

before rise of temperature, we have GMm/d^2 . After rise we have

$$\frac{GMm}{d^2} [1 + \alpha(T + t)]$$

If we, however, assume that increments in $g.m.$ are multiplied separately, we should have

$$\frac{GMm}{d^2} [1 + \alpha^2(Tt)]$$

Neither of these formulæ helps us to reconcile the above facts.

But now suppose that gravitational attraction between two masses consists of two parts:—

(a) The essential-mass term. Attraction between the masses occurs in virtue of the ether displaced by the Faraday tubes attached to their electrons. This would be like Maxwell's stress theory of gravitation: compression of ether radially from each body and tensions in directions perpendicular to the radii. This term is represented by the usual form $f_1 = GMm/d^2$. It is independent of molecular vibration and exists at absolute zero.

(b) The temperature term. Attraction is due to vibration of the Faraday tubes, which are carried to-and-fro by the molecules in their vibratory motion. This is like Challis's wave theory of gravitation, whereby bodies in a vibrating medium attract one another if their phases are in close agreement. Dr. C. V. Burton suggested that for very high velocity of wave transmission the vibrating bodies might resonate one another and have approximately like phases. Presumably the waves in this case are longitudinal and their velocity nearly infinite. If the power that one mass has of setting another to resonate depends on the ratio, mass of vibrator/total mass, this attraction would be

$$f_2 = \frac{G}{d^2} \left[M\alpha T \left(\frac{M}{M+m} \right) m + m\alpha \left(\frac{m}{M+m} \right) M \right]$$

Adding (a) and (b) terms,

$$f = f_1 + f_2 = \frac{GMm}{d^2} \left[1 + \alpha \left(\frac{MT + mt}{M+m} \right) \right] \dots i.$$

This expression was suggested, though not derived, by Poynting and Phillips. Evidently, when M preponderates greatly over m (the only case we need consider),

$$f = \frac{GMm}{d^2} (1 + \alpha T),$$

so that a change in temperature of M might affect f appreciably, but no such change in m could do so.

This expression, then, would make all the facts compatible. We have supposed that the temperature effect depends on the first power, but it would be more natural to consider that the intensity of vibration varies as the square or higher power of temperature. In that case we should have for variation in the Newtonian constant, $G = G_0(1 + \alpha T^n)$.

It may be significant that the coefficient of cubical expansion of lead (the material used), viz. 8.4×10^{-5} , is of the same order as my result, 1.2×10^{-5} , the increment of f being $1/7$ of the increment in volume in the lead.

Above we have taken $g.m.$ and $i.m.$, so far as these depend on ether displacement, to be invariable, but as the body rises in temperature from absolute zero, the vibrations may, especially at high temperature, cause such violent agitation of Faraday tubes that the effective displacement of ether is increased. If this were so, of course both $g.m.$ and $i.m.$ would increase, since in that case the essential mass would increase. Mathematicians might assist in deciding this point. But, at present, for temperatures up to,

say, 500°C. , we might suppose neither $g.m.$ nor $i.m.$ to change from this cause to any perceptible amount.

To make clear the action of the above formula, imagine the case of sun, earth, and moon. If the mean temperature of the earth were to rise greatly, say through sudden radio-activity in its interior of some element previously inactive, then the temperature term for the earth would increase by an amount small compared with the essential mass term of (sun + earth), but large compared with that of (earth + moon). Thus the earth's orbital motion would not change appreciably, but attraction between earth and moon would increase and the moon's orbital motion might be greatly affected.

Applying our formula i. to the comments of "J. L.," we should not anticipate change due to temperature in $g.m.$ or $i.m.$ in the cases of pendulum experiments or planetary orbital movements, nor should we expect "kicks" in moving masses the temperatures of which are suddenly changed. In like manner, a comet, even though considerably heated or cooled, would be expected to have regular motion. The great difficulties suggested by "J. L." would all vanish if formula i. or something akin were true.

It might be thought that my research, standing alone, is slender evidence on which to raise such important results; but I would mention that, as shown in my paper, my result is buttressed by indirect evidence.

If the formula i. be true, my contention is strengthened (see NATURE, October 7, 1915) that a laboratory value of G should not be considered valid for application to the attraction between masses (e.g. the heavenly bodies) the temperatures of which are far from ordinary. The whole problem is complicated by the high temperatures involved in the members of the solar system. We know that the rigidity of the earth, taken as a whole, is very great, so that the immense pressure in the core counteracts the fluidising influence of the very high temperature. Elasticity is, at a surface view, a molecular property; gravity is primarily an electron/ether property; nevertheless we are on unsure ground in reasoning that any property will be the same, say, at 5000°C. and at 0°C.

Following the guidance of the formula i., we may expect fruitful research if we vary the temperature of the large mass; but we should anticipate that no good results could be derived from experiments on temperature change of the small mass.

Poincaré pointed out (Report to the International Congress in Physics, 1900) that the mass of Jupiter, as derived from the orbits of its satellites, as derived from its perturbations of the large planets, and as derived from its perturbations of the small planets, has three different values. This would lead one to give to G a different value in each of the three cases. It will be seen to accord with equation i. above, for in the three cases the ratio $\left(\frac{m}{M+m} \right)$ is very different.

It may be a useful fact in the present argument.

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Payment for Scientific Research.

