

NEW
PRINCIPLES
OF
GUNNERY:

CONTAINING,
The Determination of the FORCE of
GUN-POWDER,

AND
An Investigation of the Difference
in the RESISTING POWER of the AIR
to *Swift* and *Slow Motions*.

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P R E F A C E.

ABOUT a Twelvemonth since, I had some Intentions of exhibiting a public Course of Fortification and Gunnery; And tho' for Reasons not necessary to be here mentioned, I afterwards desisted from that Design; yet, as I had proceeded so far as to distribute some Manuscript Copies of the Particulars, of which I propos'd it to consist, I have thereby been in some Measure engaged in the present Undertaking:

For, as I had resolved to render this Course as compleat as I possibly could, both by large Models of different Fronts of Fortification, and their different Attacks, and by an

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experimental Exemplification of the Precepts of Gunnery with real Artillery; I found it necessary to insert under this last Head a Theory of the Force of Gunpowder, and certain Propositions relating to the Resistance of the Air, which I had discovered, and confirmed by Experiments. But these Principles being set down in the Schemes, which I delivered out, as Assertions only, without any account of the Nature of the Experiments made use of for proving them, and being liable to great Contestation on account of their Inconsistency with all the received Opinions of the Writers on this Subject; I thought it incumbent on me to clear up in a more particular Manner any Difficulties, which might have arisen about them, and to evince their Certainty by a number of unquestioned Experiments. And this has principally given Rise to the ensuing Treatise, in which the Force and varied Action of Powder is so far determined, that the Velocities of all kinds of Bullets impelled by its Explosion, may be thence computed, and the enormous Resistance of the Air to swift Motions (much beyond what any former Theories have assigned) is likewise ascertained. And on these Principles it will appear, that the Original Velocities of Bullets, when impelled by full Charges of
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of Powder, and the Track described by their Flight, are extremely different from what, the Writers on these Subjects have hitherto supposed.

As the principal Disquisitions of the following Sheets relate to the Force of Powder, and the Flight of Shells and Bullets; it may not perhaps be unacceptable to the Reader to peruse a few Particulars, relating to the Invention of Powder, and the History and Improvements of Gunnery, and its Sister Art Fortification; especially as the Nature and Purport of what, we shall hereafter advance, will receive some kind of Illustration, by being compared with the Opinions, which have formerly prevailed in these Enquiries. And tho' our immediate View is the promoting the Theory and Practice of Gunnery, yet the present Methods of fortifying are so connected with the Invention and Management of Artillery (these Arts having in some Measure given Laws to each other) that I presume a short Recital of the Rise and Changes of the modern Military Architecture, will not be impertinently prefixed to an Account of those powerful Machines, which first gave it Birth.

WITH regard to the first Invention of Baf-tions, there are many Opinions amongst Au-
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thors, it being as yet a Point undecided in what Place, and at what Time they were first put in Practice. Some have attributed this Invention to *Zisca the Bohemian*. Others to *Acemet Basbaw*, who having taken *Otranto* in the Year 1480, fortified it in a particular Manner, which is supposed to be the first Instance of the Use of Bastions (a). But these are the Positions of later Writers. Those, who wrote on the Subject of Fortification near two Centuries ago, seem to suppose, that Bastions were a gradual Improvement in the ancient Method of Building, rather than a new Thought, that any one Person could claim the Honour of. *Pasino* in particular, in the first Part of his Book, imputes the Changes in the ancient Fortifications, and the Introduction of the modern Form, to the increased Violence of the later Artillery, without pretending that it was effected at one Time, or by one Person (b).

(a) *Vid.* The Commentary of the Chevalier *Folard* on *Polybius*. Tom 3. pag. 2.

(b) *Vid.* *Discours sur plusieurs points de l'Architecture de Guerre concernant les Fortifications, tant anciennes que modernes, &c.* Par M. Aurelio de Pasino Ferrarois, architecte de tres-illustre Seigneur, Monseigneur le Duc de Buillon. Printed by *Plantin* 1579. It appears by a Copy of Verses prefixed to the Book, that this Author fortified *Sedan*.

So

So that I believe we cannot with Certainty affirm more in Relation to the Invention of Bastions, than that they were well known soon after the Year 1500. For in 1546, *Tartalea* published his *Questi & inventioni diverse*, in the sixth Book of which he mentions, that whilst he resided at *Verona* (which must have been many Years before) he saw Bastions of a prodigious Size ; some finished, and others building ; and there is besides, in the same Book, a Plan of *Turin*, which was then fortified with four Bastions, and seems to have been completed some time before.

AND tho' we cannot certainly assign the Time, when the old circular Towers were first converted into Bastions, yet in all Probability it did not long precede the Date, we last mentioned. For in the same Book the Prior of *Barleta*, who was himself a Soldier, esteems *Turin* to be impregnable ; and tells us, that this was the general Opinion of all Men of Skill ; he likewise makes it a Question, if in the fortifying of Cities, the Genius of Mankind was not arrived at its utmost Limits of Perfection ; which seems to evince that the Invention was a recent one ; and that it was greatly the Object of the Esteem and Consi-

deration of his Cotemporaries, as a new Contrivance of this kind would naturally be.

THE first Bastions, such as those of *Turin*, of *Antwerp* (a); and others of the same Age, were but small, and removed at a great Distance from each other; for at that Time it was the universal Practice to attack the Curtain, and not the Bastions. But a few Years after there were introduced Bastions much larger, and much nearer together, than what had been constructed before; as appears by the Citadel of *Antwerp*, which was built under the Direction of the Duke *D'Alva*, about 1566, and which by the frequent Encomiums on it in some early Authors, seems to have been the first Instance of this Improvement.

FROM this Period, the modern Practice of military Architecture may be supposed to have taken its Rise; most of the Improvements of the present Times, being little more than the putting in use such Methods, as were proposed within a few Years of this Æra; for many celebrated Authors flourished soon after, as (b) *La*
Treille,

(a) *Antwerp* was fortified about the Year 1540, as we learn from *Speckle*. Lib. I. chap. 10.

(b) Vid. *La Maniere de fortifier Villes, Chasteaux, et faire autres lieux forts*: mis en François par le Seigneur de *Be-rail*

Treille, Algbisi, Marchi, Pasino, and above all *Speckle*, (a) who was one of the greatest Genius's that has applied to this Art.

THE better to judge of the Pretensions of the Moderns, and the Merit of the Systems of Fortification now in vogue, we must enter into a short Discussion of the various Methods, which have been proposed for covering the Flanks, and consequently for securing the Ramparts from the Approach of an Enemy. For if it be agreed, that the principal Defence of a Fortress is its Flanks; the best Standard to judge of the Merit of any System of Fortification, is the Manner, in which it provides for the Safety of the Flanks, against the Efforts of the Enemy.

Now the most usual Contrivances for this Purpose, have been Orillons, Ravelins placed before the Curtains, Half-Moons placed before

roil François de la Treille Commissaire en l'Artillerie. A *Lyon* 1556. This Author was the first I have seen, who proposed the Retired Curtain, which has since been published by others, under the Name of the re-inforced Order.

(a) *Daniel Speckle* was Architect of the City of *Straßbourg*; he died in the Year 1589. He published a Treatise of Fortification in *German*, which was re-printed at *Leipsic* in the Year 1736.

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the Points of the Bastions, and Contregards, each of which we shall separately consider, both as to their Use and Antiquity.

THE Orillon is as old as the Bastion, since in *Turin* and *Antwerp* (mentioned above) there is a lower Flank, which is cut out of the Substance of the Bastion, and has thereby a Shoulder of a considerable Thickness, to screen it from the Field-Batteries. But besides this, the Drawings of *Pasino*, *Speckle*, &c. abound with Orillons of the same Form with those now used, the only Difference being, that the Modern ones are less massive than the Antient ones. This Invention has had the good Fortune to stand its Ground in almost every System, which has prevailed; although it be rather on the Fame of the Services, it has formerly done, than for any Advantages the Moderns have received from it. For in ancient Sieges, it was the Custom for the Besieged to have a Retrenchment behind the Breach, by which Means the Besiegers were obliged to lodge themselves on the Ruins of the Breach, in order from thence to batter the Retrenchment. In this Case the Piece or Pieces of Artillery, which being covered by the Orillon, could not be dismounted, were of wonderful Service to the Besieged.

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And many Instances might be given, where the Enemy have been hereby so gauled, after they had lodged themselves in the Ruins of the Breach, that they have desisted from their Enterprize. But as it is now no longer the Fashion to hold out, after a Breach is made in the Body of the Place, and the Ditch is near filled up, we rarely hear in the present Times of any great Feats performed by the Orillon.

THE Ravelins placed before the Curtains, (or Half-Moons as they are called in the modern Systems) were intended to protect the Flanks from cross Shot, and to confine the Batteries, which should be raised against the Flanks to the opposite Part of the Counterscarp only, where they would be more exposed to the Besieged, and more difficult to preserve. This Invention likewise is nearly as ancient as the Art of Fortification; it being to be found in great Numbers of old Places, and in almost every old Writer, and is still continued in most Fortifications.

BUT the ancient Writers, whose principal Care was the securing of their Flanks, did not rely solely on the Advantages, they received from the last-mentioned Invention. For though by
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that Means the Batteries for destroying the Flank were confined to one Place, yet they found on Examination, that on that Place the Enemy would have more room than was sufficient for erecting of his Counter-Batteries; and therefore they added Half-Moons before the Points of the Bastions; these were intended to possess the Ground, to which the Enemies Batteries against the Flanks were already confined, and thereby to render the Construction of those Batteries still more difficult. However, they did not compleatly answer this Purpose, and have been long since laid aside.

THE Intention of Contregards, (*a*) which are likewise very Ancient, is the same with that of the Half-Moons last mentioned, that is, the Protection of the Flanks, to which Purpose (if properly constructed) they are most wonderfully adapted; for the Enemy in order to ruin the Flank, must either plant his Counter-Battery on the Contregard itself, which, if the Contregard be of a proper Profile, it will be impossible for him to do, or he must demolish a

(*a*) *Pasino*, whom we have mentioned above, claims the Invention of Contregards, tho' they were afterwards much mended by *Speckle*. But the Contregards of this Author were not before the Bastions only, but surrounded the whole Place.

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Part of the Contregard to enable his Battery on the Contrescarp to view the Flank, which is a tedious Work, attended with great Hazard and Difficulty. The same Inconveniency likewise attends him, when he would batter in Breach.

BUT notwithstanding the Excellence of this Invention, it has been almost entirely neglected in the modern System of a neighbouring Nation. There have indeed been two or three Places fortified by the *French*, in which there are Pieces called by them Contregards; but they have nothing but the Name in common with those, we here treat of. However their Experience of the Efficacy of this Work at *Turin* may possibly have induced them to think more favourably of it. For I have lately seen them adding Contregards to the old Works of a very considerable Frontier, although it was before esteemed one of their compleatest Places.

FROM all that we have said then, it appears, that the Covering of the Flanks, was a Subject much more attended to by the ancient Engineers, than by those, who have succeeded them; and consequently that the Art of Fortification has not received from the Moderns

Moderns those great Improvements, which unskilful Writers would sometimes perswade us to believe: For indeed in the securing of the Flanks consists the greatest Strength of a Fortress; since, though all the other Defences by being exposed to the Field-Batteries of the Enemy should be ruined, yet as long as the Flanks are entire, the Rempart of the Place cannot be approached by the Enemy: And therefore, since this Circumstance hath been so little heeded by some amongst the Moderns, it must be owned, that the true Principles of this Art have been very imperfectly comprehended by them. For it has often happened, that they have disputed about a few Fathoms in the Length of a Flank, a Face, or a Curtain, or a few Degrees in the Magnitude of a particular Angle; when at the same Time they have too much disregarded this most important Consideration of all, the screening of the Flanks from the Batteries of the Enemy.

BUT this neglect hath been sometimes owing to the Authority of erroneous Maxims, one of which in particular is, that, (*a*) whatever sees, is itself seen; whence it has been inferred, that,

(*a*) See this Maxim, urged with this View in *Pagan's Fortification*, Chap. iv.

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if the Flank can see the Enemy, the Enemy can ruin the Flank with his Batteries. But the Fallacy of this Reasoning lies here, that the Flank if properly covered cannot see the Enemy, when he is in a Situation, where it is possible for him to raise Batteries, but only when he gets in a Place, where he must be exposed to the Fire of the Flank without having it in his Power to return it. For Instance, a Piece of Cannon covered by an Orillon in the common Manner cannot be seen by the Enemy, till he is got over the greatest Part of the Ditch, or is mounting the Breach, in either of which Places it is impossible for him to raise a Counter-Battery: And the more compleat the Artifice is, by which the Flank is screened, the greater will be the Space, in which the Enemy will be thus exposed.

OTHER Engineers have endeavoured to undervalue this Art as ineffectual, and with this view they have expatiated much on the Force of the modern Methods of Attack, and have declared, that no place how artfully soever constructed can stand before them. With these Gentlemen it is a Maxim, that when the Counterscarp is once lost, the whole Contest is in a manner over; and they endeavour to support themselves

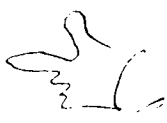
themselves in this Perswasion by the Examples of Places of great Note, which have been reduced in a much shorter Time, than was expected. If these Opinions could be relied on, the greatest Part of the Money laid out in Fortifications would be extreamly ill employ'd, since a simple Rempart and a Contrescarp would fully answer the whole Purpose intended. But the Truth is, that, when a Place is well constructed, and skilfully defended, the taking of the Contrescarp is but a small Step towards the Possession of the Place^(a). Indeed the Rashness and Precipitancy of the Director of the Approaches, hath often intimidated a weak and ignorant Governor; but when the Attacks have been thus eagerly hurried on against a Place commanded by a brave and knowing Officer, he has sometimes taken such Advantages of these incautious Steps, as have made them too fatal to be copied by any pretending to Prudence or Humanity. By this means the easiest Enterprizes have been often rendered impossible, and the Pretence of

(a) In the last memorable Siege of *Barcelona*, the Loss of the Contrescarp (which was taken in a Fortnight) did not determine the Fate of the Town, the great Resistance being after the Body of the Place was opened by several Breaches.

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gaining a Day or two has often occasioned the loss of the whole (a).

BESIDES, those Inventions for screening of the Flanks, which we have already mentioned, there have been others proposed of a different Nature, which by reason of their Singularity, have been less attended to; such is the Constructing of a Line, which should pass through the Ditch, from the Point of the Bastion to the opposite Point of the Contrescarp. This is mentioned by General *Montecuccoli* in his Memoirs, as a Method much less liable to Exception, than it appears to be at first Sight (b). But though a Line thus constructed, will doubtless cover the Flanks from the View of the Batteries placed on the opposite Part of the Contrescarp, and is it self very defensible,



(a) Many Instances of the Difficulties and Hazards, to which the Allies were often exposed in *Flanders*, during the late War, may be seen in *Landsberg*, who was then an Engineer in the Service of the States-General; these Accidents, according to him, were generally owing to the Presumption of the Directors, who under the Pretence of Expedition, contracted the Front of their Attacks, and thereby often left the Enemies Works in their Rear, which rendered their Progress next to impossible. Vid. *Nouvelle Maniere de Fortifier les Places*.

(b) Vid. *Memorie del general Principe di Montecuccoli*, pag. 116.

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yet I have never heard of its being put in Execution.

ANOTHER Way of securing the Flanks, is by interposing the Entree Angle of the Counterscarp, (or of the Ravelin) between them and the Counter-Batteries. This Practice is described by *Errard of Barleduc*, (a) and is by him said to be the Invention of the Count of *Lynar*. And though some Authors, who were ignorant of the Advantages hereby proposed, have severely censured the having any Part of the Ditch hidden from the Flank, a Circumstance which must necessarily attend this Construction; yet the greatest Genius, who ever applied himself to the Study of this Art, has thought it worthy of his Imitation; the celebrated Fortress of *Berghen-Op-Zoom*, having its Flanks in part covered by this Artifice.

BUT in a proper Soil there is still a more efficacious Defence, than any we have yet men-

(a) Vid. *La Fortification démontrée*, Lib. iii. Chap. ii. Besides this Invention here mentioned, there occurs in this Author, the Contrivance of placing a Gallery under the Cover'd-Way, with Loop-Holes into the Ditch, which is practised at *Tournay*, but more compleatly at *Berghen-Op-Zoom*. Vid. Lib. iv. Chap. vii.

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tioned; and that is, by the Means of Contre-mines. For supposing the Fortifications of a Place to be constructed with no more Art, than what is necessary to oblige an Enemy to bring his Batteries on the Glacis, when he proposes either to batter in Breach, or to ruin the Flanks, (which may be effected by a good Profile and a Ravelin before the Curtain only.) If the Soil be free from Water to a considerable Depth, it is always in the Power of the Besieged to ruin the Batteries of the Enemy by their Mines, which may be repeated too a number of Times, in Proportion to the Depth of the Soil. For these Batteries being by Supposition confined to one Situation, the Besieged can always be prepared for these Operations before-hand, and would have infinite Advantages over an Enemy; who should endeavour to dig them out; which, however in such Circumstances, would be his only Resource.

THE first successful Application of the Blowing of Mines in Sieges, was in the Kingdom of Naples, where *Pietro de Navarre* by this Means possessed himself of a Fort garrisoned by the French. But the first celebrated Use of these Mines in opposing the Progress of the Besiegers, was in the Years 1666, 67, 68, at the Siege of

Candia ; not but they had been often practised in the Defence of Places before, though in a less memorable Manner ; for by the Assistance of this Invention principally, the City of *Candia* kept the whole Power of the *Ottoman* Empire at a Bay for three Years successively. Since that Time the Advantages of Countermine have been better understood. The last eminent Instance of their great Usefulness, was in the Defence of *Turin*, in the Year 1706 ; for so effectually were the Besiegers traversed thereby, that after near four Months of open Trenches, they were not in the Possession of more than the Counterscarp, and even there, eleven Pieces of their Cannon were blown up by the Defendants, but three or four Days before the Place was relieved.

BEFORE I leave this Head, I cannot but in Justice mention the great Improvement in the Doctrine of Mines, which is contained in that excellent Dissertation (a) annexed to the third Volume of the *French Polybius*. For nothing can be more compleat, than the Manner in

(a) This in the Preface, is said to be the Performance of Monsieur *de Valiere*, Marechal des Camps, and Captain-General of the Miners.

which

which the different Stages of Mines are there distributed; indeed, the Form there assigned to the Excavation, cannot be rigorously, what the Author seems to suppose; but this Exception has nothing to do with his general Rangement of the Chambers, which is extremely well contrived for the Husbanding of the Ground, and the Annoyance of the Enemy.

I HAVE already taken notice of the Defects in the Writings of many of those, who amongst the Moderns have undertaken to form Systems of Fortification. But when I speak of these Authors and their Copiers, I must at the same Time avow the superior Merit of the great *Coeboorn*, who was undoubtedly the ablest Fortifier, that ever the World knew. This Author has published two Treatises on this Subject; the First containing a Method of Fortifying a Pentagon, to which is annexed a Project for the Amending the Fortifications of *Coevoerden*. In his Second, he has proposed three different Manners of Fortifying, one applied to a Hexagon, another to a Heptagon, and a third to an Octagon; and he has besides added, the Manner of Fortifying that Side of a Fortress, which happens to be contiguous to a River. In this Work he has particularly examined all the possible

Attacks, that can be formed against his proposed Places, thereby to evince the great Superiority of his Defences; so that it is in some Measure a Discourse on the Attack and Defence of Places, as well as a System of Fortification; and upon the Whole is the most excellent Performance, that has ever been produced on this Subject. It was written in *Low-Dutch* (the Author's Native Language) but has been translated into both *French* and *English*, but very imperfectly; tho' in a new Edition of the *French* Translation lately printed in *Holland*, many of the Errors of the former are amended, and some particular Passages are cleared up by the Notes of the Editor, who seems to have understood his Author very well.

I HAVE been told by those, who were well acquainted with this great Man, that his Treatises were far from acquiring him either the Advantages or Reputation, which he might reasonably have hoped from them. For that his Cotemporary Engineers, wedded to their old Road, decried him as an unskilful, self-conceited Pretender; but that he at last surmounted these Effects of their Envy and Prejudice by his Defence of Fort *William* at *Namur*, when that Place was besieged by the *French*;
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after this, which established his Reputation, he rose apace to the greatest Military Commands, and immortalized his Name by his Conduct of the Siege of *Namur* under King *William*, and afterwards at *Bon*, *Limburg*, the Citadel of *Liege*, &c. And his Death at the Beginning of the late War in *Flanders* was a very great Misfortune to the Allies, of which almost every Siege formed by them after the Year 1707, was a melancholy Proof.

BESIDES being entrusted with the Direction of Sieges, he was employed too in the repairing and new-modelling many of the *Dutch* Frontiers. His last work, which is left unfinished, was *Bergen op Zoom*, which will always do Honour to his Memory. Tho' he is yet so little out of the Reach of Censure, that I have heard Military Men, even in that Place, condemn as Imperfections, those very Circumstances, whence it derives its principal Defence.

CONSIDERING the great Fame which General *Coeboorn* acquired in real Service, it is difficult to account for the little Regard which hath been paid to his Writings. The most natural Reason I can discover for this Negli-

gence, is the Proneness, which we have always shewn for the Opinions of a neighbouring Nation, who, whatever other good Qualities they may have, were never famous for doing justice to the Merit of those, who were of another Country, and were engaged in an Interest opposite to their own. However, I presume his Reputation as an Author is at present increasing. For I saw not long since in one of the most considerable Frontiers belonging to *France* a Piece of Fortification carrying on, which was evidently copied from the printed Works of *Coeboorn*.

THOUGH with Regard to the modern Writers on Fortification, I cannot find another to place in the same Article with the great Genius, last mentioned; yet there are two Authors on the Methods of attacking and defending Places (a Subject nearly connected with Fortification) who merit the highest Applause, I mean *Goulon* and the *Marechal de Vauban*. The first in a short Treatise, intitled *Memoires sur l'Attaque et la Defence des places*. In which he has very distinctly inculcated the principal Maxims necessary in those Operations. The other in a Work which he presented in
Manuscript

Manuscript to the late King of *France*, of which Copies getting Abroad, it was published four Years since in *Holland*. In this Book Mr. *Vauban* has very circumstantially described those Parts of the Attack, which were more immediately of his own Invention ; such as the *Batteries à Recobret* ; the Parallels, and a peculiar Conduct of the Sap. Not but that he has likewise given very ample Instructions on every other necessary Head ; and the whole must be owned to be a very masterly Performance, worthy of the Experience and Capacity of its great Author.

It might, perhaps, be expected, that I should here mention with Approbation the Skill of this last-mentioned Engineer in the Art of Fortifying. But as he has never written any Thing himself on this Subject, that may excuse me from ranging him in the List of Authors. But to speak the Truth on this Head, I cannot but believe from all, I have hitherto seen of his Works, that he was much more to be esteemed for his other Talents than for the Fortifications he has erected. For though I have a very high Opinion of his good Sense and Discernment ; I do not conceive that
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his Invention in this Art was to be compared with that of his Cotemporary *Coeboorn*.

Thus much may suffice on the Origin and Variations of the present Military Architecture. We must next discuss what is more immediately connected with the Purport of the ensuing Treatise, I mean the Invention of Powder and Artillery, with their respective Improvements, and the different Theories, they have given Rise to.

THE Invention of Gunpowder is usually ascribed to one *Bartholdus Schwartz*, a German Monk, who discovered it, as is said about the Year 1320, and the first Use of it in War is commonly supposed to have been by the *Venetians* against the *Genoese* about the Year 1380. But both these Suppositions are undoubtedly false; for a Composition resembling that, which we call Powder, is mentioned by *Roger Bacon* as well known in his Time, and he lived near fifty Years before *Schwartz*; and there are indisputable Proofs of the Use of Artillery much earlier than the Year 1380.

INDEED, as the Time of the Discovery of Saltpetre is confessedly uncertain, it is not to be

be wondred at, that the Invention of Gunpowder should be obscure and unknown ; for these two Discoveries are so connected, that it is difficult to conceive, how the first could be long known, before the latter was found out.

THE distinguishing Property of Saltpetre is the prodigious Increase of Inflammability, which it produces in all burning Substances, when mixed with them ; altho' alone and unmixed it will neither flame, nor burn. For Instance, Saltpetre put into a Crucible, and placed in the hottest Fire will only melt, and grow red hot, but will neither explode nor flame ; yet, if any inflammable Substance (Sulphur suppose, or Coals) be thrown into it, a violent Blaze will be instantly produced, in which a Part of the Saltpetre will be consumed in Proportion to the Quantity of the inflammable Substance, which was put to it. And a like Explosion will take place, if Saltpetre be thrown upon any Fire. Now it cannot be reasonably supposed, that this Quality of Saltpetre could be long unknown, after the Substance itself was discovered ; for the accidental dropping of any small Part of it into the Fire, would prove its prodigious explosive Power when mixed with burning Bodies. And this

this being once observed, it was thence a very natural and obvious Thought to invent a Composition of Saltpetre mixed with any inflammable Substance which would burn more violently than any known before. And our present Gun-Powder is only the Improvement and Perfection of such a Mixture.

