The Origin of the Solar System

A COPY of a paper published in the *Messenger of Mathematics* in March 1898, entitled "On the Oscillations of a Heterogeneous Compressible Liquid Sphere and the Genesis of the Moon; and on the Figure of the Moon", has been sent to the Editor of NATURE by Mr. W. F. Sedgwick, who graduated from Trinity College, Cambridge, in the year 1894. Mr. Sedgwick has also submitted the manuscript of an unsuccessful essay sent in by him for the Smith's Prize at the end of the year 1895 "On the Vibrations of a Heterogeneous Liquid Sphere, with Applications to the Solar System; and on the Elastic Solid Theory of the Earth". He states that the applications to the solar system on pp. 170-171 of his published paper were based on considerations set out in much greater detail in his essay. His communications throw additional light on the history of ideas concerning the origin of the solar system. His paper has been generally overlooked in this connexion—not unnaturally, considering the somewhat distant relation between its subject and its setting—but there is no doubt of its relevance, and Sir James Jeans, in a letter to the Editor, remarks: "I regret that Mr. Sedgwick's work had entirely escaped my notice until my attention was recently directed to it. His theory of the origin of the solar system appears to have nothing in common with my own, except that both postulate tidal actions—of very different kinds. But I very gladly acknowledge that Mr. Sedgwick's hypothesis of a tidal origin for the solar system was earlier than my own."

In view of the interest and importance of the question, the Editor considers that a general survey of the hypotheses which have been advanced to account for the existence of the planets is desirable, and he has asked me to make such a survey. Presumably the choice has fallen on one so ill-qualified because a few years ago (NATURE, April 13, 1929), in reviewing a book by the late Prof. Chamberlin, it unexpectedly became my duty to comment on a controversy which had arisen in connexion therewith. Unwillingness to shirk any legitimate consequence of that review must be my excuse for accepting this further charge, but before proceeding with it there are three explanatory remarks which I wish to make.

First, this article is not to be regarded as the result of an exhaustive examination of scientific and quasi-scientific literature: it is simply an ordered account of material, most of which has been placed in my hands *ad hoc*. No claim on behalf of a hypothesis is more questionable than that of novelty. An idea which the author thinks original as likely as not proves aboriginal, and no experienced person would be unduly surprised if it transpired that modern ideas of the solar system were held by some mad precursor of Thales. If this article brings to light material so far overlooked, it will serve a useful purpose.

Secondly, no account is taken of other than published material. However strong the presumption might be that ideas in a certain unpublished paper antedated similar ideas presented in a later published paper, it is still a presumption and is consequently ignored. For the same reason no attempt is made to judge of independence or otherwise of thought. The dates of presentation or publication being given, the reader can form his own opinion on such matters.

Thirdly, the material is presented without comment on its value. It is admittedly an important matter whether a criticism of, say, Laplace's nebular hypothesis, is a legitimate one or not, but it is a matter which must be left to others to decide. Similarly, it is an important matter whether a casual mention of an idea is entitled to priority over a slightly later thorough exploration of it, but that also must be left to others to decide. In brief, this article is nothing but a probably incomplete statement of facts relating to dates of publication of relevant matter.

According to Dr. Harold Jeffreys (*Observatory*, **52**, 173; 1929), Buffon's "Natural History" (1750) contains the suggestion that the prevalence of direct revolution among the planets might be due to the system having arisen from a grazing impact between the primitive sun and a comet. (By a 'comet', it should be said, Buffon understood something much more dense and massive than a comet as we now picture it.)

Five years later Kant, in his "Universal Natural History and Theory of the Heavens" (1755), gave another explanation, bearing strong resemblances to Laplace's later and more famous nebular hypothesis. There were, however, some important differences, among which, perhaps, the chief was that Kant imagined the angular momentum of the system to be developed during its evolution, whereas Laplace put the angular momentum among the original data.

Laplace's hypothesis, which was published in his "Exposition du système du monde" (1796), is too well known to need description. Its author does not mention Kant's work, but devotes a few lines to Buffon, whose suggestion he rejects.

On the whole, the nineteenth century was satisfied with Laplace's hypothesis, but there were some dissentients, as well as some supporters, who offered what, in view of later work, are interesting additions. Thus Croll (*Phil. Mag.*, May 1868, p. 373; "Stellar Evolution and its Relations to Geological Time", 1889) conjectured that the original nebula which Laplace postulated might have been generated by the collision of two dark stars. Proctor ("Other Worlds than Ours", 1870), on the other hand, attacked Laplace's hypothesis and advocated the idea that the planets were built up by aggregation of meteorites. The revolt against Laplace was continued by Bickerton, who (*Trans. New Zealand Inst.*, **12**, 193-194; 1879, and subsequent papers) substituted for the nebular hypothesis an idea, similar to Buffon's, of a grazing collision between two stars,

followed by the building up of planets by accretion of small bodies.

