loaded with an excess of classroom assignments, but it is one of our collective duties as a society to show to college administrative officers a better way. Students who possess ambition and courage to study, reflect and learn under guidance ultimately may go far; but those who insist that they are paying their money "to be taught" are out of place in an engineering school.

Can any one doubt that the influence on their students and in engineering education of, for example, Irving Church, Mansfield Merriman and George Swain was in large part due to their resourceful originality and investigative spirit, standing foremost day by day as an example before their students? Engineers must deal with physics, economics and psychology, materials and forces, the philosophy of wealth (in the technical sense), and man. It is not a mental accumulation of facts alone that fits young men for doing this, but an understanding of interrelations of facts and the methods of detecting and identifying facts is of the essence. Our theories of action, relating to man or to mechanical design, are formulated from relatively few fundamental facts associated with a multitude of keenly recognized permutations of their relationships.

Detecting and identifying facts and discovering their varied relationships is research. Properly directed research is a potent instrument to arouse the ambitions and exercise the reasoning powers of students. It also teaches them to use foresight in planning and a responsibility for carrying through. It exposes them to early observation of the many faceted purposefulness and everlasting persistence of nature. It may result in important discoveries, which is another function. Universities and colleges (including engineering schools) have several functions to perform. One of these is the search for truth, and in science we interpret this as seeking for new facts, disclosing previously unobserved interrelations, and more fully illuminating facts and relationships previously announced but still partly obscure. In engineering we weave this function into a fabric along with economics and psychology and have a still more complex compass of research than characterizes any exclusive science. The field is magnificent and must be cultivated. Research is an inspiring part of the life of colleges. The spirit for establishing research laboratories and foundations in engineering schools is a presage of good educational spirit in the schools. The proper interpretation of the situation is important.

We must remember that universities and colleges have education for their prime function; and it being my thesis that research is part of engineering education, students of suitable advancement should be invited into the research precincts to there take up tasks. To set up research laboratories and libraries and bar out students of suitable advancement from pursuing work therein would be inappropriate to university ideals and to the weaving of the best fabric of engineering education. Research laboratories, dedicated to and carrying on fundamental research of high order, gloriously serve the advancement of mankind through their investigations and discoveries. When so directed that the atmosphere of independent achievement is allowed to spread into the haunts of upper-class students, as well as to graduate students and teaching staff, they collaterally make large contributions to educational results and thus their value to civilization is multiplied.

THE UNSOLVED RIDDLE OF THE SOLAR SYSTEM

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The public is being played upon and utterly misled by the dreamery of the rival mathematical astronomers and physicists.—*Professor Henry E. Armstrong, in Nature, Aug. 23, 1930, p. 275.*

Dr. Harold Jeffreys, university lecturer in mathematics and fellow of St. John's College, Cambridge, expounds in *The New York Times* of May 3 his newly developed theory that the planets of the solar system owe their origin to a shearing collision between the sun and another star. The article is a summary of two papers which Dr. Jeffreys contributed to the *Monthly Notices* of the Royal Astronomical Society, Vol. LXXXIX, Nos. 7 and 9, 1929. Because this theory, in contrast with its original rather unexcited reception by scientists, is now presented as something of exceptional importance; and because the present version indicates, as the original papers did not, an apparent final abandonment of Sir James Jeans's famous development of the "tidal theory," by the man who was long Sir James's chief supporter, an examination of both theories may prove to be worth while.

The essential feature of the new theory is that at the end of the collision there was drawn out between the two bodies a ribbon of matter in which circulation had been set up by their opposed velocities. This ribbon presently broke up and the parts condensed into planets, endowed with rotation by the circulatory motion of the ribbon. Dr. Jeffreys assumes that the mass of the star was comparable with that of the sun and that their surface of separation approached the center of the sun within a distance equal to about half the sun's radius. In the absence of any assignment of definite mass and radius to the star, we shall in this discussion provisionally take them as equal to the sun's.

In the first of the original papers Dr. Jeffreys says that "the velocity of escape of a particle from the sun's surface is 450 km/sec.," and that the relative velocity of the sun and the colliding star would be of this order, or 400 km/sec. This figure for relative velocity still stands, two years later, in his article in *The New York Times*. Parabolic velocity for a particle at the surface of the sun is 620 km/sec., not 450 km/sec., as Dr. Jeffreys states, and for a large body like a star it would be much greater—about 875 km/sec. in the case here considered. So we have the parabolic velocity for a particle understated by 170 km/sec., the larger parabolic velocity for the star ignored, and a relative velocity assumed that is less than half the actual parabolic velocity. It is difficult to understand how Dr. Jeffreys reached the conclusion that such planets as ours could have emerged from conditions which would imply that the star was a permanent member of the solar system. If the laws of motion of two bodies did not provide sufficient evidence of such an association in this case, we should still have Dr. Jeffreys's word for it. Discussing the tidal theory, on page 26 of the second edition of "The Earth," he says: "But the velocity of the sun relative to the star was at all instants greater than the parabolic velocity, otherwise the sun and the star would still be associated." The necessity for a larger relative velocity is made strikingly apparent when, taking it at 400 km/sec., it is found that the collision would have established the sun and the star in an elliptic relative orbit, of which the major semi-axis would have been less than the sun's radius. In such an orbit the sun and the star would have tended to revolve in contact until they merged into one body—a process likely, however, to have been disturbed by internal reactions. The possibility that in the collision theory Dr. Jeffreys may have had in mind the sun in another than its present state is excluded by his assignment to it of its present mass and radius.