ON this Supposition then, if we knew the Time, when Saltpetre first came in use, we might give some guess, when Mixtures resembling our present Gun-Powder were first invented. Now the most general Opinion on this Head is, that Saltpetre was first discovered, either by the *Arabians* or the later *Greeks* about the middle Ages of our *Æra*, when Alchymy and Chymistry were eagerly pursued by both Nations. For its *Arabick* Name is said to be expressive of its explosive Quality; and the *Greek Fires* used in War by the later *Greek* Emperors (if the Effects attributed to them by many Authors are true) must have had Saltpetre in their Composition.

INDEED some Moderns (misled by a Similarity of Names) have supposed Saltpetre or Nitre to have been known to the Ancients. But Chymists are now agreed, I think, that
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the Substance mentioned by some ancient Writers, and described by *Pliny*, by the Appellation of Nitre, is a Salt altogether different from what we call Saltpetre.

Now, that the first Invention of Gun-Powder (or of Compositions resembling it) did long precede the Time of *Schwartz* or of *Bacon*, and may thence be reasonably supposed nearly coeval with the Knowledge of Saltpetre, appears from *Bacon* (a) himself; for it is not a new Composition, which he proposes, but the Application of an old one to military Purposes. And from his Words it plainly appears, that a Mixture of Saltpetre with other Sub-

(a) *Bacon* tells us, that Sound like Thunder, and Lightnings greater than those produced by Nature, might be made by Art; and this many Ways, by which a City or an Army might be destroy'd; and he supposes it to be by an Artifice of this Kind, that *Gideon* defeated the *Midianites*: And having in another Treatise mentioned almost the same Thing in different Words, he adds, *Et experimentum hujus rei capimus ex hoc ludicro puerili quod fit in multis mundi partibus, scil. ut instrumento facto ad quantitatem pollicis humani ex violentia illius salis, qui SAL PETRÆ vocatur, tam horribilis sonus nascitur in ruptura tam modicæ rei, scil. modici pergameni, quod fortis tonitruui excedere rugitum, & corruscationem maximam sui luminis jubar excedit. Vid. Doctor Jebb's Preface to his Edition of Bacon's Opus Majus.*

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stances was then vulgarly used for the making of recreative Fire-works. And this appears yet plainer from the Treatise of *Marcus Græcus*, intitled, *Liber ignium* (a); for this Author describes two kinds of Fire-works, one for flying, and the other for making a Report. The Case or *Cartouche* (*Tunica*) for the first, he directs to be made long and slender, and the Composition to be very close rammed; the Case for the second he orders to be thick and short, to be strongly tied at both Ends, and to be but half filled; and the Composition he prescribes for both is two Pound of Charcoal, one Pound of Sulphur, and six Pound of Saltpetre, well powdered and mixed together in a Stone Mortar: And this will be allowed to be a stronger Composition, than what great Quantities of Powder are every Day made with. Now tho' the Age of this Writer is not well ascertained, yet it must have preceded the use of Artillery; for he does not in any Place (as I can learn) mention these Compositions as used in War; and as he pretends not to be the Inventor of these Serpents or Crackers (for such we should now call them) nor

(a) This is a Manuscript in the Possession of Dr. Mead; but what is here mentioned is quoted by the Editor of *Bacon's Opus Majus* in the Preface.

speaks

speaks of them as recent, we may reasonably presume, they were in use long before his Time.

THE first Application of this Mixture to Military Affairs, seems to have been soon after the Year 1300. *Bacon's* Proposal (which was about the Year 1280,) to make use of its enormous Explosion for the Destruction of Armies might give the first Hint, which others might afterwards pursue. *Schwartz* instead of being the first Inventor of Gun-Powder, might possibly be one of the first, who thus apply'd it; and indeed the common Account of the manner, in which he came at his Invention, very much favours this Opinion (a). And perhaps the different Improve-
ments

(a) The usual Manner in which it is told is, that *Schwartz* having pounded the Materials of Gun-Powder in a Mortar, which he afterwards covered with a Stone, a Spark of Fire accidentally flew into the Mortar, and the Explosion blew the Stone, which covered the Mortar, to a considerable Distance. Now we have proved, that *Subartz*, who was a Chymist, could not discover the Composition by this Means, because it was commonly known before; but he might from hence be taught the simplest Method of applying it in War: For *Bacon* seems rather to have conceived the Manner of using it to be by the actual Effort of the Flame against the Bodies it might meet with in its Expansion,

ments soon added by others, or the Prosecution of *Bacon's* Thought in different Places, may have given Rise to the different Dates assigned by Historians for the first use of Artillery.

GUN-POWDER for some Time after the Invention of Artillery, was of a Composition much weaker (*a*) than what we now use, or than that ancient one mentioned by *Marcus Græcus*; but this, I presume, was owing to the Weakness of their first Pieces, rather than to the Ignorance of a better Mixture. For the first Pieces of Artillery were of a very clumsy, inconvenient Make, being usually framed of several Pieces of Iron fitted together lengthways, and then hoop'd with Iron Rings; and as they were first employed in throwing Stone Bullets of a prodigious Weight, in Imitation of the ancient Machines, to which they

sion. The Figure and Name of Mortars given to a Species of the old Artillery, and their Employment (which was throwing great Stone Bullets at an Elevation) very much corroborate this Conjecture.

Vide Tartalea in his *Questi & Inventioni*, Lib. 3. *Questi* 5. where there are set down twenty-three different Compositions made use of at different Times. The first of which being the most ancient, contains equal Parts of Nitre, Sulphur, and Charcoal.

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succeeded, they were of an enormous Bore. But the Difficulties of conducting and managing these cumbrous Pieces, and the discovering that Iron Bullets of much less Weight than Stone ones, would be more efficacious, if impelled by greater Quantities of stronger Powder, soon occasioned an Alteration in the Matter and Fabric of these first Pieces, and gave Rise to what we stile Brass Cannon, which though lighter and more manageable, were yet much stronger in proportion to their Bore, by which Means they would endure great Charges of a better Powder, than what had first been used, and their Iron Bullets (which were from forty to sixty Pound Weight) being impelled with greater Velocities were more effectual, than the weightiest Stones could ever prove (a).

By

(a) The Time when this Change took Place, and the Advantages arising from it, are mentioned by Guicciardin, who speaking of the French Army intended for the Invasion of Italy, in the Year 1494, says,

— Et per unirsi con questo esercito erano state condotte per mare a Genoua quantità grande d' artiglierie da battere le muraglie, & da usare in campagna, ma di tale sorte, che giamai non haveua veduta Italia le simiglianti. Questa peste trovata molt' anni innanzi in Germania, fu condotta la prima volta in Italia da Vinitiani nella guerra, che circa l'anno
della

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By this Means, Powder compounded in the same Manner, which is now practised by

della salute 1380, hebbono i Genouesi con loro. ——— Il nome delle maggiori era bombarde. le quali, sparsa dopo questa invention per tutta Italia s'adoperavano nell'oppugnatione delle terre, alcune di ferro, alcuni di bronzo, ma grossissime, in modo che per la machina grande, & per l'imperitia de gl' huomini, & mala attitudine de gl' instrumenti tardissimamente & con grandissima difficoltà si conducevano, piantavanfi alle terre con medesimi impedimenti, & piantate era dall' un colpo all' altro tanto intervallo, che con piccolissimo frutto a comparatione di quello, che seguito dopo, molto tempo consumavano, donde i defensori de luoghi oppugnati havevano spatio di potere otiosamente fore di dentro ripari & fortificationi. ——— Ma i Francesi fabricando pezzi molti piu espediti, ne di altro che di bronzo, i quali chiamavano Cannoni, & usando palle di ferro, dove prima di pietra, & senza comparatione piu grosse & di peso gravissimo, s' usavano, gli conducevano su le carette, tirate (non da buoi, come in Italia si costumava) ma da cavalli con agilità tale d'huomini, & d'instrumenti deputati a questo servizio, che quasi sempre al pari de gl'eserciti caminavano, & condotte alle muraglie erano piantate con prestezza incredibile, interponendosi dall' un colpo all' altro piccolissimo intervallo di tempo, si spesso, & con impeto si gagliardo percolavano, che quello che prima in Italia fare in molti giorni si soleva, da loro in pochissime hore si faceva. Vid. Guicciardin's History, L. I. p. 45. What this Author observes of the prodigious Size of the Stone Bullets used whilst the old Pieces were in Fashion, will be better understood by knowing, that when Mahomet the second besieged Constantinople in the Year 1453, he battered the Walls with Stone Bullets, and his Pieces were some of them of the Calibre of 1200 Pounds; but then they could not be fired more than four Times a Day.

all

all *Europe*, came in use (a). But the Change of the Proportion of the Materials composing it, was not the only Improvement it receiv'd. The Invention of graining it, is doubtless a considerable Advantage to it. For Powder at first was always in the Form of fine Meal, such as it was reduced to, by grinding the Materials together. And it is doubtful, whether the first graining of Powder was intended to increase its Strength, or only to render it more convenient for the filling into small Charges, and the charging of small Arms, to which alone it was applied for many Years, whilst Meal-Powder was still made use of in Cannon. But at last the additional Strength, which the grain'd Powder was found to acquire from the free Passage of the Fire between the Grains,

(a) We learn from *Tartalea*, that the Cannon Powder in his Time (*polver grossa moderna*) was made of four Parts Saltpetre, one Part Sulphur, and one Part Charcoal, and the Musquet Powder of forty-eight Parts Saltpetre, seven Parts Sulphur, and eight Parts Charcoal; or of eighteen Parts Saltpetre, 2 Parts Sulphur, and three Parts Charcoal. These Compositions for Musquet Powder are very near the present Standard; the first having in one hundred Pounds of Powder, about one Pound of Saltpetre more than is at present allowed, and the second three Pounds more.

occasioned the Meal-Powder to be entirely laid aside (a).

THE Formation of Artillery hath been very little improved in the last two hundred Years; the best Pieces now cast, not differing greatly in their Proportions from those made in the Time of the Emperor *Charles V.* Indeed lighter and shorter Pieces have been often proposed,

(a) That Powder was first used in Meal; and that long after the Invention of Graining it for the Use of small Arms, Cannon-Powder continued in its old Form, are Facts not to be contested. *Tartalea* in his *Quæstis*, L. 3. *Quæst.* 9. and 10. expressly asserts, that then the Cannon-Powder was in Meal, and the Musquet-Powder grained. And our Countryman *William Bourn*, in his *ART OF SHOOTING IN GREAT ORDNAUNCE*, published forty Years after *Tartalea*, tells us in Chap. 1. that Serpentine-Powder (which he opposes to Corn or Grained-Powder) should be *as fine as Sand, and as soft as Flour*: And in his third Chapter he says, that two Pounds of Corn-Powder will go as far as three Pounds of Serpentine-Powder. Also Sir *Henry Manwayring*, in his *SEAMAN'S DICTIONARY* presented to the Duke of *Buckingham* in the Time of *Charles I.* under the Word *Powder*, tells us, *there are two Kinds of Powder, the one Serpentine-Powder, which Powder is Dust (as it were) without corning — The other is Corn-Powder.* Though he informs us the Serpentine-Powder was not used at Sea. Indeed when that Book was wrote, I believe, Powder was usually corned, for the foreign Writers on Artillery had long before recommended its general Use.

and

and essayed ; but, though they have their Advantages, and are extremely useful in particular Circumstances, yet it seems now to be agreed, that they are altogether insufficient for general Service. But though the Proportions of Artillery have not been much varied within that Period, yet its Use and Application have undergone considerable Changes, the same Ends being now generally pursued by smaller Pieces, than what were formerly thought necessary. Thus the Battering-Pieces now universally approved of, are the Demi-Cannons, carrying a Ball of twenty four Pound weight, it being found by Experience, that their Stroke, tho' less violent than that of larger Pieces, is yet sufficiently adapted to the Strength of the usual Profiles of Fortification, and that the Facility of their Carriage and Management, and the Ammunition they spare, give them great Advantages beyond the whole Cannons formerly employed in making Breaches. The Method too (now generally followed) of forming a Breach, by first cutting off the whole Wall as low as possible, before its upper Part is attempted to be beat down, seems also to be a considerable modern Improvement in the practical Part of Artillery. For I do not remember to have seen this Procedure recom-

mended by any ancient Author; and *Gabriel Busca* (a) who boasts much of his great Experience, expressly directs the Contrary. Indeed *Collado* mentions it as the Practice of the *Turks*, (b) but it is without commending it, or proposing it as an Example to be followed.

BUT the most important Improvement in the practical Management of Artillery (for of the scientific Part we shall treat by its self) is the Method of firing with small Quantities of Powder, and elevating the Piece so that the Bullet in its Descent may just go clear of the Pa-

(a) Vid. His *Instruttione de Bombardieri*, printed at *Carmagnola* in 1584, Cap. xxxvii. In which Place he orders the Breach to be begun at the upper Part of the Wall, and from thence to be continued downwards.

(b) Vid. *Pratica Manuale di Artigliera d'al Mag. Signor Luigi Collado Hispano, Bettico, Nebresense*, printed at *Venice* in the Year 1586, Cap. xx. Where he says. — *nelle fattioni del gran Turco — sempre si adoperano i pezzi — da tagliare le muraglie per di sotto di esse transversalmente, et di poi di alto in basso a perpendicolo, & applicandovi poi tutti a un tratto i basiliſchi, con che fanno cascar giu quella parte di muraglia che era gia tagliata.* This Book here quoted was composed and published in *Italian*, altho' the Author was a *Spaniard*. But he served as an Engineer in the *Spanish Army in Italy*. And he tells us in his Preface, that he soon intended to re-publish it in *Spanish*, which last Edition is, I presume, what is quoted by *Blondel* in his *Art de jeter les Bombes*.

rapet

rapet of the Enemy, and drop into their Works. By this Means the Bullet coming to the Ground in a small Angle, and with a small Velocity, it either bounds or rolls along in the Direction it was fired in; and therefore, if the Piece be placed in a Line with the Battery it is intended to silence, or the Front it is to sweep, each Shot rakes the whole Length of that Battery or Front, and has thereby infinitely more Chance of disabling the Defendents, and dismounting their Cannon, than it would have, if it was fired against the same Works in the common Manner. This Disposition of Artillery, which is indeed a most useful one, is the Invention of the Marechal *de Vauban*, and is by him stiled *Batterie à ricochet* (a) and was first put in Practice at the Siege of *Aeth*, in the Year (b) 1692.

AFTER this brief Recital of what has been done in the mechanic Part of Gunnery, we must next mention the different Theories, which have been from time to time advanced in relation to the Motions of Shells and Bul-

(a) Vid. his Book *De l'Attaque et la Defence des places*.

(b) Vid. The Journal of this Siege printed at the end of the last Edition of *Goulon's Memoires*.

lets; in which Inquiry we shall not indeed find many Things worthy of Approbation, or even of Attention; but however, as it is a Theme in some Measure connected with the Subject of the following Treatise, we must beg the Reader's Indulgence.

THE first Author I have seen, who has professedly written on the Flight of Cannon Shot, is *Tartalea*, a celebrated *Italian* Mathematician, famous for having invented the Method of solving cubic Equations, which is usually ascribed to *Cardan*. This Author in his *Nova Scientia*, printed at *Venice* in the Year 1537, and afterwards in his *Questi et Inventioni diversi*, printed at the same Place in 1546, has professedly discussed several Particulars relating to the Theory of these Motions. And though the then imperfect State of Mechanicks furnished him with very fallacious Principles to proceed on, yet he was not altogether unsuccessful in his Inquiries: For he is supposed to be the first who asserted that the greatest Range of Projectiles was at an Elevation of 45° . He likewise determined (contrary to the Opinion of Practitioners) that no Part of the Track described by a Bullet was a right Line, although the Curvature was in some cases

cases so little, as not to be attended to, he comparing it to the Surface of the Sea, which though it appears to be a Plain when partially considered, is yet undoubtedly incurvated round the center of the Earth. He also assumes to himself the Invention of the Gunner's Quadrant, and has often given shrewd Guesses at the Event of some untried Methods which were proposed to him. But as he had never been conversant in the Practice of Artillery, but founded his Opinions on Speculation only, almost all the Writers who succeeded him were perpetually carping at him, though often without naming him; of which many Examples might be given from the Works of *Busca*, *Collado*, (a) *Ufano*, *Simienowicz*, &c. And the Philosophers of those Times often intervening in the Questions hence arising, there were hereby many Disputes on Motion set on

(a) *Collado* cap. lxiii. denies that *Tartalea* was the Inventor of the Gunner's Quadrant, and quotes *Daniel Santbech*, or *Regiomontanus* (for he confounds them) as having known it many Years before. But the Truth is, that *Santbech's* Book from whence his Quotation is taken (*Problematum Astronomicorum & Geometricorum sectiones septem*) was not printed till the Year 1561, which was long after *Tartalea*. Nor did *Santbech*, though he talks of the different Elevations of Artillery, know the Method of framing a Quadrant proper for his Purpose.

foot

foot (especially in *Italy*) which continued till the Time of *Galileo*, and perhaps gave Rise to his celebrated Dialogues on Motion, which were first printed in the Year 1638. And in this Interval, or before the Doctrine of *Galileo* was established, many Theories of the Motions of military Projectiles, and many Tables of their comparative Ranges at different Elevations were published, all of them egregiously fallacious, and utterly irreconcilable with the Motions of those Bodies, although some of them were the Labours of such who had spent the greatest Part of their Lives in Employments relating to the Artillery. Such were the Tables of *Ufano*, of *Galeus*, of *Ulrick*, &c. taken notice of by *Blondel* (a). To which might be added, many more not mentioned by that Author. Indeed there have been very few ancient Writers on this Subject (and they are a numerous Sect) who have not indulged themselves in some Speculations on the Difference betwixt natural, violent, and mixt Motions, although in the Application of these

(b) Note, the Opinion discussed by *Blondel* in his *Art de jeter les Bombes*, Cap. v. is not originally of *Rivaltius* whom *Blondel* quotes for it, but of the last mentioned *Santbech*, from whom *Rivaltius* stole it. Vid. *Santbech*, Sect. 6.

mistaken

mistaken Notions, scarce any two of them agreed.

BUT what is most strange, is, that during these Contests so few of those, who were intrusted with the Charge of Artillery, should think it worth while to examine their respective Theories by proper Experiments. However thus it has happened, for I do not remember to have met with more than four Authors, who have actually tried the Ranges of Shot and Shells at different Elevations. The first of these is *Collado*, who has given us the Ranges of a Falconet carrying a three Pound Shot to each Point of the Gunner's Quadrant. But from his Numbers it is manifest, that the Piece was not charged with its customary Allotment of Powder (*a*). The next is our Countryman *Bourne* in

(*q*) The Result of his Trials was, that the point blank Shot extended 268 Paces. At an Elevation of one Point (which is the twelfth Part of the Quadrant or $7^{\circ}\frac{1}{2}$) the Range was 594 Paces; at an Elevation of two Points, the Range was 794 Paces; at three Points, 954 Paces; at four Points, 1010 Paces; at five Points, 1040 Paces; and at six Points 1053 Paces. The Range at the seventh Point fell between those of the third and fourth, at the eighth Point it fell between the Ranges of the second and third; at the ninth Point it fell between the Ranges of the first and second;

in a Treatise printed the next Year after *Colado*. His Elevations were not regulated by the Points of the Gunner's Quadrant, but by Degrees, and he ascertains the Proportion between the Ranges at different Elevations, and the Extent of the Point blank Shot (*a*). But he has not informed us, with what Piece he made his Trials; tho' by his Proportions, I presume, it must have been a small one. It were to be wished, that he had set down this Circumstance; for we shall hereafter shew, that the Relation between the Extent of different Ranges will vary extremely, according to the Velocity and Density of the Bullet. The other

cond; at the tenth Point it fell between the point blank Distance and that of the first Point; and at the eleventh Point it fell very near the Piece. *Vid. Cap. lxi.* And note, that the Paces used by this Author are not geometrical Paces, but common Steps, as he informs us *Cap. xlii.*

(*a*) If 1 represents the Extent of the point blank Shot, then according to this Author, the Range at 5° will be $2\frac{2}{3}$, at 10° it will be $3\frac{1}{3}$, at 15° it will be $4\frac{1}{3}$ at 20° it will be $4\frac{1}{2}$, and the greatest Random will be $5\frac{1}{2}$; which greatest Random, he tells us, in a calm Day is at 42° ; but according to the Strength of the Wind, and as it favours or opposes the Flight of the Shot, it may be from 45° to 36° . *Vid. His Art of Shooting in great Ordnance, Cap. vii.*

two,

two, which have occurred to me, are *Eldred* and *Anderson*, both *Englishmen*; the last of these having vitiated his Experiments by his too great Attachment to an erroneous Theory, I shall have Occasion to mention him hereafter. But *Eldred* (a) deserves a better Character; his Principles were sufficiently simple, and tho' not rigorously true, they were, within certain Limits, near the Truth. He has given us the actual Ranges of different Pieces of Artillery at small Elevations, all under ten Degrees. His Experiments are numerous, and appear to be made with great Care and Caution; and he has honestly set down some, which were not reconcileable to his Method; and upon the whole seems to have taken more Pains, and to have had a juster Knowledge of his Business, than is to be found in many of his practical Brethren. For they have been generally too much attach'd to some incorrect Theory, or to the common Usage which they have always followed, to think of extending their Art by proper Expe-

(a) His Book is intitled *THE GUNNER'S GLASSE*, and the Experiments he relates were most of them made at *Dover-Castle*, of which Place he was many Years Master-Gunner. The earliest Date I find to any of his Experiments is 1611, but his Book was not published till 1646.

riments,

riments, or indeed to conceive, that it was not already compleat; it would otherwise have been impossible, that Positions so little to be reconciled with Experience, should have held their Ground so long as they have done, a remarkable Instance of which, is the Doctrine which has taken Place in this Subject, since the Time of *Galileo*.

GALILEO printed his Dialogues on Motion in the Year 1638, as we have already observed; and in these he has pointed out the general Laws observed by Nature in the Production and Composition of Motion, and was the first, who described the Action and Effects of Gravity on falling Bodies; and on these Principles he determined, that the Flight of a Cannon-Shot, or of any other Projectile, would be in the Curve of a Parabola, unless so far as it were diverted from that Track by the Resistance of the Air. And what Inequalities would thence arise, he has proposed the Means of examining; for he has described a Method of discovering what sensible Effects that Resistance would produce in the Motion of a Bullet at some given Distance from the Piece.

WHEN *Galileo* had thus shewn, that, independent of the Resistance of the Air, all Projectiles

jectiles would in their Flight describe the Curve of a Parabola, it might have been expected that those who came after him, would have tried how far the real Motions of Projectiles deviated from a parabolic Track, in order thence to have decided whether the Resistance of the Air was, or was not, necessary to be attended to in the Determinations of Gunnery. But instead of this cautious Procedure, the subsequent Writers on Gunnery have boldly asserted (without an experimental Examination) that no considerable Variation could arise from the Resistance of the Air, in the Flight of Shells or Cannon-Shot, supporting themselves in this Persuasion chiefly by the Consideration of the extreme Rarity of the Air, compared with the dense and ponderous Composition of those projected Bodies. And hence (this Maxim of the inconsiderable Effects of the Air's Resistance to the Motion of Shells and Bullets, being continually repeated and copied by succeeding Authors,) it is now become an Axiom almost generally acquiesced in, that the Flight of these Bodies is nearly in the Curve of a Parabola.

FOR in the Year 1674, our Countryman *Anderson* published his Treatise of *the genuine Use and*

and Effects of the Gun. In which he proceeds on the Principles of *Galileo*, and strenuously asserts the Flight of all Bullets to be in the Curve of a Parabola ; undertaking to answer all Objections, that could be urged to the contrary. And in the Year 1683, Monsieur *Blondel* published at *Paris*, *L'Art de jetter les Bombes*, where the Doctrine of *Galileo* is likewise applied to the Motion of Shells and Bullets of all kinds, and the Variations of this Doctrine which can arise from the Resistance of the Air, are particularly mentioned ; and after a long Discussion the Author concludes, that they will be so very minute as scarcely to affect the Accuracy of his Conclusions (a). Also, the same Subject is treated of in our Philosophical Transactions (b) by Dr. *Hally*, who, swayed by the Consideration of the very great Disproportion between the Density of Bullets and of the Air, thinks it reasonable to believe, that the Opposition of the Air to large Metal Shot is scarcely discernable, although in small and light Shot he acknowledges, that it ought and must be accounted for.

IN consequence then of these Opinions about the inconsiderable Effects of the Air's Re-

(a) Vid. Page 345. of the first Quarto Edition at the Bottom, also Page 355. and following.

(b) Vid. No 216. p. 68.

istance

sistance on heavy Shot, and the Demonstrations of *Galileo*, that all Projectiles moved in the Curve of a Parabola, if they were not disturbed by that Resistance, it is now an Opinion generally advanced by the Writers on the Theory of Gunnery, that the Flight of Shot and Shells is nearly in the Curve of a Parabola ; for the Truth of which, we may appeal to the professed Authors on this Subject, who have wrote within the last forty Years.

