

be considered to be in simultaneous collision with one another. This may be where the spectrum crux fails. Perhaps somebody would be so very kind as to point out where exactly in these generalised coordinate investigations the postulate of collisions in pairs is used, and so save lazy people like me the trouble of hunting it up.

This raises the third point as to how this difficulty about the spectral lines is to be surmounted. I cannot follow either Mr. Bryan's, or what I understood to be Mr. Larmor's view, that any help can be got by supposing spectral lines to be due to electromagnetic vibrations. The example Mr. Bryan gives of smooth solids of revolution is quite beside the point. In this case there is *no interchange of energy* between rotation round the axis of revolution and the other degrees of freedom. This is quite contrary to what we know to be the case in respect of ethereal and molecular energy. We know that radiations cause bodies to become cooler, and therefore there is interchange of energy. This could not be otherwise, as is evident from what we know of the mechanical forces—electric, magnetic, and electromagnetic—that interact between matter and ether. It is rather hard for Mr. Bryan to say that the *onus probandi* lies with physicists to explain exactly *how* transference takes place; surely the fact that transference does take place is sufficient to prove that a complete theory should take in both sets of coordinates—ethereal as well as material—and I should have thought that those formidable arrays of $dp_1 dp_2 \dots dp_n$ &c., of coordinates and momenta to the n th, with dots between to signify their indefinite number, should include everything of this kind that could possibly be required. Here, however, the postulate of collisions in pairs entirely breaks down, and thus shows a way out of this spectral difficulty. A second way was suggested to me, by whom I forget, at Oxford, namely, that the complicated systems of lines that we see in the spectrum—of iron, for instance—are so connected as to their amplitudes, periods, and phases, as to represent only a single coordinate. Anybody who has tried to expand a simple function in Fourier series will easily understand how a very simple motion might produce a fearfully complicated spectrum.

It seems, then, to me that the questions which need solving in our study of the dynamical foundations of thermodynamics at present are (1) how to explain spectra, and (2) how to deal with several bodies in simultaneous collision?

GEO. FRAS. FITZGERALD.

Trinity College, Dublin, December 19, 1894.

THE difficulties in the way of harmonising the spectroscopic phenomena of heated gases with the conclusions of the Boltzmann-Maxwell theorem of distribution of kinetic energy, appear to me to have been exaggerated.

This theorem asserts, as a purely dynamical proposition, that a very large number of, say, billiard balls, perfect as to sphericity and elasticity, having been set in haphazard motion in a space bounded or interrupted by perfectly elastic rigid surfaces, if the volume of free space be sufficiently large as compared with the total volume of the balls¹ will, if left to itself, tend to a certain state of density and velocity distribution, and having reached this state, will remain in it permanently. The billiard balls may be replaced by rigid bodies of any form, or indeed by material systems capable of any changes of shape or motion of the parts among themselves, and a corresponding proposition still prevails—one general property holding good in all cases, viz. that if u, v, w be the motions of translation of each system as a whole, m its mass, T its total kinetic energy, and n the total number of its degrees of freedom inclusive of the 3 of translation, then in the permanent state

$$\frac{m\bar{u}^2}{2} = \frac{m\bar{v}^2}{2} = \frac{m\bar{w}^2}{2} = \frac{T}{n},$$

where $\bar{}$ denotes mean value throughout the whole medium.

Primâ facie there is a discontinuity in this abstract proposition unfitting it for the basis of physical investigation, e.g. the smallest possible want of perfect sphericity in the billiard balls would appear to effect as complete a change in the physical properties of the medium as if each ball became an ellipsoid or tetrahedron, the mean $\frac{m\bar{u}^2}{2}$ being changed *per*

saltum from $\bar{T}/3$ to $\bar{T}/6$, and the difficulties introduced by the spectroscopy are founded upon this discontinuity. Doubtless we cannot make n fractional, but it should be remembered that the dynamical proposition only speaks of an *ultimate* state, and ignores the rate of approach to that state. The continuity of physical properties is maintained by attending to the continuity of change in this rate. This rate I have estimated for a particular case in the new edition of my short treatise on the subject, showing that there *may* be a *sensible* permanence long before the law of equal partition is established.

And, again, there is yet another point to be considered. The passage from the thermal to the optical properties resembles the passage from mere noise to music. Dynamically it is the passage from irregular, haphazard motion, *heat*, to regular periodic motion, *light*; the former must be decomposed into its equivalent harmonic motions, and the most important terms retained, but there would be no necessary relation between the number of these terms (sensible bright lines) and the number of degrees of freedom of the molecule. H. W. WATSON.

The Horn Expedition to Central Australia.