Dr. Jeffreys has made an important correction as to the duration of the collision. In the original paper this was given as about half an hour, arrived at by dividing the radius of the sun by the relative velocity —an error that leaps to the eye. He now writes: "The star would travel the diameter of the sun and be clear again in about an hour." This greatly affects the planet-forming mass that "trailed off after the star," at the end of the collision, and other elements of the problem as well. He gave this mass originally, and gives it now, as equal to 1/500 of the mass of the sun, but, by his own formula for one of its factors, as will be shown below, doubling the distance traveled by the star during the collision would double the thickness and mass of the planetary ribbon. It would also double the volume and mass of an equivalent sphere of the same density.

The rotational period of the "equivalent sphere" is employed by Dr. Jeffreys to show that his collision would impart a sufficient rotational period to the great planets. One of the chief difficulties in all theories of the origin of the solar system is that of securing for the planets, originally fluid, their observed rotation, or, indeed, any rotation at all. It is because he thinks it offers means of solving this difficulty that Dr. Jeffreys now revives the collision theory, 180 years old, of Georges Louis Leclerc, Comte de Buffon, who, in addition to theorizing about everything, translated Newton's Fluxions into French, and thereafter, quite appropriately, died of calculus.

Dr. Jeffreys writes the equation

$$\pi\rho a^2 d.du = \frac{8}{15}\pi\rho b^5\omega,$$

where the left-hand member is the angular momentum of the layer of matter displaced by the collision to form the planetary ribbon, the axis being taken as passing through the center of mass and perpendicular to the general motion, and the right-hand member is the rotational angular momentum of the "equivalent sphere"; ρ the density, a the radius of the sun, d the thickness of the layer, whose mass varies with it; u the relative velocity of the sun and the star, b the radius of the sphere and ω its angular velocity. This equation reduces to

$$\omega = \frac{a^2d^2u.}{\frac{8}{15}b^5}.$$

On his original calculation this led to a rotational period of 8 hours for the "equivalent sphere." There is, however, in the published paper a curious tangle of errors that seriously affects his result. In determining d, the radius of the sun is treated as the diameter of the interface formed by the star's passage through the sun—or, as Dr. Jeffreys puts it, the distance traveled by the star (within the sun's boundary, of course) during the collision—but it is also treated both as the radius and as the diameter of the interface in factors which determine b; and, in raising b^3 to b^5 from the equation $b^3 = \frac{4}{3}a^2d$, the numerical coefficient $\frac{4}{3}$ has been carried unchanged into the denominator of the formula, making 4/3, instead of $(4/3)^{5/3}$, a factor of the angular velocity. There is, besides, a considerable error in the final computation, due to the use of round, instead of accurate, numbers. The correction of the errors mentioned reduces the period to about 6½ hours, but is of no consequence while the assigned relative velocity remains so small as to assure the permanent association of the sun and the star.

Dr. Jeffreys is free to assign a larger relative velocity, but if he does so he will diminish the duration of the collision, increase the angular velocity of the "equivalent sphere," and correspondingly reduce its rotational period, the approximation of which to the 10-hour rotational periods of the great planets he seems to consider important. When all corrections are made, including the necessary increase in the relative velocity and the substitution of the true dimensions of the interface—these, as newly assigned by Dr. Jeffreys, are still incorrect—it is found that the rotational period of the "equivalent sphere" falls to about 2½ hours, or only one fourth the value aimed at.

Even if Dr. Jeffreys's assigned values had been such as to make the rotational period of the "equivalent sphere" equal to the observed average rotational period of the great planets—which would have required the relative velocity of the sun and the star to be less than 220 km/sec.—the plausibility of the demonstration would have been illusory. For it is readily shown that if the mass and the angular momentum of such a body had been distributed proportionally to form several spheres, including the great planets, the rotational periods of the latter could not have been, as they are now, nearly equal, but would have differed widely, and all would have been much less than they are observed to be.

