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# RIFLED ORDNANCE.

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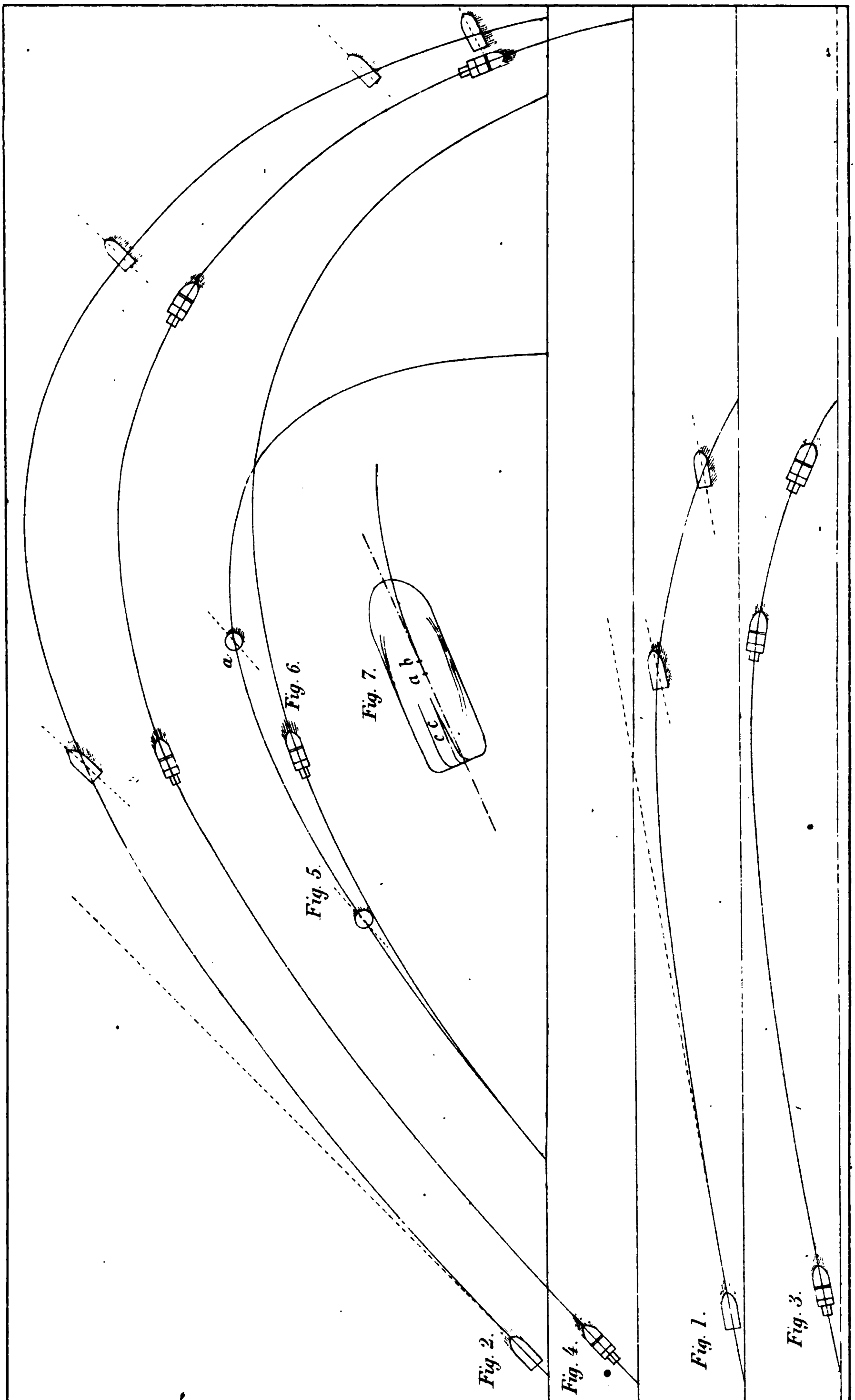

# RIFLED ORDNANCE:

## A PRACTICAL TREATISE

ON

THE APPLICATION

OF

# THE PRINCIPLE OF THE RIFLE

TO

## GUNS AND MORTARS OF EVERY CALIBRE.

TO WHICH IS ADDED

A NEW THEORY OF THE INITIAL ACTION AND FORCE OF FIRED GUNPOWDER.

**(Read before the Royal Society, 16th December, 1858.)**

FIRST AMERICAN,

FROM THE FIFTH ENGLISH EDITION, REVISED.

BY

LYNALL THOMAS, F.R.S.L.

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# PREFACE.

When the greater part of this little work was written, that is to say, in the years 1856–7–8, a very limited knowledge only of the subject of which it treats had been generally acquired; since that time, however, a considerable advance has been made in the science of Rifled Gunnery—and I have thought it better to omit, in this edition, a considerable portion of the matter contained in the former—especially that which had reference to lead-covered projectiles—as obsolete, or likely, ere long, to become so.

The expanding projectiles represented in the Plates of the last edition, were—as there stated—merely experimental, and described for the purpose of illustrating certain principles, rather than with the view of recommending the projectiles for practical adoption. It has always appeared to me that the employment of any system which entailed the use of lead-coated projectiles could only be advocated in the absence of a better, or, until a more satisfactory system could be worked out; though I confess that, at the time this work was written, I saw no way to the solution of the problem. Since then, however, I have succeeded in perfecting a system which, I believe, will be found to meet all the requirements of our Service, without necessitating the use of lead-coated projectiles. This system, being actually under the consideration of the War Authorities, I have not judged it expedient to publish, until it has been thoroughly tested. I may state, however, that, up to the present time, the results—obtained under most trying conditions—have been highly satisfactory.

The introduction of iron-plated ships of war, and general improvement in the means of defence, whilst rendering the acquisition of a good system for the construction and employment of heavy rifled ordnance a matter of paramount importance, have greatly altered the conditions to be observed for the attainment of the greatest destructive power with these guns.

When the employment of vessels of war built entirely of wood was universal, the most destructive effect would have been produced by the employment of heavy shells fired with necessarily limited velocities; *now*, the attainment of great force of impact and penetrating power has become of much greater importance, and a stronger and heavier description of gun is therefore rendered necessary.

Enough, however, has been done to show that guns of almost any power may be obtained; the only limit to their practical employment being the weight of the gun itself.

Notwithstanding that this has been incontestably proved by experiment, the country—owing to the want of a proper system of rifling for guns of this description—is still without a heavy rifled gun.

When the last edition of this little tréatise was published, the only rifled gun which had been actually adopted into our service was the Armstrong field-piece. From the enthusiastic encomiums lavished upon it at the time, I was led, with others, to suppose that the gun we heard of was the first of a series about to be constructed upon a well-matured and approved system; and, as great secrecy was observed on the part of the officials, I awaited with much interest the development of this new and perfect theory.

During the four years which have elapsed, 20-pounder, 40-pounder, 70-pounder, and 100-pounder Armstrong guns have been constructed, but I have awaited in vain for the development of the theory or system which was expected to throw so much light on the science of Rifled Gunnery. At last it was discovered that there never had been any system; but that the chief difficulties, scientific as well as mechanical, were supposed to have been completely overcome by the revival, simply, of the obsolete and objectionable method of loading the gun at the breech; in fact, that we owed the adoption of this method of rifled cannon into our service solely to the mechanical skill displayed in the construction (after numerous experiments) of a cannon of very small calibre, to which the manufacturer, by means of an ingenious breech-loading apparatus, had succeeded in applying the principle of the rifle. Consequently, when guns of a larger size were required, each must necessarily have been the subject of fresh experiment; which may, in some measure, account for the vast expenditure of the last four years.

Still, it is an undeniable fact, that as these guns increase in size their comparative efficiency is not so great as it ought to be. This may be partly attributed to the very erroneous opinions which are generally entertained respecting the quantity of rotary motion required for rifled projectiles of different sizes.

In the absence of a proper theory, "the rule of thumb," as it is called, appears to have been employed in determining the length of turn to be given to the rifling in the Armstrong guns of different calibres; that is, a gun of twice the calibre of another has twice the length of turn, and so on.

Now, as the projectile's stability during its flight depends entirely upon its receiving a proper degree of rotary motion, the length of the turn for large guns is a most important matter for consideration, and if it is not given in a proper ratio, the efficiency of these guns will be proportionally diminished.

The ratio of increase for the turn in the Armstrong guns is clearly insufficient; and that the inventor himself has some suspicion that it is so, is manifest from the circumstance that, for the heavier guns recently constructed, the comparative length of the projectiles has been considerably reduced. These projectiles require, therefore, a comparatively smaller turn; but they compel the employment (when they are of a given weight) of a gun of large calibre, which (if my experiments in gunpowder are to be depended upon) subjects the gun to a greater local strain; and may partly account for the mishaps which frequently occur with these guns, and for the diminished efficiency of the shot.

The only motive, if there be one, for giving the length of turn in proportion to the calibre of the gun—since there can be no scientific reason for it in connection with the flight of the projectile—must be to diminish the relative force expended in the gun in giving the projectile its rotary motion; but if the Armstrong method of rifling does not admit of the employment of the proper turn for giving a proportional rotary impetus to the shot in the larger guns (see p. 55), it must be radically wrong; as this would prove the existence of excessive friction.

The turn employed by myself, and the still greater one used by Mr. Whitworth for heavy rifled guns, have been stated to be disproportional and excessive; but this is an error. A proper investigation of the subject would convince all who entertain such a notion that, so far from being excessive, the turn I have used with 7-inch and 9-inch guns is comparatively less than that of the Whitworth musket.

I would here observe that this, in common with every other question relating to dynamical science, cannot be considered a mere matter of opinion, since it admits of the clearest mathematical demonstration.

The absurd practice of reckoning the length of the turn in calibres instead of in feet and inches, doubtless gave rise to the idea that the turn was excessive. A turn of 6 feet, whether in a musket or in a 7-inch gun, gives (with an equal velocity of translation) the same angular velocity to both shot; and, reckoned in this way, whether the turn be short or long, we can at once form some idea of the comparative quantities of rotary motion imparted to the shot; but if the turn is reckoned in calibres, an estimate of the rotary impetus of the shot can only be formed after a long calculation. How absurd it would be considered if the velocity of translation of shot were reckoned in calibres! and yet it is equally absurd to take this course with regard to the angular velocity. The laws which govern rotating projectiles appear to have been sadly disregarded in this matter.

The Armstrong must still be considered an experimental gun. The science of Rifled Gunnery was in its infancy when we adopted this method. The result was, that it put a stop to all progress; in that experiment, since then, has been almost exclusively confined to attempts to improve a method, the faults of which are inherent; instead of being directed to the acquisition of a knowledge of the science which would have led to the formation of a system based upon sound principles.

An additional obstacle to progress exists in the strong party feeling which, unfortunately, has been created; and which has impressed the public with the idea that, having pledged themselves to this inventor, the authorities are unwilling, or unable, to accept any system which does not emanate from Sir W. Armstrong.

This feeling has gained ground from the circumstance that (his original method not being applicable to large guns) he has been allowed, for several years, and at a great cost, to try and produce a heavy rifled gun (the shunt) upon a principle which must be condemned by all scientific artillerists.

Although, in advocating what I believe to be the cause of science and truth, I have attacked Sir W. Armstrong's principles of rifling, I must do justice to his abilities as an engineer and mechanician. The country is indebted to him for the only successful method of constructing wrought-iron guns; and although I disagree with him in nearly all that relates to the projection of the shot, I can yet admire the ingenuity and mechanical ability he has displayed in carrying out his own views.

In the chapter "On Rifled Cannon," I have suggested that the brass service howitzer should be converted into a rifled field-piece. This was written some time ago; but, since this edition was in the publisher's hands, the trial of a method (proposed by Capt. Palliser) of strengthening the service guns, by placing a coiled tube in a novel manner in the interior of the gun, has been attended—in the case of a 68-pounder gun—with sufficient success to show that it might be applied with great advantage to the brass howitzers, in converting them into rifled field guns; especially as, by this method, an additional length is given to the bore of the gun. This would be preferable to casting the guns over again, as I have suggested; and if all the old brass field guns were recast as howitzers, and the latter were converted into rifled field guns, the country would possess, at a comparatively small expense, a reserve of most efficient rifled field artillery, which might eventually prove of the utmost importance.

If, after being knocked about a little on service, and then exposed to the crushing fire of the finest rifled artillery on the continent, the breech-loading Armstrong gun should fail—and there are many, and I confess to being of the number, who look upon such a catastrophe as almost inevitable (a breech-loading field-piece, with its many complications, being, in the present day, so very hazardous an experiment)—a reserve of the above kind might, at a critical moment, be the means of saving the honor of the country.

I have a few words to add on the subject of the "Theory of the Action and Force of Fired Gunpowder." This theory, although received with much disfavor and incredulity at first—as entirely subversive of all hitherto received opinions—is gradually gaining ground as, day by day, fresh proof is furnished of its truth.

Had it been recognized earlier, there is no doubt that much greater progress would have been made towards the attainment of a powerful and perfectly *safe* rifled gun for the Naval Service and Coast Defences; the ordinary theory affording no law or method by which the proper distribution of the metal, or the best disposition for a given charge of powder in a gun, can be ascertained.

## LIST OF WORKS

TO WHICH REFERENCE HAS BEEN MADE IN THE FOLLOWING TREATISE, WITH THE DATES OF THEIR PUBLICATION.


| | |
|---|---|
| *The Gunner's Glasse*, by W. Eldred . . . . . . | 1646 |
| *New Principles of Gunnery*, by Benjamin Robins (new edition, corrected by Dr. Hutton) . . . . | 1805 |
| *Scloppetaria*, by a Corporal of Riflemen (Colonel Beaufoy) . . . . . . . . . . . . . . . | 1808 |
| *Mathematical Tracts*, by Dr. Hutton . . . . . . | 1812 |
| *Cours d'Artillerie*, by General Piobert . . . . . | 1841 |
| *Experiments on Gunpowder*, by Maj. Mordecai, U.S.A. | 1845 |
| *Taylor's Scientific Memoirs* . . . . . . . . . . | 1853 |
| *Artillerist's Manual*, by Major Griffiths, R. A. . . . | 1854 |
| *On the Science of Gunnery*, by Captain Boxer, R. A. . | 1854 |
| *On the Rifle Musket*, by Captain White Jervis, R. A. . | 1854 |
| *Naval Gunnery*, by Sir H. Douglas . . . . . . | 1855 |
| *Treatise on Artillery*, by Captain Boxer, R. A.. . . | 1856 |
| *Rifle Practice*, by Lieut.-Col. Jacobs . . . . . . | 1857 |
| *On the Improvement of the Rifle*, by Lieut.-Col. Lane Fox . . . . . . . . . . . . . . . . | 1858 |
| *Etudes sur les Canons Rayés*, par le Captne. F. Gillion | 1858 |
| *On Mechanical Subjects*, by J. Whitworth, F. R. S. . | 1858 |

*Philosophical Transactions of the Royal Society.*
*Mechanics' Magazine.*
*Encyclopædia Britannica*, eighth edition.

# TABLE OF CONTENTS.

APPENDIX.

# LIST OF ILLUSTRATIONS.

# RIFLED ORDNANCE.

## INTRODUCTION.

The treatment and discussion of a subject purely military by one who is not in immediate connection with the Service, may seem to demand some apology. The theory of projectiles, however, presents to the scientific inquirer ample scope for investigation and experiment. To pronounce upon the most proper method of *applying* any fresh discovery affecting that theory, is solely for the consideration of competent military authorities; and whenever I have ventured to express any opinion on this point, I wish it to be understood that it is under the correction of those who are better qualified than myself to judge of what may best accord with the practical requirements of the different Services. In other respects I have advanced nothing which has not previously been proved by experiment. The suggestions, therefore, which I have hazarded in the following pages, however imperfect, may not, I hope, be destitute of value.

As yet we have no satisfactory or conclusive authority to which we may refer on the subject of rifled cannon. The absence of all such authority may partly plead as an excuse for any imperfections in this slight attempt to supply the deficiency; and if the opinions expressed here do not meet with the entire concurrence of those who are best informed upon the subject, the publication of them may at least induce other and perhaps abler persons to assist in the development of this interesting, but difficult question.

With the exception of those who are immediately interested in the matter, few persons are aware how much scientific research, as well as practical experience, is necessary for the acquirement of a knowledge of the laws respecting rifle projectiles, and for correctly ascertaining their capabilities. Indeed, it would be difficult to find a subject more comprehensive in its nature than that of rifled cannon.

My object has been to throw as much light upon the matter as possible. Whatever contributes to this is all the more important, as improvements in small arms render a corresponding improvement in cannon, if not absolutely necessary, at least extremely desirable. It is quite evident to all who have attentively studied the subject, that the same precision and relative efficiency which characterize the ordinary rifle can be as certainly obtained with cannon; still I must admit, after much investigation and many experiments, that the laws which govern the description of projectile to which I purpose more particularly to direct attention—that is to say, elongated shot, to be fired from rifled guns—are, apparently, not of a nature to admit of any of those very startling results, such as ranges of ten and even fourteen miles, which have been occasionally spoken of as possible.

The difficulties in the way of perfecting the construction of elongated shot and rifled guns are very much greater in the case of ordnance than in that of small arms. The want of a material sufficiently strong to resist and control the power inherent in a large charge of gunpowder, is also an impediment to such an increase of effect as would otherwise, perhaps, be attainable.

Very effective results may, nevertheless, be realized with rifled cannon, if properly constructed. The hope of extending the capabilities of this description of cannon induced me to turn my attention to the subject, and to make various experiments to test the soundness of the conclusions to which I had been led in the course of my investigations. Some of the results thus obtained are explained in the following pages, and may, perhaps, lead to useful improvements in gunnery. These results relate principally to the laws which should regulate the rifling of cannon for the purpose of enabling them to throw a heavier description of shot or shell. Hitherto the rifling of cannon seems to have been determined rather by accident or caprice than by any fixed and determinate rules; so that, in fact, every rifled cannon which has been constructed can only be regarded as an "experiment."

One of the chief objects which I have in view is to show in what manner the principle of the rifle should be applied—from an accurate standard obtained in the first instance—to guns of all sizes. There would thus result to the Government at least this obvious advantage: the cost of endless experiments would be spared; a cost which personal experience has convinced me must be enormous. If the present work, and the experiments upon which it is in a great measure founded, tend to further the acquisition of sufficient data for arriving at the object indicated above, it will be a source of gratification to know that the outlay has not been in vain.

# ON RIFLED CANNON.

That great and striking advantages may be derived from the employment of rifled cannon, has been long and fully acknowledged; yet is it within a comparatively recent period only that any serious intention of testing the applicability of this description of ordnance has been entertained. This is a remarkable fact. The cause must have proceeded either from ignorance of the capabilities of rifled cannon, or from insufficient data whereon to establish a fixed principle for their construction. Be this as it may, their trial was never attended with sufficiently advantageous results to warrant their permanent adoption into our service. Had Robins lived to carry out his ideas upon the subject fully, it is probable that rifled cannon and long projectiles (the advantages of which that acute and scientific experimentalist was the first to appreciate and point out) would have been in use a hundred years ago. It would be curious to speculate upon the effects which their introduction into warfare might have produced upon the events which have happened during that period.

The passage so frequently quoted from Robins's "Principles of Gunnery," where he predicts the great superiority which will be acquired by the nation first employing rif barrels in warfare, evidently refers even more to cannon than to small arms. This will be seen by a perusal of the *whole* of the passage in question, which is as follows:

"From the nature of these (*i. e.*, rifled) pieces, it is plain that they can only be made use of with leaden bullets, and consequently cannot be adapted to the adjusting of the motion of either shells or cannon-balls. However, from the same principle, whence these pieces derive their perfection, other artifices may be deduced for the regulating the flight of these more ponderous bodies. On some of these methods, which have occurred to me, I have already made several experiments; and there are others, which I have more lately considered, and which appear to me infallible. But there are many reasons why I should not now engage in a circumstantial discussion of this kind. I shall, therefore, close this paper with predicting, that whatever State shall thoroughly comprehend the nature and advantages of rifled-barrel pieces, and, having facilitated and completed their construction, shall introduce into their armies their general use with a dexterity in the management of them, they will by this means acquire a superiority which will almost equal any thing that has been done at any time by the particular excellence of any one kind of arms; and will perhaps fall but little short of the wonderful effects which histories relate to have been formerly produced by the first inventors of fire-arms."—*New Principles of Gunnery*, p. 341.

The use of rifled cannon not only prevents the deflection of shot, and insures greater accuracy of practice, but also, without increasing the weight of metal in the gun, admits the employment of heavier shot or shell, and obtains more extended ranges than is possible through the medium of any other kind of ordnance.

When the power and means of projection are limited, two methods suggest themselves for increasing the effect of projectiles: the one is by increasing their weight, and consequently causing a decrease in their velocity; the other, by diminishing their weight, and thus increasing their velocity. A very superficial knowledge of gunnery will lead to the conclusion that the best effect is produced by increasing the *weight* rather than the *velocity* (particularly when the latter has reached a certain magnitude), since the laws of the resistance of the air preclude the attainment of any great advantage by giving a shot more than a certain velocity.

As, however, it is not so much required in practice to produce the greatest effect with a given shot, as with a *gun* of a given weight, the latter should first be taken into consideration. Now, the production of great velocity requires heavy charges of powder, and greater thickness of metal in the gun, than is required for a proportionally increased weight of the shot; it will therefore be advantageous to give additional weight to the shot, even at the expense of its velocity.

To illustrate this point, let us consider the results obtained by two different kinds of guns used in our service, both of about the same weight (17 cwt.): namely, a 32-pounder carronade, and a 9-pounder iron gun. The latter throws a 9-lb. shot nearly 1400 yards at an elevation of 4°, and the former, with rather a smaller charge of powder, throws a 32-lb. shot about 1000 yards at the same elevation (as may be seen by referring to the Tables of Ranges in any of the Treatises on Gunnery): so that, with the same weight of metal and a smaller charge of powder, the carronade throws a shot nearly four times the weight of that thrown by the 9-pounder a distance equalling five-sevenths of that attained by the latter. Here we see that the greater comparative result is produced with the shot of the greater weight, and not with that possessing the greater velocity.

Now, if with a gun of the same weight as either of these, a 32-lb. shot could, with the above elevation, be sent to a greater distance than the 9-lb. shot, it is undeniable that the means employed would be a sensible improvement upon existing arrangements. Still further would this be the case, if at the same time a greater degree of accuracy could be imparted to the shot.

This can be accomplished by the use of elongated shot—shot in which, while the *weight* is the same as that of the larger of the two shot above mentioned, the *diameter* is that of the smaller, and, therefore, the surface upon which the resistance of the air acts will be the same, or nearly so, as that of the smaller.

It is true that *at low elevations* the range of an elongated shot of nearly four times the weight of a spherical shot of the same diameter will not, with its necessarily reduced charge and initial velocity—supposing both shot to be fired from the same gun—be greater than that of the spherical shot, but at greater elevations, when the time of flight is longer, the superiority of the elongated shot is manifest. The weight of the latter being greater in proportion to the surface exposed to the resistance of the air, the velocity will suffer less diminution from that resistance; the curve of flight will therefore approximate more nearly to a parabola, and the range will be much greater than that of spherical shot.

The practical advantages to be derived from the substitution of rifled for smooth-bored guns are of sufficient importance to render it desirable that this substitution should take place within as short a time as possible. Very efficient rifled field-guns might be at once obtained by boring the brass howitzer blocks to a reduced size, and rifling them; a course I would suggest as preferable to that of rifling the ordinary field-gun, for reasons hereafter mentioned; and because the howitzers are stouter and handier. As an example of the superior advantages to be gained by this method, take either a 12-pounder, a 24-pounder, or a 32-pounder brass howitzer, as at present in use. The first of these, the 12-pounder of 6½ cwt.—having an elevation of 5°, * and a charge of 1¼ lbs. of powder—will throw a shell of 8 lbs. weight to a distance of 1100 yards. If this howitzer were rifled, the greatest general effect, perhaps, would be produced by the use of a shell of reduced diameter and about 9 lbs. weight; this shell would have an advantage in weight, over the spherical shell of 8 lbs., as well as a much more extensive range. Were a shell more than 9 lbs. weight employed, the charge of

* I assume an elevation of 5°, because the difference in the ranges will be appreciable at that elevation, and it is also one which may be frequently used. But it must be remembered that the difference between the range of the long and round shell will continually increase with any increase in the elevation.

powder which could be conveniently used with so light a gun would be too small, in proportion to the weight of the shell, to produce a sufficient range at low elevations.

A rifled 12-pounder howitzer, when fired at an elevation of 5°, and with a charge of 1¼ lbs. of powder, will throw a shell of 9 lbs. weight and reduced diameter nearly 1700 yards; a distance one-half greater than that which, with the ordinary bore, it would propel a round shell of 8 lbs. weight. A rifled howitzer, of 6½ cwt. only, could be as quickly loaded as a smooth-bored gun of the same weight. As the object of rifled field-pieces would be either to act in unison, or to cope with the improved rifle muskets, their comparatively low trajectory and precision of fire might be considered, in certain cases, to counterbalance any disadvantage in other respects which might attend their use, such as their defective *ricochet*, etc. Besides an incomparable accuracy, a rifled gun of the above description would have the advantage in range and weight of projectile over either a 6-pounder field-piece or a 12-pounder howitzer, both of which would be of the same weight with it. It would also be equal to a 9-pounder field-piece, which is double the weight. The same kind of comparison may be instituted, and superiority evinced, in the case of the other pieces which I have mentioned.

I do not pretend to affirm that the use of rifled howitzers, to the entire exclusion of others, would be advantageous, since there may be many occasions on which round shells would be preferable to others, as for *enfilading* and *ricochet* firing, for instance; but I am certainly of opinion that such field-pieces as are used for throwing solid spherical shot only might be entirely abolished the Service. For these, 12-pounder and 24-pounder howitzers, rifled on the principle which I have described, might be substituted with immense advantage, and all the old brass guns might be recast and converted into howitzers.

The alteration which I have suggested would be attended with the smallest possible expense, both with respect to its adoption, and (if necessary) to its suppression afterwards; in the latter case they would simply have to be bored out afresh to fit them for service as ordinary howitzers, which are not likely to become so soon obsolete as the 6-pounder and 9-pounder field-guns.

By pursuing the above method, a most efficient arm could easily, and at once, be introduced into the Service, with the smallest possible expenditure, and without materially affecting existing arrangements; indeed, the Service would be more uniform on this account.

I consider all breech-loading rifled guns, small as well as large, to be objectionable; and have no doubt that, from their general want of simplicity, they will eventually become as obsolete as the old breech-loading guns of former days.

The expenditure of time and labor which would be necessary to keep a large number of breech-loading guns constantly in a fit state for service, both when stored and in use, must prove a great obstacle to their general employment. Their general efficiency in action also has yet to be tested.

The increase in the relative weight of the breech-loading apparatus—the increased difficulty and danger attending the loading—their enormous cost, are all circumstances which militate against the use of large breech-loading cannon; so that the general adoption of this principle would tend to restrict the employment of rifled cannon to those of a comparatively small size. Even if breech-loading guns of a large size could be employed, the results attainable with them would not equal those produced with rifled guns of equal size loading at the muzzle.

Breech-loading field-pieces, unless of the simplest construction, must always be objectionable in action. It may be an easy matter enough for well-trained men under the immediate superintendence of a number of scientific officers at Shoeburyness, to fire so many rounds a minute; but would this be so in action? A system of breech-loading, which a man of common capacity cannot learn and retain after ten minutes' teaching; one which causes the employment of *detached pieces* of metal; any, in fact, which requires more than two simple movements, one to open and one to close the chamber, is open to objection. If it is considered necessary to employ breech-loading pieces, they ought to be made to act with a movement which a man can perform mechanically, as a soldier loads his musket, or a sportsman his gun, without having to think over it.

Great care must be taken, with guns upon this principle, that no escape of gas is allowed, otherwise the men between decks in a ship, or in a casemated battery, would be stifled in working them; not to speak of the loss of power occasioned by it.

