

amphibians. I have since sought diligently for the ancestors of birds among the early reptiles, with, I trust, some measure of success, but this is a simple problem compared with the origin of mammals which we have before us to day.

In various interviews with Francis Balfour, in 1881, at the York meeting of the British Association, we discussed the same questions, and agreed that the solution could best be reached by the aid of embryology and paleontology combined. He offered to take up the young stages of recent forms, and I agreed to study the fossils for other evidence. His untimely death, which occurred soon after, prevented this promised investigation, and natural science still suffers from his loss. Had Balfour lived he might have given us to-day the solution of the great question before us, and the present discussion would have been unnecessary.

The birds, like the mammals, have developed certain characters higher than those of reptiles, and thus the two classes seem to approach each other. I doubt, however, if they are connected genetically, unless in a very remote way.

Reptiles, although much lower in rank than birds, resemble mammals in various ways, but this may be only an adaptive likeness. Both of these classes may be made up of complex groups only distantly related. Having both developed along similar lines, they exhibit various points of resemblance that may easily be mistaken for indications of real affinity.

In the amphibians, especially in the oldest forms, there are hints of a true relationship with both reptiles and mammals. It seems to me, therefore, that in some of the minute primitive forms, as old as the Devonian, if not still more ancient, we may yet find the key to the great mystery of the origin of mammals.

C. O. MARSH.

ZOOLOGICAL BIBLIOGRAPHY.

TO THE EDITOR OF SCIENCE: I am glad to see from Mr. Bather's letter in SCIENCE (No. 207) that the recommendations of the Committee on zoological and botanical publications are not what one would be justified in inferring from the printed abstract on which my remarks were founded. All zoologists are under obligations to Mr. Bather and his associates for their labors

in the more arid, but not the less essential, branches of the subject. We hope to be still more grateful to them when their present task is completed, and, therefore, avail ourselves freely of the invitation to criticise the incomplete work in order that the completed structure may become more universally acceptable.

Nevertheless, I find even in his new presentation of the subject a lingering trace of the assumption that certain things are settled which do not appear to me to be determinate. What is the definition adopted by the committee of the phrases 'distributed privately' and 'published in the regular manner'? Upon this depends whether all that follows may be acceptable or not. How many is 'a few'? What is 'public' and what is 'private'? This sort of thing should not be left doubtful. The answers are by no means a matter of course.

When an author, to avoid two or three years' delay, pays for the prompt publication of his researches he does not, in my experience, lock up his copies in a safe and take his name out of the Naturalist's Directory. On the contrary, he at once distributes copies to the journals interested in his branch of science and to the experts in his special line, and sends a copy to Friedländer for the *Natura Novitates*, where it is advertised at a price. If he should omit the latter (a most improbable suggestion), and the paper is of interest, he will certainly be called on and glad to furnish copies to those desiring them. The author who does not desire publicity for his work, and has no known address, in my opinion is a myth. Why otherwise should he print at all?

I quite agree that the paper must be made available to those who wish to purchase it, but I do not for a moment admit that this must be solely through the Society in whose Proceedings it sees the light.

How about the highly genteel persons who publish in *éditions de luxe* of 100 copies? Such works are frequently far more inaccessible than those separata distributed by authors.

It seems to me that the committee would do well to state in the fullest detail their ideas of what constitutes publication and how this shall be registered.

My own opinion is that the sort of thing crit-

icised by Mr. Bather is very rare, if not entirely non-existent. In a tolerably active and rather long experience I have never known of an instance of the sort he mentions. Of course, there may be such, but in the lines I am familiar with I have never come across one.

Of far more practical importance to workers are the concealment by Societies of the true date of issue of their publications and the false dates of some well-known periodicals. Glaring instances of this unscientific procedure will occur to everybody. This is an evil which the committee would be generally supported in denouncing. Every issue of a periodical, or, better, every signature, should have the actual date of printing upon it. When this is delayed until a whole volume is printed the possessor of an extract is left in the lurch. The dating would cost nothing to the Societies and would often save the isolated worker hours of weary labor.

WM. H. DALL.

SMITHSONIAN INSTITUTION, December 21, 1898.

LEHMAN AND HANSEN 'ON THE TELEPATHIC PROBLEM.'

TO THE EDITOR OF SCIENCE: Professor Titchener in to-day's SCIENCE assumes that Messrs. Lehman and Hansen have performed a work of definitive demolition in the well-meant article of theirs to which he refers. If he will take the pains to read Professor Sidgwick's criticism of their results in the S. P. R. *Proceedings*, Vol. XII., p. 298, as well as the note to my report of his paper in the *Psychological Review*, Vol. IV., p. 654, he will probably admit that, owing to the fewness of the data which they collected, they entirely failed to prove their point. This leaves the phenomena in dispute still hanging, and awaiting a positive interpretation from other hands.

I think that an exploded document ought not to be left with the last word, even for the sake of 'scientific psychology.' And I must incidentally thank Professor Titchener for his admission that 'aloofness, however authoritative' (which phrase seems to be *style noble* for 'ignorance of the subject, and be d — d to it'), is an attitude which need not be invariably maintained by the 'Scientific,' even towards matters

such as this. I only wish that his admission were a little less apologetic in form.

CAMBRIDGE, MASS., WILLIAM JAMES.
December 23, 1898.

SCIENTIFIC LITERATURE.

Footnotes to Evolution. A series of popular addresses on the evolution of life. By DAVID STARR JORDAN, PH.D., President of Leland Stanford Junior University. With supplementary essays by EDWIN GRANT CONKLIN, PH.D.; FRANK MACE MCFARLAND, PH.D.; JAMES PERRIN SMITH. New York, D. Appleton & Co. 1898. Price, \$1.50.

Although the title of this book does not seem entirely self-explanatory or expressive, the lay reader will gain from a perusal of the volume a clear idea of what evolution means. He will also realize that what has been worked out in the world of animal life applies equally well in the main to man himself. Though man is an animal he is much more, and problems of existence arise in the social, moral and spiritual realms which are quite foreign to the subjects investigated by the zoologist only.

Dr. Jordan himself discusses, in a homely but clear and attractive and at times pithy and telling way, the 'kinship of life,' 'evolution: what it is and what it is not,' 'the elements of organic evolution,' 'the heredity of Richard Roe,' 'the distribution of species,' latitude and vertebræ; finally attacking such subjects as 'the evolution of the mind,' 'degeneration,' 'hereditary inefficiency,' 'the woman of evolution and the woman of pessimism,' 'the stability of truth' and 'the struggle for realities.'

While the facts of organic evolution, or, to use Geddes' term, bionomics, are discussed in an interesting way, we have given us few new facts or views, but current facts, opinions and inferences are presented in a readable form. We should naturally have expected, in the chapter on the distribution of species, to be treated to the discussion of data drawn from a study of the animals of California, for the relation of the local varieties or incipient species to their environment is very striking on the Pacific coast, and could be made very interesting and suggestive to readers not possessing a special knowledge of the matter. To be sure,