

XXVII. *An Experimental Examination of the Quantity and Proportion of Mechanic Power necessary to be employed in giving different Degrees of Velocity to Heavy Bodies from a State of Rest.* By Mr. John Smeaton, F. R. S.

R. April 25,
1776.

ABOUT the year 1686 Sir ISAAC NEWTON first published his *Principia*, and, conformably to the language of mathematicians of those times defined, that “the quantity of motion is the measure of the same, arising from the velocity and quantity of matter conjointly.” Very soon after this publication, the truth or propriety of this definition was disputed by certain philosophers, who contended, that the measure of the quantity of motion should be estimated by taking the quantity of matter and the square of the velocity conjointly. There is nothing more certain, than that from equal impelling powers, acting for equal intervals of time, equal increases of velocity are acquired by given bodies, when unresisted by a medium; thus gravity causes a body, in obeying its impulse during one second of time, to acquire a velocity which would carry it uniformly forward, without any additional impulse, at the rate of 32 ft. 2 in. *per* second; and if gravity is suffered to act upon it for two seconds, it

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will

will have, in that time, acquired a velocity that would carry it, at an uniform rate, just double of the former; that is, at the rate of 64 ft. 4 in. *per* second. Now, if in consequence of this equal increase of velocity, in an equal increase of time, by the continuance of the same impelling power, we define that to be a double quantity of motion, which is generated in a given quantity of matter, by the action of the same impelling power for a double time; this will be co-incident with Sir ISAAC NEWTON's definition abovementioned; whereas, in trying experiments upon the total effects of bodies in motion, it appears, that when a body is put in motion, by whatever cause, the impression it will make upon an uniformly resisting medium, or upon uniformly yielding substances, will be as the mass of matter of the moving body, multiplied by the square of its velocity: the question, therefore, properly is, whether those terms, the *quantity of motion*, the *momenta* of bodies in motion, or *forces* of bodies in motion, which have generally been esteemed synonymous, are with the most propriety of language to be esteemed equal, double, or triple, when they have been generated by an equable impulse, acting for an equal, double, or triple time; or that it should be measured by the effects being equal, double, or triple, in overcoming resistances before a body in motion can be stopped? For, according as those terms are understood in this or that way, it will necessarily follow, that the *momenta* of equal bodies will be as the velocities, or as the squares of the velocities respectively; it being certain,

that, whichever we take for the proper definition of the term quantity of motion, by paying a proper regard to the collateral circumstances that attend the application of it, the same conclusion, in point of computation, will result. I should not, therefore, have thought it worth while to trouble the Society upon this subject, had I not found, that not only myself and other practical artists, but also some of the most approved writers, had been liable to fall into errors, in applying these doctrines to practical mechanics, by sometimes forgetting or neglecting the due regard which ought to be had to these collateral circumstances. Some of these errors are not only very considerable in themselves, but also of great consequence to the public, as they tend greatly to mislead the practical artist in works that occur daily, and which often require very great sums of money in their execution. I shall mention the following instances.

DESAGULIERS, in his second volume of *Experimental Philosophy*, treating upon the question concerning the forces of bodies in motion, after taking much pains to shew that the dispute, which had then subsisted fifty years, was a dispute about the meaning of words; and that the same conclusion will be brought out, when things are rightly understood, either upon the old or new opinion, as he distinguishes them; among other things, tells us, that the old and new opinion may be easily reconciled in this instance: that the wheel of an undershot water-mill is capable of doing quadruple work when the velocity of the water is doubled, instead of
double

double work only; “ because (the adjutage being the same), says he, we find, that as the water’s velocity is double, there are twice the number of particles of water that issue out, and therefore the ladle-board is struck by twice the matter, which matter moving with twice the velocity that it had in the first case, the whole effect must be quadruple, though the instantaneous stroke of each particle is increased only in a simple proportion of the velocity.” See vol. II. Annotations on lecture 6th, p. 92.

