error is once again seen, of mistaking the axis of rotation of the earth for the plane of the equator; but such oversights are easily excused in presence of the collection of a large number of facts, well arranged and tersely expressed. W. E. P.

The Province of South Australia. By J. D. Woods, J.P. With a Sketch of the Northern Territory, by H. D. Wilson. Pp. 446. (Adelaide: C. E. Bristow, 1894.)

THIS account of the province of South Australia, from its discovery to the end of 1892 was, the preface informs us, written under the authority of the Government of the Colony. It may therefore be taken as an authoritative work of quite a different and a better kind than the many descriptions of Australia that have appeared during the past few years. The physical features, fauna, flora, climate and meteorology are fully described, and the story of the explorations of the interior of the continent is full of interest. There is a chapter on the agriculture of South Australia, and one on the minerals in which the province is so wonderfully rich. Those familiar with the history of education in South Australia will remember that prior to 1874 the colony did not possess a university. It was in 1872 that an endowment of £20,000, given by Sir W. W. Hughes, was applied to the founding of two professorships—one for classics and comparative philology and literature, and the second for English language and literature and mental and moral philosophy. Science was benefited shortly afterwards by a like donation from Sir Thomas Elder, to found a professorship for mathematics and another of natural science. The same benefactor gave £10,000 for the establishment of a medical chair in 1883, and £1000 for evening classes; and the Hon. J. H. Angas gave £6000 for the creation of a chair of chemistry, and £4000 for the establishment of scholarships and exhibitions. Though the Adelaide University was incorporated in 1874, the present University buildings were not opened until 1882. The School of Mines and Industries, as it is officially designated, was opened in 1889, and has steadily increased in influence and usefulness since then.

The chapter on the aborigines of South Australia is perhaps the best in the book, and as the author has had more than forty years' experience with the blacks, he writes upon what he is well qualified to describe. Altogether the volume includes much that has not hitherto appeared in print in a collected form, and therefore deserves to rank with the best books on Australia, its people, and its resources.

Measurement Conversion Diagrams. By Robert H. Smith, Professor of Engineering, Mason College, Birmingham. (London: Charles Griffin and Co., Limited, 1895.)

THE scope of this work is described on the title-page as follows:—"Forty-three graphic tables or diagrams for the conversion of measurements of different units, comprising conversions of length, area, volume, weight, stress, density, work; energy in mechanical, thermal, and electrical units; horse-power, and temperature." Only those who are familiar with graphic statics know what can be done by diagrams, but even they will be astonished at the wide range of conversions covered by Prof. Smith's graphic equivalence plates. The diagrams will principally aid the conversion of English and metric measures, and *vice versâ*, but they also represent the relations between different systems of English, and of French, measurement. We have always been attracted by the method of expressing equivalents by means of squared paper, and Prof. Smith's graphic tables have greatly increased our admiration of it.

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.*]

The Kinetic Theory of Gases.

I DO not feel as if those who heard me ask some questions at the British Association at Oxford, about the kinetic theory of gases, exactly understood my difficulties. They are those of an onlooker, and so they may be of general interest. As several of them have been fairly satisfactorily answered, it may be worth while stating the present position of such an onlooker as myself.

In the first place, consider the difficulty as to reversibility and as to the number of possible ways in which a system could be started on a reverse path so as to obtain a *given* initial state. This is, I think, completely answered in the way Mr. Larmor gives in his letter on p. 152. As well as I can recollect, Mr. Culverwell and I had been mutually satisfied by this kind of explanation previous to the meeting at Oxford, and it was not then referred to.

The question of reversibility lately started, as I understand it, has reference to the introduction of the postulate of chance in the deduction of the theorem about H. Mr. Burbury, in his recent letter, has indicated a proof of this theorem, in which he explicitly postulates chances, and so far justifies the possibility of proof on these lines. I understand that Mr. Culverwell is so far satisfied, and only asks for more, *i.e.* an extension of this form of proof to other cases than the simple one of colliding spheres.

Secondly, as regards the solar system, &c., I am not yet quite clear why a finite number of particles moving about for an indefinitely long time does not satisfy the conditions of the problem as usually stated, just as well as a large number of bodies for a short time. As to the necessity for *collisions* among the parts of a system, I cannot see why the earth, moon, Jupiter, and sun are not to all intents and purposes of the generalised coordinates in collision at present and always; and I desired to know why any other kind of collision is required for the application of the investigation. I think I now see, through conversations with Mr. Culverwell, where the existing investigations may fail to apply to solar systems. I may explain my position as follows. It was always, I knew, postulated that more than two particles should not be in collision at once, and I therefore asked how this could be an essential part of the investigation when applied to the case of air near the earth subject to gravitation. I did not see why the *earth* was not (so far as the generalised coordinates investigation was concerned) a particle in collision with every particle of the air during every one of their collisions with one another, and consequently violating the postulate requiring only *two* particles to be in collision simultaneously. I now understand that when dealing with gravitation and such like forces, these are supposed to be directed to *fixed* centres, and that in the case of a large particle like the earth this is very nearly true, but that it could not be even approximately true if we had three fairly equal particles acting upon one another simultaneously. This may also explain why the equal partition of energy does not hold in the solar system where the bodies do not act upon one another in pairs, but are all always subject to one another's action. This, as I understand, is also the reason why the direct distance law is not an exception to the equal partition of energy theorem. It also may explain how we can have water and steam in equilibrium with one another, notwithstanding the apparent *uniqueness* of the Boltzmann-Maxwell solution. From experience it would seem that when we can extend the investigation to the case of several bodies in simultaneous collision, we shall find that there are *three* solutions corresponding to the solid, liquid, and gaseous states. At the same time, some of the very general investigations that seem to me, as a physicist, as if they were intended to apply to complex molecules in collision with one another, and with a partition of energy amongst the atoms, appear to violate the postulate of collisions in pairs; for I find it hard to conceive of these molecular systems of atoms as other than systems, the various parts of which are held together by mutual actions, and which must consequently 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 $dp_1\ dp_2\ .\ .\ .\ dp_n$ &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 balls [1] 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

$$\frac{m\overline{u^2}}{2} = \frac{m\overline{v}}{2} = \frac{m\overline{w^2}}{2} = \overline{\mathrm{T}}/n,$$

where $-$ 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 billard 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 $\frac{m\overline{u^2}}{2}$ being changed *per saltum* from $\overline{\mathrm{T}}/3$ to $\overline{\mathrm{T}}/6$, and the difficulties introduced by the spectroscope 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.

[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.

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 *Chætocercus 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,