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D I S C O U R S E

ON THE

*Theory of Gunnery.*



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*Theory of Gunnery.*

DELIVERED AT THE

Anniversary Meeting of the ROYAL SOCIETY,

November 30, 1778.

By Sir JOHN PRINGLE Baronet.

PUBLISHED BY THEIR ORDER.



LONDON:

PRINTED FOR THE ROYAL SOCIETY.

MDCCLXXVIII.







GENTLEMEN,

**A**MONG the several experiments communicated to the Society, during the course of the preceding year, none seeming so much to engage your attention, as those contained in the Paper, intituled, *The force of fired gun-powder, and the initial velocity of cannon-balls, determined by experiments*: with much pleasure therefore I acquaint you, that, on account of the pre-eminence of that communication, your Council have judged the author, Mr. CHARLES HUTTON, worthy of the honour of the annual medal, instituted on the bequest of Sir GODFREY COPLEY Baronet, for raising a laudable emulation among men of genius, in making experimental inquiries. But, as on former occasions, so now, your Council, waving their privilege of determining the choice, have acted only as a select number deputed by

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you,



you, to prepare matters for your final decision. I come then, on their part, briefly to lay before you the state of the *Theory of Gunnery*, from its rise to the time when its true foundation was laid, in order to evince how conducive those experiments may be to this improvement of an art of public concern, as well as to the advancement of *natural knowledge*, the great object of your institution. And if, upon a review of the subject, you shall entertain no less favourable an opinion of Mr. HUTTON'S performance, than what your Council have done, it is their earnest request that you would enhance the value of this prize, by authorizing your President to present it to our ingenious brother in your name.

ARTILLERY (in the large acceptation of the term) took place long before the invention of gunpowder. We trace the art to the remotest antiquity, since the Sacred Records acquaint us, that one of the kings of Judah, eight hundred years before the Christian æra, erected on the towers and bulwarks of Jerusalem engines of war, the contrivance of ingenious men, for shooting arrows and great stones for the defence of that city<sup>(a)</sup>. Such machines were afterwards

(a) 2 Chron. xxvi. 15.

known to the Greeks and Romans by the names of *balista*, *catapulta* and others, which had amazing powers, and were not less terrible in their effects than the cannon and mortars of the moderns. It appears that the *balista* was contrived to shower volleys of darts and arrows of a very large size upon the enemy, whilst the *catapulta* or *onagra* (as it was otherwise called) was fitted not only for that purpose, but for discharging stones of an enormous weight; I might say *rocks*, since some of them are reported to have weighed several hundred pounds. Batteries composed of numerous pieces of that kind of artillery, nothing could withstand. Yet, if we are rightly informed, their sole principle of motion consisted in the spring of a strongly-twisted cordage, made of animal substances singularly tough and elastic. These warlike instruments continued, not only during the time of the Roman empire, but to the 12th and 13th centuries, as we find from history; nor indeed is it probable that they were totally laid aside, till gun-powder and the modern ordnance, attaining a good degree of perfection, superseded their use. The very intelligent commentator of POLYBIUS<sup>(b)</sup> is of opinion, that the military art rather lost than gained by the ex-

(b) M. FOLARD.



change of the *catapulta* for the mortar: but however that point may be determined in speculation, it is not likely that the ancient *tormenta militaria* will ever be revived; but that all nations will keep to the art of gunnery and study how to improve it; that is, they will adhere to a system of artillery, wherein the moving power depends on the expansive force of gun-powder, or of some other substance of a similar nature.

Upon the first application of this principle to the purposes of war, nothing perhaps was less thought of than to assist so empirical a practice by scientific rules; for, however aiding in these matters the ancient mechanics might have been, who, like ARCHIMEDES, had invented or perfected some of the *balistic* machines, no praise seemed now due to the mathematicians for either the discovery or improvement of the new artillery. In fact, we find the practice of the art had subsisted about 200 years, before any geometer considered it as one that admitted a theory, or at least such a theory as was grounded on geometry.

