acceptance which his endeavour to solve the climatal problems of past epochs by astronomical computation has very deservedly met with on the part of Geologists, his denial of the possibility of a thermal circulation in the ocean is utterly repudiated alike on mathematical and on experiential grounds, by those whose authority as physicists ought to make him feel less confident in his own conception of the question. W. B. CARPENTER

Source of Volcanic Energy

A FEW words of explanation are necessary concerning my letter which appeared in NATURE, vol. xii. p. 396. Mr. Mallet's *prime* source of energy for producing tangential pressures is the force of cohesion developed in a cooling globe, gravitation giving only partial assistance; and when I spoke of "gravitation of the whole mass to itself," I wished to convey that, setting aside altogether the force of cohesion and its accompanying motions, there still remains the force of gravitation, which, acting in a globe of such size as the earth, and composed of heterogeneous materials, must of itself produce enormous local pressures.

Mr. Fisher objects to my supposing the possibility of the development of heat without room being left for motion, but so far as I understand the doctrine of energy, it is only necessary to have *force* for the production of heat when motion is impossible.

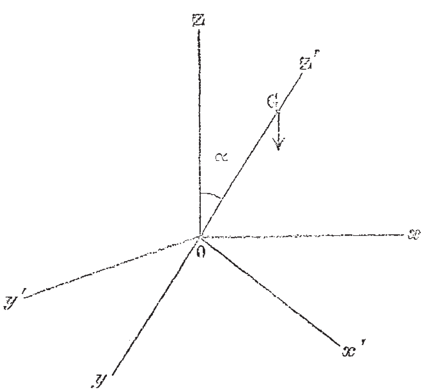
In Mr. Fisher's interesting paper his objection appears to be to the localisation of *fusing*, and not to the localisation of heat, fusing in some cases being prevented by the accompanying pressure. But in my little diagram I attempted to explain that the forces producing the high temperature might act in one set of strata, the neighbouring strata above and below at the same time being under much lower pressure, the pressure upon them being equal to the pressure of the rocks doing the work, minus the cohesion of said rocks; this difference of pressure being sufficient to allow one set of rocks to melt while others are crushed.

Kenmare, Co. Kerry WM. S. GREEN

Gyrostat Problem: Spinning-top Problem

IN vol. xi. p. 424 is given the solution, by Sir W. Thomson, of his gyrostat problem at p. 385. I venture to send a slightly different method * of obtaining the result (far inferior to Sir W. Thomson's in elegance and simplicity), in which Euler's equations for the motion of a rigid body about a fixed point are employed.

1. Take point of suspension for origin; the string for axis of z. The axis of the wheel Ox' revolves in horizontal plane xOy with uniform angular velocity Ω, and the wheel revolves round its axis Ox' with angular velocity w_1. The weight of wheel and axis will have moments round an axis Oy' in horizontal plane



perpendicular to Ox'. Let w' = weight of wheel and axis; A, B, B, moments of inertia round Oz, Ox', Oy'; w_2 angular velocity round Oy' at time t; a = the distance of C. G. from Oz, $xOx' = \phi$ = angle described by Ox' in time t. Taking moments about Oy', we have

$$B\frac{d w_2}{d t} + \overline{A - B}\, w_1 \Omega = w' a g \quad . \quad . \quad . \quad (1)$$

(Pratt, "Mech. Phil." 446). Also since there is no velocity about an axis in horizontal plane perpendicular to resultant axis of $w_1 w_2$,

$$w_1 \sin. \phi - w_2 \cos. \phi = 0 \quad . \quad . \quad . \quad . \quad (2)$$

where $$\phi = \Omega t.$$

$$\therefore \frac{d w_2}{d t} = w_1 \Omega \sec.^2 \phi = w_1 \Omega \text{ for } t = 0 \text{ in (1), since } w_1, \Omega \text{ are}$$

independent of the time; whence (1) becomes

$$A w_1 \Omega = w' a g,$$

where $$A = w k^2, \Omega = \tfrac{1}{4} \quad . \quad . \quad . \quad . \quad \text{Q.E.D.}$$

2. A similar question (concerning a spinning top) was proposed in the Senate House, Cambridge, in 1859, of which indeed the preceding is a particular case.

A uniform top spins upon a perfectly rough horizontal plane, its axis being inclined to the vertical at a constant angle α, and revolving about it with constant angular velocity Ω. Prove that the velocity of rotation of the top about its axis must be $\dfrac{(a^2 + k^2)\,\Omega^2 \cos.\alpha + g a}{k_1{}^2 \Omega}$, where a is the distance of the centre of gravity from the extremity of the peg, $k' k$ the radii of gyration about the axis of figure, and about an axis through C. G. perpendicular to it respectively. Take O, the extremity of the peg, which remains fixed, as origin, and let Oz' be position of axis at any time t; $OG = a$; $zOz' = \alpha$. Let M = mass of the top; A, C, C, moments of inertia about Ox', Oy', Oz' (rectangular axes moving with the top); $w_1 w_2 w_3$, angular velocities about Ox', Oy', Oz' at time t.

