LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his staff is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Hereditary Stature

PERMIT me to correct one word in my memoir on "Hereditary Stature" in the last number of NATURE (p. 297, col. 1, line 6 from bottom), which should read "seven" on an average. I should be glad at the same time to amplify the passage in which it occurs, as follows—:

The chance that the stature of the son will at least rival the stature of the father, is not uniform; it varies with the height of the father. When he is of mediocre stature, that is, 5 feet 8 inches, out of every 100 sons born to a group of fathers of that height, 50 will be taller and 50 will be shorter than their fathers (the practically impossible case of absolute equality being neglected). Here then the chance of which we are speaking = 50 per cent. When the father is tall, the chance in question diminishes; when he is very tall, say 6 feet 5 inches, the chance is reduced to seven per thousand. The following table shows the probabilities in various cases. Columns A contain the height of the fathers; Columns B show how many per cent. of the sons will rival or surpass the height of their fathers:

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<thead>
<tr>
<th>Height</th>
<th>A per cent</th>
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<td>5½</td>
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<td>6½</td>
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<td>7</td>
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<td>90</td>
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<td>9</td>
<td>100</td>
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FRANCIS GALTON

Deposits of the Nile Delta

TWO communications from Sir William Dawson, published in NATURE of January 7 and 28 (pp. 221, 298), appear to call for a short notice from me. The report on the above subject which I read before the Royal Society on November 19, 1886, and of which an abstract appeared in NATURE of December 10, ought not to be referred to as "the report of the Delta Committee of the Royal Society." The origin of this report was as follows—As there was no other geological laboratory available for the examination of the samples of deposits sent home by Col. Maitland than the one connected with the Normal School of Science and Royal School of Mines, the other members of the Delta Committee requested me to undertake the microscopic and chemical examination of the specimens. In preparing my report on them I was struck by the remarkable and unexpected characters which they presented, and I ventured to suggest a mode of accounting for them. When my report was submitted to the Committee I was requested to lay it before the Society; and, it would seem quite superfluous to add, neither the Committee nor the Society thereby accepted any responsibility for the views which I expressed in the report.

As Sir William Dawson lies under a manifest disadvantage in attempting to criticise a report which he has not seen, it will not be necessary to enter at length upon the subject of his communications. If I understand the first of these, he takes the opportunity in it of withdrawing his untenable assertion that "at a depth of 30 or 40 feet the alluvial mud rests on desert sand" in favour of the totally different statement that "the modern Nile mud" lies on a "Pleistocene or Oligocene deposit." In the absence of any palaeontological evidence I can offer no opinion as to the truth of this latter view; but it is certain that the deposits above and below the limit mentioned are of precisely similar mineral characters. With respect to the second communication, I need only add that when its author has the opportunity of reading the report in question, he will find that the very obvious considerations to which he refers have been by no means lost sight of.

JOHN W. JUDD

Stone Implements and Changes of Level in the Nile Basin

I incline a letter from my brother at Wady Halfa. The scrapers sent home are all made out of flat oval pebbles of
Disregarding now the systematic character of some of the errors, and treating them as purely casual, we get as the average error the value as got by the machine and by calculation from the twenty-four hourly means of 0° 065. It may be noticed, however, that the numbers are unusually large (and at the same time not so systematically as in the case of the first order) for which the average is as much as 0° 150, the seventh of a degree.

If δ be omitted, the average for the remaining cylinders of the machine is reduced to 0° 052, but the general results may here be stated.

We see, therefore, that, with the exception perhaps of δ, the constants got by the machine for the mean of the days constituting the month are as accurate as those got by calculation, which requires considerably more time, inasmuch as the hourly lines have to be drawn on the photographs, then measured, then corrected, and the constants deduced from the means by a numerical process involving a plan approved by the Council.

No direct check, short of passing the curves a second time through the machine, can be presented by any other portion of the results except as regards the means, which have been compared with the means calculated from the hourly readings obtained by measurement from the curves. The results of this work will be published in the Monthly Readings for 1883, but the general results may here be stated.

As a rule, the monthly means yielded by the harmonic analyser agree well within a tenth of a degree with those obtained by calculation from the hourly means of the curves; and although in some exceptional cases larger differences have been found, amounting in rare instances to as much as half a degree, it is probable that these cases are due to defects in the working of the instrument the scale-values first used were less accurately determined than has since been found possible.

