XI. Letter to Peter Barlow, Esq., F.R.s., &c., &c., respecting some parts of his reports addressed to the Directors and Proprietors of the London and Birmingham Railway Company

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n congeneric formulæ produce a rational equation of the m th order; thus,

$$2x + \sqrt{x^2 - a} = b? \text{ is of the } \frac{2}{2} \text{ order.}$$

$$a + \sqrt{x} + \sqrt{x - b} = 0? \quad \frac{1}{4} \dots\dots$$

$$a \sqrt[3]{a + x} - \sqrt[3]{a - x} = 0? \quad \frac{6}{9} \dots\dots$$

Are the chances of solution equal for each individual congener? I leave that question in good hands, and remain,

Yours, very affectionately,

Bath, Nov. 12, 1835.

W. G. HORNER.

XI. *Letter to Peter Barlow, Esq., F.R.S., &c., &c., respecting some parts of his Reports addressed to the Directors and Proprietors of the London and Birmingham Railway Company. By the Rev. DIONYSIUS LARDNER, LL.D., F.R.S., &c.*

DEAR SIR,

IT was not until my return to London within the last few days that I had the pleasure of receiving a copy of your Second Report addressed to the Directors of the London and Birmingham Railway Company. The previous communications which had passed led me to anticipate some collision of opinion between us, but I confess I did not expect that any difference should exist on a question of a nature so elementary as that which you have noticed in your Report. In page 87 you say:

“If (as was assumed in the Parliamentary Committee on the question of the Great Western Railway) as much power was gained in the descent as was lost in the ascent, the odds would be made all even. But that assumption is altogether erroneous both in theory and practice.”

And again, in page 91, you say, referring to your theory of the deflexion of bars:

“The only doubt, therefore, which can remain is, how far I ought to reject as inconsiderable any increase of power on the descending side. This point cannot be met experimentally, and I am therefore obliged to depend here only on demonstration. The case certainly involves no difficulty of conception to those acquainted with theoretical mechanics; but the question having been seen in a different light by a gentleman of considerable scientific eminence, I should have been glad to have exhibited the effect experimentally; but as the whole turns upon velocity, this is of course impossible.”

To those who have taken an interest in the question respect-

ing the effect of gradients, raised in my evidence before the Great Western Railway Committee, and subsequently more fully developed by me at the meeting of the British Association in Dublin, it will of course be evident that I am the person here alluded to. But since your Report must needs fall into the hands of many persons who have neither seen my evidence before Parliament, nor heard the discussion in Dublin, I think it right to explain briefly what the conclusions are at which I arrived, and which you declare to be erroneous both in theory and practice.

There are on railways certain inclined planes, forming so small an angle with the horizon, that a load placed upon them will not descend by its gravity, the friction being greater than the tendency down the plane by gravity. Let the angle of elevation of such a plane be ϵ , and let the greatest angle of elevation which is compatible with this conclusion be ϵ' . I shall call this angle, ϵ' , the angle of repose. Now let us suppose an inclined plane at the inclination ϵ , the length of which expressed in feet is L : let a load be placed upon the plane, the amount of which we shall take as the unit of weight. Let t be the ratio of the friction to the pressure peculiar to the nature of the road, the carriages, &c. which is of course a constant quantity, so long as the carriages and the road continue the same. Now the pressure upon the plane will be expressed by $\cos \epsilon$; but as ϵ must be a very small angle, we may, without sensible error, take $\cos \epsilon = 1$, and consider the whole weight as pressing upon the inclined plane. In fact, ϵ' is an angle so small that its sine does not exceed 0.004, and ϵ being still smaller, it is clear that $\cos \epsilon$ is so nearly equal to the unit that we are justified in this assumption.

To determine the tractive force which must be applied to the load to draw it up the inclined plane, it is only necessary to add together the forces necessary to overcome the friction and the gravity: now the friction is t , and the gravity $\sin \epsilon$; therefore the force which resists the motion up the plane will be $t + \sin \epsilon$. The moving power, therefore, which will keep the load moving up the plane at a uniform speed will exert a pull upon it which shall be expressed by $t + \sin \epsilon$. The unit being the weight of the load, it is clear that the total expenditure of mechanical power in drawing the load the entire length of the plane will be expressed by $L(t + \sin \epsilon)$.

