**Correspondence D**

**Publication/Creation**

1841-1910

**Persistent URL**

https://wellcomecollection.org/works/cpgdtdu2



Wellcome Collection
183 Euston Road
London NW1 2BE UK
T +44 (0)20 7611 8722
E library@wellcomecollection.org
https://wellcomecollection.org

H Graham Dakyns.

2. XII. 10

HIGHER COMBE,
HASLEMERE,
SURREY.

My dear Sir Francis

I very much enjoyed Prof^r Dorchne's account of Kantharkare – yesterday – & like the additions – particularly about the resemblance of the young women to Guido's Horus. – Would not a reproduction of the engraving or a photogravure of the picture – make a pretty frontispiece to the book?

I hope you didn't bother over my minute criticism. I can't recall the particular sentence referred to here I boggled over the grammatical form; the sense being plain enough. "Its absence are." Wouldn't if "Its freedom from..." or "Immunity from" will please me better –

I am looking forward to my next visit – but I find on Tuesday Wednesday & Thursday I have to be in London. So it must be Monday?? or Friday? Perhaps Miss Biggs will kindly let me know if Friday suits – or that fortnight Monday you will have seen Methuen by that time.

Yours affectly
H Graham Dakyns.

GALTON/3/3/4/1

Nov. 8. 1910

HIGHER COMBE,
HASLEMERE,
SURREY.

Dear Sir Francis.

A line (1) to say I am looking forward to lunching with you tomorrow. and also (2) to congratulate you most heartily on having the Copley medal awarded to you for researches on heredity. by the President & Council of the R.S.

I am just off to London for the day. I am yours ever truly

H G Graham Dakyns.

To Sir Francis Galton F.R.S.

INGLESIDE,
LEE, S.E.

May 18. 1896

Dear Sir Francis Galton

In reply to your interesting Enquiry I may say that amongst the saprophytic forms I studied with Dr Dreydale, and have been continuing to study "arrest" in fission, and "abortion" in fission was extremely common. I cannot readily appeal to old Diaries

at this moment; but I am safe in saying that at least five per cent of ordinary fissions would go on to about one half or less of the characteristic division, and there stop. Some arrest of vital action occurred, which while it did not leave

the organism inactive, left it powerless to continue the process of self-division; and after half an hour or more of aimless or at least irregular swimming it would become still and dead.

The same was true, but not probably to a greater extent than three per cent in the fusions. They often commenced, and often at the point where the nucleus in each of the organisms took part in the fusion, there was an arrest of action

leading to abortion.

In like manner I have known "crippled" or distorted forms, which have been followed from the germ. I have however never seen a crippled or malformed putrefactive form which has has gone completely through the act of fission.

If the class of work for which science is so much indebted to you had been initiated when my earlier work on these unicells was done, I am sure I could have given answer to many curious questions. I hope I have made my note plain and remain very sincerely yours W H Dallinger

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

F.1r

# POST CARD

THE ADDRESS ONLY TO BE WRITTEN ON THIS SIDE.

HALFPENNY

BECKENHAM 15

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

From Mr. C. Darwin, Down, Beckenham.

One line to thank you for Photograph.
My little Biography has turned out, alas,
very dull & has disappointed me much.
May your tour turn out pleasant
under a better sky than our
detestable one. —

C. D.

15th —

f.2c

POST CARD

THE ADDRESS ONLY TO BE WRITTEN ON THIS SIDE.


Prof Galton

5. Bertie Terrace

Leamington

From Mrs. C. Darwin, Down, Beckenham.

f.2 v

The specimen parcel of drawings, M. S. &c arrived quite safely last night. Everything shall be safely kept.

Many thanks. C. D.

April 2d.

April 27th

My dear Cousin Emma.

I am writing for my Mother to thank you for your letter. She begs me to give you her affectionate love & to say that your letter comforted her. We have had a great deal of sympathy & it is soothing

to feel how many appreciated our dear Father's goodness.
He always held a very real affection for your brother & took real pleasure in his company.
My mother is well & is able to go out & lead a usual life.
Yours affectionately
E Darwin
My mother desires her best love & thanks to Mrs Wheler for her sympathy

Down.
Bromley.
Kent. S.E.

Aug 5.

My dear Emma

How shamefully I behaved & I should never have remembered but here are the signatures. George will be very proud to do the Wedgwood arms

in his best Style for

you.

Jessie Allen is my

second cousin or so &

I know all about

her now.

We have not seen

the Edin. Rev. yet but

f.3

we shall certainly
read the Article with
interest now we know
the Author.
Will you remember
us both very kindly
to Mrs Galton &
I am yours affectly
C. Darwin

Down Beckenham
Sep 2 -

My dear Emma

The death of dear Erasmus has been a very bitter blow to Charles - though it ought not to have been unexpected; but he had recovered so often before that we were hopeful

almost to the last –
He was so kind &
charming to the young
that all my children
feel the loss to be that of
more than an uncle.
He was taken ill on
Monday with a sort
of bilious attack, not of

a severe character; but he had no strength to bear up against it - The end was without suffering. It is a happiness to feel how fond every one was of him outside of his immediate circle - With Charles's love & kind regards to your sister

your affectionate Cousin
Emma Darwin

f.8

f.9



Charles Darwin

Charles Darwin

f. 1

WYCHFIELD,

CAMBRIDGE.



Dear Galton

Our Board and Board I agreed to recommend £100 for our Lool^n Comm^ee. I thought you wd be glad to hear this, tho' as you know

f.2

it has to pass a sort
of general commee

Yrs sincerely
Francis Darwin

WYCHFIELD,

CAMBRIDGE.

Feb 25.97



My dear Galton

At the meeting of Board E for the preliminary consideration of applications for Sort Grants, there was an application for £100 from the Measurement of Animals & Plants Committee. It was rather

coldly treated; & more especially it was asked what we proposed to do with £100. I have written to Weldon who cannot give any definite inform". I suppose the fact is that we have nothing definite.

It might do good if ~~we~~ I could mention some of the schemes of people

like Wallace Morgan &c who have mentioned things they want doing - Could you give me some of them?

The meeting is on May 6.

I hope you are better?

Yours truly

Francis Darwin

47

F.9

WYCHFIELD,

CAMBRIDGE.

Nov 7 '89



My dear Galton

I see a letter from my father dated Nov 7.75 he writes to you about your Theory of Heredity published in Journal Anthropolog Institute '75 & also I think in the Contemporary Review

I cannot be sure of
his meaning in one
or two places because
he refers to numbers
marked on a copy of
yr paper or on a proof
Have you by good luck
got this marked copy
& if so might I see it

f.11

It is a very interesting letter both humanly & scientifically

Yours truly

Francis Darwin

WYCHFIELD,
CAMBRIDGE.

Nov 4 · 88



My dear Galton

Many thanks for your kindness I have a copy of Heredity & I therefore send back your copy –

I also enclose a copy of ~~the~~ letter and ~~you~~ my copy of heredity – if you would mark it with numbers corresponding to the letter

I shall be glad

Numbers 2, 3, 4 are plain enough.

Number ① seems to refer to p 329 of the pamphlet But in my father's sentence he speaks as if you denied that the organism was ~~not~~ affected by use & disuse apart from inheritance. And this of course you do not

No 5 doesnt matter

No 6, 7, 8 I have not

made out – I should like to know what they refer to, but it is not worth any real trouble on your part

Yours truly
Francis Darwin

f.16

13, MADINGLEY ROAD,
CAMBRIDGE.

Oct 6.08



Dear Francis Galton

I have been away or I should have written to thank you sooner for your letter. I am ashamed to think that you should have read my poor address 5 times. What I say p17 about Weismann's (p17) theory of unequal division is I think right – but it is not always easy to get the dernier cri of Weismannism. Your view of somatisation being connected with latency, instead of with the sorting out of determ

ments. seems to enormously
superior to Weismann; and
also I should say (tho' you
will not I am afraid)
much more applicable to
a memory-theory of develop
ment than is ~~Weismann's~~ "
biophor mechanism.
I must think over what you
say about gemmules, and
indeed all that you say
I am glad you allow that
acquired characters may
conceivably be inherited
I don't think I shall make
a book of the address. I want

to get back to nice straight forward experiment instead of making my brains sore with theoretical stuff.
I think you have treated me very kindly, I expect I shall get some strong language from the Weismannians.

Yours sincerely
Francis Darwin

The 1st par on p 12 was written after much talk with James Ward our psychologist here who told me that like all biologists I try to get the best

f.26 l

of both worlds the psychic & the physiological; and in despair I tried to define my position as regards psychology. I believe the psychological objection to my kind of stuff is finally mystic. They are driven to something which is neither matter nor mind. to which various names are given. For all practical purposes one must treat psychology as part of physiology

Frank Darwin f.21

13, Madingley Road,
Cambridge.

Oct 16. 09

Dear Sir Francis

One Tocher who is the author of two papers in Biometrika is a candidate for the Sorby Fellowship of which I am an elector. I wonder whether you would give me your opinion of the work

in question viz.

Anthropometric Characters of Inmates of Asylums Biometrika V 1907, and

Pigmentation Survey &c Biometrika VI 1908

I do not mean to trouble you for a minute, detailed opinion, but for your general of view of the man's power, & of the value of his work.

f.23

He proposes to continue
the Pigment Survey
if he gets the Fellowship
Do you consider it
worth going on with
Please do not answer
me unless your health
& other circumstances
are favourable
Yrs sincerely
Francis Darwin

f.24

F.1

Down Beckenham
Kent.
Sep. 23. 70

My dear Mr. Galton,

I was much interested in the paper which you read before the Brit. Assoc. the other day on barometric predictions of weather; and so I thought that you wd not be displeased by my sending

you the following observation, (you will excuse my troubling
on what you said then — (you) is that this letter
or rather ~~&~~ on what ~~shd~~ be waste paper.
I imagine you said, for and that I ~~shdd~~ find out
I am afraid that I was when I have gone wrong
unable to follow all when I read the volume
your arguments. If however of the Assoc. Transac.
I have attributed ~~to~~ you As I understand it, the
opinions which you do object of your paper was
not hold — the worst to show — that there is a
that can happen (provided general + pretty exact similarity between

the barogram & the wind velocity curve – the wind being observed every 12 hours; & further that at any period the barometer is worse than no guide at all (? for the prediction of weather during the next 6 hours) in the proportion of 4 : 3.

If this statement be

verbally correct them by (2

making a rising barom.

indicate bad weather

& vice versâ we obtain

a prediction of the future

weather wh. is better

than none at all in

the proportion of 4 : 3. –

But this is of course

absurd & therefore I

must suppose – ~~that~~

bearing in mind the method by

wh. I believe you obtained the

~~meaning of that~~

result — that your meaning is that it is better to suppose the weather will continue as it is than to consult the barometer ~~is~~ that is, better as 4:3 to make the round curve a straight line (for the next 6 hours?) than to make it follow the barogram.

This I suppose possible

to construct the barogram & wind curve up to any clock; & it is ~~must~~ then ~~be~~ upon these two unfinished curves that the prophecy of the future weather must be based. Now it is not so much the absolute height of the barometer wh. ~~indicates~~ goes with ~~approaching good or bad~~ varies with the weather as it is that change of

height goes with change of weather. We must therefore include in our idea of the agreement betw the two curves their parallelism as well as their coincidence. The directions of the tangents of the barogram + wind curve at the time at which the prophecy is to be made indicates the rate of rise + fall ~~Now if the barogram~~ of the barometer + wind

Now there is a general agreement betw. the 2 curves, ~~it is only~~ but sometimes, the barogram lags behind & sometimes it outstrips the wind curve. It is only in the case when the barogram outstrips the wind that a ~~false~~ prophecy

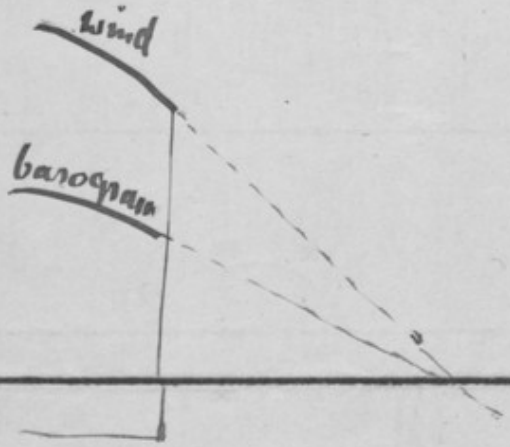
can be made, for in the other case the change comes before the ~~means of~~ indication of it. ~~Now~~ If the very barometer is rising or falling faster than the second curve it is possible to make a prophecy. At the spot at wh. we are to prophesy tangents shd be drawn

h: the two curves &
their position shd indicate
whether the barogram
is outstripping or lagging
behind the wind curve
& accordingly whether
a prophecy is possible
or not. I have drawn
on the next page of
paper the 10
various forms in wh.

F8

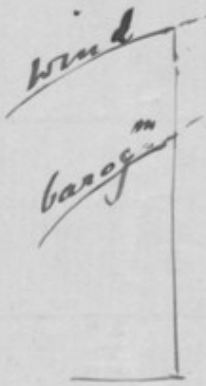
the two curves may
lie, grouping together
there in wh. predicters
shd be allowable

Prediction inadmissible


Prediction admissible

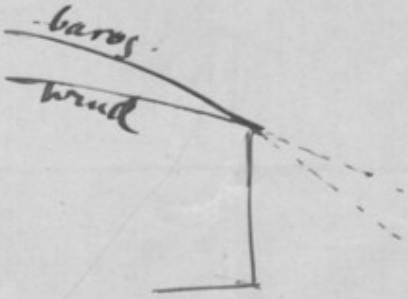
5

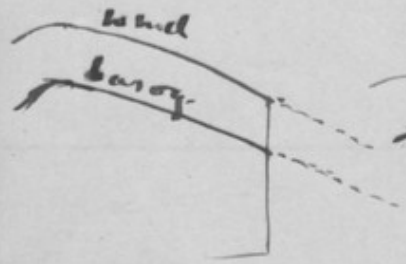




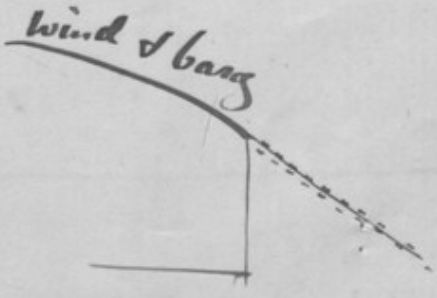


f.9r








f.9v

Again in the first 3
cases in which I have
put prediction inadmissible
it wd seem more likely
that the wind shd go
on rising or go on falling
than that the curves shd
come to an abrupt
change. So that I
shd have thought that
in those cases too
when the change has

once set in that the barometer wd still be better than no guide at all..

Of course of the 3 cases in which I ~~thought~~ think that prediction shd be ~~true~~ ~~been~~ possible do <u>not</u> occur in nature the use of the barometer in prediction wd be restricted to the cases

I last spoke of — but I thought I understood you to say that the barometer was as often before the wind as not. That is to say besides the cases in which prediction might be made there are the cases in which a prediction of still greater increase or diminution of wind might be made.

I am thus rather at a loss to understand in what sense the barometer could be said to be worse than no guide at all.—

In Springtime N.E winds prevail there must be considerable portions of the barogram & wind curve which do not agree at all well because the barometer always rises with such a wind

I believe you did take such winds into consideration, but was it not necessary to compare the barograph found curve in a different manner, in order to show their agreement in such a matter?

If my complete ignorance of the experimental side of the subject has

led me to say anything very absurd in this letter, I hope you will be lenient to me & pardon me for troubling you. –

Of course I do not wish ~~[illegible]~~ thus wishing you to enter into a controversy on the subject but only because it has amused me to write it – & I thought it might possibly interest you. – I went away from Liverpool sooner than I intended because I was not very well.

Did you hear Stuart's Paper?

Yours very sincerely

G H Darwin

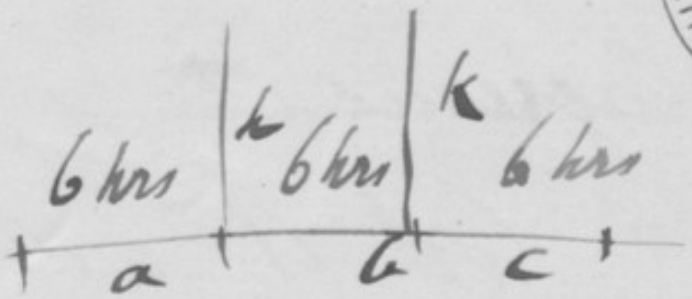
Down
Beckenham
Kent
Tuesday Sep 27.70

My dear Mr. Galton,

Thank you for sending me the abstract of your paper. I see that I did misunderstand you. One thing which misled me (& I think others) was the expression "worse than

no guide at all" which wd have explained itself better to me if it had been "worse than an assumed constancy in the av: wind vel:". –

I see in your method of prediction from the barometer your formula ~~leaves to~~ eliminates the av: vel: during b;




Altho no advantage seems to be taken of our knowledge of the immediately previous state of the world. The desideratum seems to be some formula which wd involve this knowledge — altho I confess I do not see

the least fault in your argument.

Query? Would it not do to assume

av: vel: dur. c = A(h-k)

+ B av. vel dur. b

& find A & B so as to suit experience as well as possible. I feel however that this is a feeble suggestion

Sept 27. 70 F21

My father sends his thanks for your rabbit message & says that he is deeply interested in the success of the experiment. –

Yours very sincerely

George H Darwin

F.21

Monday
Oct 3. 70

Down.
~~Farnborough~~ Beckenham
~~Farnborough S.E.~~

Dear Mr Galton,

In my first letter I tried to separate the cases in which prediction of the weather shd be possible from those in wh. it shd not be so, & altho' I now see that what I then said is not exactly applicable yet I think that the same idea ought to enable one to make use of that knowledge of the

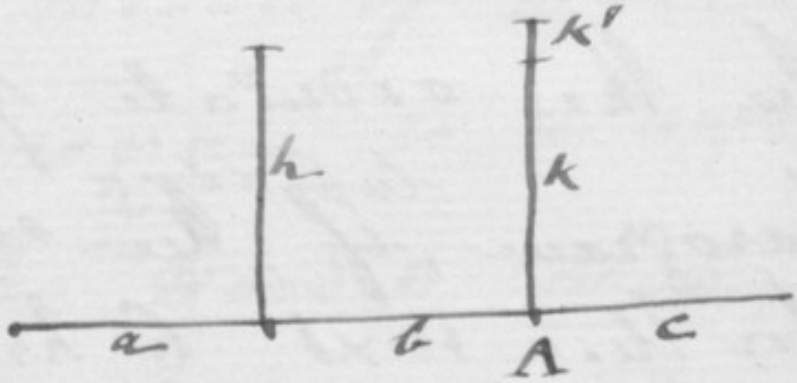
present weather wh. shd be taken advantage of in the prediction.

Consider a 12 hr-period at the middle of which the barogram is in agreement with the av. wind. v. curve; now if during the first 6 hrs the av. wind vel. has been too great to produce the agrmt betw. the 2 curves, during the last 6 hrs the av. wind. vel. must

be smaller than the avge
for the whole 12 hrs &
vice versa. At any period
we have sufficient data
to determine what ought
to be the ordinate of the
barogram, supposing the av. wind vel
for the next 6 hrs shd
be identical with what
it is for the past 6 hrs &
on comparison with the
actual ordn. of the barogram
we can see whether the
~~future~~ next 6-hr av. wind vel will be

greater equal or less than what it has been for the last 6 hrs. This will explain itself more clearly if I use symbols






Let a, b, c be 3 succ. 6 hr periods A being the present time so that c is future & let $v_{\frac{a+b}{2}}$ mean the av. wind vel for the period x – then

$$v_{a+b} = \frac{1}{2}(v_a + v_b)$$

$$\& \quad v_{b+c} = \frac{1}{2}(v_b + v_c)$$

I see you talk in yr paper of the height of barom. but

The Mote,
Tunbridge.

here mean by $h+k$ the ord. of the barog.
i.e. – ht of barom.

Let $k'$ be the ordte of the barogm in the supposition that $v_c$ will $= v_b$ – then $v_{b+c} = v_{b+b}$
$= v_b$

& by yr formula

$$k' = h - 2(v_{a+b} - v_{b+b})$$
$$= h - v_a + v_b$$

Now if the actual ord. of the barogram be $\left\{\begin{array}{l}\text{smaller than}\\ \text{equal to}\\ \text{greater than}\end{array}\right.$ this hypothetical one (i.e. on the supposition

of the future steadiness of the
avge wind vel.) it shows
that $v_b$ is $\left\{\begin{matrix}\text{above}\\ \text{at}\\ \text{below}\end{matrix}\right.$ its due
value & therefore that during
the next 6 hrs (period c) there
will be a $\left\{\begin{matrix}\text{fall}\\ \text{steadiness}\\ \text{rise}\end{matrix}\right.$ in $v_c$ the
avge wind vel.

This may be put in symbols

$$v_c \lesseqgtr v_b \text{ as } K \lesseqgtr h - v_a + v_b$$

$$\text{or as } K - h \lesseqgtr v_b - v_a$$

$$\text{or as } \begin{matrix}\text{fall in}\\ \text{barometer}\end{matrix} \lesseqgtr \text{rise in wind}$$

f27

I do not see any flaw in this reasoning & if you do not either (& I hope you do not) it might be worth while to apply it.

But in this formula, as well as in Eqs, ~~not~~ only as much ~~weight~~ but even more weight in the determination of the future wind vel. is attached to the value of the wind vel. in long past periods than to the present wind vel. –

I think they might be weighted in descending order as follows

$$v_a = 2(h-h') - (1+p)v_f + (1-q)v_e + (1-p+q)v_d$$

[illegible] by the [illegible]

The sum of the 3 coeffts must of course = 3
& $1-q$ must be $> 1-p+q$
put $p = 2q+r$
Then it might be written

$$v_a = 2(h-h') - (1+2q+r)v_b + (1-q)v_c + (1-q-r)v_d$$

$q$ & $r$ wd of course have to be determined from experience

This formula is very similar to the one I suggested the other day & seems to have some feeble reasons for its existence



Your formula might be treated somewhat similarly

$$h-h' = 2\left[(1+p)v_{a+b} - (1-p)v_{b+c}\right]$$
$$= (1+p)v_a + 2p\,v_b - (1-p)v_c$$

$$\therefore v_a = \frac{1}{1+p}(h-h') - \frac{2p}{1+p}v_b + \frac{1-p}{1+p}v_c$$

as the coeff of $v_b$ must be $>$ that of $v_c$
$p$ must be $> \frac{1}{3}$
Perhaps it wd be necessary to restore yr coeff $m$ & not to put it $= 2$

All such weighing (qu. do you agree that it should be attempted?) as that in the two above formulae must of course be arbitrary & there are so many possible ways of doing it that one is at a loss to know what is likely to be the best.

Pray do not trouble Lyell to answer this unless you have anything you **want** to say.

Yours sincerely
C. Darwin

The Mount,
Cumbridge.

I am here till Wednesday – My uncle Josiah Wedgwood has taken it for 2 years. Most of the house is of 1400 AD & is quite

f.33

6 Queen Ann St
W
Sunday Aug 28. 81

My dear Galton,

My uncle died late on Friday night, just sinking away under the prostration following one of his attacks. His death is a great loss to all of us.

Ever yours

C. H. Darwin

4 Bryanston St
Thursday Dec 7. 82

F.34

My dear Galton

Your etching of the study awaits you here. I have told the servant to let you have it when you call. It is on thin paper lightly gummed to a large sheet of card-board ~~about~~ 1ft 9 × 2ft 4 ~~[illegible]~~ ~~[illegible]~~. I think you might take it safely ~~on~~ a 4-wheeler by help of a large sheet of brown paper — or yr picture-framer might send for it with a note from you. I hardly think I shall be able to call this time. I intended to be in town sooner, but was delayed by a severe cold on my chest

which has still not quite left me. I therefore intend to stay in doors as much as my business will let me. I leave for Glasgow on Sat. M.? if the lines are not blocked by snow.

Old Challis died on Sunday as you may have seen & so I am preparing to be a candidate for his chair. The absurd thing is that there is apparently no one to elect. The old electoral board is dead & the new one not fully constituted. I suppose the difficulty will be got over somehow by driving a coach right thro' the statutes.

I hope to leave for somewhere Southerly shortly after Xmas

This autumn has been partic^y
trying & I seem only to shift
out of one cold into another
I hope Mr Galton is well

Ever yours

G. H. Darwin



f.36

Camb. 6.45. 16 Jan.

My Dear Salton

Since I telegraphed I have heard that I only had a majority of one & that the votes were.

| D. | R. |
|---|---|
| Spottiswo. | Stokes |
| Smith | Christie |
| Cayley | Ferrers |
| Adams | Porter |
| Todhunter | |

Thus except Stokes (an important exception) I had all the weighty names on my side.

Well it doesn't signify how it happened, so that it did so — but I confess perverted to having been a little too confident of the result. I suppose I ought not to know how the votes went, for neither Spottiswo nor Stokes said a word, but the V.C is not judicious & told me all about it. Thanks for yr sympathy. I shall be here at least to end of week

G.H.D

F.38

Dear Galton

Many thanks for yr letter. It was a new thing all the same – for Stokes, who I thought wd be for one went for Routh Spott. Smith – Adams Cayley Todh. for me

Ever yours

G H Darwin

From. Coll. Camb.
Sept 26. 84

My dear Galton,

I had the pleasure of unpacking your charming present yesterday in London & of showing it to my wife. We both like it very much. I am down here alone for a day & return to Down tomorrow. It is ever so nice being back at the old place.

We landed last Sunday after a good voyage.

f.42

I must tell you of an extraordinary leader in the N.Y. Herald on you when we meet — & may that be not long hence!

I hope Mr Galton is well

Ever yours affectionately

G. H. Darwin

P.S. I want a manservant — does Giffy (?) know of a compatriot of his who wants a place

May 28. 85

NEWNHAM GRANGE,
CAMBRIDGE.

My dear Galton

I am sure Mrs Galton & you will be happy to hear that my wife was confined of a girl on ~~Thursday~~ Wednesday & that both are doing well.

We had somewhat made up our mind

for a boy "Charles Galton"
but shall soon get
used to the fact.
I have taken the
first step by getting
the Life History
Album.

Ever yours

G H Darwin

F.47

Dec 21. 87

NEWNHAM GRANGE,

CAMBRIDGE.

