A Collection of Documents by and about

Richard Badnall

of Ashenhurst,
Silk Manufacturer, Railway Engineer, Poet,
Politician and Thinker
1797-1839

Volume 1
Railway Engineer


QUOD ERO SPERO
Richard Badnall, Junr.

Contents

Richard Badnall Junior, of Ashenhurst, Silk Manufacturer,
Railway Engineer, Poet, Politician and Thinker
1797-1839

THE UNDULATING RAILWAY

by Alan Bednall

The 1830s was, for all practical purposes, the first decade of rail travel and a period of rapid growth in the spread of this form of transport nationally. It was the period too, which established George Stephenson and his son Robert as the foremost railway engineers not only in Great Britain but also in the World. Their genius and perseverence enabled them to overcome the very considerable doubts which were expressed concerning the viability of railways.

The success of the Liverpool to Manchester line created a considerable enthusiasm for new railway projects all of which required the services of engineers, not only to survey and plan the lines but also to take charge of the construction and commissioning of the line when Parliamentary approval had been obtained and the necessary finance found. The Stephensons, Brunel and other established engineers were thus in great demand, but they alone could not carry out all the projects and the high fees and salaries offered attracted new " railway engineers".

Samuel Smiles describes how these engineers became leaders of the battle when two or more rival lines were planned between the same points. Such battles were trials of individual ambition as well as professional skill and considerable personal feeling was involved. According to Smiles "many new men laboured to mature and bring out railway projects more striking and original than anything heretofore proposed" and amongst this group of "fast engineers" he identified Isambard Kingdom Brunel, Dr Lardner and a certain "Mr Badnell".

Both Lardner and Badnall were, according to Smiles, proponents of railways constructed with "rising and falling gradients" with Mr Badnall claiming that "an undulating railway was much better than a level one for the purposes of working".

Richard Badnall, Railway Engineer.

The "Mr Badnell" referred to by Smiles was Richard Badnall of Cotton Hall in Staffordshire . Richard Badnall, a Staffordshire man by birth, was the eldest son of Richard Badnall of Highfield, a Leek, silk manufacturer, banker and dyer.

Richard Junior, was a well educated man, a poet, author and inventor who played the flute and whose romantic disposition is evident in his writings and in his choice of home. His book *The Legend of St.Kilda*, *Zelinda: a Persian tale*, and his poem *The Pirate* illustrate his Byronic, romantic, view of the past and the Staffordshire homes he chose for himself - Ashenhurst, Woodseaves and Cotton Hall - give further clues to his nature.

His sister Mary Elizabeth Cruso, described him on one occasion as "as usual full of schemes " and of "talking up and down the town (Leek) of his plans for enriching himself and his family". In 1837, after hearing that her brother proposed to stand as a Parliamentary candidate for Newcastle-under-Lyme, she described him as "strange in his proceedings".

He was also, for a time, a silk manufacturer and dyer and perhaps would not have ventured into railway engineering had not his first partnership with his brother in law Henry Cruso and Francis Gybbon Spilsbury come to an early and abrupt end in 1826 with the bankruptcy of the partners and the parents of both Badnall and Spilsbury.

Following the announcement of his bankruptcy, Richard Badnall Junior had tried strenuously, both in this country and in France, to remedy matters, pay his creditors and sustain his wife and their young family. The contents of Ashenhurst were sold off and his wife's uncles, Samuel and William Philipps, took over the considerable mortgage on the property. For several years Badnall's life was extremely unsettled and between 1827 and 1832 he lived at six different addresses in London & Liverpool before going to live with his father in Liverpool. For much of this time he appears not to have had any settled occupation, except for a short period when he acted as a silk broker and merchant.

In 1832 his petition as an insolvent debtor was heard at Lancaster Court and later that year a patent application revealed that, although he may have been without an occupation, his mind was as active as ever. It was this patent that formed the chief item of discussion between Richard Badnall and Robert Stephenson, younger brother of George Stephenson, over dinner at the Manchester home of JL Gardener, and subsequently led to the formation of the Stephenson & Badnall partnership to exploit the patent's potential.

Richard Badnall Junior was born in Leek at the turn of the 19th century. He was the son of one of the town's most successful silkmen, Richard Badnall of Highfield, Leek whose family firm had been established in the town for some 75 years.

Richard Junior eventually took over the family firm in 1824 when his father decided to concentrate on his young family, his duties as a JP and his farm. Richard Junior entered into partnership with his brother-in-law Henry Cruso and Francis Gybben Spilsbury and began to develop the firm to exploit their various patents relating to tanning and the manufacture of silks. Unfortunately, they entered into the euphoria of the times and within a short time they were bankrupt and their failure subsequently caused the bankruptcy of Richard Badnall, senior. Following a period in which he kept moving from place to place in a vain attempt to restore the family fortunes, he lived for a while on the Isle of Man and whilst there developed his theories of the "Undulating Railway". Though his theory proved in the end to have been flawed he was convinced of the value of his railway design, primarily because of the practical tests which he had carried out using, initially, clockwork models but subsequently, the real trains of the Manchester & Liverpool Railway. There is a lengthy correspondence in the Mechanics Magazine in which he vigorously defended his ideas. In 1833 he entered into partnership with Robert Stephenson, senior, of Pendleton Colliery, to exploit the undulating railway patent.

Richard Badnall was politically a Whig, and had a somewhat romantic nature. He wrote poetry and several books, registered several patents for improvements in silk machinery and stood as Parliamentary candidate for Newcastle-under-Lyme in the elections of 1837. He appears to have had a tendency to enjoy the fruits of success before they had been harvested. For example, on first entering into partnership with Cruso and Spilsbury, he took on a mortgage of £12,000 in order to acquire the Ashenhurst estate near Leek, and following his bankruptcy continued to try to live in a style which his income couldn't maintain. Unfortunately too, he suffered all his life from gout, and the combination of this and the stress under which he lived from 1827 resulted in his death in 1842, at the relatively early age of 42.

A

TREATISE

ON

RAILWAY IMPROVEMENTS,

EXPLANATORY OF THE CHIEF

DIFFICULTIES AND INCONVENIENCES

WHICH AT PRESENT ATTEND THE GENERAL ADOPTION OF

RAILWAYS,

AND

THE MEANS BY WHICH THESE OBJECTIONS MAY BE OVERCOME;

AS PROVED BY A SERIES OF

INTERESTING EXPERIMENTS.

TO WHICH ARE ADDED,

VARIOUS REMARKS ON THE OPERATION AND EFFECT

OF

LOCOMOTIVE POWER.

BY RICHARD BADNALL, ESQ.

LONDON:

PUBLISHED BY SHERWOOD, GILBERT AND PIPER,

PATERNOSTER-ROW.

1833.

A

TREATISE

ON

RAILWAY IMPROVEMENTS,

&c. &c.

E. H. BLAGDON, PRINTER,
(Successor to B. M'Millan),
BOW-STREET, COVENT-GARDEN.

TO

E. J. LITTLETON, Esq. M. P.

TEDDESLEY PARK, STAFFORDSHIRE.

MY DEAR SIR,

As a very humble token of my esteem for your high Character and Public Worth, and for the valuable Services which, for a period of Twenty Years, you have rendered, as its Independent Representative in Parliament, my Native County, permit me to dedicate to you the following pages.

I have the Honor to be,

MY DEAR SIR,

Very faithfully yours,

RICHARD BADNALL.

Manchester,
March 1, 1833.

PREFACE.

THE only apology which I think it necessary to offer, in submitting the following pages to public judgment, is to those men of science who may deem that I have entered too minutely into the explanation of some mechanical laws, which explanation must appear to them entirely useless and unnecessary: but, conceiving it probable, at this particular period, when RAILWAYS form a subject of universal interest and conversation, that this short Treatise may fall into the hands of many altogether unacquainted with mechanics, I hope I shall be pardoned for having extended my observations, especially on the laws of friction and gravity, farther than I should otherwise have done. My anxiety is, to be understood by all; and my earnest desire, that public benefit may be the result.

Manchester,
March 1, 1833.

A TREATISE

ON

RAILWAY IMPROVEMENTS,

&c. &c.

INTRODUCTORY REMARKS.

THE chief object of the following pages, is to submit to public notice the particulars of what I deem a most important improvement in the construction or formation of Railways.

In the accomplishment of this duty I shall endeavour to be as concise as possible, seeking not fame, on such an occasion, by the extent or brilliancy of language, but by the innate and material value of my subject. How far I am justified in appreciating that value highly, time and experience will testify. At the present moment, after the most mature reflection,—after regarding the nature of the improvement to which I allude in all its bearings, and after numerous and impartial experiments, I cannot help feeling a strong presentiment that it will prove one of those important sources of public wealth and advantage, which are decreed at intervals to swell the ever-growing current of civilization.

It is, I believe, universally acknowledged, that in all countries, the rise of prosperity mainly depends upon the convenience of conveyance from place to place. Let those who dispute this doctrine, direct their attention to the resources of any nation with whose condition they are acquainted; let them refer to those historical records which treat of ages gone, and they will find my assertion indisputably verified.

Science, Religion, Morality, Industry, may advance among any particular body, or in any particular district, and according to their joint progress, the comfort and prosperity of that body or district will be promoted; but as the advantages of local situation vary, and as the surplus produce of labour above consumption increases, the necessity of intercourse between man and man proportionately increases; and upon the convenience of that intercourse, whether between nation and nation, district and district, or town and town, in a chief measure depend the value of produce, the general increase of wealth, and, consequently, the advancement of happiness and civilization.

If such be true, no nation can promote its real interests more effectually, than by encouraging in every possible way the establishment of good roads, and rapid and convenient modes of travelling; for according to such convenience will be the equality in price, and the abundance of the supply of produce; the *real* value of landed and other property, and, as before stated, the increase of wealth and comfort among all orders of society.

Entertaining such opinions, it is not to be wondered at that I have for some time watched with anxiety and delight, the successful establishment of the Liverpool and Manchester Railway, and that I look forward with hope that I may live to see the whole of my country interspersed with the same description of roads, every year witnessing improvements, and every community exerting its combined talent and means to promote them.

Thus feeling, and being passionately attached to mechanical pursuits, it was impossible that I could travel along the road to which I have alluded, without endeavouring to make myself thoroughly acquainted with the nature and operations of locomotive power, and drawing my own conclusions as to the perfections or imperfections which attended the present modes of conveyance by such power.

These considerations naturally led to the wish of endeavouring to overcome any difficulty which I believed to exist; and the result of my reasonings upon these subjects, will, I hope, be clearly laid down in the succeeding pages.

ON THE

ADVANTAGES OF RAILWAY CONVEYANCE OVER ANY OTHER MODE, &c.

THE leading advantage which the public can expect to derive by this excellent mode of conveyance, is that of SPEED, either as it regards passengers, cattle, or merchandise.

The limit to speed which has hitherto existed on common roads, is attributable to two causes,—inequality of surface, and circumscribed power. From these two causes, many acknowledged inconveniences, though until now comparatively unfelt, have originated.

About the year 1795, it was a twelve hours' journey from Liverpool to Manchester; a man, therefore, having business to transact in either town, was compelled to sacrifice twenty-four hours of valuable time in travelling.

In the year 1830, the journey from Liverpool to Manchester was, on the average, about four hours; the sacrifice of time was consequently reduced at every journey 1-3rd.

The time it occupied to convey merchandise at these respective periods from one town to the other, was proportionably inconvenient; inasmuch as, before the establishment of canals, the expense and delay by land-carriage was severely felt; and the advantage derived from their establishment was, considering the rates of freight, and the convenience of conveying large quantities of merchandise, fully equal to any advantage derived by passengers from the increased speed of coach-conveyance.

The improvements which have taken place in the speed of conveying goods and passengers by Railways, as at present established upon one line of road, have reduced, since 1830, the loss of time occupied in travelling 5-8ths, and in the conveyance of merchandise 21-24ths; and carrying the same principle into general adoption, how enormous would be the national advantage!

Passengers, merchandise, cattle, &c. can now be conveyed with perfect ease from Liverpool to Manchester, or the contrary way, in one hour and a half, a distance of thirty-one miles.

A person may leave Liverpool at 7 o'clock in the morning, he may devote nine hours to business during the day at Manchester, and he may return to his own house by 7 o'clock the same evening. By similar means, the bale of cotton which leaves Liverpool in the morning, may be in progress of rapid manufacture before the close of evening; and thus the tide of wealth rolls on, swelled by every improvement in science, and essentially promotive of public good.

Momentous, however, as are the benefits which we derive from this speed of conveyance, as an immense assistant to national industry, it is attended with other advantages of great, if not equal importance: it is by such means that we equalize the price of provisions, and of all productions, rendering the produce of one town or district subservient to the wants of others, reducing the expense of travelling, and, as *all wealth* must continually be invested in land or building, thus increasing the real and absolute value of all soil*.

* I trust I may be pardoned for introducing, in the form of a Note, a sentiment, bearing upon my present subject, which I expressed in my "Letter to the Lords and Com-" mons, on the Commercial and Agricultural Condition of "Great Britain," published in 1830: "All the accumulated "wealth of the world must gradually be invested in land, "and every increase of population, cultivation, building, "and civilization, must necessarily enhance the *bonâ-fide* "value of the soil of all countries; as long, therefore, as "any country can maintain her rights and independence, "and is governed by wise and equitable laws, so long "will her land be the best and only security for wealth; "for wealth, like expanded steam, which by ingenuity "is rendered the most powerful engine of production, has "its origin in the self-productive power of land, until, "expanded by labour and ingenuity into immeasurable "space, it falls again condensed upon its native earth, "which is its source, its strength, and its reality."

If this be true, how blind are men to their own interests, who oppose any species of improvement!

Who then can dispute the advantages derivable from every improvement in conveyance, and from the rapid intercourse between place and place, and between man and man?

To deny that these advantages can best be attained by the adoption of Railways, appears to me irrational: the smoothness of surface opens every facility to the acquirement of convenient speed, by the reduction of friction; and the power of steam possesses the most undoubted advantage over every other known assistant power in promoting that speed; and in proportion to the extent of such power employed, in a great measure *ought* to depend the amount of load conveyed.

These advantages have been distinctly and indisputably proved by the result of two years' experience on the Liverpool and Manchester Railway; for, despite of the original expense of this wonderful undertaking, the heavy cost of the numerous locomotive engines at work upon it, the great expense of the servants and labourers employed upon it, and the many necessary attendant outlays, it has, while conferring the greatest possible benefit upon the public, by conveying both passengers and merchandise so rapidly, and at such reduced charges, hitherto produced, after the payment of interest, an ample equivalent to the enlightened, patriotic, and high-spirited individuals who have conferred this blessing on their country.

I cannot leave this subject, without offering my tribute of respect and praise to Mr. G. Stephenson, who has so very ably conducted this undertaking; he has proved himself, in perseverance and in talent, well deserving of the regard of his country and mankind. His fame can never be tarnished by the improvements which others may suggest; and to his candour I have little hesitation in referring the following opinions:

On the Objections which naturally occur to the immediate general Establishment of Railways, and Imperfections in the present Mode of Railway Conveyance.

Among the most important objections to the general establishment of Railways at the present period, and to their successful adoption throughout Great Britain, are the following:

1. The great inconvenience which persons must, for a while, sustain by their accustomed modes of conveyance being withdrawn from the roads to which their property is contiguous.—The great expense of excavations and levelling, of building bridges, tunnels, &c. and of purchasing property;—and, The opposition which landed proprietors are, in general, disposed to offer to new roads being

cut through their estates, and canal proprietors to a competition so injurious to their interests.

2. The difficulty of ascending inclined planes.

3. The great weight of locomotive engines, which are consequently so destructive of the rails or trams, and their rapid wear and tear.—The limit which exists to the full employment of great steam power, and, consequently, of conveying in one train so great a number of passengers, or weight of merchandise, as could otherwise be carried.

1.—*The great Inconvenience which Persons must, for a while, sustain, by their accustomed Modes of Conveyance being withdrawn from the Roads to which their Property is contiguous, &c. &c. &c.*

So great have been the improvements in the common turnpike-roads throughout Great Britain within the last fifty years, that, taking them in general, it would be almost impossible to select more convenient lines of road from town to town. It is true, that many alterations are essential for the saving of distance, and to avoid mountainous ground; but we find these desirable alterations continually going on, as the various Trusts can command, or, otherwise, the requisite funds.

Now, when we seriously consider the enormous expenditure which has been incurred to establish these roads,—the numerous buildings and farms which are contiguous to them,—the well-constructed bridges by which we cross every stream and river throughout the empire,—how conveniently they stretch through every town and village of the slightest note,—and how much the value of property in those towns and villages depends upon direct and convenient communication with other districts,—we cannot help confessing, that the establishment of new lines of roads, in different directions, must, for a time, have the effect of depreciating the value of immense property, and producing excessive inconvenience to many individuals, and to many estates.

It is true, that the present roads may still exist, but where will be the stage-coaches and stage-waggons?—where the necessity of expending the same money in repairs?—what will become of the numerous taverns already erected for the convenience of travellers?—and how serious the inconvenience to the inhabitants of country residences, and to farmers, by these alterations!

Again, when we consider the great outlay that has been made in the establishment of canals for the conveyance of merchandise, we are equally struck with the loss of property which the canal proprietors must suffer, by the construction of new lines of road for Railway conveyance; and the recent fate of the London and Birmingham Railway Bill, in the House of Lords, is rather staggering evidence of the disinclination of some landed proprietors to disfigure (in their opinion) even a small portion of their property for the public good.

I do not thus argue, with a view of advocating the undue protection of private, at the expense of public interests, but to prove how natural and how extensive is the opposition which for many years may be reasonably expected to the general adoption of new lines of road, however great the public convenience.

The preceding may therefore be fairly stated as strong objections to the *immediate general* establishment of Railways; and to these may be added, with truth, the enormous first cost of purchasing property, and of levelling Railroads, throughout a country like England; an opinion of which may be formed, by considering that the Liverpool and Manchester line did not cost less than one million sterling; and if thirty-one miles cost one million, how great would be the expense, upon the most economical plan, of interspersing Great Britain with such lines of road!

To these objections, therefore, my attention has been particularly directed. How desirable would it be to remove such objections,—to reconcile as much as possible the landholder and the canal proprietor,—to free the public who now reside in particular districts, from that inconvenience and loss which they must sustain, from the depreciation of their property, by the removal of their accustomed means of conveyance,—to give to the towns and villages through which a turnpike-road now passes, the same facilities which they at present possess,—and how desirable to save, if possible, the immense outlay which must be made for the purchase of land for new lines of road!

It is quite obvious, that if Great Britain could command sufficient funds,—if the majority of landed and canal proprietors were favorable to such a measure,—and if Parliament would sanction the public wish, the construction of new lines of road would not only give employment to thousands who, *under our present unwise and restricted commercial laws, require it*, but be of undoubted public advantage. But I am inclined to think, if there prove to be no *practical* objections, that more general satisfaction would be given, much valuable time would be saved, and very great, though not equal advantage would result, by the establishment of Railways upon, or contiguous to, the present turnpike-roads, or by the conversion of canals to Railways. These are important considerations, and well worthy of attention. The objections which arise are, nevertheless, numerous, especially in reference to turnpike-roads; I think them, however, by no means insuperable, nor do I conceive the difficulty of ascending the *locks*, nor the indirect course of canals, any serious obstacles to their conversion to Railways.

These are matters, however, for public judgment. If men would consent to make private sacrifices for public good, *entirely new lines of road are best;* but if not, we had far better take all the immediate advantage possible of every improvement in science, than allow those improvements to slumber, even for a single year.

Men are too apt to form their opinions of general welfare by a comparison with self-interest. Nothing can be so egregiously erroneous,—nothing more fatal to the prosperity of a state; and although I ought, perhaps, to apologize for mingling a political sentiment with scientific matter, yet I cannot withhold an opinion, which History and modern example fully establish, that Governments, like individuals, seldom commit an error in judgment where the immediate advantage of the few is sacrificed to the prospective advantage of the many.

2.—*The Difficulty of Ascending Inclined Planes by Locomotive Power.*

This difficulty is amply proved by the Railways already established, and may be witnessed either upon the Liverpool and Manchester, or upon the Bolton lines. The cause is obvious. Whenever the gravity of a body, or its natural tendency to descend, is greater than the resistance produced by the adhesion, or friction, of the wheels upon the surface of the plane, an advance of the carriage is impossible; consequently, upon all inclined planes, the power as well as the speed of ascent is altogether governed by the amount of friction; which friction may be termed the fulcrum upon which the power of steam can be more or less effective; from this has arisen the necessity, which has hitherto existed, of levelling land for Railroads, so as to avoid inclined planes. It is true, that either stationary engines, or locomotive engines with cog-wheels, would enable carriages to be drawn up inclined planes, but such modes would be attended with serious, if not insuperable inconveniences; and hence the great difficulty, hitherto, of conveying merchandise or passengers by steam upon common turnpike-roads*, even if rails, or trams, were laid down thereon: to this second objection, therefore, my attention was particularly directed.

* I am aware that steam-carriages have been constructed to ascend hills with heavy loads on common turnpike-roads; but when I consider the *amount of friction* necessary to be overcome, I cannot believe that steam power can ever be rendered *advantageously* effective on such roads.

3.—*The great Weight of Locomotive Engines, which are consequently so destructive to the Rails, or Tram-plates, &c. &c.*

I had not long witnessed the experiments tried by Mr. Stephenson and others upon the Liverpool and Manchester line of road, before I drew the conclusion, that the load conveyed, and the speed of conveyance, did not so much depend upon the *extra power of steam*, as upon the *extra weight of the engine;* in other words, that in proportion to the weight of the apparatus, is the amount of pressure upon a common Railroad, and, consequently, the amount of power absolutely at work. Indeed, I soon formed the opinion, that if 30-horse power of steam were applied on a common Railway, with an extremely light apparatus, it would not convey so great a weight of goods, or so rapidly, as 10-horse power of steam with a heavy apparatus; and an observation is scarcely necessary to prove the disadvantages attendant upon heavy engines, where light ones can be rendered effective.

Referring to the following figure, let A B, be a common Tramroad, upon a level,—C,

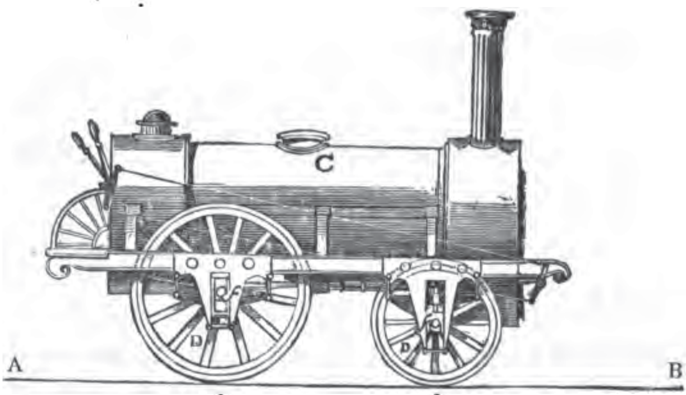

Fig. 1.

the engine,—D D, the wheels,—*e e*, the points of friction upon the plane,—*f f*, the points of friction upon the axles.

Now the only power of steam which the engine C, can effectually employ, is in proportion to the amount of friction at the points *e e* and *f f*; and the amount of this friction is in proportion to the weight of the engine C; governed of course by the diameter of the wheels and axles.

Now, supposing the amount of this friction be just sufficient to render 10-horse power of steam effective in dragging 50 tons;—and supposing 10-horse power of steam just sufficient to move the wheels, D D, along the plane, without slipping, with the load attached;—if we were to add 30-horse power of steam in addition, without increasing the weight of the engine, the machine would not be more effective; for if we added 20 tons to the weight dragged, the friction or adhesion at *e e* would not be increased by any power of steam, and, consequently, the wheels would simply turn upon their axis, and the whole train would remain stationary:—that is, the comparative quantity of goods or passengers conveyed upon a Railroad, as at present constructed, by different engines, does not depend upon the extent of steam power we have at disposal, but upon the *weight* of the engines, which altogether governs the amount of effective power.

This compulsion to employ such extremely heavy engines, is, it cannot be disputed, a serious subject of scientific consideration. That such engines have been found necessary by Mr. Stephenson, is obvious; for without them, he could not convey the loads of merchandise and the number of passengers in one train, which he now does. The result is obvious, that the immense pressure produced upon the rails, naturally leads to their more rapid wear; and in many parts of the Liverpool and Manchester line, this effect is even now visible. Nor is the injury confined to the rails; for the wear of the engine-wheels is proportionately great; and, by comparing them with the wheels of the lighter vehicles, the truth of these objections will be evident. I therefore am strongly impressed with the belief, that one of the greatest desiderata in

Railway improvements, *is that of reducing, as much as possible, the weight of the locomotive engines.*

From the preceeding observations it will be obvious, that as the effect of locomotive power, as also proved by experience, depends altogether upon the amount of friction produced by the pressure of the periphery of the wheels upon the rails, if a given power of steam be employed, and if such power be just sufficient to turn the wheels without slipping, with a maximum load attached, any increase in that power is altogether useless; indeed, upon the Railways already established, the full power of steam is seldom or ever exercised; and for the evident reason, that the friction, or fulcrum, is not sufficient to allow of its full activity; for, as before observed, whenever the load to be conveyed is just such as to permit of the steam power moving the wheels of the engine along the plane without slipping, no increase whatever of that power, whether 50, 60, or 100-horse, could enable the engine to draw the load.

From this reasoning I was led to form the opinion, that locomotive steam power never could be brought to any thing like effectual operation upon a Railway, until greater activity could be given to its exercise, and until we depended more than we now do upon the *extent* of that power, for the amount of weight we should be capable of conveying.

The speed of a steam-vessel depends principally upon the *extent of power* employed to overcome friction;—the quantity of machinery moved in any manufactory, or the quantity of ore or water drawn from any mine, depend upon the same *extent of power*;—but upon Railways, as at present constructed, the speed of the train, and the weight conveyed, depend more upon the *extent of the weight of the engine*, than upon the *extent of steam power*.

The weight, too, of a locomotive engine, as now used, is one great cause of its heavy first cost. The body of the carriage being so ponderous, it becomes essential that all the wheels, the axles, and every other part of the machinery, should be proportionately so; and this great weight leads, as before observed, to their rapid wear; so much so, that few of these engines last beyond 12 to 18 months; they are frequently out of repair, and the original cost of each is from £600 to £800. Now, when we consider the length of time which the engine in a vessel will endure*, and how many years a stationary engine will work, we are at once compelled to infer, that there is some great error in the present mode of adapting locomotive power to Railways. The whole riddle is, that *fulcrum*, or *resistance*, *is wanted*; and until this can be given to the fuller activity of steam, neither light nor cheap

* It is quite true, as Dr. Lardner observes, that the immense distance which locomotive engines travel, ought to be taken into consideration when we speak of their short duration; but this, in my opinion, is not a sufficient justification of the evil I speak of.

engines can be employed: it is, in a word, the want of this resistance that, upon Railways, as at present constructed, prevents the ascent of inclined planes, and leads to the more rapid wear and tear of the engines, which are constantly overstrained, and, in my opinion, far too ponderous, and, however highly we may already appreciate them, are by no means perfect in their operations.

How to remove these evils, at least in part, I shall endeavour to explain in a following Section. Before closing this, however, I am anxious to point out another objection which occurs to me in the present adaptation of carriages to Railway conveyance; I allude to the unpleasant jolting which, at every start of the carriage, is felt by the passengers.

Although this is a very minor evil, it is, nevertheless, important that it should be remedied. The plan I propose is, to attach the carriages together by strong rods, moving on ball and socket-joints.—(See *fig*. 2).

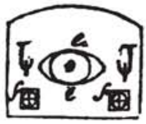

Fig. 2.

A B, is a rod by which two carriages are to be joined together, to the end of which are the balls, *c c;*—*e e*, are the sockets which open to admit the balls, and which are firmly attached to each carriage by the screws, *f f*, and are so made as to admit of the rod, A B, being moved with ease in any direction.

By this means, it is evident that each carriage will accommodate itself to the motion of the others, and that the chains now employed being thus dispensed with, the jolt which is now experienced at the starting of the carriages, and which is owing to their being drawn together when stopping, by their momentum, will be no longer experienced.

ON THE NATURE OF THE IMPROVEMENT TO WHICH THESE PAGES REFER; THE PRINCIPLES ON WHICH IT IS FOUNDED, AND THE ADVANTAGES LIKELY TO ACCRUE THEREFROM.

The improvement in the formation or construction of Railways, to which these pages principally refer, is the substitution of a *curved*, or *undulating*, or, what I denominate, a *serpentine Railway*, for the horizontal Railway now in use.

The improvement occurred to me on the 7th June, 1832. The impressions upon my mind, before the trial of any experiments, were, that by an undulating Railway, a greater resistance would be opposed to the power of steam, or any other locomotive power, than upon a level Railway; that much would be gained by the power of gravity, multiplied by active power, down a descent; and that, consequently, a locomotive engine of any given power, would travel at a greater speed, or drag a greater weight, than upon a horizontal Railway.

I was also of opinion, that the increased resistance, or fulcrum, offered by the descending part of each curve, and the advantage gained by the power of gravity, multiplied by active power, would be sufficiently great to render locomotive engines more effective than they have at present proved to be, up inclined planes. How far these opinions are warranted by experiments, the following remarks will shew; before I proceed, however, to describe them, I consider it necessary to make some observations on the subjects of *friction* and *gravity*, which are essential to a clear understanding of my discussion.

If one plane surface, being a dead weight, be dragged over another plane surface, the amount of friction will be in exact proportion to the weight of the body dragged; but if such body be placed upon four wheels, and rolled along a plane surface, the attrition produced by the dead weight is principally removed, and the only friction is that produced by the rolling of the peripheries of the wheels along the surface of the plane, and the friction, or attrition, produced by the revolution of the axles; the total amount of this friction being altogether determined by the weight of the vehicle, the smoothness of the plane upon which the body moves, and the diameters of the wheels and axles.

It is difficult to establish any decided data as to the proper comparative amount of rolling friction and axle friction, as they vary in almost every carriage or engine; but, taking the experiments of Mr. Nicholas Wood as a guide, they may, jointly, be stated to be, in Mr. Stephenson's earlier locomotive engines, about the 240th of the entire weight, and their proportions as 6 to 19;—the rolling friction being 6, the friction upon the axle being 19.

Adopting also the formula of Mr. Wood,

the power required to move a locomotive carriage on a Railway, will be as follows:

Let W, be the weight of the carriage.
W′, that part resting upon the axles.
f, = to the rolling friction on the plane.
g, = to the friction upon the axles, in part of the weight, $W' = f'\frac{d}{D.}$
D, = diameter of the wheels.
d, = diameter of the axles.
P, the power required to move the vehicle.

Then, $$P = \frac{W}{f} + \frac{W'}{f' + \frac{d}{D.}}$$

The power, P, would be equally effective, and in the same proportion, were a stationary engine employed in moving a whole train of carriages; but when locomotive power is employed in one vehicle to drag numerous vehicles, it will be evident, that the extent of such power will depend upon the weight of the vehicle on which it immediately operates, or, what is tantamount, upon the absolute amount of friction between the periphery of the wheels of such particular engine, and the plane on which it travels.

Remarking upon the preceeding formula, Mr. Wood observes, "We have no decided "experiments to prove the value of f, with "different-sized wheels; but as large wheels "more easily surmount obstacles than wheels "of small diameter, we may *suppose* the "former will always be preferable."

Now, in my opinion, there is no mechanical principle more established, than that the greater the diameter of a cylinder of a given weight, rolling on a plane, the less is the amount of friction. For instance, if a carriage weighing 1 ton, move on 4 wheels of 4 feet diameter each, whose axles are of given dimensions, and the same carriage, with the same axles*, move on wheels of 2 feet diameter each, the amount of pressure being the same, the rolling friction of the 2-feet wheels would exceed the rolling friction of the 4-feet, in the proportion of 4 to 2.

* Of course, axles of the *same* dimensions would not, in this case, be necessary: it is for the purpose of clearer elucidation that I have considered them, in this instance, alike.

Again, I look upon the amount of friction, or attrition, produced by the moving of a wheel upon its axis, or of an axle within a stationary cylinder, to be clearly demonstrable, the diameter of the axle and the diameter of the cylinder, as well as the amount of pressure, being ascertained. In this instance, the amount of friction, or attrition, does not altogether depend upon the amount of pressure, but on the leverage gained by the less or greater diameter of the wheel.—Thus:

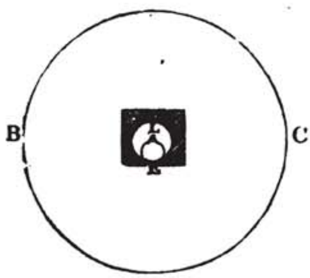

Fig. 3.

A, is the axle of the wheel, B C, revolving in the hollow cylinder, D E, the point of friction, or attrition, being at E, with a leverage, E L.

Now, let P be the power employed to move the wheels round, and it is evident, that the wheel, when revolving, acts with a leverage, B A, in overcoming the friction, F.—Thus:

$$F : P :: B A : E L$$

$$F = P \frac{B A}{E L};$$

shewing, that the point of attrition at E, is equal to the power applied, P, multiplied by the radius of the wheel, B A, and divided by the radius of cylinder, D E, in which the axle revolves.

Having thus, I conceive, sufficiently explained, for our present purpose, the nature of rolling friction, and of the attrition produced by the pressure of the axles, I have only on this point to add, that it is the former which alone and altogether determines the amount of resistance, or extent of fulcrum, upon which the power of steam in locomotive carriages can be rendered effective. Hence it will be evident, that if a load be attached to a locomotive engine, sufficiently great to counteract or overcome the amount of the adhesion, or friction, produced by the gravity of such engine, the engine itself cannot progress, but the wheels will simply revolve, without progressing; consequently, *the load which a locomotive engine will convey upon a horizontal Railway, as at present constructed, is always in proportion to the gravity, or weight, of the engine, and the friction upon the rails produced thereby.*

Having said thus much on the subject of friction, I am anxious to make a few remarks on the laws of gravity, which I consider it requisite to define, in order to elucidate, as clearly as I can, the merits and nature of my improvement.

If a carriage descend freely down an inclined plane, and pass over a certain space in the first minute of its fall, it will pass over 4 times that space in the 2 first minutes, and 9 times that space in the 3 first minutes; the force of descent down the incline, being to the force with which it would descend perpendicularly, as the height of the plane to its length; or to ascertain the space which it would pass over down a regular incline, in any number of minutes, we have only to multiply the space through which it descends in one minute, by the square of the number of minutes in the time of the fall. Thus the velocities with which bodies descend, and the spaces which they pass over, whether down inclines, or perpendicularly, increase in the following ratio:

1, 3, 5, 7, 9, 11, 13, 15, 17, 19, 21;

shewing, that if a carriage descend five yards in the first second of time, in the next second second it will fall *three* times five, or 15 yards; the whole space passed over, as above remarked, being 20 yards; and so on in exact proportion. This motion of descending bodies, is termed their *accelerating motion;* and if any power be employed to assist this force of gravity, the descent is more or less rapid, in proportion to the extent of that power. For instance, let v represent the velocity which a carriage dragged down an inclined plane would acquire in one second of time, and let N be the number of seconds taken in descending from the top to the bottom of the incline, and let V be the total velocity gained; then we have,

$$V = v N.$$

Again, let s be the *space* the carriage would pass over in the first second, and $s N^2$ the space it would pass over in the number of seconds expressed by N, and let S represent such space; then we have,

$$S = s N^2;$$

or, to be more explanatory, the space over which a carriage will travel down an inclined plane in any number of seconds, may always be ascertained, by subtracting the space passed over in one second, from the space passed over in the first two seconds. Thus, if s represents the former, $4s$ will represent the latter, and the difference is $3s$, or,

$$4s - s = 3s;$$

and the space passed over in the third second of time, will be ascertained in like manner, viz. by subtracting the space passed over in the first two seconds, from the space passed over in the first three seconds, which latter space is nine seconds; thus,

$$9s - 4s = 5s.$$

To those of my Readers who may be unacquainted with mechanics, the following Table may be useful, in reference to the particular object of these pages; it represents the spaces over which bodies pass, the *velocities* which they acquire, and the *times* in which they descend, either perpendicularly, or down inclined planes; the comparative difference between the time of descent down a perpendicular, and inclined plane, being in proportion to the angle of inclination, and the friction; and the total time of descent altogether depending upon the time of descent, and space passed over, in the first second.

No. of Seconds taken in Descending.	Velocities acquired at each Second.	Whole Spaces passed over in each Second.	Separate Spaces passed over in each Second.
1	2	1	1
2	4	4	3
3	6	9	5
4	8	16	7
5	10	25	9
6	12	36	11
7	14	49	13
8	16	64	15
9	18	81	17
10	20	100	19

and so on.

My remarks on gravity have hitherto been confined to the operation of bodies, in their descent down inclined planes, &c.; but this operation widely differs in their *ascent* of inclined planes, and it is to this difference I am anxious to call particular attention.

If a power be employed to drag a body up an inclined plane, the velocities are equal, and the spaces passed over in each second are equal; the ascent, therefore, will be,

Number of Seconds.	Separate Spaces passed over.	Whole Spaces passed over.
1	1	1
2	1	2
3	1	3
4	1	4

namely, it will pass over the same space in the second second as it did in the first second, and the same in the third as in the second, and so on. This, however, would not be the case if a body were driven by a single blow up an inclined plane; in such case, the motion would be gradually retarded, in the proportions,

10, 9, 8, 7, 6, 5, 4, 3, 2, 1,

until the power were expended; when it would of course descend, or run back, by its own gravity.

It is true, that the counteracting power of gravity upon all bodies dragged up inclined planes diminishes the higher the body ascends; but this diminution is so extremely minute, that it may be considered altogether unimportant, as, in practice, it is; for if the rise of the plane be one yard, the difference in the counteraction of gravity at the top and bottom of the plane, is merely as one yard to the distance between the surface and centre of the earth.

Having thus endeavoured to explain the laws which govern the motion of bodies in the descent or rise of inclined planes, and allowing the principle I previously laid down to be correct, viz. *that the load which a locomotive engine will convey upon a horizontal Railway, as at present constructed, to be always in proportion to the gravity, or weight, of the engine, and the friction upon the rails produced thereby,*—I will proceed to an explanation of the principles upon which I hope to establish the merit of my improvement in Railways.

Supposing the three wheels, or carriages, A B C (*figs.* 4, 5, 6), to be all of exactly the same weight and diameters, and the diameters of their axles the same, and that a given power be required to move the carriage A along the line D E, viz. from D to E, by

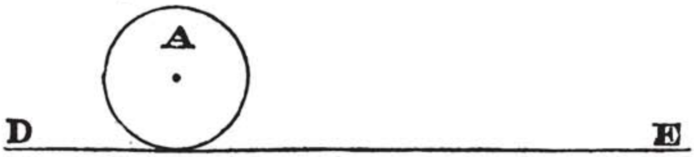

Fig. 4.

a *stationary* engine. Now, if the same power be applied to move the carriage B up the inclined plane F G, it will be ineffectual. It

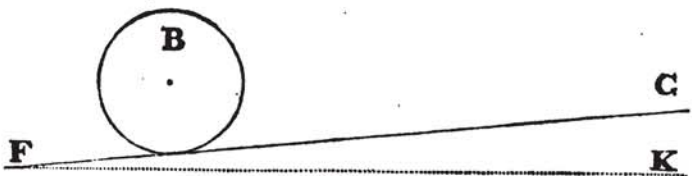

Fig. 5.

must, therefore, be increased in proportion to the angle G F K; but, supposing the angle L I H equal to the angle G F K, the

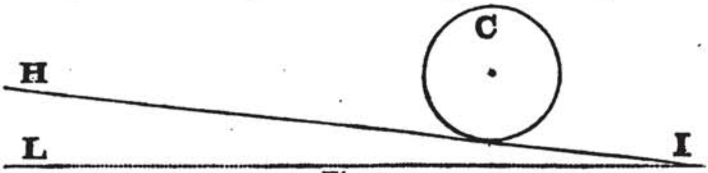

Fig. 6.

power required to move the carriage C down the incline H I, at the same speed, would be much less in proportion to the power required to move the carriage A on the level, than the excess of power required to move the carriage B up the incline F G.

This difference will be the gravity of the descending carriage, C, which gravity is an accelerating or multiplying motive power*, in addition to the assistant power supposed to be employed; whereas, the ascent up the inclined plane G F, is equal, and the resistance of the carriage B, by gravity and friction, is a regular, and not a multiplying resistance.

We will now suppose that the vehicles A B C are locomotive engines, and that a given power is employed, sufficient exactly to move A over the surface D E, dragging any

* In using the words "*accelerating, or multiplying* "*motive power,*" it may be said that I ought, more properly, to have appropriated the words, "this difference "will be the *accelerated velocity, produced by the con*"*stant power of gravity;*" but as accelerated velocity produces increased momentum, or as accelerated velocity produces increased centrifugal force, I know not how I can better describe the action of gravity on descending bodies, than by terming it "an accelerating, or multi"plying motive power."

maximum weight after it, (meaning, by maximum weight, such a weight as will not be too great to counteract the friction between the engine and the plane, which friction, as a fulcrum, renders steam power effective). Now, the power required to move the engine B up the incline F G, will be as before, according to the angle G F K, but if we attach the same weight to B as to A, no increase of steam power could drag such weight up the incline; inasmuch as when the weight was attached to A, it was exactly that which the friction of the carriage A upon the plane, as a fulcrum, could enable the engine to surmount; but on the incline F G, the resistance, or friction, of the engine B upon the surface, is reduced in proportion to the angle G F K, and, therefore, exclusive of the difference in power requisite to move the two engines and their loads by fixed engines, is the difference in the fulcrums, or resistance, by which locomotive power can be rendered effective.

The comparative quantity of goods, therefore, which can be drawn up an inclined plane, and upon a level, by any given locomotive power, differs very materially from the comparative quantity of goods which can be drawn up an inclined plane, and upon a level, by a given power from a fixed engine.

Let us now look at the descent, and we shall find these properties widely different. If the carriage C move down H I, assisted by the power of a stationary engine, that power will be, as before stated, less in proportion to the power employed to move A, than the excess of power employed to move B, owing to the accelerating power of gravity; but, supposing C to be a locomotive engine, with the same weight attached to it as was attached to A, and employing the *same* power as A, that power would not only be more effective than it was along D E, in proportion to the angle H I L, but, in addition to the resistance offered to steam power, by the friction of the engine-wheels upon the plane, and which friction, like that on F G, will be in proportion to the angle of the incline, is the resistance of the accelerating power of gravity, or accumulating momentum, not only of the engine, but the load behind it, which becomes an additional and most important fulcrum for the effective power of steam.

The preceding remarks will shew the basis upon which the serpentine or undulating Railway, as a mechanical question, depends; it will also prove how desirable it is that upon all roads the surface should be undulating, instead of the perfect level, provided the descents be not too great, to render the force of gravity dangerous, when multiplied by other constantly effective power.

In further elucidation—Supposing the car-

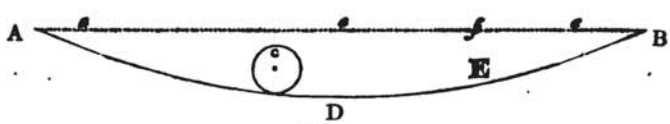

Fig. 7.

riage C to be travelling from A to B, along the curve A D B, impelled by any *constant* *and equal power*—in its descent to D, the speed will be increased by the accelerating power of gravity, multiplied (if I may so express myself) by the accumulating effect of constant and equal power; whereas, in its ascent from D to B, it has, notwithstanding the counteraction, by gravity, of the momentum gained (though, *in this case*, uniformly retarding), the advantage of such constant and equal power to support its motion.

Now, supposing that, without the employment of power, a carriage be placed at the point A, it would not *move* upon the level, *e e e*, but it would run down A D, and ascend the line D B to a certain distance, which distance depends upon the momentum gained by the accelerating force of gravity, and which momentum, as in all other cases, is *in proportion to the velocity of the body, multiplied by its weight*. Now, supposing the carriage to have risen up the incline D B, as far as E, we have here a space travelled over equal to

A *f*; and it is to the result of this accumulating momentum produced by gravity, multiplied by the accumulating effect of power, that I principally look for the advantage derivable from an undulating line of road.

If locomotive power be employed to work a carriage along the dotted line *e e e*, to which engine is attached another vehicle, just heavy enough to admit of progression, it would accomplish the task in a given time. Now, if these carriages were placed at the point D, it is evident that they could not ascend, for the reasons stated in page 47; but if the same carriages be placed at the point A, the descent to D will not only be much more rapid than any part of the advance along the line *e e e*, but the momentum produced by the accelerating power of gravity, multiplied by the accumulating effect of power, will have increased to such a degree, that it will *counteract the difficulty of ascent from* D *to* B, and the vehicles would arrive at B in *very* *much less time* than they would have occupied in travelling along *e e e*.

By way of proving the truth of these observations, I had a curve made of the following proportions: From A to B was 4 feet; depth of the curve 2 inches. *a*, is a roller so constructed as to move easily along the curve, and to revolve upon its axis, to each end of

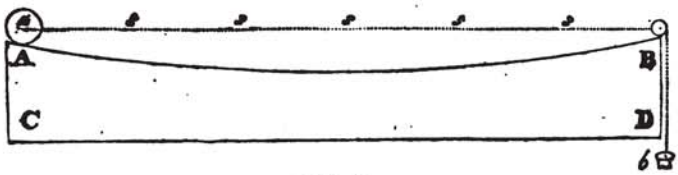

Fig. 8.

which was attached the string, *s s s s s*, which, passing over a pulley at B, had a small paper box, *b*, suspended from it.

By reversing the solid piece of wood, A B C D, the same roller, string, and suspended box, were made to operate on the horizontal surface, C D.

The following experiments were made with different weights, just sufficient to move the roller, *a*, along the surface, C D, when perfectly horizontal, and at different inclinations:

Inclinations.	Time in passing over the Horizontal Plane.	Time in passing over the Curve.
	Seconds.	Seconds.
On a perfect Level	$2\frac{1}{2}$	$1\frac{1}{2}$
Inches. Inches. Rise of 3 in 48	3	2
4 in 48	5	2
5 in 48	5	2
6 in 48	6	$2\frac{1}{8}$

Thus shewing, that the greater the angle of the incline, the longer was the time required in passing along the plane, C D; whilst on the curve, A B, the same exact weight being employed at each experiment as along C D, the speed scarcely varied, and, upon all occasions, was considerably greater than upon the horizontal plane.

By way of reducing my opinions, however, to certainty, and in order to judge of the effect of locomotive power on an undulating line, by the test of experiment, I ordered a small engine to be manufactured, on clock-work principles, with a strong spring in a barrel, and a fusee sufficiently large to admit of travelling the length of 50 or 60 feet, being also particularly anxious that the power of the spring should be sufficient to overcome the pressure of the engine-wheels on the plane, when kept from progressing. Wishing to try these experiments as privately as possible, during the time which the manufacture of the engine occupied, I was engaged at Douglas, in the Isle of Man, in superintending the making of two Railways, the one curved, the other horizontal.

These were each 32 feet in length (the length of the most spacious room I could find unoccupied); the length of the ascent and descent of each curve, or undulation, was *one foot;* and the height and depth of each curve from the centre, was half an inch, or one inch from the summit of the convex to the base of the concave of the curve.

I had also ordered a small carriage to be made, to be attached to the engine, when necessary, and to run upon four wheels of the same diameter as the wheels of the engine.

On the 23rd July I received the engine and carriage from Liverpool; their weights were as follows:

Weight of engine	9lbs. 6 oz.
Weight of carriage	3lbs. 10 oz.
Diameter of wheels	3 inches.
Width of the periphery of the wheels	3-8ths of an inch.

On trying the strength of the spring, I was sorry to observe that it was not sufficient, when I placed the carriage on a smooth surface, and prevented its progression, to turn the wheels; that is, it had not power, as I wished it to have, to overcome the adhesion, or friction, between the wheels of the carriage and the surface of the plane.

I, however, resolved to try a series of experiments with it, and afterwards to return it to Liverpool, to have a stronger spring attached to it.

Accordingly, I had the Railways placed firmly down, and upon as exact a level as circumstances would permit. The distance between the lines on each Railway was eight inches; the width at the surface of the rails was half an inch; the distance between the wheels of the engine governed, of course, the width between the lines; and care was taken to give the carriages sufficient play, to prevent them being bound by friction against the sides of the rails.

Having ascertained that both Railways were level, the spring was wound up, by drawing the engine backwards from the end of the line to the commencement. It was started without any weight attached, and the following was the result:

On the Curved Railway,	On the Horizontal Railway,
6 seconds.	7 seconds.

I then placed 7 lbs. weight upon the engine itself, which had a platform for such purpose; the result was,

On the Curve,	On the Horizontal Railway,
8 seconds.	9 seconds.

I then attached the small carriage to the engine, and, without load, I found the speed of travelling along either line, was in the same proportion as before.

I then tried various weights in the carriage, and invariably found a decided advantage in the curved Railway. This advantage was, however, more evident in the following experiments:

With 17 lbs weight in the carriage.

From North to South,

Curved Railway,	Horizontal Railway,
15½ seconds.	20½ seconds.

From South to North,

Curved Railway,	Horizontal Railway,
17 seconds.	22½ seconds.

Now, omitting the half-seconds, and taking the averages, the difference of space which the engine would have travelled over on the curve, in the time required to travel 32 feet on the horizontal plane, is as follows:

16 : 32 :: 21 : 42 feet;

shewing a difference of nearly 1-3rd in the speed.

Thinking it probable that, by the variation in the time occupied in traversing the lines from different sides of the room, that they might not be perfectly level, I had them again examined and adjusted with particular caution; after which, on again trying, with the same weight, viz. 17 lbs. the result was as follows:

From North to South, and South to North,

On the Curve,	On the Level,
16 seconds.	22 seconds.

This last experiment was repeatedly tried, and without any distinct variation; the time was ascertained by a second-hand watch, and carefully noted by Mr. J. L. Gardener, of Manchester, who witnessed the experiments, as well as myself.

Although I perceived that 17 lbs. was as great a weight as the engine could well convey upon the horizontal Railway, I was anxious to try the result of greater, and increased the load to 22 lbs. The result was, *From North to South,*

On the Curve,	On the Horizontal Line,
17 seconds.	30 seconds.

From South to North,

On the Curve,	On the Horizontal,
18 seconds.	28 seconds.

It was here quite obvious, that the curve produced a far more decided advantage; and this advantage was evident at starting; as, on the horizontal road, the engine moved very slowly at first, and traversed 12 or 13 feet before it attained its average speed; whereas, upon the curved line, its motion was apparently regular throughout.

Although these experiments were, in every point of view, so satisfactory, in regard to speed, I was surprized to find that the advantage was not so great as I anticipated in regard to the difference of load the engine was capable of dragging on the two lines. I, however, clearly proved that we could convey a much greater weight upon the curved line than upon the plane; for when the engine would not move at all upon the horizontal road, it would travel without difficulty upon the curve; and it is extraordinary, that in conveying any weight, from 15lbs. upwards, on the latter, the time occupied in doing so, varied in a very trifling degree.

The same comparative results took place up an inclined plane of 1 in 144.

After repeated trials, and the most evident proofs of the success of these experiments, I sent the engine back to Liverpool on the 31st July, with instructions to the maker to increase the strength of the spring as much as possible, under the conviction, that when I had the opportunity of employing greater power, the result would be much more decisive, and the advantage more determinable.

During the delay which necessarily occurred in altering the engine, I ordered another Railway to be laid down, feeling quite convinced that, if my curves were longer than those which I had already tried, the advantage would be proportionate. It also occurred to me, that as the distance between the hind and fore-wheels of the engine was equal to the length of the descent of my curve, it was impossible that I could have gained any very material advantage by gravity, except by means of the carriage attached, containing the load, the hind and fore-wheels of which were only $6\frac{6}{16}$ inches apart.

The Railway above alluded to, was the same length as the two former ones, viz. 32 feet; the ascent of each curve being 5 feet, and the descent 5 feet; and the height and depth from the centre 1 inch, or 2 inches from the summit of the convex to the base ot the concave of the curve.

On the return of the engine from Liverpool, I was gratified by finding a great increase in its power, though still it was not sufficient to move the wheels round when the body of the engine was kept from progressing;

and this could not be remedied without altering the arrangement of the wheels and pinions, and of course reducing the distance over which the engine was calculated to travel. I considered, however, that it was sufficiently powerful to answer every purpose of preliminary experiment, and on the 8th August I proceeded to try my further experiments, of which the following is the result:

	Number of Seconds.		
	Horizontal Plane.	1-foot Curve.	5-feet Curve.
Engine alone, weighing 9 lbs. 4 oz.* and the hind and fore-wheels $6\frac{1}{16}$ inches apart	5	4	$3\frac{1}{2}$
Ditto, and carriage, weighing together 12 lbs. 14 oz.	$5\frac{1}{2}$	$4\frac{1}{2}$	4
Ditto, with 5 lbs. in carriage	$6\frac{3}{4}$	$5\frac{1}{4}$	$4\frac{1}{2}$
Ditto, with 10 — in ditto ..	$8\frac{1}{4}$	$6\frac{1}{2}$	$5\frac{1}{2}$
Ditto, with 15 — in ditto ..	$9\frac{3}{4}$	7	6
Ditto, with 20 — in ditto ..	$13\frac{1}{2}$	$8\frac{3}{4}$	$7\frac{1}{2}$
Ditto, with 25 — in ditto ..	18	11	9
Ditto, with 30 — in ditto	30 could scarcely go.	14	11
Ditto, with 35 — in ditto ..	could not go.	18	12
Ditto, with 40 — in ditto ..	....	....	13
Ditto, with 45 — in ditto ..	....	....	15

* The cause of the engine being lighter than before the alteration, is owing to the iron platform having been removed, with a view of bringing the hind and fore-wheels nearer to each other.

From the preceding statement, two most important results are evident:

1st, That upon a curved line of road, a given weight, moved by a given locomotive power, will travel at much greater speed than upon a horizontal road; and,

2ndly, That a given locomotive power will impel a weight along a curved line of road, which that same power cannot move upon a horizontal road.

The amount of advantage may be in some measure judged of, by reference to the experiments; for instance, it required upon the horizontal plane 13½ seconds to convey 20lbs. over a space of 32 feet; and on the curve, the same weight was conveyed in 7½. Thus:

$$7\frac{1}{2} : 32 :: 13\frac{1}{2} : 57\frac{3}{5} \text{ feet};$$

shewing a decided advantage of full 3-4ths in speed: the comparative advantage increasing with every addition to the weight of load. Moreover, it will be seen, that the same power impelled 40lbs. upon the curve in 13 seconds; proving the capability of carrying upon the curve twice the load that the same engine could impel upon the horizontal plane in the same time.

It will also be seen, as in the previous experiments, that a much greater load could be impelled by a given power upon a curved than upon a horizontal road: inasmuch as the engine had great difficulty in dragging 30 lbs. upon the latter, whereas, upon the former, it carried 45lbs. with facility.

After repeatedly trying the above experiments, in the presence of an individual whose object was to discover, if possible, any defect, and who, on every occasion, when a doubt existed as to time, gave the benefit of that doubt to the horizontal road, I next proceeded to try the effect of the engine up an inclined plane.

The inclination was 9 inches in 32 feet, or 1 foot in $42\frac{2}{3}$. The following is the result:

	Number of Seconds. Horizontal Railway.	Number of Seconds. 5-feet Curved Railway,
Engine alone	$6\frac{3}{4}$	$5\frac{1}{4}$
Engine and carriage, without load	$7\frac{1}{2}$	6
Ditto, ditto, with 1 lb.	$8\frac{1}{2}$	$6\frac{3}{4}$
Ditto, ditto, 2 —	$9\frac{1}{2}$	7
Ditto, ditto, 3 —	11	$7\frac{3}{4}$
Ditto, ditto, 4 —	12	$8\frac{1}{4}$
Ditto, ditto, 5 —	$13\frac{3}{4}$	$8\frac{3}{4}$
Ditto, ditto, 6 —	$16\frac{3}{4}$	$9\frac{1}{4}$
Ditto, ditto, 7 —	22	10
Ditto, ditto, 8 —	30 would not carry all the way.	12
Ditto, ditto, 9 —	would not go.	14

From this statement, the comparative advantage shewn, is as great as upon the level; for instance, it required 11 seconds to move 3 lbs. along the horizontal plane, whereas, upon the curve, 7 lbs. were carried in less time, viz. 10 seconds.

After trying these experiments repeatedly, I ordered a fourth Railway to be made, upon a different principle; thinking that, upon double lines of road, it might be found better to have the undulations of long descent and short ascent, from the supposition that, in traversing from A to B, the momentum gained

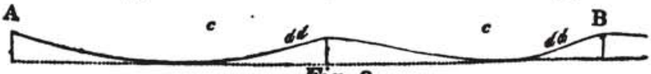

Fig. 9.

at the points *cc*, would be such, aided by the gravity of the carriages and load attached, as to enable the engine to rise the steep ascents*, *d d*, with ease, when the resistance, or fulcrum, offered down the next descent, would be more continued than upon a regular curve, and would, consequently, give far greater effect to locomotive power; for it will be evident, that, except in the momentary ascents up *d d*, in which ascents the engine is assisted by the still acting gravity of the following carriages, the power will always be acting against the side of a hill, and will, consequently, be more effective.

This Railway was accordingly completed, the length of each descent being 8 feet; the length of each ascent 2 feet; and the whole line being 32 feet. It consisted of 3 descents,

* It is almost unnecessary to remark, that *fig.* 9 is not drawn to a scale; it is solely to render the explanation more distinct, that such deep undulations are represented.

3 ascents, and a platform of 1 foot at each extremity, the tops of which were on an exact level with the summits of each ascent; the depth of descent at the lowest point being 2 inches from the highest rise, as in the Railway whose curves were 5 feet.

It is necessary to remark, that the descents in this Railway, except about a foot from their lowest points, were regular inclined planes, curving off at the bottom, to render the ascents more regular, which ascents were also curved.

The following was the result of experiment:

Number of Seconds.

	On the perfect Level.			Weights.	Inclination of 1 in 42⅔.		
	Horizontal Plane.	5-feet Curve.	Long & Short Curve.		Horizontal Plane.	5-feet Curve.	Long & Short Curve.
Engine alone	5	3½	4		6½	5¼	5¼
Do. and carriage	5¼	4	5	lbs.	7¼	6	6
Do. with 5lbs. ...	6¼	4¼	6	1	8½	6¾	6½
——— 10 — ...	8¼	5½	7	2	9½	7	7
——— 15 — ...	9¾	6	7½	3	11	7¾	7¼
——— 20 — ...	13¼	7½	8	4	12	8¼	7¾
——— 25 — ...	18	9	8½	5	13½	8¼	8
——— 30 — ...	33	11	9	6	16¾	9¼	8¼
——— 35 — ...	....	12	10	7	22	10	9¼
——— 40 — ...	....	13	11	8	30	12	9¾
——— 45 — ...	....	15	12	9	....	14	10¾
——— 56 — ...	....	......	14	10	....	......	11
——— 58 — ...	....	......	14½	11	....	......	12
——— 59 — ...	....	......	14¾	12	....	......	13
——— 60 — with great difficulty			16	13	....	......	15½
	....	......	....	13½	....	......	16

Such was the result of my experiments up to this period, shewing, that on the Railway last made, there was considerable advantage in the speed, and the power of conveying heavy weights; and on trying the same engine on the Railway *the reverse way*, the result was found to be the same, although it is a little remarkable, that the speed with which very light weights could be conveyed on this line, when on the perfect level, was, on this trial, not so great as upon the regular curve.

Being, however, satisfied, that both this and the 5-feet curved Railway presented most important and indisputable advantage over the plane, or horizontal road, I induced Mr. Gill, of Manchester (one of the Directors of the contemplated Manchester and Leeds Railway), to visit Douglas, for the purpose of witnessing the experiments.

On his arrival, he very wisely suggested the propriety of fastening all the Railways firmly down upon strong 3-inch planks, to obviate the evident vibration which the Railways sustained, when heavy loads were passing over them: this was accordingly done. Previously, however, he was anxious to witness a few experiments upon an incline which I had prepared, which was about 1 in 94, and these were principally confined to the horizontal Railway, and to the 5-feet curve. The result was as follows:

			Number of Seconds.	
			Horizontal Plane.	Curve.
Engine and carriage			7	5¼
Ditto,	ditto, with 1 lb.		7¼	6
Ditto,	ditto,	2 —	8	6
Ditto,	ditto,	3 —	8½	6¼
Ditto,	ditto,	4 —	9	6¼
Ditto,	ditto,	5 —	9½	6½
Ditto,	ditto,	6 —	10½	6¾
Ditto,	ditto,	7 —	11	7
Ditto,	ditto,	8 —	12½	7¼
Ditto,	ditto,	9 —	13	7¾
Ditto,	ditto,	10 —	14	8
Ditto,	ditto,	11 —	15	8¼
Ditto,	ditto,	12 —	18	8½
Ditto,	ditto,	13 —	20	8¾
Ditto,	ditto,	14 —	24	9
Ditto,	ditto,	15 —	25	9

			Number of Seconds.	
			Horizontal Plane.	Curve.
Engine and carriage,	with	16 lbs.	could not go.	9¾
Ditto,	ditto,	17 —	. . . . . .	10¼
Ditto,	ditto,	18 —	. . . . . .	10½
Ditto,	ditto,	19 —	. . . . . .	11
Ditto,	ditto,	20 —	. . . . . .	11½
Ditto,	ditto,	21 —	. . . . . .	12
Ditto,	ditto,	22 —	. . . . . .	12½

In the preceding statement, the advantage offered by the curved Railway over the horizontal, is very decided; shewing an extraordinary difference in speed upon all occasions, and that 7 lbs. more could be conveyed upon the one than upon the other; 15 lbs. being the maximum weight that could be dragged upon the horizontal Railway, and 22 lbs. upon the curve; while, in the latter instance, the line was traversed in 12½ seconds, whereas upon the former it required 12½ seconds to take 9 lbs.

On trying how much greater weight could be taken up this incline, upon the irregular curved road, we found it to be as follows:

21 lbs. in 12 seconds.
22 lbs. in 13 seconds.
23 lbs. in 14½ seconds.
24 lbs. in 16 seconds, which was the maximum.

Being anxious to ascertain, if I reduced the rise of the 5-feet curve nearer to the level, what difference there would be in result, I ordered it to be planed down to a *one*-inch, instead of a two-inch curve; thinking it desirable to ascertain with how little declination the force of gravity, &c. could be rendered practically advantageous, and being of opinion, that the advantage over the horizontal Railway would be, even with the most trifling undulation, very important.

The following are the particulars of my experiments when this alteration was completed, and when all the Railways were nailed firmly down upon 3-inch planks, and when there was no possibility of vibration; the first series being tried on an exact level, and the last on an inclination of 1 in 96, which is the same ascent as the Rainhill incline, upon the Manchester and Liverpool line:

On a Perfect Level.

	Number of Seconds.		
	Horizontal Plane.	5-feet Curve, rise, 1 inch.	Irregular Curve, rise, 2 inches.
Engine and carriage, without load	5¾	4½	4¼
Ditto, ditto, with 5 lbs.	6¾	6	6
Ditto, ditto, 10 —	8¼	6¾	6½
Ditto, ditto, 15 —	9½	7½	7¾
Ditto, ditto, 20 —	11½	8½	7¾
Ditto, ditto, 25 —	13¼	9⅛	8½
Ditto, ditto, 30 —	15½	10	9
Ditto, ditto, 32½ —	18¼	10½	9½
Ditto, ditto, 35 —	20	11	10¼
Ditto, ditto, 37½ —	21¼	11½	10½
Ditto, ditto, 40 —	23½	12	10¾
Ditto, ditto, 42½ —	29	12½	12
Ditto, ditto, 45 —	......	13½	12¼
Ditto, ditto, 50 —	......	16	12½
Ditto, ditto, 55 —	......	19	17

Experiment up an Inclined Plane of 1 in 96.

	Horizontal Plane.	5-feet Curve, rise, 1 inch.	Irregular Curve, rise, 2 inches.
Engine and carriage, without load	5½	4¾	4¾
Ditto, ditto, with 2½lbs.	6½	5½	5½
Ditto, ditto, 5 —	7¼	6	6
Ditto, ditto, 7½ —	8¼	6½	6¼
Ditto, ditto, 10 —	9	7½	7
Ditto, ditto, 12½ —	11	8	7½
Ditto, ditto, 15 —	12½	8¾	8
Ditto, ditto, 17½ —	14	9½	9
Ditto, ditto, 20 —	15¾	11	10½
Ditto, ditto, 22½ —	19	12	11
Ditto, ditto, 24 —	27	12¾	12
Ditto, ditto, 25 —	would not go.	13	13
Ditto, ditto, 27½ —	......	16½	16
Ditto. ditto, 29 —	......	18¾	17

It is necessary to observe, that before trying the previous experiments, owing to the cord upon the spring-barrel having broken, I was compelled to re-attach it, and regulate the power of the spring accordingly, which will, in some measure, account for the increased speed upon all the Railways; and there being no longer the slightest vibration, and the level and incline having been most cautiously adjusted, I feel little hesitation in stating, that confidence may be placed in this published result of these experiments.

Referring to those on the level, it appears, that the speed on the irregular curve was greater than on the 5-feet curve, the amount of load capable of being conveyed on each being equal; whereas, on the incline of 1 in 96, so great an advantage did not appear, though both, *as in every other case*, proved far more effective than the horizontal Railway.

Referring, also, to the experiments tried upon an incline of 1 in 94 (page 69), we find that 15 lbs. was the maximum load conveyed upon the horizontal Railway; whereas, in the preceding statement (1 in 96), 24lbs. was conveyed in 27 seconds. This difference is no doubt attributable to the difference in the inclination, and to the renewed strength of the spring, as well as to the freedom from vibration.

Again, in the experiments upon the perfect level (page 67), it appears, that 30lbs. was the utmost load which could be conveyed on the horizontal Railway; whereas, in the preceding statement, 42¼ lbs. were conveyed in 26 seconds. This difference is in like manner accounted for.

In allusion to the comparative difference in the speed between the two curved Railways, in the preceding statement, and in the statement at page 67, I confess myself in difficulty, and can only account for it in the difference in the vibration of the two Railways, or to some inaccuracy in levelling, especially as, in the experiments at page 67, the depth of each curve was similar.

It may, probably, be thought unwise by some, that I should have published these conflicting statements; but I have considered it my duty to give a detailed and faithful account of my experiments, being sufficiently satisfied with the fact, *that, however different has been the comparative result of experiment between the two curved Railways, they have invariably, whether upon the level or incline, proved an unquestionable and decided superiority over the horizontal Railway.*

It will be seen, too, that in the experiments at page 67, where the inclination was 1 in $42\frac{2}{3}$, that the irregular curve was decidedly superior to the regular curve, both in speed, and in the amount of load capable of being conveyed upon it; whereas, in reference to the preceding statements (1 in 96), the results are altogether different; the advantage shewn on both curves, over the horizontal line, in the amount of weight conveyed, being nearly equal. This is more extraordinary, as the 5-feet curve had, in the recent experiments, been reduced 1 inch in depth; and I hold it an established principle, that in proportion to the length and depth of the descent, will be the proportion of advantage gained. I, therefore, expected a more favorable result from the irregular curve up the incline of 1 in 96, whose depth had not been altered.

On consideration of the subject, however, I decided, that the difference was attributable to the same causes as before stated in regard to the horizontal Railway, namely, to the difference of vibration, and to the extreme difficulty of levelling with perfect exactness upon an unequal floor, which, however particular I had been, had, no doubt, considerable effect, until I had the Railways affixed to strong 3-inch planks, which planks were levelled, or inclined, with greater precision and ease.

Perceiving, also, that in the irregular curve the abruptness of ascent was, when heavy weights were attached, a considerable obstacle, I ordered them to be planed down, so as to make the ascent 4 feet instead of 2, and, consequently, the depth of descent 1½ inches instead of 2.

The result of the following experiment proves, that the speed of conveyance upon the two undulating lines, when upon a level, more nearly assimilated; and that the abruptness of ascent being removed, a greater weight could be conveyed upon the 6 and 4-feet curve (altered from 8 and 2 feet), depth of descent 1½ inches, than upon the regular 5-feet curve, depth of descent 1 inch.

Hence I am led to the conclusion, that whether, in practice, it may be found better or not, to adopt undulating Railways with short ascents and long descents, or with the ascents and descents proportionate, as regular curves; the advantage gained over a common horizontal Railway will be in proportion to the length and depth of descent; taking care *never to have the ascents so short as to render them abrupt, nor the descents so deep as to render them dangerous.*

Experiment on a Perfect Level.

			Irregular Curve, 4 feet ascent, depth of descent 1½ inch. Seconds.	5-feet Curve, as before. Seconds.
Engine and carriage			4¾	4¾
Ditto,	ditto, with 5 lbs.		5½	6
Ditto,	ditto,	10 —	6¼	6¾
Ditto,	ditto,	15 —	7¼	7½
Ditto,	ditto,	20 —	8½	8½
Ditto,	ditto,	25 —	9⅛	9⅛
Ditto,	ditto,	30 —	10	10
Ditto,	ditto,	35 —	11	11
Ditto,	ditto,	40 —	12	12
Ditto,	ditto,	50 —	14½	16
Ditto,	ditto,	55 —	15¼	19
Ditto,	ditto,	59 —	20	

Having thus submitted to public notice, the particulars of my experiments, by which it is distinctly proved, that a curved line of Railway presents the most undoubted advantages over a horizontal one, for the rapid conveyance thereon of merchandise and passengers, and for the effective and economical exercise of locomotive power, few observations are necessary, beyond those I have already made, to make evident the cause of this advantage, which altogether results from rendering the power of gravity, which is a natural motive power, or the accumulation of velocity and momentum produced thereby, applicable to a *most important purpose;* and I confidently believe, that such assistant power being taken advantage of on Railways, any locomotive steam-engine (assisting gravity down each descent, and opposing it up each ascent) will traverse a given space, when drawing what may be, at the present time, deemed its full load upon a horizontal line, in *half the time* which it could otherwise do, or will, *in the same time*, convey *twice the weight* of merchandise. It must be borne in mind, *that the previous experiments have been tried with a power not sufficient to overcome the friction of the periphery of the wheels of the engine upon the plane, when stopped from progressing: I look, therefore, to a far more advantageous result than is thereby shewn, when, upon a regular line of road, efficient* *steam power can be employed—a result which cannot fail, in my opinion, to be peculiarly advantageous in the ascent of inclined planes.*

To state, from the preceding experiments, what the exact amount of advantage is, would be difficult, as it varied with different loads; but, referring to those trials which are last detailed, and which were made with a strengthened spring, on the perfect level, and when the roads were free from vibration; and taking 30lbs. as the medium weight the engine conveyed on the level, and 15lbs. upon the incline of 1 in 96, we have as follows:

Weight carried.	Average on the two Curves.	On the Horizontal Plane.	Length of Lines.
lbs.	Seconds.	Seconds.	Feet.
30	9½	15½	32

$$9\tfrac{1}{2} : 32 :: 15\tfrac{1}{2} : 52\tfrac{4}{19};$$

shewing an advantage of full 5-8ths on the level.

Weight carried on Incline.	Average on the Curves.	On the Horizontal Plane.
15lbs.	8½ seconds.	12½ seconds.

$$8\tfrac{1}{2} : 32 :: 12\tfrac{1}{2} : 47\tfrac{1}{17};$$

shewing an advantage of very nearly *one half.*

It must not, however, in this calculation of advantage, be overlooked, that the *comparative* advantage increased with *every increase of load*, and that, exclusive of this advantage in speed, was the capability of conveying, with the same power, a much heavier load upon the undulating than upon the horizontal Railway.

Being desirous, if possible, of laying before the public an unobjectionable and practical proof of the advantage above stated, it occurred to me, that I might do so by giving a *limited* power to the engine, and by seeing what difference there was in the momentum gained upon the curved and horizontal lines.

For this purpose I wound up the spring, by drawing the engine-wheels back exactly *six feet;* and loading the carriage with 30lbs.

Upon the horizontal road it traversed with this power,
7 feet 9 inches = 1 foot 9 inches momentum.

Upon the curve,
10 feet 2 inches = 4 feet momentum.

Again, on winding up the spring equal exactly to 10 feet, it traversed, with 30 lbs.

On the horizontal plane, in 12 seconds,
14 feet 7 inches = 4 feet 7 inches momentum.
On the curve, in 10 seconds,
20 feet = 10 feet momentum.

Loaded with 20 lbs. and wound up 10 feet, it traversed,

On the horizontal plane, in 13 seconds,
20 feet 8 inches = 10 feet 8 inches momentum.
On the curve, in 12½ seconds,
28 feet 4 inches = 18 feet 4 inches momentum.

Without load, and trying the engine and carriage alone, the spring being wound up 6 feet, the result was as follows:

On the level,
17 feet 9 inches = 11 feet 9 inches momentum.
On the curve,
21 feet 11 inches = 15 feet 11 inches momentum.

I was induced to try these latter experiments, not with the view of corroborating the *exact* amount of advantage proved by the previous trials, but to offer decided proof of the *cause* of that advantage, the maximum of which would of course depend upon the uniform action of gravity and power: for instance, when the spring was wound up 6 feet, and the carriage loaded with 30lbs. the difference in space traversed, was as 93 inches to 122; but when the spring was wound up 10 feet, and the carriage similarly laden, the difference in space traversed, was as 175 to 240; shewing very different proportions.

From a careful examination of all the previous experiments, it cannot possibly be doubted, that Nature presents to us, in gravity, a most important assistant power, which, united with mechanical power, will not only enable us to travel with far greater speed, and with far greater loads, but renders locomotive power far more effectively applicable.

How little the laws of motion upon Railways are at this moment understood, may in some measure be judged of, by the remarks of Mr. Nicholas Wood, who seems to express a doubt whether,

1st, According to the general received opinion, the intensity of power required to keep a body in motion at twice the velocity, should be twice that required to keep it in constant motion at half that velocity—as,

Supposing velocity to be represented by	1,	2,	3,	4,	5,	6
Distance passed over . .	1,	1,	1,	1,	1,	1
Power required to move a carriage of any weight	10,	20,	30,	40,	50,	60

Or,

2ndly, Whether the intensity of power required to urge the body forward at twice the velocity, is not four times that which is required to keep that body in motion at half the velocity—as,

Velocity, V, at which the body is moved	1, 2, 3, 4
Resistance of body, B	10, 20, 30, 40
Space passed over	1, 1, 1, 1
Mechanical force required to propel B over S, at velocity V . . .	10, 40, 90, 160

Or,

3rdly, Whether the intensity of power required to urge the body forward, is not in the ratio of the square of the velocity—as,

V	1, 2, 3, 4
B	10, 40, 90, 160
S	1, 1, 1, 1
Mechanical force . . .	10, 80, 270, 640

I feel little hesitation in discarding the two last positions, and in founding my belief upon the first position.

Now, if this position be correct, viz. that it requires *twice* the mechanical power to move a body 20 miles per hour than to move it 10 miles per hour, upon a horizontal Railway, how evident is the advantage derivable from the curved line of road, when a given power being employed to move a weight of 40lbs. 32 feet upon a horizontal plane in 21 seconds, moved on the curve the very same weight, the same distance, in 10½ seconds; shewing, that the power of gravity, multiplied by any given mechanical power, is fully equal, when made jointly available, to compensate for the loss of mechanical power sustained upon a horizontal plane by a doubled velocity!

It would be extremely desirable to reduce the laws of locomotion and friction to some decided data. The difficulty of doing this, is not only evident from the diversity of opinion in regard to the latter, which has so long existed, but, after a series of most interesting experiments upon Railways, Mr. Wood himself, to whom we are indebted for much valuable and practical information, confesses, that such is the difficulty of the subject, that, although his experiments may be sufficient, in many cases, for practical purposes; "yet they by no means tend to bring the "enquiry into any more settled state."

