is to be remarked that the efficacy of the course he lays down must depend very largely on the adjustment to it of lecture and tutorial instruction. How successfully this difficulty has been met in the University of Chicago we have not the means of judging. The book as it stands leaves the reader under the necessity of constructing in imagination the whole course of lectures or preferably "conferences" to which it is essentially related, and it is hardly likely to be put in use by English teachers unless they are prepared to recast their oral teaching to suit it.

The particular order in which the topics of general chemistry are to be treated, the particular points to be elucidated by the student's own experiments, are matters which may afford room for endless choice, but after that, in the point of method, there is a clearer right and wrong to choose between. We believe that some chemists may take exception to Dr. Smith's order and choice of topics, but his method of bringing them before the student will probably meet with general approval. He strives throughout to cultivate the investigator's attitude of mind, bearing in mind however that it is impracticable for the laboratory training of a chemist to be wholly carried out on this plan. The text is interspersed with question marks and with parenthetical injunctions to the student to interpret and correlate his facts.

The exercises are all drawn from inorganic and physical chemistry, and include an elementary study of the cardinal points of theory. We can well believe that this course of practical work, combined with properly conducted class meetings, will furnish a much more effectual introduction to the study of chemistry than students ordinarily obtain in Universities, where the continuous expository lecture to junior classes, unlimited in size and containing all sorts and conditions of students, is still the customary, if not the inevitable, method of procedure.

A. S.

Ueber den Habitus der Coniferen. Von Dr. A. H. Burt. Pp. 86. (Tübingen: Verlag von Franz Pietzcker, 1899.)

THIS inaugural dissertation is mainly concerned with an analysis of the forms exhibited by the different groups of conifers. Following Vöchtung, the author recognises two principal types—the monocormic and polycormic respectively. The former is characterised by the presence of a decided main axis, the lateral axes being dominated by its growth; the common spruce fir is an example of a monocormic conifer. Polycormic forms are met with in cypresses and junipers, in which the lateral branches are not all reduced to subordinate and graduated positions; whilst in the cedars, forms are met with which combine the characters of both of the principal types.

Elaborate tables of measurements of the relative lengths of main and lateral shoots, and of the angles made by them, are given in the text, and clearly bring out the factors on which the shapes of conifers depend.

Beasts: Thumb-nail Studies in Pets. By Wardlaw Kennedy. Pp. xvi + 152. (London: Macmillan and Co., Ltd., 1899.)

THE spirit of a true naturalist prevails throughout this book. The author records his experiences with a number of uncommon animal pets, among which were a young crocodile, a python, an armadillo, and a mongoose. His observations are of real scientific interest, and his humorous descriptions are pleasant to read as well as instructive. Though natural history cannot be learnt from books, the boy who reads the essays in this volume will be encouraged to observe the habits of animals for himself, and will thus learn to depend upon the evidence of his senses rather than to trust upon second-hand information. The book would be an acceptable Christmas present for any boy interested in natural history.

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LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Next Week's Leonid Shower.

THE anticipated return of the great Leonid shower within a few days is looked forward to with so much interest that it appears desirable to examine what data we possess for forming a forecast in relation to it.

In a paper on the perturbations of the Leonids, which is published in the *Proceedings* of the Royal Society for March 2, 1899, we gave the perturbations which have during the last thirty-three years affected those meteors which are situated near a particular station in the stream, namely the station through which the earth passed in 1866 and of which Adams had computed the orbit. It will be convenient to speak of this position in the immensely long stream of ortho-Leonids as station A.

By these calculations it was ascertained that this portion of the stream has been subjected to an unusual amount of perturbation during the revolution which has occupied the last thirty-three and a quarter years; so much so, that the advance of the node, upon which the epoch of the coming shower largely depends, has during this revolution been of more than three and a half times its average amount. Its having had this exceptional value has been caused by unusually close approaches of the great planets Jupiter and Saturn to the meteors during this revolution.

Another effect of these abnormal perturbations has been to shift the position of the ellipse in which station A moves in a direction perpendicular to the earth's path, so that the point in which it pierces the plane of the ecliptic will, on the 15th of the present month, lie inside the earth's orbit at a distance from it of 0.0141 of the earth's mean distance from the sun. This would be a sufficiently large displacement to carry the stream entirely clear of the earth, if it were a mere cylindrical stream; in which case we should have no great shower this year. But we have satisfied ourselves, by an examination made by one of us into the dynamical conditions which prevailed when the Leonids were drawn by the planet Uranus into the solar system, that the stream is not thread-like but strap-shaped, so that its intersection with the plane of the ecliptic is an oval of some sort—probably a long oval—of which the longer axis originally lay nearly perpendicular to the earth's path. On account of this oval form of the section of the stream, the earth is likely to receive one of the great showers this year, notwithstanding the fact that the situation in the stream through which the earth passed in 1866, will on its return pass at some distance from the earth.

If the longer axis of the oval now lay perpendicular to the earth's path, the most probable epoch for the middle of the shower of this year would be 1899 November 15d. 18h., which epoch we offered with careful reservations in the paper which is above cited. A further examination, however, has shown us that perturbations have been acting on this oval ever since the Leonids became members of the solar system, tending very slowly to rotate its major axis in a retrograde direction through an angle which will ultimately become nearly a right angle. The data at our disposal do not enable us to compute how far this retrograde shift has carried the axis in the seventeen centuries during which perturbing forces have been acting upon it. Under these circumstances we must have recourse to observation to ascertain the amount of the shift. A laborious attempt to estimate it in this way has furnished 52° as the angle of shift, which would indicate that the epoch for the shower of this year is as much as twenty-two hours earlier than 1899 November 15d. 18h., that is it would bring it back to 1899 November 14d. 20h.; but we do not attach any value to this particular determination, inasmuch as the data which are as yet at our disposal are too uncertain for us to rely on them. What appears tolerably certain is that some shift of the position of the oval section of the stream has taken place, and that the middle of the shower is accordingly likely to come earlier than 1899 November 15d. 18h., probably some hours earlier, and possibly a considerable number of hours. Under these circumstances it appears desirable that a watch shall be maintained in the latter

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 a on the plane of reference, or in symbols,

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

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