

VII. *An Account of a Book intituled, New Principles of Gunnery, containing the Determination of the Force of Gunpowder; and an Investigation of the resisting Power of the Air to swift and slow Motions; by B. R. F. R. S. as far as the same relates to the Force of Gunpowder.*

Read April 14.
and 21. 1743.

THIS Treatise contains Two Chapters. The First treats of the Force of Gunpowder, and the Velocities communicated to Bullets by its Explosion: The Second considers the Resistance of the Air to Bullets and Shells moving with great Velocities; and endeavours to evince, that this Resistance is much beyond what it is generally esteemed to be; and consequently that the Tract described by the Flight of these Projectiles, is very different from what is usually supposed by the modern Writers on this Subject.

The principal Points endeavoured to be established in the First Chapter are these, “ That the Force of
“ fired Gunpowder is no more than the Action of a
“ permanent elastic Fluid, which is produced by the
“ Explosion; that this Fluid observes the same Laws
“ with common Air in their Exertion of its Pressure
“ or Elasticity;” and consequently, “ That the Ve-
“ locities communicated to Bullets by the Explosion
“ may be easily computed from the common Rules,
“ which are established for the Determination of the
“ Air’s Elasticity.”

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The Two first Propositions contain the Proofs that a permanent elastic Fluid is constantly generated in the Explosion of Gunpowder; this is evinced by well-known Experiments daily repeated, and acquiesced in by all who have frequented the usual Courses of Experimental Philosophy, of which these Experiments generally make a Part; so that the Author presumes he may consider this Point as incontestably established, at least he has never yet met with any who have questioned it.

The Third Proposition is, That the Elasticity of this Fluid produced by the Firing of Gunpowder, is, *ceteris paribus*, directly as its Density; and the Experiment by which this was confirmed, was letting fall separately Two Quantities of Powder, the one double the other, on a red-hot Iron included in an exhausted Receiver; and it appeared by the Descent of the Mercury, that the Elasticity of the Fluid produced from the double Quantity of Powder, was nearly double the Elasticity of that produced from the single Quantity; that is, the Elasticity was nearly as the Density of the Fluid.

But it may perhaps be thought, that a single Experiment is too slender a Foundation on which to build so material a Principle, since all subsequent Reasonings on the Force of Powder in some measure depend on it. In Reply to this it may be said, that the Author recited this single Experiment on account of the great Quantity of Powder made use of in it, which was Three-sixteenths of an Ounce; but that he had really made many more equally conclusive, which he thought it unnecessary to mention. However, those who doubt of this Proposition, may
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satisfy themselves herein by some Experiments made by the late Mr. *Hauksbee* before this SOCIETY, though with a different View; where, by the firing of Twenty-six Quantities of Powder successively, the mercurial Gage was sunk from Twenty-nine Inches and an half, to Twelve Three-fourths; for by comparing these Experiments together, and making the necessary Allowances, it will be found, that the Elasticity was nearly proportional to the Density in all that Variety of Densities.

In this Proposition, the Analogy between the Fluid produced by the Explosion of Powder and common Air, is established thus far, that they exert equal Elasticities in like Circumstances; for this Variation of the Elasticity, in proportion to the Density, is a well-known Property of common Air. But other Authors, who, since the Time of Mr. *Boyle*, have examined the factitious elastic Fluids produced by Burning, Distillation, &c. have carried this Analogy much farther, and have supposed these Fluids to be real Air, endued with all the Properties of that we breathe; particularly the Reverend Dr. *Hales*, who has pursued this Examination with the greatest Exactness, in a Series of the best contrived Processes, constantly affixes the Denomination of Air to these factitious Fluids, he having found, that their Weight is the same with that of common Air, and that they dilate with Heat, and contract with Cold; and that they vary their Densities under different Degrees of Impression in the same Proportion with common Air; and from hence, and other Circumstances of Agreement between them, he supposes them to be of

the same Nature with Air, and conceives them to be fitly designed by the same Name.

