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.

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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