Again, in the same volume, lecture 12th, p. 424. referring to what went before, he tells us, “ The knowledge of the foregoing particulars is absolutely necessary for setting an undershot wheel to work; but the advantage to be reaped from it would be still guess-work, and we should be still at a loss to find out the utmost it can perform, if we had not an ingenious proposition of that excellent mechanic M. PARENT, of the Royal Academy of Sciences, who has given us a *maximum* in this case, by shewing, that an undershot wheel can do the most work, when its velocity is equal to the third part of the velocity of the water that drives it, &c. because then two-thirds of the water is employed in driving the wheel with a force proportionable to the square of its velocity. If we multiply the surface of the adjutage or opening by the height of the water, we shall have the column of water that moves the wheel. The wheel thus moved will sustain on the opposite side only four-ninths of that

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“ weight,

“ weight, which will keep it in equilibrio; but what it
 “ can move with the velocity it goes with, will be but
 “ one-third of that weight of equilibrio; that is, $\frac{4}{27}$ ths
 “ of the weight of the first column, &c.—This is the
 “ utmost that can be expected.”

The same conclusion is likewise adopted by MACLAU-
 RIN, in art. 907. p. 728. of his Fluxions, where, giving
 the fluxionary deduction of M. PARENT’S proposition,
 he says, “ that if A represents the weight which would
 “ balance the force of the stream, when its velocity is
 “ a ; and u represents the velocity of the part of the en-
 “ gine, which it strikes when the motion of the machine
 “ is uniform, &c.—the machine will have the greatest
 “ effect when u is equal to $\frac{a}{3}$; that is, if the weight that
 “ is raised by the engine be less than the weight which
 “ would balance the power, in the proportion of 4 to 9,
 “ and the *momentum* of the weight is $\frac{4Aa}{27}$.”

Finding that these conclusions were far from the
 truth; and seeing, from many other circumstances, that
 the practical theory of making water and wind-mills was
 but very imperfectly delivered by any author I had then
 an opportunity of consulting^(a); in the year 1751 I began
 a course

(a) BELIDOR, *Architecture Hydraulique*, greatly prefers the application of
 water to an undershot mill, instead of an overshot; and attempts to demonstrate,
 that water applied undershot will do six times more execution than the same
 applied overshot. See vol I. p. 286. While DESAGULIERS, endeavouring to
 invalidate what had been advanced by BELIDOR, and greatly preferring an over-
 shot

a course of experiments upon this subject. These experiments, with the conclusions drawn from them, have already been communicated to this Society, who printed them in vol. LI. of their Transactions for the year 1759, and for this communication I had the honour of receiving the annual medal of Sir GODFREY COPLEY, from the hands of our very worthy President the late Earl of MACCLESFIELD. Those experiments and conclusions stand uncontroverted, so far as I know, to this day; and having since that time been concerned in directing the construction of a great number of mills, which were all executed upon the principles deduced from them, I have by that means had many opportunities of comparing the effects actually produced with the effects which might be expected from the calculation; and the agreement, I have always found between these two, appears to me fully to establish the truth of the principles upon

shot to an undershot, says, Annotat. on lecture 12. vol. II. p. 532. that from his own experience, “a well-made overshot mill ground as much corn in the “same time with ten times less water;” so that betwixt BELIDOR and DESAGULIERS here is a difference of no less than 60 to 1.

Again, BELIDOR, vol. II. p. 72. says, that the center of gravity of each sail of a wind-mill should travel in its own circle with one-third of the velocity of the wind; so that, taking the distance of this center of gravity from the center of motion at 20 feet, as he states it p. 38. art. 849. the circumference will be exceeding 126 feet English measure: a wind, therefore, to make the mill go twenty turns *per* minute, which they frequently do with a fresh wind and all their cloth spread, would require the wind to move above eighty miles an hour; a velocity perhaps hardly equalled in the greatest storms we experience in this climate.

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which they were constructed, when applied to great works, as well as upon a smaller scale in models.

Respecting the explanatory deduction of DESAGULIERS in the first example abovementioned, which, indeed, I have found to be the commonly received doctrine among theoretical mechanics, it is shewn, *Phil. Trans.* vol. LI. p. 120, 121, and 123. part 1. maxim 4. that, where the velocity of water is double, the adjutage or aperture of the fluice remaining the same, the effect is eight times; that is, not as the square but as the cube of the velocity; and the same is investigated concerning the power of the wind arising from difference of velocity, p. 156. being part 3. maxim 4.

