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Origin of the lunar terrestrial system by capture, with further considerations on the theory of satellites and on the physical cause which has determined the directions of the rotations of the planets about their axes.

By *T. J. J. See.*

I. Comparison of the moon with other satellites of the solar system.

In A. N. 4308 the writer has adduced arguments tending to show that the planets and satellites of the solar system have in no case been detached from the central masses which now govern their motions, but have all been captured, or added from without, and have since had their orbits reduced in size and rounded up under the secular action of the nebular resisting medium formerly pervading our system. And in A. N. 4341–42 an outline of the dynamical basis of this new theory of the origin of our satellite systems has been developed in sufficient detail to render it intelligible. The methods there given appear to be entirely rigorous, and sufficiently general to be convincing without the examination of particular phenomena, except in the case of the earth and moon, which is the only planetary subsystem about which any doubt could arise.

The principal circumstance which might make our moon seem different from the other satellites is its relatively large mass, which amounts to $\frac{1}{81.45}$ of the mass of the earth. (cf. A. N. 3992, p. 117). This long ago led Professor Sir G. H. Darwin and others to the belief that its mode of origin probably was quite different from that of the other satellites of the solar system. But the considerations adduced by former writers rest on the hypothesis that our moon and the other satellites have been detached from the central masses which now govern their motions, and in A. N. 4308 this hypothesis has been shown to be no longer admissible. If our reasoning that the satellites have been captured is valid, it becomes advisable to examine the special case of the moon with some care, and to inquire whether the moon is, after all, relatively so large, or the earth merely comparatively small. In the following table will be found what I believe to be the best available diameters of the satellites of the solar system.

Table of satellite diameters.

Planet	Satellite	Diameters in kilometers	Mass in terms of the earth's mass as unity	Density
The earth	The moon	3480.5	1 : 81.45	3.31
Mars	Phobos	58	—	—
	Deimos	16	—	—
Jupiter	V	50	—	—
	I	3145 *)	1 : 111.2	3.29
	II	2817	1 : 135.5	3.76
	III	4770	1 : 38.75	2.70
	IV	4408	1 : 146.5	0.90
	VI	160	—	—
	VII	50	—	—
	VIII	50	—	—
Saturn	Mimas	351	1 : 143200	1.8
	Enceladus	528	1 : 42100	1.8
	Tethys	866	1 : 9450	1.8
	Dione	1032	1 : 5642	1.8
	Rhea	1331	1 : 2632	1.8
	Titan	5049 *)	1 : 49.4	1.79
	Hyperion	315	1 : 197600	1.8
	Themis	300	1 : 200000	1.8
	Japetus	1314	1 : 1053	4.77
	Phoebe	320	1 : 200000	1.8
Uranus	Ariel	1030	1 : 5700	1.83
	Umbriel	835	1 : 10670	1.83
	Titania	1350	1 : 2522	1.83
	Oberon	1295	1 : 2856	1.83
Neptune	Satellite	2962	1 : 238.7	1.83

*) A. N. 3764.

II. Further considerations on the capture of the satellites.

In the paper on the dynamical theory of the capture of satellites (A. N. 4341–42), it has been shown that all the satellites of the solar system are well within Dr. G. W. Hill's closed surfaces about the several planets; and it is made quite clear how these bodies have been brought within these folds by the secular action of the nebular resisting medium formerly pervading our planetary system. As is there pointed out, this disturbing cause has the effect of adding a secular term to the Jacobian integral, which thus becomes of the form:

$$x_i^2 + y_i^2 + \frac{2(1-\mu)}{\sqrt{(x_i - x_1)^2 + y_i^2 + z_i^2}} + \frac{2\mu}{\sqrt{(x_i - x_2)^2 + y_i^2 + z_i^2}} = C_i + \alpha_i t_i \,. \quad \begin{vmatrix} i = \infty \\ i = 0 \\ t = \text{time} \end{vmatrix} \qquad (A)$$

In accordance with the usual notation of dynamics, the subscript i may be used in this equation; for it will hold for an infinite number of particles of nebulosity in the system, and each particle will have its own surfaces of zero relative velocity. The secular coefficient is different for different particles, even when the coordinates are the same; because it depends on the velocity and direction of motion at the initial epoch. It will be determined by the resistance encountered along the actual path, and as infinite variation in the trajectory is possible, the value of the coefficient α_i cannot be exactly specified for any given case. It is easy to see, however, that it will always be a definite one valued function. In the long run it will be positive, though, through the accidental collisions of the particle with others having different velocities and directions, it may temporarily become negative. If $a_1, a_2, a_3 \cdots a_i$ be the values which this coefficient acquires at the epochs $t_1, t_2, t_3 \cdots t_i$, owing to accidental collisions of the particle, some being positive and others negative, it is clear that for a long interval of time we may take

$$\alpha_i = \frac{1}{i} \sum_{i=0}^{i=\infty} a_i \,. \qquad (B)$$

For any given path, starting at an initial epoch, t_i, this function will always be definite and comparatively small; but as the collisions are countless, and the values of the terms in the series $a_1, a_2, a_3 \cdots a_i$ will vary from one particle to another according to the path, no two of the coefficients α_i can be expected to be the same. We may form some idea of the numerical values of these coefficients by taking $\alpha_1 = 0.00000001$, and $t_1 = 10000000$ years. Then for a particle with such a path the second member of equation (A) will, after the lapse of ten million years, have increased by 0.1. This will bring the Hill surface of the particle considerably nearer the central masses than it was at the outset, so that in time it will become closed for that particle about one of the bodies, and the particle will therefore become a permanent satellite of the sun or planet.

Moreover, as the numerical value of the coefficient α_i fluctuates somewhat with the time, owing to collisions, it is clear that the Hill surface is not strictly of constant dimensions, but varies slightly, according to the nature of the collisions which the particle suffers in its path about S. and J.

III. Hill's closed surface about the earth.

We shall now consider somewhat more fully the problem of the origin of the terrestrial moon. From the data given by the table in the article above mentioned on the dynamical theory of the capture of satellites, we see that in this case the closed surface extends to about 1497577 kilometers from the centre of the earth, or about four times the present distance of the moon. This agrees very well with Dr. Hill's estimate of the extent of this surface in his »Researches in the Lunar Theory«, pp. 300–301–334, where he finds the value of maximum lunation to be 204.896 days.

It is true that in his Mécanique Céleste, Tome I, p. 109, Poincaré has traced a looped orbit of even wider extent and longer period, and Lord Kelvin has drawn an orbit of similar type in the Philosophical Magazine for November, 1892, p. 447; but Professor Sir G. H. Darwin justly points out (cf. Periodic Orbits, p. 192), that both of these eminent mathematicians have neglected the solar parallax, so that the solutions given do not quite correspond with the ideal conditions of the problem. We are, of course, concerned here only with the space within the cusps as given by Dr. Hill, and not at all with the loops found by Poincaré and Lord Kelvin.

If our moon has therefore been captured by the earth, it has at length come well within Hill's closed surface. In fact, the moon revolves at a distance corresponding to the inner fourth of the possible radius. The same thing is true of the other satellites of our solar system, and they, too, are near the central portions of their several closed surfaces.