The constant improvements which are likely to be made in breech-loading pieces should render us cautious about adopting any one method in particular until its superiority has been thoroughly established in actual service.

It is not my intention to disparage Sir W. Armstrong's admirable inventions, but simply to question the advantages of the breech-loading principle. Regarding the Armstrong gun as a scientific engine or machine for the projection of an elongated shot, it is a *chef d'œuvre;* but so much has yet to be learned respecting rifled cannon that no one can assert, at present, that the Armstrong gun—untried as it is—is really the best suited in every respect for actual warfare.

Supposing, in the event of a European war, that the Armstrong gun be found to fail in achieving all that has been anticipated from it, what are we then to do for rifled cannon? Surely, some such method as that which I have suggested might be adopted, in order, by utilizing the principle of the rifle, to place us at once on an equal footing with other nations; without prejudice to our finally adopting that description of gun which is found by experience to be the most suitable for practical purposes, whether it be the Armstrong or any other gun.

By following the course I have suggested, as heavy a projectile, as great a range, and very nearly, if not quite as great, precision may be obtained as with the Armstrong gun; and the howitzers might be turned out by the dozen, and employed at a very little more than the ordinary expense.

What we particularly require, for our navy and coast defences, are large rifled guns. Our floating and shore batteries should be furnished with rifled guns of sufficient size and number to put all idea of a successful attack upon them by ships of war out of the question.

No rifled gun has yet been adopted in the Service of a size sufficiently large for this purpose, or even large enough to compete successfully with the 68-pounder gun; and it is questionable if a breech-loading gun, capable of throwing shells of 2 or 3, or even 1 cwt., with a proper velocity, without being too heavy and unmanageable, could be made; since the weight of the apparatus will bear a much larger proportion to the whole weight of the gun in a large gun than in a small one. All large guns must therefore be muzzle-loading.

No cost should be spared in this matter; besides, whatever may be the expense attending the production of a like description of ordnance, it would cost other nations at least as much, and probably more, to produce ordnance of a similar kind; and every circumstance which tends to render war a costly affair to all concerned, gives England (from her wealth and mechanical skill) a considerable advantage; and serves also as an additional guarantee for the preservation of peace.

## ON THE TURN OF THE RIFLING.

In the construction of rifled cannon, too little importance seems to have been hitherto attached to the determination of the turn, or pitch of the grooves—a point upon which, in reality, failure or success almost entirely hinges. It is remarkable, also, that no one has attempted any discovery or definition of the laws by which that turn should be determined; for, however much opinions may differ on this head, it is tolerably clear that there must be some fixed laws, applicable to guns of every variety of calibre, by which this matter should be governed.*

Although the object I have in view is to explain the principle by which the grooves of guns of various sizes should, by means of a correct standard, have a proper turn assigned to each according to its calibre, rather than to attempt to determine the proper amount of turn which should be given to the grooves of any particular kind of rifle, still it will be desirable to consider, first, upon what circumstances the length of the turn to be given to the grooves of a rifle depends. It will be unnecessary for this purpose to enter upon a discussion of

*Since the above was written, several small publications on Rifle Ordnance by foreign artillery officers have appeared; but as none of the theories there put forward are apparently the result of experiment, there is nothing to be learned from them of the smallest practical use, although they are extremely ingenious, and interesting to read.

all the laws relating to projectiles: those only will be noticed which bear immediately on the subject at present before us.

The question which suggests itself as the first for our consideration is this: What is the cause which renders a projectile of an elongated form more efficient when it has imparted to it a rotary motion about an axis situated in the direction of its flight?

It will be admitted on all hands that the correct answer to this question is, that such a rotation enables the projectile to resist the deflecting power of the air better. Robins's definition (which has hitherto been usually received as the correct one) of the action and utility of rifled projectiles is as follows: "* * * * a bullet discharged from a rifled barrel is made to whirl round an axis which is coincident with the line of flight, and hence it follows that the resistance on the foremost surface of the bullet is equally distributed round the pole of its circular motion, and acts with an equal effort on every side of the line of direction; so that this resistance can produce no deviation from that line. And (which is still more of importance) if by the casual irregularity of the foremost surface of the bullet, or by any other accident, the resistance should be stronger on one side of the pole of the circular motion than on the other; yet, as the place where this greater resistance acts must perpetually shift its position round the line in which the bullet flies, the deflection which this irregularity would occasion, if it acted constantly with the same given tendency, is now continually rectified by the various and contrary tendencies of that disturbing force during the course of one revolution."—(*Tracts on Gunnery*, p. 330.)

Recent investigation of the circumstances attending the flight of elongated projectiles shows, however, that a rotary movement about an axis situated in the direction of its flight alone tends to keep the projectile in the plane of its trajectory; not because the pressure of the air is thereby equally distributed round the pole of the circular motion, but because the rotary movement gives a *stability* to the projectile, which enables it actually to *resist* the tendency which the projectile has, from the pressure of the air, to become unsteady, or to turn completely over in its flight.

Experiment further shows that, in order to obtain the best effect with a particular shot, a *given* velocity of rotation should be imparted to it, which will not be the same for different kinds of bullets.

Opinions have differed as to whether the rotary motion of the shot arises solely from its constrained passage along the grooves formed in the barrel of the gun; or whether the grooves simply impart a first rotary impulse to the shot, the rotation being continued during the shot's flight by the action of the air against the projections upon the surface of the shot. Although many persons possessing experience in such matters (amongst others, Colonel Beaufoy, the author of "*Scloppetaria*") have asserted the latter to be the case, all the experiments made with elongated shot give very decided proofs that the former is the correct view—viz., that the rotary motion given to the shot is caused *solely* by the twist which it receives in passing through the barrel of the gun; and that the pressure of the air, so far from promoting or assisting it in any way, acts continually as a check upon it, in proportion both to the length of the shot and the velocity with which it is fired.

The idea that the air promotes the rotary motion which a rifled shot receives is partially founded upon a certain supposed analogy between a shot and an arrow; and, consistently with this idea, attempts have been made at various times to procure for the former a rotary motion by means of wings or grooves similar to the feathers of an arrow. The two are, however, by no means analogous. An arrow is a shaft of wood pointed with iron, which has very little tendency to turn over in its flight, for two reasons: firstly, because the resistance of the air to such a motion is very great, owing to the length of the shaft and its comparatively small density; secondly, because the direction of the original impulse passes almost exactly through the centre of gravity of the arrow, which cannot be secured in the case of iron shot. Supposing, also, an arrow to begin to turn over in its flight, this motion would be checked by the resistance of the air, in consequence of the forward position of the centre of gravity.

The feathers on an arrow are usually placed in a straight, and not in an oblique, direction (as some imagine), upon the hinder part of the shaft. Placed obliquely, they diminish the speed and range of the arrow. Both the action and effect of these parts of an arrow have been greatly misunderstood. In consequence of the *breadth* of the surface presented by the feathers, any tendency of the arrow to rotate about its shorter axis is checked, and the flight rendered steady. Three feathers will thus keep an arrow more steady than two, as with three, in whichever direction the shaft moves, the flat surface of the feathers will encounter the resistance of the air. The feathers also materially contribute to the arrow following the curve of flight, so as to be a tangent to it at every point.

But with long shot the case is altogether different; these require an equivalent to the long, well-balanced, and feathered shaft, with which the arrow is provided, in order to counteract the tendency they would have to rotate about their smaller axis, or turn over. This can only be obtained by imparting to the projectile a rapid rotary motion about an axis situated in the direction of its flight.

It is true, that, by constructing a compound shot of iron and some light material in such a manner that its centre of gravity may be thrown forward considerably, and by supplying it with projections, or some substitute corresponding to the feathers in an arrow, it may be made to approximate more nearly to the condition of the latter; and in such a case, a certain analogy may appear to exist between them; but the shot, from its deficiency in length and its more uniform density (unlike the arrow), possesses no means of checking any tendency to turn over or rotate about its shorter axis, and consequently requires a large rotary velocity to counteract it.

I made a few experiments with long shot, of which the front part was composed of iron, and the hinder of wood and other light material, firing them from a smooth-bored gun; the resistance of the air prevented these shots turning completely over, but they obtained only a very low velocity, and there was an obvious oscillating of the "tail" of the shot during its flight. And it is clear that, if the projections of the shot are sufficient to counteract its tendency to turn over, they must materially reduce its velocity, and thus impair its effect. In fact, however ingenious many of these and other inventions connected with projectiles may be, unless the full power of ordinary shot, either with regard to velocity or range, can be obtained with them, they are practically useless.

Elongated shot, when fired from a *rifle*, have the defects of the shot above mentioned remedied by having a rotary motion imparted to them in the first instance. This gives them a proper degree of stability, and allows of their being fired with a sufficient velocity to enable them to be used with great effect; and even if fired with a high velocity, the velocity of their rotation, being in proportion to that given to their flight, is sufficient to secure steadiness of motion. If, however, they are fired with too great a velocity, *they* also will have an irregular flight, but not attributable to the same cause which produces unsteadiness of flight in an arrow.

There were formerly more grounds for the opinion that the rotation of the shot was caused by the action of the air upon it during its flight, when rifles with deep cut grooves were used; but to suppose it possible that such an effect can be produced by the action of the air upon such a bullet as, for instance, that fired from the Enfield rifle, upon which the projections caused by the grooves are scarcely perceptible, seems altogether absurd. If the air *promoted* the rotary motion in the shot, its effect in that way would be more apparent upon elongated than upon spherical shot of the same diameter.

On the other hand, if the pressure of the air retards the rotation, this effect, too, will be more perceptible with long shot than with round; and as experiment clearly shows that the rotation of long shot is as rapidly diminished as that of round ones, by the action of the air, the necessary inference is that the action of the air checks the rotation, instead of assisting it.

Since it is principally, if not entirely, from the resistance of the air that the necessity for rifling shot arises, the proper degree of rotation to be given to a shot will depend chiefly upon its length, form, and size; all of which circumstances greatly modify the resistance of the air, and its effect on the shot.

And again: since the rotary motion is imparted to the shot by means of the grooves in the barrel of the gun, part of the energy of the charge will be expended in overcoming the resistance of the grooves supposed smooth, and a still further portion in overcoming the friction caused in practice by their roughness.

In the present chapter I propose to apply these principles, together with the results of experiments presently to be stated, in a general way to the subject of the proper turn for rifled guns; reserving for a future chapter the statement and proof of an approximate rule by which the variation of the turn with the calibre of the gun may be determined.

The question as to the best turn to be given to the grooves in small arms, where the ordinary bullet of about an ounce weight is used, appears to be settled in favor of a whole turn in about thirty inches;* but touching that which is best for elongated shot, opinions somewhat differ.

It will be found, however, upon examination, that it is chiefly the difference in the length and shape of the shot which causes one rifle to shoot better with a less, and another with a greater turn. If the proper length of turn were fixed by a correct standard, according to the description of the bullets used, the existing disparity between the turns of different rifles would be accounted for, since the same laws must equally govern both.

The length and form of the shot are undoubtedly the first objects for consideration in this matter, as having a direct influence upon the length of the turn. For when a shot of a given description is to be fired with a given charge, the only way of increasing the velocity of rotation is by the employment of a greater turn; therefore, as a general rule, whatever tends to check or render inefficient that velocity, such as the form of the shot offering great resistance to the air,

* The length of turn, whole turn, or complete spiral are synonymous terms, signifying the length of barrel through which the shot would be compelled to move in making one complete revolution. The length of turn, however, is considered apart from the length of the bore of the gun, which may be of such a length as only to admit of the grooves describing a portion—such as a half, or a quarter, of a turn; for instance, the bore of the gun may be only two feet in length, while the length of the turn is four feet—in which case the gun would be said to have half a turn. In general, in speaking of the turn of the grooves, we call that a "great turn" which gives a great rotary velocity to the shot, or will cause it to make a complete turn in a short space; a small turn is productive of an opposite result.

being of proportions adverse to its stability, or any other cause, will necessitate the use of a greater turn, proportional to the additional velocity of rotation required to be given. Thus a greater or less rotary velocity will have to be given according to the position of the centre of gravity; since the tendency of the shot to turn over in its flight depends greatly on the position of the centre of gravity, the tendency being less when the centre of gravity is forward.

Each of these circumstances unquestionably affects the flight of the shot, and therefore exerts more or less influence upon the turn. Notwithstanding this fact, we frequently find persons advocating the employment, some of a greater and others of a less turn, either as their fancy seems to dictate, or from considerations altogether unconnected with the form of the bullet.

No one, I think, will deny that of two bullets, the one which requires the least angular velocity consistently with the attainment of a given result is that which presents the greatest advantages. A large angular velocity requires a sharp turn of the grooves, in consequence of which the velocity of the shot is diminished, both in consequence of the friction, properly so called, against the grooves, and the resistance of the grooves, whereby the shot is constrained to move in a spiral, instead of directly forward. The strain upon the gun, the recoil, and the tendency of the projectile to fracture or to *strip*, are also thereby considerably increased; hence, any advantages which might result from great angular velocity, imparted by means of a grooved bore, are so nullified by the increase of friction, that in some cases more is gained, in a practical point of view, by lessening the friction, especially with guns used solely for horizontal fire, than is lost by diminishing the angular velocity of the projectile.

When, however, a great velocity of translation is given to a shot without also imparting to it a sufficient turn, the rotation, diminishing with the time of flight, becomes at last insufficient to preserve the coincidence between the axis of the shot and the line of flight, and therefore accuracy of fire at long distances is rendered impossible. This is a result which must, of course, be avoided, since with rifled guns great accuracy is the first thing to be secured. Extent of range, which is the next important point, will also be diminished, unless the turn be sufficient to maintain the flight steady.

The superior range and accuracy of the Whitworth, as compared with the Enfield bullet, is due, not alone to its small diameter and undiminished weight, but to its also having a rotary velocity (a turn in twenty inches) in a proportion suitable to its form and length.

For shot of great length a great turn is absolutely necessary. I made frequent experiments with shot, ranging in length from one and a half to between four and five diameters, in order to ascertain certain points in connection with this circumstance, and I invariably found that the greater the length of the shot the greater was the turn that was required for the grooves.

In my experiments I found that shot of great length turned completely over on leaving the muzzle of the gun, although the turn was sufficient to keep a shot of less length perfectly straight. This fact, I find, is fully corroborated by Mr. Whitworth in his paper on rifled fire-arms (*On Mechanical Subjects*, p. 77); in which he states that the length of turn (6 feet 6 inches) used with the Enfield rifle, and which is found to give the bullet the rotary velocity necessary to keep it straight, was so inadequate for a bullet nearly double the length of the ordinary bullet, that the former turned over within a distance of 6 feet from the muzzle of the gun.

From this it must be inferred that the *stability* of a long projectile of a given diameter is diminished by an increase in its length; and, therefore, that it will require a greater rotary velocity than one of shorter length to keep it straight. This is perfectly in accordance with the mechanical laws, for the greater the length of a body, in proportion to its diameter, the more unstable will be its equilibrium; that is to say, a smaller amount of force will be necessary to disturb it. Thus, in spinning two tops of the same diameter, one of which is three times the length of the other, the longer will require the greater rotary velocity to preserve its equilibrium; but, the equilibrium once disturbed, the movement about its smaller axis will be less rapid than the movement made by the smaller under similar circumstances.

To obtain the best results, therefore, with rifle projectiles, it appears, for various reasons, that their length must be limited.

The greatest length which can be used with effect appears, from experiments which I have made, to be about three times the diameter of the shot. When the shot were longer than this, I found the range that could be obtained with them was diminished, on account of the great turn and the low initial velocity it was necessary to give them. On the other hand, when their length was less than this, the range was also diminished, in consequence of the smaller weight of the shot, and the greater comparative effect of the resistance of the air; on the whole, therefore, one of three diameters in length has sufficient advantage over one of two diameters to compensate, in most cases, for the greater turn required for it. For small arms, however, the circumstances of its length and the greater turn may render it practically less fit for service than a bullet of the size and form of that now in use.

The turn of the grooves, whether for rifled ordnance or for small arms, will depend, not only upon the description of projectiles employed, but also upon the nature of the service for which such arms are required; being greater or less according as the gun is intended to be used with a high or low elevation.

It is clearly established by experiment, that a velocity of rotation which is sufficient to keep the flight of a shot true for a given range, may be quite insufficient for the purpose when the elevation is increased. This probably arises from the circumstance, that the friction of the air on the shot continually diminishes its velocity of rotation, so that, when the time of flight is great, the velocity of rotation may be reduced to an amount quite insufficient to secure steadiness of flight. When, therefore, the elevation of a gun is great, and the time of flight increased, a greater turn will be required for the grooves, in order to secure sufficient velocity of rotation throughout the flight of the shot.

With the military rifle, forty years ago, one turn in 10 feet only, or a quarter turn in a barrel 30 inches long, and a very heavy charge of powder were used; and it was found, upon trial, to shoot as accurately at 100 yards, as others with four times the turn; but the latter were found so immeasurably superior to it at all distances beyond this, that the turn was afterwards considerably increased. (*Scloppetaria*, p. 82–86.)

I will now proceed briefly to consider the question as to whether—with a gun of a given calibre—a different *velocity of projection* would affect the turn; and I think that if we attentively examine the causes which exist for giving to shot a rotary velocity, it will be found that the rotary movement being obtained by means of a grooved bore, the velocity of projection will not, necessarily, affect the length of the turn, as we might at first be led to imagine, that is to say, provided a suitable turn be given to the rifling in the first instance.

For the velocity of rotation is always in exact proportion to the velocity of projection; and although the resistance of the air increases in a higher ratio than the velocity, yet, with any increase in the velocity of projection, the *vis viva* of rotation increases in the same proportion as the pressure of the air, and therefore the same turn will always be sufficient to keep the shot steady and true.* Circumstances even occur in practice when nearly as great a turn may be required for a low as for a high velocity of projection. Thus, when the

* See next chapter, p. 57.

velocity of projection is small, the flight of the shot becomes more incurvated, and high elevations usually accompany low velocities (as with mortars), in which case the time of flight is also extended; both of which circumstances conduce to the employment of a great turn.

Rifled projectiles fired from mortars would generally acquire moderate and even low velocities; but a comparatively great turn will be necessary, on account of the great elevation at which they are always fired, to enable them to maintain the accuracy of their flight to the full extent of their range; for the velocity of the projectile has its least value soon after passing the vertex of the curve of flight, and during the descent of the shot its velocity will continually increase, whilst its rotary movement will continually diminish.

For horizontal firing, on the other hand, great range and force of impact being an object, it would be necessary to employ the highest possible projectile velocities. With guns of heavy calibre, this would be promoted by the use of a shorter projectile, and a comparatively smaller turn; the *weight* of the projectile being maintained by increasing the diameter of the bore.

With guns of large calibre, the greatest range at low elevations may be obtained in the manner above mentioned; but, if fired at the higher elevations, the flight of the projectile will not by any means be so accurate as that of a longer shell fired with a greater turn and longer velocity; nor will its range at the higher elevations be so good.

This proceeds both from the better sustained velocity of the longer projectile, and from the fact that, when the turn is sufficient to keep a projectile steady throughout its whole flight, the surface exposed to the direct resistance of the air becomes greater in proportion as its flight becomes unsteady; and this not only causes deflection, but also diminishes the velocity.*

Thus we obtain widely different results, according as we give, on the one hand, a considerable turn and a low velocity to a shot, or, on the other, a slight turn and a greater velocity. If, therefore, it were found expedient to employ both these methods, care should be taken that neither be exaggerated; as in the one case the range itself, and in the other the range at which a proper degree of accuracy could be attained, would be too much diminished for practical purposes.

* From the circumstance that the actual range and force of impact of shots which have a rapid rotary motion imparted to them, is frequently greater, from their steady flight, than that of others which have only a slight rotary motion, has probably arisen the absurd idea that a violent rotary motion causes the shot to penetrate, or *bore* its way into hard substances.

## ON THE MANNER IN WHICH A DIFFERENCE IN THE CALIBRE INFLUENCES THE TURN OF THE RIFLING.

This question, if not the *first*, is one of the earliest to which attention should be directed; for, certainly, no experiments connected with the mechanical part of the application of the principle of the rifle to cannon can possibly be productive of satisfactory results, until the effect actually produced on the flight of the projectile by an alteration in its *size* be first clearly ascertained.

The difficulty of estimating the exact amount of angular velocity required for shot of different forms, and the effect of the action of the air upon them, preclude the possibility of any determinate length of turn being assigned beforehand, by theory, for any particular kind of shot, this can only be learnt by experiment. For shot of different diameters, but of similar forms, a graduated scale may, however, be formed; since in whatever manner a difference in the diameters of shot may influence the turn of the rifling, it will always be in a fixed ratio.

As, therefore, some modification of the turn will be found necessary in all cases where the shot materially differ in form, length, etc., it must be supposed, before entering into any explanation respecting the relative turn to be employed for guns of different calibres, that the description of projectile, and the turn most suitable for it when fired from a gun of a given calibre, have first been decided upon. Assuming these to be already ascertained, I propose to inquire what turn should be given to a bullet similar in all other respects, but of different diameter. Lest, however, the propositions which I am about to advance should be thought to savor too much of theory, I may observe, that all the arguments made use of have been suggested, and all the conclusions confirmed, by a long course of practical experiments.

Hitherto, in rifling cannon, two extreme methods appear to have found most favor: one consisting in giving the grooves a length of turn of about the same number of calibres as in a rifle carrying an ounce ball; the other, in giving them a turn of scarcely greater length than that used for small arms; no satisfactory reason, that I have ever heard of, being assigned for either system. The advocates of both these methods appear to overlook the fact that the circumstances attending the projection of the shot, the resistance of the air, time of flight, range, velocity, etc., will all be in a different ratio to each other with a gun of large calibre, to what they are in one of small calibre, and must therefore be considered.

Others, again, are of opinion that the length of the turn should depend, in some measure, upon the length of the *gun;* that a quicker turn should be used with a short, than with a long gun. This is clearly a fallacy; for if the shot would acquire a sufficient rotary velocity when fired from a long gun with a less turn, it would not require a greater when fired from a short gun. The length of turn must depend alone upon the rotary velocity required for the shot; and the comparative turn for shot which differ in size only will depend entirely upon the comparative influence of the air upon them.

All who have any knowledge of the science of gunnery are aware that, the velocity of projection being the same, large shot range further than small ones, *cœteris paribus*.

This circumstance is noticed and explained by Robins in his "Tracts on Gunnery." At page 256, he observes, that: "A 24-pounder loaded in the customary manner, and elevated to 8°, ranges its bullet at a medium to about a mile and a half; whereas a 3-pounder, which is half the diameter, will, in the same circumstances, range but little more than a mile; and the same holds true in the other angles of elevation; though, indeed, the more considerable the angle of elevation, the greater is the inequality of the ranges. Now this diversity in the range of unequal bullets cannot be imputed to any difference in their velocities, since, when loaded alike, they are all of them discharged with nearly the same celerity; but it is to be altogether ascribed to the different resistances they undergo during their flight through the air: for, though a shot eight times the weight of another has four times the resistance, yet, as it has eight times the solidity, the whole retarding force which arises from the comparison of the resistance with the matter to be moved will be but half as much in the larger shot; and thus it will always happen (whatever be the size of the shot) that the retarding force of the air on the lesser shot will be greater than the retarding force on the larger, in the same proportion as the diameter of the larger shot is greater than the diameter of the lesser."

We have, therefore, to consider, in rifling guns of large calibre, in what manner the relative increase in the weight and size of shot will affect the turn, this being absolutely the only point on which those projectiles of the same form and density will differ.

A long shot fired in a vacuum would in general require no rotation at all; for although it would always turn over in its flight, still, there being no resistance to its flight, this would not be attended with an injurious effect on its velocity or direction; the practice would be equally accurate whether the gun were rifled or not. It would, under such circumstances, only become necessary to rifle the gun in order that the projectile might always present the same end foremost; but a small velocity of rotation would probably suffice for this purpose.

A round shot fired in a vacuum would require no rotary velocity whatever, for it is only the resistance of the air, acting in an oblique direction, in consequence of the rotation of such shot about an uncertain axis, which causes the deflection always noticed in practice.

The resistance of the air being, therefore, the chief cause which renders the rifling necessary, whatever tends to lessen this resistance, or its effect on the shot, will allow a corresponding diminution to be made in the velocity of the shot's rotation.

In estimating the amount of resistance offered by the air to shot of different diameters, it is a common error to take into account the action of the air upon the projectiles at the first instant of their motion only; or, in other words, it is generally believed, and often stated, even by able writers, that a shot double the diameter of another, and therefore having half the extent of surface as compared with the weight, will only suffer half the amount of resistance from the air.

In order to show the error of this opinion, let us suppose two shots of similar form and the same density, but the diameter of one double that of the other, fired with the same initial velocity. It is then perfectly true that, at the first instant of projection, the resistance of the air on the larger shot is four times that on the smaller; and since the weight of the larger is eight times that of the smaller, the larger will be retarded only *half* as much as the smaller. In consequence of this, its velocity is maintained for a longer time, so that at every other instant of its flight it will be greater than that of the smaller, and the resistance of the air will therefore be *more* than four times as large; or, in other words, the total resistance of the air will be relatively more than half what it is on the smaller, and therefore not in proportion to the difference between their weights and surfaces. So that, if the angular velocity or turn of the rifling were reduced one-half in a shot twice the diameter of another, it would not be sufficient to meet the additional resistance arising from the greater mean velocity of the larger shot; or, in other words, the *vis viva* of rotation would not be in the same proportion to the *vis viva* of translation, or to the resistance of the air, in the larger, as in the smaller shot.

By having the turn in the ratio of the diameter of the bore, or of the same length, in calibres, the rotary impetus is in proportion, neither to the quantities of motion, nor to the quantities of resistance in shot of different sizes; nor does it secure an equal degree of stability to each.

Instead of the proportion thus shown to be erroneous, theory and practice both point to the conclusion that the proper degree of rotary velocity, which should be given to shot of different sizes, but of the same form and density, should vary very nearly as the *square roots of their diameters.* This I will now proceed to establish.

In considering the question of the rotary velocity required for shot of different diameters, it must be borne in mind that the rotary and progressive movements are separate and distinct from each other; and that when the velocity of progression is constant, the rotary velocity will vary with the length given to the turn of the rifling.

Now, it is evident that the proper degree of angular velocity for maintaining the stability of a shot of a given size and form will not be the same for a shot of a different size, since the conditions alter with the size.

But whatever the size of the shot, the resistance of the air to its progressive movement will always be in proportion, not only to the surface and square of the velocity, but also to the *vis viva* of the shot; for although, when the initial velocity is constant, the resistance of the air, from the smallness of its surface as compared with its weight, is comparatively smaller upon a large shot when it first leaves the gun, yet the retarding force of the air upon it during its *whole* flight will depend upon the *vis viva*, or relative quantity of work accumulated in the shot at the moment when it quits the gun. It remains, therefore, to ascertain the degree of angular velocity, or length of turn, necessary to secure in shot of different sizes the same relative amount of *vis viva*, or work, in their rotary as in their progressive motion.

Now, supposing the same initial velocity to be used, and the *vis viva* of translation, therefore, to be relatively the same for each shot, if the length of the turn remain constant, the angular velocity will be the same, however large the diameter of the shot; but the *vis viva* of rotation will differ with the diameter: for instance, in a shot twice the diameter of another in size, the *vis viva* of rotation—in obedience to the laws which govern rotating bodies—will be in the proportion of 16 to 1, or relatively twice as great; and therefore would be greater than required. If, on the other hand, the turn be of a length in proportion to the diameter of the shot, the angular velocity will then be reduced one-half; that is to say, the larger shot will make only one revolution in the same space of time in which the smaller revolves twice; but the *vis viva* of rotation will be in the proportion of 4 to 1 only, or *relatively one-half* that of the smaller, which is *less* than is necessary.

