First, Dr. Bather quotes an observation of Dr. Mortensen's in which he describes the larva of an Ophiurid dropping the young brittle-star and then proceeding to regenerate itself, and states that this observation suggests that the metamorphosis of Echinoderms is an alternation of generations. I can only say that this observation of Dr. Mortensen stands in urgent need of confirmation, and that it is totally opposed to what we know of the normal development of Ophiurids. In the development of *Ophiothrix fragilis* the adult takes over from the larva the mouth, cesophagus, stomach, intestine, peritoneal sacs, and aboral integument, and what is left of the larva after this abstraction is merely the ciliated band, the larval organ of locomotion. This development is no more an alternation of generations than is the development of the veliger into the adult mollusc. The same type of metamorphosis is found in the pelagic larvæ of Holothuroids and Echinoids; in Asteroids the only additional feature to be observed is the shrivelling and disappearance of the præoral lobe which acts as stalk during the earlier stages of metamorphosis. Secondly, Dr. Bather states that Dr. Mortensen has shown that the Brachiolaria stage in Asteroid development (in which the larva uses its præoral lobe as a stalk) cannot be homologous with the similar stage of development in Crinoids, since it is found only amongst the "more specialised forms of Asteroids."

No more rash statement could be made nor one more devoid of foundation. Modern Asteroids are divided into five groups, viz. Forcipulata, Valvata, Velata, Paxillosa, and Spinulosa. Nothing whatever is known of the development of any valvate and velate form, but the fixed stage is found not only in the development of the Forcipulata (which Dr. Mortensen arbitrarily regards as the most specialised forms), but also in the development of the Spinulosa (which all admit to be the most primitive group). In the Paxillosa, which include the British genera *Astropecten* and *Luidia*, and which, *mirabile dictu*, Dr. Mortensen appears to regard as primitive forms, the fixed stage is omitted; the larva apparently amputates its præoral lobe and does not use it as a stalk.

The Paxillosa, so far from being primitive, are quite a modern development of Asteroid structure in the Ophiuroid direction. They have in most cases lost the anus and in all cases the sucking discs of the tube feet, and they have developed a quite un-Asteroid mobility and muscularity of the arms. *Luidia* even snaps off the arms on irritation exactly like an Ophiuroid.

The reason why the fixed stage is omitted in their development is not far to seek. What we know of their habits points to their being inhabitants of the sand and mud. Such a habitat is utterly unsuitable for the support of a fixed stage, and consequently this stage has been omitted in their life-history.

When, however, we reflect that the Echinoderms are admitted by all to be descended from the same stock, that this stock must have passed through a fixed stage, since primitive Crinoids are fixed, and that the stalks of Crinoids and Asteroids are formed from the same region of the larva, we shall be in a position to estimate the value of Dr. Mortensen's views. His ideas of the ancestry of Echinoderms would carry more weight if he had worked out with thoroughness the complete life-history of any Echinoderm.

Lastly, I should like to protest against the idea that those interested in Echinoderms agree with the over-estimate of the importance of trifling peculiarities in the structure of pedicellariæ in which Dr. Mortensen indulges. As Dr. Bather says, they are of no use to the palæontologist, and Dr. Bather, who is not only a systematist, but also a first-class morphologist, will

realise that Dr. Mortensen's views are accepted by few except himself.

E. W. MACBRIDE.

Royal College of Science, South Kensington,
London, S.W.7, December 14.

PROF. MACBRIDE has allowed his enthusiasm for the truth, as he sees it, to blind his eyes to what I actually did say. I said the idea of alternation of generations, though recalled by Dr. Mortensen's account of an observation, was "not really justified." I also characterised Dr. Mortensen's own inference from that observation as "audacious." I am glad to find that Prof. MacBride agrees with me, even if his mode of expressing agreement be unusual.

