

and the hair-colour from seven to fourteen are but little, in England at any rate, subject to environmental influence. The cephalic index remains almost constant throughout life. Now the mean value for the fraternal correlation for these three characters is 0.5161, and for stature, span, forearm and health, which might be supposed to be largely influenced by environment, it is 0.5179. It thus seemed to me that environment was not an important factor in modifying the correlation of the *physical* characters between brothers. In other words, *environment does not influence the constant uniformly in one direction*. This view was apparently appreciated by Prof. Cockerell when he wrote "the treatment of successive children is not the same." Any influence of environment, strange as it may seem, was thus found to be negligible. Turning to the intellectual characters, my own *a priori* conception was that I should for the first time be able to distinguish between nurture and heredity. I expected much higher correlations in the case of temperament and probity than in that of physique. I was therefore somewhat surprised when the values came out much the same as in the case of the physical characters, say an average of 0.5. Of course it is open to Prof. Cockerell to say that 0.3 of this only is due to heredity and 0.2 to environment, or whatever other division appears to him probable, but he will then have to explain why the sum of the two makes 0.5, and why the influence of heredity is less in the intellectual than it appears to be for the physical characters. There is the obvious direct scientific interpretation which seems to me the true one, environment does not act in one direction in *either* case, and the mental and physical characters are inherited precisely at the same rate. To those who have taken the trouble, as I have done, to examine carefully the mental characteristics of a family at intervals of a century apart, so that we are not troubled by the co-environment peculiar to brothers, it is needless, perhaps, to urge the very strong inheritance of mental qualities. If Prof. Cockerell attributes it to his third factor, "pre-existing soul," I should, indeed, be proud to have aided in the demonstration of its reality, although I fail entirely to see how it is to be done "by just such methods as Prof. Pearson employs." Meanwhile most people will, I think, prefer to stick to heredity.

Of the last paragraph of Prof. Cockerell's letter I understand not a word. Correlation is quite independent of variation, and although skull capacity is highly variable as compared with length of femur, I see no reason for supposing the former is therefore less strongly inherited than the latter.

I have not touched on the influence of "local races" on my data, because that appears to be a factor which has escaped Prof. Cockerell, and so I am not bound to state a doubt which I have well considered in order to reject it.

KARL PEARSON.

Magnetostriction of some Ferromagnetic Substances.

WE avail ourselves of your valuable Journal to give a short notice of some new results, obtained in our researches on magnetostriction, being a continuation of our investigation on the same subject, given in the "Rapports présentés au Congrès international de Physique," t. ii., by one of us.

It was generally believed that ferromagnetic bodies show change of length by magnetisation, but not of volume. Minute as the effect generally is, it is now placed beyond dispute that iron, nickel, cobalt and especially steel differ also in bulk in the magnetised state from that in the unmagnetised. In the course of our researches on the magnetostriction of different ferromagnetic bodies in the form of ovoids we came across a substance which shows a remarkably large increase of volume.

Examining the magnetostriction of reversible nickel-steels of different composition, which were kindly placed at our disposal by Dr. Guillaume and M. Dumas, we find that alloys containing 46 per cent., 36 per cent. and 29 per cent. of nickel all show increase of length several times greater than that observed in ordinary iron. But far more striking is the change of volume; of the three above-mentioned alloys, the effect increases as the percentage of nickel becomes less; with 29 per cent. alloy we observed a motion of 5 mm. of the liquid in the capillary tube (diam. 0.4 mm.) attached to the volumometer containing the specimen (volume $v = 10$ c.cm.) under examination. Thus the greatest change of volume by magnetisation amounts to $\delta v/v = 51.1 \times 10^{-6}$ in $H = 1690$ C.G.S. With ordinary iron, the same change $\delta v/v = 1.2 \times 10^{-6}$ in the same field, being

only about 1/40th of that observed in nickel-steel. The magnetisation of 25 per cent. nickel alloy is so feeble that it is impossible for it to be magnetometrically measured, and the change of length is inappreciably small, but the volume change is measurable and amounts to 0.2×10^{-6} in $H = 1790$. It is thus quite probable that there is an alloy containing somewhat more or less than 29 per cent. of nickel that indicates largest increase of volume by magnetisation.

We at first thought it would be possible to trace some connection between the thermal expansion and the change of length by magnetisation. No such relation seems to exist; the 36 per cent. alloy, which is the least expansible by heat, indicates tolerably large elongation by magnetisation.

As regards the Wiedemann effect, nickel-steels behave very much like iron, showing the maximum amount of torsion in moderate fields.