Now, seeing that relatively twice the *vis viva* of rotation is produced in a shot twice the diameter of another by the same angular velocity, and that relatively one-half only is produced by half the angular velocity; it follows, that, to produce relatively the *same vis viva* of rotation, the *square of the angular velocity* (and not the angular velocity) must be one-half less in the larger shot. The angular velocity of the large shot must, therefore, be reduced in the proportion of 1 to $\sqrt{2}$, and the length of turn for giving this velocity will consequently be in the proportion of $\sqrt{2}$ to 1, or in the ratio of the square root of the diameter of the shot. By maintaining this proportion the same stability to the shot will always be secured, as the *vis viva* of rotation will always be in the ratio of the *vis viva* of translation, and the retarding force of the air, in shot of all sizes; and although the turn of the rifling becomes comparatively greater as the calibre of the gun is increased, the angular velocity of the shot is diminished in proportion, and *relatively the same force will be expended in guns of every calibre in giving the rotary movement to the shot.*

Again: we find (from a proposition demonstrated by Professor Euler, and which may be regarded as one of the few *certain* rules we possess with regard to the flight of projectiles), that "*bodies of the same density and form, projected with the same elevations, and with velocities as the square root of their diameters, will describe similar curves, as the resistance will be in the ratio of their quantities of motion:*" a fact which shows that if the resistance of the air varies as the square of the velocity, it is also in the ratio of the relative *vis viva* of the shot.

As the resistance of the air upon shot of different diameters, projected with velocities as the square root of their diameters, is in the ratio of their quantities of motion, it follows that the rotary impetus should also be in the same ratio; consequently, for the quantity of rotary motion to be in the ratio of that of the progressive motion—that is, for the large shot to make one revolution in the same space of time, or have the same angular velocity as the small, when fired with the above velocities—the length of the turn must be increased in the same proportion, or as the square root of the diameter of the shot.* If, then, the turn be sufficient in one instance, by following this method, *the angular velocity* (whatever the velocity of translation) *will always be in the same ratio to their comparative resistances with shot of all sizes.*

Another method of computing the comparative effect of the air upon shot of different sizes is to compare their *terminal velocities.*

A shot descending through the air solely under the influence of gravitation, will gradually increase in velocity until it meets with a resistance from the atmosphere equal to its own weight; the impelling force and resistance being then equal, it will continue to descend with the same uniform velocity, which, of course, will differ according to the weight and diameter of the shot, but which is in every instance called its "terminal velocity."

It has been computed that the terminal velocities of shot are proportional to the square roots of their diameters nearly; thus the terminal velocity of a 3-lb. shot is about 290 feet a second, and that of a 24-lb. shot (which has twice its diameter) about 420 feet (Hutton,

* In fact, if $\omega$ is the angular velocity, and $v$ the linear velocity of the shot, and $l$ the length of the turn of the grooves, $\omega = \frac{2\pi v}{l}$, and therefore $l \propto v$ when $\omega$ is constant.

*Tract* 37, *Problem X.*); so that if these two shot, in falling through the air, had each attained its terminal velocity, the smaller would continue to descend with a velocity of about one-third less than that of the larger, the retarding force of the air being, relatively, really so much greater upon it.* It should be noticed, however, that the shot in falling would never actually acquire this terminal velocity, but one which would approximate closely to it.

It is observable in practice, that the ranges of round shot of various sizes, when fired with ordinary charges and similar elevations, will, as a general rule, be found proportionate to the terminal velocity of each shot.

Supposing long projectiles of the same form, but of different sizes, always to move point foremost, their comparative terminal velocities will bear the same ratio to each other as those of round shot, provided the descent of each be equally steady; it is for this reason that the perfectly steady flight of elongated shot is of so much importance, for a want of steadiness not only causes more or less deflection in the projectile, but will considerably diminish its range.

The following is a table of the different terminal velocities of round shot, of various sizes, taken from

* When shot, similar in form, but of different sizes, are fired with the same initial velocities, the larger will always maintain a superior degree of velocity. When, therefore, the weight of the gun is limited, and the circumstances under which it can be used conveniently admit of the employment of greater elevations, the advantages—especially with the heavier kinds of ordnance—are enormously in favor of a heavy, over a light, projectile, although the latter be used with a greater initial velocity; as (except at short distances) a greater proportionate effect can be produced with a larger shot, and a lower initial velocity.

Hutton's Tables (*Tract* 37, *Problem X.*); to which I have added their respective ranges, when fired with 5° of elevation, and with charges equal to one-third of their weight; that is to say, with initial velocities of about 1,800 feet a second.

| Weight in lbs. | Diameter in inches. | Terminal Velocity in feet. | Range at 5° in yards. |
|---|---|---|---|
| 1 | 1·928 | 247 | 1,100 |
| 2 | 2·423 | 277 | 1,210 |
| 3 | 2·773 | 297 | 1,300 |
| 4 | 3·053 | 311 | 1,400 |
| 6 | 3·494 | 333 | 1,520 |
| 9 | 4·000 | 356 | 1,650 |
| 12 | 4·403 | 374 | 1,700 |
| 18 | 5·40 | 400 | 1,780 |
| 24 | 5·546 | 419 | 1,850 |
| 32 | 6·106 | 440 | 1,950 |
| 42 | 6·684 | 461 | 2,050 |
| 68 | 7·95 | 530 | 2,240 |

The terminal velocities of spherical shells are to those of spherical solid shot of the same diameter, in the ratio of 1 to $\sqrt{1·5}$.

The terminal velocity of a long shot, as compared with that of a round shot of the same diameter, will be about as the square root of the weight; and the additional range acquired by the long shot should be in about the same proportion.

An Enfield bullet, which, from its form and density, has very nearly the same terminal velocity as a 1-lb. iron round shot, has also a similar range when fired with the above elevation.

In order to illustrate the method here proposed for finding the proper length of turn for guns of various calibres, we will suppose that it is required to find the proper length of turn for a gun with a bore 4·2 inches in diameter, it having been previously ascertained that a turn in 64 inches is that most suitable for a gun having a bore of 1·2 inches in diameter; the shot used in each case being of similar construction.

It has been shown that the length of the turn should be increased in proportion to the square root of the calibre, so that the rule for finding the proper turn may be expressed at length in words, as follows: divide the larger diameter by the smaller, extract the square root of the quotient, and multiply the quantity thus obtained by the length of the turn.

In order, therefore, to find the length of turn required for a gun with a calibre 4·2 inches in diameter, which is to be used for firing a similar shot or shell as another with a bore 1·2 inches in diameter, for which the proper length of turn has already been ascertained to be 64 inches, divide 4·2 by 1·2. The quotient is 3·5, the square root of which—1·87—multiplied by 64, gives the length of the turn 119·7 inches, or ten feet within a fraction. The proper turn for guns of any other calibre may be found in a similar manner.

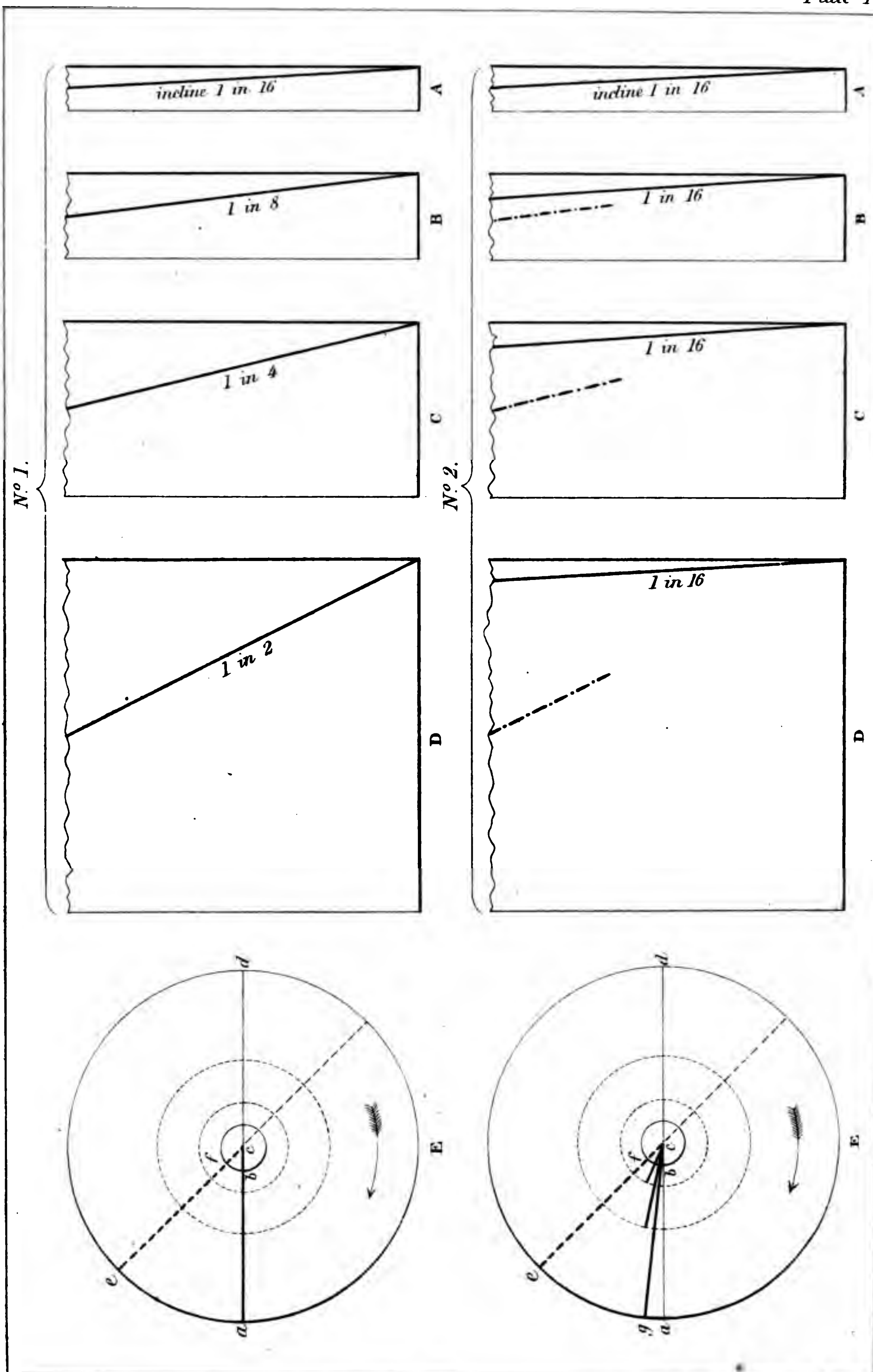
The following is a scale of the different lengths of the turn required for guns of different calibres, according to the above method, taking the Enfield rifle as a standard, and supposing leaden shot of a form similar to those used with that rifle to be employed:

| Description of Guns. | Diameter of Bore. | Length of turn for Leaden Shot. | |
|---|---|---|---|
| 6 pounder | inches. | ft. | in. |
| 9 " | 3·66 | 16 | 4 |
| 12 " | 4·2 | 17 | 3 |
| 18 " | 4·62 | 18 | 3 |
| 24 " | 5·29 | 19 | 3 |
| 32 " | 5·82 | 20 | 6 |
| 42 " | 6·41 | 21 | 6 |
| 56 " | 6·97 | 22 | 6 |
| 68 " | 7·65 | 23 | 6 |
| 8 inch | 8·12 | 24 | 4 |
| 10 " | 8· | 24 | 3 |
| 12 " | 10. | 27 | 0 |
| 13 " | 12· | 29 | 6 |
| | 13· | 30 | 6 |

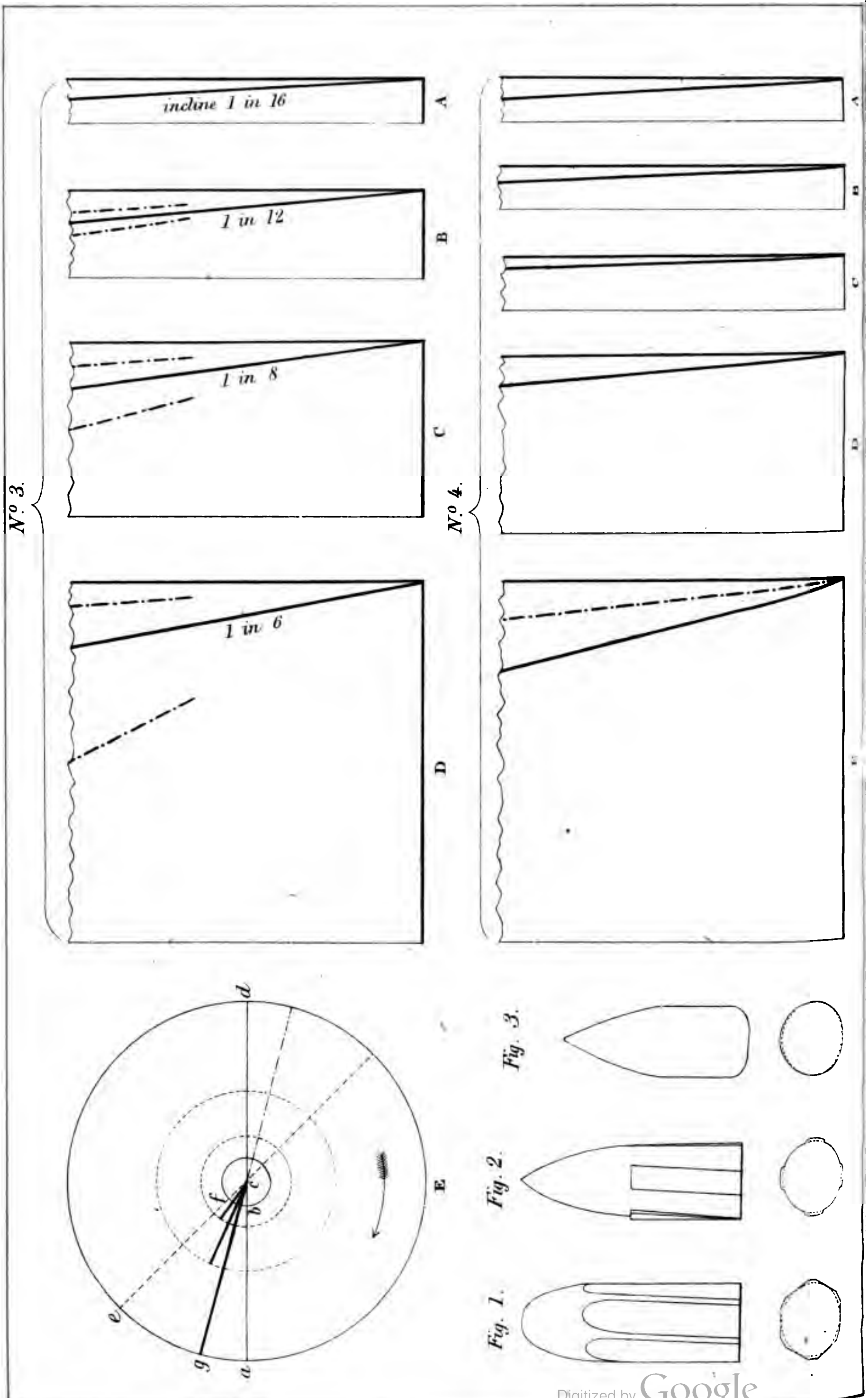
I am desirous of correcting an error which, in common with all who have written on the subject, I formerly entertained; namely, that the *density* of the projectile will influence the turn of the rifling.

When a shot is of the same size as, but of greater density than another, it is erroneously said that the resistance of the air upon it is *less*, whereas—from the higher mean velocity of its flight—it has to encounter a *greater* resistance from the air; in fact, when the initial velocity of their projection is the same, the resistance of the air is *relatively the same* on shot which differ in density.

For instance, if two shot of the same size and form, but differing in density, were fired with equal velocities, rotary as well as progressive, the resistance of the air, it is true, would not be relatively the same—that is, in proportion to their densities—on each shot at the moment of projection, but the higher mean velocity which the denser shot would acquire would give rise to a pro-



incline 1 in 16
A
1 in 8
B
1 in 4
C
Nº 1.
1 in 2
D
incline 1 in 16
A
1 in 16
B
1 in 16
C
Nº 2.
1 in 16
D
d
e
f
c
b
a
E
d
e
f
c
b
g
a
E


incline 1 in 16
1 in 12
1 in 8
1 in 6
A
B
C
D
E
Nº 3.
Nº 4.
Fig. 1.
Fig. 2.
Fig. 3.
a
b
c
d
e
f
g

portional increase in the quantity of resistance which would render the employment of a greater turn necessary, were it not that the greater density causes a proportional rotary impetus to be maintained; so that the same turn will be sufficient to allow the denser shot to maintain the necessary degree of stability throughout its more prolonged flight.

Increased density, in fact, although of great importance in giving to projectiles greater range, force of impact, and accuracy at a given distance, does not affect the turn in the manner usually supposed; for the turn required for shot of greater density than others must be such as will give them an equal stability or steadiness of flight at the greater distances to which with equal elevations they will range. Hence, when shot have their forms, length, and general conditions alike, the *vis viva* of rotation should always be in proportion to that of translation. To diminish the turn, therefore, as usually advocated for shot of a greater density than others, is manifestly incorrect.

A proof of the general correctness of these views is afforded by the fact that, although their weights, as compared with their diameters, are greatly increased, a much greater turn or rotary velocity is required for shot of greater, than for others of less, length.

The diagrams in Plates 1 and 2 are intended to represent the different degrees of rotary velocity acquired by the shot, and the effect produced in the gun by an increase in the calibre, accompanied by turns of different angles.

In order, however, to render the diagrams intelligible, it should be remarked that the figures A, B, etc., represent portions of barrels of different bores, cut through in a longitudinal direction, and then laid open, so that instead of a cylindrical, they shall present a flat surface. This suggests itself as the easiest method of showing, in the most distinct manner, the angle which is formed by the inclination of the turn.

As we may suppose that the proper angle for the turn has already been discovered and applied in various instances—as, for example, in the case of bullets upon Mr. Whitworth's principle, which require (with a calibre of 45 inches) a turn 20 inches in length; Colonel Jacob's, which requires a turn of 3 feet; and that of the Enfield musket, which has a whole turn in 6 feet 6 inches only—we may infer that each has its most suitable angular velocity imparted to it, since it is only reasonable to believe that each has been amply tested by expeirment. Under this impression, therefore, we may fairly assume that the turn for bullets of the diameter of those just mentioned will, for practical purposes, vary between those of Mr. Whitworth, as the *maximum* turn, and those of the Enfield musket as the *minimum*. As in these diagrams it is necessary for the purpose of illustration to have a standard, I have taken the angle (or nearly so) of the turn for Mr. Whitworth's bullet, or the *maximum* turn, as most suitable for the object in view.

Of these diagrams, then, figure A, in Plate 1, represents the angle formed with the axis of the bore by a turn of 2 feet, in a barrel of a half-inch bore; figure B, the angle formed by a turn of the same length, in a barrel of twice the diameter; and figures C and D, angles formed by turns of a similar length, in guns increasing in the same ratio. Of this diagram it need only be remarked, that shot fired from guns varying in their bores, in the relative proportion which is here shown, will all acquire the *same* angular velocity—supposing the velocity of projection to be the same in each instance.

In Plate 2, diagram No. 2, figure A represents a barrel of the same bore, with a turn of the same length as that of figure A in No. 1. Figures B, C, D represent portions of barrels of similar size to those denoted by the corresponding letters in No. 1; only here, the *angles* formed by the turn are the same in each figure. In this case, the angle of the turn being the same in each instance, it will be seen that the angular velocity will be diminished inversely as the diameter of the shot is increased.

In Plate 2, diagram No. 3, the figures A, B, C, D represent portions of barrels corresponding in size with those denoted by the same letters in the two former diagrams. Here figure A shows an angle of the same magnitude as in the former cases; but figures B, C, D show the magnitude of the angle when the length of the turn increases as the square root of the calibre.

In diagram No. 4 are shown the different angles formed by the turns adopted for several kinds of shot of various sizes—A, Mr. Whitworth's; B, Colonel Jacob's; C, the Enfield musket; D, Mr. Armstrong's 2-inch gun; and E, Mr. Whitworth's rifled 4-inch gun. The dotted line represents the turn employed for a 4·2-

5

inch gun rifled at Woolwich (after the author's design), for experimental purposes.

In the diagrams 1, 2, and 3, the figure E is intended to represent the relative angular velocity which would be acquired by rifled projectiles, of different sizes, supposing them to be fired from guns with turns which vary in the proportion of those shown in figures A, B, C, and D, in each diagram.

To explain this figure, a line, *a*, *b*, *c*, *d*, is drawn through the common centre of all the circles, and intersected at *c* by another line drawn at any angle, as *a*, *c*, *e*.

In figure E, No. 1, the angular velocity produced by the different turns being in each case the same, the largest shot (the velocity of projection being the same in each case) will describe the arc *a*, *e*, in the same space of time as that in which the smallest will describe the similar arc *b*, *f*.

The angular velocity given by the different turns shown in No. 2 being diminished in a proportion as the diameter of the shot, the largest shot will describe the arc *a*, *g*, in the same time as the smallest will describe the arc *b*, *f*.

In No. 3, where the angular velocity of the different sized shot varies as the square root of their diameters, the largest shot describes the arc *a*, *g*, in the same space of time as the smallest describes the arc *b*, *f*. In this case it will be seen that although the inclination of the turn is considerably increased, the angular velocity of the shot is diminished.

In figure E, in each diagram, the relative angular velocity of four shots of different sizes is given; but it appears needless to institute a comparison between more than two, inasmuch as these are sufficient to illustrate the relative angular velocity of each shot.

Though a large gun rifled with a much smaller turn than would be given by the method I have described, may project a shot with the same accuracy that would be attained with a gun of smaller calibre, at equal distances, yet this is by no means a proof that such a turn is sufficient to give the larger shot the proper degree of rotary velocity throughout its flight, when the elevation is increased. A large gun should throw its shot with the same precision as the Enfield rifle (or any other which may be taken as a standard), not at the same distance only, but at the ranges which would be acquired by the same elevation of the gun in each case; that is to say, a gun with a bore 4·2 inches in diameter (or 9-pounder) should throw an elongated shot—the weight of the shot and the charge of powder being proportionate to that used with the Enfield musket—to a distance of about 2,000 yards, with the same accuracy as that which is attained by the latter at 900 yards, the degree of elevation at which these respective changes should be acquired being nearly the same for each weapon. Until this or an approximate result be obtained, the proper method for rifling cannon cannot be considered to be established.

In a small work of this kind it is impossible to enter into a complete investigation of a subject which involves so many considerations; all I have attempted is to give a general outline of the method which appears to me to be the correct one of applying the principle of the rifle to cannon, and I will conclude my remarks upon it by briefly summing up the principal points which I consider worthy of attention.

First, then, as a general rule, we find that the length of the turn of the rifling in guns of different sizes should be in the ratio of the square roots of the diameters of the bores of the guns; as the rotary motion will then always bear the same proportion to the progressive motion and to the resistances with shot of all sizes; thus securing the same relative degree of stability to each shot, and relatively the same expenditure of force only in giving the rotary motion to each.

In the second place, we find that the turn will, accordingly, be greater in proportion to the size of the gun as the diameter of the bore increases, thereby causing it to have an increased angle in large guns, in a ratio about at the square roots of their calibres;* but as the shot in large guns, fired with relatively the same charges of powder, is a longer time, and has to pass through a greater distance in acquiring its velocity, the comparative strain upon the gun is no greater on this account.

Thirdly: that, the form of the projectile having, next to the length, the chief influence on the turn, that form is preferable which will require the least turn; it

* Let $a$ be the angle that the grooves of the rifle make with the axis of the gun, and $l$ the length of the turn, $d$ the diameter of the bore; then $\tan. a = \frac{\pi d}{l}$. But the rule here explained and proved gives $l \propto \sqrt{d}$, whence $\tan. a \propto \sqrt{d}$; and since the angle of the grooves is generally small, $a \propto \sqrt{d}$, nearly.

For this, and the formula given in page 58, as well as for many valuable suggestions, I am indebted to my friend Mr. T. B. Sprague, Fellow of St. John's College, Cambridge.

being important to avoid, as much as possible, increasing the angle, in proportion as the size of the gun is increased.

Lastly, it may be remarked of projectiles of a given length, that those which require the least turn are, first, those of the most perfect concentricity, and the surfaces of which offer the least resistance to the air, thereby suffering the least impediment in their rotary motion; secondly, those which have their centre of gravity in their fore part, as their stability is greater, and consequently they require less turn to keep the axis about which they rotate, steady; thirdly, those of which the form approaches more nearly to a cylinder, as in them the accumulated work (due to the rotation) is greater than in any other form of shot of the same diameter and length, and they consequently require a less turn than is the case when they are made in the form of a lengthened cone. The less prominent the foregoing conditions appear in shot, the greater will be the turn required, although some of them have a greater influence upon the turn than others.

It only remains for me to suggest a simple course of experiments, by which the principle I have already enunciated may be tested and established; and I may add, by the way, that such experiments as I would propose, in order to ascertain the proper angular velocity which should be given to shot, would be rendered, by a fixed system of conducting them, much less complicated and expensive than any vague series which might be undertaken without reference to facts derived from scientific conclusions, and in the vain endeavor, by an endless and uncertain process, to arrive at any satisfactory or conclusive results.

It must, indeed, be obvious to all who have a practical knowledge of this subject, that such experiments as these would be interminable, and that they would involve an endless expenditure, both of time and money, before any satisfactory data could be obtained, supposing it possible that such could *ever* be thus acquired.

In reference, moreover, to the experiments which I am about to propose, I would remark, that they may be made with any kind of shot similar in form and weight. It would be of little consequence whether the shot were of solid iron, or a compound of iron and lead; for it must be remembered that these experiments would be solely for ascertaining the exact ratio according to which the length of turn, as dependent upon the size of the shot, should be increased or diminished; which ratio will of course be always the same, of whatever material the shot may be formed.

I will now proceed briefly to notice the manner in which the experiments should be conducted.

For this purpose, then, I would propose to have a certain number of guns—say seven 12-pounder brass howitzers, bored each with a 2-inch bore, and rifled with grooves of the same form, but different turns. For these seven guns, the greatest turn should be 3, and the least, 16 feet in length; whilst the intermediate turns will be, respectively, 4, 6, 8, 10, and 12 feet. I give these different lengths advisedly, as they comprehend all the different degrees to which the turns used for small arms could be reasonably reduced, supposing such to have their bores increased to the above-mentioned 2-inch diameter.

Upon a general view of the question, it will, I think, be found that the most effective length of turn for any kind of shot, of this diameter, which can be available for practical purposes, will be somewhere between these two points (viz., the 3 and the 16-feet), provided the shot does not exceed three of its diameters in length. If, however, the shot selected for such experiments be of iron (*i. e.*, unexpanding), or more than two of its diameters in length, in that case the length of the different turns used for the above guns might be, respectively, 3, 4, 5, 6, 7, 8, and 9 feet.

Supposing, then, that the guns are bored and rifled according to the plan which I have suggested, I should deem it advisable that each should be fired with the same charge and elevation, and that this charge should be the greatest that would ever be practically used; also, that the elevation should be the highest which would be used in practice with such a charge.

I would give this elevation (say about 15 degrees), because it is proved that the length of turn which is suited to a shot fired with such an elevation, will be found to be amply sufficient for it when fired with any lower elevation, but not *vice versâ*. For it has been shown, by numerous experiments, that a given velocity of rotation may keep a bullet true for a certain distance, but that by elevating the gun, and thus increasing the *range*, a greater turn will be rendered necessary, and that the longer the shot, the more appreciable this fact will be.

Having in this manner ascertained what is the least, and therefore the best, turn which could be used for the shot, the next step will be to endeavor to arrive at the exact ratio of decrease in the angular velocity or inclination of turn which should be assigned to shot of similar form upon any increase of their size.

For settling this question, three, or at least two, guns would be necessary, and these might be 24-pounder howitzers, with bores 4 inches in diameter, or twice the size of those employed for the first portion of the experiment.

In rifling these guns, the same kind of grooves should be used as those employed in the first instance, but the turns given to them should be in the following proportions—there being, in fact, none other for which any reasonable grounds can be assigned.