It seems but just to trace and commemorate the inventors of the ingenious arts which furnish matter for  
discourses



discourses on these occasions; and not only the main inventors, but even those who first turned their thoughts upon the subject: for, though such men may not have produced any thing perfect, yet they may have suggested ideas to others of a less inventive, but of a more executive genius, and who, unprovided with those hints, would never have made any notable discovery. I must therefore observe, that the *Italians* were the first who emerged out of those thick clouds of ignorance and barbarism which had so long overspread this quarter of the world. They profited by the unhappy fate of Constantinople; for by liberally receiving the learned emigrants on that distressful occasion, they were largely repaid by their arts and sciences, and still more abundantly by their language, whereby they were enabled to read and to translate those ancient manuscripts, which the Greeks had saved out of the wreck of their country. The art of printing, which was established soon after, was the means of quickly diffeminating those treasures of knowledge, and concurred with the fall of the eastern empire to form an epoch for the advancement of learning, unparalleled in the annals of letters.

The end of the 15th century, and the whole of the 16th, were chiefly employed by the Italians in the study  
and



and in the translation of the old Greek authors. The geometry of the ancient Greeks, as well as the arithmetic in numbers and species of the Arabians, were cultivated; but both remained, as it were, sciences by themselves, unaffisting to, or at best but weak and reluctant auxiliaries to the philosophy of the schools: and indeed how could the abstracted doctrines of numbers and quantities be strained to co-operate with a system, in which neither the laws of motion, nor any but the superficial, and often delusive properties of matter, were to be met with? The genius of the Greeks, all acute and brilliant as it was, had never been properly directed to the interpretation of nature, and was indeed unfit (as Lord BACON pronounced) for a study that made so slow and painful a progress, by re-iterated and varied experiments and observations. It was no wonder then, if the *mixed mathematics*, as they are called, descended to the moderns in a state no-wise corresponding to the elegance and certainty of those parts of the science which were elementary and pure; and that those mixed parts should have been found defective and erroneous, in proportion (if I may so express myself) to the physical considerations that were to be taken into the inquiry. The imperfection of the ancients, with regard to natural philosophy,



philosophy, was not perceived at that time; nay, at the period we are treating of, the learned were firmly persuaded of the contrary, and that all that was wanting to be known concerning the laws of nature, and the properties of matter, was to be taken either directly, or by deduction, from the physics of ARISTOTLE. It was not till the 17th century was somewhat advanced, that men of science began to listen to Lord BACON and GALILEO, the great founders of the experimental and the true philosophy.

Mean while, in the beginning of the 16th century, unqualified as the Italians then were for entering upon physico-mathematical inquiries <sup>(c)</sup>, they nevertheless made the attempt, and in particular took the theory of projectiles into consideration. Some imagined that a body impelled with violence, such as a ball discharged from a cannon, moved in a right line till the force was spent, and that then it fell in another right line perpendicularly to the earth. Upon this principle, absurd as it was, we find one of the earliest authors ground-

(c) The chief exception that occurs to this general remark, is the rapid progress which in that age COPERNICUS made in astronomy; who was not indeed an Italian, but was supposed to have profited by his early travels into Italy, which he enlightened afterwards by his admirable discoveries.



ing his whole theory of gunnery<sup>(d)</sup>; whilst others, dissenting from his hypothesis, admitted only the straight line, in which the ball moved for some time after coming out of the piece, and that other straight line in which it fell to the ground; but asserted that these two were connected by a curve line, and that this curve was the segment of a circle. NICOLAS TARTAGLIA of Brescia, a mathematician of the first rank in those days, and still celebrated for his improvements in algebra, hath been supposed to be the author of this doctrine, no less erroneous than the former, and for which two of his books have been quoted<sup>(e)</sup>. Those I have never seen; but from another of his works, professedly written on this subject, and translated into English under the title of *Colloquies concerning the art of shooting in great and small pieces of artillery*<sup>(f)</sup>, him I find, contrary to the opinion of his contemporaries, maintaining that no part of the track of a cannon-ball is in a right line, though the curvature in the first part of its flight be so small, that it needeth not to be attended to. But TARTAGLIA is far from supposing, that the line in question hath any relation to a *parabola*, or to any regular curve. It

(d) See MONTUCLA, Hist. des Mathem. vol. I. p. 623.

(e) Those were *La Nuova Scientia*, and *Questi ed Inventioni diverse*.

(f) Published at London, A. 1588.



would seem then, that if this mathematician had at first been so far mistaken, as to fancy that some part of the course of a projectile was in a straight line, he had afterwards changed his opinion, and was perhaps singular in what he finally embraced.

