

valve engine is well illustrated, and the description does credit to what is probably the most economical steam engine ever designed. A description is given of rotary engines, but none are illustrated. The "Rota" engine, designed and made by MacEwan Ross, of Glasgow, might have been included with advantage. The locomotive is outlined, and the compound type described; but no information as to tests is given, probably because no trustworthy data can be obtained; and as no British Railway Company, with one possible exception, is likely to build any more compound engines, it seems probable they are not the unqualified success they were originally claimed to be, although the Vaucrain system, with four outside cylinders, appears to be a success in the States. But it must not be forgotten that the American rival is a very uneconomical engine when compared with our own.

N. J. L.

OUR BOOK SHELF.

Das Verhältniss der Philosophie zu der empirischen Wissenschaft von der Natur. By David Wetterhan. (Leipzig: W. Engelmann, 1894.)

THIS is the essay which gained the prize of 1000 marks offered, in 1891, by the Philosophical Society of Berlin. It consists of 110 pp., of which about twenty are occupied by notes and abstracts from various writers, in small print.

Naturally, in giving forth his own views, some of which possess considerable originality, the author makes continual and extensive use of the theories of Kant, Schopenhauer, Wundt, Bunge, and others; and one noticeable feature about the work is the full share of recognition accorded to English philosophers and scientists, such as Faraday, Herbert Spencer, Darwin, Romanes, and Huxley. The writer well remarks that the limits of scientific knowledge are everywhere and nowhere.

In the earlier pages the author discusses the relation between the physical and the psychical sides of nature. The theory of the conservation of energy has nothing to do with mental processes: it governs the quantitative relations of all processes of nature, but does not explain their qualitative differences. Sensation, consciousness, motor impulse, are not forms of energy, and do not correspond to them, but to the causes of qualitative changes in forms of energy.

The world of psychics cannot be separated from that of physics, and we must look forward to the future progress in the latter science to bring the qualitative changes into connection with the theory of the conservation of energy. The author shows by a very simple example—"Shall I kill that spider, or leave it alone?"—the effect of his will on surrounding nature; and the divergent effects thereon which would result from each of the two alternative modes of procedure.

Memory he believes to be caused by an impulse of a certain kind, producing in the particular arrangement of the smallest particles in the ganglion cells and nerve fibres a modification in the same direction as was produced by the original impulse, and resulting in corresponding physical phenomena. But he acknowledges that, at present, we cannot explain "brain oscillations."

The principle of evolution sheds a light upon the psycho-physical problem: physical development is not the cause but the effect of psychical development, and the modifications in the brain and nervous system throughout the animal kingdom are intelligible as resulting from psychical causes, whereas the physical causes, if

they exist, remain hidden. He considers that even in palæontology we can detect traces of this psycho-physical process by the examination and comparison of the cranial capacity of the skulls of extinct reptiles and mammals. As man is the culminating point in mental development amongst mammals, so is the ant amongst insects; but clearly this position has in each case been attained independently, and is independent of the structure of the nervous system. The inheritance of acquired characters is discussed, and the old difficulties presented by a disbelief in it are once more brought forward; and especially the difficulty in the adaptation of terrestrial mammals to a life in water, such as must have occurred in the ancestors of the Cetacea. The author endeavours to show that the principle of progressive psycho-physical development may admit of a vital-mechanical explanation, if the transference of acquired characters, as a consequence of changed functions, is possible for "keimplasma."

The author is apparently a practical man of science, and not a mere arm-chair philosopher; he fully recognises that philosophy must be based upon scientific experiments, and quotes Huxley's words, "The Laboratory is the forecourt of the temple of Philosophy."

Meteorology, Practical and Applied. By John William Moore, B.A., M.D., M.Ch., F.R.C.P.I. (London: F. I. Rebman, 1894.)

IT is to be hoped that this little book may meet with the popularity it deserves. Well written and well illustrated, it ought to recommend itself to that numerous class of whom some knowledge of meteorology is now required. The author, a medical practitioner, has evidently, first of all, but by no means exclusively, sought to interest medical officers of health and those who seek a qualification in preventive medicine and its allied branches. Writing for such students, the author has prudently not burdened his work with technical terms, or attempted to discuss with any completeness the general motions of the atmosphere depending upon the application of thermodynamics. Neither does he fall entirely into the popular and pleasing style of writing; though he does seek legitimate interest by exhibiting the many points in which meteorological inquiry bears on social and sanitary science, how it may benefit the agriculturist, protect the traveller, or instruct the physician.

The book is divided into four sections. In the first we find a very full and, considering the source from which it is drawn, probably accurate account of the history and development of the United States Weather Bureau. It seems to have occurred to the author, that if he shows to the reader at an early stage the interest and devotion which the shrewd American gives to this subject, he will convince him that there is something in meteorology after all, beyond the dreary and wearisome accumulation of barometer and thermometer readings. Then we have, of course, the description of the necessary instruments in use, with their corrections. We are glad to see in this section due prominence given to Mr. Aitken's interesting work on atmospheric dust; and in the chapter on evaporation we notice that Mr. Apjohn's formulæ are given correctly, which is not the case in some other well-known elementary works. The third section of the book treats of climate and weather, a section that might with advantage have been made fuller; but in reviewing the whole subject of meteorology within moderate compass, it is necessary to curtail somewhere. The last section considers the influence of season and weather on disease. Here the author is apparently on very familiar ground, and the small space devoted to this topic is full of interest and suggestion. There are one or two slips in the text, as, for instance, on page 10, where the oft-repeated

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 versa*, 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.

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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. Iarmor 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