The conclusion in the second example abovementioned, adopted both by DESAGULIERS and MACLAURIN, is not less wide of the truth than the foregoing; for if that conclusion were true, only $\frac{4}{27}$ ths of the water expended could be raised back again to the height of the reservoir from which it had descended, exclusively of all kinds of friction, &c. which would make the actual quantity raised back again still less; that is, less than one-seventh of the whole; whereas it appears, from table 1. p. 115. of the said volume, that in some of the experiments there related, even upon the small scale on which they were tried, the work done was equivalent to the raising back again about one quarter of the water expended; and in large works the effect is still greater, approaching towards half, which seems to be the limit for the undershot mills, as the whole would be the limit for
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the overshot mills, if it were possible to set aside all friction, resistance from the air, &c. see p. 130.

The velocity also of the wheel, which, according to M. PARENT's determination, adopted by DESAGULIERS and MACLAURIN, ought to be no more than one-third of that of the water, varies at the *maximum* in the above-mentioned experiments of table 1. between one third and one half; but in all the cases there related, in which the most work is performed in proportion to the water expended, and which approach the nearest to the circumstances of great works, when properly executed, the *maximum* lies much nearer to one half than one third; one half seeming to be the true *maximum*, if nothing were lost by the resistance of the air, the scattering of the water carried up by the wheel, and thrown off by the centrifugal force, &c. all which tend to diminish the effect more, at what would be the *maximum* if these did not take place, than they do when the motion is a little slower.

Finding these matters, as well as others, to come out in the experiments, so very different from the opinions and calculations of authors of the first reputation, who, reasoning according to the Newtonian definition, must have been led into these errors from a want of attending to the proper collateral circumstances; I thought it very material, especially for the practical artist, that he should make use of a kind of reasoning in which he should not be so liable to mistakes; in order, therefore, to make this matter perfectly clear to myself, and possibly so to others,

I resolved

I resolved to try a set of experiments from whence it might be inferred, what proportion or quantity of mechanical power is expended in giving the same body different degrees of velocity. This scheme was put in execution in the year 1759, and the experiments were then shewn to several friends, particularly my very worthy and ingenious friend Mr. WILLIAM RUSSELL.

In my experimental inquiry concerning the powers of water and wind before referred to, I have, p. 105. part I. defined what I meant by power, as applied to practical mechanics, that is, what I now call mechanical power; which, in terms synonymous to those there used, may be said to be measured by multiplying the weight of the body into the perpendicular height from which it can descend; thus the same weight, descending from a double height, is capable of producing a double mechanical effect, and is therefore a double mechanical power. A double weight descending from the same height is also a double power, because it likewise is capable of producing a double effect; and a given body, descending through a given perpendicular height, is the same power as a double body descending through half that perpendicular; for, by the intervention of proper levers, they will counter-balance one another, conformably to the known laws of mechanics, which have never been controverted. It must, however, be always understood, that the descending body, when acting as a measure of power, is supposed to descend slowly, like the weight of a clock or a jack; for, if quickly descending,

it is sensibly compounded with another law, *viz.* the law of acceleration by gravity.

DESCRIPTION OF THE MACHINE.

AB is the base of the machine placed upon a table.

AC is a pillar or standard.

CD is an arm, upon the extremity of which is fixed a plate *fg*, which is here seen edge-ways, through which is a small hole for receiving a small steel pivot *e*, fixed in the top of the upright axis *eB*; the lower end of this axis finishes in a conical steel point, which rests upon a small cup of hard steel polished at B.

HI is a cylinder of white fir, which passes through a perforation in the axis, and therein fixes; and, upon the two arms formed thereby, are capable of sliding

KL two cylindric weights of lead of equal size, which are capable of being fixed upon any part of the cylindric arms, from the axis to their extremities, by means of two thin wedges of wood. The two weights, therefore, being at equal distances from the center, and the axis perpendicular, the whole will be balanced upon the point at B, and moveable thereupon by an impelling power, with very little friction.

