sections. He also, in his desire to be abreast of current literature, uses such terms as "allotriomorphic," "micro-felsitic," "magma-basalt," without adequate definition or discussion; and in speaking of a "porphyritic ground-mass" he will throw many beginners into confusion. If the student is to seek elsewhere for instruction both in the manipulation of the polariscope and in the use of technical terms, the book must be held to fail in its fundamental object as a primer. It will probably serve well, however, to remind the learner of the broader features that mark out one rock-forming mineral from another. The author, moreover, insists, as befits a geologist, on the purely supplementary character of microscopic study—a warning that seems more than ever needed when micro-petrography, by the change of a few letters, has been exalted to the level of a science.

The statements in this book are essentially accurate, and the illustrations, excepting that of ophitic structure, may be useful in recalling diagrammatically what has been seen in actual sections. On p. 32, however, there is an incorrect account of the pleochroism of muscovite, which probably has arisen from a blending of two totally independent notes. On p. 36 the sections of augite should be described as having, not six, but eight sides; and talc reappears on p. 50 as a constituent of protogine granite. The cleavages in drawings on pp. 33 and 36 are not in every case consistent with the descriptions.

G. C.

Theoretical Mechanics. By J. E. Taylor, M.A. (London: Longmans, Green, and Co., 1888.)

So long as examinations on prescribed courses are in vogue, so long, we suppose, will text-books be written for them. The book before us has been prepared chiefly to help those who are studying for the elementary stage of the Science and Art Department's examination in the subject, but it also covers the requirements for London matriculation. There is not much scope for originality in a work of this description, and in looking through it we find ourselves in familiar, well-worn paths.

In his preface, Mr. Taylor states that he has endeavoured to make the subject comprehensible to the beginner, but we are afraid that his efforts to explain the difference between mass and weight will be far from successful. This is always a delicate point to touch upon, but we venture to say that few beginners will be likely to understand the explanation given on page 8. This is as follows:—"Whilst mass is always measured by weight, yet the two terms must be kept distinct, the weight being the amount of force which the attraction of the earth exerts on the mass. If g represent this attraction, W , weight of the body, m , mass, we have $W = mg$." Most beginners are likely to imagine from this that W should be equal to g , instead of to mg .

The book is well illustrated throughout with many new diagrams and several old ones from well-known text-books. Numerous examples, worked and unworked, are also given.

With the exception referred to, the book is on the whole well written, and completely covers the Syllabus. The admirable style in which it has been issued, and its comparative cheapness, will commend it to many teachers.

Instructions for Observing Clouds on Land and Sea. By the Hon. R. Abercromby. With Photographs and Engravings. 22 pp. (London: Stanford, 1888.)

THE phrase *Nascitur non fit* may be applied to cloud observers with almost the same confidence as to poets; at least, such is the experience of most persons who have attempted to teach an ordinary observer to record cloud phenomena.

Mr. Abercromby's pamphlet, however, contains a valuable stock of instruction which may be placed in the

hands of intending observers, and will at least indicate to them what they have to observe.

The actual nomenclature of cloud forms used by Mr. Abercromby is that which has been, for the time at least, rejected by the International Committee at the recent meeting at Zürich (NATURE, vol. xxxviii. p. 491), but this is a minor matter. The illustrations of cloud perspective and cloud motion are new and good, while the difference between the motion of advance, the "propagation" of a cloud bank, and the rotation of the clouds within that bank is, for the first time, clearly stated. The importance of the R. point (radiation point), the point towards which the stripes of cirrus converge, is explained.

Mr. Abercromby concludes as follows:—

"It (cloud observing) cannot be learnt in a day, but with a little attention and practice the knowledge is soon acquired. The observer, who begins by taking simple cases of low, fast-moving clouds, till he has fully realized the meaning and importance of R. points, will soon attain such proficiency as will enable him to make valuable observations in the most recent branches of modern cloud science."

Laboratory Manual of General Chemistry. By R. P. Williams, A.M. (Boston: Ginn and Company, 1888.)

AFTER a few preliminary matters, including some good rules for students in the laboratory, each two pages of this book has in large type consecutive directions for performing an experiment or exercise. The rest of the two pages is left blank for written notes. One hundred exercises are given, and they are of a quite elementary character. It is a pity that contractions are so frequently used, especially when there is a large amount of vacant space and so small an amount of matter. "Ap.: p.t., 4 rec., t.t., d.t., r.s." indicates to the student the apparatus he needs for the purpose of preparing hydrogen. It would have been better to adopt a recognized system of shorthand throughout, for that would have rendered the book more useful to some and quite useless to others, instead of troublesome to all.

