

ammonia salts in producing uric acid in birds, and the remarkable confirmation of this by Minkowski, who found, after extirpation of the liver, the uric acid of the bird's urine replaced by ammonia.

The subject most fully treated is that of the pigments, but here again the important works of Nencki and Sieber on hæmoglobin and its derivatives, find no mention. A work like the present is necessarily a compromise. It does not give so equable and well-judged an account of what it is important to know in physiology as might be expected from the reputation of the author and the size of the book; but it shows the judgment of an experienced teacher in endeavouring to make every subject perfectly intelligible and in leaving no branch of physiological science untouched.

L. C. WOOLDRIDGE.

OUR BOOK SHELF.

The Mind of the Child. Part I. The Senses and the Will; Observations concerning the Mental Development of the Human Being in the First Year of Life. By W. Preyer, Professor of Physiology in Jena. Translated from the original German by H. W. Brown. "International Education Series." (New York: Appleton and Co. London: Whittaker and Co. 1888).

It is with no small satisfaction that we notice the issue of this work in the English language. It has already remained much too long in the German and French tongues only; and it speaks ill for the enterprise of British publishers that now the name of Appleton appears upon the cover. For, although comparisons as a rule are invidious, in the present instance there can be no doubt that the work in question holds the first place in the literature of the subject with which it deals. And since the study of infant psychology was inaugurated by M. Taine and Mr. Darwin, it has become so popular a branch of scientific literature that an English translation of "Die Seele des Kindes" must be an assured success, even from a commercial point of view.

In the case of a book already so well known, it is needless to say much by way of analysis. We must remark, however, that the present volume comprises only Parts I. and II. of the original—the remainder being reserved for publication as a second volume. Hence the instalment of the translation now before us deals only with the senses and the will; the next instalment having to treat of the intellect, and all supplementary matter. As everyone who has read the original is aware, Prof. Preyer has devoted himself to his subject with an assiduity and a thoroughness which only an assured conviction of its importance could inspire. And, in the result, his patiently continuous observation, his skilled intelligence as a well-read psychologist, together with his high attainments as a professed physiologist, combine to render his work, not only as before remarked the most important, but also in many respects the most interesting, that has hitherto appeared upon the subject of psychogenesis. Therefore we recommend this work to all our English readers as the best that they can procure on "the mind of the child"—and this whether their interest in such a mind be scientific only or likewise parental.

G. J. R.

Arithmetical Exercises. By H. S. Hall, M.A., and S. R. Knight, B.A. (London: Macmillan and Co., 1888.)

IN this book we have a collection of examples comprising about eighty progressive miscellaneous exercises and a set of fifty papers taken from such examinations as the London University, Oxford and Cambridge Local, Previous Cambridge, Army Preliminary, &c. The examples

are judiciously chosen, and great care seems to have been taken to make the work as progressive as possible. An appendix is added, consisting of two hundred graduated questions in logarithms and mensuration, preceded by a list of the numerical constants and formulæ used in the latter. The answers to the examples are all collected together at the end.

An Elementary Treatise on Mensuration. By E. T. Henchie. (London: School Books Publishing Company, 1888.)

IN this work we have an excellent treatise for those who are about to begin the study of this subject. All reference to trigonometry has purposely been avoided, and the author has in the second chapter added the enunciations of Euclid's propositions which bear on the work, together with an explanation of each.

Plain rectilinear figures, curvilinear areas, the circle, surfaces and volumes of solids, are dealt with in turn, and each chapter is accompanied by a set of illustrative examples thoroughly worked out and explained, followed by a separate set to be worked out by the student. Land surveying forms the subject of the eighth chapter, in which are described the various instruments with the methods of using them. The figures throughout are very clear, and the shading used in those of the chapter on solids is excellent. The book concludes with a set of miscellaneous examples, making in all about 1260, together with the answers to the above.

LETTERS TO THE EDITOR.

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Lamarckism versus Darwinism.