Substituting in the formula for the angular momentum of the planetary ribbon the corrected values of various factors, which the present discussion has shown, or will show, to be necessary, the least possible value for the relative velocity—875 km/sec., in the case considered—would have given to the planetary ribbon an angular momentum of $8.13(10^{46})$ c. g. s. units. Neglecting the insignificant participation of the smaller planets, this is 10¼ times the total rotational angular momentum of the four larger planets, which is, by a computation that necessarily leads to a highly excessive result, $7.94(10^{45})$ c. g. s. units. The rotational angular momentum of each of the great planets is computed as if it were a homogeneous sphere of the same mass and mean radius as the planet. Thus the considerable increase of density toward the center is not taken into account. The true rotational angular momentum of each planet and the total for all the planets are therefore much less than they are computed to be. If data existed for an exact computation, the ratio 10¼ might be doubled, or even tripled. Taking the mass of the ribbon as equal to about 1/500 of the sun's (why this is correct, in spite of Dr. Jeffreys's conflicting data, will be shown below), it would have contained a little less than 1½ times the mass of the great planets. Thus a minimum of more than seven times their total rotational angular momentum would have been available for them. Its distribution in proportion to mass—any other distribution would have to be accounted for—would imply that the planets came into being rotating so fast that their equatorial surface velocities of rotation would have exceeded the parabolic velocities, or so-called "velocities of escape." The result would have been for Jupiter 1¼ times, for Saturn 2.4 times, for Uranus 9.6 times, and for Neptune 8.7 times the parabolic velocities at their respective equatorial surfaces. The effects of such a distribution of a little more than two thirds of the ribbon's angular momentum among the great planets, if it could occur, are shown in the subjoined table. The data used in the calculations are those given in Russell, Dugan and Stewart's "Astronomy."

	*Angular Momentum of Rotation		†Velocities Compared		Rotational Period	
	Observed, less than	By theory	Rotational at equator, by theory	Velocity of escape	Observed (Hours)	By theory (Minutes)
Jupiter	6.59	39.8	74	60	9.83	98
Saturn	1.28	11.9	87	36	10.23	66
Uranus	.04	1.8	202	21	10.7	13
Neptune	.03	2.1	200	23	15 ?	13?
	7.94	55.6				

* The numbers in the first two columns, multiplied by 10^{45}, represent c. g. s. units.

† The numbers in the third and fourth columns represent kilometers per second.

The great disparities revealed by the table show that the fundamental assumptions of the theory prohibit the formation of planets. It is possible that Dr. Jeffreys's formula overstates the angular momentum of the planetary ribbon. But any reduction which might be effected by correction of the formula would probably be more than offset by the reduction that an accurate computation, if it were possible, would make in the rotational angular momentum of the great planets.

If we accept Dr. Jeffreys's formula for the angular momentum of the planetary ribbon, a relative velocity of 125 km/sec. for the sun and the star would have enabled it to supply to a system of planets the total rotational angular momentum of our system. In his recent article Dr. Jeffreys said his theory was supported by new mathematics only recently made available. If he has any mathematics that will reconcile the necessary reduction of the relative velocity to 125 km/sec., to equip our planetary system with the correct rotational angular momentum, and the equally necessary increase of that velocity to 875 km/sec., to dissociate the sun and the star after the collision, he possesses something that reduces the theory of relativity to insignificance as a modifier of Newtonian mechanics.

If the difficulties thus far described could be removed, would the mass of the supposed planetary ribbon be adequate to the formation of our planetary system? This mass is fixed by a quantity which, as Dr. Jeffreys points out, has been "determined by experiment"—the pure number .002, coefficient of a product of factors of the problem determining the thickness of the layer to which the opposed velocities of two fluids may communicate vorticity, when the difference of the velocities is high, as it is in this case. The mass of the ribbon is the product of the area of the interface and the thickness and density of the displaced layer. The momentum communicated to the layer in time t is $.002\rho u^2 t$ per unit area, where ρ is the density, which is taken as approximately equal to the mean density of the sun. But ut equals the distance traveled by the star during the collision. To this quantity Dr. Jeffreys originally gave the value a. Substituting this value in the formula for momentum, he found that a velocity u would be communicated to a layer whose thickness was $.002a = d$. Using this value of d in his formula for the mass of the layer, $\pi\rho a^2 d$—here, as will be seen, the diameter of the interface has been doubled, without explanation—that mass becomes $.002\pi\rho a^3$. Dividing this by the mass of the sun, $\frac{4}{3}\pi\rho a^3$, the mass of the planetary ribbon comes out 1/666 of the mass of the sun, instead of 1/500. Since he now doubles the distance traveled during the collision, making it $2a$, instead of a, he doubles the value of ut, and so doubles the thickness and mass of the layer. The mass should now be 1/333 of the sun's, instead of 1/500.

There is, however, another discrepancy to be considered, which also affects the mass of the planetary ribbon. Dr. Jeffreys says of his estimates, "All rest on the assumption that the area of contact had a radius comparable with the radius of the sun." So he writes the area of the interface equal to πa^2, giving it a radius equal to that of the sun. But the interface, whose center he places approximately at the midpoint of the sun's radius, would have a radius of about half the chord perpendicular to the sun's radius at that point, that is, about $.866a$. This is comparable with a, of course, but if we substitute its square for a^2 in πa^2, we reduce this factor of the planetary mass to its proper value, which is three fourths of that given by Dr. Jeffreys. The quantity ut is now the diameter of the interface, or $1.73a$, and the momentum will impart a velocity u to a thickness $.00346a = d$. Using this new value of d in the formula for the mass of the layer, and remembering that $.75a^2$ must be substituted for a^2, the mass of the layer becomes $.0026\pi\rho a^3$. Dividing this by the mass of the sun, we find the final corrected total mass of the planetary ribbon to be equal to 1/513 that of the sun. This is practically the value Dr. Jeffreys gave two years ago, which could not be obtained from the data he gave then; it is also his present value, which likewise, can not be obtained from the data he gives now, except with the corrections here indicated.