In both these respects the two methods were on a par in the years dealt with, and therefore the greatest care is to be had with their means.

For 1883, the average difference of the monthly means for all the seven observatories is 0° 09; for 1881 it is 0° 05; and for 1882 0° 06; and in these three years the scale-values at all the observatories were the same. The calculated monthly means occurred but once, and of 0° 2 in five times.

What has been said is sufficient to show that the instrument is completely applicable to the analysis of barograms. It has also been employed on the discussion of barograms, and the curves for the years 1871, 1872, and 1876 have been passed through the machine.

The year 1883 was marked owing to the existing facilities for comparing the resulting figures with those obtained by calculation from Mr. Eaton's means, and the result in this case was equally satisfactory with that for temperature already mentioned.

May 27.—"Family Likeness in Eye-Colour." By Francis Galton, F.R.S.

This inquiry proved that certain laws previously shown by the author to govern the hereditary transmission of nature also governed that of eye-colour: namely, that the average ancestral contributions towards the heritage of any peculiarity in a child are from each parent 1/2, from each grandparent 1/4, and so on; also that each child of any person will on the average possess 1/4 of that person's peculiarity. The eye-colours were grouped into light, hazel (or dark gray), and dark; then it was shown that 1/4 of all the fundamentally light and 1/4 of all the dark were statistically allotted between light and dark in that proportion. The desired test of the truth of the laws in question was thus reduced to a comparison between the calculated and observed proportion of light and dark-eyed children born of ancestry whose eye-colours presented various combinations of light, hazel, and dark. The inquiry was confined to children of whom the eye-colours of both parents and of all four grandparents were known. There are six possible combinations of the three eye-colours in the parents and fifteen in the grandparents, making a total of ninety possible classes, but of these one half were wholly unrepresented in the returns, and many others were too scanty to be represented to be of use. The remainder were disposed of by different ways: that is to say, in two groups, a and 6, and each group by three methods. In a the families were classified and grouped according to their several ancestral combinations in the order of 0° 052, but only those that consisted of twenty or more children were used; there were 26 of these groups and 827 children. In b the families were treated separately, but only large families were taken, viz. those that consisted of at least six children: they were 78 in number. In both a and b separate calculations were made on the suppositions (1) that the parental eye-colours were alone known; (2) that the grandparental were alone known; (3) that the parental and the grandparental were alone known.

The conformity between the calculated and the observed numbers throughout every one of the six sets of calculations was remarkably close, and the calculated results obtained by the method [3] were the best.


In this paper an endeavour has been made to ascertain the nature of the changes which are induced in a few typical vitreous rocks by the action of heat only. The specimens experimented upon were—

(1) The pitchstone of Corrigills, Arran.
(2) Black obsidian from Ascension.
(3) Black obsidian from the Yellowstone District, U.S.A.
(4) Glassy basalt lava of Kilauea, Hawaii.
(5) Basalt of the Giant's Causeway, Antrim.

The Arran pitchstone was heated for 216 hours at a temperature ranging from 500° to about 1100° C. The clear, greenish-brown beities of hornblende, so plentiful in the unheated rock, were found to have turned to a deep rusty brown through peroxidation of the protoxide of iron which was present in the hornblende. The dusty matter mixed with clear specks of hornblende, which occurred between the beities and shaded gradually off into the clear glass which immediately surrounded the beities in the normal state of the rock, has segregated to some extent, a sharp line of demarcation now existing between the dusty matter and the areas of clear glass, while the specks of hornblende have somewhat increased in size if not in number. No actual devitrification of the glass has resulted from the heating.

The obsidian from Ascension showed only a band of structure connected with streams of colourless micromelites and a few felspar crystals when a section of the unaltered rock was examined microscopically. Two specimens of this rock were artificially heated, the first for the same period and at the same temperature as the Arran pitchstone, while the second was kept for 701 hours at a temperature ranging from 850° to 1100° C. In the first specimen the band of structure disappeared entirely, or almost entirely, but numerous micromelites are present in the altered rock, in which the most remarkable change consists in the development of an excessively vesicular structure.