BUT though this Hypothesis went smoothly on with those, who contented themselves with Speculation only ; yet *Anderson*, who made a great Number of Trials, found it impossible to support it without some new Modification. For though it does not appear, that he ever examined the comparative Ranges of either Cannon or Musquet Shot when fired with their usual Velocities ; yet his Experiments on the Ranges of Shells thrown with small Velocities (in respect of those last mentioned) convinced him, that their whole Track was not Parabolical, as appears by his Treatise, intituled, *To hit a Mark*, published in the Year 1690. But instead of making the proper Inferences from hence, and discovering the Resistance of the Air to be of considerable Efficacy, he from his great Attachment to his first Opinions framed

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a new Hypothesis, which was, that the Shell or Bullet at its first Discharge, flew a certain Distance in a right Line, from the End of which Line only it began to bend into a Parabola. And this right Line which he calls the Line of the Impulse of the Fire, he supposes to be the same in all Elevations. By this Hypothesis (though an indefensible one) it was always in his Power, by assigning a proper Magnitude to this Line of Impulse, to reconcile any two Shot made at different Angles, however opposite they might prove to the common Principles. But even this new-modelled Theory, was not, I believe, confirmed by his following Experiments ; for he has nowhere ventured to give us Experiments of three Ranges made at three different Elevations, with the same Quantity of Powder ; as finding, I presume, that though by this Scheme he could reconcile two jarring Ranges, the Irregularities of three were insurmountable. And, if such Inequalities were produced by the Resistance of the Air in the Motion of a Shell, impelled from a Mortar by an inconsiderable Quantity of Powder, what may not the Action of the Air be supposed to effect in the Motions of Bullets, which being impelled by a full Charge of Powder through a much longer Cylinder, move perhaps three or four Times

as fast, and consequently undergo near fifty Times the Resistance, as will be more particularly evinced hereafter.

THAT the Resistance of the Air, which acts with such prodigious Power on all swift Bodies, should be entirely unattended to by the Practitioners in Gunnery, is not the only remarkable Circumstance which occurs in this Enquiry; for after the Publication of Sir *Isaac Newton's Philosophiæ Naturalis Principia Mathematica*, it might have been expected, that all Mathematicians should have been convinced of its Energy, since in that immortal Work the Law and Quantity of this Resistance to slow Motions is determined, and confirmed by many Experiments. Indeed the same Law when extended to swift Motions will be defective, and will exhibit the Resistance greatly short of what it really comes out by Experiment, (of which Sir *Isaac Newton* himself has given us (a) warning;) yet even upon his Principles it would appear, that the Action of the Air on Bullets is by far too considerable to be neglected. But notwithstanding this obvious Proof of the Necessity of considering the Action of the Air on military Projectiles, I can recollect but one Instance, where any Computa-

(a) Vid. Phil. Nat. Prin. Math. p. 351. l. 17.

tions founded on Sir *Isaac Newton's* Doctrine, have been applied to these Motions (a).

To sum up now at once all we here intend to observe on this Head. It appears, that the modern Writers on the Art of Gunnery have been very much deceived, in supposing the Resistance of the Air to be inconsiderable, and thence asserting, that the Track of Shot and Shells of all kinds is nearly in the Curve of a Parabola. That by this Means it has happened, that all their Determinations about the Flight of Shot discharged with considerable Degrees of Celerity are extremely erroneous, and consequently that the present Theory of Gunnery in this its most important Branch is useless and fallacious.

Now to obviate in some degree these Imperfections in this Art, we have undertaken in the second Chapter of the ensuing Treatise, not only to confirm what we have here asserted relating to the Falsity of the parabolic Motion of these Projectiles, but likewise to ascertain the actual Degree of Resistance, which every Shot undergoes according to the Velocity with which it moves ; whence, as the

(b) Vid comm. Acad. Petrop. Tom. 2. p. 333. 339.

Velocity with which the Bullet issues from the Piece is easily known by the Principles delivered in the first Chapter, the Delineation of the Track passed through by the Bullet hereby becomes a geometrical Problem, which indeed in its utmost extent is of a very complicate and operose Kind; but in the Instances which are most frequent in practice, it admits of some very easy Approximations, which enable us readily to compare the actual Ranges of Bullets with the Result of this Theory.

AND though such as examine the following Treatise with Attention, will not, I believe, entertain many Doubts of the Certainty of the Determinations therein contained; yet it might have been expected perhaps, that the Accuracy of those Principles should have been still more irrefragably established by Experiments on the real Ranges of Pieces, compared with Computations founded on this Theory. And, indeed, I did once intend to have added a Chapter with this view; but two Reasons have diverted me from this Design. The first was, the Difficulty I found in ascertaining the extended Ranges; a Difficulty which none but those who shall attempt Experiments of the same Kind, can be judges of. The second Reason was an Irregularity which
in-

intervened in these Ranges, and which rendered all my Endeavours fruitless; for the same Piece at the same Elevation would convey the Bullet to very distant Places, so that no two Trials agreed with each other, as I have more particularly recited in the 7th Proposition of the 2d Chapter.

BUT notwithstanding these Difficulties, which have hindered me from inserting in the following Treatise such Experiments on the Ranges of Shot, as might corroborate the Theory of Resistance there delivered, I have yet resolved to pursue this Subject; and I flatter myself, that I have invented a Method of preventing the last-mentioned Inequality from taking Place, which unless it can be done, it is sufficiently obvious, how fruitless all Experiments of this kind must prove. The Result of my future Trials on this Head, I intend for a second Part to this Treatise, in which, besides these Experiments on the Track described by the Flight of Bullets, and the necessary geometrical Determinations with which they must be compared, I propose to insert many other Experiments, which though of a miscellaneous Nature, are yet all of them connected in some degree with the Theory or Practice of Gunnery. I shall also annex to this

this second Part many Maxims and practical Precepts, which will arise from the preceding Principles, and will, I hope, be of some Consequence in the future Management of Artillery. A considerable Part of this second Work I have already by me, as likewise an Apparatus purposely intended for compleating it. But those Experiments which are yet wanting, will require great Leisure and a proper Season to execute.

As the following Sheets, besides the Determination of the Quantity of the Air's Resistance, do likewise contain the Theory of the Force and Action of Powder; it may perhaps be expected, that I should give some Account of what preceding Authors have advanced on this Subject. But all, I have ever met with on this Head, hath been so vague and indistinct, that it is often difficult to determine the true Meaning of the Writer. The most intelligible Hypothesis on this Head, and what seems indeed to have been the Original of all the others, is that of Monsieur *de la Hire*.

IN the History of the *French Academy* for the Year 1702, Monsieur *de la Hire* has supposed, that the Force of Powder may be owing to the increased Elasticity of the Air contained

rained in, and between the Grains, in consequence of the Heat and Fire produced at the Time of the Explosion. Now, if this Air, to whose augmented Spring the Violence of Gunpowder is imputed, be in its natural State, at the Time when the Powder is fired (and surely what is in the Intervals of the Grains must be allowed to be so) the greatest Addition its Elasticity could acquire from the Flame of the Explosion, would not amount to five times its usual Quantity, as we shall more particularly evince hereafter (*a*); that is, it would not suffice for the two hundredth Part of the Effort, which we have found to be exerted by fired Powder.

HOWEVER, this Hypothesis hath given rise to many Dissertations and Treatises in a neighbouring Nation, and one Author in particular conceives he has made a very reasonable Postulate, in supposing the Elasticity of the Air, when heated by the Explosion of the Powder, to be only an hundred Times greater than when it is heated to the Degree of boiling Water. But as I think I have shewn the Impossibility of accounting for the actual Force of Power on these Principles, I will not detain the Reader any longer with a particular

(*a*) Vid. Prop. V. Cap. I. of the following Treatise.

Re:

Recital of the Speculations of these different Writers ; especially, as I flatter myself, that I have established that Theory of the Force of Powder, which is contained in the following Sheets, by such decisive Experiments, as will render a formal Confutation of any other Opinion unnecessary.

E R R A T A.

P A G E 28. line 3. *for L M read L N.*

Pag 32. lin. 6. *Put a full Point after Velocity, and erase that after indeed.*

Pag. 33. lin. 3 and 4, *for sensibly, read sensible.*

Pag. 36. lin. *the last, for D 2 read D.*

Pag. 42. lin. 8. *after Length add and Weight.*

Pag. 53. lin. 2. *for two read too.*

Pag. 62. lin. 33. *for of read for. Also lin. 35. for clear read clean.*

Pag. 63. lin. 20. *for by read from.*

Pag. 72. lin. 5. *erase it.*

Pag. 93. lin. 19. *for rotary read rotatory. Corrected.*



CHAP. I.

Of the Force of Gunpowder.

PROPOSITION I.

Gunpowder, fired either in a Vacuum or in Air, produces by its Explosion a permanent elastic Fluid.



IF a red hot Iron be included in a Receiver, and the Receiver be exhausted, and Gunpowder be then let fall on the Iron, the Powder will take Fire, and the mercurial Gage will suddenly descend upon the Explosion; and, tho' it immediately ascends again, yet it will never rise to the Height it first stood at, but will continue depressed by a Space proportioned to the Quantity of Gunpowder, which was let fall on the Iron. This is a well known Experiment, and is circumstantially described by Mr. *Hauksbee*, in the *Philosophical Transactions*, No. 295; in which Place he tells us, that he by this means (firing small Quantities at a Time) reduced the Gage from

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29:

29 $\frac{1}{2}$ Inches to 12 $\frac{1}{2}$. Now this Experiment, which has been often repeated, proves the Proposition with respect to the Production of a permanent elastic Fluid in a *Vacuum*; for the Descent of the Gage could only be effected by the Pressure of some new generated Fluid in the Receiver, ballancing in Part the Pressure of the external Air. That this Fluid or some Part of it at least was permanent, appears from what Mr. *Hanksbee* relates in the same Place, that tho' the Quicksilver ascended after the Operation, yet it next Day had ascended no higher than to 22 $\frac{1}{2}$, at which Place it seemed to continue fix'd. And, that this Fluid is elastic, is proved from the Descent of the mercurial Gage; since the Quantity of Matter contained in this Fluid, could not by its Gravity alone have sunk the Quicksilver by the least sensible Quantity; also from its extending itself through any Space however great; the Experiment succeeding in either a large or small Receiver, only the larger the Receiver the less will be the Descent of the mercurial Gage to the same Quantity of Powder; the Pressure of the generated Fluid diminishing as its Density diminishes.

The same Production likewise takes Place, when Gunpowder is fired in the Air; * for if a small Quantity of Powder be placed in the upper Part of a glass Tube, and the lower Part of the Tube be immersed in Water, and the Water be made to rise so near the Top, that only a small Portion of Air is left in that Part where the Gunpowder is placed; if in this Situation the Communication of the upper Part of the Tube with the external Air be closed, and the Gunpowder be fired (which may easily be done by a Burning-glass) the Water will in this Experiment descend on the Explosion, as the Quicksilver did in the last, and will always

* Vid. *Hanksbee Phys. Mechan. Exper.* Page 81.

continue

continue depressed below the Place, at which it stood before the Explosion, and the Quantity of this Depression will be greater, if the Quantity of Powder be increased, or the Diameter of the Tube be diminished. From whence it is proved, that as well in Air as in a *Vacuum*, the Explosion of fired Powder produces a permanent elastic Fluid.

S C H O L I U M.

It has been known ever since the Time of Mr. *Boyle*, that many Substances, in Fermentation and other chymical Operations, produce elastic Fluids analogous in some of their Effects to the common Air. It is likewise known, that other Mixtures will in many Cases absorb a Part of the Air contiguous to them; in particular, it is observed, that all burning Bodies and all sulphureous Fumes destroy great Quantities of Air, either by absorbing it into their own Substance, or at least by depriving it of its Elasticity. This Creation and Consumption of Air in chymical Processes has been lately most diligently and successfully examined by the Reverend Mr. *Hales* in his *Vegetable Statics*. And on these Principles it follows, that in the last Experiment the sulphureous Fumes arising from the burning of the Charcoal and Brimstone contained in the Powder must soon absorb some of the Air, in which the Powder is fired; for which Reason it is necessary, that the Bulk of the Air, which the Powder is placed in, before it is fired, should bear as small a Proportion as possible to the Quantity of Powder, so that the Success of the Experiment may not be disturbed by the absorbed Air, approaching to an Equality with the generated Fluid.

There is besides another Reason, that, when Powder is fired in the Manner of the last Experiment, the Bulk of the Air, in which it is placed, should be as little as possible; which is, that the Fire at the instant of

the Explosion will greatly augment the Elasticity of that Air, and the Pressure arising from this increased Elasticity, being added to the Force of the generated Fluid, will endanger the bursting of the Tube.

P R O P. II.

To explain more particularly the Circumstances attending the Explosion of Gunpowder, either in a Vacuum or in Air, when fired in the Manner described in the Experiments of the last Proposition.

WHEN any considerable Quantity of Gunpowder is fired in an exhausted Receiver, by being let fall on a red hot Iron; the mercurial Gage instantly descends upon the Explosion, and as suddenly ascends again; and after a few Vibrations, none of which except the first are of any great Extent, it seemingly fixes at a Place lower than where it stood before the Explosion, and this stationary Point is, what we have always attended to in our Experiments. But even, when the Gage has acquired this Point of apparent Repose, it still continues rising for a considerable Time, altho' by such imperceptible Degrees, that it can only be discovered by comparing together its Place at distant Intervals; however, it will not always continue to ascend, but will rise slower and slower, till at last it will be absolutely fixed at a Point lower than where the Mercury stood before the Explosion.

The same Circumstances nearly happen, when Powder is fired in the upper Part of an unexhausted Tube, whose lower Part is immersed in Water.

Now these Appearances all arise from the different Modifications, which the Fluid produced from the Explosion undergoes. The first sudden Descent of the Mercury is effected by the Action of that Fluid, while in the Form of Flame. When the Flame is extinguished,

extinguished, and consequently the Heat of the Fluid is diminished, its Elasticity is likewise diminished; and this being effected in a very short Time, occasions the sudden Rise of the Mercury after the first Descent. When the Fluid is reduced to the Temperature of the containing Receiver, its Elasticity is then more fixed and invariable; and this must usually happen by the Time the Mercurial Gage first appears to be stationary. The subsequent flow Ascent of the Mercury is partly owing to the Decrease of the Heat of the Receiver, occasioned by the cooling of the hot Iron contained in it; but much more to the Action of the sulphureous Fumes of the Brimstone and Charcoal, which absorb a Part of the generated Fluid, and thereby diminish its Pressure on the Gage.

SCHOLIUM.

In the following Propositions we shall irrefragably demonstrate, that the Force of fired Gunpowder is nothing more than the Pressure of the Fluid, which is generated in the preceeding Experiments, and that this Fluid in its Action observes the same Laws with other elastic Fluids, particularly the Air; so that whatever Power is produced by the firing of a given Quantity of Gunpowder, the same would be exerted by substituting in its stead a Quantity of Air equal to the Fluid generated in the Explosion, provided that Air be included in the same Space, and be heated to the same Degree, as the other Fluid is, at the Instant of its firing. Mr. *Hales* has concluded, that the Weight of the factitious elastic Fluids produced from chymical Processes is the same with that of common Air; he having tried that produced from Tartar with great Exactness. He has found too, that they expand with Heat, and contract with Cold, and that with the same Pressure they are condensed in the

same Degree with common Air. And that, when they are cleared of their sulphureous Fumes, which is done by making them pass through Water, they will then continue for many Months, nay Years, without losing any considerable Part of their Elasticity. And from these and other Circumstances he doubts not to assert, that these Fluids are true permanent Air. Now if this be supposed of all, or any of the elastic Fluids produced by Distillation, Burning, &c. it must be preferably allowed to be true of that Fluid, which is generated in the Explosion of Gunpowder; since it is from Saltpetre alone, that this Fluid seems to be derived (for neither the Brimstone nor the Charcoal yield it, when fired by themselves) and Saltpetre is known to be a Substance imbibed from the Air by Earth; for the same Parcel of Earth by being properly exposed to the Air will furnish Saltpetre over and over again for ever. However, tho' it be highly reasonable to suppose, that the elastic Fluid arising from the firing of Powder, is genuine and permanent Air; yet the Truth or Falseness of this Supposition no ways affects the Certainty of our Conclusions. It is sufficient for our Purpose, that it is an elastic Fluid; whether it be Air or another Composition our Reasoning will be still the same, since it is by Experiments on this Fluid itself, and not by obscure Speculations on its Nature and Qualities, that our future Deductions relating to its Force and Action are confirmed.

P R O P.

P R O P. III.

The Elasticity or Pressure of the Fluid produced by the firing of Gunpowder, is cæteris paribus directly as its Density.

THIS follows from hence, that, if in the same Receiver: a double Quantity of Powder be let fall, the Mercury will subside twice as much as in the firing of a single Quantity. For the Vapour produced from the double Quantity being contained in the same Receiver will be of double the Density of that produced from the single Quantity; whence the Elasticity or Pressure estimated by the Descent of the Mercury being likewise double, the Pressure is directly as its Density. Also the Descents of the Mercury, when equal Quantities of Powder are fired in different Receivers, are reciprocally as the Capacities of those Receivers; and consequently as the Density of the produced Fluid in each.

But as in the usual Method of trying this Experiment, the Quantities of Powder are too very small, that it is difficult to ascertain these Proportions to a requisite Degree of Exactness, I took a large Receiver containing about 520 Inches, and letting fall at once on the red hot Iron, 1 Dram or the $\frac{1}{16}$ of an Ounce Avoirdupoise of Powder (the Receiver being first nearly exhausted,) the Mercury after the Explosion was subsided 2 Inches exactly, and all the Powder had taken Fire. Then heating the Iron a second Time, and exhausting it as before, 2 Drains were let down at once, which sunk the Mercury $3\frac{1}{4}$ and a small Part of the Powder had fallen beside the Iron, which (the Bottom of the Receiver being wet) did not fire, and the Quantity, that thus escaped, did appear to be nearly sufficient, had it fallen on the

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Iron,

Iron, to have sunk the Mercury $\frac{1}{2}$ Part of an Inch more ; in which Case the two Descents, viz. 2 Inches, and 4 Inches, would have been accurately in the Proportion of the respective Quantities of Powder ; from which Proportion, as it was, they very little varied.

Hence then it appears, that the Elasticity of the Vapour produced by Gunpowder in its Explosion, is directly as its Density.

P R O P. IV.

To determine the Elasticity, and Quantity of this elastic Fluid, produced from the Explosion of a given Quantity of Gunpowder.

AS different Kinds of Gunpowder produce different Quantities of this Fluid in Proportion to their different Degrees of Goodness, before any definite Determination of this kind can take Place it is necessary to ascertain the particular Species of Powder that is proposed to be used ; and therefore I shall in every Examination and Position relating to this Subject suppose the Powder in Question to be of the same sort with what is made for the Use of the Government ; that being by Contract to consist of a known and invariable Proportion of Materials ; and is therefore much properer for a Standard, than what is compounded according to the arbitrary Fancy of the Artist.

This being settled, we must further premise these two Principles, which we have already mentioned in the Scholium to Prop II. the first, that the Elasticity of this Fluid increases by Heat, and diminishes by Cold, in the same Manner as that of the Air ; the second, that the Density of this Fluid and consequently its Weight is the same with the Weight of an equal Bulk of Air, having the same Elasticity, and the same Temperature.

Now from the Experiment recited in the last Proposition, it appears that $\frac{1}{16}$ of an Ounce Avoirdupoise or about 27 Grains Troy, of Powder, sunk the Gage on its Explosion 2 Inches; and the Mercury in the Barometer standing at near 30 Inches, $\frac{5}{16}$ of an Ounce Avoirdupoise, or 410 Grains Troy, would have filled the Receiver with a Fluid, whose Elasticity would have been equal to the whole Pressure of the Atmosphere, or the same with the Elasticity of the Air we breathe; and the Content of the Receiver being about 520 cubic Inches, it follows, that $\frac{5}{16}$ of an Ounce of Powder will produce 520 cubic Inches of a Fluid, possessing the same Degree of Elasticity with common Air; whence an Ounce of Powder will produce near 575 cubic Inches of such a Fluid.

But in order to ascertain the Density of this Fluid we must consider, what Part of its Elasticity at the time of this Determination was owing to the Heat it received from the included hot Iron and the warm Receiver. Now the general Heat of the Receiver being manifestly less than that of boiling-Water, which is known to increase the Elasticity of the Air, somewhat more than $\frac{1}{4}$ of its augmented Quantity, I collect from hence and other Circumstances, that the Augmentation of Elasticity arising from this Cause was about the $\frac{1}{5}$ of the whole, that is, if the Fluid arising from the Explosion had been reduced to the Temperature of the external Air, the Descent of the Mercurial Gage instead of 2 Inches would have been only $1\frac{1}{5}$ Inch; whence 575, reduced in the Proportion of 5 to 4, becomes 460, and this last Number represents the cubic Inches of an elastic Fluid equal in Density and Elasticity with common Air, which are produced from the Explosion of 1 Ounce Avoirdupoise of Gunpowder, the Weight of which Quantity of Fluid according to the usual Estimation of the Weight of Air is 131 Grains; whence

whence the Weight of this Fluid is $\frac{17}{437}$ or $\frac{1}{26}$ nearly of the Weight of the generating Powder.

If the Ratio of the Bulk of the Gunpowder to the Bulk of this Fluid be wanted, this will be determined by knowing, that 1 Ounce 1 Dram, or 17 Drams Avoirdupoise of Powder, fill 2 cubic Inches, if the Powder be well shook together; wherefore augmenting the Number last found in the Proportion of 16 to 17, the resulting Term 488 is the Number of cubic Inches of an elastic Fluid, equal in Density with the Air produced from 2 cubic Inches of Powder; whence the Ratio of the respective Bulks of the Powder, and the Fluid produced from it, is in round Numbers, 1 to 244.

And farther, to confirm this Determination, I fired the Quantity of a Dram of Powder four times successively, in an exhausted Receiver by a Burning-glass; the Capacity of this Receiver was 470 cubic Inches. These Experiments were more troublesome than those, in which it was fired by a hot Iron, because it was sometimes long before it would fire; in which Interval the Air would often insinuate itself, and thereby disturb the Measures of the Descent; and besides, near Part of the Powder was usually dissipated unfired by the Blast: However, by collecting the Grains that were thus scattered, and weighing them, and increasing the Descent by a proportional Quantity, the Subsiding of the Mercury, corresponding to one Dram of Powder, was the first time 2, 1 + Inches, the second time 1, 8 — Inches, the third time 2, 1 —, and the fourth time 1, 8 5 Inches, or at a Medium 1, 9 6 Inches; and this, diminished in the Ratio of 520 to 470, becomes 1, 77 for the Descent to a like Quantity in the first Receiver. Now the Deduction to be made on Account of the Heat of the Receiver was but little in these Experiments; for, by including a small Thermometer, I found that the Fluid within the Receiver

ceiver was not hotter after the Blast than that of the Summer Air; whence, if the Descent 1,77 be reduced in the Ratio of 13 to 12, which is nearly that of the Elasticity of hot Summer Air to temperate Air, it becomes 1,63 nearly, which differs little from 1,5, or 1,6; which is, what we found it to be in the preceding Experiment: Whence the Proportion between the respective Bulks of the Powder, and the Fluid produced from it, may be still assumed to be that of 1 to 244.

And this Ratio agrees very well with the Experiment recited by Mr. *Hauksbee* in his *Physico-Mechanic Experiments*, p. 81; for he there found, that one Grain of Powder produced when fired in the Air, a cubic Inch of elastic Fluid, which, supposing the Density of Powder to be what we have here assigned, gives the Ratio of their respective Bulks to be that of 1 to 232; a Difference, from what we have assigned above, that may easily arise from the Difference of the Powder only. Whence we may conclude, that the Presence of a greater or less Quantity of Air does not affect the Production of this Fluid; since, by comparing Mr. *Hauksbee's* Experiment with our own, it appears, that the same Quantity of this Fluid is generated in a *Vacuum* as in the Air.

If this Fluid, instead of expanding, when the Powder was fired, had been confined in the same Space, which the Powder filled before the Explosion; then (its Elasticity having been shewn to be as its Density) it would have had, in that confined State, a Degree of Elasticity 244 times greater than that of common Air, and this independent of the great Augmentation this Elasticity would receive from the Action of the Fire in that instant.

Hence then we are certain, that any Quantity of Powder fired in any confined Space, which it adequately fills, exerts at the Instant of its Explosion
against

against the Sides of the Vessel containing it, and the Bodies it impells before it, a Force at least 244 times greater than the Elasticity of common Air, or, which is the same thing, than the Pressure of the Atmosphere; and this without considering the great Addition which this Force will receive from the violent Degree of Heat, with which it is endued at that Time; the Quantity of which Augmentation is the next Head of our Enquiry.

P R O P. V.

To determine how much the Elasticity of the Air is augmented, when heated to the extreme Heat of red-hot Iron.

