tively affirm, that he bought the Small Pox when at School, and of ſuch a Lady, now living, and gave her three Pence for the Matter contained in 12 Puſtules. That hundreds in this Country have had the Small Pox this way is certain; and it cannot produce one ſingle Inſtance of their ever having them a ſecond time.

Haverford Weſt,
Feb. 15. 172$\frac{2}{3}$.

Richard Wright.

IX. *An Account of ſome Experiments made to prove, that the Force of Moving Bodies is proportionable to their Velocities: (or rather that the* Momentum *of Moving Bodies is to be found by multiplying the Maſſes into the Velocities) In Anſwer to ſuch who have ſometime ago affirm'd, that that Force is proportionable to the Square of the Velocity, and to thoſe who ſtill defend the ſame Opinion. By the Reverend* John Theophilus Deſaguliers, *LL.D. F.R.S.*

AS far as I can learn, Monſieur *Leibnitz* was the firſt that oppos'd the receiv'd Opinion, concerning the Quantity of the Force of moving Bodies; by ſaying, that it was to be eſtimated by multiplying the Maſs of the Bodies, not by their Velocity, but by the Square of it. But, inſtead of ſhewing any Paralogiſm, in the mathematical Demonſtrations, which

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are made uſe of to prove the Propoſition, or any Miſtakes in the Reaſonings from the Experiments made to confirm it, he uſes other *Mediums* to prove his Aſſertions [a]; and without any Regard to what others had ſaid on that Subject, brings new Arguments, which the Reverend and Learned Dr. *Clark* has fully anſwered in his fifth Letter to him. Meſſieurs *John Bernoulli*, *Wolfius*, *Hermannus*, and others, have follow'd and defended Monſieur *Leibnitz*'s Opinion, and in the ſame manner, ſo that what is an Anſwer to him, is ſo to them.

Polenus (Profeſſor at *Padua*) has acted after the ſame manner in the experimental Way, making ſome Experiments to defend Monſieur *Leibnitz*'s Opinion, without having ſhewn thoſe to be falſe, which are made uſe of to prove the contrary [b]; and now lately, an ingenious Profeſſor abroad (who was of the Opinion commonly receiv'd, and in his Writings had demonſtrated it in the uſual way [c], confirming it with the common Experiments made in that Caſe) happening to make ſome Experiments like thoſe of *Polenus*, has drawn Concluſions from them to ſhew the Force of moving Bodies to be proportionable to the Square of their Velocity; and being wholly come over to that Opinion, endeavours to deduce it from Phyſical Principles.

As there can hardly be ſaid any thing new, or better than has been ſaid, to ſhew the Force abovemention'd to be proportionable to the Maſs multiplied into the Velocity; I only repeat here the Subſtance of what others have ſaid, and make ſome old Experiments over again; but then I conſider ſome Circum-

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[a] Acta Erudit. ad ann. 1686. p. 162. [b] Polenus de Caſtellis, p. 56, 57, &c. [c] Graveſande Introductio, Vol. I. No. 132.

ſtances

ſtances, that perhaps have been overlook'd, and at laſt, by a new Experiment, endeavour to ſhew, what has led into an Error ſome of thoſe, who defend the new Opinion.

If a Man with a certain Force can move a Weight of fifty Pounds, through a Space of four Feet, in a determinate time; it is certain he muſt employ twice that Force to move one hundred Pounds Weight, through the ſame Space in the ſame time. But if he uſes but the ſame Force, he will move the one hundred Pounds Weight but two Feet in the ſame time. For as the one hundred Pounds Weight contains two fifty Pound Weights, if each of them has two Degrees of Velocity given to it, it will exactly require the ſame Force that would give one of them four Degrees of Velocity; hence it appears, that the Force is proportionable to the Maſs multiply'd into the Velocity.

EXPERIMENT I.