The curious agreement between Dr. Jeffreys's result and the one here derived, for the mass of the planetary ribbon, may have been due, in the first instance, to his taking the ratio of the two masses as roughly equal to his original 1-to-500 ratio of d to a, or to an accidental canceling of errors and omitted factors in his first calculation; but the fact that he left this value unchanged in his recent article, notwithstanding the doubling of the distance traveled by the star during the collision, suggests that a revised calculation has led him to substantially the same result as that found here. In any case, it appears that a mass equal to 1/500 of the sun's would have been displaced to form the planetary ribbon, if the interface penetrated the sun to the midpoint of its radius and the average density of the matter adjacent to the plane of the interface equalled the mean density of the sun. On the latter point Dr. Jeffreys says: "Its density would be comparable with the mean density of the sun" (second paper, p. 737); and he had previously taken the final density to be "the same as the initial one" (first paper, p. 640). These and other parts of his discussion indicate that in estimating the penetration of the interface and the average density in its neighborhood he has taken into account the compression that would occur during the collision.

The computed mass of the displaced layer, although it is about 1½ times the mass of our planets, is nevertheless inadequate to the formation of our system. As the two bodies began to separate, the layer, suddenly released from compression, would expand transversely and much of it might be lost in interstellar space; and under various conditions, the planets, if any were formed, might go to the star or be divided between the sun and the star. Moreover, it will appear when Jeans's theory is discussed—and largely by Dr. Jeffreys's own contributions to that theory—that the formation of our system of planets would require the extraction of much more than their mass from the sun. The collision theory, like the tidal theory, requires not only matter enough to form the planets, but an allowance for such matter as would fall back into the parent body and an additional allowance for the "resisting medium," whose function it is to modify the highly eccentric original orbits of the planets. Each of these allowances, in the tidal theory, requires, as we shall see, a mass about equal to the mass of all the planets, and these requirements, at least for the resisting medium, can not be much less in the collision theory.

If the planetary ribbon "stretched out all the way from the sun to the star," "or three times the radius of the sun," as Dr. Jeffreys says, there is no apparent reason why, at its rupture, a system of planets should not have formed around the star as well as around the sun. If the division of the ribbon between the two were equal and none of the matter were lost in interstellar space, the part available for the formation of the planets in the solar system could hardly have been more than 1/1000 of the sun's mass, or almost exactly the mass of Jupiter.

It should perhaps be noted that when Dr. Jeffreys derives the duration of the collision from a division of the diameter of the interface by the relative velocity, we have taken his meaning to be something that might be called "effective duration." Actually, from the moment of making to the moment of breaking contact, under the conditions laid down, a star of equal mass and radius with the sun would travel twice the diameter of the interface. But the duration of contact and the density of the displaced layer, which has been averaged and treated as constant, both vary from zero to a maximum value for different parts of the interface, so as to affect the same mass as if the density were constant, the area of contact that of the interface, and the duration very nearly that required to traverse the diameter of the interface.

Jeffreys's Work on the Tidal Theory

It may be instructive to consider for a moment Dr. Jeffreys in his now abandoned rôle of upholder of Sir James Jeans's theory that the planets condensed from a stream of matter drawn out of the sun by the gravitational power of a passing star, without contact. In this theory some restrictions have to be imposed upon the mass and density of the passing star. Jeans holds, in the only example he has given, which is "made to order" to fit the theory, that a mass twice the sun's is sufficient, at a proper distance, to draw out of the sun enough matter to form the planets.

Jeffreys reduced Jeans's theory to an extremely simple formula. Representing the mass of the sun by M, that of the passing star by M', the radius of the sun by a, and the distance between the centers of the two bodies at closest approach by R_0, he wrote:[1]

$$\frac{M'}{R_0^3} > \frac{M}{2a^3},$$

saying: "This is the condition that the mass M shall be broken up." It is extraordinary that upon arriving at this formula its author did not at once reexamine its foundations or abandon Jeans's theory. For hardly more than inspection is required to discover that, unless M' is much greater and of much higher density than M, R_0 will be so small that a collision must occur. If we test the formula, assuming the density of the sun and that of the star to be equal, we shall find that if M' be less than 57 times M, there would be a collision. At the latter value of M' the two bodies would make a momentary grazing contact at a point on their normal surfaces. We have here neglected the tidal protuberances on both bodies. To prevent their contact the value of M' would have to be far greater.