Now to estimate the mechanical force necessary to draw the load at a uniform speed down the plane, we have only to consider that the force which is opposed to the drawing power is the friction t , diminished by that component of the weight of the load which is directed down the plane, and which of course

conspires with the drawing power. The effective resistance, therefore, to the drawing power will be $t \sin \epsilon$, referred to the weight of the load as the unit. The total expenditure of mechanical force, therefore, necessary to cause the required descent will be $L (t - \sin \epsilon)$. Now if we wish to determine the whole mechanical power expended in ascending and descending the plane, it is only necessary to add $L (t + \sin \epsilon)$ to $L (t - \sin \epsilon)$: the sum is $2 L t$. Now it is obvious that this would be the amount of mechanical force expended in drawing the same load backwards and forwards on the level plane of the length L .

If the angle of inclination of the plane were the angle of repose, then the tendency of the weight down the plane by its gravity would be precisely equal to the friction or $\sin \epsilon' = t$: there would in fact be no resistance to the motion down the plane, and consequently any velocity imparted to the load down the plane would be continued uniform without any drawing power to the bottom, supposing of course the plane to be free from the inequalities which would alter the amount of friction.

To ascend such a plane, on the other hand, would require a drawing force of twice the amount necessary for a level, since $t + \sin \epsilon' = 2t$; and we accordingly arrive at the same conclusion,—that in ascending and descending planes whose inclinations do not exceed ϵ' , the total expenditure of mechanical power is the same as on the level, the only difference being that on the level it is expended by one continued uniform exertion, and that on the inclinations it is greater in the ascent and less in the descent, the mean being the amount upon the level.

I explained fully, both before Parliament and at the British Association, that this reasoning would not extend to greater elevations than ϵ' , for that in these cases the power saved in the descent would be less than the excess expended in the ascent, and that, consequently, such gradients would always occasion a loss of mechanical power.

Now really this conclusion is so plain a result of first principles that I have been utterly at a loss to discover in what can originate our difference of opinion about it. It struck me, therefore, that this discrepancy must have its origin, not in the above reasoning, but in some difference of conception respecting the very foundations of mechanical science. It occurred to me, therefore, to look over your Reports, to see whether the same difference as to first principles would lead you to conclusions upon other points different from those at which I should have arrived: in this I was not disappointed, for I found in another case, in which the force of gravity is considered, and

indeed one which is the extreme case of an inclined plane, viz. that of a perpendicular fall, you have arrived at a conclusion which certainly is totally at variance with the views which I have been accustomed to take of the theory of forces. In your First Report to the Directors of the London and Birmingham Railway Company, page 90, in speaking of the effect produced by the wheels of wagons passing over the joints of rails where one has sunk below the level of the other, so as to form a sort of slip, you say :

“ It has perhaps never occurred to such persons that a difference of level at a joint-chair will, when the carriage is moving from the higher to the lower level at its greatest speed, cause the wheel to pass the distance of a foot without pressing on the rail, and consequently throwing the whole weight, which ought to be borne equally by the two rails, wholly upon one. Yet this is a fact resting upon a natural law, and cannot be otherwise. To fall $\frac{1}{10}$ th of an inch by the action of gravity requires $\frac{1}{4}$ th part of a second, and in that time the carriage will have advanced a foot, and consequently in that space the whole weight has been borne by one rail only.”

I freely confess that I am one of the persons you allude to, to whom such an idea never would have occurred. I am entirely ignorant of the natural law to which you allude, but I am not ignorant of a natural law which is altogether incompatible with your conclusion ; and my conviction that the whole weight cannot press on the remaining wheel is quite as clear and strong as yours is that it will so press. It is quite true that the force of gravity will cause a body to fall freely $\frac{1}{10}$ th of an inch in $\frac{1}{4}$ th part of a second, but when the force of gravity is thus employed it cannot at the same time cause the whole weight of the same body to press upon a fixed point. The fact is, that when the wheel passes from the higher to the lower level, the centre of gravity being unsupported falls, and the only pressure exerted upon the remaining wheel depends on the moment of inertia of the load in receiving an incipient angular motion, and it is evident that this pressure must be extremely slight ; but, whatever be its amount, it is totally different both as to effect and cause from that which you allude to. If you consider that during the moment of a perpendicular fall, in the case of one rail being below the level of the other, the weight while it falls still presses with its whole force upon the rails, your view even of the most elementary principles of this part of mechanics is so essentially different from mine, that the wonder is, not that we should differ in one instance, but that we should agree in any.