Dr. Hill remarks that »If the body whose motion is considered, is found at any time within the first fold (the closed space about the earth), it must forever remain within it, and its radius vector will have a superior limit.« Neglecting the secular effects of the resisting medium upon Jacobi's integral, which has not been considered by previous writers, Moulton and others have drawn the unwarranted conclusion that because a satellite cannot now escape from a planet, so, also, conversely such a satellite cannot have come to its planet from a great distance (cf. Astrophys. Journal, Vol. 22, pp. 177–178). But in the paper on the dynamical theory of the capture of satellites, we have established the erroneous character of this reasoning. Probably a considerable number of astronomers and mathematicians have been misled by this deceptive argument, which has the appearance of sound mathematics, but is easily shown to lead to false conclusions.

In no other way can we account for the failure of previous writers to recognize a truth which is of the first order of importance in our theories of the heavenly motions, and which alone gives us a clear insight into the nature of cosmical evolution. This process by which satellites are captured and reduced to order and stability by revolving against resistance, is undoubtedly one of nature's greatest laws, and it operates uniformly throughout the physical universe.

IV. Physical grounds for classifying the moon with the other satellites, all of which have been captured.

It will be seen from the foregoing table that two of Jupiter's satellites, III and IV, are considerably larger than our moon; while Saturn's satellite Titan is much larger. Jupiter's satellites I and II have diameters nearly as large as that of the moon, and the same is true of the satellite of Neptune, to which, however, considerable uncertainty attaches, owing to the great distance of that planet. In all cases where the satellites present no telescopic discs the diameters are calculated from the brightness, the albedo being taken to be the same as that of the planets about which they revolve, and the density one-third that of the earth.

If therefore two satellites larger than the moon and two almost as large exist in the system of Jupiter, and if Titan in the system of Saturn is much larger, while the satellite of Neptune is almost as large, and the two larger satellites of Uranus probably have diameters about half as large, it cannot really be said that, when judged by the size of the satellites observed in other parts of the solar system, our moon is abnormally large. The real fact is that the earth is comparatively small. And this makes the moon seem relatively large, and gives rise to a mass-ratio of $\frac{1}{81.45}$, which is much the largest in the solar system, Jupiter being $\frac{1}{1047.35}$ of the sun's mass, and Titan only $\frac{1}{4700}$ of the mass of Saturn. So far as one may judge from these considerations, therefore, there is nothing improbable in the view that the moon, too, was captured by the earth.

If we recall that our planet is considerably the most massive body within the orbit of Jupiter, and that the sun's enormous mass has been built up by the gathering in of small bodies, many of them certainly as large as the satellites, and perhaps even as large as the terrestrial planets, it will be seen that the capture of the moon by the earth presents no inherent improbability. The throwing of hundreds of small planets within the orbit of Jupiter (cf. A. N. 4308), and the capture of dozens of periodic comets in the same way, affords us a good idea of the state of the solar system in the remote past. As the illustrious Euler remarked before the cosmogonic theories of Kant and Laplace were proposed, the earth itself at one time moved as far out as where the asteroids now circulate, and we may add, in an orbit of considerable eccentricity. That such a planet as the earth should capture a companion planet (for the moon is nothing but one of the neighboring planets which were once so numerous in our system), is perfectly natural, and now demonstrated to be entirely within the range of possibility.

V. The chief objection to the theory that the moon was captured based on Darwin's researches on tidal friction and cosmogony.

The chief objection to the theory that the moon was captured is based on Darwin's celebrated researches on tidal friction and cosmogony (Proc. and Phil. Trans., Roy. Soc., 1878–1882).

The present writer has studied this work closely during the past twenty years and considers that the conclusions drawn by Darwin are quite justified in the premises. On the traditional view that the satellites were detached from the planets which now govern their motions, as taught by Laplace and his successors for more than a century, no other outcome than that traced by the masterly hand of Sir George Darwin was possible. But if our point of view is now changed, and we see clearly that all the other satellites were captured, the question naturally arises whether any good grounds can be adduced to show that the moon should be considered to be an exception in the cosmogony of the solar system. After a very careful consideration of all the relations involved, it seems to me that we shall have to give up this idea, and regard the moon as in the same class with the other satellites.

It is true that Darwin's work appears to be put together very powerfully by the relations he has brought out between such elements as the earth's time of axial rotation, the obliquity of the ecliptic, the eccentricity of the lunar orbit, etc., and the secular changes of these elements during past ages. With admirable philosophic frankness Darwin asks whether all these apparent confirmations of his theory can be accidental. If we still believed the satellites were formed by any kind of separation or process of detachment, as was taught by Laplace, we should unhesitatingly answer by saying that the relationships which Darwin has so skillfully traced could not well be the result of chance. But with the whole point of view now changed, and the capture of the satellites shown to be possible, in the way above described, — by the extension of the methods of Hill, Poincaré and Darwin, the latter's work being especially useful and suggestive, all of which have come into use since the work on tidal friction and cosmogony was published thirty years ago, — it is difficult to escape the impression that the relationship there brought out will, after all, prove to be largely or wholly accidental.

It might be best to leave the settlement of this question to the future, and avoid drawing hasty conclusions on so weighty a matter. For the probabilities in the case will appear different to different minds. Some will, no doubt, prefer the traditional view, and believe that the moon has been detached from the earth, while others will think it more probable that, like the other satellites, it came to us from the planetary spaces, and has since neared the terrestrial globe about which it revolves. In any case, tidal friction has exercised some influence on the past history of the lunar terrestrial system; but here, as elsewhere in nature, the influence of the resisting medium has largely counteracted the secular effects of tidal friction. If the moon came from the heavenly spaces, the eccentricity of the lunar orbit is more likely to be the survival of an original eccentricity than a development due to tidal friction, because in this event the latter cause will have been much less powerful than has been heretofore supposed.

If the moon was captured, and not detached from the earth, as Darwin supposed, there would be no necessary relationship, and but little exchange need have taken place, between the moment of momentum of the earth's axial rotation (0.7044) and the moment of momentum of the moon's orbital motion (3.384). And the great moment of momentum of the whole lunar terrestrial system might be the more easily explained. The moon's great distance and relatively large mass is favorable to a large orbital momentum, and thus it might well be 4.8 times that of the earth's axial rotation (cf. Appendix to Thomson and Tait's Nat. Philos., Volume I., Part II., p. 508), even if the latter had not been decreased and the former increased by tidal friction. In fact, this very large moment of momentum of the moon's orbital motion is a very suspicious circumstance, and is not easily explained, except on the supposition that it points directly to the capture of our satellite. If so, we shall have to give up the accepted view that the earth formerly rotated so rapidly that it was highly oblate and finally became unstable and broke up into two masses; and the corresponding problems of Astronomy, Physics of the Earth and Geology will have to be re-examined from the ground up.

VI. Darwin's graphical method of representing the past history of the earth and moon under the secular action of tidal friction.

On account of the high importance of realizing fully the great strength of the celebrated graphical method which Darwin developed at the suggestion of Sir W. Thomson, as well as the weakness underlying the interpretation of it heretofore adopted, it becomes necessary to explain briefly the fundamental equations with the accompanying diagram.