In your issue of September 27, 1894, occurs a short notice of the work of the Horn scientific expedition to Central Australia. Reference is made therein to the discovery of a "new type of marsupial" by Dr. Stirling. The animal in question was found by Mr. South, a mounted trooper (or rather by his cat, who brought it into the house), at Alice Springs. By him it was presented on our arrival to Dr. Stirling, who had charge of the anthropological work of the expedition, by whom it was kindly handed on to me as officer in charge of the zoological department. The specimen was a male, and being desirous of securing more, I stayed behind the party, and by aid of the blacks procured two more specimens, both of them females. The animal, which lives in holes amongst the rocks and stones, is by no means common, as I had to offer considerable quantities of flour and tobacco to the blacks as a reward for its capture. After a number of them had been out hunting for several days the total result was two more specimens, though as these were females they formed a welcome addition to my zoological collections. As the expression, "a new type of marsupial," gives rise to too great expectations, it may be as well to state that it is merely a new species of the genus *Phascologale*, distinguished, amongst other points, by its remarkably fat tail and by the nature of the striated pads on the soles of its feet. I was able to make drawings of the animal alive, and on showing these to the blacks at Charlotte Waters, some 250 miles to the south of Alice Springs, they were at once greeted with cries of "Amperta," the native name for the animal which they took it to represent. Through the kindness of Mr. Byrne, the head of Charlotte Waters Telegraph Station, I have since been provided with specimens of the "Amperta," which on examination turns out to be the rare form—only as yet, I believe, known from a single specimen—described by Krefft under the name of *Chatocercus cristicanda*, and subsequently placed by Mr. Oldfield Thomas in the genus *Phascologale*.

These two species, and a new one of the genus *Sminthopsis*, which we secured amongst the sand-hills near Lake Amadeus, are the most important finds amongst the marsupials which, owing to the country traversed by us being in the main of a desert description, were by no means plentiful. In this region animals can only be secured in numbers after rain, an experience which, during three and a half months' wandering, did not fall to our lot. However, as one result of Mr. Horn's generous action in equipping the expedition, I hope to be able to give a fairly good general account of the fauna of the central desert region of Australia. Towards the close of the notice referred to, it is said that there is some doubt as to the manner of publication of our results. Mr. Horn's intention is, I believe, to issue a separate volume, the various parts of which will deal with the branches of science represented by the members of the scientific staff as follows:—

Prof. R. Tate (chairman of the scientific staff), geology and botany; Dr. E. C. Stirling, anthropology; Mr. C. Winnecke (leader of the expedition), surveying and meteorology; Prof. Baldwin Spencer, zoology; Mr. J. A. Watt, geology and mineralogy.

The volume will contain an accurate map, compiled by Mr. Winnecke, of the central district drained by the Finke river,

¹ So large that the average number of cases in which three or more spheres are at any instant in collision is infinitely smaller than the number of cases in which two only are in collision.

and will, by means of the reproduction of photographs, illustrate some of the more important physiographic features of Central Australia.

BALDWIN SPENCER.

Palæontology at the Royal School of Mines.

IN reading the excellent review of the biography of Sir R. Owen, which appeared in last week's NATURE, I observe an error which, though small, requires correction. It is stated that when Owen surrendered his appointments at the College of Surgeons he was "enabled to accept the lectureship on Palæontology at the Royal School of Mines, in 1857." The records of that Institution will show that Owen never held a lectureship there, nor was he in any way connected with the School.

The large theatre of the Museum of Practical Geology was frequently employed for other purposes than those of the School, by permission of the Director-General; and it was in virtue of such permission from Sir Roderick Murchison that Prof. Owen used the theatre for the delivery of his lectures on Palæontology to the public, in 1857 and subsequently.

T. H. HUXLEY.

Eastbourne, December 27, 1894.

Eocene Fossils at Murren.

I HAVE read with surprise the extracts from the letter which you have received from Dr. Fellenberg on this subject. Of course it has long been known that there are Eocene strata at Murren. But below them lie calcareous rocks coloured on the Swiss map as Malm. These are so described on p. 211 of the "Livret Guide," published by the International Geological Congress which met this autumn at Zürich. During the subsequent excursion, under the able guidance of Prof. Renevier, Prof. Gollier, and M. Lugeon, we were taken to Murren and shown these rocks, and Prof. Gollier gave us the reasons which had led some geologists to regard them as Trias rather than Malm. It was in these calcareous beds that the layer containing nummulites was met with. The train was just starting, and we had to leave, but the find excited so much interest that M. Lugeon returned to Murren the next day, with some of the party, and verified the exact locality.

JOHN LUBBOCK.

High Elms, Farnborough.

The Use of the Globe in Crystallography.

IN your issue of December 20, Mr. Buchanan revives a graphic method of crystallographic calculation which seems to have been used in the early part of the century (*vide* "Zur physischen Krystallonomie, &c.," Grassmann, 1829, p. 37), and claims that by use of the globe and *microsphere* "every problem in the geometry of crystals can be solved with ease and accuracy."