I did not say that Dr. Mortensen had "shown" (which I take to mean "proved") those statements and conclusions concerning the Brachiolaria and its sucking disc to which Prof. MacBride takes exception. By using the expression "none the less" I meant to imply that his conclusion on this point was not on all-fours with his general conclusion. Prof. MacBride differs from me in the vigour with which he rubs in that argument. I am glad that my remark has aroused so doughty a champion to the defence of the Brachiolaria, but I confess that I am not as yet prepared to brooder any published classification of the Asteroidea on my own banner.

It is not for me to break any lances in defence of Dr. Mortensen, but if Prof. MacBride is acquainted with Dr. Mortensen's "Studies in the Development of Crinoids" (see NATURE, vol. 107, p. 132, March 31, 1921) I am rather astonished that he should so belittle our Danish colleague's work on those lines. As for the importance that Dr. Mortensen attaches to pedicellariæ, I incline to think that it is his critics who "overestimate" it. He himself has written (1907, "Ingolf Exped., Echinoidea," vol. 2, p. 12):—"I have never stated that the classification has always to be based on the pedicellariæ as the most important factor; on the contrary, I am of opinion that where structural characters of some significance occur in the test, these are, upon the whole, of higher classificatory value than the characters in the pedicellariæ."

Prof. MacBride is as friendly and complimentary to me as he always is, even when we differ, but, however much he may differ from Dr. Mortensen, I do hope he realises that the latter has furnished us in this memoir with a number of novel observations obtained with labour and recorded with skill.

F. A. BATHER.

Some Problems in Evolution.

THE address of Prof. Goodrich on "Some Problems in Evolution," which I read in NATURE of November 24, incidentally deals in a slight but somewhat dogmatic manner with the question, "What share has the mind taken in evolution?" I do not propose myself to attempt to answer this question, but only to point out that the grounds on which Prof. Goodrich deals with the matter are quite inconsistent with well-established phenomena which are familiar to psychologists and psychotherapists.

Prof. Goodrich says, "I would maintain that there is no justification for the belief that it (mind) has acted, or could act, as something guiding or interfering with the course of metabolism." He scouts the idea "of the influence of the mind on the activities of the body," and says, "we cannot conceive how a physical process can be interrupted or supplemented by non-physical agencies." He tells us that the student of biology "should realise that the mental series of events lies outside the sphere of natural science," and relegates such matters to the realm of *philosophy*.

I have no doubt that Prof. Goodrich is familiar with recent advances in psychotherapy, and I cannot imagine how he reconciles them with the above statements. The treatise on "Suggestion and Autosuggestion," by Prof. Badouin, which describes the practice of the new Nancy school, gives plenty of examples of curative processes, which necessarily involve metabolism, set in motion by purely mental processes. A good example is furnished by the article of Prof. J. Stanley Gardiner in *NATURE* of December 15, p. 505. If autosuggestion can initiate metabolism in the individual, why is it to be ruled out as a possible factor in evolution? Prof. Goodrich is not an advocate of the view that only congenital characteristics can affect the progeny of the individual. If the requisite "environmental stimulus" persists I gather that in his opinion an acquired modification may also persist. In this connection it is surely a loss to his theory to rule out the effect which environment may produce primarily on the mind, and through the mind on metabolic processes. Why is the student of biology to regard all this as "outside the sphere of natural science"? W. R. BOUSFIELD.

In answer to Mr. W. R. Bousfield's question I may say that he is mistaken if he thinks that I would deny the possibility of curative processes being set going by "suggestion." But I would maintain that suggestion is not a mental process. There is a popular error, widespread, that thought can be directly transferred, whereas, as a matter of fact, we know that one organism communicates with another by physical means through the organs of sense—by touch, smell, taste, sight, and hearing. Suggestion consists in bringing to bear appropriate stimuli which directly or indirectly set going certain metabolic processes; or, to put it in another way, the stimuli excite in the organism responses which from one aspect appear as a series of metabolic processes, and from another aspect as a series of mental processes. The one series cannot be altered without also altering the other.

