.6	6.1	6.1	6.2	6.	5.4	3.7	13.5	7.9	12.	6.9	10.9	10.	15.4	15.4	11.1	11.1	59.4	99.6	144.6	50.1	46.1	42.3	119.8
99	27-.9	104.3	1490	790	474	724	903	1244	528	291	701	762	569	668	569	806	492	492	317	315	269	267	184	184	265	264	204	204	290	268	154	154	155	151	134	93	342	200	304	173	274	252	388	388	284	284	1500	36	50	17	17	13	1-58
		229.6	58.6	31.7	16.1	28.5	35.6	49.	20.8	11.8	27.6	30.	22.4	26.3	22.4	31.8	19.4	19.4	12.5	12.4	10.6	10.5	7.2	7.2	10.4	10.4	8.	8.	11.4	10.6	6.1	6.1	6.1	5.9	5.3	3.7	13.4	7.9	12.	6.8	10.8	9.9	15.3	15.3	11.1	11.1	59.	97.6	142.6	49.1	45.1	41.5	117.8
1	17	40.	1451	775	365	694	868	1198	515	282	696	726	547	602	541	771	474	473	305	303	263	263	175	175	255	256	191	191	290	268	154	154	133	132	116	90	330	194	294	164	264	242	376	376	284	284	1450	30	42	14	14	11	1-32
		88.4	57.2	30.5	14.4	27.3	34.2	47.2	20.3	11.1	27.4	28.6	21.5	23.7	21.3	30.4	18.7	18.6	12.	11.9	10.4	10.4	6.9	6.9	10.1	10.1	7.5	7.5	11.4	10.6	6.1	6.1	5.2	5.2	4.6	3.5	13.	7.6	11.6	6.5	10.4	9.5	14.8	14.8	11.1	11.1	57.1	94.6	136.6	46.1	42.1	39.9	111.8

Also the measures of Miss _____, taken _____, 189 , by _____.

Mrs Anna Wood
Wellesley College
Mass.
USA
Oct/92

UNIVERSITY COLLEGE LONDON
GALTON PAPERS

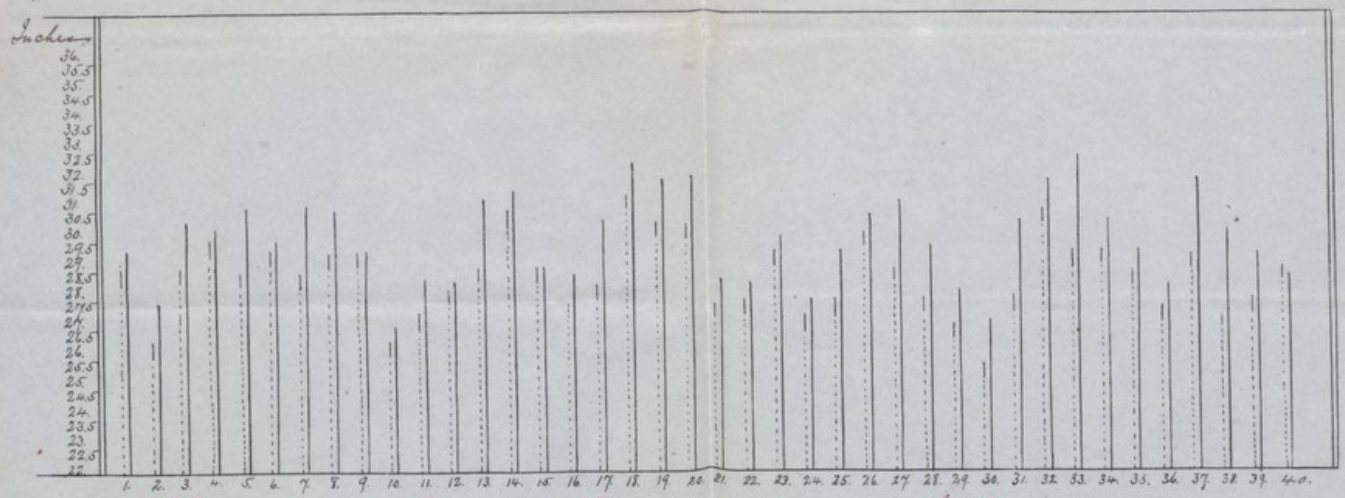
Department of Physical Training. Wellesley College, Wellesley, Mass.