Much, however, as I appreciate the talent and public services of Mr. Wood, I cannot avoid mentioning, that he appears to me to have laboured under a mistake in some of his calculations; for instance (page 202, second edition), he calculates the resistance *up* a plane to be a given amount, say 56, and down the plane a given amount, say 22, and then draws his mean resistance, or friction, upon a level plane, 39—thus:

$$\frac{56 + 22}{2} = 39.$$

Now, I dispute the correctness of this mode of ascertaining the mean resistance on a level, inasmuch as the power of gravity, assisted or not by mechanical power, down a descent, being a constantly accumulating power, diminishing resistance,—and when a given power is employed to drag a body *up* a plane, the resistance being uniform through every space and time,—the two powers of resistance added together and divided, cannot shew the mean resistance on a level, and for the reasons before stated in page 47.

I name this, because it particularly bears upon the principle on which I found my improvement; for, if Mr. Wood be correct, it appears to me impossible that any advantage could accrue from the adoption of a curved or undulating line of road; whereas, experiments corroborate my assertion, that such advantage is indisputable. The power of gravity, which produces accelerated velocity down the descending part of a curve, may be said to be counterbalanced by the opposing power of gravity up the ascending part; but we must not forget, that before the ascent commences, a given power or momentum is generated, and a given space must be travelled over before this power becomes inert; and as gravity alone thus enables a load to descend an inclined plane, and to ascend an opposite one until momentum cease, it is evident, that, locomotive power being employed *from such point of stoppage to the highest summit of ascent*, the number of the revolutions of the axles and of the wheels upon the plane, must necessarily be considerably less than along a level line proportionate to the extent of the curve. It must also be evident, that, either on the descending or ascending line of a curve, the axle and rolling friction must be less than upon a horizontal plane, inasmuch as the line of gravity *is never vertical*, except at the lowest point of descent and the highest point of ascent; whereas upon the horizontal plane it is *continually* so.

It is true, that the power of gravity has been applied to Railways in many useful ways; such as the loaded descending carriages drawing the empty carriages up an incline, &c. &c.; but how little it has been thought of as an assistant to locomotive power, will appear from Mr. Wood's remarks: "On public, and other Railroads, "where the quantity of goods to be conveyed "is fluctuating, and is, or is likely to be, the "same in both directions, this species of "power cannot be resorted to."

Again, "It is only where a preponderance "of goods has to be conveyed in one direc-"tion, and where, upon any declivities oc-"curring in the line of road, that prepon-"derance is capable of overcoming the gravity "of the returning carriages, that the action "of gravity can be used to advantage."

This is quite true, in regard to a descending body drawing up an ascending body; the former must be heavier than the latter; but apply mechanical power to each, and that which will be requisite to propel the carriage down the plane, will be much less, as before observed, in proportion to what would be required upon the level to move the same load, than the amount of power necessary to move the carriages *up* the incline, above that employed upon the level.

With these explanations, I shall content myself with laying down what appear to me a few leading principles, in reference to this subject: results will prove how far they are correct.

1. *That the friction of carriages upon all Railroads, is in proportion to their weight, and the diameters of their wheels and axles; and when propelled by any constant power, this friction is an* uniformly active force.
2. *That the load which a locomotive engine can convey,—or, in other words, the amount of steam power which can be rendered effective on a horizontal Railway,—is always in proportion to the weight of the engine, or the friction produced thereby.*
3. *That the difference in load which a loco-*

motive engine will convey up an inclined plane and on a level,—or the amount of steam power which can be rendered effective on each,—is in proportion to the inclination of the plane.

4. *That the difference in load which a locomotive engine will convey down an inclined plane and on a level, can form no basis whatever for the calculation of friction, or speed, upon a level, inasmuch as the load never can be too excessive for the engine down the plane; and the momentum gained from the effect of constant mechanical power, in addition to gravity, accumulates in exact accordance with the laws which govern gravity.*

5. *That if it require a given power to move a body* 10 *miles per hour, at maximum velocity, upon a horizontal Railway, it will require twice that power to move the same body* 20 *miles per hour.*

6. *That the same law governs the motion of bodies up inclined planes*.*

7. *That the same law does not apply in any degree to the motion of bodies down inclined planes, inasmuch as, whatever be the power employed, the spaces over which the body will travel, will be as the squares of the times from the commencement of the descent.*

8. *That, upon the above principles, a much greater speed can be attained by the exertion of a given power, or a much greater load carried by that power, upon a curved† or undulating Railway, than upon a horizontal plane.*

* I mention this as an established principle, in reference to all useful purposes; although it may be argued, that the higher the ascent, the less becomes the power of gravity. Such difference is, however, as before stated, altogether immaterial.

† It is obvious, that a continuation of inclined planes, gradually curved at their bases and summits, would prove quite as, if not more effective than, regular curves, or segments of circles. Cycloids may, in practice, be found the most advantageous description of curves.

9. *That the maximum speed which can be attained, and the maximum load which can be dragged by a given locomotive power, upon a curved line, depends upon the length and depth of the curve; consequently, the longer and deeper the descent, though the rise be equal to the fall*, the greater the advantage.*

10. *That in the ascent of inclined planes on a curved line of road, the depth of each curve,—or, in other words, the angle of each descent,—should always exceed the angle of the inclined plane.*

11. *That it requires a greater power to move a body from a state of rest to a given state of motion, than to keep it in that state of motion when such motion is attained.*

* As before observed, it is a matter of future experiment, how far short, may be more advantageous than long ascents, though the height of ascent may be equal to the depth of descent; my opinion is, that they will prove the most effectual.

12. *That the momentum of a moving body is exactly as the velocity of the body multiplied by its weight.*

13. *That such momentum, when produced by any given power, on a horizontal Railway, is equal to the difference between the force required to move a body from a state of rest to a given uniform velocity, and the force required to keep such body in motion, at such given attained velocity.*

In reference to Definition 13, let P represent the power required to keep a body, B, in motion at the velocity expressed by V, and P′ the power required to move the body from its state of rest to the required velocity, and let M represent the momentum of the body when the velocity is acquired; then,

$$M = P' - P;$$

or, supposing it require 10 lbs. to move B from a state of rest to a given velocity, and

8lbs. to keep it in motion when such velocity is acquired; then,

$$M = 10 - 8 = 2.$$

Referring to Definitions 4, 5, 6, 7, it will be seen, that upon them the superiority of the curved or undulating Railway over the horizontal Railway, in a great measure depends; for it is by the accumulation of speed down each descent, and the increased momentum by the accumulative force of gravity, multiplied by mechanical power, that overcomes the chief resistance of each ascent, and thereby produces the advantage which I have endeavoured to explain, and which I affirm to be obtained upon a curved Railway. How far that advantage may be increased by making the length of ascent shorter than the length of descent, I will not now discuss; I am inclined, however, to think it will prove considerable, as the power of steam would thereby be more effectively employed, by working more constantly on the descent, where the retarding force of friction is necessarily less, and the fulcrum presented to the activity of steam power greater than on a level, and where the gravity of the descending carriages would more than compensate for the loss of power, by the comparative steepness of the ascent.

Having thus far treated of the advantages likely to accrue from the adoption of my improved Railway,—advantages as clearly demonstrable, in my opinion, by figures and mathematical reasoning, as by experiment,—I now proceed to anticipate and to answer such objections as may naturally arise in the public mind, on a perusal of these pages.

1.—*The Motion may be Inconvenient and Unpleasant.*

This is altogether ideal; even if the curves were deep and short, I much question whether the motion would not be found rather agree-able than otherwise. Any one who has travelled in a carriage upon an undulating surface of turf, may form some opinion of what it would be upon a deeply-curved Railway; but as the curve upon which the preceding experiments were principally tried, is only 1 inch in 5 feet, and supposing 100 feet, upon a regular line of road, rising and falling 20 inches, or 100 yards rising and falling 5 feet, to be a sufficient extent of curve for practical purposes,—it will be evident, that the undulating motion could not produce any serious inconvenience. Indeed, whether the curve be longer or shorter, the motion, in my opinion, and in the opinion of those who have witnessed the experiments, would be any thing but objectionable; for in short curves, any very deep undulation would be unnecessary, and in long ones, comparatively unfelt. To ascertain, however, the most advantageous description and extent of undulation, must be the object and result of careful practical experiment. Allowing my principle to be correct, the difficulties of practice are easily overcome.

2.—*The Difficulty of Ascent, provided the Engine should stop, by accident or otherwise, at the Base, or in other particular part of the Curve.*

I am of opinion, that it will be found desirable, at all parts of the line where a train is intended to stop, to have the road for a short distance perfectly level, though I do not conceive that any practical difficulty would arise were it not so. I would also recommend the same level road, for any required length, at the places of starting.

But let us take the worst view of the question, and suppose the carriages to stop in such a position, and to be so laden, that the engine could not rise the ascending part of the undulation.

It will be evident, as a locomotive engine can be worked either backwards or forwards, that, by reversing the action of the power, the carriages will be driven back by that power, and their own gravity, sufficiently high up the opposite incline to give them in a few moments the full power of progression.

This is, however, supposing a case which will seldom or ever, I should think, occur; as it would be just as easy to stop the train when the engine was at the bottom or other part of the ascent, with the gravity of the following carriages at disposal to assist the rise, as when the whole train were ascending: the necessity of this, however, depends upon whether the engine be overladen or not. I am only anxious to prove, that, let the load be what it may, if once capable of progressing, it never can be delayed beyond a few minutes upon an undulated line of road; and *that delay* the engines are *now* subject to, in acquiring their average velocity upon a common Railway.

There are no other objections which arise in my mind to the general adoption of undulating Railways; and if there be, how little do they depreciate the advantages offered by the increased speed, and the economy of power which I have proved to be derivable from them, added to the facility of ascending inclined planes, the reduction in the expense of excavations, and other advantages!

I am aware that many plans have been suggested of ascending inclined planes, by friction-wheels, &c. &c.; but Nature presents us with so powerful an assistant, in gravity, and at so little expense, that it would be madness to refuse her aid.

I have merely now to add, that, in the experiments which I have tried, the loads were attached to the engine by means of a metal rod, with a moveable joint at each end, something similar to the one described in *fig*. 2; and whether, upon the lines of road now established, such mode of connecting the carriages be thought advisable or not, instead of by the chains now employed, it will be very desirable that such should be the case upon the undulating lines, as the gravity of the descending load is a principal and indispensable advantage, which, were it attached to the engine by chains, would be comparatively lost, as, in the ascents, the unpleasant jerks I am so anxious to obviate, might be even more objectionable than they now are.

I have also to add, that should the ascents (as locks, bridges, &c.) on canal-lines, or other roads, be so steep as to become inconvenient for the effective exercise of locomotive power, and for the progress of the train, it may probably be found desirable, to obviate the expense of levelling, to have *stationary weights* so constructed as to be drawn either up or down the inclined planes; acting, in their ascent, as a lock for the engine and carriages down the hill, and in their descent, as an assistant to the rise of the engine and carriages up the hill. The gravity of each of these weights might be such as to act beneficially upon all average loads; indeed, the same weights might be so regulated, *by machinery at the top of each ascent*, as, without the employment of a stationary steam-engine, to *accommodate themselves* to *different loads;* supposing, which I do, that a suitable person be employed to attend to them, and that the trains of carriages are traversing the roads each way in pretty equal succession. This, however, can only be necessary where the momentum required to assist the engine and load up any ascent is such as to produce a velocity dangerous to passengers.

In conclusion, as I have reason to believe that I may soon have an opportunity of practically proving the importance of the improvement which I have promulgated in this short Treatise, and the extent of the advantage I anticipate, I will only add, that it is my intention, at such time, to pursue the subject by a second publication; in the interim, offering this to an enlightened and reflecting Public, with humble confidence.

ADDITIONAL REMARKS.

ADDITIONAL REMARKS.

The publication of the preceding pages having been unavoidably delayed much longer than I anticipated, and having had an opportunity in the meantime of trying a series of experiments upon models of a more extended scale than those to which I have hitherto referred, I am happy in being able to state, that the result has been equally favorable to my expectations in every point of view.

The models in question have been for some weeks open to the inspection of the scientific world, and the experiments have been witnessed in London and at Manchester by numerous individuals, whose high mathematical and mechanical acquirements would, I conceive, naturally have led to the immediate exposure of a fallacy, had any such fallacy existed. On the contrary, however, the facts elucidated by experiment remain uncontroverted; and although it may have proved difficult to *some* to account satisfactorily and theoretically for the advantage gained, yet I am happy, on the other hand, in being able to state, that I have received, from *other* unquestionable authorities, assurances of their conviction, that the principle which forms the leading subject of this Treatise, is a correct one, and that, however opposed to received opinion, it is in strict accordance with true mechanical and mathematical reasoning.

It would, I confess, have very much relieved the responsibility which I incur in offering this publication to the world, had I felt myself at liberty to embody the various correspondence, formula, diagrams, and calculations, with which I have been favored, in solution of this, I trust, important subject: but, fully sensible of the averseness which men naturally feel in permitting others to make public their friendly opinions, I, from delicacy, abstain from stamping a value upon this Work, which would, otherwise, have raised it much higher in public estimation than any observations which I feel it in my power to offer.

I shall therefore content myself by adding hereto, in a more compressed and mathematical form than I have hitherto done, a statement of my own opinion as to the cause of advantage derivable from a curved or undulating Railway; to which I shall annex, by permission, the Reports of Mr. Robert Stephenson, senior, who, although prevented by indisposition from witnessing the experiments, favored me some months ago with his opinions;—to which I am also desirous to attach an article upon an Improved Axletree, published by that Gentleman in the 66th Number of "The London Journal of Arts and Sciences," in the year 1826 (the particulars of his invention appearing in the same Number), especially as this improvement will, in my opinion, be found particularly applicable to carriages working upon an undulating line of road; as, were the common axletrees to be used upon such Railway, they would, where the lateral curves or windings are abrupt, no doubt materially interfere with the advantage I expect to derive from the full combined effect of gravity and locomotive power; whereas, by adopting the axletrees recommended by Mr. R. Stephenson, senior, the advantage would, in comparison with existing roads, be as great upon a *winding* as upon a *direct* undulating Railway.

The following is my explanation, which, I conceive, bears me out in all my previous remarks:

Suppose the dotted line, E A, to be a horizontal Railway,—A B, to be a descend-

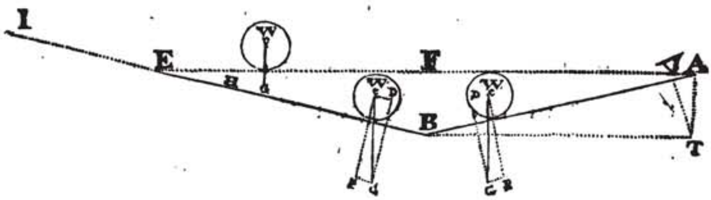

Fig. 10.

ing one,—and B E, an ascending one, on which are placed the three wheels, W W W,

1. Now, the amount of friction produced by the pressure of the wheel W on the plane E A, is in exact proportion to its weight, or to the weight of any vehicle which rests upon it; and upon such weight also depends the amount of attrition produced by the revolution of the axle within the nave or cylinder in which it moves.

The reason why the amount of friction, or attrition, is proportionate to the weight of the vehicle, is because (supposing C to be the axle, or centre, of the wheel), the perpendicular line, C G, is the line of gravity.

On a horizontal Railway, therefore, the amount of pressure upon the rails, and the amount of axle and rolling friction produced by that pressure, are in exact accordance with, and altogether dependant upon, the weight of the carriages and load; and when locomotive power is employed to overcome this pressure and friction, and when a maximum velocity is attained, such velocity (the power being kept up) is uniform through spaces and times, and such pressure, or friction, is an *uniformly opposing power*. Moreover, as before frequently observed, the amount of load which any locomotive engine will convey, is in exact accordance with the amount of its pressure upon the rails and axles; or, in other words, with the axle and rolling friction.

2. Let us now suppose the wheel W to be

traversing from A to B. From the point A, it is evident that a body would fall to T, according to the laws of bodies falling perpendicularly; and if upon the line A B we draw the perpendicular line V T, a body would descend, by gravity, down the plane from A to V, in the same time as it would fall, perpendicularly, from A to T: and the power of gravity, which enables it to do this, acting *equally* (practically speaking) throughout the whole descent from A to B, would produce *an uniformly accelerated motion;* in consequence of which, on the arrival of the carriage at the point B, the velocity would (allowing for the difference of friction) be mathematically equal to what it would be at the point T, had it fallen perpendicularly from A to that point. Now the extent of the power of gravity, or cause of the wheel W descending down the incline A B, will be easily comprehended by reference to the parallelogram, D C P G: where the diagonal, C G, is the line of gravity, C P the line representing the amount of pressure on the rail, and C D the line of motion;—that is, the line or power of gravity, C G, instead of acting perpendicularly, and with full intensity, on the rail, as on the line E A, becomes divided into two separate and distinct powers, viz. C D and C P; the latter, if I may so express myself, endeavouring to stop the progress of the wheel, and the former employing every effort to urge it forward; and as C D is to C P, so is the one power exactly to the other;—and thus, if the carriage or wheel, W, weigh 5 tons, and if C D be 1-5th of the power or force, C P, the pressure upon the rails is reduced from 5 tons to 4 tons; and *not only reduced*, but the amount of power thus saved, is actively employed in opposing the resistance offered by C P.

Such would be the *commencement* of the progress of a carriage descending the incline A B, by its own gravity, until, as before observed, on arriving at B, it would attain the same velocity as it would have attained at T, had it fallen perpendicularly from A to T; and if locomotive power were constantly employed to assist this force of gravity, the progress of a body down the descent would be the result of these united powers; the motion would be *uniformly accelerated*, and although the velocity would be increased in proportion to the increased power employed, yet the descent would be in proportionate accordance with the laws of falling bodies, both as to spaces and times.

3. But we will now suppose the same carriage, W, to be propelled from a state of rest at B, to the position on the

incline B E, described in the diagram. The angle F E B being equal to the angle F A B, and the line of gravity, C G, being drawn, the parallelogram, C D G P, is exactly equal to that described on the descending plane; consequently, C P is the line representing the amount of pressure on the rails, and C D the line of power opposing such pressure; from which it is evident, that, unless prevented by some greater power than C D, the carriage would roll back to B, but if opposed by any regular and greater power, which we will call locomotive power, the carriage would rise gradually up the plane B E, with uniform velocity, and through equal spaces in equal times; for the power C D, which is a portion of the force of gravity represented by C G, *being opposed by a greater power* than itself, does not in this case act as *an uniformly retarding* *power*, but as *an uniformly opposing power*. It will also be seen, that, throughout the ascent, the pressure upon the rails, and, consequently, the amount of friction, is precisely the same as it was down the descent A B, viz. as much less than it was on the horizontal line E A, as the line C D to D G.

4. But to prove the advantage to be derived by an undulating Railway, we must not allow the carriage to stop at B; we will therefore suppose it to travel as far as it is able, by gravity alone, along the undulated line A B E.

Now, as before observed, it would descend from A to B, according to the laws of falling bodies, at which point will have attained its greatest speed, and, consequently, its greatest momentum, and it is evident that it will rise the ascent B E, as long as the force of momentum is greater than the force C D; but the instant such force of momentum, which in this case is *an uniformly retarding force*, becomes less than the force C D, the latter would effectually operate, and the carriage, W, would roll back, and finally settle at the point B.

Supposing, however, that the momentum gained by the descent to B, be sufficient to advance the carriage as far up the ascent as the point H,—it is evident, that, could sufficient power be *then* employed, to overcome C D, the ascent H E would be made in much less time, with fewer revolutions of the wheels and axles, and with much less expense of power, than it would require to move up the whole ascent B E, as stated in Position 3.

We will now suppose, that an assistant power, equal to the available power C D, be employed to propel the carriage, W, along the undulation A B E, and that such power were withdrawn at the point B,—it becomes evident that, as *gravity alone* enabled the carriage to rise the ascent as far as H, which is more than one-half of the whole ascent, now that double power is employed, double momentum at the point B will be the result; and the power C D will thus effectually be opposed up the whole ascent B E. If this be true, how much more effectually will the power C D be counteracted, if the assistant power be continued up the whole ascent B E!

From this reasoning, it appears to me indisputable, as decidedly proved by experiment, that not only can a given load be conveyed along a curved line in *very much less* time than upon a horizontal plane, or *a very much greater weight* in the same time, but that loads which no locomotive power could move on the horizontal plane E A, would, impelled by gravity, assisted by other active power, descend down A B, and rise the ascent B E, with facility; and it will be also evident, that whatever power may be left on arriving at the point E, will be the power of ascending the farther incline E I; to which surplus must of course be added, the continued active power employed to oppose C D.

5. It must be remarked, that although the disposable *power* of gravity, in opposition to pressure, is only as C D to C P, yet this is no criterion of the extent of advantage gained in speed; in fact, CD may as properly be stated to represent the saving in friction: in whatever light, however, it may be viewed, C D represents a *constant* and *equal* power throughout the whole descent; but the spaces passed over down that descent, in consequence

of such power, are *not* equal in equal times, but, owing to accelerated velocity, as the squares of the times. Supposing, for instance, A V to be 10 yards, and the carriage was 1 second in reaching V, and allow the same space to be travelled over on the horizontal plane in the same time, at maximum velocity,—now, on the latter, the carriage would travel 30 yards in 3 seconds; but down A B it would travel 90 yards in 3 seconds; because $3 \times 3 \times 10 = 90$; and this velocity, although retarding up the ascent, if assisted by an equal power to that employed on the horizontal plane, would be so kept up, as to arrive at a given distance in far less time than it could do, with an average load, on the horizontal plane. Supposing, for instance, the horizontal line E A were 175 yards long, the descent A B 90 yards, and the ascent B E 90 yards, making the undulating line 180 yards, and that locomotive power were employed, sufficient to overcome the friction and the resistance of the atmosphere on both lines, and to move a carriage along E A, at maximum velocity, 10 yards per second: it is obvious, that the time required to travel from E to A, would be 17½ seconds; because $\frac{175}{10} = 17\frac{1}{2}$.

Let us now apply the same power to the same carriage travelling along the undulation A B E, and take 10 yards as the space travelled over in the first second down the descent A B: it is obvious, that it would reach the point B, or, in other words, traverse the 90 yards represented by A B, in 3 seconds; because, according to the laws of descending bodies, $3 \times 3 \times 10 = 90$. This being admitted, and even presuming that the power employed up the ascending part of the undulation, were *only just* sufficient to *overcome* the friction and resistance of the atmosphere, the carriage would naturally, as proved by the action of the pendulum, rise the ascent B E in the precise time it occupied in traversing from A to B. Hence, if a given power be employed, sufficient to overcome the friction and resistance of atmosphere, and to impel a load 10 yards in the first second, upon an undulating line, such as A B E, 180 yards in length, the whole distance, if the power be constantly kept up, will be traversed in *less* than 6 seconds; whereas, if a given power be employed, sufficient to overcome the friction and resistance of atmosphere, and to impel a load 10 yards in the first second of time, *at maximum velocity*, upon a horizontal line, such as E A, 175 yards in length, the whole distance cannot be traversed in less time than 17½ seconds.

Thus, if we ascertain the maximum velocity at which a body can be impelled upon a horizontal line *in the first* second, and down the descending part of a given curve in the first second, such power being sufficient to overcome friction in both cases, the comparative time occupied in traversing each distance, is easily determinable; the difference in advantage varying in proportion to the length and depth of undulation, as compared with the length of the horizontal line. Nor must it be overlooked, in considering this subject, that a much greater load can be conveyed along an undulating line than along a horizontal one. The axle and rolling friction to be overcome, is necessarily less upon the former than upon the latter, and the fulcrum presented to the effective power of steam, down the descending part of each undulation, is a most important object of advantage. It will be seen, that in this explanation I have calculated the velocity of a body traversing a curve, according to the laws which would govern its descent down a regular inclined plane; there would of course be some difference, but in this instance it cannot be material to describe it.

ENUMERATION

OF

THE ADVANTAGES WHICH I PREDICT WILL BE DERIVABLE FROM THE ADOPTION OF UNDULATING OR SERPENTINE RAILWAYS.

1.

A great gain in speed.

2.

A great saving of power.

3.

A great saving in the expense of excavating, levelling, &c.

4.

The much greater facility with which inclined planes can be ascended.

5.

The strength of the rails, and, consequently, their cost, will be considerably diminished, or, if not diminished, there will be a great saving in their wear.

6.

That lighter and less expensive locomotive engines may be advantageously used.

7.

That much greater activity can be given to the effective power of steam.

8.

That the motion of the carriages will be easier, and more free from vibration, than upon a horizontal Railway.

9.

That the carriages may be attached together by moveable joints, instead of chains, so as to obviate the disagreeable jolting which is now so frequently experienced.

10.

That serpentine or undulating Railways can be laid down with advantage across a country where horizontal ones would be altogether inapplicable.

11.

That the necessity of crossing turnpike or other roads, will be obviated; as bridges could, in most cases, be constructed, over which the carriages might proceed, without the employment of stationary engines; and canals might be crossed in a similar manner.

APPENDIX.

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APPENDIX.

Copy of Mr. ROBT. STEPHENSON, *Senior's, First Report to Mr.* BADNALL, *on the subject of his Undulating Railway.*

"*Pendleton Colliery,*
"*Sept.* 11, 1832.

"Mr. BADNALL.

"SIR,

"SINCE I had the pleasure of your "company yesterday, I have been very much en-"gaged; but, according to your request, the time "I have had to spare, has been employed in "trying to establish some true cause why the un-"dulating Railway will be superior to the hori-"zontal one. To make it short, I will suppose a "carriage, four tons weight, regularly distributed "amongst the four wheels, and placed on the level: "now, if we hang two plumb-lines from the centre "of the axles, by their own gravity they will pass "on the inside of the centre of that part of the peri-"phery of the wheels in contact with the rails, and "each plummet-weight will apply its power to turn "the wheels; but they being applied in opposite "directions, the carriage will remain stationary.

"Now, let us lower one end of the same carriage "to the desired angle of the descending part of your "road; the plumb-lines being still attached to the "centres of the axles, and acting by the laws of "gravity, they will pass considerably on one side of

"the centre of that part of the periphery in contact
"with the rails; the great portion of the weight on
"one pair of the wheels is therefore taken off, and
"conveyed towards the other *axles;* and it is my
"opinion, that four tons are not now resting on the
"four wheels as before, but a portion of the whole
"is called into *motive* action, and will proceed
"down the incline, according to the laws of falling
"bodies, to the foot of the plane, with this *addi-*
"*tional* advantage,—the wheels being now in motion,
"will call into action the power of rolling force*,
"which will begin near to the resting part of the
"periphery of the wheels in contact with the rails.
"This being allowed, viz. that the force begins at

* Since this Report was written, I have had various conversations with Mr. R. Stephenson, senior, on the subject of the *force* to which he here alludes, and I confess myself inclined to attach considerable weight to his opinion. If I may be pardoned the invention of a word, I would term it, "*periphfugal force;*" inasmuch as it commences at the point where the wheel is in contact with the rail,—it increases or gathers to the extreme part of the wheel, and is all thrown off between that extreme part and the point where it again comes in contact with the rail. To demonstrate its effect, Mr. Perkins suggested that a wheel, *not* in contact with the earth, should be put in very rapid motion, and, when in such state of motion, be allowed to fall with its periphery in contact with a level plane; the result would be, that although no power were employed to urge it in a forward direction, it would continue to roll along the plane until motion ceased; evidently proving, that the centrifugal force, equally distributed through every particle of matter in a fly-wheel, is very different in effect to the force gathered and thrown off when such fly-wheel, in a state of rapid motion, comes in contact with the earth: in the latter case, the *point of contact* should be regarded as an ever-changing centre, from which the force originates, without any regard to centrifugal force, as commonly understood in reference to a revolving wheel. Such, at least, are my present, perhaps imperfect, ideas on this subject.—R. B.

" this point, it will certainly increase its power to " the extreme part of the wheels; the force will " commence throwing off immediately after passing " this extreme point, and will be all discharged " between that point and the rails, when it becomes " *inactive*. By this I mean, that the wheels are con- " stantly taking up power *on one side*, and throwing " off on the other, nearly between that part of the " wheel which is moving fast, and that which is " moving slow; in consequence of which, it will " have a great tendency to raise the wheel from the " rails, and, at the same time, will assist in the " progress of the forward direction; it will also " prevent the wheels from being pressed so much " *oval*, and indented so far in the rails, and *very* " *much reduce the friction*.

" This sort of force, perhaps, not being thoroughly " understood, you will allow me to compare it to " a man on horseback, riding at a great speed, and " the animal stopping himself with all the power he " is master of; we should, in such a case, naturally " expect to see the rider thrown forward, taking " along with him both bridle and stirrups.

" My time being so limited, I cannot at present " proceed further on the subject; and, in the interim,

" I am,

" Yours, respectfully,

(Signed) " R. STEPHENSON, Sen.

" P.S.—It has never been my opinion, that falling " bodies have been exactly attracted to the centre " of the earth.—R. S."

Second Report of Mr. R. STEPHENSON, *Senior.*

" *Pendleton Colliery,*
" *Sept.* 13, 1832.

" MR. BADNALL.

" SIR,

" IN my Letter to you on the 11th inst. " respecting your Railway, I think I left off at the " bottom of the first inclined plane. At this part, " the carriages will have attained their greatest " speed; the wheels will also be acting with their " greatest power; I mean as to rolling force. Here " it would require too much of my time to calcu- " late the required strength of the rails over this " part, and particularly at the beginning of the " next angle; but I will venture to say, the load " will not be so heavy on the rails, as many, at " first sight, would anticipate. It will be impos- " sible for me to tell exactly, until I have ascer- " tained, what power the wheels throw off at dif- " ferent speeds. I assure you I intend to try some " interesting experiments on this grand motion, " which, I trust, will turn out valuable. I think " I have just hit upon a plan which will prove it " mathematically correct. You will be aware that " this requires great nicety, and time.

" But to return to the carriages passing *up the* " *ascent*:—the wheels will be still gathering and " throwing off their power, as before stated, in the " descent; that is, between the rails and the ex- " treme part of the wheels, which power, you will " perceive, still assists in a forward direction.

"The weight of the carriages will now be thrown "behind that part of the periphery resting on the "rails, and the power of hauling, or propelling, will "be applied near the centre of the axles, when the "weight of the same will be raised, as if it were "with a fine tapered wedge, or screw, constantly "in motion.

"Now, I must return again to the periphery, or "prop, in contact with the rails; also, the plum-"met-line must be attached, as before, to the axles, "and watch the change of the periphery in contact "with the rails, where Nature seems to be using "every effort to run out from under the weight, yet "the carriage keeps gently rising. This regularity "of ascent will prevent the possibility of there "being time for falling bodies, in opposition, to be "called into action. The props, or parts of the "wheels in contact with the rails, are changing in "less time than can be measured by the finest chro-"nometer, yet the mind is capable of conceiving, "that stand sometimes they must. Now, according "to this reasoning, it is clear, that a portion of the "weight will be suspended in the atmosphere.

"In all revolutionary motions of this kind, the "whole of the power gained by the wheels in the "descent, will not be destroyed in ascending the "rising plane.

"I will now proceed to the top of the hill:—when "the first carriage passes over, it will move more "slowly than the rest of the train, until it has com-"pleted the greatest bend, which will be when the "centre carriage has reached the highest summit.

"At this time, the greatest strain will be applied to "bend the rails, and straighten the cord of hauling; "the wheels and axles will have made more revo- "lutions than upon a horizontal line, and the joints, "or couplings, will have rubbed a little more in "passing the segment at the bottom, and the back "of the curve at the top, &c. &c.

"After deducting the whole of the disadvantages, "it is my opinion, that there still will be a great "deal of power left in motion; and after mounting "the second summit, with the same locomotive "power still applied, the gain will be increased. "According to these laws, it will require even a "longer Railway than the Liverpool and Manchester "one, to prove the extent of its value.

"I am extremely sorry that I am at this time "so much engaged, or I would have entered more "minutely into the subject.

"Yours, very respectfully,

(Signed) "R. STEPHENSON, Sen.

"P. S.—The rails in the angles will require to be "lighter, which you can calculate yourself, with the "friction of the atmosphere.—In great haste.

"R. S."

Letter from Mr. ROBERT STEPHENSON, *Senior, to the Editor of "The London Journal of Arts and Sciences."*

[First published in 1826.]

" SIR,

" THE Specification of my Patent for " Axletrees, intended to remedy the extra friction of " carriage-wheels when passing along curves upon " Railroads, having been enrolled, and, I presume, " about to appear in your Journal, I request per- " mission to communicate to the Public, through " your medium, a few observations relative to the " inconvenience of friction which carriages now in " use labour under, when proceeding along curves " upon a line of Railway; which observation will, " I consider, shew the necessity of an invention of " the kind, and its usefulness.

" Waggons that have hitherto been used, are of " such constructions, that when passing curves in " the Railroad (if the curve be not even more than " two feet in twenty-two yards), the friction is so " great, that it requires nearly double the power " to propel the carriages that is necessary to pro- " duce the same speed on a straight line. It must " therefore be evident, that the extra power em- " ployed has the effect, merely, of grinding and " wearing away the waggon-wheels and rails; va- " rious schemes have been put in practice to pre-

"vent these inconveniences, and each has proved "ineffectual. Wheels have been used on Tram-"roads, running loose upon fixed axles, but they "have proved unsuccessful; for this reason—they "cannot be kept steady, nor can they be prevented "having play. I wish it to be understood, that "there is a great difference between the edge-rails "and Tramroads; the former being but two inches "and a quarter broad, the latter from four to five "inches. It must be known to engineers, that "wheels of a large diameter run with much less "friction than those of a small diameter. It is my "opinion, that a carriage that is to travel at the "rate of six miles an hour, ought not to have wheels "of a less diameter than three feet. If it be wished "to increase the speed of a waggon running on "Railroads, it must be evident, that increasing the "size of the wheel will do it. Suppose we take it "on an average that the wheels be four feet in dia-"meter, as the speed for carrying goods and pas-"sengers is wished to equal that of the coaches,—"the play that will soon take place in the loose "wheels, will allow them to vibrate and spread not "less than one inch and a half; and it is well known "that Railroads cannot be kept to that guage, "without sleepers or bearers, extending from rail "to rail, in order to bind the road together; and it "will also require an extra number of men to keep "the road in order.

"Carriages with loose wheels are not at all cal-"culated to rise and fall with the many irregularities "of the road they must meet with, proceeding from "various causes, such as the blocks being sunk, by "the embankments giving way, &c. &c., as their "axletrees must be firmly fixed to the body of their "carriages. It will be doubtless the case, that, "when the carriage meets with the hollow parts in "the road, it will be resting on three wheels, and "the fourth will most probably be lifted higher than "the depth of its own flange; therefore, if the car-"riage be travelling round a curve with the hollow "inside, it must inevitably be thrown off the road.

"In consequence of these inconveniences, loose "wheels have been entirely abandoned on the edge-"rails, and those that are wedged firmly to a rota-"tory axle have been adopted, and appear to be "far superior to the loose ones. In straight lines, "the waggons now in use may suffice; but when "they meet with curves of six or eight feet to a "chain (or twenty-two yards), the friction that then "takes place is enormous.

"I can with confidence say, that the carriages "above alluded to, in passing these curves, will at "least grind off the top of the rail one-sixteenth of "an inch in twelve months. The thickness of the "top part of that kind of rails generally in use, is "only about half an inch, and those are on the "most improved principle; therefore, it cannot be "disputed when I say, that in four years the rails "in these curves will be reduced below the standard "strength required for the passing of the loaded "waggons.

"The enormous expense thus occasioned by this "extra friction, will be seriously felt by proprietors

" of Railroads; the malleable iron Railroads not " having been long enough in use to prove to the " Public the real time they will last, and the dis- " advantages before named have not been yet fully " ascertained by proprietors. The only malleable " iron Railroad that has been used for any length of " time, is that which extends from Lord Carlisle's " Colliery to Brampton, in Cumberland, about ten " miles in length, and has been made upwards of " ten years. The straight line of Railroad appears " not to be much reduced, but the smallest curve " lines have been replaced many years ago. From " these circumstances I am led to believe, that a " straight line of Railroad, where there is a great " traffic, will not last more than forty years; and " those of sharp curve lines, not more than four, " with the waggons now in use.

" These disadvantages induced me to direct my " attention to the construction of a waggon that " would obviate these difficulties; and the one I " have made will, I doubt not, overcome them.

" The wheels on my waggon are firmly wedged to " the axle, and yet will roll round the sharpest " curve without any additional friction from the " sliding of the wheels. Carriages of the ordinary " construction that travel on Railroads, having one " side more exposed to the heat of the sun than the " other, must have their wheels soon worn to a " smaller diameter, by their being exposed to a " greater heat; every practical scientific man know- " ing, that wheels exposed to the south, wear away " much sooner than those exposed to the north;

" but the two wheels being wedged upon one axle, " as in the carriage at present used, the friction will " be much increased, even if the carriage be moving " on a straight line, in consequence of the wheels " that have not been worn, travelling over a greater " surface than the others.

" Each wheel on my carriage is so adapted, that " it will revolve with its own axle, and every wheel " will travel over the surface required, either in a " straight or curved line, without any increase of " friction, even though all of them should be of " unequal diameter.

" There are still many disadvantages not yet " mentioned, which Railroad carriages now in use " labour under; for when they are lightly laden, " and are moving at the rate of five or six miles per " hour, and come in contact with sharp curves, they " are generally thrown off the road; and should " they be precipitated to the bottom of the embank- " ments, which are in some places fifty or sixty feet " high, the consequences might prove dreadful, and " the expenses great, for the carriages would doubt- " less be much injured. Suppose (which most likely " would be the case) that there were passengers at " that time in the waggon,—they must some of them, " if not all, be killed upon the spot. My improved " carriages will remove all the above-named diffi- " culties, and passengers may travel with the greatest " safety.

" These carriages have been tried before scientific " men, and have answered beyond all expectation; " they will not only move round curves with the "same ease as on a straight line, but will travel "over the hollow parts of the Railroad that may "have sunk, in consequence of the embankments "having given way, with perfect safety.

"I am, Sir,

"Yours, &c.

"R. STEPHENSON."

THE END.

E. H. Blagdon, Printer, Bow-street, Covent-garden.

Nineteenth Century views of Leek, Staffordshire

THE

MECHANICS' MAGAZINE,

MUSEUM,

Register, Journal,

AND

GAZETTE,

OCTOBER 5, 1833—MARCH 29, 1834.

VOL. XX.

"——— we must deem the mode
In which Sir Isaac Newton could disclose
  Through the unpaved stars, the turnpike-road,
A thing to counterbalance human woes:
  For ever since, immortal man hath glow'd
With all kinds of mechanics."

BYRON.

LONDON:
PUBLISHED BY M. SALMON, MECHANICS' MAGAZINE OFFICE,
NO. 6, PETERBOROUGH COURT.

1834.

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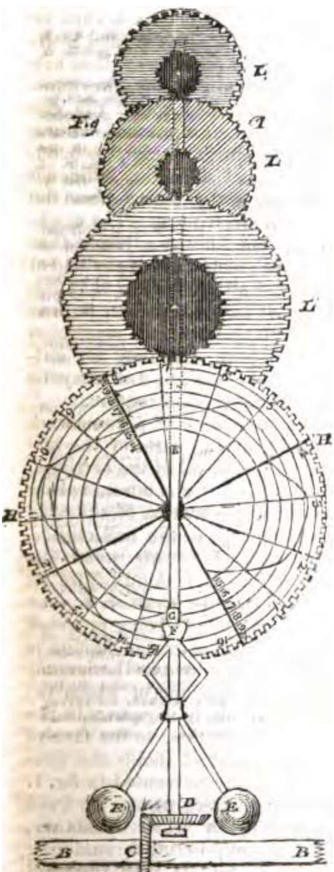
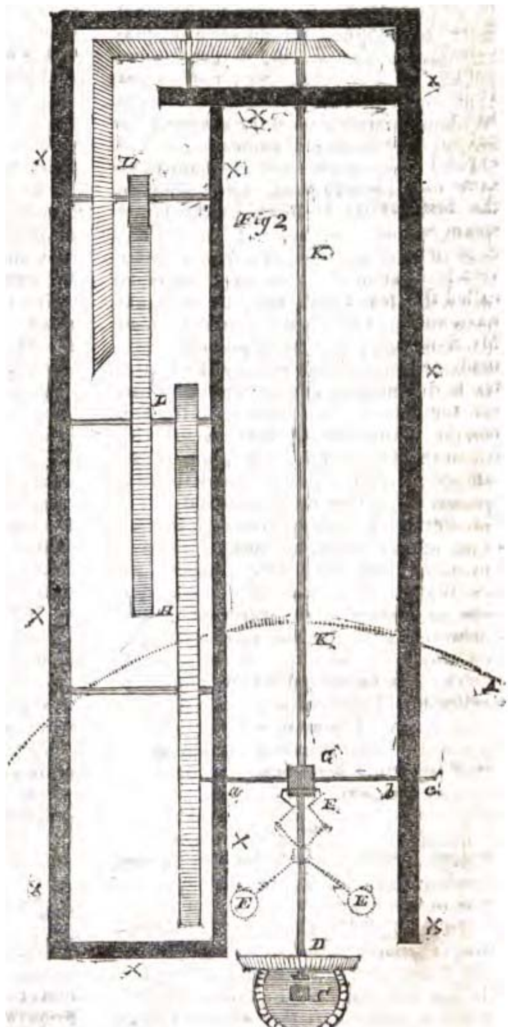
Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 530. SATURDAY, OCTOBER 5, 1833. Price 3d.

SPEED REGISTERING APPARATUS.





lively sketch of the simultaneous attack of the whole concentrated beggary of Boyle on a carriage and four; his strictures on the failure of the attempt to force the growth of a linen-manufacture in Ireland; or his ludicrous remarks on the deplorable "state and condition" of most of the inns he put up at; his pleasant picture of a market-day at Strokestown; or his description of the manufactory of tobacco-pipes, at euphonious Knockecroghery; his exposition of the mysteries of the trade in turf, gleaned from the authentic source of conversation with a *bona fide* turf-cutter; his suggestions for the improvement of inland navigation, especially that of the Shannon; or one of his elaborate enumerations of chimneyless and windowless cabins, more comfortable cottages, or most comfortable three-storey slated houses, in each of the towns and villages. All of these might well have been extracted, for they are all equally distinguished by quickness of observation, usefulness of purpose, apparent correctness of detail, and, above all, by that racy spirit of good-humoured philanthropy which runs through the whole work, and makes the perusal of it very delightful. If we could have extracted all of them, accompanied with some half-dozen pages of tables of the population, number of schools and scholars, amount of crime, and a thousand other matters, from the concluding portion, we might have done justice to the "Statistics of Roscommon."

THE UNDULATING RAILWAY.

Sir,—I feel suprised that Mr. Badnall, the inventor of the Undulating Railway, has hitherto met with so few supporters amongst your numerous correspondents. It argues a want of courage on the part of those (for some there must be) who privately acknowledge the general truth of his theory, though they cannot, perhaps, but perceive in it some imperfections. No theory is found perfect on its first development. We know that silver is contained in the ore; but in order that it may be fit for currency, it must undergo the processes of smelting, refining, and stamping. Diamonds are not despised because they may chance to have been first discovered in a rough state: a little cutting and polishing, soon determines their intrinsic value; and with regard to mathematical propositions, that which at first may appear, to prejudiced minds, a *fallacy*, may in process of time be universally received as a genuine truth; as in the case of Galileo's doctrine of the force of gravity, for which, in spite of the clearest experimental evidence, he suffered all the misery that the diabolical malice of his enemies could inflict.

Having satisfied *myself* sometime ago, of the truth of Mr. Badnall's undulating theory in its general form, I can no longer resist the desire of expressing my humble opinion in your Magazine, if possible *before* certain experiments are tried on a practical scale, which I understand are now contemplated, and which I think are highly necessary for the satisfaction of those whose money is about to be extensively embarked in attempts to reduce the undulations of nature to a dead level, or to the smallest attainable inclination. In doing so I shall not set myself up as the thick and thin advocate of Mr. Badnall, for I dare say that gentleman can get through very well without my—anything but powerful—aid. On the contrary, should I happen to differ from him in opinion on minor points, I shall not scruple to say so. But on the main point, namely, *the available nature of the force of gravity for the horizontal projection of bodies*, I quite agree with him, and hope to see it ere long palpably exhibited to the wondering, yet doubting, gaze of the new-fledged race of railway engineers.

Delicacy in the discussion of mathematical subjects, which are or ought to be founded on the most rigid truth, is a thing not to be expected; but I must also add, that I think *coarseness* equally unnecessary, and more injurious in its operation. As one of your correspondents, I would fain steer clear of both the one and the other. I should wish to be not only manly, but *gentle*manly, in the expression of my sentiments; and, Sir, if you find me tripping, pray shew me no mercy: you cannot offend me by the exercise of due castigation even for the *first* offence of this description. It comes entirely within your jurisdiction, in the same way as the preservation of order in the House of Commons belongs to the Speaker.

In writing a private letter on this subject about five weeks ago, (previous to which I had not seen your Magazine for the then current month, as I get it only in monthly parts,) I endeavoured to lay down some general principles as a foundation for future reflection or argument; and though there may be little or nothing original in my ideas, to those of your readers who have thoroughly studied the subject, I will give you a transcript of what I then wrote, in the hope of being set right where I may have had the misfortune to get wrong. I have not since had an opportunity of improving upon those ideas, or I should gladly have availed myself of it, being ever open to conviction. They are as follows:—

"I can perceive, or fancy I perceive, *three* kinds of force in general operation, though they may, perhaps, by mathematical reasoning be resolved into *one*. I mean, *permanent pressure, momentary impulse, and mere obstruction*. These may also be called three distinct or visible modes of existence or application of 'force.' Pressure, being constantly and permanently applied, produces a velocity uniformly accelerated according to the times; that is, if unopposed by any contrary pressure, impulse, or obstruction. If opposed by contrary pressure of inferior force, acceleration will still continue, but will be diminished in the ratio of the one force to the other. Thus, the force of gravity being opposed or deflected from its perpendicular direction by an inclined plane, produces a diminished increment of velocity, varying as the sine of the angle of inclination. When the angle is equal to 90°, the opposing force of the plane becomes a maximum—it amounts to a total obstruction.* When the inclination of the plane is reversed, it permits the force of gravity to act in the opposite direction; and in an ascending plane, succeeding a descending one, the deflected momentum upward becomes absorbed or neutralised in the same ratio as it was acquired during the descent. In this case the force of gravity becomes a constantly retarding force; it is '*permanent pressure*' acting on a rebounding body, and destroying its own previous effect.

"Some kinds of power, which may be deemed of the class included in the term *pressure*, are, in fact, a succession of almost momentary impulses, which require a given time—say half a second—for their repetition. For instance, the steam-engine, whose power is governed by the opening and shutting of the valves, and which is limited by the friction of the piston and other moving parts—such a machine soon attains its maximum of accelerated velocity. It is a very imperfect substitute for the comparatively infinite power of gravity. I mean, infinite in *constancy* and *permanency*, not in *amount*. Gravity is permanent and constant with regard to *place* as well as *time*; it possesses the property of *ubiquity*. The fly-wheel of the fixed engine tends to convert the impulse of the steam into a permanent pressure at the point of application, and the duplicate piston of the *locomotive*, by its alternate action, has a similar effect; but in the most improved shape this kind of force is only a succession of impulses, owing to the artificially reversed action of the steam, which cannot be done instantaneously. The unfolding of a spring appears to me to be the best mechanical exhibition of permanent pressure, so long as it lasts; but this kind of force is necessarily of very short duration.

"When permanent pressure (as the force of gravity) is opposed by a contrary momentary impulse, a portion of the acquired velocity of the body acted upon becomes lost or abstracted; and this portion will be equal to the effect which that impulse would have had on the same body at rest. This opposition being overcome by the constant pressure of the greater force, acceleration continues as before, though with the diminished space (reckoning from the commencement of the time) occasioned by the contrary impulse. Suppose a large body descending an inclined plane with great velocity, and that a man attempts to stop it by the stroke of a hammer, although he cannot altogether arrest its progress, he deprives it of so much of its momentum as he could have imparted to it in the upward direction, if supported at rest upon the same plane; and a rapid succession of such blows, from a number of men placed below each other, might

* On reading over and considering the latter part of this paragraph commencing at "thus," I feel surprised that I should have ever considered the inclined plane as an *active* power capable of permanent pressure *upward*; it is certainly no more than a *mere obstruction* to the permanent pressure of the force of gravity *downward*. On a dead level the obstruction is total; at any intermediate angle it is only partial; down a perpendicular plane (free from friction) there is none whatever.

have the ultimate effect of reversing its motion.

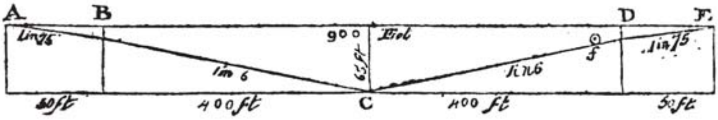
"But if what is termed a 'scotch' be placed in the way of a carriage descending an inclined plane, I should call this kind of opposing force simply an *obstruction*. What is technically termed 'rolling friction,' I conceive to be a constant and close succession of minute obstructions, having a tendency to diminish the increment of acceleration on a descending plane (as well as to increase the retardation on an ascending plane) in the ratio of its amount, compared with the force of gravity on the same plane. Its effect on a regularly formed horizontal plane, when opposed to the effect of a single impulse, or to the momentum acquired at the *cessation* of temporary pressure, will be *uniform retardation*, the velocity becoming diminished directly as the times. Opposed to the most perfect mechanical permanent pressure (a spring uncoiling itself) it will diminish the increment of acceleration as well as its maximum velocity; and this effect will be more visible when opposed merely to a rapid succession of impulses, as in the case of a locomotive steam-engine, or the force exerted by a man or a horse. The effect of all animal or mechanical power is limited by the imperfection of the machine, or the friction and inconstancy of its moving parts; and *if* we could invent any artificial power as active and constant as the force of gravity, it is clear that a uniformly accelerated motion, *for any length of time*, would be as possible on a horizontal as on an inclined plane; but not otherwise.

"When the force of gravity is assisted by another distinct pressure acting constantly on the descending body, the increment of acceleration will be proportionably greater; not, however, in the ratio of the two forces *multiplied*, but simply *added* together. When assisted by a rapid succession of momentary impulses the effect will be *similar*, but not so great as will be produced by the constant pressure of the same force acting equally on the *greater* as the *less* degree of velocity. The force of gravity being constantly present with unabated energy, acts with uniform effect upon a body, whether descending with a velocity of 100 or 1,000 feet per second; but as no artificial force can produce any additional velocity beyond a certain maximum, which is soon attained, it follows, that as the velocity of the moving body, urged by the force of gravity, increases beyond that maximum, the accelerating effect of the artificial force diminishes till it is altogether lost,—the motion of the machine being unable to keep pace with the body which it would propel, unless *dragged* along with it; and then it assumes the shape of an *obstruction*, instead of proving an 'assistant power.' As when a plane is so much inclined to the horizon that the velocity of a locomotive engine travelling thereon becomes greatly increased beyond that rate which gives to the pistons a motion of 4 feet per second, the moving parts of the engine then become an obstruction to the force of gravity, unless the wheels can be thrown out of gear, so as to permit the axles to revolve freely.

"A descending body, assisted by a momentary impulse or temporary pressure, retains the additional momentum acquired not only through every part of its descent, but also in its deflection upward (if diverted up an ascending plane), until the force of gravity becomes neutralised by reversation, when the effect of the impulse or temporary pressure will remain alone visible, until that also is absorbed or neutralised by the opposing force of gravity.

"A few words more upon friction. The obstruction termed '*rolling friction*,' must undoubtedly be greater to an ascending than a descending body, yet the mean amount of this *minor* division of the friction of a wheel and axle (taking both directions) would be the same on an inclined as on an horizontal plane, if the length were the same; but upon the same base the former would be somewhat more than the latter, being of greater length. The '*rubbing friction*' of the axle revolving in its chair, (which is merely a portion of the friction or obstruction presented by the surface of the plane, transferred to the moving parts of the machine employed to overcome that obstruction,) is *diminished*, either upon an ascent or descent, *in proportion to the square of the sine of the angle of inclination*; the perpendicular pressure of the load being decreased in the same ratio. But allowance must be made, on the other hand, for the additional length occasioned by the inclination."*

Having given those ideas, which I have attempted to dignify with the name of "general principles," for the amusement or instruction of your readers, I will now transcribe from my copy of the same epistle a diagram and calculation founded upon the supposition that a lateral branch is to be made from some existing railway to the opposite side of the valley through which it runs; or that a hollow, like that where flows the Sankey, is to be crossed by a main line:—



Calculation.—Suppose we have 4 tons of goods in a carriage weighing 1 ton, at the point A, which are to be conveyed to the point E in the shortest possible time:—The friction on a level plane being equal to 50lbs., it would require the utmost power of *a horse* to convey the weight at the rate of 15 feet per second (about 10 miles an hour) $= 66\frac{2}{3}''$ for the whole distance of 900 feet, allowing only half the rate from A to B and from D to E. But on the inclined planes it will require only *two men* to give the same carriage and load a velocity of 15 feet per second, by running with it from A to B (50 feet), on the descent of 1 in 75. On arriving at C, the velocity will have increased to 75 feet per second, by the action of the gravitating force, notwithstanding the obstruction presented in the shape of friction; this momentum will carry the load up to *f*, being but a very short distance from D;* and no part of the impetus given to the load at B being yet expended (the force of gravity having alone done all the business from B to *f*), it follows, that the distance from *f* to E will be accomplished without any further expenditure of manual or mechanical power; because if the inclined plane D E immediately succeeded the inclined plane A B, the force of gravity alone on the descent of 1 in 75 would overcome one third of a similar ascent in spite of the friction; and as the additional momentum given at B, by the action of the men, is considerably more than three times as great as that which would have been acquired by gravity, I contend that the whole ascent to the point E would thus be accomplished; but if any doubt remain, increase the momentum at B till it be sufficient. In this case I calculate upon a saving of more than one half in point of time; and that the expense of the assisting power will be as 20 to 1 in favour of the inclined planes.

It would certainly give me great pleasure to see an experiment tried in the way represented by the preceding diagram, and upon the *full* scale. I have no doubt it would give general satisfaction, as it would be impossible to play any trick for the purpose of procuring an unfair result. There would be simply the wheels and axles of the carriage to examine, the load to determine, and the two men to be kept in check—but they must be good runners for a short distance. With a locomotive engine there may be a good deal of chicanery prac-

* The great diminution of rubbing friction by acceleration of speed should also be borne in mind; I think, however, it can never be *thus* reduced to a less amount than the rolling friction of the same carriage. When the one becomes equal to the other, as might be the case in descending an inclined plane by the force of gravity, the maximum of *speed*, combined with *safety*, would be undoubtedly attained; as a further increase of velocity would absolutely cause the carriage to jump from the rails. This idea is the result of a conversation which I recently had with Mr. Robert Stephenson, of Pendleton, but whether it is precisely the same which he intended to convey or not I cannot say.

One of your correspondents (an engineer), says, (page 242), "The pressure against the inclined plane will be to the pressure on the level as $\frac{b}{L}$ is to 1; *b* representing the base, and L the length." In this notion he is quite wrong, as it is easily demonstrable that the pressure will be *as the square of the cosine of the angle of inclination*; and its diminution, as the square of the sine of that angle. He should therefore have adopted the formula $\frac{b2}{L2}$. The notions of your correspondent "W. W." on this subject are too vague to admit of serious refutation.

* I leave it to others to calculate how far *f* falls short of D in their own way;—on this hinge chiefly turns the dispute. The other *hinge* is the value of the remaining momentum at *f*.

tised; or there may exist accidental causes of defective performance without the least *intention* of fraud. But I anticipate a perfectly fair result from the experiments about to be tried to which I have before alluded.

It must be admitted that a great expense might have been saved in that immense erection called the Sankey Viaduct and its connecting embankments, had it been deemed practicable to descend 65 feet and to rise again, without loss of time or danger to passengers. With such a descent the greatest velocity would be about 50 miles an hour; but that would only continue for two or three seconds, and, in my estimation, would form one of the chief pleasures of the ride. It appears from Mr. Badnall's description of his models and experiments, that he chiefly contemplated a succession of *small* undulations from one end of the line to the other, producing by that means increased velocity with the same quantum of artificial power. But the saving of expense in the *construction* of railways where numerous hollows occur, is, to my view of the principle, a very prominent feature in its economical application.

Mr. Badnall certainly carries his theory too far, when he talks of semicircles and angles of 45 degrees, and challenges the whole scientific world on the strength of it. If I had been the author of the undulating railway I should not have ventured to have gone quite so far; but in doing so, he undoubtedly looks only at the *theory*, forgetting how necessary it is to have regard also to the practical application of a discovery, of which the unmathematically-thinking public are called upon to judge as to its economy and utility. Mr. B. is a little mistaken, too, in referring to the principle of the pendulum in support of the doctrine developed by the diagram, page 261. The falling body would not reach the several points in *precisely* equal times according to that principle; because the vibrations of pendulums in circular arcs of different magnitudes are not *isochronal*. Neither, therefore, would the body rise to the respective points on the horizontal or tangent line of that diagram in *precisely* equal times. It follows, then, that the angles of 45° (represented by *l* A K and A *l k*), are not indisputably the best angles of inclinations for the ascending or descending lines of the curve, inasmuch as the body is *not* found to traverse the distance A *l* in precisely the same time as A *i*. But, on examining this demonstration a little closer, it appears to me that Mr. Badnall fails to prove his proposition that *l* is the farthest possible distance along the horizontal line to which a body (friction not being considered) can be moved by *gravity* alone. If it be meant, *in any given time*, the case is altered; but the proposition is not so expressed. The diagram shews most clearly the *isochronal* descents from A to any point in the perimeter of the circle, according to the acknowledged laws of the inclined plane; but I cannot perceive how it accords with the principle of the pendulum; the *isochronal* vibrations of which are (theoretically) performed in portions of the *cycloid*, a curve incommensurate with some of the double inclinations represented in Mr. Badnall's friend's diagram. Supposing a body to traverse a circular arc proportionate to the inclined plane A *f*, and another ascending from *f* to *l*, I question whether it would not be accomplished in less time than it would require to traverse circular arcs proportionate to A *k*, *k l*; in other words I esteem a circular arc of 60 degrees, which is equal, by its chord, to an inclination of 30 degrees, better for *speed*, and, of course, in every other respect, than the quadrant of a circle, of which the chord is inclined at an angle of 45 degrees, and of which the first portion of the descent may be considered as absolutely perpendicular.

Many of your correspondents lay great stress upon the fact, that the horizontal line is *shorter* than the line of undulation, and therefore, unquestionably, more economical both with regard to time and power. In answer to these straightforward objectors to Mr. Badnall's theory, I beg leave to say, that the *right line* is not always the *swiftest* line of passage from one point to another, though to common observers it may appear so; and I shall adduce as an instance the *cycloidal curve*, which is admitted by the most eminent mathematicians to be the line of quickest descent from one point to another not situated in the same vertical line. This was the celebrated problem of John Bernoulli, called by him the *Brachystochronon*. It was solved by

Sir Isaac Newton and others, and subsequently has been reconsidered by Venturi, who has also determined that an arc of a circle, not exceeding 60°, effects a speedier descent than any other curve which can be drawn *within* the same; and that the arc of 90° is a curve of speedier descent than any which can be drawn *without* the same. It is almost needless to add, that the cycloidal curve is *between* the two, but much nearer the former than the latter.

To sum up what I have further to say on this subject, I will conclude with the transcript of another passage from my before-mentioned copy of a private letter:—

"In addition to the saving of original outlay, I conceive that the chief part of the working advantage of the undulating railway consists in using the natural force of gravity as *a swing*—carrying the load to a considerable *distance*, but not to quite so great an *elevation*, in consequence of the obstruction arising from friction; and in being able thus to swing a *large* weight over a hollow with almost as little expense of artificial power as would be required for a *small* weight. A further advantage consists in the diminution of friction by diminution of perpendicular pressure, so that the force of gravity has more effect than common calculation would lead one to expect. The remaining part of the advantage arises from the power of transmitting the effect of an impulse from one side or summit to another, without the necessity of that impulse being repeated; as when a lad is engaged in swinging his companion, the small impulse which he gives the swing not only carrying the centre of oscillation *higher* than his hand, on the opposite side, but causing it to return again with sufficient velocity to reach the manual power by which it was first propelled, again to receive the apparently inadequate impulse.

"In using locomotive steam engines there would be an additional advantage from the economy of fuel and generation of heat during the action of the force of gravity. Where the undulations are considerably smaller, say one-tenth of the dimensions given in my diagram, I should think a locomotive engine may constantly exert its power throughout the whole descent and ascent, with advantage."

But, I should have added, the inclination of *one in six* is not a good proportion for general undulations: it was merely adopted in the diagram to shew the *extreme* use to which the undulating principle might be applied, under the consideration that six inches in a yard is a very common natural slope in a hilly conntry, taking the section laterally from the bed of a river. Perhaps for an uniform succession of undulations throughout a whole line, the proportion of twenty feet to a furlong would answer better than one that is more inclined. Thus, the distance from summit to summit would be a quarter of a mile, and the average velocity of 30 miles an hour might be maintained with the locomotive engine; varying from twenty at the summit, to forty at the bottom, of each undulation.

I am, Sir,
Your obedient servant,
HENRY SANDERSON.

Sheffield, Sept. 3, 1833.

P. S.—I have not yet seen your last Monthly Part.

MESSRS. MACERONE AND SQUIRES' STEAM CARRIAGE.

We extract the following additional particulars of Colonel Macerone's late steam trip to Windsor from a letter which has appeared from that gentleman in the "Morning Chronicle," explanatory of the account of the excursion, which we extracted from the "Observer:"—

"We stuck fast for some minutes on the bridge in *ten* inches (not 'four') depth of shingles, not for want of power, but from having only one wheel locked to the axle; the lever to move the clutch into the other wheel was wanting. The one wheel spun round in the shingles; both would have held. In fact, but the one only would have held, as was evinced by our rapid return over the same bridge, which was as deeply laid with shingles on one side as on the other, and they were actually laid during the interval of our first passing and our return. * * * In our journey to Windsor and back (48 miles), we did not consume so much as five sacks of coke, which, at 2s. per sack, *the retail price*, makes 10s., the expense of propelling a carriage which is capable of carrying many more passengers, besides luggage, than a four-horse stage coach. * * * What the cost of the repairs of a horse-coach amount to I do not know; but this I know, and can also prove that we have run our steam coach above rivers in that country, for purposes of irrigation. The rivers there have in ordinary seasons lofty embankments, between which they run with an almost imperceptible current. The embankments are generally thirty or forty feet above the stream. The machinery for raising the water should be of the most simple structure, on account of the difficulty of keeping it in repair.—AUSTRALIENSIS.

[There was a very simple method described in our Journal of June 4, 1825. Can any of our correspondents favour us with a better?—ED. M. M.]

Musical Time-beaters.—Sir, In the last number of the "Harmonicon" is a letter on metronomes and musical time-beaters: will any of your correspondents inform me, if the time-beater invented by Mr. Egerton Smith, and improved by Mr. Conlyffe, is, as it is there stated to be, the best; and if so, where they may be procured? And what is about the price? The above-mentioned writer notices a new one spoken of in a Liverpool newspaper, which is there considered as simpler and answering the same purpose as Mr. Smith's; from which opinion the writer in the "Harmonicon" differs. The price of the new one is stated to be about 2*s.* 6*d.*: now if a good one can be procured for any thing approaching that sum, and the fact were generally known among musical people, it would probably have an extended sale. For those who play much alone, and for those who have not the aid of a master, (and I am happy to see such daily increase, for the more music is diffused among the lower classes, the happier and the better will it make them,) such an instrument would be of the greatest assistance; three or four shillings might be afforded, when a guinea per quarter (naming the very least) is out of the question. It would keep more correct time than any performer possibly could, and would be of great assistance in domestic musical parties. Time is of such vital importance to music (and every thing else for that matter), that any thing that tends to simplify its acquirement cannot but be considered as doing good service to art. Should the instrument spoken of have that effect, I would remind your musical correspondents, that he who assists in making it known will in some degree share its merit. I remain, Sir, respectfully, VIOLINO. Tiverton, Sept. 10, 1833.

MISCELLANEOUS NOTICES.

American Locomotive Engine.—"The locomotive engine called the 'Pennsylvania,' invented and patented by Col. S. H. Long, of the United States' army, has been fairly tried and approved on the Germantown railroad. Recent experiments have shown that the engine is fit to draw 32 tons easily on a level road at the speed of 15 miles an hour. The whole weight of the engine is four tons and a half; the boilers evaporate 200 gallons in an hour, in which time they require the consumption of something less than two bushels of anthracite coal, the only fuel used. The wheels are made of wood, each with an iron tire of three parallel concentric circular bands, cheap in price, but very substantial, strong, lasting, and efficient."—*Philadelphia Daily Chronicle.*

Railway Lawsuit.—A long depending suit between Mr. James Wright, of Colombia, Pennsylvania, and the Baltimore and Ohio Railway Company, relative to the employment of a particular description of wheels, for which Mr. Wright has a patent, has been recently brought to an issue in the Circuit Court of the United States, and a verdict given in favour of the plaintiff. Mr. Wright's invention consists in the combination of a cone on the tread of the wheels, with a capacity of vibration in the axle to allow of their adjusting themselves to the radius of any (lateral) curve upon which they may run.

Tidal Power.—"The principle of buoyancy has been successfully employed in pulling up old piles in a river where the tide ebbs and flows. A barge of considerable dimensions is brought over a pile as the water begins to rise; a strong chain, which has been previously fixed to the pile by a ring, &c., is made to gird the barge, and is then fastened. As the tide rises the vessel rises too, and by means of its buoyant force draws up the pile with it. In an actual case a barge 50 feet long, 12 feet wide, 9 deep, and drawing 2 feet of water, was employed. Here $50 \times 12 \times (6-2) \times \frac{4}{7} = \frac{50 \times 12 \times 16}{7} = 192 \times 7\frac{1}{7} = 1344 + 27\frac{3}{7} = 1371\frac{3}{7}$ cwt. $= 66\frac{1}{2}$ tons, nearly the measure of the force which the barge acted upon the pile."—*Dr. Gregory's Mathematics for Practical Men. Second Edition.*

Velocity of Balloons.—The velocity of 80 miles per hour is that by which the aeronaut Garnerin was carried in his balloon from Ranelagh to Colchester, in June, 1802. It was a strong and boisterous wind, but did not assume the character of a *hurricane*, although a wind with that velocity is so characterised by Rance's Table. In Mr. Green's aerial voyage from Leeds, in September, 1823, he travelled 43 miles in 18 minutes, although his balloon rose to the height of more than 4,000 yards.—*Ibid.*

Data for the West Indian Planters.—"I have caused," says M. Coulomb, "extensive works to be executed by the troops at Martinico, where the thermometer (of Reaumur) is seldom lower than 20°, (77° of Fahrenheit). I have executed works of the same kind by the troops in France; and I can affirm that under the 14th degree of latitude, where men are almost always covered with perspiration, they are not capable of performing half the work they could perform in our climate."

INTERIM NOTICES.

We have received, while on the point of going to press, a full account of the experiments made last week on the Liverpool and Manchester railway, to determine the accuracy of the undulating railway system. It shall appear in our next. We may observe, in the meanwhile, that the results are decidedly in favour of the system.

Trebor Valentine complains with justice, that at a late social meeting of scientific gentlemen near Nottingham, he was "arraigned, tried, cast, and condemned, absent and unheard," (by the strong beer act of course), of being the author of the letters which appeared in this Journal, signed T. V. Robson, on Mr. Hall's steam engine improvements; and this on the strength of an answer said to have been given by the Editor to Mr. H., when questioned by him on the subject. There must be some great mistake in this: Mr. Hall never put any such question to us, and could not therefore have been so answered.

P. Yes, all but (for the present) 9 and 10.

The bearer of the letter from Mr. Brendel (Freyberg, in Saxony), is requested to favour us with his address.

Communications received from R.—W. H. B.—One who has tried it.—P.—Mr. Busby.—S. Y.—A Three-copy Subscriber—Friar Bacon—Bergein—Mr. Cheverton—R.—Mr. Shires—Mr. Hinton.—A. C.

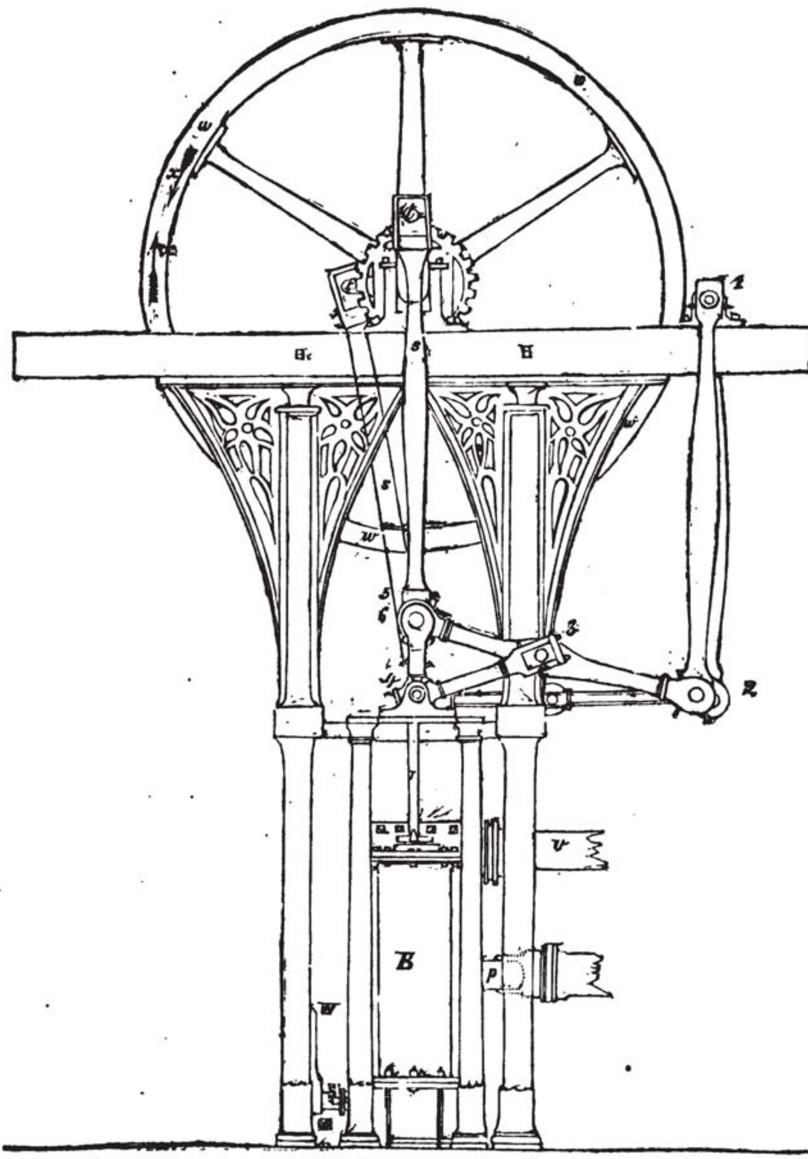
LONDON: Published by M. SALMON, at the Mechanics' Magazine Office, No. 6, Peterborough Court, between 135 and 136, Fleet-street. Agent for the American Edition, Mr. O. RICH, 12, Red Lion-square. Sold by G. G. BENNIS, 55, Rue Neuve, Saint Augustin, Paris.

M. SALMON, Printer, Fleet-street.

Mechanics' Magazine,
MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 531. SATURDAY, OCTOBER 12, 1833. Price 3d.

BRENDEL'S DOUBLE CYLINDER EXPANSION ENGINE.



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three positions, W, X, and Y', correspond with the highest, middle, and lowest stations of the intermediate slider *a a*, and the lowest, middle, and highest positions of the intermediate slider *b b*.

There are other necessary parts of the apparatus which will be readily understood from an inspection of the figures, and which need not therefore be particularly described; such as the means (1, 2, 3, 4, 5,) for maintaining the parallel motion of the piston rods, and those (K F D E) for pressing by the rollers H the sliding valves against the intermediate sliders, &c.

I will only further observe, that the engine stands on a shaft belonging to a tunnel which has been driven from the Elbe to the coal pits in the Plauen Valley near Dresden; that it works without condensing the steam—that it raises the water a height of 294 Dresden feet by common suction pumps—lifts the rubbish to the same height by ropes; and, finally, blows fresh air into the pit. The piston makes from 15 to 30 strokes per minute, according as circumstances require. The power of the engine may therefore be estimated at from 9 to 16 horses; and it can be managed with safety by the most ordinary mechanic.

(The conclusion, containing the account of Mr. Brendel's second improvement, in our next.)

EXPERIMENTS ON THE LIVERPOOL AND MANCHESTER RAILWAY, TO DETERMINE THE CORRECTNESS OF THE UNDULATING RAILWAY SYSTEM.

Sir,—In accordance with my promise I take the earliest opportunity of publishing, through your Magazine, the result of the first practical experiments which have been made, with a view of deciding the correctness or fallacy of the undulating principle.

The Directors of the Liverpool and Manchester Railway having permitted me the use of the *Rocket* engine, (the only one which could, at that time, conveniently be spared for the purpose,) I proceeded on Monday evening last to the Sutton inclined plane, accompanied by Mr. Scott, who had been employed by the Directors to stake out the requisite distances, Mr. A. Rae, foreman to Mr. R. Stephenson, sen., and two other individuals well acquainted with the management of the engine, our object being, on this occasion, to try the condition of the engine, and to determine the different lengths of line most suited, on an inclination of 1 in 96, to a fair development of the principle.

The *Rocket* engine, from the fatal calamity which resulted from its first public trial, is, no doubt, well known to your readers: it is the oldest engine on the line, and of the least power; the diameter of its wheels 4 feet 8 inches; diameter of cylinder 8 inches, and about 16½ inches stroke; weight of engine about 5 tons.

Experiment 1.

The engine and tender, weighing about 6½ tons, were worked with full power about 280 yards along the level line to the foot of the Sutton inclined plane. At this point (the piston working 15 strokes in 10 seconds) *the steam was shut off*, and they ascended the inclined plane, by momentum, 217 yards.

Experiment 2.

The engine and tender were worked 217 yards *down* the same inclined plane. The velocity at the foot of the plane was at least 16 strokes of the piston in 10 seconds: *the steam being then shut off*, they traversed the level line (by momentum) 454 yards.

Experiment 3.

The engine and tender worked 314 yards along the level, and attained a velocity (unmeasured) at the foot of the ascent, which carried them up the inclined plane (by momentum) 239 yards.

Experiment 4.

The engine and tender worked 239 yards *down* the inclined plane. At the foot of the descent the steam was shut off, and they ran on the level line (by momentum) about 500 yards.

The above-mentioned experiments were the only ones which I had an opportunity of trying on Monday evening

On Tuesday the 23d I again went to the Sutton inclined plane with the same engine and tender, and an additional load of about 35 tons.

The day was showery, the rails slippery, and the wind blowing fresh from the S.W. On leaving the Liverpool station it was evident that the *Rocket* had a *maximum* load behind her; so much so, that, had she not been assisted by the *Pluto* engine, she could not possibly have ascended the Rainhill inclined plane.

Mr. Booth, Mr. R. Gill of Manchester, and those who witnessed the previous trials, were present during the following experiments. I regretted much that unavoidable engagements prevented the attendance of Professor Ritchie, Mr. Locke, and Mr. R. Stephenson, sen., each of whom had expressed a determination, if possible, to be present.

The *Rocket*, with *tender*, and a load of 35 tons, was worked with full power down 147 yards of the inclined plane. The steam was shut off at the foot of the descent, the piston at that time working about 60 strokes per minute; the whole train then ran (by momentum alone) 546 yards upon the level.