My dear Galton,

I am sure that Mrs. Galton & you will be interested to hear that Maud bore a boy on Monday & that they are both fairly as well as possible. We intend to call him

Charles Galton Darwin

and we hope he may
not disgrace any one
of his three names.
Maud sends her love
to Mrs Galton & wishes
me to say how sorry
she was to hear of
her illness
Ever yours affectionately
G H Darwin

f.51

11. 6. 88 f.52

NEWNHAM GRANGE,

CAMBRIDGE.

My dear Galton,

I had utterly forgotten the christening service & when I came to look at it I saw the reason for your hesitation & quite sympathise with it. I am only sorry I gave you the annoyance of declining. I knew you don't wish the young man

the worse for it, but only the better.

Everyone.

G H Darwin

Such a hurly-burly that I can't write before I am half dead with fatigue

f. 55

f. 56

8 Jan

My dear Sutton

You are always measuring things & I have not noticed that you ever used one of these little things. Look you accept one from me.

Just off to Cambridge

23 May '97

NEWNHAM GRANGE,
CAMBRIDGE.

My dear Galton,

I send you herewith an extensive revision of the Report. I have noted in red several points which ought be cleared up. I have not scrupled to cut it about as much as I like. It is much more difficult to write decent English in this way than when one makes a fresh start. I feel pretty clear that most of my emendations are improvements. Scott's style is too awful for words.

Will you resettle my draft & as you will be acting Chairman ~~to~~ decide what is to be done about printing it. Buchan was going to send me his comments

but has not done so. In order not to delay I send mine now. We meet at M. O. at 11 on Thursday for audit.

Such a scene here on Friday! Most amusing it was.

Yours

G H Darwin

I feel indignant with R.S. for not consulting Stackey. – Shaw is the very best man – but know about Oxford susceptibility.

4

F.60

G. H. Darwin

on Trinity Fellowship

f. 61

Nov. 19. 02

NEWNHAM GRANGE,

CAMBRIDGE.

My dear Galton,

We are delighted at the two well-deserved honours you have just received — our hearty con-gratulations. The Master read me yr. letter to day — I hope you may shake off the asthma soon. The Litchfields go to Cannes (the Continental) on Monday, and are I believe none the worse for the journey. I mention this so that you may know their address if you go there.

We have Wolcott & his sister-in-law Theodora Sedgwick staying with us now.

He has determined to give up Southampton & will believe settle next door to Leonard in Egerton Place. I think he is very loose to move, as it will be too dismal for him all alone down here.

He is sadly crippled but the wonderful how he gets about on his hand-tricycle going about 6 miles an hour for over an hour at a time.

I think we shall hear good accounts of your fortnight

f.63

of time – I have not a very clear idea of V. plans – I think however I heard Tenos mentioned. It is a most interesting place & I hope not much spoilt since I was there in '84

Ever yours affectionately

C. Darwin

1

f.64

F.65

Sir George Darwin 20 Brock St
Bath
Jan 8. 09

My dear Galton,

You probably see the Times & will have seen the article on the Police & finger-prints — on Jan 4 — my letter on Jan 6, & subsequent letters on 7th & 8th.

I don't know whether you assent to the views put forth by some of these correspondents, but I note that today it is denied that the Scotland Yard registry is on your principles. This is just as my assertion but it is quoted verbally from your 'Memories'.

There is an implication in today's paper that yr. method of registry was condemned by a Commission on its merits. But I conceive that the truth is that the first Commission was silly & the second was wise.

I still think the Times article was very unfair to you, but I shan't enter into it any further.

I am down here with my wife because I have had a bad knee. It is beginning to get a little better now, but I suppose I shall be here for two or three weeks more.

I believe Charles was going to walk over from my sister's to see you. I hope he did so.



Yours affectionately

G H Darwin

Your niece Lucy (née Wheler) has written to me about a portrait of old Erasmus. I think she have got enough of them for the little exhibition next June. But she wd. like, I think, to come to Cambridge. I hope we may have some tickets to give to relatives for the Senate House etc. I have asked if it will be possible, but clearly it is expected that there will be a great crush

f.69

Sir George Darwin

June 25. 09

NEWNHAM GRANGE,
CAMBRIDGE.

My dear Sulzin

My wife and I are delighted to see the recognition of your great services, in the birth-day honours this morning. It comes long after it is due, but perhaps this is an almost inevitable consequence of the originality of your work. For when a man starts a new branch (or branches) of science it must be some time even before his confrères become aware of the importance and yet longer before the public knows anything about it.

I wish you could have been here during the last three days (and that the Times did not describe you as Mr. Francis Galton), for it has been brilliantly successful, and has been a marvellous recognition of my father's work.

Edward Wheler was here with Mrs Wheler. I regret the whole time have been in only allowed a few minutes talk with him.

Yours affectionately

G H Darwin

William's speech – absolutely unreported – was the best of the evening at the great dinner.

George Darwin

f.73

Nov. 8. 10

NEWNHAM GRANGE,
CAMBRIDGE.

Dear Sir Francis

I am so delighted to see the announcement of your well-deserved Copley Medal.
May we hope to see you at the R.S. on the 30th — I wish had we may.
We are all going on much as usual but we are bereft of all our children now. Gwen is drawing in London except Sat. to Mond., Margaret is at Somerville Coll. Oxford, Horace at Winchester, and Charles has got a research studentship at Manchester University. It is an endowment of Schuster's & C's nomination arose casually out of Schuster's

mentioning to me that he couldn't
hear of a mathematical physicist
appropriate for the post. He has
no duties except to research &
he is now trying to write an
essay for the Smith's Prize;
but I am not very hopeful
because he has only about
4 months to do it in, whereas
some of his fellow competitors have
had a year & a half. This is
because it is optional whether a
man shd. take the last part of
his Tripos in his 3rd or 4th year
& Charles chose the 4th.
You may have heard of my sister-
-in-law Ida's motor smash in
Bond Street. It was a wonder she

was not killed – but 3 ribs, a shoulder blade & an arm are enough breakages for ordinary purposes. She is however getting on very well & is at Abinger. I don't suppose we shall see her here until Christmas.

I have been witnessing the presentation of Westlake's portrait to Trinity today. He seems very well, but I notice he walks very slowly now.

I am not clear where you are settled now & so address this to Rutland Gate. I hope that you are getting through this autumn well.

Yours affectionately

Ch. Darwin

Pray give our regards to Mrs Bagg

f.76

Sir George Darwin

Dec 20. 1906





My dear Galton,

I am sure that you will be glad to hear that Charles Galton D. has just been elected to a Major Scholarship at Trinity. He was the only Mathematician elected & the Examiners seem to have thought very highly of him. One thing makes it still better is that his great friend Jem Butler came out first on the classical side, while he was first amongst mathematicians.

Sir George Darwin

Dec 20. 1906





My dear Galton,

I am sure that you will be glad to hear that Charles Galton D. has just been elected to a Major Scholarship at Trinity. He was the only Mathematician elected & the Examiners seem to have thought very highly of him. One thing makes it still better is that his great friend Jem Butler came out first on the classical side, while he was first amongst mathematicians.

This will please you too on the hereditary principle! -
I hope you are finding Pau a pleasant winter place. You are well out of England for it has been an abominable autumn & all our household has been having a sort of in-
-fluenza.
I had to go to Buckingham Palace two days ago & so now I'm 15 real article — previously only being half hatched

Yours affectionately

C. R. Darwin

Sir George Darwin

F.81

Dec 23. 08



My dear Galton,

I have just finished reading your memories — with the greatest delight — in 24 hours of reading. [I am unfortunately in bed with a bad knee & am to go to Bath as soon as I'm better.] We are going to book to Charles as a Xmas present, so that he may know his quasi-godfather better. I wish you had put in yr. story of your trigonometrical survey of the Hottentot lady's hinder-end. The photographer story towards the end is splendid. What a lot of work you have done!

I think he writes to
MacAlister
his cousin our Prof.
writes it as I have
done.

F.82

I noticed 8 misscriptic mistakes
wh. you may correct if a new
edn. is called for (as I expect it
will be).

✓ 1. Chree not Cree

✓ 2. The Meteor. Committee is not
large – only two more than we
used to be – 8 instead of 6.

✓ 3. Mc Alister is Principal of
Glasgow Univ.

Pray give our regards to Mrs Rigg
& we hope you are getting on well.
I don't know where you are

Yours affectionately

George Darwin

Can you give me your brother's
address? I am suggesting his name
for an invitation to the Darwin
Festival next June. He ~~perhaps~~ probably will
hardly care to face the fatigue of such
an affair.

f.85

May 12. 05

NEWNHAM GRANGE,
CAMBRIDGE.

My dear Galton

I am very sorry that you do not feel able to take the P.B.A. I confess I am not surprised, but it was courtie to chance to offer it to you. We are very glad you have had a pleasant winter.

It is a hard job that I have undertaken in S.A. & I hope it won't demolish me. My wife and Charles (now 6th 0½in) are coming.

Yours affectionately

G H Darwin

f.86

f.87

f.88

I sent a card to say you were away. A.Z.B.

Nov 6 - 05

NEWNHAM GRANGE,
CAMBRIDGE.

My dear Galton

Can you tell me whether any descendant of Babbage is alive & if yes can you give me his or her address. An American asks for genealogical purposes.

We had a grand time in S. A. I hope to come & see you when next I stay in London

Yours

G H Darwin

So glad about the lecture for College

Nov 13. 05

NEWNHAM GRANGE,
CAMBRIDGE.

My dear Galton,

Many thanks for your card. The good wishes of everybody is the best part of it. I hope Pearson will suit you. I must admit I find this climate infernal after all the sun we have seen — not but what it was awfully hot.

I hear the pictures to hang in the Hall — the only proper place.

I wrote to you some days ago to ask if you knew anything of descendants of Babbage — no doubt you will have got it & will answer

Yours affectionately

G. H. Darwin

f.91

f92

f93

f94

May 6. 05

NEWNHAM GRANGE,
CAMBRIDGE.

My dear Galton,

You will perhaps already have received an official intimation that you were yesterday unanimously nominated Pres.t of the B.A. for the York meeting.

I had the pleasure of proposing your name, and I pointed out that you ought to have been nominated years ago, but that the fact that men of science were formerly somewhat blind to the great work which you have done gave no excuse for omitting even this belated recognition.

That you may not think that this is merely my personal

opinion I should add that speaker after speaker endorsed what I had said.

We all hope that you may feel yourself able to accept this nomination. It was pointed out as an objection that your deafness would be a difficulty, in as much as presiding at Council meetings could hardly be carried out efficiently by you. To this most, ~~if not~~ perhaps all, considered that there was a complete answer – you have only to absent yourself from Council meetings. Throughout present year Balfour never comes – as we knew he would

f 96

not – and we get through our business with the aid of V.-P.'s. I hope then that you will not allow this consideration to deter you from acceptance, and, if you will take it, ~~accept~~, my advice to you wd. be that you shd. not attend any Council meetings during your year of office when you would have to take the Chair – or at least shd. ask a V. P. to preside.

I cannot of course judge whether you will feel yourself disposed to undertake the duties, but I can only very heartily express the hope that you will feel you have the strength to do so.

Yours very sincerely

G. H. Darwin

f97

F98

Tuesday May 9 1905

My dear George Darwin. It was only last night that I returned to England & found your very kind letter & that of the Gen. Sec.[y] offering me the Presidency of the B. A. for York. I am deeply sensible of the honour, & fully recognise the unique opportunity it affords of addressing the whole scientific world ~~on the~~ and of drawing their attention to subjects of especial interest to the speaker. Also I am most cordially grateful for the thoughtful ways in wh. you have endeavoured to make the duties both less laborious & less incompatible with my deafness. But the fatal fact remains that I am not physically strong enough to undertake ~~for the duties even were I to accept them~~ even in the most limited sense. I could ~~hardly~~ possibly ~~have got~~ go through the Address, ~~but I cannot stand the social excitement & duties~~. Nor I am not sure even of that, for the preceding excitement ~~of the social duties~~ would probably upset me. I cannot stand for a small space of time without being quite ill. ~~in my opinion~~ (in of intellectual work)

It is no use for me to fight against impossibilities. I have constantly to renounce what I like doing & must do so now. The only chance I have of ~~doing any~~ selling through useful work during the remainder of my life, lies in doing it quietly, & living very simply & as an invalid, ~~I can hardly hope to~~ then I can make an occasional ~~[illegible]~~ mental effort & which I am not strongly placed of undertaking any serious responsibility.

~~The office was~~ Once before, when Sir W. Flower was President, he with ~~I understand~~ the consent of the other General Officers, emphatically proposed (as the first of the three likely persons among whom the choice lay) my name at a Council Meeting at which I was present. ~~but at me~~ I begged them not to think of me, because ~~that~~ I was ~~perfect~~ (immovably) conscious of my limitations of strength. Notwithstanding kindly pressure, I persisted in the refusal. It would be horribly hard if I made the sacrifice now.

I have written by this post to Prof: Herdman

May 9 1905

42, RUTLAND GATE, S.W.

My dear George Darwin

It was only last night that I returned & found your very kind letter and that of Prof: Herdman to whom I have just written.

I am deeply sensible of the proposed honor, and fully recognise the unique opportunity afforded to the Pres: of the Brit: Assoc: of drawing the attention of the whole scientific world to such views as he may put forward. Also, I am cordially obliged for the thoughtful way in which you propose to make the work less laborious & independent of my deafness. But the fatal fact remains that I am not strong enough even under all these alleviations. The preceding excitement would be enough to upset me. I cannot stand even a moderate amount of flurry. It is of no use for me to fight against unfortunate limits. Long since I have learnt to renounce many tempting

pleasures and must do so now. The only chance I have of doing useful work during the remainder of my life, lies in doing it quietly and living very simply, and like an invalid, and never to undertake to tie myself to a day, when I might prove quite unfit.

Once before, when Sir W^m Flower was President, & the names of possible persons were to be considered at a Council meeting at which I was present, he with the previous assent of the other General Officers, emphatically proposed me at the first. I immediately begged to be left out of account, being too painfully conscious even then, of the limitations of my strength. Notwithstanding, kindly pressure, I persisted in the retained. It would be foolishly rash if I made the venture now.

Yours sincerely Ever

Francis Galton

PS.

I have had a pleasant & healthful 2½ months in the Riviera (Bordighera) I saw Miss Shaen before missed your sister. during her brief visit there.

What an eventful August you will have at the Cape. I heartily wish you every possible success & pleasure. But what a racket it will be.

The Orchard,
Huntingdon Road,
Cambridge.

June 9th 87

Dear Galton,

We have a balance of £14-12-0 & I think we might spend some of it on new instrument in Oct. The no. of the last card is 1361 so we are getting on. I forget to tell you

these facts when we met last night. I also forgot to tell you that after some correspondence with Professor Hummel & a doles hair manufactures I have come to the conclusion that the best matches can

be got by using human hair & I shall have a stock of it very shortly. We will put it in test tubes & seal it up & keep it in a box.

I hope the glass for the eyes will do, it is not very good.

Yours sincerely

Horace Darwin

I think that I could get the computation

of ~~for~~ the Cards done here very well but I don't know whether this could come out of the funds, I dont know in the least what it would cost. I ~~think~~ I would find a trustworthy careful man to do it.

Overstrand
North Repps
Norwich
July 10 87



Dear Galton,

I have been long in answering your letter of June 11th & now I have not very much to say. Thank you for your criticisms of the glasses for the eyes, when I get back to Cambridge I will look into it & see what can be done

The hair will be all right now, it has I have come by this time. I will consider ~~how~~ ~~wh~~ about skin tints, but I dont know how important this is.

I do not think that the musical instrument will do for the Phil. Soc. it will make too much noise there. The drawing for the new instrument

is made but we have
not ~~&~~ made the instrument
itself, but we could
soon do so if it was
wanted. We have sold
the old falling stick
chronograph but have
not put the new one
in hand yet but I
think we can make
one on the lines we
discussed ~~to~~ with you.

at the ~~shop~~ we were
getting rather low about
the sale of the instruments
& so we did not get
on with the new things
especially as we have
been very busy; but
I hear ~~now that~~ since
I have been here that £70
worth are ordered from Japan
& £24 from America.
I ~~think~~ the quickness tester
would be popular at the
Phil. Soc. I go back

f.9r

to Cam. on July 16th & after working off some things that must be done at once I will go in at the Author. things again.

I think I could get the computing done at Cambridge, but I dont know any enthusiast who would write the paper. As soon as I have anything to report I will write again.

f.9 v

We are having a nice time here, & the children spend most of their time on the sands. My wife wishes to be remembered to you & Mrs. Galton.

Yours very sincerely

Horace Darwin

Telegraphic Address: *INSTRUMENT, CAMBRIDGE*

THE CAMBRIDGE SCIENTIFIC INSTRUMENT COMPANY.

WORKS:– ST. TIBBS ROW,
CAMBRIDGE.

Nov. 9. 1887.

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

Dear Galton,

We can send you the following instruments:
Standard Tints for the colour of eyes.
" " " hair.
Horizontal Head Spanner. (if possible.)
Height Measurer. (standing & sitting.)
Span of Arms.
Spirometer.
Hand Dynamometer.
Arm "
Keenness of Eyesight.
Appreciation of Colours.
Judgment of the eye as regards Squareness &c.
" " " in estimating divisions of a line.
Hearing Highest Audible Note.
Appreciation of slight differences of weight.

We will send them off, so as to arrive on Friday afternoon. Please send instructions for their delivery. I sleep to-morrow night at 18 Wetherby Place, Hereford Square, and shall call on you on Friday.

Yours very truly.
Horace Darwin

P.S. We shall also send Lord Rayleigh's Colour Box, which we have borrowed from the Cavendish Laboratory.

The Orchard,
Huntingdon Road,
Cambridge.

West Hackhurst,
Dorking.
Nov. 24.1887.

Dear Galton,

A customer of ours, an American Mr. Lamborn wants to go to your lecture. Would you mind putting one of the printed bills into the enclosed envelope and posting it ~~t~~ Please do not write, he has been to the shop and will understand. This machine went wrong hence the mess.

Please do not

yours,

Horace Darwin

Frank Darwin
Horace

F12



The Orchard,
Huntingdon Road,
Cambridge.

May 1. 1903.

Dear Galton,

Thank you for your congratulations, I am very much pleased. I wish I could have also had my Father's congratulations it wd. have been a real pleasure to him.

~~Are~~ Is there any other case of three brothers F. R. S.s? & whose Father, Grandfather

& ~~great~~grand father were
also F.R.S.
I'm sure this pleases you.
Ever yours sincerely
Horace Darwin

F14

F.15

Mr Horace Darwin



f.16

Telephone.
17 Cambridge.

The Orchard,
Huntingdon Road,
Cambridge.

Feb. 17. 09

Dear Mr Galton,

I have been unwell & have only read your contribution to the book today and I was delighted with it. I have made two suggestions for your consideration, but I liked it all so much that I hardly liked doing so.

I have felt it very important to bring out the fact that the feblemended are happy

when they are segregated, & this you have done; & one of my suggestions bears on this point. It will be good policy to reiterate this point. It might be worth while to get some one who has had personal experience with the feebleminded when they are segregated to write a few pages on this point. We should be so

glad to know what you think of this. Of course it comes out in the evidence but is this enough?[2]

Ever yours

Horace Darwin

[illegible]

[illegible]

[illegible]

Horace Darwin

1.20

Telephone,
17 Cambridge.

The Orchard,
Huntingdon Road,
Cambridge.

June 28.09

Dear Mr. Galton,

We thought of you so often last week & wished you were with us — you will have heard what a wonderful gathering it was. I am sending you an account of the speeches at the banquet, they were not reported properly in the London papers.

We had Sir Joseph Hooker

with us and when we told
him that you had been
knighted all he said was
that they ought to have
done it long ago.

We are very glad. ~~although~~
I quite forgot to begin
my letter correctly but
will try to remember on
the envelope.

With remembrances* from
my wife
Yours
Horace Darwin

F22

* Ida says this ought to be love.

F.23

Telephone.
17 Cambridge.

The Orchard,
Huntingdon Road,
Cambridge.

Jan. 24. 1909

Dear Mr Galton,

I have shewn Mr Wheatham your notes. We think your proposed headings are just what will be wanted & we should be very glad if we might leave it entirely in your hands & Sir Edward Fry's to deal with these points as you think best. Possibly if our young Professor of Economics, Professor Pigou, would write a very short paper, perhaps on note 6 a — cost of seduction & gains to the State — we might get him to do so, but I am afraid it would delay the book as he is too busy anyhow

till after term is over. What do you think about this? He is a strong Free-Trader & the Cheltnams are afraid of anything which might be ~~[illegible]~~ taken by the opponents to be prompted by political motives. Horace & I feel sure that Mr Pigou would exclude rigourously anything which could sense party feeling in what he wrote for us. but personally we should be more than content to leave all these questions to you.

I hope that formidable files of Blue Books has reached you. Mr Slater writes that ~~the~~ ~~with~~ printing will begin on Tuesday. We like your title "Guardians ship of the Feeble-Minded". Should the sub.

12

Telephone,
17 Cambridge.

The Orchard,
Huntingdon Road,
Cambridge.

Title be: a Summary of the Report of the Royal Commission, with a .... by Sir Edward Fry & a .... by Francis Galton?

Will you very kindly make any suggestions which may occur to you about this, or anything else!

The proofs shall be sent to you as soon as possible.

Yours ever sincerely

Ida Darwin

Mrs Horace Darwin

f31

Mrs Horace Darwin

Telephone,
17 Cambridge.

The Orchard,
Huntingdon Road,
Cambridge.



Jan. 27. 1909

Dear Mr Galton.

I am most penitent for having worried you - & for giving you so much unnecessary trouble -

In reading your Notes, we misunderstood - & we see now that you proposed that there should be a separate author for each heading. It was my stupidity & I am very sorry -

We are anxious to talk it over with the Whetham's before answering

your letter of yesterday. May I write again on Friday as I have to be away all day tomorrow.

I am sure there is no hurry about publishing the book. Mr Slater's slips will be ready at the beginning of the week. We have hurried them on so that you & Sir Edward Fry should see them as soon as possible. But there is no other reason for hurry. It seems pretty certain that nothing will be done about it in Parliament this next Session.

I hope those mimons Blue Books have arrived & that you are not overwhelmed by them

Yours very sincerely
Leo Darwin

Telephone,
17 Cambridge.

The Orchard,
Huntingdon Road,
Cambridge.



Jan. 28. 1909

Dear Mr Galton.

Horace & Mrs Whetham had a talk about the little book today - they both feel there is no hurry about getting it done - & that if, after you have seen Mr Slater's proofs (I hope next week) & when you have had time to dive into those Blue Books, you see your way to writing a short article, say on - (3) & (4) of your notes, it would do more to make the book a success than anything else could do - Of course the more you were able to do, the better it would be for us & the public, but our original

intention was to ask you & Sir Edward Fry if you would consent to write two short articles about the length of a magazine article, & to sell the book at 1/- or 1/6 - & to distribute many copies privately. Perhaps if Sir E. Fry would write on (1). (2). (5). & (6) b.c.&d of your notes, Professor Pigou might deal with cost of seclusion & gain to the State, as well as the question of Unemployment & Poor Law on which he has been working recently. He has not yet promised to write, but may do so after seeing the proofs of the abbreviated Report & when term here is over. He was asked

Telephone,
17 Cambridge.

The Orchard,
Huntingdon Road,
Cambridge.



to write a Note on Unemployment for the Poor Law Commission & is specially interested from that point of view.

Unless we have a Symposium – which is not what we intended – will it be necessary to have an editor?

I am copying out your "Hints for consideration" & returning your copy.

Mr Pinsent's Notes have lately been sent to Education Committees all over the country. We had considered the possibility of asking her to contribute ~~[illegible]~~ perhaps the concluding chapter. After

reading her Notes we should be very glad to know if you think this would be advisable -

Please do not trouble to answer this letter till you have seen the proofs -

Yours very sincerely

Ida Darwin

(Hints for consideration)

F. Galton
Newton Cottage
Brockham Green
Betchworth Surrey
(till end of Feb)

F6

Guardianship of the Feeble Minded

1 Purpose of the book

2 Prevalence of Feeble-minded kept (from Report)

3 Fecundity
Fertility of the Feeble Minded

4 Inheritance of it (at rate of at least 1 F.M. for each ~~more~~ F.M. parent)

5 Proposed methods for preventing undesirable parentages in Report)

6 State guardianship
a Costs of Seclusion – Gains to State
b Under what circumstances justifiable
c Recognised forms of Seclusion under various motives concerts, sailors to long periods, etc
d Amends for loss of liberty

7 Conclusion

Mr Horace Darwin

47

TELEPHONE 4421 GERRARD.

ALBEMARLE CLUB,
13, ALBEMARLE STREET, W.

Feb. 11. 1909

Dear Mr Galton.

I sent you a telegram last night, as there had been some delay in showing your letter to Mr & Mrs Wheatham & Mrs Keynes, who form our little book committee.

We are all most grateful to you for consenting to write, & on the lines you suggest – Sir Edward Fry's

preface too will be very short & Professor Pigou's contribution also - This will not be ready till the middle of March.
I enclose his letter shewing the points he means to take. Also Sir E. Fry's preface -

Perhaps we may find someone -? a Doctor - who has seen the working of some of the Colonies? -

to give his views as
to the effect, as to suffering, on the
individuals, of segregation.
Do you think this might
be desirable?

Yours sincerely
& with very real thanks

Ida Darwin

I go back to Cambridge
tonight.

f.10

Telephone.
17 Cambridge.

The Orchard,
Huntingdon Road,
Cambridge.



Feb. 15 - 1909

Dear Mr Galton.

Your contribution is quite splendid - surely no one can read it without feeling deeply stirred & impressed with the magnitude of the evil - & the hopefulness of the suggested remedies. May I send it to Mr Slater with Sir Edward Fry's preface now to be printed?

I have today seen Dr Inge & have asked him if he would, after seeing your chapter & Sir E. Fry's preface,

consent to write something very short on the lines of his engenic lecture. He promised to think it over - I hope you will approve of this -

As to the possible Doctor, we quite think you are right about the evidence in the Report - emphasised by what you say in your chapter - bringing out the cheerful aspect of detention & seclusion. But would Dr Kerr for instance, with his medical knowledge of school-children, help to allay people's prejudices about special schools, if he agreed to put his views on the subject into our book? I have heard so many people object to the separate classes

Telephone,
17 Cambridge.

