

The left-hand skewness for  $p = 1$  and right-hand skewness for  $p = 3$  show in the figures. The different values of  $x_0$  were chosen so that the case rates at maximum  $C_{or} = x_0 m$  and total cases should be the same provided the approximate formula (16) were used and the value of  $x_0$  were taken from (B) in footnote 9. Slight irregularities in the numbers must be expected due to the limited number of places carried, and slight discrepancies in verifying (16) from the calculations because of the approximative nature of (16) and (B).

J. Brownlee stated, *Proc. Roy. Soc. Med., Epid. Sect., 2, Part 2, 243-258 (1909)*: . . . the symmetry of the course of the epidemic is an obvious and marked feature. The deduction from this phenomenon is direct and complete, namely, that the want of persons liable to infection is not the cause of the decay of the epidemic. On no law of infection which I have been able to devise would such a cause permit epidemic symmetry. The fall must in all cases be much more rapid than the rise, though, on the contrary, when asymmetry is markedly present the opposite holds. Ross<sup>1</sup> comments on this statement. We may point out that if we accept the generalization of the law of mass action suggested in (10) there is symmetry for  $p = 2$ , negative skewness for  $p < 2$  and positive skewness for  $p > 2$ . Thus a rather simple law has been devised which may explain symmetry or skewness of either sign. Furthermore, in the examples above which correspond to rather severe epidemics of measles the rise is at the (logarithmic) rate  $p \log x_B$  or 0.68, 0.77, 0.81, respectively, for  $p = 1, 2, 3$ ; and the rate of the fall is  $-p \log x_F$  or 0.89, 0.77, 0.74, respectively. In the first case the rate of fall is considerably greater than the rate of rise, in the second case they are equal, and in the third case the rate of fall is but slightly less than the rate of rise. With higher values of  $p$  the rate of fall would become considerably less than the rate of rise, but even with very high values of  $p$  and with the same values of peak cases and of total cases as in the illustrations above the rate of rise could probably not exceed 0.89 and the rate of fall not be lower than 0.69.

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## A LETTER FROM LORD RAYLEIGH TO J. WILLARD GIBBS AND HIS REPLY

BY EDWIN B. WILSON

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Communicated December 4, 1944

In the small collection of letters left by J. W. Gibbs and now in the possession of Ralph G. Van Name is one from Lord Rayleigh the answer to which I presumed still existed because the present Lord Rayleigh quoted three sentences from it in his biography of his father.<sup>1</sup> When I sent a copy of his father's letter to Lord Rayleigh, he kindly sent me a transcript of Gibbs's reply. As this exchange of letters between a foreign associate and a member of this Academy seems to me likely to be of sufficient interest to our members and of sufficient importance to the history of science to justify publication in full even at this late date, I have secured the permission of Lord Rayleigh and of Professor Van Name to print them here.

June 5/92  
Terling Place,  
Witham, Essex.

Dear Prof. Gibbs:

I have been experimenting lately upon the intensity of reflection from water at nearly perpendicular incidence, and the not very close agreement that I have found with Young's formula  $\left(\frac{\mu - 1}{\mu + 1}\right)^2$  has set me thinking whether there is any reason for expecting this formula to be correct, when the effect of *dispersion is included*. The case of two strings joined together one (as usual) perfectly flexible and the other with such stiffness as to introduce dispersion, shews that the reflection is *not* to be got from  $\frac{\mu - 1}{\mu + 1}$ , even though the correct  $\mu$  be used; I think the deviation from this is of the same order as the deviation of  $\frac{\mu_\lambda - 1}{\mu_\lambda + 1}$  from  $\frac{\mu_\infty - 1}{\mu_\infty + 1}$ . But this case does not much resemble optical dispersion (one would suppose), and I am writing partly to ask whether you have ever considered the problem of reflection with inclusion of dispersion on the lines of your published papers. And with respect to the latter I find a difficulty in your estimation on Kelvin's theory [is not this analytically identical with Lorenz?] of the potential energy as

$$\frac{\pi^2 B h^2}{l^2} + \frac{b h^2}{4}$$

(*Phil. Mag.*, XXVII, p. 24) in which the 1st contains  $l$  and the 2nd does not. Supposing the disturbance from uniformity to be by simple changes of elastic quality, would not the 2nd term contain  $l^2$  like the first.