The first gun should have a turn of the same length as that which has been decided, by the test of the last experiment, to be the best, provided it be not (for mechanical reasons) too great to use with the larger gun; and as it will be necessary to take some standard as an example, let us assume, for the sake of illustration, that one whole turn in 6 feet has been ascertained by the first part of the experiment to be the best.

Then the second gun (in reference to our assumed standard) should have a turn of 12 feet, or in a proportion commensurate with the increase of the diameter of the shot.

The third should have a turn of 8 feet 6 inches; that is to say, in a ratio as the square root of the increased diameter. These (two or three) guns should each be fired with the same elevations and proportionate charges of powder as those used with the seven smaller ones already mentioned.

By this simple course of experiments, the correct ratio, according to which the turn for guns of various sizes should be regulated, will be easily ascertained, and I am fully convinced that such experiments will firmly establish the truth of the principles which I have laid down.

In such experiments, however, as those to which I have just referred, although the difference between the turns of the three larger guns may not be apparently very great, yet the case will be vastly altered when the bores differ from each other more considerably in their diameters. Hence, therefore, in order to render the trial as satisfactory as possible, the difference between the bores of the guns used for the experiments should be made as great as they conveniently can be. The guns used by me in these experiments, although of smaller dimensions, differed more in the sizes of their bores than those just mentioned.

If, however, it be not convenient to make use of guns of a large size, it will be requisite that the correct turn should be ascertained in the first instance with a much greater degree of exactness. Thus, if 6 feet is found to be better than 4 or 8, still this does not determine positively that 6 feet is absolutely the best length for the turn, but only that it will be somewhere between 4 and 8 feet, so that it will be advisable to try an intermediate turn, such, for instance, as 5 or 7 feet, and so on, until the most correct turn may be as nearly as possible ascertained.

From this it will at once be seen, that if the carrying out of a single course of experiments relating only to one particular point, even when conducted upon a fixed system as simple as it can be made, may, when great exactness is required, necessitate the making a subordinate series of experiments; how infinitely complicated the matter will become when experiments are made, not only without reference to the settlement of any distinct point, but with the ill-advised intention of attempting to overcome every difficulty at once; and from this also some idea may be formed as to the incalculable loss, both of time and money, which would necessarily result in endeavoring to carry out experiments based upon no scientific grounds, conducted upon one fixed principle, and therefore liable utterly to fail in accomplishing the object which they have in view.

Since I have insisted so much upon the necessity of all experiments being conducted upon some fixed system, founded upon scientific, as well as practical data, I would now briefly state the grounds for my advocating that particular system which I have myself found by personal experience to produce the most satisfactory practical results.

It will be found that the length of turn to be given to the grooves will depend upon three points, viz:

First, *the description of shot.* On this account, therefore, any set of experiments undertaken for the purpose of ascertaining certain points connected with the turn of the grooves, must be made with the *same kind of projectile.*

Secondly, *it will change with every variation in their* *size.* Hence, in making the necessary experiments for ascertaining the turn required under these circumstances, the various conflicting arguments, in favor of the different ratios of variation in the angular velocity or turn required for the shot, should be taken into consideration and those for which any plausible reason can be assigned should be tried by a method similar to that which I have proposed.

Thirdly, *it will differ with every different elevation of the gun.* For this reason, as the turn tequired for great elevations is greater than that for small, in experiments for ascertaining the proper degree of turn, the gun should always be fired with the greatest elevation which would be used in practice.

It must also be noticed that the *velocity of projection* is to be considered; for when that velocity is greatly increased, it will not be safe to use the same degree of turn as may be employed with a lower velocity, as the shot will be liable to strip, or the gun to burst under these circumstances.

Therefore, in making experiments, the highest velocities which would be practically used, should be also employed in such experiments.

With regard, then, to the experiments which I have thus suggested, I would merely add that, whatever be the precise ratio of angular velocity to be given to shot, these experiments are best suited for its discovery; and when the proper ratio for the turn of the grooves is once ascertained, any further experiments which may be made need only be carried on for the purpose of testing what are the best mechanical appliances for improvements in the projectile, and in the form of groove, in order to reduce the friction as much as possible.

In conclusion, I would add, that as all matters connected with this subject may in reality be said to resolve themselves into a question of *friction*, and as the quantity of friction will depend chiefly upon the degree of turn, so that system for rifling ordnance will be the best which, whatever be the mechanical means used for giving the shot its rotary motion, will lead to the employment of such a turn only as will produce the least possible quantity of friction, and at the same time secure the greatest possible amount of practical efficiency.

It is to be remarked, that besides the *friction*, properly so called, of the shot against the grooves, there is that which arises from the *resistance* of the grooves, whereby the motion of the shot is altered from a simple forward motion to a rotary one. This increases rapidly as the turn of the rifle is increased, and all that has been said above as to the friction of the grooves, is applicable also to this action.

Reviewing the preceding observations, it appears impossible to apply any *strictly* accurate rules for the regulation of the length of the turn for guns of different calibres. As long as the problem of a shot's trajectory remains unsolved, no perfectly correct mathematical formula for the length of turn can be found; and even if we had such, it would be of small service, unless each gun were fired invariably with a fixed elevation; an extremely nice calculation would give a difference in the length for every elevation of the gun. Practically, therefore, we can only apply a rule based upon a general principle.

As we find that shot are affected by the resistance of the air in a ratio as the square root of their diameters nearly, and that their quantities of motion are relatively the same when the turn of the rifling is in the same ratio, we may consider that the ratio which has been assigned for the length of the turn for different guns is the best, as being the only one which is suited to meet all contingencies.*

As there have been one or two claimants to its formation, I am induced to state that this theory of giving relatively the same quantity of rotary motion to shot of different sizes, that is, proportional to their diameters, and to the relative amount of the disturbing and retarding influence of the air upon them, was first put forward by myself, and that no publication has as yet come under my notice which shows that this or a similar theory was ever previously put forward by any other person.

In 1854, wishing to have a large gun rifled, I endeavored in vain to obtain any information which might serve as a guide for this purpose—the rifling of all ex-

* The truth of this law has been exemplified in a remarkable manner. When Mr. Whitworth first attempted to apply his method of rifling a cannon, he employed a constant and very rapid turn for all guns. Previously, however, to the experiments which took place at Southport in 1860 with his new guns, and which were brought so prominently before the public by the *Times* newspaper, the attempts of that gentleman to apply his method to cannon had quite failed.

On the occasion I have alluded to, the greatest results were obtained with a gun having a 1·5-inch, and another having a 3-inch bore; the lengths of the turn of the rifling being for the former 40 inches, for the latter 60 inches.

Now, whether this was by accident or design, the turn used with each of these guns is very nearly what would have been assigned to them by the law I have put forward, taking 2 feet as a standard for a bore ·5 in. in diameter, or that of Mr. Whitworth's musket.

perimental cannon having been, apparently, regulated by caprice alone—no theory of any kind being in existence. This circumstance led me to make experiments on the subject, which resulted in my adopting the above theory as the most correct, as well as the most suitable for practical purposes.

# ON THE PROJECTILE.

Assuming that the principles laid down in the preceding pages are correct, it remains to determine the conditions to be observed in the construction of the projectile. It will be evident to all who have given their attention to the subject, that, to render an elongated projectile as efficient as possible, the following conditions are indispensable:

1. For solid shot, hardness and density; for shells, capacity for containing great bursting charges.

2. The projectile should be of a form productive of the least amount of friction in *passing* out, and offering as little resistance to the air as possible when it has *passed* out of the gun.

3. Its centre of gravity should be thrown forward before its centre of figure, in order to give it greater stability, and less inclination to turn over, and also to insure that the axis about which the shot rotates should always be a tangent, or nearly so, to its line of flight. The forward position of the centre of gravity will also allow of the use of a less turn in the rifling, which is an important object in guns of large calibre. There are also other reasons why the centre of gravity should be in the fore part of the projectile, which will be noticed hereafter.

For rifled projectiles of less than one and a half, or two diameters in length, the above condition is of no importance except in the case of shells used for vertical fire.

There is a circumstance attending the flight of all shot, when fired from rifled guns, which requires more particularly to be noticed where elongated projectiles are used: viz., when the centre of gravity in the shot is at, or behind, the centre of figure, the axis very soon ceases to coincide with the tangent to the trajectory; this not only impairs the range and accuracy of the shot's flight, but, when percussion shells are used, prevents them, when fired at great elevations, from falling point foremost upon the object aimed at.

Robins notices this in his remarks upon the rifle, where he says, "that though the bullet impelled from them (rifles) keeps for a time to the regular track with sufficient nicety, yet, if its flight be so far extended that its track is much incurvated, it will then often undergo considerable deflections. This, according to my experiments, arises from the angle at last made by the axis upon which the bullet turns, and the direction in which it flies; for that axis continuing nearly parallel to itself, it must necessarily diverge from the line of flight of the bullet, when that line is bent from its original direction; and when it once happens that the bullet whirls on an axis which no longer coincides with the line of its flight, then the unequal resistance described in the former papers will take place, and the deflecting power hence arising will perpetually increase as the track of the bullet, by having its range extended, becomes more and more incurvated." (See *a*, Fig. 5, *Frontispiece.*)

Robins proposed a remedy for this in his egg-shaped bullet; but from the remarks of the author of *Sclop-petaria*, it appears that it met with indifferent success, and that (owing, no doubt, to the difficulty of preserving the coincidence of the axis of the bullet, and that of the barrel of the gun) its flight varied very much. The same writer observes that this bullet, unlike others, always flew to windward. In the Enfield bullet, which possesses the advantages, without the defects of the egg-shaped bullet, the centre of gravity is thrown slightly forward, but hardly sufficient to give proper effect to shot when fired at great elevations.*

Some writers have contended that, whatever may be the position of the centre of gravity of an elongated shot fired from a rifle, its axis will always be a tangent to the line of flight; but this opinion is clearly erroneous. If the shot were projected in a vacuum, its axis would always be parallel to itself, notwithstanding the rotation of the shot about its axis. The resistance of the air has more effect on the light end of the shot than on the heavier end, and therefore will tend, when the centre of gravity is in the hinder part of the shot, to raise the point instead of to depress it; besides the resistance of the air, there is no cause that will operate to alter the position of the axis of the shot, and prevent its remaining parallel to itself, throughout the whole of its flight.

After a shot leaves the gun, the action of the air

* Appendix A.

effects it in a three-fold manner: in the first place, it offers a resistance against the fore end, caused by its flight in a forward direction, this being in proportion to the surface of the transverse section of the shot, and as the square of the velocity of its flight, nearly; secondly, there is a pressure upon the longitudinal surface of the shot; and, thirdly, it exerts a pressure upon its under surface, produced by the falling movement caused by gravitation, which is exactly the same as it would be subject to on falling to the ground under any other circumstances—as, for instance, from rest.

The result of these different forces is an oblique pressure upon the fore and under part, and is attended by a diminished pressure upon the upper portion of the shot's surface. (See Figs. 1 and 2, Frontispiece.) The direction of the pressure upon the under surface of the shot varies with the inclination of the longer axis to the vertical, and it is only when the shot rises or falls vertically that this pressure is solely upon the extreme end of the shot.

This oblique pressure tends greatly to increase the deflection of long projectiles, and its effects can only be reduced to a minimum by forcing the fore end of the shot continually in the direction of the pressure, so that the smallest possible surface of the shot is opposed to it. (See Figs. 3 and 4, Frontispiece.)

The best mode of effecting this is by diminishing the weight of the hinder part of the projectile, so as to allow of its having more length behind its centre of gravity. The pressure of the air, consequent upon the falling movement, will thus be greater upon that part of the shot behind the centre of gravity than upon its fore part, and so the former will be gradually retarded in its descent, and continually keep the longer axis of the shot coincident with the tangent of its line of flight.

It is a remarkable circumstance, that rifled projectiles, even those of the most perfect construction, are almost invariably deflected in the direction of the turn of the grooves; that is to say, with a right-handed turn, where the upper part of the shot turns from left to right, the deflection will be to the right, and *vice versâ*. The interesting experiments made by Professor Magnus, of Berlin, relative to the cause of the deflection of projectiles (*Naval Gunnery*, by Sir H. Douglas, p. 64), are the most remarkable of any that have been made on this subject. A full description of them (which would be too long to insert here), may be seen in Taylor's *Scientific Memoirs*, for May, 1853. These experiments were made with bodies in the form of projectiles placed in movable frames, and the results were obtained by means of a current of air directed against them.

Professor Magnus's explanation may be described briefly, as follows: It is well known that when a body, such as a solid of revolution, is rotating rapidly, any force which acts upon it in a direction passing through the axis, but not through the centre of gravity, will not have the effect of moving the axis in that direction, as it would do were the body at rest, but will cause the body to move to one side, so that if the centre of gravity is fixed, the axis will describe a cone about the direction of the force. This may be seen in the motion of a common top, spinning with its axis in an oblique direction. When a force is applied to the top in this position, as, for example, if a current of air is directed against it, vertically downwards, the effect will be to depress the axis very slightly, but to increase the velocity with which a top, spinning in such a position, will move laterally. It appeared from Professor Magnus's experiments, that in a bullet of the ordinary form, the pressure of the air on the fore part has a tendency to raise the apex of the shot; but this pressure, agreeably to what is stated above, will not have the same effect when the shot is rotating rapidly; but supposing the rotation to be from left to right, will cause the apex of the shot to move very slowly towards the right, and consequently produce a deflection of the shot in that direction; since the air pressing the centre of gravity towards the same side will cause the whole body of the projectile to deflect from the plane of the trajectory. If the time of flight were sufficiently long, the apex of the shot would describe a complete cone about the direction of flight; but in general the time of flight is so small, that the apex only proceeds to move to the right.

The experiments of Professor Magnus are, however, imperfect, as regards the whole subject of deflection; for he has not examined what effect the position of the centre of gravity being very forward in the shot would have on its deflection, nor did he experiment with bodies having projections on their hinder parts, such as exist in all rifle projectiles. These experiments, therefore, ingenious as they are, do not exhaust the whole subject, and leave much to be done by future investigators.

The theory entertained by many, that the deflection of the projectile, or *derivation*, as it is called by the French, is to be especially attributed to the greater pressure of the air upon the under surface of the shot, is not borne out by the results witnessed in practice;* for if this were the sole, or even the chief, cause of the deviation of long shot, the greater their comparative rapidity of rotation, or the quicker the turn of the grooves, the greater would be the deflection; whereas, we find by increasing the rotary velocity of the shot without increasing its translatory velocity—that is to say, by increasing the *turn* only—a contrary effect is produced.

The theory of Professor Magnus perfectly agrees with the results obtained in practice, as the movement to which he ascribes the deflection of long shot, takes place more slowly when the velocity of rotation is higher, so that the deflection arising from it will be less.

When a very high velocity of translation is given to long projectiles, we find in practice that the deflection is much increased. This also is agreeable to the theory of Professor Magnus, inasmuch as the velocity of rotation increases only in proportion to the velocity of translation, whilst the resistance of the air increases in the ratio of the square of the velocity, and therefore the rotary movement he describes would, in this case, be made with greater comparative velocity, and the deflection of the shot, in passing over a given space, would consequently be greater.

* Appendix B.

In the Frontispiece I have given representations of the positions of several shot as they would appear during their flight. Fig. 1 represents the positions, at the several points of its flight, of a shot having its centre of gravity at or behind its centre of figure. Fig. 2 shows the same shot fired at a greater angle of elevation; the effect becoming more remarkable as the elevation is increased. The pressure of the air being so much greater upon the shot, when the centre of gravity is not sufficiently forward to keep its axis nearly coincident with its trajectory, will cause the flight of long shot to deviate from the plane of the trajectory, as well as to suffer considerable reduction. Figs. 3 and 4 represent the positions in its flight of a shot having its centre of gravity sufficiently forward to enable it to keep the longer axis coincident with the line of flight throughout its extreme range. This is a much more important consideration where percussion shells are to be employed at great elevations, than with small arms or guns which are used for horizontal firing only.

Fig. 7 shows a shot moving in its trajectory. If we suppose the centre of gravity to be at the centre of figure (*a*), the shot would continue to rotate about an axis parallel to itself, since there would be nothing to cause an alteration in its position; but if the centre of gravity were situated at some point (*b*) *before* the centre of figure, this would continually tend to bring the fore part of the shot down, and thus to preserve the coincidence between the axis of the shot and the tangent of the curve of its flight. The centre of gravity should be just forward enough to keep the axis a tangent to the trajectory. To have it so far forward as to bring the fore part of the shot *below* the tangent, would cause an unsteady flight, and a diminished range; a similar effect, indeed, to that which would be produced by over-weighting the head of an arrow.

The action of the wind *across* their path is a further cause of the deflection of shot. Long shot suffer more from this cause than round shot—the *lateral* surface of the former being greater—particularly when the centres of gravity and of figure are not coincident, as the wind, in blowing directly across the path of the shot, will then have a twisting effect upon it.* Proper allowance must be made for this deflection in the aim.

Nearly all elongated shot are made with their fore ends more or less pointed. This appears contrary to the received notions of the effect of the resistance of the air, and is to be regarded rather in the light of a vulgar error. It was proved by experiments made with the whirling machine, constructed by Dr. Hutton, that the sharp ends of solids, of equal diameter, suffered somewhat more resistance than their hemispherical ends; and if the ends were flat, they encountered more than double the resistance. The resistance on the base of the hemisphere to that on the convex side being nearly as 2·4 to 1, whilst the resistance on the base of the cone is to that on the vertex nearly as 2·3 to 1. (Hutton, Tract 36, *Experiments with the Whirling Machine.*)

* It is not improbable that this circumstance gave rise to Colonel Beaufoy's statement that Robins's egg-shaped bullet flew to windward. If this were so, it would go far to prove that a shot, having its centre of gravity in a forward position, would also remain a tangent, or nearly so, to its curve of flight; since similar causes produce like effects.

Now the retarding effect of the air upon a shot depends more upon the weight or density of the shot than upon its form, its diameter remaining the same; if therefore, the resistance offered by the hemisphere is very nearly the same as, or (which is doubtful) even somewhat more than, that offered by the form called the curve of least resistance, the superior weight of the hemispherical end, in shot of equal length, would cause the retarding force of the air to have less effect upon it, and also place the centre of gravity more for ward.

The construction of the fore end of a long projectile, provided it be smooth and convex, is of little moment compared with that of the part which lies behind its centre of gravity; for on the latter most depends its stability, and the steadiness of its flight; for instance, a shot which tapers from the front to the hind part, will usually have an unsteady flight. I may here remark that the sabot sometimes used with this and other forms of shot is objectionable in practice, as it cannot be sufficiently depended on for always giving the same results.

When the hinder part only of a long projectile is of somewhat reduced diameter, its flight is improved by it. In February, 1858, I fired some 32-lb. shells constructed upon this principle—the first time, I believe, it was ever tried with cannon—and obtained a very remarkable range. Since then Mr. Whitworth, also, has reduced the diameter of his projectiles at the rear end, and has likewise obtained a great range with them.

The track described by elongated projectiles approximates more nearly than that of spherical shot to the curve of a parabola. This is accounted for by the weight of the former class of projectiles being so much greater, as compared with the resistance of the air upon them, than is the case with spherical shot; so that the resistance of the air does not reduce their velocity so rapidly.* For this reason, the heavier the description of shell, the higher is the elevation at which an increase in its range can be obtained, and the smaller the angle of its descent. When we consider the difference, not only in the curves described in their flight by round shot of equal sizes, but also the variety of curves described by shot and shell of different sizes when fired with different velocities (a circumstance which considerably affects the precision of the fire), the value of these facts will be appreciated.

The lateral deviation also of elongated iron shells, when these projectiles are fired with a velocity no greater than that with which shells are ordinarily projected from mortars, will be found both smaller and more *uniform* than that of round shells; the elongated shells acquiring a rotary movement about an axis situated always in the same direction, the effect produced by the act of rotation on their flight is likely to show less variation than in the case of round shells, which acquire variable rotary movements about an accidental axis. The elongated, therefore, would prove superior in every respect to the spherical shell for vertical fire.

* Figs. 5 and 6, in the Frontispiece, are intended to represent respectively the flights of a round and an elongated shot

A rifled howitzer of large calibre, adapted for firing at great elevations, would project a heavy shell with great accuracy to a great distance, and might thus be advantageously substituted, in many instances, for the ordinary mortar.

# ON THE GROOVES AND THE INCREASING SPIRAL.

THE point which next claims our attention has reference to the description of *groove* best calculated to give the requisite angular velocity to the projectile.

This will depend, altogether, upon the character of the shot employed; *i. e.*, whether it is made of iron, or of a compound of iron and lead.

In either case the form of the rifling should be such as to admit of easy loading, and also to cause the shot to pass truly and evenly out of the gun with the smallest amount of friction and expenditure of the force employed in projecting the shot. Although so much attention has been paid to this point, the form of groove best adapted for iron projectiles has not yet been satisfactorily determined.

When plain iron projectiles are used with rifled cannon, the conditions are altogether different from those which exist in rifled small arms, from which leaden bullets are fired; and the question as to the form of groove becomes of much greater importance.

It is customary to look upon the rifling simply as the means whereby a shot receives a rotary impulse about its longer axis during its passage out of the gun; but, when plain iron projectiles are used, it ought to do more than this—it should also effect the objects sought to be obtained by the expansion of the metal when leaden or compound shot are used: namely, to keep the shot straight and true in the bore, and also in a manner productive of the least friction. In all the different forms of rifling which have hitherto been advocated, whether they be angular, as in the case of Mr. Whitworth's gun, circular, as in Colonel Jacob's and others, or such as are formed by the oval bore of the Lancaster gun, the conditions I have noticed have either been quite neglected or only partially attained. Of these forms of bore, however, the Lancaster, although perhaps a good form for leaden bullets, must be considered as the least suitable for cannon, on account of the irregularity of its action, being frequently the cause of friction so great as to occasion the bursting of the gun.

In Plate 2, Figs. 1, 2, and 3, are elevations of the different shells fired from these guns, and under each elevation the form which the bore of the gun assumes. Fig. 1 is Whitworth's projectile (bore, 4·2 inches); Fig. 2, Jacob's (proposed width of bore, 4·2 inches); and Fig. 3, Lancaster's (bore, 8 inches). These shells are all on the same principle—each shell has projections corresponding to the grooves in the gun; the difference is merely in the *form* of groove, very little attempt being made with either to alleviate the effects produced by the necessary windage and the sharpness of the angle of the turn peculiar to cannon from which iron projectiles are fired; or to reduce the friction employed in giving the shot its rotary movement to the smallest amount necessary for that purpose. These, however, are most important matters for consideration; since the friction produced by the passing of the iron projectile out of the bore is not, as with expanding shot, equally distributed over the *whole* of the interior surface of the gun, but takes place at certain points only.

The hexagonal form of the bore, or angular groove, appears to meet with some favor; but although this form may be well adapted to small arms with which unusually long bullets are used, and which, therefore, require a great turn, it does not appear to be altogether the best form for ordnance used for firing iron projectiles.

The object of the hexagonal form of bore is perfectly intelligible, when the friction, as with expanding shot, is distributed over the whole interior surface of the bore, as this form gives the bullet a firm hold of the grooves; but with iron shot that is impossible; and therefore the angular projections on the shot will not be productive of less friction than those of another form, and are not otherwise peculiarly effective, as they, in common with all other kinds of projections upon iron shot, can impinge on the bore at one point only of its surface.

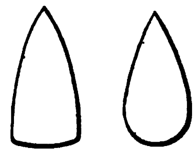
The irregularity of the friction consequent upon the windage gives rise to the chief *mechanical* difficulty in the way of all attempts at improvement in these matters. This difficulty would, however, be considerably alleviated, were a better knowledge of the subject to guide the employment of the mechanical means brought to bear upon it.

The form of the grooves for compound shot—the friction being distributed over the *whole* surface of the bore—is not a matter of so much moment as with iron shot. A sufficient number of experiments have not yet been made to establish the superiority of any particular form and number of grooves for rifled cannon employed for this kind of projectiles. It appears, however, to be generally admitted, that the shallower and the fewer they are in number, the less is the friction, and I would certainly advocate the use of three only, were the angle of the turn the same as in an ordinary rifle; but as this is not the case, and as that portion of the shot which enters the groove is, in the case of cannon-shot, smaller in proportion to its size than with a leaden bullet, I think it preferable to use a greater number, proportional to the size of the bore.

Armstrong's gun has a great number of grooves, but the circumstances under which the shot is fired are unusual, the gun loading at the breach, and the shot, which is larger in diameter than the bore, being forced through it, so that it is *compressed* instead of being *expanded.*

A considerable number of experiments will yet have to be made upon this subject, as it materially affects the range and accuracy of the shot, though more in the case of iron than of compound shot.

I should not have considered it necessary to notice the principle of the *increasing spiral* (which is employed chiefly by the Americans), but for several attempts that have been made to use it for iron projectiles. Whatever advantage or disadvantage attends this mode of rifling with *small arms* would appear to resolve itself entirely into a question of the friction of the bullet's passage out of the gun, as the bullet must ultimately acquire a velocity of rotation corresponding to the last turn given to it before quitting the piece. With long iron shot, however, the application of the increasing spiral is positively injurious. The friction increases at that part of the gun where the shot acquires nearly its greatest velocity—a part where friction should be most carefully avoided, for there it is most likely to cause a fracture either of the projectile or the gun. If this kind of turn be attended with any advantage at all, it can only be when employed either for the American flat or round-ended picket (see wood-cuts), or the ordinary spherical bullet.

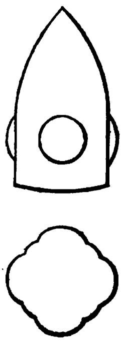


With long shot, of any other kind than the above, the use of the increasing spiral must be utterly and at once condemned. It will not only increase the friction, but, as the form of the grooves actually changes, it will also render the shot particularly liable to shift its position, to strip, or to break. I consider, therefore, that to have the grooves formed of an *equal* spiral conduces not only to the avoidance of much friction, but also to the attainment of greater precision.

It appears strange that no one (to my knowledge) has observed the impossibility of a *perfect coincidence* existing between the sides of a groove cut on the principle of the increasing spiral, or gaining twist. If the sides be made equidistant, the form of the groove will change; and the change will be still greater if the sides be not equidistant, as they cannot then be parallel to each other.

Grooves cut with a regular spiral may be described as *straight lines* applied to a cylinder; whilst those which form an increasing spiral are represented as *arcs of circles*, or other curves. If two concentric arcs of a circle be described on the interior surface of a cylinder, the distances between points taken at right angles to the axis of the cylinder will vary.

When the grooves are cut with an increasing spiral, a little reflection will show that any projection on the



bullet which fits exactly into the groove at the one end cannot do so at the other; with the single exception, when the projections are hemispheres, or other portions of spheres (see wood-cut), and the sides of the grooves are equidistant throughout (as in Fig. 3, p. 97); when, in fact, the groove forms what is called by mathematicians a *tubular* surface. When the projection, supposed of iron, has any *length*, it must either fit so loosely at the breech as to cause a great windage, or so tightly at the muzzle as to strain the gun unduly, and cause it to burst; as was frequently the case with the Lancaster gun, which has an increasing spiral.

The following cuts are intended to represent the defects of the increasing spiral.

Figs. 1, 2, and 3 show three cylinders laid open. Fig. 1 represents a groove forming a regular spiral. Fig. 2, a groove with an increasing spiral, the curves being formed by arcs of circles of the *same* radius.

