

Correspondence V

Publication/Creation

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CARTE POSTALE

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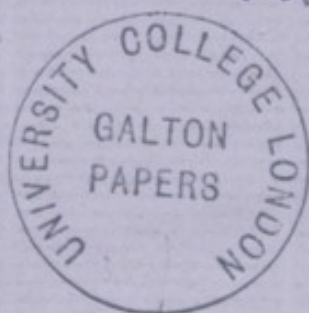
Monsieur Francis Galton
42 Rutland Gate
London.

1681
6

Dear Sir,
I do not know whether I shall be able to spend
over 2 or 3 days in London, but I may ~~perhaps~~
make a rapid visit to the Hygienic Meeting, just
the time to read a short paper on "Space, ~~and~~
~~Space~~ and Growth" and based on experiments
made by me since 3 years on Mollusks. But if
I could not come, I would send you my paper
in time. Am I obliged to put myself down as a
subscriber? I merely want to come an hour or
two, on my trip to London, Oxford, Cambridge
and Edinburgh. I am, Dear Sir, yours very truly
Henry de Varigny

Vareigny

Oct. 18/97



Dear Sir,

Many thanks for your kind letter, and
 pray be assured of my most sincere sym-
 -pathy in the painful circumstances through
 which you have been going.

I am sorry to say that nothing has been
 done in France in the line which I advo-
 -cated in my book on Experimental Social-
 -tion. Biologists are scarce here, and have
 no money.... I do not think the thing
 should be taken up by the State - as I
 have a natural and deep-rooted antipathy
 for all interference of the State with matters
 which can be done without its aid, and
 all that would be required would be some
 public-spirited and moneyed man who
 would feel inclined to give a farm fully
 equipped, with enough money to support
 it well. This farm should be entrusted to
 a Committee, or to some Scientific

Society, and after the Board had decided upon the nature of the experiments to be performed, they could be made under the supervision of a scientific man with the aid of a few trustworthy and well trained subalterns. Have you no such liberal minded man in England? I suppose he might be obtained in the United States: but they are far off, and, of course, the thing would be done ~~there~~ and not in Europe.

The Board should comprise the leading biologists: each one should make his suggestion, and the whole body, after discussion would adopt or reject each proposal. Quite a number of experiments may be performed simultaneously, especially if the site selected were on the sea-shore, where a small laboratory equipped with some simple apparatus would allow a large number of experiments to be conducted on viviparous marine animals. Provision should also be made for experiments on microbes

14

as they offer the great advantage of
yielding a large number of generations
in a short time.

The cooperation of breeders would be most
useful, as one might ask of them to
make some experiments which would
not interfere with their business.

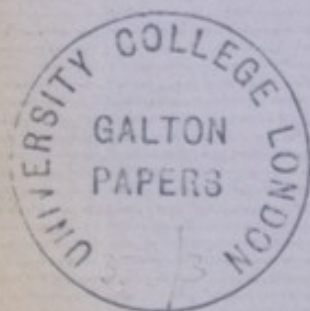
I shall be very happy to hear of what you
may have done in the Evolution Com.

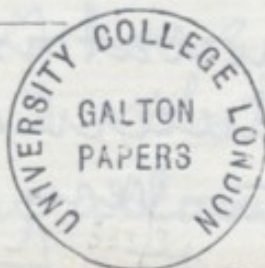
-mittee, and if I can help you in any
manner, by getting information, or by
making your intentions known to the
public in France, I shall do so most
willingly. The matter is a serious one
and many very important results, practical
and scientific, may be expected, so that
every thing should be done in order to
put the scheme on foot.

I am, Dear Sir, yours very faithfully

Henry de Varigny

7 Rue de Saxe
Paris



LABORATOIRE
de PATHOLOGIE COMPARÉE7 Rue de Saxe
ParisMarch 20th 91.

Dear Sir,

I have received, read and reread with the greatest interest your Memoir on patterns in thumb & finger marks. I am reviewing it at present for the Revue Scientifique and need some complementary information on the following points -

- 1st When two thumbs or fingers of the same person offer the same design, it is of course always reversed, that is symmetrical in both hands? ^{of both hands}
- 2^{dly} Do the two same fingers in same person more generally offer the same design? In the ratio 10, 30, 50 & 80%?
- 3^{dly} Are the patterns of the same hand liable to be very different, for instance 1 pattern of I, 1 of IV, 1 of IX? or

f6

is it more common to observe variation, or rather a species of the same genus (c^1, c^2, c^3, c^4) or k^1 or k^3 , or n^1, n^3 , etc.) In case of extreme difference could you ~~conceivably~~ explain it to me by mere reference to fig. 9. But I rather suppose differences are never extreme -

4th Have you any data concerning heredity which you choose to speak of since you wrote your memoir?

5th Have you any patterns of toes with those of fingers of same person: is there any relationship?

6th Have you any differences in patterns of cultured ^{persons} and uncultured laborers & hand-workers?

7th Have you any thing on racial differences (Amen, negroes, etc)?

8th Have you any thing on the monkey?

Then questions may be easily answered in a few words, I hope, and this is the reason why I have ventured to disturb you. I wish to make use of them - if you allow it - in my review of your memoir. I have just finished it, and will add the facts you will communicate in foot notes.

The subject is a very curious one. Could you suggest any special point on which investigation would seem necessary to you, and might be carried on here? I would be happy to study some little corner of the question -

You wrote to me a year or so ago that you were "too old" to work! That was exactly the time at which you were after this memoir! I am happy to see that you contradict

f8

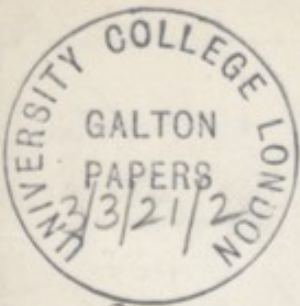
yourself so ably, and thoroughly. It
affords much pleasure to your
friends and admirers, and is
useful to the advancement of
Science, one of the very few things
that are of any interest or use
to our of dust. I hope your health is
good, and that new communica-
tions may be shortly expected on the
same topic.

I am, Dear Sir, your very
faithfully

Henry De Varigny

7 Rue de Stase

Paris



f.1
May 1. 87
Petersfield House,
Cambridge.

My dear Mr Galton

I am engaged on a new edition of my Logic of Chance, & naturally want to work through again what you have written upon the subject of 'Law of Error' &c during the last few years. I have a copy of your (1877) Royal Institution paper: What you could then only test on sweet peas, you have I think tested since by the result of ~~your~~^{the} answers to your prize questions. Can you give me a reference to any other of your papers; as I don't want to miss any of them?

What you say in your Royal Instit:

F. 2

paper seems to me very important;
& I must take rather more shame
than some others for not having
seen the necessity for such condi-
tions, because I have rather in-
sisted upon the effect where they
are absent. Might we not illu-
strate what comes in that case
(i.e. where 'reversion' is absent)
by a consideration of the following
result. Suppose a large number
of persons with equal fortunes to
begin with, incessantly & indiscrimi-
nately betting for small & equal
sums, say shillings. If we were to
look at their fortunes at the end
of a certain time we should find
these to be differentiated according to
our familiar curve. If they had had
time to gamble 100 times we should
find most of the fortunes near the
old mean, thinning down each
way till we found one with $+\pounds 5$ &

one with - £5 on his old fortune. ^{f.3}
Examine them again a little later
& the 'dispersion' will be greater; &
so on until some have been
ruined. This represents the pure
results of theory, supposing many small
& independent influences; & having
seen & enforced this, I ought to have
asked why it does not take place
where natural chances are concerned:
- the question you have asked
& answered.