Crystallographic angular measurements are said to be accurate if subject to a probable error of less than two or three minutes, and descriptions of inorganic substances are nowadays habitually published in which the probable errors are of this order. Although no details of Mr. Buchanan's method are given, it seems inconceivable that any graphic process of crystallographic calculation, involving triangulation on a sphere, could be accurately performed without the use of numberless tedious precautions and large and cumbrous apparatus.

It is usually more easy to grasp a good plane diagram of any solid figure than to understand and follow up explanations on the solid figure itself; the use of the latter is liable to lead to inaccuracy of expression and confusion of thought. Thus, when the sphere is used, the real meaning of the points which Mr. Buchanan describes as cataloguing the edges occurring on crystals, is not at once seen; using the plane projection, it is immediately apparent that these points are characteristic, not merely of the edges, but in a much wider sense, of the zones: they are merely the poles of the zone circles.

Further, the positions of the points representing the corners are dependent on the *sizes* of the faces concerned, which, as we teachers of crystallography are at infinite pains to impress on our students, have no crystallographic signification; these points, then, are not *characteristic* of the corners.

Similarly, the rather complicated piece of reasoning respecting "reciprocal inversion forms" simply yields the well-known result that in the cubic system, the octahedron truncates the corners of the cube.

WILLIAM J. POPE.

Central Technical College, South Kensington.

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"The Zoological Record."

IT has long been a matter of regret that the *Zoological Record* is not sold in separate parts; a specialist requiring any one part being required to pay for all the others, though they may be of no more use to him than so much waste-paper.

In order to remedy, to some extent, this unfortunate state of things, I am proposing (if sufficient support is forthcoming) to purchase the *Zoological Record* as published, and to issue the separated parts to subscribers. With this view, I would ask all those desiring any part (of the volume just issued, or of past volumes) of the *Zoological Record*, to communicate with me as soon as possible, stating which part they would be willing to subscribe for.

Although it is, of course, not intended to make a profit out of this scheme, it will nevertheless be necessary to charge slightly more for the separated parts than their proportionate value, as some parts are almost certain to remain unsold.

I am confident that this scheme will not in any way injure the Zoological Society; in fact, although they maintain the contrary, I am sure it would be to their advantage to issue the parts separately, if necessary at a slightly higher rate. At present subscribers are, I believe, mostly libraries and societies, requiring the whole volume. These would, of course, continue to subscribe for all parts, even if they were obtainable separately; while, on the other hand, all specialists who do not subscribe under the present arrangements would be practically certain to purchase those parts dealing with their own subjects, if obtainable at a moderate cost. If the proposed scheme meets with support, it will go a long way towards proving the justice of the foregoing contention, and in that case it will probably be possible to induce the Zoological Society to grant the concession for which many zoologists have for long been agitating.

Royal College of Science, London.

S. PACE.

Gravitation.

IN answer to Dr. Lodge's letter, I may state that Newton in his "Opticks" (Query 21) asks if an increase of density of the ether outwards from bodies will not account for gravitation, every body endeavouring to go from the denser parts of the medium towards the rarer; and if such increase of density may not, even at great distances, be effective, provided the elastic force of the medium be sufficiently great.

I do not think a tensile ether is contemplated in this theory of gravitation. Prof. Worthington's effect manifests itself only in a tensile liquid, and this constitutes its suggestiveness in connection with the hypothesis of a tensile ether. I have no such definite ideas to advance as are put forward in the "Opticks."

J. JOLY.

Trinity College, Dublin.

The Feigning of Death.

THE curious condition of apparent death, assumed by the English grass snake, which Mr. G. E. Hadow describes (NATURE, December 6, p. 127), is one that I have frequently observed, but have always been puzzled to account for. I hardly think that it has anything to do with simulation, or that it is voluntary, since I have seen snakes so affected when quite undisturbed in their cases. I have also observed precisely the same state in the common Italian snake. In my experience the condition only occurs in fairly hot weather, and when the snake has not fed for some time. This seems to point to a species of fainting fit, and I imagine that it is immediately induced by a disturbance of the cerebral circulation.

R. HARRY VINCENT.

Leytonstone, December 30, 1894.

Peculiarities of Psychological Research.

IN reply to Prof. Pearson: (1) His remark about "scientific acumen" was not made *à propos* of M. Richet's experiments, but of those of the S.P.R.; and hardly any stress is laid on M. Richet's results, either by Mr. Gurney or Mr. Podmore. Mr. Gurney, on the contrary, expressly says: "Clearly no definite conclusion could be based on such figures." But if Prof. Pearson has made experiments which are equally striking in the opposite sense, I wish he would publish them, or communicate them to the S.P.R. (2) There was nothing in my letter to indicate that I under-estimated the importance of "abnormal distributions"; but I asked Prof. Pearson to say whether he