Referring again to your second Report, where you have

noticed my inferences respecting gradients, I find that you say that,

“As the question wholly turns upon velocity, it is of course impossible to exhibit the effect experimentally.”

Now although I do not perceive this to be at all a matter of course, but on the contrary have found it very easy to reduce questions depending on velocity to experiment, yet I beg to observe that the present question does not depend either wholly or at all upon velocity. Whatever be the speed of the load upon the inclined plane, provided only it be maintained uniform, my theory of gradients (if it deserve to be so called) will still hold good. It will take the same expenditure of mechanical force to move a load on such inclined planes as I have described, and the mean between the ascending and descending forces will be the force along a level plane. You must surely be so well acquainted with the laws of friction that it is needless for me to remind you that that resistance is altogether independent of the velocity. And I would also beg to observe that the case is one totally distinct from the consideration of accelerating forces. In page 91 you say:

“This point cannot be met experimentally, and I am therefore obliged here to depend only on demonstration. The case certainly involves no difficulty of conception to those acquainted with theoretical mechanics, &c.”

I admit that it does not; but I apprehend the conception which those acquainted with theoretical mechanics form of it will be altogether different from that at which you appear to have arrived, and I therefore regret that you seem to have forgotten your expressed intention of giving a demonstration of your own peculiar view of the matter. In the next page (92) you mention the intention as one which you *had*, but seem to have immediately abandoned it.

It will be very gratifying to me, and I am sure it will be useful to all who are practically engaged in those extensive enterprises for the formation of lines of communication through the country, if you will show how these views of mine are at variance with the established principles of mechanics. Although I am not aware that any one has hitherto pointed out the property which I have explained in reference to inclined planes of less inclination than the angle of repose, yet, so far as I am informed, there is no difference of opinion whatever as to the legitimacy of the method of estimating the tractive force both in ascending and descending these planes. The same formulæ that I have used, viz. $L(t \pm \sin \epsilon)$, have been in substance universally adopted in estimating the mechanical force necessary to work railroads. You will find that many

eminent engineers, although they have not thrown the principle into the language of analysis, have nevertheless used it arithmetically; and indeed I have never before heard any doubt expressed about it.

During the last autumn I have been engaged in an extensive course of experiments on rail-roads in different parts of the kingdom, with a view to determine with greater precision than has been hitherto attained, the values of the different constant quantities which enter into their theory. The results of all these experiments are in the most perfect accordance with the principle you have called in question.

I remain, dear Sir, yours very truly,

DION. LARDNER.

36, Cambridge Terrace, Edgeware Road,
December 14, 1835.

XII. *Remarks on a supposed new Law of Magnetic Action. By the Rev. WILLIAM RITCHIE, LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution of Great Britain and in the University of London.**

IN the last Number of the London and Edinburgh Philosophical Magazine†, Mr. Fox has endeavoured to show that the mutual attraction of two magnets does not follow the law formerly adopted by all philosophers, viz. the law of the inverse *square* of the distance; but the law of the simple inverse of the distance. This law he deduces from experiments on the attraction of the opposite ends or poles of magnets placed at very small distances from each other. Thus, for example, when the ends of the magnets are at the distance of $\frac{1}{2000}$ of an inch, he found the effect to be only *one half* of what it was when they were in contact; when removed to the distance of $\frac{1}{1000}$ of an inch the effect was *one half of one half*, or *one fourth*; when separated by a distance of $\frac{1}{500}$ of an inch, the force was only *one half of one fourth*, or *one eighth*, &c.; which numbers are to each other in the inverse ratio of the distances

I *admit* the truth of the experiments, but differ from Mr. Fox in the conclusion he has drawn from them. To show that the deduction is unfounded, we must first describe what is meant by the *pole* of a magnet, and its *position* with regard to the extremity of the magnet. The pole of a magnet is the *centre of parallel forces* of all the attractions and repulsions of the elementary magnets of which it is composed. Now the position of this *centre* will obviously depend on the *form* of

* Communicated by the Author.

† Vol. vii. p. 439.