became inapplicable, what assumptions became invalid?—why, if Dr. Watson's method of generalised coordinates were valid, the ether, the solar system and the universe generally were not subject to the Maxwell-Boltzmann law, so that the mean kinetic energy of every coordinate in the universe should be the same and so on, insisting that what he wanted to know was *why the theory failed, what* assumptions were invalid.

After the other speakers had concluded, Prof. Boltzmann arose to reply, and he took up a perfectly logical position. He said that the theory as it left his hands was a mathematical theorem, a piece of pure mathematics, and that it was for *physicists* to say how far it applied to gases—that the reason of any disagreement between the theory and the facts was "a mystery, as Lord Salisbury had said."

That appears an unassailable position, and the only misapprehension which, so far as I can see, could arise, would be that Boltzmann had admitted not only that his work was a piece of pure mathematics, but that it was *nothing more*, a bare theorem without the promise that future adaptations would lead to an even closer accord between the theory and the facts. If such an impression as that has got abroad, Mr. Bryan has done good service in calling attention to the matter.

There seem many difficulties about the suggestion (made by Dr. Larmor at Oxford, and referred to by Mr. Bryan in his letter) that the spectra of gases need not be explained by the Boltzmann law, as they arise not from molecular but from ethereal vibrations set up by the molecules. Surely if so, the molecules cannot be regarded as an independent system, and Dr. Watson's generalised coordinates must include ethereal coordinates also, and the Maxwell-Boltzmann law must be supposed to hold for matter and ether alike, which does not seem to get over the difficulty.

November 10. EDWD. P. CULVERWELL.

Homogeneity of Structure the Source of Crystal Symmetry.

MR. BARLOW'S letter on this subject (p. 58) raises a problem of considerable interest, which may be stated in simple words.

He has inquired in the most general manner possible how anything can be uniformly distributed in space so as to constitute a homogeneous system; the word homogeneous may be taken to signify that round any one member of the system the distribution of the remainder is the same as round any other. It is not necessary to say that the units of which the system consists are figures or solids, but merely that, whatever the unit may be, it is homogeneously repeated.

Now repetition may conceivably take place by sliding the unit from one position to another, by rotating it about an axis, by reflecting it across a plane, or by a combination of these processes; in other words, by translation, rotation, and inversion. If the last process be excluded, we cannot arrive at all the types of symmetry presented by crystals; if it be included, we obtain all those types and no others. Therefore the crystal structure is one in which this process is operative. Mr. Barlow himself does not include inversion as a mode of homogeneity, but regards it as an additional property possessed by some crystal structures. Earlier writers have specialised the problem by taking a particular unit. Bravais and Sohncke, for example, to whom the modern treatment of the subject is entirely due, have investigated systems of points. Now the reflection of a point is an identical point, so that it is useless to introduce the principle of reflection or inversion as distinct from translation in order to derive any *one* point of such a regular system from another. The same is true of spheres and many symmetrical figures, and unfortunately molecules have usually in such investigations been treated as points or spheres or symmetrical figures.

Mr. Barlow does not consider that his solution of the geometrical problem supplies a theory of crystal structure or settles the question whether the seat of the symmetry is in the arrangement or in the configuration of the molecules.

But it appears to me that a step of very great importance has been made, for, surely, these investigations prove that the symmetry of such a structure *can* be entirely explained by the arrangement of the units. I would go farther, and ask whether the result does not suggest that the units which determine the symmetry of a crystal are units capable of repetition by the processes of translation, rotation, and inversion. If this be so, we are not justified in treating them generally as points or as symmetrical figures.

Many things besides unsymmetrical figures can be conceived which are capable of such repetitions; for example, a solenoid, a vortex motion, a system of forces in statical equilibrium.

I would add that Mr. Barlow's investigation cannot be said either to support or to contradict the theories of Fedorow and Schönflies; it is, as he remarks, purely geometrical, and in this respect is identical with their researches, and leads to the same results. It is true that Fedorow has proposed a theory of crystal structure, but this is only an application of the geometrical principles which he had previously established.

H. A. MIERS.

British Museum (Natural History), November 19.

Gravitation.

I REGRET that I cannot agree with Dr. Joly's suggestion (*vide* p. 57 *supra*) that the curious adhesion which I observed between solids immersed in a stretched liquid, lends itself to any explanation of gravitation on the lines that he indicates. The phenomenon is, and was described by me as, one of *adhesion*, and not, as Dr. Joly puts it, of *attraction*, for there was no evidence of any approach of bodies separated by a measurable thickness of liquid, and there is, further, no reason to suppose that the phenomenon would occur unless the medium were already modified in the neighbourhood of the solid surfaces, *i.e.* unless a condition which we may provisionally ascribe to gravitation already existed. For this reason Dr. Joly's suggestion appears to me to be an invitation to argue in a circle.

If there were evidence, which there is not, that the ether round celestial bodies was modified to a great distance, the suggestion would, I think, be legitimate, but it would then be necessary to explain the modification.

Devonport, November 18. A. M. WORTHINGTON.

The Foucault Pendulum Experiment.

PROF. GREENHILL gives currency to quite an erroneous idea in last week's NATURE (p. 50). He says "in the Foucault experiment of the pendulum which shows the rotation of the earth, the slightest current of air will destroy and reverse the desired motion; so that it is advisable in showing the experiment to have an elastic ball concealed in the palm of the hand, which can send a slight current of air on the bob of the pendulum, and thus accelerate the initial precession of the plane of the vibration so as to gratify the eyes of the audience and diminish their impatience at the slowness of the motion." If Prof. Greenhill will go to the Western Galleries of the South Kensington Museum any day, he will be able to see a Foucault pendulum fulfilling its purpose without being particularly protected from draughts, and without the accessory puffs to which he refers. The pendulum is suspended in a place where people are continually passing to and fro, yet its plane of vibration always rotates in the same direction as watch-hands, or rather the table under the pendulum turns in the opposite direction. I have watched the pendulum dozens of times without seeing it fail.

G. A. R.

November 19.

An Observation on Moths.

AN experiment was tried in 1894, on a number of pupæ of *Samia promethea* and *Samia cecropia*, which brought out a point of which I have seen no mention. When the moth is almost ready to burst through the thin shell which encloses it, this outer skin becomes dark-coloured and friable, and the insect can often be seen moving within.

If the enclosing envelope is then removed with a scalpel and forceps, the moth struggles out, apparently as lively as when legitimately hatched.

The only hitch in the proceedings seems to be in the non-expansion of the wings, the development of which usually takes place at once. The moth crawls about, like a forlorn penguin, for a period varying from one to three days, when the wings seem to realise the absurd state of affairs, and make a brave effort to fulfil their part of the contract. The effort, however, is only partially successful, for owing to their dry condition the expansion is irregular and incomplete, and the poor moth remains a helpless cripple.

This would seem to demonstrate that the wings do not mature as rapidly as the rest of the body, and that until complete maturity is reached, no effort towards expansion is made.

L. C. JONES.