(2) Now apply internal pressure. This distends the tube, stretching the roof and floor. The inner wall is compressed with a longitudinal thrust, and the outer wall stretched with a longitudinal tension, but the change in the diameter AB, or in the diameter AA' , will be practically unobservable.

The action on the original gauge and its enclosed fluid of the added part and its fluid, consists now of the forces indicated in the figure, and which amount, as I have in my previous letter pointed out, to a couple (counter-clock-wise in the present figure).

(3) Now replace the ends at AA' and BB' (this makes no difference in the equilibrium), and holding AA' fixed, remove the added part.

The gauge will now uncurl, for we are removing the counter-clock-wise couple necessary to maintain equilibrium. Or, to put it in other words, the outer wall ACB, on being released from the tension at B shortens, while the inner wall being released from the pressure at B' becomes longer, thus causing the gauge to uncurl.

As to Gauss's purely geometrical theorem, I fail to see how it is to be of any use in the analysis of the forces, which I take to be the real problem. All that Gauss says to us by his theorem is, "Pure bending in your gauge means uncurling; if, therefore, you can prove that the forces are such as to produce pure bending, you prove that they produce uncurling." But this is exactly what we cannot prove. Indeed, it is admitted that the bending is not pure. And it is, I think, of no use to urge, with Lord Rayleigh, that the bending is *nearly pure* on account of the comparative inextensibility of the material, for that argument would apply equally to the gauge with both ends fixed, or to a complete annulus which obviously cannot uncurl. In fact, if we could go back to Gauss and ask, "Is it any use showing that the bending is 'nearly pure'?" he would ask us what we meant by "nearly," and before we could answer that we should have to analyze the whole action. It is for these reasons that I consider the reference to Gauss's theorem not only unfruitful but misleading.

If we apply the method I have suggested to a tube of elliptical or other than rectangular section, we see that unless longitudinal stresses such as I have dwelt upon would exist in the walls were the annulus completed, the distended gauge will not uncurl on the removal of the added part, and the only reason for considering the curvature of either wall in a plane perpendicular to the circular axis, is that such curvature may, on account of the properties of the material by which it is able to distribute stress in different directions, lead to additions to or subtractions from (and conceivably therefore reversals of) the longitudinal thrust or tension that would exist in a tube of rectangular section. But this is obviously a question of the structure of the material and not of pure geometry.

A. M. WORTHINGTON.

R.N.E. College, Devonport, May 14.

A Subject-Index and the Royal Society.

THE following brief account of an offer I have made to the Royal Society will, I think, prove of interest to the readers of NATURE, and especially to those correspondents who have emphasized the importance of a subject-index for the progress of science in all its branches.

Upon the conclusion, last autumn, of five years' work, during which my "Epitome of the Synthetic Philosophy" was compiled, I commenced to look for some literary work which would be of undoubted *practical* service to science, and which would if possible aid its further development. The articles and numerous letters then appearing in your columns urging the importance of a subject classification of the memoirs arranged under the authors' names in the "Royal Society Catalogue of Scientific Papers," led me to discuss the advisability of my undertaking such a proceeding with several friends upon whose judgment I could rely; with the result that, one and all agreeing upon the value of such a work, I wrote to Sir G. G. Stokes, P.R.S. (to whom I was directed), offering to compile the manuscript of the greater portion of such a work upon the condition that, as soon as the manuscript was completed and approved by the Council of the Royal Society, the Society should guarantee all expenses of print and publication. I was forced to say "the greater portion" of such a work ("70 to 80 per cent."), for examination of a large number of titles had shown me that a certain percentage of them could only be correctly indexed by specialists in their own departments, a fact which is emphasized when we call to mind that a title may be in any one of eight European languages.

NO. 1075, VOL. 42]

After several interviews with Sir G. G. Stokes, and a somewhat protracted correspondence, I agreed to arrange a sample index of some 2000 entries upon a plan suggested by him, and warmly approved by that eminent bibliographer Dr. Garnett, of the British Museum, the plan being to take the leading word or words in the title of each paper, with a reference to the volume, page, author's name, and number of the paper, in the Royal Society's Catalogue, for subsequent arrangement in alphabetical order, by which means the subject-key would occupy but a quarter of the bulk of the Catalogue as now published. It would extend—that is, approximately—to three quartos of the size of the present volumes, in similar type, &c.

In the early part of May this plan was discussed by the Catalogue Committee of the Royal Society, when the following resolution was passed:—"That the offer of Mr. Collins be declined, and that the President be requested to convey to Mr. Collins the best thanks of the Committee for the trouble which he has taken."