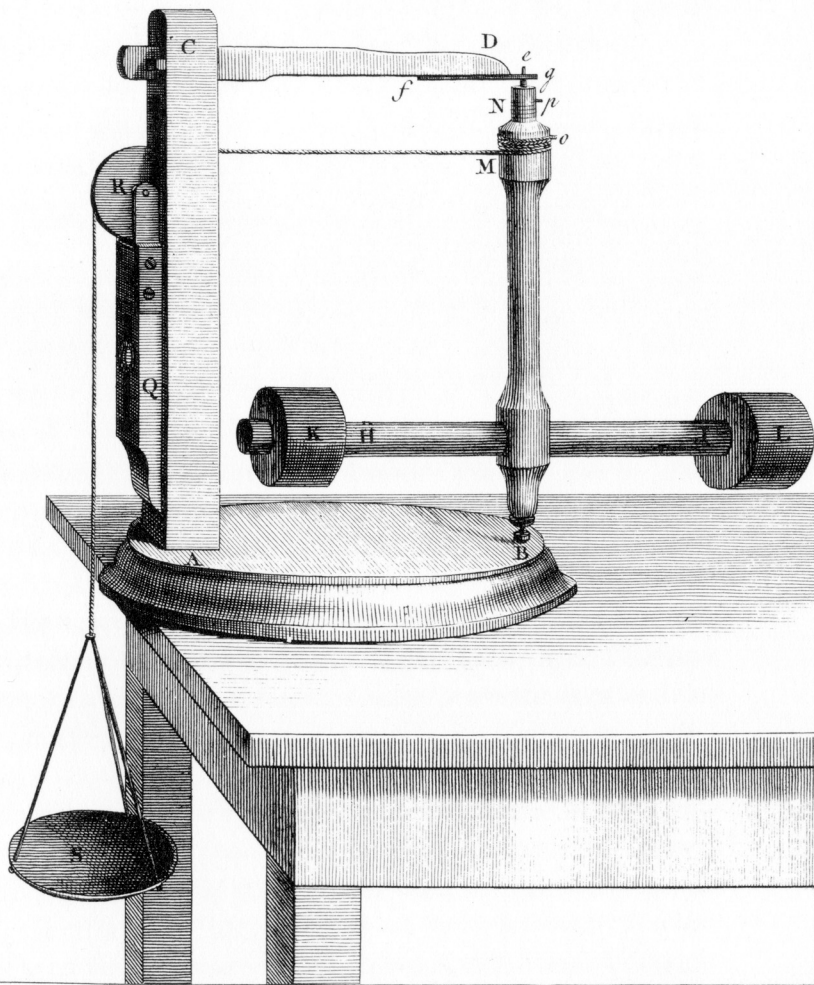
Upon the upper part of the axis are formed MN, two cylindrical barrels, whereof M is double the diameter of N; they have a little pin stuck into one side of each at *o, p*.

Q is a piece capable of sliding higher or lower, as occasion requires; and carries

R a light pulley of about three inches diameter, hung upon a steel axis, and moveable upon two small pivots. The plane of the pulley, however, is not directed to the middle of the upright axis, but a little on one side, so as to point (at a mean) between the surface of the bigger barrel and the less.

S is a light scale for receiving weights, and hangs by a small twine, cord, or line, that passes the pulley, and terminates either upon the bigger barrel or the less, as may be required; the sliding-piece **Q** being moved higher or lower for each, that the line, in passing from the pulley to the barrel, may be nearly horizontal. The end of the line, that is furthest from the scale, is terminated by a small loop, which hangs on upon the pin **o**, or the pin **p**, according as the bigger or the lesser barrel is to be used.

Now, having wound up a certain number of turns of the line upon the barrel, and having placed a weight in the scale **s**, it is obvious, that it will cause the axis to turn round, and give motion to its arms, and to the weights of lead placed thereon, which are the heavy bodies to be put in motion by the impulse of the weight in the scale; and when the line is wound off to the pin, the loop slips off, and the scale then falling down, the weight will cease to accelerate the motion of the heavy bodies, and leave them revolving, equably forward, with the velocity they have acquired, except so far as it must be gradually lessened by the friction of the machine and resistance of the air, which being small, the bodies will revolve sometime before their velocity is apparently diminished.



MEASURES OF SOME PARTS OF THE MACHINE.

	inches.
Diameter of the cylinders of lead, or the heavy bodies,	2,57
Length of ditto,	1,56
Diameter of the hole therein,	,72

Weight of each cylinder 3 lbs. Avoirdupois.