Let M be the mass of the earth, m that of the moon, Ω the angular velocity of the two bodies about their common centre of gravity, the orbit being supposed circular. Introduce a special system of units designed to reduce the analytical expressions to their simplest forms, and take the unit of mass to be $\frac{Mm}{M+m}$, the unit of length γ to be such a distance that the moment of inertia of the planet about its axis of rotation shall be equal to the moment of inertia of the earth and moon, treated as particles, about their centre of inertia, when distant γ apart from each other. Then if C be the earth's moment of inertia about its axis of rotation, we shall have

$$M\left(\frac{m\gamma}{M+m}\right)^2+m\left(\frac{M\gamma}{M+m}\right)^2 = C$$

or

$$\gamma = \left(\frac{C(M+m)}{Mm}\right)^{1/2}. \tag{1}$$

Take for the unit of time τ the interval in which the satellite revolves through 57°.3, when the satellite's radius vector is equal to γ; then $\frac{1}{\tau}$ is the orbital angular velocity, an by Kepler's law of periodic times,

$$\tau^{-2}\gamma^3 = \mu(M+m) \tag{2}$$

where μ is the attraction between unit masses at unit distance. Substituting for γ its value in (1), we get

$$\tau = \left(\frac{C^3(M+m)}{\mu^2(Mm)^3}\right)^{1/4} \tag{3}$$

This special system of units makes each of the following expressions unity:

$$\mu^{1/2}Mm(M+m)^{-1/2};\ \mu Mm;\ \text{and } C.$$

The moment of momentum of orbital motion, in a circular orbit of radius r is

$$M\left(\frac{mr}{M+m}\right)^2\Omega+m\left(\frac{Mr}{M+m}\right)^2\Omega = \frac{Mm}{M+m}r^2\Omega. \tag{4}$$

And Kepler's law gives

$$\Omega^2r^3 = \mu(M+m),\ \text{or}\ \Omega r^2 = \mu^{1/2}(M+m)^{1/2}r^{1/2}. \tag{5}$$

Therefore, by means of the special units, the moment of momentum of orbital motion in (4) becomes

$$\mu^{1/2}Mm(M+m)^{-1/2}r^{1/2} = r^{1/2}. \tag{6}$$

The moment of momentum of the earth's rotation is Cn, where C is the moment of inertia and n the angular velocity of rotation. The total moment of momentum of the system is constant, and made up of two parts, one depending on the rotation of the earth about its axis, the other on the orbital motion of the two bodies about their centre of inertia; therefore if h be this constant, we have in the special units

$$h = n+r^{1/2}. \tag{7}$$

The kinetic energy of orbital motion is

$$\frac{1}{2}M\left(\frac{mr}{M+m}\right)^2\Omega^2+\frac{1}{2}m\left(\frac{Mr}{M+m}\right)^2\Omega^2 = \frac{1}{2}\frac{Mm}{M+m}r^2\Omega^2 = \frac{1}{2}\frac{\mu Mm}{r}. \tag{8}$$

The kinetic energy of the earth's rotation is $\frac{1}{2}Cn^2$, and the potential energy of the system is $-\mu\frac{Mm}{r}$. The sum of these three energies, in the special units, becomes

$$2e = n^2-\frac{1}{r} \tag{9}$$

Putting

$$x = r^{1/2},\ y = n,\ Y = 2e \tag{10}$$

Darwin has illustrated these fundamental equations and another called rigidity, which gives the condition the two bodies should revolve as parts of a rigid system:

Momentum, $$h = y+x \tag{11}$$

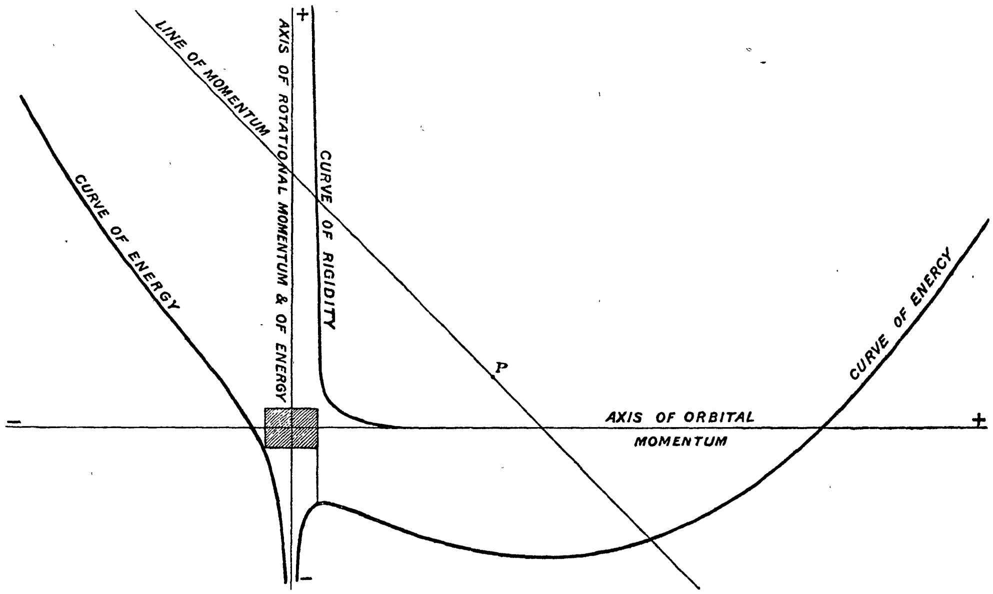
Energy, $$Y = y^2-\frac{1}{x^2} = (h-x)^2-\frac{1}{x^2} \tag{12}$$

Rigidity, $$x^3y = 1. \tag{13}$$

Equation (11) is the equation of conservation of moment of momentum; (12) the equation of energy; (13) that of rigidity. When the system is once started, h remains rigorously constant under any interaction between the two bodies, but Y degrades, and the curve of energy has maximum and minimum values defined by the condition

$$\frac{\partial Y}{\partial x} = 0 \quad \text{or} \quad x^4 - hx^3 + 1 = 0 \,. \tag{14}$$

Taking the moon's mass to be $\frac{1}{82}$ of the earth's mass, and the earth's moment of inertia as $\frac{1}{3} Ma^3$, Darwin found the special unit of mass to be $\frac{1}{83}$ of the earth's mass, the unit of length 5.26 radii of the earth (33506 kilometers), and the unit of time $2^h\, 41^m$.



In these units the present angular velocity of the earth's rotation becomes 0.7044 and the moon's radius vector 11.454. This position of the moon is indicated in the diagram by the point P, and the moment of momentum of its orbital motion is 3.384, and thus very large. This is Darwin's celebrated analysis of the interaction of the earth and moon (cf. Proc., Roy. Soc., June 19, 1879; also Thomson and Tait's Nat. Philosophy, Appendix G; or Encyclopedia Britannica, Article »Tides«).

As the energy curve has a maximum near the origin, corresponding to a small distance between the earth and moon, Darwin inferred that they had once been a single mass, rotating temporarily as a rigid system; and that after the separation, the moon had receded, according to the downward slope of the energy curve, till it reached its present distance. The time of the earth's rotation was calculated to be $2^h\, 41^m$, which would barely enable the equilibrium of the globe to maintain its stability under gravity. And as this pointed to the rupture of the globe from too rapid rotation, Darwin inferred that it had actually occurred, and that the moon had thus been detached from the earth.

