daher, wie schon früher die Vorlesungen über die Wärme, so auch jetzt die vorliegenden Vorträge über den Schall unter ihrer besonderen Aufsicht übersetzen lassen, und die Druckbogen einer genauen Durchsicht unterzogen, damit auch die deutsche Bearbeitung den englischen Werken ihres Freundes Tyndall nach Form und Inhalt möglichst entspreche.—H. HELMHOLTZ, G. WIEDEMANN."

Prof. Tyndall's work, his account of Helmholtz's Theory of Dissonance included, having passed through the hands of Helmholtz himself, not only without protest or correction, but with the foregoing expression of opinion, it does not seem likely that any serious damage has been done.]

Apparent Size of Celestial Objects

ABOUT fifteen years ago I was looking at Venus through a 40-inch telescope, Venus then being very near the Moon and of a crescent form, the line across the middle or widest part of the crescent being about one-tenth of the planet's diameter. It occurred to me to be a good opportunity to examine how far there was any reality in the estimate we form of the apparent size of celestial objects. Venus through the telescope, with a magnifying power (speaking from memory) of 135, looked about the size of an old guinea, *i.e.*, of a crescent cut off from that coin. The Moon, to my naked eye, appeared the size of a dessert plate. Having fixed their apparent dimensions in my mind, I adjusted the telescope so that with one eye I could see Venus through the telescope, and with the other the Moon without the telescope, and cause the images to overlap. I was greatly surprised to find that Venus instead of being about one-sixth of the diameter of the Moon was rather more than double its diameter, so that when the adjustment was made to bring the upper edge of the Moon coincident with the upper point of the crescent of Venus, the opposite edge of the Moon fell short of the middle of the crescent, a very palpable demonstration of the fallacy of guesses at size, when there are no means of comparison.

On another occasion a lady was looking at Jupiter through my telescope, and having first put on a power of 60 I changed it for one of 140. To my question, what difference she observed in the size of the planet, she answered, I see no difference in size, but a good deal in brightness. Here the area of the one image was more than five times that of the other.

The fallacy of guesses at size without objects of comparison is most strikingly shown in the ordinary expression of an ignorant observer looking at objects by day through a spy-glass. If you ask, as I have often done, a person unacquainted with optics whether he recognises any difference in size between an object, say a horse or a cow, seen with or without a telescope, he will always answer No, but it (the telescope) brings it much nearer. This, of course, is really an admission of increased magnitude, but the observer is unconscious of it; a horse to him is as big as a horse, no larger or smaller, whatever be the distance.

The assistance which may be derived from the degree of convergence of the optic axes alluded to by your correspondent "T. R." may be something when we know what the object is, or when it is moved to and fro, but if the object be unfamiliar, and there be no standard of comparison, I doubt whether any fair guess could be made.

Suppose all objects had never been seen but at one and the same distance, then an observer looking at a given object without any external standard of comparison, would probably make a fair guess at its size, for the picture on his retina would have a definite size, and his mind would estimate it by relation to other pictures of known objects which he had seen at other times; but as we see all the objects with which we are familiar at all degrees of distance, we have no standard of comparison for an image on the retina.

The common phantasmagoria effect where a figure appears to advance or recede from us though it really does not change its position, but its size is one of the many illusions produced by representing things as they are seen under certain circumstances which have become habitual, and habit interprets the vision. So if one lie on his back in a field, and throwing the head back, look at distant trees or houses, they will appear to be in the zenith, because when we ordinarily look at the zenith the head is thrown back.

Is the apparent size of the Sun or Moon, as expressed in common parlance, anything more than a reference to some standard which we have early adopted, and which, not having any means

of rectifying, we assume. To me the Moon at an altitude of 45° is about 6 inches in diameter; when near the horizon, she is about a foot. If I look through a telescope of small magnifying power (say 10 or 12 diameters), so as to leave a fair margin in the field, the Moon is still 6 inches in diameter, though her visible area has really increased a hundred-fold.

Can we go further than to say, as has often been said, that all magnitude is relative, and that nothing is great or small except by comparison?

W. R. GROVE.

115, Harley Street, April 4

An After Dinner Experiment

SUPPOSE in the experiment of an ellipsoid or spheroid, referred to in my last letter, rolling between two parallel horizontal planes, we were to scratch on the rolling body the two equal similar and opposite closed curves (the *polhods* so-called), traced upon it by the successive axes of instantaneous solution; and suppose, further, that we were to cut away the two extreme segments marked off by those tracings, retaining only the barrel or middle portion, and were then to make this barrel roll under the action of friction upon its bounding curved edges between the two fixed planes as before, or, more generally, imagine a body of any form whatever bounded by and rolling under the action of friction upon these two edges between two parallel fixed planes; it is easy to see that, provided the centre of gravity and direction of the principal axes be not displaced, the law of the motion will depend only on the relative values of the principal moments of inertia of the body so rolling, in comparison with the relative values of the axes of the ellipsoid or spheroid to which the *polhods* or rolling edges appertain; and consequently, that, when a certain condition is satisfied between these two sets of ratios, the motion will be similar in all respects to that of a free body about its centre of gravity.

