Letters Regarding Organic Stability and Regression

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QUESTIONS BEARING ON SPECIFIC STABILITY.[1]

AT the suggestion of your President, I beg to submit three questions to the notice of this Society. They bear on a theoretical problem of much importance, namely, the part played in evolution by "organic stability."

The questions are especially addressed to those who have had experience in breeding, but by no means to breeders only; nor are they addressed only to entomologists, being equally appropriate to the followers of every other branch of natural history. I should be grateful for replies relating to any species of animal or plant, whether based on personal observation or referring to such observations of others as are still scattered through the wide range of periodical literature, not having yet found a place in standard works. The questions are for information on:—

(1) Instances of such strongly marked peculiarities, whether in form, in colour, or in habit, as have occasionally appeared in a single or in a few individuals among a brood; but no record is wanted of monstrosities, or of such other characteristics as are clearly inconsistent with health and vigour.

[1] A paper read at the Entomological Society, April 3, 1895, by Francis Galton, F.R.S.

(2) Instances in which any one of the above peculiarities has appeared in the broods of different parents. In replying to this question, it will be hardly worth while to record the sudden appearance of either albinism or melanism, as both are well known to be of frequent occurrence.

Note.—The question is *not* asked now, whether such peculiarities, or "sports," may be accounted for by atavism or other hypothetical cause.

(3) Instances in which any of these peculiarly characterised individuals have transmitted their peculiarities, hereditarily, to one or more generations. Especial mention should be made, whether the peculiarity was in any case transmitted in all its original intensity, and numerical data would be particularly acceptable, that showed the frequency of its transmission (*a*) in an undiluted form, (*b*) in one that was more or less diluted, and (*c*) of its non-transmission in any perceptible degree.[1]

It is impossible to explain to a general meeting the precise way in which the desired facts would be utilised. An explanation that would be sufficiently brief for the purpose could not be rendered intelligible except to those few who are already familiar with the evidence, and the technical treatment of it by which the law of Regression is established, and with the consequences and requirements of that law. Regressiveness and stability are contrasted conditions, and neither of them can be fully understood apart from the other.

I may as well take this opportunity of appending a list of my various memoirs on these subjects. They appeared from time to time in various forms as the inquiry progressed and as suitable openings occurred for writing or speaking. The more important of these are Nos. 1, 3, part of 6, 7, and 8 in the following list. Nos. 1 to 5 refer to regression only.

LIST OF MEMOIRS, BY MR. F. GALTON, ON REGRESSION AND ORGANIC STABILITY.

(1) Typical Laws of Heredity. *Journal* of the Royal Institution, 1877. (This was the first statement of the law of Regression, as founded on a series of experiments with sweet peas.)

(2) Presidential Address, Anthropological Section of the British Association, 1885. (Here the law of Regression was confirmed by anthropological observations.)

(3) Regression towards Mediocrity in Family Stature. *Journal* of the Anthropological Institute, 1885. (A revised and illustrated reprint of No. 2.)

(4) Family Likeness in Stature. *Proc. Roy. Soc.*, 1886.

(5) Family Likeness in Eye Colour. *Proc. Roy. Soc.*, 1886.

(6) Natural Inheritance. (Macmillan and Co., 1889.) (This volume summarises the results of previous work.)

(7) Patterns in Thumb and Finger Marks . . . and the Resemblance of their Classes to Ordinary Genera. *Phil. Trans. Roy. Soc.*, 1891.

(8) Discontinuity in Evolution. *Mind*, 1894. (An article on Mr. Bateson's work.)

