the last two minerals, the second of which is often fairly idiomorphic. It is, in fact, a variety of quartz-diorite, for which some petrographers use the name. Enclosures of a darker, more basic rock are not unfrequent, which in some cases much resemble included fragments, but in others may have a concretionary origin. The tonalite is cut by dykes of a more aplitic character, and apophyses from it penetrate the neighbouring sedimentary rocks. The percentage of silica is rather lower than in an average granite, that of ferro-magnesian constituents is higher, while in the alkalies the soda much exceeds the potash. The monzonite of the Fassathal differs from it in being poorer in silica and richer in alkalies; the granite of that valley and of the Cima d'Asta exceeds it in both respects; but, as we see from the neighbourhood of Predazzo, these early Mesozoic ejections indicate much differentiation of any original magma. But for these interesting questions it is enough to refer to Prof. Brögger's classic memoir[1] on that district.

If this tonalite *massif* represents the supply basin to one or more volcanic orifices, all traces of the latter have completely disappeared. The blood-red "porphyries," so characteristic of a broad region east of the Adige, above and below Bozen, can also be seen west of that river, at no great distance from the Adamello, and can be traced south of it to beyond the Sesia. These were erupted in Permian times, but the tonalite, like those other holocrystalline masses already mentioned, is later than most of the Trias. On that point evidence has been accumulating since 1846, but it is made more than ever certain by Dr. Salomon's exhaustive examination of the relations of the tonalite and the various stages of that system from the Werfener Schichten (Bunter) to the Haupt Dolomit (lower part of the Rhætic). The usual proofs of intrusion can be seen in many places, and a zone of contact metamorphism traced for a considerable distance outwards from the margin of the invading rock. It can also be seen breaking into Permian sediments and into the older crystalline schists.

These schists Dr. Salomon divides into three groups: the Edolo Schiefer, the Rendena Schiefer, and the Tonale Schiefer. The first, which occur on the north and in the northern part of the west of the Adamello *massif*, are said to be phyllites, sometimes anthracitic, with quartzose-banded phyllites and quartzites; the second, consisting of phyllitic gneisses and mica schists, occur occasionally on the west, but are more developed on the east; and the third, specially characterised by bands of saccharoidal marble, often rich in silicates, and associated in one part with a zone of augen gneisses, occur on the north, on the other side of the Edolo Schiefer. Dr. Salomon regards the Rendena Schiefer as early Cambrian or late Archæan, but refers some of the Tonale Schiefer to the Mesozoic, considering the marbles to be infolded Triassic limestones, metamorphosed by intense pressure. It may seem presumptuous for one who has merely traversed this district, and that not at all recently, to express any difference of opinion, but, as Dr. Salomon supports his views by references to other parts of the Alps which the present writer has carefully and continuously studied, he has no hesitation in saying that the identification of those Alpine marbles with the acknowledged Triassic limestones is very like that of Monmouth and Macedon, and that the asserted Mesozoic age of the crystalline schists, to which most of the so-called phyllites belong, and with which these marbles are associated, is supported by no better evidence than mistakes in elementary mineralogy and the neglect of important facts, such as the presence of fragments of those crystalline schists in indubitable Triassic rock. So we venture to think that the last word has not yet been said on the subdivisions and the ages of these crystalline schists.

As dolomitic limestone occurs in the neighbourhood of the Adamello, the author discusses the relation of these rocks to coral reefs. Here we are surprised at not finding any direct reference to the Royal Society's memoir on the borings at Funafuti, and the author is apparently satisfied with the following quotation (p. 417):—"Die Bohrung auf Funafuti erscheint in demselten Licht; die dort erreichte grosse Dicke wurde wahrscheinlich in der Grunlage einer alten Kalksteines erreicht, so dass die erlangten Resultate keinesweg die Annahme der Senkungstheorie nötig machen." We can only suppose that Dr. Salomon has never seen the conclusion of Dr. C. J. Hinde's study of the cores (Memoir, p. 334):—"The evidence appears to me to indicate a continuous formation of reef rock, without any abrupt break, from the depth of 1114 feet to the present time"; while Prof. J. W. Judd (p. 175), after stating that specimens of Tertiary limestones from reefs in Indian and Pacific seas had been carefully studied for purposes of comparison, expressly states:—"The same recent forms of foraminifera, corals and other organisms occur from the top to the bottom of the series of cores. On this point the evidence appears to be conclusive, and we are justified in stating that no basis of old Tertiary limestone was reached in the deep boring at Funafuti."