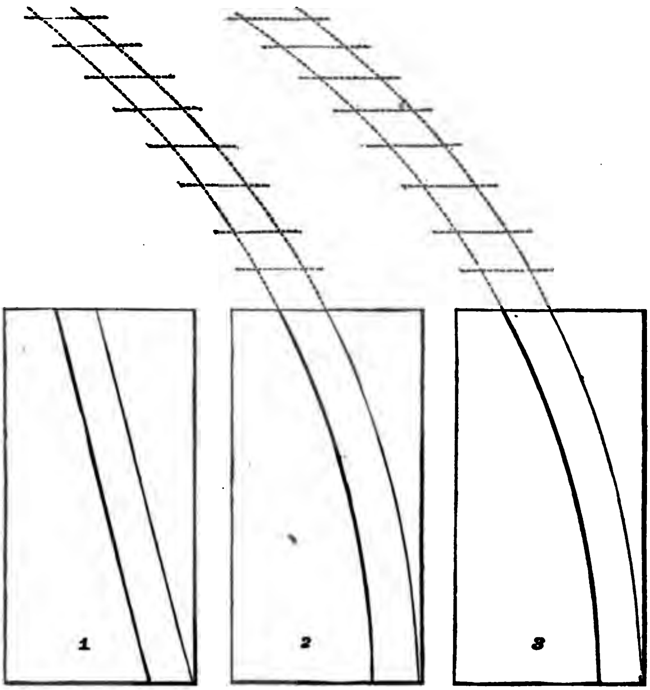



Fig. 3 shows also a groove with an increasing spiral, the sides of which are formed by arcs parallel one to the other, so that the radius of one will be *less* than that of the other. Here it will be seen that the grooves on the principle of the increasing spiral cannot be true, for it is only by making the sides of the grooves parallel to each other (Fig. 3), in the direction of their common centre, that the grooves can maintain the same width throughout, in which case they are not equidistant in a direction at right angles with the bore; and if the sides are equidistant in a direction at right angles with the bore, as in Fig. 2, they are not parallel, but continually approach each other. In the former case the grooves widen, and in the latter they contract in the direction of the muzzle of the gun. The intended longitudinal increase of the turn of the groove, therefore, is not the only alteration which takes place. It is evident, as I have shown, that other very undesirable changes attend this description of groove. Although these defects may be almost imperceptible in small arms, yet the irregularities which attend this method must be very appreciable when it is applied to guns of large calibre. These defects are also not noticed with small arms, because the bullet being made of lead instead of iron, accommodates its form to the varying shape of the grooves in its passage along the barrel; but it is clear that the friction resulting from this must be very injurious.

Many points to which attention has here been particularly drawn may possibly appear trivial to those who have not paid much attention to the subject. But these trifles make in the aggregate the total effect of the projectile, and all who *have* studied the subject, and followed it up by experiments themselves, must be well aware of the difference in the shooting of a rifled gun, which will sometimes be caused by, apparently, the most trifling alteration. There are few things which require so perfect a combination of qualities, in order to arrive at excellence, as rifled artillery; and, considering its importance, nothing should be neglected which may tend in any way to insure it.

# ON THE RANGE OF ELONGATED PROJECTILES.

On no point do the results of practice differ so much from theory as in all that relates to the resistance of the air upon projectiles, and its effect upon their ranges. Experience has compelled many (myself amongst the number) occasionally to modify opinions too hastily formed in connection with this subject. The introduction of projectiles of an elongated form, and the very extensive ranges obtained with them when fired from the rifled musket, gave rise to many speculations as to what might be accomplished with cannon-shot made on the same principle, and much disappointment has been expressed that the result has fallen short of what many predicted and more considered probable.

One writer, a thoroughly practical man in all that concerns small arms, stated it to be his opinion that with a shot of four inches in diameter, and three diameters in length, a range of ten miles was capable of being attained; whilst others have endeavored to show that even more than this could be achieved. But if we examine the matter more closely, it will at once be evident that the greatest range which such shot could acquire under the most favorable circumstances—that is to say, with the present means of projecting them—is far below this. It was, and by many is still, imagined, that by continually reducing the diameter of the shot, and increasing its length—thus reducing the resistance of the air to its flight—the range would be continually increased; but, independently of the difficulty of giving projectiles of more than a certain length a correct flight, there are other cogent reasons why this cannot be the case.

We will first suppose a shot fired in a vacuum, with only the forces of projection and gravitation acting upon it; the range acquired by a shot fired under these circumstances is a matter very easily calculated; the method for computing it is given, among other writers, by Dr. Hutton (*Theory and Practice of Gunnery*, Tract 37; see, also, *Naval Gunnery*, by Sir H. Douglas, pp. 26 and 67). It appears that if projected at an angle of 45°, and with a velocity of 2,000 feet a second, a shot would have a range of nearly twenty-four miles. As the ranges of shot fired in a *vacuum* would vary as the squares of their velocities, a shot fired at the same angle, with a velocity of 1000 feet a second, would have a range of six miles only.

If we consider, then, the difficulty of giving a *mean* velocity of 1000 feet a second to the projectile of any kind, during the time required for a flight of six miles, it will at once be evident that even this range is (with our present means) extremely difficult of attainment. For a shot to acquire a range of six miles when fired in the air, that part of its surface opposed to the resistance of the air would have to be comparatively so small that no shot could possibly acquire such a range, except one of extremely large dimensions projected with an initial velocity of more than 1000 feet a second.

If, however, wrought-iron or steel guns could be made of sufficient strength (of which, I think, there can be no doubt) to throw long projectiles of a diameter of seven or eight inches with a proper velocity, the range of six miles, and even more, if it were desired, would be easily attainable. But in whatever proportion the weight of projectiles may be increased, or the resistance of the air (opposed to their flight) reduced, *a given velocity of flight* must always be necessary for them to acquire a given range. The extended flight of heavy war rockets is entirely owing to their great mean velocity, acquired by opposing a continually greater force to the resistance of the air.*

In fact, however exaggerated the dimensions of a shot, it could not be projected to a distance of six miles if the *mean* velocity of its flight were not higher than 1000 feet a second, unless, indeed—as I am induced to believe is frequently the case with long shot—the resistance opposed by the air to its *descent* tends to prolong its flight. For although by elongating a shot (of a given weight) the resistance to its *flight* is lessened, yet—on account of the greater longitudinal surface of

* Attempts have been made at various times to fire rockets from guns, but invariably without success, the rocket being usually blown into fragments. A little consideration of cause and effect will show the futility of such attempts; for even supposing that rockets could be fired from guns, no dependence could be placed upon their flight, as the impulse which they would receive from the gun would be greater than that which they would receive from the firing of their composition. The latter would not therefore take effect until the velocity of the rocket was greatly reduced, in which case the direction of flight would be uncertain, for the impulse which the rocket would receive from the firing of the composition would always take effect in the direction of the longer axis of the rocket—a direction different from that which the rocket has in the gun.

the shot—the resistance opposed by the air to the action of gravity is increased. So that, if a shot with a very small diametrical surface, as compared with its length, were fired with a velocity not exceeding its *terminal velocity* (in which case the loss in velocity which it would sustain from the resistance of the air before it would be attracted to the earth would be scarcely appreciable), it might actually attain a greater range when fired in the air, paradoxical as it may appear, than if projected in a *vacuum;* since, owing to its undiminished velocity, and the resistance to its descent, the time of its flight would be more prolonged, and the curve consequently larger than it would be if the shot were fired (with an equal velocity) in a *vacuum.*

In computing the ranges of shot projected in a *vacuum*, all that is necessary to consider is their velocity of projection, and the angle at which they are fired. When fired in the air, besides the resistance which is opposed to their flight, their *weight* also must be taken into account, since it enables them to overcome that resistance; and, therefore, any increase in the size—and consequently in the terminal velocity of the shot—will be followed by an increase in their power of acquiring greater mean velocities, and, therefore, greater ranges. Thus, for example, when heavy projectiles are fired with low velocities (below their terminal velocities), they acquire the ranges, or nearly so, which they would have if fired in a non-resisting medium, namely, as the square of the velocity; since they oppose to the resistance of the air a greater resistance, in their own weight. For instance, a 13-inch mortar of 36 cwt., fired with a charge of 4½ lbs. of powder, will throw a shell nearly 1500 yards; and when the charge is increased to 9 lbs., 2900 yards; but when the charge is diminished to 2 lbs., the range is little more than 400 yards.* (See *Artillerist's Manual*, by Major Griffiths, R. A., *Table of Mortar Practice*, pp. 77, 78).

In proportion as the elevation of the gun is low, and the space to be traversed by the shot small, the value of a high initial velocity increases; and the shot which can acquire the highest mean velocity, or, in other words, the shortest time of flight in passing over a given space, will have the lowest trajectory.

Considering that the velocity given to elongated shot of all sizes, in horizontal firing, is very nearly the same, I think it will be found that the effect produced by any increase in the *length* of the projectile, used in this description of firing, will be less, in proportion as the diameter of the bore of the gun is increased; and my reasons for this belief are as follows: If we duly consider the cause why so great an increase in the range attends an increase in the length of shot of a small diameter, we find that shot of small size suffer a comparative resistance from the air so much greater than those of a larger size, that any reduction in this resistance is immediately productive of a very great difference in their ranges. Thus, an ordinary round musket bullet has an effective range as far as six or seven hundred yards; but by reducing its diameter, and increasing its

* The velocities acquired by shot are directly as the square roots of the charges of powder, and inversely as the square roots of the weight of the shot.—(*Artillerist's Manual*, p. 190.)

length to three diameters—thereby considerably lessening the surface which it opposes to the air, without diminishing its weight—the range is more than doubled. When, however, we have to deal with heavy cannon shot, we find that the same comparative excess of range is impossible; for, when fired in a horizontal direction, the time which would elapse before the long shot could be drawn to the earth by the action of gravitation would be too small to admit of any advantage being acquired from that greater mean velocity of flight which it would have if fired at a great elevation.

Besides, large spherical shot have already surfaces so small in proportion to their weights, when compared with those of musket bullets; and consequently a mean velocity of flight (when fired at low elevations) so much more nearly approaching that which they would have in a *vacuum*, or, which would be due to their velocity; that to attempt to materially increase their range by lengthening the shot considerably, would be of little avail, unless an increased velocity of translation were given to them. In fact, I believe it will be found that when the diameter of a long projectile exceeds eight or nine inches, very little, if any, difference will be perceptible in the range—except, perhaps, when they are fired at the highest elevations—whether the shot be two or three diameters in length; supposing the initial velocity with which each is fired to be the same; and that it do not exceed that which would be produced by firing a charge of powder not exceeding one-eighth of the shot's weight. If a larger quantity of powder, or a higher velocity than the above were employed, the advantages accruing from the greater length of the projectile of three diameters would be more appreciable.

It will also probably be found, for the same reason, that the ranges of long shot of different diameters, when the shot exceed a certain size, will not exhibit the same relative difference as those of round shot of different diameters. Indeed, in the case of long shot, the resistance or the air is so much reduced, that a shot of eight inches diameter, if fired with the ordinary velocity, would probably retain that velocity nearly undiminished throughout its flight; so that at whatever distance from it—within its range—an object were placed, it would always strike it with nearly the same force, irrespective of the distance; hence the destructive power which is to be obtained by the use of such projectiles when fired at great elevations.

For this reason, too, when long shot of very large dimensions are used—as, for instance, of 8 inch diameter—it is very probable that the turn to be given to the rifling may be rather less than that determined by the rule I have previously explained. (See the previous chapter, "On the Manner in which a Difference in the Calibre influences the Turn of the Rifling.") Unless such shot have very great velocities or very high elevations, the time of flight is not sufficient to allow of the diminished resistance of the air to increase the range.

A proper consideration of the subject will always enable those who are interested in it to form a tolerably correct opinion respecting the manner in which all these circumstances influence the rotary velocity required for long projectiles, and to modify the turn in conformity with it. Experiments for ascertaining the velocities of long projectiles, with the ballistic pendulum, are very much required: until the result of such experiments is known, it will be impossible to calculate with any degree of exactness what range is attainable with projectiles of large diameter. If (as I think probable) a heavy elongated shot may be made to acquire a velocity of about 1200 feet a second, a shot of eight inches diameter and three diameters long might, when fired at a very high elevation, attain a range of nearly eight miles.*

Some time after the foregoing portion of this chapter was written, my attention was directed to a statement made by Sir W. Armstrong, at the United Service Institution, and reported in the *Mechanics' Magazine*, June 3, 1859, to the effect, that elongated projectiles have a greater range when they are fired in the air, than they would have if they were fired in a vacuum, or non-resisting medium. This statement bears, at first sight, so much resemblance to certain opinions† which I have hazarded myself in the preceding pages, that I think it necessary to make a few remarks respecting it.

Sir W. Armstrong's observations (as reported) are as follows: "In a vacuum the trajectory would be the same whether the projectile were elongated or spherical,

* Two years ago, that is to say, two years after the above was published, I obtained at Shoeburyness, with a 7-inch rifled projectile, of 174 lbs. weight, a range of more than 10,000 yards, or nearly six miles. This is the greatest authentic range ever recorded of any gun.

† These opinions were written in 1858, and were then merely conjectural, but have since been verified by experiment in a remarkable manner. They appear, however, at first sight, so paradoxical, that I hesitated about publishing them, until they were in some degree confirmed by practice.

so long as the angle of elevation and the initial velocity were constant; but the presence of a resisting atmosphere makes this remarkable difference—that, while it greatly shortens the range of the round shot, it actually pro. longs that of the elongated projectile, provided the angle of elevation do not exceed a certain limit, which, in my experiments, I have found to be about 6°. This appears at first very paradoxical, but it may be easily explained. The elongated shot, if properly formed, and having a sufficient rotation, retains the same inclination to the horizontal plane throughout its flight, and consequently acquires a continually increasing obliquity to the curve of its flight. Now the effect of this obliquity is, that the projectile is in a measure sustained upon the air, just as a kite is supported by the current of air meeting the inclined surface; and the result is, that its descent is retarded, so that it has time to reach to a greater distance."

The reader will observe that I have not gone quite so far as to assert that long projectiles actually have their range prolonged when fired in the air; I have merely hazarded the conjecture that, under certain circumstances, such might possibly be the case. It cannot be positively asserted as a fact, until we know precisely what initial velocities these projectiles really acquire.

I supposed that it might be true, from the circumstance of finding that the ranges of long projectiles, of a certain size and form, varied in a greater degree (with the elevation) than they would do in a *vacuum*.

Some long expanding projectiles of mine, 4·2 inches in diameter, and of about 32 lbs. weight, were fired at Shoeburyness, in May, 1858, from a 32-pounder brass howitzer, with bore the size of a 9-pounder, with an elevation of 5°, and a charge of 4 lbs. of powder; these shells attained a range of about 2000 yards.

On another occasion (February, 1859), when fired with an elevation of 10°, with a similar projectile and charge of powder, a range of more than 4000 yards was attained; that is to say, the gun threw a rifled shell of one-half greater weight than the service shell (22 lbs.) to more than double the ordinary distance (1900 yards) attained with the same elevation.

But the most remarkable circumstance was, that it showed that when fired with equal initial velocity, but different elevations, the ranges of long projectiles in some cases will vary in a degree as great as, or even greater than, if fired in a *vacuum;* when their ranges are supposed to vary as the size of twice the given elevation.

The full explanation of the manner in which the resistance of the air tends to lengthen the flight of a long projectile may be given as follows: When a round projectile is passing through the air, the whole resistance to its flight* is in the direction of the tangent to the curve; but when a long projectile is used, a different result is obtained. For, in consequence of the well-known law of hydrodynamics, that when a solid body strikes obliquely on a fluid mass, the resistance will be perpendicular to the surface of the solid, the resistance on the

* This is independent of the unequal action of the air on opposite sides of the shot, caused by its rotary motion.

surface of an elongated shot will no longer act in the direction of the tangent to the curve of flight; but the resultant of the pressures on the fore end and the under side will act in a direction *above* the tangent to the curve; so that although the velocity of the projectile is diminished, it will be made to describe a path rather less curved to the horizon than it would otherwise have done. Hence, in certain circumstances, as when the elevation of the gun is not very great, the range may be prolonged.

It will be noticed that Sir W. Armstrong considers that to be a properly formed shot of which the axis remains parallel to itself during its flight; but I am convinced it will be found that a preferable form of shot will be one that has the centre of gravity thrown forward, so that the shot will remain approximately a tangent to its trajectory throughout its flight. In this case, the loss of velocity will be much less than in the former, and at the same time the obliquity to the trajectory will be sufficient to call into play the sustaining power of the air.

A projectile which maintains its axis parallel to itself, might have a certain advantage when the gun has little or *no* elevation, because the obliquity to the curve of flight would not then be so prejudicial to the velocity of the projectile, and the whole of the pressure upon its under surface would tend to sustain its flight; but the time of flight in this case (unless the gun were placed on an eminence) would be too small to admit of any great advantage being thus obtained. When, however, the angle of elevation is high, the "increasing obliquity to the curve of flight" tends to diminish the velocity more than to retard the descent of such a projectile; and its curve of flight and range are, in consequence, diminished.

Hence the effect which Sir W. Armstrong remarked in his experiments—that the prolongation of flight alluded to was only observable when the angle of elevation was below 6°.

Sir W. Armstrong's concluding sentence, that "its (the projectile's) descent is retarded, so that it has *time* to reach to a greater distance," could hardly, I should think, have been carefully considered before it was uttered; for every person familiar with the theory of the motion of projectiles will see that by diminishing the velocity of a projectile after it has passed the apex of its curve, its time of flight is lengthened, while its range is on the contrary diminished.

# ON EXPERIMENTS IN GUNNERY.

Gunnery furnishes no exception to the rule, that there must be a cause for every effect; indeed, in scarcely any investigation is a knowledge of causes more essential than in conducting experiments in gunnery. Hence, when we find that two shots, fired apparently under precisely the same circumstances, show a variation in their range, or in their accuracy, we know that a cause must exist why this should be the case; and although the fact of the variation in the effect of shot fired under similar circumstances shows that *general* rules only are applicable in gunnery, and that no single result is to be relied on, yet a proper study of cause and effect in this matter will considerably reduce the chances of failure.*

It is not sufficient, merely, to obtain a greater range, accuracy, or general effect; but we should know, also, how these results are obtained, if we wish to profit by them in the greatest possible degree.

Before any new principle can be successfully applied

* These somewhat trite remarks were called forth by an observation which was made to the author by an artillery officer of some eminence—to the effect that "the firing of the charge, with cannon, was attended with such varying results, that it was useless to think of applying any rule for regulating the thickness or strength of metal, required for guns of different calibres!"

in practice, repeated failures must always be expected. I may go so far as to assert that they are absolutely *necessary* to the complete attainment of success. Because the Lancaster, Whitworth, and other guns may have failed in fulfilling the expectations that were formed respecting them, their trial is by no means, on this account, to be considered as an entirely useless expenditure of either time or money. On the contrary, it was necessary that they should be made; and no doubt considerable advantage, in many respects, will arise from their having been made.

When men possessing mechanical skill in the highest degree—as Mr. Whitworth, for instance—undertake experiments with a view to any improvement in the construction of implements, whether of a warlike or a peaceful character, good must always result; for, even if they fail in establishing their views, considerable light will always be thrown upon the subject, to serve as a guide for the future. Still, although the aid of able mechanicians is of great importance in the practical application of the theory of rifled cannon and projectiles, the theory itself is, nevertheless, a scientific, rather than a mechanical, question.

It will require systematic and well-conducted experiments, extending over a course of many years, before a sufficient knowledge can be acquired of all the circumstances attending the application of the principle of the rifle to cannon, to enable us to decide upon the best practical system for the construction, either of the gun or the projectile. The different methods of applying the principle of the rifle to cannon—as, by having the gun to load at the breech, or the muzzle; or, having a compound, or a homogeneous projectile—will each have to be the subject of numerous experiments.

It is all the more imperative that these experiments should be undertaken by the Government, inasmuch as no person—so small is the knowledge which has as yet been generally acquired of this subject—is at present really competent to give an opinion upon any thing new in connection with it which may be brought before their notice. I think I should not be far wrong in asserting that only a very few persons connected with the War Department are yet fully aware either of the comprehensive nature of the subject of Rifled Cannon, or of their want of knowledge in every thing relating to it. To stand by and witness the trial, at Woolwich and Shoeburyness, of a number of projectiles, or a new gun—the result, in general, of private experiment—teaches little or nothing. Much more is learned by firing a single shot—made expressly for the purpose of ascertaining some particular point—and by a thorough examination and careful study of all the circumstances attending its projection, than by merely witnessing the firing of twenty thousand projectiles devised by other persons. The *results* obtained with the latter may be apparent enough, but the train of reasoning (the fruit of personal experience) which produces these results, remains altogether unknown; for although a man may communicate the result of his experiments, yet he cannot furnish another with his personal experience; and without this it is perfectly impossible to effect, or even to suggest, any important improvement.

There is scarcely a question—if, indeed, there be one—in the whole subject of dynamics, or the laws which relate to a body in motion, which is not involved in the investigation of the circumstances attending the flight of elongated projectiles; these require to be thoroughly investigated and known, before any decided opinion can be given as to the best practical method for constructing rifled cannon. A system founded upon mere guess-work would have a very unsound basis.

It has been too much the custom to speak slightingly of *theory* in gunnery. This is probably owing to the unsatisfactory state of the ordinary theory of gunnery; but the acquisition, in this instance, of a sound theory, is of the highest importance. The stride which has been made in the practice of gunnery since Robins's experiments were made known, resulted from his discoveries respecting the laws which govern the flight of projectiles, and respecting the nature of the projecting force. When once a theory has been framed, based upon sound principles, the mechanical improvements follow as a matter of course.

Sufficient attention has not hitherto been paid to the correctness of the principles upon which the cannon and projectile should be constructed. To this, chiefly, must be attributed the failure of the Lancaster and Whitworth guns—the projectile, in the former case, acting like a wedge; in the latter, like a lever, in the bore of the gun.

To produce any great and useful results, a combination of sound theoretical and practical knowledge is necessary, and this can only be obtained by a long and close study of the subject, and by numerous experiments carried on with unremitting attention.

No single invention—as of a projectile—a method of loading at the breech—a peculiar form of groove, or any other mechanical contrivance—is of the least use in itself, unless a perfect combination—such as the proper length of the gun—of the turn—the most suitable metal for the gun, etc.—is obtained for rendering it effective. If each of these be not adapted to the others, the whole must inevitably fail in practice. It is the difficulty of effecting this combination which renders experiments with rifled guns so complicated and costly. If properly conducted experiments had been at first undertaken by the Government at their own expense, instead of relying upon the inventive and pecuniary resources of private individuals for the acquisition of their data—which, to say the least, is a rather pitiful course to adopt; for, if a new thing is worth trying, it must surely be worth the cost of the trial—the public would have been spared the expense (amounting to between three and four hundred thousand pounds) of the Lancaster gun factory.

Notwithstanding, therefore, the numerous experiments which have been made in gunnery, and the immense mass of data which must have resulted from them; yet, owing to the desultory manner in which the experiments have been carried on, they have been productive of much fewer practical results than we might reasonably have expected. Experiments with rifled cannon not only require to be conducted systematically, but, to be productive of real benefit, they also require to be conducted by properly qualified persons.

The questions which constantly arise in gunnery experiments are sometimes so complicated and difficult of solution, that none but a mathematician of the highest order can really ascertain the value of the results which are obtained. To conduct experiments in gunnery, therefore, in a proper manner, it is of the first importance to have a good mathematician to collect and arrange the data, and to ascertain the exact numerical value of the results obtained.

It will be also necessary to have a practised experimentalist to profit by these results; one who, with a proper knowledge of cause and effect, is capable of suggesting the best method for the attainment of certain objects. A clever mechanician, to invent and judge of the means which can best be employed for carrying out the plans suggested, should also assist in conducting such experiments, to insure their being attended with the most useful results.

The whole of these qualifications are rarely combined in a single individual. Robins was a remarkable exception, and his experiments, in consequence, formed an era in gunnery.

But although the necessary qualities are seldom found in one person, there is no reason why several persons should not be selected, who would, collectively, possess them; and who might either be placed on the Rifled Ordnance Committee, or act with it in such a manner that their services would always be at its disposal, or at that of the presiding officer at Shoeburyness (an important office, for which no one could be better qualified than the gentleman who now fills it); the results of their operations to be finally submitted to a committee of experienced military men of all services, who would be able to judge of their practical value.

A systematic course of experiments conducted under such joint superintendence would be the right course to take for acquiring the proper *data* respecting all the circumstances connected with rifled ordnance, in a much shorter time, and more satisfactorily, than could otherwise be the case. To conduct experiments in a desultory manner is invariably a useless expenditure of both time and money.

It is impossible for a private individual to undertake such experiments on a sufficiently large scale to be productive of conclusive results; they should be conducted somewhat after the manner of Hutton's experiments, only on a much larger scale; each with a view to some well-defined object. The following are amongst the chief circumstances in connection with rifled ordnance, respecting which some certain data are required, and each should therefore be the subject of a separate course of experiments.

1. Experiments with the ballistic pendulum for finding the different velocities of rifle shot under varied circumstances; so that both the *initial* velocities given by different charges of powder, and the loss of velocity—occasioned by the resistance of the air—which the shot suffers in passing through different spaces, shall be thoroughly ascertained. At present literally nothing (certain) is known with respect to these points.

These experiments in the case of rifled shot ought to be attended with better than ordinary results, as the pendulum could be placed at a greater distance than usual, on account of the superior accuracy of flight of rifled shot.

2. The different quantities of friction in the case of both compound and iron shot of different kinds; and the loss of power occasioned by it.

3. The effect produced upon the flight of long shot by any alteration in the position of the centre of gravity.

4. The effect produced by a difference in the forms of projectiles.*

5. The difference in the ranges and deflection produced by different elevations with shot of different lengths.

6. The circumstances attending the penetration of long projectiles into various substances.

7. The angular velocity required for shot of different sizes, forms, and density, when fired with different velocities and elevations.

8. The form of groove which shall produce the necessary effect with the least amount of friction.

9. The relative strength of metal required in guns of different sizes.

10. The *curves*, and also the *times* of flight, of long projectiles; in order to ascertain the best method for securing the greatest amount of efficiency in firing them at high elevations.

* It would be a great advantage if the establishment at Shoeburyness were furnished with the means—at least on a small scale—of casting, forging, or altering experimental projectiles, so that they might be tried without delay. It would be a great saving in time, and, eventually, in expense.

11. The effect of windage, especially with iron projectiles.

12. The effect produced by altering the length of the *bore* of the gun.

13. The effect produced by using powder of a different quality.

14. The effect produced by the charge of powder in *chambers* of different forms and dimensions.

Besides those which I have enumerated, there are many other points relative to which sufficient data must be fully acquired before unerring results can be obtained. Those who, like myself, have made many experiments on a smaller scale, have acquired sufficient data, perhaps, to form by comparison a general opinion upon all these questions, and (assisted by some acquaintance with the ordinary theory and practice of gunnery) may even have arrived at the power of estimating some of their effects with a certain degree of accuracy; but the *velocities* of rifled shot when fired with different charges, the *friction*, and many other points, can only be ascertained by a regular course of experiments with the ballistic and gun pendulums, and by other means of much too extensive a nature for a private person to undertake.

Whilst the necessary experiments are being carried on, there is no reason why the country should be deprived of the use of rifled cannon. The manufacture of the Armstrong gun may be proceeded with; but we need not confine ourselves exclusively to guns of this description. There are several methods by which a supply of very efficient rifled cannon may be obtained without going to an exorbitant outlay in the exclusive adoption of cannon of the most expensive kind, previously to a thorough trial of their merits in actual service, or before the utmost simplicity in their construction is attained. Enough has been said to show that the knowledge which has already been obtained bears but a small proportion to that which remains to be acquired, and that further experiments are therefore necessary.

The accuracy attained with the Armstrong gun is a beautiful mechanical feat, but one which will be commonly performed (and by much simpler means) when a better knowledge of the subject has been acquired; it has shown what can be effected with this particular combination, but little else has been learned from it; and until the truth concerning all the points which I have enumerated, and many others, be clearly ascertained, it will be impossible to decide as to which is positively the best system for the construction of rifled cannon.

To give an example of the change in opinion which experiment will effect: it is a remarkable fact that almost all the circumstances which the Ordnance Select Committee objected to, three or four years since, as perfectly inadmissible in the practical adaptation of the principle of the rifle to cannon—such as a compound projectile—a projectile composed of many pieces—a breech-loading gun—a gun constructed of wrought iron, especially in such a manner as not to be homogeneous—are united in the Armstrong gun.