LETTERS TO THE EDITOR.

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Mr. Romanes on the Origin of Species.

WHAT the *Times* said is substantially the same as what Mr. Romanes himself says on p. 366 of his paper: "The theory of natural selection is not, properly speaking, a theory of the origin of species: it is a theory of the development of adaptive structures. Only if species always differed from one another in respect of adaptive structures, would natural selection be a theory of the origin of species. But, as we have already seen, species do not always, or even generally, thus differ from one another." Very well then, I say, if this be true, it shrivels up the part played by natural selection to very small dimensions.

The second part of Mr. Romanes's reply consists of a complaint that when I quoted from his paper the words "natural selection not a theory of the origin of species," I did not see that they were "flatly falsified" by the section at the head of which they stood. I do not see it even now, because the section finishes with what Mr. Romanes oddly calls an "insinuation" "that Mr. Darwin's great work on the 'Origin of Species' has been misnamed." If this insinuation be just, then I further do not understand how Mr. Darwin's reputation for candour is to be saved except at the expense of his intelligence.

In the third part of his reply, Mr. Romanes says "he [Mr. Dyer] must surely be aware that other botanists who have more thoroughly considered the subject are dead against him in his general conclusion." I have perhaps as many opportunities as most men of knowing the opinions of botanists, and I cannot

say I am aware of anything of the kind. The "late Prof. Nägeli" may be dead against me possibly; but I was not aware that he was dead in any other sense. Nor do I see that as he insists (quite correctly as I think) on the inutility of family characters he can afford much comfort to Mr. Romanes, who regards them as generally adaptive.

I have devoted a good deal of time to the study of Mr. Romanes's paper published by the Linnean Society. I believe I have stated the conclusions to be drawn from that paper with tolerable accuracy. If I have not done so, I undoubtedly owe him a sincere apology. But I am bound to confess that, the more I study his views, the more I find myself in disagreement with him as to the inutility of specific characters; as to the utility and mode of origin of generic characters and those of higher grade; as to sterility as a primary specific difference; and as to the value of so-called physiological selection. In all these matters he is, I am satisfied, contradicted by botanical experience. I think if he had imitated the example of Mr. Darwin, and had carefully collected a large body of evidence on each of these points with a perfectly open mind, he would have found this out for himself. What, however, I view with less patience than his unsustained generalizations, is his persistent attempt to place them on the shoulders of the Darwinian theory. I have reluctantly arrived at the conviction that his only excuse for so doing is that he has fundamentally misunderstood that theory. At any rate, I cannot in any other way account for the strained interpretation which he has put on passages from Mr. Darwin's writings. I may give, as an example, the passage he quotes "to justify the insinuation" that the "Origin of Species" has been misnamed; the obvious drift of this does not relate to specific differences at all, but to those which are characteristic of families. It is easy to see, in fact, by a comparison of pp. 170 and 176 of the sixth edition, that the passage cited by Mr. Romanes was inserted by Mr. Darwin to meet the point raised by Nägeli to which I have referred above. Certainly I think that no one would have been more surprised than Mr. Darwin when he wrote the words could he have foreseen that they would be used to impugn the validity of the title of his theory and of his book. Everyone knows that Mr. Darwin was the fairest and most generous-minded of men. He constantly admits the possibility of explanations to which he really, however, did not attach much importance. Such admissions Mr. Romanes appears to me to treat as if wrung from a hostile witness. In my judgment this is entirely to misapprehend their significance or the spirit in which they were made.

W. T. THISELTON-DYER.

Royal Gardens, Kew, December 1.

Natural Selection and Useless Structures.

In his letter on "Mr. Romanes's Paradox" (*NATURE*, November 1, p. 7), Mr. Thiseyton Dyer questions the existence of indifferent or slightly disadvantageous specific characters. That letter referred, in a highly laudatory yet somewhat deprecating manner, to a lately published (*Proc. Roy. Soc.*, No. 269) obituary notice of Mr. Darwin; and it implied that Mr. G. J. Romanes, from his unfamiliarity with the study of species, did not quite know what he was talking about when he asserted that such indifferent characters do in fact exist. I, who claim to have had some slight experience in the practical discrimination of species, ask permission to make a few observations in your columns on the subject.