Obviously we require a star of higher density, which will permit a more reasonable value for its mass. Since the formula is general it should apply to all cases. Jeans takes

$$M' = 2M, \qquad R_0 = 2.667a,$$

and, grant him the sun he describes, apparently it will be ruptured and matter will be extracted from it. These values of M' and R_0 should satisfy Jeffreys's inequality, provided Jeans and Jeffreys are really using the same theory; instead, the least value of M' comes out about $9.5M$. A star of this mass and of the density necessary to avoid contact with the sun, and with its tidal protuberance, would probably have to be especially created. Jeffreys says his formula is the equivalent of each of two inequalities which he has just previously developed, one for a "slow" and one for a "transitory" encounter. It is the equivalent of the "slow" inequality, but the equivalent of the "transitory" inequality would have 2.22, instead of 2, in the denominator of the second term. The difference this makes is considerable, reducing the $57M$ and $9.5M$ of the above computations to $35M$ and $8.5M$, respectively, but not improving the prospect of finding the required star among such stars as are known. The encounter that would produce planets is described as lying between the "typically slow" and the "typically transitory." Unfortunately, Jeffreys bases his "transitory" formula upon an arbitrary requirement that the velocity of the outermost particle of the tidal stream, at the end of the encounter and

[1] "The Earth," second edition, 1929, pp. 24–25.

relative to the center of the sun, shall exceed the parabolic velocity of a particle at the normal surface of the sun. So a large and important part of the stream—the very part from which Jeffreys says the planets must be formed[2]—having originally a postulated parabolic velocity relative to the star, and being now endowed with an additional more-than-parabolic velocity, this time relative to the sun, would escape from both the star and the sun and be lost in space.

To illustrate the working of the theory, Jeffreys offers, as Jeans does, only a single example. In this he gives the sun nearly 1,000 times its present radius, the star 4 times the sun's mass, and makes the distance between the centers of the two bodies, when rupture of the sun ends, only $1\frac{1}{2}$ times the sun's radius. Applying a formula derived from Jeans's work to determine the height of the tidal stream at periastron, it appears that Jeffreys's star, besides having to be of very high density, would be enveloped by the stream—bombarded by part of it, left behind by other parts—and possibly would acquire a system of planets for itself from the later parts having less than the assigned parabolic velocity. Whether such a sun as that described would acquire a system of planets is not important, for on the next page of "The Earth" (second edition) Jeffreys virtually admits—because, of course, of the new view of the age of the earth—that our sun could not have been in the condition described, when our planets were formed.

Thus Jeffreys's mathematical presentation has never offered the possibility of the formation of planets from the sun we know. Further, we see that Jeffreys and Jeans have all along been in disagreement, although, apparently, neither knew it until recently; for, until 1929, when the second editions of both Jeffreys's "The Earth" and Jeans's "Astronomy and Cosmogony" were issued, they were felicitating each other on having arrived at the same results. Agreeing or not agreeing, the results obtained by both have now been abandoned by Jeffreys.

JEANS'S WORK ON HIS OWN THEORY

It is well known that when Jeans proposed the tidal theory he assumed that the "birth" of the planets had occurred early in the history of the sun, to which he therefore assigned a radius equal to that of the present orbit of Neptune. Recent estimates of the age of the earth, placing it at a few thousand million years, a brief period compared with the age now attributed to the sun, constrained both Jeans and Jeffreys to the conclusion that the size of the sun could not have been appreciably greater when the planets were evolved than it is now. Neither Jeans nor Jeffreys offered new proofs to meet this new condition, although new proofs were very much needed. In the 1929 edition of Jeffreys's "The Earth" and in both the 1928 and 1929 editions of Jeans's "Astronomy and Cosmogony" there are paragraphs reducing the sun of the encounter to its present dimensions, but no change is made in the mathematics and reasoning, which clearly apply only to a sun of much larger radius and much lower density. So Jeans's work can be studied only in his original example.

Nowhere in the published works of Jeans and Jeffreys is any estimate given of the quantity of matter extracted from the sun during the tidal encounter, so that we have no assurance that after rupture occurred enough matter would be supplied to form our planetary system. Let us attempt a provisional estimate for Jeans's example.

Jeans gives no simple formula, as Jeffreys does; he does not even assemble the data in any one place. These must be searched out in the pages of his "Problems of Cosmogony and Stellar Dynamics." The quantities required, together with the numbers of the pages of the book on which they are to be found, are as follows:[3]

Mass of the sun, p. 277	$M = 2(10^{33})$ grams.
Mass of passing star, p. 277	$M' = 2M = 4(10^{33})$ grams.
Periastron distance, p. 284	$R_o = 1.2(10^{15})$ cms.
Radius of the sun, p. 278	$a = 4.5(10^{14})$ cms.
Critical distance between centers of the two bodies, when rupture of sun begins, p. 278	$R = 1.27(10^{15})$ cms.
Length of major semi-axis of sun in excess of a, at end of encounter, p. 130	$2fM'a/R_o v^2 = 2(10^{14})$ cms.
Relative velocity of M and M', p. 283	$10(10^5)$ cms./sec.
Time of encounter, p. 130	$2R_o/v = 2.4(10^9)$ seconds (about 76 years).
Constant of gravitation	$f = 6.66\ (10^{-8})$ c. g. s. units.