TO fix this Point, I took a Piece of a Musket-barrel, about six Inches in Length, and order'd one End to be closed up entirely, but the other End was drawn out conically, and finished in an Aperture of about $\frac{1}{8}$ of an Inch in Diameter. This Tube, thus fitted, was heated to the Extremity of a red Heat in a Smith's Forge, and was then immersed with its Aperture downwards in a Bucket of Water, and kept there, till it was cool; after which it was taken out carefully, and the Water, which had enter'd it in cooling, was exactly weigh'd. The Weight of the Water thus taken in at three different Trials was 610 Grains, 595 Grains, and 600 Grains respectively. The Content of the whole Cavity of the Tube was 796 Grains of Water; whence the Spaces remaining unfilled in these three Experiments were equal in Bulk to 186, 201, 196 Grains of Water respectively; and these Spaces did doubtless contain all the Air, which, when the Tube was red-hot, did extend through its whole Concavity; consequently the Elasticity of the Air, when heated to the extreme Heat of red-hot Iron,

was

was to the Elasticity of the same Air, when reduced to the Temperature of the ambient Atmosphere, as the whole Capacity of the Tube to the respective Spaces taken up by the cooled Air, that is, as 796 to 186, 201, 196, or taking the Medium of these three Trials, as 796 to 194 ;.

The Heat given to the Tube each Time was the Beginning of what Workmen call a white Heat ; and to prevent the rushing in of the aqueous Vapour at the Immersion, which will otherwise drive out great Part of the Air, and render the Experiment fallacious, I had an Iron Wire filed tapering, so as to fit the Aperture of the Tube, and with this I always stopt it up, before it was taken from the Fire, letting it remain in till the whole was cool, when removing it, the due Quantity of Water would enter.

P R O P. VI.

To determine how much that Elasticity of the Fluid produced by the firing of Gunpowder, which we have above assigned, is augmented by the Heat, it has at the Time of its Explosion.

AS Air and this Fluid appear to be equally affected by Heat and Cold, and consequently have their Elasticities equally augmented by the Addition of equal Degrees of Heat to each, if we suppose the Heat, with which the Flame of fired Powder is endued, to be the same as that of the extreme Heat of red-hot Iron, then the Elasticity of the generated Fluid will be greater at the Time of Explosion, when it is in the Form of Flame, than afterwards, when it is reduced to the Temperature of the ambient Air, in the Ratio of 796 to 194 ; nearly, that is, in the Ratio of the Elasticities of common Air, under similar Circumstances, ascertained in the last Proposition.

Now

Now that the Heat of Powder, when fired in any considerable Quantity, is not less than that of red-hot Iron, seems sufficiently evident from the Appearance of the Flame, and the known Properties of some of its Materials; for the Fire produced by the Explosion is certainly as active as any common Fire; and it is well known, that all Fires will communicate a red-hot Heat to Iron, provided the Bulk of the Iron be sufficiently small, when compared with the Quantity of the Fire.

This being supposed then, that the Flame of fired Gunpowder is not less hot than red-hot Iron, and the Elasticity of the Air, and consequently of the Fluid generated by the Explosion, being augmented by the Extremity of this Heat in the Ratio of 194 $\frac{1}{2}$ to 796, as has been shewn in the last Proposition, it follows, that if 244 be augmented in this Ratio, the resulting Number, which is 999 $\frac{1}{2}$, will determine how many times the Elasticity of the Flame of fired Powder exceeds the Elasticity of common Air, supposing it to be confined in the same Space which the Powder filled, before it was fired. For since we have shewn, in the 3d Proposition, that the elastic Fluid produced from the firing a Quantity of Powder would, if confined in the same Space which the Powder took up before its Explosion, exert an Elasticity 244 times greater than the Elasticity of common Air, supposing the Temperature of that Fluid and of the Air to be the same, it is plain from hence, that, when 244 is increased in the Ratio, in which the Elasticity of this Fluid is greater at the Time of the Explosion than afterwards, the resulting Number will ascertain, how many times the Elasticity of this inflamed Fluid, at the Instant of its Explosion, and before it has dilated itself, exceeds the Elasticity of common Air.

Hence

Hence then the absolute Quantity of the Pressure exerted by Gunpowder at the Moment of its Explosion may be assigned; for, since the Fluid, then generated, has an Elasticity 999¹/₃, or in round Numbers, 1000 times greater than common Air; and since common Air by its Elasticity exerts a Pressure on any given Surface equal to the Weight of the incumbent Atmosphere, with which it is in *Equilibrio*, the Pressure exerted by fired Powder, before it has dilated itself, is one thousand times greater than the Pressure of the Atmosphere, and consequently the Quantity of this Force, on a Surface of an Inch square, amounts to above 6 Tun Weight, which Force however diminishes, as the Fluid dilates itself, according to what has been shewn in the third Proposition.

SCHOLIUM.

Tho' we have here supposed, that the Heat of Gunpowder, when fired in any considerable Quantity, is the same with Iron heated to the Extremity of a red Heat, or to the Beginning of a white Heat, (which Determination we shall hereafter confirm by many Experiments) yet it cannot be doubted, but that the Fire produced in the Explosion is somewhat varied (like all other Fires) by a greater or less Quantity of Fuel; and it may be presumed, that, according to the Quantity of Powder fired together, the Flame may have all the different Degrees from that of a languid red Heat to the Heat sufficient for the Vitrification of Metals; but as the Quantity of Powder requisite for the Production of this last mentioned Heat, is certainly greater than what is ever fired together for any military Purpose, we shall find, by our future Experiments, that we shall not be far from our Scope, if we suppose the Heat of such Quantities

as come more frequently in Use to be, when fired, nearly the same with the strongest Heat of red-hot Iron; allowing a gradual Augmentation to this Heat in larger Quantities, and diminishing it when the Quantities are very small.

P R O P. VII.

Given the Dimensions of any Piece of Artillery, the Density of its Ball, and the Quantity of its Charge, to determine the Velocity which the Ball will acquire from the Explosion, supposing the Elasticity of the Powder at the first Instant of its firing to be given.

IN the Solution of this Problem, we shall assume the two following Principles:

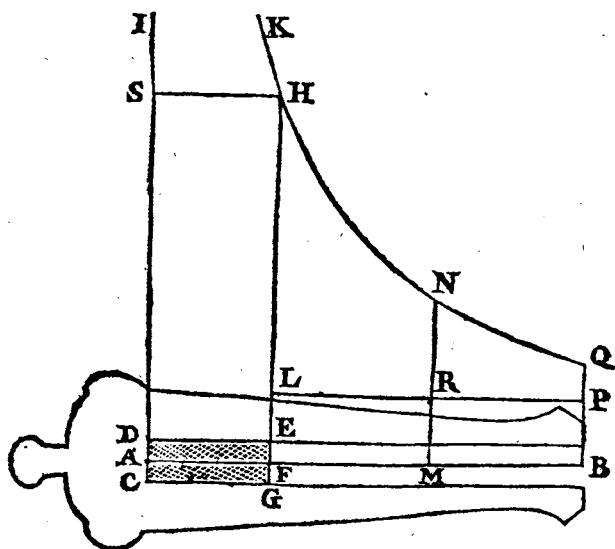
I. That the Action of the Powder on the Bullet ceases, as soon as the Bullet is got out of the Piece.

II. That all the Powder of the Charge is fired, and converted into an elastic Fluid, before the Bullet is sensibly moved from its Place.

These Postulates we shall demonstrate in an annexed *Scholium*; and they being supposed, the Proposition itself is thus determined:

Let AB represent the Axis of any Piece of Artillery, A the Breech, and B the Muzzle; DC the Diameter of its Bore, and DEGC a Part of its Cavity filled with Powder. Suppose the Ball, that is to be impelled, to lie with its hinder Surface at the Line GE, then the Pressure exerted at the Explosion, on the Circle of which GE is the Diameter, or, which is the same thing, the Pressure exerted in the Direction FB, on the Surface of the Ball, is easily known from

from the known Dimensions of that Circle; draw any Line FH perpendicular to FB , and AI parallel to FH , and through the Point H , to the Asymptotes IA and AB , describe the Hyperbola $KHNQ$; then if FH represents the Force impelling the Ball at the Point F , the Force impelling the Ball in any other Place as M will be represented by the Line MN , the Ordinate to the Hyperbola at that Point; for when the Fluid impelling the Body along, has dilated itself to M , its Density will be then to its original Density in the Space $DEGC$ reciprocally as the Spaces, thro' which it is respectively extended; that is, as FA to MA , or as MN to EH ; but we have shewn in the second Proposition, that the Elasticity or impelling Force of this Fluid is directly as its Density; therefore, if FH represents that Force at the Point F , MN will represent the like Force at the Point M .



Since the absolute Quantity of the Force impelling the Ball at the Point F is known, and the Weight

Weight of the Ball is likewise known; the Proportion between the Force, the Ball is impelled with, and its own Gravity is known. In this Proportion take FH to FL , and draw LP parallel to FB ; then MN the Ordinate to the Hyperbola in any Point will be to its Part MR , cut off by the Line LP , as the impelling Force of the Powder in that Point M , to the Gravity of the Ball; and consequently, the Line LP will determine a Line proportional to the uniform Force of Gravity in every Point; whilst the Hyperbola HNQ determines in like Manner such Ordinates, as are proportional to the impelling Force of the Powder in every Point; whence, by the 39th Proposition of Lib. I. of Sir *Isaac Newton's Phil. Nat. Prin. Math.* the Areas $FLPB$ and $FHQB$ are in the duplicate Proportion of the Velocities, which the Ball would acquire, when acted on by its own Gravity through the Space FB , and when impelled through the same Space by the Force of the Powder. But since the Ratio of AF to AB , and the Ratio of FH to FL are known, the Ratio of the Area $FLPB$ to the Area $FHQB$ is known; and thence its Subduplicate. And since the Line FB is given in Magnitude, the Velocity, which a heavy Body would acquire when impelled thro' this Line by its own Gravity, is known, being no other than the Velocity it would acquire by falling thro' a Space equal to that Line; find then another Velocity, to which this last mentioned Velocity bears the given Ratio of the Subduplicate of the Area $FLPB$ to the Area $FHQB$, and this Velocity, thus found, is the Velocity the Ball will acquire when impelled thro' the Space FB by the Action of the inflamed Powder.

Now to give an Example of this, let us suppose AB the Length of the Cylinder to be 45 Inches, its Diameter DC , or rather the Diameter of the Ball, to be

be $\frac{3}{4}$ of an Inch; and AF the Extent of the Powder to be 2 $\frac{1}{2}$ Inches; to determine the Velocity which will be communicated to a leaden Bullet by the Explosion, supposing the Bullet laid at first with its Surface contiguous to the Powder.

By the Theory we have laid down in the last Proposition, it appears, that at the first Instant of the Explosion the Flame will exert, on the Bullet lying close to it, a Force 1000 times greater than the Pressure of the Atmosphere; the Medium Pressure of the Atmosphere is esteemed equal to that of a Column of Water 33 Feet high; whence Lead being to Water as 11,345 to 1, this Pressure will be equal to that of a Column of Lead 34,9 Inches in Height, whence multiplying this by 1000, a Column of Lead 34900 Inches high would produce a Pressure equal to what is exerted on the Ball by the Powder in the first Instant of the Explosion, and the leaden Ball being $\frac{3}{4}$ of an Inch in Diameter, and consequently equal to a Cylinder of Lead on the same Base $\frac{1}{2}$ an Inch in Height, the Pressure at first acting on it will be equal to 34900×2 or 69800 times its Weight; whence FL to FH is as 1 to 69800: And FB to FA is as $45 - 2\frac{1}{2}$ (or $42\frac{1}{2}$) to $2\frac{1}{2}$; that is, as 339 to 21, whence the Rectangle FLPB is to the Rectangle AFHS, as 339 to 21 $\times$ 69800; that is, as 1 to 4324—. And from the known Application of the Logarithms to the Mensuration of the Hyperbolic Spaces it follows, that the Rectangle AFHS is to the Area FHQB as ,43429 *Œc.* is to the tabular Logarithm of $\frac{AB}{AF}$; that is, of $\frac{1\frac{1}{2}}{2\frac{1}{2}}$, which is 1,2340579, whence the Ratio of the Rectangle FLPB to the Hyperbolic Area FHQB is compounded of the Ratios of 1 to 4324—, and of ,43429 *Œc.* to 1,2340579, which together make up the Ratio of 1 to 12263, the Subduplicate of which is the Ratio of 1 to 110,7; and in this Ratio is the Velocity, which the Bullet would acquire by Gravity, in falling through a Space equal to FB, to the Velocity

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the Bullet will acquire from the Action of the Powder, impelling it through FB, but the Space FB being $42\frac{1}{8}$ Inches, the Velocity a heavy Body will acquire in falling through such a Space is known to be, what would carry it nearly at the Rate of 15,07 Feet in 1st of Time; whence the Velocity, to which this has the Ratio of 1 to 110,7, is a Velocity, which would carry the Ball at the Rate of $15,07 \times 110,7$ Feet in 1st of Time; that is, at the Rate of 1668 Feet in 1st of Time. And this is the Velocity, which, according to the Theory, the Bullet in the present Circumstances would acquire from the Action of the Powder, during the Time of its Dilatation.

And this being once computed for one Case is easily applied to any other; for, if the Cavity DE GC left behind the Bullet be only in Part filled with Powder, then the Line HF, and consequently the *Area* FHQB will be diminished in the Proportion of the whole Cavity to the Part filled; if the Diameter of the Bore be varied, the Lengths AB and AF remaining the same, then the Quantity of Powder and the Surface of the Bullet, which it acts on, will be varied in the duplicate Proportion of the Diameter; but the Weight of the Bullet will vary in the triplicate Proportion of the Diameter; wherefore the Line FH, which is directly as the absolute impelling Force of the Powder, and reciprocally as the Gravity of the Bullet, will change in the reciprocal Proportion of the Diameter of the Bullet. If AF the Height of the Cavity left behind the Bullet be increased or diminished, the Rectangle of the Hyperbola, and consequently the *Area* corresponding to ordinates in any given Ratio, will be increased or diminished in the same Proportion. From all which it follows, that the *Area* FHQB, which is in the duplicate Proportion of the Velocity of the impelled Body, will be directly as the Logarithm $\frac{AB}{AF}$, (where AB represents the Length of the Barrel, and AF the Length of the Cavity left behind the Bullet) also directly as the Part of that Cavity

Cavity filled with Powder, and inversely as the Diameter of the Bore or rather of the Bullet, likewise directly as AF the Height of the Cavity left behind the Bullet. Consequently the Velocity being computed above for a Bullet of a determined Diameter placed in a Piece of a given Length and impelled by a given Quantity of Powder, occupying a given Cavity behind that Bullet, it follows, that by means of these Ratios the Velocity of any other Bullet may be thence deduced, the necessary Circumstances of its Position, Quantity of Powder, &c. being given. Where note, that in the Instance of this Proposition we have supposed the Diameter of the Ball to be 1 of an Inch: Whence the Diameter of the Bore will be something more, and the Quantity of Powder contained in the Space DEGC will amount to exactly 12dw. a small Wad of Tow included.

SCHOLIUM.

In this Proposition we have taken for granted,
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, before the Bullet is sensibly moved from its Place.

These Assumptions we are now to demonstrate.

The first will, I presume, appear manifest, when it is considered, how suddenly the Flame will extend itself on every side, by its own Elasticity, when it is once got out of the Mouth of the Piece, for by this means its Force will then be dissipated, and the Bullet will be no longer sensibly affected by it.

The second Principle is indeed less obvious, being contrary to the general Opinion of almost all Writers on this Subject. But however it is not less certain. It might perhaps be sufficient for the

Proof of this Position to observe the prodigious Compression of the Flame in the Chamber of the Piece. Those, who will attend to this Circumstance and to the easy Passage of the Flame through the Intervals of the Grains, may soon satisfy themselves, that no one Grain contained in that Chamber can continue for any Time uninflamed, when thus surrounded and violently pressed by so active a Fire. However, not to rely on mere Speculation in a Point of so much Consequence, I considered, that, if Part only of the Powder is fired, and that successively, then by laying a greater Weight before the Charge (suppose 2 or 3 Bullets instead of one) a greater Quantity of Powder would necessarily be fired, since a heavier Weight would be a longer Time in passing through the Barrel. Whence it should follow, that two or three Bullets would be impelled by a much greater Force than one only. But the contrary to this appears by Experiment; for firing one, two, and three Bullets laid contiguous to each other with the same Charge respectively, I have found (by a Method to be mentioned hereafter) that their Velocities were not much different from the Reciprocal of the Subduplicate of their Quantities of Matter, that is, if a given Charge would communicate to one Bullet a Velocity of 1700 Feet in 1", the same Charge would communicate to two Bullets a Velocity from 1250 to 1300 Feet in 1", and to three Bullets a Velocity from 1050 to 1110 Feet in one 1". From hence it appears, that, whether the Piece be loaded with a greater or less Weight of Bullet, the Action of the Powder is nearly the same; since all Mathematicians know, that if Bodies containing different Quantities of Matter are successively impelled thro' the same Space by the same Power, acting with a determined Force at each Point of that Space, then the Velocities given to those different Bodies will be reciprocally in the subduplicate Ratio of their

their Quantities of Matter. The Excess of the Velocities of the two and three Bullets above what they should have been by this Rule, (which are that of 1200 and 980 Feet in 1st.) does doubtless arise from the Flame, which escaping by the Side of the first Bullet acts on the Surface of the second and third.

Now this Excess has in many Experiments been imperceptible, and the Velocities have been reciprocally in the subduplicate Ratios of the Number of Bullets to sufficient Exactness; and where this Error has been greater, it has never arisen to an eighth Part of the whole; but if the common Opinion was true, that a small Part only of the Powder fires at first, and other Parts of it successively, as the Bullet passes through the Barrel, and that a considerable Part of it is often blown out of the Piece without firing at all; then the Velocity, which three Bullets received from the Explosion, ought to have been much greater, than we have ever found it to be; since the Time of the Passage of three Bullets thro' the Barrel being nearly double the Time, in which one passes, it should happen, according to this vulgar Supposition, that in a double Time a much greater Quantity of the Powder should be fired, and consequently a greater Force should have been produced, than what acted on the single Bullet only, contrary to all our Experiments.

But further, the Truth of the second Postulate will be more fully evinced, when it shall appear, as it will hereafter, that the Rules founded on this Supposition ascertain the Velocities of Bullets impelled by Powder, to the same Exactness, when they are acted on through a Barrel of 4 Inches in Length only, as when they are discharged from one of four Feet.

With Respect to the Grains of Powder, which are often blown out unfired, and which are always

urged as a Proof of the gradual firing of the Charge, I believe *Diego Uffano*, a Person of great Experience in the Art of Gunnery, has given the true Reason for this Accident, which is, that some small Part of the Charge is often not rammed up with the rest, but is left in the Piece before the Wad, and is by this Means expelled by the Blast of Air before the Fire can reach it*; I must add, that in the Charging of Cannon and small Arms, especially after the first Time, this is scarcely to be avoided by any Method I have yet seen practised. Perhaps too, there may be some few Grains in the best Powder of so heterogeneous a Composition as to be less susceptible of firing, which I think I have myself observed; these, tho' they are surrounded by the Flame, may be driven out unfired. However, be that as it may, the Truth of our Position cannot in general be questioned.

Having in this Proposition shewn how the Velocity, which any Bullet acquires from the Force of Powder, may be computed upon the Principles of the Theory laid down in the preceeding Propositions of this Treatise, we will next shew, that the actual Velocities, with which Bullets of different Magnitudes are impelled from different Pieces, with different Quantities of Powder, are really the same with the Velocities assigned by these Computations, and consequently, that this Theory of the Force of Powder, here delivered, does unquestionably ascertain the true Action and Modification of this enormous Power.

But in order to compare the Velocities communicated to Bullets by the Explosion with the Velocities resulting from the Theory by Computation, it is necessary, that the actual Velocities, with which Bullets move, should be capable of being discovered, which yet is impossible to be done by any

* Dialog. 20.

Methods hitherto made publick. The only Means hitherto practis'd by others for that Purpose have been either by observing the Time of the Flight of the Shot through a given Space, or by measuring the Range of the Shot at a given Elevation; and thence computing, on the Parabolic Hypothesis, what Velocity would produce this Range. The first Method labours under this insurmountable Difficulty, that the Velocities of these Bodies are often so swift, and consequently the Time observed is so short, that an imperceptible Error in that Time may occasion an Error in the Velocity, thus found, of 2, 3, 4, 5, or 600 Feet in a Second. The other Method is so fallacious, by reason of the Resistance of the Air, (to which Inequality the first is also liable) that the Velocities thus assigned may not be perhaps the tenth Part of the actual Velocities sought.

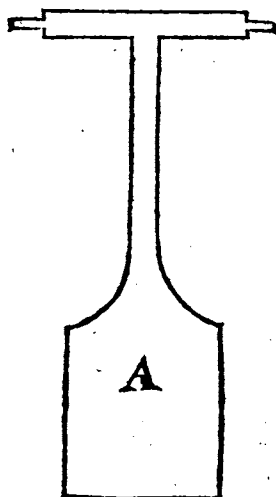
To remedy then these Inconveniencies, I have invented a new Method of finding the real Velocities of Bullets of all kinds; and this to such a Degree of Exactness, (which may be augmented too at Pleasure) that in a Bullet moving with a Velocity of 1700 Feet in 1", the Error in the Estimation of it need never amount to its five hundredth Part; and this without any extraordinary Nicety in the Construction of the Machine. The Description and Use of which is the Subject of the next Proposition.

P R O P. VIII.

To determine the Velocity, which any Ball moves with at any Distance from the Piece, it is discharged from.

THE simplest Method of effecting this is by Means of an Instrument like to that exhibited in the engraved Figure, where A B C D represents the

the Body of the Machine composed of the three Poles B, C, D spreading at Bottom, and joining together at the Top A; being the same with what is vulgarly used in the weighing and lifting of very heavy Bodies, and is called by Workmen the Triangles. On two of these Poles, towards their Tops, are screwed on the Sockets RS; and on these Sockets the Pendulum EFGHIK is hung by Means of its cross Piece EF, which becomes its Axis of Suspension, and on which it must be made to vibrate with great Freedom. The Body of this Pendulum is made of Iron, having a broad Part at Bottom, which cannot be seen in this Scheme; but its entire Shape is represented in the annexed Figure A.



The lower Part of the Pendulum is covered with a thick Piece of Wood GKI H, which is fasten'd to the Iron by Screws. Something lower than the Bottom of the Pendulum there is a Brace OP, joining

joining the two Poles to which the Pendulum is suspended ; and to this Brace there is fasten'd a Contrivance M N U, made with two Edges of Steel, bearing on each other in the Line UN, something in the Manner of a Drawing-pen ; the Strength with which these Edges press on each other being diminished or increased at Pleasure, by Means of a Screw Z going through the upper Piece. There is fasten'd to the Bottom of the Pendulum a narrow Ribbon LN, which passes between these Steel Edges, and which afterwards, by Means of an Opening cut in the lower Piece of Steel, hangs loosely down, as at W.

This Instrument thus fitted, if the Weight of the Pendulum be known, and likewise the respective Distances of its Centre of Gravity, and of its Centre of Oscillation, from its Axis of Suspension, it will thence be known, what Motion will be communicated to this Pendulum by the Percussion of a Body of a known Weight moving with a known Degree of Celerity, and striking it in a given Point ; that is, if the Pendulum be supposed at Rest before the Percussion, it will be known, what Vibration it ought to make in Consequence of such a determined Blow ; and, on the contrary, if the Pendulum, being at Rest, is struck by a Body of a known Weight, and the Vibration, which the Pendulum makes after the Blow, is known, the Velocity of the striking Body may from thence be determined.

Hence then, if a Bullet of a known Weight strikes the Pendulum, and the Vibration, which the Pendulum makes in consequence of the Stroke, be ascertained, the Velocity, with which the Ball moved, is thence to be known.

Now the Extent of the Vibration, made by the Pendulum after the Blow, may be measured to great Accuracy by the Ribbon LN ; for let the Pressure of the Edges U N on the Ribbon be so regulated
by

by the Screw Z, that the Motion of the Ribbon between them may be free and easy, tho' with some minute Resistance; then settling the Pendulum at Rest, let the Part L N between the Pendulum and the Edges be drawn strait, but not strained, and fix a Pin in that Part of the Ribbon, which is then contiguous to the Edges: Let now a Ball impinge on the Pendulum, then the Pendulum swinging back will draw out the Ribbon to the just Extent of its Vibration, which will consequently be determined by the Interval on the Ribbon between the Edges U N and the Place of the Pin.

But the Computation, by which the Velocity of the Ball is determined from the Vibration of the Pendulum after the Stroke, requires a more particular Explication; and for this Purpose we will exhibit, as an Example, the Pendulum made use of by us in some of our Experiments.

The Weight of the whole Pendulum, Wood and all, was 56 lb. 3 oz. its Centre of Gravity was 52 Inches distant from its Axis of Suspension, and 200 of its small Swings were performed in the Time of 253 Seconds; whence its Centre of Oscillation (determined from hence) is $62\frac{2}{3}$ Inches distant from that Axis. The Centre of the Piece of Wood G K I H is distant from the same Axis 66 Inches.