But so perfect a Congruity between these factitious Fluids and Air is not necessary for the Purposes of this Treatise. The fundamental Positions of this First Chapter supposing no more, than that the Elasticity of the Fluid produced in the Explosion of Gunpowder is always, *ceteris paribus*, as its Density; and that the Force of fired Gunpowder is only the Action of that Fluid modified according to this Law. It has been already mentioned, on what Grounds the First of these Principles hath been asserted, as contained in the Third Proposition; and it remains to explain the Reasons urged for the Support of the last in the Eight succeeding Propositions.

The Law of the Action of this Fluid being determined, Two Methods offer themselves for investigating the absolute Force of Powder on the Bodies it impels before it. The first by examining the Quantity of this Fluid produced by a given Quantity of Powder, and thence finding its Elasticity at the Instant of the Explosion; the other by determining the actual Velocities communicated to Bullets by known Charges, acting through Barrels of different Dimensions. The First is the most easy and obvious, but the Second the most accurate Method; and therefore the Author has separately pursued each, and he has found, that their Concurrence has greatly exceeded his Expectation, and thereby both of them receive an additional Confirmation.

The Quantity of the elastic Fluid, produced by the Firing of a given Quantity of Powder, is determined
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by firing it in an exhausted Receiver, and observing how much the mercurial Gage subsides thereby, making a proper Allowance for the Increase of its Elasticity from the Heat of the included hot Iron. But then, as the Subsiding of the Mercury is not measured till the Flame of the Powder is extinguished, and the Fluid is reduced somewhat near the Temperature of the external Air, it is evident, that the Elasticity thus estimated is much short of what it really was in the Instant of Explosion; and therefore, to obtain that Elasticity, which is the Force sought, it is necessary to make some Estimate of the Increase of the Elasticity of the Fluid by the Fire and Flame of the Explosion. For this Purpose it is examined in the Fifth Proposition, how much the Elasticity of common Air is increased by a Degree of Heat equal to that of Iron beginning to grow white hot; and it is found, at a Medium, to be thereby augmented something more than Four times; whence, as the Fluid produced by any Quantity of Gunpowder takes up, when compressed by the Weight of the incumbent Atmosphere, a Space something less than 250 times the Bulk of the Powder; it follows, that if its Elasticity in the Instant of Explosion be supposed to be increased in the same Proportion with that of the Air last-mentioned, it becomes by this means about 1000 times greater than the Pressure of the Atmosphere; that is, conceiving it to be contained in that Space only which the Powder occupied before it was fired.

Those who have not been conversant in these Experiments, may possibly suppose, that the Elasticity of the Powder at the Instant of Explosion may be im-

mediately known by the First sudden Descent of the Mercury: But many Circumstances concur to render this Method impracticable; amongst the rest it must be remembered, that some Air is constantly left in the Receiver, which is heated by the Blast, and unites its Effects in the First Instant with the Action of the Powder: Besides, the First Descent may be varied, by varying the Tube, although all things else remain unchanged.

By the Method hitherto described, it is collected, that the Elasticity of the Fluid produced from fired Gunpowder, when contained in the Space which was taken up by the Powder before the Explosion, is about 1000 times greater than the Elasticity of common Air, or, which is the same thing, 1000 times greater than the Pressure of the Atmosphere.

But, besides the Determination of the Quantity of Fluid produced from a given Quantity of Powder, (the Method on which this Deduction is founded) there is another Method of discovering the same thing, which, though less obvious, is yet (as hath been already observed) more accurate: That is, by examining the actual Velocities communicated to Bullets by the Explosion of given Charges in given Cylinders; and this is the Subject of the 7th, 8th, and 9th Propositions.