The next idea appears to be due to Mr. Sedgwick, who, in his paper in the *Messenger of Mathematics* (1898), referring to the birth of the initial planet and satellites, wrote: "The initial satellite in each subsystem might be produced in the same manner as the moon on the hypothesis suggested" (that is, by tidal action of the sun coinciding in period with the natural oscillations of the primitive planets), "whilst the initial planet of the system might be caused by a similar, or different, agency in long distant periods", the "similar agency" being tidal action coinciding in period with the natural oscillations of the nebulous sun. Mr. Sedgwick's earlier unpublished Smith's Prize essay cannot be considered here for reasons already stated.

On Aug. 20, 1897, however, Prof. T. C. Chamberlin, at the Toronto meeting of the British Association, gave an address on "A Group of Hypotheses bearing on Climatic Changes"—this was published as a paper in the *Journal of Geology* (**5**, 653-683; 1897). Chamberlin criticised the nebular hypothesis—in particular, the idea that the earth was originally molten or gaseous—and proceeded to "follow the hypothetical growth of a planet built up by the slow aggregation of small bodies which join it at low velocities and develop a minimum heat". This, so far as we can gather, was the primal germ of what later came to be known as the planetesimal hypothesis. Chamberlin, whose arguments are geological, makes no reference to the similar astronomical ideas of Proctor and Bickerton.

About three years later, Chamberlin (*Journal of Geology*, **8**, 58; 1900) and, almost simultaneously, Prof. F. R. Moulton (*Astrophysical Journal*, **11**, 103; 1900) launched a more systematic attack on Laplace's hypothesis and concluded that it was definitely untenable. Chamberlin and Moulton were admittedly working in collaboration. Shortly afterwards, Chamberlin, in a paper published in the *Astrophysical Journal* (**14**, 17; 1901), and repeated in the *Journal of Geology* (**9**, 369; 1901), considered in some detail the consequences of a close approach of two stars to one another and showed that such an event might reasonably account for the existence of spiral nebulæ, meteorites, and comets. No suggestion was made, however, that the solar system might have originated in this way. Moulton's help was acknowledged, and so far as objective evidence is concerned the position may be summed up by saying that at this time Chamberlin and Moulton were collaborating on problems of cosmogony, including in particular the origin of the solar system, and that the influence of two near cosmic bodies on one another was among the problems they considered. A casual remark in 1900 by Keeler (*Astrophysical Journal*, **11**, 348) points to still more definite conclusions. Speaking of his studies of the spiral nebulæ, he says:

> If . . . the spiral is the form normally assumed by a contracting nebulous mass, the idea at once suggests itself that the solar system has been evolved from a spiral nebula, while the photographs show that the spiral nebula is not, as a rule, characterised by the simplicity attributed to the contracting mass in the nebular hypothesis. This is a question which has already been taken up by Professor Chamberlin and Mr. Moulton, of the University of Chicago.

It was not until 1904, however, that the embryonic planetesimal hypothesis actually came to birth. In the Year Book No. 3 (pp. 195-254) of the Carnegie Institution of Washington for that year (published in January 1905), Chamberlin, remarking that "a complete statement of the planetesimal hypothesis has not yet appeared in print", proceeded to supply one. His account reveals the hypothesis as primarily geological in character. Its central feature was the idea that the earth was built up by the aggregation of a large number of "planetesimals"; the production of these bodies from solar matter drawn out by a passing star was an inessential subsidiary hypothesis. Chamberlin says:

> As the basis for developing the typical form of the planetesimal hypothesis, I have assumed that the parent nebula had a planetesimal organisation from the outset. . . . To develop the hypothesis as definitely and concretely as possible, I have further chosen a special case from among those that might possibly arise, viz., the case in which the nebula is supposed to have arisen from the dispersion of a sun as a result of close approach to another large body. The case does not involve the origin of a star nor even the primary origin of the solar system, but rather its rejuvenation and the origin of a new family of planets. The general planetesimal doctrine does not stand or fall with the merits or demerits of this special phase of it, but to be of much real service in stimulating and guiding investigation, a hypothesis must be carried out into working detail so that it may be tested by its concrete and specific application to the phenomena involved, and hence the reason for developing a specific sub-hypothesis. This particular sub-hypothesis was selected for first development (1) because it postulates as simple an event as it seems possible to assign as the source of so great results, (2) because that event seems very likely to have happened, (3) because the form of the nebula supposed to arise in this way is the most common form known, the spiral, and (4) because spectroscopic observations seem at present to support the constitution assigned this class of nebulæ, . . .

From that time onwards the hypothesis has been developed in a succession of papers; its present state is described in Chamberlin's last book, "The Two Solar Families" (1928). The hypothesis of the passing star is there presented as essential to the theory, but the precise stage at which it attained that status does not concern us here.

Sir James Jeans seems first to have turned his attention to the problem in 1901. In that year he published (*Phil. Trans.*, A, **199**, 1; abstract in *Proc. Roy. Soc.*, **68**, 454; paper received, June 15, 1901) a detailed consideration of an aspect of Laplace's hypothesis under the title "The Stability of a Spherical Nebula". At the end of this paper occurs the following passage:

> In conclusion, two particular cases of 'irregularities' may be referred to. If the nebula is penetrated by a wandering meteorite, at a moment at which it is close to a state of instability, the presence of the meteorite

will constitute an irregularity, and may easily result in the formation of a satellite. And if a quasi-tide is raised in the nebula by the presence of a distant mass, the same result may be produced.