Greater distance of the middle of each body from the center of the axis,	8,25
The smaller distance of ditto,	3,92
10 turns of the smaller barrel raises the scale, 5 ditto of the bigger ditto,	25,25

When the bodies are at the smaller distance above specified from the axis of rotation, they are then in effect at half the greater distance from that axis; for, since the axis itself, and the cylindric arms of wood, keep an unvaried distance from the center of rotation, the bodies themselves must be moved nearer than half their former distance, in order that, compounded with the unvariable parts, they may be virtually at the half distance. In order to find this half distance nearly, I put in an arm of the same wood, that only went through the axis, without extending in the opposite direction; one of the bodies being put upon the end of this arm, at the distance of 8,25 inches, the whole machine was inclined till the body and arm became a kind of pendulum, and vibrated

at the rate of 92 times *per* minute; and as a pendulum of the half length vibrates quicker in the proportion of $\sqrt{1}$ to $\sqrt{2}$; that is, in the proportion of 92 to 130 nearly; therefore, keeping the same inclination of the machine, the weight was moved upon the arm till it made 130 vibrations *per* minute; which was found to be, when it was at 3,92 inches distance from the center as above stated, which is about $\frac{2}{10}$ ths of an inch nearer than the half distance. The double arm was then put in, and marked accordingly, and the bodies being mounted thereon, the whole was adjusted ready for use; and with it were tried the following experiments, each of which was repeated so many times as to be fully satisfactory.

TABLE OF EXPERIMENTS.

No	Ounces Avoirdupois in the Scale.	Barrel used, M the bigger, N the smaller.	The Arms, W the whole, H the Half- length.	Number of Turns of the Line wound on the Barrel.	Time of the Descent of the Weight in the Scale.	Time in making 20 Revolu- tions with equa- ble Motion.
1	8	M	W	5	14 $\frac{1}{4}$	29
2	8	N	W	10	28 $\frac{1}{4}$	29 $\frac{1}{4}$
3	8	N	W	2 $\frac{1}{2}$	14 $\frac{1}{4}$	58 $\frac{1}{2}$
4	32	M	W	5	7	14
5	32	N	W	10	14	14 $\frac{3}{4}$
6	32	N	W	2 $\frac{1}{2}$	7	28 $\frac{3}{4}$
7	8	M	H	5	7	14 $\frac{3}{4}$
8	8	N	H	10	14	15
9	8	N	H	2 $\frac{1}{2}$	7	30 $\frac{1}{4}$
1	2	3	4	5	6	7

The 58 $\frac{1}{2}$ in number 3, column 7, was determined in fact from 29 $\frac{1}{4}$, being the time of making 10 equable revolutions, after the weight was dropped off, in order to prevent the sensible retardation that might take place, and affect the observation, if continued for 20 revolutions made so slowly.

FURTHER

FURTHER DEFINITIONS.

I have already defined what I mean by mechanic power; but, before I proceed further, it will be necessary also to define the following terms:

Impulse or Impulsion,	} By all which, I understand the uniform endeavour that one body exerts upon another, in order to make it move; and that, whether it produces or generates motion by this endeavour or not; and the quantity of this impelling power may be measured either by its being a weight of itself, or by being counter-balanced by a weight. It may also act either immediately upon the body to be moved, so that if motion is the consequence, they move with the same velocity; and that, either by a simple contact, or by being drawn as by a cord, or pushed as by a staff: or it may act by the intervention of a lever or other mechanic instrument, in which the velocity of the body to be moved may be very different from the velocity of the impelling power or mover; but in comparing them, the impelling powers must be reduced according to the proportional velocities of the mover and moved; or, in levers of different lengths, they may be compared by a standard length of lever, which is the method taken in the subsequent reasoning upon the preceding experiments. An impelling power, therefore, consisting of a double weight, or requiring a double weight to counter-balance it, when acting with equal levers, is a double impelling power, or an impelling power of double the intensity.
Impulsive Force or Power,	
Impelling Force or Power,	

OBSERVATIONS AND DEDUCTIONS FROM THE PRECEDING
EXPERIMENTS.