The foregoing account will be sufficient to show that, contrary to an opinion expressed more than once in these pages, something more is needed than an offer to compile the subject-index. Were the manuscript now completed, and approved by the Royal Society, there would still be wanting a sum sufficient to bring it before the public.

In conclusion, I should like to express my warm thanks to Sir George Stokes for the kind and courteous way in which he has assisted me in my endeavour to develop what I am still convinced would be of immense service to science in all parts of the civilized world; and not only to science, but to many industries besides. For would not the chemical manufacturer and the dyer profit by a complete list of all the papers that had been written on the coal-tar colours; the agriculturist, by knowing the researches which had been undertaken to ascertain the nutritive powers of the bones and phosphates, and the fattening properties of the various cakes and foods; the engineer, the analyses of iron and steel with their accompanying properties; the physician, the physiological action of the various drugs; and the electrician, all the papers, for instance, which had been written upon that little understood subject, induced currents? Finally, how many millions might have been saved in the construction of harbours all over the British Empire had all the scattered information upon the flow of water in rivers and tidal estuaries been so gathered together as to make reference possible, not to say easy?

F. HOWARD COLLINS.

Churchfield, Edgbaston.

Stream Lightning.

IF a candle-flame is put between the poles of an electrical machine, while it is giving rough angular sparks, the discharge at once changes into a smooth single line of very easy curvature: it suggests the difference between sinuous and stream lightning: it is not merely that the spark is as if shortened by the conducting flame; the whole nature of the discharge is changed. If the flame is held two inches beneath the poles, the spark will go down to it.

W. B. CROFT.

Winchester College, May 30.

Atmospheric Circulation.

ON March 9 and 10, 1887, the barometer rose to 30.92 inches over Iceland—a very exceptional height for that locality at that time of the year. The United States daily maps of the northern hemisphere show that a storm to the southward of this great anticyclone was carried westward a distance of over six hundred miles within twenty-four hours, in a manner similar to that in which West Indian hurricanes follow the course of the trade winds in August and September, although this storm was located in latitude 40° N., or in the usual situation of the anti-trades. Other instances of a similar character have been noted, but this one was unusually well defined, and throws much light upon the laws governing the atmospheric circulation.

Lyons, N.Y., May 20.

M. A. VEEDER.

Testing for Colour-Blindness.

IN answer to Prof. Lodge's query (May 29, p. 100), why those concerned in testing for colour-vision do not avail themselves of instruments like Lord Rayleigh's, having tested some thousands in this city, I may state that experience has shown that they are

not suited for testing uneducated persons. A similar instrument, introduced by Chibret and Meyer, of Paris, is to be found in ophthalmic hospitals.

I may further remark that I do not consider any test satisfactory unless made by an ophthalmic surgeon, as he alone is accustomed to deal with such people every day of the week, and can alone eliminate such errors as refraction-disease and stupidity.

D. D. REDMOND.

14 Harcourt Street, Dublin, May 3.

The Green Flash at Sunset.

YOUR correspondents (vol. xli. pp. 495, 538) seem to imply that this phenomenon is only seen at sea, but I observed it on May 17 while walking from east to west, near Worms Heath (Worlingham, Surrey). It had been an exceptionally fine day, since the morning, and about 8 p.m. there was not a cloud in the sky, except to westward, where strips of cloud were rapidly forming, and covering up the glow of sunset; the sun had sunk behind a hill, when, suddenly, my companion and I both saw a flash of green light against the thickest cloud; it lasted 1 or 2 seconds, just long enough for there to be no doubt about it. We compared it to the glare thrown by "green fire," extending over an area whose diameter appeared about four times that of the moon.

At 12 p.m. the same night it was raining.

I think this observation definitely negatives the sea-wave theory, while the appearance was seen at least in association with the condensation of aqueous vapour. Perhaps the reason it was not *bluish-green* was that this vapour absorbed the blue rays?

T. ARCHIBALD DUKES.

16 Wellesley Road, Croydon, June 2.

THE THEORY OF SCREWS.¹

THE book before us, a large octavo volume of over 600 pages, gives in a connected form the results of Sir R. S. Ball's investigation in the theory of screws, as contained in his "Theory of Screws" and a series of publications in the Proceedings and Transactions of the Royal Irish Academy. But as its scheme is that of a text-book on theoretical mechanics, it begins with a chapter on the postulates and methods of mechanics; whilst chapter vii. is on the theory of moments of inertia; chapter viii. on impulsive forces capable of imparting to a rigid body a given state of velocity; and chapter x., on kinetic energy, contains a number of propositions from analytical dynamics. Here expressions for the kinetic energy, for its change in consequence of an impulse, Lagrange's equations of motion in generalized co-ordinates, Hamilton's principle of least action, and various other propositions, are developed in the usual form—that is to say, without the use of screws. The rest of the book relates to the theory of screws and its applications. This alone, as forming the characteristic feature of the book, concerns us here, and of it we shall try to give an outline.