And First, it is evident, that this Examination cannot take place, unless a Method of discovering the Velocities of Bullets be previously established. Now the only known Means of effecting this was, either by observing the Time of the Flight of Bullets through a given Space; or by finding their Ranges when they were projected at a given Angle, and thence computing

puting their Velocity on the Hypothesis of their parabolic Motion. The First of these Methods was often impracticable, and in all great Velocities extremely inaccurate, both on account of the Shortness of the Time of their Flight, and the Resistance of the Air. The Second is still more exceptionable, since, by reason of the Air's Resistance, the Velocities thus found may be less in any *Ratio* given, than the real Velocity sought. Now, to avoid these Difficulties, the Author has invented a Method of determining the Velocities of Bullets, which may be carried to any required Degree of Exactness, and is no-ways liable to the forementioned Exceptions; for, by this Invention, the Velocity of the Bullet is found in any Point of its Track, independent of the Velocity it had before it arrived at that Point, or of the Velocity it would have after it had passed it: So that not only the original Velocity, with which it issues from the Piece, is hence known, but also its Velocity, after it has passed to any given Distance; and therefore the Variations of its Velocity from the Resistance of the Air may be also ascertained with great Facility. The Machine for this Purpose is described in the 8th Proposition, and the Principle it is founded on is this simple Axiom of Mechanics; *That if a Body in Motion strikes on another at Rest, and they are not separated after the Stroke, but move on with one common Motion, then that common Motion is equal to the Motion with which the First Body moved before the Stroke*: Whence, if that common Motion and the Masses of the Two Bodies are known, the Motion of the First Body before the Stroke is thence determined. On this Principle then it follows, that
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the Velocity of a Bullet may be diminished in any given *Ratio*, by its being made to impinge on a Body of a Weight properly proportioned to it; and hereby the most violent Motions, which would otherwise escape our Examination, are easily determined by these retarded Motions, which have a given Relation to them. Hence then, if a heavy Body greatly exceeding the Weight of the Bullet, whose Velocity is wanted, be suspended so that it may vibrate freely on an Axis in the manner of a Pendulum, and the Bullet impinges on it when it is at Rest, the Velocity of the Pendulum after the Stroke will be easily known by the Extent of its Vibration, and from thence, and the known Relation of the Weight of the Bullet and the Pendulum, and the Position of the Axis of Oscillation, the Velocity with which the Bullet is impinged will be determined, as is largely explained in the 8th Proposition. Where note, that there is a Paragraph by Mistake omitted in that Proposition, which should increase the Velocity there found in the duplicate Proportion of the Distances of the Points of Oscillation and Percussion from the Axis of Suspension; but this only affects that particular Number, for it was remembered in the Computations of the succeeding Experiments, the Numbers of which are truly stated.

It being explained how the Velocities of Bullets may be discovered by Experiment: The next Consideration is, from those Velocities to determine the Force which produced them.

And the Author thought, the best Method of effecting this was by computing what Velocities would arise from the Action of fired Powder, supposing its
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Force to be rightly assumed by the Process in the preceding Part; that is, supposing the Elasticity of the Fluid thence arising to be at first 1000 times greater than that of common Air; for then, by comparing the Result of these Computations with a great Number of different Experiments, it would appear whether that Force was rightly assigned; and if not, in what Degree it was to be corrected.

Preparatory to this Computation, the Author assumes in his 7th Proposition these Two Principles:

1st, That the Action of the Powder on the Bullet ceases as soon as the Bullet is got out of the Piece.

2^{dly}, That all the Powder of the Charge is fired, and converted into an elastic Fluid, before the Bullet is sensibly moved from its Place.

And in the annexed Scholium he has given the Arguments and Experiments which induced him to rely on these Postulates, all which is necessary at present to discuss more at large.

If the Force of Gunpowder was supposed capable of being determined with the same Accuracy and Rigour, which takes place in Subjects purely Geometrical, the First of these Postulates would be doubtless erroneous, since it cannot be questioned but the Flame acts in some Degree on the Bullet after it is out of the Piece.