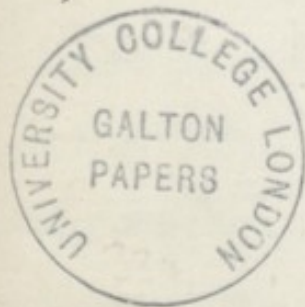
But then this seems to raise a
further difficulty. In the case of the
gamblers we see that the 'curve' being
a straight line to start with, it does
up more & more as the modulus al-
ters: i.e. the equal fortunes are
more & more differentiated. Now
your theory seems to involve perfect
stability without any alteration, &
then the question arises how the
differentiation we actually observe
was brought about? I suppose you
would say (what one always must

F. 4

allow where questions of fixity or
variability of species are concerned)
that so far as any possibility of
observation goes the species do re-
main absolutely unchangeable.
That is, your remarks in no re-
spect preclude an inappreciable
small tendency towards further di-
version. If the present variations
have resulted from equality or in-
distinguishable similarity to start
with, then it is probable that in the
course of ages our giants will really
be bigger & our dwarfs smaller, un-
less new controlling influences are
brought into play.

I was sorry to miss you here
in the winter: but, as you know, I
was severely ill: I am getting all
right now.

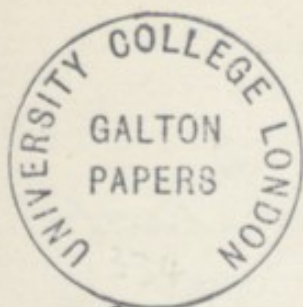
Yours very truly
J. Verreaux



May 4. 87

f5

Petersfield House,
Cambridge.



My dear Galton

Many thanks for your kindness in sending me all these pamphlets: - I have somewhat of a collection of short papers on such subjects & am going to have them bound up together. They will be given to the University some time: & as I need not say to you, there is a great convenience to any future reader in having them in one volume.

Thanks also for your kind enquiries about myself. I am getting ~~all~~ right again now, though I still have to take precautions.

As regards what I said about "gambling", I don't think there is the slightest difference of opinion between us. All that I meant was that such an example represents the outcome of the pure chance theory, & illustrates that gradual further dispersion which as you very well show (& were I think the first to show) does not prevail in regard to the vital phenomena, or natural species: Or rather its influence is counteracted by Reversion & there.

I was interested in the paper about Extinction of Family names which I had not before seen. Like yourself I had often thought about the subject (having been dabbling

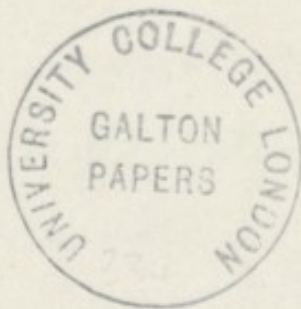
17
in genealogies during the last 2
or 3 years, & I had poked at
it a little in an empirical way,
but Watson's paper was quite
new to me.

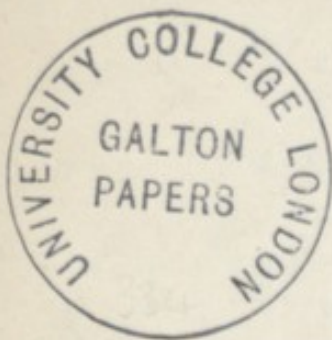
I hope if you come here again
staying at Trinity Lodge you will
let me know so that I may have
the pleasure of calling on you there.

Believe me

Yours very truly

J. Venn





f8
Sep. 23. 87

Petersfield House,
Cambridge.

My dear Galton

Many thanks for yours
just received. It will be a
very great pleasure to see you
here, & go over some of the
results of those Cambridge
measurements with you. I
worked (after my last letter)
through the breathing statistics,
& find the same marked su-
periority here again over your
S. Kensington results (ie. an
average of 253 against 219)

I think it will be best now
to wait until the clerk at the

Philosophic Library has indexed
the 1440 cards. I think I am
right in concluding that you
wished him also to undertake
such work as making separate
lists for each college: I can
then send these to the various
tutors & get the men sorted
into 3 classes as regard "ho-
nours". After this is done
the real interest of the Cam-
bridge results will commence
in the comparison between
these different results.

(Please don't trouble to reply
to the following questions until

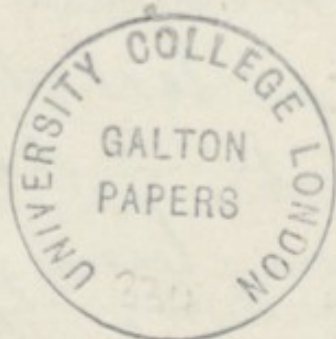
F. 11

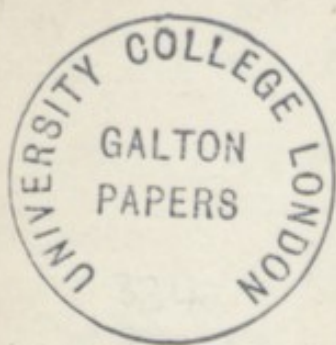
I am sorry to hear that
you got a fever in Italy.
When I did so, to a slight
extent, some 10 years ago,
I found English air a com-
plete restorative. And, though
subsequent returns of it were
fully prognosticated by my
friends, I had but one & that
for a few hours

Believe me

Yours very truly

J. Venn





F.12
Nov. 7. 87
Petersfield House,
Cambridge.

My dear Galton

We have got an alphabetical M.S. list of our "measures" here, & a rough list of the men of each college. These last I am getting the tutors to sort out for me into 3 classes; viz. { High honours
 Low
 poor or pluck

This last job will, I hope,

be got through in a few days now. I am afraid I should not have time to go through the original cards, with the view of selecting the measurements of the men ~~apportion~~ belonging to each of these classes. But if you would authorize the clerk to do this mechanical part of the work, I will undertake all the consequent calculation & comparison of these separate classes with

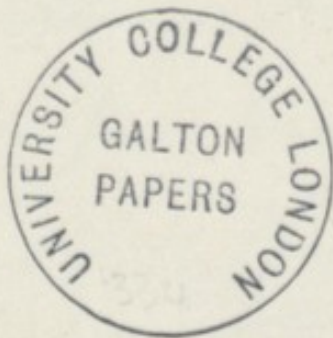
F14

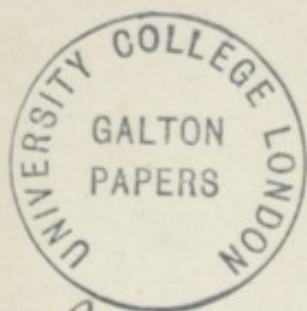
each other & with the
general average. I worked
through the general set of papers
in respect of 4 characters,
viz. Height; pull; squeeze; breathe

Are you in hopes of
being able to get down here
this term?

Yours very truly

J. Venn





Jan. 5. 88 ^{45r}
Cambridge

My dear Galton

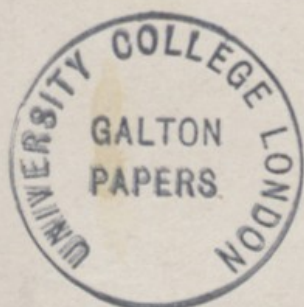
Many thanks for the handsome volume you have sent me. When you spoke of 'Crelle's Tables', I, — with my slender experience of practical computation, — imagined a small pamphlet dealing with numbers up to 100. I had no idea you were going to send such a valuable book. I set to work at once to test its use & shall find it of great value when I come to work out our Anthropometrical calculations.

I was amused at your
reference to those 'random' figures
in the Times. I take it as
a pleasing sign that my mind
is not altogether unscientific, that
it should work harmoniously
with yours, at a distance, in
this case at least. The mo-
ment my eye fell on that
column the day before yesterday
the same thought arose:—
which is as it should be,
whatever free-will may imply.