From numerous instances one would imagine, that in those days, so far were men of science from making experiments themselves, that they even shut their eyes against what chance would have presented to their sight. For, whoever had minded the roving shot of an arrow, the flight of a stone from a sling, or had attended to a stream of water issuing from the spout of a cistern, might have been convinced, that the path of every projectile was in a continued curve, whatever little he otherwise knew concerning the properties of that one.

But had the observation of the philosophers gone so far, they had still been at a distance from the truth. They might have perceived a likeness between the track of those bodies in motion and a parabola, and concluded, from analogy, that all projectiles delineated that curve in the air; but they could never have realized their conjectures by mathematical demonstration, with-



out previously knowing the law of *acceleration* in falling bodies: a discovery reserved for the next century, and for GALILEO<sup>(g)</sup>, one of the greatest ornaments of it.

It was he who first investigated the effects of *gravity* on falling bodies, and upon that foundation demonstrated, that all projectiles would move in a parabola in a non-resisting medium. And as he made little account of the resistance of the air, whose properties were then imperfectly known, he proved that a ball shot horizontally would, in its flight, describe half a parabola; and when the piece had an elevation above the horizon, the ball would describe a whole parabola, supposing it to fall on the plane of the battery. By the same method of reasoning he shewed, that whatever the ranges of the projected body, or the elevations of the piece were, the ball would still trace that curve line, of a greater or lesser amplitude, by the time it descended to the level of the place from whence it came.

Thus far went GALILEO, confining his projections to the horizontal plane of the battery; but TORRICELLI

(g) He was born in the year 1564; but few if any of his works were published till after the year 1600, and his dialogues on motion not before 1638.



his disciple soon after carried the theory farther, by tracing the shot to its fall, whether that place was above or below the plane; and still found, by geometrical deductions, that it flew in a parabola of a larger or a smaller amplitude, according to the angle of elevation of the piece, and the strength of the powder.

Various and numerous had been the disputes in Italy about the laws of motion in general, and especially about those of projectiles, from the time the mathematicians had begun the inquiry, till the publication of the dialogues of GALILEO on that subject (a space of upwards of a hundred years) but from that period, so evident did his demonstrations appear, that all contest ceased, and every man of science was convinced, that all projectiles moved in the track which he had discovered. For, as to the resistance of the air, which he had not passed unnoticed (as GALILEO himself had been the first, at least of the moderns, who started the notion of the weight of the air and the pressure of the atmosphere) yet so thin and so yielding did they esteem that fluid to be, that they were assured it could occasion no sensible, at least no material, deviation from that curve. As they had the principle from GALILEO, so they believed themselves



selfes warranted by that respectable author, not to fear from that cause any objection, which he himself had suggested, but had removed. *Among these projectiles* (says he) *which we make use of, if they are of a heavy matter and a round form; nay if they are of a lighter matter, and have a cylindrical form, such as arrows shot from bows, their track or path will not sensibly decline from the curve of a parabola<sup>(b)</sup>.*

Here then was the theory of gunnery laid, in appearance, on the most solid foundation. And thus far the Italians having proceeded, they seemed to have taken leave, and to commit the subject to other nations, whose greater power, or greater ambition, was more likely to make them avail themselves of the perfection of a military art, than their instructors. We had reason therefore to expect, that a neighbouring state, intent upon the advancement of the arts and sciences in general, would not fail to give particular attention to those that should appear most subservient to its grandeur. Accordingly we find, that our sister-society of that kingdom had not been many years established, when an ingenious member of that illustrious Body, not

(b) See his 4th Dialogue on Motion.



questioning the soundness of the Galilean principle in regard to projectiles, in the year 1677, proposed to the academy, as a problem for the improvement of artillery, how to direct a piece (suppose a mortar) so as to make the shot fall where one had a mind; or in the common expression, *to hit a mark*, the strength of the powder being given<sup>(i)</sup>. This thought met with general approbation, and so far were the academy from raising any difficulty about the obstruction which the air might occasion to a body moving with so much velocity in it, that we do not find the making experiments on that head was considered by them as an essential step to the solution; but that their principal geometers straightway set about solving the problem as it had been announced to them, some following one method, some another, and all upon the supposition of a projectile moving in the line of a parabola. But M. BLONDEL, who had been the proposer, and who more particularly had studied the question, composed a large volume on the subject, which he published a few years after<sup>(k)</sup>, under the title of *L'Art de jeter les Bombes*; a performance much celebrated at the time, and that continued in no small

(i) See Hist. de l'Academ. Roy. des Sciences, A. 1707.