But it is well known, that in Experimental Subjects no such Preciseness is attainable; for those versed in Experiments perpetually find, that either the unavoidable Irregularities of their Materials, or the Variation of some unobserved Circumstance, occasion very discernible Differences in the Event of similar Trials. Thus the Experiments made use of
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for confirming the Laws of the Collision of Bodies, have never been found absolutely to coincide either with the Theory, or with each other. The same is true of the Experiments on the Running and Spouting of Water and other Fluids, and of the Experiments made by Sir *Isaac Newton*, for the Confirmation of his Theory of Resistances; in which, though they often differ from each other, and from that Theory by One-twentieth, One-tenth, and even sometimes One-fifth Part, yet those small Inequalities have never been urged as invalidating his Conclusions, since, in Experiments of that Nature, it was rather to be wondered at, that the Difference between the different Trials was so small.

And if some minute Irregularities are the necessary Concomitants of all complicated Experiments, it may be well supposed, that the Action of so furious a Power as that of fired Gunpowder, which visibly agitates and disorders all Parts of the Apparatus made use of, cannot but be attended with sensible Variations; and it in Fact appears, that in the Table of Experiments inserted in the 9th Proposition, the Velocities of Bullets fired from the same Piece, charged with the same Powder, and all Circumstances as near as possible the same, do yet differ from each other by One-fiftieth, One-fortieth, and sometimes more than One-thirtieth, of the Whole; and yet the Author does not conceive, that these small Differences are any Exception to the Conclusiveness of his Principles; but he presumes, that had he pretended, without disclosing his Method, to have computed the Force of Powder, and the Velocities of Bullets, in different Circumstances, to a much less Degree of Accuracy than

than this, he should have been censured, as boasting of what would have been thought impracticable.

If then the Action of the Flame on the Bullet after it is out of the Piece, is so small as to produce no greater an Effect than what may be destroyed by the inevitable Variations of the Experiments, the neglecting it intirely, and supposing no such Force to take place, is both a convenient and a reasonable Procedure: For indeed, without the Assumption of Postulates of this kind, it were impossible to have proceeded one Step in Natural Philosophy, since no Mechanic Problem hath been ever solved, in which every real Inequality of the moving Force hath been considered.

Now what induced the Author to suppose, that this Postulate (though not rigorously true) might be safely assumed, was the Consideration of the spreading of the Flame by its own Elasticity, as soon as it escapes from the Mouth of the Piece: For by this means he conceived that the Part of it which impinged on the Bullet might be safely neglected, although the Impulse of the intire Flame was a very remarkable Force.

With regard to the Second Postulate, " That all
 " the Powder is fired before the Bullet is sensibly
 " moved from its Place ;" it is incumbent on the Author to be still more explicit, as this SOCIETY did some time since appoint a Committee for examining this very Position, who, after making a great Number of Experiments, have determined, * *That all the Powder is not fired before the Bullet is sensibly*

* See these *Transactions*, N^o 465. p. 172, &c.

moved from its Place; and they have at the same time assigned the Quantities remaining unfired under different Circumstances.

These Determinations of the Committee are most true; but the Author must observe, that from the Experiments recited by them, and the Quantity of unfired Powder, which they collected, it may be concluded, that in a Barrel of a customary Length, charged with the usual Quantity of Powder, the Deficiency of Velocity occasioned by the Powder remaining unfired will be scarcely sensible; and in the shortest Barrel ever used by the Author, where the Space the Bullet was impelled through was not Five Inches, and where of course this Deficiency of Velocity ought to be the greatest, it cannot amount to One-thirtieth Part of the Whole; and consequently this Postulate, though not rigorously true, may yet be safely assumed, in the investigating the Effects of Powder. But before this is more particularly examined, it is necessary to explain the Opinions, which have formerly taken place on this Subject.