I HAD hoped that my previous letter might have closed this correspondence, but Mr. Poulton's reply suggests to me the propriety of making one additional remark. This is, that while writing the sentence in the *Contemporary Review* to which he has taken exception, it never occurred to me that anyone would gather from it that I intended to disparage the work of an eminent man, who happens to be also a personal friend. But, as this appears to be the impression conveyed to Mr. Poulton, I should not like to allow his statement of it to pass unnoticed. As a matter of fact, no one can appreciate more thoroughly than I do the extensive knowledge, the clearness of thought, and the great powers of original research which are now being so conspicuously displayed by Prof. Weismann.

From the first it has been sufficiently obvious to me how the present misunderstanding arose; and if, instead of affirming that I was ignorant of Prof. Weismann's writings, Mr. Poulton had begun as he has ended, by asking me to "explain" my remark with reference to them, of course I should at once have done so. However, as stated in my last letter, it is my intention at no very distant date to deal with the whole question of so-called "Lamarckism versus Darwinism"; and, therefore, my only object in this communication is to stop from going further the impression that I hold in light esteem the highly important achievements of Prof. Weismann.

GEORGE J. ROMANES.

Geanies, Ross-shire, September 8.

Mr. Gulick on Divergent Evolution.

MR. GULICK's paper on this subject appears in the last number of the *Journal of the Linnean Society* as having been "communicated by Alfred Russel Wallace, F.L.S." It may therefore be supposed that I recommended its publication, or that I agree with its main argument; and as this is not the case, I ask permission to say a few words on the subject in the columns of *NATURE*.

In 1872, Mr. Gulick sent me his paper on "Diversity of Evolution under One Set of External Conditions," requesting me, if I thought fit, to communicate it to the Linnean Society. As the paper contained a body of very interesting facts observed by the author, I had no hesitation in recommending its acceptance by the Society, although I did not agree with the conclusions Mr. Gulick drew from his facts.

Last year Mr. Gulick sent me the manuscript of his present paper, informing me that it was the result of long-continued study of the subject, and asking me to forward it to the Linnean Society. I did so, writing to the Secretary that I had not read the paper through, and did not undertake the responsibility of recommending it for acceptance.

Having now read the paper in print, I find very little in it that I can agree with. I can discover in it no additional facts beyond those which were set before us in the former paper sixteen years ago, while there is an enormous body of theoretical statements, many of which seem to me erroneous, and a highly complex classification of the conditions under which the separation or isolation of individuals of a species takes place, with a new and cumbersome terminology, neither of which, in my opinion, adds to our knowledge or comprehension of the matter at issue.

As in almost every page of this long paper I find statements which seem to me to be either disputable or positively erroneous, any extended criticism of it is out of the question; but I wish to call attention to one or two points of vital importance. Mr. Gulick's alleged discovery is, "the law of cumulative divergence through cumulative segregation" (p. 212). He maintains that any initial variation, if isolated by any of the causes he has enumerated, but remaining under identically the same environment, will increase till it becomes in time a specific or even a generic divergence, and this without any action whatever of natural selection. Now if this is a *fact* it is a most important and fundamental fact, equal in its far-reaching significance to natural selection itself. I accordingly read the paper with continual expectation of finding some evidence of this momentous principle, but in vain. There is a most elaborate discussion and endless refined subdivisions of the varied modes in which the individuals constituting a species may be kept apart and prevented from intercrossing, but no attempt whatever to prove that the result of such complete or partial isolation is "cumulative divergence." The only passage which may perhaps be considered such an attempt at proof is that on p. 219, where he supposes an experiment to be made, and then gives us what he thinks "experienced breeders" will assure us would be the result. In this experiment, however, there is to be constant selection and reassortment of each brood, yet he asserts that "there is no selection in the sense in which natural selection is selection"; by which he appears to mean that the selection is by "separation" not by "extermination." This, however, seems to me to be a distinction without a difference.

Again, in the various illustrations of how "cumulative segregation" is brought about, natural selection must always come into play—as in the case of a change in digestive powers, and consequent adoption of a different food (p. 223), leading to partial isolation; and such cases are exactly what is contemplated by Darwin in his brief statement of the effects of "divergence of character" ("Origin," pp. 86–90), while the concurrence of "isolation" as a factor is fully recognized at pp. 81–83 of the same work (6th edition).