By the formula $2fM'a/R_o v^2$ we find that at the end of the encounter, which is taken to be at closest approach (periastron), the apex of the tidal stream is $2(10^{14})$ cms above the normal surface of the sun, or $6.5(10^{14})$ cms from the center of the sun. Let us assume that the tidal stream was separated from the sun where the attraction of the sun and the star, at periastron, just balanced. This is about $5(10^{14})$ cms from the center of the sun. The distance thence to the outer limit of the stream is $1.5(10^{14})$ cms.

[2] "Only the parts first drawn off can be permanently detached from the sun."—"The Earth," second edition, p. 20.

[3] An attempt has been made to reconcile the notations of Jeans and Jeffreys. These are almost identical, but Jeans is not always consistent, *e.g.*, using R both in the sense here conveyed and in the sense of R_o. For the a used here he uses r_o, and employs a for extensions of r_o, that is, for the major semi-axis of the tidally distorted sun. The formula $2fM'a/R_o v^2$ has been obtained by clearing the first term of Jeans's equation of its denominator and inserting in the numerator the gravitation constant, which Jeans here omits but afterward restores.

At the moment of rupture of the tidal cone the radius of a plane section of the solar spheroid at right angles to the major semi-axis and distant from the point of rupture $1.5(10^{14})$ cms would be about $2(10^{14})$ cms. As the sun approached periastron and matter continued to pour out, the tidal cone would be elongated and narrowed. So then, if we take a cone with radius of base $2(10^{14})$ cms and altitude $1.5(10^{14})$ cms, its volume should exceed that of the matter extracted by the star's gravitational pull, although its shape would not be that of the tidal stream.

This volume is $6.285(10^{42})$ cm.^{3} Jeans gives the density of the tidal stream, or "filament," as he calls it, as $5.5(10^{-13})$ gm/cm.^{3} Multiplying this by the volume, we obtain the mass of the matter extracted, $3.456(10^{30})$ grams, which is about 1/580 of the mass of the sun, or about 1.3 times the mass of all the bodies now revolving around the sun.

It has been pointed out in the discussion of the collision theory that in order that a planetary system may be formed, much more than its own mass must be extracted. The "resisting medium" necessary, according to the theory, to reduce the original eccentric orbits of the planets to their present shapes must be taken into account. According to Jeffreys,[4] it would be "reasonable" to compare the mass of the resisting medium with that of Jupiter, which is nearly three fourths of the mass of all the planets together. Since it is now known that there is a ninth planet, and possibly other planets, beyond Neptune, it may not be unreasonable to take the mass of the resisting medium as equal to that of all the planets together.

The extracted matter is also subject to other losses. It is agreed that much of it must have fallen back into the sun. The theory derives the rotation of the sun from this source, and Jeffreys[5] estimates that a mass at least equal to that of Jupiter would be necessary for this purpose. He calculates, of course, effective mass. Since it is probable that much of the matter that fell back would be ineffective in producing rotation, this estimate, too, might well be raised to an amount equal to the mass of all the planets.

A source of possible loss not considered by either Jeans or Jeffreys is radiation pressure. The importance of this would depend upon the conditions of the encounter. This pressure would be effective, if at all, against the resisting medium, which is supposed to consist of the lighter parts of the tidal stream, greatly rarefied by diffusion. The mean density of a comet has been estimated at 1/230,000 that of air,[6] which makes it $5(10^{-9})$ gm/cm.^{3} It is a commonplace that when a comet approaches the sun the radiation pressure of the latter drives off in the opposite direction the matter emitted by the comet. Jeffreys[7] tentatively offers the hypothesis that the density of the resisting medium might be as low as $4(10^{-15})$ gm/cm^3, which is about 1/800,000 that of a comet. If radiation pressure from the star, or from the star and the sun combined, drove the resisting medium away, the cosmogonists would have to find some other means of reducing eccentric orbits.

So it appears that the various elements of the tidal theory require the extraction of hardly less than three times the mass of the planetary system to assure its formation. We have, therefore, a deficit of 1.7 times the mass of the planets in Jeans's theory. Actually the total mass extracted from the sun would probably be much less than the mass of the planets, and the deficit, therefore, so much the greater. For, in order to give the theory every chance, we have chosen a crude method of estimate intended to give a result far in excess of any mass that could be extracted by tidal forces alone. Even if more matter than we have here allowed could be extracted, the radial velocity of the additional part would be so insignificant that it would probably be only so much more that would fall back into the sun.

Jeans's mechanism for starting his planets in revolution around the sun is contradicted by himself. On page 284 of "Problems of Cosmogony," he "supposes" that the tidal filament is set in motion so that it rotates as a straight line around the sun with an angular velocity of $4(10^{-10})$. This would give, at a distance from the sun's center equal to 2.1 times the radius of Neptune's orbit, "the transverse velocity appropriate to the description of a circular orbit," so that "planets formed at a less distance than this would describe eccentric orbits."