In like manner, it will probably happen, when further experiments are made, that the description of rifled gun which will be finally adopted will differ as much from the experimental (for the whole are but experimental) guns which have been tried up to the present period, as these differ from the earlier attempts which were made; for, as yet, experiment has been directed almost solely to improvement in the mechanical application of a principle of which really very little (certain) is known; instead of being directed to the acquirement of a knowledge of what relates to the principle itself.

This it is which gives rise to such expensive blunders as the Lancaster gun factory; a greater blunder, however, will be committed if we adopt breech-loading guns to the exclusion of all others.* Fortunately, there is to be found amongst the members of each branch of our service a fund of sound common sense, which (and this, upon investigation, will be found, I believe, correct) always leads, eventually, to the adoption of the best thing of its kind, when its superiority has been properly established; although, in most cases, it is effected by a very roundabout and expensive process.

By neglecting, therefore, in this instance, to make proper experiments, we throw away advantages which would enable us to acquire that superiority with regard to projectile effect, the possession of which (especially in a country like our own, the confines of which are all *coast*) should be a matter of primary importance.

* The advantage of the breech-loading system, in allowing the men who serve the gun to be less exposed, is not so prominent with long-range guns as with others; since the former would more frequently be out of the range of musketry.

# ON THE NATURE OF THE ACTION OF FIRED GUNPOWDER.

(Read before the Royal Society, December 16, 1858.)

A long interval has elapsed since the subject treated of in the following pages has engaged the attention of the Royal Society. I believe I am correct in stating that Count Rumford's paper on the initial force of gunpowder, read before this Society in 1797 (*Philos. Trans.*, vol. 87), was the latest. Previously to this, the names of Leuwenhoek, Hauksbee, Robins, Hutton, and Ingenhausz appear in the Philosophical Transactions, in connection with the subject of the force of gunpowder. Each of these eminent persons contributed largely towards its development. In consequence, however, of their limited knowledge of the initial force and action of fired gunpowder, their theories remained very imperfect. Count Rumford, fully aware of these imperfections, instituted a course of experiments for the purpose of acquiring further data; but these experiments, although extremely valuable, failed in establishing any conclusive results.

The object of the present paper is to call attention to some remarkable circumstances attending the ignition of gunpowder, and to point out their application to the construction of cannon, and, in general, to the theory of gunnery. In the course of it I shall endeavor to account for, and to reconcile, many well-known experimental facts, which are inconsistent with the hitherto received theory of the action of gunpowder; as well as to show the unsatisfactory nature of the existing theory in other respects. The present system of practical gunnery is encumbered with a mass of empirical formulæ which rather bewilder than assist the student, and afford no means of arriving at any satisfactory conclusions respecting either the relative length and strength required for different kinds of ordnance, or the initial velocities acquired by the projectile.

The theory which will be enunciated and explained in the present paper is based upon numerous experiments of a most satisfactory and conclusive nature, some of which will be described; and although further experiments will, no doubt, be required, and time for its full development, still sufficient data have been obtained to establish its correctness, and to suggest a set of more accurate and simple formulæ, and thus to afford a satisfactory point of departure for future scientific inquiries.

The principal writers on the theory of gunnery are Robins and Hutton. Although many valuable experiments have been made, and empirical formulæ deduced, among others, by Piobert and Mordecai, the writings of the two authors first named contain all that has been done to reduce gunnery to a science. (The results of their investigations may be seen collected in Captain Boxer's *Treatise on Artillery.*) I will therefore proceed at once to explain the suppositions upon which their theory of the action of gunpowder depends.

They assume (Robins, *Prop.* 7, p. 74) that the whole of the powder is converted into an elastic fluid before the ball is sensibly moved from its place; and that the ball is then moved by the pressure of this elastic fluid gradually expanding. In this way the investigation of the velocity with which the ball leaves a piece of ordnance presents no difficulty, and the formula is easily obtained for the velocity of a ball issuing from a cannon. This formula,* however, contains a quantity ($n$) the value of which is unknown—viz., the initial force of the elastic fluid generated from the gunpowder. Before the velocities given in different cases by this formula can be compared with the results of experiment, we must assign some value deduced from experiment to this quantity. In this way the formula is found to give the velocity with sufficient accuracy when the

* Hutton's formula is $v^2 = \frac{\pi g m n a d^2}{w} \times log \frac{b}{a}$; where

$a$ = the length occupied by the charge;
$b$ = the length of the bore;
$d$ = the diameter of the ball, or of the bore;
$g$ = $16\frac{1}{12}$ or 16, the accelerating force of gravity;
$m$ = 230, denoting the pressure of the atmosphere on the square inch, in ounces;
$n$ : 1 the ratio of the first force of the fired gunpowder to the pressure of the atmosphere;
$w$ = the weight of the ball.

Putting $w'$ for the weight of the powder, Piobert's formula makes the velocity of the ball proportional to $\sqrt{log\left(1+\frac{w'}{w}\right)}$; the charge being such, in proportion to the weight of the ball and to the length of the gun, that the powder may be supposed to act on the ball whilst the gaseous fluid retains a high degree of tension; *i. e.*, the weight of the powder not being more than two-thirds that of the shot: the length of the gun in calibres is, however, supposed constant, and the charge limited.

The practical formula is $v = 2000 \sqrt{\frac{w'}{w}}$

circumstances of discharge are not very different from those by means of which the numerical value of the initial force of the fired gunpowder is determined. But when a much larger charge of powder is employed, or a much longer or shorter gun, the formula gives results very erroneous.

The following conclusions result from the above suppositions:

(1) That, whatever the quantity of powder which is used, the initial force of the elastic fluid must be the *same*, since the quantity of that fluid will always be proportional to that of the powder, and therefore to the space occupied by it—supposing that other circumstances are the same. When it has been found necessary, in order to explain observed facts, to suppose the pressure of the fluid generated by the explosion of a large charge greater than that from a small one, it has been attributed to the greater heat, in the former case, increasing the tension of the fluid.*

(2) That when guns of different sizes are used, the

* (Hutton's *Phil. Dictionary*—Art. *Gunpowder*, p. 620.) "Hence it appears that any quantity of powder fired in any confined space, which it adequately fills, exerts at the instant of its explosion against the sides of the vessel containing it, and the bodies it impels before it, a force (according to Robins) equal to six tons and a half on the square inch—and it is proved by my Tracts, vol. iii., that the force is more than double this. This great force, however, diminishes as the fluid dilutes itself, and in that proportion, viz., in proportion to the space it occupies; it being only one-half the strength when it occupies a double space, one-third the strength when triple the space, and so on. Mr. Robins further supports the degree of heat to be a medium heat; but that, in the case of larger quantities of powder, the heat will be higher, and in very small quantities, lower; and that, therefore, in the former case the force will be somewhat more, and in the latter somewhat less than 1000 times the force in pressure of the atmosphere."

weights of the shot and of the charge being proportional, the length of the gun should be increased in the same ratio as the size of the bore, in order to give the same velocity to the shot.* (Hutton's *Tracts*, vol. iii., p. 313.)

But it is a well-known experimental fact, that length (in calibres) is not so important with large guns as with small. The barrel of a musket is about 67 calibres; that of a 9-pounder, 20 calibres; and of a 32-pounder, about 16 calibres;† and it is found that, by increasing the length of a cannon beyond this extent, the velocity is not increased to a degree at all proportionate.

Major Mordecai, of the United States Artillery, states that an addition of 9 calibres to a gun of 16 calibres in

* (*Treatise on Artillery*, by Captain Boxer, R. A., p. 72.) "It has already been stated that, in guns of different calibres, the length required in each to give their respective shot the same initial velocity, or nearly so, will not be the same number of *feet or inches*, but the same number of *calibres*; and why is this? Principally for the reason that the surfaces upon which the forces act in the two cases are in proportion to the *squares of the diameters*, whereas the masses which are propelled, or upon which these forces act, are in proportion to their *cubes*; and, therefore, with the same pressure upon every square inch, it requires the force to act through a greater distance in the one case than in the other to obtain the same velocity."

General Piobert—a high authority—however, clearly intimates (*Cours d'Artillerie*, p. 81), that *the knowledge of the theory of the movement of the projectile in the gun is yet to be acquired.*

† It is shown by Major Mordecai, in his valuable work (*Experiments on Gunpowder*, pp. 112, 113), that the velocity acquired by a 24-pounder shot, fired with a charge of powder one-fourth of its weight, from a gun with a bore 18½ calibres in length, is about 1600 feet a second; whilst the velocity acquired by a musket bullet, fired with a charge of rather more than one-fourth of its weight, is shown (at pp. 158, 161) to be about 1500 feet a second—the length of the bore in this case being upwards of 50 calibres.

length, adds only $\frac{1}{12}$th to the velocity of a 12-lb. ball, when fired with a charge of 2 lbs., and $\frac{1}{18}$th when fired with a charge of 4 lbs. of powder. (*Naval Gunnery*, by Sir H. Douglas, p. 44, Note.)

It would also follow, from the theory of the eminent writers already named:

(3) That guns of different magnitudes, in which proportional charges of powder are used, should be of proportionate strength at the breech.

But this also is contrary to experience; for it is found that large guns are much more liable to burst than small ones, and require, therefore, to be much stronger in proportion at the breech.

The discrepancy of experiment with the theory is more manifest in these two cases when the charge of powder is considerably increased, and becomes so great that in some instances, as in large mortars, where heavy shells are fired at great elevations and with large charges of powder, the theory becomes quite useless.

In other cases, in order to make it agree with experiment, it is necessary to assume the initial force of the elastic fluid to be different for every charge of powder, and even for every length of gun, as may be seen from the tabular statements in Hutton's *Tracts* (vol. iii., p. 296). It is not difficult to see that the initial force may be greater when larger charges are used, as the heat of the fluid may be greater; but it is manifestly absurd to suppose that the length of the gun should have any influence on the initial force of the powder. All these discrepancies conclusively prove that the recognized theory is incorrect; to these might be added, that it takes no notice of the effects of different kinds of powder, which are very observable in practice, and which will be alluded to at greater length presently.

It has been just remarked, that, in order to make the theory agree with experiment, it is necessary to suppose the initial pressure of the fluid to be different for each charge of powder; it is also necessary to suppose that it varies with the weight of the shot. Indeed, it is laid down as a well-known fact by all writers on the subject, among whom we may instance Piobert, Gillion, Mordecai, and Sir Howard Douglas, that the tension or pressure of the elastic fluid increases with the resistance that is opposed to it.

It does not seem to have occurred to them that it is an absurdity to suppose the resistance opposed to the pressure of the fluid should increase that pressure, but it manifestly is so. At the same time the experimental facts from which this conclusion was drawn are undoubted, and must therefore admit of some other explanation.

Piobert gives as the result of his experiments that the first pressure of the fluid, when one bullet is placed in the barrel, is equal to 2500 atmospheres; when two bullets, 2700; when three, 2880; and when thirteen bullets are used, 3040 (F. Gillion, *Sur les Canons Rayés*, p. 49). The proper conclusion from the experiments is, that the *total* pressure of the fluid during the passage of the bullets along the barrel is greater when their weight is increased; it being, as remarked above, manifestly absurd to suppose the *initial* pressure greater in one case than in another.

The explanation of the difference is to be looked for in a circumstance not taken any notice of in the theory, viz., the diminution of the heat, and consequently of the pressure, of the fluid, as it expands. In consequence of the heat of the expanded fluid being less, the pressure of the fluid will diminish much more rapidly as it expands, than is supposed by the theory; and every circumstance that tends to retard that expansion has the effect of maintaining the amount of the pressure at a higher magnitude.

Thus also is accounted for the very great value of the pressure found by Count Rumford—viz., 40,000 atmospheres. I propose to show presently that the value of this pressure has not been over-estimated by him.

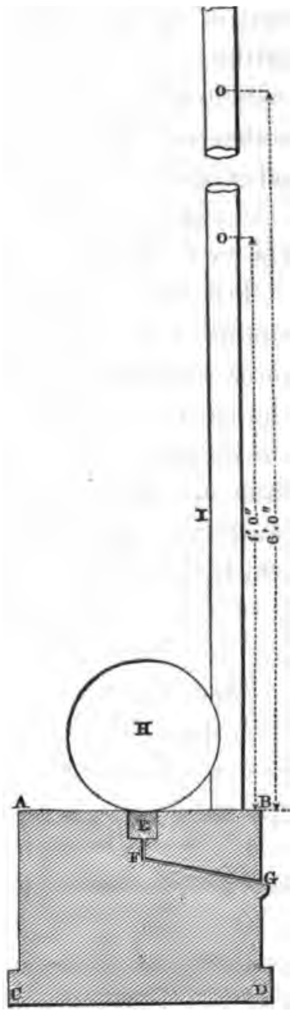
To explain this point more fully, it is very well known that when an elastic fluid is suddenly allowed to expand, so as to occupy a considerably larger space, its temperature at the same time falls, and therefore the pressure of the fluid is reduced more than it would have been, if, after occupying the larger space, its temperature had remained the same as originally. But the method by which Hutton and Robins have investigated the velocity of a ball moving in a cannon takes no account of this change of pressure as consequent upon the change of temperature, and is therefore unable to explain philosophically the increased tension of the elastic fluid, when the resistance to its expansion is increased.

It has been already remarked, that the recognized theory supposes the ball to be projected from a cannon simply by the pressure of the elastic fluid; or that the conversion of the powder into the fluid has no special action on the ball. This theory was probably put forward by its distinguished authors, not as representing correctly the actual process that takes place in the firing of powder, but as an approximation to the truth; for when we consider the violent nature of the action that takes place in the conversion of the powder into an elastic fluid, it seems highly improbable that this act of conversion should have no effect in moving the shot. It was probably thought by Robins and Hutton that this effect, if there were any, might be taken into account by assigning a larger value to the initial pressure of the gas. This is true to a certain extent with guns of the ordinary lengths, but when we apply the formula to mortars, the results given by it differ very widely from the truth. Indeed, the high velocities given to heavy shells, when discharged from mortars, cannot be explained on the ordinary principles, without assigning a very large value indeed to the initial force of the powder.

These considerations, in connection with the result of a long series of experiments, carried on by myself, in different branches of artillery, caused me to suspect that there must be something in the initial action of powder different from that usually supposed—in fact, that the ball begins to move in a cannon with a *finite* velocity, or that the initial action of the powder on the ball is *impulsive.* This being a conclusion opposed to the established views, I devised an experiment that should settle the question of *impulse** or *pressure* beyond all

* *Impulse* is not, perhaps, a strictly accurate term, but is used here for want of a better word. It is employed, in this case, to remark the distinc-

dispute. For this purpose I had an apparatus constructed, of which the annexed diagram represents a section. A B C D is a block of gun metal, in the upper part of which is a cavity E. From E proceeds a canal E F G, of small bore, terminating at G in a touch-hole. The cavity E, and the canal E F G, as above, being filled with powder, a cast-iron ball H, turned accurately spherical, is placed upon E, and the powder fired at G by a fuze.




In this apparatus a charge of powder of 1 dr. only was used, and a ball of 4 lbs. weight, and the ball was driven up to a height of about 5 feet 6 inches. With a chamber of twice the depth, and holding 2 drs., the ball was driven up to about double the height; and when a wooden ball of the same diameter was placed upon the chamber containing 1 dr., it was driven up to a height of about 30 feet; but the resistance of the air in the latter case must have been appreciable.

tion between the real action of the powder and the supposed action of *pressure*. Pressure would imply that the elastic fluid exerts a *continu us* force, arising solely from the elasticity of the fluid, and varying in pro-

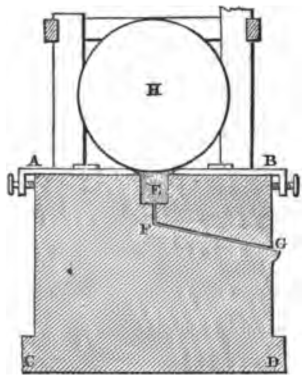
If the only force in the previous experiment had been the pressure of the elastic fluid, the ball would simply have been raised a sufficient height to admit of the escape of the fluid, and would have dropped back to its place without rising to any great height. This was the result anticipated by those friends interested in the subject of gunnery to whom I mentioned my views and my proposed experiment; and the very different result of the experiment shows the revolution that will be produced in the theory of gunnery, when these views are recognized.

The same experiment can be made under a form so simple as to admit of being performed by any person interested in the subject. If a pistol be taken, of which the barrel unscrews, and the barrel be taken off, and the chamber filled with powder, and the bullet placed thereon; then, on firing the pistol, the bullet ascends several feet, although there is no length of barrel through which the pressure of the elastic fluid could act.

In the experiment described above, when the ball was rested at a height of $\frac{1}{8}$th of an inch from the powder (see wood-cut), it was scarcely moved by the explosion of the powder. When the shot was placed close to the powder, but not touching it, it was driven upward about six inches.

portion to the spaces occupied by it; but by the word *impulse* I wish to convey the idea that the initial action of the powder is more like a sudden blow, and that its *continued* force is chiefly in consequence of the *gradual* conversion of the powder into an elastic fluid. The great force displayed by gunpowder arises, not so much from the elasticity of the fluid which is generated by its combustion, as from the actual *rapidity* of its combustion —its *explosive* force, in fact; which force, incredible as it may appear, has never been taken into consideration—in estimating the initial velocities of shot, etc.,—in any work on gunnery extant.

The inevitable conclusion from the experiment I have described is, that at the instant of explosion, the force of the confined fluid is so great that it requires no appreciable time to impart a finite velocity to a heavy ball; which is the characteristic of *impulsive* forces.




If the space behind the ball, instead of a charge of powder, were occupied by a fluid of the elasticity, and heated to the temperature, of the gas of fired gunpowder, the results would be very different; for in this case no finite velocity would be acquired by the ball at the first instant of its motion, and the initial pressure being the same in all cases, might then be compared to atmospheric pressure. It is, however, useless to attempt to compare a force so great, and so different in its nature from ordinary pressure, as that of fired gunpowder, with a finite pressure such as that of the atmosphere; and when experiments have been made specially with this view, the shorter the time during which the force of the gas has acted, and the more confined the powder, the greater has been the estimate of the force obtained; as is shown in the case of Count Rumford's experiment.

It appears, then, that the action of a charge in a cannon may be considered as of a compound character: consisting, firstly, of an impulse, which causes the ball to begin to move with a finite velocity; and secondly, of the pressure of the fluid generated from the powder; the latter of which only has hitherto been considered by writers on the subject of gunnery. The pressure will be continually augmented by the generation of fresh elastic fluid as the more perfect combustion of the powder takes place, until the whole is completely consumed; and when this is the case, the pressure of the fluid diminishes rapidly as it expands, and but little advantage is gained by giving greater length to the gun than is required to secure the complete combustion of the powder. Up to a certain quantity (peculiar to the bore of the gun, and the quality of the powder used), the whole of the charge may be *ignited* before the shot is sensibly moved; although *perfect combustion* (except of a comparatively small quantity) may not take place.

This theory will enable us to account for many facts indicated by practice and experiment, which have hitherto been very imperfectly explained. It is very well known that large charges of powder do not fire instantaneously; for a charge may be increased to such an extent that many of the grains will be driven out unfired. Also, that some kinds of powder ignite much more readily than others.

Now, supposing the action of powder to be such as I have just stated, it is clear that the initial impulse will be greater with a quick-burning powder, than with a less explosive kind. Thus is explained the result indicated by the valuable experiments of Major Mordecai, that quick-burning powder gave a higher velocity to a ball when fired from a mortar (*Experiments on Gunpowder*, p. 307); but slow-burning powders were preferable when a cannon was used. The old theory gave no explanation of this difference.

It also follows that every cause which retards the ignition of the powder—such, for instance, as mixing saw-dust or lime with it, using gunpowder not granulated, or placing the powder in a chamber of suitable form—will diminish the initial impulse, and, of course, increase the subsequent effect of the powder. Hence, again, the more slowly the powder burns, the greater is the length of bore required to allow of its complete combustion. In illustration of this, we may notice that the Affghans, and other tribes in India who manufacture their own powder, use small arms of very great length. All guns, too, in the infancy of artillery, were much longer than at present, and much weaker at the breech, as may be seen in the case of the old guns at Dover, at Woolwich Arsenal, and St. James's Park.

Robins gives an account of some experiments made with the long gun still to be seen at Dover, by Eldred, who was master-gunner at Dover in 1646, and wrote a book called *The Gunner's Glasse*, which is one of the earliest English treatises on gunnery. This gun carries a 10-lb. shot, and its length is about 65 calibres. Eldred fired this gun several times with a charge of 18 lbs. of powder, and the range of the ball was 1200 yards, at an elevation of 2°, and 2000 when the elevation was $4\frac{3}{4}$°. This is a much larger charge than it would be safe to employ with the powder now in use in the English service, and the range obtained is also greater than it would be possible to obtain with this gun by using a safe charge of the regulation powder.

This experiment agrees with the fact well known to those who are familiar with the practice of gunnery, that for some services the effect of powder is improved by mixing with it lime or sawdust, so as to diminish the rapidity of its ignition; and also suggests that this practice might with advantage be more extensively pursued. On the other hand, in a few experiments where a charge of some of the fulminating powders, which ignite with extreme rapidity, has been used, the effect has been to burst the gun, while the ball is projected but a very short distance. This experiment was made by Count Rumford, and subsequently by Gen. Piobert. Gun-cotton has an effect, when used in guns, intermediate to those of fulminating powder and gunpowder.

These examples are sufficient to demonstrate the great importance of attending to the *quality* of the powder used, having regard to the service to which it is intended. As remarked already, the old theory of the action of gunpowder took no account of this difference in the action of different kinds of powder.

Having thus noticed the effects of using various kinds of powder, I proceed to the consideration of the change in the action of powder consequent on using larger guns, and proportionately larger charges of powder.

I have already stated that it has been long known as the result of observation, that large guns require a much smaller comparative length than small ones, in order to give the same velocity to a ball; also that they require to be made much stronger in proportion at the breech. These facts are in direct opposition to the conclusions from the old theory, and have never, so far as I know, been satisfactorily explained upon sound theoretical principles.

These facts, together with the great tendency of large guns to burst, as well as the results of some experiments of my own, suggested to me that there must be a much greater increase of pressure when large charges of powder are used than had been hitherto supposed; more particularly at the instant of ignition.

It has been customary to attribute the increased effect of large charges of powder to the greater heat attending the explosion; but this seems an inadequate cause, even if the supposition of the greater heat be correct. In order to ascertain the initial effect of using a large charge of powder, I repeated the experiment described at pp. 131, 132, with apparatus of exactly double the size in all directions, so that the weights of the ball and of the powder were eight times those used in the former case. The result was, that the larger shot of 6 inches diameter was driven to a height about *double* that to which the smaller rose; that is to say, to a height of about 11 feet.

These experiments have since been repeated in the Royal Arsenal at Woolwich, in presence of the sub-committee appointed for the purpose.

From the nature of the experiment, the whole effect is produced by the initial action of the powder, the subsequent expansion of the fluid having no effect on the ball (for *neither* shot was moved when placed one-eighth of an inch from the powder). The chamber which contained the larger charge exhibited a very different appearance, after the discharge, to that of the smaller; the former showing unmistakable signs of the greater increase in the force of the powder. When the larger shot was placed upon the smaller chamber it was projected to a height of about six inches, the chamber after the explosion showing no signs of a greater force having been exerted upon it than when the smaller shot was placed upon it; also, when the smaller shot was placed upon the larger chamber, it was propelled to a height of quite seventy or eighty feet, the state of the chamber after the explosion giving evidence that as great a force (or very nearly so) had been exerted upon it, as when the larger shot was fired from it. Hence it follows that the initial impulse of the powder on the ball is greater in proportion when a large charge is used than with a small one. My experiments seem to indicate that, within certain limits, the initial velocity imparted to the ball varies as $\frac{w^{\frac{4}{3}}}{w'}$, where $w$ is the weight of the powder, and $w'$ the weight of the ball.

This greater impulse is easily accounted for, since the quantity of the powder, and consequently of the gas generated, is as the cube of the calibre, while the space to be traversed by the flame is only as the first power; and, therefore, the quantity of powder ignited in the same proportionate time will be much greater in proportion. It is remarkable that this circumstance, which at once affords a simple explanation of the greater comparative force of the charge of powder in larger guns, and puts aside all the vague hypotheses which have hitherto obtained respecting the greater heat generated by the combustion of powder in larger quantities, *has* *never yet been noticed.* The fact that in my experiments the ball was placed in the chamber so as to be propelled in a vertical direction, does not affect the initial velocity of the ball. If any proof of this were required, it would be supplied by the statement of Captain Boxer, in his *Treatise on Artillery* (p. 79), that from experiments made at Metz, in 1840, with the ballistic pendulum, it appears that the inclination of the bore of the gun does not affect the initial velocity.

Since, then, the powder, in large charges, ignites much more rapidly than in small, supposing that the charges of powder are of the same kind and placed in similar circumstances as regards the *form* of the chamber, it follows that the whole of the powder will be consumed in a shorter proportionate time, when the gun is large, than when it is smaller, and therefore a shorter proportionate length will be required in the bore, to give the same velocity to a ball.

Again: since the impulse on the ignition of the powder in a large gun is greater in proportion than in a smaller one, the large gun must be made of greater proportionate *strength* at the breech, in order to resist the force of the explosion.

Supposing that the impulse were simply proportional to the amount of the powder, it would suffice to make the thickness of the gun at the breech proportional to the calibre; but, as we have seen that the impulse increases in a higher ratio, the thickness of the gun must also increase in a higher ratio. This is well known to be the case in practice.

From this it is seen that the shorter comparative length, and the greater comparative strength at the breech, required for large guns than for smaller ones, are both consequences of the different action of the powder when used in larger quantities.

Some writers (Captain Blakely amongst others) have attempted to show that the greater liability of large guns to burst arises from their being subject to a severe strain for a *longer time* than short ones.* It is argued

* In an article in the *Mechanics' Magazine* of 26th September, 1857, "On Improvements in Ordnance," by Captain Blakely, R. A., are the following remarks: "Large guns require more strength than small ones, as the powder occupying in each the same proportional space, the small shot moves in, say, $\frac{1}{300}$th of a second, a certain number of inches, the large shot in the same time moving fewer inches; so that at the end of that time, the gas in the small gun would have much more proportional room to expand in, and would therefore press less on the gun than in the larger one. Added to this, the large shot would require more time to get its velocity, and the pressure must remain on the gun so much longer."

The views embodied in the above extract agree perfectly with the ordinary theory, but are quite contrary to my experiments. According to those views, a greater pressure is first exerted upon guns of large size *after* the shot has commenced moving; whereas it has been shown by my experiments that a greater proportionate strain takes place at the first instant of the explosion. In the former case, the breech end, or seat of the explosion—and, indeed, all the relative parts—in large guns, would require to be of the same proportionate strength only as those of smaller calibre. In the latter case, it would have to be of a *greater* proportionate strength.

With regard to the theory that larger guns would require strengthening because the pressure (supposed in each case the same) of proportionate charges of powder acts for a *longer time* upon large guns than upon small, we have no evidence to show that a metal so slightly elastic as cast iron will resist a pressure which will cause it to break when the pressure is continued for a longer time.

Captain Blakely appears to have overlooked the fact that small guns are submitted to a much severer test in the *proof* than large guns—the proof charge of a 9-pounder, for instance, being 9 lbs. of powder, or three times the service charge—whilst that of a 68-pounder, 112 cwt. gun, is 30 lbs., or one-half more only than the service charge. Supposing, therefore, that Captain Blakely's (or the ordinary) theory of pressure were correct, large guns would never, under any circumstances, be subject to the strain which small guns suffer in the proof.

that the large ball begins to move more slowly than the small one, and that consequently a strain is exerted for a longer time on the larger gun; but it is seen from the result of my experiments, that the larger ball actually begins to move more rapidly than the small one, and therefore the large gun is subjected to the strain for a shorter, instead of a longer time; but so greatly does this strain increase with the size of the gun —from the actual increase in the initial pressure—that it is a matter for inquiry how it is that large guns are able to resist the force of the explosion at all, rather than why they burst.