(k) In the year 1683. See Hist. de l'Acad. R. des Sci. A. 1707.



request long after, as containing, besides his own, the labours of several other members of that society of the most distinguished merit. So many, and such hands concurring in framing this work, it was no wonder that the learned throughout Europe were confirmed by it in the Galilean theory; and the more as M. BLONDEL had obviated the only objection they supposed could be made to it, the *resistance of the air*, which he had taken care expressly to mention, and so to combat as to persuade the reader, that the retardation arising from that cause was so inconsiderable as to be of no account in the practice.

This illusion about the small or non-resistance of the air to bodies rapidly moving in it, was so prevalent at the end of the last century, and in the beginning of the present, that in the history of the Royal Academy for the year 1707, we find their worthy and most accomplished secretary, after taking notice of the joint labours of so many able mathematicians concerned in BLONDEL's publication, venturing to say, *it did not appear that any thing was then wanting for the practice of the art [of Gunnery] except perhaps perfecting the instruments for pointing a cannon or mortar . . . . . but that geometry had*



*had done its part, so to speak, with regard to practice.*  
 &c. <sup>(1)</sup>

But far be it from our intention to relate the imperfections of others, in order to raise ourselves by the comparison. Candour requires of us not only to acknowledge, that in this country, as to the point in question, we did not surpass our neighbours; but ingenuously to own that, on the contrary, we were perhaps more liable to exception. For, some years before BLONDEL'S work appeared <sup>(m)</sup>, a treatise was published by one of our own artificers, ANDERSON (a person of eminence in his profession) intituled *The genuine use and effects of the gun*, in which the author strenuously supports the Galilean theory; nor do we learn he was ever contradicted among us, although he undertook to answer all those who should make objections to it. Nay, when he had an opportunity afterwards of making experiments on the ranges of bombs, and by those trials was assured that their flight was not in a parabola; yet so far was he from ascribing the deviation from that figure to the resistance of the air, that he had recourse to an

(1) Hist. de l'Acad. R. des Sc. A. 1707, under the article *Mechanique*.

(m) Viz. in 1674.



hypothesis, repugnant to all the laws of motion, to false appearances, and to reconcile those experiments with his former doctrine<sup>(n)</sup>.

And did not Dr. HALLEY, so long the ornament of this Society, communicate in the year 1686 a Paper, which he calls *A discourse concerning gravity*, in which, treating of the motion of projectiles, he says, that being aware of the deflexion from the parabolic curve that might be occasioned by the resistance of the air, he had made some experiments, even with cannon-balls, to estimate the force of that resistance; yet conclude, *That in large shot of metal, whose weight many thousand times surpassed that of air, and whose force is very great, in proportion to the surface wherewith they press thereupon, this opposition was not discernible.* And again, *Though in small and light shot, the opposition of the air ought and must be accounted for; yet in shooting great and weighty bombs, there need be very little allowance made; and so these rules [those, to wit, grounded on the principle of GALILEO] may be put in practice to all intents and purposes, as if this impediment [the resistance of the air] were absolutely*

(n) See his treatise *To hit a Mark*, published in 1690.



*removed*<sup>(o)</sup>. Such conclusions, which we now find to be erroneous, were the less to be expected from so eminent a person, as they argued too much haste to finish a theory, that was to be made subservient to present use.

It might indeed have been expected, that men of science applying themselves to this study, would have been sooner awakened to the consideration of the great opposition of the air, by the *Principia* of NEWTON, published a little after this Paper of HALLEY'S<sup>(p)</sup>. For in that excellent work the illustrious author had demonstrated, that the curve described by a projectile, in a strongly resisting medium, differed much from a parabola, and that the resistance of the air was great enough to make the difference between the curve of projection of heavy bodies and a parabola far from being insensible, and therefore too considerable to be neglected.

Have we not then less to plead for not attending to the *Principia* of NEWTON in this article<sup>(q)</sup>, than the mathematicians of other nations, who, as M. de FONTE-

(o) Philof. Transf. N<sup>o</sup> 179, p. 20.

(p) In the year 1687.

(q) NEWTON, Princip. Mathem. lib. ii. sect. 7.