But he has previously shown that the stream of matter can not rotate in a straight line. After the end of the encounter it is bent around by the star's gravitational pull into the shape of a boomerang. Nothing is given to show that the lagging parts of this curve would ever acquire the transverse velocities necessary to maintain orbits around the sun.

If the theory rested upon so insecure a basis when Jeans fitted to it an imagined sun, so moulded to his purpose that probably such a sun never could have existed in reality, how hopeless it becomes when applied to the sun of a few thousand million years ago that did not differ appreciably from the sun we see to-day. When we compute the parabolic velocity for the present sun and a passing star of twice its mass, at distance $R_0 = 2^2/_3 a$, and add 22 per cent. for safety, as Jeans did in his example, the required relative

[4] "The Earth," 1929, p. 60.

[5] *Monthly Notices*, R. A. S., May, 1929.

[6] Russell, Dugan and Stewart, "Astronomy," Vol. 1, p. 430.

[7] "The Earth," second edition, p. 60.

velocity of sun and star rises to 800 km/sec. Such an encounter might cause a convulsion, or a series of convulsions, in the sun, but the most optimistic cosmogonist could hardly expect it to produce planets according to the tidal theory in its present form.

Gravitation requires time to raise any considerable tidal elevation. Jeans's theory could be effective, if at all, only when the sun had a very large radius and correspondingly low density, as all his work shows. These conditions permit a wide separation of the centers of the two bodies, which reduces the velocity necessary to prevent their union in a system, and so tends to gain the required time.

In "Problems of Cosmogony and Stellar Dynamics" and in "The Earth," Jeans and Jeffreys, respectively, professed to rest their case on tidal forces alone, yet there have been manifestations of a disposition to beg the question by tacitly assuming the aid of unnamed forces. Both constantly use the term "ejection," which implies propulsion rather than attraction. This term has been replaced here by "extraction," as more appropriate to a purely tidal theory. "The second star outdoes the sun in gravitational pull, and the top of the [tidal] mountain *shoots* off toward it," says Jeans.[8] The lid has been lifted and the "shooting" has been going on for more than fifteen years in Jeans's one example, but his formulae for distance and velocity at the outer end of the "filament," at the end of this period, represent only gravitational acceleration, taking no account of "shooting."

It may be asked at this point: "Should not internal forces be considered in applying the theory to the sun as at present constituted?" To this the answer is: Certainly, if any one knows what those forces are and how they would behave under tidal provocation. Chamberlin and Moulton, of the University of Chicago, long before Jeans and Jeffreys had begun to deal with the subject, evolved a theory which employed both tidal forces and such internal forces of the sun as they were aware of. To their "planetesimal hypothesis," against which Jeans, Jeffreys and many others have directed destructive criticism, Jeans owes the concept of the extraction of matter from the sun by the tidal pull of a passing star. If he intends to take over another element of this hypothesis, it will be interesting to see how far new knowledge may enable him to surpass his predecessors in handling the combination. As to new knowledge, is it to be found in the bewildering contradictions of the debate, now in its third year in the Royal Astronomical Society and in the scientific journals, about what goes on inside a star? If not, there is always available as an alternative the fundamental principle of Jeffreys's collision theory—which the debaters have not paused to heed—that nothing goes on inside a star that need trouble a determined planet builder. Or is Sir James Jeans, indeed, about to abandon his tidal theory altogether and adopt the collision theory of Buffon, as developed by Dr. Jeffreys? We find him expressing the belief, in *The New York Times* of so recent date as June 7, that "the earth is merely a tiny fragment of the sun, which got splashed off [the exact idea of Buffon], almost by accident." The mathematician of Cambridge, rejoicing, no doubt, in an exchange of rôles, would welcome the support of a recruit so distinguished and powerful as the mathematician, poet and dreamer of Dorking.

There is no intention of offering here a theory to replace those that have been considered, but perhaps the cosmogonists will permit the suggestion that, in the tidal theory, they have left half the field uninvestigated. All have set out on the assumption that the planets must have come out of the sun and that, therefore, the encounter must have been with a star more massive than the sun. The sun may have encountered a smaller star of rather low density, quite as easily. If the mass, density and velocity of the star could be so nicely adjusted that, as it swung round at perihelion, the outer part would slide off in a barely hyperbolic orbit, leaving a sufficient part of the remainder with less than parabolic velocity, and therefore under permanent bonds to keep pace with the sun, the tidal theorist, at one happy stroke, would acquire the mass necessary for his purposes, with rotation thrown in by the slide. Or, suppose the sun had encountered a nebula? What would have happened in that case?