As is well known, the behaviour of cast cobalt, as regards the change of length by magnetisation, is opposite to that of iron, but the volume change in the same metal is much smaller. By annealing cobalt in a charcoal fire it assumes a pale ashy colour, and the magnetic character is greatly changed. The metal becomes less magnetisable, and shows constant decrease of length accompanied by increase of volume.

We have also found, by actual experiment, that the effect of stress on magnetisation and the magnetostriction in cobalt and in nickel-steel are reciprocally related to one another, as was already established for iron and nickel.

H. NAGASKA,
K. HONDA.

Physical Laboratory, Imperial University,
Tokyo, December 3, 1901.

Results of International Magnetic Observations made during the Total Solar Eclipse of May 17-18, 1901.¹

To test further the results obtained by the United States Coast and Geodetic Survey magnetic parties during the total solar eclipse of May 28, 1900, regarding a slight magnetic effect that may be attributable directly to some change produced in the electrification of the upper atmospheric strata by the abstraction of the sun's rays due to the interposition of the moon between the sun and the earth, an appeal was made for international cooperation in magnetic and allied observations during the recent total solar eclipse.

The repetition of the observations was doubly interesting owing to the fact that the present eclipse occurred in the opposite magnetic hemisphere to that of the year 1900, and hence the opportunity was afforded for ascertaining whether the magnetic effect was reversed in its general character to that of 1900, as is, for example, the case with the diurnal variation in passing from one magnetic hemisphere to the other. The conditions, however, for obtaining observations at a number of stations distributed along the belt of totality, as was done in 1900, and thus testing whether the magnetic effect again followed directly in the wake of the shadow cone, were not favourable owing to the present location of the belt of totality.

In response to the appeal, simultaneous magnetic observations were made on May 17 from 14 to 21 o'clock Greenwich mean astronomical time—an interval amply covering the time of the eclipse—at a number of stations encircling the entire globe, three of which were in the belt of totality. The prime purpose of making the observations so as to cover the entire globe was to furnish the possibility of separating a possible eclipse magnetic effect from a contemporaneous magnetic storm of the usual type. The eclipse effect, for instance, doubtless would be confined to a very small belt, whereas a customary magnetic storm, in conformity with the usual experience, would manifest itself at practically the same moment of time over a very large area and thus be felt at stations far from the totality belt.

At none of the outside stations has a disturbance of any appreciable size been thus far reported to me, the general consensus of opinion of observers at these stations being that "nothing unusual occurred."

At the three stations within the belt of totality the majority of opinion is that something unusual did occur during the time of the eclipse.

¹ Presented before the meeting of the Astronomical and Astrophysical Society at Washington, December 30, 1901.

Thus at Karang Sago, where was situated the Dutch eclipse party, Dr. W. van Bemmelen, assistant director of the Batavia Magnetic Observatory, observed the changes in the magnetic declination and in horizontal intensity, and he reports the occurrence of "an extremely interesting magnetic effect." He has courteously sent me an extract of his observations, made during several days before and on the day of the eclipse, and there certainly appears evidence of a magnetic effect in both elements different from that observed on the days prior to the eclipse.

At Sawah Loento, the site of the Massachusetts Institute of Technology party of Boston, the variations in magnetic declination were observed by Mr. G. L. Hosmer on May 17 and 18. Comparing the two days' results for the interval of the eclipse, there is indisputable proof that something different occurred on the day of the eclipse than on the day before. Namely, at this station, situated so close to the magnetic equator, the range of the diurnal variation of the magnetic declination is about one minute of arc. The magnetic effect during the time of the eclipse was of about the same amount, so that a steady decrease of east declination resulted during the time of day when, normally, there is a steady increase.

There was but one magnetic observatory directly within the belt, viz. the one at Mauritius, and this was situated not far from the place of beginning of the eclipse. No special magnetic observations were made at this place, however, but regular photographic curves giving the variations in the magnetic elements were obtained. The declination and the vertical intensity curves apparently do not show any disturbance that could easily be picked out and referred to the eclipse. Regarding the horizontal intensity curve—the more sensitive one—Mr. Claxton states "that the original curve shows slight tremors between 7.15 and 7.50, and occasionally between 8.5 and 9.0 a.m." I have plotted this intensity curve on a larger scale, and find that the curve shows no very marked disturbance that might be readily referred to the eclipse, with the exception of one producing an easily perceptible bulge in the curve amounting to about 3-4 units in the fifth decimal C.G.S. units and lasting about 30 minutes. Anyway the effect, if there be one, is very minute, and will not be so readily separated from the usual diurnal variation as in the case of the two previous stations. Whether this is due to the fact that owing to the vicinity of Mauritius to the beginning of the eclipse the minute eclipse magnetic storm did not have time to develop itself or was just in the embryonic state cannot be said.

