

(as I do) dissent from some of his conclusions; but the methods he has used are legitimate and sufficient for his immediate purpose, and, in my opinion, the work as a whole is one of the most stimulating contributions to the study of intra-racial heredity published in recent years.

December 22, 1903.

G. UDNY YULE.

WITH regard to Mr. Yule's view that there is a fundamental misunderstanding in our notice of Prof. Johannsen's book, we must direct attention to the problem at issue summed up in the words "Der Rückschlag ist vollkommen ganz bis zum Typus der Linie." The character selected for measurement by Prof. Johannsen either fully determines the type or it does not, *i.e.* in the latter case it may be subject to somatic variations having no influence on offspring as Mr. Yule suggests. If it does determine the type, then the correlation between the parent and the mean of the offspring should be perfect, and this it certainly is not. If it does not determine the type, the correlation might be imperfect because the character of the line would not be perfectly known. But since the parental character is in this case not perfectly known, it is clearly impossible for Prof. Johannsen to determine the type, and thus his experiments must fail to show whether the "Rückschlag" is perfect or not. This point is referred to in the reviews cited by Mr. Yule, but it seems to have escaped his notice.

In the next place Mr. Yule asserts that Prof. Johannsen has shown that the intensity of heredity between the first two generations sprung from a single individual may be vanishingly small. This is precisely what he has failed to do. To deal with heredity the *same* character must be selected in two successive generations, and this, as pointed out in the review in question, Prof. Johannsen has not attempted.

The remainder of Mr. Yule's letter being neither a reasoned defence of Prof. Johannsen's book nor a criticism of our review calls for little comment; it will command from the reader just that degree of assent which he may be accustomed to give to mere opinion very authoritatively stated. Mr. Yule's estimate of the value of Prof. Johannsen's experiments and statistical methods differs widely from that expressed in our review, but nothing is gained either in criticism or controversy by the mere posing of a rival *ipse dixit*.

THE REVIEWER.

#### The Heat of Radium.

A NUMBER of years ago I published a theory of the formation of the elementary bodies, based on polymerisation and its reversal. The numerics ("atomic weights") of the elements show an increasing accordance with that theory as time goes on.

Of our existing system of numerics the polymerisation points are comprised in the expression  $n15$ ; they are, 15, 30, 45, 60, 75, 90, 105, 120, 135, 150, 165, 180, 195, 210, 225, 240. As chemical change in general exhibits a great tendency to run down, we may fairly assume that most of the earlier reversals have already occurred, and that such as remain will be associated with elements of high numeric.

It is clear that polymerisation must involve the emission of heat, and I am strongly disposed to regard radium ( $Ra=225$ ) as the product of a "stoff" in the act of polymerisation, the reversal being well indicated by the discharge of helium ( $He=4$ ).

It is interesting to notice that Sb ( $=119.5$ ) and Sm ( $=149.9$ ) are extremely near polymerisation points. It would be worth while to examine compounds of these bodies for emitted heat and gaseous or other matter.

The emanation phenomenon would also appear to be in some way related to the same points. It is, for example, stronger in V ( $=238$ ) than in Th ( $=231.7$ ). Both these bodies, and the substances they emit, should be derived from a hitherto unknown polymer ( $=240$ ) undergoing reversal into simpler bodies.

EDMUND J. MILLS.

January 4.

#### Rocket Lightning.

My attention has been directed to a letter in your issue of October 22, 1903, describing certain flashes of lightning that were visible on July 22. In many respects the flashes corresponded with flashes seen by myself and friends at the same hour on the same evening, but the discrepancies are remarkable. For instance, Mr. Everett, in the letter referred

to, saw flashes "bearing a strong resemblance to ascending rockets, a luminous trail shot up about as fast as, or rather faster than, a rocket," whereas we saw flashes that appeared with about the ordinary rapidity. There certainly was a strong suggestion of ascension, but vertical lightning flashes quite commonly exhibit this appearance, which sometimes at least is due to an optical illusion.

The bearing of the flashes as seen by us, from the verandah of a house in Camac Street, was N.  $143^{\circ}$  W., and as the Sibpur College bears, from our position, N.  $86^{\circ}$  W., about 16,500 feet distant, it would be quite easy to calculate the position of the flashes if Mr. Everett had noted their bearing accurately. His description of the bearing as "in the S.S.W." suggests that this is only intended as a general indication of the direction. If S.S.W. were the exact direction, the flashes could only have been  $10\frac{3}{4}$  miles distant from Mr. Everett and 12 miles from us, but if the direction were the next point of the compass, S.W. by S., the flashes were 50 miles away. This greater distance is probably nearer the truth, because if the flashes were only 10 or 15 miles distant thunder would have been audible.

Again, the angular altitude of the highest part of the flash is given by Mr. Everett as " $15^{\circ}$  or so," which does not agree with our observation of  $10^{\circ}$  or a trifle under, perhaps nearer to  $9^{\circ}$ . At a distance of even 30 miles there should have been no observed difference of maximum altitude between Mr. Everett's observation and ours.

In other respects the observations tally precisely. The vertical flashes appeared repeatedly in the same position against a background of clear sky, so clear that a star,  $\zeta$  Centauri, was visible at an altitude exceeded by the flash.

Mr. Everett falls into an error in supposing that the lightning "must have occurred at a spot above the Sunderbunds." The direction of the Sunderbunds is not westerly, but easterly from the Sibpur College, and the flashes must have been over some part of the Twenty-four Perganas if not more than 25 miles away, over the Midnapore district if more than 35.

As to there being "not a score of men in all Bengal who would take a serious interest in such lightning if they did happen to see it," I am not aware of the precise number, and can only vouch for three, the manager of a railway, another competent observer, and myself, who observed together, but I should not be surprised if the flashes were also seen by other observers equally able to record their observations accurately.

W. A. LEE.

Calcutta, December 10, 1903.

I GATHER from Mr. Lee's account that he only witnessed one kind of lightning, whereas my son's letter describes two kinds altogether different in appearance. The inference would seem to be that the less brilliant and more unusual kind was not visible in the centre of Calcutta, though visible at Sibpur, probably owing to better atmospheric conditions.

J. D. EVERETT.

11 Leopold Road, Ealing, December 30, 1903.

#### The Recent Leonid Shower.

THE results of the observations by M. Eginitis of the recent Leonid shower indicate that there was another maximum on the night of November 15, occurring several hours previously to that seen by observers situated in or near the longitude of Greenwich. This early maximum was evidently of a very distinct character at Athens, as the observations showed a regular increase and decrease of meteoric frequency before and after the time of culmination (15-16h., local time), the watch having been prolonged for some hours further, or until 17h. 50m. Athens was evidently too far east to permit observers there taking cognisance of the later outburst of meteoric activity that added considerably to the strength of the shower here. The maximum mentioned by M. Eginitis does not appear to have been very noticeable as such to British observers, though it was anticipated here that that event should occur on November 15 at 13h. 30m. G.M.T., the calculated maximum thus falling within the hour, when we allow for the difference of longitude, during which it was actually observed at Athens. The later maximum came altogether unexpected. It is noteworthy that these maxima seem to have been characterised by quite a distinct type of meteor.

Dublin.

JOHN R. HENRY.