Those who have hitherto wrote on the Manner in which Powder takes Fire, have supposed it to be done by regular Degrees; the First Grains firing those contiguous, and they the next successively; and it has been generally thought, that a considerable Time was employed in these various Communications: For Mr. *Daniel Bernoulli*, in his excellent *Hydrodynamica*, has concluded from some Experiments made at *Petersburgh*, that the greatest Part of the Charge escapes out of the Piece unfired, and that the small Part, which is fired, does not take Fire till it is near the Mouth. Many Theories too have been
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composed on the Time of the Progress of the Fire amongst the Grains, and the different Modifications which the Force of Powder did thence receive; and it has been generally conceived, that the proper Lengths of Pieces were determinable from this Principle; " That they should be long enough to give " Time for all the Powder to fire."

But the Author being satisfied, that no such regular and progressive Steps could be observed in the Explosion; and having found, that by loading with a greater Weight of Bullet, and thereby almost doubling the Time of the Continuance of the Powder in the Barrel, its Force received but an inconsiderable Augmentation; and finding too, that doubling or trebling the usual Charge, the Powder thus added always produced a correspondent Effect in the Velocity of the Bullet; and discovering likewise in a Piece near Four Feet in Length, charged with an usual Charge of Powder, that the Velocity communicated to the Bullet, during the First Three Inches of its Motion, was full half the Velocity which it acquired in its whole Passage through the Barrel, and that the Elasticity or Force of the Powder, in the First Three Inches of its Expansion, was, at a Medium, near Eight times greater than in the last Two Feet of the Barrel; he concluded from all these Circumstances, that the Time employed by the Powder in taking Fire was not necessary to be attended to in these Computations; but that the whole Mass might be supposed to be kindled, before the Bullet was sensibly moved from its Place.

And the Experiments reported by the Committee are the strongest Proofs, (as far as they extend) that

Powder is not fired in the progressive Manner usually supposed; for when the short Barrel was charged with 12 *dwt.* and with 6 *dwt.* respectively, the Quantity of Powder which was collected unfired from 12 *dwt.* did not exceed by 3 Grains, at a Medium, what was collected from 6 *dwt.* although the Bullet was a less Time in passing through the Barrel with 12 *dwt.* than with 6 *dwt.* it having a less Way to move; consequently the Quantity remaining unfired of the 6 *dwt.* did not continue unfired for want of Time, since, when the Piece was charged with 12 *dwt.* the additional 6 *dwt.* was consumed in a shorter Time.

And again, when the Barrel was so shortened, that the Bullet, being placed close to the Wad, lay with its outer Surface nearly level with the Mouth of the Piece, so that it had not more than half an Inch to move before the Flame would have Liberty to expand itself; yet, even in this short Transit of the Bullet, only 2 *dwt.* 1 $\frac{1}{2}$ *gr.* was collected unfired, at a Medium; which is about $\frac{1}{6}$ of the whole Charge, or, if properly reduced, not more than $\frac{1}{12}$ of the Charge: An obvious Confutation of the gradual Firing of the Powder in its Passage through the Barrel, and an easy Proof, how small an Error will be occasioned by supposing the whole Charge to fire instantaneously, since the Error in the Velocity of the Bullet, arising from a Deficiency of $\frac{1}{12}$ of the Charge, is $\frac{1}{24}$ of that Velocity only.

I say, that the $\frac{1}{6}$ of the Charge, which remained unfired, amounts to no more than $\frac{1}{12}$ when it is reduced at it ought. This Reduction is founded on the

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the other Experiments reported by the Committee, and on the Circumstances of those Trials on which the Author founded the present Postulate. The Author has supposed the Powder, on which he reasons in this Treatise, to be of the same sort with that made for the Service of the Government, a Parcel of which he was favoured with by Mr. *Walton*. But this he chiefly kept for a Standard, and generally used other Powders, which, on Examination, he found to be of equal Force. These Powders were of a very small and even Grain, and the Committee have found, that by sifting the Government Powder, and making use of the smaller Grains, the Quantity remaining unfired was less, at a Medium, in the *Ratio* of 5 to 3, than when it was used without sifting.