1st, By the first experiment it appears, that the mechanic power employed, consisting of 8 ounces in the scale, deliberately descending (by 5 turns of the bigger barrel) through a perpendicular space $25\frac{1}{4}$ inches, will represent the quantity of mechanic power which causes the two heavy bodies, from a state of rest, to acquire a velocity, such as to carry them equably through 20 circumferences of their circle of revolution in the space of 29"; and that the time in which the mechanic power produced this effect was $14\frac{1}{4}$, as appears by column 6th. And this mechanic power we shall express by the number 202, the product of the number of ounces in the scale multiplied by the inches in its perpendicular descent, for $8 \times 25\frac{1}{4} = 202$.

2d, By the second experiment, as 10 turns of the smaller barrel are equal to the same perpendicular height as 5 turns of the bigger, it follows, that the same mechanic power, *viz.* 202, acting upon the same heavy bodies to accelerate them, produces the very same effect in generating motion in the bodies as it did before, *viz.* 20 revolutions in $29\frac{1}{4}$, the small difference of $\frac{1}{4}$ of a second being no more than may reasonably be attributed to the unavoidable errors arising from friction of the machine, want of perfect accuracy in its measures, resistance of the air, and imperfections in the observations themselves, which must not only be allowed for in this, but
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the rest; but as the impelling power is acting here upon a lever of but half the length, and, consequently, but half the intensity, when referred to the bodies to be moved, it takes just double the time to generate the same velocity therein.

DEDUCTION. It appears from hence, that the same mechanic power is capable of producing the same velocity in a given body, whether it is applied so as to produce it in a greater or a lesser time; but that the time taken to produce a given velocity, by an uniformly continued action, is in a simple inverse proportion of the intensity of the impulsive power.

3dly, The third experiment being made with $2\frac{1}{2}$ turns of the lesser barrel, the same weight in the scale of 8 ounces descending only one quarter part of the former perpendicular, the mechanic power employed will be only one quarter part of the former, *viz.* $50\frac{1}{2}$; but as one quarter part of the mechanic power produces half of the former velocity in the heavy bodies; that is, they make 20 revolutions in $58\frac{1}{2}$; that is, nearly 10 revolutions in 29"; we may conclude, in this instance, that the mechanic power, employed in producing motion, is as the square of the velocity produced in the same body; and that the velocity produced is as the time that an impelling power, of the same intensity, continues to act upon it, as appears by the near agreement of numbers 2 and 3, column 6th.

4thly, In the fourth experiment, the apparatus is the same as the first, only here the weight in the scale is 32 ounces; that is, the impelling power is the quadruple of
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the first, and hereby a double velocity is given to the bodies; for they make 20 revolutions in $14''$, which is a small matter less than half the time taken up in making 20 revolutions in the first experiment. It also appears, that the velocity acquired is simply as the impelling power compounded with the time of its action; for a quadruple impulsion acting for $7''$ instead of $14''$ generates a double velocity, while the mechanic power employed to generate it is quadruple, for $32 \times 25\frac{1}{4} = 808$. And here the mechanic power employed being four times greater than the first, it holds here also, that the mechanic power, to be necessarily employed, is as the square of the velocity to be generated; that is, in the same proportion as turned out in the third experiment, where the mechanic power employed was only a quarter part of the first.

5thly, The fifth and sixth experiments were made with a mechanic power four times greater than those employed in numbers 2 and 3 respectively; and since the same deductions result from hence as from numbers 2 and 3, they are additional confirmations of the conclusions drawn from them and from the last article.

6thly, In the seventh experiment, the disposition of the apparatus is the same as number 1, only here the bodies are placed upon the arms at the half-length; from whence it appears, that the same mechanic power still produces the same velocity in the same bodies; for though 20 revolutions were performed in $14\frac{3}{4}''$ (see column 7)

which is nearly half the time that 20 revolutions were performed in the first experiment; yet, since the circles in which the bodies revolved in the seventh are only of half the circumference as those of number 1, it is obvious, that the absolute velocity acquired by the moving bodies in the two cases is equal. But, by column 6th, the time in which it was generated is only half; yet, notwithstanding, this will coincide with the former conclusions, if the intensity of the impelling power is compounded therewith; for, though the barrel was the same with the same number of turns as in number 1, and therefore the lever the same, by which the impelling power acted, yet, as the bodies, upon which this lever was to act, were placed upon a lever of only half the length from the center, the impelling power, acting by the first lever, would act upon the second with double the intensity, according to the known laws of mechanics; that is, it would require a double weight opposing the bodies, to prevent their moving, in order to balance it. An impulsive power, therefore, of double the intensity, acting for half the time, produces the same effect in generating motion, as an impulsive power, of half the intensity, acting for the whole time.