Fig. 1. Let the Balance A B, whoſe *Fulchrum*, or Center of Motion, is at C, be ſo divided, that the *Brachium* A C be but the fourth Part of the *Brachium* C B; it is known to all Mechanicians, that a Weight of one hundred Pounds at A, will keep in *Æquilibrio* a Weight of twenty five Pounds hanging at B, where it will have a Velocity four times greater than that of the Weight at A. For, not only when the Balance is horizontal, there will be an *Æquilibrium*, but when the Balance is put in Motion, it will return to an *Æquilibrium* in an horizontal Poſition; the equal and contrary Forces applied at each, deſtroying one another. Whereas, if the Forces were as the Maſs multiply'd into the Square of the Velocity, the twenty five Pound Weight ſhould have been ſuſpended at D, only

twice as far from C, as the Weight at A; and in general, let the make of the Engine be what it will, let the mechanical Powers be combined in any manner, when two heavy Bodies, by means of a Machine, act upon one another in different directions, if their Velocities are reciprocally as their Maſſes, they will deſtroy each others Forces and come to reſt.

As this is true in reſpect of mechanical Powers, ſo it is in reſpect to the Shock or Blow given by falling Bodies. An heavy Body, falling with an accelerated Motion, goes through a ſpace of one Foot in a quarter of a Second, and acquires a Velocity, which would carry it two Foot in the ſame Time with an uniform Motion; the ſame Body falls through a ſpace of four Foot in half a Second, and acquires a Velocity, that would with an uniform Motion carry it eight Foot in half a Second. Therefore, as the Time of the fall through a ſpace of four Foot is twice the Time of a fall through one Foot, the Velocity in the latter Caſe is double that of the firſt, and conſequently the Blow, that the Body will give, will be double.

EXPERIMENT II.

Fig. II. Let the Weight P of one Pound, be placed in the Scale ſuſpended at the end A, of the Ballance A B, which bears upon the *Gnomon*, or Iron Supporter, *k h i*. Then if the Weight C be let fall from D, or one Foot, it will by its Stroke on the end of the Beam B, raiſe up the oppoſite end A with the Weight P, ſo high, that the Spring *g h* will fly from the Button *i*, which kept it ſtreight and upright before the Shock. If the Weight P be of two Pounds, it cannot be raiſed by the fall of C from any height leſs than F or four Foot; whereas, if the force of the Shock was proportionable to the Space,

without any regard to the time, as Monſieur *Leibnitz* and his followers have affirmed, P ought to be raiſed, when C falls only from E, or two Foot, which never happens; or, if the Stroke was proportionable to the Mais multiplied into the ſquare of the Velocity, when C falls from F, then P might weigh four Pound, whereas the Experiment will never ſucceed under thoſe Circumſtances.

I know it is objected, that the Blow cannot be always direct, and that the String that goes through the hole in the falling Weight, to guide it in its Fall, cauſes a ſenſible Friction, and therefore that ſomething of the Force is thereby loſt. But we are to obſerve, that if that was all, there would be no need of raiſing up C in the ſecond Caſe quite up to F, inſtead of E, whereas in Fact, it muſt always be raiſed beyond F, to allow for the Friction, that hinders it from producing a double Effect in falling from F. For if the Experiment be repeated an hundred times, the Weight, P when of two Pounds, will never be raiſed by letting C fall from any place between E and F.

EXPERIMENT III.

If (in order to avoid Friction) inſtead of a Blow ſtruck upon the end B, by the falling Body, the ſaid Body C be faſtned to a pretty long String tied to the button *m*, as at *c*, and firſt lifted up one Foot, and then let fall; ſo that in falling one Foot, it may pull down B, and lift up A with the Weight P of one Pound; whenever P is two Pounds, C muſt fall from a height greater than *f* or four Foot, otherwiſe it will not raiſe the *Brachium* A, eſpecially if it be let fall between *e* and *f*.

EXPE-

EXPERIMENT IV.