Entomological Society, April 3.—Prof. R. Meldola, F.R.S., President, in the chair.—Mr. C. J. Gahan exhibited two examples, male and female, of a rare Prionid beetle, *Charisis cyanea*, Serville, which had been kindly sent to him for examination by M. René Oberthür; and stated that Lacordaire was mistaken with regard to the sex of the specimen which he described in the "Genera des Coléoptères." He pointed out that the elytra of the male were relatively much shorter than those of the female; and that the joints of the antennæ from the third to the tenth were biramose. Mr. Gahan also exhibited two species of the genus *Dwarteria*, Hope, and said he believed these were the two smallest species of Longicorns known.—Dr. Sharp, F.R.S., exhibited the soldiers and workers of a species of Termites found by Dr. Haviland in South Africa. He stated that these insects possessed eyes and worked in daylight like Hymenopterous ants, and that in habits they resembled harvesting ants by cutting grass and carrying it into holes in the ground. Dr. Sharp said that although these holes were probably the entrance to the nests, Dr. Haviland was unable to find the actual nest, even by prolonged digging, so that the winged forms were still unknown. He thought this species was probably allied to *Termes viarum* of Smeathman, in which the soldiers and workers possessed eyes, and had been observed by Smeathman to issue from holes in the ground, but whose nests could not be discovered. Mr. McLachlan observed that it was possible there might be species of Termites without any winged form whatever.—Mr. Rye called attention to the action of one of the Conservators of Wimbledon Common, who, he stated, had been destroying all the aspens on the Common. He inquired whether it was possible for the Entomological Society to protest against the destruction of the trees. Mr. Gosse said he would mention the matter to the Commons' Preservation Society.—Mr. Francis Galton, F.R.S., read a paper entitled "Entomological Queries bearing on the Question of Specific Stability." (See p. 570.)—Mr. Merrifield stated that he received some years ago, from Sheffield, ova of *Selenia illustraria*, the brood from which produced, in addition to typical specimens, four of a dark bronze colour, and from these he bred a number of specimens of a similar colour.—Dr. F. A. Dixey referred to a variety of the larva of *Saturnia carpini* with pink tubercles. He said the imago bred from this larva produced larvæ of which 10 per cent. had pink tubercles. Prof. Poulton, F.R.S., said he had found larvæ of *Smerinthus ocellatus* with red spots, and that this peculiarity had been perpetuated in their descendants. Mr. McLachlan, Canon Fowler, and Prof. Meldola made some further remarks on the subject.—Mr. G. F. Hampson read a paper by Mr. C. W. Barker, entitled "Notes on Seasonal Dimorphism in certain species of Rhopalocera in Natal." Mr. Merrifield said he was of opinion that a record of the temperature at different seasons would be a very desirable addition to observations of seasonal dimorphism. Mr. Hampson said he believed that temperature had very little to do with the alteration of forms. At any rate, according to his experience, in India the wet season form succeeded the dry season form without any apparent difference in the temperature. Prof. Poulton remarked that the apparent temperature as felt must not be relied upon without observations taken by the thermometer. Dr. Dixey, Mr. Barrett, Dr. Sharp, and Prof. Meldola continued the discussion.

over

per square foot for deadwood and keels moving at 1 foot per second, and this was confirmed by the behaviour of the *Sultan*. Accepting Mr. Froude's formula, the extinctive effect due to bilge keels, such as have been added to the *Revenge*, was calculated, with the result that, in an extreme case, supposing the ship to be rolling to 20° on each side of the vertical, the extinction value due to the bilge keels would appear to be not quite 2°. This, it will be seen, is far short of the observed results obtained from the vessels themselves. Working on the their merits doubtless demand. Mr. Yarrow's paper on the aluminium torpedo boat he had built for the French Government, was a very interesting contribution. The boat appears to have been thoroughly successful, so much so that she is to be the prototype of a class. The discussion turned largely on the form of test pieces for copper alloys, it being generally conceded that there is a want of standard conditions for tests. The micro-sections of various alloys thrown on the screen were also very interesting.

The last day of the meeting (Friday) was devoted chiefly to the vibration question, the sitting proving one of the most instructive of the series. As will be seen, three papers were contributed on this important and interesting subject.

These three papers on vibration of steamers formed, with Sir William White's paper on steadiness, the two distinctive features of the meeting. It is hardly necessary to insist on the import-

the Principal, Mr. S. Cooke, offered to the [illegible] the sum of 5000 rupees towards the cost of providing residential quarters for Parsee students attending the College of Science at Poona.

THE Governors of Colfe's Foundation have selected Mr. F. W. Lucas to be Head Master of Colfe's Grammar School, Lewisham, from September next. Mr. Lucas is at present Head Master of Hipperholme Grammar School, Yorks, and was formerly Senior Assistant and Science Master at Roan School, Greenwich.