Experiment 6.

The previous load was worked along the level plane, and a velocity was attained at the foot of the ascent, approximating as nearly as the power would admit to the velocity attained at the same point in the last experiment. The steam was then shut off, and the train ascended (by momentum) 134 yards.

N. B. — In this instance the engine *propelled* the load; in the latter it *dragged* the load.

Experiment 7.

The last experiment was repeated, and the engine and load ascended the inclined plane (by momentum) 127 yards.

Experiment 8.

Reversing the action of the power, and working the engine and load down the inclined plane 127 yards, it was evident that a much greater velocity was attained at the foot of the descent than *at the same point* when the late ascent (by momentum) commenced.

Experiment 9.

The *Rocket* worked the before-mentioned load along the level plane, and attained a velocity at the foot of ascent of about 7 miles an hour. In this instance (the power being continued) the total ascent was 160 yards.

Experiment 10.

The *Rocket*, with the same load, descended 160 yards, and attained a velocity at the foot of descent of full 10 miles an hour; momentum on the level not measured.

Experiment 11.

It being found, as proved by experiments 6, 7, 8, 9, 10, that the *Rocket* was not sufficiently in order to make further trials alone, the *Caledonian* engine was placed behind the load—in consequence of which the united powers of the two engines produced at the foot of the ascent a velocity of about 12 strokes of the piston in 10 seconds. The *Caledonian* then left the train, and the *Rocket* shut off her steam, the momentum on the inclined plane being 177 yards.

It was now evident the *Rocket* was so much out of order that all further experiments would be fallacious. I was consequently deprived of the opportunity, on this occasion, of proving the *extent* of load which *could* be conveyed from one summit to the other of a curve, with an engine of a given power, or whose effective power was known upon a level. The general inference, however, to be deduced from the preceding experiments is, not only that the practical experiments evinced an advantage, by the adoption of the undulating system, *equal* to any which the models had represented, but *considerably more important*, inasmuch as the models *did not prove*, what practice, I humbly submit, *has proved*, viz. that a load (which on a dead level was a maximum load) could be conveyed from one summit of an undulation to another summit of equal altitude, by the employment of the locomotive steam force throughout only half the distance; and this, too, be it understood, with a *certainty* of acquiring any desired average velocity, without a proportionate sacrifice of mechanical power.

This fact was proved by the velocity of the train being *always* greater at the foot of the descent (the engine having *worked down* the precise distance which it *ascended* by momentum) than at the *foot of the ascent*; if L, therefore, represent the length of line ascended by momentum from the level, V the velocity acquired at the foot of ascent to produce *that* momentum, D the difference in velocity, or surplus velocity attained at the foot of the *descending* plane, we then have, in the first instance,

$$V=L,$$

and in the second

$$V+D=LD.$$

I shall now leave the results of these experiments to the consideration of your readers, especially to my opponents, promising to add to them such particulars as future trials, whether adverse or propitious to the principle, may render it necessary to publish. Our next experiments (and I trust they will prove decisive) will be made, as at present understood, on the 15th October; previous to which trials, permit me to record an additional opinion to those I have already expressed. I think it will be found, in practice, not only (as stated in my challenge) that an engine of any given power will move *a load* from one summit of a curve to another of equal altitude, which that same engine could not move on a horizontal plane, but, *that an engine of any given power will move from one summit of a curve to another nearly double the load which it is capable of moving on a level plane, provided the average velocity be not less than 15 miles an hour*.

Should this prove to be the fact, it will be evident to all your readers, that a saving in the construction of railways, *increased velocity*, and *economy of time*, are not the ONLY ADVANTAGES derivable from the adoption of *undulating railroads*.

I am, Sir,
Yours very obediently,
RICHARD BADNALL.

Liverpool, Sept. 28, 1833.

Errata.—Page 440. col. 2, line 15, for "336 *feet*," read "338 *f*."
——— 18, for "280 *feet*," read "280 *f*."

THE UNDULATING RAILWAY.—NOTE BY "S. Y."

Sir,—I confess Mr. Badnall *did* rouse my indignation, not because he could not understand my paper, for that is an affair between him and his understanding, with which I have no concern, but by what seemed to me his contemptuous manner.

But finding contempt will not do, he now labours to make it appear that in No. 516 I am speaking of the power necessary to move a body on an inclined plane, although he is distinctly told in that same paper, that I am only speaking of "*the force of traction required in consequence of friction*," considered *abstractedly*, which he himself declares to be the *same*, whether a body moves *up* or *down* an inclined plane.

There is, however, but little hope of Mr. Badnall's comprehending my paper on page 242 while he is puzzled with the particular case stated on page 181: the 8 *n*, whether with a dot or without, seems an insurmountable obstacle to Mr. Badnall, and I will therefore endeavour to state the case more plainly.

Refer to page 93, and suppose the horizontal length from A to E = 16 yards, the depth F B = 6 yards, the length of each inclined plane will be 10 yards. Suppose a carriage placed on the level railway, of any weight you please, and the tractive force rendered necessary by the friction *(friction only, remember, Mr. Badnall,)* to be *five* ounces; then, according to Mr. Badnall's own reasoning in No. 509, the friction of this same carriage on these inclined planes will be *four-fifths* of the friction on the level; and therefore the tractive force required by the friction on the inclined planes will be *four* ounces. Now, the power used by the friction in moving the carriage from A to E in the one case, is equivalent to the moving of 5 ounces through 16 yards of space; and in the other, to the moving of *four* ounces through the length of both the inclined planes = 20 yards; and I believe "few will deny" these two expenditures of power to be precisely equal to each other in amount.

It has been repeatedly stated, and is doubtless nearly correct, that the tractive force necessary on account of *friction* is precisely the same, whether the carriage moves fast or slow. If this is admitted, it will not signify what the velocity is supposed to be in the above example: it may be high or low, uniform or variable; the result will be unaltered. Mr. Badnall seems to think that some advantage is gained in this respect by high velocities; this *may be* the case, but if it is it proves nothing in favour of curved railways.

If the dynanometer used to ascertain the tractive force were drawn to the mark *five*, and I found that hanging *five* ounces to it drew it to the same mark, I should say, the tractive force was equal to 5 ounces; or, in other words, a force that would compress a spring as much as a weight of 5 ounces, would compress it.

So, while Mr. Badnall was solving a problem of his own, he imagined he was solving one of Mr. Ham's! This is amusing. Nevertheless, it was kind of Mr. Badnall to make the attempt; and, in truth, his kindness does not seem to be altogether unneeded by the "scientific individual" in whose mind such a wondrous change was produced "in the coach between London and Bristol" (see page

179), and whose very *un*-scientific solution of his own problem appears on page 379. But the worst of it is, Mr. Badnall's solution of *his* own problem is incorrect, which is manifest to every one accustomed to calculations of this kind; for they know that whatever time is occupied in passing over the first half mile, *exactly half that time will be required for the second*. It may be pleaded that the error is small. True; but it is of sufficient magnitude to prove that the principles upon which the calculation *ought* to have been conducted have *not* not been attended to.

As friction is the *only* thing of any consequence in this discussion, I cannot help thinking that Mr. Badnall should take some notice of it in his calculations, and not rest satisfied with merely *supposing* it provided for.

When I spoke of Mr. Badnall's *calling on public companies*, &c., I alluded to the conclusion of his letter in No. 519, which seems to me very like it; but I leave your readers to judge between us. At present I have no appointment of the kind he names, or any other.

In the paragraph addressed to "W. W.," page 422, there is what I take to be an omission of the press—"*is in proportion*" evidently should be "*is in* INVERSE *proportion*," &c.

It seems to me, that when Mr. Cheverton speaks of "steam power," he means abstractedly the *power of steam;* while Mr. Badnall uses the words in their more common acceptation, and means by "steam power," *the power of a steam engine*. Understood in this way, they are both pretty correct; at least if the latter gentleman confines his observations to reciprocating engines.

I do not think, Mr. Editor, I shall have further occasion to trouble you on this subject; if, however, I see any thing in Mr. Badnall's future remarks which alters my opinion of his railway, I will come forward and say so; otherwise, I decline further discussion. I request Mr. Badnall to attribute any warmth of expression on my part to the interest I feel in the argument; and that he will not fancy I "*oppose*" him, but only that I feel convinced *he is in error*.

I am, Sir, yours, &c.,

S. Y., an Engineer.

Lambeth-terrace, Sept. 17.

In my paper, page 242, line 4 from bottom, for "*all* heights" read "*all lengths.*"

P. S. Sept. 21.—I have just seen No. 528. A body which is heavy on the earth's surface would have *no weight* if placed at the centre.

In reference to an observation of Mr. Badnall's, I will venture to affirm that no *scientific man* ever said or believed that the *inertia* of the carriages was a constantly resisting force, which it must be before "the utility of an undulating railway is, according to Mr. Cheverton, *unquestionable.*"

Mr. Ham, in his calculation, considers the velocity attained by a falling body is in direct proportion to the *space fallen through;* instead of which it is in proportion to the *square root* of the space. Mr. Badnall, I see, considers this quite unimportant—a matter of no moment whatever!

I never alluded to Mr. Badnall's "proficiency" either in arithmetic or any thing else. I said he "ought to pay some little *attention* to his arithmetic," or, in other words, that if he put forth any thing which he called the solution of a problem, it ought NOT to want multiplying by *eight* to make it something like correct; and I see no reason to alter my opinion.

Mr. Badnall says, "I should feel hurt to find that any other correspondent than 'S. Y.' had doubted my proficiency in *common arithmetic.*" As Mr. Badnall knows best what has occasioned him to make this invidious distinction between his other opponents and myself, he is the best judge of whether it is gentlemanly and in good taste on his part so to do.

I will certainly admit I am in error the moment I am aware that such is the fact. Your readers will do me the justice to remember, that *the comparative quantity of friction on the two railways* is all I have attempted to ascertain; and they will perceive I have *assumed* the *friction* to be in direct proportion to the *pressure*—an assumption very much nearer the truth than the useful purposes of the investigation required.

S. Y.

THE UNDULATING RAILWAY.

Mr. Editor,—Your ingenious correspondent, Mr. Cheverton, having fallen nto an error on an important matter connected with the laws of motion in his last communication, in reply to Mr. Badnall, and his views being supported by others of your correspondents, it may not be amiss in a few words to explain that matter clearly, the more particularly as the proper application of the principle is a matter of the first consequence in practical mechanics, and is at times too often unattended to. The law on the case of a body moving by a mechanical force is, that the accelerations in equal times will be unequal. Thus, for example, when a ship is put in motion by the wind the velocity of the wind with respect to the ship will be less when the ship has acquired some motion than when it was at rest. After the first impulse the acquired velocity of the body will be subducting more and more from the relative velocity of the wind; on which account the acceleration, in equal times, will be less and less, till the resistance the body meets with in its motion becomes a balance to the force that moves it, when the accelerations cease, and the ship goes on with an equal pace. This is different from the manner gravity produces its effects, for this cause acts incessantly or continually, and with the same force upon a body that is already in motion at any velocity, as upon a body that is at rest; whence it is that it produces equal accelerations in moving bodies in equal times. This property of gravity has been satisfactorily proved by observations made on pendulons and falling bodies. (See Maclauren's Philosophical Discourses, page 241.) Hence it is, that if the wind be moving at the rate of 20 miles an hour, if the vessel be moving at the rate of 10 miles the hour, the mechanical power or impulse moving the ship will be only equal to wind moving at the rate of 10 miles an hour, and the power therefore will be reduced one-half as compared with gravity.

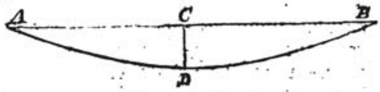
As Mr. Badnall is perfectly correct in asserting that the friction is reduced by undulating curves in proportion as the angle of the inclined plane, ascending or descending, is reduced, there can be no doubt but in theory Mr. Badnall has it all his own way. The argument founded on the difficulty of applying steam machinery to such variable movements, I conceive of no weight whatever. If the power gained by the undulating rails, when an experiment is tried on a large scale, should prove considerable, that difficulty can be got over without trouble.

I am, Sir,

MENTOR.

THE UNDULATING RAILWAY MATHEMATICALLY CONSIDERED.

Sir,—I am very glad to find, from your last *Conversazione* at Peterborough-court, that you yourself, Mr. Editor, have at last become a convert to Mr. Badnall's system of the undulating railway. His last able article (No. 528,) must go a great way to extinguish all the objections of his opponents. For my own part, Sir, I have from the beginning of the controversy been inclined to believe that his method was founded upon true principles, whether viewed in a theoretical or in a practical way. But as engineers were opposed to engineers, and as the question at issue was to discover the path by which an *engine* was to be propelled, when acted upon by given forces, so that the journey should be (safely) performed in the least time, I naturally concluded that an engineer (who is, or ought to be, a mathematician,) was the proper person for solving the question. But, Mr. Editor, be that as it may, I have only here to state, that before Mr. Ham had proposed his problem (No. 515,) I had, for my own satisfaction, proposed to myself the following question (May 12,) which I now send you:—



Suppose A B is a horizontal line, 2 miles in length, and that A D B is an undulating curved line, and that the greatest fall C D, from the middle of A B, is 16 feet. Now, suppose a body A is urged on with a force that produces an uniform velocity of 10 miles per hour in the line A B, and that an equal force is applied to another body in the curved line A D B: required the time in which each will perform their journeys, supposing the curved line A D B to be a segment of a circle.

First, the time in the horizontal line A B will be 12 minutes, and the length of the curved line A D B, by calculation, will be found to be 10560.065 feet; also the initial force per second is $\frac{42240}{3600}$=

14.666 feet, and the velocity acquired by gravity, at the lower point D, is 32 feet; hence the whole time in the curve line A D B will be 10560.065 ÷ ½ (14.666 + 32) 452½″ = 7′ 32½″; that is, the same power being applied in both cases, the time saved in the curve line A D B will be 4′ 27½″.

Some of Mr. Badnall's scientific opponents have harped a good deal on the difference of length between the horizontal and undulating curved line. In the above example this difference between the two lines will not amount to 17 *feet* IN 500 *miles*. Now, with regard to friction, it may be considered the same in both cases, certainly not more in the curved than the horizontal line. (Mr. Badnall says less, and I rather think he is right.) There will, no doubt, be a small additional resistance of the air in the lower part of the undulating line, produced by the additional velocity; and if any of Mr. Badnall's opponents will undertake to prove that this additional resistance will be sufficient to counteract that gained by gravity, then his system falls to the ground: but this I know, full well, they cannot do.

I am, Sir, yours, &c.

KINCLAVEN.

EXTRAORDINARY PHOSPHORESCENT METEOR.

Sir,—On Tuesday evening, the 17th instant, at 20 minutes before 10, my attention was accidently directed to a very peculiar luminous appearance in the heavens, the upper part only of which was visible from the chamber floor of my house, above the opposite buildings; but wishing to have a better view of it, I proceeded immediately to the beach, which is only about 150 yards from me, where a most extraordinary stream, or streak, of apparently phosphorescent light presented itself in the heavens, in a direction about W. S. W.

I found some of the seamen of the preventive service on the beach, and was informed by them that they had first observed the light at about a quarter past 9, the upper half being *at that time* obscured by a dense cloud, which shortly after cleared away. When I saw it the streak of light seemed to rise out of a dusky fog-bank over the sea, from the horizon to an elevation of about 43 degrees, its width being about 2 degrees; it had a concave curvature, and an inclination of about 15 degrees towards the south; the upper part appeared to touch the milky way—the general figure being that of the feather of a quill, but not so wide in proportion to its length. The central part of the streak was at least four or five times as bright as the most conspicuous part of the milky way, and the edges seemed to die away into the atmosphere, so as to render it impossible to discover a defined termination. The sky was perfectly clear, and the stars shone brightly during the whole time I viewed this remarkable object, which was till 5 minutes past 10, when it had faded away, as if travelling beneath the horizon, having been visible at least 50 minutes.

The light was perfectly colourless, and its station seemed to be far beyond the region of the clouds: it must, I expect, have been observed at places very distant from Brighton. As it is only by noting the apparent positions and motions of the uncommon and fleeting objects which sometimes traverse the heavens, as seen from different positions on the earth, that any thing like an accurate notion can be formed of their actual nature and real situations and directions, I took some pains, on the spur of the moment, to ascertain the particulars above related, and wish to give them publicity through your widely circulated miscellany, in the hope that they may reach the eyes of those, at other places, who may have witnessed the phenomenon, and have observed it as particularly as I did.

I am, Sir,
Your very obedient servant,
C. A. BUSBY.

Stanhope-place, Brunswick-terrace,
Brighton, Sept. 20, 1833.

P. S. The moon was only two or three days' old, and had set some time before the nebulous meteor made its appearance. The fog-bank did not rise above 5 degrees from the horizon. The evening was perfectly dry, and the wind N. W.

REPAIRS OF BLACKFRIARS BRIDGE.

Sir,—As I am indebted to your columns for the first sight of the report of Messrs. Walker and Burges respecting Blackfriars Bridge, permit me to send you a copy of my observations thereon, together with a copy of a letter I have just received from Guildhall.

Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 532. SATURDAY, OCTOBER 19, 1833. Price 3d.

ROTARY ENGINE WITHOUT VALVES.

Fig. 1.

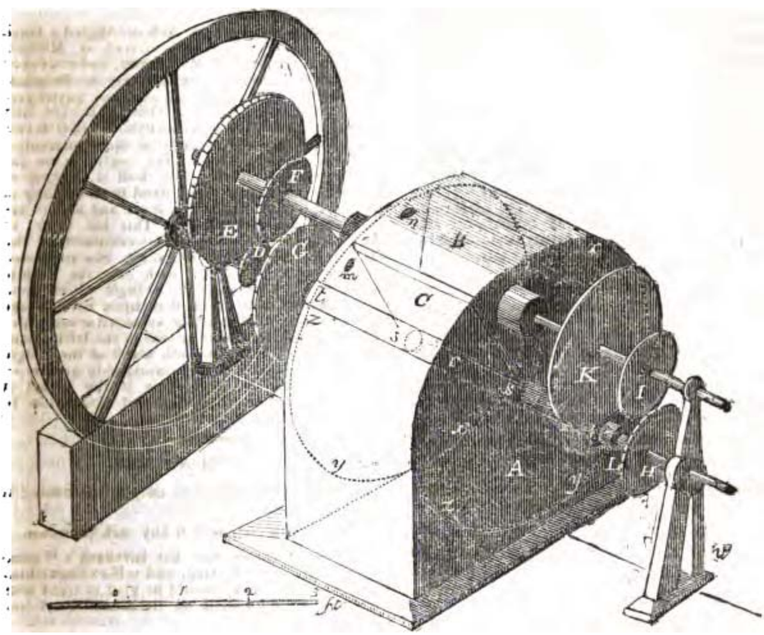


Fig. 2.

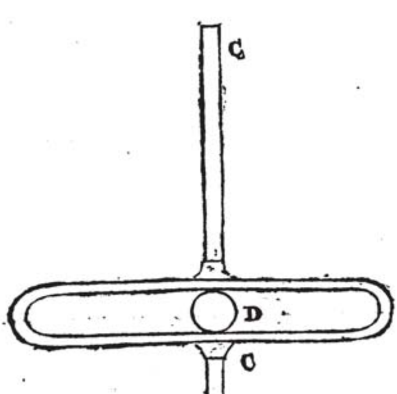
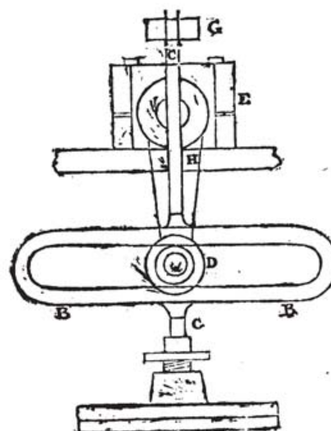


Fig. 3.



M. Salmon, Printer, Crown-court, Fleet-street.

ard, there are six points of friction, namely, four on the cross head, two for the connecting rods, and two for the guides, whether they be rods or friction rollers, and two for the cranks. I take the cross-head engine for an example, because it is more generally used than any other, and these points of friction in the cross-head engine are of a considerable size. Now, by making use of the plan of "S. D.," described at page 362, vol. xix, we have only three points of friction,—one made by the roller D, revolving on the arm or pin *d*, projecting from the crank H; the other caused by the roller D traversing the frame B B. (See figs. 2 and 3, front page.) Now, the bearing of a round body traversing a flat surface touches only at a line, and thereby produces the least possible friction. The next point of friction is the guide G. The roller D bears only at the top or bottom of the frame B B at one time, because the piston rod C gives motion to the frame B B. Suppose the piston to be ascending to the top of the cylinder A, the lower part of the frame B B would receive the friction of the roller D, beginning from a line vertically drawn through the centre of D, fig 2. The same effect would be produced on the top inside the frame B B by the descending stroke of the piston rod C. The plummer block E has the bearing of the crank H and shaft, and if any analogy can be drawn from theory and practice, I should say there must be rather a reduction of friction than an increase in the plan proposed by "S. D." Another advantage, in my opinion, is to be gained by bringing the working parts nearer to that part from which their effect is produced, thereby preventing the evils which are occasioned from springing. Mr. Baddeley states, there are several other methods by which the connecting rod can be dispensed with. I and my fellow mechanics would feel much obliged to Mr. Baddeley, through the medium of your valuable Journal, to see them demonstrated.

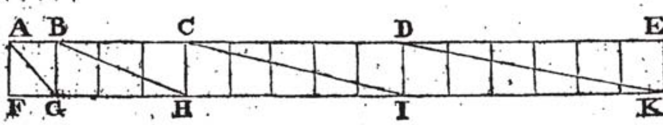
I am, your obedient,

G. MARSLAND.

Sept. 19, 1833.

THE UNDULATING RAILWAY.*

Sir,—In No. 519 Mr. Badnall says, "locomotive steam power, according to the present construction of the engines, is not constant. Supposing, however, that by improved construction it were so, it is a fact established beyond a doubt, though never YET publicly accounted for, that the friction upon the road decreases as the velocity increases; hence, though a constant power were at disposal, its effective employment would be diminished for want of fulcrum when the velocity was very great." Great indeed must the velocity be for such a result to take place; but how can its effective employment be diminished when its velocity is *very great*, seeing that velocity is the thing wanted? Passing over this part of the subject, however, for the present, I will proceed to show the cause of the diminution of friction with increased velocity. Nearly six years ago I proved, by calculation, that friction is equal in equal times, whatever the velocity might be; and being perfectly satisfied of that fact, I naturally began to inquire into the cause, and came to the following conclusion: *Friction is produced by pressure; friction is diminished by motion; therefore, pressure is diminished by motion.*



The above diagram will assist in making my meaning on this subject more clear. Let A E represent a horizontal plane, F K be parallel to A E, and A B, B *m*, &c. = to A F. Suppose a body placed at A, and let A F represent the pressure, or force of gravity; suppose, also, A B any constant force whatever = to A F: under the influence of these two forces, the body, if not prevented, would move in the direction A G; but as the body cannot take the direction A G, it

* This letter should have been published sooner, but was unfortunately mislaid. Another communication on the same subject from Mr. Mackinnon has come to hand, and shall also appear.—ED.

will take that of A B, which space we shall suppose to be gone over in the first second; the force A B being supposed constant, it will, in the second second, describe B C, in the third C D, and in the fourth D E. Hence it appears evident to me, that as the spaces are gone over in equal times, and the friction is equal in equal times, that the pressure must also be equal in equal times. In the first instant the pressure would be diminished by a quantity equal to that which would be supported by the inclined plane A G, if the body were placed on it; in the second instant it would be diminished by a quantity equal to that which would be supported by the plane B H, and so of the third and fourth, &c. I would also observe, that the word INERTIA ought to be expunged from mechanics; for if a body is at liberty to move, it will yield to any force whatever, with a velocity proportionate to the force impressed.

A word or two to "S. Y." an engineer. At p. 243 last vol. "S. Y." says, "it is possible some advocate of Mr. Badnall's plan may turn round upon us, and say, 'we have the authority of the *Scotsman* newspaper for asserting that the expense of power, in consequence of friction, is precisely the same for the same distance moved, let the velocity be what it may.'" Now, Sir, would it not be more becoming "S. Y." to grapple with the subject at once, or own that he does not comprehend it; instead of carping constantly at what has been said by the "Scotsman?" The truth is, the "Scotsman" newspaper never made any such assertion. The doctrine put forth by the "Scotsman" was, that the expenditure, on account of friction, is equal in equal times, whatever the velocity may be,—a doctrine not only founded on the experiments of philosophers of unquestionable talent for laborious investigation, but independent of all experiments demonstratable by calculation. It is in vain for "S. Y." to bring forward the revolution of wheels, and strokes of pistons, to prove the fallacy of the new theory of friction. It is the business of philosophers to establish theoretical truths, and that of practical men to apply these truths to the arts of life.

I remain, dear Sir,

Your obedient servant,

A. MACKINNON.

Brighton, Aug. 5, 1833.

Sir,—Had I addressed you on this subject at its commencement, I should have ranked among its opponents, but by attention to the evidence given on both sides I have in some measure become a convert to the doctrine of Mr. R. Badnall, and believing that I have caught a glimpse of the new light, I beg to be heard in support thereof—leaving the mathematical part of the question to more able hands, and confining my observations to such points as appear to have been misconceived, or have not met with due consideration.

Junius Redivivus, page 419, vol. xviii, in ironically remarking that much money has been wasted to render our roads worse than they were before, by levelling the hills, seems to forget that in all cases it has been done to facilitate the present mode of travelling by animal power, with which, under the most favourable circumstances, very little advantage can be taken of a descent; consequently, besides locking a wheel, the cattle are often severely strained to counteract that very power, the full and free use of which Mr. Badnall intends so materially to assist in overcoming the ascent.

Mr. B. Cheverton states, page 358—"In a mechanical and economical point of view, or as a criterion of its efficiency, it is not of so much consequence what the time is in which a bushel of coals is consumed, as what the space is through which it is the means of force operating to overcome resistance, that is, to raise water, or to propel a carriage." Now, although this may be very true, how can it apply? For, suppose a ten-horse engine sufficient to remove a carriage a certain distance on the undulating road in one hour, but that the same power can only do the same distance on the level line in two hours, is it not clear that in such case the power must be doubled, or that it will require a twenty-horse engine to accomplish the distance in the same time as on the undulating line?

I shall now proceed to give an account of some experiments of my own on the subject. Having access to some semicircular cast-iron troughs, I laid down 32 feet as nearly level as I could, and the same length with a fall each way to the centre of 23 inches; I then procured two balls, $1\frac{1}{4}$ inches diameter, and weighing 1 ounce 6 drams each; to ensure repetition of a certain impetus as often as I pleased, I added an inclined plane 3 feet 10 inches long to one end of the level line, varying its inclination until the momentum given to the balls was barely sufficient to carry them to the other end of the undulated line. I also affixed a similar plane, and making the same angle with the horizon as the one attached to the level road, thereby making the expenditure of power alike in both cases. Having thus completed my arrangements I started one of the balls on each line, and found that both travelled the same distance, proving that an equal expenditure of power produced the same effect as to space, inertia, friction, the resistance of the atmosphere, &c., but not so with time, for although 12 seconds was occupied in going the distance on the level line, it was accomplished in 6 seconds on the undulation, making a clear gain in its favour of one half the time, and thereby showing, that although there may be no generation of power, it is pretty evident that Mr. Badnall has succeeded in calling into useful operation one which already every where existed.

I remain, Sir,

Yours respectfully,

TREBOR VALENTINE.

Derby, Sept. 16, 1833.

Sir,—Your correspondent Mr. Sanderson, in No. 530, says, "if what is termed a 'scotch' be placed in the way of a carriage descending an inclined plane, I should call this kind of opposing force simply an *obstruction*."

This seems to me a distinction without a difference. What is friction of any kind at all but an "obstruction" to motion. When a carriage is stopped on an inclined plane it will scarcely be contended that its total weight is less than in any other position. What then becomes of the pressure? It is removed from the roadway to the "obstruction" or scotch. Be it so, but what supports the scotch? The roadway! What then does it signify though twenty obstructions were interposed between, if the roadway be at the bottom of them all.

Mr. Sanderson seems to me perfectly right in describing "rolling friction" and "rubbing friction" as simply portions of the same general obstruction to motion in the case of the wheel and axle. That the friction of the periphery is a rubbing friction is sufficiently evidenced by the consumption of wheel tires, and the oil which is interposed between the axle and its gudgeons forms a series of rollers, which perform the offices of wear and tear, to the saving of the metal. But this rolling friction between the axle and gudgeons causes the consumption of the softer body—the oil. We come then to the same train of consequences. Motion begets friction, and friction begets consumption of material.

In answer to Mr. Badnall's last remarks upon my arguments, I can only ask him to show what becomes of the weight of a carriage whose motion is obstructed on an inclined plane? If the rail, or ground beneath the rail, does not sustain the weight, what does—whereon is the pressure? Surely not on the atmosphere. I really should like to understand the truth in this matter, if my faculties be capable of absorbing it, and when convinced of the truth, shall be perfectly ready to acknowledge it, utterly heedless whether it make for or against my side of the argument.

But to-morrow—or rather to-day, for it has stricken twelve upon the clock bell—will give us the results which are said to be decisive, and until I can see them I must postpone saying any thing further.

I remain, Sir, yours, &c.

JUNIUS REDIVIVUS.

October 12, 1833.

MR. JOPLING ON THE REPAIRS OF BLACKFRIARS BRIDGE.

(*Concluded from page 28.*)

"Models of the bed of the river, according to both surveys, if there be any material difference, would be particularly desirable. There is every reason to suppose, from the exact agreement with the model in the British Museum, and the statements thereon, that the measurements in Messrs. Walker and Burges' survey have been taken with the greatest care.

"The 'LOWEST low water line,' according to the model in the British Museum, at the time the bridge was built, was one foot seven inches lower than the 'low water line of 1832,' in Messrs. Walker and Burges' survey; but the latter being taken in the month of December could not be the 'LOWEST *low water line*.' The lowest low water line of the present year should be ascertained, in order to show how much that line has fallen since 1766.

"It is, however, thought probable that the difference will not be found so much as Messrs. Walker and Burges have calculated be a mere trifle—a single morning's journey; while the reader of the "Times" will expect to have "yesterday's debate in the Chamber of Deputies" presented to him at the breakfast table! It is not unworthy of remark, that while our share in producing these wonders is to be achieved by means of private capital, directed by public spirit, our neighbours are to be indebted for theirs to the interference of the government, directed by "the Minister of Public Works."—Is not this a whole commentary in itself?

The New Smithfield.—The building in the Islington Lower Road, which was referred to in a recent Note, is now avowedly intended to become the London cattle-market, notice having been given of an intended application to Parliament next session for an Act to enable the proprietor to make use of it for that purpose, and *to levy the necessary tolls.* The City will, of course, strongly oppose the intended Act, which would operate to transfer a large portion of their funds from the corporation treasury to that of the "humane individual" who has embarked in the speculation with so much apparent recklessness. The accounts which have appeared of the wonders of the place are founded more on the imagination of the reporters than on the real state of things. It is, indeed, very extensive, but at present the only erections are a range of cattle-sheds, surrounding a large quadrangle, and an unfinished market-house. The "sheep-pens on a new principle," grand "tavern and stabling," and, above all, the "abattoirs," and other conveniences for slaughtering, as yet exist only in the columns of the newspapers.

City Improvements.—The line of the intended new street from London bridge to the Mansion House is now plainly perceptible, a great number of houses being already pulled down between Lombard-street and Eastcheap, together with Pidding's well-known "lucky corner," and the adjoining houses as far as the Globe Fire-office, in Cornhill. This improvement, conjoined with the long-required widening of Prince's-street, will be one of the greatest effected in the City for many years, and will, added to the extensive alterations on both sides of London bridge, give quite a new character to the central part of the City, and the opposite quarter of Southwark. West of Temple-bar, also, improvement does not slumber: the new street opposite Waterloo bridge, which is to connect the Strand and the Surrey side of the Thames with the Regent's park and the northern suburbs, having at length, after much delay and difficulty, been actually commenced.

Provincial Medical Society.—The taste for scientific associations appears to be spreading. Within the last few months a provincial Medical Society has been established, which numbers among its members gentlemen of the faculty from all the midland and southern counties. Their first anniversary meeting, which was formed on the model of that of the British Scientific Association, took place in August last at Bristol, and was most numerously attended. The utility of such a society is much more obvious than that of an union of the whole scientific body of the nation. The latter have the metropolis as a point of union, but the cultivators of medical science are necessarily scattered far and wide. Their next year's meeting will be at Birmingham, and the first volume of their "Transactions" has already appeared.

The Universal Magazine.—A translation of the "Penny Magazine" is regularly issued to the German public at Leipsic, embellished with wood-cuts by "the first English, French, and German artists," under the title of "Das Pfennig Magazin." According to the Useful Knowledge Society, imitations of their most successful publication are also printed in France, Belgium, North America, *Poland,* and *Russia!*

Great Western Railway.—This is the title of the railway from London to Bristol, an undertaking which, according to all probability, will be carried into execution next year. Preparations of all kinds are actively going on: the line has been determined upon, and the usual apparatus of directors, treasurers, bankers, &c. set in motion both in Bristol and London, while even a continuation of the line through South Wales has been proposed. The Bristolians appear to enter very warmly into the project, but they must perforce curb their impatience until the next session of Parliament, when the Act will be applied for, and it is expected will pass without opposition. So great is their enthusiasm that the editor of a Bristol paper, in a paroxysm of rapture, exclaims, "When the railway and the Clifton suspension bridge shall be completed, Bristol will become the rendezvous of men of science *from all quarters of the globe!*"

Safe Return of Captain Ross and Crew.—The gratifying intelligence has been received at Lloyds of the safety of the gallant, adventurous, and all but despaired of Captain Ross and crew. All that is at present correctly known is, that they are on their way home in the Isabella, whaler, of Hull, and that during the four years and upwards they have been lost to the world, amid the icy wastes of the arctic circle, only three of the entire number have perished! What a history of perils and disasters—of hardship and suffering—of fortitude and high courage—of fears and hopes, must have been theirs! We wait with intense anxiety for the particulars.

THE UNDULATING RAILWAY.

Sir,—I must regret that I have not an opportunity of sending to you this evening (for want of time) the particulars of the experiments which we have tried to-day on the Sutton inclined plane. You will, however, oblige me by stating in your next number that they have been *extremely* satisfactory, and have *most fully* established the correctness of the undulating principle. I will take care that the full particulars are forwarded to you early next week. The trials which have been made to-day, and which have been subject to the investigation of the *Messrs. Dixons, Mr. Robert Stephenson, sen., Mr. Daglish, Mr. Gill,* and others, will, I doubt not, fully confirm the expectations of those who have done me the honour to support my views on the subject. I am, Sir, your very obedient,

RICHARD BADNALL.

Liverpool, Oct. 16, 1833,
Wednesday night.

INTERIM NOTICES.

"Delineator" has but small reason to complain of the slight irregularity in the publication of the parts of Mr. Jopling's "Practice of Isometrical Perspective:" not six months have yet elapsed since the work was first thought of, and though extending to 64 pages of letter-press, and containing 170 engravings, it is already completed. The Fourth and last Part, and also the work complete, in boards, will be published on the 1st of November.

Communications received from—An Old Gas Maker—R. H.—Mr. S. Tozer—A Saxon—Sperans—Mr. Davy—Mr. Redmund—W. W. C.—Mr. Symington—W. J.—R. Apps.

LONDON: Published by M. SALMON, at the Mechanics' Magazine Office, No. 6, Peterborough Court, between 135 and 136, Fleet-street. Agent for the American Edition, Mr. O. RICH, 12, Red Lion square. Sold by G. G. BENNIS, 55, Rue Neuve, Saint Augustin, Paris.

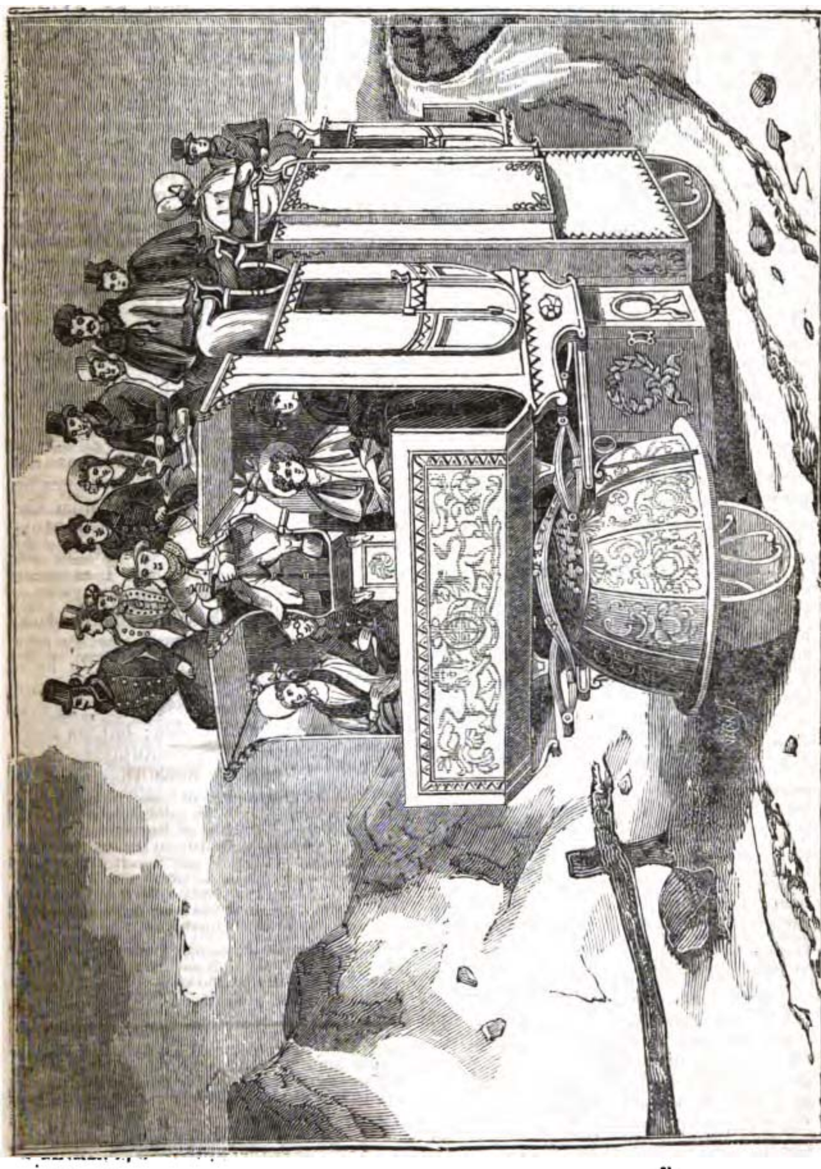
M. SALMON, Printer, Fleet-street.

Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 533. SATURDAY, OCTOBER 26, 1833. Price 3*d*.

CHURCH'S STEAM-CARRIAGE.



"resin gas," and every body knows that, during the present year, the "gas microscope" has been brought out as one of the popular exhibitions of the metropolis.

Now I feel convinced, Mr. Editor, that both these plans were originated and carried into effect independent of any thing made public by me; and just as well am I satisfied, notwithstanding the extract previously given, that the principle of generating heat, now made known, is as purely original with Mr. Rutter. Coincidences of this kind have frequently happened, and the more men are taught to think for themselves, the more frequently they will happen, which, after all, is nothing more than another proof of the value of scientific acquirements. Mr. Rutter, I feel persuaded, will not mistrust my motives in offering these observations to his notice: had I not done so, it is very probable some one else would shortly have made him acquainted with the "American water burner," and perhaps might unjustly accuse him of plagiarism at the same time,

I am, Sir, very truly yours,

W. H. WEEKES.

Sandwich, October 18, 1833.

HOW TO CLEAN WOOD CARVINGS.

Sir,—Your correspondent, "R. P.", has furnished us with many excellent and well-timed observations on wood carving, and he solicits an acquaintance with an unctuous agent calculated to remove paint and dirt from the surface of his specimens. I beg to suggest to him, that if such work has not been intentionally coated frequently with paint (a barbarous practice I have often witnessed), he will probably find in the article known in commerce by the name of *soft soap*, prepared from oil or tallow by boiling with caustic potass, a means adequate to the removal of dirt, accidental traces of paint, stains, &c. &c. It will not leave the surface of his work rough and uneven, while the colour of the wood will be rather improved than otherwise. I should hardly think he would require water, or, if so, very little, to remove the agent employed.

Perhaps he will find it worth while to try what a little time and repeated applications might effect in removing a coat of paint.—I am, &c.

W. H. WEEKES.

Sandwich, Oct. 18, 1833.

THE UNDULATING RAILWAY.

Sir,—Though the length of the reply in full to Mr. Badnall (which I have forwarded to you) may preclude it for the present from insertion in your Magazine,* you may perhaps find room in the interim for a few observations relative to the experiments instituted on the Liverpool and Manchester railway by Mr. Badnall, with the fallacious view of putting to the test the utility of the undulating principle; more especially as these observations are addressed to the consideration of facts, which, under certain conditions, it must be admitted, are of more importance than mere theoretical discussion. I need not apologise to Mr. Badnall for making any remarks on these experiments before they are fully completed, because I intend to admit the results, favourable as he considers them to be, to the fullest extent he can possibly desire, and beyond what he will be able to produce.

Facts are stubborn things, only as they have a cogent bearing on points in dispute; they are otherwise worse than useless, for they lead to bewilder when there is not the understanding mind to draw forth the only just inferences, and to point out, amidst a complication of causes in a given effect, the respective particulars which they only respectively illustrate and confirm. Though theory, unsupported by facts, is mere hypothesis, yet the facts themselves possess a value only as it is conferred upon them by a discriminative intellectual process; and though legitimately used to verify the conclusions of theory, yet they must needs be, at the very time, under its constant correction and supervision, thus co-operating with equal steps, and on converging lines, towards the attainment of truth. Thus also, it happens that facts, which at first sight appear to invalidate the conclusions derived from others of a higher order, are found, on a more rigid investigation, to confirm their correctness, and make more apparent the error of the opinion which it was supposed to uphold. Such is the case in the present instance; and I venture to assert, that the more successful Mr. Badnall considers himself to be in his experiments, the more completely will he establish

* We found it quite impracticable to find room for it this week; but it shall, if possible, appear in our next.—ED. M. M.

himself in the wrong; inasmuch as he proves, that the advantage he contends for is more and more conspicuous in proportion as the inertia of the mass to be moved forms the principal part of the resistance to its motion, and, consequently, that it is in this respect only, or in overcoming *inertia* by the aid of gravity, that any thing of any consequence is gained by his scheme. Now this advantage, as applicable to the *commencement* of locomotion, has repeatedly been conceded to Mr. Badnall; but he is not satisfied with it, and either wishes to extend it to the whole of the distance, or else contends for some other advantage, of which he has only an obscure perception, but which he believes is demonstrated by his experiments and his diagrams.

It is sufficient for our present purpose to consider resistance to locomotion as of two kinds,—that arising from inertia, which continues only until the carriage attains to the state of uniform motion; and that arising from obstructions, which continues during the whole of its progress. The first is overcome by gravity, on the undulating railway, in a shorter time, and at no expense of artificial power; instead, however, of uniform motion, an alternate accelerating and retarding velocity is the result, but the passiveness of rest is encountered and overcome with the advantage just admitted. The second, or obstructive resistance, demands a continual supply of mechanical power; for though assistance may be afforded by gravity at one moment, when a helping force, it must be returned the next, when an opposing force. There is no aid derived from gravity, therefore, in this particular. How is this, it may be asked, in one case and not in the other? The reply is obvious —the force required in *imparting* motion is imparted to, and resides for the time in, the body moved—the force acquired and *preserved* in one direction is competent to move it in the opposite direction to a similar extent, if there is nothing to oppose it; but in the case of obstructive resistance the force is dissipated and *lost* among the atoms in opposition to its course, and it adds nothing to the momentum, or the store of power of which it is in possession.

The simple question which now occurs is this—what are the respective amounts of the two kinds of resistance, as estimated for the whole of the distance from stage to stage? for it is on this the value of the scheme depends. Space will not allow me to show how trivial the one is in comparison with the other, and Mr. Badnall will not undertake the task, because it will disclose the poverty of the scheme. Indeed, he is not competent to it, if he considers inertia to constitute a resistance in constant action, and that really appears to be his opinion. I cannot believe that there is any intention to deceive, yet it suits his purdose better (though it is not dealing fairly with the public) to take a very short distance, such as 147 yards, and a very heavy load, such as the engine will scarcely move, and by these means making the resistance from inertia four-fifths, it may be, of the total resistance, it would be passing strange if the experiments did not establish a decided advantage for so short a space. What would it have proved, if, in experiment 6, the similar momentum acquired by the engine, with its load, in its descent of 147 yards, had carried it up to its starting point again, instead of 13 yards short of it, but that the engine had put forth a power in half the distance, equivalent to obstructive resistance for the whole distance, and that gravity had been a match for inertia? Surely Mr. Badnall can beat this exploit. If he cannot, two boys with a swing can,—for a single impulse, now and then, will suffice to keep the locomotion going. The promulgation of experiments like these is mischievous, if it be meant to say they prove that gravity can be advantageously employed, otherwise than in overcoming inertia. In this experiment it was used to cope in part with other resistance, and the consequence was, that the engine did not reach the point from which it descended, which else it ought to have done.

Your correspondent, Trebor Valentine, has an ingenious and, at first sight, a striking experiment on the subject; but a satisfactory explanation will soon occur to him, when he reflects, that the additional velocity implied by a shorter time, implies a corresponding resistance from inertia, which gravity gratuitously overcomes.

I am, Sir, yours, &c.
BENJAMIN CHEVERTON.

Sir,—At page 23 I promise to acknowledge any change in my opinion of Mr. Badnall's railway which may arise from his remarks; and to be enabled to perform this promise it is necessary I understand his meaning.

I cannot understand how *length* can be equal to *velocity*. I can readily understand that the *number* of feet, yards, miles, or any other unit of measure contained in a given line, may be equal to the *number* of feet, inches, leagues, &c. *passed through in a specified time;* but Mr. Badnall's equation V=L gives me almost as much information as I should obtain by asking the *length* of road between London and Brighton, and being told it was "equal to the *velocity* of Sir Charles Dance's steam-coach."

If Mr. Badnall will be good enough to explain his first equation on page 21, which I hope he will do, in consideration of the pains I have taken to enable him to understand my calculations, I shall have some hope of comprehending what he wishes to express by his second. At present I cannot imagine how one side of his first equation, when increased by the *addition* of D, should be equal to the other side of his first equation, when *multiplied* by D; but, doubtless, Mr. Badnall will make all this very clear.

Yours, &c.
S. Y. an Engineer.

Oct. 12.

A WORD OR TWO TO MR. SANDERSON AND MR. MACKINNON.—BY "S. Y."

When Mr. Henry Sanderson recollects that the carriage is supported by the axle; and the axle is supported by the wheel; that the wheel, being a circle, touches the inclined plane at one point only, which point is in a line drawn through the centre of the axle, and perpendicular to the inclined plane; and that, to ascertain the amount of pressure upon a surface, caused by a given force, the force must be resolved into two other forces, the one parallel to the surface, and the other perpendicular to it; he will, doubtless, perceive that the formula I gave at page 242, vol. xix. is perfectly correct. (See note, page 12.)

I believe that which Mr. Mackinnon "proved by calculation nearly six years ago," he may disprove by experiment whenever he thinks proper (See page 36.) If the doctrine which Mr. Mackinnon says was put forth by the Scotsman be correct, it follows, that the power expended on the friction in conveying a carriage from Liverpool to Manchester would be *ten* times as much, if the carriage travelled at the rate of *two* miles an hour, as would serve the purpose if it travelled *twenty* miles an hour. Your readers will form their own opinions of the worth of such a theory.

I have no doubt of the fact stated by Mr. Badnall, and quoted by Mr. Mackinnon on page 36, and it seems to me very easily accounted for; nor do I conceive the velocity need be very great to render the effect manifest.

When it is said, that "friction is equal in equal times, whatever the velocity may be," I take it to mean, that the friction will require the same expenditure of power to keep a heavy body revolving on its axle for one hour, whether it makes *two* revolutions, or *five hundred* in that time. *Can* this be Mr. Mackinnon's meaning?

S. Y., an Engineer.

Oct. 19.

DESTRUCTION OF BRIGHTON CHAIN PIER.

On the night of Tuesday the 15th of Oct. the magnificent chain-pier, which has for ten years past been the ornament of Brighton, fell with a tremendous crash, and lies now one vast heap of ruins. There was a strong gale of wind blowing at the time, "attended with heavy rain and flashes of exceedingly vivid lightning."—(*Brighton Gazette.*) The accident is ascribed by some parties to the wind, by others to the lightning, while, in the opinion of a third set of critics (see *Examiner* of Sunday last), the structure was originally so defective, that the only wonder is it should have stood so long. Our intelligent friend, Mr. Busby, who resides at Brighton, and minutely examined the remains of the pier on the morning after the disaster, is of opinion that it was caused by a stroke of lightning, and not by any fault in the mechanical construction of the fabric. We subjoin his reasons for coming to this conclusion, and shall only add that they seem to us at present perfectly satisfactory:—

Extract from a Letter by Mr. Busby to the Brighton Herald.

"I am decidedly of opinion that the Editor of the *Gazette* has erred in attributing the accident to the force of the wind, and for these reasons—first, the wind was not more violent at the time of the accident than it had been during the greater part of the day, nor than it was for two or three hours after; secondly, the Chain Pier has recently been subjected to gales of much greater violence than respecting this enterprising knight's steam carriage adventures, that we look with an unfriendly eye on the description of experiments in which he is engaged. Far from it! All that we want is to have the *real truth* made known; feeling well assured that the line of truth—which is straightforward—is the shortest road to success.

Mr. Hancock starts, on Monday next, his new carriage "Autopsy," (a Greek compound, which may be said to signify *ocular demonstration*,) to run between Finsbury-square and Pentonville. We shall give an engraving of it in our next, along with a description of the new patent process by which Mr. Hancock has got rid of the great drawback on steam locomotion—the clinkers.

LIST OF NEW PATENTS GRANTED BETWEEN THE 22D OF SEPTEMBER AND 22D OF OCTOBER, 1833.

Henry Davey, of the parish of St. Giles, Camberwell, in the county of Surry, gentleman, for certain improvements in machinery or apparatus for preparing linen and cotton rags, and other materials used in the manufacture of paper, being a communication from a foreigner residing abroad. Sept. 28; six months to enrol.

Andrew Smith, of Princes-street, Leicester-square, in the parish of St. Martin's-in-the-fields, in the county of Middlesex, machinist, for certain improvements in springs for doors and other purposes. Oct. 5; six months.

James Windeyer Lewty, of Lichfield-street, in Birmingham, in the county of Warwick, brass founder, for certain improvements in castors. Oct. 5; six months.

Miles Berry, of 66, Chancery-lane, in the county of Middlesex, civil engineer, for certain improvements in the construction of weighing machines, being a communication from a foreigner residing abroad. Oct. 5; six months.

Thomas Welch, of Manchester, in the county of Lancaster, cotton spinner, for a new method of taking up for power and hand looms. Oct. 5; four months.

William Tanner Young, of Liverpool, in the county of Lancaster, merchant, for a machine or apparatus for equalising draught, chiefly applicable to the towing of barges and other floating bodies on water, and moving or drawing carriages on land. Oct. 7; six months.

Joseph Maudsley, of Lambeth, in the county of Surry, engineer, for an improvement in the structure of certain boilers for producing steam for the working of steam-engines. Oct. 7; six months.

Goldsworth Gurney, of Bude, Cornwall, Esq., for certain improvements in musical instruments. Oct. 7; six months.

Robert Stephenson, of Newcastle-upon-Tyne, in the county of Northumberland, engineer, for a certain improvement in the locomotive steam-engines now in use for the quick conveyance of passengers and goods upon edge railways. Oct. 7; six months.

Robert Burton Cooper, of Battersea-fields, in the county of Surry, Esq., and George Frederick Eckstein, of Holborn, in the county of Middlesex, ironmonger, for an instrument or apparatus for pointing pencils, and certain other purposes. Oct. 7; six months.

Stephen Hutchinson, of 12, Pall Mall East, in the parish of St. Martin-in-the-fields, in the county of Middlesex, for certain improvements in machinery or apparatus for manufacturing gas for illumination, and in the mode or means of supplying gas to the consumer, and also in the construction of gas burners, parts of which improvements are applicable to other useful purposes. Oct. 7; six months.

Richard Barnes, of Wigan, in the county of Lancaster, engineer, for a certain machine and apparatus for producing, by the combustion of gas or oil, heated air for warming the interior of buildings, and which machine and apparatus may be applied at the same time to give light. Oct. 19; two months.

John Tennant, merchant, and Thomas Clark, chemist, both of Glasgow, in the county of Lanark, for a new or improved apparatus to produce or evolve chlorine for manufacturing purposes. Oct. 19; six months.

Jacque Francois Victor Gerard, of Redmond's-row, Mile-end, in the county of Middlesex, for an improvement applicable to the Jacquard looms for weaving figured fabrics, being a communication from a foreigner residing abroad. Oct. 19; six months.

Charles Attwood, of Wickham, near Gateshead, in the county of Durham, glass manufacturer, for a certain improvement or improvements in manufacturing or purifying soda. Oct. 19; six months.

Thomas Augustus Gregory Gillyon, of Crown-street, Finsbury-square, in the county of Middlesex, engineer, for improvements on ordnance and on the carriages and projectiles to be use therewith. Oct. 19; six months.

Herman Hendriks of Dunkirk, in the kingdom of France, but now of the Strand, in the county of Middlesex, Gent., for certain improvements in manufacturing prussiate of potash, and the prussiate of soda, and improvements in dying blue colours without indigo, being a communication from a foreigner residing abroad. Oct. 19; six months.

John Joyce, of South-row, New-road, St. Pancras, in the county of Middlesex, Gent., for a certain improvement or improvements in machinery for making nails, being a communication from a foreigner residing abroad. Oct. 19; six months.

INTERIM NOTICES.

The account of Mr. Badnall's second series of experiments has reached us at too late a period of the week (Friday morning) to be inserted in our present Number, but shall appear next week. We may state in the meanwhile that the results, *with double the load*, agree entirely with those obtained by the former experiments.

The publication of the Supplement to the last volume is, owing to an unavoidable delay in the engraving of a second sheet of autographical memorials of eminent men of science, engineers, and mechanics, which is intended to accompany it, deferred till the 1st of December.

The conclusion of Mr. Scott's able paper on Parallel Lines in our next.

As the holding of our Conversazione on the last Wednesday of each month renders it occasionally impracticable to publish the report within the same month, it will in future be held on the third Wednesday.

Communications received from T. P. A.—W. N. C.—Violino—R. H. C.—Mr. Buckland.

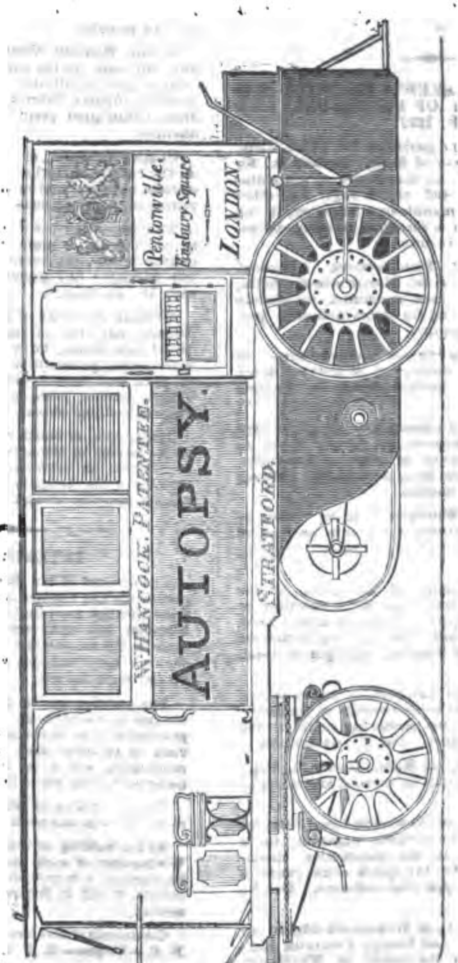
LONDON: Published by M. SALMON, at the Mechanics' Magazine Office, No. 6, Peterborough Court, between 135 and 136, Fleet-street. Agent for the American Edition, Mr. O. Rich, 12, Red Lion square. Sold by G. G. Bennis, 55, Rue Neuve, Saint Augustin, Paris.

M. SALMON, Printer, Fleet-street.

Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 534. SATURDAY, NOVEMBER 2, 1833. Price 3d.



HANCOCK'S STEAM-CARRIAGE "AUTOESY".

M. Salmon, Printer, Crown-court, Fleet-street.

effected, by a groove cast along the top of each floor at the left end, and a corresponding projection along the under side at the right end, as shewn.

The carriage proceeding, the foul floor is left behind, and from the contraction of the metal and clinkers when cold, it merely requires to be reversed to divest itself of the clinkers, and is then ready to be exchanged on the return of the carriage to the station.

This contrivance is not costly, it involves no practical difficulty, is accompanied by a material saving of fuel, and removes what has hitherto been a great obstacle to steam-locomotion.

I used to construct my boilers with partitions or flues between the chambers, formed of vertical bars of iron, framed edgeways like a grating, by which the chambers are kept a proper distance apart for the action of the fire; but I now prefer embossing the metal of which the chambers are made from the inside, and thus do without those partitions.

In fig. 3, *h* shows the sides of a chamber of this description, and fig. 4 a front view of two such chambers; and it will be readily perceived that the hemispherical projections, or embossings, meeting in horizontal as well as vertical rows, I can either have my fire under the chambers as usual, the flame rising vertically only; or I can have it in front, the flame then acting horizontally as well as vertically, as shewn by the arrows; or I can have the front of the fireplace inclined, as shewn in fig. 3, an arrangement which has its advantages, particularly in feeding the fire.

In fig. 3, *h* is a chamber; *i* the fireplace; *k* the firebars, which are either of solid metal, or of tubes connected with the boiler, or for waste steam to pass through, to prevent their burning out.

l the waste steam chamber; the steam blowing through the perforated division *m* into the fire, is decomposed into its constituent gases; *n* the tube conveying air from the blower to the fire; *o* is a slide door, by which the whole of the fire could be discharged in an instant, whenever required.

I am, Sir,

Your obedient servant,

W. Hancock.

Stratford, Essex, Oct. 17, 1833.

FURTHER EXPERIMENTS ON THE LIVERPOOL AND MANCHESTER RAILWAY, TO DETERMINE THE CORRECTNESS OF THE UNDULATING RAILWAY SYSTEM.

Sir,—Since I had last the pleasure of addressing you, we have been enabled to try some further experiments on the Liverpool and Manchester railway, the decisive result of which will, I doubt not, fully establish, in your mind and in the public opinion, the merits of the undulating principle.

On Wednesday last, the 16th instant, we met as before on *the Sutton inclined plane*. On this occasion it was agreed by the engineers present, viz. Mr. Robert Stephenson, sen., the Messrs. Dixons, Mr. Daglish, and myself, that the truth and validity of the principle, as well as the comparative advantage to be derived from its adoption, would be effectually determined by the following test:—

As great a velocity as possible being attained by the engine and load, before reaching a *given point* near the foot of the inclined plane, the *time* was to be *accurately* ascertained which the train occupied in *ascending from that point to a state of rest*.

The power being thus reversed, the time was to be accurately measured which the train occupied in *descending from a state of rest to the point* from which it had previously *ascended*.

Hence it would be obvious, that if the *descent* were made in less time than the *ascent*, the velocity generated at the foot of the plane would be proportionably greater than the velocity of the ascending train at the same point, and, consequently, the demonstration would be clear that the engine and train would not only have ascended to an opposite elevation equal to that from whence it fell, but to a *greater one*, the extent of which would be in proportion to the velocity attained.

Experiment 1.

The "Liver" engine, and a load of thirteen waggons (weighing in all 72½ tons,) after traversing a distance of three-fourths of a mile to acquire a sufficient velocity, ascended the inclined plane 278 yards, the time occupied in performing the ascent to a state of rest being 90 seconds, viz. velocity at foot of plane being about 12.60 miles per hour, and the average velocity about 6.30 miles per hour.

Experiment 2.

The *power being reversed* the engine and train *descended* 278 *yards*., viz. from a state of rest to the point from which they had previously risen, in 50 *seconds*. The velocity at the foot of the plane being about 22.70 miles per hour—average velocity about 11.35 miles.

Experiment 3.

The engine and train having traversed ¾ mile to generate velocity, ascended to a state of rest, viz. about 278 yards in 75 seconds. Velocity at the foot of the plane being about 14.12 miles per hour —average velocity about 7.6 miles.

Experiment 4.

The power being reversed, the descent of 278 yards was accomplished in 40 seconds. Velocity at the foot of the plane being about 28.32 miles per hour —average velocity 14.16 miles.

Experiment 5.

The ascent of 278 yards was made in 80 seconds. Velocity at the foot of the plane being about 14.22 miles per hour —average velocity 7.11 miles per hour.

Experiment 6.

The descent of 278 yards was accomplished in 49 seconds. Velocity at the foot of the plane being about 23.22 miles per hour—average velocity about 11.61 miles per hour.

AVERAGE.

	Total spaces passed over to generate maximum velocity before ascending.	Times occupied in ascending 278 yards.
(Experiment 1.)	1,320 yards	90 seconds
(Experiment 3.)	1,320 yards	75 seconds
(Experiment 5.)	1,320 yards	80 seconds
Total ..	3,960 yards	245 seconds
Average ..	1,320 yards	81⅔ sec.

	Total spaces passed over in generating maximum velocity in descending.	Times occupied in descending 279 yards.
(Experiment 2.)	278 yards	50 seconds
(Experiment 4.)	278 yards	40 seconds
(Experiment 6.)	278 yards	49 seconds
	834 yards	139 seconds
	278 yards	46⅓ seconds

From the preceding statement it appears, that the utmost average maximum velocity which the Liver engine could attain on this occasion, at the foot of the plane, after traversing a distance of 1,320 yards, was about 13.926 miles an hour; by which means, the power being continued, she was enabled to ascend an inclination of 278 yards.

On the other hand, it appears that the same engine, with the same load (the steam being kept up in every instance to a pressure of about 50lbs. to the inch,) generated a velocity, after descending 278 yards, of about 24.488* miles per hour, evidently proving that the engine and train would not only have mounted another summit of equal elevation to that from whence it fell, but would, *at the highest point*, have been travelling at a velocity of more than 10 miles an hour, with the full means of increasing that velocity to any desired extent over the succeding undulations.

Although the preceding experiments had, to the satisfaction of all present, decided the superiority of the undulating principle, I was anxious to know the result of a trial with a *double load*. I therefore proposed (it being too late an hour on this occasion) to attain, on a future day, a velocity of twenty miles an hour, with a double train of goods and two engines. I had, on several occasions, published my opinion of what that result would be, and I have now the satisfaction of adding the particulars of this important experiment, which, I need not say, *more than confirms* all my anticipations.

On Sunday morning last two locomotive engines, viz. the "Firefly" and the "Pluto," left Manchester with a train of loaded waggons, weighing 150 tons, exclusive of engines and tenders, the whole length of the train being about 155 *yards*.

On arriving at the Sutton inclined plane, it was determined to adopt the same method as on the last trials, of proving the merits of the principle. Our reason for appointing Sunday for this meeting will be obvious, when it is considered how dangerous and inconvenient it would be to try experiments with such a load on any other day, when the

* The velocity in these instances is calculated from the *average* number of seconds occupied in ascending and descending; thus, 278 yards being =about 6⅓ of a mile, we have the descending line 46⅓ × 6⅓=294 and 3,600 seconds ÷ 204 × 2=24.488 maximum velocity.

trains are almost constantly passing and repassing.

It may be known to some of your readers, that the French government have lately appointed a certain number of their most eminent engineers to visit this country, with a view of acquiring all requisite information, preparatory to the construction of several intended French lines of railway.

These gentlemen, nine in number, were present on this occasion; their names were as follows:—

Mons. Navier.
Mons. Goubeau, Jugement des Ponts et Chaussés.
M. Arpollet, Ingénieur en chef du Ponts et Chaussés, à Dijon.
M. Eugène Nuneann, Ingénieur des Ponts et Chaussés, No. 1, Rue Castiglione, Paris.
Mons. Dausse.
Mons. L. L. Vallée, Ingénieur en chef des Ponts et Chaussés.
Mons. J. Moistard, Ingénieur de la Marine.
Mons. Paris, Lieutenant de Vaisseau.
Mons. K. Mamgan.

The English engineers present were Mr. R. Stephenson, sen., of Manchester, (with whom I have recently entered into partnership as civil engineers,) Mr. Dagleish, sen., Mr. Dixon, sen., Mr. Dagleish, jun., and myself. In addition to whom were many other individuals deeply interested in railways, and of general scientific acquirements, among whom were Mr. Case, of Summer-hill, near Liverpool, Mr. Garnett, of Manchester (editor of the *Guardian*), and others.

The following statement cannot fail to form an interesting part of your publication:—

Experiment 1.

Two locomotive engines, the Firefly and the Pluto, being attached to the train above mentioned, and having traversed a distance of *one mile*, to generate a sufficient velocity, arrived at the point from whence the ascent was to be measured, at a velocity of about 20.28 miles per hour. The Pluto then left the train, and the Firefly alone ascended with the load (working the whole way) to a distance of 575 *yards*, 116 *seconds*—average velocity being about 10.14 miles an hour.

Experiment 2.

The power of the Firefly being reversed, the *engine and load* descended 575 *yards* in 74 *seconds*. The velocity at the foot of the plane being about 31.70 miles per hour—average velocity about 15.85 miles per hour.

Experiment 3.

The Firefly and Pluto having traversed the same distance as before, generated, at the foot of the plane, a velocity of about 14.34 miles per hour. The Pluto then left the train, and the Firefly and load ascended (power working) 315 *yards* in 90 *seconds*—average velocity about 7.17 miles per hour.

Experiment 4.

The power of the Firefly being reversed, the whole train descended 315 *yards* in 65 *seconds*. Maximum velocity 19.82—average velocity 9.91.

Experiment 5.

The same engines and load, working about 1½ miles to generate velocity, attained at the foot of the plane a velocity of about 18.32 miles an hour. The Pluto left as before, and the Firefly and load rose 457½ *yards* in 102½ *seconds*—average velocity about 9.16 miles per hour.

Experiment 6.

The Firefly and train descended 457½ *yards* in 80 *seconds*. Maximum velocity 23.22 miles per hour—average velocity 11.61. N.B. In this instance some delay occurred in reversing the power, which will account for the comparative difference in time.

Throughout the whole of these experiments it will be seen the results were so much in favour of the undulating system, that it was evident a *far greater* load than 150 *tons* could be moved by the Firefly, at an average velocity of 15 miles per hour from one summit of a curve to another. The dip of inclination being about 1 in 99, and the total length of the undulation varying from 630 to 1,150 yards.

This led me to propose a further experiment, and I think I may safely add, that one more important in result was never before tried in any country.

Experiment 7.

The two engines, as before, attained at the foot of ascent a velocity of about 19.04 miles per hour. The Pluto then left the train, and, at the same moment, the Firefly SHUT OFF HER STEAM. The whole train then rose by *momentum alone* (the weight of the train, including engine and tender, being near 164 tons,) to the distance of 323 *yards* in 70 *seconds*—average velocity about 9.52 miles per hour.

Experiment 8 and last.

The Firefly and train descended 323 *yards* (power working) in 66 *seconds!* Velocity at foot of the plane being about 20.04 miles per hour—average velocity about 10.02 miles per hour.

Thus the preceding experiments most unquestionably prove two most important facts,—not only that a given locomotive power can convey from one summit of a curve or undulation, to another summit of equal altitude, *double the load* which that same power can convey at the same velocity on the level; but that a given locomotive engine can convey, from one summit of a curve or undulation to another summit of equal altitude, *double the load* which it is capable of moving on a level at a like velocity (see last experiment), *by the employment of the steam force throughout only half the distance!*

These results lead me to go *one step farther*. It is my opinion, that if such a weight were to be added to the 150 tons moved on this occasion, as would be a *maximum load for three locomotive engines* on a level at 15 miles an hour, the Firefly alone (her power being equal to either of the other engines) *would move* the whole train from one summit of a curve to another of like altitude, at an equal average velocity, viz. 15 miles per hour.

If any of your readers, whether witnesses or otherwise of these interesting experiments, can correct any error of opinion or of statement in allusion to them, I shall be exceedingly happy to recognise and acknowledge it. In the meantime I think, Sir, I may congratulate myself upon having stamped, by this letter, a value that will be long appreciated on the correspondence (*pro* and *con.*) which your Magazine contains on this subject; and I am as happy in feeling that every individual who witnessed the recent experiments was fully satisfied with the importance of the results, as in believing that, in defiance of prejudice and long-formed erroneous opinions on this subject, the public will before long acknowledge, appreciate, and be benefited by the "UNDULATING PRINCIPLE."

I am, Sir, with great respect,

Your very obedient servant,

RICHARD BADNALL.

P. S.—I have not yet seen your last Number. "S. Y.'s" remarks in the previous one shall be noticed. In the meantime, he does me injustice in supposing I have ever indulged one contemptuous feeling toward him." I could not indulge it to a worm—much more to an individual whose good motives, in a scientific discussion, I have never questioned, and in answer to whose remarks I have bestowed time, attention, and labour.

THE UNDULATING RAILWAY.

Sir,—Mr. Badnall has replied to the observations which I addressed to him respecting the merits of his undulating railway. Can it be expected from me, or would the interests of your Magazine permit, a detailed refutation of the long tissue of errors and absurdities which characterises this reply? Is it possible that a gentleman of any reading or reflection, whatever degree of "self-assurance of the goodness of his cause" he may possess, however "imaginative and enthusiastic" he may be, could so rally his courage to the sticking point, as to make such assertions as these—"that the resistance of the air does not act as an opposing force with greater intensity at high velocities than at low velocities," and that "the continuance of uniform motion involves a very considerable expenditure of steam power, independent of what is required to cope with locomotive resistance." What can be done with such an heroic and chivalrous contempt of all knowledge and of all experience? Are we "to do honour to the new light," and acknowledge "this *beau ideal* of a good controversialist" to be one of the *illuminati*, nay, to be the hierophant himself, destined to expound "the principles of locomotion in a manner they were never so clearly elucidated before." Or must we, despite of Professor Crackwell and many more, myself included, who are "quite sick of this undulating railway controversy," and in contempt of the utilitarian character of your pages, descend to the task of "teaching the young idea how to shoot," and instruct Mr. Badnall in the very rudiments of mechanical knowledge? For myself, I prefer rather to invite him to abandon the study of the rapid "flight of swallows and gulls," to abjure the art of catching the one or cramming the other, "to glean information" no longer, nor any thing else, "from these locomotive creatures," to forget all that he has ever said or done, to take again to his books, and to bring his mind, as a *tabula rasa*, to a second pupilage in philosophy. If, however, he would wish to vary the severity of his mathematical studies, relax a little from "diagrams, symbols, and figures," and verify "mathematical demonstrations" by experiment and practice, I would advise him, like the bee, gathering from every opening flower, to accost the first old woman he meets that has an umbrella open, and, by accelerating her into a trot, he will soon find, by a uniformly increasing volubility, that the resistance of the air does in very truth act both as a provoking and as an opposing force, with greater intensity at high velocities than at low velocities."

How is it possible, Mr. Editor, to suppress a smile at errors such as I have adverted to; and these are only a specimen of Mr. Badnall's lucubrations. Can it be required of me seriously to refute such assertions as these, that "the greater the velocity of a steam-packet, the less the friction;" that "the total resistance of the atmosphere throughout a given distance is *less* at high velocities than at low velocities;" that this diminution arises "from the inclination which all bodies, when in rapid motion, have to rise from the surface of the earth, consequently, from a denser to a lighter atmosphere;" that "the power which first overcomes the resistance of the air continues to overcome it without any sensible alteration;" that "a feather even would, in a perfectly still atmosphere, descend with *uniform* acceleration;" that "the velocity of a pair of carriages, descending by gravity an inclined plane, must be *uniformly* accelerated, until the resistance of the air becomes equal to the force of descent;" and that "this resistance cannot be, until the atmosphere becomes absolutely compressed." The eye may laugh, and the lip may curl, but as to "irritability and wrath," Mr. Editor, it is quite out of the question; these emotions are not to be excited by erroneous opinions, nor by the marvellous lack of judgment which could obtrude such crude and incorrect statements as these in elucidation of the principles of locomotion. The only circumstance in scientific controversy calculated to disturb one's equanimity, is an appearance of manoeuvring in the way of managing a quotation. If a proposition is laid down, subject to limitation or explanatory distinction, there is no candour displayed in quoting it without such limitation or distinction. I had just cause of offence on a former occasion, but I forbore to complain. In Mr. Badnall's present reply, maugre his being "the beau ideal of a good controversialist," he quotes me as saying, that "it would be an idle assertion to say that locomotive steam power is not, like gravity, a *constant* force;" and upon this unauthorised assumption that I contended for steam force being equally constant like gravity, which he would gladly consider (as indeed he maintains) to be the main point in dispute between us, he proceeds, after a fashion of his own, to demonstrate that it is not a similar constant force. I did not say it was—my words were, "Forces can be considered as only of two kinds—those which operate *continually*, like gravity, and those which act only for an instant by impulsion. It would be an idle assertion to say, that steam locomotive force is not of the latter but of the former kind," &c. Here is nothing about *constant* force, but of steam force operating *continually*, in contradistinction to an instantaneous action; and yet this controversialist, who is "so courteous to his opponents, and so true to himself," (but not to others) omits all mention of this distinction, and even falsifies the terms that are employed. So far from insisting on the necessary equability of steam force, I stated, a few lines following this quotation, "that although constant or unintermitting in regard to *time*, it may not be constant in respect to *intensity* of force;" and that "it is altogether a matter of construction, whether steam produces continually an equable force." Is it to be inferred, that this controversialist, "so cool, pains-taking, and methodical," overlooked all this in his eagerness to catch at a straw, which may save him from drowning, and forgot at the moment that honesty is the best policy, even in quotation? Or may we with greater charity imagine, that this "able expounder of doctrines so sound as his," was really incapable of comprehending such like distinctions and differences? If so, I am disarmed, and asperity shall have no place.

I left the question open about the uniformity of steam force, as it was not desirable to clog the subject with a discussion on collateral points. I stated that it may or may not be equable, according to the construction of the locomotive engine. I will now, however, show how, theoretically, it may be effected, even at the highest velocities which safety will permit. Whether ingenuity can bring it practically into effect, or whether it is necessary so to do, is quite another question. The employment of steam expansively, which to a moderate extent is a useful expedient, precludes the idea of any other than an average equality of force. First and foremost, there must be plenty of steam—an incessant supply, fully commensurate with the intended velocity. (2.) There must be no wire-drawing of it, as it is technically called, nor shutting it off, but at the full stroke. (3.) There must be two or more cylinders. (4.) They must be large, and the motion of the pistons must be slow, the needful velocity being effected by intermediate machinery. This last condition will meet Mr. Badnall's first reason, why steam force "*never can* become a constant force." But in reality, the most rapid motion of the pistons is so little, in comparison to the immense velocity with which steam rushes into even a partial vacuum, that little diminution of force can arise from this cause, unless as connected with a wire-drawing of it in narrow passages, between the boiler and the cylinder. It is this last circumstance, together with the immense demand for steam at high velocities, which precludes it from having the quality of an equable force. To illustrate this, I may mention, that a really ingenious projector of a scheme in which velocity of action was every thing, actually made the passages, if I recollect rightly, one-eighth or a tenth part of the size that they ought to have been. These four conditions being fulfilled, steam force to the extent of the intended velocity (mark this limitation, Mr. Badnall,) must necessarily be constant in any sense of the term. By no means, says Mr. Badnall; for, "exclusive of the resistance of the atmosphere," which very relevant argument he is so obliging as not to enforce, there is the diminution "of the pressure of the engine on the rails, according to the velocity with which it moves." This is the second of his two causes, "equally invariable and equally true." So, then, the cause of a waste of power *after* it is developed, is a cause why it is not *equably* developed; and the reason why steam force is not constant, is because the rails being smooth the force is misapplied. Really, Mr. Badnall's logic and his notions of etiology are, equally with the "soundness of his doctrines," past all understanding. A slipping of the wheels on the rails will undoubtedly be an evil, but it will be quite time to remedy it when it arrives; at present, it is not supposed to be a very conspicuous defect, and may be rectified by a griping action on a rail, as patented by Messrs. Vignoles and Ericsson, or by Saxton's ingenious locomotive pully, or by lessening the load to be drawn by each engine; but Mr. Badnall, with a true Irish perception of things, proposes to cure it by making one-half of the road all uphill.