Nolan and others pointed out the extreme difficulty the moon would have in holding together under tidal strain within so small a distance of the earth; and the inevitable disruption of such a satellite within 2.44 radii of the planet had been well established by the earlier researches of Roche and the subsequent investigations of Darwin. So long as it was uncertain whether the moon could hold together so near the earth, it was for a time believed that the primeval satellite might have taken the form of a flock of meteorites when the separation first took place. The difficulty of making out how the moon got started as a single mass so near the earth, Darwin has repeatedly acknowledged. As the result of Nolan's criticism, he found 6500 miles from the centre of the earth to be the minimum distance at which the moon could revolve in its entirety (Phil. Trans., Vol. 178, 1887, p. 416); but this was not entirely satisfactory, and at the end of his important paper on the figures of equilibrium of rotating masses of fluid (Phil. Trans., Vol. 178, 1887, p. 422) he concluded in some despair that it is »necessary to suppose that, after the birth of a satellite, if it takes place at all in this way, a series of changes occur which are quite unknown.«

Accordingly we see that by tracing of the moon back towards the earth, this supposedly reversed process brought them into close contiguity, one rotating and the other revolving in approximately the same time, and both not far the critical period of instability for the terrestrial spheroid. »Is this«, asks Darwin, »a mere coincidence, or does it not rather point to the break-up of the primeval planet into two masses in consequence of a too rapid rotation?« In addition to the objections already advanced, another formidable one arises from the difficulty of finding any cause adequate to produce the supposed very rapid rotation of the primitive globe. This objection is now recognized to be much greater than it was supposed to be when Darwin's work was finished thirty years ago; for Laplacian conceptions were then universally prevalent, and it was natural to think of the moon as a part of the earth, while such an idea as the capture of satellites would not have been entertained. In the views current thirty years ago, the above question of Darwin was naturally answered in the affirmative, in spite of outstanding difficulties of considerable magnitude. Today with all the other satellites proved to be captured, the wonderful relations brought out by Darwin's analysis must be declared to be only an accidental but most deceptive coincidence. It is probably the most remarkable result of this kind in the annals of science.

VII. On Stratton's researches on planetary inversion.

In the Monthly Notices of the R. Astr. Soc. for April, 1906 (Vol. 66, No. 6), Mr. F. J. M. Stratton, of Cambridge, England, has a scholarly discussion of the problem of planetary inversion, which had been suggested by Professor W. H. Pickering's discovery of the retrograde motion of Phoebe, and the tacit assumption formerly adopted by all writers that the satellites have been detached from the planets about which they revolve.

In stating his problem Mr. Stratton says: »If, then, a satellite were thrown off in a very early stage of the planet's evolution, it would commence moving in a retrograde direction around the planet. If the oblateness of the planet were very small, or the satellite at a considerable distance from the planet's centre, the plane of the orbit of the satellite would not follow the plane of the planet's equator as it tilted over, but would fall back into a stable position near the ecliptic — a term used in this paper for the plane of the planet's orbit. Such a satellite would remain of the retrograde type exemplified by Phoebe. If, however, the satellite were evolved in a later stage of the planet's development (after the planet had greatly contracted and become more oblate), the satellite would move in an orbit whose stable position was almost coincident with the planet's equator, and the satellite would follow the planet's equator. Most of the known satellites of the solar system fall into this class.

»Professor Pickering urged in support of this view that the classical nebular hypothesis, according to which the planets were thrown off in the form of rings, required an initial retrograde rotation of the planet and not a direct one, as Laplace assumed. But of recent years Sir George Darwin, Professor T. C. Chamberlin, and Dr. F. R. Moulton have adduced strong reasons for discarding the ring-theory, and it would seem that such confirmation as it would undoubtedly have given to this investigation must for the present be disregarded. Though apparently the classical form of the nebular hypothesis cannot now be accepted without considerable modifications I have here followed it in general as regards the history of the planetary subsystems, and have assumed a planet to be a gradually contracting body, which from time to time may pass through a form of instability, resulting in the evolution of a satellite.«

Mr. Stratton found many difficulties and uncertainties in this work and has discussed them fully. On pages 396–8 he has the following remarks: »There remains one other difficulty in connection with the time required for the working out of the theory, and that difficulty, though an almost necessary accompaniment of any such theory, would be alone sufficient to prevent one from urging its acceptance on dynamical grounds alone. It does not appear that, for such enormous periods of time as we are here concerned with, our ordinary dynamical equations are of sufficient exactitude to prevent the entrance of some unknown factors, which may profoundly modify the course of the evolution of the system. This difficulty must be regarded as an additional cause for receiving the theory with all reserve.« · · ·

»The present small obliquity of Jupiter, requiring an almost impossibly great viscosity if explained by solar tidal friction alone, had been regarded as a natural consequence of the tidal action of the satellites. And the large angle through which Saturn[1] had tilted since the evolution of Phoebe had been looked upon as in great part due to the tidal action of its satellites.« · · ·

»We may say, then, that the theory of planetary inversion suggests, but does not absolutely require as a condition for its truth, an annular stage in the history of the satellites of Jupiter and Saturn. More than this we do not care to state till a more detailed application of the tidal theory has been made to the case of a planet attended by a group of satellites. The very doubtful question whether perturbations in a ring of satellites could ultimately lead to the formation of one or several satellites must also be discussed before the difficulties considered in this section can be removed.«

Again, in the summary of his results, on pages 400–401, Mr. Stratton continues: »Jupiter must have evolved its satellites after its obliquity had decreased below 90°; partly under their influence it has been driven down towards a stable position of small obliquity, which it has now nearly reached. Saturn shed Phoebe, and possibly also Japetus and Hyperion, while its obliquity was greater than 90°; as under solar tidal influence it passed through the critical position, where its obliquity was 90°, Phoebe sank down into the ecliptic in a retrograde orbit, while Japetus and Hyperion moved over with the planet's equator. Afterwards the inner satellites were evolved, and under their influence and the influence of the rings Saturn's obliquity has steadily diminished — and is still diminishing — towards a small stable value. As seems highly probable for a planet further removed from the Sun, and therefore less likely to have its increasing rotation checked by solar tidal friction, the satellites of Uranus were evolved in an earlier stage of its evolution, before its obliquity had decreased to 90°; they have stopped the decrease in obliquity, which would arise from the solar action, and they are now driving Uranus back to a stable position with an obliquity of 180°. Neptune, with its one satellite of extremely large tidal influence, is being driven towards an equilibrium position with an obliquity of 180°. I should add that uncertainty as to the data for the satellites of Uranus and Neptune leaves even the present direction of motion of their equators very doubtful,

[1]) Jupiter's VIIIth satellite had not been discovered when Mr. Stratton's paper was written.

but that the results above given seem on the whole the most probable.«···

»I suggest as the easiest explanation of certain remaining difficulties that the satellites of Jupiter and Saturn have passed through an annular form at some previous stage in their history. This latter idea is not essential to the successful working out of the theory; at present it is only put forward very tentatively indeed, and as a subject for further research.«