And now on another subject. Have you ever thought of bringing out a new edition of, or a treatise founded upon, your "Equilibrium of Het. Substances." The original version though now attracting the attention it deserves, is too condensed and too difficult for most, I might say all, readers. The result is that as has happened to myself, the idea is not grasped until the subject has come up in one's own mind more or less independently. I am sure that there is no one who could write a book on Thermodynamics like yourself.

I feel that I am taking a liberty in writing like this, but it is in the interest of science and you will forgive me.

I remain  
yrs very truly  
Rayleigh

New Haven.  
June 27, 1892.

My dear Lord Rayleigh,

The electrical theory of light seems to afford a very simple equation for harmonic motions of any one period in any optical field, *without* neglect of the causes of dispersion. If we consider an element of space containing

many ponderable molecules, the electrical motions in that space are presumably very complicated, but they result from the motions at a distance, and the effect of motions at a distance can be expressed by a very simple formula. There is first the electromotive force of induction, of which the components are calculated from the components of acceleration by the same law as the potential is calculated in the theory of gravitation from the density of matter. If we write  $\overline{\text{Pot}}$  to express this operation, we will have  $-\text{Pot } \ddot{F}$  for this force,  $F$  being a bi-vector representing the displacement (*i. e.*, the three components of  $F$  are complex realms). There is also (or may be) an electrostatic force which can be represented by  $\nabla Q$ , where  $Q$  is a complex realm (the electrostatic potential). It should be borne in mind that the displacements which occur in the equations of wave motion are a sort of average for elements of space which are large in comparison with the distances of neighboring molecules, but small in comparison with a wave-length. If  $F$  is understood as representing such an average, and likewise  $Q$

$$-\text{Pot } \ddot{F} - \nabla Q$$

will still correctly represent the electromotive forces acting upon the element from a distance. Whether this formula correctly represents the internal forces of the element or those forces which arise from the immediate neighbourhood of the element is of no consequence. Now I say that  $F$  is a function of  $-\text{Pot } \ddot{F} - \nabla Q$ . This is really saying very little. It is only saying that if under any circumstance an average harmonic motion  $F$  subsists in an element of space, and if we then change things in the remoter parts of the field, but so that the value of  $-\text{Pot } \ddot{F} - \nabla Q$  shall remain unaltered at the element considered, the same motion  $F$  will continue to subsist in that element.  $F$  is therefore a function of  $-\text{Pot } \ddot{F} - \nabla Q$  and by the principle of superposition of motions a linear function. Or, we may say that  $-\text{Pot } \ddot{F} - \nabla Q$  is a linear function of  $F$ , and write as in my paper on Kelvin's quasi-labile ether.

$$-\text{Pot } \ddot{F} - \nabla Q = 4\pi\Phi F$$

the operator  $\Phi$  representing in general a linear vector function, which however in the case of an isotropic body reduces to a (so-called) numerical quantity (*real*, if the body is transparent). It will of course vary in different parts of the field with the optical properties of the bodies in the field. The equation is not indeed absolutely accurate—the definitions on which it is founded are not absolutely sharp, and the phenomena of (non-magnetic) rotation of plane of polarization form a striking exception to the equation, which I have discussed at length in my 2nd "Note on the Elec. Theory of Light." We may say that the inaccuracy in the equation results from the virtual assumption that the structure of a body is infinitely fine as measured by a wave-length of light.

But I do not see that any amount of *dispersion* constitutes any reason for the failure of the equation. It will apply, so far as I can see, even to cases of selective absorptive and abnormal (so-called) dispersion, as well as to any degree of opacity.

Now this general equation (as I have pointed out in my paper on Kelvin's

quasi-labile ether) gives us on one hand Fresnel's wave-surface, and on the other Fresnel's laws of intensities of reflected light, if we suppose that  $\Phi$  is constant on each side of a mathematical plane of reflection, which is of course a very precarious supposition. You will remember that in one of your papers you refer to one of Lorentz, in which he claims to obtain a remarkable agreement with Jamin's experiments for solids by supposing the change in index (i.e., in  $\Phi$ ) to be gradual. Your own experiments have shown that at least in the case of liquids the influence of foreign substances at the surface is considerable. Besides the question of the gradual or abrupt change in  $\Phi$  there is the question (at least when the foreign substances are present) of values of  $\Phi$  which are not intermediate, as complex values where  $\Phi$  is real on each side of the surface.

