He now adds that he might as well "have used any of the other cases collected by Mr. Darwin." It is not a very material point, but I do not find that Mr. Darwin makes any reference to Wells's theory in his discussion of correlation, nor do I see any in the body of the sixth edition of the "Origin of Species," though a passage is quoted from Wells's paper at p. xi. of the "Historical Sketch" which is prefixed to it. It had, however, independently occurred to Mr. Darwin, and he discusses it in a somewhat different connection in the "Descent of Man" (i. pp. 242–245). He remarks:—"That the immunity of the negro is in any degree correlated with the colour of his skin is a mere conjecture; it may be correlated with some difference in his blood, nervous system, or other tissues." And he concludes:—"I endeavoured with but little success to ascertain how far it held good." Elsewhere he gives cases to show that "differences in colour are correlated with constitutional differences." But these, though interesting, seem to me too obscure to found any definite conclusion upon. And no attempt is made to show on what material basis, subject to variation, the constitutional difference depends.

The correlation principle as originally defined dealt then with obvious and measurable characters. It is extended by Prof. Lankester's "suggestion" to what is obscure, may be unknown, and perhaps unknowable. In considering the probable utility of any specific character we shall, if the extended principle be accepted, be always open to the objection that we cannot show that the character is not the outward and visible sign of some unobservable internal peculiarity. But that is a position which I do not think we are bound to accept till something more than a hypothetical case has been established.

To sum up: Mr. Darwin based the correlation principle on what is concrete and tangible; Prof. Lankester extends it to what is intangible and hypothetical. It is not a question of what is "apostolic and orthodox," but of what is susceptible of reasonable proof.

As I do not propose to continue this discussion any further, I will take the opportunity of saying that I think it is a matter for regret that, as Prof. Lankester was present at the meeting of the Royal Society when Prof. Weldon's paper was read, he did not deliver himself on that occasion of his somewhat belated criticism. Prof. Weldon's work is of extraordinary interest, and one cannot but admire the self-sacrifice with which such laborious investigations have been prosecuted. If they want a defence, I think the following passage from the "Origin of Species" supplies it.

"It may metaphorically be said that natural selection is daily and hourly scrutinising, throughout the world, the slightest variations; rejecting those that are bad, preserving and adding up all that are good; silently and insensibly working, *whenever and wherever opportunity offers*, at the improvement of each organic being in relation to its organic and inorganic conditions of life. We see nothing of these slow changes in progress, until the hand of time has marked the lapse of ages, and then so imperfect is our view into long-past geological ages, that we see only that the forms of life are now different from what they formerly were." (Sixth edition, pp. 65–66.)

I do not myself see how the slow and ordinarily imperceptible, but inevitable action of natural selection can be demonstrated except by the statistical method. But, firmly as I believe in the inevitableness of that action, I confess that the results attained by Prof. Weldon surpassed my expectations. I am unable to agree with Prof. Lankester, that the investigation does not satisfy the canons of scientific inquiry. The hypothesis on which it appears to me to be based is, that the mean configuration of any organism at any moment is an optimum. In order to test that by the statistical method, the choice of measurements is a mere matter of convenience.

W. T. THISELTON-DYER.

Kew, August 29.

Thermometer Readings during the Eclipse.

I STARTED on July 30 in the *King Harold*, and arrived at Vadsö on August 6. On board this vessel, amongst others, were Prof. Rambaut and Dr. Hugh R. Mill, of the Geographical Society, who I see has sent a note which appears in NATURE of August 27, as to some observations of temperature he took during the eclipse. I was constantly with Prof. Rambaut on the island at Vadsö, and he particularly requested me to observe the temperatures of sun, and shade thermometers during the eclipse at the position he had taken for his observations, which were specially directed to the degree of polarisation of different parts of the corona. I enclose a diagram of my observations, which Prof. Rambaut has suggested I should send to NATURE, should you think they are worth recording. The fall of the sun thermometer (which unfortunately was fully shaded by cloud) was, from 4h. 10m. to just after totality, 2°, and its recovery

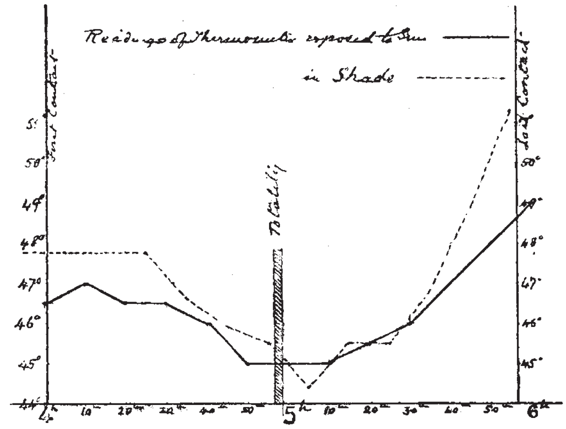

Diagram of observations of sun, and shade thermometers during the eclipse of the sun, August 9, taken at Vadsö.

from that point to 5h. 56m., last contact, was 3°·6. The shade thermometer showed greater variations, viz. a fall of 3°·35, and subsequent rise at 5h. 50m. of 5°·6.

H. WOLLASTON BLAKE.

8 Devonshire Place, W., September 3.

Sailing Flight.

MR. PEAL (NATURE, vol. liv. p. 317) having again brought up this matter for discussion in the columns of NATURE, I would like to make a new suggestion concerning it, which I have long had on my mind. It will be remembered that Lord Rayleigh (NATURE, vol. xxvii. p. 534) assumed an increase of wind-velocity with altitude to explain the facts of circular soaring, and that quite recently Langley (*Amer. Journ. Sci.*, vol. xlvii. p. 41) has tried to explain the same phenomenon by the assumption—supported in his case by direct observation—that the velocity and direction of the wind is subject to great and rapid changes. Concerning this latter statement, I must say that although in a thunderstorm great irregularities can be observed in the upper air-currents, the shape and relative constancy of small clouds in fine weather seem to show that under ordinary conditions the upper air-currents are much steadier than Langley assumes, and that, therefore, soaring birds can by no means always depend on the presence of wind-irregularities sufficiently great to sustain them. Although no doubt wind-velocities generally increase with altitude, I do not believe that such an increase will *always* be present, nor that it will, when present, be usually sufficiently great to produce the force necessary for raising a bird. We observe, however, that birds do soar nearly always, perhaps even more frequently in fine weather, when the currents are more steady, than in rough weather, when they are more irregular.

Under these circumstances it seems to me that neither Lord Rayleigh's nor Langley's assumptions concerning the source from whence these birds derive the power of overcoming gravity can be correct. It seems to me, doubtless, that a steady horizontal wind of equal velocity in different altitudes *does* enable them to soar and to rise. It is remarkable that this soaring without loss of elevation is always accompanied by *circling*. Elevation is not known to occur without circling, as it might if Langley's views were correct. Were the bird attached to the earth by a string like a kite, it could be and, if the wing-planes were placed in proper positions, would be sustained and raised by a purely horizontal and steady wind. Now it seems to me that the *circling replaces the string*. A circling top retains its position on account of the force in its rapidly circling parts. Could not the soaring bird produce—through circling—a similar stability which, acting like a kite-string, would enable it to oppose itself to the wind, and thus convert the horizontal wind-force partly into a vertical, lifting force? Mr. Peal, in his last letter (*l.c.*) very correctly remarks that the connecting-

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