Believe me
yours very truly
J. Veru

POST CARD

THE ADDRESS ONLY TO BE WRITTEN ON THIS



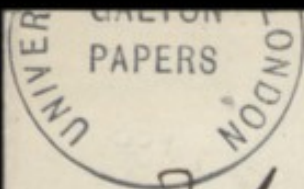
F. Galton Esq.

42 Rutland Gate

London S.W.

Cambridge
Feb. 6. 28

I have received your
cheque, & handed it
to Mr White. He tells
me that the men are
now coming in more
quickly than ever: -
in fact he has some
600 or 700 more now.
I go to Hounslow
at the end of the
week, & shall begin
to work at the papers
printing them



Feb. 16. 88

417
Fern Mount
Suffolk Road
Bournemouth

My dear Galton

I set to work on my arrival here & have worked out the results in a form which you will be able to interpret & make use of. But of course a good deal more will have to be done before a report upon them can be brought drawn up. It has involved a good deal of work. There are, as you see, 1051 men, each of whom undergoes 11 measurements. As every column was added up twice, for accuracy, it involves some 25,000 elements. I thought it better to calculate the mean in each case by the full arithmetic process, as the numbers

F18

in each subdivision were mostly too small for it to be safe to substitute the "median".

Without the help of 'Crelle', which you so kindly sent me, the work would have been vastly more laborious; but I have used it in all the multiplications and divisions.

By the way I am not sure of the nature of the head-measures I presume that they represent, in a way, the three elements of a solid measure, & that therefore the product of the three is a fair criterion of the size of the head.

I will wait for your comments before drawing up anything in the way of a Report. But some facts strike the attention

at once: -

F19

- (1) There is practically no physical difference between the reading & non-reading men; -

except, -

- d. The eye-sight deteriorates slightly with study
- β. The head is slightly larger
- γ. The "Pull" seems a trifle less strong

- (2) In all 3 classes alike the eye-sight seems to deteriorate slowly after 19 (or perhaps long before) But the apparent improvement in each case at 24 is either a chance or a puzzle

The strength seems in each case to decline after 23 or 24

Yours very truly

J. Venn

POST CARD

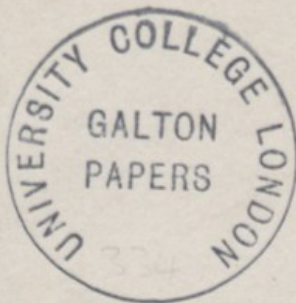
THE ADDRESS ONLY TO BE WRITTEN ON THIS SIDE.



F. Galton Esq.

*42 Rutland Gate
London*

W



Fern Mount
Bournemouth

I see there was a small error in the summary I sent you. The eye-sight should be

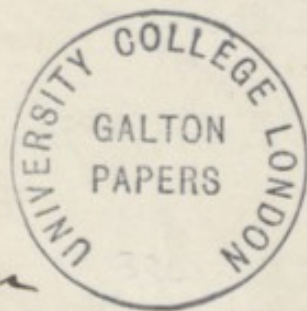
A	-----	22.7	
B	---	<u>24.4</u>	x
C	---	23.7	

This makes B (insignificantly) greater than C. I am going to verify this summary. So far as I see at present the only significant distinctions seem to be between A and C eyesight, & between some of the A and C head measurements.

Yours V.

F21
Fern Mount
Suffolk Road
Bournemouth

Feb. 26. 88.



My dear Galton

Many thanks for
your kind comments: —
if any criticisms occur to you
in reference to my new edition
I shall be most thankful for
them.

I have sent you off a
sort of 'Report' upon those Cam-
bridge measurements. But you
must regard it as offered for
your criticism & suggestion.

So pray send it back to me if there seem any serious errors or ~~errors~~ omissions.

If any minor points occur to you you might perhaps like to add them in the shape of a comment. Pray communicate it to the Anthropological or any other Journal you think best. If it is printed I should like to have a fair no. of copies as of course I must send one to each of the tutors (25 or so) who helped me with information.

If the attendance keeps up there will probably be some 1500

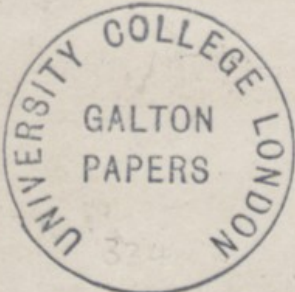
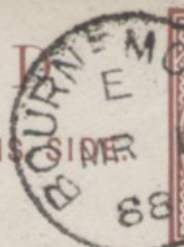
more measures for discussion in
a couple of years. This may serve
to clear up some doubtful points

Yours very truly
J. Venn

P.S. By the way I am not sure
whether (in comparing the Cam-
bridge & Kensington measurements)
I have called attention to the fact
that I have reckoned mean squeeze
(R. and L.) & that you reckoned
extreme squeeze (R. or L.) This
makes an important distinction
when comparing the absolute
strength.

POST CARD

THE ADDRESS ONLY TO BE WRITTEN ON THIS SIDE



*F. Galton Esq.
42 Rutland Gate
London S.W.*

1/3/88

Fern Mount
Suffolk Rd.

Bourne Mount

Would it be possible to test the average discrepancy between the 'Pull' & 'Squeeze' results at Kemington & Cambridge? If your former instruments are in existence & physically uninjured they might be sent to Cambridge & tried in 40 or 50 cases upon the same men as those who use our present ones. If it was in London one would like to have your former superintendent to see that his instruments were used according to his method for fair comparison. But perhaps when you are next in Cambridge you would see Mr. White & ascertain exactly what measure of difference can be allowed for difference of usage. (I hope you took no injury from your cold journey to Cambridge)

Yrs truly J. Venn

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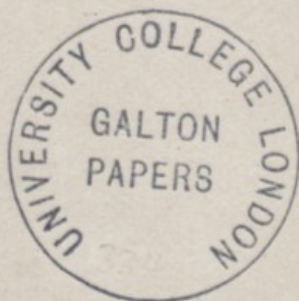
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F. Galton Esq.

42 Rutland Gate

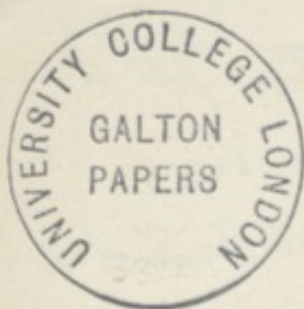
London S.W.



Camb. Ap. 30. 88

I find I wronged Mr White. What he meant was that the men tried to adopt other attitudes than the archer's in the "Pulling Apparatus". But he assures me that, after the first few cases, he always made them adopt precisely the attitude you describe. So the results may fairly be compared, supposing the instrument to be in agreement.

Yours truly
J. Venn



Petersfield House
Cambridge May 8/88

f26

My dear Galton

My only complaint of your notice in Nature is that you should have given our readers the impression that the investigation was mine, instead of my merely working closely on your suggestions.

As regards a Psychological Laboratory on any ~~very~~ small scale here, I think I could guarantee its being carefully looked after: - in fact (failing more skilled & experienced supervision) I would do so myself. But whether it would be adequately utilized & appreciated is another matter.

At present I am at work on a

127

somewhat systematic attempt to write a Treatise on Inductive Logic (something between Mill & Jevons in respect of scope & general treatment) which has occupied much of my time for many years past. As soon as this is done, - & I hope it will be published before this time next year, - I should like nothing better than such a complete change as that involved in turning to Psychologic Measurement & Calculation &c.