WE learn from the *Lancet*, that in accordance with the will of the late Dr. G. Y. Heath, Professor of Surgery in the University of Durham, and President of the University of Durham College of Medicine, the trustees of the Heath Scholarship, Prof. W. C. Arnison and Mr. Frederick Page, will award and pay to the writer of the best essay on Surgical Diseases of the Jaws the sum of £200. All graduates in medicine or in surgery of the University of Durham are eligible to compete for this prize. The essay must be type-written or printed, and delivered to the trustees not later than March 31, 1896. The essay, together with any specimens, drawings, casts, microscopical preparations, or other means of illustration accompanying it, will become the property of the College of Medicine, Newcastle-upon-Tyne, but by permission the essay may be printed for general circulation by the Heath Scholar. Mr. Stephen Scott, of Harrogate, has generously presented to the College of Medicine the sum of £1000, which has been devoted, in accordance with Mr. Scott's wish, to founding a scholarship to promote the study of hernia and allied subjects. Any graduate in medicine or surgery of the University of Durham, or any student of the University of Durham College of Medicine is eligible to compete for the scholarship, provided that such student shall have had at least one academical year in attendance at the College, and that in any case his age does not exceed thirty years at the time when the essay is sent in. The competition takes place every year. Essays for this year's competition must be sent not later than July 31, 1895, to Prof. Arnison, University of Durham College of Medicine, Newcastle-upon-Tyne.

ON Friday last [illegible]

M. de Forcrand. A thermal study of the compounds—$(C_2H_4O)_2(CaO)_2$, $(CH_4O)_2(BaO)_2$, and $(C_2H_6O)_2(BaO)_2$. The action of alcohols on the alkaline-earthy oxides does not give true metallic alcoholates, but addition compounds.—On the ammonium bases derived from hexamethyltriamidotriphenyl-methane and their action on the fuchsines, by M. A. Rosenstiehl.—On some new combinations of hexamethylene-amine, by M. Delépine.—On the gases of the swimming bladder of fishes, by M. Jules Richard. These consist of oxygen, nitrogen, and traces of carbon dioxide. The oxygen varies in three cases given from 78·6 to 87·7 per cent.—Action of the nervous system on the principal lymphatic canals, by MM. L. Camus and E. Gley.—On the genus *Eurya*, of the family Ternstroemiaceæ, by M. J. Vesque.—On the basic rocks occurring as narrow veins in the lherzolite of the Pyrenees, by M. A. Lacroix. There are two families of granular basic rocks, without peridote and felspar, which are allied to the peridotites.

BOOKS and SERIALS RECEIVED.

BOOKS.—Rainfall in the East Indian Archipelago, 1893 (Batavia).—Short Studies in Nature Knowledge: W. Gee (Macmillan).—Stereochromie: E. G. Monod (Paris, Gauthier-Villars).—Practical Micrography: G. E. Davis, 3rd edition (Allen).—Cambridge Natural History—Molluscs and Brachiopods: Rev. A. H. Cooke, A. E. Shipley, and F. R. C. Reed (Macmillan).—Manual of Geology: Prof. J. D. Dana, 4th edition (New York, American Book Company).—The Spirit of Cookery: Dr. J. L. W. Thudichum (Baillière).—The Evolution of Industry: Hy. Dyer (Macmillan).—Horses, Asses, Zebras, Mules and Mule-Breeding: W. B. Tegetmeier and C. L. Sutherland (Cox).—Sir Samuel Baker—A Memoir: T. D. Murray and A. S. White (Macmillan).—Our Teeth—Care and Preservation: Dr. V. Ditcham (Baillière).—Clinical Lectures on the Prevention of Consumption: Dr. W. Morrell (Baillière).—La Fonctionnement des Machines à Vapeur: G. Leloutre (Paris, Gauthier-Villars).—Des Marées: P. Haye (Paris, Gauthier-Villars).—Manchester Museum: Owens College, Catalogue of the Lilleyan: W. E. Hoyle (Manchester, Cornish).—Metallisches Lehrbuch der Elementar-Mechanik: Dr. G. Holzmüller, Erster und Zweiter Teil (Leipzig, Teubner).—The Book of the Dead—Facsimile of the Papyrus of Ani in the British Museum, 2nd edition (British Museum).—The Partition of Africa: J. S. Keltie, 2nd edition (Stanford).