In the compound Ratio of 66 to $62\frac{2}{3}$, and 66 to 52, take the Quantity of Matter of the Pendulum to a 4th Quantity, which will be 42 lb. $\frac{1}{2}$ oz. Now Geometers will know, that if the Blow be struck in the Centre of the Piece of Wood G K I H, the Pendulum will resist to the Stroke in the same Manner, as if this last Quantity of Matter only (42 lb. $\frac{1}{2}$ oz.) was concentrated in that Point, and the rest of the Pendulum was taken away; whence, supposing the Weight of the Bullet impinging in that Point to be the $\frac{1}{12}$ of a Pound, or the $\frac{1}{504}$ of this Quantity of Matter

Matter nearly, the Velocity of the Point of Percussion after the Stroke will, by the Laws observed in the Congress of such Bodies as rebound not from each other, be the $\frac{1}{505}$ of the Velocity the Bullet moved with before the Stroke; whence the Velocity of this Point of Percussion after the Stroke being ascertained, that multiplied by 505 will give the Velocity with which the Ball impinged.

But the Velocity of the Point of Percussion after the Stroke is easily deduced from the Chord of the Arch, through which it ascends by the Blow; for it is a well known Proposition, that all pendulous Bodies ascend to the same Height by their vibratory Motion, as they would do, if they were projected directly upwards from their lowest Point, with the same Velocity they have in that Point; wherefore, if the versed Sine of the ascending Arch be found, (which is easily determined from the Chord and Radius being given) this versed Sine is the perpendicular Height, to which a Body projected upwards with the Velocity of the Point of Percussion would arise; and, consequently, what that Velocity is, can be easily computed by the common Theory of falling Bodies.

For instance, the Chord of the Arch, described by the Ascent of the Pendulum after the Stroke measured on the Ribbon, has been sometimes $17 \frac{1}{2}$ Inches; the Distance of the Ribbon from the Axis of Suspension is $71 \frac{1}{8}$ Inches, whence reducing $17 \frac{1}{2}$ in the Ratio of $71 \frac{1}{8}$ to 66, the resulting Number, which is nearly 16 Inches, will be the Chord of the Arch, through which the Centre of the Board GKIH ascended after the Stroke: Now the versed Sine of an Arch, whose Chord is 16 Inches, and its Radius 66, is 1,93939; and the Velocity, which would carry a Body to this Height, or, which is the same thing, the Velocity, which a Body would acquire by descending

descending through this Space, is nearly that of $3 \frac{1}{2}$ Feet in $1''$.

To determine then the Velocity, with which the Bullet impinged on the Centre of the Wood, when the Chord of the Arch described by the Ascent of the Pendulum, in consequence of the Blow, was $17 \frac{1}{4}$ Inches measured on the Ribbon, no more is necessary, than to multiply $3 \frac{1}{2}$ by 505, and the resulting Number 1641 will be the Feet which the Bullet would describe in $1''$, if it moved with the Velocity it had at the Moment of its Percussion; for the Velocity of the Point of the Pendulum, on which the Bullet struck, we have just now determined to be that of $3 \frac{1}{2}$ Feet in $1''$; and we have before shewn, that this is the $\frac{1}{505}$ of the Velocity of the Bullet. If then a Bullet weighing $\frac{1}{12}$ of a Pound strikes the Pendulum in the Centre of the Wood **GKIH**, and the Ribbon be drawn out $17 \frac{1}{4}$ Inches by the Blow, the Velocity of the Bullet is that of 1641 Feet in $1''$. And since the Length, the Ribbon is drawn, is always nearly the Chord of the Arch described by the Ascent, (it being placed so as to differ insensibly from those Chords which most frequently occur) and these Chords are known to be in the Proportion of the Velocities of the Pendulum acquired from the Stroke, it follows, that the Proportion between the Lengths of Ribbon drawn out at different times, will be the same with that of the Velocities of the impinging Bullets; and, consequently, by the Proportion of these Lengths of Ribbon to $17 \frac{1}{4}$, the Proportion of the Velocity, with which the Bullets impinge to the known Velocity of 1641 Feet in $1''$, will be determined.

Hence then is shewn, in general, how the Velocities of Bullets of all kinds may be found out by Means of this Instrument; but that those, who may be disposed to try these Experiments, may not have unforeseen Difficulties to struggle with, I shall here
subjoin

subjoin a few Observations, which it will be necessary for them to attend to, both to secure Success to their Trials, and Safety to their Persons.

And first, that they may not conceive the Piece of Wood G K I H to be an unnecessary Part of the Machine, I must inform them, that if a Bullet impelled by a full Charge of Powder should strike directly on the Iron, the Bullet would be beaten into Shivers by the Stroke, and these Shivers will rebound back with such Violence, as to bury themselves in any Wood they chance to light on, as I have found by hazardous Experience; and besides the Danger, the Pendulum will not in this Instance ascertain the Velocity of the Bullet, because the Velocity, with which the Parts of it rebound, is unknown.

The Weight of the Pendulum, and the Thickness of the Wood, must be in some Measure proportioned to the size of the Bullets which are used. A Pendulum of the Weight here described will do very well for all Bullets under three or four Ounces, if the Thickness of the Board be increased to 7 or 8 Inches for the heaviest Bullets; Beech is the toughest and properest Wood for this Purpose.

It is hazardous standing on the side of the Pendulum, unless the Board be so thick, that the greatest Part of the Bullet's Force is lost before it comes at the Iron, for if it strikes the Iron with Violence, the Shivers of Lead, which cannot return back thro' the Wood, will force themselves out between the Wood and Iron, and will fly to a considerable Distance.

As there is no effectual way of fastening the Wood to the Iron but by Screws, the Heads of which must come thro' the Board; the Bullets will sometimes light on those Screws, from whence the Shivers will disperse themselves on every side.

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When in these Experiments so small a Quantity of Powder is used, as will not give to the Bullet a Velocity of more than 4 or 500 Feet in 1", the Bullet will not stick in the Wood, but will rebound from it entire, and (if the Wood be of a very hard Texture) with a very considerable Velocity. Indeed I have never examined any of the Bullets, which have thus rebounded, but I have found them indented by the Bodies they have struck against in their Rebound.

To avoid then these Dangers, to the braving of which in Philosophical Researches no Honour is annexed, it will be convenient to fix whatsoever Barrel is used, on a strong heavy Carriage, and to fire it with a little slow Match. Let the Barrel too be very well fortified in all its Length; for no Barrel, (I speak of Musket Barrels) forged with the usual Dimensions will bear many of the Experiments recited hereafter without bursting, as I have found to my Cost. The Barrel, I have most relied on, and which I procured to be made on Purpose, is nearly as thick at the Muzzle as at the Breech, that is, it has in each Place nearly the Diameter of its Bore in Thickness of Metal.

The Powder used in these Experiments should be exactly weigh'd, and that no Part of it be scatter'd in the Barrel, the Piece must be charged with a Ladle in the same manner as is practised with Cannon, the Wad should be of Tow of the same Weight each Time, and no more than is just necessary to confine the Powder in its proper Place, the Length of the Cavity left behind the Ball should be determined each Time with Exactness, for the increasing or diminishing that Space will vary the Velocity of the Shot, altho' the Bullet and Quantity of Powder be not changed. The Distance of the Mouth of the Piece from the Pendulum ought to be such, that the Impulse of the Flame may not act on the Pendulum; this

this will be prevented in a common Barrel charged with $\frac{1}{2}$ an Ounce of Powder, if it be at the Distance of 16 or 18 Feet; in larger Charges the Impulse is sensible farther off, I have found it extend to above 25 Feet; however, between 25 and 18 Feet is the Distance I have usually chosen; other Precautions, which are necessary, will better find their Place in the Account of the Experiments I have made, to which I now hasten.

P R O P. IX.

To compare the actual Velocities with which Bullets of different kinds are discharged from their respective Pieces, with their Velocities computed from the Theory.

HOW to determine the actual Velocities with which Bullets are discharged, has been amply shewn in the last Proposition; and how to compute the Velocity with which they would be discharged according to our Theory, has been likewise fully explained in the sixth Proposition; we shall here then compare the Result of our Theory with Experience, and thence evince, how accurately that Theory agrees with the real Motions of Bullets, tho' founded on Principles no ways connected with these Experiments.

The first Experiments, I shall exhibit, were made with a Barrel of the same Dimensions with the Example of the sixth Proposition, the Ball being $\frac{1}{4}$ of an Inch in Diameter, the Length 45 Inches, and the Cavity containing the Powder 2 $\frac{1}{2}$ Inches, which, as the Barrel exceeded the Bullet in Diameter by about the $\frac{1}{16}$ th of an Inch, just contained 12 dw. of Powder.

The Bullet thus made use of was $\frac{1}{12}$ of a Pound, Avoirdupoise, in Weight, and consequently the same
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with

with the Example of the seventh Proposition ; but the Board on the Pendulum used here was 4 lb. lighter than what is assigned in that Example ; from these Circumstances, and the Velocity which by the Theory the Bullet ought to be discharged with, there is known the Chord of the Arch measured on the Ribbon, thro' which the Pendulum should ascend after the Stroke, if the Theory be true : How near this agrees with our Experiments, will appear by the following Table :

No.	Quantity of Powder.	Chord of ascending Arch measured on the Ribbon.	The same by the Theory.	Error of Theory.
	Dw.			
1	12	18, 7	19, 0	+ , 3
2	12	19, 6	19, 0	— , 6
3	6	13, 6	13, 4	— , 2

The next Experiments were made with the same Barrel, but the Board on the Pendulum was now of little more Weight than that in the Example of the seventh Proposition.

No.	Length of the Cavity containing the Powder or Line AF, in Fig. 1.	Quantity of Powder.	Chord of ascending Arch measured on the Ribbon.	The same by Theory.	Error of Theory.
	Inch. s.	Dw.	Inch.	Inch.	Inch
4	2 $\frac{5}{8}$	6	11, 9	12, 1	+ , 2
5	2 $\frac{5}{8}$	6	12, 2	12, 1	— , 1
6	1 $\frac{1}{4}$	6	13, 2	13, 6	+ , 4
7	1 $\frac{1}{4}$	6	13, 9	13, 6	— , 3
8	2 $\frac{5}{8}$	12	16, 7	17, 2	+ , 5
9	2 $\frac{5}{8}$	12	17, 5	17, 2	— , 3
10	2 $\frac{5}{8}$	12	16, 9	16, 8	— , 1
11	2 $\frac{1}{4}$	12	17, 0	16, 8	— , 2
12	2 $\frac{5}{8}$	6	11, 7	11, 5	— , 2
13	2 $\frac{5}{8}$	6	11, 1	11, 5	+ , 4
14	2 $\frac{5}{8}$	12	16, 7	16, 3	— , 4

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The last five Numbers resulting from the Theory are corrected from the Quantity of Bullets lodged in the Board, which, as many other Experiments of a different kind were tried in the Interval, amounted at last to above two Pounds; whence the Weight of the Pendulum being increased, its Vibration with the same Blow must be proportionably diminished.

The next Experiments were made with a Barrel of the same Bore with the last, but only 12, 375 Inches in Length: To distinguish them, we shall for the future denominate the first Barrel by the Letter A, and this short one by C. The Board on the Pendulum was at first rather lighter than in the Example of the seventh Proposition.

No.	Barrel	Extent of the Cavity containing the Powder.	Quantity of Powder.	Chord of ascending Arch measured on the Ribbon.	The same by Theory.	Error of Theory.
		Inch.	Dw.	Inch.	Inch.	
15	C	2 $\frac{5}{8}$	12	12, 7	12, 8	+, 1
16	C	2 $\frac{5}{8}$	12	12, 6	12, 8	+, 2
17	C	2 $\frac{5}{8}$	12	12, 4	12, 8	+, 4
18	A	2 $\frac{5}{8}$	12	17, 0	17, 3	+, 3
19	A	2 $\frac{5}{8}$	12	17, 2	17, 2	, 0
20	A	2 $\frac{5}{8}$	12	17, 1	17, 2	+, 1
21	A	2 $\frac{5}{8}$	12	17, 2	17, 2	, 0
22	A	2 $\frac{5}{8}$	6	12, 4	12, 2	—, 2

In some of the following Experiments a third Barrel was used of the same Bore with the other two, but 24, 312 Inches in Length: This Barrel I denominate B; the Board fixed on the Pendulum was at first but little heavier than that in the seventh Proposition; and when in the Course of the Experiments it is sensibly increased in the Weight, I diminish the Numbers arising from the Theory by a corresponding Part.

D 2

No.

No.	Barrel.	Extent of the Cavity contain- ing the Powder.	Quant- ity of Powder.	Chord of as- cending Arch measured on the Ribbon.	The same by Theory.	Error of Theory.
		Inch.	dw.	Inch.	Inch.	
23	A	2 $\frac{5}{8}$	12	17, 1	17, 2	+, 1
24	A	2 $\frac{5}{8}$	9	15, 2	15, 0	—, 2
25	A	2 $\frac{5}{8}$	9	15, 4	15, 0	—, 4
26	C	2 $\frac{5}{8}$	12	11, 5	12, 8	+1, 3
27	C	2 $\frac{5}{8}$	12	11, 5	12, 8	+1, 3
28	C	2 $\frac{5}{8}$	6	8, 7	9,	+, 3
29	C	2 $\frac{5}{8}$	12	12, 3	12, 5	+, 2
30	B	2 $\frac{5}{8}$	12	14, 4	14, 4	0, 0
31	B	2 $\frac{5}{8}$	12	14, 4	14, 4	0, 0
32	B	2 $\frac{5}{8}$	6	10, 3	10, 5	+, 2
33	A	1 $\frac{3}{4}$	8	14, 7	14, 5	—, 2
34	A	4	12	15, 7	15, 3	—, 4

The Error in the 26th and 27th Experiments being much greater than what has occurred to me in any other Trials, I suspect, that some Mistake was made in the Weight of the Powder, or that the Barrel (which had indeed lain by in a moist Place) was very damp; which Circumstance, I know by Experience, will considerably diminish the Action of the Powder.

The following Experiments were made with a Pendulum much heavier, it weighing in the whole 97 lb. its Center of Gravity was 55,625 Inches distant from its Axis of Suspension, and 200 of its small Swings were performed in the Space of 255" $\frac{1}{2}$, whence its Center of Oscillation is 63,9 Inches distant from the Axis of Suspension. Also sometimes another Barrel was used 7,06 Inches in Length, and .83 in Diameter, its Ball was exactly fitted to the Bore without any Windage, so that it went in with Difficulty, the Weight of this Ball was 33 $\frac{1}{2}$ dw. This Barrel we shall denominate D.

No.	Burrel	Length of the Cavity contain- ing the Powder.	Quant- ity of Powder.	Chord of the ascending Arch measured on the Ribbon.	The same by the Theory.	Error of The- ory.
		Inch.	dw.	Inch.	Inch.	Inch.
35	A	2 $\frac{5}{8}$	12	9, 2	9, 2	,0
36	A	2 $\frac{5}{8}$	12	9, 5	9, 2	—,3
37	A	5 $\frac{1}{4}$	24	11, 7	11, 3	—,4
38	A	7 $\frac{7}{8}$	36	13, 2	12, 6	—,6
39	A	2 $\frac{5}{8}$	12	9, 3	9, 1	—,2
40	A	1 $\frac{3}{4}$	8	7, 6	8, 1	+ ,5
41	C	2 $\frac{5}{8}$	12	6, 1	6, 6	+ ,5
42	C	2 $\frac{5}{8}$	12	6, 5	6, 6	+ ,1
43	B	2 $\frac{5}{8}$	12	8, 0	8, 2	+ ,2
44	B	2 $\frac{5}{8}$	12	8, 3	8, 2	—,1
45	A	2 $\frac{5}{8}$	12	9, 5	9, 1	—,4
46	A	2 $\frac{5}{8}$	12	9, 1	9, 1	,0
47	A	2 $\frac{5}{8}$	6	7, 2	6, 5	—,7
48	A	2 $\frac{5}{8}$	6	6, 7	6, 5	—,2
49	C	2 $\frac{5}{8}$	12	6, 8	6, 7	—,1
50	C	2 $\frac{5}{8}$	12	7, 5	6, 7	—,8
51	C	2 $\frac{5}{8}$	6	4, 7	4, 8	+ ,1
52	C	2 $\frac{5}{8}$	6	5, 0	4, 8	—,2
53	D	2 $\frac{1}{4}$	12	7, 0	7, 2	+ ,2
54	D	2 $\frac{5}{8}$	12	7, 1	6, 8	—,3
55	D	2 $\frac{5}{8}$	6	4, 7	4, 8	+ ,1
56	D	2 $\frac{5}{8}$	6	4, 8	4, 8	,0
57	A	2 $\frac{1}{10}$	6	6, 4	6, 5	+ ,1
58	A	2 $\frac{1}{10}$	6	6, 4	6, 5	+ ,1
59	A	2 $\frac{1}{10}$	6	6, 6	6, 5	—,1
60	A	2 $\frac{1}{10}$	6	6, 7	6, 5	—,2
61	A	2 $\frac{1}{10}$	12	9, 0	9, 1	+ ,1

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The Error in the 50th Experiment, the greatest in this Set, was doubtless owing to the Wind; for the 49, which was made immediately before it in the same Manner, and with the same Quantity of Powder, differs but little from the Theory. The Excess of the 38th Experiment above the Theory was in Part occasioned by the Impulse of the Flame on the Pendulum, which in this large Quantity of Powder was plainly to be discerned.

This Theory is farther confirmed too, by Experiments made on the Action of very small Quantities of Powder. We have hitherto supposed the Powder when fired to be equally hot with Iron at the beginning of its white Heat, but we have observed, that in very small Quantities of Powder the Heat is probably less than this, and consequently the Elasticity in those Cases less than what arises from this Supposition. Now this Decrease of Elasticity in small Quantities of Powder we have found by many Trials actually to take place; for Instance, in the Example of the 7th Proposition, the Velocity, which should be given to the Ball by the Action of the Powder according to the Theory, is in round Numbers that of 1670 Feet in 1", and this Velocity we have found in the preceeding Experiments to be the medium Velocity, which the Ball really receives in those Circumstances. If now, the Barrel and Position of the Ball remaining the same, there be placed in the same Space D E G C continuing likewise the same 1 dw. of Powder instead of 12, which is the Quantity supposed in that Example, it follows from the Principles there laid down, that if the Elasticity of the smaller Charge be the same in Proportion to its Quantity with that of the larger, then the Velocity of the Bullet when impelled by the Explosion of the smaller Charge, will be to the Velocity of a Bullet impelled by the greater Charge in the subduplicate Ratio of the Quantities of the respective Charges,

Charges, that is, in the subduplicate Ratio of 1 to 12. Consequently the Velocity communicated by 12 dw. being known to be that of 1670 Feet in 1", the Velocity communicated by 1 dw. would be that of 482 Feet in 1" nearly; but I found by repeated Trials, which differed very little from each other, that the real Velocity acquired by the Ball in this Case, from the Explosion of 1 dw. was rather less than that of 400 Feet in 1"; whence it is evident, that the Elasticity of 1 dw. of Powder when fired is less in Proportion to its Quantity than that of 12 dw. as it ought to be by our Theory.

Likewise if 3 dw. of Powder be placed in the same Manner with the 1 dw. in the last Example, the real Velocity, the Ball will acquire from the Explosion, will be from 740 to 720 Feet in 1", as I have found by many Experiments; whereas supposing the Elasticity of 3 dw. when fired to be in similar Circumstances the same with that of 12 dw. the Velocity acquired by the Ball should be 835 Feet in 1"; whence, even in 3 dw. the Elasticity and consequently the Heat is less than in 12 dw. as it ought to be by the Theory.

The Ratios of these diminished Elasticities to the Elasticity established by us for larger Quantities, are the Duplicate of the Ratios of the respective Velocities communicated by them in acting thro' the same Space, whence the Elasticity of 1 dw. is to the Elasticity of 12 dw. in similar Circumstances as 2 to 3. and the Elasticity of 3 dw. to that of 12 dw. is as 3 to 4 nearly, supposing the smaller Elasticities to be uniformly diminished in each Part of their Expansion; but in all Probability this is not the Case, for as the Decrease of Elasticity is owing to a Deficiency in the Heat of the Fluid, it is most reasonable to conceive, that in these small Quantities the Heat is not only less at first, but is likewise perpetually decreasing, as the Bullet is impelled thro'

the Barrel, and consequently that the comparative Elasticity is more abated, the longer its Action continues ; a Circumstance that does not take place, when the Quantity of Powder is well proportioned to the Barrel, thro' which it is to act.

S C H O L I U M.

Thus then have we confirmed our Theory by the most indisputable Evidence, which is its Coincidence with a numerous Series of Experiments made in all the various Circumstances, that the Nature of the Subject could suggest ; and I must add, that these Experiments or the greatest Part of them were made and register'd before any of the Computations, by which they are compared with the Theory, were formed ; altho' I had ascertained that Theory, such as it is here delivered, some time before I enter'd on these Experiments. :

The Variety of these Experiments, and the Accuracy with which they correspond to the Theory, leave us no room to doubt of the Certainty of the Theory ; for we have examined the Action of fired Powder on Bullets of different Weights and in Barrels of different Magnitudes from 7 Inches to 45 Inches in Length ; we have varied too the Quantity of Powder from 6 dw. to 36 dw. and have inclosed these Quantities differently, sometimes in such Cavities as they exactly filled, at other times in such as were sufficient to have held much greater Quantities ; and we have found, that in all these different Circumstances, our Theory has truly exhibited the real Velocities with which the Bullets were impelled by the Explosion, or if (as in very small Quantities of Powder) there were any Exceptions from the general Rule we had established, it was, what the Theory did plainly demand ; and surely that Theory, which so accurately agrees with Experiment in these diversified Trials, cannot but contain
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the true and genuine Determination of the Force and Manner of acting of fired Gunpowder.

This Theory as here established supposes, that in the firing of Gunpowder about $\frac{1}{10}$ of its Substance is converted by the sudden Inflammation into a permanent elastic Fluid, whose Elasticity in Proportion to its Heat and Density is the same with that of common Air in the like Circumstances; it farther supposes, that all the Force exerted by Gunpowder in its most violent Operations is no more than the Action of the Elasticity of the Fluid thus generated; and these Principles enable us, as we have seen, to determine the Velocities of Bullets impelled from Fire-Arms of all kinds, and are fully sufficient for all Purposes, where the Force of Gunpowder is to be estimated. Whether this Fluid be true and genuine Air, or another Substance, we shall not discuss in this Place, as it is an Inquiry no Ways connected with the Design of this Treatise.

From this Theory many Deductions may be made of the greatest Consequence to the Practical Part of Gunnery. From hence the Thickness of a Piece, which will enable it to confine without Bursting any given Charge of Powder, is easily determined, since the Effort of the Powder is known. From hence appears the Inconclusiveness of what some modern Authors have advanced relating to the Advantages of particular Forms for the Chambers of Mortars and Cannon; for all their labour'd Speculations on this Head are evidently founded on very erroneous Opinions about the Action of fired Powder. From this Theory too we are taught the Necessity of leaving the same Space behind the Bullet, when we would by the same Quantity of Powder communicate the same Velocity to the Bullet; since on our Principles it follows, that the same Powder has a greater or less Degree of Elasticity, according to the different Spaces it occupies. The Method,
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which I have always practised for this Purpose, has been by marking the Rammer, and this is a Maxim, which ought not to be dispensed with, when Cannon are fired at an Elevation, particularly in those called by the French *Batteries à Ricochet*.

Weight

From the continued Action of the Powder, and its Manner of expanding described in this Theory, and the Length of the Piece, one of the most essential Circumstances in the well directing of Artillery may be easily ascertained. All Practitioners are agreed, that no Shot can be depended on, unless the Piece be placed on a solid Platform; for if the Platform shakes with the first Impulse of the Powder, it is impossible, but the Piece must likewise shake, which will alter its Direction and render its Shot uncertain. To prevent this Accident, the Platform is usually made extremely firm to a considerable Depth backwards, so that the Piece is not only well supported in the Beginning of its Motion, but likewise thro' a great Part of its Recoil. However, it is sufficiently obvious, that when the Bullet is separated from the Piece, it can be no longer affected by the trembling of the Piece or Platform; and by a very easy Computation it will be found, that in a Piece 10 Feet in length, carrying a Bullet of 24 lb. and charged with 16 lb. of Powder, the Bullet will be out of the Piece before the Piece has recoil'd $\frac{1}{2}$ an Inch; whence, if the Platform be sufficiently solid at the Beginning of the Recoil, the remaining Part of it may be much slighter, since its Unsteadiness beyond the first $\frac{1}{2}$ Inch will have no Influence on the Direction of the Shot. And hence a more compendious Method of constructing Platforms may be found out.

From this Theory it also appears, how greatly those Authors have been mistaken, who have attributed the Force of Gunpowder, or at least a considerable Part of it, to the Action of the Air, contained either in the Powder or between the Intervals of the Grains:

Grains. For they have supposed (tho' indistinctly enough) that Air to exist in its natural elastic State, and to receive all its Addition of Force from the Heat of the Explosion. But, from what we have experimented in the fifth Proposition, relating to the Increase of the Elasticity of the Air by Heat, we may conclude, that the Heat of the Explosion cannot augment the Elasticity of the Air to five times its common Quantity; consequently, the Force arising from this Cause only cannot amount to more than the 200th Part of the real Force exerted on this Occasion.

Having thus dispatched the general Confirmation of our Theory, we shall proceed to the Examination of some other Particulars relating to this Subject, which tho' easily enough flowing from the Principles already laid down, do yet from the Novelty and Singularity of the Matter merit a circumstantial Discussion.

P R O P. X.

To assign the Changes in the Force of Powder, which arise from the different State of the Atmosphere.