As regards W. E. Johnson, I know the man well. In fact he was for a short time a pupil of mine. I have a high opinion of him. He was a good Mathematician to begin with & has studied Logic & Psychology very carefully since. His notice of my

F28

Logic of Chance seemed to me
very careful & acute. But as
with most of our Moral Science
men here, few as they are, there
is much difficulty in finding any
reasonable, not to say lucrative,
employment for him.

I am not able to come
to the R.S. tomorrow, having an
engagement here; but those crowd-
ed meetings are not the best occa-
sion of getting a quiet talk, at
least not with a person like your-
self who knows everybody there.
I may be in London some day
the week after next; if so I will
write to know whether, & when, you
will be free.

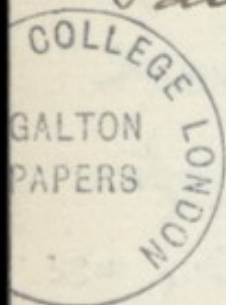
Thanks for the reference to those
papers by Dr Meyer of Göttingen.

Yours very truly
J. Venn

[Your letter was not
sent me till yesterday]

J. Venn 129
Durrant Villa
Avenue Road
Bournemouth

Jan. 10. 89



My dear Galton

I am very unfor-
tunate to have missed you
again at Cambridge, as I
should have much enjoyed
a talk on the subjects you
mention, & on others as well.
It is kind of you to think of
me in respect of your "Natural
Inheritance": I shall be de-
lighted to see it, & it can be
safely sent here, as we have
taken this house for two
months certain from now.
In respect of the "Correlations"

you speak of, if I were in Cambridge, — or rather, in Manchester, for we have moved out there now, & given up our house on Parker's Piece, — I would look up a little volume which was sent me a few years ago by the author. It is the only discussion on the subject (at least one application of the subject) which I can remember to have seen. The author ~~was a~~ French officer, much interested in the applications of the Theory of Probabilities. The particular problem which he mainly discusses is what I understand to be this 'correlation', in respect

F31

uniforms as required for the vast numbers of men in modern armies. If all men were "similar", as mathematicians say, you would only need, say, 10 sizes, the numbers in each size being determined by the Law of Error. But of course each particular length of body gives rise to the same Law of Error in respect of the width of chest; & therefore the problem before the military clothiers becomes more complicated, when they have to decide the allotment of a million uniforms. Is this analogous to your problem? Unfortunately I can't recall the name of the author, & you know how impossible it is to describe a foreign book (espe-

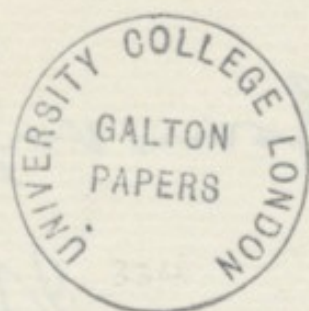
cially a little one in pamph-
let form) so that a servant
can identify it. But I do
not suppose that the author
could teach you anything on
that subject. If I remember
right he introduces the con-
ception of a solid for the
Law of Error, the usual plane
curve not sufficing for the
additional element of variabi-
lity.

If I come to London next
month I will try to see you
for a talk; I may want to
come for a day or two. We
are very fairly flourishing; dis-
like, rather than dread, of the
cold & fog having brought us
here.

Yours very truly,
J. Veru

P.S. It haunts me that my
Brenchman's name is Capt. LeGros

Feb. 18. 89



F33
1 Durrant Villa
Avenue Rd
Bournemouth

My dear Galton

I received the book yesterday,
& the pamphlet this morning.
Many thanks for them both. I
have begun reading the former
with much interest: though I had
seen the principal materials in
pamphlet form, I am glad to
set them in the enlarged shape.

As I expect to come to London
tomorrow, for a day or two, I will
defer paper-discussion in favour
of *virâ voce*: I shall certainly

Take my chance of finding
you at home.

Yours very truly

J. Venn



F36

Riversdale

Grantchester

Cambs

May 23. 89

My dear Galton

There are two little points
in your "Natural Inheritance"
that I want to ask you
about.

1. On p. 70, I think the formula $\sqrt{\frac{c^2}{c^2 + b^2}}$ must be wrong. For one thing, as it stands, the dimensions are wrong. I suppose it was meant to be printed $\frac{c^2}{\sqrt{c^2 + b^2}}$. But in the second place it ought to introduce as a factor the observed lateral distance. If this be

437

called x I make the most
probable distance from M of the
red mark $= \frac{c^2}{c^2 + b^2} \cdot x$

(I had worked it out as my-
self, & now I see that Mr Dick-
son seems to have given the
same expression on p. 224.)

2. p. 98 " As the two parents
contribute equally Therefore
the average regression from the
parental to the mid-filial
stature must be the one-half
of two-thirds-.... "

Perhaps I do not quite un-
derstand this. But it seems to
suggest that the average regression
from the single parent to the
mid-offspring will be less than
that from the mid-parent to

F38

the same. But the single parents
will of course diverge from each
other more than the mid-parents
do, since the latter are got by
averaging the former in pairs?

Yours truly

J. Venn

42 Rutland Gate May 24/89

My dear Venn

The first error is gross, and
basely perhaps, I assign it either
to a misprint on part, or in part
or wholly to a blind & careless ~~copy~~
writing out of a formula from
memory.

My excuse is that
when the formula is turned to
account in p. 127*, though the
confounded $\sqrt{\quad}$ reappears there, it has
not been used in the calculation
and though the words "of $\frac{c^2}{c^2+b^2}$ the lateral distance" do not
appear at the end of the paragraph in
p. 70, their equivalent does appear and
is used in p. 127. The paragraph

in p. 70 should have run
"... lateral distance a of a ... It is $\frac{c^2}{c^2+b^2} a$."

*

$$c^2+b^2 = (1.7)^2$$

$$\frac{c^2}{c^2+b^2} = \frac{2}{3}$$

Here the

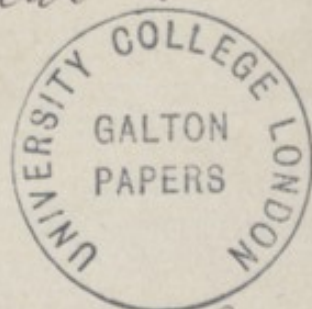
in p. 127

whence the value of b is 0.98 which
is the value given there.

Here F_m is the
equivalent of the a ,

In the second point I think
I am right - what I meant to
express was -

$$\frac{2}{3} \left\{ \frac{M+F}{2} \right\} = \frac{M}{3} + \frac{F}{3}$$

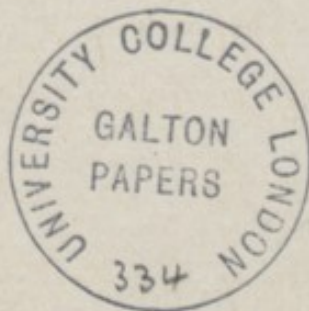


(the $\frac{M+F}{2}$ being the mid-parental deviation) It would
have been better had I put it in that
form.

I am so much obliged to you for
pointing out these blunders & faults
of expression

There is another one in p. 157, I think from
boreson. The figures there given are those
in the bottom line of ~~the~~ Table 9 B p 207 and which
compare the frequency of contrasted to
non-contrasted marriages. So far they
are correct - but on analyzing the non-contrasted
there is a tendency of art to marry art though
overborne by the tendency of non-art to refrain
from marrying non-art. Here however

I spoke with much reservation due to f41
the smallness of the figures. & which
made me clump together, instead of
analyzing, as I should have done



J. Venn

f42



Riversdale,

Aug. 4. 89

Grantchester,

Dear Cambridge.

