Received November 16, 1767.

III. *An Essay on the Force of Percussion, by* William Richardson, *M. D. communicated by* William Heberden, *M. D. F. R. S.*

Read Feb. 18, 1768.

WHEN we consider the extraordinary advancement in natural philosophy, from the surprising discoveries of the great Sir Isaac Newton, and other ingenious men, who have followed his example; it may afford matter of the greatest wonder, to find the most acute philosophers still contending, whether the force of percussion be in proportion to the velocity of bodies in motion, or the squares of those velocities.

Sentiments so opposite in their nature, and so strongly supported by their respective advocates, may well make a cautious person suspect, that nothing is to be discovered with certainty in the operations of nature. In which opinion he may be the more confirmed from this consideration, that the present dispute is not about objects far removed from our observation, but such actions of bodies as do constantly occur to our senses, and which without difficulty may be reduced to experiment.

What then can be the reason, after such variety of experiments have been made, why this matter is not, before this time, brought to a decision? The

 fault

fault cannot be in the experiments themſelves, the chief of which have been often repeated, and that in the moſt accurate manner: it muſt, therefore, be in the uſe which has been made of them, in the groundleſs inferences which have been drawn from them.

Thoſe who have wrote beſt on this ſubject (whether in ſupport of the velocities, or the ſquares of velocities) ſeem to me to have inferred more from the principle they maintain than what they bring ſufficient arguments to juſtify; by which means they blend truth with error, and the more they endeavour to illuſtrate their reſpective doctrines, the more they render them perplexed and confuſed. For the truth of what I have advanced, I appeal to the two following inſtances.

Thoſe who maintain that the force of percuſſion is as the velocity of the ſtriking bodies, when they account for the impreſſions made in ſoft bodies (which are found, by experiment, to be as the ſquares of the velocities), inform us, that the time ought to be taken into the account; which being as the velocity of the impinging body, the impreſſion will of courſe be as the time into the velocity, or (which is the ſame thing) as the ſquare of the velocity.

But this, in my mind, is to aſſert more than what can be clearly demonſtrated: for as action and reaction are equal, the more forcibly one body acts upon another, the greater muſt be the reſiſtance it meets with; and whatever be the time of its acting, ſuppoſing its force to be given, the effect produced muſt ſtill remain the ſame.

To

To which I muſt further add, that ſome of the moſt learned and zealous advocates for time being taken into the account, have not agreed among themſelves whether it be in a direct or reciprocal proportion of the velocity[a].

Thoſe who, on the contrary, inſiſt that the force of percuſſion is in proportion to the ſquares of the velocity, finding from experiment that in ſoft bodies the velocity after percuſſion falls ſhort of this eſtimate, would make us believe, that in compreſſing the parts of thoſe bodies, a certain degree of force muſt neceſſarily be loſt, which, being added to what remains after percuſſion, will ſufficiently confirm the truth of their doctrine.

To this I reply, that the parts of ſoft bodies are, indeed, removed out of their places by the ſtroke, and that ſome motion is loſt in the impinging body, being communicated to the parts of the ſoft body it ſtrikes upon; but theſe parts cannot loſe their motion any other way, than by communicating it to other parts, or by the force accruing to the whole body.

How then are theſe different effects to be accounted for, and in what manner are they to be deduced from the ſame cauſe? This diverſity of appearances, I have for ſome time ſuſpected, might proceed from the nature of coheſion: that while the force of percuſſion produced an effect on the whole maſs of matter which receives the ſtroke, in proportion to the velocity of the impinging body; it might, at the

[a] Dr. Pemberton, Philoſophical Tranſactions, N° 371. p. 57. Dr. Clarke, Philoſophical Tranſactions, N° 401. p. 382.

ſame time, in ſeparating the cohering parts from each other, produce an effect in proportion to the ſquare of the velocity.

Into which way of thinking I was firſt led from the following obſervations; that a chord, which would bear a very ſtrong pull, might eaſily be broken by giving it a ſudden jerk; as alſo that the weight of a hammer did not contribute ſo much in driving a nail, as the quickneſs of the motion given it by the driver.

