teaching in the Pittsburgh public schools, has been promoted to a full professorship of mathematics at Vassar College.

DISCUSSION

THE PLANETESIMAL HYPOTHESIS

IT is with much regret that I have read the long article by Professor F. R. Moulton in SCIENCE for December 7. I am deeply sorry that it was ever written, and still more so that, having been written, it was not withdrawn when the death of Professor T. C. Chamberlin occurred between writing and publication.

Professor Moulton sees in various passages from the writings of Sir James Jeans, Professor Eddington and myself a deep-laid scheme to rob Professor Chamberlin of the credit of the notion of disruptive approach of two stars, which is fundamental in both the planetesimal hypothesis and its derivative, the tidal hypothesis, and to claim this credit for England. When his grounds for this charge are examined, they are found to amount to (1) my omission to mention the two papers by Chamberlin and himself that contain the first accounts of the planetesimal hypothesis, or to give the dates of the three text-books quoted for fuller accounts, (2) my treatment of this hypothesis in an appendix, (3) the fact that Jeans discussed it explicitly in only one place in "Problems of Cosmogony" and did not give the original references, (4) Eddington's mention of Jeans alone in a recent article.

Now I say that to write a lengthy polemic, full of accusations of bad faith against fellow workers ("astounding tactics" is one of Moulton's expressions), on such grounds as these, is entirely indefensible. In most cases where inadequate mention of relevant work is made in scientific publications the reasons are entirely different. Usually an author simply has not seen the work at all, or has missed a point through its being in a foreign language. It has even happened that continental writers have for these reasons omitted to notice work written in English, and that the resulting work has been copied by British or American writers without addition. Often it is due to culpable, but remediable and forgivable, forgetfulness. Sometimes two authors may quite honestly differ about what is in fact relevant. In practically all cases an author is willing to repair such omissions when they are pointed out to him privately, either by immediate acknowledgment in a journal or by mention in a subsequent paper. In this I speak from experience. But in the present instance Moulton has deliberately chosen the worst interpretation and insulted his colleagues in print without the slightest preliminary effort to settle the matter in an amicable way.

The matter is made worse by the fact that the charges are, as a matter of fact, entirely trivial. Jeans and I both acknowledged indebtedness to the planetesimal hypothesis for the idea of tidal disruption, and Moulton admits this. We both gave references, Jeans to one, I to three, of the places where it is most fully treated; Moulton admits this also. Moulton's only complaint is that we did not indicate that the earliest papers appeared in 1901. But when acknowledgment of indebtedness is once made, I fail to see any circumstance that would make the interval of time of any scientific interest. Had another worker made relevant advances in the meantime it would be important to get the steps in the right order, but that does not arise in the present instance, and any one interested could extract the information by means of the clues we gave. As Moulton desires it, the first references will be inserted in the next edition of "The Earth," but they are less full and less useful than those given already. If Moulton thinks that any injury is done to Chamberlin's reputation by omission to mention his name in the Smithsonian Report, he makes an accusation of ignorance against American astronomers and geologists that would be hard to substantiate. Chamberlin, in his review of "The Earth,"[1] says that I "frankly acknowledge the parental relations of the planetesimal hypothesis to the tidal theory. This gives his [*i.e.*, H. J.'s] views good ethical standing, and with that goes unquestionable liberty to try to splice a new top on an older stump." There is no indication here of any sense of inadequate recognition. Chamberlin's objections to the tidal theory are to the nature of the alterations and not to any lack of recognition of previous work. It is strange that in Moulton's article this review and my reply to it[2] are not mentioned.

Moulton, in accusing me of having adopted the planetesimal theory as my own, says that "in every essential concept the two theories are identical." His remarks just before state, nearly correctly, the differences between the theories, and it may be inferred that he does not consider them essential. Now in this point it happens that Chamberlin agreed with me and not with Moulton. In the review mentioned above he made it perfectly clear that, whatever the differences might be, he considered them serious and fundamental: so do I. This is perhaps less surprising than might at first appear. Chamberlin and I were both interested primarily in the geophysical implications of the theory, and it is chiefly in these that the differences

[1] *Journal of Geology*, 32: 696–716. 1924.

[2] *Amer. J. Sci.*, 9: 395–405. 1925.

arise. Chamberlin's view that the earth was solid from the start and has grown greatly by accumulating solid planetesimals was to him the chief consequence of his theory, and my chief alteration was to abandon it. Moulton, being primarily an astronomer, is interested mainly in the disruption itself; here the tidal theory is more fully worked out than the planetesimal one, but fundamentally the same.