My dear Galton

I was just thinking that I ought to write & say how glad I was that you were satisfied with my attempt to summarize your 'Natural Inheritance', when I saw the comments in Nature of the last week.

I don't think there is any occasion for me to add anything to what you have said. Of course, as you say & as I have always felt, it is most important to take a second batch of statis-

tics for comparison with the first. I should hope that, in the course of this year, the accretions will about equal the original numbers. If so I will take them in hand in the winter or spring, & see what they have to say. Our critic, though he does date from 'Trin. Coll. Camb.' does not seem to have appreciated the security which gathers about a 'mean'.

Yours truly

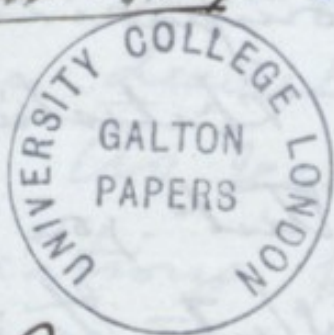
J. Venn

Safest address
Cairns College ~~Riversdale,~~

Venn

F45

Oct. 15. 89



~~Grantchester,~~

~~Dear Cambridge.~~

My dear Galton

I delayed answering
your letter as I wanted to
see Mr White (the library clerk,
& our 'measurer'); also we have
been in rather a bewilderment
about our house, which has
been giving serious indications
of unwholesomeness, culminating
just now in the illness of one of
the servants, which has caused
us to leave in a hurry somewhat.
I don't think we shall go back
to Grantchester, but our future is
very doubtful.

I will gladly undertake the reduction & comparison of the 2nd set of statistics: - do I understand that you propose to pay Mr White the same moderate charge for clerical work, as last time? I will readily undertake all the computation. He has already begun the first stage, viz. that of making the separate lists of men for each college. The second stage consists in my taking these lists to the tutors, & getting them ~~men~~ to label the men as A, B, & C, respectively. Then Mr White begins again, & copies out all the details (from the original cards) referring to these

P47

3 classes respectively. Then I
can operate upon them in any
way desired.

As regards Mr White's quali-
fications for the actual mea-
suring, H. Darwin would be a
much & better judge than I.
His work in respect of copying, &c,
seems to me clear & careful;
but as he has not much spare
time, he does not work quickly.

I doubt if it would be easy
here to get much copying work
done quickly. If you want this
done it would probably be best
to have all the cards sent up
to you in London & get it done
there. If you decide upon this
please ask H. Darwin to see
them sent: I shall only be here
from time to time during the

winter. Of course the sending
the papers away from Cam-
bridge would put a stop to Mr
White's copying them, & prevent
me from making as early a
start as I otherwise could.

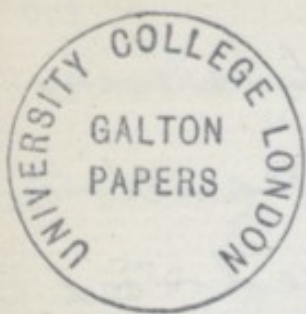
We expect to go to Novem-
ber, probably in a week
or 10 days

Believe me

Yours very truly

W. R. R.





penn

Cains Coll.

F49

Oct. 20. 89

My dear Galton

I see that "F.M.T." has got a backer up in this week's Nature. I am writing to Glaisher to ask if he can put a name upon the anonymous. I glanced at the list of names in the Calendar, but P's & I's are too common for ready identification. If you make a reply you might perhaps add that I have got a second batch of results in hand, equal in number to the first, so that even

if the same individual errors are prevalent here, the result on the averages will be reduced.

Also, that I shall take care to examine carefully the alleged inaccuracies of measurement upon the same men, by reference to the ~~individual~~ original cards.

As regards this new source of error (i.e. that of the imperfect measurer) is it not a case of the superposition of 'independent errors' & to be estimated in the same manner as regards its influence on the accuracy of our results?

I mean that if c_1 is the 'prob. er.' of the men's own dimensions, & c_2 the 'prob. error' of Mr White in measuring them, we ought to

enlarge our standard of accuracy in the ratio $\sqrt{c_1^2 + c_2^2} : c_1$?

Of course any scheme of allowing for errors is a makeshift for removing them, but I only suggest this for your consideration.

I am beginning to collect my materials for the A, B, C, division of my men, from the College tutors; & as soon as that is done, Mr White will begin to sort out the cards & copy the items as before.

Believe me

Yours very truly

J. Venn

Nov. 15. 89



1525
Apthorp
Owls Road
Boscombe

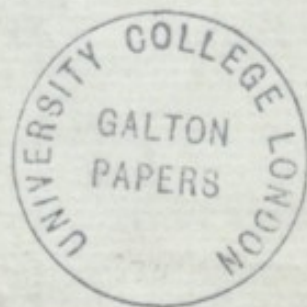
["Bourne months" seems
dispensed with now]

My dear Galton

I enclose Mr White's
notes. As you will see, the
charge is a good deal more
than last time, but he offers
an explanation; & it does not
seem to me unreasonable, re-
gard had to the intricacy of the
work. The total number of
cards seems greater this time,
which is a statistical gain.
Whatever doubts may have been
thrown upon Mr White's mea-
surements, his clerical work is
quite trustworthy, I think.

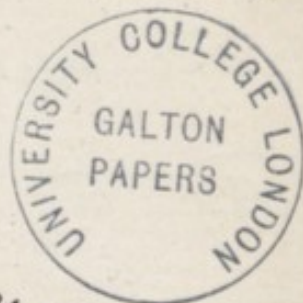
I am going to Cambridge
for 2 or 3 days in a fortnight,
I shall then bring away these
copy books & set to work on
the results. I judge, from
what you said, that it will be
best for me to bring back with
me the whole box of cards in
order to test the error of mea-
surement of one & the same
man. Any suggestions you
can offer will be gladly ac-
cepted.

Yours truly
J. Venn



Nov. 18. 89

Apthorp
Owls Road
Boscombe



My dear Galton

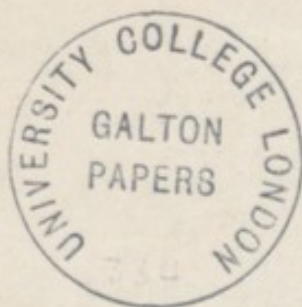
I have received
your cheque & forwarded it
to Mr White. I go to
Cambridge Saturday week,
& am sorry that I shall
thus miss you. Perhaps
I may manage to meet
you in London, about the
beginning of the following
week, as I return here.

Shall you look in at
the Phil. Library when
you are at Cambridge, &
try to see the work of
measuring actually perform-
ed?

Yours truly

J. Venin

Nov. 27. 89



F56
Apthorp
Owls Road
Boscombe

My dear Galton

Thanks for your letter.
I wish I had been with you
in your inspection of the instru-
ments. I will duly investigate,
as far as I can, the ^{instrumental} error, & will forward ^{the cards} them to you
for examination afterwards. The
little I saw of Mr White's work
gave me the impression of a care-
ful man.

As regards the "Squeeze" results
of the last set. I see, by my
tabular results, that very few

(57)

in proportion of the men reached
112; I doubt if the mean would
have been altered by more than
a fraction of an ounce by the
extension upwards of the gradua-
tion. The Eye-sight was

different: I had already noted
that the 'Median' rather than
the 'Mean' was the test there.

There are two other points
I wanted to speak to you about.
Of course you know Wallace,
the Naturalist. Have you any
objection to asking him if I
may call on him? He lives,
I find, at Parkstone, a suburb at

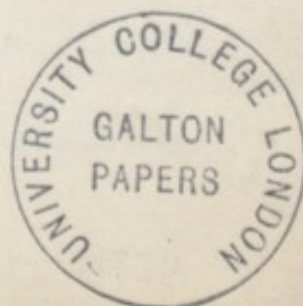
the opposite end of Bournemouth.
 I understand that he is in weak
 health & does not go out much,
 if at all; but if he would allow
 one to call I should very greatly
 value the privilege.