The intersection of planes xOy, $x'Oy'$ will move round Oz with angular velocity Ω. Let ϕ = angle which Ox' makes with this line.

If we take moments about Ox', we have by Euler's equations (Pratt, art. 446)—

$$\frac{A\, d w_1}{d t} + \overline{C - A}\, w_2 w_3 = M g a \cos. z y^1 \quad . \quad . \quad (1)$$

Also $w_1 = \Omega \sin.\phi \sin.\alpha$, $w_2 = \Omega \cos.\phi \sin.\alpha$, $w_3 = \dfrac{d\phi}{d t} + \Omega \cos.\alpha$

$$\cos. z y^1 = \cos.\phi \sin.\alpha \text{ (ibid. 447)};$$

$$\therefore \frac{d w_1}{d t} = \Omega \cos.\phi \sin.\alpha \frac{d\phi}{d t} = \Omega \cos.\phi \sin.\alpha\,(w_3 - \Omega \cos.\alpha).$$

Substituting in (1) and reducing, we get—

$$C \Omega w_3 = M g a + A \Omega^2 \cos.\alpha. \quad . \quad . \quad . \quad (2)$$

But $A = M(k^2 + a^2)$, $C = M k_1{}^2$;

$$\therefore w_3 = \frac{g a + (a^2 + k^2)\,\Omega^2 \cos.\alpha}{k_1{}^2 \Omega}.$$

If $\alpha = 90°$ in equation (2), we get the solution of the preceding question as a particular case. F. M. S.

Arnesby

* A comparison of this method with Sir W. Thomson's (which is virtually the same as that adopted by Airy in his tract on Precession and Nutation) is instructive as illustrating the dynamical meaning of Euler's equations.—ED. NATURE.

OUR ASTRONOMICAL COLUMN

THE MASS OF JUPITER.—M. Leverrier has made a special communication to the Paris Academy of Sciences with reference to the bearing of his researches on the motion of Saturn, in a period of 120 years, on the value of Jupiter's mass. Laplace, in the *Mécanique Céleste*, had fixed $\frac{1}{1067{\cdot}09}$ making use of the elongation of the fourth satellite as determined by the observations of Pound, the contemporary of Newton, observations of which it appears we have no knowledge, except from the reference to them in the "Principia;" subsequently Bouvard, comparing Laplace's formulæ with a great number of observations, discussed with particular care, constructed new Tables of Jupiter, Saturn, and Uranus, in which important work he formed equations of condition, wherein the masses of the planets entered as indeterminates, and by the solution of which their values adopted in the Tables were obtained. The denominator for Jupiter's mass, expressed as a fraction of the sun's taken as unity, is 1070·0, and Laplace stated that on applying his theory of probabilities to Bouvard's equations it appeared to be nearly a million to one against the error of the mass thus deduced, amounting to one-hundredth part of the whole. M. Leverrier then

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refers generally to the discussion of the observations of several of the minor planets with the view to correcting the mass of Jupiter, and to the observations of elongations of the fourth satellite by the present Astronomer Royal at Cambridge, which last assigned for the denominator of the fraction 1046·77. He then remarks upon the circumstance of Bouvard having deduced from his comparison of the theory of Saturn with seventy-four years' observations a mass so nearly identical with that of the *Mécanique Céleste;* Bouvard left no details of his work behind him; it is only known that he adopted at the outset the value of Jupiter's mass admitted at the time, that of Laplace, and M. Leverrier explains that on the method of procedure adopted, Bouvard could not do otherwise than reproduce at the termination of his calculations the value he had assumed at starting. This is illustrated by the result of Leverrier's solution of his own equations of condition, founded upon the much longer period of 120 years, which proved wholly insufficient for the correction of Jupiter's mass. He remarks, with respect to Bouvard's work, that any value of the mass taken arbitrarily within certain limits will allow of a tolerable representation of the observations of Saturn, on the condition that this same arbitrary value is introduced throughout in the functions representing the mean longitude, mean motion, excentricity and longitude of perihelion; the elements obtained by Bouvard are therefore found represented by these functions of his arbitrary quantity, and he reverts to the mass assumed at the commencement of his work.