SERIALS.—Scribner's Magazine, April (Low).—Meteoric Papers, No. 1: J. Calvert (London).—Natural History of Plants: Kerner and Oliver, Part 12 (Blackie).—Notes from the Leyden Museum, Vol. xvi. Nos. 3 and 4 (Leyden, Brill).—Mind, April (Williams and Norgate).—Proceedings of the Society for Psychical Research, March (Paul).—Photographic Quarterly Review, April (Piesse).—Transactions of the Natural History Society of Queensland, Vol. 1 (Brisbane).—Bulletin of the Geographical Club of Philadelphia, March (Philadelphia).—Records of the Geological Survey of India, Vol. xxviii. Part 1 (Calcutta).—Bulletin of the New York Mathematical Society, March (New York, Macmillan).—Mathematical Gazette, February (Macmillan).—Annals of Scottish Natural History, April (Edinburgh, Douglas).—Reliquary and Illustrated Archæologist, April (Bemrose).—Archiv für Pathologische Anatomie und Physiologie und für Klinische Medicin, Band 140, Heft 1 (Berlin, Reimer).—Hommel and Erde, April (Berlin, Pastell).—Proceedings of the Physical Society of London, Vol. 13 Part 3 (Taylor and Francis).

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Answers L

Organic Stability

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Nash Mills
Hemel Hempstead
10th May 1895

My dear Galton

In accordance with my promise I send you some particulars as to the variation in the number of the petals in the flowers of the common primrose.

The obsewations were carried out for me by a young freind Mr A. Godfrey James who is at present at the head of Eton College. and I think that perfect reliance may be placed upon them. The plants grew in woods about two miles to the north of Watford, Herts and about 2,000 flowers were examined, at spots some little distance from each other, on five days between April 24th and May 3rd.

It will be seen that the abnormal flowers had 4, 6, or 8 petals and that none were found with 7 petals – I have however in other years come across 7 petalled flowers

	With 5 petals	with 4	with 6	with 8
1st Day	2,000	17	1	
2d "	2,000	20	5	
3d "	2,000	18	5	
4th "	2,000	9	12	1
5th "	1889	19	4	
	9,889	83	27	1

Percentages

With 5 Petals	9,889	=	98·89 per cent
" 4 "	83	=	·83 "
" 6 "	27	=	·27
" 8 "	1	=	·01
	10,000		100 —

It will be seen that though the proportion between the 4 and 6-petalled flowers varied very considerably, yet that the proportion of abnormal to normal flowers was fairly constant 18, 25, 23, 22 and 23 — giving an average of 1·11 per cent

It was found that the irregular flowers with 4 and 6 petals generally occurred in groups, several within a small area but not necessarily on one plant, and

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that passing away from the area several hundred flowers all normal would be counted before another abnormal area was reached

One plant bore 7 flowers with 5 petals 2 with 4, and 1 with 8. I have myself noticed that where a plant bears one 4 petalled flower it frequently bears one or two others of the same character Possibly with care and patience a four petalled variety might be evolved.

You will observe that the tendency of the primrose is to produce abnormal flowers which are in defect and not in excess of the normal number of petals — In some other plants such as the cultivated clematis and the wild Paris quadrifolia I have noted an opposite tendency the varieties having in almost all cases more than the normal number of petals

Believe me

Yours very truly

John Evans

Common primrose 10,000 cases in all.