IN all the Experiments I have hitherto examined, I have never been able to discover, that the Variation of the Density of the Atmosphere did any way alter the Action of Powder, altho' I have made several hundred Shot in very different Seasons: In particular, I have sometimes compared the Trials made at Noon in the hottest Summer Sun with those made in the Freshness of the Morning and Evening, and I could not perceive any certain Difference between them, and it was the same with the Trials made in the Night and in Winter; altho' in this variety of Seasons, the Density of the Atmosphere must

must have been very different: Indeed, as we have seen, that the same Quantity of that Fluid, in which the Force of Powder consists, is generated in a *vacuum*, and in common Air, it is difficult to conceive, how this Force can be affected by the greater or lesser Density of the Atmosphere.

But tho' the Density of the Atmosphere has no Influence on the Force of Powder, yet its Moisture has a great one; for the same Quantity of Powder, which in a dry season would communicate to a Bullet a Velocity of 1700 Feet in 1st, will not in damp Weather communicate to the same Bullet, placed in the same manner, a Velocity of more perhaps than 12 or 1300 Feet in 1st, or still less, if the Powder be of a bad Sort, and has been negligently kept. And this Decrease of the Force in damp Powder appears by my Experiments to be very unsteady and variable, so that 2 Shot made with equal Quantities of such Powder, taken out of the same Parcel, will differ considerably from each other, perhaps ten times more than if the Powder was in good Order; and as far as this Uncertainty in its Effects will permit me, I seem to collect, that a small Charge loses a greater Part of its Force than a larger, provided each are equally damp. Another Circumstance attending damp Powder is a remarkable Foulness in the Piece after firing, much beyond what arises from an equal Quantity of dry Powder.

Now all these Effects are easily to be accounted for, when it is known, that Powder will imbibe Moisture from the Air; for as a certain Quantity of Water mixed with Powder will prevent its firing at all, it follows, that every Degree of Moisture in Powder, tho' insufficient to produce this Effect, will yet abate the Violence of its Explosion, and will render the Fire thereby produced less vehement than it would otherwise be; whence a less Quantity
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of Fluid will be in this Case generated, and the Heat of that Fluid and its Elasticity is likewise less; consequently, the Action of damp Powder must on this two-fold Account be diminished according to the Degree of Moisture, with which it is impregnated.

And as bad Powder usually contains some common Salt in it, by reason of the little Care taken in the refining of the Nitre, and as common Salt imbibes Moisture in a stronger Degree than Nitre, it is not difficult to conceive, how bad Powder should in a moist Season be more impregnated with Moisture than good, and should consequently lose more of its Force.

The Uncertainty in the Effects of damp Powder arises, I presume, from the different Degrees of Dryness it acquires in the Piece; for as after the first or second firing the Barrel grows warm, if the Powder is contained any Time in it, some Part of its Moisture will be thereby evaporated, and as the Heat of the Barrel, and the Time of the Charge continuing in it, are Circumstances, which in their Nature are very uncertain, it is not to be wonder'd at, that the Evaporation, and consequently the Action of the Powder, is likewise uncertain. I must remark on this Head, that, in the driest Seasons, I have found the Coldness of the Barrel, and perhaps some little Moisture condensed in its Cavity, to have sensibly diminished the Force of the Powder in the first Shot.

That small Quantities of Powder should have their Action more diminished than larger Quantities with the same Degree of Moisture, naturally follows from the smaller Degree of Heat, with which (as we have observed above) the Explosion of small Charges is attended; since the same Proportion of Moisture must of necessity clog a weak Fire more effectually, than it can do one which is more violent.

The remarkable Foulness of the Piece from the firing of damp Powder, which we have mentioned above, must likewise arise from the Diminution of the

the Activity of the Fire in the Explosion. For, when Powder is of a proper Temperature to fire readily and violently, the greatest Part of its Substance ought to be consumed to Ashes, which will then be discernable in the Form of a greyish Substance on all Bodies placed near the Mouth of the Piece; and the Foulness of the Piece is owing to those Parts of the Powder, which either by their Contiguity to the cold Barrel, or their less inflammable Composition, are but imperfectly burnt; now since moist Powder produces a less violent Flame in Proportion to the Moisture it imbibed, it must follow, that a smaller Part of the Powder will in this Case be perfectly consumed, and consequently a greater Part will remain to contribute to the Foulness of the Barrel.

SCHOLIUM.

We have asserted as the Basis of our Reasoning in this Proposition, that Powder will imbibe Moisture from the Air in a humid State of the Atmosphere; but it remains to assign the Quantity it can thus imbibe, which we shall here endeavour to do from our own Experiments.

A Parcel of very good Powder being placed on a white Paper, which was pierced with a great Number of fine Holes, and the Paper being held over the Steam of hot Water, I found, that in half a Minute the Powder was increased in Weight by about $\frac{1}{50}$ Part.

Trying another Parcel in the same Manner, but continuing it longer in the Steam, I found, that the Powder increased its Weight by $\frac{1}{24}$ Part; but in this Case some of the Grains adhered together in small Lumps, altho' the Figure of the Grains themselves was no ways changed.

To convince myself that the Moisture of the Atmosphere would likewise increase the Weight of Powder, I took about an Ounce of Powder, which
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had for some time been kept in a Room which had a Fire in it every Day, and I found by drying it before the Fire, that it lost above $\frac{1}{100}$ of its Weight, one third of which Decrease in Weight it had again acquired in less than two Hours, by being removed to a different Part of the Room, at a Distance from the Fire.

Now as the Weather is often much moister, than when I tried this Experiment, and as in open Air this Moisture abounds much more than in a Room, where there is a Fire; it cannot be doubted, but that sometimes the twentieth or thirtieth Part of the Substance of the best Powder is Water, which may be easily supposed to produce all the Effects, we have observed and described in this Proposition.

But however, the Moisture thus imbibed by Powder from the Air does not, as I have yet observed, render it less active, when it is dried again. The Reader must have observed in the Experiments of the last Proposition, how nearly those made with the same Quantities of Powder and in the same Circumstances agree with each other. In these Experiments, tho' made at different Times in the Course of three Summer Months, the Dryness of the Season prevented all the Inequalities of this Proposition. But trying the same Powder in the Winter, in a very damp Season, I found, that tho' if it was used as in the Summer, in its natural State, without any drying, its Effects were very irregular, and much short of those Experiments, yet if each Charge was well dried, just before it was used, no Diminution of its Force could then be perceived, nor did it appear to act in any manner different from what it had done in the preceding Summer. Indeed if the Powder be exposed to the greatest Damps without any Caution, or if common Salt abounds in it, the Moisture, it imbibes, may perhaps be sufficient to dissolve some Part of
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the Nitre, which is a lasting Damage, that no Drying can retrieve. But when tolerable Care is taken in preserving Powder, and the Nitre, it is composed of, has been well purged from common Salt, it will retain its Force much longer than is usually supposed. I have heard, that Powder, which had been well kept, did not at the End of fifty Years appear to be any ways injured by its Age.

Some Care is necessary in the drying of damp Powder; for there is a Degree of Heat, which tho' not sufficient to fire the Powder, will yet melt the Brimstone, and destroy the Texture of the Grains. Nay more, there is a Heat, with which the Brimstone will flame and burn away gradually, and yet the Powder will not explode; of this any one may satisfy himself by heating a Piece of Iron red-hot, and then throwing a few Grains of Powder on it at different Intervals, during the Time of its cooling, for by this Means he will find, that at a certain Time the separate Grains, that fall on the Iron, will not explode, but will burn with a small blue Flame for some Space of Time, the Grain still remaining unconsumed. Indeed, when it has begun to burn in this Manner, it sometimes ends with exploding, but this more commonly happens, when a Number of Grains lie near together; for then tho' each separate Flame is not sufficient to explode its respective Grain, yet the whole Fire made by them all together grows strong enough at last to end in a general Explosion; however, by attending to the proper Temperature of the Iron, and spreading the Grains, I have often covered two or three square Inches with a blue lambent Flame, which has lasted a considerable Time without any Explosion, and examining the Grains afterwards, I could not perceive, that they had lost either their Colour or their Shape. Now since these Grains, when the Brimstone is thus burnt or even melted out of them, will no longer act as Powder,
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it is evident, that Powder may be spoiled by being dried with too violent a Heat.

From the great Difference in the Effects of moist and dry Powder established in this Proposition, it appears, how very uncertain and irregular all those practical Operations of Gunnery may prove, where this Circumstance is not attended to; and how little Confidence can be placed in any Experiments, where this Cause of Inequality could interfere.

Before I leave this Article, I must mention a Suspicion, I once entertained about this Matter. As Water, when rarified into Vapour, is generally supposed to be near ten times more elastic than Air equally heated, I imagined, that possibly the Moisture imbibed by Powder might, in certain Cases, be so proportioned to the Quantity of Powder, that it might be converted into Vapour by the Explosion; and that thereby the Force of the Powder might be more increased by the Addition of this very elastic Vapour, than it was diminished by the damping of its Flame. And I was the more induced to believe, that this did sometimes happen, from the Experiments of a late Author, who tells us, that the Ranges of the same Shot, fired from the same Mortar, with equal Charges of Powder, were much greater in the Freshness of the Morning, than in the Heat of the Day. For I was well satisfied, that the meer Density of the Air (to which he seems to impute this Variety) could not produce such different Effects. However, upon a more accurate Examination, I cannot find, that any Degree of Moisture does at any time augment the Force of Powder; for, in all the numerous Trials I have made, I never observed that Force sensibly to exceed its mean Quantity, except in two Experiments; and even those Excesses, I had good Reason to believe, were occasioned by some Disorder in the Machine.

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However, if the Elasticity of watry Vapour be as great as it is usually esteemed, (a Point far from being ascertained at present) it is not impossible, but something of this kind may take Place in the firing of large Quantities of Powder.

P R O P. XI.

To investigate the Velocity which the Flame of Gunpowder acquires, by expanding itself, supposing it be fired in a given Piece of Artillery, without either a Bullet or any other Body before it.

IF the whole Substance of the Powder was converted into an elastic Fluid at the Instant of the Explosion, then from the known Elasticity of this Fluid assigned by our Theory, and its known Density, we could easily determine the Velocity, with which it would begin to expand, and could thence trace out its future Augmentations in its Progress through the Barrel; but as we have shewn, that the elastic Fluid, in which the Activity of the Gunpowder consists, is only $\frac{1}{10}$ of the Substance of the Powder, the remaining $\frac{9}{10}$ will in the Explosion be mixed with the elastic Part, and will by its Weight retard the Activity of the Explosion; and yet they will not be so compleatly united as to move with one common Motion, but the unelastic Part will be less accelerated than the rest, and some of it will not even be carried out of the Barrel, as appears by the considerable Quantity of unctuous Matter, which adheres to the Inside of all Fire-Arms, after they have been used.

These Inequalities in the expansive Motion of the Flame oblige us to recur to Experiments for its accurate Determination.

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The Experiments made use of for this Purpose were of two kinds: The first was made by charging the Barrel A with 12 dw. of Powder, and a small Wad of Tow only; and then placing its Mouth 19 Inches from the Centre of the Pendulum, mentioned in the seventh Proposition; on firing it in this Situation, the Impulse of the Flame on the Pendulum made it ascend through an Arch, whose Chord was 13, 7 Inches; whence, if the whole Substance of the Powder was supposed to strike against the Pendulum, and each Part to strike with the same Velocity, that common Velocity must have been at the Rate of about 2650 Feet in 1". This then is the least Velocity, which the Powder could be supposed to acquire in its Expansion; for if we suppose the elastic Part to acquire a greater Velocity in expanding, than the other gross Vapour, (which it undoubtedly does) this common Velocity here assigned must be augmented for the elastic Fluid, and diminished for the grosser Substance of the Powder. As some Part of the Velocity of the Flame was lost in passing through 19 Inches of Air, I made the remaining Experiments on this Subject in a manner not liable to that Inconvenience.

I fixed the Barrel A on the Pendulum, so that its Axis might be both horizontal and also perpendicular to the Plane HK; or, which is the same thing, that it might be in the Plane of the Pendulum's Vibration; the Height of the Axis of the Piece above the Centre of the Pendulum was six Inches, and the Weight of the Piece and of the Iron that fasten'd it, &c. was 11 lb. $\frac{1}{4}$. The Barrel in this Situation being charged with 12 dw. of Powder, without either Ball or Wad, the Powder only put together with the Rammer, on the Discharge the Pendulum ascended through an Arch, whose Chord was 10 Inches, or reduced to an equivalent Blow in the Centre of the Pendulum, sup-

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posing the Barrel away, it would be 14, 4 Inches nearly.

The same Experiment repeated again, the Chord of the ascending Arch was 10, 1 Inches, which, reduced to the Centre, is 14, 6 Inches.

To determine what Difference of Velocity there was in the different Parts of the Vapour, I loaded the Piece again with 12 dw. of Powder, and rammed it down with a Wad of Tow weighing 1 dw. Now I conceived, that this Wad being very light would presently acquire that Velocity, with which the elastic Part of the Fluid would expand itself when uncompressed; and I accordingly found, that the Chord of the ascending Arch was by this Means augmented to 12 Inches, or at the Centre to 17,3: Whence, as the Medium of the other two Experiments is 14,5, the Pendulum ascended thro' an Arch 2, 8 Inches longer, by the additional Motion of 1 dw. of Matter moving with the Velocity of the swiftest Part of the Vapour; and consequently, the Velocity, with which this 1 dw. of Matter moved, was that of about 7000 Feet in 1".

It will perhaps be objected to this Determination, that the Augmentation of the Arch, through which the Pendulum vibrated in this Case, was not all of it owing to the Quantity of Motion given to the Wad, but Part of it was produced by the Confinement of the Powder, and the greater Quantity thereby fired. But if it were true, that a Part only of the Powder fired, when there was no Wad, it would not happen, that in firing different Quantities of Powder without a Wad, the Chord of the ascending Arch would increase and decrease nearly in the Ratio of those Quantities, which yet I have found it to do; for with 9 dw. that Chord was 7, 3 Inches, which with 12 dw. we have seen was but 10, and 10, 1; and even with 3 dw. the Chord was 2 Inches;
deficient

deficient from this Proportion by .5 only ; for which Defect too, other valid Reasons are to be assigned.

And there is still a more convincing Proof, that all the Powder is fired, altho' no Wad be placed before the Charge ; which is, that the Part of the Recoil arising from the Expansion of the Powder alone is found to be no greater, when it impells a leaden Bullet before it, than when the same Quantity is fired without any Wad to confine it. We have seen, that the Chord of the Arch, thro' which the Pendulum rose from the expansive Force of the Powder alone, is 10, or 10, 1 ; and the Chord of that Arch, when the Piece was charged in the customary Manner with a Bullet and Wad, I found to be the first time 22 $\frac{1}{2}$, and the second time 22 $\frac{7}{8}$; or at a Medium 22, 56. Now the Impulse of the Ball and Wad, if they were supposed to strike the Pendulum in the same Place, in which the Barrel was suspended, with the Velocity they had acquired at the Mouth of the Piece, would drive it through an Arch, whose Chord would be about 12, 3, as is known from the Weight of the Pendulum, the Weight and Position of the Barrel, and the Velocity of the Bullet, determined by our former Experiments ; whence, subtracting this Number 12, 3 from 22, 56, the Remainder 10, 26 is nearly the Chord of the Arch, which the Pendulum would have ascended through, from the Expansion of the Powder alone, when fired with a Bullet before it ; and this Number 10, 26 differs very little from 10, 1, which we have above found to be the Chord of the ascending Arch, when the same Quantity of Powder expanded itself freely, without either Bullet or Wad before it.

Again, that this Velocity of 7000 Feet in 1" is not much beyond what the most active Part of the Flame acquires in expanding, is evinced from hence, that we have above, in the 38th. Experiment, an

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Instance

Instance of a Ball actually discharged with a Velocity of 2400 in 1st, and yet it appeared not, that the Action of the Powder on this Bullet was at all diminished on account of this immense Celerity; consequently, the Degree of Swiftneſs, with which in this Instance the Powder followed the Ball, without loſing any Part of its Preſſure, muſt have been much ſhort of what the Powder alone would have expanded with, had not the Ball been there.

And it is this prodigious Celerity of Expansion of the Flame of fired Powder, which is its peculiar Excellence; and the Circumſtance, in which it ſo eminently ſurpaſſes all other Inventions, either ancient or modern, for the Purpoſe of military Projections: For as to the Quantity of Motion of theſe Projectiles only, many of the warlike Machines of the Ancients produced this in a Degree far ſurpaſſing that of our heaviest Cannon-shot or Shells; but the great Celerity given to theſe Bodies cannot be in the leaſt approached by any other Means than by the Flame of Powder. The Reaſon of this Difference is, that the Ancients could by Weights, or the Elastiſcity of Springs and ſtretched Cords, augment their Powers to any Degree deſired; but then each Addition of Power brought with it a proportional Addition of Matter to be moved: So that as the Power increaſed, thoſe Parts of the Machine, which were to communicate Motion to the Projectile, and were conſequently to move with it, were likewiſe increaſed; and thence it neceſſarily happened, that the Action of the Power was not ſolely employed in giving Motion to the impelled Body, but much the greateſt Part of it was ſpent in accelerating thoſe Parts of the Machine, in which the Power reſided, to enable them to purſue the Body to be projected with a perpetual Impulſe, during its whole Paſſage thro' the Extent of their Activity. Hence then it came to paſs, that, tho' theſe ancient
Machines

Machines could throw enormous Weights, they could project them but with small Degrees of Celerity, compared with what we can communicate to our Cannon and Musket-shot: Whence, in all Operations, where these great Velocities are useful, our Machines are infinitely superior to those of Antiquity; altho', in more confined and shorter Projections, these last have some Advantages, which may yet render them worthy of the Attention of those military Genius's, who have Capacity enough to consider each Part of their Profession according to its true and genuine Value, independent of the partial Estimation of the Times they live in:

From the Determinations contained in this Proposition, the Force of Petards may be deduced, since their Action solely depends on the Impulse of the Flame; and it appears, that a Quantity of Powder, properly disposed in such a Machine, may produce as violent an Effort as a Bullet of twice its Weight, moving with a Velocity of 14 or 1500 Feet in 1".

P R O P. XII.

To ascertain the Manner in which the Flame of Powder impells a Ball, which is laid at a considerable Distance from the Charge.

WE have, in many of the Experiments recited by us above, laid the Ball not immediately contiguous to the Powder, but a small Distance from it; the greatest Interval, however, has not amounted to more than about 1½ Inch, from the hinder Part of the Bullet to the nearest Part of the Powder; and, in these Cases, we have seen, that the Theory agreed very well with the Experiments: But if a Bullet be placed at a greater Distance from

the Powder, suppose at 12, 18, or 24 Inches, we cannot then apply to the Motion of this Ball the same Principles which, in the 7th Proposition, we have applied to such as are contiguous to the Powder, or nearly so; for we have seen, in the last Proposition, that, when the Surface of the fired Powder is not confined by a heavy Body, which it is obliged to impell before it, the Flame dilates itself with a Velocity much beyond what it can at any time communicate to a Bullet by its continued Pressure; consequently, as in the Distance of 12, 18, or 24 Inches, the Powder will have acquired a considerable Degree of this Velocity of Expansion, the first Motion of the Ball will not be produced by the continued Pressure of the Powder, but by the actual Percussion of the Flame; and it will therefore begin to move with a Quantity of Motion proportioned to the Quantity of this Flame, and the Velocities of its respective Parts.

From hence then it follows, that the Velocity of a Bullet, laid a considerable Distance before the Charge, ought to be greater, than what would be communicated to it by the Pressure of the Powder acting in the Manner described in the 7th Proposition; and this Deduction from our Theory we have confirmed by manifold Experience; by which we have found, that a Ball laid in the Barrel A, with its hinder Part $11\frac{1}{4}$ Inches from its Breech, and impelled by 12 dw. of Powder, has acquired on its Discharge a Velocity of about 1400 Feet in 1"; when, if it had been acted on by the Pressure of the Flame only, it would not have acquired a Velocity of 1200 Feet in 1". The same we have found to hold true in all other greater Distances, (and also in lesser, tho' not to the same Degree) and in all Quantities of Powder. And we have likewise found, that these Effects nearly correspond with what was laid down in the last Proposition, about

about the Velocity of Expansion, and the elastic and unelastic Parts of the Flame.

And from hence too arises another Consideration of great Consequence in the Practice of Gunnery; which is, that no Bullet should at any time be placed at any considerable Distance before the Charge, unless the Piece be extremely fortified; for a moderate Charge of Powder, when it has expanded itself through the vacant Space, and reaches the Ball, will, by the Velocity each Part has acquired, accumulate itself behind the Ball, and will thereby be condensed prodigiously; whence, if the Barrel be not of an extraordinary Firmness in that Part, it must, by this reinforced Elasticity of the Powder, infallibly burst. The Truth of this Reasoning I have experienced in an exceeding good *Tower-Musquet*, forged of very tough Iron; for charging it with 12 dw. of Powder, and placing the Ball 16 Inches from the Breech, on the firing it, the Part of the Barrel just behind the Bullet was swelled out to double its Diameter, like a blown Bladder, and two large Pieces of two Inches long were burst out of it.

Having seen that the entire Motion of a Bullet, laid at a considerable Distance from the Charge, is acquired by two different Methods, in which the Powder acts on it; the first being the Percussion of the Parts of the Flame, with the Velocity they had respectively acquired by expanding; the second, the continued Pressure of the Flame through the remaining Part of the Barrel; I endeavoured to separate these different Actions, and to retain that only, which arose from the continued Pressure of the Flame. For this Purpose, I no longer placed the Powder at the Breech, from whence it would have full Scope for its Expansion, but I scattered it as uniformly as I could through the whole Cavity left behind the Bullet; imagining that, by this Means,
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the progressive Velocity of the Flame in each Part would be prevented by the Expansion of the neighbouring Parts: And I found, that the Ball being laid $11\frac{1}{2}$ Inches from the Breech, its Velocity, instead of 1400 Feet in 1", which it acquired in the last Experiments, was now no more than 1100 Feet in 1"; which is 100 Feet short of what, according to the Theory, should arise from the continued Pressure of the Powder only

The Reason of this Deficiency was, doubtless, the intestine Motion of the Flame; for the Accension of the Powder, thus distributed through so much a larger Space than what it could fill, must have produced many Reverberations and Pulsations of the Flame; and from these internal Agitations of the Fluid, its Pressure on the containing Surface will (as is the Case in all other Fluids) be considerably diminished; and it has been in order to avoid this Irregularity, that in all the Experiments I have made, I have taken particular Care to have the Powder closely confined in as small a Space as possible, even when the Bullet lay at some little Distance from it.

P R O P. XIII.

To enumerate the various Kinds of Powder, and to describe the properest Methods of examining its Goodness.

THE Powder, we have hitherto considered, is supposed to be such, as is made for the Service of the Government; but, besides this, there are many other Kinds, some better and some worse, which I here propose to enumerate, as far as they have come to my Knowledge.

But, first, I must premise, that the Government Powder, if properly wrought, is, I believe, nearly

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as good, as any Powder made for general Use. I have examined it with great Care, and have compared it with other Powders made here in *England*, which are esteemed the best, such as the *Battle*, &c. and I cannot find any sensible Difference between them. I have likewise compared it, in frequent Trials, with some *Spanish* Powder, taken out of the *St. Jago* Prize; and tho', if I were to give my Opinion, I should rather believe the *Spanish* Powder the better of the two, yet so small an Inequality as a fiftieth or sixtieth Part, which is the most that the Difference between them can amount to, is too little to be ascertained with absolute Certainty. I conceive too, by comparing the Experiments of others with my own, that the *French* Powder is little different from ours; altho' I cannot be so certain on this Head as I could wish, having never been able to procure any of their Powder myself. But it must be remember'd, that when I speak of our Government Powder, it must be, what is supposed to be made of the Standard Proportion of Materials, and properly wrought; for such was the Powder I made use of.

The strongest Powder, I have yet met with, is some which, I am told, was made in *Holland*; its Force, compared with that of our Government Powder, is nearly as 5 to 4. But this Powder is undoubtedly made of the choicest pick'd Materials, and is probably wrought up with Spirits; so that Quantities of it could not be made, but at a much greater Expence, than what would be repaid by its additional Strength.

The next best Powder, that has come to my Hands, is a Powder made in *Portugal*, under the Direction of a *Dutchman*, who some Years since established Powder-Mills near *Lisbon*. This is in Strength interior to the *Dutch* Powder last mentioned;

tioned ; but is, however, nearer to that than to our Government Powder.

The common Sale Powder here in *England*, such as is to be had at every Grocer's, is much worse than the Government or the *Battle* Powder, and extremely various, according to the Caprice of the Maker. I have tried some, whose Strength has been in Proportion to the Government Powder, as 2 to 3 nearly, and other Parcels have been still worse ; but the worst of all is the Powder made for the *African* Trade, usually styled *Guinea* Powder : But these weaker Powders are not worth Examination, as there is no established Standard for their Composition.

Now these Differences in the Strength of Powder may arise from three Causes ; either from the Quality of the Materials, from the Proportion observed in their Mixture, or from the Manner of working them together.