By some means or other, either in consequence of "an expensive education," or "twenty years reading and attention to scientific subjects," Mr. Badnall has got hold of the idea that a constant force necessarily implies as its result, a *uniformly* accelerated velocity in a resisting medium, "until the velocity attained be equal to that at which the atmosphere would rush into empty space," when, I suppose, the resistance commences all at once, and a change takes place from *uniform* acceleration to uniform motion, also all at once. Nevertheless, it is not to be understood, that he "*for* one moment argues that the atmosphere *does not* offer a resistance to locomotive power, or to the force of gravity," or that "*a high wind*" is not a source of inconvenience. But this, if peradventure it be an inconsistency, is a mere bagatelle, and may be explained by the circumstance, that a high wind causes an obstruction, by the air acting with a swift motion against the carriage, but which is a very different case from the carriage acting with a swift motion against the air, which on all such occasions, it is to be inferred, kindly gets out of the way. However, this subject will be more "clearly elucidated," together with the new law of the resistance which air opposes to the motion of bodies, in "the separate and distinct publication" in which Mr. Badnall "intends to introduce it to the public for the first time;" for it appears, "that swallows and sea gulls have satisfied him that our ideas are extremely imperfect" in regard to the received opinion, that "the resistance varies nearly as the squares of the velocities." In the mean time, it will be well to abide by the old law, proved as it is by "mathematical reasoning," and verified by experiment; and this brings me to the subject of the greatest velocity attainable by locomotive engines.

It was stated by me, that the resistance of the air is the immediate and only efficient cause of bringing about uniform motion—and so it is. This statement, however, is ridiculed by Mr. Badnall, and misunderstood, I imagine, by your correspondent S. Y. It should be observed, I do not state that it is the *only* cause, but the only *efficient* cause, in contradistinction to negative and conditional causes. Undoubtedly, if there is not power enough, or if there is not steam enough, or if there is not fire enough, or if there is not water enough, or if there is not attention enough, any of these causes, *sine qua non* as the schoolmen used to say, will, conjointly with the positive causes of friction, and the resistance of the air, produce uniform motion; but the last is the direct and efficient cause, inasmuch if these conditions and their like be fulfilled, it constitutes the *only* cause of the velocity becoming uniform, and such is actually the case when gravity is the moving force. It is perhaps to be regretted, that, with the jargon and inanity of the schools, we have too much discarded their distinctive and appellative phraseology. I mean, that it would be well if we were more accustomed to discriminative, exact, and current, but English expressions, in reference to causes in general. It would mark our meaning and our reasoning with greater precision, and save an immensity of misunderstanding. To wave, however, every thing like logomachy, I proceed to show how it is that the resistance of the air produces uniform motion.

Take a locomotive engine and double its velocity, and what do we do? We will say nothing of inertia. The resistance arising from friction will continue the same, but then the velocity of the force which copes with it is doubled; we, therefore, double the expenditure of power in a given time. In regard, however, to the resistance of the air, it does not continue the same, for the quantity of air encountered by the engine is doubled, and the velocity with which it is struck is doubled—the resistance therefore is quadrupled, or it increases as the square of the velocity; but the velocity of the force to cope with it is also doubled—we therefore octuple the expenditure of power in a given time. So that whilst the demand for steam is, in regard to friction, simply as the velocity, it is, in regard to the resistance of the air, as the cube of the velocity. Hence it will be readily seen how rapidly, as the velocity increases, this kind of resistance will exhaust the most powerful means for the generation of power, although at the first it may scarcely deserve notice; and also, that if the force be constant at all velocities, friction alone is incompetent to produce uniform motion, for that also is constant at all velocities; whereas the resistance of the air, increasing ever as the square of the velocity, must necessarily, sooner or later, bring about a uniformity of motion. Obliged to express myself in a manner suitable to the capacity of a child, and for which an apology is due to your readers, I hope Mr. Badnall will now endeavour to perceive that necessity:—if it is beyond his comprehension I cannot help it.

He is, however, competent to appreciate the value of any new truth in philosphy, which may happen to be discovered in these latter days of enlightenment, and hence he has adopted for his protogé—if indeed the bantling be not his own—a new kind of force—yclept *periphfugal*, which even escaped the notice of Sir Isaac Newton, who would have perceived therein "a most extraordinary confirmation of his most favourite and his noblest theory." How his mighty shade, if indeed cognitive of things in this nether world, must rejoice to see the day that we see, and to witness a portion of the universal force embodied by the "imaginative and enthusiastic" genius of Mr. Badnall in the periphery of the wheel of a locomotive carriage, performing a cycloidal dance on the railway tract, progressing head over heels with a somersault flight, and becoming at every step, "an *assistant motive power*," though the course on which these capers are cut is a perfect level; and thus enacting, before his astonished sight, the pre-eminent wonder of the nineteenth century, at the mention of which all past and future projects for a perpetual motion shall hide their diminished heads.* Philosophers of England, nay, of all Europe, ye have heard the challenge which has been given to you by this redoubtable knight-errant, on the point of whose lance the mantle of Newton's prowess has fallen: why do ye not enter the arena with this "beau ideal" of a very Quixote in philosophical enterprise and adventure? Wherefore do ye not *now* take up the gauntlet, now that he has shewn himself worthy of your onset, and not, as heretofore, in "*contemptuous* silence *mete* his praise?"

We will now leave all further discussion on incidental points, and devote the little remaining, but appropriate space, to the real merits of the undulating railway. Unfortunately, I have nothing in the way of reply to say to Mr. Badnall on this head, for he has neither acknowledged nor denied the correctness of estimating locomotive *duty*, by multiplying the resistance into the distance; neither has he attempted to show that his scheme renders the duty less in either of these particulars. Your correspondent S. Y. has very properly said, that steam locomotive force has to overcome resistance from friction, resistance from air, and resistance from inertia. The first particular does not belong to the part that I have taken in the discussion, neither is it considered by Mr. Badnall's supporters to be the prominent advantage which is to result from the undulating scheme, one of them admitting, that the *rolling* portion of the friction would be "somewhat more" upon the inclined plane, "being of greater length;" and another saying, that "with regard to friction, it may be considered the same in both cases, certainly not more in the curved than the horizontal line."† I may, therefore, be well excused for omitting to take any notice of it. The second particular, or the resistance of the air, I have shown, will be augmented by the alternate high and low velocities, not merely in respect to the greater distance gone over, for that is trivial, but in regard to the intensity of the resisting force. Now, as this kind of resistance at thirty miles an hour is, at a low estimate, three or four times that which arises from friction, any increase herein is a consideration by no means to be overlooked; and yet Mr. Badnall has not deigned to notice this point. In regard to the third par-

* "The force of gravity not having time to act vertically, is absolutely become an assistant motive power; it is gathered from the periphery in contact with the rail to the extreme part or top of the wheel, and is all thrown off again between that part and where it again comes in contact with the rail."—Mr. Badnall, page 439.

† The question regarding friction is not a simple one. The consideration of the centrifugal force which is generated, is by no means to be disregarded, for though small, the difference of effect, let it be which way it will that it has to modify, is also small.

ticular, or the resistance from the inertia of the mass to be moved, there is certainly an unimportant advantage, which has generally been conceded to him. This is the particular, I suppose, to which you, Mr. Editor allude, when you give it as your opinion, "that the force of gravity may be brought in aid of longitudinal locomotion to advantage." This is the particular advantage which your correspondent, Mr. Sanderson, has very unnecessarily proved to belong to the scheme. He is, however, too conversant with the science of the subject, to be, as he says, "the thick and thin advocate of Mr. Badnall" in all his phantasies. This, also, is the particular advantage, for the proof of which certain experiments were instituted at the National Gallery, Adelaide-street, for the purpose of convincing Mr. Badnall and his friends then present, that it was in this way their own prior experiments, as indicating the superiority of the undulating railway, was to be explained. But no, such explanation would not do—the advantage was too inconsiderable, and only at the outset: they contended for an advantage conterminous with the whole length of the line, and to be derived in some mysterious way, independent of the consideration of friction, from the curvatures of that line. I have myself repeated these experiments, and find, that when the carriage enters on its course with considerable momentum, one railway is as good as the other. If then you, Mr. Editor, and Mr. Badnall's scientific supporters, mean only to say, that apart from the consideration of friction, the advantage of the undulating scheme consists in this,—that the inertia is overcome with a saving of time and power, there is really nothing in dispute between us, for the same explanation and admission has already been presented to Mr. Badnall and his more immediate abettors, and refused; and the only observation we have to make is, that the same thing, if it were thought advisable, may be effected in a more convenient manner. If, however, you contend for other advantages, we are entitled to ask, whether you conceive there exists any other species of resistance to longitudinal locomotion than what has been enumerated, which, by the assistance of gravity as a motive power, you propose to *lessen*?— If not, then the only alternative is, that you propose to *cope* with resistance (from friction and air) by the aid of gravity; and then we ask, do you really expect to procure that aid gratuitously, and to avail yourself of that power, on a general level, without being under the necessity of repaying such assistance?—Assuredly, not. There is only one other question possible—Do you then expect, that whilst receiving aid from the force of gravity, the steam can be husbanded to such advantage, as to be more than able to return it again, when required, on the ascent? If this be the advantage you mean, then we say again, there is nothing in dispute between us; for this idea, such as it is worth, was presented to Mr. Badnall by myself, and the only observation we have to make is, that it is a mountain, or at least an undulation, bringing forth a mouse.

Whether, in engineering operations, a few curves here and there may be introduced, with a propriety and an advantage referable to the surface of the country, is quite another question, and is neither affirmed nor denied; but it is strenuously contended, and with a confidence which cannot be shaken, that there are no advantages peculiar to undulations, *as* such, except as before excepted; and that there are none whatever of sufficient weight, the disadvantages also being considered, to justify the conversion of a dead evel into a series of them.

In conclusion, I beg to observe, that Mr. Badnall was not pleased with my "gentlest treatment:"—"a pat on the cheek he has detested from a child." He was subsequently better satisfied with my "stern and magisterial advice." I therefore hope that the tone of my present communication is exactly to his taste. He has now a smile and a nod, and I hope I shall have no occasion to use a rod; but he must eschew for the future the garbling of quotations.

I am, Sir, &c.

BENJAMIN CHEVERTON.

P. S.—I have a few words to say to two correspondents in a recent Number. Does "Kinclaven" really believe, that, apart from the power afforded by gravity, to overcome the inertia arising out of the additional velocity, the same display of power in a *given time*, would not carry a body on the horizontal line A B in as quick time as on the curved line A D B? The assumption of "the same power being applied in both cases" (but let it be remarked) in *different times*, is a complete begging of the question; for what is to prevent the putting forth of that power in the shorter time on the horizontal line, as well as on the curved line, and thereby effecting the same thing, inertia being excluded. As "an engineer who is, or ought to be, a mathematician,"—but perhaps he is not an engineer, and only a mathematician,—he certainly is aware, that the same constant force is competent to maintain *any* velocity which an extra force may have imparted to a body, abstracting the resistance of the air. Whether that extra force be gravity, or any other, can (as employed in overcoming inertia) be of no consequence; and if the maintaining force be not constant, by reason of the practical difficulties attending its generation at the high velocity which has been given to the body, the same circumstance must occur, whether it traverse the curved or the horizontal line. Your correspondent "S. Y." has shrewdly observed, in reply to a statement similar to that of Kinclaven, that "we can always gain speed at the expense of power, without the inconvenience of curved rails;" meaning a greater expense in a *given time.* By "the same power being applied in both cases," I take it for granted he means efficiency, or the expenditure of power, and not the impelling force; for surely Kinclaven does not need to be reminded, that the point at issue regards the *momenta* of the resisting and impelling powers.

I should have really felt obliged to "Mentor" if he had corrected any erroneous statement of mine, but at present I have not to thank him for such an act of kindness, as I have not fallen into the very gross error which he imputes to me, and which he himself must have perceived, had he done me the favour to read what I have written; but he has relied on the perversions and false statements of Mr. Badnall, which affords me further just cause of offence against that gentleman for his disingenuous conduct. To show that I was perfectly aware of "the accelerations being unequal in equal times," in the cases he refers to, I beg to draw his attention to the following equivalent expression in my last communication, "steam is not constant, it may be in respect to intensity of force, and consequently would not produce *uniform* acceleration (independent of the air)." All my statements, in respect to the similar effects produced by steam and gravity, were hypothetical, and carefully guarded by conditions; so that neither Mr. Badnall nor any one else need have mistaken my meaning, any more than your correspondent "S. Y." In return, however, for "Mentor's" intended kindness, I beg to point out to him, that the force of steam is not very analogous to those other mechanical forces, acting by impulsion, to which he would seem to assimilate it. I see no similarity between force by impact and force by pressure, nor any relevancy in citing one in illustration of the other. Would "Mentor" calculate on the *relative velocity* of steam in an estimate of the power of a steam-engine?

ANCIENT ARTS AND MANUFACTURES.

The volume of the *Cabinet Cyclopædia* for the last month has not a little disappointed our expectations. From the more prominent portions of its title-page* we had been led to expect a considerable fund of information on a subject of much and various interest—the state of the useful arts among the two greatest nations of the ancient world. We expected it in vain: it is true that the whole of this volume is expressly devoted (in the table of contents at least) to "The Arts, especially the ordinary ones of Life," yet the author allows himself so wide a range, under this tolerably comprehensive head, that the unfortunate "ordinary arts of life," so "especially" to be attended to, come off, at any rate, only second best, in comparison with their more refined, and perhaps more attractive sisters—Painting, Sculpture, and Architecture. The editor admits in his preface that too much space has been allotted to the last, but promises that we shall find the second volume less disproportionate. This second volume, however, can make no difference as to our ground of complaint, since it will not have any reference at all to arts and manufactures, but be wholly devoted to the elucidation of the "laws, literature, philosophy, religion, manners, and customs" of the ancients.

The greatness of the disproportion is at once apparent from the fact, that, in the first section,—that "on the Arts of the Ancient World, especially Greece,"—the chapters on "Architecture, Sculpture, Armour, and Arms," occupy no less than one hundred and seventy-nine pages, while the remaining one, on "Private Life, Manufactures, and Costumes," is got through in only fifty-six, and a great portion of even this small space is filled up with excessively dry catalogues of trades, the names of tools, &c., and such interesting *useful* information as this: (from Theophrastus?)

"A Greek bore (περι αηδιας) waked people, for idle gossip, when they were just going to sleep; talked about the physic which he had taken during meals—talked about water being cold in the cistern—the garden vegetables tender—and his house as open as an inn."

It may therefore well be conceived, that where there is so much draff, little room is left for the solid grain. We will attempt, nevertheless, to exhibit a specimen, if possible:—

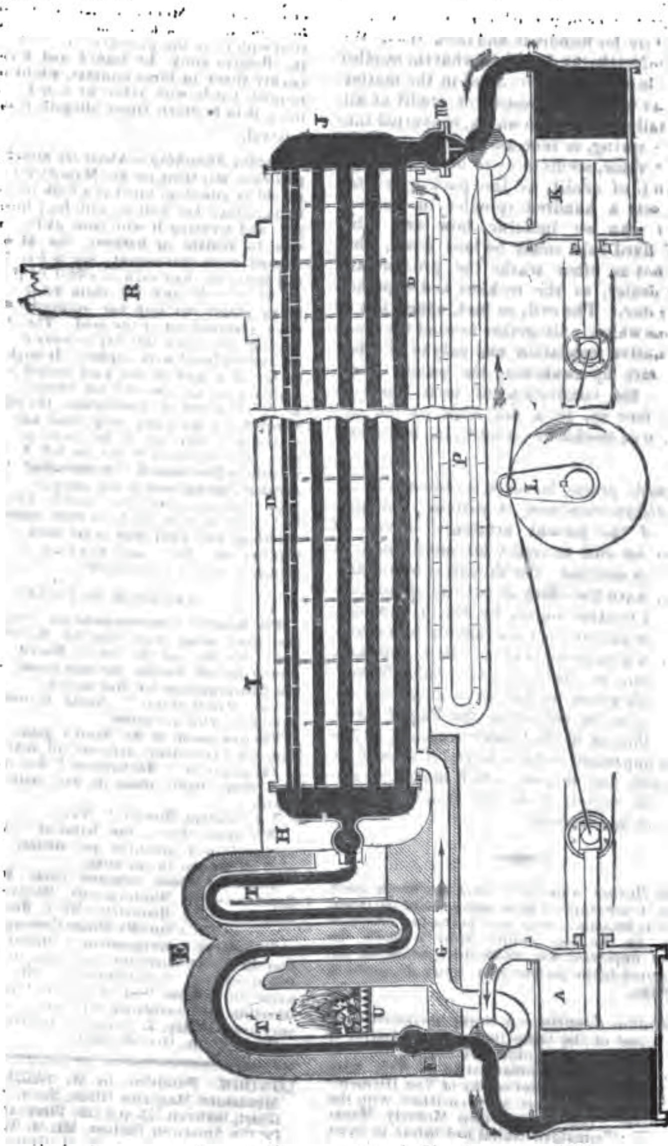
"The remains of ancient art show that there existed mechanical powers of raising large weights, and effecting superb archi-

* A Treatise on the Arts, Manufactures, Manners, and Institutions of the Greeks and Romans. In two volumes, vol. 1. (Cabinet Cyclopædia.) London, 1833. Longman & Co., small 8vo, pp. 365.

Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 535. SATURDAY, NOVEMBER 9, 1833. Price 3d.



ERICSSON'S CALORIC ENGINE.

M. Salmon, Printer, Crown-court, Fleetstreet.

the Holborn end of Field-lane to Turnstile: Field-lane rising in its whole length 1 foot 3 inches 4 dec.; and from the further ascent from the bottom of Great Saffron-hill to Turnstile, as before stated, of 15 feet 1 inch 2 dec. The level at Sharp's-alley is nearly the same as that at the lowest end of Turnstile. Therefore, if we adopt an inclined plane throughout this long line,* we shall obtain a height sufficient for a well-proportioned arch over Saffron-hill, and a roadway whose inclination will be scarcely felt.

It has been previously stated that the fall from Brook-street to the bottom of Holborn-hill is 42 feet 6 inches 2 tenths, but it must not be considered that the descent is regular throughout the distance, for in some parts there will be found an inclination of 1 foot in 15 feet, and in others, 1 foot in 19 feet, &c. As my object is to preserve the thoroughfare of Holborn as far as is consistent with the welfare of the public and the preservation of its present trade, I would consider my new line as a second-rate street, principally designed for the passage of heavy vehicles, or those that require greater expedition to and from the City. The light carriages, foot passengers, &c. might pass and repass by Holborn-hill as usual.

The new rail road that is to end northward of my line would probably be much benefitted by the improvement; as the transportation of goods, &c., eastward and westward would be thereby greatly facilitated.

I shall conclude for the present by giving the admeasurements of the portion of the new line, commencing at the north end of Brook-street, and ending at Great Saffron-hill:—

	FEET.	IN.
Length of Brook-strreet (fom Holborn to Greville-street	319	0
Length of Greville-street..................................	310	8
Width of Leather-lane...	28	2
Length of Charles-street...................................	183	0
Width of Hatton-garden.....................................	54	10
From Hatton-garden to Turnstile............................	358	0
Length of Turnstile..	47	0
Width of Saffron-hill (at that part)	18	0

The conclusion of this survey I hope shortly to place before you.

I am, Sir, yours very truly,

CHRISTOPHER DAVY.

3, Furnival's-inn.

THE UNDULATING RAILWAY.

Sir,—I have this morning (29th October) received your Numbers of the 19th and 26th instant. My principal object in now addressing you, is to make an observation or two, which Mr. Cheverton's last communication immediately demands. In the course of a few days I shall trouble you with more general remarks in answer to "S. Y.'s" objections, to which the contents of your next Number may possibly enable me to add something in further reply to Mr. Cheverton.

Mr. Cheverton says, in allusion to the comparative amount of resistance on the two lines, "Mr. Badnall will not undertake the task (of shewing how trivial is the difference), because it will *disclose the poverty of his scheme.*" In the same breath, also, he observes, that "space will not allow him (Mr. Cheverton) to shew how trivial one is in comparison to the other."

Thus Mr. Cheverton, in a most unwarrantable manner, accuses me of withholding the truth when I have the power of publishing it, which *truth, if exposed*, would (he says) prove the poverty of my scheme; and yet, after nearly a *twelvemonth's* discussion, he, *for want of space*, declines to touch upon this subject. I appeal to all your readers, *is this generous?*

But Mr. Cheverton endeavours to sweeten this bitter observation, by saying, "I cannot believe that there is any intention to deceive, *yet it suits his* (Mr. Badnall's) *purpose better* (*though it is not*

* The length here alluded to is from the west end of Greville-street to Lower West-street, at Sharp's-alley.

dealing fairly with the public) *to take a very short distance*, such as 147 yards," &c. &c.

There is no intention of deceiving, and *yet I am not dealing fairly with the public!!!* I trust Mr. Cheverton will have the good feeling to revoke these expressions, or, at least, to explain them; and if not, I trust he will exclude Mr. R. Stephenson, Mr. Dagleish, Mr. Dixon, and the other engineers under whose joint inspection and superintendence most of the experiments were made, from any participation in a wish *to deal unfairly with the public.*

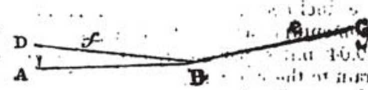
I am not one, Mr. Editor, who feels disposed to quibble about trifles, or, in discussions of this kind, to be disturbed by every burst of anger from an opponent whom, in this instance, *I feel within my grasp*; but I offer my unqualified protest against the right or propriety of any man attributing *unjust motives* to another without a cause which he is able to substantiate.

Mr. Cheverton's remarks about inertia and gravity are becoming as familiar to me as my "*hic, hæc, hoc.*"

His simile of "the two boys with a swing," would, I should have imagined (as some time ago alluded to by your correspondent "Saxula") have led him to reflect rather more deeply on the subject before us. Had I told him, before this discussion was entered upon, that I could, by means of my own arm, and without the aid of any assistant artificial power, raise a ton weight above the level of my head, he would not, perhaps, have believed me; yet he knows very well, that if a ton weight were suspended but a few inches from the earth by a rope of sufficient strength and length, and from a prop of sufficient strength, there could be no difficulty, by gradually increasing the oscillations of the load, in attaining the required elevation,—nay, by the simple application of an equally simple contrivance, to retain the load at that very elevation when attained. Now, although throughout each oscillation of this pendulum, the effect of gravity from each descent of the weight may be said to be destroyed in the corresponding ascent, would the power of gravity, or would it not, be auxiliatory in the accomplishment of this task?

With regard to 147 *yards* being too limited a distance to suit the trial of a fair experiment, Mr. Cheverton must blame the locomotive engine, not me. The inclined plane is 1½ mile in length, and if the Rocket engine had not sufficient power to reach a higher elevation, was it *my fault?* The experiments which were *last* tried will surely satisfy Mr. Cheverton that he has done me injustice, in supposing that I *chose* the *worst engine*, from any supposition that *it would best support my arguments on this subject.*

As it appears that the deductions which are drawn by engineers from these experiments, are not fully understood by many who have perused the particulars of them, I shall beg your insertion of a diagram, published by the editor of the *Manchester Guardian*, which will, probably, be found to render my ideas as clear as they can be rendered on this subject:—



A B is a level line—B C, equal to B D, forming an undulation D B C, whose summits are of equal altitude.

The train of loaded carriages, weighing 150 tons, and moved by two engines, one dragging, the other propelling the load, had acquired from A to B (the length of this line being 1 mile on the railway), a velocity of 20.28 miles an hour. On arriving at B the steam of the propelling engine was shut off, and the engine stopped. The train then, partly by the momentum acquired in travelling over the level railway, and partly by the power of the remaining engine, ascended the inclined plane to the point C, viz. 575 yards, which was as far as the united locomotive force of one engine and the momentum could carry it. The power was then reversed, and the engine pushed the carriages back from C down the inclined plane to B, at which point it was found that the descending velocity was 31.70 miles an hour. Now as the velocity of 20.28 miles an hour at B was sufficient, with the aid of *one engine*, to carry the whole train from B to C, what man living will dispute that a velocity of 31.70 miles an hour at the same point, would, with the same engine, have enabled the train to ascend the line B D, which is supposed to be precisely equal to the line B C? On the contrary, would there not be *a given velocity* generated at the point D, which, the effective power of the engine being still continued, would have enabled the train to pass over another *like* undulation? If so, one engine could move 150 tons along the undulation C B D, which amount of tonnage she could not move on a level road, and which *on a level road, whatever velocity was given to the train at starting, would bring the engine gradually to a halt.*

But let Mr. Cheverton direct his attention to the two last experiments which were tried. If *a momentum* (*momentum only*) arising from a velocity at the point B of 19.04 miles per hour, carried the train 323 yards—say as far as *e* on the inclined plane B C—would not a momentum, arising from a velocity of 20.04 miles per hour, carry the same train to the point *f* up the ascent B D? If so, the Firefly engine proved her capability of moving 150 tons along an undulation *e* B *f*, by the employment of her power throughout only *half* the distance, and if this were the case over *one* undulation, will Mr. Cheverton deny *that such would have been the case over succeeding undulations?*

During the next week I may, perhaps, have an opportunity of trying a few more experiments: if so, they shall be transmitted to you. I have expressed my opinions to the *Liverpool Directors*, that the *Firefly engine* is capable of conveying from one summit of an undulation to another the enormous load of from 200 to 250 tons. I may have exceeded the limits of her power in this prophecy; but will Mr. Cheverton admit one point before he hears the result of any further experiments? viz. that *if the Firefly*, or any other engine, can move even 200 tons from summit to summit of *one* undulation, that she is capable of moving an equal load from one summit to another summit of a *second* or greater number of like undulations?

If so, what becomes of his weakest and most untenable argument, that *all I gain* is the comparatively *trifling advantage which gravity gives me at the commencement of locomotion?*

I am, Sir, with great respect,
Your very obedient servant,
RICHARD BADNALL.

October 29, 1833.

N. B.—The subject of friction (trivial as Mr. Cheverton thinks it) I shall not be ashamed to discuss in my general answer to "S. Y."

Sir,—Mr. Babbage somewhere observes in his "Economy of Machinery," that no author can be a judge of the force of his meaning until he sees his words in print. This observation was immediately verified when I read the first paragraph of my reply to "S. Y." in your type: it was, to say the least, uncourteous to that gentleman, and quite unintentional on my part. I am anxiously desirous of discussing all subjects in perfect good humour and civility.

As "S. Y." still asserts that my problem has nothing to do with the subject of controversy, which he defines to be—"which railway is the best," I must beg you to afford me the opportunity of saying, that I have never regarded it in that light. I maintain that the question really is, *whether or not it be necessary in future to undergo the great expense of deep cutting, embanking, and tunnelling, in the formation of railways.* I am very desirous that the subject should be submitted to the most rigid investigation, because a sum little short of one million, or nearly one-third of the whole expense, is set down, *for this purpose alone*, in one contemplated road from this city to London, under the title of the Great Western Railway. We are therefore most especially interested in the true solution of the question as I have stated it.

I take the earliest opportunity of acknowledging that "S. Y." is certainly correct in his solution of part of my problem, and I only regret that he has stopped at the half of it: had he proceeded he must have confirmed the truth of my assertion, that more than 100 seconds would be gained by taking the undulating road. My calculation was in haste, and made erroneous by substituting *time for height*; but as the same mode was adopted for the upper line, the proportion was still preserved.

I should be happy if "S. Y." would also make his remarks on the latter part of my letter, page 380.

And now that this gentleman, Mr. Badnall, and myself, are agreed that a carriage, under precisely similar circumstances, will travel over my undulating road in 100 seconds less time than over the straight one, I will proceed to draw my conclusions, premising that I have not the slightest intention of advocating the belief that undulating roads afford any increase of power to locomotive carriages, for *ex nihilo nihil fit*, but as all motion in straight lines (level or descending) with a constant power, must be an accelerated motion, and if we assume that 40 miles per hour is the utmost safe velocity that can be employed, it must make a considerable difference in the time of travelling, whether we obtain this ultimate velocity soon after starting or only at the end of our journey. Herein then, I conceive, rests the true explanation of the apparent paradox—that a carriage will reach a certain point quicker over an undulating road than a straight one.

I again disclaim any allusion to the practical operation of the plan, or to the question whether or not any power of human invention will ever exist which shall be constant under all velocities; but I think that we have such a power, even beyond the limits I have assigned. I conceive that the *theory* is completely decided in Mr. Badnall's favour—let that gentleman's opponents acknowledge it, and *then* proceed to the practical objections.

Yours respectfully,

J. Ham.

Bristol, Oct. 15, 1833.

Sir,—I am sorry to find myself misunderstood by Mr. Cheverton; I must assure him he does my judgment injustice, if he supposes the observations I made upon *atmosphere* were addressed to him, or such as resemble him in talents and acquirements.

I intimated that friction *alone* was sufficient to limit the velocity of the carriage, (I alluded to reciprocating locomotive engines, such as are in common use,) and, perhaps, ought to have expressed my meaning more clearly. I think Mr. Cheverton will agree with me, that the velocity of such an engine is limited by its *construction*, even if it had nothing to do but move its own parts, and the supply of steam, of any given density, were infinite.

Yours, &c.

S. Y., an Engineer.

16, Lambeth Terrace, Nov. 5.

THE SHIP SINKING SYSTEM.

Sir,—The recent melancholy loss of human life, in the Amphitrite, Earl of Wemyss, and other vessels, again calls loudly for the public voice to be raised against the miserable method of building our merchant vessels. It cannot be denied that vessels can easily and cheaply be built, immensely stronger and safer than on the present plan,—so strong as to diminish, in all probability by nine-tenths, the number of shipwrecks and loss of property at sea. But, unfortunately, a diminution of shipwrecks would diminish the profits of the gentlemen of Lloyd's, as well as the revenue collected from timber, spars, copper, iron, flax, and all the various articles used in shipping and their equipments, and hence any plan which would produce this effect meets with the most determined opposition. Trusting, however, that you will use your influence to expose, and thereby to take the only step by which this crying evil can be remedied, I beg leave to subscribe myself, Sir,

Your most obedient servant,

A Seaman.

Oct. 1833.

We have already more than once used our best endeavours to hold up to public odium the *sinking system*, on which our merchant ships are at present constructed, and begin to think, with the clever author of the pamphlet entiled "Sea Burking," from which we gave some extracts a few months back, that there is no hope for humanity, save in the entire abolition of the practice of insurance, which is the grand bulwark of the system. Were it only once made impossible for ship-owners and ship-freighters to insure themselves against the perils of the deep, there would then be no question about the adoption of every expedient means of setting these perils at defiance. The mechanical problem to be solved is one of no difficulty whatever. All that the merchant builders have to do is to build their ships as the government—*who do not insure*—build theirs; to adopt the solid system of construction introduced into the royal navy so successfully by Sir Robert Seppings, and advocated with so much ability by Mr. Ballingall, in his "Mercantile Navy Improved," reviewed in our Journal of the 2d February last.—Ed. M. M.

NEW MATERIAL FOR BOOTS AND SHOES.

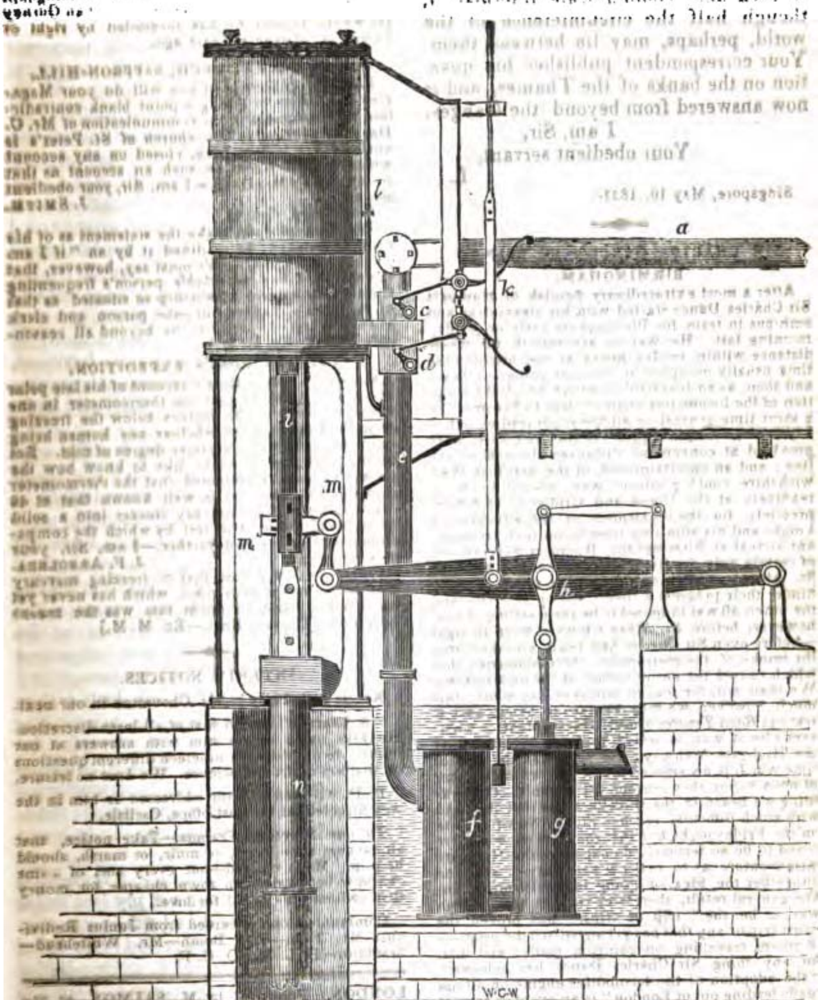
Sir,—The influences of similar education and similar experiences strongly

Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 536.] SATURDAY, NOVEMBER 16, 1833. [Price 3d.

SYMINGTON'S LIFTING STEAM ENGINE.



prolificness of the vegetable kingdom; but does not this expressly show that man is more amply provided for; and while the other animals are supported by the kingdom to which they belong, or by the vegetable kingdom, man has the three for his support and comfort: and when we consider the prolificness of the animal and vegetable kingdoms in reference to the sterility of man, is it not preposterous to say that which is really implied by imposing an artificial check, that nature has not provided a sufficiency for him? Nature, when left to herself, has a sufficiency of food for animals—man, whose reason has also endowed him with cunning, has lessened it; although at the same time it must be acknowledged that the more civilised a country becomes, the greater is the amount of comfort that it is capable of producing.

Nature has calculated with much accuracy and precision the various degrees of prolificness and sterility in the different classes of animals, and varied them both in proportion to the conditions of the means of subsistence. This law she has sealed and finally settled; and therefore the law of production must always be regulated in obedience to her provisions.

I remain, Sir,
Your obedient humble servant,
R.

Bayswater, Sept. 25, 1833.

THE UNDULATING RAILWAY.

Sir,—Your learned correspondent, Mr. Cheverton, complains, in his last article, that he is *sick* of the undulating railway question. (Does not this sound something like Satan reproving Sin?) I am not at all surprised at this: he, no doubt, must now feel both sick and *sore*.

Mr. Cheverton is, certainly, an able writer: his diction is free and fluent, and although (by his own account) not deeply read in the abstruse science of mathematics, still he seems to have a perfect knowledge of the definitions and axioms of mechanical philosophy. I would rather guess that he has paid greater attention to metaphysics than to the more certain science of logic. I have not all his articles on the undulating railway just now before me, but upon one (No. 524,) I shall make a few comments.

In addressing Junius Redivivus on the immense resistance of the air at high velocities, he says, "Has he (Junius) ever caculated how many *hundred horse power* an engine must be in order to produce a velocity of 100 miles in an hour in opposition solely to this resistance?" &c.

By what process of calculation Mr. Cheverton arrived at such a conclusion I know not. By my calculation, when the velocity is 100 miles per hour, and the barometer at 30 inches, the resistance would be something less than 2 ounces for every square inch of opposing surface, and consequently for any safe velocity that might be wanted, in the case of the undulating railway, the resistance of the air is an element that may altogether be rejected.

Again, Mr. Cheverton, in his postscript to the same article, informs us that there are not twelve men in England that can read the works of La Place. This is, truly, a libel upon the country. So far from there being any truth in the assertion, it is well known that at this moment there are more than three times that number at Cambridge alone (not including students) who can read and understand all the works of that truly great man. I believe I could give a shrewd guess why Mr. Cheverton made such a remark. About twenty-five years ago Professor Playfair stated in the *Edinburgh Review*, that there were not above *four* men in Great Britain that could read and understand the *Mechanique Celeste* of La Place. Very likely the Professor was then right; for, let it be remembered, at that time the French *Notation* of the differential and integral calculus was not taught, and consequently very little known in this country; but for the last fifteen or sixteen years the case has been notoriously otherwise. The truth is, there are no less than four ladies in this country who have mastered the writings of La Place, and I am very doubtful if France could boast of producing as many.

I am, yours, &c.
KINCLAVEN.

P. S. — Mr. Cheverton has guessed right, that I am not an engineer; but it is possible for a man to know something of the principles of religion, although he is not a parson. As to the remarks he has made upon my last solution I cannot give my assent to one of them. I thank him, however, for the trouble he has taken.

Fig. 1.

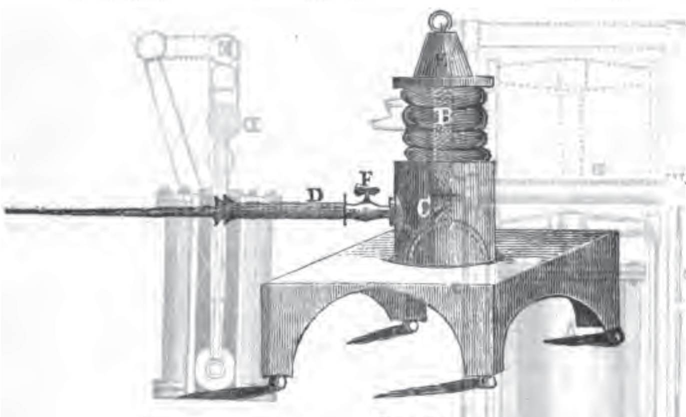


Fig. 2.

Sir,—Having, in looking over the volumes of your valuable work, the Mech. Mag., observed how often the blowpipe has been made use of for practical purposes, I thought it might be applied to glass-blowing in the manner represented in the prefixed sketches. C, fig. 1, is a vessel accurately fitted, to which is a leather bellows. E, is a weight to press down the bellows when the cock F is turned, so that the air escapes. B, is a spiral spring to uplift the bellows after the removal of the weight when the air is exhausted. Fig. 2 is a valve placed at the bottom of the vessel C, opening inward. D, is the blowpipe. G, is a small upright, made with a shoulder a little distance up to support it, with an arch at the bottom to allow the valves free action.

To the vessel C a flange may be made to fix a wooden bottom, into which the metallic valve may be inserted.—

I remain, yours, &c.

Spenans.

Horseferry-road, Oct. 8, 1833.

COMPACT CRANK MOTION FOR STEAM ENGINES.

Sir,—In compliance with the request of Mr. Marsland in a recent Number, I send a plan for converting a reciprocating into a rotary motion, by means of a crank, which will allow the main axle to be as close to the cylinder (if required)

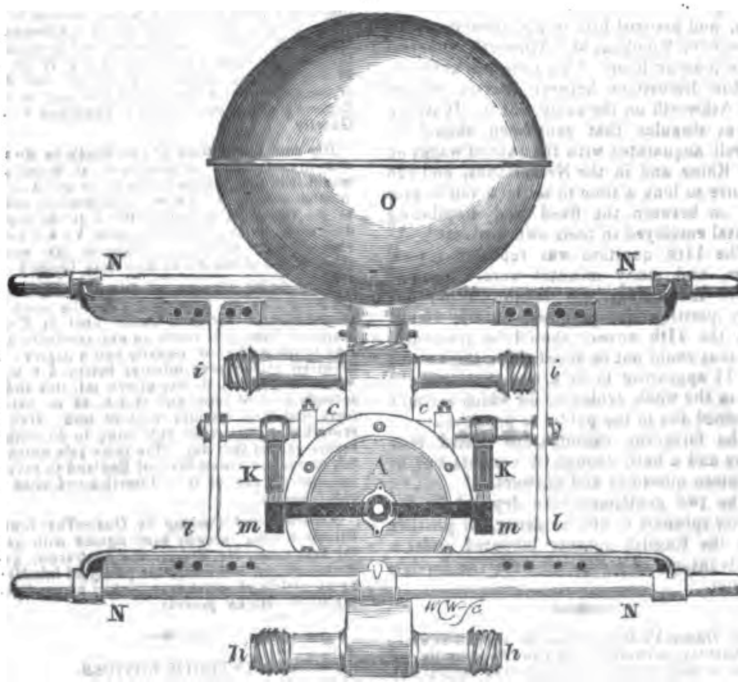
Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 537. SATURDAY, NOVEMBER 23, 1833. Price 3d.

NEW FIRE ENGINE BY MR. BADDELEY.

Fig. 1.



SIR,—The accompanying drawings exhibit a fire engine that I have contrived, on the principle of De la Hire's double acting pump, which I believe to be one of the most compact ever designed, of equal power. Fire engines, with the pistons working *horizontally*, were in use at Nuremberg, 1657; but of their internal construction nothing further is at this time known. The nearest approach to the form of engine I have adopted, will be found in Mr. Braithwaite's steam fire engines; there is, however, a considerable difference between them, my engine possessing several novel peculiarities of arrangement, which will be understood by reference to the drawings. Fig. 1 is an end view; fig. 2 is a side view of the working parts of the engine; and fig. 3 a side section. The same letters of reference apply to each. A is the cylinder, with a flange at either end, and four valve flanges *b b b b*; the journal *c* for the main axle is also cast on the cylinder. D D is the piston rod, working through stuffing boxes in the end plates of the cylinder. *e* is the piston. *f f* are the two entrance, and *g g* the exit valves. The suction pipe is screwed on at *h*, the delivery pipe at *i*. K K are two sectors, one on each side, turning on the main axle at *c*, and worked by the levers *l l*. The sectors, by means of fiddle chains, communicate motion to the frame *m m*, which is attached to, and works the piston rod. N N are the handles. The alternate up-and-down motion of the handles, therefore, causes the piston to

There is one question of Mr. Reed's, in his communication already referred to, which I take this opportunity of answering:—"In places where there are no waterworks," says Mr. Reed, "as in many towns, and in most buildings situate remote from towns, of what use are suction-pipes?" They are of this use, that they enable the engine to work from a tub, into which a supply of water can be poured from carts, buckets, &c., much more conveniently than into the engine itself; more hands can be employed in filling, there is less water wasted, and the men working the handles are not splashed. If there is a well, pond, river, or canal at hand, the suction pipe is of incalculable advantage, from the immense labour that is saved; and there is no possible situation in which it is not every way preferable to having an engine that is only capable of working from its own cistern.

The Manchester fire engines have no provision for enabling them to work from their cisterns, but invariably work by suction, each engine carrying a suitable tub: and though frequently supplied by carts, buckets, &c., it is always done very readily and conveniently. The Edinburgh fire engines also carry a tub for the same purpose.

For myself, however, I must candidly confess I still prefer Newsham's plan of working either way at pleasure, and I will, at an early opportunity, send you an account of an improved three-way suction cock which I have designed for this purpose.

I remain,
Yours respectfully,
W. BADDELEY.

London, Oct. 29, 1833.

THE LATE EXPERIMENTS MADE ON THE LIVERPOOL AND MANCHESTER RAILWAY, TO DETERMINE THE ACCURACY OF THE UNDULATING RAILWAY THEORY.

Sir,—The truth of Mr. Badnall's statements of the experiments on the Sutton inclined plane being of importance to those who have no other means of forming their judgment upon the undulating railway theory, I take the liberty of asking that gentleman, through the medium of your Journal, which contains his account of the trials on the 23d and 24th of September (see No. 530), how it happens that your printed statement doth vary from the verbal one which he was so polite as to give me at the Star hotel, Liverpool, on the evening of the said 24th, in the presence of Mr. Perkins the engineer, and several other gentlemen? Mr. Badnall read from his note-book, carefully and distinctly, while I wrote down his words (in ink), which, at my request, were repeated by him to prevent mistakes, and I have now the memorandum before me. Mr. Badnall will recollect, that I told him *the same night* that the results did not accord with each other, if the inclination of that part of the Sutton plane, upon which the experiments were tried, was really as great as he had assumed it to be—namely, 1 in 96.

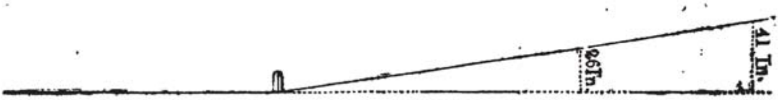
The statement which Mr. Badnall then deliberately gave me was as follows, and refers only to the first, second, and eleventh experiments:—

"Weight of the Rocket engine and tender 6½ tons—wheels 4 feet 8 inches *diameter*. *First.*—Began the ascent (without steam) at the rate of 18 miles an hour (equal to 18 *strokes of the piston in* 10 *seconds*), and ran up the inclined plane to a distance of 217 yards, by *momentum. Second.*—Returned to the foot of the inclined plane (using the full power of the engine), and acquired a velocity of 22 miles an hour (or 22 strokes of the piston in 10 seconds); shut off the steam, and ran, on the level plane, to a distance of 454 yards, by *momentum. Eleventh.*—Two engines took a load of 35 tons on the level, and reached the foot of the inclined plane with a velocity of 12 miles an hour (or 12 strokes of the piston in 10 seconds). One of the engines being left behind, the other (the Rocket) shut off the steam, and ascended a space of 177 yards, by *momentum.*"

The statement with regard to the *last* experiment, it will be seen, agrees with your printed account; but the two first do not, as to the fact of the acquired velocity; yet it is a truth which Mr. Badnall will not deny, that I at first understood him to say "*twenty to twenty-two miles an hour*; but, upon my repeating those words, thinking the expression too vague, he corrected me or himself by saying, "*twenty-two precisely.*" I feel sorry for the necessity of calling upon Mr. Badnall to explain how it was that, four days afterwards, he should send you so different an account for publication. Did he mean to deceive me, or your readers? In either case, I opine, he will be considered as having rather too much of "the man of the world" in his composition. I should have taken this step before now, had I seen the paper in No. 530 sooner; but I did not get it till the 2d instant, in the last monthly part, and have since then been closely engaged in my regular vocation.

Being desirous of finding out the error, for I felt assured of its existence, and Mr. Badnall having returned to the Isle of Man, I wrote to Mr. Booth on the subject, of which letter the following is a copy:—

"Sir,—Having been invited by Mr. Badnall to witness the experiments on the Sutton plane on Tuesday the 24th ult., I went there on that day, but, as you will probably recollect, was too late for personal observation; for, at the moment I arrived, the Rocket engine had received some damage, and Mr. Badnall, with yourself, was on the point of returning to Liverpool by the same train. Mr. Badnall gave me the principal results on our arrival at his hotel in Liverpool, which I cannot at all reconcile, upon the supposition that the inclination of *that part* of the Sutton plane is 1 in 96. Perhaps the quarter-mile post which you assumed as the *foot* of the plane is not precisely so, or some alteration may have taken place since their first construction. If accurate levels are taken, I am inclined to think the section will be found as follows:—



"Should you think it necessary to have actual levels taken for the purpose of corroborating the other facts, will you have the goodness to favour me with the result? And you will further oblige by informing me when you next try experiments with reference to the undulating theory, and by allowing me to be present during the trial.

"I am, Sir, your obedient servant, "H. S."

"Sheffield, October 1, 1833."

"*Postscript*.—I ventured to tell Mr. Badnall on the same evening, that there must be some material error, and that I thought it was in the assumption of 1 in 96 for the inclination. This well-meant observation, however, did not appear very palatable."

"To Henry Booth, Esq., Treasurer to the Liverpool and Manchester Railway Company."

It is to be regretted, for the sake of truth, and Mr. Booth's acknowledged character as a gentleman and a lover of science, that all the notice of this communication which I have had the honour to receive, is contained in a postscript of an unlooked-for letter from Mr. Badnall, dated October 19th. He says—"Your letter to Mr. Booth may lead you to a better acquaintance with the precise inclination of the Sutton inclined plane; but, however the inclination may vary from the reputed rise, it cannot affect the proportionate result of experiments." True, it cannot; but it affords the means of *checking* the statement of Mr. Badnall; and there would have been another check, if that had been done which on the 24th September I said ought to have been done, namely, accurate observations recorded of the *length of time* occupied in traversing the respective spaces by means of the acquired *momenta*. This hint, it appears, has not been forgotten in trying the subsequent experiments, and its being acted upon may be attributed to Mr. Robert Stephenson of Pendleton, who perfectly agreed with me in opinion as to its propriety. (See *Liverpool Mercury* of October 18.)

As soon as I am more at leisure I shall send you some further ideas on the undulating railway theory, which I will endeavour to condense as much as possible, and will not forget the friendly notices of your correspondents "Junius Redivivus" and "S. Y."

Yours respectfully,
HENRY SANDERSON.*

Sheffield, Nov. 10, 1833.

Sir,—I shall for this time only crave a small portion of space in your valuable Journal. When I wrote my last article (No. 536,) I had not then had time to compare Mr. Badnall's last experiments on the Manchester and Liverpool railway with the theoretical deductions. I have since done so, and I am sorry to observe that some mistake must have

* See an explanatory note on the subject of this letter among the Interim Notices at the end of our present Number.

been made either in taking or in recording these experiments. Thus, in example 1st it is stated that the velocity at the foot of the plane was 12.06 miles per hour, the rise 1 in 96, and that the engine ascended 278 yards on the inclined plane in 90 seconds. Now this was impossible; for suppose we put friction out of the question, it would have required a velocity at the foot of the plane of 16¾ miles per hour to have produced such a distance on the inclined plane, and the time of ascent woould have been only 70¼ seconds. I rather suspect there has been a mixture of errors in the report of the different experiments. Thus, for instance, if in experiment 1st we suppose the rise to be 1 in 155, instead of 1 in 96, and the velocity at the foot of the plane 12.06 miles per hour, the engine (abstracting friction) would have ascended 278½ yards in 90 seconds; but by assuming the inclination of the plane 1 in 155 this will not agree with the 3d experiment, nor will 1 in 96. In short, some very great blunders have been made some way or another, which I trust Mr. Badnall will take the trouble to rectify.

I am, yours, &c.

KINCLAVEN.

Nov. 15, 1833.

Sir,—Mr. Ham, who so candidly confesses his want of courtesy, will, I hope, excuse my having written under the influence of such feelings as his observations excited; henceforward, I trust, we shall understand each other well enough to attribute any unpleasant expression to inadvertence, and that each of us will feel satisfied the other has no wish to offend.

I have said, in No. 525, that 473 seconds would be occupied in travelling over one of Mr. Ham's roads, and the other would require 576 seconds, shewing a difference of 103 seconds; but it must be remembered this calculation is made upon the supposition that the carriage has no resistance to encounter, either from air or friction—a condition that no carriage either is or ever will be placed in, and therefore it is idle to speculate on the consequences.

Mr. Ham is mistaken in supposing that the velocity of a locomotive engine is constantly increasing; on the contrary, the ultimate velocity can be, and commonly is, attained in a few seconds.

Mr. Badnall's plan stands neither higher nor lower in my estimation than it did the first day I heard of it.

I hardly see how the question "*whether or not it be necessary*," &c., can be answered without entering into the *practical* objections to Mr. Badnall's railway, and for my part I am heartily tired of the discussion. Of one thing, however, Mr. Ham may rest perfectly satisfied, that if even it is proved *possible* to avoid "*deep cutting, embanking*," &c., by adopting Mr. Badnall's plan, it will only be advisable to do so in *very* peculiar situations, and under *very* peculiar circumstances; and if it should ever come to pass that the Great Western Railway is attempted on the up-and-down system, I take the liberty to advise Mr. Ham and all his friends to sell their shares with all possible expedition.

Yours, &c.

S. Y.

16, Lambeth Terrace, Nov. 9, 1833.

P. S.—For the benefit of all newspaper editors, who may think proper to make observations on mechanical subjects, I beg to observe,

First.—That *two* obstacles are easier overcome than *three*; and

Secondly.—That friction offers a greater resistance to the beginning of motion than to the continuation of motion.

I do not lay claim to the *discovery* of the *profound* truth contained in my first remark, because I have reason to suspect it was known before I became acquainted with it; but I hope all who were not aware of the fact, or who happened to have forgotten it, will have a due sense of the obligation they are under to me for reminding them of such an important truth,—a truth which, if the editor of the *Manchester Guardian* will condescend to remember, he will, perhaps, perceive there is nothing marvellous in the results of Mr. Badnall's experiments.

S. Y.

THE DOCTRINE OF PARALLEL LINES.

Sir,—Mr. Scott's ingenious disquisition on the subject of parallel lines has reminded me of an attempt, which has lain among my papers for nearly twenty years, to surmount the difficulty in another way. It is more concise than Mr. Scott's; whether or not it be more

Musical Taste in High and Low.—The Right Reverend Father in God, his Grace the Lord Archbishop of York, an illustrious patron of difficult and exclusive instrumentalism, beats time in the wrong place to a very familiar chorus of Beethoven's. How do the mechanics (at the Norwich Festivals) in the chorus benches use hands and feet, when the band sends forth the multifarious thunder of the most intricate harmonies of Spohr? Who, that is not a mere mercenary, would not rather play for the glory and progress of the art to a dozen well-organised Norwich weavers, than to the Right Reverend Father in God, his Grace the Lord Archbishop of York? It is no disgrace to his Grace, simply as a prelate, that he neither feels enough nor knows enough to beat time correctly, and that the mechanics beat him out and out; but the example of such patrons may allay apprehension of any deterioration of the art, by rendering more popular the performance of its noblest productions.—*From an admirable Article in the Monthly Repository on the last Norwich Musical Festival.*

Cheap Musical Festivals.—For the art there would be nothing to fear, but every thing to hope, from throwing open more widely the doors of our Festival Halls. "To every one that thirsteth, let him come;" and, if we may not yet say "he that hath no money," let it, nevertheless, be the object to make as little money as may suffice for admission. It is a favourable circumstance for our views, that most of the finest effects of the finest compositions are choral. * * Now a prima donna has often cost more for the heartless and artificial warble, repeated every week all the year round, of a few bars of unintelligible difficulties, than would a whole legion of capital chorus singers. Materials for choral bands exist in all large towns, as well as in Norwich; and what the happy union of public spirit, with musical taste, in two or three individuals, has accomplished there, may, with like facility, be realised elsewhere, even in London itself. The attempt would answer every way, pecuniary, artistical, and philanthropic. It would make money, cultivate taste, and refine the population. —*Ibid.*

Saving and Credit.—Not the least valuable branch of saving, however, is the *art of spending.* The poor are certainly the greatest prodigals. How lavishly their means are wasted! They are mulcted three ways,—in quantity, quality, and price:—in the first, owing to the smallness of their purchases at once, by the turn of the scale or the pot; in the last, by a trumpery system of credit, fostered by those nuisances called small debtor courts, which are a real injury to buyers and sellers,—fill the gaols, demoralise the poor, and spread hate and revenge in every neighbourhood. For the misfortune or the folly of being one week behind in expenditure, and being necessitated to resort to *one* shopkeeper during the week, and who must be paid in whole, or in part, on Saturday night, the poor pay, perhaps, thirty per cent. more than the rest on all they consume. The retailer is not to blame for this: he must be paid for extra trouble in weighing small quantities. It is also necessary the extra profit laid on those who do pay should make up his losses by those who do not,—a system, by-the-bye, of taxing the just for the unjust, and chiefly favourable to practitioners in insolvent courts, and the dissolute and wasteful, who are sheltered and encouraged in extravagance by the facilities the credit system affords.—*Wade's History of the Middle and Working Classes.*

The Gresham Professorship of Astronomy has become vacant by the resignation of Dr. Sandford. Several candidates have been actively canvassing for the votes of the twelve gentlemen who have the gift of the appointment. But we are most happy to hear that the committee have resolved that every candidate shall deliver before them and the public a trial lecture in the Gresham Room, Royal Exchange. If this exemplary determination be carried out by choosing the man who is best suited to fulfil the duties of the situation, they will confer a lasting benefit upon the public and their fellow citizens, and lay a foundation for the *future* usefulness of this important institution. —*Morning Advertiser.*

The Thames Tunnel.—M. Navier has communicated to the Academy of Sciences a letter from Mr. Brunel, the engineer of the Thames tunnel, in which it is stated that the English government has at length consented to furnish the funds necessary for its completion.—*Gazette de France.*—We very much doubt the correctness of this information.

Another Grand Tunnel Project.—Amongst the numerous new schemes on foot is one for making a tunnel and railway to extend from the north end of London-bridge to the Uxbridge-road, with branches to the entrance of the Birmingham railway, adjoining the Hampstead-road, and to the entrance of the Great Western Railway at Millbank. The work is intended to be carried under ground, beneath some of the most populous streets of the metropolis, in the same way as the tunnel at Liverpool. Distance only five miles!

INTERIM NOTICES.

H. H.—Hick's apparatus for gas-cooking is described in our 495 No., and Mallet's in No. 521. To the other query—Yes; and admission may be obtained at once on application to the Secretary.

W. P., of Bishop Auckland.—One is intended for insertion.

Communications received from Mr. Thorold—Mr. Booth—L. A.—R. S. G.—W.R.—Chilenito—T. G.—Practical.

THE LATE UNDULATING RAILWAY EXPERIMENTS.—On perusing the letter of Mr. Sanderson, inserted in our present Number, it occurred to us that it would be but an act of justice to Mr. Badnall to inform him of the charge of misrepresentation which it contained, in order that his answer to it might go forth to the public at the same time with the charge itself. We accordingly wrote to Mr. Badnall on the subject on the 13th instant, but in consequence of our directing the letter to Douglas, Isle of Man, where we supposed Mr. Badnall to be, instead of to Messrs. Badnall and Stephenson's offices, Liverpool, it did not reach his hands till the 20th, and his answer, though immediately forwarded, was not received by us till Friday forenoon, *after our present Number was at press.* We regret this exceedingly, for Mr. Badnall's explanation appears to us to dispose most satisfactorily of Mr. Sanderson's objections. We need not, under these circumstances, request the public to suspend their judgment until they have that explanation before them. At present we shall merely add, that the rise of the Sutton incline has been ascertained to be not 1 in 96, but from 1 in 89 to 1 in 122; which may possibly serve to account for the discrepancies noticed in the other letter on the subject by "Kinclaven."

LONDON: Published by M. SALMON, at the Mechanics' Magazine Office, No. 6, Peterborough Court, between 135 and 136, Fleet-street. Agent for the American Edition, Mr. O. RICH, 12, Red Lion square. Sold by G. G. BENNIS, 55, Rue Neuve, Saint Augustin, Paris.

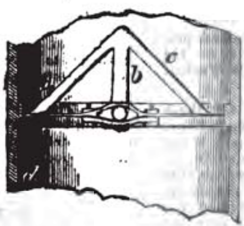
M. SALMON, Printer, Fleet-street.

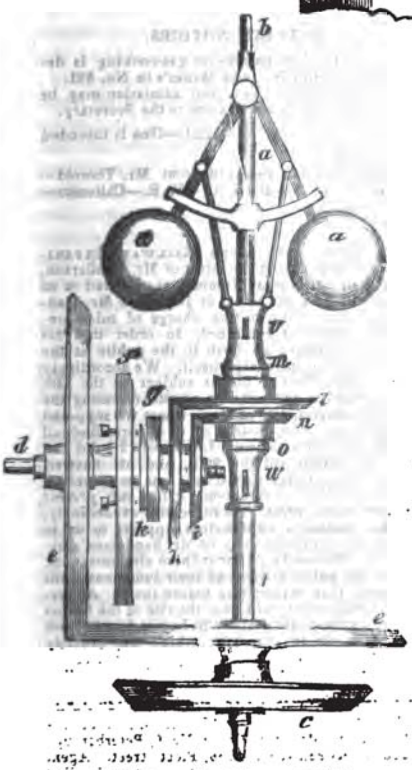
Mechanics' Magazine,

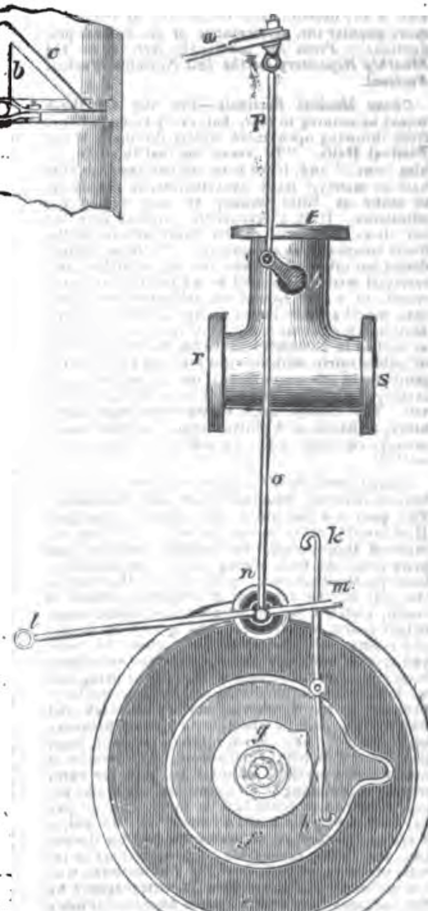
MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 538. SATURDAY, NOVEMBER 30, 1833. Price 6d.

AMERICAN METHOD OF REGULATING THE VELOCITY OF STEAM-BOAT ENGINES.







fixed on a cylinder which turns round on the shaft *d*. There is a friction plate *k*, with some springy substance between it and *g*, which keeps the cylinder level with the bevel wheels, and *g* from turning, when the tooth is acting on the catch: the three pinching screws, for pressing on the friction plate, are fixed into *f*. The bevel wheel *l* turns loose on the governor shaft, unless the friction cone *m*, which turns round with the governor shaft, be pressed into it, when the balls fall, and the bevel wheel *n* runs loose, unless the governor rises and presses the cone *o* into it. If the engine is working at its proper velocity both of the wheels run loose. The bevel wheel *l* has a tooth less in it than the wheel *h*, into which it works, and the wheel *n* has a tooth more in it than the wheel *i*, into which it works; and as the shafts *b b*, and *d*, go at the same velocity, the tooth on *g* will be shifted so as to shut the valve, sooner or later, according as the balls rise or fall from the position they are in, when the engine is going at its speed; in this position of the balls, as both of the wheels run loose, the tooth on *g* will not be shifted. Fig. 3 shews a face view of one of the wheels *e*, of the wiper *f*, and of the part *g*. *h*, *i*, *k*, is the catch for holding the valve open; it turns on the centre *i*, and takes hold of the end of the lever *l m* at *m*; this lever is for holding the friction pulley *n*, which is acted on by the wiper *f*; it also steadies the rods *o*, *p*, that work the cutting-off valve by means of the lever *q*; the lever *k m*, turns on the centre *l*. The branch *s*, of the steam pipe leads to the top steam valve, and leads to the bottom valve, and *t* goes to the boiler. *u* is a spring for closing the valve, when the catch is lifted up; a weight like what opens the valves in hand-gear engines might answer better for shutting the valve than a spring. The rod for working the under cone passes down the centre of the governor rod, and is connected to the cones by means of the cutters *v*, *w*, which also keep the cones from turning, unless with the rod *b b*. The friction cones, as they have little to do, may have a good deal of taper to keep them from jamming. The part *g* should be prevented, by a tooth fixed to *f*, from making more than a revolution; for, when it is cutting off the steam at the end of the stroke, by going a little farther the engine would have steam the whole of the stroke.

I am yours, respectfully,

JAMES WHITELAW.

Glasgow, Oct. 26, 1833.

P. S.—In all the other methods of working the steam-engine by a governor the balls are full out, and the engine is going quicker when it is doing little work; in this way of regulating, the velocity is the same, whether the engine is doing much or little work.

MR. BADNALL'S EXPLANATION OF THE ALLEGED DISCREPANCIES IN THE REPORTS OF THE RECENT EXPERIMENTS ON THE LIVERPOOL AND MANCHESTER RAILWAY, TO DETERMINE THE CORRECTNESS OF THE UNDULATING RAILWAY THEORY.

Sir,—I feel indebted by the opportunity you have afforded me of explaining the cause of the difference between the statements alluded to by Mr. Sanderson, and those sent by me to you for publication. I have no doubt that Mr. Sanderson copied my observations correctly; and those observations, at the time I communicated them to him, I believed to be perfectly correct. Mr. Sanderson is aware that, on the day the experiments were tried, I was labouring under severe indisposition; and, as the weather was extremely inclement, I did not take the same active part in the experiments as Mr. Booth, Mr. Rae, and Mr. Scott. The observations I made, as they appear in my note-book, in regard to the velocity of the engine and train at the foot of the ascent, were founded on my own calculation of the number of strokes which the engine appeared to be working when she passed the spot on which I stood. The spaces passed over were measured, and the particulars agree with the statement which I gave to Mr. Sanderson. Immediately after the experiments were concluded I proceeded to Liverpool with Mr. Sanderson, whom I met in one of the railway carriages; and the same evening I gave him the particulars to which he alludes. He will perfectly recollect that the following day I was not, through indisposition, able to get up till one o'clock; and, having an engagement at Manchester, I could not, on that day, compare my notes with Mr. Booth's. At *two* o'clock I left for Manchester, and gave, at Newton, the same particulars which I had given to Mr. Sanderson to *Mr. Allcard.* On the 26th of September I left Mr. Sanderson in Manchester, and returned to Liverpool; and, previously to addressing you, I considered it better to have an interview with Mr. Booth, who was in possession of the notes taken by Mr. Rae and Mr. Scott, and who had himself carefully taken down all the particulars. At this interview I found that I had over-estimated the velocity at which the piston was moving; and the statements, therefore, which I sent to you were *not my own*, as given to Mr. Sanderson, but those of three other individuals, which decidedly told more *against my principle* than *my own* would have done.

For instance, had the engine been travelling at eighteen miles an hour at the foot of ascent, before rising the hill, and twenty-two miles an hour at the same point, after descending the hill, which are the particulars given to Mr. Sanderson, the result would have shewn a greater gain by the undulating system, than when the velocities were fifteen strokes of the piston (or about fifteen miles an hour) before ascending, and sixteen strokes of the piston (about sixteen miles an hour) after descending, as published in your Magazine.

I have only to add, that in all the statements of the experiments which I *have sent to you*, I have cautiously avoided laying myself open to the slightest charge of error or partiality. On the contrary, my notes have always been corrected by, and compared with, the notes of others.

With regard to the particular inclination of the Sutton inclined plane, it has always been understood to be about *one* in ninety-six. After my first experiments were made, however, the levels were taken afresh, and it was found, that towards the foot of the plane the inclination was considerably less than the average rise. For instance, from the point from which the ascent of the Rocket engine, &c., was calculated, the plane rises as follows:—

1st 88 yards....1 in 122.
2d 88 — ....1 — 105.
3d 88 — ....1 — 97.
4th 88 — ....1 — 94.
5th 88 — ....1 — 92.
6th 88 — ....1 — 89.
7th 88 — ....1 — 89.

And the entire distance here denoted exceeds that to which, in any of the experiments, the train ascended.

It must, however, be evident, that however a variation in the inclinations may affect Mr. Sanderson's calculations, it cannot possibly,—which he will, no doubt, allow,—affect the comparative results of the experiments.

I remain, Sir, yours, &c.,

RICHARD BADNALL.

Farm-hill, near Douglas, Nov. 19, 1832.

N.B. There can be no doubt as to the measurement of time being the best test; which test was adopted in all the subsequent experiments, as agreed upon by the engineers present.

ON CELESTIAL PLANISPHERES AND PROJECTIONS.

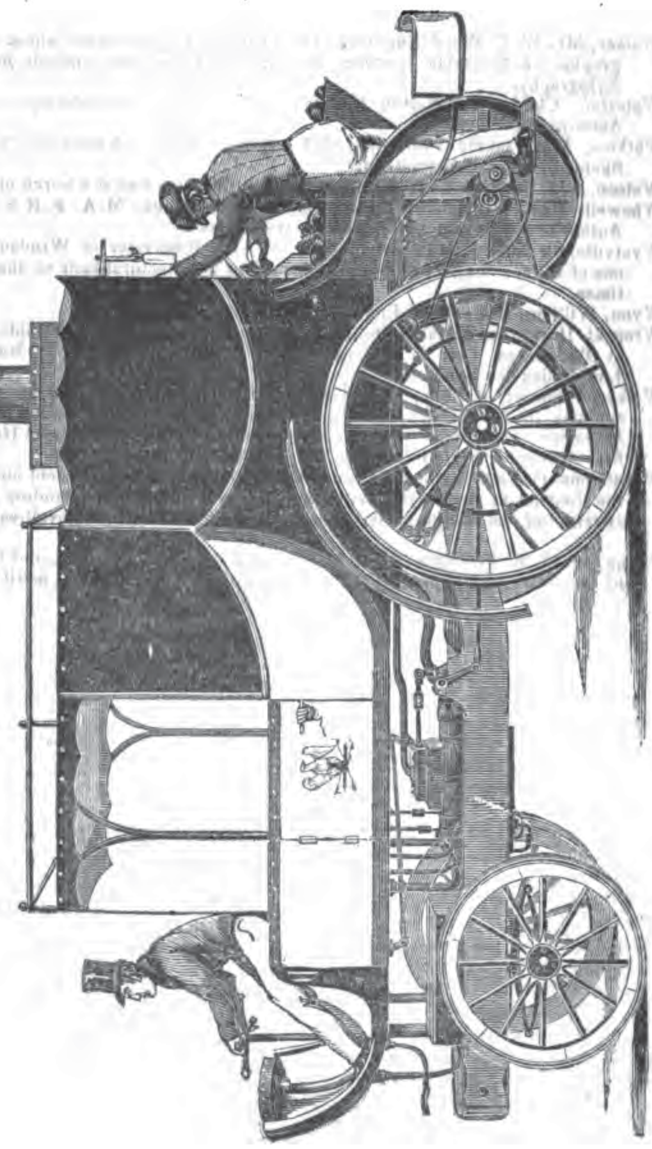
The sentiment, that novelties are not always improvements, is strongly exemplified in the instance of the more recent maps of the stars. The *stereographic* projection of the sphere, at once the oldest and most extensively useful, has been attempted to be superseded by constructions which greatly distort the true features of nature. Thus we have a pair of hemispheres, by Wollaston, on the *globular*; the six maps of the stars, by the Diffusion Society, on the *gnomonic*; and the New Celestial Planisphere, by Kerigan, on the *cylindrical* projection. On each of these we shall presently make some remarks; but we shall first give an account of a still more recent production, which has occasioned us to take up the critical pen, viz. "The Celestial Globe, stereographically projected by Robert Dickinson, Liverpool, 1832." The work comprises a pair of twelve-inch circles on the plane of the meridian of 0°. We were delighted at once more beholding our favourite projection; and the neatness of the engraving made a favourable impression at the outset, which, we regret to say, subsequent inquiry has shown to be undeserved.

The first defect is conspicuous, in the exhibition of a convex, instead of a con-

Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 539. SATURDAY, DECEMBER 7, 1833. Price 3d.



MACERONE AND SQUIRE'S STEAM-CARRIAGE.

is coming to the conclusion that Messrs. Macerone and Squire have, in truth, produced a very capital machine, and one which might be safely left to bear the brunt of competition, on its own unexaggerated merits.

Since writing the preceding notice we have read in the *Morning Chronicle* reports of two successful excursions made by Messrs. Macerone and Squire's steam carriage, to Harrow on the Hill. We subjoin an extract from the account of the first excursion :—

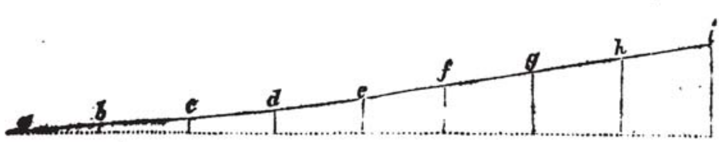
"The success of the carriage being so complete on roads of ordinary difficulty, Colonel Macerone determined to take a trip to Harrow on the Hill, which is acknowledged to be one of the most rugged and difficult in the neighbourhood of the metropolis, and which has long been held out by scientific men as the criterion by which to judge of the strength and power of a carriage of this kind * * * We started from Paddington at a quarter to three o'clock, and arrived at Harrow on the Hill (a distance of nine miles) in one hour and forty minutes. But of that period rather more than forty minutes were consumed in taking water, and trying an experiment with a new drag, so that the whole distance was completed in fifty-eight minutes, exclusive of stoppages. The hill was ascended apparently with ease, at the rate of seven miles an hour, and during no part of the journey was the full power of the steam put on."

THE UNDULATING RAILWAY.

Sir,—To prevent the possibility of any further misunderstanding in reference to the experiments recently made on the Liverpool and Manchester railway, I have this morning called upon Mr. Booth, and have compared with his statement thereof the following particulars of the rise of the Sutton inclined plane, from its base to a higher point of elevation than any which was attained in our experiments. To render this explanation more clear, on which reliance may be placed, I have accompanied it by a diagram, wherein is denoted the points from which the ascents and descents, during the experiments, were measured.

I am extremely sorry that these particulars have not before been published, as the want of them has evidently produced inconvenience to some of your correspondents, which, had it occurred to me, I ought not to have permitted.

The ascent of the Sutton inclined plane may be said (see diagram,) to commence at the point *a*—although for a distance of 2½ miles before arriving at that point there is a trifling ascent of about 1 in 2,640—viz. from the Sankey viaduct to the foot of the plane.



From *a* to *b* the distance is	88 yards	—rise 1 in 754	total elevation	.116 yards.
b to *c*	88 yards	—rise 1 in 122	....	.721 yards.
c to *d*	88 yards	—rise 1 in 105	....	.838 yards.
d to *e*	88 yards	—rise 1 in 97	....	.907 yards.
e to *f*	88 yards	—rise 1 in 94	....	.936 yards.
f to *g*	88 yards	—rise 1 in 92	....	.956 yards.
g to *h*	88 yards	—rise 1 in 89	....	.988 yards.
h to *i*	88 yards	—rise 1 in 89	....	.988 yards.
	704 yards.		Total rise ..	6.450 yards.

Now, the series of experiments first published (see No. 531), and which were tried with the Rocket engine, were made from what was considered the foot of the plane, and which in the diagram is marked letter *b*. . . .