And again, it was found by extracting the Saltpetre from the Powder collected unfired, that there was less Saltpetre contained in it than in real Powder, and this nearly in the *Ratio* of 9 to 7: These Two Proportions compounded make the Proportion of 15 to 7, and in this Proportion must the Quantities of Powder collected unfired be reduced, in order to determine the Quantities of real Powder remaining unfired, in similar Experiments made by the Author.

And from hence it follows, that in the Experiments made with a Barrel $5 \frac{1}{2}$ Inches in Length, where the Ball had not 3 Inches to move, and where the Irregularity arising from the Powder unfired ought to have been the most sensible, the Quantity of real Powder collected unfired from a Charge of 12 *dwt.* would have been no more than 16 Grains at a Medium, or $\frac{1}{18}$ of

of the whole Charge; and it being found by Experiment, that the Velocities of Bullets placed in the same Situation vary in the subduplicate Proportion of the Charges, the Deficiency of Velocity arising from the Loss of the $\frac{1}{18}$ of the Charge would be about $\frac{1}{36}$ of the whole Velocity only, which, in the present Case, is not $\frac{2}{10}$ of an Inch in the Chord of the Arch described by the Pendulum measuring the Velocity, and is a less Difference than what frequently occurs in the exactest Repetition of the same Experiments.

Other Circumstances occur, which reduce the Inequality arising from the unfired Powder still lower; but it is thought, that this is fully sufficient to justify the Postulate in Question, especially as, in all Cases of real Use, the Length of the Barrel in proportion to the Quantity of the Charge will be much greater than in the present Instance: Whence the Author presumes, that, in computing the Velocities communicated to Bullets by the Action of Powder, it may be safely supposed, that the whole Charge is fired before the Bullet is sensibly moved from its Place; at least there is no Foundation, from the Experiments made on this Subject by the Committee, to suspect that when small-grained Powder is made use of, any greater Irregularity will arise from the Application of this Supposition, than what would otherwise take place from the Intervention of unavoidable Accidents.

It has been thought necessary to discuss more at large these Two Postulates, because the last of them being almost in the very Words of one of the Questions proposed to be examined by the Committee of
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this SOCIETY, and having by them been determined in the Negative, those who have not attended to this Subject might suppose, that thereby the Author's Principles were intirely overturned: Now this would be a great Injustice to him, since he has not relied on this Postulate as rigorously true; for he knew, and has himself taken notice in the present Proposition, that some of the Powder escapes unfired; and he has there made some Conjectures on the Cause of it; but, without insisting on the Reality of those Conjectures, he adds, that, " Be that as it may, the Truth of our Position cannot in general be questioned."

And though it appears from what has been already said, that the Experiments recited by the Committee rather confirm than invalidate the general Sense of that Postulate; yet it is but Justice to own, that they are a full Confutation of the Conjectures of the Author in relation to the Cause why some Part of the Powder comes out unfired; for the Author has supposed, after *Diego Ufano*, that the Part which thus escaped, was scattered in the Barrel, and not rammed up with the rest, or else that it was of a less inflammable Composition: But the Experiments made on this Occasion intirely destroy this Supposition.

As this, or any other Conjecture on the Cause of this Accident, (for it plainly appears not to be for want of Time only) has nothing to do with the general Reasoning of the present Treatise, it is not necessary to enter into it in this Place; but it may not be improper to mention, that, on computing the Quantities of Powder collected from different Charges, one of the Committee was led to conjecture, that what was thus collected was only Parts
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of Grains that had been fired, but were extinguished by the Blast before they were intirely consumed. This Conjecture is strengthened by the extreme Minuteness of the Particles of all the Powder which was collected, and from the Deficiency of the Saltpetre found in it on Examination: It may be added too, that the Author, by gradually heating a Parcel of Powder, hath set it on Fire, and blown it out again, for at least a Dozen times successively; and he will undertake to repeat the Experiment at any time, if it should be doubted of.