In conclusion, M. Leverrier insists that the use of the elongations of the fourth satellite for the determination of the mass of the Jovian system, has at present an incontestable superiority over the employment of the theory of Saturn, on account of the too short period over which the observations as yet extend, but in the lapse of time this superiority of the former method will diminish and the use of the perturbations will become the more advantageous. It is really, he adds, the same question as that which presents itself with regard to the solar parallax, which is determinable on two methods: the one, geometrical, the method by transits of Venus; the other, mechanical, depending for instance on the large inequalities in the motion of Mars. The method by transits, so important in 1760, but limited in its means of application, must eventually give way to the method of perturbations, the accuracy of which will increase unlimitedly with the course of time.

The first evaluation of the mass of Jupiter is that of Newton in the Cambridge edition of the "Principia" (1713), inferred from Halley's observation of an emersion of Jupiter and his satellite from the moon's limb, giving for the denominator of the fraction (whereby it is usual to express the mass) 1033. In the later editions of the "Principia" the mean distance of the fourth satellite resulting from Pound's observations, to which allusion is made above, was substituted in the calculation of the mass, which was found to be 1067. (It may here be mentioned that from later observations by Pound with a micrometer on a telescope of 123 feet focus, on the mean distance of the third satellite, Bessel found for the mass 1066). The next attempt in this direction appears to have been made by Triesnecker, Director of the Observatory at Vienna. In 1794 and 1795, making use of a Dollond object-glass micrometer, he obtained a series of measures of distances of all four satellites, the notice of which appears in the Vienna Ephemeris for 1797. Bessel deduced from them, by a mean of the four values, 1055·68. Then follow Bouvard's investigations already mentioned. It is understood that Gauss was the first to bring the perturbations of the minor planets to bear upon the determination of the mass of Jupiter, and that from the perturbations of Pallas he perceived the necessity of an increase to the mass adopted by Laplace. The circumstance, so far as we know, rests upon the authority of Nicolai, who, following in the same steps, discussed observations of Juno at fifteen oppositions, between the year 1804 and 1823, and (in the *Berliner Astronomisches Jahrbuch* for 1826) deduced for Jupiter's mass 1053·92. Encke, from fourteen oppositions of Vesta, between 1807 and 1825, made its value 1050·36, in a paper published by the Berlin Academy of Sciences in 1826.

Sir George Airy's observations at the Cambridge Observatory, alluded to by M. Leverrier in his recent notice, are next in order of time. They were commenced in 1832 and continued till 1836. The final result appears in vol. x. of the Memoirs of the Royal Astronomical Society; it is 1046·77, and depends upon observations on thirty-three nights. Details of the earlier Cambridge observations will be found in vols. vi. and viii. of the same memoirs. Sir George Airy considered it very improbable that there could be an error of a single unit in the denominator of the fraction expressing the mass, being led to this opinion by the close agreement of the separate results.

In the year 1835 Prof. Santini, the present venerable director of the Observatory of Padua, by sixteen nights' measures of the distance of the fourth satellite from both limbs of Jupiter, obtained for the mass 1049·2 (*Ricerche intorno alla Massa di Giove*, Modena, 1836).

Bessel's elaborate series of measures of distances of the four satellites commenced in October 1832 and were completed in the middle of 1839. They are fully discussed in his very valuable memoir, *Bestimmung der Masse des Jupiter*, in vol. ii. of his *Astronomische Untersuchungen:* the definitive value of the mass (p. 64) is 1047·879. Bessel's mass, which has been generally adopted in the calculation of the perturbations of minor planets and comets, and which is so close a confirmation of that deduced by the Astronomer Royal, has received much additional support from recent and, as regards method, essentially different investigations. Thus Krueger, of Helsingfors, from the perturbations of Themis, one of the minor planets which approaches nearest to Jupiter, assigns 1047·16; Axel Möller, by his masterly researches on the motion of Faye's Comet, 1047·79; while Von Asten, from his last investigations relating to Encke's Comet, finds 1047·61.

THE HOPKINS UNIVERSITY, U.S.

THE munificent bequests made by wealthy Americans for the promotion of education in the United States frequently excite our astonishment, for they are unparalleled in Europe at the present time. One of the most unique and well-devised of these bequests has lately occurred. Last year there died a Mr. Jonas Hopkins, a rich citizen of Baltimore, who, after providing for his relatives and leaving various minor benefactions, bestowed the chief part of his estate to found a university with an affiliated medical school and hospital. Both the university and the hospital receive separate landed and other property of such a substantial character that the value of the total amount is over three millions of dollars. Each institution is to be controlled by a board of nine trustees, and the same persons are to be on both boards. The university will have no ecclesiastical or political character or supervision, and will be modelled as far as possible after all that is best in similar American and European institutions. It is intended to give the highest instruction that can be obtained, and the trustees are to act in accordance with the most enlightened experience of the day. The scientific and literary departments will first be organised, and then will follow the departments of Medicine and Law.

No permanent buildings will be erected till all the Faculties are in working order and the wishes of each professor can be carried out; meanwhile a building has

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