Observed by W. A Godfrey James at present at the head of the Eton College boys for Sir John Lubbock — They grows in woods about 2 miles N of Watford, Herts. About 2000 flowers were examined in each of the five days between April 24 and May 3. at Aldi some little distance apart

	Percentages
4 petals	00.83
5 "	98.89
6 "	00.27
7 "	00.00
8 "	00.01
	100.00

	Individual 5 petals	Days 4 petals	6 petals	8 petals	Total Abnormalities
1st	2000	17	1	-	18
2nd	2000	20	5	-	25
3	2000	18	5	-	23
4	2000	10	12	1	22
5	1889	19	4	-	23

The irregular flowers were chiefly found in groups, several within a small area but not necessarily in one plant

The flowers fall into 3 groups α, β, and γ, having the numbers of flower rays at their respective maxima, of 8, 13 and 21.

The individuals in each series diverge asymmetrically from its maximum, being more numerous on the side of the farther end of the series.

It appears then that the causes of the varying number of flower rays are –

(1) 3 dominant causes which are mutually exclusive.
(2) many small causes which are in some small degree correlated with the dominant ones, and cause the asymmetry.
(3) Probably many other causes which are independent of the dominant ones.

Judging from analogies (for example from Prof: Buchman's report on the experimental plots at the Agricultural College Cirencester, Journ: Brit: Assoc: 1857 p. 202, regarding agrostis vulgaris, alba, and stolonifera, also regarding Fescue, there and in 1860 p 35.), these three varieties might be "fixed" if reared on plots that severally suited them. They would then be liable from time to time to "throw" into the other forms.

F. Galton April 22/95

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7 April 1895

24, Vernon Terrace,
Brighton.

Dear Mr Galton

I send you my
notes on the eccentric
illustration which sprang up
in 1861. At the time I
thought the heat was the
cause & quite hoped to keep
I began my experiments in
1887 but pressure of [illegible]

F. Merrifield

In the winter of 1860-1 I had 7 pupæ of Selenia illustraria (tetralunaria)
How I came by them I do not know, but I have no doubt little doubt
probably from eggs sent to me the previous summer – I have always
thought by Mr James Batty of Sheffield, but have no note of this. I have
a note that two only were kept out of 7 pupæ and two females emerged
on the 14th & 15th April 1861 and on mate on the 20th on which
day I placed two, eggs from which hatched on the 15th May. It may
be assumed that the parents were of the normal Spring form or I
should certainly have been struck by any abnormality
I remember well that the larvæ were reared on potted birch trees
which stood in a long zinc-covered attic which was very hot
in summer. My next note on the subject is that on the 18th June
the larvæ began to spin up and that they were all spun up before the
14th when comes this note "22d June 2 female illustraria out –"
and appended is the following "Between the 22d and the 25th all
my Illustraria (13) came out, 8 females were of a blackish brown
with nearly all the markings obliterated. I found about 80 or 90
eggs laid on my return on the 25th [no doubt I had been away the 3 days] and about half of the number (13) were good enough to set" My next note is "July 27th of the eggs of illustraria
mentioned under date June 22 only about 1/3d hatched (though all
changed colour). I have never about 20 larvæ, some not above ¼ inch
inch long, these about an inch and quite stout"
From this, which is confirmed by my recollection, I have little
doubt that the brood (or brood) "split" – i.e. some fed up rapidly for
a third emergence in the year and some would not feed up but passed
over for a Spring emergence.
According to my recollection I got about half my larvæ
or pupæ of Dr Madden who told me that he fed them on lime or hawthorn
which failed in some way and [illegible] he gave me some of his larvæ on the
and instructed always to feed them on lime or hawthorn always. 19th June 1862
Spring of 1862 This is confirmed by the following note of the
larvæ of illustraria given me by Dr Madden & went to pupa
and one is now feeding, one of the pupæ emerged yesterday, and