Lucile Eaton Hill. Director of Gymnasium. M. Anna Wood. Physical Examiner.

The records of forty Freshmen who have used the Gymnasium forty-five minutes in the day time, three times per week for six months during the Fall and Winter of 1891-92. ×
The dotted lines represent the measurements of the students in November 1891.
The continuous lines, the same students in May 1892.

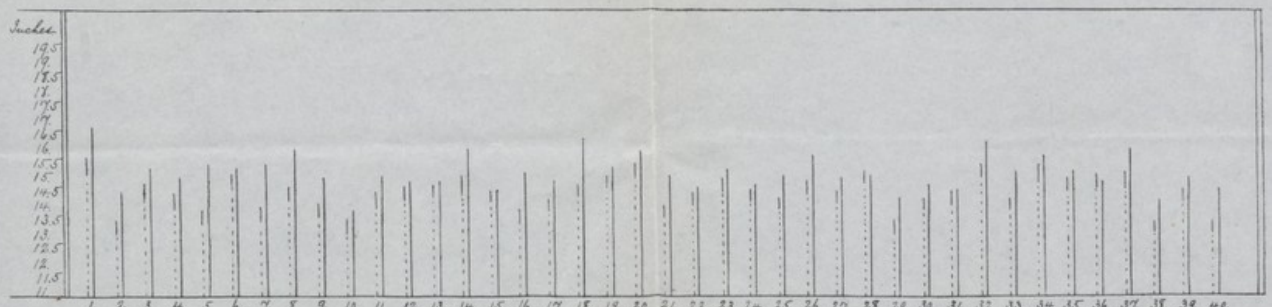
Arranged by M. Anna Wood.

Girth of Chest.

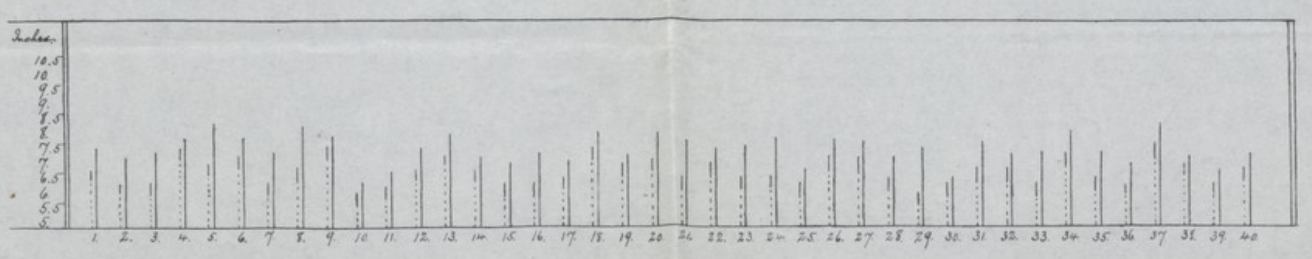


× The Swedish System of Educational Gymnastics was used in this Course.