In order not to be unintelligible to those who have no knowledge of Ball's creation, it will be necessary to begin with the very elements of the subject; and in order to form a just idea of the scope and importance of the new method, it will not be sufficient to give a sketch of the results obtained—it will be necessary to take a wider view of the subject. We shall then be able to form some idea of the inherent capabilities of the theory. These I believe to be very great—very great indeed. One of its peculiarities lies in this, that all the results obtained in modern algebra and geometry, as distinct from analysis, seem to be directly applicable to it.

Friends of synthetic geometry and of graphical methods, too, will find here a wide field for investigations. Grassmann's "Ausdehnungslehre" has already been pressed into its service, and the theory of vectors and quaternions

is easily applicable. Clifford, in fact, has generalized the latter theory into that of biquaternions to embrace screws.

Mr. Cartesius, to make use of Sir Robert's personifications, has been dethroned, and Mr. Anharmonic together with Mr. One-to-one reign in his place.

Poinsot, whose investigations form the starting-point of the theory of screws, has proved that a rigid body can always be transferred from one position to any other by a rotation about a certain perfectly determined axis, together with a translation along this axis. These two motions combine to a motion identical with that of a nut on a screw. It is completely determined if the angle through which the rotation takes place, together with the ratio of the translation to the rotation, is given. This ratio—the "*pitch*" of the screw—characterizes the screw. As the motion does not at all depend upon the diameter of the screw, we may suppose this to become infinitely small, and then we have the notion of Sir R. Ball's *screw*.

A screw, therefore, is a line in space which has connected with it a certain pitch—*i.e.* a certain length, as the pitch is a linear magnitude. The compound motion considered is called a "twist" about a screw, and is known if the screw and the "amplitude" of the twist, *i.e.* the amount of rotation, is given. In the same way a system of forces can, according to Poinsot, always be reduced, and that in one way only, to a single resultant and a couple turning about the resultant; and these two dissimilar parts Ball combines to a "wrench on a screw," the line of action of the resultant force being the axis of the screw and the ratio of the moment of the couple to the force giving the "pitch," whilst the magnitude of the resultant force is called the "intensity" of the wrench.

We have thus a new entity—the screw—and its introduction forms the characteristic distinction of the theory. Connected with it is a kinematical and a kinetic entity—the twist about a screw, and the wrench on a screw.

If we now consider a rigid body under the action of any forces, then the latter combine at every moment to a wrench on some screw, whilst the motion itself is always a twist about some other screw. If the body is constrained in any manner, then the reactions due to the constraint will also at every moment combine to a wrench about some screw.

The problem first to be solved is that of the combination of twists and wrenches. Let any two screws, α and β , be given, then wrenches on them constitute together a system of forces, and therefore combine to a wrench on some other screw, γ , which has to be determined. If the ratio of the intensities of the two given screws be varied whilst the screws themselves remain unaltered, then the screw, γ , of the resultant wrench also varies, and its axis describes a surface called the cylindroid. This is a ruled cubic surface which can be described as follows:—Let through a fixed line, l , a plane be drawn, and in it a circle be taken. Let a point, P , move uniformly in the circumference, whilst the plane itself turns uniformly about l , completing half a revolution whilst P describes the whole circumference. The perpendicular from P to l will then generate the cylindroid, and the screw on any generator will have a pitch equal to the length of the perpendicular from P to l . The line l is a nodal line of the surface and perpendicular to all screws on it. All cylindroids are similar, and through any two screws one cylindroid can always be drawn. The projections of all generators on a plane, perpendicular to the nodal line, form a flat pencil in which each ray corresponds to one screw. Also to each point on the circle corresponds one screw. We may here mention that this generation of the cylindroid stands in a very close relation to the plane representation of the cylindroid which is given in chapter xx. For if A , B are the ends of the diameter of the above circle which is perpendicular to the nodal line l , then to A and B correspond two generators of the cylindroid.

¹ "Theoretische Mechanik starrer Systeme auf Grund der Methoden und Arbeiten, und mit einem Vorworte von Sir Robert Ball, Royal Astronomer of Ireland." Herausgegeben von Harry Gravelius. (Berlin: Georg Reimer, 1889.)