The treatment of the planetesimal hypothesis in an appendix was explicitly stated in the introduction to be for reasons of convenience alone. Anybody reading this appendix would find the differences between the theories stated and could infer the resemblances for himself, even supposing him unwilling to follow up the references given.

The reference to Eddington does not support Moulton's theory of a plot in the least. The quotation given is an extract from "The Nature of the Physical World" dealing definitely with the abundance of solar systems in space. On this point Jeans was the obvious person to quote; though the views attributed to him are not quite his present ones.[3]

With regard to the reference to Kelvin rather than Helmholtz, I believe that the contraction theory of the sun's heat was due to the latter and that the estimate of the sun's life from it was a further development due to Kelvin. The latter was what was wanted in my brief reference to this theory. I do not understand why Moulton, with his zeal for early references, gives 1899 for Kelvin's work instead of 1862.

With regard to the omission of reference to the tidal work of Michelson and Gale, the same criticism was made by Chamberlin and answered in my reply, to the effect that the work appeared irrelevant to the topics actually treated in the book. Omission of investigation of the height of the bodily tide, for reasons stated in the preface, carried with it omission of work by Kelvin, Herglotz, Love, Schweydar and myself; it is only in relation to this theoretical work that reference to Michelson and Gale would have had any utility. Their work, however, is now fundamental in the question of the fluidity of the central core, and was used by me in an investigation of this.[4] Moulton's criticisms of Laplace's theory were described by me in 1916,[5] with complete references. Moulton's apparent lack of acquaintance with these facts is remarkable. It seems unlikely that when Chamberlin distributed hundreds of reprints of his review over the world he neglected to give one to Moulton, or that the *American Journal of Science* is unknown in Chicago; and Moulton is a fellow of the Royal Astronomical Society. The only reasonable explanation seems to be that Moulton has been asleep for the last twenty years and has just awaked.

Throughout Moulton's article he seems to be under a misapprehension concerning the history of the planetesimal hypothesis in England. When I began research in astronomy and geophysics in 1913 I found a curious division of opinion about it. Astronomers mostly knew little or nothing concerning this hypothesis. The reason was not recondite. Sir George Darwin had been the only British cosmogonist in the interval, and had not actively concerned himself with the hypothesis. Other astronomers, while aware that Laplace's theory was open to serious objections, were still prepared to admit the possibility that they might be met by modification without total rejection. Jeans's work of 1916 really decided this question. I had already begun investigation of the planetesimal hypothesis in 1915, mainly to see whether it was reconcilable with modern estimates of geological time, but was met by a contradiction at the outset. But so far as this hypothesis is known to British astronomers it is due to the attention called to it by Jeans and myself.

On the other hand, the theory had such a hold on geologists that I frequently found that a discussion came to a blank stop with the remark "Chamberlin won't accept that." Attention to my work in both cosmogony and geophysics was largely held up for ten years by preconceptions based on Chamberlin's views: this was fostered by Chamberlin's own refusal even to acknowledge the existence of criticism or of an alternative theory until 1924. To deprive Chamberlin of the credit he deserves from geologists would be as impossible as to deprive Newton of the credit for the law of gravitation by giving a wrong date for the "Principia."

But the death of a great innovator is a poor occasion for a personal squabble. However fundamentally one may disagree with Chamberlin on various points, one must recognize the advances he made at others; and it is very regretfully that I have had to adopt the present time to defend myself against Professor Moulton's accusations. I should not have done so had they appeared in an astronomical or geological journal, whose readers are already familiar with most of the facts; but in a journal of general science they attract attention among readers unaware of the previous history of the subject.

HAROLD JEFFREYS

ST. JOHN'S COLLEGE,
CAMBRIDGE, ENGLAND

APPARENTLY Dr. Jeffreys would have his readers infer that my comments on the scant initial and stead-

[3] Observatory, 1925, 99.

[4] M. N. R. A. S. Geoph. Suppl. 1, 1926, 371–383.

[5] M. N. R. A. S., 77, 1916, 99–107.

ily decreasing credit given Professor Chamberlin's work by a number of British scientists would not have met with Professor Chamberlin's approval. The following are the facts respecting this point.