The Postulates hitherto discussed are preparatory to the 7th Proposition. That Proposition is employed in computing the Velocity which would be communicated to a Bullet in a given Piece by a given Charge of Powder, on the Principles hitherto laid down, that is, supposing the Elasticity of fired Powder to be at first 1000 times greater than that of common Air.

In the 9th Proposition these Computations are compared with a great Number of Experiments, made in Barrels of various Lengths, from Seven Inches to Forty-five Inches, and with different Quantities of Powder, from 6 *dwt.* to 36; and the Coincidence between the Theory and these Experiments is very singular, and such as occurs in but few philosophical Subjects of so complicated a Nature.

By this Agreement between the Theory and the Experiments, each Part of the Theory is separately confirmed; for by firing different Quantities of Powder in the same Piece, and in the same Cavity, it appears that the Velocities of the Bullet, thence arising,

arising, are extremely near the subduplicate Proportion of those Quantities of Powder, and this independent of the Length of the Piece: Whence it is confirmed, that the Elasticity of fired Powder in various Circumstances, is nearly as its Density; and this does not only succeed in small Quantities of Powder, and in small Pieces, but in the largest likewise, under proper Restrictions; at least there are Experiments which could not be influenced by this Theory, where the Quantities of Powder were above 100 times greater than what are used by this Author, and in these Trials this Circumstance takes place to sufficient Exactness.

It is presumed then, that by this Theory a near Estimate may be always made of the Velocities communicated to Shells or Bullets by given Charges of Powder; at least these Experiments evince how truly the Velocities of small Bullets are hereby assigned; and the Author can shew by the Experiments of others, that in a Shell of Thirteen Inches Diameter, impelled by a full Charge of Powder, the same Principle nearly holds: It is true indeed, that when the Charge is much smaller than the usual Allotment of Powder, there are some Irregularities, which are particularly mentioned at the End of the 9th Proposition, to which Head too, perhaps, must be referred the Experiments made by the Committee on the Effect of different small Chambers; but in the customary Charges, the Velocities of Bullets resulting from all the Experiments hitherto made, are really such as the Theory laid down in the preceding Part of this Treatise requires. And it appears, that these Velocities are much greater than what they have been hitherto accounted: And there are Reasons from the Theory to believe, that in Can-

non-shot the Velocities may still exceed the present Computation.

The ascertaining the Force of Powder, and thence the Velocities of Bullets impelled by its Explosion, and the assigning a Method of truly determining their actual Velocities from Experiments, are Points from whence every necessary Principle in the Formation or Management of Artillery may be easily deduced: Considering therefore the infinite Import of a well-ordered Artillery to every State, the Author flatters himself, that whatever Judgment may be formed of his Success in these Inquiries, he will not be denied the Merit of having employed his Thoughts and Industry on a Subject, which, though of a most scientific Nature, and of the greatest Consequence to the Public, hath been hitherto almost totally neglected; or, at least, so superficially considered, as to be left in a much more imperfect State than many other philosophical Researches.

With regard to the Second Chapter of this Treatise, relating to the Resistance of the Air, the Author has in his Preface mentioned his Intention of annexing to it a Series of Experiments, on the real Track of Bullets, as modulated by that Resistance: And therefore, as he proposes to complete those Experiments this Summer, unless unforeseen Accidents prevent him, he chooses to postpone any Account of the Subject of the Second Chapter till that time, when he intends to lay the Result of those Experiments before this SOCIETY, in order that any Exceptions or Difficulties relating to them, may be examined and discussed before they are published to the World.

The Reader is desired to correct an Error in the First Paragraph of the 29th Page of the Treatise here referred to, where for *Percussion* read *Oscillation*.