In the second specimen a vesicular structure is also developed, an outer crust consisting of a very thin layer of clear brownish glass, followed by a nearly opaque layer composed of greenish-brown micromelites, which shades off into a colourless glass containing similar micromelites, which are probably some form of amphiolite or pyroxene. The remainder of the specimen has been completely devitrified.

The Yellowstone obsidian in its normal state shows little else but trichites and globules when examined under a high power. Two specimens of this rock were heated: the first at from 500° to 1100° C. for a period of 216 hours, and the second at 850° to 1100° C. for 701 hours. In the first case a remarkably vesicular structure has been developed; the trichites have entirely disappeared, and small granules and crystals of magnetite have been formed. In the second case the changes are still more peculiar. The fragment retained its original form, but the surface showed minute blisters or elevations, which, when cracked open, revealed a cavernous structure produced by
I also have a theory which, while it differs much from that of Mr. Romanes, runs on curious parallel lines to it, and was prompted by the same keen sense of an inadequacy in the theory of Natural Selection for the origin of varieties. I should not have published my views until they had been far more matured than they are had not the present occasion arisen.

It has long seemed to me that the primary characteristic of a variety resides in the fact that the individuals who compose it do not, as a rule, care to mate with those who are outside their pale, but form through their own sexual inclinations a caste by themselves. Consequently that each incipient variety is probably rounded off from among the parent stock by means of peculiarities of sexual instinct, which prompt what anthropologists call endogamy (or marriage within the tribe or caste), and which check exogamy (or marriage outside of it). If a variety should arise in the way supposed by Mr. Romanes, merely because its members were more or less sterile with others sprung from the same stock, we should find numerous cases in which members of the variety consorted with outsiders. These unions might be sterile, but they would occur all the same, supposing of course the period of mating to have remained unchanged. Again, we should find many hybrids in the wild state, between varieties that were capable of producing them when mated artificially. But we hardly ever observe pairings between animals of different varieties when living at large in the same or contiguous districts, and we hardly ever meet with hybrids that testify to the existence of unobserved pairings. Therefore it seems to me that the hypothesis of Mr. Romanes would in these cases fail, while that which I have submitted would stand.

The same line of argument applies to plants, if we substitute the selective appetites of the insects which carry the pollen, for the selective sexual instincts of animals. Both of these, it will be remembered, are mainly associated with the senses of smell and sight. If insects visited promiscuously the flowers of a variety and those of the parent stock, then—supposing the organs of reproduction and the period of flowering to be alike in both, and that hybrids between them could be produced by artificial cross-fertilization—we should expect to find hybrids in abundance whenever members of the variety and those of the original stock occupied the same or closely contiguous districts. It is hard to account for our not doing so, except on the supposition that insects feel a repugnance to visiting the plants interchangeably.

No theme is more trite than that of the sexual instinct. It forms the main topic of each of the many hundred (I believe about 800) novels annually published in England alone, and of most of the still more numerous poems, yet one of its main peculiarities has never, so far as I know, been clearly set forth. It is the relation that exists between different degrees of unlikeness and different degrees of sexual attractiveness. A male is little attracted by a female who closely resembles him. The attraction is rapidly increased as the difference in any given respect between the male and female increases, but only up to a certain point. When this is passed, the attraction again wanes, until the zero of indifference is reached. When the diversity is still greater, the attractiveness becomes negative and passes into repugnance, such as most fair-complexioned men appear to feel towards negroes, and vice versa.

I have endeavoured to measure the amount of difference that gives rise to the maximum of attractiveness between men and women, both as regards eye-color and stature, chiefly used in my collection of "Family Records," and have succeeded in doing so roughly and provisionally. To determine it thoroughly, and to lay down a curve of attractiveness in which the abscissae shall be proportional to the amounts of difference, and the ordinates to the strength of attraction, would require fresh and special data that have

THE ORIGIN OF VARIETIES

The publication in the three last numbers of Nature, by Mr. Romanes, of very important papers, induces me to send the following lines as a contribution to the discussion upon them that is sure to ensue. He ascribes the origin of varieties to peculiarities in the reproductive system of certain individuals, which render them more or less sterile to other members of the common stock, while they remain fertile among themselves.