Within two or three years after the close of the Great War, Professor Chamberlin noted a tendency on the part of certain British scientists to adopt important essentials of the planetesimal hypothesis as their own, though under another name, and he suggested that I should undertake to clarify the history of the theory. My reply was that, while a person is under the enthusiasm of recently acquired ideas, it is natural for him to overestimate his own contribution to them and to underestimate the fact that the same ideas may have been developed and advocated long before by others, and I stated that a little time would probably cure the occasion for his complaints. As the passing years showed that my hopes were not being realized, Professor Chamberlin repeated his suggestion, sometimes quite urgently, and a number of other scientists made similar suggestions. Finally, as Professor Chamberlin's last book was nearing publication, he was deeply gratified at my decision to accede to his wishes, for he felt that the history of the origin of an order of ideas that promises to be important was being effectively fogged. The first draft of my paper was read to him and he approved it in every respect. Moreover, copies of both the original and the revised drafts were sent to four scientists who are familiar with all the facts and who are competent judges in the field. These persons were urged to point out any places, if there were such, in which my charges were not abundantly justified by incontestable facts. My paper was approved in full by all four of these competent judges.

Dr. Jeffreys suggests that it would have been more satisfactory to him if I had taken up with him in private correspondence the question of Professor Chamberlin's priority. Professor Chamberlin at various times was in communication with English scientists on the subject, the details of which I do not know, without apparent results. I do know, however, that he felt there was no hope of securing a change of tactics by this method. Certainly the petulant reply of Dr. Jeffreys in the *Am. Jour. of Science* (1925) offers no encouragement. Nor does a brief correspondence I had with Dr. Jeffreys on quite another subject nearly fifteen years ago. Nor, finally, does his present reply, in which he approves of Dr. Eddington's unqualified statement, in the article printed in *Harper's Magazine,* that Dr. Jeans was the author of the hypothesis that the planets originated from the close approach of a star to our sun. When he says in regard to the points raised in my paper that "the charges are, as a matter of fact, entirely trivial," he expresses an opinion respecting what is trivial that leaves the friends of Professor Chamberlin no recourse but to state the facts openly.

The facility with which Dr. Jeffreys occupies in rapid succession every possible position with respect to the subject is remarkable. First, he diverts the attention of his readers with an interesting discourse on the possibility of a writer not seeing the work of another, or missing a point expressed in a foreign language, but he does not make perfectly clear the relevancy of this part of his essay. He then adopts the rôle of the martyr, only to annihilate me later with his sarcasm. Next he claims that references to the work of Professor Chamberlin were adequate, though before he closes he promises to remedy some of the deficiencies by additional references in the next edition of his books. As he takes pains to point out, at the time when he appeared like a new star in the scientific firmament, English geologists were under the baleful influence of the planetesimal hypothesis, and it took him ten years to break the spell. As he also takes pains to point out, English astronomers then knew "little or nothing concerning this hypothesis" (he might have said more piquantly, if not more politely, they had been asleep), and it was he alone and single-handed who effectively called it to their favorable attention, and convinced them of its merits. Whatever the final outcome of the theory, he will have played an important rôle. When it comes to the complete omission of any reference to Professor Chamberlin in the widely read Smithsonian Reports articles, he readily explains the omissions on the ground that the work of Professor Chamberlin was so well known in the United States that to have referred to it would have been wholly superfluous. A still higher compliment of the same kind was paid Professor Chamberlin by Drs. Jeffreys and Jeans in the chapters they wrote in 1925 for "Evolution in the Light of Modern Knowledge," but Dr. Jeffreys does not take the space to emphasize the point. Specifically, Drs. Jeffreys and Jeans assumed that British readers, in 1925, were not familiar with the fact that Lucretius, Descartes, Swedenborg, Thomas Wright (of Durham, England) and Babinet had advanced certain ideas; they assumed that the British public did not know that Kant was the author of a theory of the origin of the universe, or that Laplace originated the nebular hypothesis, or that Sir George Darwin developed the theory of tidal evolution, or that Dr. Eddington investigated the internal constitution of the stars, or that Drs. Jeffreys and Jeans had written much on the tidal theory, for they give extensive references to all these scientists, particularly the last two. Their meticulous attention to ascribing credit stopped there, however, for they had no hesitation in assuming that the general British public was so thoroughly familiar with the ideas of Professor Chamberlin, many of which they reproduced in the chapters which they wrote, that to refer to him, even indirectly, would be an unwarranted waste of space. While the foregoing may be accepted as the true explanation of interesting, if unusual, methods, a critical friend of mine points out still another theoretically possible explanation of these "astounding tactics" of certain English writers, an explanation which Dr. Jeffreys mentioned only indirectly. The suggested explanation is that these "astounding tactics" have been followed because other English writers have shared with Dr. Jeffreys the assumption that I have been "asleep for twenty years," an assumption probably due in part to the fact that it has not been my habit to publish the same ideas over and over again on every possible occasion.