That condition (as shown in my memoir in the Philosophical Transactions) is, that the nine-membered determinant formed by the principal moments of inertia of the rolling body, the inverse squares and the inverse fourth powers of the axes of the ellipsoid or spheroid shall be equal to zero—a condition manifestly satisfied in the case of the spheroid, provided that two out of the three principal moments of inertia of the rolling solid are equal to one another.

My friend Mr. Froude, the well-known hydraulic engineer, with his wonted sagacity, lately drew my attention to the familiar experiment of making a wine-glass spin round and round on a table or table-cloth upon its base in a circle without slipping, believing that this phenomenon must have some connection with the motion referred to in my preceding letter to NATURE: an intuitive anticipation perfectly well founded on fact; for we need only to prevent the initial tendency of the centre of gravity to rise by pressing with a second fixed plane (say a rough plate or book-cover) on the top of the wine-glass, and we shall have an excellent representation of the free motion about their centre of gravity of that class of solids which have, so to say, a natural momental axis, *i.e.* (in the language of the schools) two of their principal moments of inertia equal. For greater brevity let me call solids of this class uniaxial solids. I suppose that the centre of gravity of the glass is midway between the top and bottom, and that the periphery of the base and of the rims are circles of equal radius. These circles will then correspond to *polhods* of a spheroid, conditioned by the angular magnitude and dip of the spinning glass; to determine from which two elements the ratio of the axes of the originally supposed but now superseded representative spheroid is a simple problem in conic sections; this being ascertained, the proportional values of the moments of inertia of the represented solid may be immediately inferred. The wine-glass itself belonging to the class of uniaxial bodies, the condition that ought to connect its moments of inertia with the axis of the representative spheroid (in order that the motion may proceed *pari passu* with that of a free body) is necessarily satisfied.

The conclusion which I draw from what precedes is briefly this—that a wine-glass equally wide at top and bottom, and with its centre of gravity midway down, spinning round upon its base and rim in an inclined position between two rough but level fixed horizontal surfaces, yields, so long as its *vis-viva* remains sensibly unaffected by disturbing causes, a perfect representation, both in space and time, of the motion of a free uniaxial solid, as *e.g.* a prolate or oblate spheroid, or a square or equilateral prism or pyramid about its centre of gravity, and

conversely that every possible free motion about its centre of gravity of every such solid admits of being so represented.

To revert for an instant to the general question of the representative rolling ellipsoid, I think it must be admitted that the addition of the time element to the theory and the substitution of a second fixed plane in lieu of a fixed centre, considerably enhance the value and give an unexpected roundness and completeness to Poinso's image of the free motion of rotation of a rigid body, of which so much and not altogether undeservedly has been made. From an idea or shadow Poinso's representation has now become a corporeal fact and reality, as if, so to say, Ixion's cloud, in the moment of fruition, had substantiated into a living Juno. I heard the late Professor Donkin, of revered and ever-to-be-cherished memory, state that when as a referee of the Royal Society he first took in hand my paper on rotation, he did so with a conviction that all had already been said that could be said on the subject, and that it was a closed question; but that when he laid down the memoir he saw reason to change his opinion. I owe my thanks to M. Radau and the editors of the *Annals of the Ecole Normale Supérieure* for having been at the pains to disentomb the little-known conclusions therein contained from their honourable place of sepulture in the *Philosophical Transactions*. J. J. SYLVESTER

K House, Woolwich Common, April 2

The Principle of the Conservation of Force and Mr. Mill's System of Logic

WILL you permit me briefly to point out, what has not, as far as I am aware, been yet noticed—the very important modifications of the logical theory of induction resulting from the consideration in reference thereto of the physical theory of the correlation of forces?

As I believe the subject is now more ripe for discussion than it was when, some dozen years ago, I first began to work out the bearings of the higher results of physical research on the general theory of causation, logical, and metaphysical; the following questions which, in the course of a correspondence on this subject, I submitted to Mr. Mill so long ago as 1863, may, perhaps, contain suggestions of thought not unwelcome to some students of NATURE.