The results of those experiments evidently proved to me that the inclination of that part of *the plane* on which they were tried was not, as generally supposed, 1 in 96. I perfectly recollect Mr. Sanderson alluding to some supposed error, though why the allusion was "*not palatable to me*" is a perfect mystery. It was not, however, until after I sent you the particulars of the experiments, and my return to Douglas, that I had an opportunity of making such calculations as led me to call a second time on Mr. Booth, viz. *before* the trial of the second series of experiments, and explain to him the discrepancy which appeared. For instance, see page 21, Experiment 6:—The Rocket engine and a load of 35 tons ascended by *momentum* 184 yards, the velocity at the foot of the plane being 10 miles per hour, which ascent was equal to 4.1875 feet perpendicular elevation. Now, 10 miles per hour is 14.672 feet per second, and supposing friction out of the question, a body having gained a momentum of 14.672 feet per second, by gravity, would only ascend 3.36355 feet. Thus it was evident to me that 1 in 96 was not the proper inclination of that part of the plane. Previously, however, to the experiments afterwards detailed, the levels were taken afresh by the Messrs. Dixon,*—and in the experiments tried with the Liver and load on the 16th (see No. 534.) the ascents were measured from that part of the plane marked × in the diagram, at which point there is a cottage on the railway. On the Sunday following it was agreed by the engineers present that the place of starting should be again changed, and the experiments, with the double load, were all made from that part of the plane from which the Rocket had ascended, viz. from *b*. It will naturally occur to your readers—if Mr. Badnall were acquainted with all these particulars why did he not lay them before the public? The fact is, that I considered the result of the experiments made with the Pluto and Firefly, with the *double* load, such as to render all explanation unnecessary with regard to the previous trials, and especially *as I stated at page* 71, that the inclination upon which those latter experiments were tried was about 1 *in* 99, which will be found to be the average. Moreover. I did not at the time consider a knowledge of the exact inclination of the plane at all necessary to a clear comprehension of the nature and results of the experiments, inasmuch as *all I wished to prove was*, that *whatever velocity* was generated by one or more engines at the foot of ascent, by which velocity (either with or without the continued assistance of one engine) a given elevation was surmounted, *a greater velocity could be generated by descending the same distance*, evidently proving, beyond all rational doubt, the correctness and value of the principle.

The discrepancy alluded to by "Kinclaven" will be explained, I trust, satisfactorily to him, by referring to the statement of the experiment to which he alludes. In that instance the word *momentum* is not introduced; on the contrary, the whole power of the Liver engine was employed throughout the *whole ascent*. Had this not been the case there evidently must have been some great error. I need not say that I shall be most happy, not only to give every further information in my power, but if any of my opponents will propose any further practical test, upon the result of which they will cast the merits of the question, it shall be, if possible, immediately and most impartially tried.

As a proof of the impartiality with which I have recorded the experiments already tried, I refer to all the engineers present, whether the *steam* of the Pluto (see last experiment) was not shut off 155 yards before she arrived at the starting post, which made a very considerable difference in the rise by momentum. Seeing, however, that I had *proved* enough, I neither complained at the time, nor have I hitherto published my complaint. The error arose from the two conductors of the engines shutting off the steam of both engines when the flag dropped for the first engine to shut off on passing the mark, letter *b*.

May I again ask if Mr. Cheverton and "S. Y." will be satisfied that there is an advantage if a given locomotive engine will move, at a given velocity, *double* the load from summit to summit, which she is capable of moving on the level at the same velocity? If so, will they, if not satisfied with the impartial judgment of our northern engineers, attend on an appointed day, of which they shall have due notice, and witness the experiments themselves? If they refuse to attend, and if they dis-

* I had also an opportunity of comparing, this morning, my notes on the rise of the inclined plane with Mr. Dixon's, sen., from whom I had originally been favoured with them.

believe the results of the experiments already tried, it is needless to make a single further comment on their opposition. On the other hand if they do attend, and if they do witness a decided proof that a load which *will not move on a level*, will move from summit to summit of an undulation at a *great velocity*, what becomes of the "ASSUREDLY NOT" of the *Champion*—and why is it necessary that "S. Y." should give such friendly advice to Mr. Ham, and to the subscribers of the *Great Western Railway?* I am, however, happy in believing, that a full and impartial trial of the undulating principle will *soon* be made on rather an extensive scale; and I hope "S. Y." will state his *practical objections*, and that the *Champion's rod* may be most freely *exercised* before such *trial takes place*. As to the sickness which these gentlemen complain of, I am sorry I can administer no better restorative than my regret.

I am, Sir,

With great respect,

RICHARD BADNALL.

Manchester, November 28, 1833.

P. S.—I observe that "S. Y." makes some allusion to "*The Editor of the Manchester Guardian*." Probably he is not aware that Mr. Garnett, the editor, is an opponent of mine, and *one* of whose mechanical attainments I have a very high opinion.

NEW APPORTIONMENT OF THE GLOBE FOR REPRESENTATION IN MAPS.

Sir,—It has long ago been remarked by me, what little variety was to be found in Atlasses, with regard to the representation of large portions of the sphere.—A pair of hemispheres on the plane of a meridian (20° W. long.), and a map according to Mercator's development, constitute in general the whole, though sometimes a pair of equatorial hemispheres is added. The projections are either stereographic, or globular:* in modern times the latter, without any good reason, has prevailed. In fact, the globular meridianal projection is become so common, that its faults are softened down by familiarity, and it is only when we view the equatorial delineation upon the same principles that the great distortion near the borders is perceived; and yet the only difference between the two cases is, that the distortions are in contrary directions.

Lately, the Society for the Diffusion of Useful Knowledge published maps of the Celestial Sphere, referred to the six sides of a cube, by what is called the gnomonic projection; and, more recently, a similar set for the Terrestrial Globe. The former are liable to many objections, but the latter are excessively aukward and inconvenient. Europe, Asia, and North America are cut asunder in the midst, and there being no overlapping portions, the connexion of those parts which we wish to view in entirety is wholly interrupted.

The contemplation of these last-mentioned maps, induced me to consider whether some better method could not be devised, of representing the whole world in convenient sections, and at the same time avoiding the too great distortion or expansion of scale which occurs in hemispheres. And it appeared to me, that the globe might be projected in four equal portions, on the faces of an equilateral pyramid. This, however, would produce nothing but triangular maps, which would look badly enough; the next step therefore was, to extend the limits of each plane of projection to a circle circumscribing the triangle, whereby each map would represent a *third* instead of a fourth of the spherical surface, and an overlapping of the several portions, to a moderate extent, would take place, and be highly advantageous.*

Having pursued this idea into all its details, with reference both to terrestrial and celestial maps, the result is so satisfactory to my mind, as to induce me to submit it to the consideration of the public.

I propose to make the *North Pole* correspond to the apex of the pyramid; of course the *south* will touch the base of the solid at its centre. The middle latitude

* It should be rather called *pseudo*-globular. For, the *equal* division of the central meridian, and the longitudinal and latitudinal *circular* arcs, do not constitute a true projection. A point of view may indeed be taken which will make the divisions of the radius *nearly* equal, but then the meridians and parallels become all *elliptical*. The stereographic projection does not, strictly speaking, distort the superficial representation: an expansion of scale takes place, which is equal in every direction—but in the globular there is a real distortion.

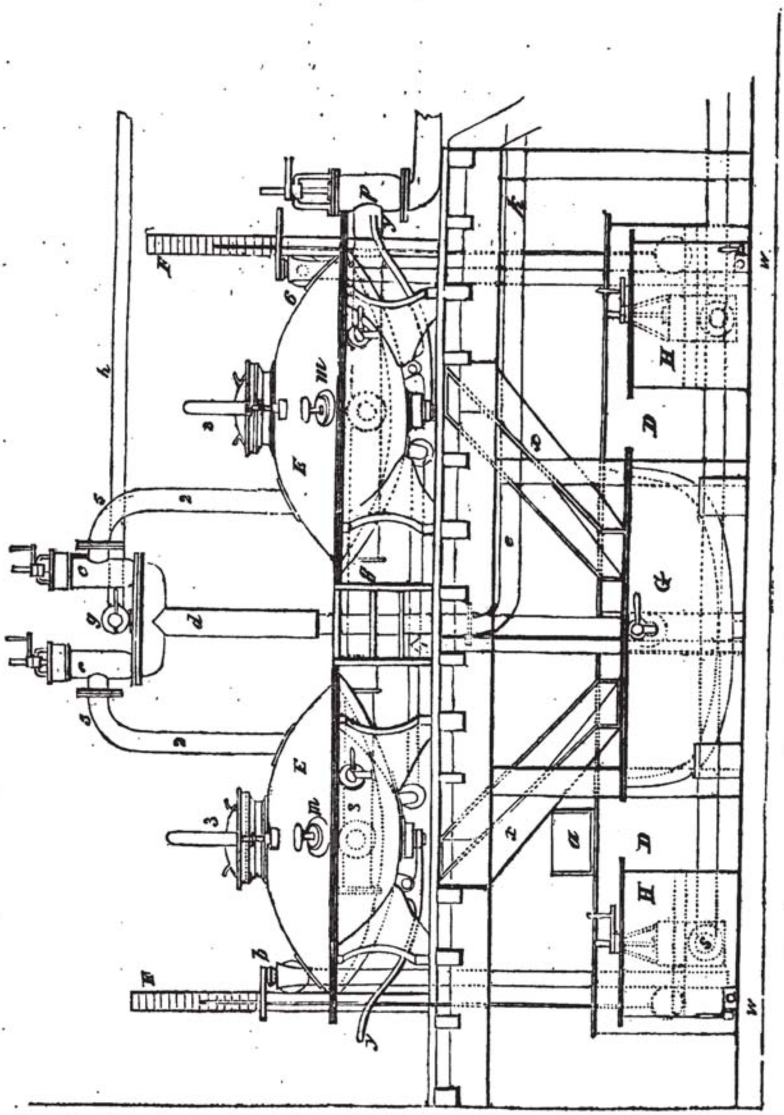
* The overlapping portions will be six, each equal to 1-18th of the sphere, and of a lenticular form.

Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 540. SATURDAY, DECEMBER 14, 1833. Price 3d.

Fig. 1.



OAKS AND SON'S SUGAR MANUFACTURING APPARATUS.

THE UNDULATING RAILWAY — RESISTANCE FROM FRICTION — RESISTANCE OF THE ATMOSPHERE.

Mr. Badnall in reply to S. Y., Junius Redivivus, and Mr. Cheverton.

Sir,—Seven months have now elapsed since the undulating railway was first introduced, as a subject of discussion, in your Magazine. During that period I have done my utmost, by fair and conscientious argument, to support the cause which I undertook to defend, and the gratification which I now feel in having witnessed your honourable and candid confession of a changed opinion, and in finding myself supported by several of your most able correspondents, far more than compensates for the disappointment which the opposition of S. Y. would naturally excite, even should it be continued after the publication of this letter, and after the important facts determined by the experiments. I say *disappointment*, because, if still unconvinced, he will, I fear, ever remain inconvincible; and judging from the occasional piquancy and asperity of his remarks, he is not likely to be more fairly defeated without losing, in some measure, that evenness of temper which I should be sorry to disturb. If I do him injustice, I apologise for it; but I feel that the time is now arrived when (practical experiments having decided the merits of the question) I have no longer occasion to defend myself by parrying the verbal attacks of my opponents. On the contrary, I waive all further hypothetical discussion on this subject, unless such discussion refer to the result of my late or future experiments. In coming to this conclusion I am not considering my own convenience, but I think your readers in general will agree with me, and with your friend, *Professor Crackwell*, that unless we draw in our horns, the undulating controversy will not only become sickening, but, judging from Mr. Cheverton's last letter, somewhat disgusting. "*Nec lusisse pudet, sed non incidere ludum,*" said Horace, and I quite agree with him.

You, Mr. Editor, or an impartial jury of your readers, must judge whether I am led to this train of thinking through *fear* of my opponents, or whether I am not justified in claiming the victory I have contended for. Those gentlemen who have advocated my side of the question—*Saxula, Mr Ham, Mr. Sanderson, Kinclaven, Mentor*, and *Mr. Trebor Valentine*, have each and all supported my position by convincing diagrams, appropriate comparisons, or disproved experiments; whereas neither S. Y. nor Mr. Cheverton have thought proper to substantiate their reasoning by a single particle of corroborative evidence. That both are clever men, I do not for one moment question, but a clever man occasionally errs; and never is be more likely to do so than when inflamed with that unhappy quantity of combustible matter,—vulgar abuse, self-sufficiency, and extreme vanity,—which have been so conspicuously displayed in the disjointed lectures which Mr. Cheverton has directed to me on this subject. For those lectures I am indebted to him, especially for the last, which I shall presently take into consideration, and which, I trust, will be headed in your title-page, "THE PROFOUND IGNORANCE OF MR. BADNALL DEMONSTRATED BY THE SUPREME SENSE OF MR. CHEVERTON"!!

My present object is to reply to all unanswered objections which have been raised by my opponents up to this time. In doing this, I may probably introduce some opinions which may appear open to further discussion, but as I fully concur in the sentiments expressed by $\phi. \mu.$ (No. 532), as to the frequently injurious effects of a too protracted controversy, I shall feel it an act of duty to your readers, to be a silent observer of any attacks upon them. I place them on record as my deliberate and conclusive opinions; and having done so, I turn from *theory* to *practice*, and now present myself to your readers as the defender of the undulating principle in a far more important point of view—I mean in defence of its *complete practicability*.

In thus a second time throwing the glove, allow me to prognosticate what will be the result of another year's experience. Within that period, engineers and mathematicians will have an opportunity of making up their minds upon the subject, and from the expiration of that time *we shall never have another level railway* (whereon locomotive steam force is intended to be employed) *laid down in Great Britain*. The Liverpool and Manchester railway, though it will ever maintain the character of being one of the most important examples of British spirit, British perseverance, and British ingenuity, will, in the eye of posterity, have one dark spot upon its fame—it will be compared to the massive and expensive aqueducts of the ancients. Our forefathers knew not that water would find its own level—and, while we praise their structures, we cannot help wondering and smiling at their ignorance. Thus, however, will posterity smile at us, exclaiming, "*could you have believed it! They expended, in about thirty-one miles, hundreds of thousands of pounds to make a railroad level*, through their disbelief *that all bodies descending on a curvilineal arc will rise again to their own level, minus friction!*"

I now turn to S. Y.—a few words afterwards to *Junius Redivivus*; and then, in perfect good humour, to Mr. Champion Cheverton.

A desire to remove, if possible, every opposition, founded on mathematical reasoning, which has been urged against the undulating theory, induces me to return to S. Y.'s first communication. Before doing so, however, I must at once contend against the liberty which he takes in stating that I have betrayed ungentlemanlike conduct by my observation, "that I should have felt hurt that any other correspondent than himself had doubted my proficiency in common arithmetic." The "indignation" of S. Y. cannot possibly justify such an observation.

In S. Y.'s letter, page 181, there is an error in print, afterwards corrected, which rendered his first formula "incomprehensible." I allude to the omission of the decimal dot before the figure 8. In the succeeding column, however, I find this misprint did not occur; I therefore ought to have understood his object better than I did. But allowing that I had fully comprehended it, and that such misprint had not occurred, I observed that the whole formula was founded on false data, and that the position which he took was altogether untenable. I refer now to the saving of friction "*abstractedly*," without allusion to the difference in velocity occasioned by the action of gravity, to which latter point he also frequently alluded, when he denied that the speed could be greater on a curve than on a level line. With regard, then, to the real difference of friction on the two roads, he gives the following proposition, which I have thought it better to describe by diagram:—

Fig. 1.

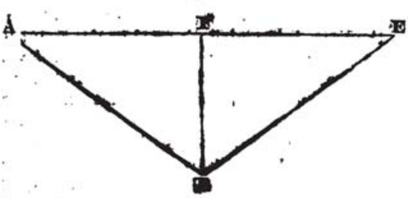
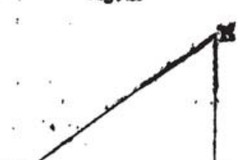


Fig. 2.



"If A E (fig. 1,) be equal to 16, and the depth F B equal to 6, the length of each inclined plane will be equal to 10, and the pressure against the plane; and therefore the friction, according to Mr. Badnall, will be equal to .8 of the friction on the level."

Now S. Y. must have misunderstood my diagram,* page 93, to which he refers, and which was, I think, clearly elucidated. If he *did understand it*, from whom did he draw his conclusion, that I imagined the friction or pressure on the inclined plane E B, whose perpendicular elevation is equal to F B, equal to ·8 of the friction on the level?

Let him suppose, then, (reversing the above diagram, as fig. 2,) the plane E B raised upon the base E F, at an elevation of F B. It cannot be doubted (as proved by the parallelograms described in my diagram, page 93,) that if the base line E F represent the pressure (or friction) of the whole weight resting on the plane E B, F B will represent the force of gravity down the plane, or, in other words, as the length of the line F B is to the length of the line E F, so is the pressure or friction taken *off* the inclined plane E B to the pressure or friction *left on the inclined plane*; or, to be more explicit, if a body be supposed to weigh 10 tons, and to be placed on the horizontal line E F, no one can dispute that, the line of pressure being vertical, the *whole weight of the mass* must necessarily press upon the rail. If, then, E F were exactly equal to F B, and the weight were removed to the inclined plane E B, the pressure would be reduced *one half*; and thus, in the above diagram, E F being equal to 8, and F B equal to 6, and supposing any weight resting on E B to be divided into 14 parts, $\frac{8}{14}$ths of the whole weight would be resting ON the rail, and $\frac{6}{14}$ would be taken *off* the rail.

By this explanation it will be evident that S. Y.'s second formula, page 242, is, like to his first, established on wrong data, *for he never takes into consideration the perpendicular elevation of the plane*; and it is this which has evidently misled him, or otherwise he would not consider his argument to hold good for "all lengths and *elevations* of inclined planes."

S. Y. considers in both formulæ the pressure to be determined by the *base*, divided by the *length of the inclined plane*: he consequently draws in each case an erroneous conclusion; for there can be no doubt whatever that, *as the perpendicular height of the inclined plane is increased, the pressure or friction of any carriage moving on that plane is reduced.*

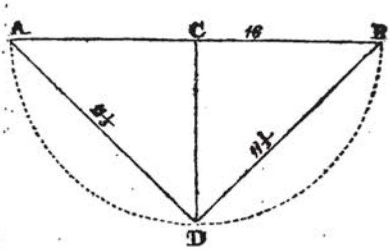
Referring to the preceding diagrams, nothing can be so easy as to determine the *exact* proportion which subsists between the

* On referring to this diagram I find the length of the level line E A=22, the length of each inclined plane =5¾ and the elevation=2·5; if, therefore, we deduct 2·5 from the length of F A, we shall find the reduction of friction or pressure nearly one-fifth.

pressure or friction on an inclined plane, and the pressure or friction on a horizontal plane, provided the angle of elevation be given. In the case before us we have the angle F E B. Now, let P be the pressure on the base, or horizontal line F E, and let p be the quantity taken off that pressure, owing to the inclination of the plane, and let a be the angle of inclination: we then have *in all cases*—

p : P :: F B : F E
but F B : F E :: tangent a : 1
therefore p : P :: tangent a : 1
and, consequently, p : P tangent a.

If, then, the amount of friction or pressure on an inclined plane (speaking abstractedly of friction or pressure) be reduced in proportion to the angle of inclination, it must, I should hope, be evident to S. Y. that his position is wrong. He, no doubt, will allow that the pressure at an angle of 45° is reduced *one half*. To make myself, therefore, perfectly clear, I will take this angle to prove his formulæ *incorrect*, and the undulating theory, *in regard to friction, perfectly correct*:—



Draw the line A B, and divide it into 16 equal parts. From the centre C describe the semicircle A D B. Draw the line C D perpendicular to A B, and from the points A and B draw the straight lines A D, B D.

Now, as before observed, because the perpendicular line C D is equal to the line B C, any weight descending on the line B D will press with exactly half the force with which it would have pressed on the level line A B, the angle C B D being an angle of 45°. Divide, then, the lines B D, A D, into an equal number of parts, each part being equal to $\frac{1}{16}$th of the horizontal line A B=$22\frac{2}{5}$. Next suppose a body, weighing 10 tons, to press upon every described part of the line A B, in passing from A to B: we then have 16×10=160; but if 10 tons press upon each part of the horizontal line, *half that weight*, according to the proposition, will only press upon each part of the lines B D, A D.

We have, therefore,

$22\frac{2}{5}$×5=112.

and, consequently, 160—112=48 difference in total pressure.

But it may be argued, that if the semicircle A D B were divided into an equal number of like parts, the total number to be passed over on the curve would be 25·142; but this argument will not obtain, it being mathematically true (see Sir Isaac Newton, Parkinson, Hutton, and others,) that the velocities which bodies acquire in falling either down inclined planes, or curvilineal arcs, are precisely alike, viz. as the square roots of their perpendicular heights; and if the acquired velocities are equal, it is self-evident that the resistance opposed to motion down each line is also equal.

Let us now examine to what result S. Y.'s formulæ would bring us.

Let B represent B C
L „ B D
n pounds equal to the force of traction on a level at any given velocity.

Then the pressure on the line B D (according to S. Y.) will be to the pressure on the level as $\frac{B}{L}$ is to 1; and therefore the force of traction required in consequence of friction on the inclined plane, will be to the force of traction on the level as $\frac{B}{L}n$ is to n.

According, then, to S. Y., the entire expenditure of power to move the wheel the horizontal distance on the level will be Bn, and on the inclined plane it will be $\frac{B}{L}n$×L=Bn as before. Thus he makes, at an angle of 45°, the pressure or friction on the lines A D and B D equal to the pressure or friction on the horizontal line A B; whereas I make the difference in friction as 7 to 10, or $\frac{3}{10}$ths in favour of the semicircle A D B.

Again, referring to S. Y.'s letter (No. 531), wherein he fully explains the bearing of his formulæ, the erroneous view which he has taken of the question is again evident. For, taking 5 ounces (as he suggests) to represent the force of traction on the level, and applying his observations to the angle of 45°, we shall find that the calculation will not be, as it would arise if his formulæ were correct, viz. 16×5=80 on the level, and 22·40×·71428571=15·999, or very nearly 16; consequently, 16×5=80 on the inclined plane, as before.

But, instead of the force of traction on the semicircle being equal to the force of traction on the horizontal line, we should have it as follows:—

16×5=80 on the level,
22·40×2·50=56 on the curve,

precisely agreeing with the reduction of fric tion before mentioned, viz. $\frac{1}{16}$ths in favour of the semicircle.

So much for the question of friction, considered abstractedly, and as *commonly* understood; but it must be evident to every man who has perused the particulars of the experiments, that the amount of reduced friction, as in this instance considered, according to the angle of the inclination of the plane, cannot be taken as the precise measurement of power saved, by the adoption of the undulations. On these interesting points I trust that some valuable information may shortly be laid before the public in a treatise on railways,* locomotive engines, &c., which Mr. Robert Stephenson, sen., and myself, are preparing for the press. Previously to that time we shall try various experiments, and I have no doubt, from the plans which we intend adopting, and the precision with which the experiments will be made, that the laws of motion and resistance, under various circumstances and velocities, will be more clearly developed than they have hitherto been. The results which I anticipate lead me to quote a remark of Hooke's in the year 1666:—"Gravity, though it seems to be one of the most universal, active principles in the world, and consequently ought to be the most considerable, yet has it had the ill fate to have been always, till of late, esteemed otherwise, even to slighting and neglect."

I have no apology to make to S. Y. for considering L as a proper symbol for the *length* of the plane, which was ascended with a given velocity, especially as the spaces in most of the experiments varied on every trial. For an error in the last equation I, however, have to apologise, and I must beg S. Y. to read L+D for L D—. The word "INVERSE," which he alludes to in his letter (No. 531, p. 23,) was an unintentional omission of mine (see page 222), where, for "*is in proportion*." I evidently intended to say, "*is in inverse proportion*." Any person who reads the sentence will, I hope, give me credit for this.

In reply to the observation of Junius Redivivus, (No. 532, page 38,) let me beg him to place a heavy ball upon a plank, then raise the plank to a vertical position—he will allow that, because the weght falls perpendicularly, there is *no pressure* on the plank. Let him, then, raise the plank on which the same weight rests to an angle of 45°. He will, no doubt, admit that the weight will descend, and that the velocity of descent on the effect of the force of gravity will be in proportion to the diminution of pressure or friction on the plank. Let him next support the weight on the plank (the latter still being inclined at an angle of 45°), by placing his hand under it, or some machine by which he can accurately measure the pressure: he would, I have no doubt, find that precisely *half* the weight was resting *on the plank*, and *half* the weight upon *his hand*, or upon the instrument by which he was measuring the pressure. Let him, then, withdraw his hand, and what becomes of the weight? Half is still remaining on the plank, and the other half is suspended in the atmosphere until it reach the earth which attracts it. Surely, on consideration, Junius Redivivus will acknowledge the truth of this reasoning, and if so, he cannot dispute that the greater the angle of the inclination of the plane the less will be the pressure or friction of any body, either ascending or descending, on such plane.

And now for *Mr. Champion Cheverton!*

The first explanation which I think due from me to your readers, and to which "*The Champion*" principally alludes, is in reference to *the resistance of the air.* I stated, in a former letter, that I thought that *the resistance of the atmosphere* did not (a constant power being employed to urge the body forwards, or, like gravity, downwards,) increase as the squares of the velocity—that the resistance of the air does not act as a greater opposing force (alluding, particularly, to the flight of birds, and to the motion of railway carriages,) at high velocities than at low velocities—that, consequently, the velocity of a train of carriages, supposed to be descending an inclined plane of interminable length, *never could in practice become uniform*; but, on the contrary, that *in theory* the uniform acceleration would not begin to cease until the resistance of the air was equal to the force of descent, which it could not be until the body had attained a velocity equal to that at which air would rush into empty space. I further stated, that it was my opinion "that the resistance of the air, when first overcome by any locomotive force which is constantly and equally *continued*, does not, throughout EQUAL SPACES OR DISTANCES, act as an opposing force with greater intensity at high velocities than at low velocities"; but that, on the contrary, it was my opinion that the *total resistance* of atmosphere, throughout *a given distance*, is *less* at high velocities than at *low velocities, from the inclination* which all bodies have to rise from the surface of the earth *when in rapid motion*, and, consequently, from a denser to a lighter atmosphere.

* The resistance of the atmosphere, and the *cause* of that resistance not increasing as the squares of the velocity, will be particularly elucidated in this treatise by careful experiment.

Now, Sir, I should have felt not only that an explanation, but that a public apology was due from me, had I published these opinions, without having *very strong reasons* for believing them to be true. I know they are diametrically opposite to received opinions: *so was the undulating railway*; but time, and careful experiments, will prove whether I am right or wrong. I will now explain my reasons for believing that *I am right.*

In the first place, that there are many doubts existing as to the *true* theory of atmospheric resistance is evident, by the following remark by Hutton:—"We conclude (he says) that all the theories of the resistance of the air hitherto given are very erroneous, and the preceding one (alluding to the generally entertained opinion) is only laid down till further experiments on this important subject shall enable us to deduce from them *another* that shall be more consonant to the true phenomena of nature." Surely this admission is a sufficient apology for the humble attempt which I have made, and for the attempt which Mr. R. Stephenson and myself are now making, to investigate this subject.

I must now request the attention of your readers to the following experiments, tried down inclined planes, by Mr. Nichoas Wood, with a view of measuring the friction of railway carriages.—(See his work on Railways, 2d edition, pp. 211—213, &c. &c.)

Mr. Wood, in reference to these experiments, thus writes:—"Standing on the end of a carriage, and aided by an assistant, at the end of every ten seconds I made a mark upon the plane where the carriage happened to be, and afterwards measured the distance between those marks, which gave the space passed over in each successive period."

Carriage weighing 9,100 lbs.; wheels, 34 inches; axles, 2¾; friction, 44·62 lbs.

Seconds.	Feet.	Real space, the descent not being uniform.
In 10 the body fell	6·6	6 feet.
20	26·4	26·4
30	59.4	59·8
40	105·6	106·2
50	165	165
60	237·6	242·8
70	321·4	326·7
80	422·4	424·3
90	534 6	525 3
100	660	635·5

The above experiment was tried at the Kenilworth colliery.

Now, in examining the result of this experiment, if Mr. Wood were correct in his measurement, and upon his correctness I have placed dependance, it is evident that the resistance of the atmosphere did not increase as the squares of the velocity of the moving body, but that, *for some reason or other*, with which reason the public will soon, if I mistake not, be acquainted, it was equable in effect through equal spaces throughout the entire distance of descent.

We know that if a body fall, in vacuo, a given space in the first second of time, it will have fallen four times the space in the two first seconds; that if it fall 16·1 in one second, it will have fallen 64·4 in two seconds; because 16·1 × 4 (4 being the square of the times) = 64·4.

Again, if it fall 1608 feet in 10 seconds, it will fall 6433 feet in 20 seconds, or twice the time; because (omitting fractions) 1608 × 4 = 6432.

Now it appears, according to Mr. Wood's *measured* experiments, that in 10 seconds the carriage fell 6·6 feet, and in 20 seconds 26·4.

Now 6·6 × 4 = 26·4, which is in exact accordance with the laws of falling bodies.

Again, in 40 seconds, the carriage fell 105·6, and in 80 seconds 422·4.

Now 26·4 × 4 = 105·6
and 105·6 × 4 = 422·4.

Again, in 30 seconds it fell 59·4, and in 60 seconds 237·6.

Now, 59,4 × 4 = 237·6.

Lastly, in 50 seconds it fell 165, and in 100 seconds 660;

and 165 × 4 = 660.

Now, had the resistance against the rolling carriage increased as the squares of velocity, the descent *could not have been in accordance with the laws of bodies falling in vacuo.*

I will, however, refer to other experiments, and try the question by another test:—

Descent of loaded carriages weighing 9,408 lbs.; wheels, 35 inches diameter; axles 3 inches.

In 18 seconds the carriage fell 25 feet.

28	71·9
38	124·6
48	205·2
58	276·5
68	384·7
78	506·1
88	645·5
98	785·3
108	939·6
118	1081·6
128	1266·5

Fall, 1 in 104—friction, 41·45 lbs.

Now, in vacuo (taking 16 ft. as the correct measurement in the first second of time), a body in 18 seconds would fall 5210·892 ft., and in 28 seconds 12608.072 ft., and in 38 seconds 23223·852. Now, according to the preceding experiment, the carriage fell 25 ft. in 18 seconds; 71·9 in 28 seconds; and 124·6 in 38 seconds: Therefore,

In open atmosphere. In vacuo.

$$71{\cdot}9 \div 25 = 2{\cdot}876, \text{ and } \frac{12608{\cdot}072}{5210{\cdot}892} = 2{\cdot}419$$

Again, omitting fractions,

In open air.	In vacuo.
$124 \div 71 = 1{\cdot}746$, and	$\frac{23224}{12608} = 1{\cdot}842$,

Again, to make the proof more indisputable (relying upon the measurement of Mr. Wood), we find that, according to his experiments, the carriage descended, omitting fractions, 25 feet in 18 seconds, and 1266 feet in 128 seconds. Now, as before observed, a body would fall, in vacuo, in 18 seconds, about 5211 feet, because $18 \times 18 \times 16{\cdot}083 =$ to the total space; and in 128 seconds it would fall 263503·872 feet.

Now $1266 \div 25$ (in air) $= 50{\cdot}64$;
and $263503 \div 5211$ (in vacuo) $= 50{\cdot}56$.

How very striking, then, is the proportion which the falling body bears in vacuo to the descending body, when opposed to the resistance of the air! So much so, that Mr. Wood must either have imposed upon the public, which I do not and cannot believe, or his experiments, though not intended to elucidate the theory of resistance, are a death-blow to the previously admitted opinions on this subject.

Again, referring to Mr. Wood's experiments (see page 225), we find a perfect regularity in the descending motions; for instance, the carriage was 29·16 seconds in moving 100 feet, and 58·33 in descending 400 feet.

In other instances :—

Time in descending 100 feet.	Time in descending 400 feet.
29·10 seconds	58.10
30	60.41
29·16	58.75
31·95	64.35

and all with different loads, varying from 1,120 to 8,960lbs.

Again, page 226, when the carriage was loaded with 8,960lbs. it fell 100 feet in 29 seconds, and 400 feet in 58.

Again, in 29 seconds it fell 57·90 feet.
Again, 29·10 .... 58·40
Again, 29·74 .... 60·25
Again, 31·88 .... 63·75
the weights varying as before.

We will next observe whether the proportions were regular. In doing this we find (page 225) that the carriage, with a load of 1,120lbs., fell 200 feet in 45 seconds, and 300 feet in 55 seconds. Now, in vacuo, a body would fall in

45 seconds ...... 32568·075 feet,
and in 55 seconds ...... 48651·075 feet.

Now $300 - \frac{1}{3} = 200$, the fall in 45 seconds on the inclined plane;
and $48651 - \frac{1}{3} = 32434$, shewing a difference of only 134 feet in about 32,000.

In another experiment, with 4,480 lbs., the carriage fell 400 feet in 60·41 seconds, and 500 feet in 67·91 seconds.

Now, in vacuo, a body would fall in

Seconds.	Feet.
60·41	58512·7871523;
and in 67·91	74315·8133523;

therefore, $400 + \frac{1}{4} = 500$
$58512 + \frac{1}{4} = 73140$, shewing a difference in comparative velocity not worth noticing.

Again, with 1,120 lbs., in which instance, owing to the lighter weight, the resistance of the air ought to have been the most felt, we find the body descending,

seconds.	
In 64·35	400 feet
72·64	500 feet

Now $400 + \frac{1}{4} = 500$, and
$66958 + \frac{1}{4} = 83247$, shewing a difference which is altogether immaterial; for had the distance traversed been 400 and 510 feet, instead of 400 and 500, the proportions *in vacuo* and in open atmosphere would have been precisely alike. Surely, then, these 10 *feet*, considering the variation of friction, by the occasional rubbing of the flanches against the rail, will be regarded as a difference altogether independent of the resistance of air!

There are many more experiments of Mr. Wood's to which I could have referred in support of my opinion. It is true there are some which show a different result; but the effect might arise from the different state of the rails at different times, and the particular point from which the wind blew. It cannot, however, be doubted, or, if doubted, denied, that the uniformity of acceleration, proved by the experiments herein detailed, *could not have occurred in any instance had the resistance of the air increased as the squares of the velocity.*

I shall, in a further communication, turn to my recent experiments on the Liverpool and Manchester Railway, for the purpose of adding additional strength to this argument. Meanwhile I am, Sir, with much respect,

Your very obedient servant,
RICHARD BADNALL.

Manchester, November 11, 1833.

A FEW WORDS IN FAVOUR OF OLD FASHIONS.

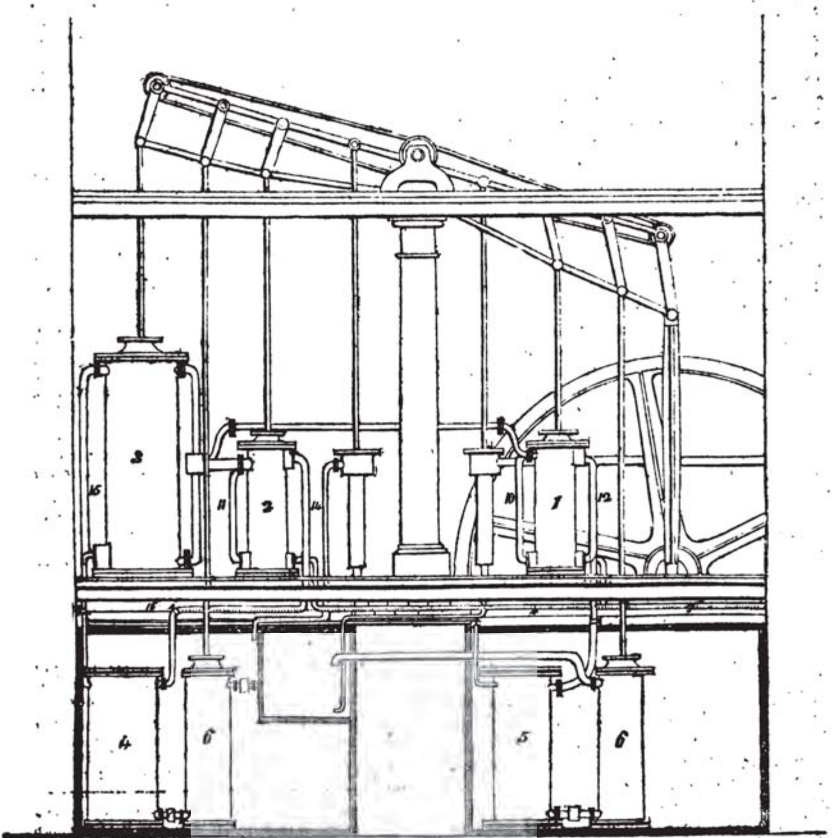
Sir,—It is an old adage, and I believe to some extent a true one, that "two of a trade seldom agree." In fulfilment thereof, I beg to differ in some respects from your correspondent A. C. (page 95) who belongs, I presume, to the same craft as myself, though not perhaps so humble

Mechanics' Magazine,
MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 541. SATURDAY, DECEMBER 21, 1833. Price 3d.

UDNY'S DOUBLE OPERATING STEAM-ENGINE.

Fig. 1.



ing the internal surface of the cylinder 1 as a fixed point of resistance or back pressure, as the other had in the last stroke, the internal walls of the opposite cylinder 2, as already set forth.

It will be seen, from the preceding description, that the steam is continually passing from the boiler into one or other of the two small cylinders, and from them again, in like rotation, into the great one, so as to give a constant, uniform, expansive motion, while there is at the same time the regular coaction of a vacuum, so as to obtain the fullest possible effect from the elastic fluid. It will also be seen that there is never any strain or irregular action on the boiler, as results from cutting off, or occasionally preventing, the ingress of steam to the cylinder from it; and that there is never any reaction on the small pistons, so as to neutralise the expansive power of the steam, one small piston being returning to the point of action with the force of the steam equally exerted on each of its faces, while the other is yielding to the propelling power of the steam; and *vice versa*.

When it is desired to use the engine with high pressure steam only, the alteration required to be made in the arrangements is extremely simple. The steam, after it has operated on both pistons, is driven off into the air by the pipes 9, 10, 15, and 16, and by having those pipes leading to a condenser, with branches with cocks to the air, the engine may be wrought at any time, either with low or high pressure steam, or the one be converted at pleasure into the other.

Fig. 1 is the plan of a double operating engine on this plan; 1 and 2 are the first action cylinders: 3, expansion cylinder; 4 and 5, condensers; 6 6, air-pumps; 7, pipe from the boiler to 1 and 2; 8, pipe from 1 to 3: 9, pipe from 2 to 3; 10 and 11, pipes from below to above pistons of 1 and 2; 12, pipe from 1 to 5; 13, pipe from 3 to 5; 14, pipe from 2 to 4; 15, pipe from 3 to 4; 16, pipe to the air, &c.

THE LATE UNDULATING RAILWAY EXPERIMENTS.

Sir,—I am surprised to see that delusion has taken such hold of the minds of those engineers who were present at Mr. Badnall's experiments on the Liverpool and Manchester Railway—for the professional talents of one or two of whom I have the highest respect. For my own part, I do not consider the experiments of Mr. Badnall as at all satisfactory, and shall, with your leave, briefly state my reasons for so regarding them.

I must, first of all, beg to ask—why Mr. Badnall chose the Liver and the Firefly locomotive engines for his experiments? Are not these engines very weak on an incline, compared with any of the others—such as the Collier, or the Samson? Was it because experiments with better engines would not have been so satisfactory to the undulating system?

Mr. Badnall has entirely omitted to mention the circumstance of the slipping of the wheels upon the rails, while ascending. Now such slipping would, in a great measure, have been obviated by employing the Collier or Samson in lieu of the Liver or Firefly. That such slipping actually did take place I will prove by Mr. Badnall's own report. The Firefly is stated to have arrived at the foot of the incline with a velocity of 20·28 miles per hour, and to have ascended 575 yards in 116 seconds, or, on an average, 4·9569 yards per second—*the engines working the whole time.* In a second experiment she arrived at the foot of the incline with a velocity of 19·04 miles per hour, and ascended 323 yards in 70 seconds, or, on an average, 4·6142 yards per second—the engines in this case *not working during any part of the ascent.* And in a third experiment the same engine arrived at the foot of the incline with a velocity of 14·34 miles per hour, and ascended 315 yards in 90 seconds—*the engines working the whole way.* So that we see the train actually ascended 8 yards higher up the incline in 20 seconds less time, *by its own momentum alone*, than when there was both *the momentum and the power of the engines* at work to propel it up the whole ascent. This fully proves that there must have been slipping of the wheels, when the engine worked during the ascent. Had Mr. Badnall made his experiments with an engine of more adhesive force, I think I may venture to assert the results would have turned out widely different, and much to Mr. Badnall's sorrow and disappointment.

Let Mr. Badnall take the Collier or the Samson locomotive engine, and make the following experiments:—

Let the engine alone, without any weight attached to it, run a distance sufficient to generate a velocity of about 30 miles per hour, or more, at the foot of the incline; shut off the steam, and ascertain the height to which she will rise by the momentum alone.

Reverse this experiment, and let the engine descend the incline, the power continuing at work, and carefully ascertain the velocity generated at the foot of the incline.

Again, take the same engine, attach to it a load of 120 tons, (or, if Mr. Badnall likes it better, 150 tons,) and proceed as in the former experiments, taking care to have a velocity of 30 miles per hour, or more if possible, at the foot of the incline.

I should wish these experiments to be made, if possible, by the Collier locomotive engine. If Mr. Badnall makes these experiments, he will, no doubt, publish them, noting down every particular accurately.

I will now, Sir, make an observation or two on the practical working of the undulating system. We will suppose the line from Liverpool to Manchester to be on Mr. Badnall's system, and that the undulations range from 650 yards to 1,160 yards in length, with the same inclination as the Sutton incline. I have the greatest doubt possible of Mr. Badnall being able to reach the top of each undulation with a heavy train, even with twice the power he contemplates. But even allowing him sufficient power from momentum, to arrive at the summit of each undulation, a velocity of 30 miles per hour, at the bottom of the undulation, will but give an average velocity of 15 miles per hour for the whole distance of 32 miles. Now, I have repeatedly seen the Firefly, with a train of from 90 to 100 tons, travelling on a level, or very nearly so, at the rate of from 18 to 20 miles per hour.

Mr. Badnall's idea of taking the maximum load of three locomotive engines, on a level, at 15 miles per hour (which would be upwards of 300 tons,) over the undulating railway, with the aid of one of those three engines only, seems to me altogether preposterous and ridiculous.

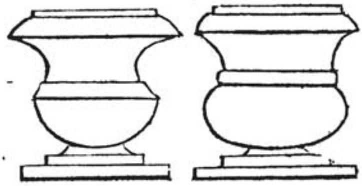
The only advantage I can see in Mr. Badnall's system, is the speed he attains almost as soon as started; but this, I apprehend, would be more than compensated by the very frequent retardations and accelerations to which the trains would be subject, and which would, of course, materially increase the wear and tear of the engines.

I remain, Sir, yours, &c.,

PRACTICAL.

London, Nov. 19, 1833.

IMPROVED FLOWER POTS.



Sir,—Among the various improvements that have been made in the form and manufacture of most of the articles produced by our potteries, it is a matter of surprise to me, that so little alteration has been made in the shape of the common flower-pot, an article which is in such common use by all classes of society, who, for years, have been using various means of showing their dislike to the thing as it is now made. Some have wicker-baskets made to hide the pots in;—others cover them with cut trefoilated paper;—while the more humble shew their dissatisfaction by painting them over. Now, as I think these useful articles could be easily and cheaply improved in their external form, I have sent you two sketches from a couple of antique vases, which, from the simplicity of their outlines, and a little practice on the part of the workman, could be got up nearly as cheap as the ill-shaped things now in use, which, from their broad top and narrow base, seem admirably adapted for making those flying crusades against innocent persons' craniums every high wind that blows.

I do not say these are the best shapes for flower-pots, but I think if something of this kind were got ready by next spring, the speculator would be well paid for his trouble, even though he had to charge a little more for them than is paid for the common ones. Most persons who have tried to produce good flowers in the common pot, must have found it extremely difficult, when they have been kept in the open air, for the pot is such a good conductor of heat, that the frequent and extreme changes of temperature injure the root and render the plant unhealthy. This difficulty could be got over by the flower-pot being placed in a loose ornamented case, and the interstice between the pot and the case filled with sand, which would keep the temperature even and regular.

There is scarcely any thing, however trifling, whether for ornament or utility, but what has been materially improved in its form. New patterns are now being introduced among those interminable thickets of red tubes called chimney-pots, which seem to vegetate on the house-tops in such picturesque variety,—some short and dumpy, like a butter-tub crowned with a cullender—others bent in as many angles as a theatrical flash of lightning—while others, if they be not as interesting, are quite as long, in proportion, as an undulating railway discussion. These improvements, in common articles, show the progress we are making in refinement and taste; nor are the people backward in individual exertion. The improvements in street illumination are truly wonderful,—the little big-bellied lamps, that used to burn as dim and cold as a barge lantern on the Thames in December, have been transformed into rows of handsome radiating gas-lights; and, as if these were not enough, the shopkeepers try to outdo each other in driving darkness from our streets; but those palaces and pests, called gin-shops, with blazing lamps and illuminated clocks, stand out like luminaries and burning shames, with their

"Ten thousand casks,
For ever dribbling out their base contents,
Touch'd by the Midas finger of the state,
Bleed gold for ministers to sport away."

I am, your humble servant,

ONE OF THE UNWASHED.

LOCOMOTIVE ENGINES—HISTORICAL RETROSPECT.

(Compiled from the Report of the Committee of the House of Commons, of Aug. 1831.)

The first locomotive engine was invented, twenty-eight years ago, by the late Mr. Trevithick, a very ingenious man, and subsequently improved and used by Mr. Blenkinsop and others, for the service of collieries.

Mr. Gurney stated that his carriage weighed only 2½ tons; that in 1825 he began to work it.
that in 1826 he went up Highgate and other hills; and in 1827 he went to Bath.
that he has run 18 to 20 miles an hour.
that he is able to compete with the coaches, with an advantage, as 2*l*. 10s. to 15s. per hundred miles.
that he makes no noise.

N. B.—Mr. G. run his carriage for some time between Cheltenham and Gloucester, to the great loss of his supporters, Sir Charles Dance and others.

Mr. Hancock stated that his carriage weighed 3½ tons,
that, with a piston of 9 inches, he has worked at 400lbs., and on an average at from 60 to 100lbs. on the square inch; consequently, could exert a power of 13 to 90 horses.
that he makes only one-third of the noise of others.

Mr. Farey stated that Mr. Hancock and the Messrs. Heaton were the only candidates likely to prove successful.
suggested that there should be 2 horses at every hill, for the help of these locomotives.
stated that passengers were annoyed from heat, noise, smoke, and dust.
condemns Gurney's, &c.

N. B.—The Messrs. Heaton, residing at Birmingham, were not examined.

Mr. Ogle stated that his engine is 20-horse power, with a pressure of 250lb on the square inch.
that his carriage weighs 3 tons.

has gone at the rate of 32 to 40 miles per hour; and has ascended hills at the rate of 16½ miles per hour.
explosion impossible.
he is on the point of establishing a factory, *so great are the demands for his carriage!*

Mr. Gibbs was very sanguine in his hopes of success—proposed to plough, and drive vans.
Mr. Summers (the partner of Mr. Ogle) stated that they had constructed 2 carriages, weighing 3½ tons, besides passengers.
that they had carried 9 persons at the rate of 9 miles, when the crank broke, and the carriage was sent back by canal.
has carried 19 persons at the rate of 10 miles.
has travelled at the rate of 30 miles during 4½ hours frequently; consequently 135 miles in 4½ hours.*
has ascended Shirley-hill, which is 1 foot in 6.

Such was the state of the locomotives in 1831.

Observations.

In 1833, Mr. Gurney, the most persevering of all the competitors, is beaten out of the field, to his great cost.

Sir Charles Dance, his substitute, has run many times to Croydon and Greenwich—made an attempt to go to Birmingham, in which he failed—and made, lastly, an attempt to run daily to Clapham, in which also he has failed.

Messrs. Hancock, Ogle, Gibbs, Summers, and Heaton, are all in movement, but merely by convulsive starts; although they are provided with powers that may be raised to twenty, thirty, forty, and even eighty horse power.

About twenty years have passed away in experiments, and not less, probably, than 100,000*l.* been expended upon them; yet, after all, nothing effectual has been done.

At one period steam guns were the terror of many: they were to have mowed down whole ranks of infantry and cavalry; even artillery were to be quite impotent before them; but nobody now hears or dreams of such things. It would almost seem as if steam-carriages were destined to run the same course. The writer hopes not; but if he were to look for grounds to anticipate a different result, it would not be in any of the prospectuses for steam-carriage companies that he has seen, of which the best that can be said is, that they circulate much easier than the wheels of the carriages that they respectively extol to the skies.

A FRENCHMAN.

LIST OF STEAM-COACHES AND DRAGS NOW BUILDING AND BUILT IN LONDON AND ITS VICINITY.

[We have been favoured with this list by a Correspondent, who states that its "accuracy may be depended on." We really had no idea that there were so *many* locomotive competitors in the field.—ED. M. M.]

Hancock	1	Infant, his own, built, experimental one.
Ditto	2	Era, (for a company) built.
Ditto	3	Enterprise, (for a company) built.
Ditto	4	Autopsy, his own, built.
Ditto	5	a new one now building, his own.
Gurney, Stone, Gibbs and Maudsley	1	a drag, built and altered by the said engineers, for Sir Charles Dance, knt.
Ogle	1	a carriage, his own, built, experimental one.
Squire	1	a carriage, himself and others, experimental one.
Fraser	1	a carriage, himself and others, building, experimental one.
Gibbs and Applegath	1	a drag, themselves, experimental one, built.
Gatfield and Bower	1	a drag, themselves, experimental one, building.
Andrew Smith	1	a drag, (for Mr. King) experimental one, building.
Palmer	1	a drag, his own, experimental one, built.
Redmund	1	a carriage, experimental one, building.
Manting, Joseph	1	a carriage, his own, experimental one, building.
Phillips and Co.	1	a carriage, their own, experimental one, building.
Silk	1	a carriage, his own, experimental one, building.
Smith and Co.	1	a carriage, (for company) experimental one, building.
Mile-end (name not known)	1	a carriage, (for a company) experimental one, building.

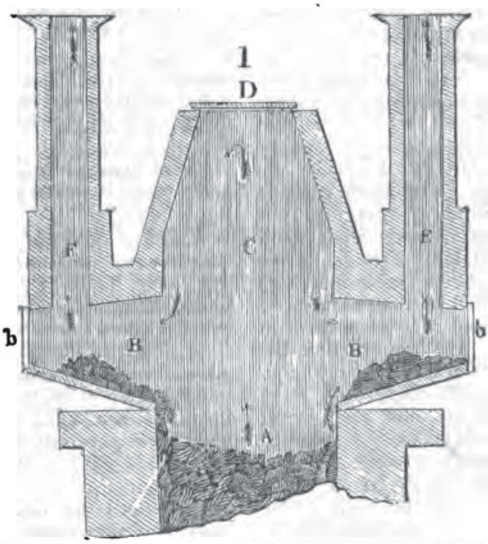
* Mr. Summers afterwards explained that what he meant to say was, that he had travelled "for the space of four *miles* and a half—not four *hours* and a half—at the rate of thirty miles an hour."—See his Letter in Mech. Mag., of March 24, 1832.—ED. M. M.

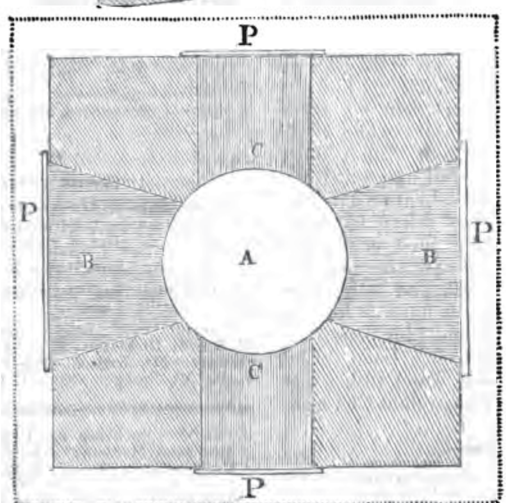
Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 542. SATURDAY, DECEMBER 28, 1833. Price 3d.

TEAGUE'S PATENT PROCESS OF MANUFACTURING IRON.





2.

other engines, neither suction-cocks nor any substitute for them are employed.

In the suction-cock of which I now send you a description, as promised, there will be found several material improvements. In the first place, the cock is placed horizontally instead of vertically, the key is screwed into its seat instead of being rivetted, and the waterway through it is the full size of all the other passages.

Fig. 1 exhibits a sectional view of the hinder portion of an engine-cistern, with the feed-pipe and suction-cock in its proper place. A is a trough running nearly across the cistern, into which the lower orifice of the cock is immersed; the trough being below the general level of the cistern, the water will run into it as long as there is any left, and the engine will work itself quite dry. The water, entering under the cock, will have to make one right-angled turning, while in the old suction-cocks it has to make *three such turns*.

b is the perforated covering drain, which has a small slip moving on hinges, to admit the handle S to traverse. When the engine is working through the suction-pipe, the handle is in this position, the lower orifice being closed; but when it is requisite to work from the cistern, the handle is drawn back to C, which shuts off the communication with the suction, and opens the cistern orifice at *a*. The slip being shut down again prevents any improper turning of the handle.

Fig. 2 is a view of the cock from above, the dotted lines showing the large waterway.

In a suction-cock there are three passages, two of which are always required to be open; if enough substance, therefore, is left to close the remaining one, that is all that is necessary. This is done at *e*, fig 3; but, to strengthen the key, two slight pillars are left, which have this effect without at all obstructing the waterway. A provision is made in this cock for introducing oil occasionally, by a thumb-screw at *d*.

Suction-cocks are much injured by frequent unnecessary turning; the cock should never be moved unless it is absolutely required to change the mode of supply.

A suction-cock, constructed on this principle, composed of good metal, and made in a workman-like manner, would, with proper care, last out the other working parts of a fire-engine. Such an arrangement would permit the barrels to be filled with water at every stroke, which many engines will not if worked quickly. Most of the older fire-engines produce a maximum effect when worked at a very moderate speed, while many modern engines, of a corresponding size, and worked by the same number of hands, will throw twice the quantity of water in any given time. I remain, Sir,

Yours respectfully.

W. Baddeley.

London, Dec. 3, 1833.

THE UNDULATING RAILWAY.

Sir,—It is unnecessary to quote extracts from Mr. Cheverton's letter to justify me in the observations I took the liberty to make on it, inasmuch as that gentleman still persists in maintaining the erroneous opinions he has taken up on the subject in question. In his last communication, page 72, he ridicules the idea that more steam power should be expended when a locomotive engine is moving at a uniform velocity than is necessary to overcome friction. Now, this is the very point in dispute. It is one of the consequences attending the laws I have been explaining, and if steam power were constant that circumstance would not take place; it does, however, take place, and to an extent increasing as the velocity of the moving body increases. This is exemplified in the case of the ship in motion before the wind: if the maximum speed of the vessel be two miles, and the wind twenty miles the hour, the power expended, more than is necessary to overcome resistance, will be one-tenth of the whole, which will increase to one-half, as I said before, when the vessel moves at the rate of ten miles an hour.

Mr. Cheverton says he argued the matter hypothetically,—that is, he assumed that gravity was not a uniformly increasing power, or else that mechanical power could, by construction (at least), up to a certain point, be made equally available as gravity; and then reasons accordingly. But such reasoning cannot be admitted. Gravity *is* a constant or a uniformly increasing power in free space; the fact is too well established to be now shaken,—it is, in fact, the foundation of the science of projectiles. On the other hand, human ingenuity has not been able to discover a mechanical power which would, even up to a certain point, be equally constant or effective with gravity. Mr. Cheverton's scheme of getting plenty of steam and large cylinders, is curious. He forgets that the machines moved by either power must start on equal terms, and that the resistance opposed to both must be the same. If so, then who can doubt whether the machine moved by the power of gravity will not first reach the winning-post? If the power which is the cause of gravity be a mechanical one, it must move with the velocity of light or electricity, as the comparative speed of the moving body, at any velocity we can command, bears no sensible proportion to it.

The distinction Mr. Cheverton draws between impulse and pressure is not applicable, and has nothing to do with the question.

As the experiments Mr. Badnall has lately performed are of the most interesting description, and require to be well authenticated, particularly as there is great nicety required in making them, it would be very desirable if some scientific gentlemen of eminence would attend, and give, under their signatures, the results. I do not mean to say there was any wilful misrepresentation made, or that Mr. Badnall's reports are not accurate, but it requires very nice observation to ascertain the acquired velocities of the carriages at the foot of the ascent.

MENTOR.

Dublin, Nov. 4, 1833.

THE UNDULATING RAILWAY—FURTHER EXPERIMENTS.

(*From the Manchester Chronicle of Dec.* 21.)

"We had an opportunity of witnessing, yesterday, some interesting experiments which were made with a view of further demonstrating the correctness of the undulating theory. Since this important question became a subject of open discussion we have narrowly observed its gradual rise in public estimation, and noticed the published statements of the experiments which have been made on the Liverpool and Manchester line, as well as in other places. The result of our observations is a decided opinion in favor of the principle. It appears that the experiments which were yesterday made were suggested by Mr. Peter Ewart, of this town, who proposed as a motive power the use of a *falling weight* instead of locomotive force. Two railways were consequently laid down by Messrs. Stephenson and Badnall at Pendleton—one of which was undulating, the other horizontal, each being terminated by an inclined plane 4 feet 11 inches in length, rising 11 inches from the base. The length of the two railways was about 45 feet, and the dip of each undulation (there being two) was about 10 inches in 21 feet; the remaining portion of the undulating line being level, in order that the wheels, at starting, might be on the flat surface. The result of all the trials fully corroborated the estimate of the advantages of the undulating line, founded on the previous experiments. The velocity on the curved line was nearly double that upon the the level; and the momentum, after traversing from level to level, was even greater on the former than on the latter. Our distinguished townsman, Dr. Dalton, was present during the whole time, and, with several other gentlemen who attended, appeared much interested in the result. It unfortunately happened that the weather became extremely unfavorable towards one o'clock, and consequently we were deprived of the opportunity of witnessing, on this occasion, so full a series of experiments as Messrs. Stephenson and Badnall were prepared to exhibit. We saw, however, enough to satisfy us that the undulating principle is a decided improvement, and we cannot help feeling the desirableness of an extensive and immediate trial of its merits, on a large practical scale, before immense sums of money are needlessly expended in levelling, &c. on the projected lines of new railroad."

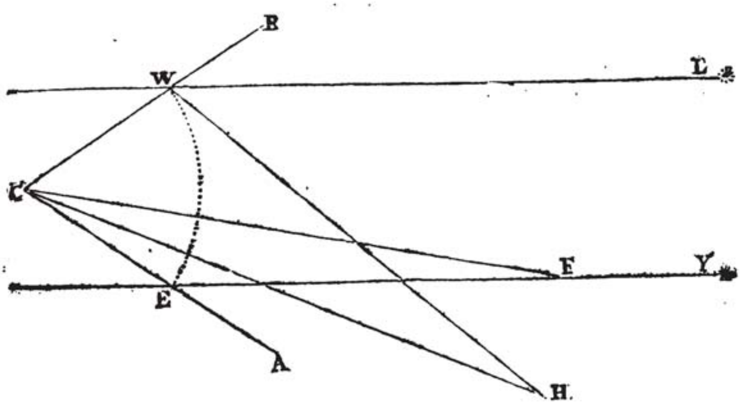
(*From the Manchester Guardian of same date.*)

"An interesting experiment, which had been suggested by Mr. Ewart, of this town, as likely to furnish a decisive test of the merits of Mr. Badnall's 'undulating principle,' was tried yesterday, near Mr. Fitzgerald's colliery at Pendleton, in the presence of Dr. Dalton and several other gentlemen. The experiment was of this nature:—Two lines of railway, of equal horizontal length, were formed, side by side, one level, the other undulating, but both commencing and terminating at the same elevation. A carriage (or rather a pair of wheels on an axle) was made to traverse these two lines in succession, by means of a weight suspended over a pulley; and the object was to ascertain whether the carriage thus started from and arriving at the same altitude on each line, and traversing in each case the same horizontal distance, would have acquired a greater momentum on the level or on the horizontal line; Mr. Ewart's opinion being that the difference in momentum, if any, would determine the relative merits of the two lines. The momentum was measured by ascertaining how far the carriage would ascend an inclined plane placed at the end of each line of railway. During the experiments the wind, unfortunately, was very boisterous, and necessarily produced some little uncertainty in the results; but the experiments, so far as they were tried, were clearly in favour of Mr. Badnall's theory; the momentum acquired on the undulating being greater than that on the level line. The experiments were at length interrupted by heavy and continued rain, when it was agreed by the gentlemen present to adjourn them to some future day, when there should be a prospect of calm and favourable weather. Of course, it is hardly necessary to say that the carriage traversed the undulating in a much shorter time than the level line. The average difference in time was from 8½ to 15 seconds.

It would seem from the last extract, that the Editor of the *Guardian*, who was formerly opposed to the Undulating Railway System, may now be ranked among the numerous converts to it.

TRIGONOMETRICAL SOLUTION TO DETERMINE THE PARALLAX OF MARS.

From Observations taken with Shires' Polar Equatorial (described in the Mechanics' Magazine of January 21, 1832). By Mr. William Shires, Mathematical Tutor.



Sir,—From the mean of nineteen observations, taken November 22, 1832, in latitude 51° 33′ N., the whole was reduced, by ratio, to an interval of four hours, two hours on each side of the meridian, and the results were as follow:—The eastern limbs of Mars and Aldebaran were brought into contact, when two hours east of the meridian; four hours after those limbs were 3′ 51″·863 apart (in the mean of results); during which interval of four hours, the geocentric change of Mars was 3′·40″ from the Nautical Ephemeris, and the cosine of the latitude of the place had described an angle of 60°, which cosine, in miles, is 2475·5, and therefore forming an angle of 30° with the meridian, at the first and last observations respectively. Whence the following was the solution:—

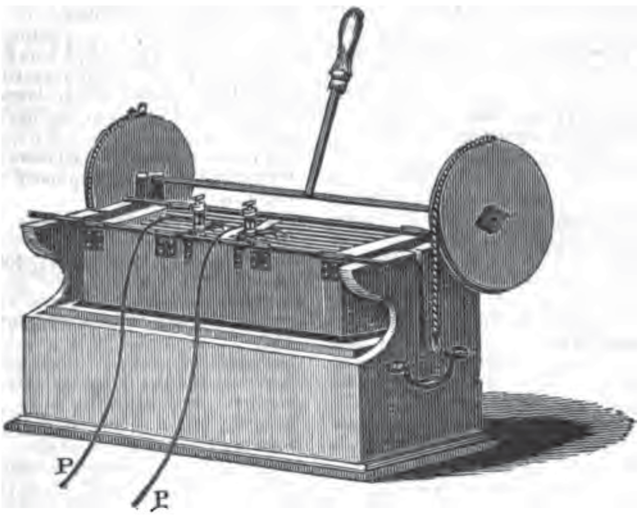
Let C W E represent the plane of the parallel of latitude, and C the centre of its diurnal rotation; E my station at the first observation, and W at the last, the earth's rotation having carried me through E W of 60° in the four hours; let also the parallel lines Y E, and L W, represent rays of light from Aldebaran to the stations E and W. Also by the problem, $\angle$A E Y$=\angle$B W L$=$30° each.

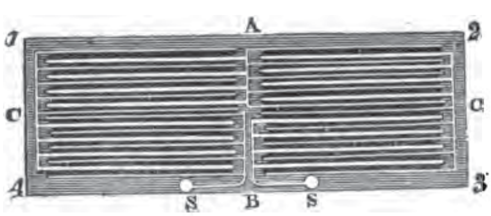
Mechanics' Magazine,

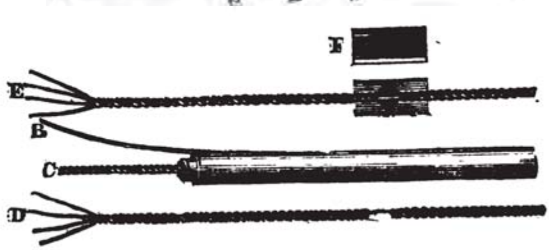
MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 543. SATURDAY, JANUARY 4, 1834. Price 3d.

HARE'S GALVANIC ROCK-BLASTING APPARATUS.





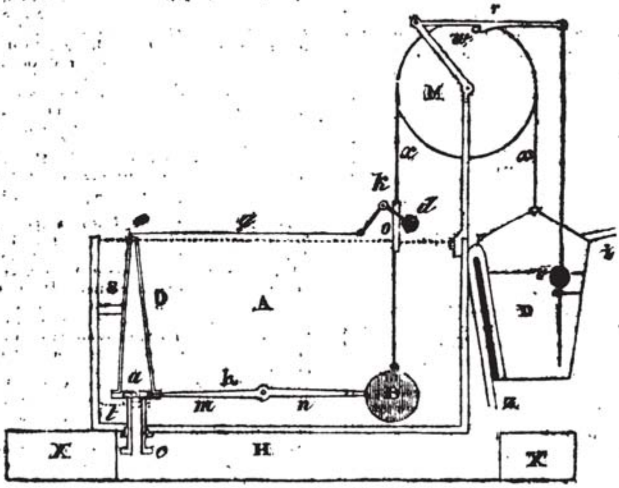


Mechanics' Magazine,

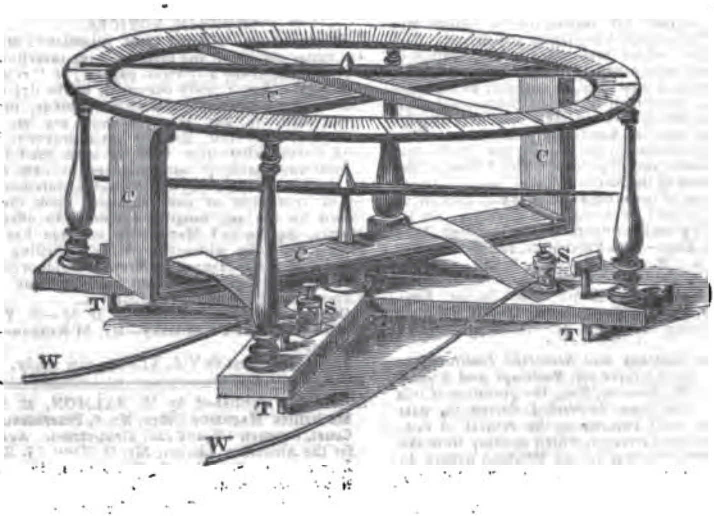
MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 544. SATURDAY, JANUARY 11, 1834. Price 2d.

WITTY'S HYDRAULIC PROJECTOR.



HARE'S GALVANIC MULTIPLIER.



THE UNDULATING RAILWAY.

Sir,—I return to Mr. Cheverton, and to his erudite, cool, collected, and polished letter, contained in your 534th Number.

In my last, I endeavoured to show that, according to Mr. Nicholas Wood's published experiments, the resistance of the atmosphere, as opposed to the motion of rolling bodies, *does not increase* according to the squares of the velocity. I will now adduce corroborative proofs of the correctness of the opinion I have formed on this subject. It is, I believe, not to be disputed—and I am again compelled to refer, for the truth of my observation, to all engineers practically conversant with railways and locomotive engines,—*that the greater the velocity on railways* (as stated in my letter of the 11th Sept., page 440,) *the less has been proved to be the total friction or resistance*; or, in other words, as it requires *more* power to move a body from a state of rest to a given state of motion, than to keep it in that state of motion when attained, so the *greater the velocity attained*, the less is the *effective* power *absolutely required* to move a rolling body *over a given space*. It is true, as I have before remarked, that the greater the velocity with which a locomotive engine travels, the greater is the unavoidable sacrifice of *steam power;* but could this sacrifice be prevented, and could the precise power necessary to be employed at maximum velocities be measured, the truth of my position would be undeniably confirmed. I cannot, perhaps, impress the truth of these observations more forcibly, than by stating a fact which has been frequently proved on the Liverpool and Manchester railway:—Suppose the engine to be drawing a train of loaded waggons at the rate of 30 to 35 miles an hour, and that Mr. Champion Cheverton were to be in the *first open* carriage. The chains by which the waggons are attached to each other would necessarily be, when thus moving, at their full stretch. Now, if Mr. Cheverton were to lay hold of the chain which fastened the *first* carriage to the *second*, he would find that he could, without difficulty, *draw up all* the following carriages to his own; thus, at a velocity of 35 miles an hour, the *friction* would be so *reduced*, and the *total* resistance *so reduced*, that the Champion's *own arm* could move a load with ease, upon a dead level, which at 5, or 10, or 20 miles an hour, it would be impossible for him to accomplish. Now, how is this? and how does this agree with the Utilitarian's declaration, as follows:—"Take a locomotive engine and double its velocity, and what do we do? We will say nothing of *inertia*. (For this I thank him, for I am heartily sick of the word.) The resistance arising from friction will continue *the same*, but then the velocity of the force which copes with it is doubled; we, therefore, double the expenditure of power in a given time." "In regard, however," he continues, "to the resistance of the air, it does not continue the same, for the quantity of air encountered by the engine is doubled, and the velocity with which it is struck is doubled; the resistance, therefore, is quadrupled, or it increases as the square of the velocity; but the velocity of the force to cope with it is also doubled:—we, therefore, *octuple* the expenditure of power in a given time, so that, whilst the *demand* for steam is, in regard to friction, simply as the velocity, it is, in regard to the resistance of the air, *as* the *cube of the velocity!*" Now, readers of the Mechanics' Magazine! bear in mind that *the Champion*, in the above paragraph, has endeavoured, he says, to express himself in a manner suitable to the capacity of a child, and for which he apologises to *you*. I am the tender suckling to whom this beautiful little lullaby is addressed! Mark, readers, the burden of it:—

1st. At double velocities a double expenditure of power is necessary, in a given time, to counteract the retarding force of friction!

2d. At double velocities an *octuple* expenditure of power is necessary, in a given time, to counteract the resistance of the air!

And yet, when a whole train of carriages are travelling at 35 miles per hour, the power of any man standing in the first carriage is sufficient, as above stated, *to increase* the velocity of all the succeeding carriages; whereas, at 5 miles per hour, such power would be altogether ineffectual!

Now, it is quite evident that either Mr. Cheverton or myself must be *most egregiously* in error. Mr. Cheverton insists upon it that the *power required* to move a carriage in a given time, at a double velocity, *is double in regard to friction, octuple in regard to air.*

I maintain, in regard to friction, *that the amount of force with which all bodies press upon the earth is in inverse proportion to the velocity at which they travel over the earth's surface.*

And, in regard to air, it is still my decided opinion, *that the resistance of quiescent atmosphere, when first overcome by locomotive power, which is constantly and equably continued, does not, throughout* EQUAL SPACES OR DISTANCES, *act as an opposing force with greater intensity at high velocities than at low velocities; but, on the contrary, it is my opinion that the total resistance of atmosphere* OPPOSED *to a train of railway carriages, throughout a* GIVEN DISTANCE, *is less at high velocities than at low velocities, from the inclination which all bodies, when in rapid motion, have to rise from the surface of the earth, or from a denser to a lighter atmosphere.*

Now the experiments which I am about to try with Mr. R. Stephenson will determine this point. What will Mr. Cheverton think should he find that, when a carriage is moving at a velocity of 30 miles per hour, the mercury in a barometer placed *underneath* the carriage rises an inch or two higher than when placed at the *top* of the carriage? Mr. R. Stephenson suggested this simple mode of determining the question, and he has good reasons, in which I fully concur, for believing that it will not be difficult to prove, when our experiments are completed, *why the resistance of the air* does not, at *high* velocities, act as an *opposing* force with greater intensity than at *low* velocities.

Mr. Champion Cheverton will, perhaps, in his wisdom, condescend to prophecy the results of these experiments; and, before he hears the particulars of them, favour us with his conclusions, supposing what I have stated to be really the case. He is, however, rather cautious in these matters. *What he cannot fathom, or dare not meet*, (my challenge, for instance,) he pretends to treat with "*contemptuous silence!*" His *contempt!* His *silence!* Alas!—

> "He has ventured,
> Like little wanton boys that swim on bladders,
> . . . .
> But far beyond his depth!"

Let me now next make an allusion to my recent experiments, in further corroboration of my opinion as to the resistance of the air not increasing as the squares of the velocity, when opposed to railway carriages.

It will be seen, see Experiment I., page 71, that when the train of carriages, weighing, with engine, &c., about 164 tons, attained a velocity of 20·28 miles an hour (pressure of steam 50 lbs. to the inch), the Firefly and load rose on the incline 575 yards; and (see Exp. III.) when the same load attained a velocity of 14·34 miles an hour, the Firefly and load ascended 315 yards, the steam being at the same pressure.

Now 14 : 315 :: 20 : 450;

but instead of 20 miles per hour producing an ascent of 450 yards, which, had the opposing forces of friction and atmospheric resistance been *even* equal through *equal spaces, it would have done*, we find that the load rose 575 yards!

Again (see Exp. V.), a velocity of 18 miles an hour enabled the load to ascend (the Firefly working 50lbs. to the inch steam) $457\frac{1}{2}$ yards. And

$18 : 457 :: 20 : 507\frac{1}{9}$;

but instead of 20 miles per hour producing an ascent of 507 yards, which, upon the before stated conditions, it ought to have produced, we find that the load rose 575 yards!

Pray, Mr. Editor, are the hot-brained assertions of Mr. Champion Cheverton, founded though he thinks them on generally acknowledged and received principles, to have more weight than *undeniable* truths elicited and established by careful practical experiments? But Mr. Cheverton will perhaps say, *your momentum was greater at* 20 miles an hour than at 18 and at 14! No doubt of it; but, according to Mr. Cheverton's affirmation, that a double power is requisite at double velocities, to cope with the opposing force of friction, and *an octuple power* is requisite at *double* velocities, to cope with atmospheric resistance *in a given time*, the greater the momentum the greater the resistance of the atmosphere, and the greater the opposing force of friciton, in a *given time!* Whereas—mark the difference in both spaces passed over and in the *times*—

20 miles an hour enabled the load to rise	575 yards in	116 seconds,
18 miles an hour	$457\frac{1}{2}$ yards in	$102\frac{1}{2}$ seconds,
14 miles an hour	315 yards in	90 seconds.

Now, whatever may have been Mr. Cheverton's notions as to the laws which govern atmospheric resistance, can he possibly maintain the *same arguments*, without satisfactorily applying them to, and substantiating them by, the statements of facts which I now publish, and which were witnessed by so many other engineers; or, if he cannot do this, will he explain the anomaly? But Mr. Cheverton asserts, "the more successful Mr. Badnall considers himself to be with his experiments, the more completely will he establish himself in the wrong!"

Mr. Cheverton would really be doing us a great service in the north, if he could find leisure to visit us, for the purpose of inspiring us with a little of his supreme sense, remarkable perception, and sober judgment! I am wrong, "because I contend," he says, "that the advantage gained is more and more conspicuous in proportion as the *inertia* of the mass to be moved forms the principal part of the resistance to its motion, and consequently that it is in this respect only, or in overcoming *inertia* by the aid of gravity, that any thing of any consequence is gained by my scheme"!! Now, had not Mr. Cheverton, the Champion, who detests "every thing like *logomachy*," much better answer my challenge by proving experimentally that I am wrong, or by proving mathematically that my diagrams are erroneous, than by supporting his tottering position by "*Mr. Badnall says this*," "*Mr. Badnall thinks that*," &c. &c.? And this Mr. Cheverton, too, is the individual who attacked Junius

Redivivus, by a lecture "on modern modes of philosophising!" and threatens me with HIS *rod!* Exquisite and most dreaded combatant!