I was very glad that you vindicated Fresnel's law from the most considerable deviation as reported by Jamin for liquids, and feel quite sorry that your later experiments show in another way a deviation. I should hope for the sake of the theory (to which you see I am somewhat attached) that the deviations can be accounted for by attributing some not unnatural qualities to a thin film at the plane of reflection.<sup>2</sup>

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With respect to reflection of waves in strings, we may cause dispersion by supporting a heavy string by threads (very close together) from the ceiling. For waves of any one period (in time) the reflection where two such strings are joined is determined entirely by the wave-lengths in the two strings.

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I am afraid that I hardly have the right to say what the potential energy should be on Kelvin's theory, in respect to a point on which he has not expressed himself. It is however natural, if we imagine a sea of jelly to be surging to and fro among fixed molecules, to suppose that the jelly is thereby distorted. There are indeed two exceptions; 1st. If we suppose that the molecules do not affect the motion of the jelly, although they may have made it denser (by their attraction); 2nd. If we suppose the molecules to move with the jelly, taking the same displacements. In either of these cases (both apparently improbable) the momentum of the jelly would have the same ratio to the velocity for all directions about a point. This is not the case in Kelvin's theory, in which the effective inertia is aeolotropic (*Phil. Mag.* XXVI, 501). If then the jelly is distorted in its motion by the molecules among which it flows, the potential energy should not vanish for  $l = \infty$ , at least apparently not. (It is hard to speak with absolute certainty in regard to so peculiar a jelly, which enjoys moreover, I suppose, the privilege of further hypotheses in its behalf as they may become necessary.) I think we may say that it is reasonable to admit such a term in the formula on *a priori* evidence, its magnitude (whether zero or otherwise) to be determined *a posteriori*. I am not claiming the existence of an absolutely constant term, but one which does not vanish for  $l = \infty$ . I should expect, however, that it would be tolerably constant in most cases. As a matter of fact, it seems to me that we can measure it, as compared with the other term, by means of the dispersion of light, as I have in-

icated in the passage in question. As thus measured, it does not appear at all constant (for constant amplitude) as I should expect it to be. This seems to me a very serious objection to the theory, although one which I should urge with great diffidence, as Lord Kelvin has not touched upon these points.

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I thank you very much for your kind interest in my "Equilib. Het. Subst." I myself had come to the conclusion that the fault was that it was too *long*. I do not think that I had any sense of the value of time, of my own or others, when I wrote it.<sup>3</sup> Just now I am trying to get ready for publication something on thermodynamics from the *a priori* point of view, or rather on "Statistical Mechanics" of which the principle interest would be in its application to thermodynamics—in the line therefore of the work of Maxwell and Boltzmann. I do not know that I shall have anything particularly new in substance, but shall be contented if I can so choose my standpoint (as seems to me possible) as to get a simpler view of the subject.

I remain

Yours faithfully

J. Willard Gibbs

<sup>1</sup> *Life of Lord Rayleigh*, London, 1924. See pp. 172–173.

<sup>2</sup> As I interpret the statement of Lord Rayleigh in his *Scientific Papers*, vol. IV, p. 12, where the results of his experiment in question are reprinted, he finally concluded that the discrepancy between theory and experiment was not sufficient to require any explanation.

<sup>3</sup> This and the preceding two sentences are the three quoted in the *Life of Lord Rayleigh*. The reference to *Statistical Mechanics* which immediately follows is of particular interest in view of the publication about a decade later of the *Elementary Principles in Statistical Mechanics*.

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## STUDIES ON GEODESICS IN VIBRATIONS OF ELASTIC BEAMS

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Communicated December 12, 1944

The transverse vibrations of a homogeneous elastic beam that is simply supported (hinged) at both ends satisfy the fourth order partial differential equation

$$\frac{\partial^2 u(x, t)}{\partial t^2} + c^2 \frac{\partial^4 u(x, t)}{\partial x^4} = 0 \quad (1)$$

and the boundary conditions

$$u(0, t) = \left. \frac{\partial^2 u(x, t)}{\partial x^2} \right|_{x=0} = 0, \quad u(l, t) = \left. \frac{\partial^2 u(x, t)}{\partial x^2} \right|_{x=l} = 0, \quad (2)$$