the experimental value of the last three or four figures? The specific gravity of each species relative to water is given as usual, so that the reference to hydrogen is only an additional torment for the learner. We doubt too the wisdom of explaining specific and atomic heats, and giving lists of their values. Isomorphism and pseudomorphism are hopelessly confused and interchanged on p. 20, while the illustrative formula is quite unintelligible. The adjusting apparatus of the ungraduated goniometer is, as usual in text-books, wrongly disposed for use. We have noticed several mistakes of fact and errors of printing; but the book is neat in style, and perhaps will not do much harm.

The Prospector's Handbook. By J. W. Anderson, M.A., F.R.G.S. 8vo, pp. 132. (London: Crosby Lockwood and Co., 1886.)

THE author, after traversing the mineral fields of New Zealand, New Caledonia, New Mexico, and Colorado, feels convinced that some simple guide or handbook for the use of prospectors as well as travellers is a desideratum, and the present volume is the outcome of this conviction. It contains a number of notes or paragraphs upon subjects incidental to metallic mining, which are distributed into chapters under the different heads of prospecting, rocks, blowpipe-testing, character of minerals, metals, and metallic ores, other useful minerals and ores, composition of various rocks, testing by the wet process, assay of ores, and surveying; to which are added an appendix of tables and a glossary of terms. As the whole text is contained in rather more than a hundred pages, not very closely printed, it will be easily understood that no one of the numerous subjects included in the author's programme is very thoroughly treated. The best part of the book is the introductory chapter on prospecting, which contains some useful generalisations on mineral deposits and the search for them, which, however, are more likely to be of use to the "tender-foot" than to the prospector properly so called. It would seem, however, that this is what the author has in contemplation, as, from some remarks on p. 9, he appears to consider prospectors and miners as two different classes of men, and evidently has no very favourable opinion of the latter. Our own experience points in the opposite direction and leads us to regard typical prospectors as representing the highest and most intelligent class of operative miners. Unfortunately it is difficult to keep them on regular mining works except during the winter time, when the mountain regions are inaccessible.

The remainder of the book is of very little value. The descriptions of minerals are short, without being clear, and in many cases far from accurate. Thus, the composition of galena is stated to be "80 per cent. of lead, the rest sulphur"; malachite is said to contain 70 per cent. of copper, and silicate of zinc about 67 per cent. of zinc. All of these statements are incorrect, and it is not easy to see why they have been made, as no more space would have been required to give the composition corresponding to the theoretical constitution.

The sections on assaying and analysis are not likely to be required by the prospector in the field, and are too vague to be of much use to sedentary students. A description of the methods adopted in sampling gold and silver-bearing vein-stuff in the Western States and Territories of America would have been of interest, but we find no notice of this or any analogous practice followed elsewhere.

The glossary at the end contains several curious definitions, many of which, however, are reproduced from previously published works. The description of the term "tribute" more properly applies to dues or royalty rents as understood in this country. It may be that the author's definition applies to some local foreign usage, but this is not stated. H. B.

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 space is so great that it is impossible otherwise to insure 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 $8\frac{1}{4}$ 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:—

A ft. in.	B per cent.	A ft. in.	B per cent.	A ft. in.	B per cent.
5 $8\frac{1}{4}$	50	6 0	15	6 4	1·4
5 9	42	6 1	9	6 5	0·7
5 10	31	6 2	5	6 6	0·3
5 11	22	6 3	3		

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, 1885, 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 delta-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 microscopical and chemical investigation 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 aright, 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 Isthmian deposit." In the absence of any palæontological 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 INCLOSE 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 difference between the constants as got by the machine and by calculation from the twenty-four hourly means 0°·065. It may be noticed, however, that the numbers are unusually large (and at the same time very decidedly systematic) in the case of the second cylinder of the first order b_1, for which the average is as much as 0°·150, the seventh of a degree.

"If b_1 be omitted, the average for the remaining cylinders of the machine is reduced to 0°·047.

"We see, therefore, that, with the exception perhaps of b_1, 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 photograms, then measured, then meaned, and the constants deduced from the means by a numerical process by no means very short."

The curves for the twelve years 1871 to 1882 inclusive have now been passed through the machine, and the results obtained have been carefully checked so far as the arithmetical work involved is concerned, upon a plan approved by the Council. No *direct* check, short of passing the curves a second time through the machine, can however at present be put on any 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 Hourly 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 measurements 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 generally these are less due to defects in the working of the instrument than to other causes. In some cases large breaks in the curves, due to failure of photography, &c., were interpolated when the curves were passed through the machine, but not when the means were worked out from measurements of the curves. Some differences rather larger than usual, and confined chiefly to the earliest years dealt with, have been ascertained to have arisen from the circumstance that when the curves were first measured, to obtain hourly values, the method of making the measurements was not the same as that found by subsequent experience to be the preferable; and also that in some cases 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 later years dealt with, and therefore the fairest comparison is to be had with their means.

For 1880, the average difference of the monthly mean for all the seven observatories is 0°·09; for 1881 it is 0°·05; and for 1882 0°·06; and in these three years a difference of 0°·3 between the analyser and calculated means occurred but once, and of 0°·2 but five times.