August 7, 1931.—It may be interesting to note that after the last question had been written, and before it could be published, an answer had been given. *Nature* (July 25, p. 156) records the fact that Mr. K. Hirayama, of the Tokio Observatory, has published in the *Proceedings of the Imperial Academy of Japan* (7, No. 5, 1931) an investigation of the effects of the impact of a star with a spherical nebula. Mr. Hirayama, according to *Nature,* concludes that if the relative velocity were barely hyperbolic before impact, it might be reduced by the impact below the parabolic value, so that the consequent elliptic relative orbit, because of the frequent overtaking of one mass by the other, might be steadily reduced by repeated impacts, at each of which a part of the nebula would be detached and captured by the star, the final result being the formation of a planetary system.

There seems to be no reason why the argument should not equally apply if the encountering bodies were the sun and a star of less mass and much lower density. The establishment of such a theory as to encounters of stars with smaller stars or with spher-

[8] "The Stars in Their Courses," 1931, p. 41.

ical nebulae would do much to destroy the idea, sedulously fostered by Jeans, Jeffreys and others, that our system is unique. It may be possible to demonstrate that stars of mass and density so high as to be capable of rupturing by gravitational power alone masses comparable with the sun, are extremely rare or non-existent; but there is no evidence that bodies small enough to be ruptured by approach to or contact with such stars as the sun have not existed in the past, or do not still exist, so that practically all large stars may have had, or may yet have, each its own planet-feeder. For the peace of the timid, it is to be hoped that nobody will suggest that the job in our system is not finished and that the remains of the sun's planet-feeder may be expected back to complete its disturbing mission.

OBITUARY

RUSSELL A. OAKLEY

DR. RUSSELL A. OAKLEY, principal agronomist in charge of the Division of Forage Crops and Diseases of the Bureau of Plant Industry, U. S. Department of Agriculture, died at Monrovia, California, on August 6. He was born on a farm near Marysville, Kansas, on September 7, 1880, and graduated with a B.S. degree from the Kansas State Agricultural College in 1903. After a short period of graduate work in the University of Chicago, he accepted an appointment as scientific aid in the Department of Agriculture and served continuously in that department from July 16, 1903, until the time of his death. Because of the excellent service rendered during the war period as chairman of the Seed Stocks Committee and of his recognized ability as an agronomist, the Iowa State College upon the recommendation of President Pearson conferred upon him in 1920 the degree of Doctor of Science. He was later elected fellow of the American Association for the Advancement of Science and of the American Society of Agronomy.

Those who knew Dr. Oakley best will remember him always as one who possessed to a remarkable degree the quality of making friends and as one endowed with almost superhuman courage and cheerfulness in long years of struggle with physical infirmities on account of arthritis. His associates never ceased to marvel at the indomitable will which enabled him to go about his work day after day uncomplaining and efficient. This heroic attitude toward his afflictions was not the result of any religious belief or any tendency toward asceticism; it came rather as the result of his unconquerable spirit refusing to surrender and always "playing the game."

This brief description of the personality of Dr. Oakley should add lustre to his achievements which are recounted in more detail in other journals. It also accounts for his recognized success as an administrator. The Department of Agriculture entrusted him with numerous assignments in the administrative field. From 1913 to 1926 he was in charge of the Office of Seed Distribution of the Bureau of Plant Industry, and in addition during the war period served as chairman of the Department Seed Stocks Committee; for several years he was assistant chairman of the Federal Horticultural Board and continued up to the time of his death as a member of the Advisory Federal Plant Quarantine Board. Since 1926 he has been in charge of what is now the Division of Forage Crops and Diseases. During much of the latter period he served as chairman of the Research Committee of the U. S. Golf Association Green Section. He also performed with credit to himself and the department several special assignments of the Secretary of Agriculture.

His research activities in agronomy were confined mostly to investigations with alfalfa and turf grasses. He is co-author with the late C. V. Piper of a book "Turf for Golf Courses," and with Dr. Piper originated and edited for many years the U. S. Golf Association *Green Section Bulletin*. Dr. Oakley is author of many department bulletins and articles in scientific journals. He leaves behind a splendid record of achievement.

H. N. VINALL

MEMORIALS

WE learn from *Nature* that in view of the approaching centenary celebration of Clerk Maxwell, the Cambridge University Press announces a book of essays written to commemorate the event by Sir J. J. Thomson, Dr. Albert Einstein, Dr. Max Planck, Sir Joseph Larmor, Sir James Jeans, Sir Ambrose Fleming, Dr. W. Garnett, Sir Richard Glazebrook and Sir Oliver Lodge.

A COMMITTEE, as reported in the London *Times*, has been formed to organize the appeal for a British national memorial to Sir Joseph Wilson Swan (1828–1914), who, apart from many lesser inventions, was the first to invent and introduce for practical purposes the electric incandescent lamp. Swan was also a pioneer in photography and the processes of photographic printing. The Institution of Electrical Engineers, of which Sir Joseph Swan was president in 1898, is presenting to the Borough of Sunderland a bronze tablet, designed by Mr. R. A. Ray, which will be erected in the entrance hall of the Sunderland Central Public Library, Museum and Art Gallery. In