But I have not yet done with my explanation of some of the points to which the Utilitarian alluded in his most *unlogomachical* and *etiological* discussion. He smiles at me for stating, "that the continuance of uniform motion (he leaves out the word *maximum*) involves a very considerable expenditure of steam power, independent of what is required to cope with locomotive resistance." Will he find any engineer in England, practically acquainted with railroads, who will tell him otherwise?—If he can, *who is he?* The Champion knows very well, though he is never guilty of "*garbling quotations,*" that I spoke of waste expenditure, or the sacrifice of steam, *not effective* expenditure.

Mr. Cheverton, however, recommends me to abandon the study of "the rapid flight of swallows and gulls."* I will when he proves himself, on this subject, to be half as well worth studying? But I must also prove the fallacy of my reasoning, "by accelerating an old woman into a trot." How nonsensical the world would think me to try such an experiment, when I have the power of accelerating *him* into a gallop, and when I find his theory, by such means, so clearly elucidated, viz., "that the resistance does in very truth act both as a provoking and as an opposing force with greater intensity at high velocities than at low velocities"! "How is it possible, Mr. Editor, to suppress a smile"! but "this is only a specimen of my lucubrations"!

The Utilitarian catches, as I thought he would, at the feather to which I alluded in my letter of the 11th Sept. About three days after that letter was written, I met Mr. R. Stephenson, sen., who made the following remark,—"Well, how goes the opposition on?" My answer, as he can well remember, was,—"Poor Cheverton, I saw him drowning and threw a feather for him to catch at!"

We now come to another point, which seems to have made the most serious impression upon him, inasmuch as "the *eye* began to laugh and the *lip* began to curl"! BEAUTIFUL PICTURE!

This gratifying change in Mr. Cheverton's countenance seems to have been made by the following observations of mine:—

"The maximum velocity could not, on the descent, be attained until the resistance of the air became equal to the force of descent, or to the relative weight of the moving body—that this could not be until the train of carriages (which in practice is impossible) moved at more than 883 miles an hour, and for the following reason,—the velocity of air rushing into empty space is estimated to be 1296 feet in 1″; therefore, at less than 883 miles an hour a vacuum behind the moving train cannot be formed, consequently there can be no compression of the atmosphere in front."

On which the Champion observes,—"Mr. Badnall has got hold of the idea that a constant force necessarily implies as its result a *uniformly* accelerated velocity in a resisting medium, until the velocity attained be equal to that at which the atmosphere would rush into empty space, when, I suppose, the resistance commences all at once, and a change takes place from *uniform* acceleration to uniform motion, also all at once."

Now, first, with regard to the velocity not becoming uniform until the resistance of the air becomes equal to the force of descent, Hutton thus remarks—vol. ii. page 205—"After the velocity is arrived at such a degree that the resisting force is equal to the weight that urges it, it will increase no longer, and the globe will afterwards continue to descend with that velocity uniformly."

Parkinson also observes,—"The *greatest** velocity, acquired by a body descending in a resisting medium, is when the resistance becomes equal to the force of descent."

Let us now ascertain at what velocity a train of loaded waggons, weighing 100 tons, must be moving before the resistance of the air could become equal to the force of descent?

In allusion to the motion of an iron ball, weighing 86 lbs., Hutton observes,—"the pressure of the atmosphere is entirely taken off the hinder part of the ball moving with a velocity of 1,600 feet per second, which must happen when the ball moves faster than the particles of air can follow, by rushing into the place quitted and left void by the ball; or when the ball moves faster than the air rushes into a vacuum from the pressure of the incumbent air."

At what velocity, then, must the train of 100 tons be moving before the resistance of the air becomes equal to 100 tons? I have no doubt but that, in a perfectly still atmosphere, a resisting pressure of 14 lbs. to the square inch, upon the front of the first carriage, would not be a sufficient resistance to render the motion uniform; and this could

* Mr. Cheverton does not condescend to explain how those birds can attain the velocity they do, supposing his theory of resistance to be correct.

* Now, can it be denied that uniform acceleration must take place until the *greatest* velocity is attained.

not be until the train were moving, if it were possible, at more than 883 miles per hour; for, supposing a body weighing 100 tons to present to the air an opposing front of 9,216 square inches, at a velocity of 1,296 feet per second, the resistance of the atmosphere would not exceed 58 tons.

In speaking of lighter bodies, and in having given an opinion, that "*as long as their specific gravities are* GREATER *than the specific gravity of the air through which they fall, they must continue to fall with an uniformly accelerated velocity,*" I do not think I have left a very fleshy bone for Mr. Cheverton to pick. Can he, barring assertions, prove otherwise? My object, in alluding to the resistance of the atmosphere in the first instance, was to prove theoretically that which is very well known in practice, viz. *that the resistance of the atmosphere is not, and never can be, the cause of that limit to velocity which we term* MAXIMUM VELOCITY *on level railways; and, moreover, that the resistance in still atmosphere never can, practically speaking, produce the uniform motion of a train of loaded carriages when descending an inclined plane.* My reasons for affirming this I gave in page 438, and all I can add is, that, whatever may be the opposite opinion of Mr. Cheverton, he cannot *prove himself* correct, nor *me* wrong; whereas he may *at any time* prove himself erroneous by watching the progress of a carriage down an inclined plane. The velocity even of an empty carriage would, under the influence of gravity, continue to be accelerated until it left the rails and broke to atoms. What, then, would be the case with a load of 100 tons? And if the atmosphere, when opposed to the force of gravity, cannot, at practicable speeds, equalise velocity, how can the atmosphere be the *only* efficient cause of equalising velocity on a dead level, the power of steam being equably continued?*

I now turn to another point of the Utilitarian's attack. He says—very innocently, "with a laugh in his eye, and a curl on his lip"—"Mr. Badnall quotes me as saying, that it would be an idle assertion to say that locomotive steam power is not, like gravity, a *constant* force. Upon this *unauthorised* assumption that I contended for steam force being equally constant like gravity, which he would *gladly* consider (as he, indeed, maintains,) to be the main point in dispute between us, he (Mr. Badnall) proceeds, after a fashion of his own, to demonstrate that it is not a similar *constant* force. I (Mr. Cheverton) *did not say it was.* My words were—'Forces can be considered as only of two kinds, those which operate *continually** like gravity, and those which act only for an instant by impulsion. It would be an idle assertion to say that steam locomotive force is not of the latter, but of the former kind. Here is nothing (continues Mr. Cheverton) about *constant* force, but of steam force operating *continually* in contradistinction to an instantaneous action, and yet this controversialist,' &c. &c. *even falsifies the terms that are applied.*"

Now, gentle reader, mark well the sophistry of this Utilitarian—measure the depth of his memory—and judge, *have I falsified him, or has he falsified himself?*

In No. 514, page 166, Mr. Cheverton, the Champion, thus writes:—

"He (Mr. Badnall) appears to think that the locomotive force which urges a carriage along a level plane is not of a *similar* kind to that of gravity, which carries it down an inclined plane: he speaks of the latter truly as being a uniformly accelerating force, but does not admit of the motive force of the engine being of an accelerating kind, though he must be aware that it is a CONSTANT force! Whether it is like gravity, uniformly accelerating, is not necessary to the inquiry, depending altogether upon practical arrangement."

Again, to prove what Mr. Cheverton really means, he follows this up by saying, "If there be a surplus of locomotive force (and this is the particular which Mr. Badnall forgets) over and above what is demanded for locomotive resistance, &c. &c. &c. (see his letter) *the time in attaining uniform motion will be precisely the same whether the carriage travels down the inclined plane, or along the horizontal line*"!

Again, in No. 514, page 355—I must beg, Mr. Editor, your attention, and that of your readers, to the Utilitarian's following remarks, where *a most unaccountable loss of memory* (to say the least of it) *is evident*—he says (and I must now *repeat* words of Mr. Cheverton's which I have before introduced)—"Forces can be considered as only of two kinds—those which operate continually like gravity, and those which act only for an instant by impulsion. It would be an idle assertion, to say that steam locomotive force is not of the latter, but of the former kind, were it not that Mr. Badnall appears to dispute it, and says that I rightly construe his opinion in believing that he *does not* consider locomotive force to be of

* The Champion did not say the *only* cause, but the only *efficient* cause. What have we to do with any but efficient causes?

* How wonderfully opposite are the two terms "*constant*" and "*continual.*"

a similar kind to that of *gravity*. The difficulty of dealing with this opinion arises (continues the Champion) from its being so evidently erroneous, that to controvert it appears like uttering truisms. *Is gravity in constant action?—so is steam! Does gravity in consequence thereof produce accelerated motion?—so does steam!*"

It is true, that Mr. Cheverton afterwards makes allusion to the possible difference in the two forces as regards *uniform acceleration*, but is not his opinion at the moment rendered clear by the following passage:—"*Undoubtedly, locomotive resistance very soon exhausts the force of the most powerful engine as rapidly as it can be produced, but such* IS ALSO THE CASE WITH GRAVITY," &c.!!

Now, Mr. Editor, have *I falsified* the terms of Mr. Cheverton or not? Has your able correspondent Mentor "relied on the perversions and false statements" of Mr. Badnall or not? Has Mr. Badnall's conduct been disingenuous in concluding from Mr. Cheverton's letters that he considered the power of steam, like the power of gravity, *a constant force*? But, Sir, the Utilitarian Champion says, "*that all his statements, in respect to the similar effects produced by steam and gravity, are* HYPOTHETICAL, *and* CAREFULLY *guarded by conditions*"!

Now, Sir, if *all his* arguments in reference to the undulating railway are, according to his own confession, *founded on hypothesis*; and, moreover, if each hypothesis is CAREFULLY *guarded by conditions*, what reliance can be placed on his opinion in opposition to the *experimental* and *mathematical* proofs which have been laid *unconditionally* before the public?

But the Champion, finding it impossible to make good his argument, and *thinking* it probable (in future he may consider *this* a truism), that Mr. Badnall will never allow, at any sacrifice of time or labour, or, much more, an accusation of wilful misrepresentation or falsehood, to be laid to his charge, has endeavoured to smooth his errors, and to explain his nonsense, by informing us what *he considers it possible* for ingenuity to bring *practically into effect* in regard to locomotive steam force.

First and foremost—*plenty of steam!*—an INCESSANT *supply!*—*fully commensurate* with the *intended velocity*.

2dly. No *wire-drawing* of it, *nor shutting it off but at the full stroke!*

3dly. There must be *two* or *more* cylinders!!

4thly. They *must be large!*—the motion of the pistons must be slow, the needful velocity being effected by intermediate machinery!

Now, Mr. Editor, had I supposed, when I took the trouble of replying to Mr. Cheverton's various communications, that the whole of his arguments were founded not only upon hypothesis, as generally understood, but upon imaginary improvements in locomotive machinery, I should have taken a very different course; but I confess, from the various communications which I had previously seen of Mr. Cheverton's in your Magazine, I had formed a different opinion of his candour and of his courage.

But he talks of the *jargon* and *inanity of the schools*—he therefore has had, perhaps, more experience in these matters than myself. He detests *logomachy*, however, (and *proves* his detestation); so do I.

A word as to "periphfugal force."

The Champion must be a *most* courageous being: he places the poor insignificant word which I have coined in the category of *his nonentities*; but immortalises his own inventive genius by the introduction of that classical word, which I hope may even be co-existent with his name, "UTILITARIAN!"

Again, the Champion talks of "*locomotive duty*," which I must estimate by multiplying the *resistance into the distance*. Let him think again—the undulating line, where gravity throughout most of the distance is a freer and powerful assistant, is far, far superior to the horizontal line, where its ever-acting opposition can only be mastered by a proportionate expense of mechanical power.

What further does the Champion say? That you, Mr. Editor, and you, Mr. Sanderson, almost agree with him. And he asks you whether you really believe that you can take advantage of the gratuitous power of gravity, on a general level, without being under the necessity of repaying this assistance? Thus he puts to you the very question in which the whole of this controversy originated, and he answers, after seven months dispute, "ASSUREDLY NOT"!! Why did not this Utilitarian Champion settle the question before?—"ASSUREDLY NOT"!!

"In dreadful wars
The high-built elephant his castle rears,
Looks down on man below, and strikes the stars!"

But Mr. Cheverton, with his dear laughing eye, and his sweet curling lip, has had more compassion upon his foes!

Mr. Cheverton says, however, that he has given me one piece of valuable information—*he presented to me for the first time the idea of the possibility of steam being husbanded* on the *undulating railway!* I should think he is not aware of the number of experiments which were tried on the models before this controversy began, with a view of ascertaining *with how little locomotive power a given load* could be conveyed from one summit of an undulation to another.

I am also desirous of correcting another error into which Mr. Cheverton has fallen. He says, that the models in the Adelaide-street gallery were put down to convince me of my error. I beg to assure him, that they were put down by the particular wish of Mr. Perkins, who had declared his approval of the principle, and without one farthing's cost to the proprietors of the gallery—both the engine and model railways being at this moment at my own or Mr. Giles's disposal.

To conclude, it appears that the Champion takes me for an Irishman—he talks "*of a true Irish perception of things.*" He is mistaken: I am an Englishman, though were I *Irish* I should have been equally proud of my country, and equally prepared to defend myself against the most uncourteous attack that, upon a scientific subject, it was ever my lot to witness.

I have now *welcomed* Mr. Cheverton's "pat on the cheek"—his "stern and magisterial advice"—and his "*wink and his nod,*" and, while anxiously waiting for "*his rod,*" I beg to assure him that although I have objected to continue, as unnecessary, any further hypothetical discussion on the merits of the undulating railway, I have no intention of reliquishing the *logomachical discussion* which he has introduced, as long as the readers of the Mech. Mag. may find amusement in, and you, Mr. Editor, find room for, a display of satirical prowess.

I have an occasional half-hour to spare in an evening, and when engaged, I can put (if it do not interfere with his more important occupations) the lucubrations of *Champion Cheverton* into the hands of my youngest son.

And now, Mr. Editor, returning to a more serious subject, I must again express my regret at the tone of argument into which Mr. Cheverton's letter has unavoidably led me; and to those of your readers who have done me the honour to peruse with interest my previous communications, I beg particularly to apologise. There are cases, in argument, wherein a man is compelled to use weapons which he would willingly forego; and I must beg those who think my remarks either tedious or unnecessary, to reperuse the extraordinary letter which provoked them. I hope they will give me credit in such case for returning, like Diogenes, blow for blow, in perfect good humour—and for a good motive.

"σὶ μὲν ἄλλοι κύνες τοὺς ἐχθροὺς δάκνουσιν, ἐγὼ δὲ τοὺς φίλους ἵνα σώσω."

I am, Sir, with great respect,
Your very obedient servant,
RICHARD BADNALL.

Farm Hill, Nov. 19, 1833.

THE PHILOSOPHER OF BOLOGNA UNMASKED; OR GALVANI NOT THE DISCOVERER OF GALVANISM.

Sir,—It appears to me very singular that no notice should ever have been taken of an experiment made by M. Du Verney, before the Fellows of the Royal Academy at Paris, in the year 1700, and published by their secretary, the celebrated M. Fontenelle, in his Account of the Transactions of the Society for that year. It is there related that M. Du Verney exhibited a dead frog, and on irritating it *with a scalpel* the nerves of the belly, that led to the thighs and legs, trembled and suffered a sort of convulsion. He afterwards cut the nerves in the belly, and stretching them with his hands, a similar convulsion was produced *by the application of the scalpel.* Now, though it may, at this distance of time, be impossible to adduce positive proof that Galvani was acquainted with this previous experiment of M. Du Verney, I cannot help thinking that it is *quite as likely* he was so, as that he should have come to a knowledge of the fact in the strange way he pretended, namely, through one of his pupils accidentally touching *with a scalpel* the crural nerve of a frog, which was being prepared in the laboratory of the professor, to make a soup for his sick wife. That a *frog* should have been the animal operated upon in both instances, and a *scalpel* the operating instrument, are coincidences pregnant with suspicion. At all events, this much cannot be disputed, that the Bolognese philosopher did at least only discover what Verney had discovered and made known to the world long before; though there is now, probably, as little chance of our seeing the name of *Verneyism* substituted for *Galvanism,* as *Columbia* for *America.*

I am, Sir, yours sincerely,
R. W. DICKINSON.

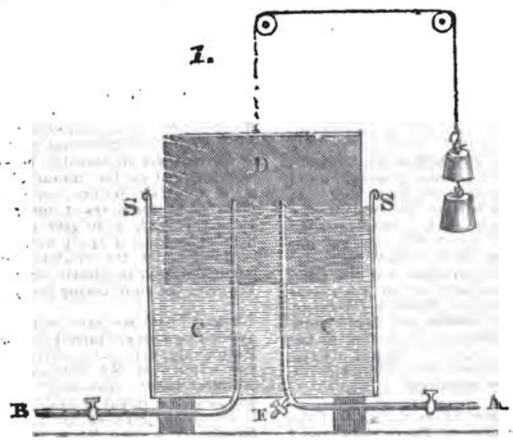
Ilfracombe, December 16, 1833.

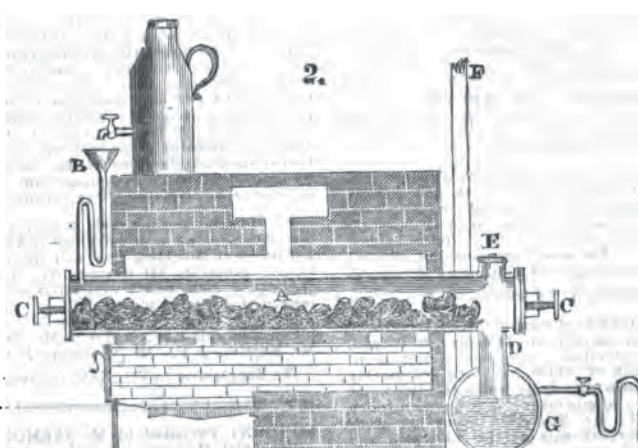
Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 545. SATURDAY, JANUARY 18, 1834. Price 3d.

DOMESTIC OIL GAS APPARATUS.





whit nearer being filled, except with uncondensed steam. Again, how is he to clean his retort? After taking off the cover, how is he to remove the coke when he finds it congealed into large masses, and, by means of the shelves inside, still more effectually bound together? The gallons of *undistilled* oil, too, which would instantly ignite? All this, moreover, behind the kitchen-grate, with, peradventure, a variety of culinary preparations basking before it!! The cup of oil, fig. 4, and the supply, fig. 8, are equally incomprehensible.

Fig. 2, of the prefixed engravings, is the plan of an improved oil gas apparatus, of which the following is the description. A is a cast-iron retort, about 7 feet 6 inches long, and 7 in diameter, set in brick-work, with the usual flue holes, &c. To one end is fixed the syphon for supplying oil, consisting of a bent iron pipe B, with a funnel on the top, to receive the matter to be converted into gas; the other end is screwed into the retort. CC are the two end plates closing the retort, called bonnets; for it is preferable to have it open at both ends, for facility in drawing, cleaning, &c. The bonnets are smeared over with a lute, to render them gas-tight—as stiff clay, or whiting and sand mixed with water—and secured by means of a cross-bar with a screw through it. The oil falls into the syphon B, and thence into the retort, which is half filled with coke or broken bricks; it is immediately converted into gas, which passes out at the other end, through a pipe on the under side of the retort D, dipping about 2½ inches into the oil contained in the hydraulic main (of which a section is given); by passing through which oil it becomes purified, and the oily particles which the gas carries with it become condensed, and the pure gas passes off by the pipe F, at the end of the hydraulic main—fixed to the top, but *not dipping into the oil*—into the gasometer. The volatile oil which condenses in the operation of purifying the gas, as it increases, is kept to the proper level by a small pipe G, fixed at the exact height the oil is required to be kept to; at the other end it is furnished with a syphon, to prevent the egress of the gas; it may be carried to a reservoir, and mixed with the oil or material in use, and worked over again. There is another opening, E, at the top of the retort, over the pipe D, also closed with a small bonnet, for the purpose of cleaning the pipe D, by passing a bar down it.

The charge of cokes in the retort may be withdrawn, and new ones substituted, once in twenty-four hours: when in constant work they will admit of being used several times.

Should you think the above description worthy a place in your instructive Magazine, it is at your service; and while it may entertain some in its perusal, the insertion of it will oblige an old correspondent and subscriber.

BRACKSTONE.

THE UNDULATING RAILWAY—REPLY OF MR. CHEVERTON TO KINCLAVEN.

Sir,—It was, I confess, with considerable surprise, that I saw an opinion in favour of Mr. Badnall's undulating railway scheme, pronounced by a gentleman of Kinclaven's mathematical attainments, grounded, as that opinion was, not on any presumed advantages of a practical nature, in respect to which it would not be extraordinary that a person who acknowledges himself not to be an engineer, should be liable to error, but on a deliberate and scientific investigation of the theory of the scheme, and advanced as it was on the credit of his character as a mathematician—a claim which I have as little inclination as I have reason to dispute. It affords another proof, that the powers of mind suitable for simple abstract reasoning, are not always the best calculated for the complex and less limited inquiries comprised in mechanical philosophy. The conclusion at which he arrived was this, that with "the same application of power" a given horizontal distance can be gone over in less time by means of an undulation, with the aid of steam, than on the level line by steam alone. This was not denied, so far as the effect is produced by the inertia of the mass to be moved being overcome by the force of gravity; but since the case evidently involves the display of "that same application of power" in the shorter period of time, and because there is no reason why the engine should not put forth that power on the level as well as on the curved line in the very same time, it was contended, that similar velocities would take place on either of those lines, for equal causes produce equal effects. The influence of gravity on the inertia of the mass is here excluded as a conceded point, and the difference of friction is not a mooted point.

Kinclaven takes it for granted, though it is a strange oversight, that the capabilities of the engine in regard to velocity and intensity of force cannot be the same on each line, or rather he assumes them to be less in one case than in the other, and thus very satisfactorily establishes his point. If, indeed, he acknowledges that no *effective* assistance can be obtained from the force of gravity, other than in overcoming inertia, he no longer ranks with the supporters of Mr. Badnall, for they claim more; nor does he stand in opposition to the opponents of the undulating scheme, for they have often conceded this point as a thing of little consequence, and more easily accomplished, if thought convenient, in other ways. Kinclaven thinks that I have "paid greater attention to metaphysics than to the more certain science of logic." He himself shall judge of my proficiency in the latter *art*, as I should rather call it, by my detecting the *petitio principii* involved in his argument—a criterion, I confess, of doubtful merit, for the fallacy is flagrant and palpable. When, however, it was pointed out to him, I did expect, from his candour and intelligence, that he would have acknowledged it. Judge, then, of my surprise, when it was announced that a *reply* from Kinclaven to Mr. Cheverton was forthcoming; and imagine if you can, Mr. Editor, my greater surprise, when, on looking over the article, I found not one word of reply, admission, or justification in it, but an addendum in the postscript, importing, that he thanked me for my trouble, but that he could not give his assent to any of the remarks that I had made on his solution of the problem! Perhaps not; but it would have been as easy for him to have given his dissent, with his reasons for it in his own behalf, as it was to volunteer dissentient observations, and calculations too, in behalf of Mr. Badnall. As considerate in his silence as he is charitable with his assistance, Mr. Badnall can draw but an ill omen from this affair as to the goodness of his cause. Nothing less than annihilation must assuredly have been the fate of one, who, "not deeply read in the abstruse science of mathematics," had the presumptuous daring to enter the lists against so approved a mathematician, if his observations had not been based on that plain common sense, which neither the differential nor the integral calculus to boot, are able to overturn. Kinclaven is correct in his conjecture as to the extent of my mathematical attainments. If he can read Laplace,* I cannot, nor others beside, and yet I have not been so unwitting as to pronounce an opinion in favour of the undulating railway scheme.

But Kinclaven is probably hesitating as to what ought, in the case of locomotion, to be considered as the true measure of force, and does not see his way clearly, whether it should be estimated in reference to the space over which it acts, or the time during which it acts, or whether these modes are applicable entirely, partially, or not at all. I am fully aware that such considerations involve the long-agitated question, about the proportionality of force being to the weight or pressure multiplied into the velocity, or into the square of the velocity—a question which has been warmly discussed among mathematicians, from Newton and Leibnitz, down to Smeaton and Dr. Wollaston. I have formed my own opinion, and Kinclaven may form his, and take his time too, for neither of our opinions is of any consequence in coming to a decision on the merits of the undulating principle. Utility requires us to ascertain only the comparative current cost of producing equal velocities on the two kinds of railway. The mere stoker of the engine is as competent to this task as the most accomplished mathematician, though it may require something more than *his* forethought to predicate the result beforehand.

It is curious to observe how often, in every department of knowledge, the parade of science is employed to obscure the plainest truths, and perplex our most simple apprehension of things. Philosophers would have taught us, from considerations regarding the maxima and

* I am happy to learn, from one who is doubtless a much better authority for the fact than I can pretend to be, that there is a greater number of persons who can read the "*Mecanique Celeste*" than my question supposed there might be.

minima of variable quantities, that our practical men were much in the dark in the application of the forces at their command, and that it was possible, in one sense, to gain in time without losing in power. Without entirely disputing the truth of this observation, it may be said, that the most perfect practice suggested by good common sense, and experimental views of the subject, will be found to coincide with the more profound scientific investigation which embraces all the circumstances of the case. There are other things to be thought of besides time and motion. Limited to these simple elements the philosopher is right, and right also in a qualified manner in cases where the whole of the force is not appropriated; but the acute practitioner is generally called upon to study economy, not merely in the working, but in the positive consumption of power; frequently to neglect time altogether; to study how to *avoid* momentum and its effects, as well as how at other times to produce it with the best effect; to study most commonly how to obtain *uniform* motion, and the velocity most appropriate to the desired result; in a word, to study the peculiar nature of the work which he would employ his force to perform, with all the contingencies appertaining to it, rather than to devote his exclusive attention to those things only which can be typified by the mathematician's skill. Philosophy concerns itself about those pure and unmixed quantities which it can symbolise with a perfect analogy; it disregards all the modifying influences which it cannot include in the most simple hypothesis; and it necessarily does so, for no calculus commensurate with the powers of the human mind is adequate to an investigation of all the concomitant causes which bear upon the final result, as exemplified in the most common effects. The science of the higher astronomy is the most complete and perfect of all the intellectual works of man, and yet it is so only because of the extreme simplicity of its fundamental truths. In our inquiries into things in general—things practical—complexity meets us at the very outset, and the business of philosophy is, first, to approximate by the aid of science, and then to modify, correct, and perfect by the aid of experiment. But the misfortune is, that the first procedure being the painfully elaborated result of our own intellectual doings, it becomes the pet child of the brain, and is too apt to be taken for nature's legitimate offspring. It is quite right to trace out and follow up the scientific research to its utmost extent, but it is altogether wrong to identify its conclusions with the results which the actual working of things produce. Hence it is that the practical man, guided by practical rules, and influenced by practical considerations, proceeds with a degree of wisdom superior to that of the mere man of science, however profound and extensive his attainments, but which the latter is the first to acknowledge and the best qualified to honour; whilst the sciolist is disposed to set it at nought, because it is what he can neither appreciate nor understand. Pardon this digression, Mr. Editor, in favour of that class of men, who, Kinclaven says, ought to be mathematicians, and who are described by your correspondent, Mr. Sanderson, with peculiar courtesy, as "the new-fledged race of engineers," but at the same time admitting that some among them, as the pages of your Magazine can witness, are neither practical men, nor men of science. It is, however, not to be inferred that there is not generally a union of sufficient science with their practical knowledge and experience, to assist them in their various undertakings. This testimony will come with the better grace from one who has not the honour to be enrolled in their body.

Perceiving that Mr. Badnall entertained very inadequate as well as very incorrect notions, respecting the resistance of the atmosphere at high velocities, I put this question to him, "whether he had ever calculated how many hundred horses' power an engine must be, in order to produce a velocity of 100 miles an hour, in opposition solely to this resistance?" Kinclaven cavils at the amount of resistance implied in this question, and states, that "when the barometer is at 30 inches, (a most commendable attention to extreme exactness, but betraying the origin of his data,) the resistance would be something less than 2 ounces for every square inch of opposing surface, and consequently for any safe velocity that might be wanted in the case of the undulating railway, the resistance of the air is an element that may altogether be rejected."

I beg to inform Kinclaven, that the authority for my datum on this point is the very compendious formula given by Dr. T. Young, of dividing the square of the velocity in a second, expressed in feet, by 500, which gives in pounds the amount of resistance for each square foot of opposing surface, which, in the instance in question, will be 43lbs. Kinclaven makes it only 18 lbs. Now suppose the area of surface presented by the *first* carriage to be 30 square feet, (I would not contend that the same mode of estimating the resistance should be extended to every succeeding carriage of the train, other than as it may expose a larger area of surface,) we shall then have the enormous resistance that is equivalent to the force of 318 horses, at the usual standard of 33,000 lbs. one foot high in a minute for each horse. Even according to Kinclaven it is equivalent to 131 horses; but his estimate of the resistance is by far too little. The tables published by Smeaton make it 49 lbs. per square foot, and if he consults Dr. Hutton's tables, derived from experiments with the whirling table, he may compute therefrom, that the resistance is more than 40 lbs. per square foot, though the highest velocity which the Doctor gives is only 80 feet per second, and though he found that the resistance gradually increased with the velocity, in a greater ratio than the square of the velocity. Kinclaven will now perceive that the resistance of the air is *not* an element that may altogether be rejected; but I hope that he too will not "now feel both sick and sore with this undulating railway question."

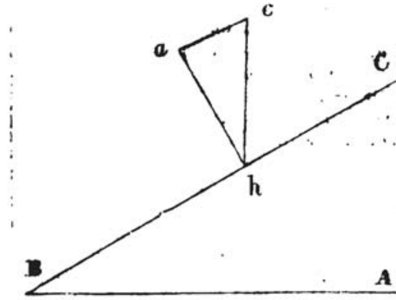
Kinclaven certainly uses singular language for a mathematician. He speaks of the *abstruse* science of mathematics," and the "*certain science* of logic." To me these appear like solecisms; for assuredly mathematics is less abstruse than it is certain, and logic less certain than it is abstruse; but, perhaps, he thinks with Leibnitz that Euclid is but a series of syllogisms from beginning to end. Neither do I understand how a greater attention to metaphysics is a lessening qualification to the study of logic, for the former comprehends, in fact, the transcendental form of the latter.

I am, Sir, yours, &c.

BENJAMIN CHEVERTON.

DEMONSTRATION OF THE TRUTH OF THE FORMULA GIVEN IN NO. 518 BY S. Y.

Sir,—I shall feel obliged by your affording a corner in your very useful Miscellany for a few words in reply to Mr. Badnall.



Draw a horizontal line A B, and a perpendicular A C, and suppose B C to represent an inclined plane, upon which any weight (W) is placed.

Draw *c h* perpendicular to A B, *a h* perpendicular to B C, and *a c* parallel to B C. The angle at *h* is equal to the angle at B, and the angle at *a* is equal to the angle at A; therefore the two triangles are similar and proportional, and A B *is to* B C, *as a h is to h c*; therefore, $\frac{A\,B}{B\,C} = \frac{a\,h.}{h\,c.}$

If *h c* represents the weight (W), then will *a h* represent the pressure (P) on the inclined plane, and P *is to* W *as a h is to h c*; or, *as h c is to* W *so is a h to* P, and $P = \frac{\times\, a\,h}{h\,c}$. When W = *one* the pressure against the inclined plane *is to* the weight as $\frac{a\,h}{h\,c}$ *is to* one, or as $\frac{A\,B}{B\,C}$ *is to one*; for $\frac{a\,h}{h\,c} = \frac{A\,B.}{B\,C.}$

If we put *b* for the base A B of an inclined plane, and L for the length B C of *any* inclined plane, *raised upon the base b*;—that is, drawn from the point

B to the perpendicular A C, produced, if necessary, we shall have L : b :: W : P, and $P = \frac{W\,b}{L}$, and when W=1, $P = \frac{b}{L}$; therefore "the pressure against the inclined plane *is to* the weight or pressure on the level *as* $\frac{b}{L}$ *is to* 1, which is the formula given by me in No. 518.

When B C makes an angle of 45° with the horizon, the pressure *is to* the weight as *one* is to *the square root of two*, and NOT as *one* is to *two*, as Mr. Badnall supposes.

I beg to say, I do not imagine myself capable of instructing Mr. Badnall, nor do I feel inclined to argue with him on this subject. I am merely stating facts which I believe are well known to most persons calling themselves engineers.

Yours, &c.

S. Y. an Engineer.

16, Lambeth-terrace.

THE LATE UNDULATING RAILWAY EXPERIMENTS—MR. BADNALL IN REPLY TO "PRACTICAL."

Sir,—The reasoning of your correspondent Practical is of so extraordinary a nature, that I almost feel a woman's curiosity to view the philosopher in *propriâ personâ;* but, through respect to the sentiments which you have advanced on the subject of anonymous correspondence, I will stifle my inclination, and satisfy myself by the endeavour to prove that Practical is not the man to convince the world of my error by his logic.

He inquires why I chose the Liver and Firefly locomotive engines instead of the Collier and the Sampson. My answer is, that I had no choice in the matter. In the first instance, I was well pleased to procure the loan of the Rocket, afterwards, much better satisfied by the loan of the Liver, and, lastly, delighted with the opportunity of trying a series of experiments with what are well known to be *two exceedingly good* engines, viz. the Pluto and Firefly. With regard to the last, she is, considering her weight, size of cylinders, and length of stroke, one of the best engines on the line, and especially noted for the loads she is capable of taking up the inclined planes. Supposing, however, that Mr. Dixon, the superintending engineer on the line, had substituted the Collier and Sampson for the two engines which he recommended as suitable for a fair trial of the experiments, can your readers, Sir, for one moment doubt that Practical would have thus exclaimed—"*I must beg to ask Mr. Badnall why he chose the Collier and Sampson for his experiments? Are not these engines* VERY STRONG *on the inclined plane compared with any others, such as the Firefly and Liver?—or was it because experiments with* WORSE *engines would not have been so satisfactory to the undulating system!*" Mr. Practical! Mr. Practical! *this is poor fighting!* But—

Practical states that I have omitted to mention the circumstance of the slipping of the wheels upon the rails, *while ascending*, which, he says, would have been obviated in a great measure had the Collier or Sampson been employed. Will this long-sighted man of practice do me the favour to explain *how the slipping of the wheels*, while ascending the inclined plane, could have produced a result more favourable to the undulating system than if the adhesion had been uniform throughout? Need I say, that the more the wheels slipped the less was the chance of attaining a high elevation, and, consequently, (when the action of the engine was reversed,) an increased velocity at the foot of descent? There can be no doubt that had more powerful and heavier engines been employed the elevation attained on the ascending plane would have been greater, and the velocity at the foot of descent consequently greater; but if I have established an advantage with two engines, which are conceded by Practical to be inferior ones, surely there are not many *practical men* who would doubt the result with more powerful and superior engines! The trial, therefore, of such an experiment as your correspondent proposes, would be, I humbly conceive, altogether a waste of time and money. Nevertheless, if S. Y. and Mr. Cheverton are willing to support his views, and throw the merits of the question on such additional test, I will address the Liverpool Directors upon the subject, and solicit the loan of the two engines alluded to, though, I confess, I entertain a doubt as to their compliance, on the ground of its being merely a useless repetition of previous experiments, and a very unnecessary expense.

In allusion to the remarks which Practical makes, as to the apparent anomalous result of the recent trials, I have only to offer a very simple explanation. It appears, that when a velocity of 19·04 miles per hour was attained at the foot of ascent, the engine and train ascended, *by momentum*, 323 yards in 70 seconds; and when a velocity of 14·34 miles per hour was attained, *the power of the engine being continued up the ascent*, the train only ascended 315 yards in 90 seconds. This difference, says Practical, proves that the wheels of the engine slipped! How so? Is there any thing very extraordinary in the fact that the momentum of a train, weighing upwards of 150 tons, and moving at a velocity of 19·04 miles per hour, should be so much greater than the momentum generated by a velocity of 14·34 miles per hour, as to be more than equal to the additional effective power of the Firefly, when dragging so vast a load up an ascent of 1 in 99? And what does the difference in the *time* prove? Simply, that the momentum generated at a velocity of 19·04 miles per hour, was sooner expended than the momentum generated at a velocity of 14·34 miles per hour, and supported by the continued action of the locomotive engine.

That the *effect* of momentum exceeded the *effect* of locomotive force, is evident from the following comparison:—

A velocity being generated of 19·04 miles per hour, the train rose *by momentum* to an elevation of 323 yards in 70 seconds; and

A velocity being generated of 20·28 miles per hour, the train rose (the power of the engine being continued) 575 yards in 116 seconds, viz. about 344 yards by momentum, and 231 yards by the power of the engine.

To argue that the wheels never slipped would be ridiculous, for when the velocity began to subside the slipping of the wheels was necessarily the cause of the train stopping; and such would be the case with the Sampson, the Collier, or any other engines ascending an inclined plane, and loaded beyond their power.

As to Practical's great doubts, that a heavy train could not be conveyed from one summit of an undulation to another "with twice the power" I contemplate, I beg to refer him to the *experiment last tried* with the Pluto and Firefly engines. He will find *my proofs* there—where shall I look for his?

Again, as to the opinion which I expressed, that *one engine would convey, from summit to summit of an undulation, the maximum* loads of three locomotive engines on a level, say 300 tons, at an average velocity of 15 miles per hour—I beg to repeat *that I am still of the same opinion.*—Indeed, I feel confident that such would be the case, the undulations being laid down in accordance with a section prepared by Mr. R. Stephenson, sen., and myself, and the greatest dip not exceeding 15 to 22½ feet in from 1,000 to 1,500 yards.

Sunday being the only day on which this experiment could be tried with safety or convenience on the Liverpool line, and the unwillingness of some of the Directors to sanction a further trial on that day, has hitherto unfortunately prevented me reducing this opinion to a certainty.

One other remark, and I leave Practical to draw a fair comparison between the extent of his own delusion and that of the body of engineers who witnessed the recent experiments. He observes,— "But allowing him sufficient power from momentum to arrive at the summit of each undulation, a velocity of 30 miles per hour at the bottom of the undulation will but give an average velocity of 15 miles per hour for the whole distance of 32 miles." Very true, Mr. Practical, provided the train came to a stand-still at the top of each summit; but how would it be, if, as shewn by experiments 1 and 2 (page 71), the carriages were passing *over the second summit* at a velocity of 10 *miles per hour*? It will be seen by those experiments that a velocity of 20·28 miles per hour enabled the train to ascend 575 yards; a velocity therefore of 31·70 miles per hour could not have been expended on arriving at the same elevation. Practical should have argued differently on this subject. He should have said—"The Firefly engine proved her power of moving 150 tons *from a state of rest* to a distance of 1,250 yards, at an average velocity of more than 15 miles per hour, by the adoption of undulations, whereas she could not have possibly moved that load upon a level line; and, with the Pluto to assist her, did not con-very the same load, after traversing a mile on the level, at a greater average velocity than 10 miles per hour, the steam blowing off throughout the whole distance?" *These are* facts—will Practical expose my delusion and disprove them? I am, Sir, yours, &c.

RICH. BADNALL.

Farm Hill, near Douglas, Jan. 3, 1834.

P. S.—I have been trying, during the last month, a series of interesting experiments at Manchester, with a view of proving the superiority of the undulating railway, or otherwise, by employing a *falling weight* instead of locomotive force. The results were quite as satisfactory to me, and as clear to others who witnessed the experiments, as all previous ones. And I can assure Practical that there were a few heads present not often susceptible of delusion on such subjects.

ATTENDANCE AT FIRES.

Sir,—As your correspondent, Pit, was a spectator of the fire at Mr. Horner's farms, Coldharbour-lane, Camberwell, on the 15th December last, at ten P. M., I cannot believe he intends to complain of delay in the arrival of the proper assistance. The heading of his communication at page 240, "*delay at fires*," however, and his remark, that "much of the property that was destroyed might have been saved *if the engines had arrived earlier*," will, I have no doubt, convey an impression to your readers in general, very wide of the truth. I must beg leave, therefore, to make the following statement of facts, which will put this particular affair in its proper light, and shew the praiseworthy alacrity with which the fire-engines are set in motion, whenever their services are required either near or far. It is very well known that, under these exciting circumstances, a few minutes appear like a much longer period, and allowance should be made for this feeling, and the true time accurately ascertained, before any remarks are put forth at all likely to convey censure.

On the night in question, the fire burnt so rapidly that it was distinctly visible at a great distance. On perceiving the light, the men of the London fire-engine establishment started off from several stations simultaneously, and the arrival of engines at the fire took place in the following order:—

1. Camberwell parish engine.
2. First engine of the Establishment (from Waterloo-road).
3. Second ditto ditto (from Southwark-bridge-road).
4. Third ditto ditto (with Mr. Braidwood, the superintendant, from Watling-street.)
5. Fourth ditto ditto
6. West of England Insurance Company's engine.
7. Fifth engine of the Establishment.
8. Hand-in-Hand Insurance Company's engine.
9. British ditto ditto
10. Kent ditto ditto (from Broadway, Deptford.)

The three foremost engines of the establishment reached the spot within a few minutes of each other, and *before the parish engine had been got to work*. These engines were met on the road by a mounted policeman, who was coming to call them to the fire; they were the first engines that worked upon the fire, which they soon stopped.

The establishment had five engines and forty-six men in attendance at this fire, in little more than an hour after it commenced.

Upon this occasion the firemen were called to the fire by the atmospheric reflection, as soon as they could possibly have been summoned by rockets, and with much more certainty as to its situation.

In conclusion, I beg to assure Pit, that I have paid considerable attention to the employment of rockets, coloured lights, &c., as fire-alarms; and after the most minute investigation of the subject, I am compelled to say, that I believe no system of *signals* can be advantageously employed in the metropolitan fire establishment.

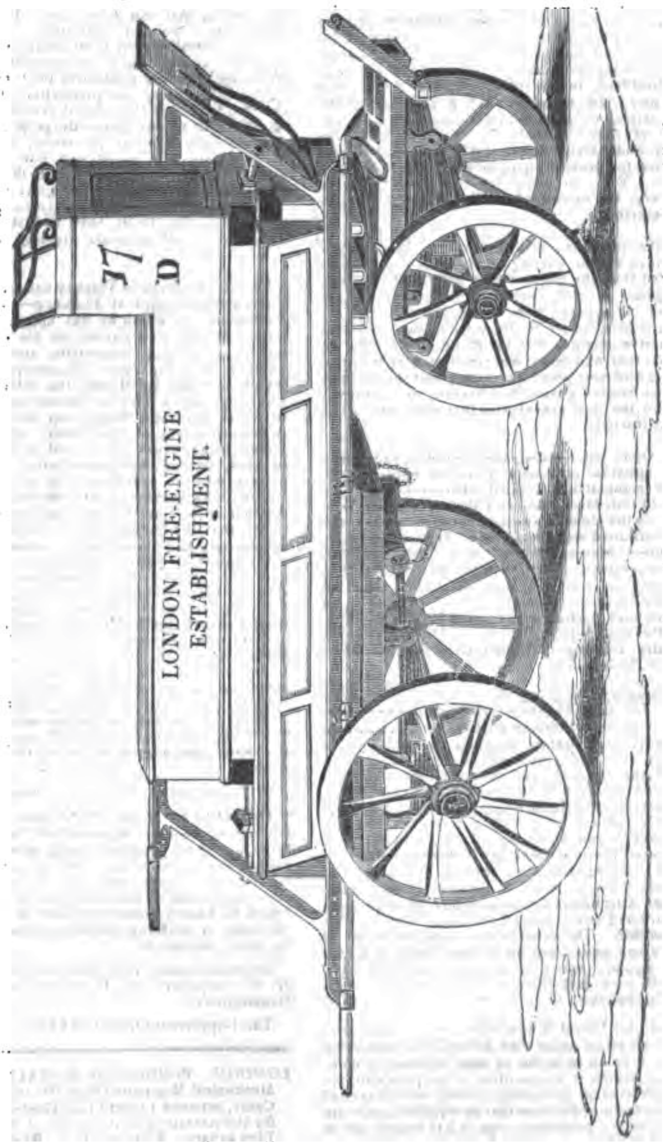
I am, Sir,
Yours respectfully.
WILLIAM BADDELEY.

London, Jan. 6, 1834.

Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 546. SATURDAY, JANUARY 25, 1834. Price 3d.



LONDON FIRE ENGINE IN 1834.

Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 547. SATURDAY, FEBRUARY 1, 1834. Price 3d.

BRAMAH'S IMPROVED TREAD WHEEL.

Fig. 1. Fig. 3.

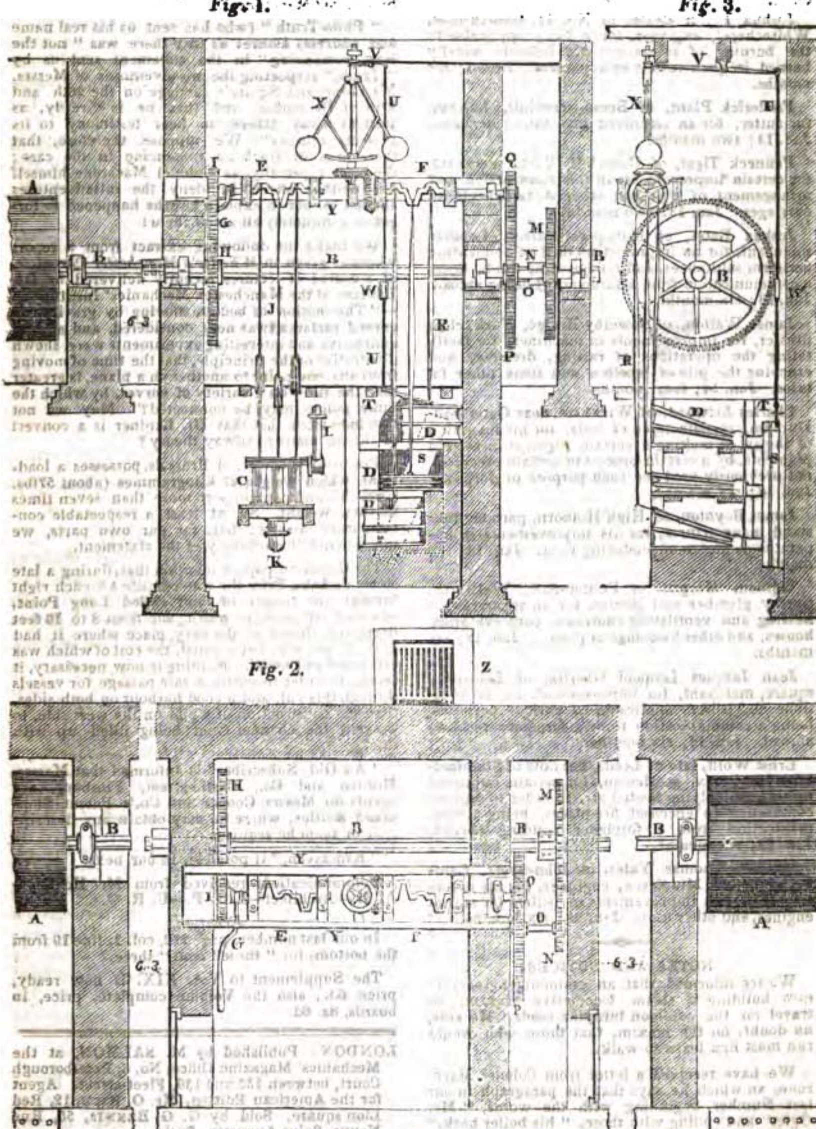


Fig. 2.

such resisting column, as is seen very frequently in the use of the diving bell. The valve *e* is then opened at the top of the conical tube, and "the compressed air rushes out." This I can understand, but not that the water should spout out from the tube. I should rather imagine, that the water contained in the cistern *A* would rise in the conical tube to *its own level*, and no further. If it has been put to the test of experience, I, of course, am wrong. Perhaps I may be so in any case, but shall be most happy to be informed of my error.

I remain, Sir,
Your obedient servant,
N. C.

THE UNDULATING RAILWAY — REMARKS BY "KINCLAVEN" ON MR. CHEVERTON'S REPLY.

Sir,—Notwithstanding the many able articles we have had from Mr. Cheverton on metaphysics, natural philosophy, and general science, I have all along been of opinion that if he ever allowed himself to come into close combat, it would then be found that he was not *invincible*. He has hitherto, however, played the part of a cautious general so well—leaving the fighting part to his aide-de-camp S. Y., his second in command of the corps of engineers—albeit, he was styled but the other day the "*young* engineer," *par excellence*—that there has been no opportunity of putting his prowess to the test. Getting at length tired and "sick" of this defensive mode of warfare, he has been tempted to grasp his sword—I beg his pardon, his *pen*—to convince the readers and correspondents of the Mech. Mag. that he can plunge as far into the sea of controversy as any of us, and "buffet it with as lusty sinews" as the best.

Without further preface, however, Mr. Editor, I shall proceed to make a few remarks on his last singular production, No. 548.

Mr. Cheverton proposes and solves the following question:—Suppose an engine is propelled with a velocity of 100 miles per hour, and that the opposing surface is 30 square feet—how many hundred horses' power will be requisite to overcome the resistance of the atmosphere alone?

Mr. Cheverton, in giving the steps of his solution (for he has not given it in full), says that, according to a formula of Dr. T. Young, if we divide the square of the velocity in a second, expressed in feet by 500, the result will be the resistance in lbs. for each square part of opposing surface. Thus, in the present question, 100 miles per hour produces a velocity of 146⅔ feet per second, and $(146\frac{2}{3})^2 \div 500 = 43$ lbs. the resistance on a square foot. Well, that is all we have from Dr. Young's formula, which, for the present, we shall take for granted (but more of this afterwards). The remaining part of the solution Mr. Cheverton derives from his *own* resources. He states, that on 30 square feet of opposing surface we shall have the enormous resistance that is equivalent to the force of 313 horses, at the usual standard of 33,000 lbs., raised one foot high in a minute for each horse. How he arrived at this result Mr. Cheverton does not explain; but we think it will be no difficult matter to show that, even according to his own data, it is erroneous. For, a velocity of 100 miles per hour produces a velocity of 8,800 feet per minute, and $43 \times 30 = 1290$ lbs. the resistance on 30 square feet. Hence $1290 \times 8800 \div 33000 = 344$ horse power; whereas Mr. Cheverton states the result at *only* 313! A mere slip of the pen, perhaps! But what if both data and calculation are wrong? Let us see.

When Mr. Cheverton calculated this enormous resistance, it seems passing strange that he had not the curiosity to try what would be the corresponding effect upon some velocity that has actually been performed. Thus, take the case of a carriage, with two horses, a driver, and two or three outside riders—we may estimate the whole at 30 square feet of opposing surface. Now, suppose the velocity to be 20 miles per hour (which is less than has sometimes been performed); then, by Mr. Cheverton's *rule*, the resistance arising from the air alone would amount to nearly the force of *three* horses, although, in point of fact, *two* horses can overcome it—aye, and overcome *inertia*, (a favourite word of Mr. Cheverton's, which he contrives to slip in on every occasion,) friction, and the drawback of a heavy load to boot

Let us now generalise Mr. Cheverton's wonderful rule.

Assume 32000=h, 500=e, a^2 and b^2 to be any two opposing surfaces expressed in square feet, V and v the corresponding velocities in feet per second; then 60 V and 60 v will be the velocities per minute. Now, according to Mr. Cheverton's rule, $\frac{a^2V^2\times 60V}{he}$ and $\frac{b^2v^2\times 60v}{he}$ will express the two resistances in horses' power, and rejecting the constant quantities h, e, and 60, the ratio of the resistances will be as $a^2 V^3$ to $b^2 v^3$, and when $a^2 = b^2$, the ultimate ratio of the resistances will be as the cubes of the velocities! Exquisite fudge this is, and yet all in perfect conformity with *the Chevertonian theory of the law of atmospheric resistances.*

After the above, Mr. Badnall need not be much surprised at the stern opposition which Mr. Cheverton has uniformly made to the principles of the undulating railway. Indeed, if I had been myself a believer in such monstrous absurdities, I too must of necessity have been of the "opposite faction."

I have still a good deal more to say on the subject, Mr. Editor, but at present time presses hard upon me, and I must postpone the remainder of my remarks for a future letter.

I am, Sir, yours, &c.
KINCLAVEN.

THE UNDULATING RAILWAY—PROPOSITION FOR THE SETTLEMENT OF THE CONTROVERSY.

Sir,—Upon noticing the turn which the undulating railway question has taken in your work, I had determined to remain a quiet spectator of the proceedings, notwithstanding that I had stated I would mathematically demonstrate the conclusions of Mr. Badnall to be erroneous.

I am now induced to address you for the purpose of requesting Mr. Cheverton to repeat the *experiments* I made at the Adelaide Rooms, and related in your pages at an early stage of the controversy, and at the same time to keep in view the *observations* of Mr. Badnall in reply to them. By this proceeding he will bring back the disputation to something like a practical view; and I am much surprised that these experiments have not previously had the effect that was intended—namely, that of meeting Mr. Badnall on his own ground, and with his own weapons. I must beg to add, that the experiments at Liverpool have not as yet opened my dull eyes to the advantage of the theory; and, to save Mr. Badnall the trouble of replying to my present observations, I will conclude by stating that I concur in almost all Mr. Cheverton's views, except that I consider him to have over-estimated considerably the resistance of the air—that is, the *initial* resistance, but not the *ratio* of its increase (as square, or nearly so, of velocity), which latter theory, in my humble opinion, Mr. Badnall has much more to do than he has yet done to controvert.

Trusting you will find an early place for these remarks in your periodical, as coming from one who took an early part in the discussion. I am, Sir,

Your most obedient servant,
T—— H——D.

London, Jan. 13, 1834.

NEITHER LIGHTING NOR HEATING BY GAS OF MODERN ORIGIN.*

In several situations removed from any volcanic action, so far as is visible on the surface, natural jets of inflammable gases are seen to issue, affording decisive evidence of chemical changes that are taking place at various depths beneath. Of these, some have served the purposes of the priest to delude mankind, whilst part of the others have been more usefully employed.

Carburetted hydrogen gas is well known to be the "fire-damp" of the coal districts, and to issue from the coal strata; collecting in the ill-ventilated galleries of collieries, and, when sufficiently mixed with atmospheric air, exploding with great violence when approached incautiously with an unprotected flame, spreading mourning and misery among the families of the miners. If the genius of Davy had merely produced his safety

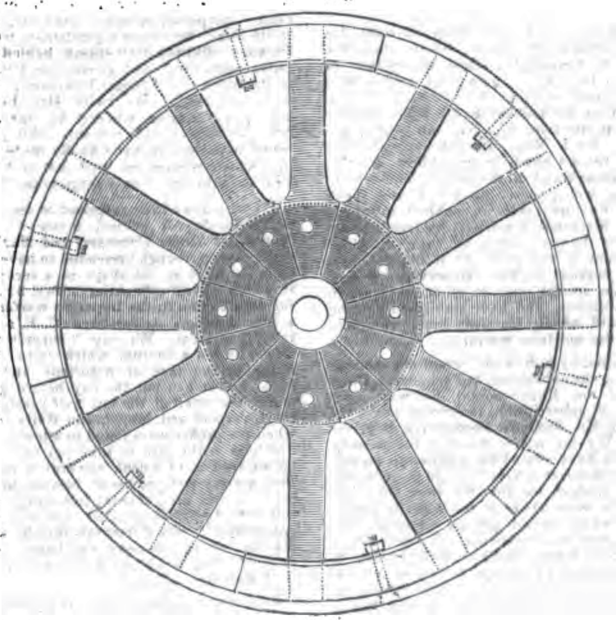
* From Mr. De la Beche's *Geological Manual* (third edition, considerably enlarged, 18[illegible]), one of the most instructive and entertaining works which the new and important science of geology has yet produced.—Ed. M. M.

Mechanics' Magazine,
MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 548. SATURDAY, FEBRUARY 8, 1834. Price 3d.

HANCOCK'S WEDGE WHEELS.

Fig. 1.



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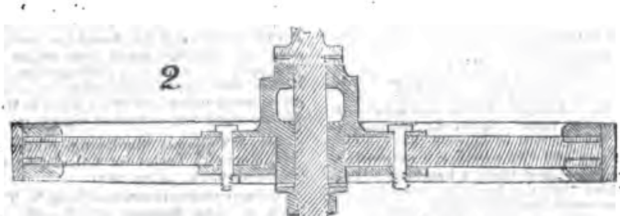


3

have two bolts to screw up the packing, it will give more room for putting in the stuffing, if the stuffing box at *k k*, and the one at the bottom of the cylinder, are set at right angles to each other. A cup for greasing the top stuffing box is shown at *l*. Some oil or grease may also be inserted at the top of the part *h h h* for the same purpose: *m* is the bottom part of the cylinder, and *n* is the discharging part of the air pump *o*. One advantage in this sort of stuffing box is, that there is only one half of the friction that there would have been if stuffing boxes of the ordinary construction had been used. Another advantage is, that it is more easily packed.

Either of the arrangements described in No. 504 may be used for setting on, stopping, or reversing this engine. If the wiper shaft be placed nearly on a level with the bottom of the cylinder, the eccentric rod will be about vertical, and the starting bar will be nearly level in every position of the valve, in the arrangement seen in figs. 3 and 4. Mr. Brendel's arrangement for the same purpose, described in No. 533, will not work well, as in it there is no way of setting on the steam before the piston reacts on the ends of the cylinder, to let the inertiæ of the parts of the engine be overcome by the elasticity of the steam. If Mr. Brendel had produced the wiper-shaft lever *c*' B to an equal distance beyond the centre of the wiper shaft *c*', and put another pin for the eccentric gab to hold into on the end produced, this would have answered the end as well, and been much simpler than so many rods and levers. By means of a pair of compasses, and a piece of board, he might have determined the lengths of his levers without the aid of *formula*. There are a good many engines with my apparatus attached to them, working in the neigbourhood of Glasgow, which give great satisfaction.

If an engine like the one for steam-boats, formerly described, had the crank shaft under the cylinder, one piston rod would do, and two rods might be taken out at the bottom of the cylinder to work the air pump. A vacuum, or any other kind of pump, might be wrought in this way, and the steam-engine would be of a very simple construction.

In my last communication I forgot to mention, that the part *g* should be prevented from making a complete revolution, by having it wrought by a friction plate with a projecting part cast on it, to catch on a tooth fixed to the wiper *f*, which opens the valve for setting on and shutting off the steam. By giving this letter a place in your valuable Miscellany, you will very much oblige,

Yours respectfully,

JAMES WHITELAW.

Glasgow, Jan. 4, 1834.

ON THE RESISTANCE OF THE AIR, AS AFFECTING THE UNDULATING RAILWAY QUESTION—REPLY BY MR. CHEVERTON TO KINCLAVEN.

Sir,—I turn with pleasure from Mr. Badnall to my more scientific opponent Kinclaven; and yet, methinks, I ought to tremble at an encounter with such a man of science, and a mathematician to the backbone, for "he has all along been of opinion, that if ever I allowed myself to come into close combat, it would then be found that I was not *invincible*." Perhaps not; for like Antæus, who, when lifted up in the air, found all his struggles powerless, so on this subject of the air's resistance, and in the brawny arms of this second Hercules, it may be that my strength is shorn, and that now my fate is come—*or coming*. Having, however, come to a close grapple with my respectable opponent, it is not my intention easily to relinquish my hold, especially as he exhibits—in one point in particular—decided symptoms of weakness, manifesting that he, at any rate, is not invulnerable. Twice has the fallacy been pointed out to him of establishing his position (of the shorter time on a curve), by assuming what he should first prove, and what we shall never admit; and twice has he thought it more discreet to refrain from taking any notice, either in the way of justification, explanation, or admission, of this paralogism in argument, this *petitio principii* of the logicians. Really, I am afraid that this term will become as great a favourite with me, as it is charged against me the word *inertia* is, and which it seems I "continue to slip in on every occasion;" but somehow or other it so happens, which my opponents can best explain, the occasions are so fitting, and the words so apt, that instead of being slipped in, they slip in of themselves; and as to their being so *apropos*, it is not at least *my* fault—in short, they are the key words which unlock the mystifications in which the subject has been enveloped.

The point now in dispute is, as to the power required to overcome the resistance of the air at the velocity of 100 miles per hour, and was raised by my asking the question—how many hundred horse power an engine (locomotive) must be, in order to produce this velocity? Kinclaven made "the resistance something less than two ounces for every square inch of opposing surface," or 18lbs. per square foot. It was stated by me, on the authority of Dr. Young's rule, at 43lbs. the foot, that Smeaton's table makes it 49lbs., and that it may be computed from Dr. Hutton's tables at about 40lbs. per foot—to which I may add, that the writer of the article in the *Scotsman*, which appeared a few years since on railroads, takes for his datum 48lbs. To these authorities Kinclaven makes no reply, nor does he justify his own estimate; and yet he permits himself to say that I am *wrong* in my *data*.

But I am wrong in my calculation too: for having taken the opposing surface at 30 square feet, and the horse power at 33,000lbs., one foot high in a minute, I made the total power required to cope with the resistance to be 311 horses; and here I confess to a slip of the pen, for it is *too little*, it being 344 horses. But even this corrected result is wrong: in fact, some grand error (which it would but be kind and charitable in Kinclaven to point out) pervades the whole affair; for if the thing were true, "a carriage, with a driver, and two or three outside riders," which may be supposed to present a surface of thirty square feet, would require three horses to draw it at the rate of twenty miles an hour, in opposition to the resistance of the air alone. But it has been performed, says Kinclaven, with *two* horses—*ergo*, &c., Q.E.D. Barring the breakneck nature of the experiment, I confess I should like to see this feat performed; but taking the premises for granted—as usual—is there any thing astonishing that spirited horses should, in a panic, or by violent incitement, be able to put forth for a *short time*, three or four times, it may be, the *energy** which they exert in the ordinary routine of labour, and on which, as capable of being maintained the whole of the horse's day, the usual estimate of the horse's power is alone founded. This is the grand mistake which, until very lately, the steam-coach speculators made—particularly Gurney. They thought as stage coaches had only our horses to them, that if they embarked six or seven, or at most eight horses power in their machines, they should have enough and to spare, not heeding the importance of the fact, that those hhrses frequently do their day's work in an hour.

There are some practical considerations, in regard to a carriage moving with great velocity, which would induce one to believe that the theoretical rule for the resistance of the air, of its increasing with the squares of the velocities, may be modified both ways; that is, that some circumstances may tend to increase, and others to diminish, the rate of resistance. There are other circumstances which may tend to increase and diminish the absolute amount of resistance on a square foot, derived from the consideration of the hydrostatic pressure, and the height due to the velocity; and, on the whole, I am inclined to believe, with your correspondent T. H—d, that, in practice on a large scale the influence of the diminishing circumstances may be found to preponderate in favour of a less resistance than I have assigned; but experiments only can determine such points. Ignorant of the value which ought to be attached to Smeaton's table, and inclined to depend more on Dr. Hutton's experiments, I can yet very easily imagine, how the whirling table may give results different from and exceeding what may be obtained from rectilinear motion. As to 30 square feet of opposing surface, it is certainly a large estimate for a *single* carriage, though Kinclaven scruples not to adopt it, and would undoubtedly be too great, if a proper form was given to its exterior. However, after all abatements, can any one doubt of my being justified in putting the original question—how many hundred horses power, &c.?

Kinclaven is determined to doubt it, for not only my data, but the rule by

* I purposely use this word, to avoid any quibble as to the rapid diminution of a horse's power of draft with the increase of his velocity.

which I calculate the amount of the requisite power, is wrong—it is, in fact, "a wonderful rule," "a monstrous absurdity," and why?—because it furnishes results which are in the ratio of the cubes of the velocities!! But the reader must have before him his own account of the matter, and the argument *ad absurdum* with which he would overwhelm me.

"Assume $33000 = h$, $500 = e$, a^2 and b^2 to be any two opposing surfaces, expressed in square feet, V and v the corresponding velocities, in feet, per second; then 60 V and 60^2 will be the velocities per minute. Now, according to Mr. Cheverton's rule $\frac{a^2 V^2 \times 60 V}{h e}$ and $\frac{b^2 v^2 \times 60 v}{h e}$ will express the two resistances in horses' power, and rejecting the constant quantities h, e, and 60, the ratio of the resistances will be, as $a^2 V^3$ to $b^2 v^3$; and when $a^2 = b^2$, the ultimate ratio of the resistances will be as the cubes of the velocities! Exquisite fudge this is, and yet all in perfect conformity with the Chevertonian theory of the law of atmospheric resistances."

This array of Mr. Badnall's "symbols and figures" will greatly delight him, for he has a vast reverence for such things. Let us, however, see what they are worth. I admit the correctness with which my rule is expressed, as above, and that the ultimate ratio is truly as the cubes of the velocities; but the ratio of what—of the *resistances*, says Kinclaven. Not quite so fast, my friend;—it has not been said by me—this "fudge," so "exquisite," is none of *mine*. What I undertook, was, to give (in regard to one particular resistance) *the ratio of the horses' power*, or of the expenditure of power in a *given time*, the distances varying, as it is, in this way, the measure of the engines' capabilities are expressed; which consideration, includes the *velocity* of the impelling force; and, therefore, $a^2 V^2$ is multiplied into 60 V. Exclude the velocity, and the measuring units h and h, and the remainder will correctly enough express the *resistances* in the ratio of the *squares* of the velocities, which is the well-established and *only* law recognised by me, for, as to the "Chevertonian law," it is all "fudge." Has, then, my worthy opponent really confounded the *intensity* of force requisite to balance a resistance, with its *momentum* as an impelling force? And has he to be informed, that uniform motion takes place because the *momenta* of the impelling and resisting forces are equal, and that, having provided a propelling equal to a resisting force at any particular velocity, it is necessary to give *it* the same velocity, and that it is upon this consideration, that power is furnished to the engine, and by which its consumption is determined, as any engineer, though not "as he ought to be, a mathematician," could have told him. Now, the resistance in question, being as the square of the velocity, and the force to cope with it having the same velocity, it obviously follows, without the necessity of this algebraical language to prove it, that the ratio of the powers of engines at different velocities must be (*as to this particular resistance of the air*) as the cubes of the velocities.

Probably, Kinclaven imagines, though I have been very guarded on that point, that in giving *this rule* for *this purpose*, I consider the proper measure of the mechanical effect, in this speed-required-case, is the proportionality of the force to the *cube* of the velocity, as it would be, if stated in terms of the distance, and not of the *square* of the velocity, as it will be, if stated as it ought to be, in terms of the time, that is, that I consider the expenditure of power in a given *distance* to be in the ratio of the cubes of the velocities; but I have always expressed myself to the very contrary. Time, as is well known, is an element, which above all things must be included in the measure of the effect in the case of locomotion; and, therefore, if not given, the product must be divided by it. Now, if the distance is given, the time is as the velocity, inversely; and, therefore, we can divide by the latter. Taking then Kinclaven's own expressions, for the nonce, as above, and omitting h as useful only to bring the estimate to the measure of a larger unit, we shall have $\frac{a^2 V^2 \times 60 V}{e \times 60 V}$ and, $\frac{b^2 v^2 \times 60 v}{e \times 60 v}$, which, by similar rejecting, will present the ultimate ratio as the *squares* of the velocities, which is that of the cost or expenditure of power (*as to the particular in question*) for any *given distance*, and such as would arise, from the force being more expressly stated in terms of the time, which Kinclaven himself can supply. It should be observed that in his own expressions, the time being unity respectively, 60 V and 60 v, represent the respective *different* distances as well as the velocities, and that the force is therefore multiplied into the *space* over which it travels.

In plainer language to you MESSIEURS STOKERS OF THE ENGINES, if your locomotive has gone 10 miles in an hour, and you now make her go with twice the velocity, you will admit that she will travel twenty miles in the next hour, and that notwithstanding you have been about the job in only the same time to a minute, you have got rid of double the quantity of steam, for you have gone a double distance, or made double the number of strokes; and some among you may comprehend, peradventure, that you have doubled the power of your engine by so doing, and perhaps it is not *quite* beyond your capacities to understand, that if, in addition to this double-quick time, you hitch on what will make four times the load you had before, you will now make your engine do *eight times* the work she did before, and that she may now be considered as of eighty-horse-power, if she was ten-horse power before. Again, MESSRS. STOKERS, OF THE ENGINES, you will not stumble much at my meaning, when I tell you, that in going the twenty miles in the hour, you have emptied your cylinder just the same number of times, as you would if you had gone the same distance at your old rate of ten miles an hour; that is, that you have made no more than just the same number of strokes; but, as you have some sort of suspicion that your steam must be four times stronger than before, by reason of the load being four times greater than before, or else, that you must have a cylinder to contain four times more steam than before, you will not be greatly bothered when I tell you, that you have spent but *four times* the steam you would otherwise have done. I will not make you shake your heads by telling you that the conclusions to which I have led you about *eight times* and *four times*, are to the increased velocity, in the ratios of the square and the cube, for that is all nonsense; but, if you can make your imaginations soar to the pitch of supposing, that the additional resistance may arise not from an extra load, but from the obstruction of the air being four times what it was before, and that, *so far as this goes*, you must equally provide steam accordingly, you will then know all about the matter, quite as well as myself, or as Kinclaven(?)

I remain, Sir, yours, &c.,

BENJAMIN CHEVERTON.

NOTES WORTH NOTICE.

"Small things, properly applied, may lead to great results. What was the guide of Columbus through unknown seas to a new and magnificent world?—*The tiny needle.*"

The British Museum Duplicates.—The destination of those volumes in King George the Third's Library, of which duplicates exist in the British Museum, is not yet determined on; but it has been proposed either to sell them by auction, or to dispose of them in a less paltry manner, by presenting them to some public institution—the latter seems certainly a mode more worthy of the nation. If the City Library at Guildhall were thrown open to the general body of the citizens (especially if the Gresham Lectures were attached to the same foundation), where could a more appropriate repository be found for these superfluous literary treasures?

March of Cheapness.—Penny literature is making the tour of the globe. A new periodical in the French language, "at the small charge of one penny," has been started at Montreal, under the title of *L'Abeille Canadienne* (The Canadian Bee); and the latest literary novelty of the Indian seas is *The Penny Gazette* of Penang! On the Continent *The Penny Magazine* is triumphant; it formed the chief article at the last Leipzig fair, and is said to have already attained a sale in Germany of great extent, with every prospect of reaching 60,000 before the Easter fair. It threatens to drive all literature of a more aspiring character out of the market altogether.

A Distant Contemporary.—It is an interesting task to watch the progress of periodical literature in all countries, more especially in our own colonies. New South Wales, it is well known, already abounds in newspapers; but a production of a more *sterling* (perhaps they would say *current*) character has lately issued from the Australian press, in the shape of *The New South Wales Magazine*, a monthly work, devoted, as its prospectus states, particularly to "colonial literature,"—"poetry,"—and the editor boasts of having secured the assistance of the best poets in the colony (!)—"the new fashions," &c. &c. &c. One principal object of the publication is, to *encourage emigration*—an aim which most would think could hardly be answered by a work published in the country *to* which emigrants are invited, not *from* which they are expected.

Brother Jonathan at Fault.—The Americans either cannot or will not make their steam-boilers so secure as the English; a fact proved by the accounts in the United States' papers of twelve fatal steam-boat accidents in the short space of six weeks, and the recommendation by the President of a new law upon the subject. This must surely be a puzzling fact for those sneerers at "over-legislation," who maintain that the public are quite able and quite willing to take sufficient care of themselves in such matters, without the *officious interference* of parliaments.

Steam Communication with India.—There is at length a very fair prospect that this desirable object will soon be accomplished. The plan has been warmly taken up at all the three presidencies, and no less than two lacs of rupees have been already subscribed—accompanied by no lack of good wishes. Among the names of the subscribers are those of the Rajah of Tanjore, the Ranee of

Mechanics' Magazine,
MUSEUM, REGISTER, JOURNAL, AND GAZETTE.
No. 549. SATURDAY, FEBRUARY 15, 1834. Price 3d.

DERENZY'S HAND FOR THE ONE-HANDED.

Fig. 1.

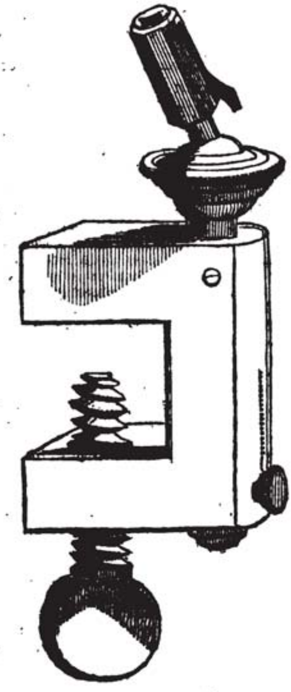


Fig. 2.

Fig. 3.

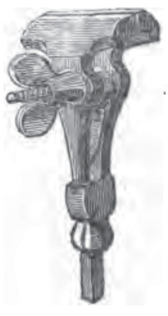
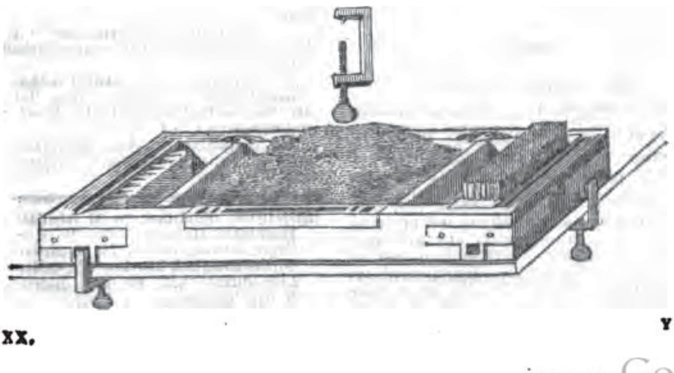


Fig. 4.



of his one-handed knife and fork (fig. 7) —two articles as essential, perhaps, as any to the comfortable enjoyment of life:—

"We will now turn our attention to the dinner-table, a scene of action at which, I believe, most of my readers will agree with me, it is particularly desirable that every one should be able to shift for himself; and here I flatter myself that I shall be enabled to render an essential service to the one-handed, by drawing their attention to the most perfect KNIFE and FORK which has ever been offered to the notice of persons so circumstanced. It will be seen by the engraving that the knife and fork form but one instrument; the knife is curved in the form of a cheese-cutter, and terminates in four prongs, which act as a fork; it cuts by pressure, and as quickly as any other knife can accomplish, or as the most active *gourmand* can desire.

"Three blades form a set. They are curved according to the hand for which they may be required; and the fork of one of them is plated so as to answer the purposes of a silver one. They are all made to fit the same handle, within which is a spring to hold them tight; and they can be changed with the utmost facility, by slightly pressing on the table the end of the handle, where the spring projects a little, holding at the same time the blade which is to be removed between the fore-finger and thumb, in order to prevent its falling out upon the table-cloth.

"For portability, the handle and blades have a small red morocco case, which opens by the slight pressure of a spring; and thus a person who has only the use of one hand, may sit down in any company with as much ease and independence as the rest of the guests,

'No fear least dinner cool,'

who would otherwise be obliged to tax the politeness of his next neighbour for assistance."—pp. 42-44.

We do not ourselves perceive much room for improvement in any of Captain Derenzy's inventions; but of this we are sure, that we could not give them a better chance of obtaining every improvement of which they are susceptible, than by thus bringing them under the notice of our many clever mechanical readers. We should rather expect to see some valuable additions made to Captain Derenzy's list of articles, than a material improvement in any of them. The peculiar wants of the one-handed among the manufacturing and trading classes must doubtless have given rise to contrivances different from any here recorded; and it seems reasonable to hope that the excellent example which Captain Derenzy has set of publishing the results of his personal experience, for the benefit of all similarly circumstanced, will not be left without numerous imitators.

THE LIVERPOOL AND MANCHESTER RAILWAY.

