



XLVIII. On the theory of gradients on railways

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The law in question, then, being a *function* of so many *variable* quantities, must be one of extreme complexity, perhaps beyond the powers of the most refined analysis to unfold.

XLVIII. *On the Theory of Gradients on Railways.*
By Mr. W. S. B. WOOLHOUSE.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

AS Dr. Lardner and Mr. Barlow, in your Numbers for January and February, hold out conflicting opinions on the theory of gradients on railways, and have left the subject in a state more calculated to create doubts in the minds of the less informed of your readers than to lead them towards the formation of settled conclusions, perhaps you will favour me with the insertion of a few words, by way of explanation, as far as the philosophy of the question presents itself to my mind. Mr. Barlow, without absolutely saying which of the two solutions is wrong, though probably quite conclusive in his own view of the matter, first states his objection to the arithmetical results of the formula employed by Dr. Lardner for the velocity, in certain cases, then gives an outline of his principle of investigation, and finally expresses himself "quite content to leave the decision to those whose minds have not already received a bias from preconceived notions of the forces." Whatever sentiments may prevail as to the competency of my opinions on such a subject, it will at least be acknowledged that I possess the qualification of being free from the bias here alluded to, and I am induced to hope that your readers will, on this very ground, acquit me of any imaginable interference in thus undertaking, voluntarily, the examination of a point that has already had the attention of such distinguished individuals. By close and continued application of particular opinions to particular subjects, it is indeed surprising how they fix themselves in the mind, and become ultimately, whether true or false, of almost a fundamental character. But I do not consider this observation to be applicable to the present case. It is my wish to simplify and expose the truth as far as I can perceive it. I do not, however, intrude the present remarks in elucidation of the subject without some degree of hesitation, although quite free from apprehension as to their theoretical soundness. To many of your readers, who must be far from satisfied with the present situation of the question, I nevertheless feel myself justified in submitting them.

According to Dr. Lardner, the subject is "totally distinct from the consideration of accelerating forces"; he considers it to be essential that the velocities be continued uniform, and therefore discards everything in the shape of an accelerating force. Now, in order that such a theory may be sustained, it is a well known elementary principle of forces, that the power employed must be always precisely equal to the resistance, or the amount of friction combined with the proper resolved effect of gravity along the railway, observing, however, that in the term friction, we must include the resistance to the motion experienced by the carriages, &c., in passing through the atmosphere. We shall not here discuss the practicability of preserving this exact balance between the forces at the various changes of inclination; nor shall we offer any serious objection to the principle that the friction is the same for all velocities, which has received the sanction of general practice, though doubtless inaccurate, as far as regards the effect of the atmosphere.

Continuing the notation of the preceding letters, we have t for the moving power that will keep the load moving at a uniform speed V along the level plane; $t + \sin \epsilon$ for the moving power to keep the load moving at the same uniform speed up the inclined plane; and $t - \sin \epsilon$ for the moving power to sustain the same uniform speed down the inclined plane. To the truth of this there cannot be any doubt, if we assume, as Dr. Lardner has done, that the friction t is not altered by the slight inclination of the plane. By following Dr. Lardner's reasoning, we are hence fairly led to the result that the same amount of mechanical force will be expended in ascending and descending the inclined plane, as in drawing the same load backwards and forwards along the level plane of the same length L .

Though Dr. Lardner is certainly justified in stating this conclusion to be a plain result of first principles, it should at the same time be remembered, that it rests solely on the hypothesis that the power in each case is to be precisely adapted to the amount of resistance, so as to preserve throughout the the same uniform velocity V . This hypothesis has not been admitted by Mr. Barlow, and it must necessarily fail in determining the effect produced by the deflection of a rail during the transitory passage of the carriages. In this way, it appears to me that the principle advocated by Dr. Lardner carries with it a restriction that entirely unfits it for an objection to what has been advanced by Mr. Barlow, in his Second Report, addressed to the directors of the London and Birmingham Railway Company. On the other hand, "however, I can

only come to Mr. Barlow's conclusion, that it is altogether erroneous, both in theory and practice," when the assumed maintenance of uniform motion is objectionable, as it most certainly is, in the case of the deflections of rails. Contenting myself at present, then, with the opinion that the contending parties thus view the question of power expended, on different suppositions as to the way in which it is applied, I shall just take a very brief sketch of the question of velocity, when the motion is not assumed to continue the same through planes of different inclinations.