I took the Weight C of ſeventeen Ounces Troy, which was a round ball of Lead with a hole through the middle of it, and having paſſed the String N-X through it, before it was faſtened to the Hook X, I placed the whole Machine in ſuch manner, that the String being ſtretched by the Weight N, went through the hole of the Weight C, and likewiſe through the hole of the *Brachium* B, upon which *C* lay, without touching the ſides of the hole either in the Weight or Ballance; then having put ſuch a Weight P in the oppoſite Scale, as C falling from the height of one Inch, was able to raiſe high enough, to let looſe the Spring *g h* from the Button *i*: I added to P another Weight equal to it, and then letting fall C along the String that guided it, from an heighth of two Inches, then of three, and then exactly of four, it would not raiſe the double Weight P to the former height, but falling from five Inches, or a little higher, it raiſed it up.

EXPERIMENT V.

Leaving every thing as it was before, I changed the Weight C for another leaden Ball of twice the Weight, which falling from one Inch, raiſed the double Weight P to the uſual height; then changing the Weight *P* in any Proportion, whatever height was requir'd for the heavieſt Ball C (or C 2) to fall from, in order to raiſe the Weight at P; more than four times the height was required for the firſt Ball C, to raiſe the ſame Weight ſo high. as to let looſe the Spring.

EXPE-

EXPERIMENT VI.

I tried the Experiment with the Weight C hanging at the String *m c* (as in Experiment III.) and a Fall from an height of five, or near five Inches, was required to raiſe double the Weight in the oppoſite Scale, that a fall from one Inch would raiſe; only here the heighth above four Inches was not ſo great as in the former Experiment, the Friction being ſomething leſs. Then I ſuſpended the great Ball C (or C 2) by the String *m c*, and when by falling one Inch it raiſed the Weight P, the little Weight C could not produce the ſame Effect, without falling from a greater height than four Inches.

It is here to be obſerved, that which way ſoever theſe Experiments are tried, the Objections riſing from the Friction do no way ſerve to confirm the new Opinion, becauſe they ſhew that (upon account of the Friction) the Heights muſt be ſomething more than in a duplicate Proportion of the Velocities, but never leſs, to give a Blow with the ſame Body in Proportion to the Velocity.

That the *Momentum* of Bodies is in Proportion to the Maſs multiplied into the Velocity, is alſo moſt evidently ſhewn from the Congreſs of elaſtic Bodies, as has been demonſtrated by Sir *Iſaac Newton* in his *Principia*, in the Corollaries to his Laws of Motion. I had often tried the Experiments there mentioned with Balls of Ivory and Balls of Glaſs, and ſome of them with two Balls of Steel, of two Ounces each, and found every thing anſwer, allowing for the want of perfect Elaſticity in the Bodies. But now upon this Occaſion, as the Objections to the receiv'd Opinion were renewed, I was willing to repeat the Experiments with the utmoſt Accuracy; and therefore, as Ivory Balls

are

are not equally denſe in all their Parts, and Glaſs Balls break after two or three ſtrokes; I cauſed Balls to be nicely turned of Steel, and made as hard (as the Workmen call it) that is, as elaſtick as poſſible, and the Weights of them were preciſely as follows: Two Balls of twelve Ounces Troy each, one of ſix Ounces, one of three, one of two, and one of Eight-penny-weight. Then making Pendulums of theſe Balls, and hanging them upon the Machine contrived by *Mariotte* for the Congreſs of Bodies, and lately improved by Dr. *Graveſande*[a], I meaſured 57 ½ Inches between the Center of Suſpenſion and the Center of Gravity of the Balls, and then every Degree of the Circle they deſcribed in their Oſcillation was one Inch, and the Degrees being marked upon a line of Chords on a Braſs Ruler above the Balls, by their Strings ſucceſſively covering the croſs Lines of Diviſion, the Degrees that the Balls fell from, and thoſe to which they roſe, were very diſcernable to an Eye placed at a convenient Diſtance.

EXPERIMENT VII.

I took the two Balls 12, and removing each from the loweſt Point of their equal and reſpective Circles, up to 4 Inches, or 4 Degrees, I let them fall ſo that they met at bottom, and were both reflected again to 4, the Place from whence they fell.

EXPERIMENT VIII.

Every thing elſe being as before, inſtead of one of the Balls 12, I took the Ball 6, then letting 6 go from

[a] Introd. No. 170. Vol. 1.