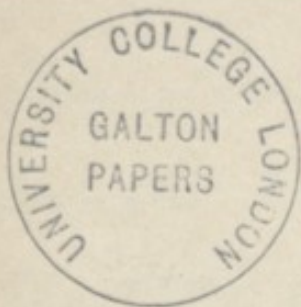
The other point is this. I am
 anxious to put up^{F.S.} Edgeworth's name
 for the Roy. Soc. I have long
 had a very high opinion of his
 work; & since his presidency at
 the Brit. Assⁿ this year, he must
 be known to a wider circle. Very
 likely you have seen some of his
 many contributions to the theory of
 Statistics, & the Laws of Errors, &c;
 if not, I can send you a list
 of them. Should you have any
 scruple in adding your recom-
 mendation to his proposal?

We are getting on here very
well, as regard health & weather.

Believe me
Yours very truly

J. K. M.





Apthorp.
Boscombe Spa.
Bournemouth.

Dec. 15. 89

f60

My dear Galton

I have worked out the new set of results for comparison separately, & combination jointly, with the old ones; & send you the tabulated statistics.

I am going to Cambridge tomorrow for the inside of the week, so if you like to keep the papers during that time, & return them to me with any suggestions, I can work at them again on my return. When

I go there I am intending to collect the cards of all the men who were measured five times or upwards (I told Mr White to select them for me, & have them ready on my visit there). This will enable us to form some idea of the amount of "error" to be assigned to this cause of inaccuracy.

The papers I send will explain themselves. No.(I) is our first set of statistics, of two years ago. No.(II) is the new set. No.(III) is the combined result of the

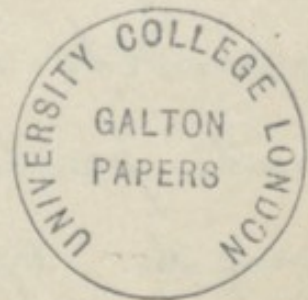
two, (with of course due 'weighting'
for the relative numbers of each),
No (V) is the diagram of the
"Head-products" of the A, B, & C,
men ~~of the~~ separately, & of all
three together.

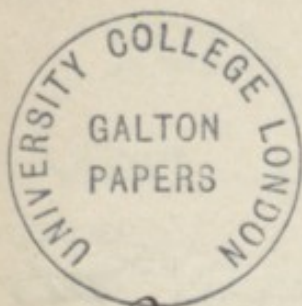
The general conclusions
seem in entire accordance with
our former results. That there
is head-growth after 19 & 20
is surely indisputable, — for
look at the Last diagram, —
but I doubt the evidence of
a different degree or rate of
growth as between A, B, or C.
But perhaps your more experi-
enced eye may detect such things
things better than mine.

When I return I will draw up a sort of 'Report' on the results. Would it not be well to send it to Nature (if they will take it), as the former results were criticised there?

Yours very truly
W. W.

No (IV) of the enclosed papers is to show the effects of age merely, irrespective of mental distinctions. A, B, & C, are here all thrown into one group.





Apthorp,
Boscombe Spa,
Bournemouth.

5. 1. 90

My dear Galton

I have finished my paper on the Camb. measurements, & am going to try to get the editor of Nature to insert them. If he won't have them I will send them to you for the Anthrop. Journal. I rather prefer the former, as being the more popular, since anything we can do to stir up a little more general interest in these schemes seems desirable.

As regards the inferences to

be drawn from the fresh batch, it seems to me that every previous conclusion ^{gets} ~~gets~~ more or less further justification: there is no opposite indication.

When I wrote to you, some weeks ago, about Edgeworth, you said you would kindly look over the form of application, & make suggestions. I enclose it in the form in which it first suggests itself to me.

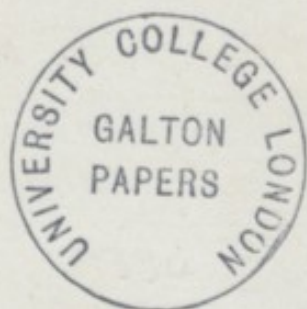
Believe me
Yours very truly
J. Vernon

P.S. as the 'Application'
 'Recommendation' may need
 alteration, I send only a
 rough copy: - with you
 read & make suggestions?

~~[Then follows list of a
 number of Scientific
 papers]~~

The introduction of the fresh
 "prob. error", (i.e. that springing
 from the imperfection of the head-
 measuring instrument) has, I
 find, very little effect. The "prob.
 error" of the observed Head-
 volumes is about 17 cubic
 inches; that arising from the
instrument is about 5 inches.
 But $\sqrt{17^2 + 5^2}$ only equals 17.7

Jan. 10. 90



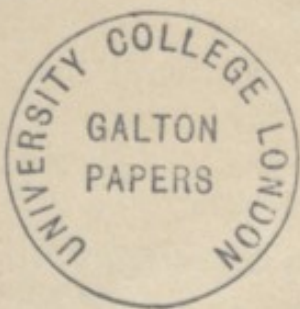
F67
Apthorp
Boscombe

My dear Galton

Will you kindly sign
the enclosed, & return it to
me?

I sent my paper to Nature
yesterday. The editor
does not seem to see his way
to more than 3 of his pages being
occupied, but I can get in most
of what I have to say into that
scope. Of course I forwarded
your appendix.

Yours truly
W. R. R.



Athorp.
Boscombe Spa.
Bournemouth.

Feb. 23. 90

My dear Galton

I have been thinking about what you were saying on the subject of some Scientific-popular Introduction to the Theory of Statistics. I think, with you, that there is a good opening for such a treatise. In some respects I should much like to attempt something of the kind, as I want to prepare (for this or next year) some lectures in that way: — The subject "Theory of

"Statistics" is now formally introduced into the Schedule of the Moral Sciences Tripos, in the department of Scientific Logic. But it would require a good deal of search in the volumes of the Statistical Society, Philosophical Magazine, &c, &c; to say nothing of special treatises on Probability. If it were desirable to go into the mathematical foundation of the various Rules no one would do it better than Edgeworth.

170

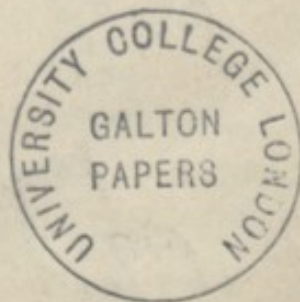
I am sorry to hear that you have been troubled with Bronchial weakness. We have been by no means free from the Influenza down here: it did not come in a sudden gust, as in London & many other places, but has been dropping in upon people from time to time during the last 6 weeks. We have just sent home one of our servants, whose mother lives close by, & who seems to have got it, though not badly: so we must wait for some days yet to know whether we have got through this last sphere of infectious influence.

(71)

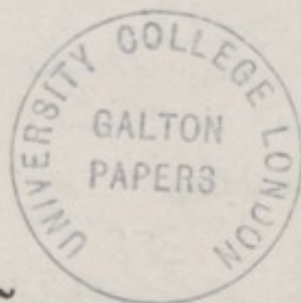
When I next have a few
hours in London I will try
to see you.