7thly, The eighth and ninth experiments afford the same deductions and confirmations relative to the seventh experiment, that the fifth and sixth do respecting the fourth, and that the second and third do respecting the first; and from the near agreement of the whole, when
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the necessary allowances before mentioned are made, together with some small inequality arising from the mechanical power lost by the difference of the motion given by gravity to the weight in the scale: I say, from these agreements, under the very different mechanical powers applied, which were varied in the proportion of 1 to 16, we may safely conclude, that this is the universal law of nature, respecting the capacities of bodies in motion to produce mechanical effects, and the quantity of mechanic power necessary to be employed to produce or generate different velocities (the bodies being supposed equal in their quantity of matter); that the mechanic powers to be expended are as the squares of the velocities to be generated, and *vice versa*; and that the simple velocities generated are as the impelling power compounded with, or multiplied by, the time of its action, and *vice versa*.

We shall, perhaps, form a still clearer conception of the relation between velocities produced, and the quantities of mechanic power required to produce them; together with the collateral circumstances attending, by which these propositions, seemingly two, are reconciled and united, by stating the following popular elucidation, which, indeed, was the original idea that occurred to me on considering this subject; to put which to an experimental proof gave birth to the foregoing apparatus and experiments.

Suppose then a large iron ball of 10 feet diameter, turned truly spherical, and set upon an extended plane of the same metal, and truly level. Now, if a man begins to

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push

push at it, he will find it very resisting to motion at first; but, by continuing the impulse, he will gradually get it into motion, and having nothing to resist it but the air, he will, by continuing his efforts, at length get it to roll almost as fast as he can run. Suppose now, in the first minute he gets it rolled through a space of one yard; by this motion, proceeding from rest (similar to what happens to falling bodies) it would continue to roll forward at the rate of two yards *per* minute, without further help; but supposing him to continue his endeavours, at the end of another minute he will have given it a velocity capable of carrying it through a space of two yards more, in addition to the former, that is, at the rate of four yards *per* minute; and at the end of the third minute, he has again added an equal increase of velocity, and made it proceed at the rate of six yards *per* minute; and so on, increasing its velocity at the rate of two yards in every minute. The man, therefore, in the space of every minute exerts an equal impulse upon the ball, and generates an equal increase of movement correspondent to the definition of Sir ISAAC NEWTON. But let us see what happens besides: the man, in the first minute, has moved but one yard from where he set out; but he must in the second minute move two yards more, in order to keep up with the ball; and as he exerted an impulse upon it, so as at the end of second minute to have given it an additional velocity of the two yards, he must also in this time have gradually changed its velocity from the rate of two yards *per* minute to that of four, and the space, that he will of conse-

quence have actually been obliged to go through in the second minute, will be according to the mean of the extremes of velocity at the beginning and end thereof, that is, three yards in the second minute; so that being one yard from his original place at the beginning of the second minute, at the end of it he will have moved the sum of the journies of the first and second minute, that is, in the whole four yards from his original place. As he has now generated a velocity in the ball of four yards *per* minute, in the third minute he must travel four yards to keep up with the ball, and one more in generating the equal increment of velocity; so that in the third minute, he must travel five yards to keep up the same impelling power upon the ball that he did in the first minute in travelling one, so that these five yards in the third minute, added to the four yards that he had travelled in the two preceding minutes, sets him at the end of the third minute nine yards from whence he set out, having then given the ball a velocity capable of carrying it uniformly forward at the rate of six yards *per* minute, as before stated. We may now leave the further pursuit of these proportions, and see how the account stands. He generated a velocity of two yards *per* minute in the first minute, the square of which is four, when he had moved but one yard from his place; and he had generated a velocity of six yards *per* minute, the square of which is thirty-six, at the end of the third minute, when he had travelled nine yards from his place. Now, since the square of the velocity, generated at the end of the first minute, is to that