The Orchard,
Huntingdon Road,
Cambridge.

on the grounds that it was bad for the dull & backward children to be taught with the more deficient ones. Whereas this special teaching may be the only possible means of discovering that some children are deficient only in one small part of their minds - such as (tone or word deafness or blindness - & they may be above the average in other respects. Perhaps you will think this too much detailed in one direction & that the evidence contained in the summary is enough? Your advice on this point would be greatly valued.

Yours most gratefully
Ed Darwin

Telephone,
17 Cambridge.

The Orchard,
Huntingdon Road,
Cambridge.

Feb. 26. 1909

Dear Mr Galton,

George had told me yesterday of the death of your brother. It must be a great break & sorrow to you - Horace & I both send you a warm message of sympathy -

I cannot remember why I asked Mr Slater to send your paper back to you - I thought I had only asked

her to send you Miss Denny's Report which I thought might interest you. This I did before I knew you had been corresponding with her & would have her information first hand.

She has written a paper for us showing how happy these poor boys & girls can be made in seclusion.

As her death is not yet
21 she cannot speak
from personal experience
of the older men & women
but she can say ~~stress on~~
the burden & misery their
lives are to many of them
when at large.
~~Mr~~ Professor Pigou has
written a short paper on
the economic aspect, ~~of~~ which

he considers too obvious to be worth printing, but which we all think is admirable - I will send it you as soon as I get the page proofs - I hope next week -

Yours very sincerely

C. Darwin

What a cold journey you will have if you are going to the new forest tomorrow!

March 15. 1909

The Orchard,
Huntingdon Road,
Cambridge.

Telephone.
17 Cambridge.



Dear Mr Galton.

Mr Pinsent, one of the Feeble-Minded Commissioners, was staying with us last week & gave an admirable address in the Guildhall.

We asked her to read through our proofs & make any suggestions which might occur to her. Mr Slater was here too & found her criticisms on the Summary this most helpful.

Rutland Gate. In case you are at home again

f.18

On p. 1. line 4 of your paper she suggested substituting the words "a great deal of the evidence before them emphasised the view that" etc. for: "they have shown that" etc. which she thought all the Commissioners might not have agreed to. She also thought that the other alteration on this page would be an improvement.

May I suggest that Question 5323 on p. 3 should

be omitted? We talked it over & she pointed out that the low grade cases were the most likely to be legislated for in the first place, & that, although she thoroughly agreed as to the seriousness of the danger of having the mild cases at large, it might be bad policy to emphasise this danger at the present moment. She - & we - wondered whether the answer about the strength material of going to the brain, going

to the body was a true statement physiologically?

We hope for Professor Tuke's paper in a few days now that term is over - Miss Dendy's was rather too long & diffuse & has needed a good deal of trimming. I hope she will not mind. Mr Pinsent admires her work at Sandlebridge immensely, but says that hers are all picked, unfavorable cases -

I am afraid you have had wretched weather in the New Forest - I am sendy

Telephone.
17 Cambridge.

The Orchard,
Huntingdon Road,
Cambridge.



Mrs Horace Darwin

March 24. 1909

Dear Mr Galton.

I do hope your lumbago is better - If it is not, please do not trouble to answer the following question - away out of the Z. M. book - which is very nearly done - at last!

Two members of our editorial Com^ee take different views as to the order in which the papers should come - i.e. whether the Abstract of the Report should come before or after

the papers by you, Professor Inge, Professor Pigou & Miss Dendy. If the Abstract came first, it would follow directly on Sir Edward Fry's Introduction — & be followed by your article. Then Dr Inge's, Professor Pigou's & Miss Dendy's. Your opinion on this point would be much valued on a posh-card. I hope your proof has come back to you by this time. We had suggested the alteration in Prof. Pigou's first paragraph & he has altered it partly.

Yours very sincerely.

Ida Darwin

Horace & Ruth are at Charmouth

Mrs Francis Darwin

x f.22



The Orchard,
Huntingdon Road,
Cambridge.

March 25 - 1909

Dear Miss Bigg

I am not sure whether Mr Galton ought to be bothered with any letters, & so I am sending Dr Drugg's paper for the J. M. book to you for you to give him or not, as you think best.

f.23

Perhaps you will kindly send it back to me when you have done with it? & please do not trouble him with my letter either if you think it will be likely to worry him in the least -

Miss Dendy is rather unhappy because the

Deflection of women has been treated as of greater importance — or rather, more prominently, than that of men, in all the papers written for this book. She points out that among f. mos the proportion is 2 men to 1 woman. This last fact, taken

f.25

from the special schools in Manchester & London, has been stated in the abstract.

Believe me
Yours sincerely
Ida Darwin

F.26

Mr Horace Darwin

Telephone,
17 Cambridge.

The Orchard,
Huntingdon Road,
Cambridge.

March 25. 1899

Dear Mr Galton,

Very many thanks - the question is now settled & I hope no further difficulties may arise - I should like you to see Dr Ingis paper - which we only received last week - & which is now being printed - Does the sentence about "the Chardes" strike you as too much

assuming an opposition view on their part?
I admire his outspoken ness but we do not want to arouse antagonism where it doesn't exist -
We have made some inquiries from Church workers in Penitentiaries & others & they ~~seem to~~ agree that detention is necessary in f-m- cases -

F28

I thought you had returned to London. I am so very sorry to hear what a bad time you have had in the new forest.

Very sincerely yours

Ida Darwin

POST CARD

THE ADDRESS ONLY TO BE WRITTEN ON THIS SIDE.

CHATHAM NOV 21 79

Francis Galton Esq

42 Rutland Gate

London



F.1bv

P.S to my letter with regard to what I wrote at the top of the first page; it is exactly wrong so to speak; signalling should be done by interruptions. The reason is that they should be the same as the signalling lamps, which now work by flashes of light, but are about to be changed to interruption in a steady light; this I did not know the first row.

L. Darwin

F.1 b r

It is better to send dots and dashes in light than in darkness; that is to say e should be a short flash of light, not a short interruption. This would make it better not to have the light on the distant object when the instrument is in its normal position; but only when the key is pressed; there should be some method of tying the key down

Leonard Darwin

Brompton Barracks
Chatham
May 20th

2/79

Dear Mr. Galton

Your instrument arrived here safely, and I think I understand its working all right though practice would no doubt make one much better at it.

I will write again when I know how and when the trials are to be conducted; at present I can only say that



f.1a

the only reason that yours would be rejected would be that the other patterns are considered better. Yours of course is only in the rough, and the other one I have seen is carefully worked out in every detail. — it will be a difficult job to beat it. One part, which is claimed as a patent (I dont know whether rightly or not) is particularly good. The mirror has a certain amount of play, and is kept in one position by a spring; when the

spring is pressed down the flash is taken off the object; so that a telegrapher can by the exact same motion of the hand to which he is accustomed, send dots and dashes with nearly the same rapidity as on an ordinary instrument.

The mirror has slow motion screws, and a sight in front; there is a small hole in the centre of the mirror by which the mirror can be sighted on an object, and the reflection is kept on by the black spot made by the reflection (or non-reflection) of this hole being

kept on the sight. You should get some easy contrivance for sending the flashes; as that is considered a very important point.

Take the case of Ekowe, where when once they had got their line, the instrument ~~could~~ remained in position for several days; rapidity and ease in signalling would then be most important.

If none of this is new to you forgive me for troubling you.

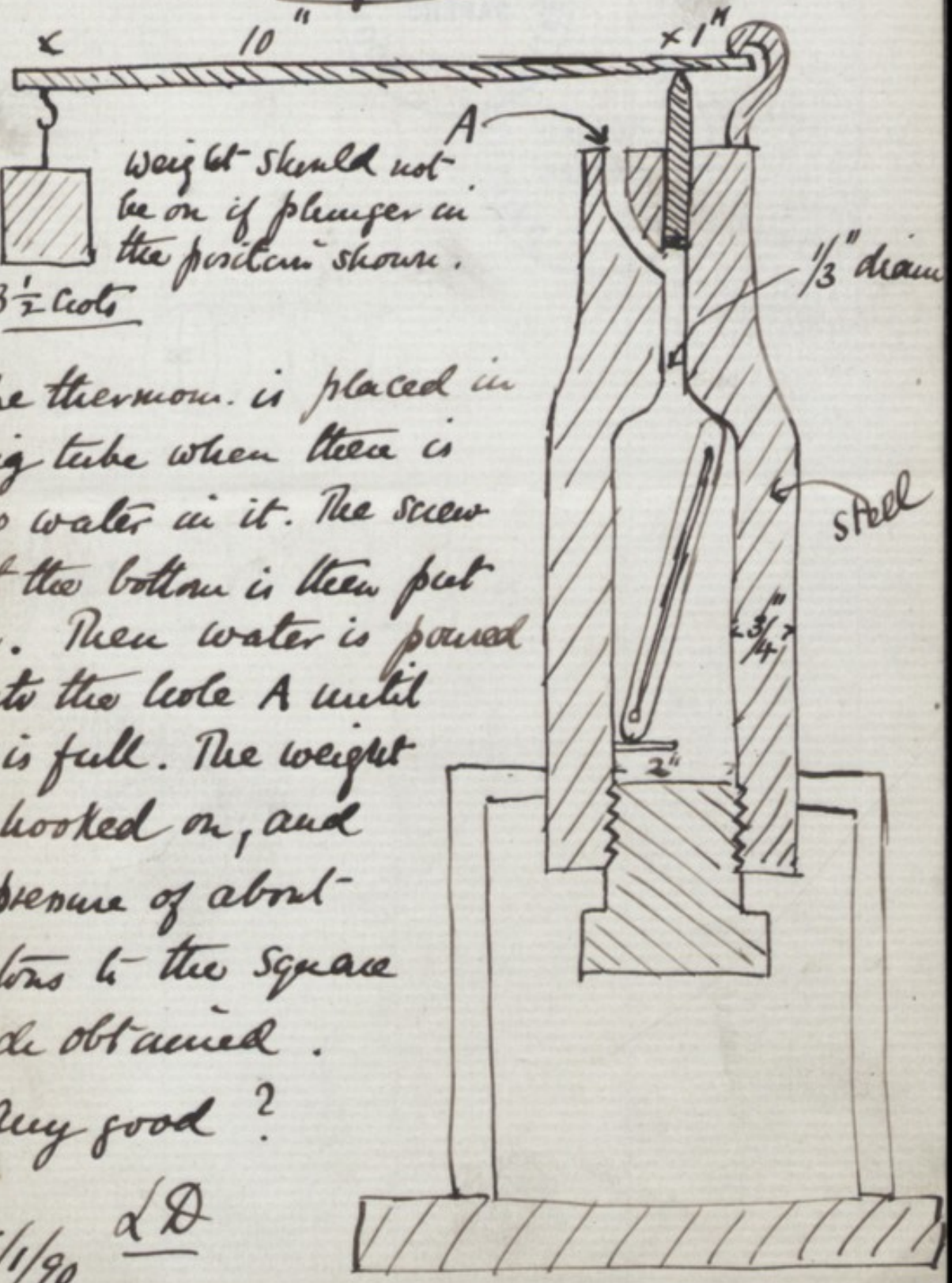
Yours truly

Lennard Darwin

(5 weights
each representing
one ton to the square inch

18, WETHERBY PLACE,
SOUTH KENSINGTON. S.W.

## Diagram

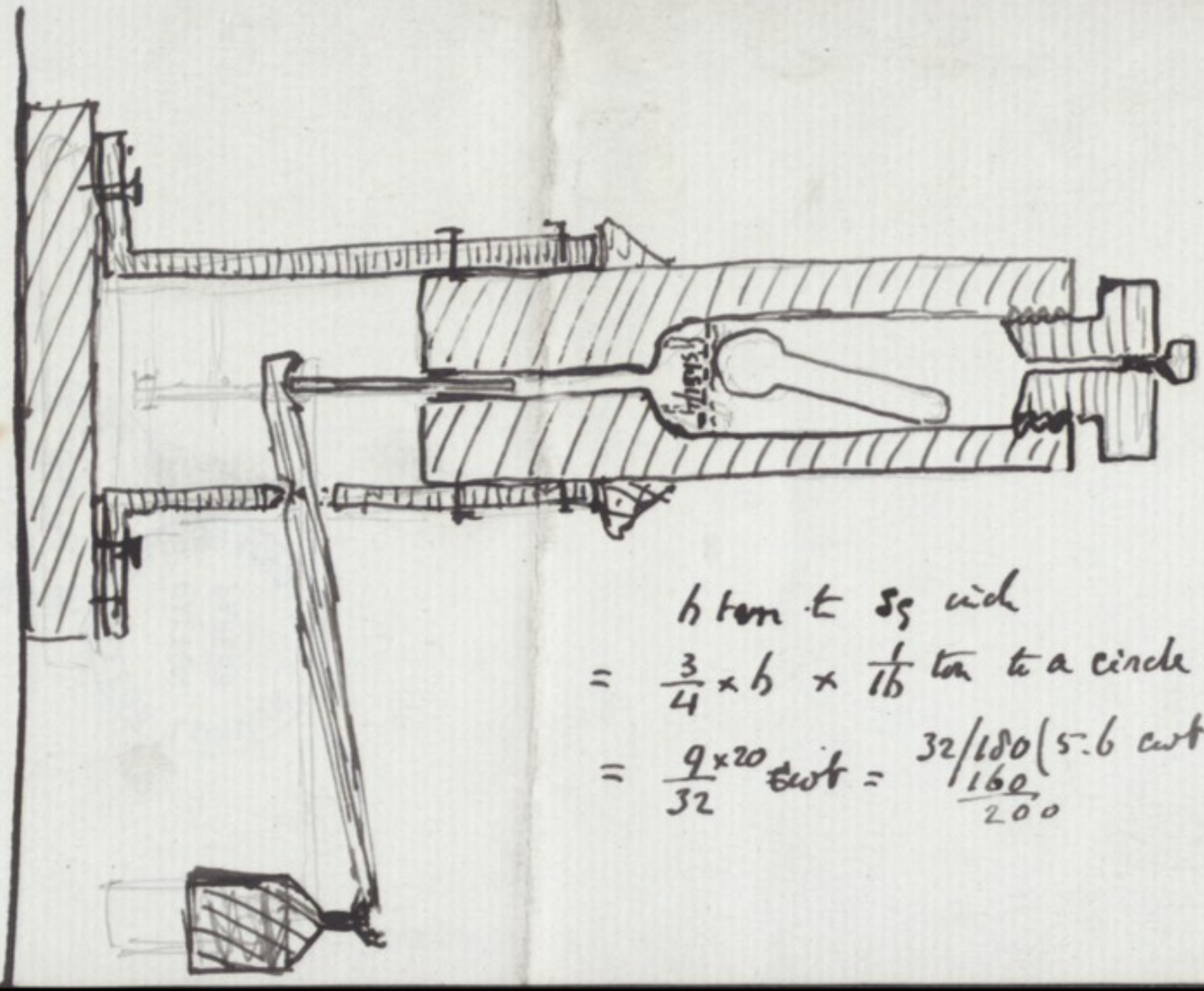



The thermom. is placed in big tube when there is no water in it. The screw at the bottom is then put in. Then water is poured into the hole A until it is full. The weight is hooked on, and a pressure of about 5 tons to the square inch obtained.

Any good?

26/1/90 LD

f.6



h tons t sq inch

$= \frac{3}{4} \times h \times \frac{1}{16}$ ton to a circle $\frac{1}{4}$ inch diam

$= \frac{9 \times 20}{32}$ cwt $=$ 32/180 (5.6 cwt.
160
200

f.9r

18, WETHERBY PLACE,
SOUTH KENSINGTON, S.W.
Jan 6 th 1892

Dear Galton

The enclosed will interest you. Kindly let me have it back when quite done with.

Please send me a post card to say if you are back yet. There is one point about the Kew work that I should like to talk over with you sometime.

Yours ever

L. Darwin

See over

From B. H. Chamberlain of Japan
Extract letter to Major Leonard Darwin

-- "I will refer it to our best local authority Dr E. Baelz author of a memorable "Die Körperlichen Eigenschaften der Japaner" German Asiat. Trans Parts 28 & 32. Such inquiries as Galton describes have not yet, so far as I know, been investigated by Baelz, but he would undoubtedly be the proper person to take the matter up, if leisure permits, which is doubtful. He will probably be visiting London next summer, and of all people he is the one whom Mr Galton or any one interested in the physiology of the Japanese, or indeed in most questions concerning this nation, would find the most fruitful and accurate. A month or two hence I shall send you Baelz's answer.

June. 3 1903.

1. Savile Row,
Burlington Gardens,
W.

Dear Galton.

With reference to your letter to Keltie of the 31st ult, I have been asked to write in the hope that you will reconsider your decision as to the Research Committee. No responsibility is involved, and you need not attend meetings. In fact the Committee will do

f.12

little or nothing more than elect an Executive Sub-Committee. Our object is to stimulate the Society into doing more scientific work, and we are therefore anxious to retain on the list of this electing body the names of those who have done good work in the past in order to give it as much prestige as possible. I hope therefore we may keep your name on

Yours sincerely

L. Darwin.

Leonard Darwin

Oct 31 08

12, EGERTON PLACE,
S.W.

My dear Galton.

I got back to town for the winter on Thursday, and yesterday I went as my first visit to see you — and found the bird flown. I wanted particularly to thank you for your Book. My wife has been reading it aloud to me, and we have both enjoyed it much. I am sorry

for your sake that it is finished, for I am certain that it must have interested you writing it. No book that interests readers does not interest the author, I believe. I have not finished yet, for we are keeping it for our quiet evenings, which alas get less frequent in town, especially now I have the misfortune to be a President.

My wife is in Lincolnshire with her brother, who is recovering from an illness; but I know she joins with me in all I say.

I wish you had pitched on Sussex, not Surrey for your abode. I wonder if it is permanent. I was motoring near you a short time ago, without a notion that you might be there. Probably you were not.

I find the Presidency gives me rather more bother than

I thought — chiefly silly jobs, like finding out that the Prince of Wales will not come to hear Sven Hedin — anyhow better than hearing he will come!

Yours sincerely

Leonard Darwin

Fern
Buxton
Derbyshire.

May 4
1877.

Dear Henry

I am very glad that you are pleased with the Medallion. You can send me the 11/ at your convenience. There is nothing to pay

P.2

for packing up as
I did it myself.
Also if you like I-
send you back the
little Basket (parcel
Post. 3) you can
do so —
With our love
Your affect Cousin
C Darwin.

f.4

Fern
Buxton
Derbyshire.

May 7
1877.

Dear Sir,

Many thanks for the orders which I received this morning. We have Spring at last — Cuckoos — Swallows & trees budding very nicely — Kind love

Yours truly
C. Darwin.

f. 8

Fern
Buxton
Derbyshire.

April 29
1877

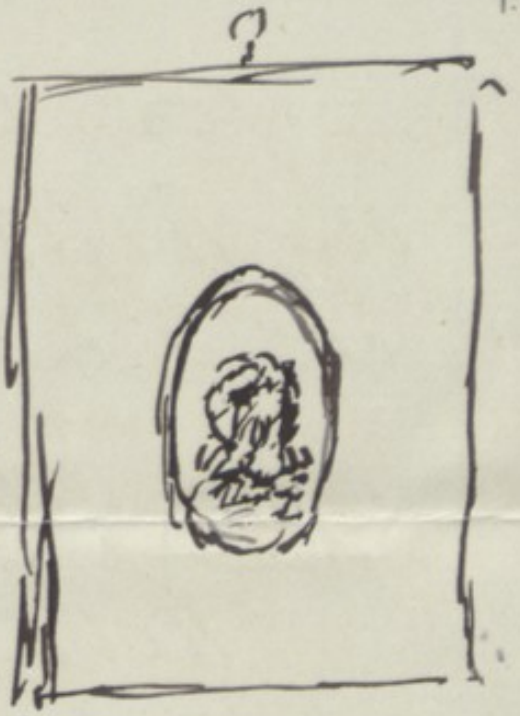
Dear Henry

Your letter has given me very much pleasure. I am most happy to be able to help you – You will find one of the medallions – They are beautifully made as you will see. I am exact

copies of Mr Dickens, to
a button, or a wrinkle,
almost to a hair — I
have had them mounted,
& it hangs under Mr
Dickens alongside the
little engraving by
John Wedgwood, both
of Mr. Dickens. for Mr
Jonas Dickens — a
piece of board. 10
inches by 8, covered
with Maroon Velvet,
& an oval for Mr

Medallion.

It looks very well – Francis Lichfield Bust – was evidently done from one of the Medallions – Your Family news is very interesting. I have but little to tell you – we old folks sit by the fire & look at the vacant chair so recently occupied by Chas. Now a "great man". Great.



in last days! –
Our kind love to you
all. even dear Bessy
from affect. Cousin
Reginald Darwin

L 1808 5061
A 8 779090

10/ Reginald Darwin
11

f.13

BUXTON
1
MY 4
88

155

Mr Wheler
Tow Wheler's hy
Hill Lesh
Knutsford

f.1

Monday Oct 11.

My dear Bessy – We
have heard in an
indirect manner this
morn that Mr. Galton
is very ill which we
are exceedingly concerned
to hear: but my Father
& we hope very much
the account may
be exaggerated. &

that you may be able
to tell us he is better. –
I am afraid my Aunt's
health will suffer from
the anxiety she must
be feeling – pray mention
how she is. –

No doubt you must have
so much to do my
dear Betsy that I will
not trespass on your

time any further but with
an affectionate remembrance
& sincere wishes for good
news. believe me ever
Yr affect. Cousin
Susan E Darwin



f.3

f.4

Susan Darwin

11 Oct 1841

Mrs C. Darwin
Maer
Newcastle
Staffordshire

Sun

II, EGERTON PLACE,
S.W.

My dear Galton.

I have just finished your 'Memories', & must send you a line to say how very much they have interested me.

Your story is all so pleasantly told, and one is led to read on and on by some new and ingenious notion in every page or two, which answers to some interesting event in a history book.

I am very glad you had the energy to write the book both from a personal points of view, and because it seems to me a very useful thing

~~that~~ the general reader who has ~~little to do~~ with scientific journals, or even with serious books should understand the general bearing of all your work during so many years.

It is wonderful in how many papers the improvements of the breed or the principle of heredity come to bear notice.

There are several very amusing passages which made me laugh greatly.

I should have liked to have heard a little more about your grandfather, but as he died in 1846 I think, you probably saw but little of him as a young man.

It would be a very interesting

tribute to your book if some one would undertake to breed some animal for intelligence alone.

How I wish, as you suggest, that some of the millions annually wasted in helping the unfits to breed could be used in bringing together poor but Eugenic young couples.

I am sure many persons who do not care twopence for science would be specially interested in your book.

I hope you find your home pleasant and the country around you; judging by our Western Reserve, it's sweet

F.4

have been delighted for since you
have been there.
I heard of your paying Henrietta
a visit.
Please remember me very kindly
to Mrs Biggs.

Yours very sincerely
C R Darwin



1904.

Suggested title
Share of Religion in human Evolution
the Religious Spirit

Wm Darwin
F.S

32. HANS PLACE.
S.W.

May 1st

My Dear Galton,

My friend Markse Editor of the National Review has asked me to try whether you can be persuaded to review "Social Evolution" in the 'National'. It would give him the greatest satisfaction to him if you could see your way to contributing such an article.

He further adds that he has been in communication

f.6

with Mr Kidd & has reason to
believe that he will undertake
to answer his reviewers generally
in the National Review.
Whether the book is founded on
a misconception or not it is
extremely suggestive and inter-
-esting to very many persons.
We were very sorry to miss you
when you were kind enough to
call the other day.

Yours very sincerely
W. E. Darwin

F.8

Wm Darwin

19



11, EGERTON PLACE,
S.W.

Dec 2nd

My dear Galton,

I was delighted to read the President's speech when handing the Copley Medal to George & to be conveyed to you.

It is a great honour and all your friends will be

Greatly pleased.

I hope you find Grayshott a pleasant abode, & that you have some interesting neighbours.

We are having some decimal weather here, but as it is October I hope it may suit your bronchial delicacy.

I off early next week to vote
in Lincolnshire with the hopes
of turning out this Government,
and I doubt in the least minding
being what the Yankees call
a Mugwump.

Please remember me warmly
to Miss Biggs.

Yours very sincerely
W.C. Dawson

What a capital reply Prof Pearson
made in the Times of Nov 10th
to Dr Dorkin

1910

RIDGEMOUNT,
BASSET,
SOUTHAMPTON.

April 26th

Dear Mrs Wheler,

It is very kind of you to have written me so pleasant a letter on our ancesters. Some years ago we were at Buxton for my wife' health, & we called on Reginald Darwin, & saw these portarits, but Ihad not much opportunity to have a good look at them, & I am glad to see the photographs, which I return with many thanks.

We have at Down a rather poor copy of Dr Erasmus leaning on a table & showing his hands as if they had beenthought to be fine hands, no doubt this is a copy of the one at Buxton. When Sacheverell was in the Servic we saw him once or twice at Southampton, but sinc he has retired we have not been in the nieghbourhoood of Buxton.

I remember some years ago Mr Francis Darwin told me that there were sever al family picture taken out of the frame, & rolled up, & put away in a cupboard, at eleven & he was impious enough to add that they were very ugly.

I am much amused by your story of Robert Darwin's cautiousness, it might be an anecdote from Sir Roger de Coverley's memoirs.

If anything should bring us anywhere in the neighbourhood of Warwickshir my wife and I shall have very much pleasure in making your acquaintance, & in seeing Miss Galton again.

My brothers have often spoken with pleasureof having met your son at Alnwick, if I am able to take my wife there some day to see the Castle I shall try & find out your son.

My wife wishes me to send you her kind regards, & with many thanks for your letter & the photographs,

Believe me yours very sincerely,

W. E. Darwin



P.S. I recall the fact that Reginald Darwin gave my brother George photographs of these pictures, probably they were from the same negative as yours.

Charles B. Davenport

11 Francis Av., Cambridge, Mass.
Mar. 20, 1896 — f.1

(1897 really)

Dear Mr. Galton,

Permit me to extend to you, although somewhat tardily, my heartiest congratulations upon your having completed your seventy-fifth year together with the hope that you may see many more anniversaries in the best of health, strength and happiness.