Thus we see how readily the theory of the action of gunpowder here put forward explains and reconciles experimental facts hitherto at variance with theory.*

* It was attempted to account for the greater height to which the larger shot was driven in my experiments by the supposition that the fluid of the exploded powder continued to act upon the shot after it was in motion. If this were the case it must have done so for a very short distance, for (as I have explained) when either shot was placed ⅛th of an inch only from the chamber, the charge failed to move it. But allowing that the fluid acted in this manner, it must have exerted the same influence on both shot, and explains in no way why the larger shot was driven to double the height of the smaller: on the contrary, if any thing, it would influence the smaller (supposing the initial force of the powder the same in both instances) in a higher degree than the larger; its surface, as compared with its weight, being greater.

Putting aside the futility of an argument which would tend to show that a heavy body in motion could receive a considerable accession of velocity from the *pressure* of a fluid which is allowed freely to expand in the open air; the fact that the larger shot, when placed nearly *on* the powder, that is to say, in a position $\frac{1}{32}$d part of an inch only further from the charge than when it rested on the chamber, received a velocity which drove it up one foot only, proves, by the second law of motion, that the ball must have received its whole impulse (minus that which would drive it up one foot) in the space of $\frac{1}{32}$d of an inch, which is sufficient evidence that the force is an impulsive one.

It was asserted that there was no proof that large guns were required to

The theory now advanced, that the initial action of powder upon a projectile is *impulsive*, instead of being that of ordinary pressure, has scarcely even been hinted at by any writers on the subject of gunnery. They have all, with one or two exceptions, supposed that the whole action of powder is that which is due alone to the expansive powder of the generated gas; although admitting that there is a good deal of obscurity about the action of powder on this supposition, as well as that there are some facts extremely difficult to explain.

Gen. Piobert in his treatise (*Cours d'Artillerie*, p. 48) relates an experiment made in 1826 by Gen. Pelletier. Several pounds (4) of powder were spread on a light wooden table, which was placed upon soft earth; the powder, being inflamed, caused only a slight depression of the table; but when the experiment was varied by placing a sheet of paper over the powder, the table was shattered to atoms. Piobert concludes from this experiment that the action of the powder is a pressure which increases with any resistance opposed to it; but the experiment, on the contrary, proves that the initial action is *impulsive*, since it would have required but a very moderate *pressure* to remove the paper—a pressure, in fact, that would have exercised no injurious effects on the table.

be of less length (in calibres) than small ones. But the correctness of my theory, that the same comparative charges, when fired in guns of various calibres, must be converted into fluid much more rapidly in large guns than in small, was admitted: whether, therefore, the initial action of the powder be that of an impulse or a pressure, it must necessarily be admitted that it is *stronger* in large guns. It follows, therefore, as the shot must receive a given velocity, in either case, in a comparatively shorter space with large guns than with small, that the former will require to be of a fewer number of calibres in length than the latter.

I mentioned that there were one or two writers who had ventured to offer different opinions from those usually entertained respecting the initial action of powder. One of those writers, Count Rumford, assigns a very large value to the initial force of fired gunpowder, and a very small value to the elasticity of the fluid. He supposes that this great initial force consists in the temporary action of a fluid not permanently elastic. At p. 232, vol. 87, *Phil. Trans.*, he has these remarks: "There is no doubt that the permanently elastic fluids generated in the combustion of gunpowder *assist* in producing those effects which result from its explosion; but it will be found, I believe, upon ascertaining the real expansive force of fired gunpowder, that this cause alone is quite inadequate to the effects actually produced; and that, therefore, the agency of some other power must necessarily be called to its assistance," Count Rumford, however, made no attempt to apply this practically.

The second of these writers, Capt. Boxer, in his *Lectures on the Science of Gunnery*, printed in 1854, remarking upon Count Rumford's experiment for ascertaining the force of fired gunpowder, observes, "that the effect there produced is not merely that of an ordinary pressure steadily applied; but rather the effect which would result from a body in motion coming in contact with a body at rest; or, in other words, be similar to a blow."

In Capt. Boxer's *Treatise of Artillery*, which was published two years later, he, too, fails to apply this in any way to the theory of gunnery, or the construction of cannon, for he supposes, with other writers, that the initial force of the powder upon the shot is the same in all cases: and in all the formulæ there collected, given for ascertaining the velocities of shot, the action of the fired gunpowder upon the shot is assumed to be that of *pressure*, or that which is due to the elasticity of the fluid alone; an assumption which is totally at variance with his previous remark on Count Rumford's experiment; unless, by some extraordinary train of reasoning, he supposes that the powder acts with an impulsive force on the sides of the chamber in which it is completely confined, but not upon a shot placed before the powder in the chamber of a gun.

All the experiments hitherto made with cannon have failed to show the initial action of the powder, because they gave the result of the *total* action, without distinguishing the initial from the subsequent action.

The experiments of Capt. Dahlgren, of the United States Navy, showed that a greater strength was required at the breech with large guns than was previously supposed, and that the requisite strength diminished rapidly from the breech to the muzzle.

These experiments, of which no detailed account has been published, were made by boring a series of holes in a 68-pounder gun, perpendicular to its length. All of these holes being plugged, with the exception of one, a bullet was placed in this, and the cannon fired in the ordinary way. The velocity with which the bullet issued being determined by the ballistic pendulum, indicated the pressure of the gas in the bore of the cannon at that point of its length, and, consequently, the thickness of the gun necessary to resist that pressure. These experiments showed a much greater pressure at the breech than had been previously supposed, and the Americans have constructed guns of the figure thus indicated. This perfectly agrees with the theory of the action of powder here laid down, but it would not have been possible to deduce the theory from these experiments alone.

To recapitulate the conclusions to which my experiments point, and which have never been put forward previously:

First. The initial action, when a charge of powder is fired in a gun, is an impulse* which causes the ball to commence moving at once with a finite velocity.

Secondly. When the charge of powder is increased, this impulse increases in a higher ratio than that of the increase in the quantity of powder; and, consequently, the subsequent action of the powder is comparatively diminished.

This circumstance, which, strange to say, has escaped the observation of even the most eminent writers and experimentalists, is due to the fact of the gradual conversion of the powder into an elastic fluid.†

Instead, therefore, of a finite pressure of so many

* Subsequent investigation showed me (as I have remarked) that Count Rumford and Captain Boxer had hazarded similar conjectures, although neither appears to have been aware of the important consequences which would follow any proof of the incorrectness of the ordinary theory of pressure, and therefore made no attempt at any practical application of their ideas.

† The question whether the whole of a charge of powder is fired before the shot is moved, was determined in the year 1742 by a Committee of the Royal Society, formed, at the suggestion of Dr. Jurin, for the purpose of making some experiments in connection with this subject. The results,

atmospheres, as supposed in the formulæ at present in use, we find that powder exerts a force of a totally different character. It would, in fact, be as correct to estimate the force of impact of the ball itself, at so many atmospheres. We find, also, that a portion only of the force of each charge is exerted before the shot is moved, and that this portion increases in a certain ratio with the calibre of the gun.*

These propositions will not only explain, as I have shown, many experimental facts hitherto thought anomalous, but will produce many changes, amounting almost to a revolution, in the construction of cannon and the use of powder.

In addition to the facts already mentioned, the following may supply illustrations of the value and importance of the knowledge of the true action of gunpowder:

In the first place, it will be seen, by consulting the tables of the ranges of shot and shells in various books on gunnery, that mortars of different calibres, fired with proportional charges of powder, will project their shells

which proved that the whole of the charge was *not* fired, may be seen in vol. 42, p. 172, of the *Philosophical Transactions.*

That these results were not turned to practical account, probably arose from the defective state of the theory of the action of powder; and it was, no doubt, imagined that the effect produced by the gradual ignition of the powder, was *proportional* in guns of all calibres.

* Hence, we may account for the difference of opinion which has existed respecting the initial force of fired gunpowder. Robins, who experimented with a musket with a bore less than an inch in diameter, estimated the first pressure at 1000 atmospheres; Hutton, whose experiments were conducted with cannon of the smallest calibre—somewhat under two inches—considered it equal to 2000 atmospheres (Hutton's *Tracts*, vol. 3, p. 302); whilst Count Rumford, who confined the powder completely, estimated it, at least, at 40,000 atmospheres.

to distances which vary nearly as their calibres; whilst cannon of different calibres project their shot to distances which vary nearly as the *square root* of their calibres.

No existing theory affords the least explanation of this circumstance, nor have I seen it noticed in any work on gunnery; it is, however, easily explained by the help of the conclusions first stated. It is seen that the action of the powder on a shell fired from a mortar is limited almost entirely to its initial action, in consequence of the shortness of mortars. Now it is proved by my experiments that this initial force increases in a higher ratio than the weight of the powder, and, therefore, shells discharged from large mortars will have a higher velocity than those from small mortars. On the other hand, in cannon, the *whole* action of the powder is to be considered, and there is no reason to think that this total action, as distinguished from the initial, increases in a ratio very different from that of the weight of the powder, so that the velocities of balls fired from cannon of the ordinary length (with proportional charges) will be nearly the same.

Thus the ranges of the balls will not increase so rapidly as those of the shells.

One of the chief errors which will be exploded by a recognition of this theory, is that which has hitherto existed with regard to the initial effect of the charge of powder upon the *gun*. We now see that this becomes relatively greater in a ratio, which can be determined by experiment; and also, as the gun is increased in calibre, that the shot is moved from its place with a greater rapidity; and therefore the strain upon the breech of the gun increases progressively with the calibre.

Hence, a 68-pounder gun, fired with the service charge of powder, *i. e.*, a third of the weight of the shot, will have nearly the same strain upon the breech end (supposing the powder to be all of the same quality) that a 9-pounder gun (which has a bore of half the diameter) would experience were the proof charge continually used: so that, in fact, a 9-pounder, of similar proportions to a 68-pounder gun, has, relatively, at least double the strength at the breech end; or twice the power of resisting the first strain, caused by the discharge before the shot is moved. We need scarcely wonder, then, at the liability of large guns to burst. It shows how fallacious have been the opinions respecting the relative strength of cast-iron guns.

The accompanying figures will serve to illustrate the proportionate increase in the strength and length* required for the guns of various calibres.

Some idea may be formed of the force exerted upon

* Since it appears that the rapidity with which shot of different diameters are first moved, when fired with proportionate charges, is in the ratio of the square root of the calibre, we may reasonably infer that the length (in calibres) of guns of different calibres should vary nearly in the same ratio. For example: at page 126 (note) I have given an extract from Major Mordecai's "*Experiments*," showing the different velocities acquired by a 24-lb. shot, and a musket bullet, when fired with proportionate charges of powder. Here the diameter of the 24-pounder gun was 5·82 inches, that of the musket 0·69 inches; the former being rather more than eight times the diameter of the latter. If, therefore, we multiply the length (in calibres) of the 24-pounder gun (18·5) by 2·82 (the square root of 8), it will give more than fifty calibres—the length required for the musket, in order to give the bullet the same initial velocity as the 24-lb. shot. In the instance given at page 126, it will be seen how very nearly this approaches to the truth—the large shot acquiring, indeed, rather a higher velocity than the bullet.

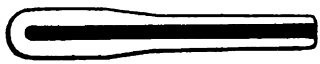


Fig. 1. Diameter of Bore, 8 inches. Length of Bore, 25 calibres.

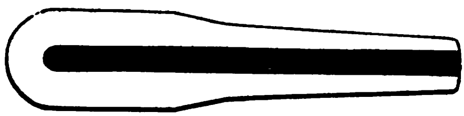


Fig. 2. Diameter of Bore, 6 inches. Length of Bore, 17·75 calibres.

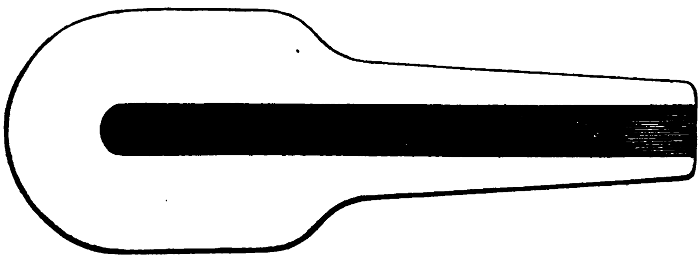


Fig. 3. Diameter of Bore, 12 inches. Length of Bore, 12·75 calibres.

the gun by the powder, before the shot is moved, as compared with that which is subsequently exerted upon it, from an experiment which I made for the purpose of ascertaining the proportion which the initial force of the charge of powder upon the shot bore to its *whole* force. I caused a tube, three inches long (the diameter of the smaller ball in the former experiment), to be screwed tightly on to the block of gun-metal in which the chamber, containing one drachm of powder, was formed. The cubic content of this tube was 280 times greater than that of the chamber. Upon firing the charge of one drachm, the ball was driven to the height of about 30 feet, or about six times the height to which it was driven without the tube. Upon employing a tube of rather more than 2 feet in length (the cubic content of which was 2000 times greater than that of the chamber containing the powder), the difference caused by the use of a longer tube was scarcely perceptible. The initial force, therefore, in this instance, appeared to be to the whole force nearly in the ratio of 1 to $\sqrt{6}$; the resistance of the air, in either case, being scarcely appreciable.

In these instances the whole force of the powder, excepting that which escaped by the windage, acted upon the shot; the size of the tube being in such proportion to the size of the chamber as to allow of this. When we consider that the tube, or bore, of a cannon is rarely more than eight or ten times the size of the part occupied by the powder, it may be imagined how large a proportion the initial force of the charge of powder in a gun must bear to the whole force.

Several facts which have remained altogether unknown, or respecting which vague hypotheses only have been formed—such as the first movement of a shot when the charge of powder is ignited—the initial effect of various charges in the same gun—both upon the gun and upon the shot—with many other circumstances of greater or less importance, may now be clearly ascertained, and the knowledge thus acquired applied to the construction of guns, so that they may be made in future in a manner which, while economizing the metal, will preclude the possibility of their bursting from any other cause than defects in the metal itself.

Another important practical fact may also be deduced from this new theory, namely, that when the form or make of guns of different calibres is constant, the *quality* *of the powder*—to give the shot equal velocities—must vary according to the calibre of the guns; and, when the quality of the powder is constant, the *form and proportions of guns* must vary with the calibre.

It would be out of place here to attempt a mathematical investigation of the full effects of this action of gunpowder, now for the first time pointed out. I may notice, however, that in place of the formulæ given by Hutton and Robins for the velocity of a ball issuing from a cannon, viz.: $v^2 = \frac{\pi g m n a d^2}{w} \times log \frac{b}{a}$, where $b$ is the length of the bore, and $a$ the length occupied by the charge of powder, we shall have the following, $v^2 = V^2 + \frac{\pi g m n' a d^2}{w} \times log \frac{b}{a}$, where V is the velocity with which the ball begins to move, and is to be determined by experiment for each particular kind of powder, and for each charge. A sufficient number of experiments would enable us to assign an empirical formula for V, which will probably vary inversely as $w$ (the weight of the shot) as in the formula I have already suggested.

All the experiments which I have as yet made indicate that V does not increase in proportion to the weight of the powder used, but in a higher proportion. The quantity $n'$ in the formula must depend for its value partly on that of V, being smaller, as V is larger, so that $v$ will be a function of the weight of the powder used, or rather of the fraction $\frac{w'}{w}$, $w'$ being the weight of the powder, and $w$ of the shot.

Before concluding this subject, there is a circumstance attending the firing of several balls from the same gun to which I would particularly call attention, as it bears both upon the question of the complete ignition of the charge before the shot is moved from its place, and upon the nature of the action of gunpowder generally.

When Robins wished to prove beyond a doubt his principle, that the whole of the charge was converted into an elastic fluid before the shot was sensibly moved from its place, he made the experiment of placing two or three bullets in the same gun, instead of one, firing them with the same charge of powder with which he had already fired the single ball.

He observes (*New Principles of Gunnery*, p. 80): "I considered that if a part only of the powder is fired, and that successively, then, by laying a greater weight before the charge (suppose two or three bullets instead of one), a greater quantity of powder would be necessarily 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; by firing one, two, or three bullets laid contiguous to each other, with the same charge respectively, I have found that their velocities are not much different from the reciprocal of the sub-duplicate of their quantities of matter. * * * 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 through 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 sub-duplicate ratio of their quantities of matter. * * * 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 through 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 experiments."*

Now if the existing theory of the action of gunpowder (that it acts by *pressure*) is correct, Robins is right in his conclusion, and the whole of the charge must under-

* Robins further observes, that although, in general, the velocities were reciprocally in the sub-duplicate ratio of the number of bullets, yet they were sometimes greater; but never more (when three bullets were fired) than one-eighth of the whole. If the reader will turn to the third paragraph at page 128, he will see how closely this agrees with Piobert's experiments. Robins, however, differs from Piobert in his manner of accounting for the excess of velocity; instead of ascribing it (as Piobert does) to the increased tension of the elastic fluid, he supposes that the flame, escaping by the windage, past the first bullet, acts upon those beyond it.

A series of ballistic pendulum experiments, with elongated projectiles, would throw considerable light on this question; and I believe it will be found that the explanation which I have given at p. 129 is the correct one.

go perfect combustion before the shot is moved; but if, as we know to be the case by experiment, the whole of the charge is *not* consumed before the shot is moved, Robins's conclusions, as well as the present theory of pressure, must be erroneous. If, however, we accept the fact that the first action of fired powder is *impulsive*, we have a solution of the whole question, as in that case the whole of the powder need not necessarily be consumed before the shot is moved. The relative velocities acquired by the balls may also be easily accounted for, since it is clear that the *whole* force of a charge of powder (in a gun of a length sufficient to insure the perfect combustion of the charge) can neither be increased nor diminished by altering the weight of the object to be moved; although the manner of its action will vary with the form of the chamber, or the manner in which the charge is placed therein.

In another experiment of Robins's (*New Principles of Gunnery*, p. 116), he placed the bullet eleven inches from the breech of the gun, and instead of confining the charge in the usual manner, he scattered it behind the ball in as uniform a manner as he could. The result was, that the velocity of the ball was considerably diminished; an effect which he ascribed to the "intestine motion of the flame;" and he remarks, that "the accension of the powder thus distributed through a so much 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 in the case of all other fluids) be considerably diminished."

A better explanation of this diminution of force will, however, be found in the theory I have put forward, as in this case the explosive action of the powder would be very much diminished in force by being scattered (as in Robins's experiment), owing to the slowness of its ignition.

The force of a charge of powder was found (by Count Rumford, *Phil. Trans.*, vol. 71. p. 277) to be weakest when the *vent* was placed at the top of the charge, *i. e.*, near the ball—especially with large charges. This was probably owing to the fact that the first impulse of the powder upon the shot was weaker, on account of the smaller portion, which (in this case) was lighted before its action on the shot took place. At page 273 of the same volume, Count Rumford remarks that nothing can with certainty be determined with respect to the best form of chamber for pieces of ordnance, or the best situation of the vent; nor can the force of powder, or the strength that is required in different parts of the gun, be ascertained with any degree of precision, until the manner of the initial action of the powder is known.

When we consider how very defective is the old theory of gunnery, it is remarkable that no improvements of an important kind have been made in it for fifty or sixty years. This is probably in a great measure traceable to the opinion held by all military men, that the conclusions of Hutton and Robins are perfectly true and incapable of correction; thus adding another example to the many already existing, of the injury that is done to the cause of science and truth by a blind, unreasoning deference to "authority." Whatever alterations have taken place in the practice of gunnery and in the construction of ordnance during the last half century, may nearly all be traced to the difference in the quality of the powder at present, to that which was formerly in use;* as an attentive consideration of this question will show.

There are, at least, four elements of force to be considered in the action of fired gunpowder. 1st. That which proceeds from the sudden conversion of the powder into a fluid; 2nd. The elasticity of the fluid itself; 3rd. The expanding force of the fluid when heated to a certain temperature; 4th. The higher temperature which it retains, and also the *time* which is allowed for its more complete combustion, by confinement. The powder at present in use being a much stronger and quicker burning powder than that which was formerly used, its initial action has, therefore, as regards all the above circumstances, a much higher value—its force, in fact, being more *concentrated;* whence it follows that the guns of the present day require to be shorter and stronger.

Probably if two kinds, one a quick, and the other a slow burning powder, were both completely confined, the pressure produced by the elasticity of the fluid generated by the latter would be as great as that produced by the former; although the quick-burning powder would exhibit a greater explosive, or fracturing force, owing to the more sudden exertion of its force. By using a quick-burning powder in gunnery, we obtain (from its rapidity of conversion) a greater force in a smaller space. Thus, we see, in blasting, a quick-burn-

* Appendix C.

ing powder is not so effective; since, the force of the charge being expended with a rapidity in proportion to that of the ignition of the powder, no time is allowed for overcoming the inertia of the substance required to be moved.*

Now the chief reason why gunpowder is preferable, as a projectile force, to all other explosive compounds, is because its combustion is more *gradual*; and, although an extremely quick burning powder may be good for mortar practice, it is extremely questionable if it is so good for ordinary gun practice. In fact, the quickness of ignition may be carried, in this case, to too great an extent; for it may be increased to such a degree that no gun would bear the strain caused by the initial action of the charge.

It is hoped that these remarks will contribute, in some measure, to the improvement of what must be acknowledged to be a noble science; but it will require a long and careful series of experiments to perfect the theory of the action of fired gunpowder. It is, however, confidently thought that the suggestions here made will indicate the proper direction for experimental inquiry, and lead, eventually, to most important results in the theory and practice of gunnery.

* For this reason, a quick-burning powder will be the best to employ for shattering or bursting open the gates of a town or fortress. Piobert's experiment (page 142) suggests the idea that the effect of the explosion might be considerably heightened by opposing a plane surface to the action of the powder on the contrary side to that against which the force is directed. A flat iron screen, which the men might push or carry before them when placing a bag of powder, would serve both to protect them against musketry, and (if it were afterwards left standing close to the powder) to increase the effect of the explosion.

[The correctness of the foregoing theory may be tested in a very simple and conclusive manner, by the *recoil* of guns. If two guns, constructed of similar metal, one being exactly of twice the linear dimensions of the other, were fired with proportionate charges of powder, and (in other respects) under precisely the same circumstances, the *recoil* would show the force expended in each gun. The argument, that the fluid acted upon the shot (in my experiments) after it had left the chamber, and, therefore, that the experiment was not conclusive, could not possibly be brought forward in this instance; since the fluid, after it had once escaped at the muzzle, could not be supposed, even by the most inveterate quibbler, to act upon the gun in a manner to increase the recoil. The recoil would, therefore, be a very conclusive test of the relative force exerted by the powder on each gun. (I am at a loss to understand, by-the-way, how the argument, "that the fluid acted upon the shot after it had left the chamber," can be reconciled with the axiom laid down by Robins (*New Principles of Gunnery*, Prop. 7, p. 74), and found in all works on gunnery, that "*the action of the powder on the ball ceases as soon as the ball is got out of the gun.*") A matter of this importance is surely a worthy subject for experiment; especially when the trouble and expense attending it would be so small.]

# ADDENDA.

## ON THE NATURE OF THE ACTION OF FIRED GUNPOWDER.

The theory put forward in the preceding pages appears to have been so much misunderstood, that I am induced to offer a few additional remarks upon it.

It has hitherto been assumed that the initial pressure of the fluid against the sides of the chamber and upon the shot is constant in amount, and equal in all directions; propositions which are not borne out by experiment.

For it is evident that when the fluid of fired gunpowder is excited to action by the agency of heat, a great commotion must take place in the chamber of the gun; and that the magnitude of the effect then produced must mainly depend upon the number of the atoms of the fluid which are set in motion in a given space*

* "All gases and vapors are assumed to consist of numerous small atoms, moving and vibrating in all directions with great rapidity; but the average velocity of these vibrations can be estimated when the pressure and weight of any given volume of gas is known, pressure being, as explained by Joule, the impact of those numerous small atoms striking in all directions, and against the sides of the vessel containing the gas. The greater the number of these atoms, or the greater their aggregate weight, in a given space, and the higher the velocity, the greater is the pressure. A double weight of a

and in a given time, and upon the celerity with which the whole charge is affected by the heat.

The exact nature of the action which takes place can only be ascertained by proper chemical analysis; but whatever its nature, it is clear that there must be a certain quantity of mechanical work performed,—that is, a pressure must be exerted through a certain space,—before the fluid can exert any pressure against the sides of the containing chamber; and that the value of the pressure which then takes place must depend upon the quantity of work performed in a given space and time.

The amount of force which is exerted by different quantities of powder in guns of different sizes appears to be a question of the *magnitude of the pressure at a given time*, rather than (as ordinarily supposed) of the *time of action of a given pressure.*

In the absence of any fixed or reliable *data* concerning the chemical action which takes place during the combustion of a charge of powder, I offer the following propositions for consideration; not as affording the most complete or satisfactory solution of the question of the relative force of different charges of powder, but to account in some manner for the fact—which I believe to be indisputable—that the force of powder attains a much higher magnitude in large guns than in small:

Firstly, That the *initial pressure* of the fluid of fired gunpowder upon the sides of the chamber of a gun, or

**perfect gas, when confined in the same space, and vibrating with the same velocity—that is, having the same temperature—gives a double pressure; but the same weight of gas, confined in the same space, will, when the atoms vibrate with a double velocity, give a quadruple pressure."—*Encyclop. Brit. 8th Edit., Art. Steam.***

upon the shot, is in proportion to the *relative amount of 'work' done previously*, or to the momentum acquired by the particles of fluid in passing through a given space —and will therefore vary according to the rapidity of combustion of the charge or quantity of fluid set in motion in a given time, the heat, density, and velocity of the fluid, and the celerity with which the whole is affected by the degree of heat—before the action of the fluid takes place upon the sides of the chamber, or upon the shot.

Secondly, That the *whole force* of the explosion of a charge of powder is represented by the *amount of work which must be done before the particles of fluid can be brought to a state of rest.* This force, therefore, can only be measured by the resistance which would be necessary to bring these particles to rest.*

From the first of these propositions it will follow, that the amount of initial pressure will depend upon the size and form of the chamber, upon the quality and disposition of the powder, the manner of its combustion, etc.; and from the second, that the amount of pressure exerted against the sides of a containing vessel of a given size, during a given time, will depend upon the nature of the resistance which is offered to the motion of the fluid during that time. Thus may be explained the circumstance that when powder is not free to expand, or is completely confined, it exhibits a force so much

* If we suppose a quantity of the fluid brought to a state of quiescence in a chamber, it would then—and not till then—exert a uniform pressure in all directions, in proportion to its density and elasticity; and this pressure is practically considered, in all works on gunnery extant, to be the only force produced by fired gunpowder.

superior to that which is assigned to it by persons who have formed their estimate of its force from the velocities acquired by shot only. It is one of many circumstances which admit of no reasonable explanation by the ordinary theory. The only doubt which at first existed in my mind respecting the truth of the proposition that an increased resistance causes a greater exhibition of force, arose from the circumstance that when, in my experiments, I placed a larger shot upon a small chamber, there was no appreciable difference in the amount of force produced in the charge; but this can be explained by the circumstance that the initial action of the powder was so violent that the difference in the time required simply to move the larger shot was inappreciable.

The combustion of gunpowder, although extremely rapid, is not instantaneous. It follows, therefore, that the number of particles of fluid set in motion in a given time must vary both with the quality of the powder and with the size of the chamber. This was clearly shown in my former experiments, for, when the linear dimensions of a chamber containing powder were increased to twice the size, a shot of eight times the weight was moved to exactly twice the height. In this instance the quantity of powder ignited was eight times, and the distance traversed by the flame twice as great; so that we may reasonably assume that the work accumulated in the charge before its action on the shot took place, and consequently the motion communicated to the shot, were comparatively much greater.