Believe me
Yours very truly

J. Venn



after reading
a note by Henry
return to F. Galton
J. Venn
Summerleigh
Chine Crescent Rd
Bournemouth
13.6.91



My dear Galton

I think I could, without difficulty, compress into a 15-minute paper some remarks about our Camb. Anthropological results which would be of interest. The only doubt is about my own presence to read them, as it is possible that I might find it very inconvenient to be in London on Aug. 14. - If that it happens that I were absent I suppose the paper could

be read by some one else.
I would send you a sketch
of the paper some time before: the
tables of figures could, I presume,
be appended for subsequent pub-
lication, & taken for read at
the time.

À propos of your recent
paper at the R.S., I was
reminded of some experiences of
my brother last year. His
vicarage at Sittingbourne was
cleverly 'burgled' one night. On
looking at the windows next
morning they saw the finger
marks of the professional perfor-
mers on the glass outside, as
they had tried to press up the

f74

windows. It was too late when I heard this, or I should have been inclined to say at once, Have the frames cut out & sent up to Scotland Yard for the chance of any suspected man being subsequently identified.

The police were certain, from the signs of operation, that the men had belonged to some high branch of their profession; - but it seems that even burglars do not always keep abreast of current science. If they have read your paper they will wear gloves next time.

What do you think of my seal?

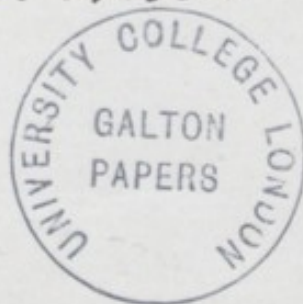
Yours truly
J. W. W.

J. Venn

f75

Hope Bank
Colwyn Bay
N. Wales

Aug. 21. 91



My dear Galton

I duly got your letter: it was very good of you to take the trouble to write amidst all the tearing hurry of your closing meeting.

My only feeling is that I got a good deal more than my deserts in such a full summary in the Times. I had all the week an uneasy ^{satisfaction} feeling such as the skink feels, - or at least should feel, - at not being in London. But it

~~It~~ would have absorbed three days from my stay in Wales, which was a large expenditure for a short paper.

I hope if you pick up any experiences or hints, about intercomparison of results & processes such as ours, you will let me have them. Within the next 12 months the Cambridge Statistics ought to bear cropping again, & one would like to have some references for comparison if possible.

I am gradually putting together my ideas for my lectures on "Theory of Statistics" next

177

term. But there are difficulties. Our Cambridge Scheme, admirable as it is for those who study along the main routes, does certainly not give much encouragement to those who want to turn down what is considered a by-lane, however important the places to which it leads. So the existence of an audience or class becomes somewhat doubtful. But the benefit to the writer of the lectures is mainly in the fact of his having to put his thoughts in order, whatever he do with them afterwards.

Hoping you are having a pleasant holiday. Believe me
Yours very truly, W. Venn

Hastings ^{f78} Dec. 30.

My dear Galton

I shall be delighted to
sign for Edgeworth, who, I have
long felt, ought certainly to be
F.R.S.

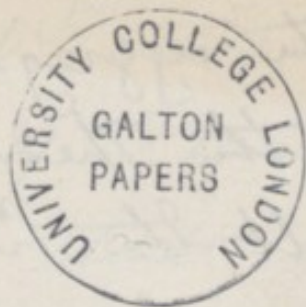
Reciprocation of all good wishes,

Yours very truly
J. Venn

Ap. 25. 96

Trinity Lodge
Dean Park
Bournemouth

179



My dear Galton

Your letter has followed me down here, where I have had to come for a week or 10 days, after a turn of Influenza. It was not much; but, as usual, it left something else which the doctors will not give the same name to, but call a consequence of it: - in this case a certain amount of bronchitis. However, I am mending fast, & expect to return to work at Cambridge in a week. I am sorry to hear what you say about Edgeworth, but I think your

remarks exactly hit the point. I have a very high opinion of the aggregate of his contributions to the Theory of Statistics, & have felt the help of his work in a number of directions; but it is difficult to point to any single essay of his which, like K. Pearson's, is decisive at once in the way of power & originality. I feel strongly that Edgeworth ought to be on the R.S., & I can't but think that if I had a vote it would go for him, but the more I think about it the more difficult I feel it to say anything quite definite which ought to decide others in his favour.

I am replying at once to

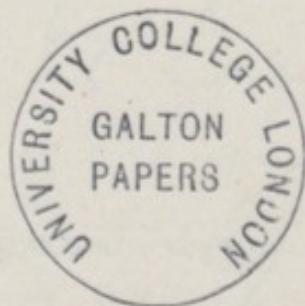
481

your letter, as it has been
delayed in coming here;
but I will think over the
matter carefully. If I can
recall anything, of that tho-
roughly definite character
which you rightly ask for,
I will write again.

With kind regards to Mrs
Galton

Believe me
Yours very sincerely

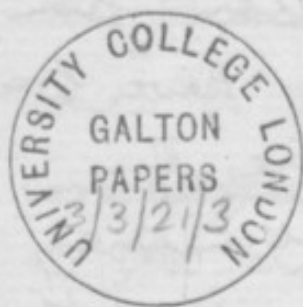
J. W. M.



H. M. Vernon

f1

Station Zoologica
Naples



Sept 19th 1894

Dear Sir

I am taking the liberty of writing to ask you about certain details of your Anthropometrical work. During the last six months I have been investigating the effect of environment upon the development of sea-urchin larvae, by growing them under various conditions, and then measuring them in fifties, and taking the means. The normal breeding season of the sea urchins is ~~the~~ in the winter months, but artificial fertilisation can be performed all the year round.

During April and May the mean size of the larvae kept more or less constant, but from that time they rapidly decreased in size, till by the middle of August they

had become 20 to 25% smaller. From this time till the present they have been increasing again rapidly in size. This enormous effect is due almost entirely to the comparative immaturity of the ova and sperm in the off-breeding season. What I wished to ask you was whether you had any ^{results} ~~data~~ ~~also~~ tending to show that the youngest offspring of families, frequently conceived at a time when the genital organs were becoming functionless through age, were on an average of smaller size than the other offspring. Possibly also the eldest offspring & slightly differ in size from the intermediate ones. I have not been able to find any discussion of these points in any of your published works, though as far as I can gather they could easily be looked from ^{some of the data} ~~the Special Data~~ which you work upon in your book on 'Natural Inheritance'.

In my measurements I determined the length of the body, and of the oral and aboral ^{of the larvae} arms. I ~~have~~ shall have measured in all

ten thousand larvae, so do you think it would be of interest to apply the test of correlation, as Prof. Weldon has done in the case of Crayfish & Carcinus homas, especially with a view to determining whether the deviations of each dependent organ are distributed about their mean with a probable error of $Q\sqrt{1-r^2}$. If this problem has already been worked out, and there is nothing to be gained by repeating it, it is scarcely worth while to undertake it, considering the labour involved. Apologising ~~again~~ for taking the liberty of writing to you

I am

Yours very sincerely

Francis Galton Esq, F.R.S.

J. L. M. Vernon

H. M. Vernon

44



Stazione Zoologica

Naples

Nov 5th '94

Dear Sir

Thank you very much for your letter of a few weeks ago, in which you answered so fully the questions I had asked you about the offspring of aged animals. I must apologise for not having written before, but I thought there might possibly be one or two other points I should like to ask you about, and I did not want to trespass on your kindness more than was necessary.

I had perhaps no right to say that the dwarf larvae obtained during the summer were accountable to the immaturity of the ova in the off breeding season. The grounds were merely, as you say, that none other suggested themselves. I have however made test experiments which prove that it is not due to change in the temperature of the

composition of the water: to any effects of nutrition or change in the amount of light. Also during these summer months the ova and sperms are undoubtedly less mature than in the winter. The ovaries and testes are shrunken, and many of them on shaking in water yield no fertilisable ova or spermatozoa at all. There is thus a considerable degree of probability that my supposition is correct.