NELLE observes<sup>(r)</sup>, partly from the difficulty of understanding that concise and profound work, and partly from a misapprehension of its tendency (which they fancied was to revive the exploded doctrine of *occult qualities*) were late in becoming acquainted with it? But it is not so easy to account for their inattention to HUYGENS, a known and even then a much esteemed author, and who indeed was second to NEWTON alone in science and in genius. For he in the year 1690 had published a treatise on *Gravity*, written in a popular manner, wherein he gave an account of some experiments he had made at Paris, and in the academy, by which, as well as by mathematical investigations, he was convinced of the truth of NEWTON'S conclusions, in regard to the great opposition of the air to bodies moving swiftly in it; and, by consequence, believed that the tract of all projectiles was very different from the line of a parabola<sup>(s)</sup>.

But excepting NEWTON and HUYGENS, the learned seemed universally to acquiesce in the justness and sufficiency of the principles of gunnery invented by GALI-

(r) Eloge de NEWTON.

(s) Discours de la Cause de la Pesanteur. Leide, 1690.



LEO, enlarged by TORRICELLI, confirmed and reduced to system by ANDERSON, BLONDEL, HALLEY and others; and so far were the theorists, in that branch of science, from suspecting any defect or fallacy in these principles, that they seemed rather to reproach the practical artilleryists, for not profiting more by the instructions which they had so liberally imparted to them. Nor do we find that an apology was made for the empirical exercise of the art, by any author of note in that line, earlier than the sixteenth year of this century, when M. de RESSONS, a French officer of artillery, distinguished by the number of sieges at which he had served, by his high military rank, and by his abilities in his profession; when he, I say, thus qualified to bear testimony, presented a *memoire* to the Royal Academy (of which he was a member) importing, that *although it was agreed that theory joined to practice did constitute the perfection of every art, yet experience had taught him, that theory was of very little service in the use of mortars. That the work of M. BLONDEL had justly enough described the several parabolic lines, according to the different degrees of the elevation of the piece; but that practice had convinced him there was no theory in the effects of gun-powder: for that having endeavoured, with the greatest precision,*

to



*to point a mortar agreeably to those calculations, he had never been able to establish any solid foundation upon them* <sup>(1)</sup>.

Thus, after the theory of gunnery had exercised the genius of the learned for nearly two hundred years, and for almost fourscore of that time had rested on fundamentals which had never been contested, it was pronounced at once to be almost intirely uselefs, and that by one of the most competent judges. Now, whether it were owing to the deference due to the authority of that experienced artillerist, or to some other cause, I shall not determine, but observe, that it appears not from the history of the academy, that the sentiments of M. de RESSONS were at this time controverted, or any reason offered afterwards for the failure of the theory of projectiles when applied to use. Nor can I pass unnoticed the pause that ensued before any further attempts were made to improve the theory of the art, either upon the old principles or upon new ones, except by such authors as seemed ignorant of this transaction, and who of course were not sufficiently apprized of the inefficacy of the properties of the parabola for directing practice.

(1) Mem. de l'Acad. R. des Sc. A. 1716.



Or by those who were employed in speculatively investigating the nature of the curve traced by a ball in the air; a curve which began at last to be considered as one deviating much from the line of a parabola. Or, finally, by such as, having taken notice that NEWTON's ideas had not been duly attended to, endeavoured to avail themselves of them, and of some experiments that had been made by others, for proving the great opposition of the air to bodies of swift motion; but without ascertaining the degree of that resistance, or enriching the art by any practical rules<sup>(u)</sup>.

Such was the unhinged state of this part of the mixed mathematics, when within our memory Mr. BENJAMIN ROBINS took cognizance of it: nor could the subject have fallen into abler hands, endowed as he was by nature with a superior genius and unwearied application. Mr. ROBINS was deeply versed in geometry and the doctrine of numbers; but he knew the limits as well as the powers of both, and how insufficient they were for establishing any theory where matter was concerned, without preparing the way, by finding out the physical properties of that *matter*, by many and varied

(u) DAN. BERNOULLI, Comment. Acad. Petropol. T. 2. & 3.