At about the time of your anniversary I invited my class in the experimental and statistical study of phylogenesis to meet at my house to review your writings. You will believe me that my arms ached for days in consequence of having to transport your books and serials containing your briefer articles from the libraries; and it took an hour and a half to call attention to the titles of such of your papers (perhaps two-thirds) as I had gathered. I had two main regrets. The first was that no copy of your narrative of a Journey in Damara Land existed in the libraries about Boston, except a translation

of it into German, which I found at the American Academy. I shall ask our library to get at least the recently published edition. My other regret was that I had no photograph of you. The only representation I could get was a wood-cut in the Popular Science Monthly of about ten years or so ago. Fortunately I could know the class your handwriting in a letter you were kind enough to write me three years ago.

We considered your scientific activity and interests as exhibited in your writings. There was great interest shown and surprise expressed at the quantity of your work, its worth and its unity in the midst of variety. The son of the physicist, Prof. Am. Mayer, who was with us was much interested to know that you were the inventor of what he had long known as Galton's hydrogen whistle. There was also present Mr. Moenkhaus, a pupil of Prof. Carl H. Eigenmann, of Indiana who like his teacher, has applied your methods in the study of variation of fishes. I am sure that the appreciation of your work was an intelligent one.

It is no desire to flatter that leads me to write this letter to you, but merely to express to you that in this country, as elsewhere in the world, there is a high appreciation - a just appreciation - of the value of your work which has so profoundly affected biological investigation and will do so still more in the next half century. Although we gain inspiration from your record we recognize the truth of the fact first scientifically established by you of the preponderating importance of nature and feel that it is useless for any of us to hope that he may even attain near to that magnificent position in science which you occupy. All that nurture can do we feel is done by the glorious example of originality, simplicity and singleminedness in science which has been set by the descendants of Erasmus Darwin.

With expressions of the highest Esteem and admiration, I remain,

Yours very truly,

Chas B Davenport

To Francis Galton, Esq.
42 Rutland Gate
London.

f.4

C. B. Davenport] 11 Francis Av., Cambridge, Mass.
May 16. 1897 F.5

My dear Mr. Galton,

Your kind letter of May 5 makes me newly conscious that I am already indebted to you for a letter and the gift of a photographic reproduction of your portrait. For these please accept my sincerest thanks.

In my last letter I told you of a "conference" which one of my classes had held on your life and works. Some of the students were so much interested in your work that an opportunity was given me to present the same matter before a larger audience at Cambridge. I find that merely reading the titles of your papers, as they appear, for instance, in the Roy. Soc.'s List awakens a very great interest in the author.

In your last letter you ask how we contrived to get hold of 4000 pigs. Mr. Bullard, a

former student at the University, has charge of the Government microscopical inspection of Export pork, which is located at ~~the~~ one of the largest "pork packing" establishments near Boston. Here they must kill and clean from 1,000 to 3,000 a day. The pig's carcasses are hung up by the hind legs, and the determination of the number of glands on the fore legs was easy and rapid. At the present time Mr. Bullard is counting the number of nipples on the two sides of the body. It would be comparatively easy to make measurements — although naturally not so easy as counting — and I should be very glad of suggestions from you as to any pairs of organs whose correlation you would like to have investigated.

A copy of the pig-gland paper was sent to Prof. Pearson, Univ. Coll., London, W.C. I have been much interested in his work and have been brushing up my calculus to follow his

F.7

demonstrations. I am now reading his III. Contribution and am sending to Sulan & Co for Reprints of his two earlier ones.

It seems highly probable now that I shall visit England this Summer, reaching Liverpool July 10th and London about a week later. It would be a never-to-be-forgotten pleasure to see you, if you could do me the honor of appointing a time and place where I might meet you.

I am interested to see, in the last number of Science, that you are on the committee to introduce experimental psychology into ~~Cambridge~~ University College; and, from the last number of Nature, that you are still extending your quantitative studies in Evolution.

Very sincerely yours,

Chas B Davenport

To Dr. Francis Galton, F.R.S.,
42 Rutland Gate,
London

F.8

Davenport 11 Francis Av., Cambridge, Mass.
July 5, 1897.

My dear Dr. Galton,

First let me thank you for your last kind letter concerning my proposed visit to you. I now much regret to say that this pleasure which I had long thought might be realized this summer must be deferred. Owing to the ill-health of my wife I have been obliged, ~~to~~ within a week or two of sailing to give up my trip. And the vessel which I had thought to sail on leaves to-day.

I shall take pleasure in sending you to-day a paper by one of my students containing an application of your methods of studying variation to one of the Laws of Variation formulated by Darwin, namely, that specific characters are more variable than generic ones. Another student, a botanist, is using your method to invent an (arbitrary) distinction between species and variety; but his work is not finished yet. Another has just

completed a thesis showing for a particular case that an ~~highly~~ abnormal character (simious outline of the wing of a species of Sphinx moth) is more variable than various other dimensions of the wing.

In Mr. Brewster's paper, we met with a difficulty, namely, that stature, e.g., is found to ~~be~~ have a larger Q than nose length, just because mean stature is so much greater dimension than mean nose-length. It has seemed to me that a truer, or at least more valuable, index of variability would be $\frac{Q}{\text{Mean}}$. Then the Q will always be a certain fraction of the mean whatever ~~that be~~ mean may be. Would you kindly tell me if this is wrong; or if you have considered it unnecessary. I should be sorry to go on perpetuating an error and bringing confusion into the magnificent science you have established.

You must be so very busy that I feel it is presumptuous for me to ask you questions but your kind responses to my earlier letters have emboldened me, you see.

Sincerely yours,

Chas. B. Davenport.

Davenport





Oct. 14. '02.

Dear Mr. Galton:—

Word has just reached me that it is possible that some action may be taken by the Carnegie Institution at a Board meeting in November relative to my application for the establishment of a station for the experimental study of evolution at Cold Spring Harbor, Long Island.

The Board would be glad of any testimony relative to (1) the importance of such a station, (2) my qualifications as possible director and (3) the suitability of the locality. Of the last point of course you cannot speak but I feel that it would be a great help to the cause if you could find time to write a short letter concerning the importance of the

station proposed any anything concerning my equipment for the position that you may care to hazard. The establishment of the station is a thing I have much at heart and for which I am ready to sacrifice a great deal.

Trusting that you will not feel my request too bold, I remain,

Sincerely yours,

Chas B. Davenport.

— The letter had best be addressed to Dr. John S. Billings,
Astor Library,
New York, N.Y.
who is chairman of the biological Committee —

Yes, but only if the posters in Wesley Library. Nov 21

Davenport

f.17

RESIDENT STAFF
C. B. DAVENPORT, DIRECTOR
FRANK E. LUTZ GEORGE H. SHULL
ANNE M. LUTZ SECRETARY
MABEL E. SMALLWOOD, LIBRARIAN

Carnegie Institution of Washington
DEPARTMENT OF EXPERIMENTAL BIOLOGY
STATION FOR EXPERIMENTAL EVOLUTION
COLD SPRING HARBOR, LONG ISLAND, N. Y.

October 27th, 2905.

Mr. Francis Galton.,

42 Tutland Gate,

London, S.W., England.



Dear Mr. Galton.

As you may have heard already, the scheme for a Station for Experimental Evolution materialized. We should be glad to have associated with us a few of the workers on this subject in other countries and cordially invite you to become one of our Correspondents. Acceptance of this invitation implies ~~imp~~ only mutual intention to exchange publications, and occasionally ideas by letter.

Yours truly,

Chas B. Davenport

THE LABORATORY
CITADEL HILL
PLYMOUTH

Oct. 11, 1902

Dear Mr. Galton,

You will not readily understand how much pleasure it gave me to see and hear you and speak with you. I thank you again for the opportunity.

I regretted very much that owing to my very short stay in London I could not call upon you before leaving. Had a pleasant call upon Weldon at Oxford the next day and there met Sergi (?) also. I shall return to America with renewed courage for the fight for the quantitative study of Evolution.

Kindly remember me to your niece.

Sincerely yours,

Chas. B. Davenport

f.22 Charles Davenport.

AMERICAN BREEDERS' ASSOCIATION—EUGENICS SECTION

DAVID STARR JORDAN, CHAIRMAN  C. B. DAVENPORT, SECRETARY

EUGENICS RECORD OFFICE

H. H. LAUGHLIN
SUPERINTENDENT

COLD SPRING HARBOR, LONG ISLAND, N. Y.



Oct. 26, 1910

Sir Francis Galton,
42 Rutland Gate,
London S.W.

My dear Galton: —

Your postcard of Oct. 14, just received. I thank you for taking the trouble to reply. You must think me a nuisance to add thus even a letter to your correspondence. But I must tell you of recent events here.

As the enclosed printed matter will show in some detail there has been started here a Record Office in Eugenics. — so you see the seed sown by you is still sprouting in distant countries. And there's great interest in Eugenics in America, I can assure you.

We have a plot of ground of 80 acres, near New York City, a house with a fine proof addition for our records. We have a superintendant, a stenographer, and two helpers besides 6 trained field workers. These are all associated with the Station for Experimental Evolution, which supplies experimental evidence of the methods of heredity. We have a satisfactory income for a beginning and have established very cordial relations with institutions

for imbeciles, epileptics, insane, and criminals, ~~and~~ we are studying communities with high consanguinity also. Altogether the work is developing in a satisfactory and interesting manner. We have thought that, though our work is mostly in "negative eugenics", we should put ourself in a position to give positive advice. We cannot urge all persons with a defect not to marry; for that would imply most people, I imagine, but we hope to be able to say, "despite your defect you can ~~with propriety~~ have sound offspring if you will marry thus-and-so."

I want to tell you how much I have enjoyed reading your autobiography. You have quite put yourself into it; and that makes it much more valuable than any "Life" by another hand.

It would please you to realize how universal is the recognition, in this country, of your position as the founder of the science of Eugenics. And, I think, as the years go by, humanity will more and more appreciate its debt to you. In this country we have run "charity" mad. Now, a revulsion of feeling is coming about, and people are turning to your teaching.

With best wishes for continued strength and health & expression of my profound esteem, Yours faithfully

Chas B Davenport

Any? Chief help was the data we gave I & he would be no use. K. Pearson

EUGENE DAVENPORT
DEAN AND DIRECTOR

UNIVERSITY OF ILLINOIS

COLLEGE OF AGRICULTURE
AND
AGRICULTURAL EXPERIMENT STATION



Urbana, Ill., May 23, 1906

Dr. Francis Galton

Care of Macmillan Company

Dear Sir:

I am desirous of making use oftables 12 and 13, pages 209 and 210,of "Natural Inheritance" in some text I am preparing, of course giving credit to the original as published by Macmillan Company. I find, however, that the text in my possession has a number of typographical errors. I have written the publishers and they are unable to supply me with corrected data, and suggest that I communicate directly with the author.

I would be exceedingly obliged if I could receive from your hand the data for the correction of these tables.

Very truly yours,

E. Davenport

Vittoria House
Cheltenham
Dec 6. 1890.

My dear Sir

Many thanks for your letter. We shall make fresh measurements every year and if the various results as to Indian boys are confirmed I shall hope to publish the results.

Laws

As regards the calculation of the odds, what I mean is this - that if 244 public school boys were taken at random, the odds against the mean chest girth (say) of these boys being as much as or more than .42 in excess of the general mean would be about 5000 to 1.

The way I arrive at this conclusion is as follows — First - From the B.A. tables I find that the probable deviation from the average chest girth is about 1.3 inch i.e. of those ~~half~~ boys who exceed the average, one half exceed it by more than 1.3 inch & of those who fall short of the average chest girth, one half fall short of it by more than 1.3 inch.

Now if a great number of sets of 244 boys were taken & the average chest girth for each set were taken, then these averages would obey

the laws of frequency & the probable deviation of these averages would be $1.3 \div \sqrt{244} = .083$.

The modulus corresponding to this probable deviation is (Airy's Theory of Errors of Obs^ns page 22)

$$.083 \times 2.1 = .17 \text{ (nearly)}.$$

I then use the table given in "Airy" page 22 to calculate the probability that the excess of chest girth for any set of 244 boys taken at random from Public school Boys would be as great as it was for our 244 boys i.e. as great as .42 inch. In this table it will be

$$\frac{.42}{\text{modulus}} = \frac{.42}{.17} = 2.5 \text{ nearly.}$$

The value of the integral corresponding to this value of $x$ is .4998 from which I gather that if 10000 sets of boys were taken, 5000 sets would exceed the average in chest girth & of this 5000 sets 4998 sets would not exceed it by more than .42 inch, i.e. only 2 ~~boys~~ sets of boys out of 10000 sets would reach or exceed our average so that the odds against our average being the result of chance are nearly 5000 to 1.

I should be greatly obliged if you could see any flaw in this reasoning if you will point it out to me. As to the "Remark" which I find I have not made intelligible

I simply mean this, that (taking again the chart with average – ·42) this result is got by ~~of~~ finding the value of

~~8×(1·2) + 12×(2·2) + 13×~~

$$\frac{8\times1\cdot2 + 12\times2\cdot0 + 13\times1\cdot5 + 14\times\cdot5 - 30\times\cdot3}{244}$$ &c

because ~~for~~ a mean derived from (say 30) observations must have more weight attached to it than another smaller mean derived from (say 8) observations.

Believe me, Dear Sir
yours faithfully
A. S. Davis.

All the Peers & their fathers & grandfathers (7 G) (or peeresses in their own right) stated to have married an 'only child' or 'daughter & heir', in Lodge's P. 1864 filling $\frac{400}{580}$ pages ∴ nearly $\frac{3}{4}$ of whole peerage

| | sons | dts |
|---|---|---|
| Abergavenny 7 | 4 | 2 |
| Abingdon | 6 | 3 |
| Airlie 7 | 9 | 1 |
| Airlie 7 2nd nip | 4 | 5 |
| Aldborough G | 1 | 4 |
| Antrim Countess | 7 | 8 |
| Ashbrook | 3 | 3 |
| Aylesford | 3 | 3 |
| Brougham 7 | 5 | 1 |
| Broughton 7 | 3 | 1 |
| Buchan G | 2 | 2 |
| Buckingham G | 1 | 0 |
| Bute G | 2 | 0 |
| Byron poet | 0 | 1 |
| Camden 7 | 1 | 3 |
| Churston 7 | 3 | 1 |
| Clonbrock 7 | 2 | 2 |
| ——— G | 1 | 2 |
| Combermere (but in at 65) | | |
| Crawford 7 | 4 | 2 |
| Cromartie (Duchess) at least | 3 | 1 |
| Sutherland (an only child) | | |
| Decies 7 | 1 | 3 |
| De Clifford Baroness | 3 | 3 |
| De Grey 7 | 2 | 1 |
| De Lisle | 4 | 2 |
| De Manley 7 | 4 | 1 |
| De Ros Baroness | 6 | 6 |
| | 86 | 64 |

Total 57 | 175 | 130

| | sons | dts |
|---|---|---|
| De Saumarez 7 | 4 | 3 |
| Donoghmore | 5 | 2 |
| Dunraven 7 | 2 | 1 |
| Elgin | 0 | 2 |
| ——— 7 (but divorced) | 2 | 3 |
| Elibank 7 | 6 | 7 |
| Elphinstone 7 | 4 | 4 |
| Exeter 7 (had only a 4th child) | 0 | 0 |
| Falmouth | 3 | 3 |
| Frankfort 7 | 0 | 0 |
| Gage 7 | 2 | 0 |
| Gainsboro 7 (m. baroness) | 12 | 5 |
| Gort | 5 | 6 |
| Granard 7 | 2 | 0 |
| Granville (a widow) | 0 | 0 |
| Harrington 7 | 2 | 2 |
| Harris 7 | 3 | 1 |
| Headfort 7 | 2 | 2 |
| Headley G | 2 | 2 |
| Henniker G | 3 | 1 |
| Hill | 2 | 0 |
| Kingsale 28th Bard | 3 | 1 |
| Lauderdale 8th Earl | 4 | 5 |
| Leeds (Baroness Conyers) at least | 2 ? | 0 |
| Leinster 7 | 5 | 8 |
| Lorton | 2 | 5 |
| Lovelace | 2 | 1 |
| Manchester 7 | 4 | 2 |
| Mazzarene G | 5 | 3 |
| Mountgarret | 1 | 1 |
| | 89 | 70 |

only $\frac{11}{57}$ had < 2 sons; 4 are quite young for transmission

∴ heiresses have av. sons $3\frac{1}{14}$ & 2·3 daus

The average of all married peers (the first 54 in Lodge) had exactly 3 sons & very nearly 3 daus: ∴ marrying heiresses loses nearly ·7 of a daughter & gains a small fraction $\frac{1}{14}$ of a son.

By former papers coheiresses have fewest sons 2·63; 'only daus' are apparently where there also no sons, have 3.2 sons $\frac{99}{31}$ & ~~nearly~~ just 3 daughters $\frac{92}{31}$

∴ an only daughter in a sonny family has most sons of all; next an only child; next one of ordinary family; worst, one of a purely daughtery family.

E.S.D. 9 Dec 09

Denison

15

12 Dec 69

Dear Galton I think I mentioned to you I had observed that nearly any peer of any distinction or unusual ability (as I see them in committees) has some non-noble blood within 2 degrees at most. I have looked very carefully through them till today, & I made my list first, independently of results, so as to be quite unbiased.

I take them just as they came into my head, & I need not notice such as Ellenboro'

& Canning whose fathers we all know of; & of course all the created ones.

Derby (late) M. d of Rev G Hornby
— present M d of a first peer
Palmerston M. d of 7 Lee Esq
Russell no Richmond no
Granville no
Salisbury M d of B Gascoigne Esq
Carnarvon grandd. d of Sir T Acland
Elgin (late) M d of J T Oswald Esq
Argyll M. d of W Campbell Esq
Stanhope M. d of first Lord Carrington
Rosslate M d of John Lloyd
— present M d of W Field
Rayleigh's son senior wrangler M d of Lord Viscount & Cowper first peer
Lyttelton grandm. d of J Bristow & M granddaughter of first Lord Spencer
Powis. a nearer than great Lord Clive his G Grandfather through female line

The first 126 of present ~~marriage~~ peers
marriage-table that their
families appear complete
(omitting newones who married
commoners also) have only
2·59 sons & 2·85 daughters
72 of these married under 28
& have 3 sons & 3 daughters
The 54 who married over 27
have only 2·24 sons & 2·54 d.

Often the later marriages
have fewer children, but it is
remarkable that the deficiency
in sons is entirely among
them. It is well known
that more boys than girls
are born; it appears

that peers are an exception & that not by reason of their marrying early

NB These cannot fairly compared with those in my former lists containing many Fs & some Gs, except in showing that ·4 of a son is gained by excluding them.

---

Of the first 72 peers 54 have either 2 degrees or have married non-noble blood: 18 not: or just 3 to 1.
But of distinguished peers 8 to 1 have non-noble within 2 degrees.

F G

Devonshire grandfather first L^d Spencer
Stanford (late) M d of Mr 7 Burke
Rockingham (a very clever man) M d of
W Pitt Esq
Devon - 7. first Lord
Grey PM 7. first Lord Mr d of C Grey Esq
Wellesleys 7 was first Lord Mornington

I remember nothing worth
looking for

Yours ever truly
E B Denison



I find a very small fraction
of the peers with < 3 sons
have m^d only children:
but I am not satisfied with
the length of my list yet. O under
50 is safe deduction from -

33 Queen Anne Street W
10 Dec 09

Dear Galton

Lodge hardly ever gives the mothers of 'Collaterals': so I cannot pursue the uncles. But he differs in some astonishing way from your Table of 1858: for every one of my 'sons' are named in the families where you say I have overrated them; and Arthur & Alice are quite right too. The dates that I overrated are against my own theory, & the mistake came from my having first written down 'children' in my 2d column & then turned them into dates in the paper I sent you, & occasionally I forgot to subtract.

The only result is that I gave rather too many daughters to the heiress marriages. I have finished the rest of the passage & the result as to sons is just the same – a small fraction over 3, & daughters 2·58 – rather less than before.

It is true that fathers must be people with sons; but I will do, from a first second or 3 marriage: the number of marriages (of this class) with no sons is so exceedingly small that the result can't be materially affected by my inability to pursue the collaterals.

I am satisfied that you have raged against the marriage of heiresses too hastily. If you had said the marriage of

only daughters tends to lower the number of daughters you would be quite right. And (as far one may judge from the small no. of 11 heiresses in my first list) it would be right to say that they are rather non-productive of sons but have rather > the average of daughters (which is quite natural).



I am surprised to see how few peers – only 81 out of the 450 or thereabouts, & a great lot of fathers & grandfathers & some GG's + many with 2 or 3 m's in Lodge & probably at least 1200 examinees on the whole, have married heiresses. I should have guessed much more than .06. By the bye Lady Antrim

is rightly included; because she was married as being a sole heiress though she had had a sister. Indeed I might have added one or 2 more of that kind, but they were not so certain; & also a few others who strongly appear to have been only ♀♀, though it is not quite clear.

Perhaps my Airlie 7 may be G in your book, as I see that wd. reconcile them: but the fact wd. remain the same.

I have no means of doing any more, & so here I stop, having learnt something. On the whole I like the book very much, though I am doubtful about some of your 'divine' speculations, on wh. statistics are all but impossible.

Yours very truly E B Denison

33 Queen Anne St W

9 Dec 69

Dear Galton

your remark about the 'only daughs' is just. But I had not included 'Lecanfield' by some mistake (wh you object to): wh is only an instance of casual omission on one side as well as the other, as he had 3 sons. Nor does the 'Lindsay' family (in Lodge 1864) furnish any case at all.

But now here is a list for you wh you may keep of all the heiresses & only children married of $\frac{3}{4}$ of the peers & their fathers & grandfathers; & you see the result is that they have actually more sons & fewer daughs than the average! Your 'extinct' Duke must have extinguished some dead children and left them sine vate zero. Such a large list as this of mine can't be affected by a few omissions, if there are any. Yours very truly E D Denison

f1v

I shall be at AB. tomorrow, so I send this

33 Queen Anne St W
14 Dec 69

Dear Galton I haven't had AA. today or tomorrow, so I send you all my gatherings: on which I make no remark except that I wish the Darwinians joy of the problem of selecting a wife with a view to sons, except that they had better avoid only daughters with brothers & cultivate copiresses: exactly opposite to what one would expect.

Yours very truly
E B Tylor

You need not assume till we meet on Thursday.

Results of whole peerage of 1864 Lodge
except where otherwise stated

married 'only child or daughter & heir' (but some ~~many~~ have had brothers dead)
including many fathers & some grandfathers, wherever given. ~~many cases~~

| m | sons | daus | | sons | daus |
|---|---|---|---|---|---|
| 100 | 296 | 246 | ∴ average | 2.96 | 2.46 |
| 50 | 164 | 144 | coheiresses ∴ av = | 3.28 | 2.48 |
| ∴ 150 | 460 | 390 | heiresses of both kinds ∴ av = | 3.07 | 2.6 |

ex Byron ∴ separated sons

m. 'only daughter', apparently with brothers.

| | | | | | |
|---|---|---|---|---|---|
| 81 | 214 | 196 | av = | 2.64 | 2.4 |

wives of all present peers with < 3 sons (∴ all question of fathers & grandfathers is eliminated.)

| with sons | with no son | with one son |
|---|---|---|
| 0 1 2 | 7 only daughters | 13 only daus |
| 97 . 81 . 45 | 2 coheiresses | 2 doubtful |
| | × 2 only children | 0 coheiresses |
| | × ~~of~~ one ~~[illegible]~~ ~~[illegible]~~ ~~[illegible]~~ | 3 only children |
| | | |

85 of the 450 peers are single.
∴ only 3 of the whole 450 are sonless through marrying heiresses or coheiresses

× by another m.

(NB Granville's wife had a son before ∴ omitted here)

Average of first 64 or 96 or 128 marriages (including F & G where given), excluding all the 3 classes above, is just 3. sons & about 2.85 daus.

And as these, like the heiresses, include F & G, that comparison is fair.

Of the first 120 present peers 110 succeeded their F or G — sometimes with a brother between who may have d. single.

Of the first 52, 30 m. daus not of noble families; 22 with brs. say 3 to 2.

Add Aberdeen, Malmesbury & late Lansdowne & Sligo (a very dem man) to fam list, & we have only 4 out of 32 who have entirely noble blood up to G on both sides of peers at all eminent present & very late; omitting all created ones.

E B D
14 Dec 69

f4

# From Demicus list.

children of
Only daus.

| Sons | daus | children | Sons | daus | chil |
|---|---|---|---|---|---|
| 5 | | 10 | 1 | | 1 |
| 5 | | 13 | 4 | | 5 |
| 5 | | 7 | 1 | | 1 |
| 0 | | 1 | 1 | | 5 |
| 4 | | 6 | 2 | | 3 |
| 1 | | 1 | 4 | | 4 |
| 1 | | 1 | 0 | | 0 |
| 5 | | 13 | 1 | | 2 |
| 6 | | 7 | 2 | | 2 |
| 3 | | 7 | 3 | | 8 |
| 4 | | 9 | 4 | | 9 |
| 1 | | 1 | 5 | | 6 |
| 2 | | 3 | 1 | | 1 |
| 1 | | 5 | 0 | | 0 |
| 8 | | 10 | 2 | | 2 |
| 3 | | 10 | 0 | | 1 |
| 8 | | 13 | | | |
| 4 | | 4 | | | |
| 1 | | 2 | | | |
| 4 | | 4 | | | |
| 4 | | 6 | | | |
| 6 | | 9 | | | |
| 9 | | 10 | | | |
| 3 | | 7 | | | |
| 4 | | 4 | | | |
| 1 | | 4 | | | |

43 cases

| n° of Sons | | |
|---|---|---|
| 0 | 𝍸 \|\|\| | 4 |
| 1 | 𝍸 𝍸 \| | 11 |
| 2 | \|\|\|\| | 4 |
| 3 | \|\|\|\| | 4 |
| 4 | 𝍸 \|\|\|\| | 9 |
| 5 | 𝍸 \| | 6 |
| 6 | \|\| | 2 |
| 7 | | 0 |
| 8 | \|\| | 2 |
| 9 | \| | 1 |

| Mar only daus | Sons | daus | chil. |
|---|---|---|---|
| Lauderdale 8th Earl | 4 | 5 | |
| only daus not heir — Lecinfield 1st Bar | 3 | 6 | |
| Le Despencer | 3 | 3 | |
| Leinster 2nd Duke surviving | 2 | 6 | |
| Limerick 2nd Earl 2nd wife | 6 | 1 | |
| Lindsey (Duke of Lincoln) | | | |
| 3rd Duke succ 1742 | 0 | 0 | |
| 5th Duke | 0 | 0 | |

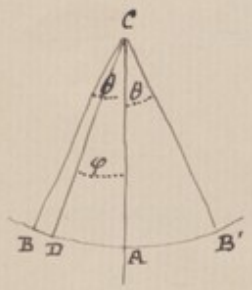
5 St. Peter's Terrace, Cambridge
18 Dec. 1888

My Dear Mr. Galton

Your letter came when I was very busy closing the Term hence the delay in answering it.