Dr. Lardner supposes that in cases of uniform velocity, the resistance into the velocity is constant, and on this assumption deduces the equations stated by Mr. Barlow in page 97, viz.

$$(t - \sin \epsilon) v = t V \qquad v = \frac{t V}{t - \sin \epsilon}$$

This assumed principle is, in my opinion, decidedly inaccurate, more especially when it is contemplated that the carriages will pass along with the uniform velocity so expressed. For uniform motion can only be continued when the moving force continues equal to the resistance; and assuming with Dr. Lardner that the amount of friction is independent of the velocity, the speed will in such a case be quite indeterminate; or, in other words, the power so applied will sustain uniformly *any velocity* that may have been previously communicated. If the friction were *really* independent of the velocity, while a moving force which exactly balances the resistance would maintain uniformly *any previously imparted motion*, a moving force which exceeded the resistance would transmit the carriages with a velocity continually accelerated, in conformity with what has been said by Mr. Barlow: but as the portion of resistance arising from the atmosphere at least, increases with the velocity, it is evident that the resistance will gradually augment till it balances the moving force, and so a uniform motion will eventually succeed. If the carriages be so acted upon as to retain a uniform velocity v along a level plane, and with such velocity and moving power they arrive at the upper end of, and proceed down, an inclined plane, the investigation given by Mr. Barlow, pages 98—100, will be strictly accurate on two suppositions, viz. 1. That the friction is independent of the velocity and inclination of the plane; 2. That the action of the moving power is not diminished by the increase of velocity. The former supposition is sanctioned by Dr. Lardner; the latter, as Mr. Barlow justly observes, if not true, will have the effect of giving the velocity and space passed over, rather in excess of the truth, and therefore the more favourable for a comparison with Dr. Lard-

ner's velocities, which are so much in excess. There can be no doubt as to the inaccuracy of the preceding formula, from which the last-mentioned velocities are calculated, as the principle from which it is derived is not founded in theory.

Yours, &c.

February 20, 1836.

W. S. B. WOOLHOUSE.

XLIX. *Note respecting the Undulatory Theory of Heat, and on the Circular Polarization of Heat by Total Reflexion.*
By JAMES D. FORBES, Esq., F.R.SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WHEN a subject so vast and so little explored as that of radiant heat is undergoing investigation, it is hardly to be expected either that experimentalists should abstain from speculation, or, on the other hand, that such speculations should be, in all cases, happily devised by their authors, or fully apprehended by men of science generally. The more immediate results of M. Melloni's researches as to the nature of heat, do not seem to me to have been very philosophically stated in such expositions of them as I have seen (at least in English); but it is not of this that I at present mean to speak. M. Melloni lately read a paper to the Academy of Sciences stating certain objections to the undulatory theory of heat, on which M. Ampère has lately published some ingenious speculative views, but which (so far as I know) has received little or no experimental support except that which I have given in investigating the laws of its polarization. I wish to point out what I conceive to be the present state of the subject, speculatively regarded, and to mention an additional discovery which I have recently made in confirmation of these views.

The arguments which M. Melloni adduces to prove that light and heat are not the same modification of matter all amount to this,—that they may be separated, often in the most irregular and capricious manner, as when the action of a coloured medium absorbs certain rays of the luminous spectrum and yet leaves unaltered the symmetry of the heating spectrum*. Such experiments, or many simpler ones, show that *heat is not light*, but nothing more. If M. Ampère really meant that the light of the solar spectrum is the same thing with the heat of the solar spectrum, nothing is easier than to refute it, and I pointed out as distinctly as words can express the fact, that light and heat are apparently separable in my

* *L'Institut* (Journal), 23rd Dec. 1835.