experiments and attentive observation. Those who had hitherto treated of the foundation of gunnery, by being too forward in the application of their mathematics, had in a manner hurt the credit of that admirable science. They ought to have seen the necessity of minutely examining every circumstance which could affect the course of a projectile, besides that of gravity. Mr. ROBINS perceived the error of his predecessors in that inquiry, and corrected it. Persuaded as he was from Sir ISAAC NEWTON'S *Principia* of the great resistance of the air to bodies moving in it, and also of the uncertainty of the force of gun-powder, and of the variations in the flight of shot, occasioned by the unavoidable varieties in the make of it, and in the make of the pieces of artillery which discharged it; apprized, I say, of so many causes of aberration, he justly concluded, that the foundation here was at least as much an affair of physics as of geometry, and that if the art of throwing bombs had not been advanced by theory, it was not because the art admitted of none, but because the theory which had hitherto been devised had been both defective and erroneous. He suspected that most of the writers on gunnery had been deceived, in supposing the resistance of the air to be inconsiderable, and thence asserting the track.



track of all shot to be nearly in the curve of a parabola, by which means it came to pass that all their determinations, about the flight of projectiles of violent motion, had declined considerably from the truth. But in order to clear this point from every doubt, he found it necessary to ascertain the force of gun-powder, and by that step to estimate the velocity of the shot impelled by its explosion. That being done, he proceeded to measure the quickness of a musket-bullet, shot out of a given barrel, with a given quantity of powder; and to confirm the truth of his conclusions, he contrived a machine, by which the velocity of a bullet might be diminished in any given *ratio*, by being made to strike on a large body of a weight justly proportioned to it; whereby the swiftest motions, which otherwise would escape our examination, were to be exactly determined by these slower motions that had a given relation to them. The machine was a large wooden pendulum, which swung freely, but in so slow a manner, that its vibrations could easily be counted, whatever was the celerity of the bullet discharged against it. The thought was simple, ingenious, and incontestably his own.

He next inquired into the resistance made by the air to projectiles of rapid motion, and which he discovered to be much greater than had been supposed by any writer on the subject; and indeed so great, that it was manifest the curve described by any shot was very different from a parabola, and consequently that all the applications of the properties of that conic section to gunnery were so erroneous as to be totally useless. For by means of this pendulum, placed at different distances from the mouth of the piece, he clearly demonstrated how much a bullet, flying with a given velocity, would gradually lose of that motion by the opposition of the air: therein furnishing to the learned a signal and instructive instance of the fallacy of the most specious theories, that do not proceed hand in hand with experiments.

I should too much exceed the just bounds of a discourse of this kind, were I to enter more minutely into the system founded by Mr. ROBINS, confirmed and improved, as I find, by the labours of several of the learned in foreign parts of great celebrity<sup>(w)</sup>. I shall only add,

(w) It is also much to the honour of Mr. ROBINS, that his writings on this subject have been translated into foreign languages by men that were the best judges of their merit. I need only name M. M. EULER, and LE ROY.



that his performance well deserves the title he gives it of *The new principles of gunnery*, since the author may more properly be said to have invented a new science than to have added to an old one. And I believe I may venture to say, that no physico-mathematical disquisition hath done more honour to this country, or to the age, than the writings of Mr. ROBINS on this subject, which have been published, partly by this Society, partly by himself, and partly since his death (in the collection of his whole mathematical tracts) by his learned friend.

But though our worthy brother will ever be celebrated for being the inventor of the true principles of gunnery, yet it would be too flattering to his memory, to say he had carried the theory of this art to perfection. He himself was far from entertaining so high an opinion of his labours; nay he expressly declared, that he left some material points to be inquired into at more leisure (which other occupations and his immature death deprived him of) and he much regretted that he wanted conveniency and opportunities for making experiments on balls of a greater weight, than what he had used for ascertaining the initial velocity of them.

Much therefore are we indebted to Mr. HUTTON, who, treading in the footsteps of the deceased, hath resumed and prosecuted this last *desideratum*, and hath shewn himself not unequal to so difficult an enterprize.

Mr. ROBINS, for determining the initial velocity of shot, arising from different quantities of powder, made use of balls of about an ounce weight; whereas Mr. HUTTON, for the same purpose, hath employed those of different weights, from one pound to nearly three; or, in other words, Mr. ROBINS made trial with musket-shot only, Mr. HUTTON with cannon-balls from 20 to about 50 times heavier. This was a considerable step gained in a disquisition on that part of the science, in which the resistance of the air and other circumstances were not concerned; and where neither analogy alone, nor mathematical deductions alone, nor the two combined, were sufficient for establishing principles applicable to the motion of cannon-balls, without making a new series of experiments: and with what labour and judgment these have been performed, you understood by the account which Mr. HUTTON gave of them in his Paper.