»Viewed broadly, then, the theory of planetary inversion, though it entails some difficulties of detail, remains a tenable hypothesis. As explained by Sir George Darwin's tidal theory it involves three main assumptions: (1) that the outer satellites of a planet were evolved before the inner ones; (2) that the determining factor producing secular alterations in a planet's obliquity has been tidal friction; and (3) that the time involved in the scheme is not so great as to invalidate the ordinary dynamical equations. A justification for these assumptions may perhaps lie in the satisfactory explanation which the theory affords both of the large obliquities of Uranus and Neptune and of the presence of a satellite such as Phoebe. The secular motions with which the theory is concerned are so extremely slow that it can hardly yet be proved or disproved by reference to the gravitational theory of the motions of planets and their satellites; the theory would gain some support by the discovery of satellites to Uranus and Neptune of the same type as Phoebe, if their motion were retrograde; it would be overthrown if their motion were direct. The theory remains then at present a speculative hypothesis, which is on the whole well supported by the theory of tidal friction, and which gives the only explanation so far offered for certain facts.«

It is impossible to convey the contents of this lengthy and well prepared paper, even by quotations of such considerable length as are here given; but this seemed the only way of doing the author even moderate justice, because of the difficulty of condensing the results into smaller compass, without omitting some important considerations. The chief significance of Mr. Stratton's investigation lies in the continued adherence to Laplacian traditions, in spite of the negative and therefore unsatisfactory criticisms of Moulton and Chamberlin; and in the avoidance of any suggestion that the observed satellites might have been captured, though Sir George Darwin, under whose inspiration Mr. Stratton's work was done, had eight years before published his celebrated memoir on Periodic Orbits (Acta Mathematica, vol. 21), and during the previous year had given valuable suggestions on cosmical evolution in his Presidential Adress to the British Association at Capetown, 1905. One cannot but wonder to what extent Moulton's misleading criticism of Professor W. H. Pickering's suggestion of the possible origin of Phoebe by capture (Astrophys. Journal, Vol. 22, pp. 177–180), with the accompanying fatal misinterpretation of Jacobi's integral, may have been responsible for the rejection of the only idea which could simplify our theory of the observed satellites, and bring it into harmony with the purely mathematical results arrived at by Professor Sir G. H. Darwin in his justly celebrated memoir on Periodic Orbits.

VIII. On the true physical cause which determines the direction of planetary rotation.

It will be seen from the considerations already adduced, and examined with some care in the paper on the dynamical theory of the capture of satellites, that we explain the direction of rotation of the planets on the same principle by which we account for the direction of revolution of the satellites in their orbits. About each planet, within the Hill closed surface, and in the hour-glass surfaces which are not closed, waste matter from the nebulosity circulation about the sun passes freely. As the hour-glass surface is not entirely closed for most of the particles, they naturally enter the region about the planet with a direct motion; and this same direction is naturally preserved when they fall down near the planet so as to pass within the closed surfaces. Therefore in general the satellites have direct revolutions in their orbits and the planets have direct rotations on their axes. Only crossing satellites, or those of irregular foreign origin have retrograde revolution: and most of these are destroyed. Those which fall into the planet under the secular effects of resistance check its rotation but slightly.

Accordingly, while we admit Mr. Stratton's theory of planetary inversion under his postulated conditions, involving enormous duration of time, we deny that such history has been enacted in the solar system, unless possibly a slight effect of the kind has arisen in the systems of Uranus and Neptune, which are so remote from the sun. In our view the direct rotations of the planets are inevitable consequences of the capture of nebulosity in the sheltered regions enclosed within the Hill closed surfaces. These closed spaces are regions into which waste material drifts as inevitably as water runs down hill. In these sheltered and sequestered regions systems of satellites develop, because the nebular vortices collected there circulate incessantly, and the waste nebulosity finally goes to the building up of the planets or the satellites. This conception of the sheltered vortex inside the Hill closed surfaces gives one a very clear idea of what takes place about the planets as they develop in the vaster extent of nebulosity circulating about the sun.

As the planets originate at much greater distance from the sun than they now have, we cannot assume that their rotations may not be partly fixed before they reach their present positions. Even retrograde rotation might be started in remote planets; and it may be that this still partially survives in the systems of Uranus and Neptune. Accident has much to do with the rotations of remote bodies, but in the inner parts of the system a more orderly development prevails, because the retrograde motions are largely obliterated, as we see in the actual solar system. Various causes have modified the rotation and axial tilt of the planets, but direct rotation is natural; while planetary inversion seldom if ever takes place.

IX. The moon and other satellites, being small captured bodies, probably never had much rotation, but even this has been destroyed by resistance and tidal friction.

This proposition is almost obvious without elaborate analysis of the reasons why the smaller bodies have little rotational moment of momentum. For in coming together the elements of such a mass could hardly give it a rapid rotation about any axis, because the closed Hill surface about it is too small to give a large vortex for the collection of waste matter; and nothing but a large amount of this gathered rubbish revolving under strong central force could produce a rapid rotation in the planet formed by the subsequent condensation of the material. Thus owing to the small size of the Hill closed surface, and the feeble central attraction — both being due to the smallness of the mass — the rotation of a small body like the moon can never be very rapid. Accordingly neither the terrestrial moon nor any of the other satellites of the solar system ever had rapid axial rotation, and the same remark applies to the planet Mercury. Yet what little rotations the moon, the satellites of Jupiter, Saturn, and other planets may have had, have been exhausted by subsequent resistance, and especially by the tidal friction of the planets about which they revolve. It is not surprising, therefore, that they show only one face towards their several planets. The result has long been regarded as probable; but previous writers, being unaware of the causes which determine rotation and that the satellites were captured, have perhaps overrated the chances of primitive rapid rotation, and made the destruction of the axial rotations seem more important than it really is. For as the earth has been thought to have rotated in about $2^h\ 41^m$, according to Darwin, it might naturally have been supposed that the rotation period of the moon also was at one time comparatively short. If the present views are correct, this has never been the case; and although tidal friction has been the main cause working to exhaust the rotations, there never was much rotation to be destroyed. The force of this argument becomes more apparent by remembering that if the moon is a captured body, there is no good reason to suppose that the earth ever did rotate much more rapidly than it does at present.

Problems such as the loss of the atmospheres of the moon and of other satellites also take on a new aspect; for we have no reason to believe any sensible atmosphere ever existed about these small captured bodies. Nor is it probable that there is snow or ice on the moon's surface, as many writers have supposed. Whether the large craters can have been formed by the impact of small satellites upon a heated and molten surface, as the geologist C. K. Gilbert believed, must be left to the future to determine.

The moon being in the present hypothesis a planet and not a portion of the earth, we have to give up most of the supposed analogy between terrestrial and lunar volcanoes and mountains. The mountains on the moon apparently were formed before it was captured by the earth. And therefore while we lose by giving up the assumed analogy with the earth, we gain by our new privilege of studying at close range a planet from the celestial spaces formed quite independently of the earth. If this view be correct, there will be a considerable advantage to science; for we never expected that this privilege of such close telescopic inspection of another planet would be given to the inhabitants of our terrestrial globe.

In this connection I may say that on one or two occasions when the seeing was at its best during the observations of the planet Mercury at Washington in 1901 and 1902, I believed I obtained glimpses of the planet's surface of the same type as that of the moon. It may well be that these brief glimpses gained at moments of best seeing, supported as they are by the evidence of photometric measures, showing that the planet has a rough surface, rest on a more substantial basis than any one heretofore has ventured to believe. One gets the impression that the origin of the moon and of the planet Mercury is essentially the same, and that at one time both revolved in the planetary spaces between the present orbits of Mars and Jupiter.