one—a splendid dark variety male this morning. I have about 28 of Dr. M.'s on hawthorn; these have just begun to spin up."; "26th June—of the illustraria larvae given me by Dr. Madden to spin up about the same time and have all now come out, one of them normal colour, another rather dingy, and 2 males and 2 females quite dark (black with rich deep orange clouds). I paired off one male [no doubt dark] with the two females and saw them in cop. with each. The first cop. took place between 10.30 p.m. of Monday [23rd] and 8 a.m. of Tuesday, and ~~last~~ carefully some time after. No eggs were laid till last night when about a dozen appeared. The other cop. took place after 10.30 p.m. of yesterday; when over I stuck the ♂ male (rather a shabby specimen now)... all Dr. M.'s illustraria larvae except about 3 have now spun up." — "6th July of Dr. Madden's hawthorn-fed illustraria only one is still feeding (except 2 which I think will die); none have yet come out" — "16th July During the last week I have bred, from Dr. Madden's lot of illustraria 3 normally coloured (one of them much stunted) and 3 black specimens. One normal male coupled (I believe they had the chance & I found them in the morning in most suspicious proximity) on Sunday night [13th] with a black female, which on Monday night laid me nearly 80 eggs and last night about 18 more. On the Monday night he coupled with a normal (perhaps rather dark) female which last night laid me about 14 eggs. Yesterday I bred out a dark male, which I keep for breeding, and today another dark male (rather a small specimen) which I have stuck." My only subsequent note is ~~20 Feb~~ "1863, Feb 20 my pupae are kept in our dining room, a warm one with a fire all day — this day a S. illustraria appeared"

This is my last entry.) It appears that from an almost certainly normal pair, 13 emerged of which 3 were of the abnormal form, that ~~many~~ for several generations pairings took place which one certainly not always, perhaps not generally, confined to

the abnormal form and that normal and abnormal forms ~~a~~ resulted from these pairings. The notes quite confirm my recollection that there were no intermediate forms, because the ~~one mentioned~~ description of the one mentioned on the 26th June 1862 as "[illegible]" ~~[illegible] [illegible] [illegible] the [illegible]~~ would be satisfied by many a normal specimen. I have a very distinct recollection that a considerable number of the abnormal form appeared both with me and ~~[illegible]~~ with friends to whom I gave them. and that the proportion ~~kept~~ increasing with me as I selected such ones, ×whenever I could do so. As far as my recollection goes ~~all~~ the abnormal ones were of the small ~~summer~~ form, but perfect full sized specimens. The race came to an end I believe ~~in 1863~~ after very few generations, as usually happens ~~at~~ unless especial care is taken, especially where a double brooded species is reared indoors ~~[illegible]~~ so as to be forced into a third generation with the disturbance of habits which that involves ~~[illegible]~~. I have little doubt that there was a third generation here

The difference in appearance between the abnormal and the normal forms is most striking; & all the abnormal ones were to the best of my recollection with the modified form ‡.

It is not at all likely that the stock originally used ~~me~~ had been bred artificially so as to produce the abnormal form, for ~~this is a [illegible]~~ they would have been much prized ~~[illegible]~~ and I should have been told of it. moreover the first ~~[illegible]~~ generation bred by me were * normal. I may add that Mr* Doubleday, the great English authority on Lepidoptera, to whom I sent a specimen which may now be seen in the collection at Bethnal Green prized it highly and wrote that he had never seen anything of the kind, and, ~~for~~ notwithstanding the extent to which attention has been called to this species for the last 8 years, & the thousands I have bred under varying conditions of temperature moisture and food plant, I never heard of any others approaching them except one which I can ~~[illegible] [illegible]~~ trace to ~~one~~ a gentleman who had them from me in 1862. Probably others exist in collections of Brighton entomologists or their friends, but in that case with the same origin

‡ certainly the 2 I have & Mr Doubleday's are so

an organism has a particular configuration which has been evolved under an environment A, of which it is a function. When removed to another environment B, the original configuration is maintained for a time owing to a certain inherent ~~[illegible]~~ due to its inherent stability. That gives way and it varies owing to a variety of influences, extrinsic and intrinsic. It tends to vary & in time acquires a new stable configuration, a function of the new environment, B. It may ~~has~~ in fact become a "geographical race". The new form is adapted to the new conditions. It is easy to see how many persons jump to the conclusion that the conditions have produced the form directly.

Kew, April 19. 97

My dear Galton,

I am more than ordinarily pressed and I am afraid that, in consequence, I cannot discuss your most interesting memorandum as fully as I could wish. There are many points on which I could say a good deal. They must pass, however.

