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, œsophagus, 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 overestimate 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 broider 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*.