X. The terrestrial spheroid itself shows little if any evidence of having had more rapid rotation in former times.

The theory that the moon is a captured body carries with it several important corollaries, which deserve careful consideration. Foremost among these is the question whether the earth rotated much more rapidly in former times than it does now. It has long been believed that the earth once had a much more rapid rotation than at present, and tables of the changes in the earth's figure and physical constitution arising from such supposed rapid rotation have been calculated and published in various works on Geology and Physics. But it is a remarkable fact that if we examine this work carefully, we shall find that it rests not on observed phenomena, but on Darwin's celebrated papers on the origin of the lunar terrestrial system, which have been analyzed above. On the other hand, the terrestrial spheroid itself gives little if any evidence of more rapid rotation in former times. No well established facts in Geology, Physics, or Geodesy support such a view.

It is true that the changes in the rate of rotation of our planet might be supposed to be so slow that all traces of the former state of the earth would have been wholly obliterated by the transformations which have intervened; yet it is not certain that this would be so, and it seems more probable that the greater oblateness once existing would have left sensible traces of incomplete adjustment to modern conditions. So far as may be judged from accurate measurements of gravity, and from many trigonometric measurements carried out in all latitudes and in both hemispheres, by various Geodetic Surveys, no certain inequalities pointing to a former rapid rotation of the earth have been disovered. The inequalities found all seem to be local, and connected with the formation of the continents, which owe their elevation and outlines to the secular leakage of the oceans (cf. Further Researches on the Physics of the Earth, and especially on the Folding of Mountain Ranges and the Uplift of Plateaus and Continents produced by Movements of Lava beneath the Crust arising from the Secular Leakage of the Ocean Bottoms, Proc. Am. Philosophical Society, Philadelphia, No. 189, 1908).

In his valuable work on Tides and kindred Phenomena in the Solar System, pp. 300–304, Sir George Darwin discusses this question of the earth's adjustment with some care.

He admits that Lord Kelvin did not share his view that the earth had adjusted its figure to suit its rate of rotation. He says Lord Kelvin held »that the fact that the average figure of the earth corresponds with the actual length of the day proves that the planet was consolidated at a time when the rotation was but little more rapid than it is now.« And adds: »The difference between us is, however, only one of degree, for he considers that the power of adjustment is slight, whilst I hold that it would be sufficient to bring about a considerable change of shape within the period comprised in geological history.«

Sir George Darwin then proceeds to analyze four classes of facts derived from observation, — gravity, the ellipticity of the earth, the lunar inequality depending on the earth's figure, and the precession and nutation of the earth's axis — and says that they are so intimately intertwined that one of them cannot be touched without affecting the others. In conclusion he adds: »Edouard Roche, a French mathematician, has shown that if the earth is perfectly plastic, so that each layer is exactly of the proper shape for the existing rotation, it is not possible to adjust the unknown law of internal density so as to make the values of all these elements accord with observation. If the density be assumed such as to fit one of the data, it will produce a disagreement with observation in others. If, however, the hypothesis be abandoned that the internal strata all have the proper shapes, and if it be granted that they are a little more flattened than is due to the present rate of rotation, the data are harmonized together; and this is just what would be expected according to the theory of tidal friction. But it would not be right to attach great weight to this argument, for the absence of harmony is so minute that it might be plausibly explained by errors in the numerical data of observation. I notice, however, that the most competent judges of this intricate subject are disposed to regard the discrepancy as a reality.«

The views here expressed by Darwin, who may be considered the highest authority on the subject, accord sufficiently well with those reached by the present writer on the theory that the moon is captured, to justify the statement that the earth itself shows little if any evidence of more rapid rotation in former times.

If the supposed greater tidal efficiency of the moon in past ages is given up, various tidal and physical questions will be left unsettled, and most of the problems of the physics of the earth will have to be re-examined. The uniformitarian theories in Geology will gain some additional importance by changes in fundamental principles which exclude the moon from a more active part in the past history of the earth.

Before finally dismissing this important subject it is worth while to remark that some further light on the question of the earth's rotation in past ages may be gathered from the study of the other planets in space. If we consider attentively the present slow rotations of the other planets, we shall perceive how extremely improbable it is that the earth once rotated rapidly enough to detach the moon. The best determined rotation periods of the several planets seem to be the following (cf. A. N. 4308):

Mercury	88 days	Jupiter	9.928 hours
Venus	225 days, or 1 day	Saturn	10.641 hours
Earth	24 hours	Uranus	10.1112 hours
Mars	24.62297 hours	Neptune	12.84817 hours

In the case of Venus I have given preference to Schiaparelli's period confirmed by Lowell, though there is perhaps still a little doubt attached to the rotation period of this planet. Working with the spectrograph at Pulkowo, Belopolsky obtained apparently slight spectral displacements corresponding to a period of one day (cf. A. N. 3641), but this result was not confirmed by Lowell, who repeated the experiment at Flagstaff under favorable conditions. There are, however, two additional reasons for being very cautious about concluding what the period of Venus is: 1) From the mass of the planet, namely 0.8153 of the earth's mass (cf. A. N. 3992, p. 118), one would expect an original rotation nearly as rapid as that of the earth, owing to the physical cause which determines rotation, as set forth in the present paper. 2) If a rapid rotation once existed, in a period of about one day, the question arises whether it could have been destroyed by tidal friction. Heretofore we have been inclined to answer this question in the affirmative, but it is not clear that we have been right. It is true that the tidal frictional resistance due to the sun's action on Venus would be about 5.8 times what it is on the earth; but Dr. Hecker's recent observations at Potsdam indicate a yielding of the solid earth under the action of the moon of only about six inches, according to a statement by Professor Sir G. H. Darwin in a public lecture at Cambridge, May 10, 1909. This corresponds to a solar tide in the solid earth of only two inches, and this would make the bodily tide in Venus not over twelve inches. For in the paper on the rigidity of the heavenly bodies, A. N. 4104, I have shown that the rigidity of Venus must be taken to be but little less than that of the earth. If then the solid earth yields to the sun's attraction to the extent of about two inches, and the solid globe of the planet Venus not over twelve inches, the question arises whether the frictional resistance against the rotation would not be excessively slow, and in fact almost insensible. If the moon has been captured, as set forth in this paper, it appears that we cannot point with certainty to any sensible retardation of the earth's rotation, due to the action of the sun and moon; nor should we expect such a result from a tidal yielding of the earth's mass of only about two and six inches, respectively, for these two disturbing bodies. Under the circumstances, it seems necessary to preserve an open mind about the rotation period of Venus.

However this question may be decided by future events, the period will in no case be appreciably less than a day, and this minimum value is sufficient for our present purposes. What is true of Venus, is even more certainly true of Mercury.