F. R. MOULTON

EULER'S TENSOR AND HAMILTON'S CUBIC

WE may begin with the usual Eulerian tensor constructed for arbitrary axes in i, j, k, but write it in dyadic form $\phi i = i\phi = iA - jF - kE$; etc. To refer it to the principal axes of the momental ellipsoid, the scalar function $\lambda(A, B, C, D, E, F)$ is introduced. The outcome is the determinant

$$\begin{bmatrix} A-\lambda & -F & -E \\ -F & B-\lambda & -D \\ -E & -D & C-\lambda \end{bmatrix} = 0,$$

which implies three vector equations $(\phi i - \lambda_1 i) \cdot w_1 = 0$, etc., for the three principal axes w_1, w_2, w_3.

The determinant when expanded in powers of λ, with the coefficients expressed as volumes, is $\phi i \cdot \phi j \times \phi k - \lambda \Sigma i \cdot \phi j \times \phi k + \lambda^2 \Sigma i \cdot j \times \phi k - \lambda^3 = 0$ where Σ refers to the three dimensions i, j, k. If, therefore, the initial volume is $i \cdot j \times k$, the coefficients of λ^0, λ, λ^2, λ^3 are identical, respectively, with m, m_1, m_2, 1, in Hamilton's cubic of the scalar dyadic ϕr. Of course this is not to be wondered at; but it ought, I think, to be more frequently accentuated; for a problem in rigid dynamics thus takes the form appropriate to a homogeneous strain applied to an initial volume, and this is somewhat unexpected.

CARL BARUS

BROWN UNIVERSITY

NOTICE TO ZOOLOGISTS ON THE POSSIBLE SUSPENSION OF THE RULES IN THE CASE OF NYCTERIBIA LATREILLE

IN accordance with the provisions governing possible suspension of the rules, the undersigned has the honor to invite the attention of the zoological profession to the fact that application for suspension of the rules has been made in the case of *Nycteribia* Latreille, 1796, monotype *Pediculus vespertilionis* Linn., 1758. The commission is requested to set aside the monotype designated in 1796 and to validate *Nycteribia pedicularia* 1805 as type of *Nycteribia*. *Pediculus vespertilionis* Linn. was based on an acarine (described and figured by Frisch, 1728) which is now classified in *Spinturnix*. Latreille was dealing with an insect which he erroneously determined as *Pediculus vespertilionis*. Unless the rules are suspended *Nycteribia* should be transferred from the Diptera to the Acarina and should supplant *Spinturnix*; this would cause extreme confusion and upset generic and supergeneric nomenclature which has been accepted without challenge for about a century.

A vote on the foregoing proposition will be delayed until about January 1, 1930, in order to give zoologists interested in the case ample opportunity to express their opinions, *pro* or *con*, to the International Commission on Zoological Nomenclature.

C. W. STILES
Secretary of Commission

U. S. PUBLIC HEALTH SERVICE,
WASHINGTON, D. C.

SPECIAL CORRESPONDENCE

EINSTEIN'S APPRECIATION OF SIMON NEWCOMB

THE following letter, which has recently been deposited in the manuscript division of the Library of Congress, will be of value to American scholars, especially to those interested in the physical sciences. The letter was written by Dr. Albert Einstein in response to an inquiry from Mrs. Josepha Whitney, of New Haven, Connecticut, daughter of the late Simon Newcomb, and was forwarded by her to her sister, Dr. Anita Newcomb McGee, of Washington, D. C., for deposit with the Newcomb papers in the Library of Congress.

In view of the present interest in the new work of Dr. Einstein, Dr. McGee has asked to have the letter translated and published. As the letter has an important bearing upon the history of astronomy in America and the particular part Newcomb had in this development, it is herewith published with Dr. Einstein's permission, and I therefore take pleasure in sending it to SCIENCE for publication.

The letter states briefly the history of the problem of perturbation in a system of three bodies in