Powder, as is generally known, is composed of Saltpetre, Sulphur, and Charcoal : Of these Materials the Sulphur and Charcoal are much the cheapest ; and tho' there are peculiar Kinds of these, which are fittest for this Purpose, yet the Expence of having the very best is so small, when compared to the whole Cost of the Powder, that it is strange, if Powder, which would be otherwise good, is spoiled by bad Sulphur or Charcoal.

The most expensive Part of the Composition, and consequently the Part to which the Defects of Powder are oftner owing, is the Saltpetre. This is a Substance imbibed by the Earth from the Air ; for a Quantity of Earth, which has had its Saltpetre washed out of it, will, when it has been exposed to the Air for some time, produce Saltpetre again ; and this as often as the Experiment shall be repeated.

Saltpetre

Saltpetre is of itself an unflammable Substance; for if it be placed in the most violent Fire, it only melts, and never flames, provided no combustible Matter is permitted to mix with it: But tho' of itself, unmixed with other Bodies, it will neither flame nor burn; yet, if it be joined with burning Substances, it prodigiously augments the Violence of their burning; performing, in this Case, what the Air, forcibly mixed with Fire by the Blast of a Pair of Bellows, does in a much inferior Degree.

Powder then being a Mixture of Sulphur and Charcoal, which are very inflammable Substances, with Saltpetre, which in itself is not, if the Saltpetre be too much in Quantity, when compared with the other two, their burning may not be sufficient to consume the whole of the Saltpetre; whence the Fire may be less violent, and consequently (according to what we have observed in the 10th Proposition) the Powder less vigorous, than if some of the Saltpetre was taken away, and a like Quantity of the other Materials were added in its Stead. On the other hand, if the Saltpetre in the Composition be less, than what the burning of the other two Substances can easily consume, the Fire will be less active than it ought to be, because it is not augmented so much as it would be, if a larger Quantity of Saltpetre had been added to the Composition.

Hence then it appears, that the Goodness of Powder is not to be estimated only from the Quantity of Saltpetre contained in it, altho' that Substance seems to be the Basis of the elastic Fluid, in which its Force consists; for since the converting of the Saltpetre into that Fluid, and the Elasticity of the Fluid afterwards, depend in some measure on the Violence of the Fire produced at the Explosion, it is plain, that there is a certain Proportion in the Mixture of the Materials, which will best contribute

contribute to this Purpose, and consequently to the Perfection of the Powder.

What this Proportion is, has been ascertained by Experience; and it seems now to be generally agreed, that in any Quantity of Powder $\frac{1}{4}$ of it should be Saltpetre, the remaining $\frac{3}{4}$ consisting of equal Quantities of Sulphur and Charcoal. This is the Proportion followed by the *French*, and, I believe, by most Nations in *Europe*: We, indeed, pretend to a greater Degree of Nicety in our Proportions; tho', I am told, they do not greatly differ from what I have mentioned; nor am I convinced, that they are preferable: This I am sure of, that no Methods of proving Powder, hitherto generally practised in *England*, could at all ascertain the Difference; and other Powders, made with the usual Proportions, are no whit inferior to ours.

But it is not the due Proportion of the Materials only, which is necessary to the making of good Powder; another Circumstance, not less essential, is the mixing them well together; if this be not effectually done, some Parts of the Composition will have too much Saltpetre in them, and others too little; and in either Case there will be a Loss of Strength in the Powder.

As the Excellency of Powder then depends on so many Particulars, in the Quality and Quantity of the Materials, and in the working them together, it is, doubtless, of great Importance, that those who receive the publick Stores should have it in their Power to satisfy themselves about the Goodness of what is delivered to them. The Method most commonly followed for this Purpose, here with us, is (if I am rightly informed) to fire a small Heap of it on a clean Board, and to attend nicely to the Flame and Smoke it produces, as likewise to the Marks it leaves behind it on the Table; from all which instructive Particulars the Merit of the Powder

der is ascertained with great Accuracy, as is pretended: But besides this uncertain Method, which, I presume, (how much soever it may be practised) none will seriously undertake to defend, there are, on particular Occasions, other Contrivances made use of; all which bear some Analogy to the common Powder-tries, sold at the Shops; only they are more artfully fabricated, and instead of a Spring they move a Weight, which is a more certain and equable Power.

But these Machines, tho' more perfect than the common Powder-tries, are yet liable to great Irregularities; for as they are all moved by the instantaneous Stroke of the Flame, and not by its continued Pressure, they do not determine the Force of the fired Powder with that Certainty and Uniformity, which were to be desired in these kinds of Trials: And therefore, I cannot but think, the Method followed by the *French*, in the receiving of Powders ^{from} the Makers, to be much better. Their Practice is thus:

They have, in each Magazine, a small Mortar cast, with its Bed, according to a determined Pattern, which is the same throughout the Kingdom: This Mortar is always pointed at 45° , and it contains just three Ounces of Powder; and it is a standing Maxim, that no Powder can be received into their Stores, unless three Ounces of it, placed in the Chamber of this Mortar, throws a solid Ball of $7\frac{1}{2}$ Inches Diameter to the Distance of at least 55 *French* Fathom.

It has been objected to this Method, that if each Barrel of Powder was to be proved in this manner, the Trouble of charging the Mortar, and bringing back the Ball each time, would be intolerable, and the Delay so great, that no Business of this kind could ever be finished; and if a Number of Barrels are received on the Merit of a few, it is great Odds

Odds but some bad ones will be amongst them, which may prove a great Disappointment in time of Service. Add to this another Exception, which to me has much more Weight; and that is, the monstrous Disproportion between the Weight of the Ball and the Powder that projects it; so that the Powder continues in Action a longer time, and expands through a much larger Space, in Proportion to its Quantities in these Trials, than it ever does in any real Service; whence it happens, that the Vapour cools, and great Part of it escapes thro' the Touch-hole, or by the Side of the Bullet; so that the Quantity of Motion produced by the Explosion is, in this Instance, but little more than half of what it ought to be, if the Powder acted on the Ball with its full Force undiminished by these Accidents; consequently, as this Diminution of Force may not be always constant, the Action of the same Powder, by the varying of these adventitious Circumstances, may, at different Times, convey the Ball to different Distances.

Now this last Exception does no ways hold against the Method by which I have tried the comparative Strength of different kinds of Powder, which has been by the actual Velocity given to a Bullet, by such a Quantity of Powder as is usually esteemed a proper Charge for the Piece: And as this Velocity, however great, is easily discovered by the Motion which the Pendulum acquires from the Stroke of the Bullet, (according to the Principles laid down above) it might seem a good Amendment to the Method used by the *French*, to introduce this Trial by the Pendulum instead of it. But tho' I am satisfied, that this would be much more accurate, less laborious, and readier than the other, yet, as there is some little Attention and Caution required in this Practice, which might render it of less Dispatch than might be convenient, when a great Number of Barrels

Barrels were to be separately tried, I should myself choose to practise another Method not less certain, but prodigiously more expeditious; so that I could engage, that the weighing out of a small Parcel of Powder from each Barrel should be the greatest Part of the Labour; and, doubtless, three or four Hands could, by this Means, examine 500 Barrels in a Morning; besides, the Machines for this Purpose, as they might be made of cast Iron, would be so very cheap, that they might be multiplied at Pleasure. However, I shall defer the Description of this Method at present, and shall proceed to the Consideration of the Resistance of the Air, a Subject of the greatest Importance to the Perfection of Gunnery.



F

CHAP.



CH A P. II.

Of the Resistance of the Air, and of the Track described by the Flight of Shot and Shells.



BEFORE I more minutely discuss the Subject of this Chapter, it is necessary to premise, that the greatest Part of Authors have established it as a certain Rule, that, whilst the same Body moves in the same Medium, it is always resisted in the duplicate Proportion of its Velocity; that is, if the resisted Body move in one Part of its Track with three times the Velocity with which it moved in some other Part, then its Resistance to the greater Velocity will be nine times the Resistance to the lesser. If the Velocity in one Place be four times the Velocity in another, the Resistance to the greater Velocity will be sixteen times the Resistance to the lesser, and so on. This Rule, tho' excessively erroneous, (as we shall hereafter shew) when taken in a general Sense, is yet undoubtedly very near the Truth, when confined within certain Limits; and therefore, in our future Disquisitions, we shall suppose, that in all small Changes of Velocity in the resisting Body it does accurately hold true; so that when we speak hereafter of the Resistance of the Medium being increased or diminished by the varying of the Velocity, we shall not hereby include that Increase or Diminution, which ought to take Place according to this Law, but shall thereby intend a Resistance, greater

greater or less, than what the moving Body ought to undergo from the Application of this Principle ; that is, we shall thereby understand an Increase or Diminution in the resisting Power of the Medium, similar to what might be occasioned by increasing or diminishing its Density : the principal Purport of our present Attempt being to evince, that, according to the different Compression of the Medium, or the different Degree of Velocity in the moving Body, such Changes may arise in the resisting Power of the Medium, as could scarcely be effected, according to the Principles commonly received on this Subject, by a treble Augmentation of its Density. This we doubt not, irrefragably to confirm in the following Dissertation.

PROPOSITION I.

To describe the general Principles of the Resistance of Fluids to solid Bodies moving in them.

IN order to conceive the Resistance of Fluids to a Body moving in them, it is necessary to distinguish between those Fluids which, being compressed by some incumbent Weight, perpetually close up the Space deserted by the Body in Motion, without permitting for an Instant any Vacuity to remain behind it ; and those Fluids in which (they being not sufficiently compressed) the Space left behind the moving Body remains for some time empty. These Differences, in the resisting Fluids, will occasion very remarkable Varieties in the Laws of their Resistance, and are absolutely necessary to be considered in the Determination of the Action of the Air on Shot and Shells ; for the Air partakes of both these Affections, according to the different Velocities of the projected Body.

If a Fluid was so constituted, that all the Particles composing it were at some Distance from each other, and there was no Action between them, then the Resistance of a Body moving therein would be easily computed, from the Quantity of Motion communicated to these Particles: For instance, if a Cylinder moved in such a Fluid in the Direction of its Axis, it would communicate to the Particles it met with a Velocity equal to its own, and in its own Direction, supposing that neither the Cylinder, nor the Parts of the Fluid, were elastic; whence, if the Velocity and Diameter of the Cylinder be known, and also the Density of the Fluid, there would thence be determined the Quantity of Motion communicated to the Fluid, which (Action and Re-action being equal) is the same with the Quantity lost by the Cylinder, consequently the Resistance would be hereby ascertained.

In this kind of discontinued Fluid, the Particles being detached from each other, every one of them can pursue its own Motion in any Direction, at least for some time, independent of the neighbouring ones; wherefore, if, instead of a Cylinder moving in the Direction of its Axis, a Body, with a Surface oblique to its Direction, be supposed to move in such a Fluid, the Motion the Parts of the Fluid will hereby acquire, will not be in the Direction of the resisted Body, but perpendicular to its oblique Surface; whence the Resistance to such a Body will not be estimated from the whole Motion communicated to the Particles of the Fluid, but from that Part of it only, which is in the Direction of the resisted Body. In Fluids then, where the Parts are thus discontinued from each other, the different Obliquities of that Surface, which goes foremost, will occasion considerable Changes in the Resistance, altho' the Section of the solid by a plain Perpendicular to its Direction should in all Cases be the same. And Sir

Isaac

Isaac Newton has particularly determined, that in a Fluid thus constituted, the Resistance of a Globe is but half the Resistance of a Cylinder of the same Diameter, moving in the Direction of its Axis with the same Velocity.

But tho' the Hypothesis of a Fluid, thus constituted, be of great Use in explaining the Nature of Resistances; yet, in reality, no such Fluid does exist within our Knowledge: All the Fluids with which we are conversant are so formed, that their Particles either lie contiguous to each other, or at least act on each other in the same manner, as if they did; consequently, in these Fluids, no one Particle, contiguous to the resisted Body, can be moved, without moving at the same time a great Number of others, some of which will be distant from it; and the Motion thus communicated to a Mass of the Fluid will not be in any one determined Direction, but will in each Particle be different, according to the different Manner in which it lies in Contact with those, from whence it receives its Impulse; whence, great Numbers of the Particles being diverted into oblique Directions, the Resistance of the moving Body, which will depend on the Quantity of Motion communicated to the Fluid in its own Direction, will be hereby different in Quantity, from what it would be in the preceding Supposition, and its Estimation becomes much more complicated and operose.

If the Fluid be compressed by the incumbent Weight of its upper Parts (as all Fluids are with us, except at their very Surface) and if the Velocity of the moving Body be much less than that with which the Parts of the Fluid would rush into a void Space, in consequence of their Compression, it is evident, that in this Case the Space left by the moving Body will be instantaneously filled up by the Fluid, and the Parts of the Fluid against which

the foremost Part of the Body presses in its Motion, will, instead of being impelled forwards in the Direction of the Body, circulate in some measure towards the hinder Part of the Body, thereby to restore the Equilibrium, which the constant Influx of the Fluid behind the Body would otherwise destroy ; whence the progressive Motion of the Fluid, and consequently the Resistance of the Body, which depends thereon, would be in this Instance much less than in our first Hypothesis, where each Particle was supposed to acquire, from the Stroke of the resisting Body, a Velocity equal to that, with which the Body moved, and in the same Direction. Sir *Isaac Newton* has determined, that the Resistance to a Cylinder moving in the Direction of its Axis, in such a compressed Fluid as we have here treated of, is but one fourth Part of the Resistance, which the same Cylinder would undergo, if it moved with the same Velocity in a Fluid constituted in the Manner, we have described in our first Hypothesis, each Fluid being supposed to be of the same Density.

But again, it is not only in the Quantity of their Resistance that these Fluids differ, but likewise in the different Manner, in which they act on Solids of different Forms moving in them.

We have shewn, that in the discontinued Fluid, which we first described, the Obliquity of the foremost Surface of the moving Body would diminish the Resistance ; but in compressed Fluids this holds not true, at least not in any considerable Degree ; for the principal Resistance in compressed Fluids arises from the greater or lesser Facility, with which the Fluid, impelled by the Fore-part of the Body, can circulate towards its hindermost Part ; and this being little, if at all, affected by the Form of the moving Body, whether it be cylindrical, conical, or spherical, it follows, that while the transverse Sec-
tion

tion of the Body, and consequently the Quantity of impelled Fluid be the same, the Change of its Figure will scarcely affect the Quantity of its Resistance.

And this Case, that is, the Resistance of a compressed Fluid to a solid, moving in it with a Velocity much less than what the Parts of the Fluid would acquire from their Compression ; this Case, I say, has been very fully considered by Sir *Isaac Newton*, who has ascertained the Quantity of such a Resistance according to the different Magnitudes of the moving Body, and the Density of the Fluid. But he very expressly informs us, that the Rules he has laid down are not generally true, but upon a Supposition that the Compression of the Fluid be increased in the greater Velocities of the moving Body : However, some unskilful Writers who have followed him, overlooking this Caution, have applied his Determinations to Bodies moving with all kinds of Velocities, without attending to the different Compressions of the Fluids they were resisted by ; and by this Means they have accounted the Resistance of the Air to Musket and Cannon-shot to be but one third Part of what I have found it by Experience.

Indeed, from all we have said, it appears plain enough, that the resisting Power of the Medium must be increased, when the resisting Body moves so fast, that the Fluid cannot instantaneously press in behind it, and fill the deserted Space ; for when this happens, the Body will be deprived of the Pressure of the Fluid behind it, which in some measure ballanced its Resistance, and must support on its Fore-part the whole Weight of a Column of the Fluid, independant of the Motion it gives to the Parts of the Fluid ; and besides, the Motion in the Particles driven before the Body is, in this Case, less affected by the Compression of the Fluid, and consequently they are less deflected from the Di-

rection in which they are impelled by the resisted Surface; whence this Species of Resistance approaches more and more to that described in our first Hypothesis, where each Particle of the Fluid being unconnected with the neighbouring ones, pursued its own Motion, in its own Direction, without being interrupted or deflected by their Contiguity; and therefore, as we before observed, that the Resistance of a discontinued Fluid to a Cylinder, moving in the Direction of its Axis, was four times greater than the Resistance of a Fluid sufficiently compressed of the same Density, it follows, that the Resistance of a Fluid, when a Vacuity is left behind the moving Body, may be near four times greater than that of the same Fluid, when no such Vacuity is formed; for when a void Space is thus left, we have shewn the Resistance to approach in its Nature to that of a discontinued Fluid.

This then may probably be the Case in a Cylinder moving in the same compressed Fluid, according to the different Degrees of its Velocity; so that if it set out with a great Velocity, and moves in the Fluid till that Velocity be much diminished, the resisting Power of the Medium may be near four times greater in the Beginning of its Motion than in the End. In a Globe the Difference will not be so great, because, on account of its oblique Surface, its Resistance in a discontinued Medium is but about twice as much as in one properly compressed; for its oblique Surface diminishes its Resistance in one Case and not in the other: however, as the Compression of the Medium, even when a Vacuity is left behind the moving Body, may yet confine the oblique Motion of the Parts of the Fluid, which are driven before the Body, and as in an elastic Fluid (as is our Air) there will be some Degree of Condensation in those Parts, it is highly probable, that the Resistance of a Globe, moving in a compressed

pressed Fluid with a very great Velocity, will be between that of a Globe and of a Cylinder, in a discontinued Medium ; that is, (in Proportion to its Velocity) we may suppose it to be more than twice, and less than four times the Resistance of the same Globe, moving slowly through the same Medium ; whence; perhaps, we shall not much err in supposing the Globe in its swiftest Motions to be resisted near three times more, in Proportion to its Velocity, than when it is slowest.

And as this Increase of the resisting Power of the Medium will take Place, when the Velocity of the moving Body is so great, that a perfect Vacuity is left behind it, so some Degree of Augmentation will be sensible in Velocities much short of this ; for even when, by the Compression of the Fluid, the Space left behind the Body is instantaneously filled up, yet if the Velocity, with which the Parts of the Fluid rush in behind, is not much greater than that, with which the Body moves, the same Reasons we have urged above, in the Case of an absolute Vacuity, will hold in a less Degree in this Instance ; and therefore we are not to suppose, that the increased Resistance, which we have hitherto treated of, immediately vanishes, when the Compression of the Fluid is just sufficient to prevent a *Vacuum* behind the resisted Body ; but we must consider it as diminishing only, according as the Velocity with which the Parts of the Fluid follow the Body, exceeds that with which the Body moves.

Hence then we conclude, that if a Globe sets out in a resisting Medium, with a Velocity much exceeding that with which the Particles of the Medium would rush into a void Space, in consequence of their Compression, so that a *Vacuum* is necessarily left behind the Globe in its Motion, the Resistance of this Medium to the Globe will be near three times greater, in Proportion to its Velocity, than what we are sure, from Sir *Isaac Newton*,
would

would take Place in a slower Motion. We may farther conclude, that the resisting Power of the Medium will gradually diminish, as the Velocity of the Globe decreases, till at last, when it moves with Velocities, which bear but a small Proportion to that, with which the Particles of the Medium follow it, the Resistance becomes the same with what is assigned by Sir *Isaac Newton* in the Case of a compressed Fluid.

And from this Determination we may learn, how false that Position is, which asserts the Resistance of any Medium to be in the duplicate Proportion of the Velocity of the resisted Body; for it plainly appears, by what we have said, that this can only be considered as nearly true in small Variations of Velocity, and can never be applied in the comparing together the Resistances to all Velocities whatever without the most enormous Errors.

These Principles being laid down, we shall next proceed to an experimental Examination of the Resistance of the Air in particular, both in order thence to evince how nearly these Speculations agree to the real observed Action of Fluids, and likewise to shew, how egregiously all those Theorists have been mistaken, who have conceived, that the Resistance of the Air to Shells and Shot of all kinds was scarcely worthy of Attention.

P R O P. II.

To determine the Resistance of the Air to Projectiles by Experiments.

BY Means of the Machine described in the 8th Proposition, I have it in my Power to determine the Velocity, with which a Ball moves in any Part of its Track, provided I can direct the Piece so as to cause the Bullet to impinge on the Pendulum.

lum placed in that Part ; and therefore charging a Musket-barrel three times successively with a leaden Ball of $\frac{3}{4}$ of an Inch Diameter, and about half its Weight of Powder, and taking such Precaution in the weighing of the Powder, and placing it, that I was assured, by many previous Trials, that the Velocity of the Ball could not differ by 20 Feet in 1" from its Medium Quantity, I fired it against the Pendulum placed at 25 Feet, at 75 Feet, and at 125 Feet Distance from the Mouth of the Piece respectively ; and I found that it impinged against the Pendulum in the first Case with a Velocity of 1670 Feet in 1", in the second Case with a Velocity of 1550 Feet in 1", and in the third Case with a Velocity of 1425 Feet in 1" ; so that in passing thro' 50 Feet of Air, the Bullet lost a Velocity of about 120 or 125 Feet in 1" ; and the Time of its passing thro' that Space being about $\frac{1}{12}$ or $\frac{1}{10}$ of 1", the Medium Quantity of Resistance must, in these Instances, have been about 120 times the Weight of the Ball, which (as the Ball was nearly $\frac{1}{12}$ of a Pound) amounts to about 10 lb. Avoirdupoise.

Now if a Computation be made according to the Method laid down for compressed Fluids in the 38th Proposition, Lib. 2. of Sir *Isaac Newton's Principia*, supposing the Weight of Water to be to the Weight of Air, as 850 to 1, it will be found, that the Resistance to a Globe of $\frac{3}{4}$ of an Inch Diameter, moving with a Velocity of about 1600 Feet in 1", will not, on those Principles, amount to any more than a Force of $4\frac{1}{2}$ lb. Avoirdupoise ; whence, as we know, that the Rules contained in that Proposition are very accurate in slow Motions, we may hence conclude, that the resisting Power of the Air in slow Motions is less than in swift Motions in the Ratio of $4\frac{1}{2}$ to 10, a Proportion between that of 2 to 2 and 1 to 3.

Again,

Again, I charged the same Piece, a Number of times, with equal Quantities of Powder, and Balls of the same Weight, taking all possible Care to give to every Shot an equal Velocity; and firing three times against the Pendulum, placed 25 Feet only distant from the Mouth of the Piece, the Medium of the Velocities with which the Ball impinged, was nearly that of 1690 Feet in 1": Then removing the Piece 175 Feet from the Pendulum, I found, taking the Medium of five Shots, that the Velocity, with which the Ball impinged at this Distance, was that of 1300 Feet in 1"; whence the Ball, in passing through 150 Feet of Air, lost a Velocity of about 390 Feet in 1"; and the Resistance computed from these Numbers comes out something more than in the preceding Instance, it amounting here to between 11 and 12 Pounds, Avoirdupoise; whence, according to these Experiments, the resisting Power of the Air to swift Motions is greater than in slow ones in a Ratio which approaches nearer to the Ratio of 3 to 1, than in the preceding Experiments.

Having thus ascertained the Resistance to a Velocity of near 1700 Feet in 1", which must be allowed to be more than sufficient for leaving a *Vacuum* behind the Ball, I next examined the Resistance to smaller Velocities; and for this Purpose I charged the same Barrel with Balls of the same Diameter, but with less Powder; and placing the Pendulum at 25 Feet Distance from the Piece, I fired against it five times with an equal Charge each time; the Medium Velocity, with which the Ball impinged, was that of 1180 Feet in 1"; then removing the Pendulum to the Distance of 250 Feet, the Medium Velocity of five Shots made at this Distance was that of 950 Feet in 1"; whence the Ball, in passing through 225 Feet of Air, lost a Velocity of 230 Feet in 1"; and as it passed through

through that Interval in about $\frac{1}{3}$ of 1", the Resistance to the Middle Velocity will come out to be near 33 $\frac{1}{2}$ times the Gravity of the Ball, or 2 lbs. 10 oz. Avoirdupoise. Now the Resistance to the same Velocity, according to the Laws observed in slower Motions, amounts to $\frac{7}{11}$ of the same Quantity; whence, in a Velocity of 1065 Feet in 1", the resisting Power of the Air is augmented in no greater a Proportion than that of 7 to 11; whereas we have seen in the former Experiments, that to still greater Degrees of Velocity, the Augmentation approached very near to the Ratio of 1 to 3.

But farther, I fired three Shot, of the same Size and Weight with those already mentioned, over a large Piece of Water; so that their dropping into the Water being very discernable, both the Distance and Time of their Flight might be accurately ascertained; each Shot was discharged with a Velocity of 400 Feet in 1"; and I had satisfied myself, by many previous Trials of the same Charge with the Pendulum, that I could rely on this Velocity to 10 Feet in 1". The first Shot flew 313 Yards in 4" $\frac{1}{2}$, the second flew 319 Yards in 4", and the third 373 Yards in 5" $\frac{1}{2}$. According to the Theory of Resistance established for slow Motions, the first Shot ought to have spent no more than 3" $\frac{1}{2}$, 2, in its Flight, the second 3" $\frac{1}{2}$, 28, and the third 4"; whence it is evident, that every Shot was retarded considerably more than it ought to have been, had that Theory taken Place in its Motion; consequently, the resisting Power of the Air is very sensibly increased, even in so small a Velocity as that of 400 Feet in 1".