Now the period of $2^h\,41^m$, or $2^h\!.7$, found by Darwin for the earth when rotating as if rigidly connected with the moon, is only about one-ninth of the present rotation period of the earth; and even Jupiter, which has the largest mass and shortest period of any of the planets, rotates about 3.7 more slowly than our primitive earth is supposed to have done. By dividing the primitive earth's hypothetical period of $2^{h}7$ into the period of the other planets, we obtain for the several planets the following minimum numbers, namely: Mercury 9; Venus 9; Mars 9.1; Jupiter 3.7; Saturn 4.0; Uranus 3.7; Neptune 4.8; and may calculate the probability that in seven different cases the observed periods would so much exceed that of the primitive earth, or that the earth's original period would have been so much shorter than that of any of the other planets. If the earth as an ordinary planet of very modest size could really have attained to a rotation in so short a period as $2^{h}7$, the chances that seven planets would not all miss in the same direction, and by these amounts, the average being about 6.2, would be about as the continued products of the above numbers, which is 193745. Thus the chances that the earth could have had such a short period as $2^{h}7$ when calculated from the data furnished by the other planets scarcely exceeds 1 in 200000, or the chances are 200000 to 1 that no such short period as $2^{h}7$ ever existed. And if the known physical cause of the rotations, as established in this paper, be introduced, the probability becomes practically infinity to one that such a short rotation period as $2^{h}7$ never existed; and the probability remains enormous that the earth never rotated much more rapidly than it does now. So far as one may judge, therefore, by the data furnished by the other planets, we are justified in rejecting once for all the hypothesis that the day was ever appreciably shorter than at present.

Summary and conclusions.

These several considerations may be briefly summed up as follows:

1) As all of the other satellites are proved to be captured bodies, the overwhelming presumption is that this is true also of the moon, and this enormous probability is naturally increased by the demonstrated fact that all the planets likewise have been captured by the Sun, and not one of them detached from that central globe, as was formerly supposed by Laplace and other early writers on Cosmical Evolution.

2) If we calculate the probability that the otherwise uniform rule of capturing companions has been broken in the single case of the planet Earth, we shall find the chances against it so overwhelming as to wholly exclude it from consideration.

3) Thus the companions or satellites could originate in but one of two possible ways; namely, by capture, and by detachment. Let us make the case as favorable as possible to the theory of detachment, and put the probability of the two events each equal to $^1/_2$. Then as we have eight principal planets, 25 satellites (besides our moon), and over 660 asteroids — all certainly captured — the chances are at least $(2)^{693}$ to unity that the moon has been captured. This number exceeds a decillion decillion (10^{66}) to the third power, $(10^{66})^3$, and is so enormous that it passes all comprehension.

4) Even a decillion decillion (10^{66}) is so large that we are compelled to resort to a method employed by Archimedes to illustrate it. Imagine sand so fine that 10000 grains will be contained in the space occupied by a poppy seed, itself about the size of a pin's head; and then conceive a sphere described about our sun with radius of 200000 astronomical units (α Centauri being at a distance of 275000), entirely filled with this fine sand. The number of grains of sand in this sphere of the fixed stars would be a decillion decillion (10^{66}).

5) But to correctly understand the actual probability of the origin of the moon by capture, we must extend the method of Archimedes and conceive all the grains of sand included within this sphere with radius extending to α Centauri, to be arranged in a continuous straight line as close together as possible (such a line will of course extend to infinity), and then imagine a cube erected on this infinite line as a base; and when this infinite cube is entirely filled with the finest sand, all the grains included within it against one is the probability that our moon also has been captured, and that the Lunar Terrestrial system forms no exception to the general rule of cosmical evolution by capture prevailing in the development of the solar system.

6) As this mode of calculation by the theory of probability is entirely rigorous and not merely approximate, it therefore incontestibly follows that our moon too has been captured and added to our terrestrial system from without, and therefore never has been nearer us than at present, but has come to earth from heavenly space.

7) Consequently we conclude that the events traced by Darwin depend on accidental coincidences, and do not represent the true physical history of nature. Accordingly all our previous conceptions in Astronomy, Physics of the Earth, and Geology as dependent on the moon's supposed detachment from our planet[1]), must be wholly abandoned, and all the questions again re-examined, in the light of the new theory, from the ground up. This affords us an impressive illustration of the incompleteness of the physical sciences today.

8) The present distance of the terrestrial moon in the inner part of the closed Hill surface about the earth corresponds with the theory that this body has been captured, in which case it could hardly have remained very near the outer portions of this space. When the moon was first captured, however, its distance can hardly have been less than twice what it is now; so that the distance probably has been greatly reduced in the lapse of ages.

9) If this view be admissible, it follows that the mean distance has been reduced principally by the secular action of the resisting medium; and the month has been shortened from some eighty days to 27.32166 days, as at present. The original month may have exceeded 100 days, but as Dr. Hill has shown cannot have exceeded 204.896 days.

10) If the mean distance has been so much reduced, it follows that the eccentricity of the orbit has also been correspondingly diminished. The present eccentricity of 0.05489972 therefore agrees well with the capture-theory. The view that the present eccentricity is a survival of a larger value appears probable in itself; and is in harmony

[1]) The theory that the moon was thrown off from the earth seems to date back to Anaxagoras, B. C. 500—428.

with the tendencies observed in other satellite systems, where the same cause has been at work.

11) The inclination of the lunar orbit to the ecliptic, 5° 8′ 43″.35, is about what would be expected from the capture theory, and naturally the orbital motion would be direct. For when a body is captured the chances by theory are much greater that it will move direct rather than retrograde, and we see this theory confirmed by what is observed in the other satellite systems. This follows naturally from the circumstances that a captured satellite has to cross the line of conjunctions before coming under the control of the planet, in order to give a retrograde motion, unless of course such satellite has come in at random and follows no law whatever.

12) The great preponderance of the moon's moment of momentum of orbital motion (3.384) over that of the earth's axial rotation (0.7044) is of itself a suspicious circumstance, and difficult to account for, without introducing violent hypotheses. But if the moon is captured this unusual circumstance presents no difficulty.

13) Darwin's celebrated diagram does not show how the system of the earth and moon came to be started; but only shows what will follow from a given condition of the system. Now if the bodies were started to revolving in a perfect vacuum, they might separate as he supposed, but if the resisting medium is more effective than tidal friction, the bodies will approach one another in spite of the energy curve in the diagram; for this curve rests on dynamical equations which postulate no resistance. When the resisting medium is introduced the energy curve is no longer valid, but the outcome will depend on the relative importance of the two rival forces — tidal friction and the resisting medium, the secular effects of which are exactly opposite. In order to judge which is likely to predominate, it is sufficient to recall the circularity of the orbits of the planets and satellites noticed elsewhere in our system, and directly traceable to this latter cause and no other.

14) Halley first suspected the existence of a secular acceleration of the moon's mean motion in 1693. It was confirmed by Dunthorne in 1749, and in the same year Euler advanced the view that all the heavenly bodies were subject to the secular effects of a resisting medium. Notwithstanding Laplace's celebrated discovery in 1787 that the secular decrease in the eccentricity of the earth's orbit was responsible for most of the observed secular acceleration of the moon, it continues to be an unsettled question. The correction of Laplace's process of calculation by Adams in 1853, and the verification of the latter's procedure by Delaunay, Plana, Lubbock, Hansen, Cayley, and others, allows gravitational theory to account for only about two thirds of the observed effect indicated by the most ancient observations, 6″.11 according to Delaunay, while the most ancient eclipses of the sun make the observed secular acceleration about 12″.00. And recently Mr. Cowell has confirmed a secular acceleration of the moon of at least 9″ by new researches on eclipses, and besides found a sensible secular acceleration of the sun, which could not be accounted for by any hitherto recognized cause. Why not go back to Euler's sagacious suggestion of the resisting medium to explain both of these outstanding anomalies? If the resisting medium has shaped the orbits of the heavenly bodies, it has not yet entirely disappeared, but must produce small effects which are sensible to observations extending over long ages.