The suggestion contained here made no further appearance until it had blossomed into the formal 'tidal theory'. In a paper received by the Royal Astronomical Society on Nov. 3, 1916, and published in *Mem. R.A.S.*, **62**, part 1, 1917, Jeans considered "the motion of tidally distorted masses, with special reference to theories of cosmogony".

In recent years [he wrote] the position of this hypothesis [Laplace's] has been challenged by speculations based ultimately upon the conception of tidal forces providing the required tendency to separation, the most complete and definite of these speculations being found in the Planetesimal Hypothesis of Chamberlin and Moulton. In the present paper I have attempted to follow up mathematically the changes in a mass of matter as the tidal forces acting on it continually increase.

As a result of the investigation the following conclusion was reached:

The genesis of our solar system can very probably be attributed to tidal action; the explanation leaves room for a good deal of uncertainty in matters of detail, but does not demand anything impossible or very improbable. The evidence we have been able to obtain suggests that a system generated by tidal action might quite well have characteristics, both qualitative and quantitative, such as are observed in our system. The origin which seems most probable is not that of the planetesimal hypothesis.

Further developments—showing, like the planetesimal hypothesis, some modification of the original conjectures—are recorded by Jeans in "Theories of Cosmogony and Stellar Dynamics" (1919), Supplement to NATURE, March 1, 1924, and "Astronomy and Cosmogony" (2nd edition, 1929).

In *Ast. Nach.* 4308 (Jan. 1, 1909), T. J. J. See advanced the idea that the planets were not detached from the primitive sun but were "captured, or added from without, and have had their orbits reduced in size and rounded up under the secular action of the nebular resisting medium" formerly pervading our solar system. The idea was further developed in "Researches on the Evolution of the Stellar Systems", vol. 2, p. 357, 1910.

Dr. Harold Jeffreys discussed the planetesimal hypothesis in a paper in *Mon. Not. R.A.S.*, December 1916 (**77**, 84). He considered that it was open to the objection that the planetesimals would be fused and volatilised by collisions, and so could not build up the planets by the slow aggregation postulated by Chamberlin. In *Science Progress* (July 1917) he outlined some considerations on the early history of the solar system which were developed more fully in *Mon. Not. R.A.S.*, **78**, 424, April 1918. Starting, like Chamberlin, from geological data, he came to the opposite conclusion; namely, that the planets were formerly liquid or gaseous. Dismissing the nebular hypothesis of their origin by an argument described as "a modification of that of Jeans", he turned his attention to the tidal theory.

This theory [says Jeffreys] forms part of the Planetesimal Hypothesis of Chamberlin and Moulton; its dynamical possibility has been proved by Jeans; and I have shown here and elsewhere [in *Science Progress*] that the system it would lead to would resemble our own in several striking features. It will be definitely adopted as a postulate in the present paper.

Jeffreys's further work is summarised in his book "The Earth" (2nd edition, 1929). The only subsequent development of significance here is the substitution (in *Mon. Not. R.A.S.*, **89**, 636, 731; 1929) of an actual collision for a close approach between the sun and the visiting star.

We end, therefore, where we began; the latest, like the earliest, known theory attributes our existence to the impact of another cosmic body on the primitive sun. HERBERT DINGLE.

The History of Ergot

ERGOT has been under active scientific investigation for two generations, and has provided sociological problems for ten centuries; its secrets are now almost all laid bare, and at the moment when investigators are likely to turn aside from its study to that of other subjects, Prof. Barger has come forward to write its biography.* Usually a subject of so much interest attracts the notice of the mere collator; fortunately, ergot has not done so, and it has been left for the story to be written by a distinguished chemist, who may be congratulated on having sufficient imagination to realise how rich a story it could be. Few even of those who have worked on ergot would have guessed it.

The main importance of ergot in the past was not medicinal but as a cause of epidemic disease; instead of a healing draught, it was a scourge. Strange as it may seem to us in Great Britain, "rye is still the chief cereal in a large belt of Europe extending from Holland across Northern Germany, Czecho-Slovakia, Austria, Poland, and Central Russia", and in Poland, for example, four times more rye is eaten than wheat. From the ergot present in the rye have come the many epidemics of ergotism.

Ergotism occurs in two forms, known respectively as gangrenous and convulsive; in the first form the symptoms are due to the effect of ergot on the blood vessels, as a result of which the blood supply to the extremities is cut off, so that gangrene occurs and the limb drops off; loss of the leg below the knee is common. Convulsive ergotism, on the other hand, is not an affection of the blood vessels, but of the central nervous system; it is characterised by the appearance of areas of degeneration in the spinal cord, and the symptoms are convulsive seizures.

* Ergot and Ergotism: a Monograph based on the Dohme Lectures delivered in Johns Hopkins University, Baltimore. By Prof. George Barger. Pp. xvi+279+6 plates. (London and Edinburgh: Gurney and Jackson, 1931.) 15*s.* net.