But these two defects, for such we deem them, do not blind us to the many merits and great value of this memoir. The facts will remain, even if, in a few cases, Dr. Salomon's interpretation of them be ultimately set aside. The book is the outcome of years of patient toil, and, when completed by the petrographical and palæontological studies of his collections, will be a permanent monument to his scientific energy, zeal, and acumen.

T. G. B.

[1] "Die Eruptionsfolge der triadischen Eruptivgesteine bei Predazzo," 1895.

TIDAL PROBLEMS.[1]

THE authors of the planetesimal theory have in this volume made a further important contribution to the discussion of the problems of cosmogony. In their endeavours to establish their own theory on a sound footing, much work has necessarily to be done in the way of criticising earlier theories. The classical nebular hypothesis of Laplace has already been discussed in a series of papers by Profs. Chamberlin and Moulton, and found wanting in many respects. To them in part is due the general abandonment of this hypothesis in anything like its original form by most astronomers of the present day. The present volume of papers is directed mainly against the tidal theories developed by Sir George Darwin, and more particularly against the view that at some far-distant epoch the moon separated from the earth.

Prof. Chamberlin's paper on "The Former Rates of the Earth's Rotation" opens with an attack on the theory of centrifugal separation of the heavenly bodies. We are compelled to admit the force of many of his criticisms with regard to the separation of the planets from a parent sun, but the similar criticisms levelled against the formation of the planetary sub-systems are lacking in weight. In particular, the argument from a comparison of the present orbit of Phobos with the ring system of Saturn fails if a change in the dimensions of the orbit of Phobos, assumed negligible, is allowed for. Also the mere statement

[1] "The Tidal and other Problems." By Profs. T. C. Chamberlin, F. R. Moulton, and others. Pp. iv+264. (Washington: Carnegie Institution, 1909.)

that the recently discovered retrograde satellites complicate the centrifugal hypothesis is no serious argument against this hypothesis. They can be made to fit into a general modified scheme. With regard to the earth-moon system, the criticism based upon the irregularity of present-day ocean tides seems irrelevant to the main issue. In the past, at any rate, we must look to regular body tides in the earth as the main factor in tidal evolution; of these tides, as mathematical analysis shows us, a natural result is a retardation of rotation. There are several further criticisms of interest in the paper, notably the reminder that the moon, if brought down to the surface of the earth (assumed to be of its present size), would be inside the limit given by Roche's criterion of stability, and would split into fragments. This point has already been considered by Sir G. Darwin (NATURE, 1886). In his discussion of it he abandons any idea that his theory explains the actual method of genesis of the moon, while he claims that it is of importance in dealing with the moon's later history.

The dynamical arguments of the paper are reinforced by geological arguments. It is shown that there is no geological evidence supporting the view that in earlier times the earth's rotation was much more rapid, and consequently the earth itself much more oblate than at present. The greater part of the changes must have taken place before the earth had solidified and its surface had become a book for the geologist to read. Only on such terms will the geologist accept the tidal theory, and on this particular point he has the support of Kelvin. In view of recent discoveries of unsuspected sources of heat and energy, it seems that the necessary extension of time involved in this requirement of the geologist may be granted by the physicist, and that the theory need not be abandoned by the mathematician at the bidding of the geologist.