15) And of all the bodies in our system adapted to disclosing the secular effects of this slowly acting cause, the moon is by far the most sensitive, as was long ago remarked by Euler. It is like a delicately adjusted chronometer, and the slightest disturbance will at length become sensible to observation. The next most sensitive of the heavenly bodies is undoubtedly the sun (or rather the earth), because of the accuracy of our modern observations and the considerable period over which they have extended. And here it is that Mr. Cowell of Greenwich has recognized the anomalies which heretofore have been attributed to the secular effects of tidal friction in changing the length of the day.

16) If the views set forth in this paper be admissible, they will tend to restore our confidence in ancient eclipse observations, and also in the steadiness of the earth as a time keeper, while they will give a severe shock to those who consider the heavenly spaces devoid of sensible resistance. And while the effects attributed to tidal friction seems to be less important than they have been supposed to be, on account of the present great distance of the moon, and the indication that it has never been sensibly nearer the earth, yet the importance of this cause will always be considerable, both in our own system, and in other systems observed in the immensity of space. The change in our point of view of course does not diminish the value of Professor Sir G. H. Darwin's celebrated work on this subject, but simply limits the scope of the results when applied to the systems nearest at hand. Even if inapplicable to the moon or applicable to but a limited extent, his beautiful analysis will always be the basis of future researches in this extensive subject, which deals with one of the most important physical causes effecting the figures and motions of the heavenly bodies.

U. S. Naval Observatory, Mare Island, California, 1909 May 22.

T. J. J. See.

Addition. On p. 368, line 7 from top, after the words: »four times the present distance of the moon« insert: The value thus obtained is the maximum value, corresponding to the part of the surface nearest the sun; of course other parts of the surface are nearer the earth.

July 3, 1909.

T. J. J. See.

Elizabeth Thompson double star micrometer.

A new micrometer especially adapted to the requirements of double star observers is being constructed »all steel«, so as to effect constant readings of the coincidences. The micrometer screw, on which is fixed a divided head and concentric revolution counter, is an inch long and each revolution amounts to a hundredth of an inch. It carries four parallel wires, the intervals being respectively 0.0975, 0.1050, and 0.1025 of an inch. The fixed wires are also four in number, their distances apart being 0.1000. They are fixed on a sliding box (Dawes's slide) which is moved by a screw, the head of which is opposite to and of the same dimensions as the head of the micrometer screw. In Repsold's micrometer there is a screw for changing the coincidence of the wires by part of a revolution in order to eliminate periodic errors of the screw, but the bisection of one of the components is supposed to be made by aid of the slow motion of the telescope[1]), which is very inconvenient, not to say impossible. In Burnham's micrometer, which is the best, there is a screw sliding the box by aid of which one component is bisected while the other component is bisected by the micrometer screw, but there are no means of varying the coincidence, so that periodic errors are not eliminated and the screw will be unequally worn in time. By the new arrangement explained above one screw serves for both purposes (Comp. A. N. 2187). There is a quick screw movement to slide the eyepiece (a single lens so as to minimise the loss of light), but every pair of wires need not be used on the same night. They may be used on different nights. In case of a good screw two wires placed $10^1/_2$ revolutions apart would suffice. There are no perpendicular wires. The wires are illuminated by an electric lamp, the brightness of which can be sufficiently increased to make use of a high power, and sufficiently decreased to be able to measure the faintest objects. Mr. Burnham states that in his micrometer any object that can be seen can also be measured. The screw-head and position circle are read (through single fixed lenses) by a small bull's eye lamp held in the hand. The cost of this instrument is defrayed from a grant voted out of the Elizabeth Thompson Science Fund.

Sutton, Surrey, 27. May 1909. *W. Doberck.*

[1]) Die Repsoldschen Positionsmikrometer besitzen seit 1875 neben der oben beschriebenen veränderlichen Endwiderlage der Mikrometerschraube zur Elimination der periodischen Schraubenfehler auch die zweite Schraube für die Verschiebung des ganzen Mikrometerkastens. Vergl. auch Winnecke V. J. S. **10**.300. *Kb.*

Photographische Aufnahmen von kleinen Planeten.

1909	Objekt	M.Z.Kgst.	α 1909.0	δ 1909.0	tägl. Bew.	Gr.	Bb.
Juli 21	(322) Phaeo	11^h41^m8	$21^h\ 3^m0$	$-4°\ 52'$	—	11^m1	L
	(313) Chaldaea	»	21 18.3	− 2 49	—	12.0	»
22	(322) Phaeo	11 33.5	21 2.2	− 4 51	—	11.1	»
	(313) Chaldaea	»	21 17.4	− 2 55	—	12.0	»
	(462) Eriphyla	11 39.6	20 23.8	−20 51	$-0^m9-4'$	12.8	K

K = *A. Kopff*, L = *W. Lorenz.*

Astroph. Institut Königstuhl-Heidelberg, 1909 Juli 24. *M. Wolf.*

Komet 1909 a. 1909 Juli 20 $11^h19^m40^s$ Wien $\Delta\alpha = -1^m28^s67$ $\Delta\delta = +1'51''0$ Vergl. 6 α app. $3^h55^m24^s95$ (9.800n) δ app. $+62°5'57''9$ (0.810) Red. ad l. app. -0^s46 $-8''6$. Vergl.-Stern 1909.0 $3^h56^m54^s08$ $+62°4'15''5$ AG Hels 3338.

Der Komet ist kaum zu sehen, nur ein Nebelhauch; dabei war freilich die Höhe ziemlich gering. Korrektion der Ephemeride A. N. 4337 $+16^s$ $+0'1$. Der fixsternartige Kern ist noch immer vorhanden. *J. Palisa.*

(124) Alkeste. Genauer Ort nach Ausmessung der photographischen Aufnahme: 1909 Februar 25 12^h51^m(zirka) M.Z.Kgst. α 1909.0 $10^h33^m3^s57$ δ 1909.0 $+6°\ 30'19''1$. Vergleichsterne Kü 4697,4727.
Astrophys. Institut Königstuhl-Heidelberg 1909 Juni 18. *M. Wolf.*

(95) Arethusa. Korrektion der Ephemeride (B. J. 1911): 1909 Juli 20 $+28^s04$ $+3'16''5$. *W. Luther.*
(217) Eudora. Phot. Aufnahme: 1909 Juli 21 12^h30^m Wien $\alpha = 19^h38^m48^s$ $\delta = -6°10'6$ Gr. 11^m5. *J. Rheden.*
(569) Misa. Korrektion der Ephemeride (V. R. I. 37): 1909 Juli 21 -5^m44^s $-25'7$ Gr. 12^m5. *J. Palisa.*

Geschlossen 1909 Juli 31. Herausgeber: H. Kobold. Druck von C. Schaidt (G. & A. Oheim). Expedition: Kiel, Moltkestr. 80.