What has been said is sufficient to show that the instrument is completely applicable to the analysis of thermograms.

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 1876 was selected 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 stature 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 $\frac{1}{4}$, from each grandparent $\frac{1}{16}$, and so on; also that each parent and each child of any person will on the average possess $\frac{1}{3}$ of that person's peculiarity. The eye-colours were grouped into light, hazel (or dark gray), and dark; then it was shown that $\frac{2}{3}$ of the hazel were fundamentally light, and $\frac{1}{3}$ of them were dark, and they 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 scantily represented to be of use. The remainder were discussed in six different ways: that is to say, in two groups, *a* and *b*, and each group by three methods. In *a* the families were classified and grouped according to their several ancestral combinations of eye-colour, but only those groups that consisted of twenty or more children were used; there were 16 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.

"Notes on Alteration induced by Heat in Certain Vitreous Rocks, based on the Experiments of Douglas Herman, F.I.C., F.C.S., and G. F. Rodwell, late Science Master in Marlborough College." By Frank Rutley, F.G.S., Lecturer on Mineralogy in the Royal School of Mines. Communicated by Prof. T. G. Bonney, B.Sc., F.R.S.

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 Corriegills, 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 belonites of hornblende, so plentiful in the unaltered 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 spiculæ of hornblende, which occurred between the belonites and shaded gradually off into the clear glass which immediately surrounded the belonites 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 spiculæ 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 banded structure coupled with streams of colourless microliths 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 banded structure disappeared entirely, or almost entirely, but numerous microliths 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 microliths, which shades off into a colourless glass containing similar microliths, which are probably some form of amphibole or pyroxene. The remainder of the specimen has been completely devitrified.

The Yellowstone obsidian in its normal state shows little else but trichites and globulites 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, the second from 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 specimen the changes are very peculiar. The fragment retained its original form, but the surface showed minute blisters or elevations, which, when when cracked open, revealed a cavernous structure produced by of something in these cows competent to produce scarlatina in persons consuming their milk, and the inquiry was narrowed to determining what this was. All comparison with former experiences was for the present left out of consideration, the investigation proceeding strictly on the circumstantial evidence obtained and obtainable. A consideration of all that had gone before, and the absence of any alternative, led to the provisional adoption at this point of a theory of disease in the cows, and the probability was that this was an infectious disease, communicable from cow to cow, a disease, moreover, the existence of which was compatible with the animal affected feeding well, and milking abundantly.

The discovery of vesicles and ulcers on the teats and udders of cows in the large shed soon followed; the first to show the disease was one of the Derbyshire cows, the second one from Oxfordshire. After this the matter passed into Dr. Klein's hands; but with his report we have nothing to do here. A painful incident soon gave Mr. Power ample corroboration of the result which he had reached. The Marylebone dealer returned on the farmer's hands, on December 15, all his milk from the larger shed, and this was destroyed by pouring it into a pit dug on his land. The news of the destruction of milk spread among some of the poor people of Hendon, and some of them succeeded by the favour of friends amongst the cowmen in obtaining some of it on December 16. By the 20th scarlatina made its appearance amongst half-a-dozen of the families thus supplied. Conversely in South Marylebone about Christmas, when these Hendon families were falling ill, the disease ceased almost suddenly, and there were no fresh attacks, except such as were referable to infection from previous sufferers.

A thorough examination of all the cows showed that the disease had spread to every one of the three sheds, and the farmer was accordingly advised to seek out every cow then or afterwards affected with sore teats or udder, or any other ailment, to isolate her and keep all her milk out of the business, and prevent cowmen employed about the sound cows from attending the infected ones. These precautions were taken from January 1, and were barely in time to prevent an alarming increase of scarlatina in all the districts served from Hendon, including St. John's Wood, where the appearance of scarlatina corresponded to a nicety with the appearance of the cow-disease in the animals in the small shed. The milk from the Hendon farm was ultimately given up by all the dealers concerned, with the result that scarlatina has disappeared from amongst the customers of the dealers here referred to in Marylebone, St. Pancras, Hampstead, and St. John's Wood. The work of demonstrating the nature of the cow-disease, and its connection with human scarlatina was not Mr. Power's, and from him the matter passed on to Dr. Klein. The former had succeeded in gathering up and connecting the scattered links of a chain of presumptive evidence against certain cows so strong as to be unassailable; and he had done this by the exercise of patience, sagacity, and acuteness which would have done credit to a great criminal lawyer weaving the web of circumstantial evidence around an unusually cunning forger or murderer.

THE ORIGIN OF VARIETIES

THE publication in the three last numbers of NATURE, by Mr. Romanes, of very important papers,[1] 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. I also have a theory which, while it differs much from that of Mr. Romanes, runs on curiously parallel lines to it, and was prompted by the same keen sense of an inadequacy in the theory of Natural Selection to account 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.

[1] I write from abroad, and have not yet seen the original memoir published by the Linnean Society.

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 infertile 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-fertilisation—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 negresses, and *vice versâ*. 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-colour and stature, chiefly using the data contained 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 abscissæ shall be proportional to the amounts of difference, and the ordinates to the strength of attraction, would require fresh and special data that have