We mentioned briefly in a recent Number the highly satisfactory pecuniary results of this important concern, for the half-year ending the 31st of December last—a dividend at the rate of 9 per cent. having been declared (only 1 per cent. less than the maximum allowed by the Act of Parliament incorporating the Company of Proprietors), and 4,088*l*. 8*s*. 10*d*. having been appropriated as a reserve fund (being at the rate of 10 per cent. on the total net profits—which is the *utmost* allowed to be so reserved by the Act of Incorporation). We have since then been favoured with a copy of the Report made by the Directors on the occasion, and find in it so much matter of fact that is of universal interest on the subject of railways and locomotive power, that we need offer no apology for transferring it (with but little abridgement) to our pages. Mr. Grahame, and the other partisans of canal navigation, who still persist with so much honesty and candour, in representing that the profits of this railway arise mainly from the conveyance of passengers, and that it cannot possibly compete with canals in the conveyance of goods, will observe in this Report some rather stubborn facts on both these heads. The common-road steam-carriage charlatans too, who tell us that the expense of working a steam-carriage on a granite highway will be not more than *sixpence per mile*, and the tear and wear *next to nothing* (for "1,700 miles" at least), may learn from the circumstantial details here given of the actual expense of working such carriages on a railway, where the friction is many times less than on the best granite road that can be constructed, how much occasion they have to blush for the delusive representations they have sent forth to the public. We do not of course include in this class of public deceivers any of those honest and intelligent individuals—the Heatons, Hancocks, and Saxulas of the day—who frankly subscribing to the undeniable fact, that there is more friction to be overcome on a common road than on a railway, have proposed to themselves to determine by experiment whether it would not be cheaper to work steam-carriages against that greater friction, than to be at the expense of laying down railways to avoid it—in some cases at least, if not in all. These last are adventurers of a very different stamp; they speculate on a particular result, which, though as yet unascertained, is neither impossible nor improbable; and as long as they pursue the reasonable object they have in view by fair and honourable means, they shall command as they deserve our best encouragement and support.

LIVERPOOL AND MANCHESTER RAILWAY—FOURTH HALF-YEARLY MEETING.

REPORT.

Liverpool, January 23, 1834.

The Directors, in submitting to the Proprietors a statement of their accounts and proceedings for the half-year ending 31st December, 1833, have to report a considerable increase in the general business of the concern, as compared with the corresponding Six Months of the previous year.

The total quantity of Merchandise conveyed in the Six Months between Liverpool and Manchester was		69,800 tons
To and from different parts of the Line, including Warrington and Wigan		9,783
Between Liverpool and Manchester and Bolton		18,700
Total quantity conveyed		9,8247
Quantity of Coal from various parts to Liverpool		32,304
Ditto to Manchester		7,830
Total to Liverpool and Manchester		40,134
The number of Passengers booked at the Company's Offices	215,071	
The number of Trips of 30 miles performed by the Locomotive Engines with passengers was	3,253	
With Merchandise	2,587	
Total	5,840	
Compared with the corresponding six months of the last year, the increase in Merchandise conveyed has been		11,468 tons

In passengers 32,248.

The present winter has been in an extraordinary degree stormy and wet, which has no doubt diminished the amount of travelling.

The wetness of the season also has prevented the railway from being maintained in that complete order which is desirable; while the boisterous weather, with the dirty state of the Rails, has impeded the passage of the Trains, not unfrequently rendering assistant engines necessary to ensure their progress, even on the level parts of the way. These circumstances have unavoidably increased the charge for locomotive power. On the other hand, the navigation of the river, owing to the long continuance of tempestuous weather, being frequently dangerous, and sometimes impracticable, the utility and importance of the Railway conveyance have become more manifest and striking, and the natural consequence has been an accession of Traffic to the Company proportioned to the required accommodation afforded to the Public.

The following is a statement of the receipts and expenditure for the half-year; and the subjoined table exhibits a detailed classification of the disbursements;—

HALF-YEAR ENDING 31st DECEMBER, 1833.

Receipts.

	£	s.	d.
Coaching Department	£54,685	6	11
Merchandise do.	39,957	16	8
Coal do.	2,591	6	6
	£97,234	10	1

Expenses.

	£	s.	d.	£	s.	d.
Advertising....Account	£6	10	0			
Bad Debt....do.	374	10	1			
Coach Disbursement ..do., viz., guards and porters' wages, £1,168 4*s.* 6*d.*; parcel carts, horsekeep and drivers' wages, £361 1*s.* 7*d.*; materials for repairs, £689 12*s.* 6*d.*; men's wages repairing, £1,041 1*s.* 3*d.*; gas, oil, tallow, cordage, &c., £196 4*s.* 11*d.*; duty on passengers, £3,224 11*s.* 11*d.*; stationery and petty expenses, £277 4*s.* 5*d.*; taxes on offices, stations, &c, £116 0*s.* 8*d.*; guards' clothes, £64 15*s.*	7,138	16	9			
Carrying Disbursement Account, viz., agents and clerks' salaries, £1,728 16*s.* 9*d.*; porters and brakesmen's wages, horsekeep, &c., £5,006 6*s.* 10*d.*; gas, oil, tallow, cordage, &c., £529 17*s.*; repairs to jiggers, trucks, stations, &c., £366 9*s.* 11*d.*; stationery and petty expenses, £429 5*s.* 1*d.*; taxes and insurance on offices, &c., £456 17*s.* 7*d.*; sacks for grain, £110 3*s.* 10*d.*	8,627	17	0			
Coal Disbursement Account	82	0	9			
Cartage (Manchester)....do.	3,173	18	0			
Charge for Direction ....do.	312	18	0			
Compensation (Coaching) do.	142	4	8			
Compensation (Carrying) do.	228	10	11			
Coach-office Establishment do., viz., agents and clerks' salaries, £602 6*s.* 8*d.*; Rent, £30	632	6	8			
Engineering Department Account	319	3	4			
Interest....do.	5,140	6	4			
Locomotive Power ...... do., viz., coke and carting, £3,197 4*s.* 4*d.*; wages to coke fillers and waterers, £348 8*s.* 5*d.*; gas, oil, tallow, hemp, cordage, &c. £865 14*s.* 9*d.*; brass and copper, iron, timber, &c., for repairs, £3,755 3*s.* 7*d.*; men's wages repairing, £4,401 4*s.* 10*d.*; engine & firemen's wages, £784 8*s.* 5*d.*; out-door repairs to engines, £613 3*s.* 9d.	13,965	8	1			
Maintenance of Way Account, viz., wages to plate layers, joiners, &c., £2,937 19*s.* 2*d.*; stone, blocks, sleepers, keys, chairs, &c., £2,411 2*s.* 4*d.*; ballasting and draining, £925 16*s.* 11*d.*; new rails, £150 16*s.* 3*d.*	6,425	14	8			
Office Establishment Account, viz., salaries, £607 2*s.*; rent and taxes, £75 14*s.* 3*d.*; stationery and printing, £22 7*s.* 8*d.*; stamps, £17 2*s.* 3*d.*	722	6	2			
Police....Account	1,022	7	6			
Petty Disbursement do.	61	19	6			
Rent.... do.	603	10	8			
Repairs to Walls and Fences	665	3	4			
Stationary Engine and Tunnel Disbursement Account, viz., coal, £302 6*s.* 5*d.*; engine and brakesmen's wages, £319 11*s.* 2*d.*; repairs, gas, oil, tallow, &c., £419 15*s.* 5*d.*; new rope for tunnel, £266 3*s.* 6*d.*	1,307	16	6			
Tax and Rate Account	3,409	11	0			
Waggon Disbursement do. viz., smiths' and joiners' wages, £718 19*s.* 7*d.*; iron, timber, castings, &c., 700 9*s.* 1*d.*; cordage, paint, &c., £28 5*s.* 2*d.*; canvass for sheets, £163 6*s.* 5*d.*	1,611	0	3			
Cartage (Liverpool)	80	17	10			
Law Disbursement	300	3	9	56,350	1	9
Net Profits for Six Months				£40,884	8	4

1st JULY to 31st DECEMBER, 1833.

Disbursements apportioned under the different Heads of Expenditure.

	Per Passenger Booked.	Per Ton of Merchandise Liverpool and Manchester.	Per Ton of Coal.	Per Ton on Bolton Tonnage.	Coaching Department.	Merchandise Department.	Coal Department.	Bolton Tonnage.	TOTALS.
	s. d.	*s. d.*	*s. d.*	*s. d.*	£. *s. d.*	£. *s. d.*	£. *s. d.*	£. *s. d.*	£. *s. d.*
Disbursements in the Merchandise Department, consisting of Porterage, Salaries, Parish Rates, and Insurance, £456. 17*s.* 7*d.*, Carting, Stationary Engine, &c. Disbursements	....	3 9¾	....	0 3¼	....	15150 9 11	....	249 0 8	15399 10 7
Disbursements in the Coaching Department, comprising Porterage, Salaries, Repairs, Duty on Passengers £3,224. 11*s.* 11*d.*, &c. &c.	0 9	....	....	....	7913 8 1	....	....	....	7913 8 1
Porterage, &c. in the Coal Department, after deducting amount received for weighing Coal	....	....	0 0½	....	....	....	82 0 9	....	82 0 9
Locomotive Power Account, proportioned according to the number of Trips of 30 Miles in each Department, comprising Repairs of Engines, Wages, Coke, &c. &c.	0 8¾	1 6¼	....	....	7779 0 1	6186 8 0	....	....	13965 8 1
Sundry Disbursements, proportioned according to the Receipts as between the Coaching and Merchandise Departments, and according to the number of Tons and Miles conveyed, as between the Liverpool and Manchester and Bolton Trade, comprising Maintenance of Way, Police, and Gate Establishment, General Office Establishment, &c. &c.	0 6⅛	0 10½	0 1⅛	0 7	5532 0 2	3404 5 1	262 2 10	547 18 2	9836 6 3
Rates and Taxes, Interest on Loans, and Chief Rents, proportioned according to the amount of Profit in each Department, calculated *exclusively* of these items of Disbursement	0 6¾	0 7¾	0 2½	0 1¼	6129 19 11	2526 6 3	411 1 5	95 0 5	9153 8 0
Total Disbursements..................	2 6¾	6 10⅛	0 4½	0 11⅛	27345 8 3	27357 9 3	755 5 0	891 19 3	56350 1 9
Net Profit	2 6½	2 10	0 11	0 5¼	27339 18 8	11283 19 7	1836 1 6	424 8 7	40884 8 4
Gross Receipts	5 1¼	9 8⅛	1 3½	1 4¾	54685 6 11	38641 8 10	2591 6 6	1316 7 10	97224 10 1

STATEMENT OF RECEIPTS AND EXPENDITURE ON CAPITAL ACCOUNT.

From the Commencement of the Undertaking to 31st December, 1833.

TREASURER, DR.	£	s.	d.
To Amount of joint capital in shares and loans	1,086,885	0	0
.. Ditto of Dividends not paid	1,087	3	1
.. Surplus in hand after payment of the 6th Dividend, in August, 1833.................	395	10	2
.. Net Profit of the Concern for the half-year ending 31st December, 1833..............	40,884	8	4
	£1,129,252	1	7

TREASURER, CR.	£	s.	d.
By Amount of Expenditure on the construction of the Way aud the Works, including the Tunnel Excavation, &c., now in progress	1,089,818	17	7
.. Ditto in the hands of Moss and Co., Bankers	28,476	11	9
.. Ditto in the hands of the Treasurer	242	15	9
.. Ditto of Arrears on Calls..... .	25	3	6
.. Ditto Balance of Book Debts due to the Company	10,688	13	0
	£1,129,252	1	7

During the past six months the excavation of the new tunnel from the vicinity of Wavertree-lane to Lime-street has proceeded regularly and satisfactorily, and is now more than half completed.

In order to extend the advantages of a railway conveyance to the northern docks, and those parts of the town which are at a considerable distance from the railway station, the Directors transmitted a memorial to the Common Council, the Dock Committee, and the Commissioners of Sewers, proposing to construct, at the expense of the Company, a line of railway from Wapping to the Clarence Dock, by means of which merchandise deposited at the north end of the port might possess the same facilities of conveyance by railway into the interior of the country as goods in the southern portions of the town, besides relieving the streets from the noise and interruption of numerous waterside carts. This memorial, as might be expected, from the evident utility of the scheme, has been favourably received, especially by the Dock Committee, and the Commissioners of Sewers; the principal objection to the plan being that it was not sufficiently general and extensive to afford to the public at large that measure of accommodation which appeared so easily practicable. The Directors, however, confidently look forward to the establishment on a comprehensive plan, probably to be undertaken by the Dock Trustees, of a line of railway with the requisite branches, along the dock quays from the northern to the southern extremities of the port; which measure seems alone wanting to give to the mercantile public those advantages of economy and despatch, which a railway conveyance is so peculiarly calculated to afford.

The Proprietors are aware that the subject of locomotive-engines has always been one of great interest and importance. The charge under this head continues very heavy, arising in a great measure from the necessity of renewing and strengthening the frame work of the machinery; and from the purchase of copper and brass plates for the renewal of fire boxes and tubes.

The charge for coke has been a heavy item in the locomotive expenditure, amounting to nearly 6,000*l*. per annum. The Directors have lately been induced to try gas coke to a very considerable extent. The cost per ton is less than one-half the cost of Worsley coke; and although a greater weight is required to do the same service, and an extra consumption of fire bars and some other difficulties attend the use of it, the Directors have considered the experiment well worth making, in the hope of diminishing the expenditure in that department.

Several new schemes for an improved locomotive power have lately been brought under the consideration of the Directors. Past experience forbids any very sanguine anticipations of success in respect of untried speculations; at the same time, the Directors will not fail impartially to investigate the pretensions of any scheme from a respectable source, which professes to introduce improvement into so important a branch of the Company's establishment.

The charge for the maintenance of the way is another heavy item of the current expenditure. In particular parts of the road, especially on the descending lines of the inclined planes, the rails prove too weak for the heavy engines, and the great speed at which they are moved; and from the breakages which have taken place, the Directors have thought it expedient to order a supply of stronger and heavier rails, to put down in those districts where the present rails have been found insufficient. This proceeding will in the first instance subject the Company to some increased expenditure. The Directors, however, have contracted (for the ensuing year) for that portion of the maintenance of way which consists of labour and small materials on terms of comparative advantage to the Company, which they expect will balance the increased outlay required for the purchase of stronger rails.

COMPARATIVE EXPENSE OF LOCOMOTIVE POWER ON RAILWAYS AND COMMON ROADS.*

It is known, that the earliest engines used on the Liverpool and Manchester Railway were not more than six tons weight, and that they have been progressively increased to above ten tons. Now, according to M.†, the heavier engines are the more expensive in working. The contrary is the fact, ascertained by experience.

Again, *it is known*, that the smoothest road yet made, viz., one of granite (like that proposed in the Steam Coach Prospectus), presents a resistance which requires a power of traction to draw a weight at 2½ miles an hour, of 1-70th of that weight.

It is known, that the mean resistance on a railway does not exceed 1-200th of the weight at the highest speed. If then the "violent percussions and blows" which the engines are "necessarily subject to" on the railway cause the wear and tear to be at 1*s*. 9*d*. per mile, what will the still more violent percussions and blows which the engine will inevitably be subject to on the granite road cause their wear and tear to cost?

Again, one of Gurney's boilers, in a locomotive engine employed on a railway consumed about 6lbs. of coal per ton per mile. On the granite road before described, it would require a supply of 18lbs. per ton per mile, or, in other words, 6lbs. of coal would move only ⅓d of a ton one mile. Be it remembered that this boiler was of the construction which alone obtained the unqualified approval of Dr. Lardner in his lectures on the steam-engine, as being superior to the tubular boilers.

Now, if about the same heat be raised by a pound of coke as by a pound of coal, the quantity required for a steam coach, with a load weighing in the whole six tons (the weight given by M.), will be 108lbs. instead of 43lbs., which is thirty-six times greater than the consumption of the most improved locomotive engines now used on a railway, and about twenty-four times the average consumption.

According to our present knowledge, the resistance on a railway is in the ratio of one-third of that on the best granite road that has yet been formed, one-seventh of that on the best formed common road, and one-twelfth of that on the ordinary turnpike-roads. Motion on a railway is as skaiting upon ice, compared as to the effects of concussion, with running upon a pavement; and whatever be the results attained on any of these roads, by either animal or mechanical power, the same power will effect results upon a railway of which the denominators of the preceding fractions are multipliers, with this further advantage, that a rate of speed may be attained on a railway far exceeding that on any other known surface of road.

* From an able letter of a Correspondent of *Aris's Birmingham Gazette*.

† A partisan of the London and Holyhead Steam Coach and Road Company.

ON THE RESISTANCE OF THE AIR AS AFFECTING THE UNDULATING RAILWAY QUESTION—KINCLAVEN IN CONTINUATION. (FROM p. 302.)

Sir,—I do not know whether Mr. Cheverton will still persist in maintaining the soundness of his new theory of the law of atmospheric resistances, or if he will at once candidly admit that he is wrong. If, however, he adheres to his own principle, that when the opposing surfaces are equal, the atmospheric resistances are as the cubes of the velocities, he is certainly bound to give us a demonstration of it. For no one can now deny (except himself, perhaps) that such is the law that follows from his own premises.

Mr. Cheverton very kindly informs me, (No. 545, page 259,) that "not being an engineer" it is not extraordinary that I should be liable to error; but that I have "injured my reputation as a mathematician." In reply to the latter remark, I have only to state, that I never arrogated to myself the character of a mathematician; and, moreover, being only an anonymous contributor, I have, in fact, no "reputation" at stake. Notwithstanding this, however, I feel as anxious in endeavouring to support truth and expose error, as if I were to give my name, profession, and place of abode. Mr. Cheverton, it is true, may boast of being more manly; for, on the credit of his name and character as an engineer, he has boldly asserted that it would require a force equivalent to that of 344 horses to overcome the resistance of the air alone, on an opposing surface of 30 square feet, when propelled with a velocity of 100 miles per hour. While I (the unknown Kinclaven), on the contrary, do assert that (according to Dr. T. Young's formula, or, to speak more accurately, according to Dr. Charles Hutton's Experimental Tables on Atmospheric Resistances,) so far is this from being true, that, instead of 344 horses, it would not require the $\frac{1}{12}$th part of the force of a single donkey to overcome the resistance, allowing two donkeys to equal the strength of one horse.

Mr. Cheverton calculates, from Dr. Young's formula, that when the velocity is 100 miles per hour, the atmospheric resistance is 43lbs., and consequently on 30 square feet 1290lbs. He then supposes that a weight of 1290lbs. is projected *with a new velocity of* 100 *miles per hour*, or 8800 feet per minute, and then he gives us the enormous force of resistance $=1290\times8800=11,352,000$lbs. Here Mr. Cheverton is at fault; he forgets that it is only in consequence of the engine's having attained a velocity of 100 miles per hour that the *force of resistance* amounts to 1290lbs.; and if the velocity is kept up to 100 miles per hour, the force of resistance (1290lbs.) becomes constant. So that instead of 11,352,000lbs. being the force of resistance, it is only 1290lbs.; and even this last resistance is considerably overrated.

Dr. T. Young's formula is only intended for finding the force of atmospheric resistances on military projectiles, and when the velocity happens to be from 800 to 1,000 feet per second, or from 545 to 681 miles per hour; the results deduced from his formula will nearly agree with those obtained from Dr. Hutton's Experimental Tables. Thus: suppose a ball of 1·965 inches diameter is projected with a velocity of 900 feet per second; then $(1{\cdot}965)^2\times\frac{{\cdot}7854}{2}=$ $1{\cdot}516293=$area of half a great circle of the ball $\therefore$ By Dr. Young's formula. $900^2\times\frac{1{\cdot}516293}{500\times144}=17{\cdot}05$lbs.$=$the force of resistance; and, by Dr. Hutton's Experimental Tables, the resistance is 16·94lbs., only differing by $\frac{11}{100}$th parts of a pound. But for a less velocity, Dr. Young's formula will give too great a resistance. Thus, taking the velocity at 100 feet per second, the formula gives a resistance of ·2106lb.; but from the Experimental Tables it is only ·167, which is about $\frac{1}{4}$th more than it should be. Or lastly, by calculating the force of resistance on 30 square feet of opposing surface, for a velocity of 100 miles per hour, or $146\frac{2}{3}$ feet per second—from the Experimental Tables it will be found to be 10·75 lbs., or 3·98 oz. on the square inch. And here I must confess that Mr. Cheverton was right, when he stated that I had estimated the resistance by far too low; when I stated it to be something less than two ounces per square inch, I ought certainly to have said something less than four ounces on the square inch; but whether this arose from a "slip of the pen," or a fault in the head, I am unable just now to determine.

Mr. Cheverton's supporter, T—s H—d, seems to have some doubts about the truth of his new theory. He states that he concurs in *almost all* Mr. Cheverton's views (what views?), except that he considers him to have *over-estimated considerably the resistance of the air*. Yes, Mr. T—s H—d; he certainly has done so; as he makes it more than ten thousand times what it ought to be. Still, after all, I have no doubt that Mr. Cheverton will tell us that he took his authority from a certain eminent writer. I know he has been misled, but not by the writer alluded to (see chaps. xi. and xiii.). The truth is, he has confounded one subject with another. His aide-de-camp, S. Y., may, if he pleases, try to prop him up; but without an abler sustaining power, he must fall to the ground. In fact, so far as the resistance of the air is concerned, he has not a leg to stand upon.

I am, Sir, yours, &c.

KINCLAVEN.

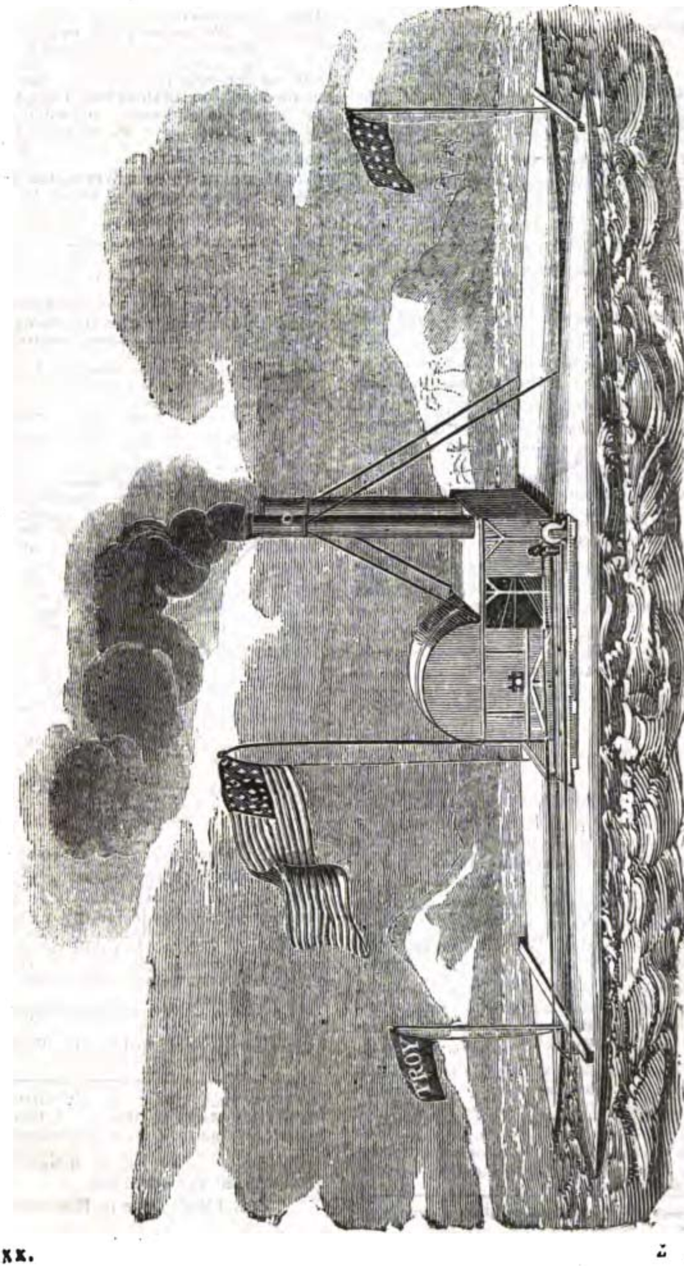
P. S.—Since writing the above, I have seen Mr. Cheverton's reply to the first portion of this paper. As I anticipated, he is determined to uphold his own theory; but he seems now to grasp the subject with a less steady hand, and has been driven to some singular shifts. Of all this, however, more anon.

K.

Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 550. **SATURDAY, FEBRUARY 22, 1834.** **Price 3*d*.**



THE GREAT AMERICAN STEAM-RAFT.

TABLE B.

Experiments with equal Arms of the Yoke at 3 Miles per Hour.

Models compared.	Depth of Immersion in inches.	Weight of Vessels with their Loads.	Difference.	
Flat Vessel....No. 1 Coaster ......No. 2	4·91 8·5	256 lbs. 288 ..	32	No. 2 carries $\frac{1}{8}$ more than No. 1.
No. 1 No. 2	6·083 10·083	320 .. 392 ..	72	No. 2 carries $\frac{2}{9}$ more than No. 1.
Flat Vessel....No. 1 Schooner......No. 3	4·17 8·41	192 .. 234 ..	42	No. 3 carries $\frac{2}{9}$ more than No. 1.
No. 1 No. 3	5·75 10·25	320 .. 362 ..	42	No. 3 carries $\frac{2}{15}$ more than No. 1.
Flat Vessel....No. 1 Twin Vessel ..No. 4	4·17 4	256 .. 256 ..	0	No difference at this rate of speed.

N. B.—The depth of immersion entered above is that observed when the vessels were at rest, and which did not appear to alter when in motion.

TABLE C.

Experiments with equal Arms of the Yoke at 6 Miles per Hour.

Models compared.	Immersion in Inches.	Weight of Models when loaded.	Difference.	
No. 1 Flat Vessel .... No. 2 Coaster	$4\frac{2}{12}$ $6\frac{4}{12}$	192 lbs. 192 ..	—	The draught of water noted in the column of immersions was that observed when the models were at rest previous to the commencement of each experiment; the actual immersion during the experiment was considerably less, especially in the flatter vessels; but there were no means of ascertaining it precisely.
No. 1 .... No. 2	$4\frac{11}{12}$ $8\frac{1}{12}$	256 .. 256 ..	—	
No. 1 .... No. 3 Schooner shape	$4\frac{2}{12}$ $7\frac{9}{12}$	192 .. 192 ..	—	
No. 1 .... No. 3	$4\frac{11}{12}$ $9\frac{2}{12}$	256 .. 256 ..	—	
No. 1 .... No. 4 Twin Boat	$5\frac{9}{12}$ $5\frac{7}{12}$	320 .. 320 ..	—	

THE UNDULATING RAILWAY.

Sir,—It is demonstrated in a satisfactory and impartial manner in Hutton's *Philosophical and Mathematical Recreations*, published thirty years back, that when two points situated so that they can be connected by inclined planes, two or more of which make angles with the horizon of 45°, that the line of these planes will be that of the quickest motion, or that on which a given power will be most efficient. If the controversy, therefore, on this subject were confined to planes at such angles, the question of friction, which Hutton does not take notice of, would be one of importance in the investigation, inasmuch as the pressure and consequent friction of a *moving* body, on such an incline, would be to the friction on the horizontal nearly as one is to two, as Mr.

Badnall observed;* but as the angle of the Sutton incline plane is so small, as to have a rise of but one in one hundred, the question of friction must be of little consequence, and may be altogether dispensed with. The line also so nearly approaches a straight one, and is so far removed from the planes above described, that the very unexpected results of the late experiments must be altogether caused by the variable power of the engines, amounting to considerably more than has been hitherto contemplated—for no direct experiments were ever made to ascertain the point. The undulating question has therefore taken a different turn from what it was at the commencement of the inquiry; and the small incline of the Sutton plane has been found to change the variable power of these engines to a constant† power of the greatest intensity the engine is capable of. This leads me to make an observation on the manner these experiments were made. It is to be observed that, as the engine is moving from a state of rest to a state of rapid motion, the intensity of the power is rapidly diminishing; on the other hand, when moving from a state of rapid motion to a state of rest, as in moving up the incline plane, the power of the engine is increasing in intensity, and will, when near the top of the ascent, be considerably increased, and recover the power that had before ceased to be available; it therefore follows that the velocity of the carriage, as ascertained by calculations in these experiments, must be very erroneous; but I have no doubt the actual velocity, from the same circumstance, if the results are given accurate, are more in Mr. Badnall's favour; and this was my impression when I made the remark about these experiments in my last.

MENTOR.

Capel-street, Dublin,
Feb. 13, 1834.

* Your correspondent S. Y. must recollect, that it is the pressure of *a body at rest* on an inclined plane that is to the weight as one is to the square root of two, and not of a body in motion, as the power acting in the line drawn parallel to the plane in the former case is in the latter removed.

† The word *constant*, as applied to mechanics, has been misunderstood. It is not made use of to distinguish a continuous from an intermitting power, but to distinguish a variable from a power that always, and under all circumstances, continues at equal intensities, whether at high, at low velocities, or whether running in a resisting or unresisting medium.

THE NEILGHERRY HILLS.—NO. I.

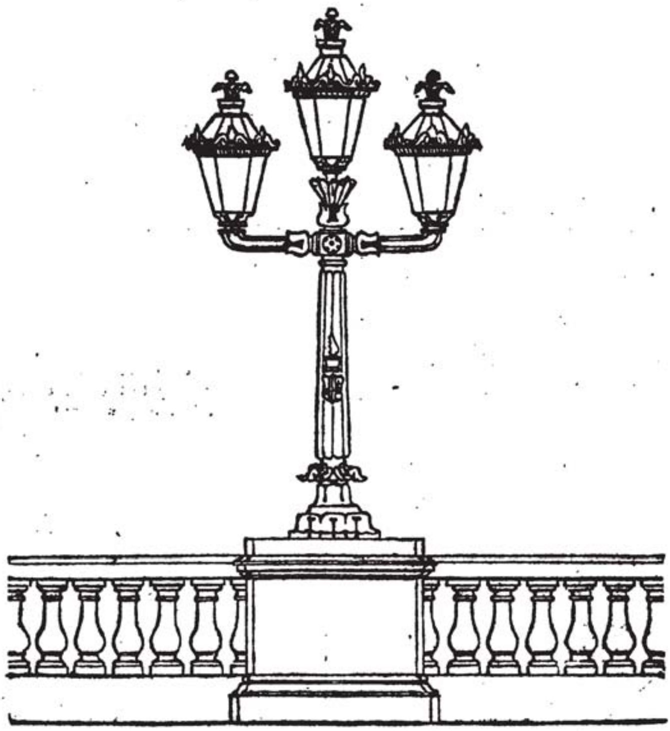
Sir,—As there appears now, in the renewal of the East India Company's charter, a great probability that emigration to India will be permitted, and as some of your readers, willing, perhaps, to improve their condition, are deterred from venturing to these realms only by the report of the excessive heat and unhealthiness of the climate, it may not be unacceptable to them to be informed, that between the parallels of 11° 7′ and 11° 30′ N. lat. and 76° 30′ and 77° E. long., a tract of country exists, about 40 miles in length and 25 in breadth, at an elevation of from 4,500 to 8,700 feet above the level of the sea, where they may enjoy a mild and equable climate, well suited to the European constitution; besides deriving many other advantages, to enumerate which is the object of the present communication.

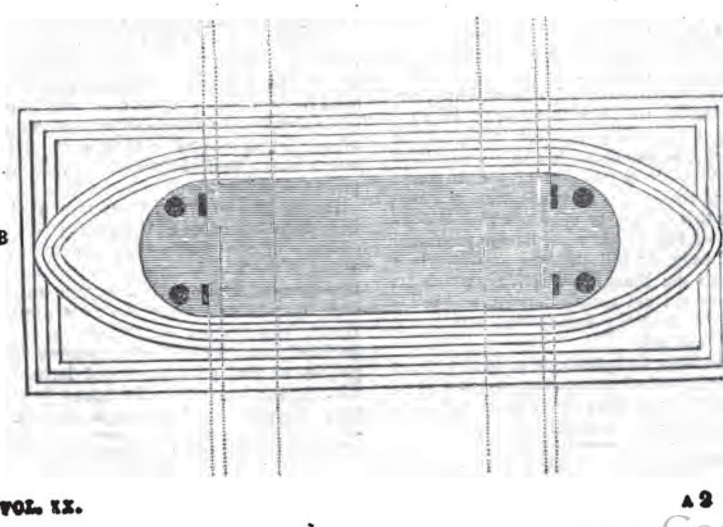
The dread entertained by the natives of the numerous and nameless dangers to be encountered in surmounting these hills, appears to have prevented their being explored, until 1819, when some gentlemen from Coimbatoor, who had wandered hither on a shooting excursion, pleased with the mildness of the climate, tempted others to follow them, some of whom built bungalows, and, in the course of a few years, these hills have become a most valuable convalescent station for the invalids of the two southern presidencies of India.

The aborigines of the Neilgherries consist of three classes or castes, which are perfectly distinct from each other and from the inhabitants of the low country. They distinguish themselves by the names of "Thoder," "Burghur," and "Kota." The Thoders are one of the finest races of men in India, being tall, strong and athletic; they allow the beard and mustachios to grow, and wear no covering on the head: their dress consists of a large square piece of cotton cloth: their women are fair, and would be handsome, were it not for the extreme breadth of their mouths; their dress is similar to that of the men, but worn in a different manner; when decked out for any ceremony, they curl their hair in ringlets, which hang down all round the head. The only occupations of the men are feeding their buffaloes, and attending to the duties of the dairy, into which a woman is not allowed to enter,

Mechanics' Magazine,
MUSEUM, REGISTER, JOURNAL, AND GAZETTE.
No. 551. SATURDAY, MARCH 1, 1834. Price 3d.

BLACKFRIARS' BRIDGE REPAIRS.

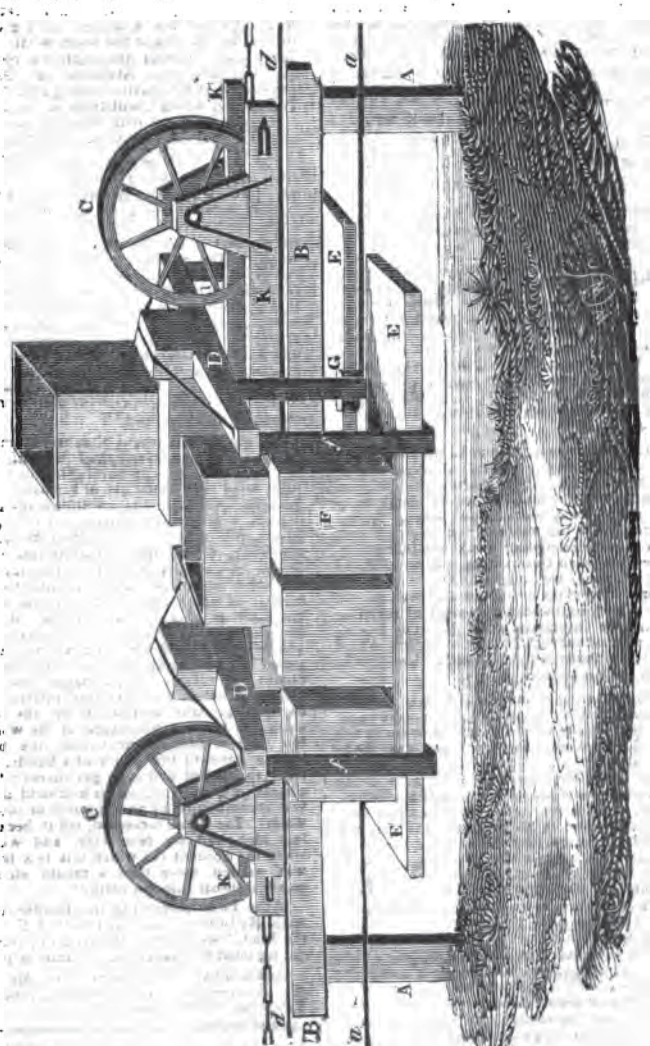
A  A

B  B

Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 552. SATURDAY, MARCH 8, 1834. Price 3d.



SARGENT'S SUSPENSION RAILWAY.

and strengthened by oblique braces. Being merely an experiment, the cars to be placed upon it are intended only for the transportation of passengers to a place of entertainment at the farther end of it.

The only serious objection that has been made to the suspension railway is, that being elevated so far from the ground, it may not be so sufficiently permanent, and so capable of bearing heavy loads, at a *rapid* rate, as the iron rails which are elevated only a few inches. We do not wish to discuss this question, though many persons, whose opinions in these matters are of great weight, believe that it may be made sufficiently permanent for all practical purposes.

THE UNDULATING RAILWAY—GENERAL REPLY OF MR. CHEVERTON TO MR. BADNALL.

Sir,—I should ill consult my own character, or that of your scientific Journal, or the good taste of your readers, were I to make your pages, though with your permission, the medium of a personal quarrel between me and Mr. Badnall. I will not descend to an imitation of that dull vituperative style in which he has indulged in his last communication. If he could have made a piquant exhibition of errors, or even held up tenable positions and arguments of mine in a ludicrous point of view, it would have been allowable, as a legitimate mode of warfare in a keen encounter of the wits; but pointless personalities, perverse misconstructions, false statements, and falsified quotations, are not to my taste, and therefore, with this communication, I close my controversy with Mr. Badnall. Satisfied with having forced from him the admission, that he has nothing farther to advance on the merits of the theory of the undulating railway (and the experiments have long since been explained and disposed of), I will not be a party to that wordy and worthless discussion to which he invites me, for the "amusement," forsooth, of your readers; and long may it be, before, in lack of better means of entertainment, "you *will* find room for a display of that satirical prowess," as with overweening fondness he calls it, of which his communication before me is to be the commencement. It were a sorry object of ambition to put to silence a writer, whose utmost effort at cutting sarcasm consists in calling his opponent "Champion Cheverton," and whose keenest reproach is displayed in branding him with the epithet "The Utilitarian;"* but whose will to do ill is as evident as the deed is impotent. No, no, a challenge from such a combatant has no allurement for me. If circumstances should ever force me to enter the arena of literary gladiatorship, let me have for my antagonist one with whom a contest would be no disgrace, even though I should be foiled in the strife—one of a noble bearing, who can dexterously wield a keen and polished weapon, and not a coarse practitioner from the *abattoir*—a mere hacker and hewer of flesh and bones.† If my sword *must* leap from its scabbard, it shall not be at the sound of a Mohawk's war-whoop, but at the clarion call of high-minded chivalry—not to avenge a barbarous tomahawk assault, but to parry the scientific thrusts of a weapon, sharper than a two-edged sword, and piercing even to the dividing asunder of soul and spirit, and of the joints and marrow. Is it then my wish, because nature has been as niggardly in her gifts to my opponent, of the means of attack and self-defence, as she is to the feline race, that therefore he should not have the privilege of employing them, such as they are? By no means—he must use his own discretion as to that. I merely decline to quarry such ignoble game, or even with sonorous tin to worry and run it down. He has done his best, and I provoked him to it (but not until he had misquoted me), for I dared to laugh at the ludicrous pretensions of a man, who unacquainted almost as much as any one possibly can be with even the first principles of mechanical philosophy, has had the assurance to give a challenge to all the men of science in England, aye, and in Europe too. Was it not to such an one sufficient cause for sore offence, that, by a simple iteration in his own language, and without comment, of his crude or erroneous notions, and half-digested scraps of knowledge, it was shown, that he who boasts of his "expensive education," and "twenty years attention to scientific subjects," and of his being "a labourer in science," has not even passed the threshold of its portals? Besides, how could I expect forgiveness after having brought home to conviction the charge of his having quoted me as advancing a proposition, which I did

* Be it so—I would covet no better epitaph; but the capital hit, as Mr. Badnall thinks it to be, consists in giving me this sobriquet, as the supposed coiner of a new-fangled term, as though it were not in constant use in reference to that modern school of economists, to which your able correspondent Junius Redivivus has, if I mistake not, a *peculiar* claim to belong.

† Perhaps Mr. Badnall, with that acuteness of intellect which has enabled him to interpret the expression "a true Irish perception of things," to mean, that I believed him to be an Irishman, will now, with equal ingenuity, imagine that I take him to be a butcher.

not advance, and of his having garbled the quotation, and omitted many words and foisted in others, in order to further the culpable deception. For this literary dishonesty I owed him no courtesy, and yet showed more to him than was his due. Mere error, yea, gross ignorance, should be requited only with correction and with instruction—even charlatanical ignorance may not always provoke one to administer the lash which it deserves; but when accompanied by wilful mis-statements, perverse misconstructions, and a fraudulent quotation, forbearance in such a case becomes a fault. To this fault, however, I must plead guilty, and crave your readers pardon, for I am not disposed to return epithet for epithet, nor avail myself of all the privileges which an undisguised signature confers upon me; but should certain terms be supplied and applied by your readers, in the way of inference—and should certain laudatory expressions escape from their lips in spite of their teeth, it is more than I can help, or care to prevent. It is thus, Mr. Editor, that controversies which commence amicably enough, as in the present instance, terminate so often with acrimony, because one of the parties has not sufficient integrity of mind and purpose to withhold him from taking unfair advantages of the other. If Mr. Badnall had acknowledged that the misquotation was an oversight, the explanation would have been accepted; but as he has not done so, and has chosen to mystify the accusation, by treating it as though he had only made an inference of his own, I shall leave it to your readers to draw their own conclusions, whether that which I charge him with was an inadvertency or not.

He now brings together all that I have said on the similarity of the two forces of steam and gravity, with the exception of some qualifying passages, and then triumphantly asks, "Have I falsified the terms of Mr. Cheverton or not?" He has both falsified the terms, and drawn false conclusions—the one cannot be animadverted on with too much severity, and will exist as a witness against him as long, and be as enduring, as the type which records it—the other is merely, as S. Y. says, "an affair between him and his understanding." However, I will briefly show that I have not been guilty of the egregious folly which Mr. Badnall so perseveringly attempts to fasten on me, of stating that the force of steam (in common locomotive practice) is *equally* constant like that of gravity. From the very first, I asserted, that the similarity of the two forces consisted in their both being of the accelerating kind, without entering into the question about their respective intensities, or whether one could be as equally maintained as the other. Because I have spoken of the "constant action of steam," and of its producing "accelerated motion," Mr. Badnall, with laughable simplicity, thinks I must mean a force *equably* constant like gravity, and capable like it of producing uniform acceleration in an unresisting medium. The fact is, he has no conception of the value of the word *uniform*, as applied to accelerated motion, for he uses it indiscriminately under all circumstances. For instance, he talks of "a feather descending in a perfectly still atmosphere with *uniform* acceleration," and of its being undeniable "that *uniform* acceleration must take place until the greatest velocity *is* attained," namely, that which is "acquired by a body descending in a resisting medium."

There is, as I explained once before, but which explanation Mr. Badnall has not thought it worth his while to notice or even to understand, an ambiguity about the word "constant;" for it may either have a reference to time, as to a thing constantly or continually acting, or it may refer to quality or degree, as to a thing remaining always the same. In one instance (and quoted by him), I used the word in the former sense as applied to steam. My words were, "he (Mr. Badnall) speaks of gravity truly as being a *uniformly* accelerating force; but does not admit of the motive force of the engine being of an accelerating kind, though he must be aware that it is a *constant* force." Observe, I did not say that the force of the engine is of a *uniformly* accelerating kind, or of its being a constant *uniform* force; but I immediately added, "whether it is *like gravity*—uniformly accelerating is not necessary to the inquiry, &c." Now every one acquainted with these things must know, that it *would* be uniformly accelerating, and *be like* gravity, if the force of steam were *constant* in the sense of an equable force. I evidently alluded to the *constant action* of a force, which, according to circumstances, may, or may not, be variable in respect to its intensity, and obviously suppose that it *may not* be "like gravity" in this particular. Mr. Badnall himself, with another instance of laughable simplicity, admits, "that it is true Mr. Cheverton afterwards makes allusion to the possible difference in the two forces, as regards *uniform acceleration*," and yet he does not perceive that this admission entirely destroys his conclusion, that I consider "the power of steam, like the power of gravity, a constant force." This is not an instance, I conceive, of a perverse misconstruction of my meaning, but a self-denying and ingenuous display of the most entire ignorance in regard to the point in discussion. There is nothing, therefore, to censure herein, excepting always the garbled quotation as aforesaid.

Suppose, however, that I had committed the blunder which he would impute to me—what then? Would he rest the merits of the undulating principle upon the circumstance of gravity being a constant force, and of steam not being such a force? This would appear to be his opinion, and also Mr. Ham's and Mentor's; and, perhaps, some other of your readers may coincide with them in this view of the subject. It is, however, confuted by the fact, that the undulating principle is supposed to triumph, in the experiments instituted for that purpose, even when the carriages are urged by acknowledged constant forces, such as spring-power in one case (equalised by means of a fusee), and more recently at Manchester, the power obtained from the descent of weights. What, then, becomes of the vaunted efficacy of this quality of constancy in the gravitating force, seeing that the reason of its alleged superiority must be sought for in some other principle? This, also, the prolific Mr. Badnall has brought forth, for he has discovered the exact counterpart of his "*periphfugal*"* force in the affirmative to the question—"whether you can take advantage of the gratuitous power of gravity on a general level, without being under the necessity of repaying that assistance?" By the way, this admission of the *gratuitous* power of gravity, and the contradictory nonsense of *repaying* it, is a vile travesty, by this "beau-ideal of a good controversialist," of what I really did ask. My question was, "Do you expect to procure that aid (the assistance of gravity to cope with resistance from air and friction,) *gratuitously*, and to avail yourself of that power on a general level, without being under the necessity of repaying such assistance?"—and when I answered, "Assuredly not:" I little expected, even from Mr. Badnall, the frantic extravagance of objecting to the only sane reply which such a question can receive. According to him, this is "the very question in which the whole of this controversy originated," and that I have settled it at once by answering "assuredly not," an answer in which all your readers, who are not perpetual motion-seekers, will readily concur. Is it then, at last, avowedly come to this, that Mr. Badnall expects to gain more in going down hill than he will lose in going up hill?—the very absurdity which I reproved Junius Redivivus for attributing to him. Here, then, is the discovery of a principle on which to ground the advantages of the undulating railway, that shall for ever be the crowning fame of the perfecter of the Newtonian philosophy; and worthy is it of the progenitor or protector, as the case may be, of that wondrous thing the "*periphfugal force*," in which, if Newton were now alive, he would perceive "a most extraordinary practical confirmation of his most favourite and his noblest theory." There must certainly be some among Mr. Badnall's supporters who are scientific individuals, for the scheme is not so obviously absurd, that certain vague notions respecting the curve of quickest descent, and its supposed applicability to this case, may not mislead those whose penchant for science is more abstract than practical. Why do not those among them who are his friends interfere to save him from himself, and avert that derision which must fall on the scheme, through the self-exposed imbecilities of its projector. One of these is so outrageous as to be almost incredible. What think you, Sir, of "doctrine so sound" as this, that the force of descent of a train of loaded waggons is equal to its own weight!! But you must have his own words for it. "Let us now ascertain at what velocity a train of loaded waggons, weighing 100 tons, must be moving before the resistance of the air could be *equal to the force of descent*?"—"At what velocity, then, must the train of 100 tons be moving before the resistance of the air become *equal to* 100 *tons*?" Perhaps, after all, Mr. Badnall may be in the right, for we cannot tell but what he was contemplating the case of a *perpendicular* descent of a train of loaded waggons, bringing to him a fresh supply of the commodity he deals in—moonshine.

There is only one more instance of perverse misconstruction of my meaning, to which it is worth while to allude. I had observed, in reply to Mentor, that "all my statements in respect to the similar effects produced by steam and gravity were hypothetical, and carefully guarded by conditions," meaning, what I had already fully explained, that though these forces were similar in *kind*, they must be supposed to be similar in *degree*, ere they could produce the same effects—a supposition, which could be easily realised in practice, but having no other application than to convince Mr. Badnall that these forces are not so essentially different as he imagines. Again, I had stated, that if it were an object of any importance, steam force in locomotive engines could be rendered constant up to any intended velocity, and at the same time assigned what was requisite to produce this effect. Now Mr. Badnall has chosen to torture these statements into an admission that *all* my arguments about the undulating, aye, about the *undulating* principle, are hypothetical, and that they are founded upon imaginary improvements in locomotive ma-

* The spelling, the word, the thing signified, and the erudite scholar, the coiner, and the discoverer of these counterfeits, are all worthy of each other.

chinery. Here are his words:—"Now, Sir, if *all* his arguments in reference to the *undulating railway* are, *according to his own confession*, founded on hypothesis, and, moreover, if each hypothesis is carefully guarded by conditions, what reliance, &c." "Had I supposed, when I took the trouble of replying to Mr. Cheverton's various communications (he is infinitely condescending), that the *whole* of his arguments were founded, not only on hypothesis, as generally understood, but upon imaginary improvements in locomotive machinery, I should have taken a very different course." Mr. Badnall knows very well that both of the above statements refer to unimportant collateral points, and were avowedly argued with him as such, and merely to correct some erroneous notions of his concerning them; and his conscience is fully alive to the fact, that my principal argument, as invalidating the undulating principle, refers to the amount of, and mode of estimating, the locomotive *duty*, and that he has not uttered one word of reply to it, though his notice has even again and again been challenged to that particular.

As this argument may have been somewhat lost sight of during the latter part of the discussion, it may be as well to state it concisely, thus—very little steam power is employed in producing motion—almost the whole of it is expended in doing *work* or *duty*, that is, in overcoming resistance from air and friction—thus employed it is lost or dissipated. Now, Mr. Badnall never pretended to show, either by his "diagrams" or his experiments, that the force of gravity can be used in this way *gratuitously*—the mere statement is sufficient to convince of its impossibility on a general level. He can only derive advantage from it, then, when its employment does not infer its being lost or dissipated, that is, in imparting motion—such advantage is only at the commencement—to this he is welcome.

In adverting to the instances just passed under review, of the total disregard, on the part of Mr. Badnall, of all the principles of honour and fair discussion by which a disputant should be guided, I have been induced to undertake the disagreeable task, less with a view to my own justification, as to bring forward sufficient reasons why I decline further controversy with such an opponent. It is not, however, my intention wholly to abandon the subject, if any thing from the more judicious and scientific supporters of the undulating principle should require my notice. Mr. B. will, most probably, reply to this, and it shall be to him a satisfaction to have the last word.

There are, however, two or three particulars in Mr. Badnall's recent letters which are entitled to a slight notice. He says that, at a velocity of 65 miles an hour, the total resistance of a train of loaded waggons is so reduced, that any man standing in the first carriage can, by laying hold of the connecting chain, draw up all the following carriages to his own, whereas at 5 miles an hour such power would be altogether ineffectual. It is quite a matter of course that the *tugging* effort of the engine should be greater with the said lesser velocity, for, up to the point at which uniform velocity is attained, it has, in addition to the usual constant resistance, the inertia of the moving mass to contend with; and yet it is quite in accordance with this circumstance to state, that the obstructive resistance is much greater with high than with low velocities, and that with a double velocity, the expenditure of power is octupled in regard to one portion of that resistance. There is no inconsistency here, and yet, I fear me, it will long continue an enigma to Mr. Badnall, notwithstanding I did my best, unsuccessfully it seems, to suit my explanation of the subject to the calibre of his mind. "It is quite evident," says Mr. Badnall, "that either Mr. Cheverton or myself must be most egregiously in error,"—there is no doubt of it. Is it, however, a fact what is stated by Mr. Badnall? I can easily believe that there are *occasional moments* when a man's strength may be exerted with such an effect; but nothing less than the carefully observed average indications of a dynamometer, and reported by a better experimentalist than Mr. Badnall, will convince me that the draft of a train of loaded waggons, say 150 tons, is not on a level more than equivalent to a man's strength in that position—say a hundred weight. Why this would make the resistance from friction, let alone that of the air, to be the three-thousandth, instead of the three hundredth, part of the weight, and this latter is a too favourable estimate of its amount. Besides, what a libel on the powers of these far-famed locomotive engines—what vile pieces of machinery they must be, to have their working force reduced to this.

Mr. Badnall still insists on our "doing honour to the new light" he has received, on the resistance of the air "being less at high velocities than at low velocities," at any rate "of its not increasing as the squares of the velocities." What, in the name of charlatanry, are the experiments of Dr. Hutton worth, though instituted for the express purpose of ascertaining the law of resistance, when compared with "the *very strong reasons*" which the scientific Mr. Badnall has "for believing *his* opinions to be true?" Besides he and Mr. R. Stephenson, the discoverers of "the periphfugal force," are "now making *attempts* to investigate this subject," and therefore nothing less can be expected than that *now* "the laws of motion and resistance, under various circumstances, and velocities, will be *more clearly developed* than they have *hitherto been*;" for, be it recorded, that "in the year 1666," Hooke gave utterance to the following remark, and, of course, it is true and applicable at the present time, although one Newton has lived in the interim, else Mr. Badnall would not have "been led by the results which he anticipates to quote it."—"Gravity," says Hooke, "though it seems to be one of the most universal active principles in the world, and consequently ought to be the most considerable, has had the ill fate to have been always, till of late, esteemed otherwise, even to slighting and neglect." Oh Newton! what a dunderhead were you, to miss all the bright imaginings of our two philosophers; and oh happy age! to be distinguished hereafter as the era in which they flourished.

But Mr. Badnall has the experiments of Mr. Woods to cite in corroboration of his opinions. I have not Mr. Woods' book at hand, but I have no doubt that these experiments, referred to by Mr. Badnall, were instituted with a wish to *avoid*, as much as possible, the interference of the resistance of the air, in the conclusions he intended to draw from them relative to the nature of friction. With this view, the utmost velocity he allowed the carriages to attain, even at the termination of the descent, was not more than about 18 miles an hour; and nobody but Mr. Badnall would suppose that the acceleration of a weight of more than four tons, would be affected in any considerable degree in the gradual attainment to this moderate ultimate velocity by the resistance of the air. However, he says it exerted no influence whatever, as the details of the experiments prove—for instance:—

In 18 seconds the carriage fell	25 feet
28	71·9
58	276·5
128 the terminal velocity	1266·5

Now the first and last distances which the carriage descended are very nearly as the squares of the times, and, therefore, as Mr. Badnall infers, the resistance of the air could not have *increased* with the velocity. Let us, however, examine the subject a little more closely than Mr. Badnall has done. In the first and second instances there is a discrepancy the other way, showing, as might be expected, that the friction was greatest just at the commencement of the experiment. Let us, therefore, compare not the first and last, but the distance travelled over in 58 seconds with that gone over in 128 seconds, and then we shall find that, had there been no *increase* of resistance, the latter distance should have been 1375·6 feet, instead of 1266·5 feet, as in the experiment—only 109 feet difference! It is not worth while to sift the table more exactly, and show the variations in relation with the velocities.* The same result is observable in the other series of experiments which Mr. Badnall quotes, and where, too, he has stumbled on the wrong column for his data. He has actually taken the distances as *calculated* according to the squares of the times, in order to prove that they *are* as the squares of the times. On comparing the two columns, it will be seen that beyond a certain point there is with the increased velocity a series of *increasing* differences—a circumstance which it was convenient for Mr. Badnall to overlook. Thus it is that he possesses the happy knack of making the experiments he takes in hand, whether his own or other persons, support any object he has in view, and, accordingly, he announces that his interpretation of those in question "is a *deathblow* to the previously admitted opinions on this subject." His second deathblow, for the thing must be *still dead*, consists in similar inferences derived from his Liverpool and Manchester railway experiments, and are of equal worth; but there is no necessity for saying a single word more upon the subject.

Mr. Badnall invites me to suggest any new mode of performing his experiments. I would wish him, then, in reference to the model railway, not to make up the resistance to his motive power with *weight* so much as he is accustomed to do, but to produce it more in that manner in which he will have to encounter it in practice upon a *long scale*. This he can do by extending a sail of considerable comparative dimensions, on either the drag or the carriage that follows it, so as to expose a larger surface to the resistance of the air; and by moderately loading both carriages, and locking all their wheels except the propelling pair, so as to produce greater friction. The obstructive resistance thus constituted, from air and friction, is that only which locomotives have to encounter after the greatest velocity is obtained; and this taking place in a few minutes, a very inconsiderable portion of the total expenditure of steam power is employed in first putting the mass into motion; but, in Mr. Badnall's experiments, which terminate

* If the reader will take the trouble to add to the table in No. 540, which Mr. Badnall gives from Mr. Woods' book, two other columns of first and second differences, he will perceive that the results are rather anomalous, showing that they have no value whatever in determining the law of atmospheric resistance.

where they ought to begin, almost all the power is thus occupied. The results so procured, cannot therefore coincide with those which would obtain in practice, for the conditions are not alike. If Mr. Badnall will, in the manner suggested, make the obstructive resistance to form a much larger portion of the total resistance than he is in the habit of doing, he will find that the advantages, as indicated by his former experiments, will vanish in proportion as the inertia of the mass forms the least part of it; and if he will extend his railway in length, take advantage of the air's resistance to avoid a *general* acceleration, compare results only on the latter half of the line; (supposing the acceleration not to continue beyond the first half of it,) and thus make the circumstances of the experiment comport with actual practice, he will then find, not only that his supposed advantages will disappear altogether, but that the results will be extremely unfavourable to his scheme. If he is an honest man, and wishes to avoid the imputation of dealing craftily with the public, he will put the undulating principle to this test forthwith, and, in the interim, abstain from that indecorous, because unwarranted, confidence of statement, by which he would enlist the support of that public in furtherance of his plan. I may, perhaps, if I have time, make the first part of the experiment myself at the National Gallery of Practical Science, &c., in Adelaide-street, not, indeed, for my own satisfaction, but for the especial gratification and delight of Mr. Badnall himself, unless, frightened at this announcement, he should order the railway models to be immediately removed, in which case your readers can draw their own conclusions.

With regard to the experiments instituted at the Adelaide-street Gallery some months since, by a very ingenious gentleman, whose name I have not asked his permission to mention, for the purpose of explaining to Mr. Badnall, and his *friends* then present, the rationale of the apparent advantages which his own prior experiments indicated as belonging to the undulating system—in regard, I say, to these experiments, so kindly made to warn him of the error of his *ways*, I did not assert, as he chooses to say I did, that "some models were put down" for that purpose, because I knew very well that they had been provided long before by Mr. Badnall himself; but I alluded simply to the fact, of such *illustrated* admonitions and corrections having taken place, and to the *lecture* which was then and there delivered *gratis* by Professor *******, of the London University, and to the sundry conversations and discussions appertaining thereunto, whereat one of Mr. Badnall's friends and supporters was so chagrined, or convinced against his will, that he declared he would be off to the country, and would not stay to be "*badgered*" there any longer.

Your correspondent, T——s H——d, will perceive, by these remarks, that similar experiments to those which he proposed, and in part instituted, and to a discussion of which he invites my attention, were made in the presence of Mr. Badnall, and failing to convince, it were vain to expect that any thing I could say respecting them would produce a greater impression.

It may be interesting to be informed, that at a late meeting of the Society of Civil Engineers, when Mr. Badnall's undulating railway was the topic of discussion, it was unanimously voted to be a fallacy.

I shall conclude with a practical view of the subject, the correctness of which it is impossible Mr. Badnall can impugn. Let all that he contends for be allowed, and still his scheme must ever be inferior to the ordinary mode; for the greatest velocity which can be permitted to obtain is that which is compatible with safety. Now, the locomotives are, perhaps, already capable of making an effort up to that limit, and if they are not, they shortly will be; and this velocity, be it observed, will with them be constant, but in Mr. Badnall's scheme it can only take place at the bottom of the undulations, and therefore his rate of speed must necessarily be always considerably slower than the other.

To part, however, in good humour, I beg to express to Mr. Badnall how deeply sensible I am of the extreme obligations I am under to him, for the *Greek* quotation with which he closes his communication, for, not understanding a word of it, it has excited my admiration vastly, and edified me exceedingly; and in return for this most excellent piece of *larning*, part and parcel, no doubt, of his "expensive education," I beg to assure him, that though some ill-natured persons may take him for a pedant, I, on the contrary, am *duly* penetrated with astonishment at the extent of his talents and attainments of *all sorts*; and that, notwithstanding certain awkward and untoward, though some may think apt and elucidatory metaphorical illustrations, at the commencement of this article, I now eagerly seize this opportunity of renewing to him the assurances of my most distinguished consideration,—remaining, Sir, with a humble sense of my own littleness, in the presence of such profound scholarship,

Your most obedient servant,

BENJ. CHEVERTON.

called a natural taste, as does the enjoyment of pictures, statuary, or music. Architecture is more an art of reason than of imagination; and there is hardly any great feature of beauty or deformity in a building, the propriety or absurdity of which could not be made obvious to the most ordinary understanding, even if the possessor of that understanding had paid very little attention to the subject."—p. 3.

There can be no better proof of the predominant share which *reason* has in determining the state of the architectural art in any country than the very low state in which it is in Great Britain at the present day. We are a *reasoning* people,—none more so,—but our public buildings would seem to indicate that we are a nation of fools. The cause of this is simply, that the *reason* of the community has never been brought to bear with sufficient force on the character of our architecture. Precedent and caprice have been for a long time the presiding influences, and the consequence has been the covering of the land with a multitude of edifices, scarce one of which is in harmony with the common sense character of the age. There is now in the course of erection a "Gallery," which is to be called *par excellence* "*National*," and which, they say, has been approved of by the select of both Court and Parliament, but which we hesitate not to assert will be universally pronounced, a few years hence, a *National Disgrace*. Let any person who doubts this, go and inspect the model of it, at Mr. Day's excellent collection of architectural models, in King William-street, Charing-cross (we recommend them to Mr. Loudon's attention);—let him consult only his own common sense (if he has any) as to its merits, and we feel quite sure he will agree with us, that a more contemptible design for a grand "National" Museum could not easily have been contrived. Mr. Loudon's plan of bringing the "reason" of the community into play, is the only thing to put an end to the perpetration of "National" offences like this. Give but such efforts as his for the diffusion of a taste for architectural beauty the encouragement they deserve, and we shall hope to see, ere many years, the majority of the works of the *tom-tits* (not *Wren's*) of our present time, become a common theme of derision and scorn.

NOTES AND NOTICES.

Ample information respecting the economy of the process of manufacturing sugar in *vacuo*, recently introduced into Demerara, will be found by J——s in the Numbers of this Journal for 28th July, 1832, and 27th July, 1833.

Great improvements have been lately made in France, in the manufacture of tiles used for painting. A stone called the Volvic stone, produced by the lava of the rocks of Auvergne, is found to answer the purpose of tiles, much better than any stone previously in use. This stone has also been introduced into the manufactory at Sevres, and is found to make very excellent china as well as tiles, and is used for enamelling. Some beautiful specimens of enamel painting upon this stone have lately been executed at Sevres. One is a copy of a master-piece of Gerard Dow, and another a beautiful specimen from Rubens. This stone sustains any heat without injury, and is so hard that it may be employed for pavement. Indeed, it is intended to be used in Paris, in those streets which are to be improved after the English fashion.—*Athenæum.*

The outcry at the decline of science in England is every day growing weaker and weaker. From the following passage in Professor Powell's "History of Natural Philosophy," we suppose he is not inclined to join in it:—"The great increase of institutions and societies established for the purpose of promoting, in various ways, *the prosecution of science* and the diffusion of philosophical information, during the eighteenth and present centuries, has been a striking feature, equally *demonstrative of the extended diffusion of a taste for such pursuits*, and efficacious towards its further progress."—p. 389. Yet (will it be believed?) the motto which fronts the title of this very volume, taken from a paper in the *Quarterly Review* by Sir David Brewster, intimates that the chief object of its publication is to "devise a cure" for the present "*declining taste* for science"! How is this startling inconsistency to be accounted for? Did one hand write the book, and another supply the motto? We shall next week notice the Professor's book more at length.

The reproach which has so long rested on England, that she is the only nation whose monuments require protection, is passing away, at least so far as France is concerned; for the Parisian government has found it necessary not only to surround the Exchange with an iron palisade, but also to defend the *ci-devant* Temple of Glory, the Madeleine, and the approaches to the Pantheon, by similar barricades.—*Architectural Magazine.*

M. Polonceau, the engineer of roads and bridges, has just completed his survey for the great rail-road from Paris to Bourdeaux, which will have branch-roads to many other places.—*Ibid.*

We have made inquiry into the truth of the statements sent to us respecting the Greenwich Railway, by a "Proprietor on the Line," and are satisfied that he has been, in most material points, misinformed. We have always entertained a high opinion of the productive capabilities of the line, and are happy to find that the arrangements for its speedy completion are in a most forward and satisfactory state.

Mr. Badnall's recapitulation in our next.

Communications received from Mr. Jopling—Mr. Woodhouse—Omicron—Brackstone—I.—Mr. Quantrelle—Mr. Witty—Mr. Rutter.

Erratum.

Page 331, column 2, line 12 from the top, for "10·75 lbs." read "1075 lbs.

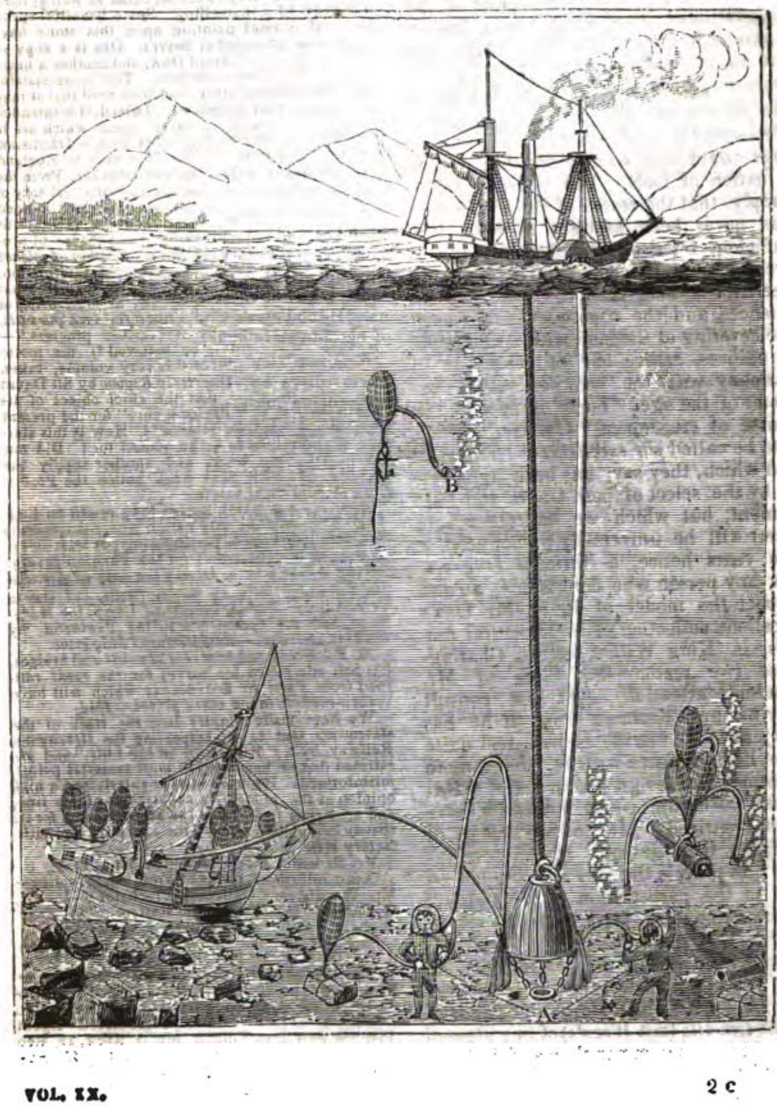
LONDON: Published by M. SALMON, at the Mechanics' Magazine Office, No. 6, Peterborough Court, between 135 and 136, Fleet-street. Agent for the American Edition, Mr. O. RICH, 12, Red Lion-square. Sold by G. G. BENNIS, 55, Rue Neuve, Saint Augustin, Paris.
M. SALMON, Printer, Fleet-street.

Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 553. **SATURDAY, MARCH 15, 1834.** **Price 3d.**

MILNE'S PLAN FOR RAISING SUNK VESSELS.



NOTES AND NOTICES.

Mr. James Heard, of Blackheath, states, in a communication to the Society of Arts, published in the last part of their Transactions, that he had seen, at St. Petersburgh, some excellent specimens of street-paving, formed of wooden blocks, set with the grain upwards; and he speaks of it as "a mode of constructing roads in cities," not only new to Russia, but "*hitherto totally unknown in England.*" Mr. Heard has been misinformed. As far back as 1825, this mode of paving was recommended to the British public by Mr. John Finlayson, as particularly "suited to the streets of London and other great towns;" and one of the proofs he gave of its durability, was, that in the case of a causeway in Scotland, which had, for the purpose of experiment, been partly constructed of wooden blocks and partly of granite, the wooden portion was found, *after a lapse of twenty-four years*, to be much less worn than the granite. May we not, therefore, fairly conclude that the Russians have only availed themselves of Mr. Finlayson's suggestion?

Sennefelder, the inventor of the art of lithography, died on the 26th of February last, at Munich, in the 63d year of his age.

The Society (of Arts) has been informed by one of its members, that he has known an iron steam-boiler, using Thames water, preserved in constant use for seventeen years, by cleaning it often, and smearing the inside with oil or tallow after each cleaning. Another method of producing the same effect has been pointed out to the Society by Mr. James Bedford, of Leeds, druggist. He put into a large steam-boiler between two and three gallons of sperm oil *foots*, and found that, after eight weeks' constant use, the deposit of crust was very small, compared to what it used to be, from the same water alone; and also that the crust could be cleared off by means of a common stiff broom. —*Trans.* 1833. Part II.

In the Mysore country, towards the close of Tippoo Sultan's reign, a mass of ice fell near Seringapatam of the size of an "elephant." This fact is recorded and well authenticated, and the late Colonel Mark Wilks informed me, in the presence of Mr. Anthony Dunlop, at his residence, in the Isle of Man, that he investigated the circumstance on the spot, where it was authenticated by the independent testimony of many individuals then alive, and who distinctly remembered the extraordinary occurrence.—*Murray's Observations on the Phenomena of the Thunder Storm.*

The Greenwich Railway Act declares that no more than certain specified rates of fare shall be exacted for the conveyance of goods and passengers, but does not limit in any other way the profits that may be derived by the shareholders. C. L. complains of this as unfair. "Why," he asks, "should the proprietors of the Liverpool and Manchester line be restricted to a dividend of ten per cent. and those of the Greenwich be at liberty to divide ten times ten?" We cannot, certainly, suggest any satisfactory reason; and are persuaded, that had the attention of Parliament been drawn to the circumstance, so important an advantage over other railway speculators would not have been conceded to the Greenwich proprietary.

The projectors of the London, Holyhead, and Liverpool Steam Coach and Road Company, now give out that it is not their intention to go to Parliament "during the present session." Have the shares, then, not been subscribed for? Oh! shrewd public!

The *Oxford Journal* states, that "the advance in the price of wool has put all the farmers on the alert who have any of that article to sell and sheep are actually travelling to London in *flannel waistcoats*, having been previously shorn for the sake of the wool, and covered up with flannel to prevent their flesh from being injured by the exposure to the cold." What next?

The Weaving of Straw in the hand-loom is stated to have been accomplished by Messrs. J. and A. Muir, straw-hat manufacturers, Greenock. It is expected that this process will entirely supersede the old mode of plaiting; and that, ere long, ladies straw bonnets will exhibit as endless a variety of figures as the Edinburgh and Paisley shawls.

The Queen of Spain has, by a recent decree, declared that all artisans are qualified to obtain municipal offices, titles of nobility, and places under government; for, says the preamble of the decree, very sensibly—"they serve the country in their station, and their trade, whatever it may be, should not be an obstacle to them."

A native of Fars some time ago made a considerable improvement in the manufacture of porcelain. His fame quickly spread till it reached the court, when the king immediately dispatched an order, commanding him to repair to Teheran, to make china for the shah (of Persia). Now the poor fellow knew, that once there he would have to make china not only for the shah, but for all the shah's officers and courtiers, and that too without the hope of any payment, unless it might be an occasional good beating. Seized with consternation he collected as large a sum as possible, and presenting it by way of bribe to the minister, besought him to report that he was not the man who made the china, but that the real potter had run away. The business was managed according to his wish, and he returned pennyless to his own country, vowing never again to make a bit of china, nor to attempt an improvement of any sort as long as he lived.—*Fraser's Persia.*

The feeling of the House of Commons, on the subject of railways, was remarkably exemplified this week on the second reading of the Great Western Railway Bill, which, though powerfully opposed, was carried by a majority of 182 to 92. The title of "Great Western," by-the-by, is become somewhat inappropriate; since it is now merely proposed to carry the line, at the one end, from London to Reading, and at the other from Bristol to Bath.

A lock of a very novel and ingenious construction has been invented by one of our correspondents, who is desirous of meeting with some person who would join him in taking out a patent for it, and afterwards working it to advantage. His address may be obtained on application to the Editor.

We are obliged, from want of [illegible] to defer Mr. Badnall's last communication [illegible] week.

Communications received from Mr. Woodhouse—Viator—Mr. Jopling—Enort—A Lover of Practical Science—A. B.—Mr. Pearson—A Wettle—Mr. Whitelaw—Mr. Denis—Mr. G. B. Hamilton.

Errata.

In the astronomical problem, p. 335, l. 4, for "obtuse," read "oblate;" and l. 10, for "presiding," read "preceding."

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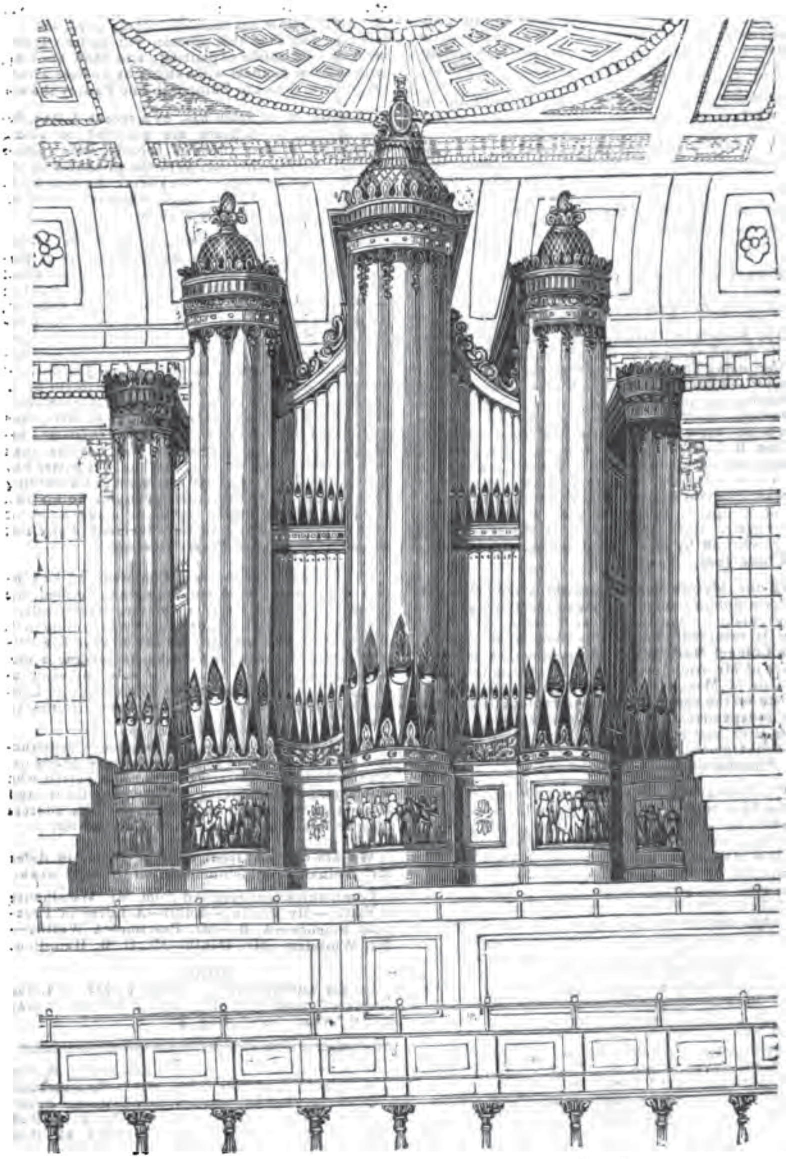
M. SALMON, Printer, Fleet-street.