In order to make a further diſcovery, whether or no this my ſuppoſition was really founded in nature; I determined firſt to make experiments on ſuch ſoft bodies as have a conſiderable degree of coheſion; and then to try thoſe bodies, when dried and reduced to powder, and by that means deprived of their coheſion; which experiments, when compared with each other, would, I flattered myſelf, give me an inſight into this intricate affair, and at the ſame time diſcloſe that beautiful ſimplicity, which nature obſerves in all her operations.

My apparatus for making the experiments conſiſts of four balls exactly ſpherical, two iron branches, and a ſmall lead ciſtern.

The balls are each of them two inches in diameter; two are of braſs, and two of box-wood; one of each ſort is ſolid, and the other hollow; that which is hollow is only half the weight of the ſolid one, and may be opened by means of a ſcrew in the middle.

The iron branches are to give the balls their proper directions; they have each of them a ſmall braſs pully in the fore part, and in the hind part a kind of hook, which faſtens them to ſtaples at different heights; one of the branches is two inches long, and the other four inches, excluſive of pullies; by which means the balls when let fall are directed to different parts of the ſurface they ſtrike upon.

The lead ciſtern is of an oblong form, that the matter therein contained may, at the ſame time, receive two diſtinct impreſſions; either when balls of different weight are let fall, or the ſame ball is let fall from different heights; its length is ſix inches, its breadth four inches, and its depth two inches.

The matter I have found beſt ſuited to the purpoſe is ſtiff clay, tempered in ſuch a manner as to be ſmooth and uniform, with the ſame reduced to powder, after having been baked in an oven, as alſo after having (by a ſtill ſtronger heat) been converted into brick.

The ciſtern is by turns filled with theſe different materials, which are to be cloſely and uniformly preſſed down, ſo as to leave the ſurface quite level: in effecting which, great caution is required, more particularly in regard to the powders; as they will not diſtinctly retain the impreſſions, unleſs they have ſome ſmall degree of moiſture, or be very cloſely preſſed down; in both which caſes they acquire ſuch a degree of coheſion, as of courſe muſt render the experiments more or leſs imperfect.

Things being thus prepared, in order to try the neceſſary experiments, I fixt the ſtaples, and by their means the branches, at the following heights, viz two feet, four feet, and eight feet; the reſult of which experiments was as follows.

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When the braſs balls, in weight to each other as two to one, were let fall on tempered clay, from four feet and eight feet reſpectively, the impreſſions made were on various trials found to be equal.

When the wood balls, being to each other as two to one, were let fall from the ſame heights, on dried clay pulveriſed, that from four feet generally made the deeper impreſſion.

When the wood balls were let fall from the ſame heights on brick-duſt, that from four feet conſtantly made the deeper impreſſion.

When the lighter braſs ball was let fall on tempered clay, from two feet and eight feet, the impreſſions were to each other as one to four.

When the lighter wood ball was let fall on dried clay pulveriſed, from the ſame heights, the impreſſions were (ſo far as the eye could judge) nearly in the proportion of one to three.

When the ſame ball was let fall on brick-duſt, from the like heights, the impreſſions were not much ſhort of the proportion of one to two.

From theſe experiments it plainly appears: Firſt, That the impreſſions made in ſoft clay are in proportion to the heights, from whence the balls are let fall, conſequently as the ſquares of their reſpective velocities. Secondly, That the impreſſions, in pulveriſed clay, recede conſiderably from that proportion, being as it were in the medium between the ſquares of the velocities and the velocities themſelves. Thirdly, That the impreſſions in brick-duſt are nearly in a ſubduplicate proportion of the heights from whence the balls are let fall, conſequently vary but

but little from the proportion of the velocities acquired.

Whence I ſhould apprehend it clearly follows; that the impreſſions made in ſoft bodies, by hard ones ſtriking upon them, do vary from each other, according to the degree of coheſion in the reſpective ſoft bodies; and that the impreſſions would be in exact proportion of the velocities, could their form be perfectly retained by bodies quite void of coheſion.

Nothing, however, being more evident to me than that actions ought to be meaſured by their effects, and at the ſame time fully depending on the accuracy of the experiments, I am determined to reſt this important point entirely upon them. Shall not, therefore, attempt any illuſtration in the mathematical way, leſt, by too far indulging a favourite opinion, I ſhould bewilder myſelf in intricate calculations. Much leſs ſhall I endeavour to eſtabliſh my doctrine on metaphyſical principles, which ſeem to me in themſelves too obſcure, to throw any clear light on ſubjects of this nature.