The question you have proposed is a well known one. Assuming a new angle $\psi$ such that

$$\sin\frac{\varphi}{2} = \sin\frac{\theta}{2}\cdot\sin\psi \quad \text{------} (1)$$




and if $CA = a$

$g$ = acceleration due to Earth's attraction

= 32 ft. per sec. per sec.

then the time from A to D is

$$t = \sqrt{\frac{a}{g}}\cdot F(k,\psi) \quad \text{---} \quad \text{where } k = \sin\frac{\theta}{2} = \sin\alpha \text{ (say)} \quad \text{-----} (2)$$

$F(k,\psi)$ being the first Elliptic Integral, modulus = k, amplitude = $\psi$.

If we call T the time from A to B, we have also

$$T = \sqrt{\frac{a}{g}}\cdot F\left(k,\frac{\pi}{2}\right), \qquad \text{or} = \sqrt{\frac{a}{g}}\cdot F_1(k) \quad \text{as it is sometimes written}$$

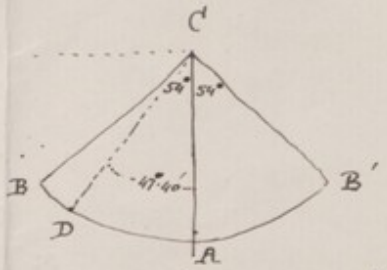
whence, knowing T we may write

$$t = T\,\frac{F(k,\psi)}{F\left(k,\frac{\pi}{2}\right)} \quad \text{------} (3)$$

I have been trying to get some suitable Tables of these F's, but not with much success up to the present. However in Hoüel's Recueil de Formules et de Tables Numériques, (3d Edn, Gautier-Villars, 1885) pp 58, 59, I find two Tables, one for $F(\sin\alpha,\psi)$, the other for $\frac{F(\sin\alpha,\psi)}{F(\sin\alpha,\frac{\pi}{2})}$, for each tenth part of a quadrant both for $\alpha$ and for $\psi$. The former table will be useful with formula (2), the latter with formula (3). To save trouble I have reproduced those Tables.

For example

The max. swing (AB) of a pendulum is 54°; and we want to know the time for 47°40′ (AD)




$\theta = 54°$ ∴ $\alpha = 27° = \cdot 3^g$

$\frac{\varphi}{2} = 23°\,50'$

Here $\sin\frac{\varphi}{2} = \sin\frac{\theta}{2} \cdot \sin\psi$

becomes

$$\sin 23°50' = \sin 27° \cdot \sin\psi$$

whence

$$L \sin\psi = L \sin 23°50' + 10 - L \sin 27°$$
$$= 9{\cdot}6065 + 10 - 9{\cdot}6570 = 9{\cdot}9495$$
$$= L \sin 63° \text{ nearly}$$

$\therefore \psi = 63° = \cdot 7^g$ [The small letter, g, like an index means "grades"]

Thus $t = \sqrt{\frac{a}{g}} \cdot F(\sin\alpha, \psi) = \sqrt{\frac{a}{g}} \cdot F(\sin \cdot 3^g, \cdot 7^g) = \sqrt{\frac{a}{g}} \times 1{\cdot}1386$ sec by Table I

Or, if we know that the double swing, BAB′AB, takes 1·52 sec., that is time from A to B = ·38 sec. Then by formula (3)

$$t = \cdot 38 \frac{F(\sin \cdot 3^g, \cdot 7^g)}{F(\sin \cdot 3^g, \frac{\pi}{2})} = \cdot 38 \times \cdot 6848 \quad \text{Tab. II}$$
$$= \cdot 26 \text{ sec. nearly.}$$



---

I shall try to get some more minutely divided Tables, but it is better to send you this in the mean time.

I am always glad to have problems, and if you don't mind the delay I usually take in sending answers, please do not hesitate about sending them.

With all kind Xmas wishes from Mrs Dickson and myself believe me

Yours very truly

J. Hamilton Dickson

Table I. Values of $F(\sin\alpha, \psi)$. (Legendre)

| | $\alpha = 0.0$ | 0.1 | 0.2 | 0.3 | 0.4 | 0.5 | 0.6 | 0.7 | 0.8 | 0.9 | 1.0 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| $\psi = 0.0$ | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 |
| .1 | .1571 | .1571 | .1571 | .1572 | .1573 | .1574 | .1575 | .1576 | .1577 | .1577 | .1577 |
| .2 | .3142 | .3143 | .3146 | .3152 | .3159 | .3167 | .3176 | .3183 | .3189 | .3193 | .3195 |
| .3 | .4712 | .4716 | .4728 | .4747 | .4772 | .4800 | .4829 | .4856 | .4878 | .4892 | .4897 |
| .4 | .6283 | .6293 | .6320 | .6365 | .6423 | .6491 | .6563 | .6633 | .6690 | .6729 | .6743 |
| .5 | .7854 | .7872 | .7924 | .8009 | .8123 | .8260 | .8411 | .8561 | .8692 | .8781 | .8814 |
| .6 | .9425 | .9454 | .9540 | .9682 | .9879 | 1.0124 | 1.0405 | 1.0700 | 1.0971 | 1.1169 | 1.1242 |
| .7 | 1.0996 | 1.1039 | 1.1168 | 1.1386 | 1.1694 | 1.2093 | 1.2575 | 1.3118 | 1.3664 | 1.4097 | 1.4268 |
| .8 | 1.2566 | 1.2626 | 1.2807 | 1.3116 | 1.3564 | 1.4167 | 1.4939 | 1.5886 | 1.6964 | 1.7972 | 1.8427 |
| .9 | 1.4137 | 1.4215 | 1.4454 | 1.4866 | 1.5478 | 1.6328 | 1.7481 | 1.9028 | 2.1094 | 2.3685 | 2.5421 |
| 1.0 | 1.5708 | 1.5805 | 1.6105 | 1.6627 | 1.7415 | 1.8541 | 2.0133 | 2.2435 | 2.5998 | 3.2553 | log ∞ |

Table II. Values of $\dfrac{F(\sin\alpha, \psi)}{F(\sin\alpha, \frac{\pi}{2})}$. (Houël)

| | $\alpha = 0{\cdot}0$ | 0·1 | 0·2 | 0·3 | 0·4 | 0·5 | 0·6 | 0·7 | 0·8 | 0·9 | 1·0 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| $\psi = 0{\cdot}0$ | 0·0000 | 0·0000 | 0·0000 | 0·0000 | 0·0000 | 0·0000 | 0·0000 | 0·0000 | 0·0000 | 0·0000 | 0·0000 |
| ·1 | ·1000 | ·0994 | ·0976 | ·0945 | ·0903 | ·0849 | ·0782 | ·0702 | ·0606 | ·0484 | ·0000 |
| ·2 | ·2000 | ·1988 | ·1954 | ·1896 | ·1814 | ·1708 | ·1577 | ·1419 | ·1227 | ·0981 | ·0000 |
| ·3 | ·3000 | ·2984 | ·2936 | ·2855 | ·2740 | ·2589 | ·2399 | ·2164 | ·1876 | ·1503 | ·0000 |
| ·4 | ·4000 | ·3981 | ·3925 | ·3828 | ·3688 | ·3501 | ·3260 | ·2956 | ·2573 | ·2067 | ·0000 |
| ·5 | ·5000 | ·4980 | ·4920 | ·4817 | ·4664 | ·4455 | ·4178 | ·3816 | ·3343 | ·2698 | ·0000 |
| ·6 | ·6000 | ·5981 | ·5924 | ·5823 | ·5673 | ·5460 | ·5168 | ·4769 | ·4220 | ·3431 | ·0000 |
| ·7 | ·7000 | ·6984 | ·6935 | ·6848 | ·6715 | ·6522 | ·6246 | ·5847 | ·5256 | ·4329 | ·0000 |
| ·8 | ·8000 | ·7988 | ·7952 | ·7888 | ·7789 | ·7641 | ·7420 | ·7081 | ·6525 | ·5521 | ·0000 |
| ·9 | ·9000 | ·8994 | ·8975 | ·8936 | ·8887 | ·8807 | ·8683 | ·8481 | ·8114 | ·7276 | ·0000 |
| 1·0 | 1·0000 | 1·0000 | 1·0000 | 1·0000 | 1·0000 | 1·0000 | 1·0000 | 1·0000 | 1·0000 | 1·0000 | 1·0000 |

J. H. Dickson

Pedestrian Cards.
after 9/1/91

4 Heriot Row. Edinburgh
Christmas day 1890

My Dear Mr. Galton

You must be kind to me in not having answered your last till now. I have been engaged every spare moment in a labour of love viz. trying to assist school-masters (as a rule, not a pecunious class) in getting up a scheme for their life insurance at somewhat reduced premiums. A committee of two - of whom I am one - has had all the work to do in examining the some hundred Life Assurance Comp^ies of this country, & we have been making calculations & meeting to discuss them sometimes daily. This is why I have not written till now: and

— being Xmas day — I thought I could not do better than write to-day.

I am very glad that you have got Dr. Watson — whose name I know well — on your side. I often wished that I could take your problems to a great man, but I felt that bashfulness — rather, fear — which an ordinary individual may be presumed to feel in presence of a giant, when I thought of the only one whose opinion I would accept simply because it was his — Prof. Cayley. With Dr. Watson on your side I shall look "keenly for his solution of your problem + my difficulty. For, quoting your words, I feel now "unhappy, but not convinced".

" Your proposal of the two letters — one from yourself + Dr. Watson's reply —

to be sent to 'Nature', would be an excellent method of publishing the matter. But would it not be well to get Dr. Watson to draft the question which you propose to add? All questions involving probability depend on so very slight shades of meaning, that this would give him the opportunity of making clear to his satisfaction that the question proposed is the one to which his letter has reference.

As far as my view is concerned (for instance) I dont see that your proposed question agrees with me. It is "What is the Prob: Error of a term "in the series $(x_a + y_a) - x_a$, $(x_b + y_b) - y_b$, "&c. where $x_a$, $x_b$, ... $y_a$, $y_b$, ... are "particular values of the variables $x$ & "$y$, and where the Prob: error of an "$x$ value is known to be $k$, & that of "an $(x+y)$ value is known to be $h$?

"The law of frequency to be dep = "proved applicable throughout."

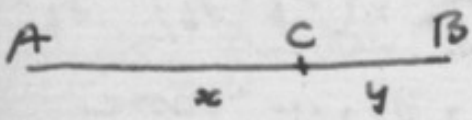
Then you proceed

"I think this justly expresses the "point at issue, that your reply to "the question is $\sqrt{h^2+k^2}$. ~~Is that~~ Is it "not so?"

To bring out my objections:—

1. The first is, or is not an objection according as the answer to the following question is no or yes:— Have you two series of <u>observations</u> viz, $x_a, x_b, x_c, \ldots$ + $x_a+y_a$, $x_b+y_b$, $x_c+y_c$, .... ?

2. I have a rod AB with a mark C upon it. Let me take three observations of the length AB, in inches, Let them be




$$x_1 + y_1 = 23{\cdot}46$$
$$x_2 + y_2 = 24{\cdot}31$$
$$x_3 + y_3 = 23{\cdot}39$$

Now, how am I to get? — or — have I already got? — or — when am I to get — the value of $x_1$? of $x_2$? of $x_3$? For instance, when I was measuring for the 1st observation I got — AB = 23·46, I happened to notice that the point C was just opposite 19·11 on my tape, — is that to be called $x_1$? Then (1') if it is, why is it to be $x_1$? would it have been another $x$ (say $x_n$) if my tape starting from A had been stretched out past C & B to some other point D on the table whose distance from A I wanted to know? or (2') if it is not, when & how am I to get $x_1$? & when comes, & what is the condition that another measurement of $x$ is to be called $x_1$? In fine I cannot conceive of a reason for

answering either (1') or (2') with either a yes or a no.

To pass to another subject. When you have leisure would you kindly send me a set of your 'dice' observations to a particular probable error — say a couple of sets. I am not sure that the method of pairs arranging the probable error, and second getting the observations, is consistent.

Many thanks for your kind inquiries for Mrs. Dickson. She has been improving, though very slowly, all this year: and while

f11

I do not fear a return of the serious complaint, she is often & frequently seized with sudden nervous fits which are very depressing, lasting sometimes for a week at a time. Meanwhile I am glad to say she has not had one for a month now.

Trusting that you & Mrs Galton are well, and wishing you — both from myself & Mrs. Dickson — a very happy Christmastide

believe me

Yours very truly

W. Hamilton Dickson

P.S. Bryan (a relative of Pet. Cole.) had told me that he had heard from Dr. Watson. Bryan is a very good Mathematician — a Smith's Prizeman. qo W.

f12


CAMBRIDGE
Francis Galton Esq
42 Rutland Gate
London
UNIVERSITY COLLEGE LONDON
GALTON
PAPERS
50

April 9, 1895.

Wadham College,
Oxford.



Dear Mr. Galton

In accordance with my promise, I send the reference to the recorded case of hereditary transmission of pink tubercles in S. capini. It will be found in the "Trans. Entom. Soc. Lond.", 1887, pp. 310–312, and in the same Society's "Proceedings", 1887, pp. xvii, xviii.

f2

The percentage in the second generation was lower than I thought, being a little over 70 instead of 90.

I take this opportunity of sending you one or two papers of mine which may possibly be of some interest ~~of~~ to you.

Believe me
Very truly yours
F. A. Dixey.

F3

f4

ON

# THE EPIDERMIS OF THE PLANTAR SURFACE

AND

# THE QUESTION OF USE-INHERITANCE.

BY

FREDERICK A. DIXEY, M.A., M.D.,

FELLOW OF WADHAM COLLEGE, OXFORD.

(Abstract.)



---

[*From the British Association Reports*, 1894.]

*On the Epidermis of the Plantar Surface and the Question of Use-Inheritance.* By F. A. Dixey, M.A., M.D., Fellow of Wadham College, Oxford.

## (Abstract.)

It is well known that in the human adult the skin of the sole of the foot is thickened as compared with that of the dorsum, and it is also known that this local thickening on the plantar surface is present before birth. It is therefore not the direct result of the use of the sole in walking, though it might possibly be held to be due to use-inheritance. But in addition to the general plantar thickening, there is also known to exist a special thickening, in the adult, of the skin covering the heel and toe-ball, which is no doubt correlated with the heel-and-toe gait specially characteristic of man. It was suggested to me by the late Mr. G. J. Romanes that it would be of importance to ascertain the time of appearance of this special as distinct from the general plantar thickening, inasmuch as its appearance before birth, should that be proved to occur, would seem to lend more support to the theory of use-inheritance than to that of pure natural selection. The general plantar thickening in the embryo might be held to be simply representative of the condition in a prehuman ancestor; not so, however, the special thickening of the heel and the toe-ball.

Six embryos, whose ages varied from about the third to the ninth month, were examined in concert with Mr. Romanes. As the inner limit of the corium is in most cases not exactly determinable, the epidermis alone was measured, and the results were as follows:—(1) The general plantar thickening had begun in the earliest embryo examined. (2) The special thickening of the toe-ball had also begun at the same age. (3) The special thickening of the heel was not discoverable in any one of the specimens, which ranged up to the time just preceding birth.

These results were unexpected, for it had been anticipated that both the special thickenings would have been found in the embryo to be either absent or present together, and at first sight it seemed as if no light were thrown by the observed facts on the question at issue. On further consideration, however, and after special study of the gait of the lower primates, it appeared to the author that the thickened epidermis of the toe-ball in the embryo simply represented an ancestral condition when the gait resembled that of most monkeys, who walk, as a rule, with the heel raised from the ground, only using the whole length of the sole when resting or squatting. The phenomena would therefore seem to admit of a far more easy explanation under the theory of natural selection pure and simple than under that of use-inheritance.

*August*, 1894.

f2r

f2v

# *VARIATION AND HEREDITY.*

## II.

(3) *Temperature Experiments in 1893 on several species of* VANESSA *and other Lepidoptera.*

BY

FREDERIC MERRIFIELD, F.E.S.

(4) *Mr. Merrifield's Experiments in Temperature-Variation as bearing on Theories of Heredity.*

BY

FREDERICK A. DIXEY, M.A., M.D., F.E.S.,

FELLOW OF WADHAM COLLEGE, OXFORD.



---

[*From the Transactions of the Entomological Society of London, 1894.*]

# *Experiments in Temperature-Variation on Lepidoptera, and their bearing on theories of Heredity.*

[Extract from the "Proceedings of the Entomological Society of London," Part I., April 1894.]

14th March, 1894.

Mr. F. Merrifield read a paper entitled "Temperature experiments in 1893, on several species of *Vanessa* and other Lepidoptera." He said that the winter-pupating brood of *Pieris napi*, like the summer-pupating brood, depends for its characteristic seasonal colouring partly, but by no means entirely, on the temperature to which the individual is subjected. *Pararge egeria* was rendered more vivid in its markings and colouring by low temperatures, but there was no approach to the bright South European form, either at high or at low temperatures. In *Cidaria silaceata* of the summer-pupating brood, low temperatures caused a resemblance to the winter-pupating brood, high temperatures caused a duller and more uniform appearance, and reduced the size. In *Araschnia levana*, the brood reared from eggs laid in the spring, high temperatures in every case produced the blackish normal summer or *prorsa* form, while severe cold in every instance transformed the insect completely into the bright fritillary-coloured spring (or *levana*) type. In *Vanessa polychloros*, to use the language of Dr. Dixey, who had examined the insects, forcing invariably tended to produce yellow, whether the pupa was previously warmed or cooled, and refrigeration produced increased breadth of the dark border, whether followed or not by forcing. By a succession of high and low temperatures, presumably ancestral markings were brought out in the shape of a faint sub-marginal chain on all the wings of yellowish cloudy spots with minute black centres. Some of the individuals subjected to severe cold were very dark and closely resembled *V. xanthomelas*. In *Vanessa atalanta* very high temperatures developed the golden brown parts, both in extent and brightness, new small scarlet spots

beneath, and, above, a scarlet cloudy patch on the forewings, between the large white costal spot and the third of the outer row of white spots, about which it tended to form a scattered ring. Low temperatures caused a great extension of the blue-green, lavender and white markings, and much suffusion. Some specimens obtained or bred under natural circumstances, but in very cold weather, and forwarded to him by Mr. J. J. Walker and Dr. Chapman respectively, showed effects similar to these, but very much less in degree. In *Vanessa c-album* both the first or July emergence and the later or September emergence are affected by moderately low temperatures, but the former responds in a much greater degree than the latter; this seemed to be quite in accordance with Mr. W. H. Edwards' observations and experiments on the allied North American *Grapta interrogationis* and *Grapta comma*. In *Vanessa io* low temperatures disintegrated the ocellus on the forewing, reducing it to a chain of white spots, and brought out other presumably ancestral features. In *Vanessa antiopa* no results had been obtained, but this was perhaps owing to the pupæ being too old. The experiments generally confirmed the previous conclusions of the writer; but there was much more to be learned by further experiments in the same direction, and probably in the direction of other circumstances affecting the insects—moisture, foodplant, and possibly light, electricity, and magnetism, though as to the three last, such experiments as had yet been tried by the writer had produced no results.

Dr. F. A. Dixey read a paper entitled, "Mr. Merrifield's experiments in Temperature-variation as bearing on theories of Heredity," which was supplemental to the previous paper. The author said that the interest of Mr. Merrifield's experiments was much enhanced by the fact that many of the changes produced were of a reversionary character, and that the restored ancestral features differed with the nature of the disturbance. The present series of experiments not only confirmed former conclusions to this effect, but added new evidence of the same kind, reference to which was made by Dr. Dixey in detail, with the help of diagrams.

The question might still be raised whether these were, after all, genuine cases of reversion, or whether they were not simply to be explained as the like effects of like causes, produced *de novo* in both ancestor and descendant. The latter supposition, no doubt, was sufficient to account for some of the phenomena observed; but there was a residuum, comprising the more special reversionary features, which could hardly be so explained. What was the bearing of these latter on the general doctrine of reversion? Current explanations of atavism as a result of disturbance were inadequate, inasmuch as they gave no real reason why the more recently-established features should be less stable than those with a longer ancestral history behind them. As to the two more definite explanations afforded by the theories of Darwin and Weismann, there was no doubt that if Darwin's hypothesis of centripetal gemmules were granted, the most usual cases of atavism (those following hybridisation) could be explained under the theory of pangenesis. The present cases, however, stood on a quite different footing, as the new conditions determining atavism were only applied at an advanced period in the life of the individual, and had no reference to the ovum from which that individual originated. On the other hand, it seemed that if Weismann's theory of centrifugal carriers of heredity were assumed, the present instances could be explained as being due to the critical influence of abnormal temperature-conditions on what Weismann called "the struggle of the ids in ontogeny"; the new external conditions favouring some of the ancestral determinants (which *ex hypothesi* exist in the germ plasm) at the expense of those more proper to the species.

Certain observations seemed to show that some, at least, of these features might be hereditary; and it would be most desirable to ascertain whether this were so with all or most of them. Their transmission, though not their first appearance, could be accounted for by pangenesis; but under the rival hypothesis it would be necessary—in these cases of heredity—to postulate, as Weismann now does, a direct effect upon some of the determinants wherever they occur, even in

the germ-plasm itself. If *all* could be shown to be hereditary, it would seem to follow that the supposed influence upon the struggle of the ids was really inoperative, but the issue would still remain open between pangenesis and a direct modification of the determinants in the germ-plasm. If some, as seemed probable, turned out to be non-transmissible, the effect might be tried of varying the period in the ontogeny during which the disturbance was applied, with the view of ascertaining whether the intervention of new conditions at different stages of the struggle of the ids would not produce different results.

In conclusion, Dr. Dixey pointed out the importance of a well-established phylogeny as a basis of investigation, since it was only in groups of which the phylogeny was known that the ancestral character of these variations could be pronounced upon with certainty. For many reasons the Lepidoptera formed a peculiarly suitable group for such experiments, which might fairly be expected to throw much additional light on the complex subject of heredity.

Colonel Swinhoe referred to Dr. Dixey's remarks as to the phylogeny of *Argynnis* and *Vanessa*, and asked if he considered the male or the female of *Argynnis niphe* the older form.

Mr. Hampson pointed out that the peculiar aspect of *A. niphe* ♀, was generally attributed to mimicry of *Danais chrysippus*, though he was not himself prepared to endorse that opinion.

Colonel Swinhoe thought that the facts of the distribution of the two insects were not incompatible with the supposed mimicry.

Dr. Dixey, in reply, said that he believed the marginal and apical areas of dark ground colour in *A. niphe* ♀, were of more ancient origin than the tawny colour of the male. The special features of mimics were often retained rather than acquired, and whether *A. niphe* were a mimic or not, he should be inclined to consider these areas as relics of an ancestral feature. The evidence as to the original dark ground colour of *Argynnis* was cumulative, and he begged to refer to his paper in Trans. Ent. Soc. Lond., 1890, pp. 102-105, for a fuller discussion of the question.

subjected, adding that the spring emergence appears to be less sensitive than the summer emergence is to temperature. Neither of the broods experimented on by me has proved so sensitive as those operated on by Prof. Weismann, and described by him ("Studies in Heredity").

*Pararge egeria.* This insect has two well-known climatic forms, the light spots in the South European form having the bright ochreous colouring of *P. megæra*, instead of the straw colour of the English var. (*egerioides*). But the experiments tried afford little or no reason for supposing that these differences in appearance are the direct result of temperature.

I obtained, between the 25th May and the middle of June, more than one hundred pupæ, which were subjected to various temperatures from between 80° and 90° down to 33° (for many weeks), with various transfers from the lower to the higher temperatures.

The chief difference, in general appearance, is between Classes I. and II. (forced) on the one hand, and Classes IV.–XIV. (those at 56° and under). The former have the light spots smaller and less clearly defined, and the dark ground colour considerably lighter, and in many cases freckled with small dark brown spots. Class III. (open air, at about 66°) are not quite so dark as Class IV., but much nearer to them than to the forced.

Though the light spots in those forced are smaller than in the others, they are somewhat more numerous. Class VIII. (iced and then forced) have the ground colour dark, but the light spots as numerous as in those which were forced, and as large as are the spots in those at the lower temperatures. One of these indeed, which I exhibit, has an inner row of light spots or traces of them on the secondary wings in nearly all the interspaces, and on the underside a perfect submarginal chain of six light spots, pupilled with dark brown, on a light ground colour.

A few examples I reared from eggs laid in August showed the same effects generally as those from the eggs laid in April and May, so that there does not appear to be any marked constitutional difference in this respect between the spring and summer emergences of this species; my experiments would, however, lead me to expect those which emerge in spring to be in general

more vivid in their markings and colouring than those which emerge later, after a spell of hot weather.

*Cidaria silaceata.* This is known to be very variable in its markings, and I was tempted to experiment on it by the remark of M. Guenée that the spring and summer broods appear to vary as in the *Selenias*, and by other observations (Ent. Record, ii., 297), to the effect that the summer brood is smaller, and the band across the forewings less broken.

Mr. Nicholson kindly gave me some eggs early in April, from which I had pupæ, which were subjected to about the same variety of temperatures as were the pupæ of *P. egeria*, above mentioned.

The main difference is between those at or over 80° and the rest; the latter being more strongly marked than the former, the transverse band perhaps showing a slightly greater tendency to be broken, and their light markings being of a rather more ochreous tint; as a consequence, those at 80° or over have a duller and more uniform appearance than the others.

But the most distinctive feature is in the size. Those at or over 80° are, as a class, smaller than the others. This species, therefore, must be added to those in which temperature, during the pupal period, affects the size of the imago. The difference in colouring and marking is hardly as great as I should have expected, and seems scarcely so great as that sometimes met with between the spring and summer natural emergences, though it is in the same direction. I intended to experiment on the winter pupating brood, but a brood which I had from the second emergence came out as a third emergence, and were nearly all spoiled before I discovered that they had emerged.