Your idea of 'stability' is a little different to mine. We find that if a plant is transferred from one environment (in which it is stable) to another, e.g. placed under artificial culture conditions, it takes a period of at least 5 or 6 years to break down the 'stability'. It then begins to vary in almost any direction. You seem to imply that stability is progressively

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broken down. To me it seems that it is destroyed once for all, as when a system in statical equilibrium is upset.

Whatever idea the breeder may have it is generally agreed that he has to start with variation of any sort. That is the primary phenomenon. Then out of multifarious variation, he rejects everything that does not proceed along the path he has marked out for himself. As he begins to attain his object he prevents his work being 'swamped' by closer and closer inbreeding. By this means he reattains 'stability'.

I am much interested at what you say about Regression. In the remarks I made at the R.S. two years ago which I wrote out for 'Nature', I fixed my mind on a 'mean form' in any one generation and it was to this that I attributed stability. My 'mean form' seems identical with your 'ideal parentage', or nearly so.

Our problem is:- Can the process by which a stable form has been obtained as the resultant of selection after variation be reversed? In some measure I believe it can. That is Regression. I have no doubt that you could go back to the point at which in the original stock, stability was first broken down.

The difficulty to my mind is going back beyond this to the original and primitive stable ~~condition~~ configuration.

This ~~point~~ difficulty arises from the fact that the necessary conditions are not present.

My point is this:— How can one recover a configuration due to [illegible] when one is still working under D.
For example in the case of my Cyclamens I see no difficulty in going back to the primitive cultivated form, but I doubt the possibility of ~~[illegible]~~ restoring that to the primitive wild form.

I hope I have made my point plain. It is clearly suggested to me by what Mr Darwin says in Origin of Species, 6th ed. p. 11 which is worth your consideration.

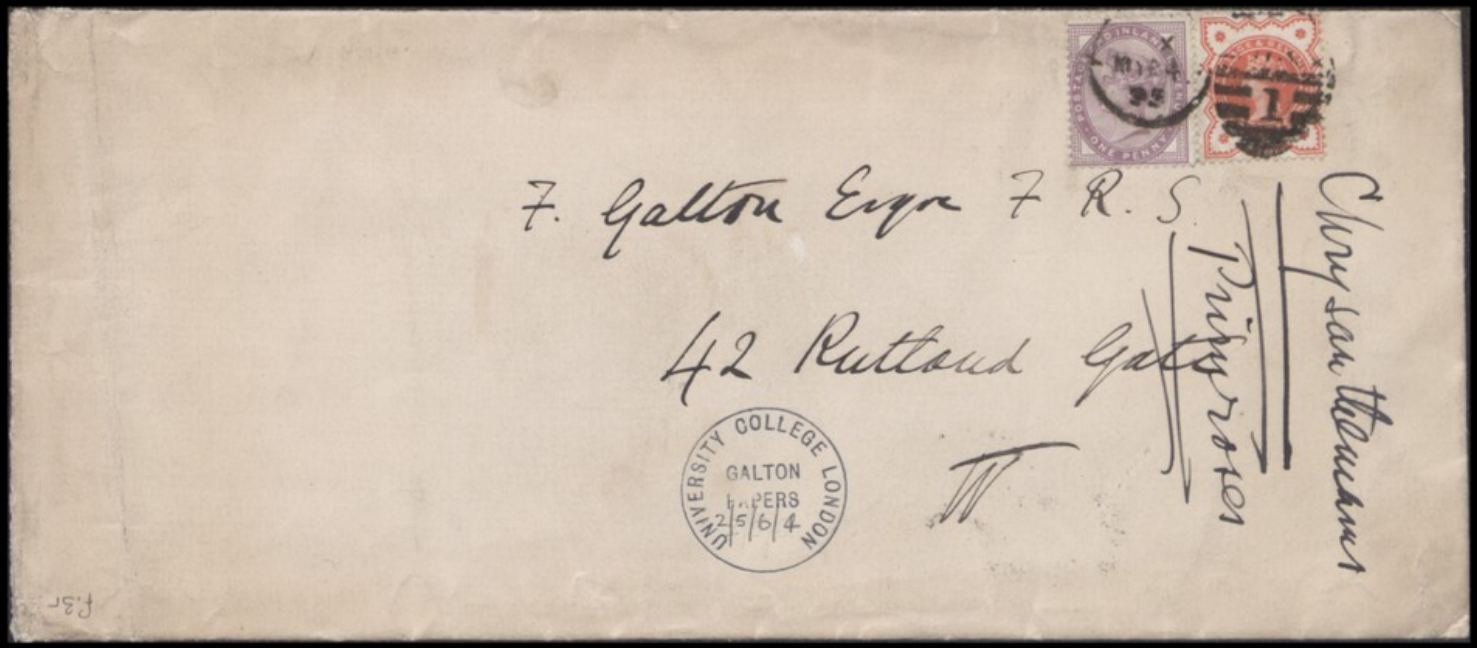
It is a pity that variation has been so little studied scientifically. I do not know anything like the facts I have put together in the case of Cyclamen.