The magnetic effect observed at Karang Sago and at Sawah Loento does not appear to have extended very far outside the belt of totality, it being scarcely appreciable at the Batavia magnetic observatory.

My grateful and appreciative acknowledgments are due to all who have participated in this interesting investigation—one, to my mind, of fundamental importance to the theory of the diurnal variation of the earth's magnetism as elaborated by Schuster and von Bezold.

L. A. BAUER.

U.S. Coast and Geodetic Survey, Washington, D.C.
December 30, 1901.

The Roots of the Equation $u = \tan u$.

In many treatises on optics it is stated that the roots of the equation, $u = \tan u$, were calculated by Scherwd. Verdet ("Oeuvres," t. v. p. 266), says:—"These roots have been calculated by Scherwd, who arrived at the following values: $u_1/\pi = 1.4303$, $u_2/\pi = 2.4590$, . . ." up to u_n/π . Preston ("Light," p. 255, second edition) says:—"The values of u corresponding to the maximum values of the illumination have been given by Scherwd as follows:—" The values given are precisely the same as Verdet's. Rayleigh ("Encyc. Brit.," vol. xxiv. p. 430, art., Wave Theory) gives a method for calculating the roots of the equation, and remarks that they were obtained in another manner by Scherwd. (There is a misprint in Rayleigh's value for u_1). Other references might be added.

Will someone kindly indicate where Scherwd gives the results referred to?

In his "Beugungserscheinungen" he shows that the roots of the equation are approximately the values of $(2n+1)\pi/2$, obtained by giving integral values to n ; and he remarks (in § 63, p. 28) that for values $n=1$, and $n=2$, the true values of u differ

by $12^\circ.5$ and $7^\circ.5$ from $3\pi/2$ and $5\pi/2$, respectively. In table i., at the end of the book, he gives values of the expression for the intensity, $\sin^2 u/u^2$, for values of u increasing by 15° ; and at the foot of the table he states that the first and second maxima are at $257^\circ.5$ (i.e. $270^\circ - 12^\circ.5$) and $442^\circ.5$ (i.e. $450^\circ - 7^\circ.5$). Further details I have not found.

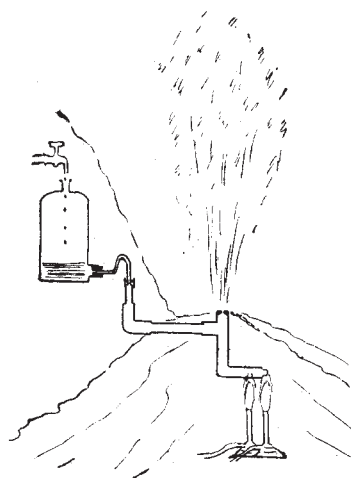
It may be remarked that the roots of the equation under consideration were given long before Scherwd's time. In Euler's "Introductio in Analysin Infinitorum" (Lausanne 1748) the question is fully discussed. See t. ii., cap. xx., prob. ix.

C. A. CHANI.

University of Toronto, December 27, 1901.

A Model Geyser.

If the following working model, which was the outcome of a good many trials, is in any way original it may prove of interest, as it works automatically and with excellent effect as a geyser of regular period, suitable for a lecture table. The figure needs but little description. A small aspirator with a bent glass tube exit acts as an intermittent syphon. The water is discharged into a half-inch iron pipe, the long horizontal limb of which measures some 13 centimetres. The glass syphon tube slips through a rubber ring at the top of the pipe (gauge fitting), or a cork would doubtless answer the purpose. The lower closed end of the tube is heated by the equivalent of about four ordinary Bunsen burners, and should be placed as shown, as if placed



under the exit, steam is generated too fast and the water may be blown back into the aspirator. Water drips into the aspirator at such a rate that the syphon discharges about 300 c.c. of water once in every ten minutes. A jet of steam some six feet high and water some two feet high results, with many appropriate gurglings. The diameter of the exit is about 6 millimetres. Of course the apparatus is concealed; a large circular tin canister to which the iron pipe is screwed forms a good foundation and serves to keep the water off the burners. Furnace clinker, which is not wholly unsuitable, forms a readily obtainable material for completing the external features of the geyser.

Felsted, January.

A. F. MUNBY.

Birds Capturing Butterflies in Flight.

WITH reference to Mr. Latter's letter in NATURE of November 16, 1899 (vol. lxi. p. 55), which has been brought to my notice, I would say that the supposition that birds do not attack butterflies in flight is not strictly correct.

The common King Crow (*Dicrurus ater*, I believe) invariably captures butterflies on the wing; I have seen these birds scores of times do this. Their usual prey seems to be a small deep yellow butterfly with black on the tip of the wings, but I have occasionally seen other butterflies so captured by them.

India, December 18, 1901.

A. E. MCKAY