*Araschnia levana.* Desiring to experiment with pupæ of the summer emergence (var. *prorsa*) for their whole pupal period, I obtained, through Mr. Edwards, from North Germany, a large number of pupæ from which I had, towards the end of April, thirteen good pairs, which I placed over growing nettle, in headless casks, and fed on orange, etc. All circumstances, including abundant sunshine, appeared most favourable; but I only obtained thirty-two eggs, laid (I believe by a single parent) mostly in strings of from two to eight, projecting from the undersides of the leaves. Only eleven

## XV. *Temperature Experiments in* 1893 *on several species of* Vanessa *and other Lepidoptera.* By Frederic Merrifield, F.E.S.

[Read March 14th, 1894.]

Plate IX.

In my last paper (Trans. Ent. Soc. Lond. 1893, p. 57) I described some experiments on pupæ of *P. napi*, offspring of the spring emergence, and mentioned that I had some pupæ offspring of the summer emergence. The parents consisted of two males and two females, taken at Hailsham, and kindly sent to me by Mr. Vine on the 30th July, and two females taken at Petworth on the 5th August, and kindly given to me by Mr. Fletcher. From these I had several hundred eggs, which were laid much more freely on *cardamines* than on cabbage, though the larvæ seemed to feed as willingly on the latter as on the former.

Early in September nearly all pupated. Ten were placed at 90° for ten days, a period amply sufficient to have caused their emergence had they belonged to the earlier brood, but it produced no effect either on the date of their emergence, when afterwards placed out of doors, or on their markings or colouring. The rest were kept out of doors. Some were forced about the middle of February, and some more early in March, emerging in from eight to seventeen days. The difference in appearance between these and the rest, which emerged out of doors between the 20th April and 9th May (except one which emerged 6th June), is the same in kind (though somewhat less in degree) as the difference between those of the summer emergence, which were forced all through, and those of that emergence which were cooled for the greater portion of their pupal period, as described Trans. Ent. Soc. Lond. 1893, p. 57.

Consequently I may apply to the winter pupating brood the general remark made there, as to the brood which pupates in the summer, viz., that a part but not all of the characteristic seasonal colouring of this species depends on the temperature to which the individual is

hatched, beginning 15th May, and all of them pupated. Three were forced at 80°, producing in from six to seven days the characteristic black *prorsa* form; four others were, at from one to eight hours' old, placed in the refrigerator on the 18th June, and remained there, at about 48°, till 30th or 31st August (seventy-three or seventy-four days), when, observing signs of emergence, I placed them in the room at about 65° to 70°, and there three of the four emerged in a day or two, the fourth not until thirty-two days more, *i.e.*, on the 2nd October.

The remaining four were placed at 33° till 29th September (eighty-four days), then moved to the refrigerator at about 48°, whence, after twenty days more, they were moved to the room, about 59°, three of them emerging respectively in ten, eighteen, and twenty days more (total 114–124 days). The first of them was a cripple, and the fourth died. This left six of the eight subjected to a low temperature. These six all emerged in perfect condition, and were unmistakably of the true *levana* type; two of those cooled, without having been iced, showing slight traces of the intermediate *porima* form, but the other four being of the pure *levana* type, and nearly as different in colouring from the three that were forced as one of the common fritillaries—say *A. selene*—is from a "white admiral" (*L. sibilla*). I exhibit examples of the two forms.

I now proceed to describe some experiments on four species of the genus *Vanessa*. In reference to these I have had the great advantage of submitting the specimens experimented on in the manner described in this paper, to the careful examination of Dr. Dixey, whose paper on the phylogenetic significance of the wing-markings in certain genera of the *Nymphalidæ* will be found in Trans. Ent. Soc. Lond. 1890, p. 89, and he has favoured me with valuable observations upon them, which I am permitted to append; they are distinguished by being placed within brackets. The "series" and "spots" referred to in these observations are described in Dr. Dixey's paper, and may be indicated generally as follows, *V. urticæ* and *V. polychloros* being convenient examples for the purpose:—Four dark patches on the costa, continued in series more or less perfectly across the wings, distinguished by the Roman numerals I., II., III., IV., the latter including the dark submarginal band.

Four lighter areas, A, B, C, D, alternating with the dark ones, A being the innermost, and coming before I.; these also continued in series more or less across the wings.

*Vanessa polychloros.* There were sent me on the 6th May, a brood of between 130 and 140 larvæ found on sallow in the New Forest; they were about three-eighths of an inch in length. I found they would eat cherry and birch, but seemed to prefer willow, on which accordingly I placed them, at first in two, and afterwards in four, large sleeves. On the 27th May, one had begun to spin, and all were brought indoors and fed on cut willow. There were 138; by the 31st 127 had spun up, and the rest followed in a few days. Their pupæ were subjected to temperatures ranging from 100° to 32°, being in many cases transferred, after a time of varying length, from the lower to the higher temperature, or *vice versa.* I proceed to give their treatment in detail:—

Class I., forced at from 90° to 100° (Plate IX., fig. 1). Most of these died, but a few at 90°, or a few degrees lower, did well; forced as pupating larvæ or pupæ under twelve hours, they emerged in seven days. A second lot of ten (Class II.) were put in a shady place out of doors, where the temperature averaged about 62°, and all emerged in from twenty to twenty-two days. A third lot (Class III.) were placed in a cellar at a temperature averaging about 56°, where all emerged on the fortieth day. A fourth lot (Class IV.) were placed in the refrigerator at about 48°, and, after periods ranging from fourteen to forty-six days, transferred to (*a*) the forcing box at 80°–90°, emerging in from three to five days more; (*b*) the cellar at 58°, emerging in from twelve to twenty-five days more; or (*c*) the room, at 68°–75°, where they emerged in from four to five days more. A fifth lot (Class V.) were iced for periods ranging from fourteen to forty-two days, and then (*a*) placed at 80°–90°, emerging in five to seven days more; (*b*) in the cellar at about 59°, emerging in twenty-seven to thirty days more; or (*c*) the refrigerator at about 49°, for from six to thirty-two days, and then, either the cellar at 58°, emerging in from twenty-two to twenty-three days more, or the room at about 68°–75°, emerging in two to twenty-eight days more. With the exception of a few that were injured by accidents, of those that were killed by excessive heat, as mentioned before, and of four or five among those longest iced, all emerged, and except among some of those iced, there were no cripples.

The effect on colouring was as follows :—

Class I. (forced at about 90°, or upwards), emerging in seven days). The ground colour of a lighter and yellower hue of brown than is normal, with many yellowish clouds and broad streaks, especially in the interspaces of the nervures on the outer half of the forewings. [Black spots generally are more sharply defined than in normal specimens. There are no blue submarginal crescents in the forewings, but many bluish scales on the extreme margin. The spots in "Series D" are particularly well-defined near the costa, and are not pupilled.]

Class II. (shade, out of doors, at 51°–69°, averaging about 62°, emerging in twenty to twenty-two days). Ground colour of a redder brown, and with few yellowish clouds; the yellow submarginal outer line is especially reduced, and the dark band inside it widened and darkened.

Class III. (cellar at 54°, rising to 58°, averaging 56°, emerging in forty days). Effects intensified; the yellow submarginal line has almost disappeared, and there is a scattering of dark spots on the ground colour, in some cases forming a streak in front of the inner edge of the forewings. [There are indications of blue submarginal crescents in the forewings, but less blue in the fringe or extreme margin than in I. The spots in "Series D" sometimes bear minute black pupils.]

Class IV.*a* (refrigerator, about 49°, fourteen to forty-six days; then forced at 80°–90°, emerging in five days). Effect rather a mixture of those in Classes I. and III.; the scattering of dark spots exists, but the yellowish clouds and yellow submarginal streaks are partially restored; in those exposed to the low temperature for forty-two days, there were several that died or were crippled, and the dark markings in some others are varied with a paler hue, giving rather a "greasy" appearance to these dark parts. [The spots of "Series D" often with minute black pupils; "Series C" is indicated in the hindwings by a row of black points; a new dark spot tends to be formed between "II.8" and "III.8."]

Class IV.*b* (refrigerator at about 49°, fourteen to forty-six days; then cellar at 56°–60°, emerging in twenty-five to twelve days, or room 65°–75°, in five to four days). Much like Class III., except that the ground colour is duller, and the submarginal blue tends to be supplanted by black; in those longest exposed to cold, the dark parts tend to spread. [Tendency to formation of new dark spots continues.]

Class V.*a* (iced at 33°, fourteen to thirty-eight days, then forced at 80°–90°, emerging in seven to five days). These, unless iced

twenty-nine days or more, are very like IV.*a* ; iced for that or a longer period, they are darker; in all cases they show a return of the yellowish markings.

Class V.*b* (iced at 33°, fourteen to forty-two days, then at various temperatures, such as cellar at 59°, emerging in twenty-seven to thirty days ; or refrigerator at about 49°, six to thirty-two days, and then in cellar or room, emerging in twenty one to two days more). These are classed together, because the effects seem to depend on the duration of the icing. No great effect is produced under twenty-nine days' icing ; the extreme darkness, often without crippling, is produced by icing thirty-six days, followed by the refrigerator at 49°, for six to nine days (Plate IX., fig. 3); but some taken straight from the ice to the cellar are nearly as dark. Of those iced from thirty-eight to forty-two days nearly all died, or were more or less crippled ; one of the latter has nearly all the four spots on the forewings obliterated ; and it may be observed that the icing for thirty-six days or more, followed by the refrigerator, which produces the extreme dark effect, has a tendency to cause the normal spot near the inner edge, which is nearest the base of the forewing, to disappear.

As regards the general appearance of those which show the extreme effect of the low temperature, it may be said that they much resemble *V. xanthomelas.* [Tendency to formation of new dark spots continues. An additional dark spot may also appear in cell (forewing) below "I" and "II." The border may become uniformly dark.]

A second company of *V. polychloros*, just changing their last skin, reached me on the 2nd June, also on sallow. Sleeved on cherry they did very well. Some were forced, emerging in six and a half to seven days. Others, placed out of doors at a temperature ranging from 67° to 59°, averaging about 64°, emerged in sixteen to seventeen days. This was a rather lighter coloured company of butterflies. There is the same kind of difference in appearance between the forced and the others, as there is between Classes I. and II. of the first company, but it is less in quantity.

*V. polychloros, general conclusions.* The colouring is considerably affected by temperature in the pupal stage, low temperatures producing a deepening of the ground colour and an extension of the dark markings; and high temperatures producing a lightening of the ground colour and an extension of the yellowish markings. The blue and bluish markings are strongest in those at moderately

low temperatures, Classes III. and IV., in many of which they form some rather bright crescents on the forewings; but at the extremely low temperatures they tend to be supplanted, in some cases entirely so, by black.

[Forcing invariably tends to produce yellow, whether pupa previously warmed or cooled. Refrigeration produces increased breadth of dark brown, whether followed or not by forcing.] I may add that among the specimens I exhibit, one belonging to Class IV.*a* (refrigerator thirty-eight days, then cellar four days, and forced three days) (Plate IX., fig. 2), to which my attention has been directed by Dr. Dixey's observations, is particularly interesting, showing "Series D" as a nearly complete chain of faint yellowish spots, or rather clouds, on both forewings and hindwings, the anterior three or four on the forewings, and all those on the hindwings having each a small black point in the centre. It seems as if it required cold, succeeded by heat, to cause this chain of *yellowish spots centred with black* to be brought out.

Nothing has been said about the colouring of the under sides. This varies moderately in darkness or lightness, but I have not been able to associate this variation definitely with temperature.

*Vanessa atalanta.* In looking carefully, and aided by a strong light, at the *V. atalanta* upon which experiments were made in 1892, as recorded, Trans. Ent. Soc. Lond. 1893, pp. 58–62, I noticed a feature which had escaped me before, viz., that eight out of the ten which were subjected to the high temperature of 80°–90° had a few dull orange scales on the upper side of the forewings, between the large white costal blotch and the row of smaller white spots nearer the hind margin.

I determined to develop this tendency by exposing some pupæ to a greater heat. The effect was a great development of this orange colour, both in intensity, it becoming distinctly scarlet, and in quantity, so as to form a scarlet cloudy patch sufficient to attract attention on casual observation. I exhibit several examples. This patch is between the large white costal patch and the third of the row of white spots beyond, and tends to form a scattered ring around this third spot; other scales, from golden brown to scarlet, are developed along the outer part of the costa and near the base, and elsewhere on the forewing.

The following is a description of the treatment to which the pupæ were subjected, and of its results :—

A considerable number of pupæ were placed at a temperature of about 100°, at which nearly all died, after progressing so far as to show their imaginal colouring. I then lowered the temperature to about 90°–95°, with the result that the great majority of them show these markings, the scales being scarlet instead of dull orange, and in several cases being so increased in quantity as to form a scarlet clouding sufficient to attract attention when the insect is held at arm's length and more. These scarlet scales follow the nervure which separates the second from the third of the row of white spots above referred to; they are not on the nervure, but on each side of it, and in some cases extend to the nervure next below, and have a tendency to form a scattered ring round the third of the white spots. These scarlet scales are also to be found on the costa, extending in some cases from the beginning of the large white costal patch almost to the apex of the wing (an ochreous colouring in this region is to be observed in captured specimens). Associated with these markings is an increase of the brightness and warmth of the golden-brown colouring of the costa and nervures in the basal part of the wing; in some crippled specimens this golden-brown is very vivid, and makes some approach in hue towards the scarlet band across the wing; in some of these also a patch of the scarlet scales is to be found between the middle of the scarlet band and the large white costal spot.

In my paper on the experiments of 1892, I mentioned that on the underside of two out of ten specimens at from 80° to 90°, a new small scarlet spot appeared between the scarlet band across the forewings and the inner edge. In only six individuals out of those subjected to a high temperature in 1893 do I find scarlet in this region. I find it occurs in two places, both below the median nervure, viz., (*a*) just before it forks, (*b*) a little below the lower branch of the same nervure. One specimen (No. 4) shows both (*a*) and (*b*); three (Nos. 21, 39, and 49) show (*a*) only, and two of them but slightly; and two (Nos. 48 and 188) show (*b*) only.

The scarlet scales on the upper side are found in the great majority of those (about thirty) that were subjected to a temperature of 90°, or upwards, during their whole pupal period, and in three out of thirty-three that were subjected to 80°–100° during the earlier part of their pupal period; they are not found in any of the forty or so that were subjected to lower temperatures. The

scarlet patches on the under side are only found in six out of forty-eight which were subjected to 99° or upwards, and five out of the six were so exposed for practically their whole pupal period.

In the opposite direction of low temperature, I tried further experiments with *V. atalanta*, the most marked results of which I exhibited at the meeting of the Entomological Society on the 8th November, 1893. They confirm, and in some cases carry further, the results obtained in the previous year. The low temperature causes much substitution of white, lavender, or metallic blue-green scales (one of these colours seeming readily in this insect to pass into another of them) for the black in normal specimens; the large white spot on the costa is greatly enlarged and spread, and the tendency of the third of the row of submarginal spots to ocellation which is above referred to, as caused by a high temperature, is shown in a different manner by the low temperature, which tends to form a whitish ring round, and very near to it (this third spot is on the underside ocellated in normal specimens).

The extreme low temperature forms are, on the whole, so decidedly smaller than the average, that I am inclined to think the low temperature is a cause of reduced size in this species, more especially as those at the high temperatures, even where this is so extreme as to kill some of them, are all of full size.

[*Forced.*—Resolution of inner margin of red band fairly marked.

*Refrigerated.*—Marginal blue much extended, especially about anal angle of hindwing and in centres of spots of "Series IV."; spots of "Series D" often ringed with pale blue, apart from ocellation; indications present of bluish centres to "Series III." in hindwings; greater general blackness.]

Mr. J. J. Walker has kindly given me a specimen from Gibraltar which resembles, in its main features, some of my earliest individuals. It was taken, recently emerged, on the 17th February, and I find that the mean temperature of January and February at Gibraltar may be as low as 48·7° and 50·9°. Dr. Chapman has kindly sent

me some *atalanta* of a very late brood reared last October and November at Hereford, which also present some of the characteristic appearances of my cooled specimens.

*Vanessa* (*Grapta*) *c-album.* Owing to the kindness of Mrs. Hutchinson, who sent me some eggs laid by hibernated butterflies in the spring, and of Mr. Nesbitt, of Llandogo, who sent me larvæ of the second brood at the end of July, I have been able to ascertain that while both broods are affected by temperature in the pupal stage, the first brood is much the more sensitive of the two. It is remarkable that there should be so great a difference in constitution between these two broods, as, under natural conditions, the pupæ of both broods are exposed to temperatures differing by a very few degrees, the one passing the pupal stage in England about June, and the other about August. It is entirely consistent, however, with Mr. W. H. Edwards' experiments and observations on the two closely-allied American species, *Grapta interrogationis* and *Grapta comma*, as described in the "Canadian Naturalist" for 1877 and 1878, and much light is thrown on the subject by Prof. Weismann's observations on Mr. Edwards' experiments, in the Professor's "Studies in Heredity," by Prof. Meldola, vol. i., p. 149. The individuals experimented on, eight of the first brood and eleven of the second brood, were not sufficiently numerous to justify me in describing the results in detail; but I hope to try experiments on a much larger scale during the present year.

*Vanessa io.* Mr. Morris, of Lewes, kindly gave me a company of about one hundred and twenty larvæ, all in their last skins, or nearly so, on the 15th June, which in the extraordinarily early season of 1893 was late for them. I was much occupied in other ways, and perhaps it was owing to insufficient attention that I did not obtain more than about sixty pupæ, and those were a little under the full size. They were subjected to various temperatures from about 100° downwards. Those at 100° all failed to emerge. Sixteen, which were at 90° for six days, and then at 80°, all emerged in one day more, making seven days. As the temperature was lowered, there appeared a gradual tendency to disintegration of the ocellus on the forewing, until in one

(Plate IX., fig. 4), iced at 33° for twenty-two days, then in the refrigerator for twenty days, and then in the cellar for eighteen days, it ceases to be an ocellus, being resolved into a chain of small white spots, which are bright, with only a very slight bluish shade about them, and affording a remarkable confirmation of Dr. Dixey's views of the origin of that ocellus, as exemplified in the plate attached to his paper in the Transactions for 1890. In these iced and cooled specimens the blue becomes more vivid, and a narrow dusky marginal band, slightly darker in hue than the chestnut brown ground colour, appears, with a submarginal incomplete row of small dusky spots, very distinct. On the hindwing there is little change, but a tendency to disintegration of the blue in the ocellus.

Dr. Chapman kindly sent me, on the 30th August, part of a second brood he had found rather more than half grown. From about forty larvæ I obtained thirty-eight fine and healthy pupæ, but nearly all were killed by the severe cold to which I subjected them, though only a little in excess of that to which No. 61 was exposed.

[I. *Forced*: a tendency shown to the development of dark spots at the apices of the interspaces ("Series II."); tendency towards fusion of bluish constituents of ocellus in hindwing. II. *Cooled*: tendency of "IV." (marginal chain) to separate from "D" (light apical spots); "IV." rendered more distinct in forewing. III. *Iced*: separation of "D" and "IV." as in II. "Claw-mark" tends to lose regular curve, and to become angulated. Bluish constituents of ocellus in hindwing tend to become separated into two parallel series—"III." and "IV.," *i.e.*, a marginal and submarginal.]

*V. antiopa*. About seventy pupæ, mostly rather fresh, were obtained for me from near Berlin by Mr. Edwards, on the 19th July, and were subjected to various temperatures from about 100°, emerging in from three to five days, and 80°, when they took a day or two more, downwards. The most severe temperature survived, without injury, was twenty-seven days in the refrigerator, at about 47°. All that were placed in ice (33°) for twenty days or upwards died, except one that was a cripple. The results are negative, as none show any marked differences in marking or colouring that can be assigned to temperature. The absence of positive

results is very probably owing to the circumstance that the pupæ were all of them several days old when they reached me.

The experiments now recorded confirm in general the conclusions drawn from such as have preceded them, and some of which may be briefly enumerated as follows:—(1) The effects of temperature are different when applied at different periods of the pupal stage. (2) A great range of temperature may cause but little difference in appearance, while a very few degrees near the top or bottom of the range the insect will bear may cause a great difference. (3) There may be a great constitutional difference in sensitiveness to temperature between two seasonal emergences of the same species. (4) This may be so even when both pass the pupal period at about the same temperature (this is in accordance with Mr. W. H. Edwards' observations above referred to). (5) While some kinds of effect seem to be what may be called the direct result of temperature, in others, and perhaps the most important, temperature appears to operate by causing the individual to "throw back" to some ancestral form; this last circumstance has been considered to explain the reason why a low temperature in some species causes darkening of the colours, and in other species produces the opposite effect. (6) In these cases of "reversion," the kind of effect produced appears to depend on the stimulus applied, low temperatures producing one class of effects and high temperatures a different class of effects.

The whole subject is one of much complication, and calls for further experiments in many directions. The direction which mine have taken, following in the lines initiated by Weismann and W. H. Edwards, especially if pursued with species belonging to regions where the seasonal or other occasional differences of temperature are extreme—North America, Siberia, Japan, or the vicinity of mountains—will help to trace, and separate from the rest, such of the causes of variation as depend, directly or indirectly, on temperature. Systematic experiments on a number of well-selected species belonging to countries where the seasonal difference is hygrometric rather than thermometric, would probably produce valuable results. The nature of the food-plant,

which undoubtedly influences size and vigour, and is generally considered also to influence markings and colour, offers another line in which experiments of a more systematic and comprehensive character than any yet tried would undoubtedly well repay the labour attendant upon them. There are other natural surroundings, most potent for many purposes, the effect of which might usefully be tested by experiment, such as light and electricity and magnetism. As to light, I tried, in 1891, some experiments on *B. cynthia* and *S. illustraria*, recorded in the Trans. Ent. Soc. for 1892, p. 42; but, so far, with negative results. And in 1891 I tried some experiments with strong magnetic currents on some Lepidoptera in all their stages; but these yielded no positive result.

NOTE.—As this paper and Dr. Dixey's, which succeeds it, contain observations on some of the same facts by two different and independent observers, there is necessarily some repetition, but in order to reduce this as much as possible, I have greatly condensed my own observations.

---

## EXPLANATION OF PLATE IX.

FIG. 1. *Vanessa polychloros*: pupa at 90°–95°, emerging in seven days.

FIG. 2. *V. polychloros*: pupa about 49°, for thirty-eight days, then about 58° for four days, then about 85°, emerging in three days more.

FIG. 3. *V. polychloros*: pupa at 33° for thirty-six days, then about 49° for nine days, then about 58°, emerging in thirteen days more.

FIG. 4. *V. io*: pupa at 33° for twenty-two days, then about 49° for twenty days, then about 60°, emerging in eighteen days more.

# XVI. *Mr. Merrifield's Experiments in Temperature-Variation as bearing on Theories of Heredity.* By Frederick A. Dixey, M.A., M.D., F.E.S., Fellow of Wadham College, Oxford.

[Read March 14th, 1894.]

The results of Mr. Merrifield's experiments on the variations produced in butterflies by the exposure of the pupa to different conditions of temperature, are in themselves of great interest. But the interest becomes enhanced when it is recognized that many of the new features which make their appearance under these conditions are identical with those occurring normally in other species more or less closely allied to the subjects of experiment; that in not a few instances the disturbance of natural temperature-conditions appears to have caused reversion to an earlier stage in the phylogenetic history of the species; and further, that the ancestral features thus revived seem to vary with the nature of the disturbance.

Examples of these phenomena, from a previous series of experiments, were given in Trans. Ent. Soc. Lond. 1893, p. 55, and were commented on by me (*Ibid.* p. 69). The latest results obtained by Mr. Merrifield, besides confirming many of the former, furnish further instances of the same nature, as follows:—

## I. Vanessa atalanta.

A. *Warmed.*

(1) The occurrence of red scales in the dark ground-colour between the middle of the scarlet band and the large white costal spot c. This is an approach to the condition in *V. huntera* and *V. myrinna*, and more remotely to that in *Grapta* and *Argynnis*. A corresponding feature is seen in *V. io*, which in this respect is more ancestral than *V. atalanta*.

(2) The tendency towards the formation of a scattered ring of red scales round the spots $\beta$ and $\gamma$ of Series D.

This again recalls a common condition in *V. myrinna* and *V. huntera*.

(3) The appearance of a new red spot on the underside of the forewing, just below the stem of the median nervure before bifurcation. This represents a pale patch of various shades in *V. callirrhoe*, *V. myrinna*, *V. huntera*, *V. cardui*, etc.

(4) The appearance of another red spot on the underside of the forewing, just below the first median nervule. This represents a patch visible on both surfaces of *V. callirrhoe*, and fully developed in all the species nearly allied to *V. cardui*.

(5) The tendency towards resolution of the inner margin of the red band, as in *V. callirrhoe*.

(6) The suffusion of the dark ground colour with golden brown, also as in *V. callirrhoe*.

Of these, Nos. (1) (2) and (3) are points now observed for the first time; while (4) (5) and (6) are confirmations of previous results. (See a former paper by the author in Trans. Ent. Soc. Lond. 1893, p. 70.)

B. *Cooled.*

(1) Much substitution of lavender or metallic blue-green scales for black. This points to the ancestral condition seen in the females and parts of the males in many species of *Argynnis*, *e.g.*, *A. paphia*, var. *valesina*, *A. sagana* ♀, *A. niphe* ♂ and ♀, and *A. diana* ♀.

(2) The presence of minute patches of bluish scales near the margin of the dark ground-colour in the hindwing, indicating the blue centres of the almost completely merged Series III.—an ancient feature of *Vanessa* and *Grapta*.

(3) The marked increase of marginal blue, especially about the anal angle of the hindwing. This appears to represent the condition seen in *Argynnis niphe*, and ultimately to point back to the primitive Argynnid colouring of *A. valesina* and *A. diana* ♀.

These are all confirmations of former results.

## II. Vanessa io.

In this species, warming tends to revive, in the forewing, a series of dark spots (II), occurring normally in

*Araschnia levana.* Cooling tends in the first place to separate certain constituents of the ocellus, and when carried to a high extent has the remarkable effect of causing an unmistakable resolution of the ocellus in the forewing, the appearance finally produced being that of the ordinary *Vanessa* character in a comparatively unmodified form. It is interesting to see how completely these cooled specimens bear out the views which I ventured to express, some years ago, on the origin and constitution of this ocellus (Trans. Ent. Soc. Lond., 1890, pp. 99, 100, pl. i., fig. 12). The ocellus of the hindwing is also affected in the same direction.