I have for the present brought to an end the experiments I have been making on schistosome larvae, and I hope to publish a paper on the subject shortly. In reference to these dwarf breeds I have said, 'Mr. Gallon has informed me that a provisional inquiry made by him did not seem to show any difference between the children of old couples, and those of ordinary couples, or any difference between the produce of old and young thoroughbred stallions'.

If you would rather that I did not mention your conclusions, or if you would like them worded differently, please be kind enough to let me know, so that I may erase or alter the sentence.

Thank you for your suggestion of communicating the statistical examination of the measurements to Prof. Weldon, but I think that as the my

Measurements are not very well adapted to this purpose, it is better for us to examine them at not very great lengths, and introduce an account of them in the paper with the rest of the results. I find the body length measurements agree beautifully with the law of error, none of the means of the coefficients varying by more than .02 from the theoretical, but the arm length measurements are not quite so good. I am very sorry I troubled you about the problem of correlation, as I find it cannot be applied to my measurements. Change of environment (such as the temperature of development of the larvae) alters the ratio between the arm lengths, a rise of 6° causing a change of 4% . It ~~seems~~ ^{doubtful} to me therefore ^{if} that the constant r , is in all cases a constant quantity for different local races of an animal, as you and Prof Weldon are inclined to regard as probable. I don't think I can be wrong about the ratio between the arm lengths being affected by temperature, as I have classified all my measurements thus.

Number of measurements	Temp of development	Ratio of arm lengths
1050 { 50 x 21 }	$16^{\circ} - 18^{\circ} C$	99.8
3200 { 50 x 64 }	$18^{\circ} - 20^{\circ}$	101.8
1550 { 50 x 31 }	$20^{\circ} - 22^{\circ}$	103.2
2150 { 50 x 43 }	$22^{\circ} - 24^{\circ}$	104.1

the conditions of environment other than that of Temperature ~~being~~ ^{being} ~~different~~ ^{varying}. As however the larvae grow under different conditions were measured in sets of fifty, ~~to~~ I don't think this would affect the result much, for differences thereby introduced would on an average neutralise each other.

I am afraid I am troubling you unnecessarily, as I can't give here all the reasons which induce me to suppose that the constant representing the correlation between two organs in an animal is ~~constant~~ not ~~can~~ always constant in different local races of the animal.

I thought I would mention it, in case you ~~are~~ might know of stronger evidence ~~to~~ of its constancy having been obtained, than Weldon arrived at in the case of shrimps. ~~center~~ If there is such evidence, I to may of course have to modify my opinion, and find some other explanation of my results.

I hope you won't put yourself to the trouble of writing to me again, unless you wish me to alter the above mentioned sentence I have quoted from my paper, or have any other questions you would like to ask about my work. I fear I have already trespassed far too much on your valuable time

Yours sincerely

Francis Galton Esq

H. H. Vernon



Answered
Sep 21.



Stazione Zoologica

Naples, Italy

Sept 17th '97

Francis Galton Esq

Dear Sir

A few days ago

I ventured to send you a short paper entitled 'Reproductive Divergence', in which what I believe to be a new theory was enunciated. This theory supposes that in some cases it will be found that dissimilar varieties of certain species are less fertile *inter se* than similar varieties. That this condition of affairs may possibly obtain in the case of man is shown by the results ~~of~~ of Korozi, as calculated from the Buda Pesth natality tables.

Thus he found that in males & females more alike in respect of age were on an average more fertile inter se than those of dissimilar ages.

In your work on 'Natural Inheritance' you give on page 206 a table of marriage selection in respect of stature. You do not mention however whether the average number of offspring was the same for each group. According to my theory I think it very possible that couples of dissimilar heights, ~~both~~ ST and TS , will have fewer children than those of slightly dissimilar heights, ~~as~~ *i.e.* Sm , Ms , MT & Tm , and these slightly fewer than those of similar heights, *i.e.* Ss , Tt , and Mm . I am writing to ask you if you have worked out your results in respect of stature and other characteristics in this manner I have mentioned, and if so,

if you have published or intend publishing them? If you have not, I should be very interested if you could at any time find an opportunity to calculate them out in this manner.

Of course even if the result were quite a negative one, it would not in any way invalidate my theory, but if it were a positive one, it would add considerably to its interest and importance.

If you could find time to write and let me know about this point, and also if you could give me a criticism as to the validity of the theory of 'Reproductive Divergence' itself, I should be exceedingly pleased. Thus supposing you thought the theory to be a valid one, I should be greatly encouraged to continue making observations with reference to it.

Yours sincerely

H. H. Vernon

[Vernon]

fl

3 Brington Road

Oxford

Apr 16 '00



Dear Sir

I am very glad to hear that you are interested in my case of 'identical' twins. I showed the finger prints to Prof. Weldon, and he examined them briefly for me. They were not nearly so similar as I had supposed, they differing quite enough for easy identification. Still the two right hand thumbs, for instance, were much more like each other than they were to either left thumb. I hope, in a few weeks time, I may

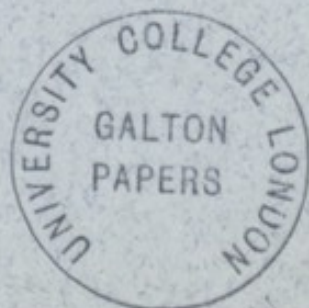
have an opportunity of getting the
twins to print their digits again,
and I will try my best to get
clear ~~terse~~ impressions. I will
then most gladly send them on to
you at your London address, for
you to examine whenever you can
find time.

Yours sincerely

H. W. Vernon

Francis Galton Esq

42 Rutland Gate



I believe that
I told you of this
but since it can go for
the necessary, I told Mr. Vernon you
were abroad.

Dear Sir,



3, Bevington Road.

Oxford.

March 11, 1900.

I have recently had an opportunity of making measurements on two so-called "identical" twins, and I thought it might be of interest to also take their finger prints. As I have had absolutely no practise at it before, I am afraid they are not very good, but still they are sufficiently so to show that there is a very close resemblance indeed between the corresponding prints. If such cases as this are of any interest to you, I wonder if you would care to see the prints? If so, I should be very glad to send them to you to look at. The measurements themselves are extraordinarily close, as the following figures show:

Subject	S.M.	Stature,	173.0cm.	Span,	179.9cm.	Middle finger,	10.98cm.
	R.M.	"	172.67cm.	"	179.88cm	"	10.99cm.

"	S.M.	Cubit,	21.33cm.	Length of skull,	18.52cm.	Width do	12.53cm.
"	R.M.	"	21.30cm	"	18.42cm	"	12.43.

Yours sincerely,

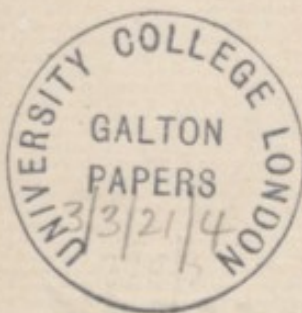
H. H. Vernon

H. M. Vernon

The prints are in the black
particor box that holds
the baby prints

Rev C. S. Vyayan

F1/2



THE RECTORY,
THE LIZARD, R.S.O.
CORNWALL.

Aug: 17. 1903.

F. Galton Esq.

Dear Sir / Thank you for
yrs of the 14th. What Mr
Perry-Loste says of the Lizard
folk is true to a great ex-
tent, tho' not quite so
much so as it used to
be. I fear I have not suf-
ficient leisure time for
the carrying out of the

f. 2

work that you suggest,
otherwise I sh^d be glad
to do my best in the
matter.

James Hfy.

C. S. Vignani