be the value of α. This means, in the case of the earth, that a ray starting at an angle of elevation of more than 1°·5 cannot return to the earth if the above law for K holds. If, on the other hand, the signs of both ρ and α are changed, rays at any angle would eventually return.

The equation, when we take account of the earth's curvature, is

$$\cos\phi/\cos\phi_0 = R[1+(1-e^{-\alpha y})\rho/2]/(R+y),$$

where ϕ is the angle of elevation of the ray at any point, y the distance of the point from the surface, and R the radius of the earth.

When the ray is parallel to the earth's surface $\phi = 0$. Therefore

$$\sin\phi_0 = 1 + y/R - (1-e^{-\alpha y})\rho/2.$$

From this equation conclusions can be drawn similar to those obtained above when considering the earth's surface as plane.

We thus arrive at the result that the general condition under which a ray can return from the upper atmosphere is that the second differential of the dielectric constant with regard to height should be negative. In the case of the earth's atmosphere the density, according to the assumption given above, varies in such a manner that this differential is positive. The conclusion does not hold for rays at angles of elevation of less than 1° or 2°.

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Jan. 28.

Melanism in the Lepidoptera and its Evolutionary Significance.

IN the interesting article by Dr. J. W. Heslop Harrison in NATURE of Jan. 22 on experiments with Lepidoptera, there appears the following: "Thus we are dealing with a case of evolution directed by the environment, and presumably, therefore, of the Lamarckian order. Naturally, this view has been strongly contested by the opponents of the Lamarckian position, but, let it be emphasised, not one of those so opposed has studied the subject in the field. On the other hand, field workers are unanimous in giving it vigorous support."

I do not underrate the importance of the work of Dr. Harrison, nor do I question the accuracy of his fascinating experiments. But I must confess that I cannot understand the reference to field workers in the passage quoted. Does not the question at issue here concern the interpretation of laboratory experiments? Let us admit that in the industrial areas a tendency for certain Lepidoptera to become melanotic has been clearly demonstrated. Harrison and Garrett have shown that feeding the caterpillars with metallic salts produces the same effect, and that this induced melanism is inherited. There is, however, no evidence in the experiments that a somatic change has produced a germinal change. On the contrary, the melanotic changes did not appear in the first generation treated, but in succeeding generations. It seems just as likely, therefore, that the germ plasm has been influenced before the soma. As Harrison states near the end of his article, the experiments "demonstrate . . . that the germplasm can be influenced by external agencies." They do not supply any further evidence in support of what is usually understood by the term "Lamarckism," although their importance in other directions will be far-reaching.

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Jan. 24.

IF reference is made to my original article, it will be found that in his letter Prof. Dakin simply repeats my own views, as he himself realises when he quotes from my concluding remarks.

This position I emphasise further in a paper with the title "Experiments on the Egg-laying Instincts of the Sawfly, *Pontania salicis* Christ., and their Bearing on the Inheritance of Acquired Characters; with some Remarks on a New Principle in Evolution," which appears in the current *Proceedings of the Royal Society* (B), the 'new principle' being seen in a differentiation of that governing the melanism work from those covered by the term 'Lamarckism.' In that communication, without any possibility of misunderstanding, I state: "Clearly such evolution as is pictured at work here is not of the Lamarckian order; most likely the influences at work act directly and simultaneously on soma and germ alike, or even on germplasm alone, and, indeed, the latter view obtains concrete support from the *Selenia bilunaria* work."

However, in my opinion and in that of many other workers, if a chemical substance ingested with the food can influence the germplasm, then the germplasm can be affected through the soma. In connexion with this I ask Prof. Dakin to consider, in particular, the relation between the two existing in plants, as demonstrated in plants raised from leaf-cuttings and by other vegetative means.

J. W. HESLOP HARRISON.

Changes in the Length of the Day.

THE article by Dr. E. W. Brown on "Changes in the Length of the Day" in NATURE of Feb. 5 cannot fail to attract attention. To avoid any possible misunderstanding I think I ought to explain that the references which Dr. Brown makes to my results refer to my paper, "A Solution of Ancient Eclipses of the Sun," published in the *Monthly Notices of the Royal Astronomical Society*, Dec. 1920, not to my paper entitled "Trepidation" in *Monthly Notices* for Dec. 1926, which Dr. Brown had not seen at the time of writing. In the latter paper I show that if we adopt 4″·8 as the change in the apparent longitude of the moon in a century, due to any acceleration not recognised in the existing gravitational theory or to changes in the length of the day, the Greenwich meridian observations give $1''\cdot36 \pm 0''\cdot15$ as the corresponding change in the apparent longitude of the sun, thus confirming the result which Dr. Brown cites from my work on ancient eclipses. I also find that any correction to the assumed century accumulation for the moon requires a correction of 1/9·5 as much to the deduced accumulation for the sun, so that the latter term is very little dependent on the value obtained for the lunar term.

In the same paper I show that the fluctuations to which Dr. Brown refers are found not only in the longitudes of sun, moon, and planets, but also (1) in the amplitudes of the inequalities produced by the action of Venus in the motion of the earth and of Mars, and (2) in the motion of the equinox. These results are inconsistent with the theory which attributes the fluctuations to a variation in the rate of the earth's rotation. I also find that the Greenwich meridian observations give not 1 in 13·3 as Dr. Brown has assumed, but 1 in 9·5 as the ratio between the fluctuations of the sun and moon.

It is probably too early to give a final explanation of these interesting fluctuations, but it is clear that there is a fluctuation in the total action on the earth, which determines its mean motion, amounting to between one-third and one-fourth of the fluctuation in the action of Venus, but in the opposite direction, so that an increase in the action of Venus is

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