### III. Vanessa polychloros.

Cooling tends to produce several features which appear to be ancestral. The chief of these are (1) the pupilling with black of the spots of Series D in the forewing; (2) the occasional indication of Series III. in the hindwing; and (3) the tendency towards the formation of a new dark spot between II. 8 and III. 8. These points approximate towards the condition in *Grapta.*

### IV. Grapta c-album.

In both broods cooling tends to induce or increase a darkness of ground-colour; this being undoubtedly an ancestral character.*

In all cases of this kind the obvious question occurs—are we to consider these phenomena as true instances of reversion, or is it merely that like causes have produced *de novo* a like effect in descendant and ancestor? The latter explanation may account for some of the facts, but, I think, not for all. It may perhaps give the reason for a general diffusion of bluish scales, or for a change of the ground-colour from black to brown, but it is scarcely adequate to explain the special formation of a definite pattern, as of Series III. with its blue centres in *V. atalanta,* or the reduction of the ocellus in *V. io* to the primitive *Vanessa* condition. Without raising the vexed question of sexual selection, we may yet affirm that

* The observations on the three last species are new; those on *V. atalanta,* as has been seen, are partly new and partly old. On the whole subject of the ancestral markings in *Argynnis* and *Vanessa,* see the author's paper in Trans. Ent. Soc. Lond., 1890.

among the features induced or revived by altered temperature-conditions, there is at least a residuum which must have owed its first origin to causes other than the direct action of temperature on the organism. Nor, again, are these to be considered as cases of "arrested development"; for the stages reproduced are stages in the phylogeny of the species, not in the ontogeny of the individual.

If, then, these revived features are really ancestral, how is their revival to be accounted for? The whole subject of reversion abounds with difficulty. An explanation commonly offered is that the characters last developed in the history of a species, or of an individual, are less stable than those that have a longer history behind them, and that have become firmly established under the operation of a long-continued process of heredity. Any disturbance—such as an exceptional condition of temperature—of the normal course of growth, may therefore be expected to act in the first place on the newer and less stable features, interfering with their usual line of development, and shaking back the species as it were to an earlier and more firmly-founded stage of its development—just as in an earthquake the freshly-built wing of a house, where the mortar was not yet dry, might fall and leave the older portions standing. Such an explanation, however, is in itself at best but partial, for it gives no real reason why the newer features should be less stable than the old; and indeed it comes to little more than restating the difficulty in another form.

The two attempts to find a more definite explanation of reversion which may be said at present to hold the field, are those which pass respectively under the names of Darwin and Weismann. If the Darwinian assumption of centripetal "gemmules" be granted, the commonest case of reversion, that namely which results from hybridization, especially between recently-established species, is capable of explanation under the hypothesis of pangenesis. But it may be questioned whether pangenesis as stated by Darwin is capable of accounting for such cases as the present, inasmuch as in them the condition of full maturity is almost reached before the introduction of the modifying disturbance. Although the ovum from which the individual has originated may under the Darwinian hypothesis have contained numerous gemmules of an

ancestral type, which though usually dormant might under certain circumstances become active in the ontogenetic process, it would yet seem a legitimate conclusion from the hypothesis, that the introduction of any cause analogous to hybridization in its action on the developing organism must belong to a far earlier stage in the ontogeny than the beginning of the pupal condition; it must belong, in fact, to the stage of fertilization of the ovum. There are, however, a few facts on record, such as the assumption of ancestral characters by an old hen (Darwin, "Animals and Plants under Domestication," 1868, vol. ii., p. 54), and the appearance of an earlier vertebrate condition in limbs of Amphibia reproduced after amputation (*Ibid.*, ii., p. 15), which seem in some respects analogous to the present instances, as being apparently cases in which a disturbance of normal conditions at a comparatively late ontogenetic stage has in some way led to reversion in the course of the individual growth. These cases are regarded by Darwin as not incompatible with pangenesis, though not fully explained by it.

If, on the other hand, we postulate with Weismann the existence of "ids" and "determinants," endowed with the nature and properties that he supposes, the instances that we are considering become more explicable. For according to this theory every feature in the structure of the individual organism is the result of a "struggle of the ids" in ontogeny, the final character of each histological unit being fixed at the moment of the liberation of its proper determinants by the disintegration of the "ids." The competition between the carriers of heredity, many of which must under the theory be ancestral in character, so far from being confined to the ovum, is being waged throughout the entire ontogeny, and is renewed at every successive stage of development. This being the case, it is to be expected that any external influence, such as temperature, on coming into force at any given stage, should be able to exert an effect upon the struggle proceeding at that particular time between determinants which are just beginning to play their parts in the ontogeny, and should in consequence be able to modify *pro tanto* the resulting adult organism. It would be, moreover, natural to expect the different determinants to be affected by different temperatures, nor would it be surprising to find that temperature-conditions, which are

*ex hypothesi* diverse from those normal to the species, should favour one or other set of ancestral determinants at the expense of those more proper to the species. This would explain why the effect of heat differs from that of cold, though both lead to reversion.

There is, however, one fact which shows that the above explanation is not entirely adequate—the fact, namely, of the hereditary transmissibility of certain temperature modifications, as determined in the case of *Polyommatus phlœas* by Weismann himself ("The Germ-Plasm," 1893, p. 399). This phenomenon admits of a ready explanation under the theory of pangenesis; the point that pangenesis fails to explain is the reversionary character of the original change, unless, indeed, we suppose a "struggle of gemmules," analogous to the "struggle of determinants," and continued, like the latter, throughout the ontogeny; in which struggle certain conditions favour the ancestral rather than the modern gemmules. But just as the theory of pangenesis seems to require some such addition as that suggested, so also, under the rival hypothesis, it seems necessary to supplement the explanation above given with another supposition already propounded by Weismann, namely, that the temperature-conditions are capable, in some cases, of actually altering the constitution of unexhausted determinants wherever they occur, even in the germ plasm of the ovum itself.

I am myself inclined to think that, granting Weismann's general theory of heredity, the more special cases of reversion are to be chiefly explained, as above, by the critical influence of the temperature-conditions on the struggle of the determinants, rather than by an intrinsic effect on the determinants themselves. The latter may account for such cases as a general lightening or darkening of the ground-colour, as in Weismann's *P. phlœas*, which strictly speaking are not really but only accidentally reversionary; it will not, however, account in my opinion for the special ancestral marks shown by Mr. Merrifield's *V. atalanta* and *V. io*.

The point is capable of verification. If it be true that there is a selective influence which is exerted upon the actual struggle of the determinants, that influence would find a different expression in the adult according to the particular stage in the ontogeny at which the influence was applied, as it would affect those determinants only

between which at that time the struggle was being waged.* If, on the other hand, there is no such influence, but the effect is entirely a direct one and modifies the individual determinant, then all the as yet unexhausted determinants that are capable of reacting to this particular disturbance would be affected in some degree; though no doubt, as Weismann supposes, to a greater extent if they had reached the point of disintegration than otherwise.

Again, it seems to me to be of great importance to ascertain if possible which of these modifications are transmissible to descendants. If all the modifications, including those which I have supposed to be produced in the first way, can be shown to be hereditarily transmissible, this would amount to a demonstration that the second explanation is adequate; and the first may then be abandoned as unnecessary. Should only some be inheritable, the presumption would be in favour of the co-existence of both modes of action; moreover, the greater the number of non-transmissible variations that can be produced, the more will the case be strengthened against pangenesis, and in favour of the "centrifugal" theory.

I am anxious to see, if possible, the results of breeding experiments on specimens like these for yet another reason. It seems to me that by comparative experiments, with and without artificial selection, on such variations as may be transmissible, a measure might be obtained of the relative importance of selection and the mere action of external influences in the transformation of a species. I think, too, that no better group for such experiments as these of Mr. Merrifield's could be selected than the Vanessas. For, in the first place, it is only among poikilothermic animals that the *direct* effects of temperature can be fully studied; then among these the Lepidoptera are pre-eminent for the extremely delicate register of variation afforded by their wings; and, lastly,

---

* The fact that in *V. polychloros* forcing invariably tends to produce a certain effect, whether preceded by warming or cooling; while refrigeration brings about another definite effect, whether followed or not by forcing, seems so far favourable to this hypothesis. See above, p. 432.

f.15v



among the Lepidoptera the Vanessas belong to an assemblage the phylogeny of which may claim to be at any rate partially known.* It is hardly necessary to point out how much service may be rendered to researches of this kind by the careful working out of the true internal affinities of Lepidopterous groups. In proportion as their phylogeny is placed on a secure basis, we shall be able to pronounce with confidence on the real character, whether reversionary or not, of these remarkable variations; and shall accordingly be able to estimate at its proper value the evidence they bring towards the solution of the great problem of Heredity.

---

* See the author's paper already referred to, in Trans. Ent. Soc. Lond., 1890, p. 89.

Driesmans

Berlin W. 15.
Ludwigkirchstr. 3.
17. V. 03.



Hochverehrter Herr!

Noch immer habe ich Ihnen zu danken für Ihre freundliche Zusendung aus Rom vom 27. Januar d. J., in der Sie meiner kleinen Arbeit so anerkennende Worte widmeten, die mir zu grosser Ehre gereichen und mir eine herzliche Freude bereitet haben.

Heute nehme ich mir nun die Freiheit, Sie in einer Angelegenheit anzugehen, die mit dieser Arbeit und Ihrer Würdigung derselben in Beziehung steht. Ich habe inzwischen ein neues grösseres Werk vollendet unter dem Titel „Zuchtwissenschaft und Menschenveredelung", dem ich die einschlägigen Gedanken

aus Ihren Werken zu Grunde gelegt hatte, so dass es sich unmittelbar auf Ihre Aussenwegbelungstheorie (Eugenies) stützt. Nachdem ich die Zustimmung aus Ihrem Munde empfangen, dass ich Ihre Gesichtspunkte vollkommen erfasst hatte, darf ich darum wohl hoffen, auch in dieser neuen grösseren Ausarbeitung und Darlegung dieser Frage das Richtige getroffen zu haben und Ihre Würdigung zu finden.

Und diese Hoffnung sowie die freundliche Gesinnung, welche Sie mir in Ihrem Schreiben gezeigt, giebt mir den Mut zu der ehrerbietigen Anfrage, ob Sie geneigt wären, die Widmung meines neuen Werkes anzunehmen, wodurch Sie mir eine ausserordentliche Ehre erweisen würden, für die ich Ihnen unendlich dankbar wäre. Sollten Sie

gewillt sein, meine Bitte zu erfüllen, dann würde ich mir erlauben, Ihnen die Probebogen zuzusenden, sobald die Arbeit in Druck geht.

Wie gern würde ich Ihrer liebenswürdigen Aufforderung Folge leisten, Sie zu besuchen, und Ihnen bei dieser Gelegenheit das Werk persönlich zu überreichen. Allein an eine Reise nach England kann ich vorerst noch nicht denken, wie sehr mich auch meine Sehnsucht dorthin zieht, um endlich das Land durch den Augenschein kennen zu lernen und damit einen der heissesten Wünsche meines Herzens zu erfüllen.

Die Versicherung meiner ausgezeichneten Hochschätzung verbinde ich mit dem Wunsche, dass es mir noch einmal vergönnt sein möge, Sie, den grossen Gelehrten und Denker, den ich wie Keinen anderen der gegenwärtig lebenden verehre, von

A4

Angesicht zu Angesicht zu sehen,
und empfehle mich Ihnen, mit der Bitte
um einen freundlichen Bescheid,
als

Ihr aufrichtig ergebener

Heinrich Friesmann.

X Driesmans

f5


Berlin W. 15, den 24. X 1908.
Pariser Strasse 58.



Hochverehrter Herr!

Frau Gutschlich-Deutsch, eine in den litterarischen u. socialpolitischen Kreisen Deutschlands stehende u. auch in der deutschen Frauenbewegung hervorragende Persönlichkeit, hat mich durch ihren Rückhalt von dem überaus liebenswürdigen Empfang berichtet, den Sie der Dame haben zu teil werden lassen. Ich hatte die Dame gebeten, während ihres Aufenthaltes in London Ihnen mein Buch „Mensch der Urzeit" zu überreichen, u. Sie waren so gütig, sie zu diesem Zwecke zu empfangen, wofür Sie mir meinerseits meinen verbindlichsten Dank entgegennehmen wollen.

Wenn die Versuche, einen Verleger zur Einführung meines Buches in Ihrem Vaterlande auch bisher noch von keinem Erfolg begleitet waren, so haben die inzwischen damit gemachten Erfahrungen gleichwohl einen hochbedeutsamen kulturellen u. kulturpolitischen Wert für mich. Nachdem mein Buch in deutscher Sprache in dritter Auflage 30.-40. Tausend erschienen ist, auch für deutsche Verhältnisse ein überaus seltener u. selten erreichter Erfolg, handelt es sich für mich weniger darum, auch in englischer Sprache weiterhin publiziert zu werden, der ich wohl annehmen darf, dass mein Buch von der englischen Fachwissenschaft auch im Original gewürdigt werden wird; als vielmehr,

dann, die publicistischen Erfolge für populäre wissenschaftliche Litteratur in England kennen zu lernen. Wie ich durch Herrn Prof. Ernst Haeckel den gegenwärtigen bedeutendsten Vertreter des Darwinismus in Deutschland höre, hat ihm die Uebersetzung der Volksausgabe seines „Welträtsel" in England nur etwa M. 200 eingetragen. Danach kann der materielle Erfolg meines so sehr viel bescheidteneren Buches für mich kaum in Betracht kommen, und die Erfahrung über die publicistischen Verhältnisse in England ist für mich unvergleichlich wertvoller, als irgend ich es für meine eigene Arbeit dadurch zusehen habe. Ich möchte darüber

in der deutschen Presse streiten u. wäre Ihnen sehr verbunden, wenn Sie in der Lage wären, mir über diese Verhältnisse einige Auskunft zu erteilen, die gewiss für die kulturpolitischen Verhältnisse unserer beiden Länder höchst interessant sind u. deren Erörterung dazu beitragen dürfte, auf die Kultur diesseits wie jenseits des Rheines erziehlich einzuwirken.

Frau Dentsch hat mir zu meiner Freude erzählt, in welch rüstigen körperlichen und geistigen Verfassung sie Sie angetroffen hat, und so darf ich denn hoffen, Sie, hochverehrter Herr, der Sie mir durch Ihre Werke so viel Belehrendes u. Förderliches gegeben haben, für das ich gewiss gewiss danken kann, auch für Ihren bevorstehenden neunzigsten Geburtstag in der deutschen Presse würdigen zu können, dem ich mit ganz besonderer Freude entgegensehe. Yours very truly and sincerely

Th. Roosevelt.

BERLIN W. 15
Pariserstr. 58.



11. IX. 08.



Hochgeehrter Herr!

Die Ueberbringerin dieses Schreibens, Frau Stadtrat R. Hentschel aus Berlin, hat es freundlichst übernommen, Ihnen während ihrer Anwesenheit in London gelegentlich des internationalen pädagogischen Kongresses mein Buch „Mensch der Zukunft" zu überreichen sowie Ihnen meine Grüsse zu übermitteln, u. ich bitte Sie höflichst, sie Ihrerseits zu diesem Zwecke empfangen zu wollen.

Ich beabsichtige, das Buch ins Englische übersetzen zu lassen, u. wäre Ihnen sehr dankbar, wenn Sie in der Lage wären, eine geeignete Persönlichkeit dazu in Vorschlag zu bringen sowie einen dortigen Verlag für das Unternehmen zu interessieren.

f10

Vielleicht wäre es eine passende Arbeit für die Dame Miss Coleridge, die mir einen Essay zu Ehren Ihres achtzigsten Geburtstags seinerzeit so vorzüglich übertragen hat, wenn Sie ihr dabei in wissenschaftlicher Beziehung ein wenig an die Hand gehen wollen.

Jedenfalls wäre ich Ihnen für ein wenig Rat u. Beihilfe in dieser Sache sehr verbunden u. empfehle mich Ihnen

in vorzüglicher Hochachtung

sehr ergeben

H. Friesmann

f11

Dressmann

F135

Separat-Abdruck aus der „Gegenwa

Vom Verfasser
überreicht.
Berlin W. 15.
1. 2. 03

# Francis

Von Heinrich

Die Reihe der großen Denker und Forscher, welche die angelsächsische Rasse im vorigen Jahrhundert gezeitigt, läßt sich in zwei Gruppen theilen, von denen ich die eine als die empirische, die andere als die esoterische bezeichnen möchte. Jene beginnt mit Charles Darwin, diese mit Thomas Carlyle. Zeigen diese Beiden noch Wesensverwandtschaft und geistige Berührungspunkte — in der „Auslese der Tüchtigsten" auf dem Wege der natürlichen Zuchtwahl dort und im „Heroencultus" hier — so gingen ihre jeweiligen Nachfolger in scharf divergirenden Linien aus einander. Für die Empiriker Thomas Huxley, John Stuart Mill, Herbert Spencer, Thomas Buckle wurden mehr und mehr die mechanische Naturgesetzlichkeit und das Milieu zum Ausschlag gebenden Moment; für die Nachfolger Carlyle's, John Ruskin, Walther Pater, und — wenn wir die Amerikaner hinzurechnen dürfen — Ralph Waldo Emerson, Henry Thoreau, trat der Mensch, traten die immanenten Kräfte, der Adel und die Würde der Menschenseele in den Vordergrund aller geschichtlichen, naturwissenschaftlichen und künstlerischen Betrachtung. Zwischen Beiden schien das verbindende Glied verloren. Hie Naturgesetzlichkeit, hie Geisteswürde. Es fand sich keine Persönlichkeit gleich den zehn aufgeführten, von denen wir jede einzelne der fünf letzten als entschiedenen Gegensatz einer der fünf ersten gegenüberstellen könnten — oder wenigstens wurde keine andere gleichermaßen lautbar in der großen Oeffentlichkeit, die den Adel der Persönlichkeit und Menschenwürde mit der naturwissenschaftlichen Forschung in höherem Sinne zu vereinigen und in Einklang zu setzen gewußt hätte. Man pries den Fortschritt der modernen Cultur auf der einen Seite und erblickte in den technischen Errungenschaften, in der gefälligen und bequemen Ausgestaltung aller Verhältnisse, in der Wohlfahrt der Meisten das Ziel und die Erfüllung der Wünsche alles menschlichen Lebens und Strebens. Und eben dieser „Fortschritt" wurde von Seiten der Anderen als der größte Rückschritt gedeutet, den das Menschengeschlecht je erlebt das durch die gesteigerten Culturbedürfnisse entwürdigt und heruntergedrückt werde. Das Persönlichkeitsgefühl und die Menschenwürde zu stärken, galt diesen als das einzig Erstrebenswerthe und konnte nach ihrem Dafürhalten nur im Kampf gegen das moderne Culturtreiben und seine verderblichen Einflüsse erreicht werden.

Und doch lebt ein englischer Gelehrter, der zugleich Denker und Forscher ist, und von dem man sagen kann, daß er den Geist Carlyle's und Darwin's in sich vereinigt trage. Zudem ein Zeitgenosse dieser Beiden, der sie überdauerte und in diesem Jahre seinen achtzigsten Geburtstag beging. Einer, der nur den herausfordernden Ton der Nachfolger von Darwin und Carlyle nicht anzuschlagen wußte oder gewillt war und darum verborgen blieb oder doch nicht solchermaßen in die weitere Oeffentlichkeit drang, wie sein Genius es verdient hätte, trotz eines John Ruskin oder John Stuart Mill! Ich meine Francis Galton, den Enkel des Erasmus Darwin und den Vetter von Charles Darwin. Ein anspruchsloser Forscher, geboren 1822 in Duddeston, Warwickshire, und in Birmingham erzogen, vollendete er seine Studien in Cambridge 1844, bereiste in den folgenden Jahren Nord- und Südafrika und legte die Ergebnisse seiner ersten Forschungen in dem Werke „Meteorographica" (1863) nieder, das den ersten Versuch darbietet, die Witterungsverhältnisse in großem Maßstabe zu cartographiren, wodurch die Existenz und Theorie der Anticyclone zum ersten Mal zur Frage gestellt wurde. In späteren Jahren veröffentlichte Galton Werke, die sich

Sinne eine Fortsetzung desjenigen seiner Vorfahren. Seine Kraft, seinen Charakter und seine Krankheiten hat es im Grunde von ihnen überkommen; zuweilen sind seine Fähigkeiten Mischung vorväterlicher Eigenschaften, aber häufiger sind sie Mosaike, Flicken, die bald der einen, bald der anderen von diesen ähneln, die bald hier, bald da herausgucken. „Die Lebensgeschichten unserer Angehörigen sind prophetisch für unsere eigene Zukunft; sie sind weit lehrreicher für uns als die Fremder, bei Weitem geeigneter, uns zu ermuthigen und zu warnen. Wenn es so etwas wie ein natürliches Geburtsrecht giebt, dann kann ich kein größeres erkennen als das Recht des Kindes, zunächst stellvertretend durch seine Wärter, und später persönlich unterrichtet zu werden über die Lebensgeschichte, medicinische und andere, seiner Vorfahren. Das Kind wird ins Leben hineingestoßen ohne irgend welches Stimmrecht in der Angelegenheit, und das geringste Entgelt, das diejenigen, die es hierher brachten, ihm gewähren können, ist dies, es mit der ganzen Lebensleitung auszustatten, die in ihrem Vermögen liegt, einschließlich der vollständigen Lebensgeschichten seiner nächsten Vorfahren" (Inquiries).

Längst ist uns der Satz Haeckel's geläufig, daß die Ontogenese nur eine Wiederholung der Phylogenese ist. Aber welcher Naturwissenschaftler hätte wohl versucht, ihn in seiner ganzen Tragweite zu erfassen, auszubeuten, zu veranschaulichen? Auch sein Urheber nicht. Denn dazu gehörte mehr als bloß ein naturwissenschaftlich gebildeter, dazu gehörte ein philosophischer Kopf im besten Sinne, nämlich Denkergeist, der der tiefsten Lebenserfassung fähig ist. Wie gesagt, der Geist eines Carlyle mit dem Forscherblick eines Darwin in Eins verbunden. Und als ein solcher zeigt sich hier Galton. Für ihn schlummert in jedem einzelnen Menschen der ganze Lebensinhalt seiner Vorfahren, in nuce, der ganze Inhalt der lebendigen Kette von ihrem plasmischen Ursprung an, als deren jüngstes Glied ein Jeder erscheint. Die ganze Erbschaft eines Menschen muß demgemäß ein größeres unterschiedliches Material einschließen, als zur Bildung seiner persönlichen Structur verbraucht worden ist. Die Existenz eines solchen unverbrauchten Bestandtheils in verborgener Form erweist sich in seinem Vermögen, vorväterliche Charaktere zu übertragen, die er in seiner Person nicht offenbarte. „Aus diesem Grunde," meint Galton, „muß die organisirte Structur jedes Individuums als die Erfüllung nur einer einzigen aus einer unbestimmbaren Zahl von Möglichkeiten angesehen werden, die einander gegenseitig ausschließen. Seine Structur ist die zusammenhängende und mehr oder weniger stetige Entwickelung eines nur mehr unvollkommenen Musters einer weitreichenden Verschiedenheit von Elementen" (Natural Inheritance). Galton ist Anhänger der Vererbungstheorie; aber die erworbenen Eigenschaften scheinen ihm nicht sowohl von den Eltern auf die Kinder, als auf die Enkel und Urenkel überzugehen. „Der Eierstock der Mutter ist ebenso alt wie die Mutter selbst," heißt es a. a. O: „Er war wohl ausgebildet in ihrem eigenen embryonischen Zustand. Die Eier, die er in ihrem erwachsenen Leben enthält, waren actuell oder potentiell gegenwärtig, bevor sie geboren war, und sie wuchsen in dem Maße, wie sie wuchs Es zeugt mehr dafür, sie als altersgenössische „Seitenlinien" (collateral with) der Mutter anzusprechen, denn als Theile von ihr. Das Gleiche gilt mit geringem Vorbehalt für die männlichen Elemente. Es ist deshalb außerordentlich schwierig zu ergründen, wie erworbene Fähigkeiten auf Kinder vererbt werden können. Leichter hingegen läßt sich demgemäß die Vererbung auf Enkelkinder erweisen."