From all that we have related then, it appears, that the Theory of the Resistance of the Air, established in slow Motions by Sir *Isaac Newton*, and confirmed by many Experiments, is altogether erroneous, when applied to the swifter Motions of
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Musket or Cannon-shot ; for that, in these Cases, the resisting Power of the Medium is augmented to near three times the Quantity assigned by that Theory ; that, however, this increased Power of Resistance diminishes as the Velocity of the resisted Body diminishes, till at length, when the Motion is sufficiently abated, the actual Resistance coincides with that supposed in the Theory ; that therefore the Resistance is not in the duplicate Proportion of the Velocity of the moving Body, as is usually asserted, but varies from that Proportion according to the different Compression of the Fluid compared with the Velocity ; consequently, from the Consideration of these Particulars, we may venture to assert, that whilst the Resistance of the Air was thus imperfectly and faultily conceived, the Track of Musket and Cannon-shot through that Medium could not be ascertained with the least Degree of Certainty ; and therefore the Art of Gunnery could not but continue extremely imperfect : However, it is not sufficient to have shewn the Resistance to be augmented in great Velocities, beyond what has been usually supposed ; for, that we may be enabled more definitely to compute the Motion of Projectiles, it is necessary that we should assign the Rate of this Augmentation according to the different Velocities of the resisted Body. This shall be the Subject of our next Proposition.

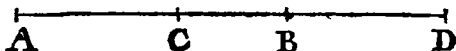
P R O P. III.

To assign the different Augmentations of the resisting Power of the Air according to the different Velocities of the resisted Body.

AS no large Shot are ever projected in Practice with Velocities exceeding that of 1700 Feet in 1', I have not as yet made any Experiments on the

the Resistance of Bodies which have moved with a swifter Motion than this, esteeming the Determination of the Variation of the Resistance to all lesser Velocities, to be sufficient for the Purposes of this Treatise.

According to the Trials I have made, the resisting Power of the Air to Velocities less than that of 1700 Feet in 1", may be thus nearly exhibited.



Let AB be taken to AC in the Ratio of the Velocity of 1700 Feet in 1", to the given Velocity to which the resisting Power of the Air is required; continue the Line AB to D, so that BD may be to AD, as the resisting Power of the Air to slow Motions is to its resisting Power to a Velocity of 1700 Feet in 1", then shall CD be to AD, as the resisting Power of the Air to slow Motions is to its resisting Power to the given Velocity represented by AC.

P R O P. IV.

To determine the Velocities with which Musket and Cannon-shot are discharged from their respective Pieces by their usual Allotment of Powder.

FROM the Computations of the 7th Proposition of the 1st Chapter, confirmed by the succeeding Experiments, it plainly appears, that a leaden Ball of $\frac{3}{4}$ of an Inch in Diameter, and weighing nearly 1 $\frac{1}{2}$ oz. Avoirdupoise, if it be fired from a Barrel of 45 Inches in Length, with half its Weight of Powder, will issue from that Piece with

with a Velocity which, if it were uniformly continued, would carry it near 1700 Feet in 1".

If instead of a leaden Ball an Iron one of the same Diameter was placed in the same Situation in the same Piece, and was impelled by the same Quantity of Powder, the Velocity of such an Iron Bullet would be greater than that of the leaden one, in the subduplicate Ratio of the specific Gravities of Lead and Iron; and supposing that Ratio to be as 3 to 2, and computing on the Principles laid down in the last-cited Proposition, it will appear that an Iron Bullet of 24 lb. Weight, shot from a Piece of 10 Feet in Length, with 16 lb. of Powder, will acquire from the Explosion a Velocity which, if uniformly continued, would carry it nearly 1650 Feet in 1".

This is the Velocity which, according to our Theory, a Cannon-ball of 24 lb. Weight is discharged with, when it is impelled by a full Charge of Powder; but if, instead of a Quantity of Powder weighing two-thirds of the Ball, we suppose the Charge to be only half the Weight of the Ball, then its Velocity will, on the same Principles, be no more than at the Rate of 1490 Feet in 1"; and the same would be the Velocities of every lesser Bullet, fired with the same Proportions of Powder, if the Lengths of all Pieces were constantly in the same Ratio with the Diameters of their Bore: And altho', according to the usual Dimensions of the smaller Pieces of Artillery, this Proportion does not always hold, yet the Difference is not considerable enough to occasion a very great Variation from the Velocities here assigned; as will be obvious to any one, who shall make a Computation thereon.

But in these Determinations, we suppose the Windage to be no more, than is just necessary for the easy putting down the Bullet; whereas, in real Service, either through Negligence or Unskiffulness,
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it often happens, that the Diameter of the Bore so much exceeds the Diameter of the Bullet, that great Part of the inflamed Fluid escapes by its Side; whence the Velocity of the Shot, in this Case, may be considerably less than what we have assigned. However, Part of this may possibly be compensated by the greater Heat, which (as we have observed in the 6th Proposition) in all Probability attends the firing of these large Quantities of Powder.

C O R O L L A R Y.

From the great Velocity of Cannon-shot assigned in this Proposition we may clear up that Difficulty which has driven some Writers, on the common Theory of Gunnery, into a very extraordinary Hypothesis. The Difficulty, I mean, is the Extent of the supposed point-blank Shot, or the Distance to which it is conceived to fly in a straight Line. Our *Anderson* having found, by many Experiments, that the Track of Shells and Bullets, in the first Part of their Motion, was much less incurvated, than what it ought to be on the Principles of *Galileo*, when compared with the distant Ranges, he supposed, in order to reconcile this Circumstance with his Theory, that every Shot was impelled to a certain Distance from the Mouth of the Piece, in a straight Line, or that for some Distance it was not affected by the Action of Gravity. By this Means he defended, as he thought, the Hypothesis of a parabolic Motion, and at the same time assented to the vulgar Opinion of the practical Writers, who, in general, asserted the same thing. But could no better Account be given of his Experiments, it would yet be unnecessary, I presume, formally to confute so strange a Supposition as that of the Suspension of the Action of Gravity. Indeed, *Anderson* was deceived, by his not knowing how

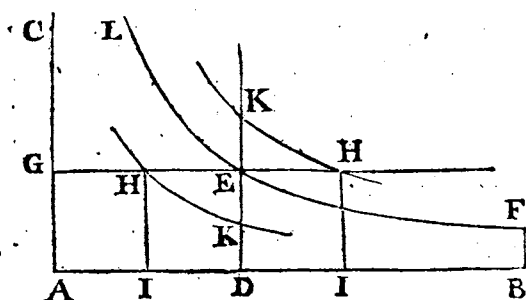
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greatly

greatly the primitive Velocity of the heaviest Shot is diminished in the Course of its Flight by the Resistance of the Air. And the received Opinion of practical Gunners is not more difficult to account for, since, when they agree, that every Shot flies in a straight Line to a certain Distance from the Piece, which imaginary Distance they have denominated the Extent of the point-blank Shot, we need only suppose, that within that Distance, which they thus determine, the Deviation of the Path of the Shot from a straight Line is not very perceptible in their Method of pointing. Now, as a Shot of 24 lb. fired with two-thirds of its Weight in Powder, will, at the Distance of 500 Yards from the Piece, be separated from the Line of its original Direction, by an Angle of little more than half a Degree; those, who are acquainted with the inaccurate Methods often used in the directing of Cannon, will easily allow, that so small an Aberration as this may, by the generality of Practitioners, be unattended to, and the Path of the Shot may consequently be deemed a straight Line, especially as other Causes of Error will often intervene, much greater than what arises from the Incurvation of this Line by Gravity.

In the present Proposition, the Velocity of a Shot is determined, both when fired with two-thirds of its Weight of Powder, and with half its Weight of Powder, respectively; and, on this Occasion, I must remark, that on the Principles of the Theory, which we have ascertained in this Treatise, the increasing the Charge of Powder will increase the Velocity of the Shot, till the Powder arrives at a certain Quantity; after which, if the Powder be increased, the Velocity of the Shot will diminish. The Quantity producing the greatest Velocity, and the Proportion between that greatest Velocity and the

the Velocity communicated by greater and lesser Charges, may be thus assigned:



Let AB represent the Axis of the Piece; draw AC perpendicular to it, and to the Asymptotes AC and AB , describe any Hyperbola LF , and draw BF Parallel to AC ; find out now the Point D , where the Rectangle $ADEG$ is equal to the Hyperbolic Area $DEFB$, then will AD represent that Height of the Charge, which communicates the greatest Velocity to the Shot; whence AD being to AB , as 1 to 2,71828, as appears by the Table of Logarithms, from the Length of the Line AD thus determined, and the Diameter of the Bore, the Quantity of Powder contained in this Charge is easily known.

If, instead of this Charge, any other filling the Cylinder to the Height AI be used, draw IH Parallel to AC , and through the Point H , to the same Asymptotes AC and AB , describe the Hyperbola HK ; then the greatest Velocity will be to the Velocity communicated by this Charge AI , in the subduplicate Proportion of the Rectangle AE , to the same Rectangle diminished by the trilinear Space HKE . All this easily follows from the Principles laid down in the 7th Proposition of the 1st Chapter.

P R O P. V.

When a Cannon-Ball of 24 lb. Weight. fired with a full Charge of Powder, first issues from the Piece, the Resistance of the Air on its Surface amounts to more than twenty times its Gravity.

FOR we have shewn, in the second Proposition of the present Chapter, that the Resistance of the Air on the Surface of a Bullet of $\frac{1}{4}$ of an Inch Diameter, moving with a Velocity of 1670 Feet in 1", amounted to 10 lb. Now we have seen, in the last Proposition, that an Iron Bullet weighing 24 lb. if fired with 16 lb. of Powder, (which is usually esteemed its proper battering Charge) acquires a Velocity of about 1650 Feet in 1", scarcely differing from the other; whence, as the Surface of this last Bullet is more than fifty-four times greater than the Surface of a Bullet of $\frac{1}{4}$ of an Inch in Diameter, and their Velocities are nearly the same, it follows, that the Resistance on the larger Bullet will amount to more than 540 lb. which is near twenty-three times its own Weight.

S C H O L I U M.

We have observed, in the Introduction, that the Theorists who have professedly written on the Subject of Gunnery, have generally agreed in supposing the Flight of Shot and Shells to be nearly in the Curve of a Parabola; and it is against this Hypothesis that the two last Propositions are particularly aimed.

For the Reason, which has been given by these Authors, in Support of their Opinion, is the supposed

posed inconsiderable Resistance of the Air; since, as it is agreed on all Sides, that the Track of Projectiles would be a Parabola, if there was no Resistance, it has from hence been too rashly concluded, that the Interruption, which the ponderous Bodies of Shells and Bullets would receive from so rare a Medium as the Air, would be scarcely sensible, and consequently that their parabolic Flight would be hereby scarcely affected.

Now the prodigious Resistance of the Air to a Bullet of 24 lb. Weight, such as we have here established it, sufficiently confutes this Reasoning; for how erroneous must that Hypothesis be, which neglects as inconsiderable a Force, which amounts to more than twenty times the Gravity of the moving Body? However, we shall not content ourselves with having demonstrated the Reality and Quantity of the Air's Resistance, but we shall proceed to a more particular Examination of the Flight of Bodies in that Medium, where we shall evince, by many Experiments, how greatly the Track, described by almost every Projectile, deviates in every Circumstance from what it ought to be on the generally-received Principles. But, first, it is necessary to assume a few Particulars, the Demonstrations of which may be found in almost every Writer on the common Theory of falling Bodies.

Post. 1. If the Resistance of the Air be so small, that the Motion of a projected Body be in the Curve of a Parabola, then the Axis of that Parabola will be perpendicular to the Horizon, and consequently the Part of the Curve, in which the Body ascends, will be equal and similar to that in which it descends.

Post. 2. If the Parabola, in which the Body moves, be terminated on a horizontal Plain, then the Vertex of the Parabola will be equally distant from its two Extremities.

Post. 3. Also the moving Body will fall on that horizontal Plain in the same Angle and the same Velocity with which it was first projected.

Post. 4. If a Body be projected in different Angles, but with the same Velocity, then its greatest horizontal Range will be, when it is projected in an Angle of 45° with the Horizon.

Post. 5. If the Velocity, with which the Body is projected, be known, then this greatest horizontal Range may be thus found: Compute, according to the common Theory of Gravity, what Space the projected Body ought to fall through to acquire the Velocity with which it is projected, then twice that Space will be the greatest horizontal Range, or the horizontal Range, when the Body is projected in an Angle of 45° with the Horizon.

Post. 6. The horizontal Ranges of a Body, when projected with the same Velocity, at different Angles, will be between themselves, as the Sines of twice the Angle, in which the Line of Projection is inclined to the Horizon.

Post. 7. If a Body be projected in the same Angle with the Horizon, but with different Velocities, the horizontal Ranges will be in the duplicate Proportion of those Velocities.

These Postulates contain the Principles on which the Motions of Projectiles are computed by the modern Writers on the Art of Gunnery.

If any of these Postulates hold not true, when applied to the Motion of a Projectile, then that Projectile deviates in its Flight from a parabolic Track; we shall therefore effectually destroy the common Theory of Projectiles, if we can shew, that in general none of these Postulates correspond to the observed Motions of those Bodies.

PROP.

P R O P. VI.

The Track described by the Flight of Shot or Shells is neither a Parabola, nor nearly a Parabola, unless they are projected with small Velocities.

FOR we have determined, in the fourth Proposition of the present Chapter, that a Musket-ball $\frac{1}{4}$ of an Inch in Diameter, fired with half its Weight of Powder from a Piece 45 Inches long, moves with a Velocity of near 1700 Feet in 1". Now, if this Ball flew in the Curve of a Parabola, its horizontal Range at 45° would be found, by the fifth Postulate, to be about 17 Miles. Now all the practical Writers assure us, that this Range is really short of half a Mile. *Diego Ufano* assigns to an Arquebuse, 4 Feet in Length, and carrying a leaden Ball of $1\frac{1}{2}$ oz. Weight (which is very near our Dimensions) an horizontal Range of 797 common Paces, when it is elevated between 40 and 50 Degrees, and charged with a Quantity of fine Powder equal to the Weight of the Ball. *Mersennus* too tells us, that he found the horizontal Range of an Arquebuse at 45° to be less than 400 Fathom, or 800 Yards; whence, as either of these Ranges are short of half an *English* Mile, it follows, that a Musket-shot, when fired with a reasonable Charge of Powder, at an Elevation of 45° , flies not the $\frac{1}{14}$ Part of the Distance it ought to do, if it moved in a Parabola.

Nor is this great Contraction of the horizontal Range to be wondered at, when it is considered, that the Resistance of this Bullet, when it first issues from the Piece, amounts to 120 times its Gravity, as has been experimentally demonstrated in the second Proposition of the present Chapter.

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Again, lest it should be said, that this Aberration of the Flight of a Musket-ball from the Curve of a Parabola is no Proof, but that heavier Shot, whose Resistance is much less in Proportion to their Weight, may sufficiently coincide with the common Hypothesis; our next Instance shall be in an Iron Bullet of 24 lb. Weight, which is the heaviest in common Use for Land Service. Such a Bullet, fired from a Piece of the customary Dimensions, with its greatest Allotment of Powder, has a Velocity of 1650 Feet in 1st, as we have determined in the fourth Proposition of the present Chapter. Now if the horizontal Range of this Shot, at 45°, be computed on the parabolic Hypothesis by the fifth Postulate, it will come out to be about 16 Miles, which is between five and six times its real Quantity; for the practical Writers all agree in making it less than three Miles: And St. *Remy* informs us of some Experiments made by Mr. *Du Metz*, in which the Range, at 45°, of a Piece ten Feet in Length, carrying a Ball of 24 lb. and charged with 16 lb. of Powder, was 2250 *French* Fathom, which is 222 Fathom short of three Miles; consequently an Iron Bullet of 24 lb. Weight, when impelled with its full Allotment of Powder, flies not, at 45°, to the fifth Part of the Distance which it ought to do, if it described the Curve of a Parabola.

But farther, it is not only, when Projectiles are moved with these very great Velocities, that their Flight sensibly varies from the Curve of a Parabola; the same Aberration often takes place in such as move slow enough to have their Motion traced out by the Eye; for there are few Projectiles, that can be thus examined, which do not visibly disagree with the first, second, and third Postulate, they obviously descending through a Curve which is shorter and less inclined to the Horizon than that
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in which they ascended; also the highest Point of their Flight, or the Vertex of the Curve, is much nearer to the Place, where they fall on the Ground, than to that from whence they were at first discharged. These Things cannot be a Moment doubted of by one, who in a proper Situation views the Flight of Stones, Arrows or Shells thrown to any considerable Distance.

I have found too by Experience, that the fifth, sixth and seventh Postulates are excessively erroneous, when applied to the Motions of Bullets moving with small Velocities: A leaden Bullet $\frac{1}{4}$ of an Inch in Diameter, discharged with a Velocity of about 400 Feet in 1", and in an Angle of $19^{\circ}:5'$, with the Horizon, ranged on the horizontal Plane, no more than 448 Yards; whereas its greatest horizontal Range being found by the fifth Postulate to be at least 1700 Yards, the Range at $19^{\circ}:5'$ ought, by the sixth Postulate, to have been 1050 Yards; whence, in this Experiment, the Range was not $\frac{1}{3}$ of what it must have been, had the common-received Theory been true.

Again, a Ball was fired with the same Velocity as in the last Experiment, but at an Elevation of $9^{\circ}:45'$, its horizontal Range was at a Medium 330 Yards.

Now this Range, according to the fifth and sixth Postulates, (if its original Velocity be considered) should have been 566 Yards. But if it were to be deduced from the last Experiment, by Means of the sixth Postulate, it should have been no more than 241 Yards; either of which Numbers are extremely distant from 330.

Again, a Ball being fired at an Elevation of 8° , but with a Velocity of 700 Feet in 1", the horizontal Range at a Medium was 690 Yards.

But computing this Range from the original Velocity of the projected Body, according to the fifth and

and sixth Postulates, we shall find, that if the Theory, on which those Postulates are founded, could be relied on, the Range in the present Instance ought to have been 1400 Yards; whence it appears, that the Body flew not to half the Distance which, had it moved in a Parabola, it ought to have done.

Again, a Ball being fired with the same Velocity as in the last, but at an Elevation of 4° , its horizontal Range was 600 Yards.

Now this Range, if deduced from the last Experiment by the sixth Postulate, should not have been more than 350 Yards; hence then is evinced the Falsity of that Postulate, and consequently of the parabolic Hypothesis, on which it is founded.

Having thus proved, that the Track described by the Flight, even of the heaviest Shot, is neither a Parabola, nor approaching to a Parabola, except when they are projected with very small Velocities; we shall refer to a second Part, a more distinct Explication of the Nature of the Curve which these Bodies really trace out in their Motion through the Air: But, as a Specimen of the great Complication of that Subject, I shall here insert an Account of a very extraordinary Circumstance, which frequently takes Place therein.

As Gravity acts perpendicularly to the Horizon, it is evident, that if no other Power but Gravity deflected a projected Body from its rectilinear Course, its Motion would be constantly performed in a Plane perpendicular to the Horizon, passing through the Line of its original Direction: But we have found, that the Body in its Motion often deviates from this Plane, sometimes to the Right-hand, and at other times to the Left; and this in an incurvated Line, which is convex towards that Plane; so that the Motion of a Bullet is frequently in a Line, having a double Curvature, it being bent towards the Horizon by the Force of Gravity,
and

and again bent out of its original Direction to the Right or Left by the Action of some other Force: In this Case no Part of the Motion of the Bullet is performed in the same Plane, but its Track will lie in the Surface of a kind of Cylinder, whose Axis is perpendicular to the Horizon. The Truth of this Assertion we shall evince by indisputable Experiments.

P R O P. VII.

Bullets in their Flight are not only depressed beneath their original Direction by the Action of Gravity, but are also frequently driven to the Right or Left of that Direction by the Action of some other Force.

IF it was true, that Bullets varied their Direction by the Action of Gravity only, then it ought to happen, that the Errors, in their Flight to the Right or Left of the Mark they were aimed at, should increase in the Proportion of the Distance of the Mark from the Piece only: But this is contrary to all Experience; the same Piece, which will carry its Bullet within an Inch of the intended Mark, at 10 Yards Distance, cannot be relied on to 10 Inches in 100 Yards, much less to 30 Inches in 300 Yards. This Increase of the Uncertainty of the Shot in great Distances, more than in the Proportion of those Distances, must have been observed by all who have been at any time conversant with the practical Part of Artillery. Now this Inequality can only arise from the Track of the Bullet being incurvated sideways as well as downwards; for by this Means the Distance between that incurvated Line, and the Line of Direction, will increase in a much greater Ratio than that of the Distance; these Lines being coincident at the Mouth

Mouth of the Piece, and afterwards separating in the Manner of a Curve and its Tangent, if the Mouth of the Piece be considered as the Point of Contact.

But that those, who have not been themselves accustomed to these Matters, may entertain no Doubt about what we here assert, I shall recite some Experiments, I have made, which will put the Matter out of all Question.

I took a Barrel carrying a Ball of $\frac{1}{4}$ of an Inch Diameter, and fixing it on a heavy Carriage, I satisfied myself of the Steadiness and Truth of its Direction, by firing at a Board 1 $\frac{1}{2}$ Foot square, which was placed at 180 Feet Distance; for I found, that in 16 successive Shot I missed the Board but once. Now the same Barrel being fixed on the same Carriage, and fired with a smaller Quantity of Powder, so that the Shock on the Discharge would be much less, and consequently the Direction less changed, I found, that at 760 Yards Distance, the Ball flew sometimes 100 Yards to the Right of the Line it was pointed on, and at other times 100 Yards to the Left. I found too, that its Direction in the perpendicular Line was not less uncertain, it falling one time above 200 Yards short of what it did at another; altho', by the nicest Examination of the Piece after the Discharge, it appeared not to have the least started from the Position it was placed in.

This then sufficiently confirms the Proposition, since it was impossible the Bullet could have flown in the Manner here described, had not the Line of its Flight been bent round to the Right or Left as well as downwards.

S C H O L I U M.

The Reality of this doubly-incurvated Track being thus demonstrated, it may perhaps be asked,
What

What can be the Cause of a Motion so different from what has been hitherto supposed? And to this I answer, that the Deflection in Question must be owing to some Power acting obliquely to the progressive Motion of the Body, which Power can be no other than the Resistance of the Air. If it be farther asked, How the Action of the Resistance of the Air can at any time be in a Line oblique to the progressive Motion of the Body? I farther reply, that it may sometimes arise perhaps from Inequalities in the resisted Surface, but that its general Cause is doubtless a whirling Motion acquired by the Bullet about its Axis; for by this Motion of Rotation, combined with the progressive Motion, each Part of the Bullet's Surface will strike the Air in a Direction very different from what it would do if there was no such Whirl; and the Obliquity of the Action of the Air arising from this Cause will be greater, according as the rotary Motion of the Bullet is greater in Proportion to its progressive Motion.

I have now finished all that I proposed to determine in this Place, relating to the Force of Powder and the Resistance of the Air: But as the Knowledge of the Resistance of Solids to the Penetration of Shot is of great Importance in the practical Part of Gunnery, especially in battering in Breach, I shall end the present Treatise with a Proposition relating thereto, which is as follows.

P R O P.

P R O P. VIII.

If Bullets of the same Diameter and Density impinge on the same solid Substance with different Velocities, they will penetrate that Substance to different Depths, which will be in the duplicate Ratio of those Velocities nearly. And the Resistance of solid Substances to the Penetration of Bullets is uniform.

THE first Part of this Proposition I have found to be true in a great Number of Instances; for when a leaden Bullet, $\frac{1}{2}$ of an Inch in Diameter, was fired against a solid Block of Elm, with a Velocity of about 1700 Feet in 1", I found that in a great Number of Trials it had penetrated from $4\frac{1}{2}$ to $5\frac{1}{2}$ Inches deep. When a Bullet of the same Size was fired against the same Block, with a Velocity of about 730 Feet in 1", its outer Surface was always near $\frac{1}{4}$ of an Inch within the Surface of the Wood, so that its Penetration was at a Medium about 1 Inch; or, if the Cavity be considered, and reduced to a Cylinder, about $\frac{1}{3}$ of an Inch, and with a Velocity of 400 Feet in 1", the Bullet penetrated the same Block usually to about half its Substance, which, reduced to a cylindric Cavity, is $\frac{1}{4}$ of an Inch in Depth.

Now 55, 10, 3, are nearly in the duplicate Proportion of these Velocities; whence, if the Penetration to the greatest Velocity be supposed 5 Inches, the Penetration of the others ought by the Proposition to be $\frac{10}{11}$ and $\frac{3}{11}$ of an Inch respectively; and these Numbers scarcely differ from $\frac{1}{2}$ and $\frac{1}{4}$, which are what we have found in our Experiments; a greater Coincidence than this cannot be expected, when the unequal Texture of the same Piece of Wood, and the

the Change of the Form of the Bullet by the Stroke, are considered.

Now, from the Penetration being in the duplicate Proportion of the Velocity of the impinging Body, the uniform Resistance of the Wood is easily evinced on the same Principles, that the uniform Action of Gravity is demonstrated, from its communicating to falling Bodies Velocities in the duplicate Proportion of the Spaces they descend thro', or from the rising of Bodies when projected upwards, to Heights which are in the duplicate Proportion of the Velocities with which they begin to ascend.

F I N I S.