You are right as to stages of [illegible].

I pointed out in Cyclamen that an organism first abandons its specific, i.e. adaptive characters and these are more tenaciously to its generic

Yours sincerely

W. T. Thiselton-Dyer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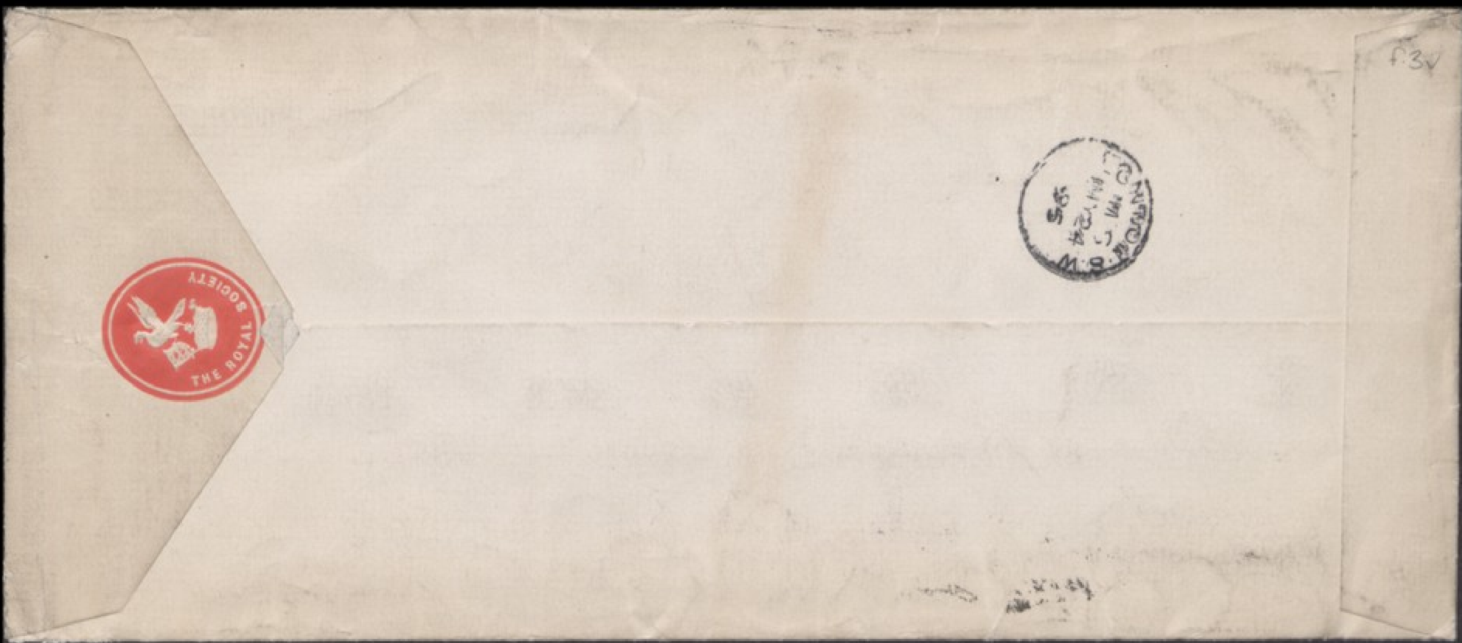
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