Die Civilisation ist für Galton eine neue Lebensbedingung, die dem Menschen auferlegt wurde durch den Verlauf der Geschehnisse, genau so, wie in der Geschichte der geologischen Umwälzungen die verschiedenen Thierrassen fortgesetzt neuen Bedingungen unterworfen wurden. Allein die Entwickelung unserer Natur, gleichviel ob unter Darwin's Gesetz der natürlichen Auslese oder auf dem Wege veränderter vorväterlicher Gewohnheiten, hat nicht Schritt gehalten mit der Entwickelung unserer moralischen Cultur. „Der Mensch war gestern noch Barbar, und daher ist nicht zu erwarten, daß die natürlichen Triebe seiner Rasse bereits in Einklang gebracht sein sollten mit seinem ganz neuerlichen Fortschritt. Wir Menschen der gegenwärtigen Jahrhunderte sind gleich Thieren, die plötzlich unter neue Bedingungen des Klimas und der Ernährung versetzt worden: unsere Instincte versagen uns unter den veränderten Umständen" (Hereditary Genius). Die Erfordernisse der Centralisation, des Verkehrs und der Cultur verlangen mehr Kräfte des Gehirns und Geistes, als der Durchschnitt unserer Rasse aufweist. „Wir befinden uns in einem schreienden Mangel um einen größeren Bestand an Fähigkeit in allen Lebensverhältnissen; denn weder die Classen der Staatsmänner, Philosophen, Künstler, noch der Arbeiter vermögen der modernen Verwickeltheit ihrer verschiedenen Berufsarten Genüge zu thun. Eine ausgedehnte Civilisation wie die unserige umfaßt mehr Interessen, als die gewöhnlichen Staatsmänner oder Philosophen unserer gegenwärtigen Rasse zu berücksichtigen fähig sind, und sie erfordert intelligentere Arbeit, als unsere gewöhnlichen Arbeiter und Handwerker zu leisten vermögen. Unser Geschlecht ist überlastet und scheint zur Entartung getrieben werden zu sollen durch Anforderungen, die seine Kräfte übersteigen" (a. a. O.). Galton zeigt nun im Einzelnen, wie unter diesen gesteigerten Anforderungen gerade der bedeutende Mensch zerbricht und von der Fortpflanzung ausgeschlossen wird; er erweist dies am religiös veranlagten Menschen, am Künstler, und am geistig hochgebildeten Weibe. Kinder streng religiöser Eltern pflegen erwiesenermaßen oftmals übel auszuschlagen, eine Thatsache, die in argem Widerspruch mit dem Vererbungsgesetz zu stehen scheint. Dennoch glaubt Galton diese offenbare Anomalie aus der moralischen Natur des religiösen Menschen erklären zu können, dessen Eigenthümlichkeit er als „bewußte Unbeständigkeit" erkennt: „Er neigt zu Extremen, jetzt im höchsten Maße vom Enthusiasmus, der Verehrung und Selbsthingebung ergriffen, im nächsten Augenblick wieder der Selbstsucht und Sinnlichkeit hingegeben. Sehr fromme Leute pflegen sich als die elendsten Sünder zu kennzeichnen, und sie dürften in hohem Grade beim Wort genommen werden. Ihrer Veranlagung nach scheinen sie häufiger zum Sündigen und wiederum zu lebhafterer Reue angethan, als Leute von stoischer Beschaffenheit und ebenmäßigerem, geordneterem Charakter. Die moralische Pendelweite religiöser Leute ist größer als die Anderer, deren durchschnittliche moralische Verfassung gleich ist" (Hereditary Genius). Solche Menschen sind von Natur mit hohen moralischen Eigenschaften ausgestattet, verbunden mit Unstetigkeit des Charakters, Eigenschaften, die in keinerlei Verbindung unter einander stehen. „So werden die Kinder denn häufig den einen Theil mitbekommen ohne den anderen. Vererben sich die moralischen Gaben ohne große Unbeständigkeit, dann wird der Abkömmling nicht das Bedürfniß nach außerordentlicher Frömmigkeit empfinden; erbt er hingegen die Unbeständigkeit ohne die Moralität, dann pflegt er seinen Namen zu entehren" (a. a. O.). Nun zu den Künstlern. Sie sind offenbar nicht zu Gründern von Familien geschaffen. Denn „ein großer Künstler zu sein, erfordert eine seltene und sozusagen unnatürliche Verbindung von Eigenschaften. Ein Dichter muß außer seinem Genius die Strenge und den standhaften Ernst Derjenigen besitzen, deren Veranlagung nur geringer Versuchung zum Genießen ausgesetzt ist, und er muß zu gleicher Zeit das äußerste Entzücken an der Uebung seiner Sinne und Neigungen empfinden. Das ergiebt einen seltenen Charakter, der nur unter ganz besonders günstigen Bedingungen zu Stande kommt und daher in der Vererbung unbeständig ist. In der Regel gehen Menschen mit starken sinnlichen Trieben im Leben fehl, und diese Tendenz erweist sich deutlich an zahlreichen Individuen solcher Abkunft, welche nur den gefährlichen Theil vom Charakter eines Dichters ererbten und nicht zugleich seine anderen Eigenschaften, die diesen aufwiegen und im Zaum halten" (a. a. O.). Endlich zu den Frauen von geistiger

weniger unter diesem System, aber immerhin auch schwer genug. So wurden allein in der Diöcese Como Jahr für Jahr mehr als 1000 Menschen von der Inquisition gerichtet, und in dem einen Jahre 1416 allein 300 verbrannt. In Frankreich endeten während des 17. Jahrhunderts 3 bis 400 000 Protestanten im Gefängniß, auf der Galeere, bei Fluchtversuchen oder auf dem Schaffot, und eine gleiche Zahl wanderte aus. Und im modernen Frankreich des 19. Jahrhunderts wurden nach im Jahre 1870 in den Tuilerien gefundenen Papieren seit dem 2. Dezember 1851 nicht weniger als 26 642 Personen wegen politischer Vergehen verhaftet und 14 118 transportirt, verbannt oder in's Gefängniß geworfen — eine „Menschenauslese", die in gewissem Sinne doch der durch die religiösen Verfolgungen der früheren Zeit bewirkten entsprochen haben muß, denn jedenfalls sind es immer die intelligenteren, geweckteren, nicht die knechtischen, stumpfen Individuen, welche revolutioniren. An sittlichem Werth freilich mag diese jüngere Auslese des französischen Volkes sich mit der älteren nicht entfernt vergleichen können.

Und diesem düsteren Gemälde der neueren ethnischen Cultur Europas stellt nun Galton das lichte des atheniſchen Alterthums gegenüber. „Athen öffnete seine Arme den Einwanderern, allein nicht unterschiedslos, denn sein sociales Leben war so beschaffen, daß nur sehr befähigte Menschen irgend Lust daran gewinnen konnten; auf der anderen Seite bot es Genüsse, wie Menschen von höchster Fähigkeit und Cultur sie in keiner anderen Stadt finden konnten. So baute Athen durch ein System halb unbewußter Auslese eine prachtvolle Zucht von Menschen-Thieren auf, die im Zeitraum eines Jahrhunderts, nämlich zwischen 530 und 430 a. C., die folgenden erlauchten Menschen erzeugte, 14 an der Zahl: Themistokles (von fremder Mutter), Miltiades, Aristides, Cimon (Sohn des Miltiades), Perikles (Sohn des Xanthippus, Sieger von Mykale), Thukydides, Sokrates, Xenophon, Plato, Aeschylos, Sophokles, Euripides, Aristophanes, Phidias" (H. G.). Und das kleine Gebiet von Attika enthielt während seiner blühendsten Zeit weniger als 90 000 eingeborene Freie, 40 000 wohnhafte Fremde und eine Arbeiter- und Handwerkerbevölkerung von 400 000 Sclaven. Wenn nun nach gewöhnlicher Schätzung eine Bevölkerung sich dreimal während eines Jahrhunderts erneuert, so haben wir es mit einer Gesammtbevölkerung von 270 000 freigeborenen Menschen oder 135 000 Männern zu thun, berechnet Galton, die in jenem Jahrhundert geboren waren. Von diesen hätte etwa die Hälfte oder 67 500 das Alter von 26 Jahren, und ein Drittel oder 45 000 das von 50 Jahren erreicht. Da 14 Athener unsterblich wurden, verhält sich die Auslese wie 1 zu 4822 hinsichtlich der ersteren und wie 1 zu 3214 hinsichtlich der letzteren Berechnung.

Galton liebt es, die statistische und mathematische Methode in der Ethnologie und Anthropologie zur Anwendung zu bringen. Es könnte danach seltsam erscheinen, daß ich diesen vorzugsweise nüchternen Statistiker mit den englischen esoterischen Philosophen des 19. Jahrhunderts in Parallele gestellt habe. Sein Styl hat nichts oder wenig von dem Schwung dieser; er ist kein Mann der hohen, schönen und großen Worte, aber um so mehr einer der tiefen realen Werthe. Und wo er, wie bei der gekennzeichneten Tragödie des spanischen Volkes in seiner Schilderung plastisch und dramatisch wird, da wirkt seine schlichte Sprache nur um so ergreifender, schlagender und nachdrucksvoller, eben um ihrer schlichten Realität und Sachlichkeit willen. Darum schätze ich ihn höher selbst als die Ruskin und Emerson auf der einen Seite, denen er an esoterischer Geistesbildung nachsteht, und höher gewiß als die Mill und Spencer auf der anderen, denen er an naturwissenschaftlicher Durchbildung zum Mindesten nicht nachsteht. Er besitzt die schlagende Kraft der realen, sprechenden Darstellungsweise, die den mit hohen und schönen Worten genugsam gesättigten und überfütterten modernen Menschen allein noch zu überzeugen und dauernd in Mitleidenschaft zu ziehen vermag.

Becker in Leipzig.

" (Berlin, W. 57) 1902, Nr. 51.



# Galton.

## Driesmans.

vorzüglich mit der Vererbung und der Abwägung der verschiedenen menschlichen Fähigkeiten befaßten, und mit diesen haben wir es hier allein zu thun, indem sie Forscher- mit Denkergeist gleichermaßen verbinden und die Würde, den Adel der Menschennatur auf naturgesetzlichem Wege zu begründen unternehmen. Diese Werke sind: „Hereditary Genius, its laws and consequences" (1869); „Inquiries into human faculty and its development" (1883); „Natural Inheritance" (1889). Als Ergänzungen und Erläuterungen zu diesen drei Hauptwerken sind ferner zu zählen: „English men of science, their nature and nurture" (1874); „Finger-Prints and Hereditary Genius" (1892); sowie verschiedene Arbeiten über zusammengesetzte oder besser „verschmolzene" Porträt-Photographie, durch welche Galton, indem er z. B. alle Glieder — männliche wie weibliche — einer Familie über einander photographirte, den typischen Charakter einer solchen herausbringen wollte. Entsprechend verfuhr er bei den verschiedenen Berufsarten, Verbrecherclassen und Krankheitsfällen, wie Tuberculose und Auszehrung.

Ueberall läßt Galton es sich angelegen sein, das geistige Moment nicht nur auf das physische zu begründen, sondern es zugleich gegen die materialistische Denkweise auszuspielen, die als höchstes, einziges Lebensziel des Menschengeschlechts allein das Wohlbehagen und „Glück der Meisten" kennt. Er ist aristokratisch Empfindender und zugleich naturwissenschaftlich Gebildeter durch und durch. Seine Naturwissenschaft hat ihn zu aristokratischer Auffassung des Lebens geführt, d. h. zur Nothwendigkeit der Auswahl und fürsorgenden Umgebung der hervorragenden Typen, die einer heroischen Lebensauffassung fähig sind und in dem nivellirenden Treiben des modernen Culturlebens, in dem materialistischen Marasmus unterzugehen und auszusterben drohen. Der Geist Carlyle's und Darwin's weht uns somit gleichermaßen aus den Schriften Galton's an, und mitunter glauben wir einen Nietzsche sprechen zu hören, der die naturwissenschaftliche Schulung besitzt, die diesem abging und die dieser sich so gern noch dazu erworben hätte. Ich will nun die markantesten Stellen aus Galton's Werken durchgehen und glaube, die Leser, welche ich für diesen nicht nach Verdienst gewürdigten Denker zu interessiren hoffe, werden mir darin beipflichten, daß er einzigartig in der englischen Geisteswelt steht, und daß auch Deutschland z. Z. keinen seines Gleichen aufzuweisen hat, der gleichermaßen frei von ideologischer wie von materialistischer Einseitigkeit wäre — ein Haeckel und Nietzsche in einer Person. Aber, was das Werthvollste ist, in dem bescheidenen Style eines maßvollen Gelehrten, ohne das herausfordernde, anspruchsvolle Wortgepränge Jenes, noch die spätere Exaltation und gelegentliche ekstatische Verstiegenheit dieses großen und gewiß größeren Geistes.

„Da macht sich ein meist ganz unvernünftiges Gefühl geltend gegen die allmälige Austilgung einer minderwerthigen Rasse," heißt es in den „Inquiries". „Es beruht auf einer Vermengung der Rasse mit dem Individuum, wie wenn der Untergang der Rasse gleichbedeutend wäre mit dem einer großen Zahl von Menschen. Nichts der Art findet statt, sobald der Auslöschungsproceß still und langsam wirkt auf dem Wege früherer Verehelichung der Glieder der höheren Rasse, ihrer größeren Lebensfähigkeit unter gleichen Verhältnissen, ihrer besseren Aussichten, einen Lebensunterhalt zu gewinnen, oder ihrer Ueberlegenheit in gemischten Ehen. Daß die Glieder einer unfähigen Rasse sich nicht gerade gern verdrängen lassen, ist eine Sache für sich; aber freilich mag

es ja etwas roh geurtheilt sein, daß, wenn zwei Individuen um denselben Platz kämpfen und eines weichen muß, kein größeres Unglück für das Ganze eintreten kann, wenn der Unfähige dem Fähigen weicht als umgekehrt, indem die Welt durch den Erfolg des letzteren dauernd bereichert wird." Und von diesem Gesichtspunkt aus führt Galton einen entscheidenden Stoß gegen die Malthus'sche Theorie der Hinausschiebung der Verehelichung und der Verhinderung der Empfängniß. Die praktische Verwerthung dieser Lehre, meint er, werde immer nur beschränkt bleiben, und wenn so, würde sie zum größten Nachtheil der Rasse ausschlagen. Sie würde nämlich nur von den „Klugen" — soll wohl heißen höher Gebildeten — und „Selbstverneinenden", d. i. Uneigennützigen, befolgt werden; hingegen vernachlässigt von den Impulsiven und Selbstsüchtigen. „Gerade diejenigen, deren Rasse wir besonders brauchen, würden wenige Abkömmlinge hinterlassen, während die Anderen, die wir los sein wollen, den Platz mit ihrer Nachkommenschaft ausfüllten, womit der Uebervölkerungszustand nach wie vor bestehen bliebe." Damit wäre nur eine zeitweilige Erleichterung für einige Generationen geschaffen, keine dauernde Vermehrung der allgemeinen Wohlfahrt, während die Rasse der Nation verschlechtert würde. Die praktische Verwerthung der Malthus'schen Lehre müßte daher indirect zu den schlechtesten Ergebnissen führen, die übersehen wurden, weil man es verabsäumte, ihre Tragweite für die Rasse in Betracht zu ziehen. „Wenn diese Lehre alle Bevölkerungsclassen gleichmäßig beeinflußte," heißt es in „Hereditary Genius", „wäre hier nichts darüber zu sagen; allein, da sie als Verhaltungsregel dem klügeren Theil des Menschengeschlechts vorgeschrieben wird, während es dem anderen anheimgegeben bleibt, sie unbeachtet zu lassen, zögere ich nicht, sie in ihrer Folge für die Rasse eine äußerst verderbliche Regel zu nennen." Das nothwendige Ergebniß würde sein, daß die Rasse der „Klugen" (Vornehmen) nach einigen Jahrhunderten zu einer verschwindenden Zahl gegenüber den Anderen zusammenschrumpfte und völligen Ruin über das Volk brächte, wo die Regel herrschte. Galton protestirt daher mit aller Entschiedenheit dagegen, daß die fähigeren Elemente ermuthigt werden, sich auf solche Weise vom Kampf um's Dasein zurückzuziehen. „Es mag ja ungeheuerlich erscheinen," meint er, „daß der Schwache vom Starken verdrängt werden soll; allein noch weit ungeheuerlicher ist es, daß diejenigen, welche am besten ausgestattet sind, um ihre Rolle auf der Lebensbühne zu spielen, verdrängt werden sollten von den unfähigen, krankhaften und verzweifelten."

Hiermit habe ich Galton's aristokratische Grund-Anschauung dargelegt. „Jede That," ruft er emphatisch aus (Natural Inheritance), „die zuerst einen Guinea-Stempel dem vollen Guinea-Werth des natürlichen Adels verleiht, müßte eine große sociale Lawine in Bewegung setzen." Und weiter (Hereditary Genius): „Die Natur strotzt von verborgenem Leben, das zu erwecken dem Menschen weitreichende Kräfte verliehen sind unter den Formen und in dem Maße, wie er es wünscht." Zur Erweckung solch verborgener, vereinzelter, in die Minderheit gedrängter und mit der Gefahr des Aussterbens bedrohter edlen Lebenskeime beizutragen; die Rassenhaftigkeit, zunächst seines Volkes, zu ergründen und die Gefahr vor Augen zu stellen, der die modernen Völker durch die planlose, gedankenlose Vermischung mit jeglichem Menschenmaterial entgegengehen, daran hat Galton seine Lebenskraft gesetzt. Er veranstaltete zu diesem Zweck Körpermessungen in ausgedehntem Maßstab, der Statur, der einzelnen Körperhältnisse, der Gliedmaßen; er legte eine Statistik der Haar- und Augenfarbe an, kurz jeglichen kennzeichnenden Merkmals, und suchte die Stabilität jedes Einzelnen bei der Kreuzung zu bestimmen. Er erließ ferner ein genealogisches Preisausschreiben, um zu Familienaufzeichnungen und Abstammungsurkunden anzuregen, oder solche an's Licht zu fördern, zu erhalten und als die kostbarsten Schätze zu verwerthen, die es für einen Menschen geben kann. Denn, so führt er aus: das Leben des Individuums ist in wahrem

Begabung. „Die Tanten, Schwestern und Töchter hervorragender Männer pflegen sich im Durchschnitt nicht so häufig zu verehelichen wie andere Frauen, da sie an eine höhere Cultur und einen geistigen und moralischen Ton in ihrem Familienkreis gewöhnt sind, den sie nicht so leicht anderswo finden, zumal wenn bei ihren gewöhnlich bescheidenen Mitteln ihre Beziehungen auf die Personen ihrer nächsten Umgebung beschränkt bleiben. Auch wird ein Theil von ihnen sicherlich von dogmatischem und selbstherrlichem Charakter und deßhalb nicht anziehend für die Männer sein, und Andere wiederum werden diesen ihren weiblichen Beruf verfehlen durch scheues, seltsames Wesen, das sich oft bei jungen Mädchen von Geist zeigt und ihren Aussichten auf Verehelichung hinderlich ist" (a. a. O.).

So sehen wir unter den gegenwärtigen Verhältnissen die höhere Menschenform, welche einen seltenen und schwer haltbaren Complex von Qualitäten erfordert, die einander in der Regel ausschließen, und dessen erbliche Uebertragbarkeit höchst fraglich erscheint, überall vor der gröberen und gewöhnlicheren das Feld räumen. Dieser bieten sich allenthalben die besten Fortpflanzungsmöglichkeiten, während Jene wie dauernd auf den Aussterbe-Etat gesetzt ist. Um so zwingender nur muß die Pflicht werden, dem höheren Menschentypus auf jede Weise zu Hülfe zu kommen. Galton weist mit allem Nachdruck darauf hin. „Den seltenen Typus, der hervorragende Eigenschaften gegensätzlicher Natur in sich vereinigt, typisch zu machen, d. h. dergestalt in sich zu festigen, daß er zu einer dauernden und sich forterbenden Specialität und schließlich zu einer eigenen Rasse sich auswächst — das ist das Problem. „Die gelindeste Form dessen, was ich Rassen-Veredelungslehre zu nennen wage" — erklärt Galton — „würde darin bestehen, nach Ankündigungen höherer Menschenbildung auszuspähen und diese überall solchermaßen zu begünstigen, daß ihre Nachkommenschaft die der übrigen Rasse überwächst und diese allmälig durch ihre überlegene Zahl verdrängt. Solche höheren Bildungen sind durchaus keine seltene Erscheinung. Es ist leicht, Familien ausfindig zu machen, die sich durch charakteristische Ebenbilder auszeichnen, deren Züge und Charaktere gewöhnlich diejenigen ihrer Frauen und Männer in deren gemeinsamem Abkömmling überwiegen und die zu gleicher Zeit ebenso fruchtbar sind wie der Durchschnitt ihrer Classe" (Inquiries). Durch ein System solcher entsprechender, ergiebiger Eheschließungen würde das menschliche Geschlecht unendlich gefördert werden können; neue Formen und Varietäten würden auf diese Weise durch einen Wechsel in den Lebensbedingungen erstehen. „Das Menschengeschlecht hat somit eine ausgedehnte Controle über die Formen seiner künftigen Lebensgestaltung, weit mehr als dem einzelnen Individuum über seine eigene eingeräumt ist, da die Freiheit des Individuums engbegrenzt ist in der Ausübung seines Willens. Alles Lebendige ist einfach in seinem Wesen, aber mannigfaltig, ewig wechselnd und wechselwirkend in seinen Erscheinungen, und Menschen wie alle Lebewesen sind Kräfte und Glieder eines unendlich ausgedehnteren Systems kosmischer Wirkung, als irgend ein Einzelner zu fassen vermag. Wir Menschen können daher, mehr oder weniger bewußt, zur Erscheinung eines weit höheren Lebens als unseres eigenen beitragen, gleichwie die einzelne Zelle im Organismus des höheren Thieres zur Erscheinung seiner höheren Lebensform beiträgt" (Hereditary Genius).

Wahrhaft dramatisch ist die Schilderung, welche Galton vom Schicksal des spanischen Volkes entrollt. Der Abschnitt liest sich wie eine erschütternde Tragödie und steht in der Kunst der Darstellung dem Besten nicht nach, was der Meister auf diesem Gebiete — Gobineau — an tragischer Wirkungskraft geleistet hat. Die nüchternen Zahlen wirken hier für sich allein schon wie Schlag auf Schlag mit dramatischer Kraft in einem zu Ende und zu Tode gehenden Menschenschicksal. Es ist der fünfte Act im Leben eines hochbegabten Culturvolkes, dessen erste Scenen Galton hier darbietet und dessen folgende wir mit erlebt haben unter den Stichworten „Cuba" und „Philippinen", und weiter erleben

in den unaufhörlichen revolutionären und religiösen Wirren dieses unglücklichen Volkes. Das Mittelalter mit seinen Klostermauern und dem Cölibat, das gerade vorzugsweise die feinsten, vornehmsten und edelsten Naturen umfing und von der Fortpflanzung ausschloß, hat nach Galton die Herunterzüchtung der modernen Menschheit in die vielberufene allgemeine Decadenz in die Wege geleitet. Am schwersten ist davon betroffen worden und hat darunter gelitten Spanien, die Hochburg der katholischen Kirche. „Wo immer ein Mann oder Weib eine feinere Veranlagung zeigte, die zur Nächstenliebe, zur Betrachtung, zur Literatur oder der Kunst neigte, gab es der socialen Zeitlage nach keine andere Zuflucht für sie als den Schooß der Kirche. Diese aber forderte Ehelosigkeit. Die Folge war, daß derartige Naturen sich nicht fortpflanzen konnten. So entwürdigte die Kirche durch diese unkluge und selbstmörderische Politik die Zucht unserer Vorväter, gleich wie wenn sie gerade darauf abgezielt hätte, den rohesten Theil der Menschengemeinschaft zum Erzeuger der künftigen Generationen zu bestimmen. Sie handelte Züchtern gleich, die wilde, dumpfe und stumpfe Creaturen hervorbringen wollen. Kein Wunder, daß der Parteigeist auf Jahrhunderte hinaus Europa in Banden hielt; das größere Wunder ist vielmehr, daß genug Gutes in den Adern der Europäer zurückblieb, um sie fähig zu machen, ihre Rasse zu dem gegenwärtigen recht bescheidenen Grad natürlicher Sittlichkeit heranzubilden" (H. G.). Die religiöse Politik wirkte in Europa aber noch in einer anderen Richtung mit kaum geringerer Grausamkeit gegen die Natur der künftigen Geschlechter, durch die Verfolgungen, welche Tausende der hervorragendsten Denker und Männer von politischer Befähigung auf's Schaffot oder während eines großen Theils ihres Mannesalters in den Kerker brachten oder sie aus dem Lande trieben. Ueberall betraf dies Schicksal gerade die Furchtlosesten, Wahrheitsuchenden, Intelligentesten, die die geeignetsten Erzeuger einer hohen Civilisation gewesen wären, und behinderte sie an der Fortpflanzung, während dessen die knechtischen, gleichgültigen und stumpfen Naturen in dieser Hinsicht des freiesten und um so freieren Spielraums genossen. „Es genügt, um das Blut kochen zu machen" — ruft Galton aus — „dieses blinden Wahns zu gedenken, der die ersten Völker der ringenden Menschheit zu Erben einer solchen hassenswürdigen Ahnherrenschaft machte, die unsere Instincte in einen unnötig lange hingehaltenen Antagonismus zu den wesentlichen Erfordernissen einer stetig fortschreitenden Civilisation brachte. In Folge dieser angezüchteten Unvollkommenheit unserer Naturen und in Hinblick auf die Bedingungen, unter denen wir zu leben haben, werden wir noch jetzt fast ebenso vom Gefühl der moralischen Unfähigkeit und Sünde heimgesucht, wie die ersten Bekehrten der Barbarenzeit, und wir flüchten uns in halb unbewußte Selbsttäuschung und Heuchelei wie in ein gewisse Zuflucht vor seiner Beharrlichkeit. Unsere Glaubensbekenntnisse bleiben in Widerspruch mit unserer wirklichen Lebensführung, und wir führen ein doppeltes Leben von unaufrichtigem religiösen Sentimentalismus, verbunden mit groben materialistischen Gewohnheiten" (H. G.).

Die Ausdehnung, welche die religiöse Verfolgung in Europa gewonnen hat, läßt sich an nachstehenden statistischen Angaben ermessen. Es wurde die spanische Bevölkerung während der drei Jahrhunderte von 1471 bis 1781 jährlich von 100 000 Ketzern gereinigt, von denen 100 durchschnittlich hingerichtet und 900 eingekerkert wurden. Im Laufe dieser drei Jahrhunderte wurden im Ganzen 32 000 Personen verbrannt, 17 000 in effigie, von denen wohl die Meisten im Gefängniß starben oder aus Spanien entflohen, und 291 000 zu verschiedenen Zeiträumen von Einkerkerung oder anderen Strafen verurtheilt. „Es ist unmöglich, daß eine Nation eine solche Politik nicht mit einer schweren Einbuße bezahlt haben sollte, mit der Verschlechterung ihrer Zucht" — sagt Galton — „wie dies offenkundig hervortritt in der Gestalt des abergläubischen, unfähigen spanischen Volkes unserer Tage" (G. H.). Die übrigen romanischen Völker litten zwar

Druck von Hesse &

f.1r

HOURS OF CONSULTATION
9 TO 11 A.M.
6 TO 9 P.M.
SUNDAYS 8 TO 9 P.M.

90, KELLETT ROAD,
Brixton S.W.
AND AT 85, BRIXTON HILL,
S.W.



May. 22. 04.

Dear Sir

I listened with great interest (& sympathy) to your comprehensive paper on 'Eugenics.' The limits of the discussion did not permit of any questions being asked, but I should like to know if there is any likelihood of a committee for organised research on the subject being formed. I hope shortly to commence a research on Development — the occupation of a medical practitioner, in my judgment, giving excellent

opportunities for such observations.
I have not yet joined the Society as I am waiting to see whether its work is to be of a practical nature or mainly theoretical.

Should the committee of which I have spoken be formed, will you kindly let me know!

Yours faithfully
Walter Dunsten.
(M.B. (Lond)).

---

P.S. I have applied to the B. Medical Assocn for a grant & if successful shall start immediately.