It appears to me that throughout his paper Mr. Gulick omits the consideration of the inevitable agency of natural selection, arising from the fact of only a very small proportion of the offspring produced each year possibly surviving. Thus when, at p. 214, he states that "the fact of divergence in any case is not a sufficient ground for assuming that the diverging form has an advantage over the type from which it diverges," he omits from all consideration the fact that at each step of the divergence there was necessarily selection of the fit and the less fit to survive; and that if, as a fact, the two extremes have survived, and not the intermediate steps which led to one or both of them, it is a proof that *both* had an advantage over the original less specialized form. Darwin explains this in his section on "Extinction caused by Natural Selection" (p. 85). On the whole, I fail to see that Mr. Gulick has established any new principle, either as a substitute for, or in addition to, natural selection as set forth by Darwin. Others, however, may think differently; and I shall be glad if any naturalists who have studied Darwin's works will point out, definitely, in what way this paper extends our knowledge of the mode in which species have originated.

ALFRED R. WALLACE.

The Death of Clausius.

I DO not know by what unfortunate accident it happened that I did not hear of the death of the great Clausius until after the meeting of the British Association. I write this in order to explain how I neglected to express the sorrow of the scientific world in Britain in the loss, and our sympathy with the scientific world in Germany. It is not the part of a young disciple like me to eulogize the giants of the passing generation, but I regret greatly that any appearance of want of appreciation of the labours of one of the most brilliant lights of the nineteenth century should attach to British science owing to my silence.

GEO. FRAS. FITZGERALD.

Trinity College, Dublin, September 15.

The March Storms.

THE accounts of March storms in England which reach us lead me to think that it would be interesting to note the following. On March 13, barometers in Western Australia had fallen suddenly 0.20 inch; the cyclone passed rapidly eastward along the south coast of Australia. On the 15th we had a heavy gale of wind at Sydney; the anemometer showed 55 miles an hour. Lake George was so disturbed that the observer was wind-bound in the small house which holds the recording machine for several days, and the tidal register at Sydney shows considerable disturbance like earthquake-waves during the 15th, 16th, and 17th. On the 15th the level of the Sydney transit instrument was found to have changed suddenly since the 14th, 0.7, the western pier having fallen. A tidal wave reached New Guinea and New Britain on the 13th; at the latter place it is supposed to have risen 40 feet.

H. C. RUSSELL.

Sydney Observatory, July 26.

INTERNATIONAL METEOROLOGY.

THE International Meteorological Committee held a meeting at Zürich, in the Polytechnikum, from the 3rd to the 5th of this month. All the members were present. The most important point on which action was taken was the subject of future meetings to be held instead of Meteorological Congresses organized by diplomatic means. The following was the resolution adopted:—

"The Committee, in view of the circumstance that the assembling of an international meeting, of the same character as the Congresses of Vienna and Rome, presents great difficulties, considers that the commission it received at Rome is exhausted, and that it ought to dissolve itself.

"At the same time, in order to continue the relations between the different meteorological organizations, which have been productive of such good results during a series of years, the Committee appoints a small bureau with the duty of using its best endeavours to bring about, at some convenient time, an international meeting of representatives of the different Meteorological Services."

By a subsequent resolution the bureau was made to consist of the President and Secretary of the Committee (Prof. Wild and Mr. Scott).

Among other matters on which action was taken may be mentioned:—

Cloud Classification.—It was decided that the proposals of Messrs. Hildebrandsson and Abercromby were not ripe enough to be recommended for general adoption.

Meteorological Information from Travellers.—On the motion of Dr. Hann certain rules were laid down, to be recommended to all Geographical Societies, &c., as to the conditions which must be observed in order to render published records of meteorological observations of any real service to meteorology. These relate to instruments and their corrections, exposure, methods of calculation, &c., &c.

The Committee finally dissolved itself.

ROBT. H. SCOTT.

Meteorological Office, September 19.