IN future discussions on this difficult but important question, it will be well that a distinction should be drawn between the case of a specialist who engages in research on a subject of his own choice, devoting as much or as little of his time as he cares to give to it, and that of a scientific expert who agrees to undertake work for the Government or some other body

on definitely stated subjects, and who is, as a general rule, expected to complete the duties within a more or less definite time-limit.

For investigations falling under the first category the problem of remuneration presents serious difficulties, and we may at least console ourselves with the knowledge that a step in the right direction has been taken by the Board of Education in requiring returns to be made of researches conducted by the staffs and graduates of our university colleges. In this connection it is, further, becoming recognised that teachers in these institutions should have sufficient opportunity in term time, as well as in vacation, for research.

It is with regard to the second class of investigation that the claim for remuneration is most urgent. From personal knowledge, I consider that it is impossible for an average skilled labourer in the scientific industry to earn a living wage consistent with his necessary expenses unless his whole time is available for remunerative duties. It is true that intervals occur, sometimes quite unexpectedly, during which he may be temporarily unemployed, and these can be utilised for purposes of research; on the other hand, there are certain periods of the year when the work is extremely heavy, and latitude of time is necessary even for the performance of paid work.

There are probably very few scientific labourers who would be justified in refusing an invitation to mark 500 examination papers at a fee of 1s. per paper in order to complete an investigation for the Government for which they received no fee. As soon, however, as the labourer accepts remuneration for a definite undertaking, his employer has some guarantee that he will not let future engagements interfere with the fulfilment of his contract. This at least applies to scientific specialists who are not members of trade unions.

I am very much afraid, however, that a great many people are undertaking unpaid work under conditions quite incompatible with the present depressed conditions of the scientific labour market. In some cases this is being done from a sense of patriotism. Undoubtedly their labours may have the effect of reducing the duration and the severity of the lesson which the enemy countries are teaching us in regard to our national neglect of science—a lesson which is the one good turn the Huns are doing us. But they are certainly tending to diminish the efficacy of that lesson.

G. H. BRYAN.

Negative Liquid Pressure at High Temperatures.

In my paper with Lieut. Entwistle on the effect of temperature on the hissing of water when flowing through a constricted tube (Proc. Royal Soc., A, 91, 1915) I have determined the temperature coefficient of an effect which indicates that the tensile strength of water would be zero at a temperature between 279°C. and 363°C., with a mean from all the experiments published of 328°C. Sir Joseph Larmor's calculated result, 265°C., quoted by him in his letter in NATURE of June 29, agrees satisfactorily with the experimental value if we take into account the difficulty of getting the precise point at which hissing ceases, and that the result was obtained by extrapolation from observations taken at temperatures between 12°C. and 99°C. Lieut. Entwistle and I have experimented with other liquids—alcohol, benzene, acetone, and ether—and obtained results of a similar character. Experiments are now in abeyance, for my colleague is otherwise engaged.

My own view, formed from physical conceptions, was that the tensile strength of a liquid would become zero at its critical temperature. It is of very great

interest that Sir Joseph has been able to show mathematically that the negative pressure can only subsist at absolute temperatures below 27/32 of the critical point of a substance.

The conclusions appended to our paper are:—

1. That the phenomenon of hissing of water passing a constriction is due to a true rupture of the stream at the point where the pressure is lowest.

2. That the temperatures at which the hissing just occurs, between 0° and 100°C., follow a law which may be expressed $V = C(\theta - t)$, where V is the velocity of the stream at a temperature t , θ the critical temperature of water, and C a constant.

If we adopt Sir Joseph Larmor's view the latter law will require to be expressed

$$V = C \cdot 27/32(\theta + 273) - (t + 273),$$

or by a slightly more complex formula.

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July 3.

THE PROPAGATION OF SOUND BY THE ATMOSPHERE.

SINCE the beginning of the war the sound of gun-firing in Flanders and France has often been heard in the south-eastern counties of England. There can be little doubt as to the origin of the sounds, for the reports of distant heavy guns have a character which is readily recognised. A correspondent of the *Daily Mail* (July 6) states that at Framfield (near Uckfield), in Sussex, it is easy to identify the particular kind of gun which is being used. The great distance to which the sound-waves are carried under favourable conditions is evident from the letters recently published in the *Daily Mail*. As firing has occurred lately over a great part of the Western front, the exact position of the source of the sound is uncertain. But if it were in the neighbourhood of Albert the waves must have travelled about 118 miles to Framfield, 150 miles to Sidcup, and 158 miles to Dorking.

Of far greater interest are the form and discontinuity of the sound-area. A remarkable example of the inaudibility of neighbouring reports in the face of a gentle wind was given in the last number of NATURE (p. 385). This is a subject on which many observations have been made since the beginning of the present century, especially in connection with the sounds of volcanic and other explosions. The source of sound is always surrounded by an area of regular or irregular shape within which the sound is everywhere heard, though the source is not always situated symmetrically with reference to the boundary of the area. On several occasions a second sound-area has been mapped, separated from the former by a "silent region" in which no sound is heard. Sometimes this second area partly surrounds the other, sometimes it consists only of isolated patches. As a rule, according to Dr. E. van Everdingen, who has made a detailed study of the subject,¹ the least distance of the second area from the source is much more

¹ "The Propagation of Sound in the Atmosphere." *Koninklijke Akad. van Wetenschappen te Amsterdam*, Proc., vol. xviii., 1915, pp. 935-960.