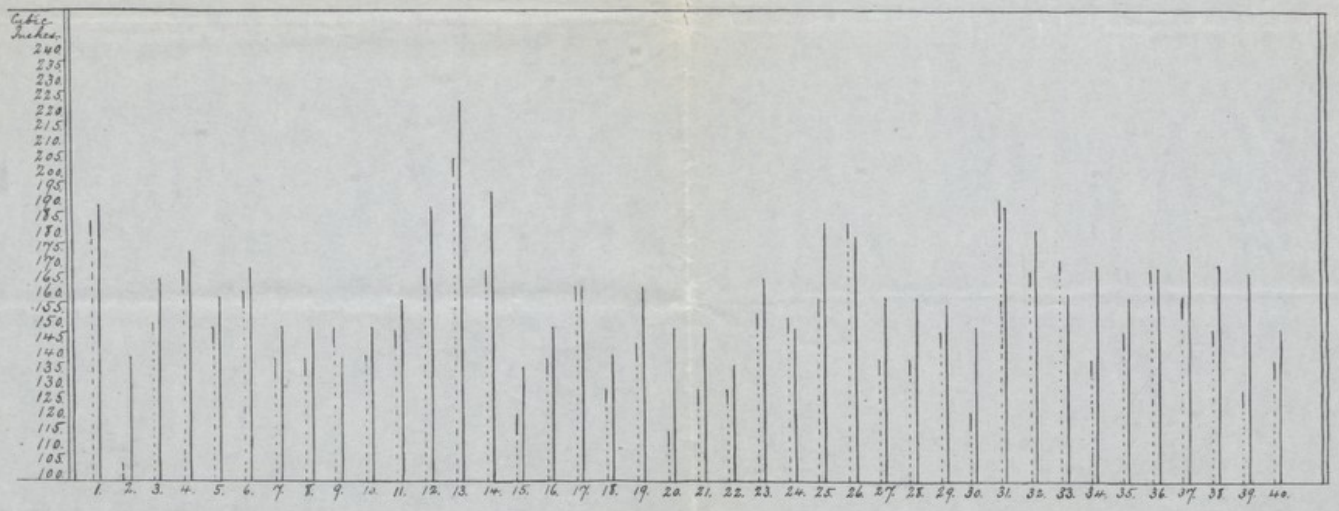
Breadth of Shoulders.



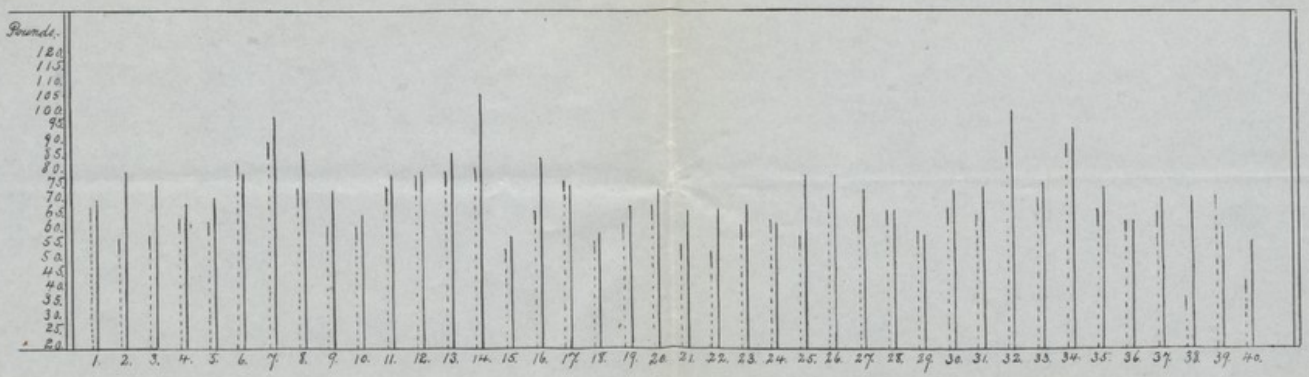
Depth of Chest.