"How then," I wrote, "do our new views of force affect the established theory of causation? Now I would rather, if you will allow me, submit the whole subject interrogatively to you, than give dogmatically my own thoughts. And, more particularly, allow me to submit to you these two questions—1st, Whether the physical theory of transformation (and identity) does not necessitate all such logical changes of expression, at least, as may be implied in the abolition of the conceptions of "permanent causes," and of "kinds," as real and absolute existences? And, 2ndly, whether—"if, as I have endeavoured to show, the inductive facts on which are based the principles of conservation and correlation lead to such a more general principle as may be thus expressed, *every existence has a determined and determining co-existence*,—whether, I say, "we are not justified in enunciating such a principle as the complement of that fundamental axiom of our present logic, 'every effect has a cause'?"

I believe I am at liberty to say that, though affirmative answers to these questions would necessitate very important changes in the "system of logic, inductive and ratiocinative," Mr. Mill, as to the first, admitted the necessity of certain changes of expression, at least, and generously encouraged me in the prosecution of the researches indicated by the second question.

Of the results of these researches I shall here only say that, as the axiom, "every effect has a cause," is the foundation of a logic which must be distinguished as a *logic of sequence*, the new axiom above stated may be shown to be the basis of a *logic of co-existence*, of which *Geometry* appears as an example. But as to this, as to the conception of force implied in this idea of co-existence, and as to the bearing of this new conception of force on the speculations with regard to space of a fourth dimension, perhaps I may have another opportunity of addressing you.

J. S. STUART GLENNIE

Athenæum Club, March 30

Dust and Germs of Life

PROF. TYNDALL's exceedingly interesting article in No. 20 of NATURE seems to me to leave unexplained a fact very

familiar to naturalists. It is well known that collections of natural history, say a Herbarium or an Entomological cabinet, will, if left undisturbed for a number of years, and unpoisoned, become infested with animal life, chiefly Acari and larvæ of Coleoptera; and that the surest way of preventing such attacks is thorough ventilation. Now if the floating matter in the air settles so readily after only a few days' stillness, as Prof. Tyndall's experiments seem to indicate, and does not even enter into an uncorked flask, it is out of the question that it can penetrate through the keyholes or chinks of our cabinets. Setting aside the theory of spontaneous generation, we are then forced to the conclusion that this life must arise from germs already existing in the specimens when they are preserved, or in the very limited amount of atmosphere originally confined in the cabinet. Is either of these explanations tenable? A strong argument against the former alternative seems presented by the fact that, as far as I am aware, the same species of *Acarus* infests plants in a Herbarium brought from the most widely diverse localities, an inland meadow or the seashore, the plains of England or the Alps of Switzerland. Can any of your physiological readers throw light on this subject? F. L. S.

Catkins of the Hazel

WHILE looking at some hazel bushes to-day, I noticed that where the red tuft of stigmas was protruded, the male catkins adjacent on the same twig were immature; while, on the other hand the stigmas had fallen, and the fruit was already swelling, where the scales of the male flowers were open to show the stamens.

A week or two back (in another locality) I could not find a single female catkin which had not lost its stigmas; while nearly all the male catkins had opened, and many had shed their pollen.

Is this always the case with the hazel? If so, it would be a striking illustration of Darwin's aphorism, "Nature abhors perpetual self-fertilisation."

I ought to add, that my observations are not confirmed by the illustrations in the books to which I have access, namely, Balfour's "Class Book," Lindley's "School Botany," and Lemaout and Decaisne's "Traité de Botanique." In all these, a female catkin with its tuft of stigmas is represented on the same twig as a bunch of fully developed male catkins.

Will some one of greater experience inform me if I am in error as to the above facts? MARCUS M. HARTOG

University College, London, March 24

ANCIENT BRITISH LONG BARROWS

II.

THE chambered long barrows of North Wilts, Somerset, and Gloucestershire differ, as a rule, but slightly in external form from the simple or unchambered long barrows of South Wilts and Dorset. They are, however, generally of somewhat smaller dimensions, being from about 120 to 200 feet in length and from 30 to 60 feet in breadth. The side ditches characteristic of the unchambered barrows are seldom to be met with, but the margin of the grave-mound is, or rather was, usually defined by a low wall, built of loose tile-shaped fragments of oolitic stone. In some cases, as at West Kennet (see fig. 1), there is good evidence that the mound was originally surrounded by a series of obelisks of sarsen stone, the intervals being filled up with the usual dry walling just described. Sometimes, too, large monoliths or triliths are found at the broad end of the tumulus. As regards orientation, or position in reference to the points of the compass, the direction of east and west commonly observed in the simple barrows prevails in four out of five cases with the chambered barrows; and as in the former class of monument the interments were at the eastern end, which is also the higher and broader, so likewise do we find that the stone chambers or cists occupy the same position in the chambered barrows.

In internal structure the chambered barrows exhibit many varieties, but three principal types are recognised by Dr. Thurnam, viz.—(1) those in which the chamber