part of the night between the 14th and 15th, as well as throughout the night between the 15th and 16th of this month, beginning the watch at 10.30 p.m. Before that hour observations cannot be made, the radiant point of the Leonids being below our horizon.

A computation of the perturbations of two other stations in the stream, in which we have made use of Dr. Berberich's results to supplement our own, has established a fact which we had anticipated, viz. that different parts of the immensely long ortho-stream have been so variously affected by perturbations that the stream cannot now be a uniform one evenly extended along a portion of its elliptic orbit. We must accordingly recognise that it is more or less sinuous, and that, moreover, the distribution of meteors along it is uneven. All such circumstances introduce further elements of uncertainty into any attempt which we may make to form a forecast.

On account of the abnormal amount of the perturbations within the last thirty-three years, the method by which the prediction was made in 1866 is not sufficient on the present occasion. It was based on the average amount of the shift of the node. If employed on this occasion, it would assign for the shower of this year the epoch 1899 November 14d. 12h., which is almost certainly too early.

London, November 4.

G. JOHNSTONE STONEY.
A. M. W. DOWNING.

Undercurrents.

I AM much obliged to Admiral Makaroff for so courteously answering my queries in my letter to NATURE in the number for August 3, p. 316; and I regret that having mislaid my copy of his book, the "Vitiaz," I have not been before able to reply. On now studying his observations in the Strait of Bab-el-Mandeb, and his remarks in his letter (NATURE, October 5, p. 544), I fear I cannot any more share his opinions than I did before.

My point is that it is not sufficient to ascertain that there is a difference of specific gravity either between the surface water or either side of a strait, or between the surface and lower strata of water in a strait, to be able to come to the conclusion that such difference is the primary cause of surface and undercurrents in opposite directions. It has been shown by experiment that such differences give rise to a slow interchange of water, and to this extent I am of course prepared to agree that differences of specific gravity cause opposing currents; but the currents we are dealing with are of a vastly different character and strength.

I have already pointed out that my observations in the Dardanelles and Bosphorus in 1872 showed that the currents did not always run in the normal directions, and that their variations were traceable to the varying winds; and to that I have nothing to add.

On looking at Admiral Makaroff's density observations in the Strait of Bab-el-Mandeb, I see that the specific gravity varied from 1.0279 at the surface to 1.0292 at 200 metres, while the surface waters of the Red Sea itself and of the Arabian Sea near Sokotra are given as 1.0300 and 1.0279 respectively.

In saying that "here are none of the differences of specific gravity demanded by Admiral Makaroff's hypothesis," I was referring to the great contrast between the difference in the densities of the Black Sea and Mediterranean, viz. .017, and those of the Arabian Sea and Red Sea, viz. .002, and never thought for a moment that the very small variation in density in the latter case could be held capable of setting up currents of $1\frac{1}{2}$ knots in opposite directions at surface and bottom, as found by "Stork's" observations in the N.E. monsoon.

No observations have yet been made on the undercurrent in Bab-el-Mandeb in the S.W. monsoon; but the surface current is known to run in the contrary direction to what it does in the N.E. monsoon, i.e. again with the wind, or out of the Red Sea, and I should be much surprised to find that the undercurrent does not also run in the contrary direction, probably with much greater strength than the surface current, because the great evaporation of the sea has also to be made up.

Absolute proof of the causes of such phenomena as these under discussion comes slowly, and only after laborious observation; but I certainly think that the work of the last twenty-five years has tended to show that the influence of density as compared with wind is insignificant.

W. J. L. WHARTON.
Florys, Wimbledon Park, November 4.

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"Anlage" and "Rudiment."

SOME months ago Prof. Herrick, who is in charge of the department of Neurology in the "Dictionary of Philosophy and Psychology," which I am editing (now in the press of Macmillan and Co.), addressed a circular to various authorities asking their opinion on certain matters of terminology. The results were collated and discussed by Prof. Herrick in the *Journ. of Comp. Neurology*, vii. 3-4, 1898. Among the matters in question was the English equivalent of the German term *Anlage*. Prof. Herrick came to the conclusion that *Proton* and *Rudiment* were more available than any other words suggested (ruling out the use in English of *Anlage* with its German inflexion).

It now happens that the French and Italian committees, who are recommending equivalents, in their respective languages, for the terms in the Dictionary, make reports which I think are of importance. Prof. Delage, of Paris, for the French committee, recommends *rudiment*, and, as it happens, Prof. Morselli, of Genoa, sends in *rudimento* as the preferred Italian term. This agreement—and to say this is my aim in writing—affords a strong argument for the adoption of *Rudiment* in English. It is evident that it would be of immediate and very great advantage—for example, to translators from any one of these languages into any other—if *Rudiment* were made the common rendering of *Anlage* in the three other languages of modern science. The other great advantage would be that we already have the adjective form, *rudimentary*, in use.

Furthermore, the psychologists may use the same term for the German *psychologische Anlage* which has crept into recent German discussions. In English, biologists and psychologists will then have the common term *rudiment* with a well-understood signification. I am recommending this to the committee on terminology of the American Psychological Association, of which I happen to be secretary.

J. MARK BALDWIN.
Oxford, October 22.

Interference Curves depending on Perspective.

CLOSELY allied to the halo round an observer's shadow (referred to in Mr. S. Newcomb's letter in NATURE, October 5) are a number of phenomena due to perspective, which may be seen every day by any one who is on the look out for them.

Among these may be mentioned the dark waves which seem to accompany a traveller when he looks through two series of upright palings which lie parallel to each other and his course; also the patterns like the grain of wood which appear when two superposed sheets of gauze are held against the light.

As these and the like appearances have not, as far as I know, hitherto been looked at from a mathematical point of view except in one instance,¹ the three following examples, which are typical but simple, may be of interest:—

(1) Interference rings due to parallel lines on a spherical surface, and their shadow or reflection on a plane or in a plane mirror.

Let a small part of a sphere of radius r be ruled with equidistant parallel lines, the distance between the lines being small compared with r . Let the convex surface of the sphere touch a plane mirror, and let surface be viewed from a distant point, the line joining the distant point, and the point of contact making an angle i , with the normal to the plane.

Taking the plane containing the point of view and the normal through the point of contact as the plane of reference, let α be the angle between the plane containing a line and its reflection and the plane of reference. Let θ be the angle which a point on the spherical surface distant ρ from the normal through the point of contact subtends at the centre of the sphere (so that $r \sin \theta = \rho$), and ϕ the angle which ρ makes with the plane of reference.

It is plain that where, from the point of observation, any part of any line hides the reflection either of itself or any other line, the field will look brighter in that direction than where the line and the reflection are both visible, and the condition which must be fulfilled in order that one line may hide the reflection of another n lines off is (neglecting second order quantities) that the distance between the hidden reflection and the line reflected (that is, twice the distance of the line from the reflector) multiplied by $\tan i$ should be equal to n times the projection of α on the plane of reference, or in symbols,

$$2r(1 - \cos \theta) \tan i = \frac{n\alpha}{\cos \alpha}$$

¹ Lord Rayleigh's "Theory and Manufacture of Diffraction Gratings" (*Phil. Mag.*, 1876).