of the velocity generated at the end of the third minute, as 4 : 36, that is, as 1 : 9; and since the spaces, moved through by the man to communicate these velocities, are also as 1 : 9, it follows, that the spaces through which the man must travel, in order to generate these velocities respectively (preserving the impelling power perfectly equal), must be as the squares of the velocities that are communicated to the ball; for, if the man was to be brought back again to his original place by a mechanical power, equally exerted upon the man equally resisting, this would be the measure of what the man has done in order to give motion to the ball. It therefore directly follows, conformably to what has been deduced from the experiments, that the mechanic power that must of necessity be employed in giving different degrees of velocity to the same body, must be as the square of that velocity; and if the converse of this proposition did not hold, *viz.* that if a body in motion, in being stopped, would not produce a mechanical effect equal or proportional to the square of its velocity, or to the mechanical power employed in producing it, the effect would not correspond with its producing cause.

Thus the consequences of generating motion upon a level plane exactly correspond with the generating of motion by gravity; *viz.* that though in two seconds of time the equal impulsive power of gravity gives twice the velocity to a body that it does in one second, yet this collateral circumstance attends it, that at the end of the double time, in consequence of the velocity acquired in

the first half, the body has fallen from where it set forward through four times the perpendicular; and, therefore, though the velocity is only doubled, yet four times the mechanical power has been consumed in producing it, as four times the mechanical power must be expended in bringing up the fallen body to its first place.

This then appears to be the foundation, not only of the disputes that have arisen, but of the mistakes that have been made, in the application of the different definitions of quantity of motion; that while those, that have adhered to the definition of Sir ISAAC NEWTON, have complained of their adversaries, in not considering the time in which effects are produced, they themselves have not always taken into the account the space that the impelling power is obliged to travel through, in producing the different degrees of velocity. It seems, therefore, that, without taking in the collateral circumstances both of time and space, the terms, quantity of motion, *momentum*, and force of bodies in motion, are absolutely indefinite; and that they cannot be so easily, distinctly, and fundamentally compared, as by having recourse to the common measure, *viz.* mechanic power.

From the whole of what has been investigated, it therefore appears, that time, properly speaking, has nothing to do with the production of mechanical effects, otherwise than as, by equally flowing, it becomes a common measure; so that, whatever mechanical effect is found to be produced in a given time, the uniform continuance of the action of the same mechanical power will, in a double
time,

time, produce two such effects, or twice that effect. A mechanical power, therefore, properly speaking, is measured by the whole of its mechanical effect produced, whether that effect is produced in a greater or a lesser time; thus, having treasured up 1000 tons of water, which I can let out upon the over-shot wheel of a mill, and descending through a perpendicular of 20 feet, this power applied to proper mechanic instruments, will produce a certain effect, that is, it will grind a certain quantity of corn; and that, at a certain rate of expending it, it will grind this corn in an hour. But suppose the mill equally adapted to produce a proportionable effect, by the application of a greater impulsive power as with a less, then, if I let out the water twice as fast upon the wheel, it will grind the corn twice as fast, and both the water will be expended and the corn ground in half an hour. Here the same mechanical effect is produced; *viz.* the grinding a given quantity of corn, by the same mechanical power, *viz.* 1000 tons of water descending through a given perpendicular of 20 feet, and yet this effect is in one case produced in half the time of the other. What time, therefore, has to do in the business is this: let the rate of doing the business, or producing the effect, be what it will, if this rate is uniform, when I have found by experiment what is done in a given time, then, proceeding at the same rate, twice the effect will be produced in twice the time, on supposition that I have a supply of mechanic power to go on with. Thus 1000 tons of water, descending through 20 feet of perpendicular,

dicular, being, as has been shewn, a given mechanic power, let me expend it at what rate I will, if when this is expended, I must wait another hour before it be renewed, by the natural flow of a river, or otherwise, I can then only expend twelve such quantities of power in 24 hours; but if, while I am expending 1000 tuns in one hour, the stream renews me the same quantity, then I can expend 24 such quantities of power in 24 hours; that is, I can go on continually at that rate, and the product or effect will be in proportion to time, which is the common measure; but the quantity of mechanic power arising from the flow of the two rivers, compared by taking an equal portion of time, is double in the one to the other, though each has a mill, that, when going, will grind an equal quantity of corn in an hour.