In *NATURE* of November 17, Dr. J. T. Cunningham complains that I have ignored "the greater part of all the new conceptions and new results obtained by recent research on heredity and genetics." Now it is difficult within the limits of a short address to guard against all possible misunderstanding, yet I think Dr. Cunningham might have gathered that my object was to concentrate on certain fundamental problems, avoiding all unnecessary detail. Although yielding to none in my admiration for the triumphs of Mendelism, I purposely set aside (as stated in a footnote) complications due to hybridisation, the formation of heterozygotes, segregation, etc., because they did not seem to me to bear directly on the questions discussed. My innocent statement "that the newest characters may be inherited as constantly as the most ancient, provided they are possessed by both parents," in no way contradicts Dr. Cunningham's own statement "that a character may be inherited when it is apparent only in one parent or in neither," a fact which, by the way, was known before Mendelism was invented. I still hope that nothing said in the address is inconsistent with even the newest sound conceptions and newest results of experimental genetics.

In the third paragraph of his letter, while seeming to agree that a change of character must be due either to an alteration of the germinal factors or of the environmental conditions, Dr. Cunningham confuses the issue by failing to distinguish consistently between the variation and the resulting character. This is just one of the important distinctions which, as I endeavoured to show in the address, must be

realised if we are not to waste time in endless and futile controversy.

Great as is our debt to Weismann, it must be acknowledged that not a little of the confusion in discussions on "acquired characters" is traceable to obscurities and inconsistencies in his writings. In this matter I gladly acknowledge my indebtedness to the works of Sir Archdall Reid. No one, I think, has so clearly shown that, as some others saw before him, there are two kinds of variation, but only one kind of character. E. S. GOODRICH.

Oxford, December 9.

World List of Scientific Periodicals.

THE Conjoint Board of Scientific Societies proposes, if sufficient support is obtained, to arrange for the issue of a world list of periodical publications which contain the results of original scientific research, and has entrusted preliminary arrangements to a committee, of which the following are members:—Sir Sidney F. Harmer (chairman), Mr. F. W. Clifford, Sir Richard Gregory, Dr. P. Chalmers Mitchell, Mr. A. W. Pollard, and Prof. W. W. Watts, secretary.

The list will be an octavo volume containing, in alphabetical order, the titles and places of publication of all such periodicals in existence on January 1, 1900, and of all issued after that date.

Libraries in London, Oxford, Cambridge, Edinburgh, Dublin, and Aberystwyth which take in these periodicals will be indicated in the list, and, wherever possible, at least one library in the United Kingdom will be indicated for each title.

The copies will be printed on one side only to facilitate alterations and additions.

The objects of the proposed volume are: (1) To supply as nearly as possible a complete list of current scientific periodicals; (2) to indicate, where possible, at least one library where each periodical is taken; (3) to form a basis for co-operation between libraries, so that both the number of duplicates and the list of periodicals not taken in may be reduced; and (4) to enable each library to use the list for its own purposes, by placing a mark against the title of each periodical it possesses, by cutting up for a card index, etc.

The trustees of the British Museum, recognising the importance of this work to scientific research and bibliography, have consented to allow the compilation to be undertaken by the staff of the Museum. They are unable, however, to defray the cost of printing and publication.

Although the value of a list of this kind to libraries and scientific societies would be very great, it is scarcely possible that the production of so costly a work would be entertained by a publishing firm as an ordinary commercial enterprise. If, however, a sufficient number of libraries and institutions agree in advance to purchase one or more copies, when issued, the compilation of the list will be put in hand at once. Already a large bulk of material has been collected in the British Museum by various societies and by the Conjoint Board.

I shall be glad to receive by January 31, if possible, the names and addresses of institutions or individuals who will support this proposal by undertaking to subscribe for one or more copies of the list. The price per copy will be 2l. 2s. net. W. W. WATTS.

Conjoint Board of Scientific Societies,

Burlington House, London, W.1. Tel. 460 p. 542.

Old Observations Bearing on the Duration of Sunrise.

IN 1769 the French academician Le Gentil went on a journey to Mauritius, Pondichéry, and Manila to