But



But should it now be inquired, what advantages may be derived from Mr. HUTTON's experiments, for the advancement of the art of gunnery, and of philosophy in general? I would reply, that as to the former it may be sufficient to observe, that though the improvements be only such as can be deduced from the force of fired gun-powder; yet they are in a higher, more certain, and in a more general manner, than what resulted from the labours of Mr. ROBINS; who indeed led the way, but who made, as it were in miniature, those experiments which Mr. HUTTON hath executed at large, and which ROBINS himself wished to have made, as well as others who have considered the subject since his time. Now these experiments, though made by Mr. HUTTON with cannon-balls of a small size, may nevertheless form just conclusions when applied to cannon-shot of the largest size. And such conclusions inform us of the real force of powder when fired, either in a cannon or a mortar, impelling a ball or bomb of a given weight; that is, they discover with what velocity a given quantity of powder drives those projectiles in a second, or in any other assigned portion of time. They also shew the law of variation in the velocity arising from different quantities of powder, with the same weight of metal, and likewise

wise that law which takes place upon using balls of different weights. Further, they point out the advantage obtained by diminishing the windage in cannon, and teach us how we may increase the weight of the shot in the same piece, by making it of a cylindrical form, instead of a spherical: by this device, a smaller ship may be enabled to do the execution of a larger one. And experiments of the same kind will also determine the just length of cannon for shooting farthest with the same charge of powder.

Lastly, it is from these experiments, or from others that may be made after the like manner, we are instructed how to answer every question relative to military projectiles, except such as depend on the resistance of the air to bodies moving swiftly in it. This indeed is a consideration which leaves room for greater improvement in the art, and for conferring fresh honours on those, who, like Mr. HUTTON, shall have opportunities and abilities for continuing and perfecting this very curious and useful inquiry.

As to the advantages accruing to philosophy from the labours both of Mr. ROBINS and Mr. HUTTON, speak  
they



they not for themselves? The sciences of motion and pneumatics are promoted by them; and of what avail their perfection would be for the farther interpretation of nature, you need not be informed. In fine, we have here before us, in these experiments, the surest test of our advancement in true knowledge, which is, the improvement of a liberal art, and the enlargement of the powers of man over the works of creation.

Some however may think, that the objects of this Society are the arts of peace alone, not those of war, and that considering how numerous and how keen the instruments of death already are, it would better become us to discourage than to countenance their farther improvement. These naturally will be the first thoughts of the best disposed minds. But when upon a closer examination we find, that since the invention of arms of the quickest execution, neither battles nor sieges have been more frequent nor more destructive, indeed apparently otherwise; may we not thence infer, that such means as have been employed to sharpen the sword, have tended more to diminish than to increase the number of its victims, by shortening contests and making them more decisive. I shall not however insist on  
maintainings

maintaining so great a paradox; but only surmise, that whatever State would adopt the Utopian maxims, and proscribe the study of arms, would soon, I fear, become a prey to those who best knew how to use them. For yet, alas! far seem we to be removed from those promised times, *when nation shall not lift up sword against nation, neither shall they learn war any more!*

*Here ended the President's Discourse: after which he turned to Mr. HUTTON, and said,*

YOU have heard, Sir, the account I have given of the rise and progress of the *theory of gunnery*, and of your improvement of it; a recital, which by no means would have done either you or the subject justice, had it been addressed to any other audience than to the present. But as my intention was only briefly to recall to the memory of these gentlemen what they knew of this subject, antecedently to your Paper, and to remind them of the result of your experiments, I flatter myself I have said what was sufficient on the occasion: being now authorized



thorized by them to deliver into your hand this medal, as the perpetual memorial of their approbation. And let me add, Sir, that they make you this present with the more cordial affection, as by your other ingenious and valuable communications they are assured, not only of your talents, but of your zeal, for promoting the interests and honour of their Institution.



E R R A T A.

- Page 4. l. 5. *for this read the*  
 16. 9. *for combate read combat*  
 20. 17. *for tract. read track*  
 26. last line of the note, *for M. M. read MM.*