Prof. Moulton's line of attack is quite distinct from that of his colleague. By considerations of energy and momenta alone he traces back the earth-moon system under various simplifying assumptions to the time when day and month were equal. He obtains in all cases a distance between the centres of the two bodies of more than 9000 miles. This result is very much the same as that originally found by Sir G. Darwin, but the latter suggested, without examining the point very closely, that a consideration of the sun's tidal effect would greatly diminish this distance. Prof. Moulton proves that the actual difference due to this cause is very slight, and his conclusion strongly reinforces the view that fission must have taken place, if at all, at a time when the earth was much less dense than at present. The further contention that the fission could not have been the result of increased rotation alone has already been recognised as valid by Sir G. Darwin. The latter's suggestion that the coincidence of a solar tide with a free period of oscillation of the earth might have led to a condition of instability has been examined by Prof. Love (Phil. Mag., March, 1889), who showed the idea to be quite feasible.

Of the remaining papers in the volume, which deal mainly with special points discussed by Chamberlin or Moulton in relation to the wider problem, the most interesting is an examination by Dr. Lunn of the heat which would be developed in the building up of a planet according to the planetesimal theory. A general qualitative agreement with the known requirements of facts is reached. More could, perhaps, hardly be looked for. Perhaps the following general criticism of the planetesimal theory may be made at its present stage of development. In many respects it gives a general *qualitative* agreement with observed facts, while its supporters are criticising older theories on the ground that they lack at times a close *quantitative* agreement with observed facts. It remains to be seen whether the newer theory will come up to the standard by which the older theories are being judged.

F. STRATTON.

PROF. SIMON NEWCOMB.

BY the death of Prof. Simon Newcomb science has sustained one of the most severe blows of recent years. America has lost her most eminent man of science, and not since the death of Adams has the world been deprived of so illustrious an investigator in theoretical astronomy. Newcomb's career up to 1899 was described by Loewy in the article on "Scientific Worthies" in NATURE, vol. lx., p. 1, and his activity and marvellous powers of work continued up to the date of the illness that has just terminated fatally. Since 1899 he has given us his interesting book entitled "The Reminiscences of an Astronomer" (1903), in which he described the early incidents of his life and related the extraordinary circumstances by which his steps were guided into the career which led him to such eminence.

Newcomb commenced his reminiscences with the words:—"I date my birth into the world of sweetness and light on one frosty morning in January, 1857, when I took my seat between two well-known mathematicians (Winlock and Runkle) before a blazing fire in the office of the 'Nautical Almanac' at Cambridge, Mass."

Though born at Wallace, in Nova Scotia, March 12, 1835, Simon Newcomb was of almost pure New England descent. His father was, he tells us, the most rational and the most dispassionate of men, who, when he had reached the age of twenty-five, set forth to search for a wife who possessed the qualities most suitable in a helpmeet. His search had extended nearly a hundred miles before, in the village of Moncton, he found in Emily Prince what he desired, and his son says the marriage was "in all respects a happy one, so far as congeniality of nature and mutual regard could go." . . . "My mother was the most profoundly and sincerely religious woman with whom I was ever intimately acquainted, and my father always entertained and expressed the highest admiration for her mental gifts, to which he attributed whatever talents his children might have possessed. The unfitness of her environment to her constitution is the saddest memory of my childhood. More I do not trust myself to say to the public, nor will the reader expect more of me."

How Newcomb's early years were passed may perhaps be conjectured from the fact that the autobiographical chapter in which he records them bears the title of "The World of Cold and Darkness." He had, however, from his earliest years a keen desire for knowledge, and read whatever books were available. His first introduction to the intellectual career he desired was not promising. In those days there was a so-called physician, Dr. Foshay, living near Moncton, who was reputed to have effected cures of sick persons given up by other doctors. As Newcomb says, "Diomeds of the medical profession before whose shafts all forms of disease had to fall were then very generally supposed to be realities." By the intervention of an aunt, young Newcomb agreed to live with the doctor, rendering him all assistance in preparing medicines, while the doctor, on his part, undertook to supply Newcomb's bodily needs and teach him "the botanic system of medicine." After a little experience it began to dawn upon Newcomb that Dr. Foshay, notwithstanding his