Mechanics' Magazine,
MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 554. SATURDAY, MARCH 22, 1834. Price 2d.

THE BIRMINGHAM ORGAN.



Switzerland—or at least to Westmorland or Wales—in search of mountain-scenery, when, all the while, they had that tremendous range, the Middlesex mountains, under their very noses! What do foreigners mean, too, by praising the smoothness and evenness of English roads? Why, here we have a travelled Englishman, who assures us, that there is a mountain pass, some ten miles from London, "far steeper and more difficult than the one by which he once climbed over the Simplon into Italy!" Positively, the next number of the *Annual Tour* ought to be devoted to the wonders of the Edgeware-road! He must allow Colonel Macerone to look over his proofs, by-the-by, if he would not wish that gentleman to contradict every particular, as to his steamer, in the same moment that he quotes it in proof of its wonderful superiority over all others. Mr. Ritchie says, the carriage made a disagreeable noise.—No such thing, rejoins the party enjoying the *benefit* of his testimony—it made no noise at all. Mr. Ritchie calls it "a vehicle of enormous weight"—"Pooh!" exclaims the Colonel, "in a note," "'tis as light as a feather!" "It left a line of black smoke behind it," quoth the Middlesex mountaineer.—"Not a particle," responds the contradictory patentee; "a little waste steam, that's all." And then concludes with a "regular blow-up" of his valuable eye-witness, whose narration of a steam-trip of some couple of miles is so often and so complimentarily referred to.

What a thing it is that the roads have always been newly-gravelled just before the steamers begin to run—at least our worthy Colonel's steamer—for he insinuates that Sir Charles Dance, and every body but himself, always had better luck. All the stage-coachmen tell me the Edgeware-road is the worst bit of road in England. There must be *destiny* at work in this. We all know that when the Colonel went to Windsor the road became tremendously hilly "for that occasion only." And now, notwithstanding the immense sums which the commissioners have laid out in what they facetiously style "improvements" on the Edgeware-road, it is "the worst in England." Surely, the 1700-mile-journeys of the Colonel have had no share in producing this state of things?

One instance of Colonel Macerone's candour, and I have done. At the outset of his pamphlet he professes that he has printed "the *whole* correspondence," yet, *for reasons best known to himself*, he has thought proper to *omit* Mr. Hancock's letter, denying his broad assertion that he (Mr. Hancock) has "seldom been out without being brought home by horses;"—as well as the *second* letter (chiefly in reply to one of the Colonel's, *which he takes care to give at full length*), from, Sir, yours respectfully,

H.

London, March 13, 1834.

THE UNDULATING RAILWAY—A FEW WORDS IN BEHALF OF THE "MESSRS. STOKERS," IN ANSWER TO MR. CHEVERTON—BY MR. BADNALL.

Sir,—My gratification would be equal to that which Mr. Cheverton expresses in relinquishing his combat with me to grapple with my powerful and very able supporter, Kinclaven, could I, like him, be fortunate enough in meeting with an opponent over whom a victory would be a source of greater credit and honour; but as I am never likely to find a man of more established scientific reputation than Mr. Cheverton, bold enough to contend against the advantages which, in theory at least, the undulating railway presents over the horizontal one, I cannot be an inactive observer of the movements of so harsh an enemy.

Were his letter to Kinclaven (p. 316) confined to the subject of atmospheric resistance, I should have left him to enjoy the full pleasure of that new combat which is to decide his invincibility, or otherwise; or, at least, I should have patiently awaited the dreaded infliction of his rod, and his explanation of the causes of the extraordinary anomalies which, if his theory of resistance be correct, the various experiments of Mr. Nicholas Wood, as well as those tried on the Liverpool and Manchester railway, evinced on this subject. But, Sir, Mr. Cheverton has again thought it prudent to recapitulate those fallacious sentiments on the question of locomotive steam-force, on which his original arguments were founded; and, as I am not inclined to lose sight of these original arguments, as long as he is bold enough to defend them, I shall hope to be pardoned by your readers for recalling their attention to his earlier remarks.

Mr. Cheverton hopes that the Messrs. Stokers of the engines will admit various conclusions, many of which, if admitted, would form rather important data for his own erroneous opinions. Among these he observes:—

(1st.) "If your locomotive has gone ten miles an hour, and you now make her go with twice the velocity, you will admit that she will travel twenty miles in the next hour."

Admitted.

(2dly.) "And that, notwithstanding you have been about the job in only the same time to a minute, you have got rid of double the quantity of steam, for you have gone double the distance, or made double the quantity of strokes."

Denied—

Inasmuch as it is well known that a locomotive engine, when travelling at twenty miles per hour, *does not*, in equal times, consume twice the quantity of fuel which she consumes when travelling ten miles per hour; nor does she, although the number of strokes are doubled, expend a double quantity of steam. To elucidate this, we will suppose a safety-valve placed at the top of the cylinder as well as on the boiler; at starting, the steam shall blow off at each valve at a pressure of 50lbs. to the inch. Now, if the steam in the boiler be kept up to that pressure *at all speeds*, the steam in the cylinder would diminish in pressure or quantity as the velocity increased. Could then the requisite pressure on the cylinder-valve be measured at twenty miles per hour, it would be found that 25lbs. on the inch would be more than sufficient to prevent the blowing off.

(3dly.) "And some among you may comprehend, peradventure, that you have doubled the power of your engine by so doing."

Denied—

For it is a well-established fact that the effective power of a locomotive steam-engine decreases as the velocity of the engine increases.

(4thly.) "And, perhaps, it is not *quite* beyond your capacities to understand, that if, in addition to this double quick time, you hitch on what will make four times the load you had before, you will now make your engine do *eight times* the work she did before; and that she may now be considered of eighty horse-power, if she was ten horse-power before."

Prodigious!

The power of the locomotive engines on the Liverpool and Manchester railway may be fairly rated at 15 horse to 20 horse-power. The fullest effect of that power is employed at the lowest speeds; the more rapidly they travel, the less is the effective power; consequently, instead of the power of the engine being *octupled* under the circumstances to which Mr. Cheverton alludes, it decreases, as before observed, with every increase of velocity; and were not the decrease of friction, or total resistance throughout equal distances, greater than the decrease of power, the velocities which are now obtained could never be obtainable. Maximum velocity is therefore attained when the engine can command the least effective power, and at that particular period when there is the least friction or total resistance.

(5thly.) "Again, Messrs. Stokers of the engines, you will not stumble much at my meaning, when I tell you that, in going the twenty miles in the hour, you have emptied your cylinder just the same number of times as you would if you had gone the same distance at your old rate of ten miles an hour; that is, you have made no more than just the same number of strokes; but as you have some sort of suspicion that your steam must be four times stronger than it was before, by reason of the load being four times greater than before, or else that you must have a cylinder to contain four times more steam than before, you will not be greatly bothered when I tell you that you have spent but four times the steam you would have otherwise done."

Extraordinary!

The steam must be four times stronger than before, or the cylinder must contain four times more steam than before! For what? May I hope that Mr. Cheverton will, in good humour (or otherwise, if he prefer it), refer to the 4th paragraph which I have quoted, and, after perusing my remarks, explain the ambiguity by which the preceding observation is surrounded. Until he can do this—until, in accordance with his own theory of atmospheric resistance, it happens that a decreasing force (UNLIKE THE FORCE OF GRAVITY) produces an *increasing* velocity *until that force is* *equal to an opposite resistance,*—or until he prove that SUCH IS NOT THE CASE,—I must earnestly recommend your readers to "shake their heads" far more than either "four times" or "eight times" at the conclusions to which he would lead them; in the mean time, resting satisfied that they know "quite as much about the matter" as my friendly opponent, Mr. Cheverton.

As Kinclaven has done me the honour to take an important part in this discussion; and as Mr. Cheverton has expressed his wish to measure swords with him, I feel at present relieved from the necessity of noticing any further observations in reply to his late most erroneous calculations on atmospheric resistance, merely prognosticating that, when his mind is a little more divested of erroneous prejudices, he will be more inclined to measure theory by practical results.

In fairness to S. Y., I shall make some allusion to his last communication in my next. I am, &c.

RICH. BADNALL.

Manchester, Feb. 17, 1834.

[Having, for the sake of expediting this inconveniently protracted discussion, forwarded to Mr. Cheverton a copy of the preceding letter, in order that he might favour us with any remarks he might have to make upon it, in time for the publication of both together, he has sent us the following note.—ED. M. M.]

Sir,—When, in my last communication, I declined all further controversy with Mr. Badnall, I was not aware that you held in reserve a fresh attack from him, by way of diversion, in aid of my respectable opponent, Kinclaven. It requires me, therefore, to append a few words of explanation to these really very amusing remarks of Mr. Badnall, in order to settle up and fairly balance accounts between us.

As he has taken some pains to show that "the more rapidly the locomotives travel, the less is the effective power," he must, I suppose, really imagine that those observations of mine, which he quotes, contain something opposed to that opinion; and yet he might have found passages in my former communications which asserted the same thing. I do not dispute it—it may be as he intimates, that the steam is very much wire-drawn. I can imagine, though I will not give credit to such a libel, except on better authority than Mr. Badnall's, that the construction is so bad, that an engine may be unable to give any assistance to a train when urged by another more powerful locomotive, or to display any better efficiency than a useless expenditure of its steam. Be that as it may, it does not concern my argument in the least, for it is founded on the *supposition* of certain effects *being* produced, and of the cause *being* adequate to their production; and then it states that such cause must be proportional either to the square of the velocity or the cube of the velocity, according as it is stated in terms of the time or the distance; and this argument, or conclusion rather, Mr. Badnall may controvert, if he chooses, but, simple as it is, it will be as well for him to understand it first. I congratulate Kinclaven on the valuable aid which has so officiously flown to his assistance.

Whether the *same* locomotive engine could, by a little contrivance, be *made* to undergo equal variations of velocity and development of power which the argument supposes, I do not know, nor is it necessary to know. Mr. Badnall says it cannot; perhaps not, and the argument does not require it, but allows one to imagine the introduction of another engine if needful, as Mr. Badnall must have been well aware, for he has himself quoted these words of mine—"or else you must have a cylinder to contain four times more steam than before." The stokers themselves would be ashamed of such quibbling.

I am truly sorry to occupy your pages with rebutting such petty cavils, but I will sin no more. Yours, &c.

BENJ. CHEVERTON.

P. S.—Kinclaven will perceive that my last reply to him contained an answer, not only to the first part, but by anticipation to the second part also, of the observations with which he favoured me; I therefore wait his further remarks.

WELSH SLATE THE FITTEST MATERIAL FOR SEPULCHRAL MEMORIALS.

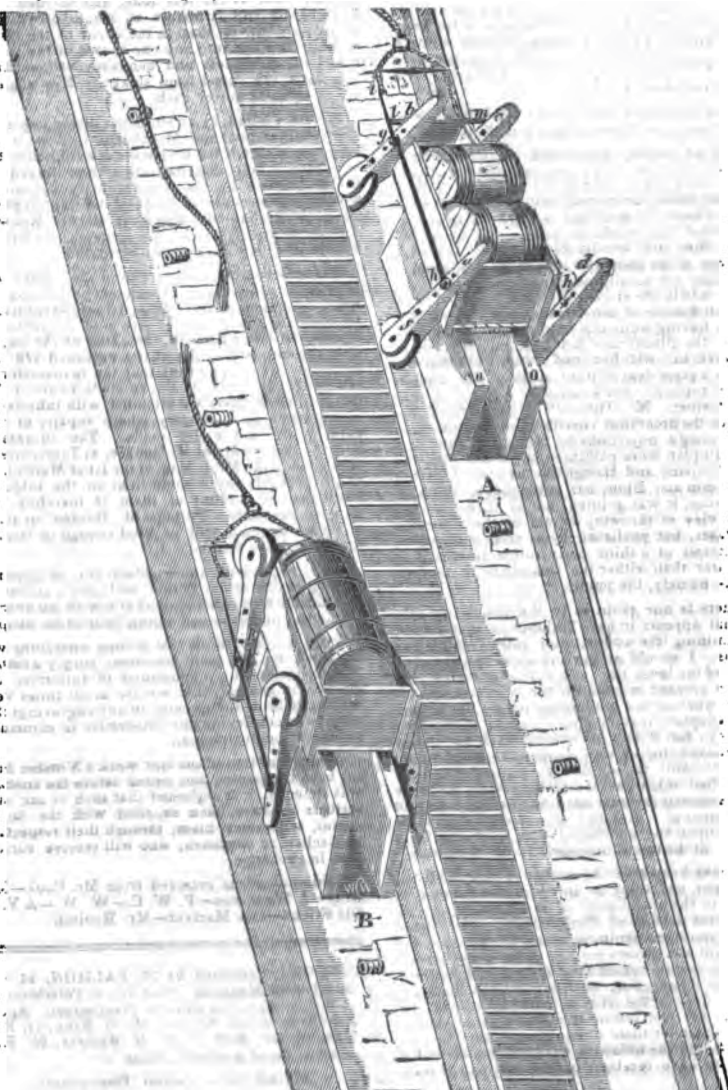
Sir,—In your Number of the 25th January last, under the signature "Enert," appears a communication "On the Fragility of Modern Gravestones." I perfectly agree with your correspondent as to the frailty of the memorials raised over the remains of the dead in the burial-places in and about London.

Mechanics' Magazine,

MUSEUM, REGISTER, JOURNAL, AND GAZETTE.

No. 555. SATURDAY, MARCH 29, 1834. Price 6d.

LADDER-HILL RAILWAY, ST. HELENA.



THE UNDULATING RAILWAY.

Sir,—It has been said, that Buonaparte remarked to one of his Generals at the battle of Waterloo, that British soldiers did not know when they were beaten. The same observation, I am inclined to believe, may with equal justice be applied to some of our British men of science. Indeed, I have on more than one occasion seen a display of this in the Mechanics' Magazine, when the defeated party, although beaten to a mummy, "would argue still." I am afraid that my scientific opponent, Mr. Cheverton, holds a commission in this hardy corps of veterans; for, in defiance of all reasoning, whether mathematical or physical, he still maintains the perfect soundness of his *own newly-discovered theory of atmospheric resistance.* Nay, more, he has given us (No. 548) something in the shape of a demonstration of his theory, (an Irish demonstration, G. S. would no doubt call it,) which he probably fancies we ought to swallow without examination. But, Mr. Editor, I must honestly confess that though I have examined it well, I do not understand one word of it. It is in fact as unintelligible to me, as Mr. Badnall's Greek quotation was to him. (See No. 552, page 372.) However, if his demonstration shall be found to be orthodox, by those who can understand it, then of necessity I must be in error; and, if so, I suppose no one can blame me if I contrive to *hitch* the saddle upon the right horse, and throw the whole blame on old Mr. Euclid. Well, let us to this precious gear then. Assume $\frac{60}{h\,e}=$ P (See Nos. 547 & 48) then by Mr. Cheverton's new theory, a^2 P V^3 and b^2 P v^3 will express the atmospheric resistances in horses' power. Now Mr. Cheverton (kindly) allows, that when $a^2=b^2$ the four quantities a^2 P V^3, b^2 P v^3, V^3 and v^3 are in proportion; but although the two first terms express the ratio of the resistances (this he also admits), he *denies* that the two last terms express the same ratio!!! What a blundering old fool, then Euclid must have been; for he positively asserts, aye and pretends to demonstrate too, that *ratios which are the same to the same ratio, are the same to one another.* (Book 5, prop. 11.) Now Mr. Editor, I need scarcely inform you, that the proof of the 11th proposition, solely depends upon def. 5 and 5. Indeed, I may add, that the whole of the 5th book does. It therefore follows, as sure as the sun will rise to-morrow, that if Mr. C. has succeeded in establishing the truth of his proposition, the whole of the 5th book is nothing else but a heap of fudge. Q. E. D.

Cor. 1. Since the 6th book depends upon the 5th, it must share the same fate: that is, in short, there is no truth whatever in geometry.

Cor. 2. Arithmeticians are wrong when they assert that $\frac{6}{12}=\frac{1}{2}$; because it is not in accordance with the Chevertonian theory.

Mr. Cheverton's method of trying to account for the possibility of the carriage question with two horses, and 30 square feet of opposing surface, &c. is, I confess, strictly in conformity with his own theory. He says "there is nothing astonishing that (two) spirited horses should in a panic, or by violent incitement, be able to put forth for a short *time* (a precious short time no doubt,) three or four times *it may be* (or it may not be), the energy which they exert in the ordinary routine of labour, &c."

I have no objection to Mr. C.'s application of the terms *panic, incitement,* or *energy*; but with regard to the "short time," that I deny. The time was thirty-six minutes, and the distance gone over twelve miles. Well, surely, one would require to swallow *credulity* in pailfuls before he could force himself to believe that two horses, galloping at the rate of twenty miles per hour, could, in addition to that velocity, overcome a resistance which would require three horses to perform at a dead pull. How he can reconcile such a doctrine with prop. 9, chap. 11, vol. 3, of Dr. Hutton's Course (a chapter which he has drawn many of his resources from, although he ought to have taken his position a few steps further down the ladder), I cannot conceive, unless he has become an apostate to the belief of mechanical philosophy as well as geometry, for both equally declare the unsoundness of his theory. In the proposition above alluded to, it is stated that the maximum action of a horse is when his velocity is $2\frac{1}{2}$ miles per hour, and the load $\frac{4}{9}$ of what he can move at a dead pull. Ah, Charles Hutton! you must have been as great an impostor as Euclid was. It is true, Mr. Cheverton has in some degree prepared us for his sudden apostacy; see Number 545, pages 260 and 261; also compare the same with the postscript to his article 524, and Number 548, page 318, where the reader will find some other amusing specimens of his consistency.

The truth is, Mr. C. is a mathematician among mere practical men, but only a practical man among mathematicians. He is a smooth writer; his sentences are well arranged, and read well; but with regard to much of his reasoning, whatever value it may have in the eyes of metaphysicians, it can have but little in those of men of science. A person so ignorant as he manifestly is of the art of swimming, should not be so fond of plunging into deep water.

As I now despair of bringing Mr. Cheverton *mathematically* to a right understanding of the question at issue, I will, with your leave, Mr. Editor, change my tactics, and endeavour to bring my antagonist at once to a *practical trial of strength*. Mr. C. is, we know, acquainted with the common rules of arithmetic; common sense, too, he, no doubt, holds in a superlative degree; and he is evidently versed in the first principles of the laws of falling bodies. With these three requisite qualifications, if rightly applied, he will find no great difficulty in solving the following *practical undulating railway questions*:—

Suppose a carriage to move from a state of rest by the force of gravity down a regular inclined railway, the declination being one in 80; suppose that at the end of 12 seconds the distance gone over is 18 feet; and that at the end of 200 seconds, counting from the time of starting, the whole distance descended is 4,800 feet. It is required to determine, 1. How much of the velocity is destroyed by friction per second? 2. The whole amount of the resistance of the atmosphere? 3. How much of the velocity is destroyed by the joint operation of both causes? 4. The last acquired velocity? And, 5. What part of the time of descent is lost through the resistance of the atmosphere?

Again: suppose that at the bottom of the plane another plane of the same inclination is to be ascended with the last acquired velocity, and that the friction and resistance of the atmosphere are the same as in the case of the first plane: required, to determine the distance to which the carriage will have ascended when it comes to a stand still?

Or, supposing that at the bottom of the first plane the carriage were to move forward on a horizontal plane, what distance would it reach?

In all the three cases, let it be assumed that the resistance to motion produced by friction is in proportion to the times—which is well known to be consistent with the actual fact.

If Mr. C. will endeavour to solve the above questions, I am certain it will cure him of the erroneous opinions which he has imbibed regarding atmospheric resistance. But whether he does or not, I promise to give a solution of it.

I was very glad to find from your Notes and Notices, No. 551, that Drs. Dalton and Lardner are both favourably disposed towards Mr. Badnall's system. Dr. Dalton's opinion adds a tower of strength to the system; and Dr. Lardner, although not a practical engineer, is a profound mathematician, and, as such, his opinion must be held in high estimation. He states, "that he has never read any thing (and I happen to know that he reads the Mechanics' Magazine) which is conclusive against the system."

Mr. Cheverton, as a damper to the above, states (No. 552) that "at a late meeting of the Society of Civil Engineers, when Mr. Badnall's undulating railway was the topic of discussion, it was unanimously voted to be a fallacy!"

Mr. Cheverton does not, however, inform us who composed this meeting—he does not give us the name of a single engineer who was present. We may, if we choose, suppose that it was a hole-and-corner meeting, got up for the purpose, with Mr. Cheverton for Chairman, and a certain able auxiliary of his, of the corps of "*Young Engineers*," as S (ecretar) Y.

I am, Sir, yours, &c.

KINCLAVEN.

March 20, 1834.

Sir,—In my general reply to Mr. Badnall, I made some remarks upon his assertion, that, at 35 miles an hour, a man standing on the first carriage could draw up to himself a whole train of loaded waggons; and misled by his own experiments, which were made, he says, with a weight of 164 tons, I assumed such train to be 150 tons;-but it is probable that he alluded to the lighter loads carried on the Liverpool and Manchester Railway, I beg, therefore, to correct my argument by taking the *average* number of tons per trip, and to re-state it thus:—According to Mr. Badnall, the draught of a train of loaded waggons, say 60 tons, is not on a level more than equivalent to a man's strength, which, in the posture alluded to, may be taken at one cwt. Now this is little less than a miracle which THE BADNALL announces—for it makes the resistance from friction, let alone that of the air, to be the twelve-hundredth, instead of the two-hundredth part of the weight!!

Yours, &c.
BENJAMIN CHEVERTON.

THE INDETERMINABLE CASE IN LAND-MEASURING (p. 366).

Sir,—Your correspondent Agricola has proposed a geometrical problem, which, he states, is indeterminable from the given conditions. It certainly is so, as there are only *three* conditions given to construct the quadrilateral; whereas it is well known that to do so, *five* things must be given. From the given conditions, however, Agricola expects the problem can be solved, when the area of the quadrilateral is a maximum or a minimum.—Now, when the area is a maximum, the contents will be equal to half the rectangle contained by the diagonals, whether the figure can be inscribed in a circle or not. But in no case will the problem admit of a minimum.—However, I have endeavoured to patch up a problem which admits of a simple and elegant construction from Agricola's, which I shall be glad to see solved by him, or any other of your contributors. The problem is this: to inscribe a plane quadrilateral figure in a given circle, having given each of the diagonals and the area.

I am, yours, &c.
IVER MACIVER.

Pimlico, March 10, 1834.

P.S.—Pray, Mr. Editor, when do you intend bringing the undulating railway question to a termination? I had a notion some months ago of grappling with some of Mr. Badnall's scientific opponents, but I am glad to find that the subject has been taken up by an abler hand.—I. M.

GRAY'S RAILWAY SYSTEM.

"In 1811, Mr. Blenkinsop, of the Middleton colliery, near Leeds, took out a patent for the application of a rack or toothed rail, into which wheels turned by the engine were to work, and thus produce a progressive motion; and shortly afterwards he erected some engines on the Middleton colliery railroad. They worked well, and to this day they continue to work well. Indeed, there never could or ought to have been any doubt on the subject. For the conveyance of great weights, the rack-rail must manifestly have an immense advantage over every description of plain rail: no gripe, by mere adhesion of surfaces, can ever match the gripe of a good set of teeth.

"Mr. Gray, of Nottingham, has strongly advocated the adoption of a mixed railway-system—plain rails for speed, and toothed rails for heavy weights; and we incline to the opinion, that when economy comes to be more regarded by railway proprietors (as it will be ere long), this is the system which will be found most advantageous in practice."—Mech. Mag. vol. xv. p. 190.

Sir,—As common sense, honesty, and truth, must and ever will prevail over duplicity, fraud, and falsehood, so do I hope that plain, practical knowledge will ultimately supersede the vain attempts of those delusive theorists who have lately taken the field, in your Magazine and the public papers, to introduce an undulating railway scheme, backed and supported as it is by mathematicians, and the irresistible power of their figures! Common sense and daily practice might furnish proofs innumerable to show that, for the purposes of general trade—the daily accommodation of passengers—and the conveyance, also daily, of thousands upon thousands of tons of merchandise on lines of railway in each direction of traffic, the most simple machinery, and the perfectly level plane, or the nearest possible level which can be obtained, are the fundamental and only true principles in the construction of railways.

Had the Liverpool and Manchester railway been agreeably to my humble suggestions—toothed rails for heavily laden waggons, and plain rails for light carriages, which systems may be united in the same rails—the proprietors of the above mentioned railway would have saved little short of 20,000*l.* annually.

With respect to running steam carriages on the common turnpike-road, the idea is obviously preposterous; and how it can now be listened to or tolerated *savours of something more than ignorance*: the sad experience of late adventurous, but mere fallen *engineers*, supported too as they were by mathematicians, seems rather paradoxical! How ignorant must that individual be of locomotive steam machinery, who dares venture even to hope for success from such attempts; and, how painful is it to see innocent individuals becoming dupes to such visionary speculations! Alas, poor John Bull!

Every locomotive steam-engine will always, under all circumstances, effect *ten times* more on railways, and at a greater speed, than on the common turnpike-road.

Will you, Sir, be pleased to give me room in your Magazine, and also to recollect that I am your first correspondent on this subject.

I am, Sir,

Very respectfully,

Your obedient servant,

THOMAS GRAY.

No. 143, Fore Street Hill,
Exeter, March 12, 1834.

THE MUSCULAR FORCE EXERTED IN ARCHERY.

Sir,—If your correspondent A. M. (p. 366) will tie a line to the centre of the bow-line, and another to the centre of the bow-stem, and pass these two lines over two small pulleys at each end of a deal table, leaving the bow to rest thereon, he will discover that, instead of two 25lbs., he will require two 50lbs. (allowing a little more for the friction of the pulleys) to affect the bow-line to the extent he speaks of; consequently, a separative force of two 50lbs. will be required in counter directions.

I am, Sir,

Yours, respectfully,

J. W.

"PRACTICAL ADVICE TO EMIGRANTS."*

The press has of late been singularly prolific of works on emigration; but considering how very many thousands of our fellow-subjects have during the last three or four years been driven by the difficulty of thriving at home to push their fortunes abroad, it is reasonable to presume that the supply of "Guides," "Directories," "Companions," "Pocket-books," &c. &c. has been no more than commensurate with the demand. We wish we could say as much for the quality as the quantity of these literary wares; but with not a great many exceptions, some of the more remarkable of which† have had their due meed of praise at our hands, they have been in general of a very spurious and trashy description—either got up for the purpose of serving particular local interests, with the most perfect indifference imaginable to truth, or put together with the help of scissors and paste, to serve no other purpose than to bring money into the pockets of the putters-together and their employers.

The present emigrating season has produced a new volume of "Practical Advice to Emigrants," which appears to us to have strong claims to be added to the list of honourable exceptions. It is not free from the suspicion of having been compiled in subserviency to the interests of a particular commercial speculation, for it has for its avowed object to turn the emigration from the mother country into a new channel—namely, the portion of the eastern townships of Lower Canada, lately sold by Government to the "British American Land Company;" and the author speaks of his "particular acquaintance" with "the operations" of that Company in a way which leaves little room for doubt that he is a shareholder—perhaps *a Director*—of it. But there is, at the same time, so little of the odour of partisanship about it—it is writ-

* "Practical Advice to Emigrants on all Points connected with their Comfort and Economy, from making Choice of a Ship to Setting on and Cropping a Farm."—120 pp. 8vo. E. Wilson, 1834.

† We do not say *all*, for there is at least one as entitled to notice as any, which has, somehow or other, never reached our library table. We allude to the "Hints" of that admirable writer and true friend of his country, Martin Doyle, which has been pronounced by good judges to be one of the ablest of his many able productions, and has sold (we believe) better than any other work on emigration which has yet appeared.

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TO

THE TWENTIETH VOLUME.

N.

O.

P.

T.

END OF THE TWENTIETH VOLUME.

Printed by M. Salmon, Crown-court, 72. Fleet-street.

Ashenhurst Hall near Leek, Staffs.
Home of Richard Badnall jnr.1825

Highfield Hall, Leek, Staffordshire -1820

Highfield Hall was the home of Richard Badnall Snr. and where Richard Badnall Jnr. was brought up

A Wild Ride on Badnall's Famous Undulating Railway

By Joseph C. Meredith

If there is a special heaven for frustrated inventors, a place where everything works, surely the soul of Richard Badnall is there, riding around on the kind of railroad he envisaged for us all, designed according to his "undulating principle." Badnall lived in the springtime of the industrial revolution and its most exciting development: the use of steam power for land transport—not along country roads (unreliable) but along rails, and not by the use of relays of stationary engines pulling on cables (awkward) but by the use of moving engines to pull trains of cars wherever the rails might lead.

Men and animals had been pulling heavy loads along rails of wood and iron for centuries, and even the idea of steam propulsion was not exactly new. As early as 1641 the Frenchman Solomon de Caus declared that steam could propel carriages and even ships. He wrote a book about it, only to be locked up as a madman by Cardinal Richelieu.[1] In 1769, James Watt patented his steam engine, and in 1804 Oliver Evans invented a self-propelled vehicle. Others followed, with models, demonstrations, claims, and counterclaims, all groping toward a workable combination of machine and method. Such a combination was finally attained with the opening of the Liverpool & Manchester Railway (L&M) on September 15, 1830, a date that marked the beginning of the age of the railroad.

Richard Badnall was probably one of the ten thousand spectators at the grand opening, but he could claim no part in the triumph. Like many others,

Joseph C. Meredith served in World War II and Korea and in 1962 retired from naval service as a lieutenant commander. He then embarked on an academic career, earning advanced degrees in library and information science from the University of California and Indiana University and retiring for a second time in 1982 as a university professor at Governors State University in Illinois. Meredith is the author of a recent book from Naval Institute Press, *A Handful of Emeralds: On Patrol with the "Hanna" in the Postwar Pacific.*

he may have ridiculed the self-taught man of the coal pits who had brought it off—one George Stephenson, whose main qualification was that he could make things work.

Stephenson's genius lay in his ability to recognize a good idea and to adapt it to a particular purpose. His first locomotive, patented in 1815, was simply an improved version of one built by John Blenkinsop in 1811. In successive versions over the next fifteen years, Stephenson incorporated the tubular boiler, spring suspension, and the "steam blast," whereby exhaust steam was used to draw air through the firebox. When the directors of the newly organized L&M sponsored a competition in 1828, his four-ton *Jupiter* ran off with the prize of five hundred pounds.[2]

Stephenson saw the locomotive as part of a complete system, all parts of which must work well together. Experience convinced him that cast-iron rails would always be breaking under the weight of engines and cars, and that only wrought iron—in spite of its cost—would do. He also rejected the popular idea of cog-wheel traction and considered smooth wheels and smooth rails better suited to reliable long-distance hauling, even though they limited positive traction and required a nearly level roadbed. Accordingly, the L&M route as designed by Stephenson was a marvel of cuts, fills, tunnels, and bridges. It was also a marvel of costs, but these were quickly compensated by revenues.[3]

Railroad projects blossomed on every hand, but much remained to be done to make the trains safe and reliable, to carry more freight and passengers, and to carry them ever faster, even to 25 miles per hour as envisaged by Stephenson. There was plenty of room for improvement, and ideas poured forth for all kinds of devices—brakes, couplings, signals, whistles—all the paraphernalia of a completely new system of land transport. Some ideas were useful and some were ludicrous, but they all made copy for a new breed of technical journals, notably the *Mechanics' Magazine, Museum, Register, Journal, and Gazette,* all of London, and the *Mechanics' Magazine and Register of Inventions and Improvements,* of New York. Their innocent enthusiasm makes these journals great fun to read even today.

Among all these schemes, none was more fanciful or aroused more controversy than the one proclaimed by Richard Badnall.

Inspiration

Our hero was a gentleman of sufficient means to enable him to publish long treatises such as *A View of the Silk Trade, with Remarks on the Recent Measures of Government in Regard to That Branch of Manufacture* (London, 1828).[4] The trouble with Stephenson's railway, he decided, was that it was too level. Stephenson had called for gradients no more pronounced that 1 in 200, having found that his locomotives spun their wheels on anything steeper. So why not start the train so that it would first be moving downhill, Badnall reasoned, in order to gain speed to carry a train over an opposite rise, then down again, and up again, and so on? Would this not permit using lighter engines, and save wear and tear on the rails?

To test the idea, he fashioned a plank 4 feet long, concave on one side to a depth of 4 inches, and flat on the other. He rigged a wooden cylinder so that it could be pulled along either surface by a string run through a pulley at the end of the plank. At the other end of the string he hung a small weight. He then compared the length of time it took the cylinder to traverse the concave plane and the time it took on the flat plane, and found to his delight that the former was quicker by half, even when he propped up one end of the plank to simulate an overall rise.

Badnall believed he had discovered a new scientific principle. He constructed a model railroad on which to run a tiny clockwork engine and carriage, with two tracks—one horizontal and the other "serpentine," as he called it, dipping and rising every 2 feet along its 32-foot length. At first the little engine gave him some trouble and he had to send it back to its maker for a stronger spring. Satisfied, he packed the contraption off to London and put it on display at the Gallery of Practical Sciences and Works of Art, on Adelaide Street. He also entered a patent application covering his discovery.

Proclamation

Badnall then published a *Treatise on Railway Improvements, Explanatory of the Chief Difficulties and Inconveniences Which at Present Attend the General Adoption of Railways, and the Means by Which These Objections May Be Overcome: As Proved by a Series of Interesting Experiments to Which Are Added, Various Remarks on the Operation and Effect of Locomotive Power* (London, 1833). After an obligatory bow to George Stephenson, he plunges into a discussion of "the imperfections of the present mode of railway conveyance, . . . the difficulty of ascending inclined planes . . . and the excessive weight of locomotives all hindering further development."

The solution, he said, occurred to him on June 7, 1832, and he went on to explain it in a numbing series of formulae, tables, diagrams, and "additional remarks and enumeration of advantages." The book ended with an appendix consisting of letters from "Mr. Rob't Stephenson Senior" to Badnall and to the *London Journal of Arts and Sciences, and Repertory of Patent Inventions.*[5]

Although the letters are not particularly relevant, the name of Stephenson was all that counted. Here Badnall may have deliberately obscured the fact that the Stephenson with whom he had the most contact was Robert. The Stephenson who had designed and built the L&M was *George,* the father. Robert had drafted much of his father's correspondence, however, and may have signed some of it himself. Badnall never bothered to explain that his only contact was with the son.

Badnall's treatise intrigued promoters, politicians, and armchair enthusiasts alike, because it promised a way around the major problems of cost and rights of way. The ensuing flood of letters, queries, and challenges amounted to a classic in the field of journalism, beginning as a stately gavotte, and ending—as we shall see—in a virtual free-for-all.

The first press notices were somewhat skeptical, and the March 1833 issue of the London *Mechanics' Magazine* carried a sarcastic piece by one "Junius Redivivus":

> I have been casually informed that there is exhibiting somewhere about town, a model of an undulating railway, whereby the inventor undertakes to convince the public that the notion of level surfaces being best adapted for wheeled carriages is entirely wrong; and, of course, if his position be correct, the road surveyors have wasted a "pretty amount of money" to make roads worse than they were before, by leveling the hills, which ought to be restored without delay.[6]

Clearly, he went on, undulation would produce no gain in power, "or of any thing but amusement." As for Badnall's claim to originality, the critic noted that from times past, the Russians had enjoyed winter fun on ice hills built on the Neva, which the French copied in a kind of roller coaster they called the *montagnes Russes*, set up on the Champs Elysées. In conclusion, the writer warned that "fallacies like the undulating railway tend to discredit all inventors as plotters of absurdities."

Badnall's response is a superb example of the haughty rejoinder addressed through an editor:

> Sir,—I should not have considered it worth my while to have noticed the letter contained in your last number . . . signed by "Junius Redivivus" had it not been accompanied by some remarks of your own, which I feel it necessary to reply to.[7]

Badnall trusts that the editor will do him justice. He has indeed invented and patented the undulating railway and has exhibited models of it in London and Manchester, "which engaged the anxious attention of some of the most scientific men in this kingdom." He invites the editor (not Junius) to see his engine perform at the Adelaide Street Gallery.

The editor then noted that not having yet seen the model or received a copy of Mr. Badnall's "pamphlet," he will not venture an opinion either way, even though the author's talents, information, and experience seem to "shake his incredulity" in the matter. Somewhere amid the courtly phrases lay a hint of derision.

The next letter came from Benjamin Cheverton, chiding Junius Redivivus for arguing beside the question and for failing to detect the error of the experiments. Cheverton then labored to explain the experiments' flaws, so that Badnall and his friends would not "allow themselves to be deceived, to their bitter cost."

All three communications—from "Junius Redivivus," from Badnall, and from Cheverton—were reprinted in the October 1833 issue of the New York *Mechanics' Magazine,* a younger, livelier version of its British counterpart. Publisher D. Kimball Minor filled its pages with spirited articles on mechanics, chemistry, agriculture, and natural philosophy (such as "Mallet's Plan for Cooking by Gas Flame" and "C. H. McCormick's Self-sharpening Horizontal Plough"), lavishly illustrated with wood-block engravings and nicely printed on a modern press. Wisely, Minor refrained from passing judgment on even the most chancy schemes and contraptions, preferring to let time and trial determine their worth.

Journals in those days freely reprinted each other's material, so that Minor's evenhanded coverage of the Badnall affair was probably as comprehensive as any. Of course the vagaries of postal service jumbled the sequence, and sometimes a reply appears in one issue before we encounter the item that inspired it, in the next.

Retorts Courteous and Discourteous

In a second letter, dated April 22, 1833, "Junius" expressed his resentment of Cheverton's calling him an "unpracticed thinker" whose views were "incomplete and superficial." As for Badnall ("who seems sore with me"), he declined to answer the questions posed, having "neither the time nor the inclination" to enter into the necessary calculations.[8] Soon afterward, having obtained a copy of Badnall's treatise, he fired off a third letter agreeing with Cheverton that the experiment was too crude to prove anything. Minor carried this letter in his next issue, along with a comment from "S. D." declaring that the amount of "friction" would not be any less on an undulating railway than on a horizontal one.[9]

"Friction" apparently meant more than just the resistance of surfaces rubbing together and was taken to include foot-pounds of work. Likewise "velocity" covered all aspects of motion—speed, momentum, acceleration, deceleration—making it easy to overlook how passengers might feel about riding on such a railway.

Demonstration

Badnall was not about to take any of this criticism without a fight. Somehow he persuaded George Stephenson's son Robert to arrange a full-scale trial of his theory, using an L&M engine and cars. The trail would be conducted on the "Sutton plane," a section of rail line near the town of Rainhill with a grade of 1 in 96.

Since the Sutton plane amounted only to half an undulation, Badnall planned to make it do double duty by comparing the times of ascent and descent. First, the train would be brought to top speed before beginning the ascent, and would be timed from a mark at the foot of the slope to the point where it slowed to a standstill at the top. The engine would then be reversed and would push the train down the incline. The time would be measured again for the same distance. If the descent took less time than the ascent, Badnall asserted, the soundness of his principle would be proved.

On the appointed day, several officials and other spectators gathered to watch the experiment. A train of thirteen cars weighing about 72 tons was moved into position about ¾ mile from the marker at the foot of the incline. The engine used was none other than Stephenson's *Rocket,* which had won the famous £500 competition. After getting off to a good start, the engine, the cars, and all finally clattered to a stop 278 yards up the slope, clocking 90 seconds from the marker to that point. The trip back downhill took 50 seconds, indicating that the train acquired enough speed to have carried it to an even greater height in an opposite ascent, had their been one. Badnall was delighted, repeated the test twice, and averaged the results: 81 seconds up, 46 seconds down. No one bothered to measure the time it took the train to reach top speed from a standing start on the level.

A few days later, Badnall and his supporters gathered to conduct additional tests. The crowd this time included nine observers sent over by the French government. The train was twice as long and twice as heavy, and it started a full mile down the track and was brought up to speed by two engines, *Firefly* pulling, *Pluto* pushing. When it reached the foot of the incline, traveling at about 19 miles per hour, *Pluto* dropped away and *Firefly* pulled it on up the slope before coming to a stop 575 yards from the marker, in 1 minute 56 seconds. The trip down took only 14 seconds. The second and third trials produced similar results. For the fourth and most curious test, *Firefly* shut off her steam at the beginning of the ascent, coasted with the train up the slope in 70 seconds, then pushed the train down in 66 seconds.

Almost everyone was impressed. John Knight, the newly hired editor of the New York *Mechanics' Magazine,* was enthusiastic:

> The experiments undoubtedly proved two most important facts, not only that a locomotive can convey, on an undulating line, double the load which it is capable of conveying at the same velocity on a level, *but that it can accomplish this* by the employment of *only half its power*. . . . Admitting the possibility that the use of steam may ultimately be superseded by this plan, the immense saving which would be accomplished in fuel, carriages, machinery, &c. fills an amazing gap in the contemplation, and would be sufficient to counterbalance any attendant disadvantages. Among the principal of these would be the additional capital and labor required for the construction of such a railway, in which a level tract of country, so important a desideratum under the present method, would present one of the most formidable obstacles.[10]

In other words, where no hills existed, railroads would have to create them. Was he joking? Apparently not, as his report had none of the usual rib-poking signals of contemporary satire.

A month later came Badnall's own report, conveying the same information and prophesying that the public, in spite of earlier prejudice and erroneous opinions, would soon acknowledge, appreciate, and benefit from the undulating principle. The same issue carried an endorsement from an American reader, A. Canfield, of Paterson, New Jersey, predicting that the invention would prove to be one of the most substantial improvements ever made to railroads.[11]

In the March issue, however, came a dose of common sense from "a Civil Engineer" of Albany, New York:

> Mr. Editor,—You must pardon me—my patience is exhausted—I can no longer look on and see your respectable and useful paper countenancing an absurdity which none but the blindest species of infatuation (that of an inventor for his favorite project) can support after a moment's serious reflection. . . . I allude to Mr. Badnall's "undulating railway", and *especially to his late experiments on the Liverpool road,* which, it would seem, were seriously witnessed by Mr. Stephenson, the Engineer of that great work.[12]

He went on to say that the trials proved something that needed no proof—that a locomotive could push a train downhill faster than it could pull it uphill. Without bothering to disguise his low opinion of Badnall, he wondered if the English and French seriously approved of all this, or did they smile and shake their heads?

From other letters, it seems there was already a good deal of head-shaking going on, and Knight may have wished that he had shaken his own instead of heralding the trials as a triumph of genius. Oddly enough, English commentators continued to pick over details that Badnall had no trouble dealing with, such as the unevenness of the gradient at Sutton plane, the effect of diameter on locomotive drive wheels, the measurement of speed by counting piston strokes, and so on. But at length the London *Mechanics' Magazine* carried a letter by Benjamin Cheverton, who came down hard on the whole scheme. Badnall was goaded into a reply:

> Mr. Cheverton in a most unwarrantable manner accuses me of withholding the truth when I have the power of publishing it, which truth *if exposed* would (he says) *prove* the poverty of my scheme. . . . He endeavors to sweeten this bitter observation by saying "I cannot believe there is any intention to deceive, *yet it suits his* (Mr. Badnall's) purpose.". . . I am not one, Mr. Editor, who feels disposed to quibble about trifles, or, in discussions of this kind, to be disturbed by every burst of anger from an opponent whom, in this instance, *I feel within my grasp*, but I offer my unqualified protest against the right or propriety of any man attributing unjust motives to another, without a cause which he is unable to substantiate.[13]

Along with more words in the same vein, Badnall begged Cheverton to exempt Mr. Stephenson and other engineers from his remarks, which was a neat way of dragging them in as supporters.

But even among those partly convinced by the demonstrations, there grew a sense of distrust of this man who pressed his claims with such fervor. One wrote suggesting that Badnall had too much of the "man of the world" in his composition, that is, that he was a crook. Many, on the other hand, continued to support the inventor, since the promotion of new lines had become a business in itself, and Badnall's theory could sometimes be turned to advantage. The debate overflowed into newspapers like the *Manchester Guardian* and the *Liverpool Mercury*. Letters signed "Saxulus," "Champion," "S. Y.," "Professor Crackwell," and "Friend" abounded.

Badnall Rampant

Amid the welter, Badnall produced his masterpiece, a three-thousand-word polemic that rose like a rococo fountain among the squirts and dribbles of his enemies. He had something for everyone: injured virtue, manly fortitude, patience toward the uninstructed, humility, pathos, anger, pity, and contempt. He described Cheverton as a clever man:

> but a clever man occasionally errs; and never is he more likely to do so than when inflated with that unhappy quantity of combustible matter,—vulgar abuse, self-sufficiency, and extreme vanity,—which have been so conspicuously displayed

> in the disjointed lectures which Mr. Cheverton has directed to me on this subject. . . . Unless we draw in our horns, the undulating controversy will not only become sickening, but, judging from Mr. Cheverton's last letter, disgusting.[14]

He then veered off to proclaim a new theory of air resistance: that instead of increasing with velocity it actually decreases, as shown by the tendency of all bodies "to rise from the surface of the earth when in rapid motion, such as in the flight of birds." He realizes that such a view is diametrically opposite to received opinion, "but so was the undulating railway!"

Sure of having overwhelmed his critics, he rushed on to predict that within a year, engineers and mathematicians would have an opportunity to make up their minds, and that from that time forward, "*we shall never have another level railway laid down in Great Britain.*" The L&M would always be a monument to British spirit, British perseverance, and British ingenuity, he added, but posterity would smile and exclaim, "Could you have believed it! They expended hundreds of thousands of pounds to make a railroad level!"

The implied slight to the L&M was Badnall's first real blunder, and its effect on the Stephensons and the directors of the L&M can well be imagined. Badnall ended the diatribe by promising more tests and announced that he would shortly publish a new treatise on railroads, with George Stephenson as coauthor.

Frustration

For his new tests, Badnall wanted to use part of a line being built between London and Birmingham under the supervision of the Stephensons. A rail link between the two cities had been proposed as early as 1826, but was not finally approved by Parliament until 1832, after noble landowners along the projected route had been amply paid to withdraw their protests.[15] Since the directors of the L&M were heavily committed to the undertaking, Badnall figured that they could be persuaded to save cut-and-fill costs by letting the new line freely undulate. He reckoned that the directors might overlook his critical remarks about the L&M if they could be convinced of an economic advantage. And so they were. At least they entertained his proposal that 10 miles of the road be built according to his specifications.

Badnall nevertheless failed to reckon as cannily on the Stephensons. Although the elder Stephenson had held Badnall's theory in silent contempt from the beginning, his son's cautious interest had enabled Badnall to snuggle up to the Stephenson name whenever it suited him. Robert apparently did bother to object. Badnall probably did not blame Robert for the flatness of the L&M; it was only his father's notion to have it run level.

Before responding to Badnall's proposal, the newly constituted London and Birmingham board of directors called on Robert Stephenson for an opinion. His report, duly rendered on May 5, 1834, spelled disaster.

Stephenson began by saying that he had been favorably impressed by the model exhibited at the Adelaide Street Gallery, and that he had paid special attention to the experiments made at Sutton plane. On careful consideration, however, he concluded that the apparent advantage in Badnall's scheme was illusory, because for one thing it made no account of the initial velocity. A train could get off to a better start going downhill, of course, but that could not always be arranged. What really mattered was the average performance over distance, and nothing in the formulae or the results showed any gain through undulating.

Turning to practical aspects, he asked what would happen if a train were forced to stop at the bottom of a dip. How would it manage to climb out of it? The only solution offered would be to work it back and forth until it got up enough speed to attain the next summit.

What about the locomotive, flailing downhill one minute and laboring uphill the next? Nothing could be more destructive to the engine. As the world's leading manufacturer of locomotives, Stephenson noted that although steam power was wonderfully flexible, locomotives performed best within a very narrow range of speed. Throughout the report, Stephenson knew just where to apply a very sharp chisel to weaken Badnall's case. At last, he brought down the entire argument, ending with, "No saving in power could by any possibility be effected."[16]

A man less resilient than Badnall would have given up. But Stephenson had left an opening, either because of some lingering doubt or perhaps wishing to bestow a little comfort. Whatever the reason, Stephenson said that a trial on some branch line might be worth considering, since the saving of first costs on such lines was "of paramount importance."

Badnall pounced on the remark. He wangled permission from the little Whiston Branch Railway to use a segment of track being laid out on the natural contours of the land to serve a local colliery. He then appealed to the directors of the L&M for the loan of one of their locomotives. They declined. Finally he located the *Manchester*. Built elsewhere than at Stephenson's Newcastle works, the engine was available—as Badnall would learn—possibly because it was virtually unfit for ordinary service. With customary fanfare, he announced a new series of tests on a truly undulating railway.

The *Manchester,* although said to be powerful, was also considered unsafe, with a regrettable habit of galloping off the rails at high speed, as had been shown by a recent mishap on the Sutton incline. The descent from the colliery would be twice as steep, and since the owners of the engine insisted that it be returned in good condition, Badnall faced a problem. His theory required that full power be applied downhill as well as on the upgrade, but he was warned that a solitary car had careened 30 miles an hour down that particular slope. It began to look as if he were arranging the most spectacular wreck of the decade.

With these doubts about the *Manchester*, he applied again to the L&M for a suitable engine, guaranteeing to pay for any damage, only to receive the following reply:

> Dear Sir:
>
> I submitted to the Board your renewed application for the loan of a locomotive engine for your proposed experiment on the Whiston Branch Railway, and am required to inform you that the Directors regret they cannot comply with your request. They gave the matter due consideration previous to their former decision, and they do not see reason to alter the determination they came to. I am, dear Sir, yours most obediently,
>
> H. T. Booth, Treasurer [17]

At last Badnall got the message.

So the test went ahead on October 24 with the *Manchester* alternately pulling and pushing a load of 80 tons back and forth through a dip in the line, first a distance of 760 yards, then 794 yards, and successively 824, 862, 900, 1,071, and 1,167 yards. By now the poor engine was in sad shape, "almost an encumbrance." The engineer was terrified and kept spoiling things by applying the brakes. The test had to be abandoned, having proven only that a locomotive could push a train.

Badnall took to his bed in October and was no more heard from until the end of the year, when he published a long letter blaming the *Manchester* ("from every point of view unsuitable") and hinting that H. T. Booth had earlier misled him. Castigating the Whiston Branch Railroad for not providing an accurate survey, Badnall apologized for not having proven his theory as promised. For the first time, he conceded that "for practical purposes a dip of about fifteen feet in a curve of 1,000 to 1,200 yards [i.e., 1:200 to 1:240] should seldom be exceeded." This was exactly the standard on which George Stephenson had insisted from the beginning.

Beyond Badnall

The notion of an undulating railway faltered to a stop. The New York *Mechanics' Magazine* reprinted Badnall's last letter and quietly let the whole matter drop, perhaps embarrassed at having ever credited the idea. Moreover, it was obvious to railroad builders in America that they had plenty of natural undulations to contend with, without creating artificial ones for the sake of a dubious theoretical advantage.

In fact, railroading in America quickly branched away from the English model. There were greater distances, hills, and rivers for the railroads to contend with. On the other hand, there was less local opposition calling for circuitous routing and fewer costly viaducts and bribes. American locomotives soon displaced English imports. Better traction was gained by doubling and tripling the number of drive wheels. When combined with greater power and weight and stronger rails, these measures went far to overcome the difficulties of terrain that gave American railroads their unique character.

Special conditions called for special fittings and equipment, and Yankee ideas for improved brakes, valves, couplings, wheels, carriages, bridge trusses, and rails spilled across the pages of Minor's *Mechanics' Magazine* and other technical journals. It was the age of the amateur inventor. Huge profits beckoned, and everyone got a hearing. The railroad was seen from the beginning as a harbinger of prosperity and growth, compensating for the deplorable state of American roads.

By 1836, the railroad mania was in full swing on both sides of the Atlantic. With new lines being planned wherever shares of stock might be sold, promoters in top hats and frock coats proclaimed glorious opportunities to eager audiences. Fortunately, the panic of 1837 nipped the American version of the mania in time. The *Mechanics' Magazine* passed through some hard times, but survived under the editorship of engineer George Schaeffer after Minor merged it with the *American Railroad Journal*. Minor himself sold out to his printer in 1839, only to resume control in 1843, and finally to sell out again in 1848 and depart for the California gold fields.

During this same time, the railroad fever in England continued unabated. A Parliamentary committee continued to control approvals for new lines, but proposals came in faster than they could be properly considered, while speculators traded merrily in the stock of projected roads. Prospectuses flowered with the names of the high and mighty. At one time, 157 members of Parliament were listed as subscribing to various issues, often for huge amounts.

Old George Stephenson would have nothing to do with these joint stock companies, but other engineers were not as fussy. One even sold his name for a thousand guineas. There grew a school of "fast engineers" who ridiculed Stephenson's conservatism and promised all kinds of innovations, such as the "pneumatic railway," which was to be powered by a piston in a long vacuum tube.

In 1845 Parliament approved construction of 2,883 miles of new railway, and in 1846 an additional 4,790 miles. More than 620 new lines were projected when at last the market in shares began to falter, and Parliament called for complete surveys of all proposed lines. Suddenly it was discovered that there were not enough surveyors in England to meet the deadline, and most of the projects went under. For Richard Badnall and his adherents, it was the end of the line. □

Notes

[1]Samuel Smiles, *The Life of George Stephenson, Railway Engineer*, 2nd ed. (London, 1857), pp. 60–61.

[2]Ibid., pp. 79–82, 136.

[3]The L&M carried 445,047 passengers and 99,374 tons of freight in its first full year of operation and regularly paid 10 percent on capital thereafter. Richard Tames, *The Transport Revolution in the Nineteenth Century, a Documentary Approach*, vol. 2, *Railways* (Oxford, 1970), pp. 15.

[4]Badnall's name appears in "List of Men of Science, Engineers, and Mechanics," in *Mechanics' Magazine* (London) (March 1831): v–viii, as follows: "Badnall, Richard, Esq., Silk Manufacturer, Inventor of several improvements in Silk Machinery, Author of 'A View of the Silk Trade,' and other Works."

[5]Merged in 1832 with *The London Journal of Arts, and Sciences; Containing Reports of All New Patents, with a Description of Their Respective Principles and Properties; Also Original Communications on Subjects Connected with Science and Philosophy, Particularly Such as Embrace the Most Recent Inventions as Applied to the Arts.*

[6]*Mechanics' Magazine* (New York) (October 1833): 208–11, reprinted from *Mechanics' Magazine* (London) (March 1833): 419–22.

[7]*Mechanics' Magazine* (New York) (October 1833): 211–12, reprinted from *Mechanics' Magazine* (London) (April 1833): 29–31.

[8]*Mechanics' Magazine* (New York) (November 1833): 265–68, reprinted from *Mechanics' Magazine* (London) (April 1833): 72–76.

[9]*Mechanics' Magazine* (New York) (December 1833): 317–18, reprinted from *Mechanics' Magazine* (London) (May 1833): 107–8.

[10]*Mechanics' Magazine* (New York) (January 1834): 27. Knight had been hired away from the London *Mechanics' Magazine* and probably witnessed the tests before leaving England.

[11]*Mechanics' Magazine* (New York) (February 1834): 67.

[12]*Mechanics' Magazine* (New York) (March 1834): 161–63.

[13]*Mechanics' Magazine* (New York) (March 1834): 163–64, reprinted from *Mechanics' Magazine* (London).

[14]*Mechanics' Magazine* (New York) (April 1834): 196–201, reprinted from *Mechanics' Magazine* (London) (November 1833).

[15]Smiles (n. 1 above), p. 321.

[16]*Mechanics' Magazine* (New York) (week ending 21 March 1835): 169–72, reprinted from *Journal of the Franklin Institute* (January 1835): 1–6.

[17]*Mechanics' Magazine* (New York) (May 1835): 257, reprinted from *Mechanics' Magazine* (London) 22: 253.

Richard Badnall Senior, father of the railway engineer, and a businessman and silk manufacturer
1770-1838

Extract from the:

American Railroad Journal and Advocate of Internal Improvements.

Saturday March 28, 1835 [Volume IV - No. 12]

[From the London Mechanics' Magazine.]

The Undulating Railway—Mr. Badnall in Explanation.

Dear Sir: Your readers might naturally expect that I should have taken a much earlier opportunity, either of publishing the particulars of those experiments to which I so urgently invited their attention some time ago, or that I should have given some satisfactory explanation of my motive for withholding them. I am not, however, without a hope that this letter will prove a sufficient vindication of my conduct against any charge of neglect or wilful delay.

In my letter of the 20th August last (1834,) I stated that the Whiston Branch Railway was completed, but that our experiments were unavoidably delayed by the refusal of the Directors of the Liverpool and Manchester Railway to accommodate us by the loan of a suitable locomotive engine. At the same lime, I intimated that, under such circumstances, I had no other resource than to endeavor to obtain an engine elsewhere, either on loan or hire.

With this view I made every necessary inquiry; but the only engine which I could hear of as being disengaged was the Manchester, which was buih by Messrs. Galloway, Bowman and Glasgow; and though capable of dragging a considerable load, she was, from her particular construction, by no means adapted to the safe attainment of that velocity which could alone determine, on the Whiston line, the comparative superiority of an undulating or horizontal railway. I say upon the Whiston line, because the dip of the undulation was greater than I should ever recommend in practice; which may be judged of from the fact, that a loaded waggon, descending from the colliery by gravity alone, attained, after traversing about 500 yards, a velocity of upwards of 30 miles per hour.

Perceiving, however, that I had little, if any other, chance of trying immediate experiments, I consulted my partner, Mr. R. Stephenson, sen., on the subject, who, from being well acquainted with the capabilities of the engine, strongly objected to her being employed for the purpose in question, it being his opinion that, although a very powerful engine, when in good repair, the Manchester could not be trusted at those velocities which, upon the Whiston undulation, it would be necessary to attain. Mr. George Stephenson was also of a similar opinion.

Thus situated, I resolved on making a second application to the Directors of the Liverpool and Manchester Railway, offering, at the same time, a guarantee for payment of any damage which the engine might sustain, and explaining the difficulty in which I was placed, by having publicly announced the trial of my experiments; which announcement I was induced to make in consequence of a verbal communication which I previously had with Mr. Booth, and from which I had formed, it appears, erroneously, an opinion that, if a *guarantee were* given, there would be no longer an objection to the loan of an engine being granted me. To this second application I received the following reply :

Railroad Office, Oct. 13, 1834.

"Dear Sir: I submitted to the Board your renewed application for the loan of a locomotive engine, for your proposed experiments on the Whiston Branch Railway, and am required to inform you that the Directors regret they cannot comply with your request. They gave the matter due consideration previous to their former decision, and they do not see reason to alter the determination they then came to. I am, dear Sir, yours most obediently,

H. T. BOOTH.

Under these circumstances, and finding how impossible it was for me to obtain a suitable engine, and feeling how deeply I had committed myself in your pages, by a declaration that the perion was arrived when the whole question should be determined without further delay, I was resolved to adopt the only means which were left me of even partially fulfilling my pledge to the public. On the 21st October, I therefore called on Messrs. Galloway and Co., and solicited the loan of the Manchester engine for a few experiments, under the promise that the maximum velocity attained should not exceed from 20 to 25 miles per hour. With a liberality for which I feel greatly indebted, Messrs. G. and Co. granted my request, merely requiring a guarantee that I would return it in as good condition as I received it. On examining the state of the engine, it was found that the pistons required repacking; but as we could not expect to come to any very decided result, (the velocity being limited,) it was thought unnecessary to make any alteration for a first trial. On the 24th October, Mr. Robert Stephenson, sen., and Mr. Gill, accompanied me, therefore, to Whiston; and the Railway Directors having accommodated us with empty waggons, we proceeded, after loading them with coal, to the trial of such experiments as we deemed compatible with safety, and likely to produce some data, whereon an opinion, pro. or con. could be formed.

It is necessary that I should here state, that the Whiston branch line was completed by Mr. McKenzie, under contract. That gentleman had previously given me a section, and although, upon that section, the summit levels were, no doubt, accurately defined, no intermediate levels had been denoted, either on the section or by stakes, on the ground itself; our only course, therefore, was to start from such parts of the descending line from the colliery, as would prevent our attaining a dangerous velocity, and to ascertain how far the train would rise on the opposite ascent; marking the starting and .resting points, until future levels could be taken. Had the inclinations been regular, this precaution would have been unnecessary, as the measurement of the distances would, in such case, have determined the elevations. But the Whiston line forming one extensive undulation, varying according to the surface of the land, no criterion could be formed from such calculation.

An accident, which some time ago occurred to the Manchester engine, which left the rails, when descending the Sutton inclined plane, had unfortunately given her the character of an unsafe engine at high velocities; and it was evident that, on the present occasion, both the engineer and fireman were afraid of her. We, however, made eight experiments; Mr. Stephenson, myself, or Mr. Gill, accompanying the engine-man on each occasion. Our load was 80 tons ; which, considering the condition of the engine, was an ample one, as she was proved to be only capable of drawing on that day about 15 tons up an inclination of 1 in 84, her steam being at from 40 to 50 lbs. on the inch pressure, though partially escaping between the piston and cylinder.

By these experiments, we were enabled to prove one very satisfactory circumstance— which was, that, notwithstanding the many disadvantages under which we labored, the train invariably rose to a higher summit than that from which she had previously started. For instance, in the first experiment, the train started from a given point, which was carefully denoted, and rose to a higher point on the opposite ascent, which was also denoted ; the total distance being 760 yards—time, 2' 35". The power then being reversed, the train rose 34 yards higher than it had originally started from; again being reversed, the train rose 64 yards higher than on the first experiment; total distance 862 ¼ yards, time 2' 26". The power being again reversed, the train rose 38 yards higher than before—total distance 900 yards; when again reversed, the train rose 171 yards higher than before—total distance 1071 yards; and on the next occasion the total distance was 1167 yards.

On the 7th trial, no accurate result could be deduced, as some of the brakes were on the waggon wheels.

On the 8th experiment, the steam was brought down to 10 lbs. upon the inch ; and I have every reason to believe, that when I am enabled to transmit to you a precise statement of the level from which it started and at which it rested, it will be found that the 80 tons were conveyed at that low pressure, very nearly, if not quite, from summit to summit.

Your readers will, I fear, be disappointed, and by no means satisfied with the rude statement which I now feel it imperative upon me to publish ; but until I have a better opportunity of deciding the full merits of the question, I can only put them in possession of facts as they really occurred— this I have done myself. In justice to myself, however, and to the cause which I have conscientiously advocated, and still continue to advocate, I ask them, what could I have done more? or what, under existing circumstances, can I do: All men of science must, I am persuaded, sympathise in my regret, that experiments of such a nature should be delayed, when such delay could be so easily avoided; and with regard to any chance of a satisfactory conclusion being come to, by employing the Manchester engine, I need only refer to Messrs. Galloway &. Co. themselves, who, I am sure, will hear testimony to her being in every point of view unsuitable to the purpose. She is, as before observed, an engine capable of drawing heavy loads at moderate velocities, but her many moving parts and general construction, render her altogether unfit for the trial in question. It.was my wish to have sent with this letter a correct section of the Whitson branch, with the exact distances denoted thereon, which the train traversed at our recent ex. periments; but severe indisposition, which has, since the end of October, with the exception of a few days, confined me to the house, has prevented my paying that at. tention to the subject which I should otherwise have done. Wishing, however, to have an impartial survey of the line made, I wrote to Mr. Hall, of Warrington, begging him to prepare the necessary sections, showing the various levels, &c.; but he has not yet been able to undertake it, owing to his time being entirely occupied in completing a survey of the Grand Junction line, to deposit for Parliament: when the section alluded to is completed, I will forward you a copy of it. As a proof of the unfitness of the Manchester engine, for the trial of experiments on the Whiston line, I need only remark, that over a great portion of each undulation, her power was not only ineffectual, but she was almost an incumbrance, owing to the loaded waggons attaining, by gravity, a greater velocity than she could effectually command in advance of them. This will easily be understood by those who consider the difference in the friction of a railway waggon, and a locomotive engine of her description. It also shows, that upon deep undulations, such as the Whitson line (where we have a fall of more than 30 feet in 500 yards), none but engines capable of sustaining an excess of speed above that which is produced by gravity, can be employed with/u/i *effect.* Such was my view when, in allusion to the Whitson line (see page 214, No. 516), I said, " *the full effect of practical experiment must depend upon the momentum acquired by the combined forces of gravity and steam being safely and effectually maintained down the descending line of each undulation."* For the same reason, also, I have before mentioned that, for general practical purposes, a dip of about 15 feet in a curve of 1,000 to 1,200 yards, would be the proportion I should most strongly recommend: though exceptions might, of course, be advantageously made, where the nature of the ground required it—especially in passing under or over cross-roads, canals, &,c., or where the inclined planes would not be so deep or so extensive as to produce a dangerous velocity without the employment of the brakes—considering, as I do, that velocity on railways is always dangerous when the engine (her full power being employed) is not able to keep up a dragging influence on the succeeding carriages, or, if behind the train, a propelling influence on the carriages in advance.

So convinced did I feel that no further impediment would be thrown in the way of a full and impartial trial of the undulating railway theory at Whiston, that at the meeting of the British Association at Edinburgh, in September last, I as publicly announced my intention of immediately bringing the question to issue, as I had previously done in your pages. I hope, therefore, I shall not be accused of any disrespect to your readers, to yourself, or to the public at large, for a breach of engagement which it has been entirely out of my power, for the present, to obviate. I am, Sir,

Yours most respectfully,

RICH. BADNALL.

Farm-hill, near Douglas, Isle-of-Man,
Dec. 31, 1834.

Badnall's Undulating Railway

from the book "The Engineer's And Mechanic's Encyclopaedia", by Luke Hebert (1849).

A very singular and interesting proposition has been made by Mr. Richard Badnall, for travelling upon undulating lines 6f railway in preference to straight or level lines, with the view of saving locomotive power, by the application of the natural force of gravity in the descent, so as to obtain a great momentum in making the succeeding ascent. His plan is best explained by himself in the specification of a patent, dated the 8th of September, 1832, which he obtained for that object.

"If a plummet suspended by a string, (as in Fig. 1 in the annexed engraving,) from the point *z*, be drawn away from the perpendicular line to the point *a*, and there let go, it will fall by its gravity to *b*, in the arc a *b*; but, in its falling, it will have acquired so much momentum, as will carry it forward up to a similar altitude at the point c.

Fig.1.

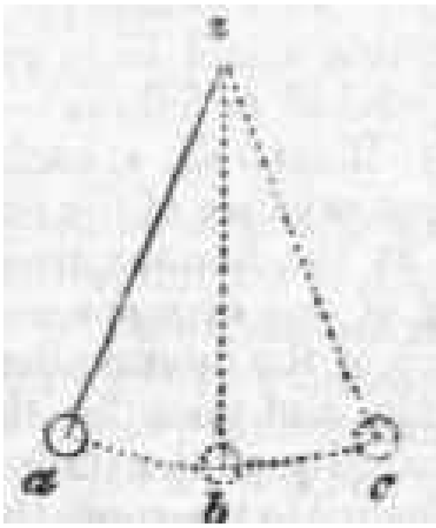


Fig. 2

"Let it be supposed that a line of rails, or tram-way for carriages, be so constructed from the summit of two hills, as Fig. 2, across a valley, that the descent from one hill, as *a*, to the valley *b*, shall subtend a similar angle from the horizontal line to the ascent up the other hill from *b* to *c*. Now if a train-waggon, as *d*, be placed at the summit of the declivity a, it will, by its gravity alone, run down the descending line of rails, to the lowest point *b*; but in so running, according to the principles of the oscillating pendulum, it should have acquired a momentum that would carry it forward without any additional force up the ascending line to the summit of the hill *c*, being at the same altitude as the hill *a*. It is quite certain that this would really take place if the force acquired by the momentum was not impeded by the friction of the wheels of the carriage upon their axles, and upon the rails on which they run. Hence, subtracting the amount of friction as a retarding force from the momentum which the carriage has acquired in descending from *a* to b*,* it will be perceived, that the force of momentum alone would only impel the carriage part of the way up the ascent *b c*, say as far as *z*.

It must now be evident, the carriage *d* would not only pass down the descending line of road from *a* to *b* by its gravity, but that the momentum acquired in the descent would also impel it up the second hill as far as z*,* unassisted by any locomotive power. In order, therefore, to raise the carriage to the top of the second hill, I have only to employ such an impelling force as would be sufficient to drive it from *z* to *c*, the whole expense of locomotive power for bringing the carriage from *a* to *z* being saved. If now I employ a locomotive power to assist in impelling my carriage from *a* to *b*, I, by that means, obtain a greater momentum than would result from the descent of the carriage by gravity alone, and am enabled by that means to surmount the hill *c*, having travelled the whole distance from *a* to *c*, on the undulating line of road, with the exertion of much less locomotive power than would have been requisite to have impelled the carriage the same distance upon a perfectly horizontal plane." Having thus explained the principle of his invention, Mr. Badnall claims the formation of tram and railroads, with such undulating curves as are adapted to his object.

This invention has been the subject of much able controversy in the Mechanics' Magazine, and some other public journals, of which our limits render it impossible to give any account. The plausible arguments which were raised in support of the inventor's theory, led to some public trials on the Manchester and Liverpool railway; which, although conclusive as to its inefficacy in the minds of most persons who doubted before, has apparently had the effect of confirming the patentee in his prepossessions of its utility.

Liverpool and Manchester Railway

Liverpool 17th May 1834

Dear Rathbone

I thank you for the loan of Robt. Stephenson's Report on Mr. Badnall's proposed undulating railway, which I am glad to have had the opportunity of reading; it contains, however, an observation on the Inclined Panes of the Liverpool and Manchester Railway, in the Correctness of which I cannot coincide. After alluding to the "destruction of machinery from varying velocities", the Report proceeds to state that "the objectionable nature of inclined planes is evident to all, Conversant with machinery" – "from whence it is probably not too much to say a very large portion of the wear and tear of the Locomotive Engines on the Liverpool and Manchester Railway has sprung". –

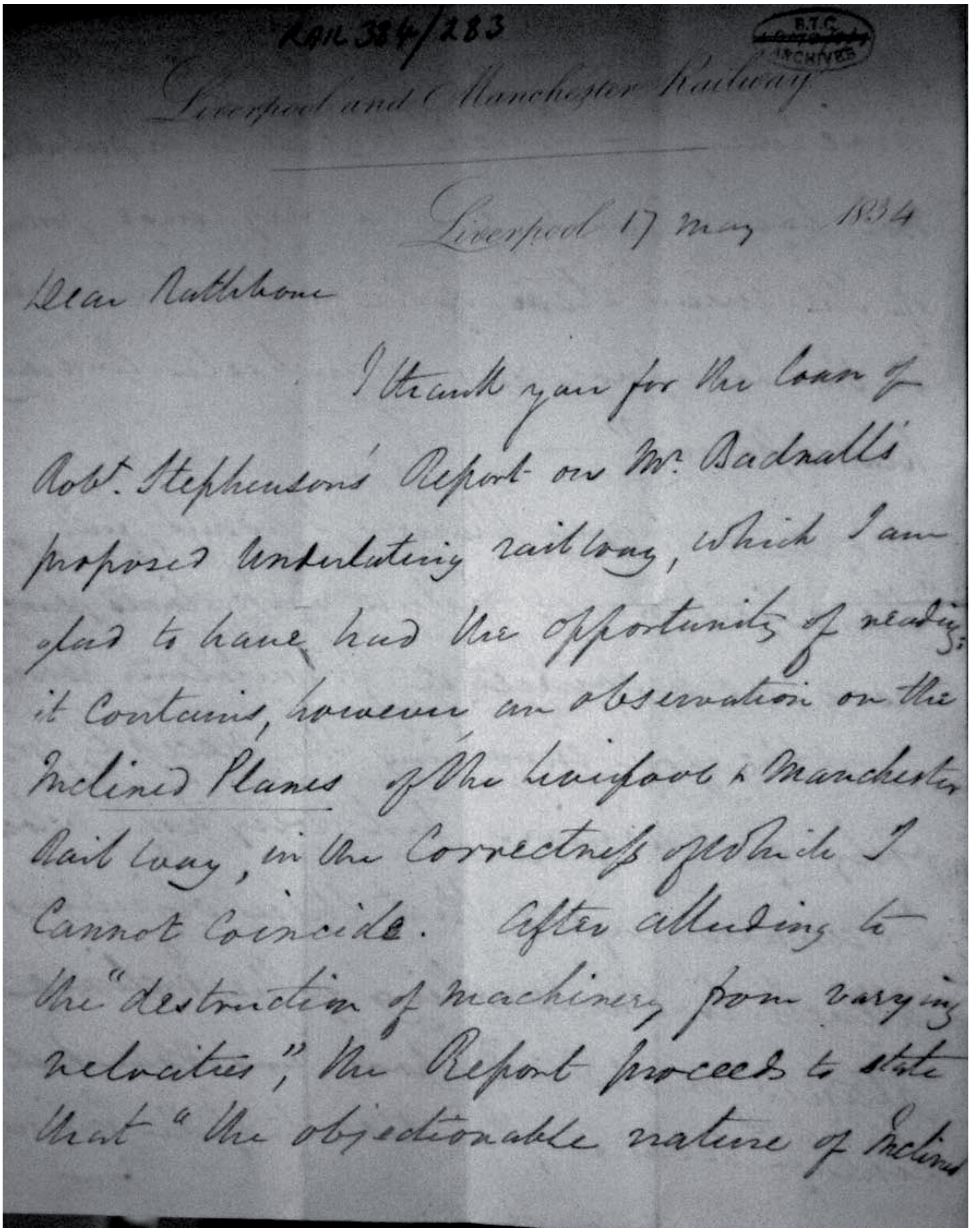
Now, though I admit, fully, "the objectionable nature of Inclined planes", used as a source of gravitating power, and therefore involving the neccesity, not only of varyiing, but very high velocities, I cannot agree that these objections attach to the working of the Inclined planes on the Liverpool and Manchester Line – because, the descent of the Engines upone them, is not used as a means of Power which it might be, if we would consent to the increased "wear and tear" which such a mode of working, would involve. With ordinary Care, the descent is managed without either a "varying" or a high velocity: indeed, it is a rule on our line to traverse the inclined planes at a slower speed than the level portions of the way.

As a question of Fact, then, I should say that "a very large portion of the wear and tear of the Locomotive Engines" was not owing to the Inclined planes; but that it was owing to the very high velocities: to the comprising within a small machine, a concentration of power and heat – of action and reaction – far beyond what its construction and original strength of materials, were calculated to bear.

I am
Dear Rathbone
Yours Truly,
H[enry] Booth

Letter by Henry Booth, who in 1825 established, along with other wealthy merchants from both Liverpool and Manchester, the Liverpool to Manchester Railway Company, becoming its secretary and treasurer. In 1825 the company presented a Bill to Parliament to build the line, though the Bill failed it was re-submitted the following year and was passed. He was also the designer of the multi-tubular engine used by the Rocket Engine, the winner of the Rainhill Trials.

Rathbone was a committee member of the newly formed Liverpool and Manchester Railway Company.



RAIL 384/283

B.T.C. ARCHIVES

Liverpool and Manchester Railway.

Liverpool 17 May 1834

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Booth's letter to Rathbone of the Liverpool and Manchester Railway, regarding Richard Badnall and his views (Four pages)

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As a question of Fact, then I should say that "a very large portion of the wear & tear of the Loco. Motive Engine" was not owing to the Inclined planes, but that it was owing to the very high velocities: to the compressing within a small machine, a concentration of power & heat – of action & reaction – far beyond what its construction & original strength of materials, were calculated to bear.

I am
Dear Rathbone
Yours truly
H. Booth

[illegible] 1834
H Booth to WR
Recd 17 Nov
Ansd [illegible]