Everyone would, I suppose, regard the frequent absence of the toe-nail on the hallux of the orang as an indifferent matter, but I am inclined to consider the feeble development of that digit itself as a slightly disadvantageous one. However that may be, I am strongly of opinion that the abortion of the index in the Potto can never have saved the lives of the earliest individuals so distinguished. I have, as yet, heard no reason assigned for the life-saving action of the thumbless hands of *Colobus* and *Ateles*, or of the tail of the one chameleon in which alone (so far as I know) that organ is not prehensile. The metallic lustre of the peritoneum of some fishes is hard to explain by either "natural" or "sexual" selection; as also are such specific characters as the extension, or non-extension, of the premaxillæ to the frontals, or the pattern of the foldings of enamel and cement in various Rodents. The complexity of the teeth of *Labyrinthodon*, or the similar multiplicity co-existing in those of *Orycteropus* and *Myliobatis* (which can hardly have been derived from a common ancestor, though their resemblance extends even to microscopic structure), are unquestionably good taxidermic

characters; yet they can hardly have been due to the action of natural selection, as I pointed out in my "Genesis of Species" in 1870. But if such "selection" cannot originate characters which form the diagnosis of a species, then it cannot possibly be the origin of such species. To say that the rudimentary index of the Potto is a character which, though itself useless, has been carried on the back, as it were, of some possible but unknown useful simultaneous variation which co-exists with it or did co-exist with it in some unknown ancestor is a purely gratuitous assertion. Such assertions are the less warranted because we have evidence that the energy of Nature's destructive forces has been exaggerated. Prof. Dyer tells us that natural selection is a hard taskmaster; but it is not, I think, so hard a one as some persons suppose. This seems to me clear from such facts as the finding of hares and rabbits in which an incisor tooth has grown so as to complete the circle it always tends to form—a condition which shows a remarkable preservation of life under extremely disadvantageous circumstances. A stoat, three of whose feet had been cut off at different times by traps, has nevertheless (I am informed) lived long enough for its injured limbs to heal so thoroughly that the beast could get a living on its one foot and three stumps. Cases of prolonged life under trying circumstances are not so rare. I recollect the skeleton of a monkey which must have long suffered from acute rheumatism in its native forests.

Prof. Dyer deprecates the admission, by the author of the obituary notice, that indifferent or slightly disadvantageous characters may be evolved in spite of "natural selection." But the obituary notice admits *much more than that*, since, according to its author, a maintainer of "natural selection" is free to affirm the genesis of species by sudden, considerable, definite variations, directly produced by the reaction of the innermost nature of an organism on the stimulus of its environment, according to precise innate laws of its being. This certainly is not "natural selection," as understood and taught by Mr. Darwin, and the inventor of a new term has alone the right to fix what its meaning shall be.

The statement of the obituary notice seems equivalent to an unintentional but virtual abandonment of "natural selection," while still retaining the name—reducing it, in effect, to that merely subordinate rôle we all admit that it plays. To call such a mode of origin "origin by natural selection" seems much the same thing as declaring an elaborately prepared theatrical transformation scene to be brought about by the chains and cords which prevent its moving pieces from passing beyond their assigned limits. The true meaning of "natural selection" is frankly declared by that distinguished biologist upon whose shoulders the mantle of the deceased prophet seems to have fallen. Prof. Lankester, in his article "Zoology" (in the last volume of the "Encyclopædia Britannica") has just given a most straightforward, lucid, and forcible representation of Darwinism. Nevertheless, the article (in the same volume) on "Variation" by Prof. Geddes, appears to me to be more in harmony with the facts of biology. It is, of course, open to anyone to say: "All species which succeed do so from some cause, and this may be metaphorically said to 'select' them." Therefore, since all causes are "natural" causes, every species which does succeed must succeed through "natural selection." This is equivalent to saying: "Nature is so conditioned as to produce the results it does produce"—an assertion most true, but somewhat trivial. When a term is so stretched as to mean "anything," it thereby comes to mean "nothing," and its use can serve no purpose save the preservation of a phrase it may be desired, for some reason, not to discard.

ST. GEORGE MIVART.

Hurstcote, Chilworth, Surrey, November 28.

A Mussel living in the Branchiæ of a Crab.

LATE this autumn, while searching for Crustacea at Amroth, in South Wales, I found rather an exceptionally good specimen of the common shore crab (*Carcinus maenas*), which I took back to the hotel to clean and preserve. On removing the carapace, I found a mussel living among the branchiæ, and fastened to them by means of its byssus. It was in good condition, and measured $\frac{3}{4}$ of an inch in length. The carapace of the crab measured $2\frac{1}{4}$ inches wide by $1\frac{1}{8}$ inches long. I could find no signs on the exterior of the crab of anything remarkable within, nor was there any damage to the shell, or hole through which the mussel could have passed. It seems that the mussel, while yet minute, or in a larval condition, must have been carried