8 Degrees,

8 Degrees, and 12 from 4, after Reflection 12 was driven up again to 4, as before.

EXPERIMENT IX.

The Ball 3 falling from ſixteen Degrees met the Ball 12 that fell ſtill from 4, and after Reflection 12 went up again to 4.

EXPERIMENT X.

The Ball 2 falling from 6° and 12 from 1°, 12 was reflected to 1, and when 2 fell from 12 Degrees, and the Ball 12 from 2, the 12 was reflected to 2.

EXPERIMENT XI.

The Ball of eight Penny Weight (which weigh'd but $\frac{1}{30}$ of the Ball 12) falling from fifteen Inches or Degrees, rais'd up 12 (that fell from half a Degree) to the ſame Place again.

In all theſe Experiments the Error, or want of perfect Reflection, was greater in the little Balls than in the great ones, on account of their going thro' a greater *Arc* of a Circle, whereby they deviated more from a Cycloid than the great ones; as likewiſe on Account of the Reſiſtance of the Air, which muſt be greater becauſe of the little Balls going through a greater *Arc*, moving with more Velocity, being ſuſpended by a String as thick as that of the great ones, and having more Surface in Proportion to their Weight. But all the Errors do not bring the *Phænomena* any thing near what they ought to be, if the Force of the Bodies was as the Square of their Velocities multiply'd into their Maſſes, for then the Ball 12 would have been driven to Heights very different from what it roſe up to.

In the eighth Experiment, the Ball 12 ſhould have riſen to near five Inches and three quarters, for the Ball 6 falling with the Velocity, 8 muſt have had its Force $= 8 \times 8 \times 6 = 384$; and then, that the Ball 12 might have the ſame Force or Quantity of Motion, it muſt riſe near to 5, 7 becauſe $5{,}7 \times 5{,}7 \times 12 = 389{,}88$.

In the ninth, 12 ſhould have riſen to 8; for the Ball 3 muſt have had its Force $= 16 \times 16 \times 3 = 768$, and if 12 receiv'd its whole Force it muſt have riſen to 8 becauſe $8 \times 8 \times 12 = 768$.

In the ſecond Part of the tenth Experiment, 12 ſhould have riſen to near 5, becauſe $12 \times 12 \times 2 = 288$, and $5 \times 5 \times 12$ is but 300.

In the eleventh, the Ball 12 (thirty times heavier than the little one) muſt have gone to $2\frac{1}{4}$ Inches, becauſe the *Momentum* of the little Ball being $= 15 \times 15 \times 1 = 225$, that of the Ball 12 muſt be $= 2{,}75 \times 2{,}75 \times 12 = 226$ &c.

It may be here alledg'd, that one ought to ſubtract the *Momentum*, with which the great Ball comes upon the little one; but that won't mend the Matter much, tho' indeed the Difference will be leſs. For,

In the eighth Experiment, if we ſubtract $4 \times 4 \times 12 = 192$ from 389,88 there will remain 197,88, and the Ball 12 will go but to 4; but then in Experiment 9, if we ſubtract the ſame N° 192 from 768, we ſhall have 576, which would carry 12 to near ſeven Degrees, becauſe $7 \times 7 \times 12 = 588$.

In the tenth Experiment, there is only 48 to be ſubtracted; and in the eleventh only 15; and therefore the Velocity of 12 will very much fall ſhort of what is agreeable to the new Opinion.

After the Experiments made, and what has been ſaid, till theſe Conſequences are overthrown, no notice ought to be taken of any Objections, or new Experiments.

ments. But to give the Objectors all poſſible Satisfaction, I ſhall, in another Paper, endeavour to ſhew the Fallacies of the Arguments, and ſolve the *Phænomena* of the Experiments made; ſhewing, both by Reaſon and Experiment, that the Facts ought to be as they are, in conſequence of the receiv'd Opinion and Laws of Reſiſtance.

FINIS.

ERRATUM.

PAG. 250, in the Note for, de la Hire, &c. *read* Milnes, ibid.

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