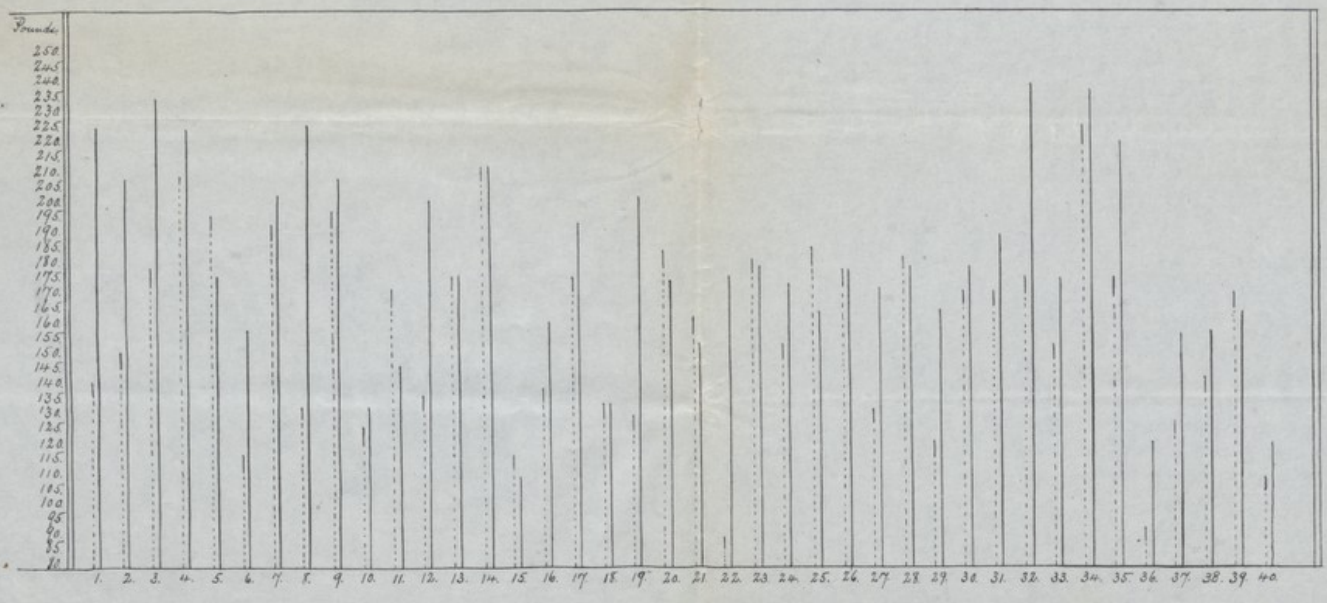
Capacity of Lungs.



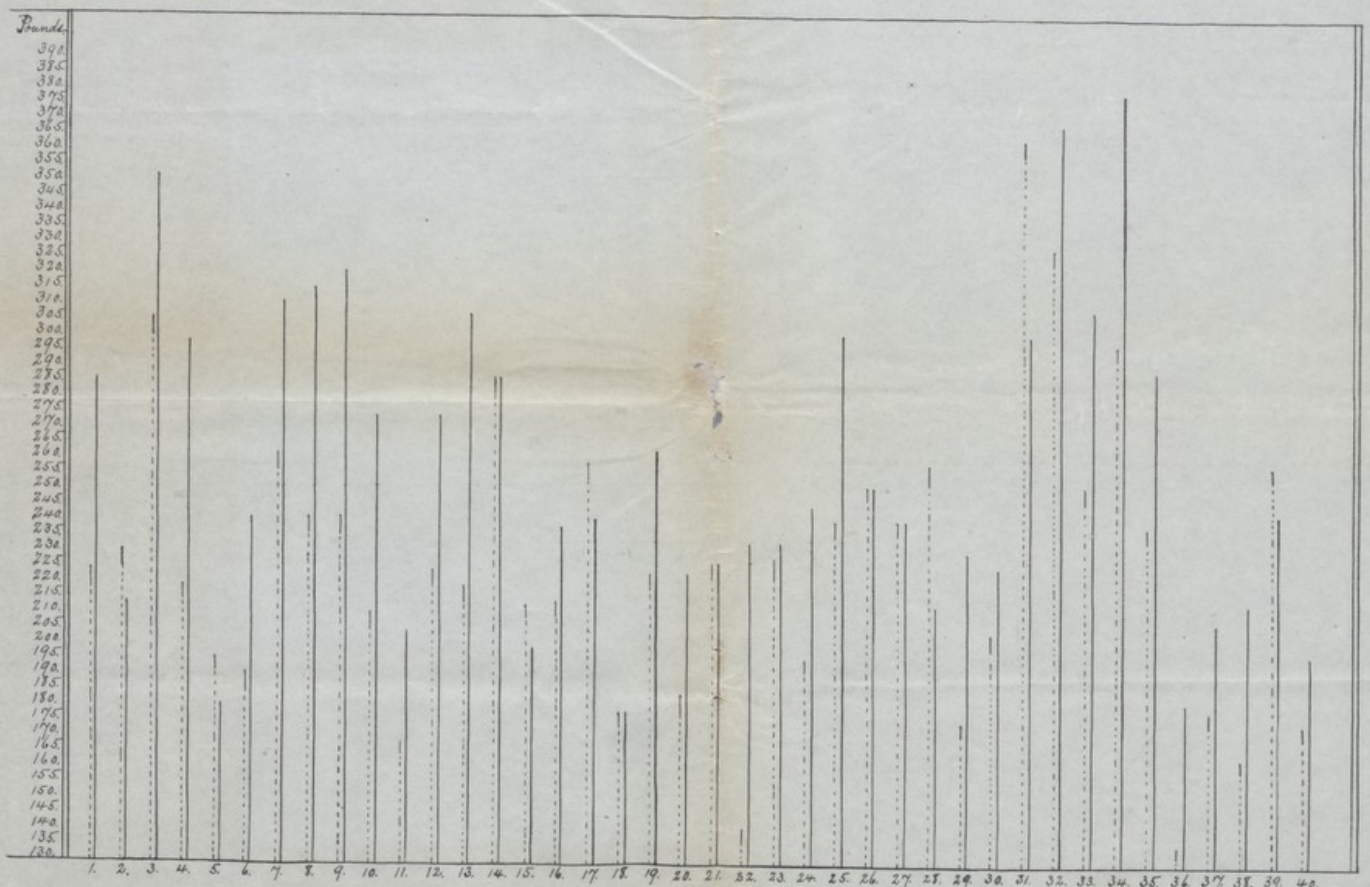
Strength of Chest.



Strength of Back.



Strength of Legs.



Frederick A Woods
Feb 16 1903

936 Beacon St
Brookline
Mass.
U.S.A.

rec'd March 12
answ March 12
will send copy of Bacon Hounds
on my return

UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
3/3/22/45

Francis Galton Esq.

Dear Sir:

I was pleased to receive your letter of Aug 4th relative to heredity articles in Popular Science Monthly. I was away in Japan at the time so I could not reply until now.

Regarding the addition of a fourth factor namely "Sports", I have

what is more important
the fear that my measure-
ments of them are not
sufficiently accurate.
However it would be
very interesting to see
such results and I
hope at some time to
do it in a proper
manner. In the March
number there will be
a summary and some
curves — I am very
anxious to know what
you think of them

especially the 3rd point of regression on the mean. Will you be good enough to send me criticism about this when the magazine comes? I find that the second degree of relationship gives only $\frac{2}{3}$ the resemblance that the nearest or first degree gives. Do with this pretty nearly right? Of course I appreciate that it would have been better to have taken each degree of

much to include that under heredity, using the term in its broadest sense and expecting a certain number of exceptional inheritances to appear. Such were Catherine II of Russia, William the Silent and others. The March article will treat of this.

I have always wanted to correlate them according to your law; but have been deterred for two reasons — the great amount of extra work and record

relationship seperately instead of including in one group fathers sons brothers sisters and in the so called "second degree", grand fathers grandsons uncles and aunts.

This lumping together, you will see I have done and I may take them up more in detail later on.

It has seemed to me all along that

these records show that environment must have played a small part, at least among these people. Does this strike you as a proper deduction to make in this instance?

I have another favor to ask you for besides the above mentioned criticisms. Have you still left, one or two of the reprints on Basset Hounds that you

could spare me? (7)

I should like two as I want to supply a deficiency in one of the smaller libraries here.

Thanking you again for your letter -

Very truly yours

Frederick A. Woods.

Feb 16th 1903.

UNIVERSITY COLLEGE LONDON
GALTON PAPERS