Thus may be accounted for the different estimates formed by various writers of the initial force of fired gunpowder; the experiments of some having been made with guns of small, and those of others with guns of large calibres. The pressure exerted in the chambers of very large guns (especially when rifled, and when the friction produced by the first movement of the projectile is great) will be found, I believe, of much greater value than any which has yet been assigned to the initial pressure of fired gunpowder, except that which was given by Count Rumford.

It may naturally be asked how it happens that the considerable difference in the force of gunpowder which is exerted in the chambers of guns of different sizes, was never discovered before; but this is easily explained. A very limited number of persons have interested themselves sufficiently in the question to make experiments upon it, and those who have made experiments, have drawn their ideas, and made their calculations, of the initial effect of powder upon shot, chiefly from observing the *whole* effect produced by the powder upon the shot when fired from guns of different calibres; and not from observing the effects produced in chambers containing a quantity of powder only.

Now, since the work done in the chamber of a large gun before the shot is sensibly moved, is relatively much greater than the work done in the chamber of a small gun, it follows that the whole time of action of the charge will be relatively of shorter duration; hence *length* (in calibres) is not of the same importance in large, as in small guns. There are also other reasons why an addition to the ordinary length of large guns adds very little to the velocity acquired by the shot.

It is well known that the expansive power of the elastic fluid is enormously increased by the heat which attends the combustion of the powder;* now, the first impulse given to a large shot being greater, it is moved through the first portion of the bore with greater rapidity, and, consequently, the temperature falls much more rapidly than is the case in smaller guns, so that the quantity of energy lost in the same time is comparatively much greater, and the accelerating force of the powder is thus considerably diminished. Also, there is a greater degree of resistance at first offered to the motion of the fluid, and consequently a more rapid expenditure of force.

Robins's experiment of placing the ball in a musket at some distance from the powder—under which circumstances he obtained a velocity of 200 feet a second greater than when the ball was placed in immediate contiguity with the charge—may be explained by the circumstance, that the momentum or moving force acquired by the particles of fluid before they encounter the ball is much greater when the ball is placed at a little distance from the charge; consequently, a higher velocity will be acquired by the shot in a given time. But the distance at which the shot can be placed from the charge, with increased effect, will vary with the length of the gun and the quantity of powder contained in the charge.

* The temperature in large guns is probably heightened from the comparatively greater density of the fluid in the chamber, arising from the more rapid and complete combustion of the powder; there is no doubt that the latter produces a higher velocity in the motion of the particles of fluid.

In the same manner may be explained the circumstance that when (in my own experiments) I increased the *depth* only of the chamber, and then filled it with powder, the same ball was driven to a much greater height; the fluid acting through a greater space before it took effect upon the shot, time was allowed for the more complete combustion of the powder; and therefore the motion of the fluid was considerably accelerated, and the impulsive action increased.

It is remarkable that so many persons should have misunderstood my meaning, when I stated that the first action of the powder on the shot was impulsive, or percussive. One gentleman has observed that I reject altogether the notion that the force of powder is due exclusively to the gas it generates, and assume an additional impulsive force; another, that I believe powder, independently of the pressure exerted by its freed gases, exerts instantaneously a force of its own; and another, that I had stated that gunpowder had some mysterious action; that before the gases acted upon the shot, there was some kind of oscillation or undulation, which by some means communicated an initial velocity to the shot irrespective of their expansive action.

All such remarks are answered very simply, and as I think conclusively, for my experiments *prove* that the shot acquires a considerable velocity before it has moved to an appreciable distance. If the results of my experiments are admitted, this follows inevitably, and the only proper course for those who doubt my conclusion is to repeat my experiments. I feel well assured that after doing so, they will be forced to admit the accuracy of my conclusions. I did not put forward any thing to explain the above result, for I was convinced that we did not know sufficient of the action that takes place in the conversion of the powder into vapor to form a theory with any reasonable prospect of its being supported bv further investigations. I simply said that the theory at present received is false, and is not an approximation to the truth. The theory, in fact, supposes that the action is the same in all respects as if the powder were compressed gas, which expanded in the usual way, and omits all notice of the chemical action that takes place in the conversion of the powder into vapor.

However, practically, it is of so little consequence whether the shot be supposed to acquire its initial velocity instantaneously, or by the exertion of a very large pressure for a very short time, and through a very small space, as to be beneath consideration; and that the shot *does* acquire a finite velocity in a space equal to the small fractional part of an inch, and by the application of a force continued for so short a time as to be quite inappreciable, is shown by my own, and other experiments. Such a force is called, in mathematical language, an impulse, and this is all I have meant by using the term impulse.

It was shown by my experiments, that a shot of twice the diameter of another (the charge of powder being proportional and the quality the same) acquired, in a space so small as to be inappreciable, a velocity which caused it to move through a space equal to its own diameter in a shorter time than was required for the smaller shot to pass through an equal space—a fact directly opposed to all existing ideas on the subject; hence the increased strain upon large guns, and the shorter proportionate space that large shot move through in acquiring a given velocity. That the time required to move a shot is really inappreciable, appears from the circumstance that when, in my experiments, the large shot was placed upon the smaller chamber, the effect produced by the powder was no greater than when the smaller shot was placed there; the weight of the larger shot being, in this instance, more than eight thousand times greater than that of the charge of powder; a fact which goes far to prove that a very much greater bursting effect is produced in a gun by an increased quantity of powder, than by an increase in the weight of the shot, provided the latter is free to move in the gun. It should also be noticed that when the smaller shot was placed on the larger chamber there was no appreciable difference either in the time of action, or in the force exerted.

It is singular that all who have done me the honor to notice my work have, without exception, overlooked the circumstance of the difference in the action and effect of powder described as taking place when the chambers differ in *size*. The *impulsive* action of gunpowder has been noticed by previous writers (already enumerated), but the theory respecting the difference in the degree of force exerted upon the sides of the chamber, before the shot is moved, when the sizes differ, is entirely new, and totally opposed to the received *formulæ*; all writers, without exception, have assigned a constant (but each one a different) value to the initial pressure upon the shot, irrespective of the size of the chamber.*

The *form* of the chamber will also make a considerable difference in the action of a charge of powder. Thus, a chamber may be so formed (such as being slightly contracted towards the aperture) that a larger initial force may be obtained with a smaller quantity of powder; but this would only be of advantage in mortars or extremely short guns, for the action along the bore is less forcible, since the actual amount of the whole force of a given quantity of powder cannot be increased, or, if at all, very slightly.

It has been considered by some to be advantageous that the first movement of the shot in the gun should be retarded by increasing the friction of the shot against the sides of the gun; but this is a great error. It is true that a larger force is obtained in a smaller space, but this force is expended upon the sides of the gun and in overcoming the friction of the shot, and in a gun of great length of bore a large amount of force would thus be altogether lost, or, what is worse, would tend to increase the recoil and the bursting effect upon the gun; but if the length of the gun were considerably reduced and an equal charge fired, then the retardation of the shot in the gun might perhaps be productive of some slight advantage.

* In a small elementary work on Artillery, by Capt. Boxer, R. A., lately published for the use of the Military College at Sandhurst, the author, who strenuously opposed the idea when first started by myself, admits the fact, that the initial force of the powder varies with the calibre of the gun.

It is a common error to suppose that the elastic fluid produced by the combustion of gunpowder, acts with a pressure which is inversely as the space occupied by it. The action, on the contrary, is more violent in proportion to the nearness to the seat of the explosion. This arises from the greatly diminished heat of the gases, and is evident from experiment, since it is shown that *length* is of much less consequence in large guns (where the initial action of the charge is greater) than in small. If a charge of powder were supposed to be completely confined in the chamber of a gun of sufficient strength to contain it, what would be its action? Its force would rapidly increase as combustion took place until it reached a *maximum*, when it would gradually diminish; and its chief, or *explosive*, force would then be expended.

The greatest result would naturally be produced with powder which would exert the greatest amount of force for the greatest space of time. It by no means follows that the quickest burning powder will have this effect; for as only a portion of the force of a charge of powder is exerted before the shot is moved, and as the amount of this initial force will depend upon the quality of the powder, the action of a quick-burning powder, although greater at first, will not be maintained for a sufficiently long time for the shot (except in a short gun) to acquire so high a degree of velocity as when the action of the powder is slower. Thus, although a fulminating powder, much exceeding the strength of gunpowder, would act with many times the initial effect upon the shot, still, its action would be so rapid, that its strength would be exhausted in its first effort, and there would be no time for the shot to acquire that velocity which would be obtained by the employment of a powder of a less impulsive kind. The experiment has been made, and with the result I have already mentioned.

When the bore of a gun is larger or smaller in diameter —of greater or less length—when a large amount of friction attends the shot's passage, or the commencement of its passage, through the bore of the gun, a different quality of powder may be advantageously employed.

If a slower burning quality of powder were employed, its *bulk* should not be sensibly increased; that is, it should not occupy too much space in the gun, otherwise too large a portion of its energy will be exerted on the gun. To illustrate this, suppose a chamber of very great length (say six feet) and but half an inch in diameter; if such a chamber were filled with powder, and the powder ignited at one extremity, the action of the fluid would be so much greater at that end that, if the sides of the chamber were not strong, it might burst through them before its action at the other extremity of the chamber would be perceptible. In a similar manner we may account for the effect obtained by Hutton in some of his experiments. He states that when he increased the charge of powder to a very large quantity, the velocity acquired by the shot was smaller, whilst the *recoil* was greatly increased. No greater proof of the error of the ordinary theory of the action of gunpowder could be required than this; for if we suppose a shot acted upon by a fluid exerting (as assumed) a uniform initial pressure, it is true that its velocity would, in the above case, be smaller, since the action would take place through a shorter distance—but as action and reaction are equal, the *recoil of the gun* would be in proportion to the velocity acquired by the shot; now, according to the views which I have here put forward, an enormous pressure may be exerted upon the gun without a proportionate velocity being acquired by the projectile.

The work done upon the *shot* should be as great, and upon the *gun* as small, as possible, and the powder which produces this result, in a gun of given dimensions, is the best.

Suppose the bore of a gun to be filled with a gas exerting a uniform pressure of a very large magnitude, and closed at both ends; take away the obstruction at one end, and the gas would escape, but the dynamical effect produced upon the gun would be comparatively small. But fill a portion only of the bore with the gas, and place a ball before it, and the gun would have a recoil in proportion to the velocity acquired by the shot in moving through the bore: and that quantity of gas which would give the highest velocity to the shot, would cause the greatest recoil in the gun, and *vice versâ*; so that by either increasing or diminishing the quantity of gas the recoil would be lessened. I would simply ask, then, how can the increase in the recoil, which takes place at every increase in the charge of powder, be accounted for by the ordinary theory?

The difference between the ordinary theory and the one now put forward may be summed up in two words: the former is based upon *hypothesis*, and the latter upon *fact*. The former theory supposes no intermediate action to take place between the first ignition of the charge and the movement of the shot, but supposes that a chamber filled with a quantity of powder becomes, in a mysterious manner, suddenly and completely occupied by a permanently elastic fluid exerting a uniform pressure on all sides. The latter, on the contrary, assumes that an intermediate action *does* take place, and that of a most violent character; and, further, that upon the manner in which this commotion is produced, depends entirely the amount of pressure first exerted upon the shot and upon the sides of the chamber, as well as the nature of that pressure.

It appears incredible that the old theory could have been put forward, except for the purpose of affording a kind of basis for certain *formulæ;* but that it should be received "*au pied de la lettre*," as it now is, appears still more incredible.

In conclusion, I would state that I claim to have discovered and established, by my experiments, first, that the pressure produced by the fluid of fired gunpowder upon a shot, or upon the chamber of a gun, in no way resembles the uniform pressure of a confined gas, as assumed by the ordinary theory; but that, from its previous action through a given space, it acquires an impulsive or percussive character.*

* Some doubt has been raised as to the novelty of this proposition. It is true that Capt. Boxer, R. A., in a lecture which he gave on the Science of Gunnery, in March, 1854, advanced an opinion very similar to this; but as he made no attempt to verify or establish an opinion so subversive of the ordinary theory, or even to notice it in his subsequent *Treatise on Artillery*, and as I was quite unaware, when I first put it forward, that such an opinion had ever been entertained before, I do not hesitate to class the above amongst those facts which I consider to have been discovered, as well as established by my experiments. I consider also that those who first see the

Secondly, that, owing to the gradual combustion and decomposition of the powder, the pressure of the fluid has never, at the different periods of its action in a gun, the *same* value, but that previous to its action on the shot it attains a certain magnitude, and then (when the shot has begun to move) gradually diminishes.

Thirdly, that (the charges being proportional) *the height to which the pressure rises in each gun varies with the size of the gun; and that it increases, in a higher ratio, with any increase in the size of the gun.*

The manner in which I have attempted to account for the results obtained by experiment may, or may not, be correct, and mathematicians may cavil at the terms I have employed: the estimate also of the relative difference in the pressure exerted by different quantities of powder, etc., may, or may not, be perfectly exact; but this in no way alters the broad facts, which at least have the appearance of being sufficiently established by my experiments, not only to merit attention, but—if the science of gunnery be of any importance at all—to render it absolutely necessary that they should, at as early

practical application of an idea, and act upon it, are entitled to be considered the real discoverers.

Had it been possible for Captain Boxer to have spared from his official duties, as Superintendent of the Royal Laboratory at Woolwich, sufficient time to sift the question thoroughly, or to conduct the experiments which would have been necessary for that purpose, he would then probably have anticipated all that I have done, and no doubt have made further progress towards a solution of the question; but, as remarked before, he made no attempt, either to prove the truth of his proposition by experiment, or to apply it in any way to the practice of gunnery. As the trouble and expense of the only experiments which have been made with a view of substantiating the idea which I had conceived—as I can prove most clearly—equally with himself, were borne by me, he cannot, I think, with any show of reason, claim that idea as his own.

a period as possible, be either accepted, or (if not borne out by further experiment) rejected. Upon their reception or rejection depends entirely the course to be pursued in future with regard both to the construction of large guns, and the quality of the powder which should be used with them.

The introduction of rifled cannon into our service renders it a matter of the highest importance that the whole of the *formulæ* in gunnery which refer to the action and force of gunpowder should be revised; and that proper experiments should be made for this purpose.

I would therefore strongly urge the expediency of making experiments, as well for ascertaining the relative force exerted by different charges of powder, as the effect produced by increasing the weight of the shot, and by the retardation arising from the friction of rifled shot; so that we might have, at least approximately—which is far from the case at present—some idea of the relative strength required for guns of different kinds and calibres. The science of gunnery is in that state that practice has gone ahead of theory; it is therefore evident, that—for real progress to be made—a further knowledge of *cause* must be acquired, as well as of *effect*.

If nothing else is done, it appears at least indispensable that the careful and scientific experiments of Dr. Hutton should be repeated with the rifled guns now in use in the service, in order that we may learn their exact capabilities, the circumstances under which they may be most advantageously employed, the charges and the quality of powder which conduce most to their efficiency, and other circumstances of the greatest practical importance.

[I have been severely criticized for treating the explosive force of fired gunpowder as distinct from the pressure which is produced by the elasticity of the gas; but the fact that it is so admits of so simple a proof, that it will at once be evident to all who are conversant with the ordinary results which attend the combustion of gunpowder; namely, that when the grains of powder are completely crushed, or (as it is called) *mealed*, the explosive force is thereby very considerably diminished; but no one, I think, will venture to assert, that the *elasticity* of the gas which is evolved from the powder can undergo any alteration on this account.

The element of force which is wanting is to be sought for, probably, in the circumstance that (owing to the less rapid production of the elastic fluid) an equal quantity of fluid is not affected by the degree of heat at the same moment; so that the sudden and violent expansive force which is produced from this cause—and which is altogether distinct from the pressure which is due to the *elasticity* of the fluid—is wanting.

It has also been remarked, that I am in error in supposing that impact and pressure differ in kind, whereas they differ in degree only; but as impact, or impulsive pressure, and simple or continued pressure, differ so completely that they admit of no numerical comparison one with the other—or, as the proportion between a blow and a pressure cannot be defined—they cannot be said to differ in degree only. In fact, a comparison can no more be instituted between them than between a quart and a yard measure—which are both measures of quantity (as impact and pressure are, of force), with this distinction, however, that one is a measure of capacity and the other of length.

The *instantaneous* pressure which constitutes an impulse, involves also the separate consideration of the action which *produces* the instant exertion of the degree of pressure, and of *velocity*, for which there is no fixed unit or standard of measurement—so that although velocities may be compared with each other, yet a given velocity cannot be compared with a pressure, since they cannot be measured by the same standard—therefore, a pressure of so many atmospheres, or of so many pounds on the square inch, can convey no distinct idea of the comparative degree of force which is exerted by impact. An effect may be produced by an impulse which cannot possibly be produced by a pressure, and the contrary; and either may be of greater or less magnitude without changing their character.

It is not the *degree* of pressure alone which is the cause of a bullet making a clean hole through a pane of glass, but a rapid transmission of force through a given space, or, in other words, the high velocity with which the bullet is made to pass through the space of the thickness of the glass.

But whether impulse and pressure be considered to differ in kind or in degree only, it must be admitted that the value of each has to be estimated by a different method of calculation; and this is all I wished to establish. My motive for desiring to prove that the initial action of fired gunpowder should be treated as an impulse rather than as a simple pressure, may be easily explained.

The main source of the numerous errors and anomalies which now exist, clearly arises from the circumstance that the *formulæ* at present in use are based upon the hypothesis that the action of the fluid upon the shot is of the nature of a simple pressure; and—as the whole of the charge of powder is always supposed to be completely converted into a permanently elastic fluid before the shot is sensibly moved from its place—that the degree of pressure is uniformly the same; that is, in proportion to the density and elasticity of the fluid, or to the space occupied by the charge; consequently, when any fact has been discovered which is at variance with this theory—such as the apparently greater force of the powder when several shots have been fired at once from the same gun—the bursting of guns under certain circumstances, etc.—those who accept the ordinary theory are driven, in seeking for a solution, to the adoption of all kinds of vague hypotheses—such as supposing that the *initial pressure* of the gas increases when a greater weight is placed before it—that although the degree of pressure is the same, it is maintained for a longer *time* in large guns than in small, etc.—none of which will bear the slightest investigation.

By treating the initial force of the powder, however, as an impulsive or percussive force, all the existing anomalies are easily accounted for, and theory becomes at once reconciled with practice, since the initial pressure, in this case, is not supposed to be uniformly the same, but to vary under different circumstances.

It is remarkable that so very large an *initial* pressure as that which is always supposed to be exerted by gunpowder should ever have been treated otherwise than as an impulse, or impact; besides, it is a recognized principle, that heat—which is the principal element in the explosive action of gunpowder—and mechanical force are identical and convertible, and that the action of a given quantity of heat may be represented by a constant quantity of mechanical work performed. Now *pressure* alone is insufficient to constitute mechanical work.]

# APPENDIX.

# APPENDIX.

## (A.)

## ELONGATED BULLETS FOR RIFLED MUSKETS.

THE form of the bullet, for rifled muskets, has undergone various changes during the last few years. For a detailed account of these different modifications, up to the present time, I cannot do better than refer the reader to two excellent little works, the *Rifled Musket*, by Captain White Jervis, and *Rifle Practice*, by General Jacob; also to an extremely clever pamphlet on the *Improvement of the Rifle*, by Lieutenant-Colonel Lane Fox.

The bullet at present in use is, I believe, the invention of Colonel Hay, and is found, in practice, preferable to that introduced by Pritchett. It is hollowed out more behind, so as to admit of a wooden plug being placed in it, by means of which the necessary *rapidity* of expansion (which apparently is wanting in the Pritchett bullet) may be obtained, and its centre of gravity thrown more forward. The objection to this arrangement, however, is, that the plug is placed loose in the bullet. It may, in consequence, be driven up into the bullet before the latter is fired, so as to cause a premature expansion, and thus to occasion a difficulty in loading. It may also alter its position in the bullet, and thus give rise to a want of uniformity in the results obtained in practice. It appears to me, however, that these defects might in some measure be remedied by attaching the plug to the bullet by means of an iron pin, as shown in Fig. 1, or by the additional use of two or more

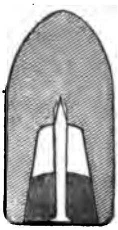


Fig. 1

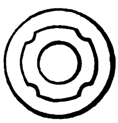


Fig. 2.

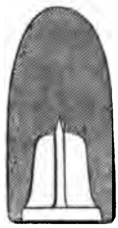


Fig. 3.

grooves or guides, as represented in Fig. 2, so that when the plug is driven forward, it must naturally be in a straight direction, and, being fixed, it could not so easily shift its position or be accidentally driven in.

I would suggest, however, as a still better arrangement, the one shown in Fig. 3. This has a simple pin of iron, or other material, with a broad head. I have fired this bullet with excellent effect—it is both simple in its construction and effective in its action.

The real utility of a plug, or cup, in the bullet, is not that either one or the other should be driven up into the hollow; but to prevent the action of the powder upon the upper part of the hollow, and to allow the lower part of the bullet to be expanded before the inertia of the fore part is overcome; for the hinder part of the bullet having less solidity than the fore part, the lead there will be more easily *upset*, and it will thus expand and fill the grooves with greater rapidity than if the bullet were of solid lead.

It is evident that the efficiency of the plug must depend materially upon the size of its base, as compared with the whole diameter of the bullet. To produce a proper effect, the area of the base of the plug should be fully two-thirds that of the bullet; the larger the base of the plug the greater will be the expansion; but when it exceeds the size above mentioned, means must be taken so to secure it that it will not be driven quite through the bullet; which may happen if the plug be placed loosely there.

The chief cause of a bullet *stripping* arises from its not expanding with sufficient rapidity to fill the grooves previously to acquiring a certain velocity.*

* The necessity of a quick expansion is very remarkable in expanding projectiles used for cannon. When the expansion does not take place with sufficient rapidity, deep furrows, caused by the flame of the gunpowder passing beyond the projectile, are distinctly visible in the bullet.

An open hollow in the hinder part of a bullet tends to prevent a proper expansion, as it allows the inertia of the upper part to be overcome in the same time as that of the lower part, and, if the hollow be broad and deep, the fore part of the bullet may even be blown off. A loose cup or plug placed in the hollow affords no certain remedy for this, as it is apt either to shift its position, or to be driven forward with violence, and jam the sides of the bullet against the barrel; in which case, owing to the diminished surface presented by the hinder part of the bullet to the action of the powder, and the impeding friction, the fore part of the bullet is liable to be blown off, from the plug being forced completely through it.

Bullets of increased length and diminished diameter are less liable to the above mishaps, and there is this to be said in favor of the use of such bullets, that they acquire—from their length—a greater expansion; and although they require a greater turn, yet the angle formed by the grooves with the axis of the bore (with a turn of a given length) becomes less as the calibre is diminished. Thus the angle formed by a turn of 3 feet in a musket of a calibre of ·577 in. would be greater than that formed by a turn of the same length in a musket having a calibre of ·5 in. only; so that a greater turn may be used with the latter, without much increase in the *angle* of the turn, whilst the expansion of the longer bullet insures it against stripping. The lower trajectory, or less incurvated track of such a bullet, subjects it also to a smaller *lateral* deviation.

There are many advantages attending the use of a bullet which can be made to expand quickly. It may probably be found necessary to increase the difference between the diameter of the bore of the Enfield musket and that of the bullet, in which case a quick expansion of the bullet will be of the first importance. The accounts lately received from India respecting the difficulty of loading—whether it arises, as appears likely, from the effects of the climate, or from some other cause, not otherwise to be remedied—tend to show that some alteration in the windage, or difference between the diameter of the bore and that of the bullet, will be absolutely necessary.

It is asserted that there is an advantage attending the employment of a simple lead bullet, that is to say, such as would require no plug or detached pieces, from the soldiers—in case of their ammunition failing when on distant service—always being able to make their own bullets: but this is no reason why a simple lead bullet should be *always* employed in the service. Bullet-moulds for casting solid lead bullets, in case of need, might form part of the regimental equipment, without prejudice to the use of a different kind of bullet for the ordinary service bullet. A solid lead bullet requires, moreover, a very fine, quick-burning powder.

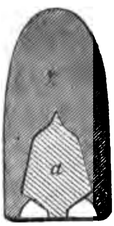


Fig. 4.

Fig. 4 has a plug, *a*, of tin, zinc, or other light metal, which is placed in the bullet when cast. This will throw the centre of gravity forward, and has the advantage of allowing the bullet to be of one piece. Bullets, however, being at present made by compression, their construction in this form renders them less eligible for military purposes than if made similar to the one represented in Fig. 3. The arrangement shown in Fig. 4, however, is preferable for bullets of more than two diameters in length.

## ( B. )

## ON THE DEFLECTION OF LONG PROJECTILES.

The theory, that the deflection, or *derivation* (as it is called), of rifle bullets is caused by the unequal pressure of the air consequent upon the bullet whirling about an axis which continues parallel to itself, and which forms an angle with its trajectory, so that the pressure of the air is greater upon the under than upon the upper surface of the bullet (see Frontispiece, Fig. 2), has been very generally entertained—with various modifications since the time of Robins. Captain Tamisier, a French officer, who adopted these views, endeavored to obviate the deflection of rifle bullets by constructing a bullet with circular grooves round its base, for the purpose of creating a resistance on the hinder part of the bullet; so that its apex should be brought down in such a manner that its axis would be a tangent to its line of flight, and the pressure of the air upon the surface of the bullet be thus equalized.

This, which has been called Captain Tamisier's theory, has been accepted by numerous persons; it is, nevertheless, very defective, as I will endeavor to show. In order, however, to do so, it will first be necessary to give some further explanation of it; for this purpose I cannot do better than quote Captain White Jervis's little work on the Rifled Musket. After noticing the circumstance of the greater pressure upon the lower surface of a long projectile, when its longer axis remains parallel to itself; and the supposed effect of Captain Tamisier's circular grooves; he thus proceeds:

"The lower side of the projectile, therefore, moving in the compressed air, and the upper in the rarefied air, deviations must ensue. For, as the upper part of the bullet moves from left to right, the bottom must move from right to left. But the lower resistance to the motion of rotation, being produced by the friction of the compressed air, is greater than the upper resistance, which depends on the friction of the rarefied air. By combining these two resistances, there results a single force, acting from left to right, which produces what Capt. Tamisier termed *derivation*, and it was to overcome this *derivation* that that officer proposed the circular grooves to the bullet, which he considered would act like the feathers of the arrow to maintain the moving body in its trajectory. By applying this new principle, bullets could be made of any form and length. * * Before, however, being able to seize all the consequences of this new principle, which consists in bearing out the theory of the air's resistance upon the cylindrical portion of the bullet to insure its keeping the right direction, it may be necessary to enter into some details, to give, if possible, a clear idea of this phenomenon. When a spinning-top is projected on the ground, animated by a very strong motion of rotation, it, first of all, leans very much on one side, then raises itself little by little, and, at last, finishes by turning round on its axis, which has become vertical, in such a manner as to make it appear motionless. What is the cause which induces the top to raise itself, and keeps it from falling? It evidently arises from the motion of rotation acted upon by the resistance of the air. For when the top leans on one side, whilst turning round rapidly, each portion of the lower part of its surface strikes successively, by virtue of the double motion of falling and of rotation with which it is endowed, the layers of the atmosphere with which it comes into contact; whilst the upper part of the surface comes, through this falling motion, into a part of the space formerly occupied by the body of the top, and has no resistance of the air to overcome to take up its new position, so that the force imparted to the lower portion, or rather to the inclined part of the surface of the top, is not neutralized by an equal force engendered on the opposite part.

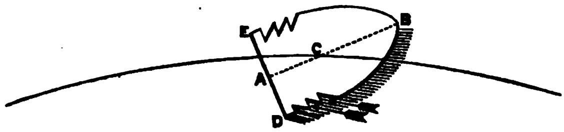
"The resistance of the air tends, therefore, to raise up the top, and this resistance is greater in proportion as the motion of rotation is more rapid.

"If, however, we would wish to obtain some idea of the rotary motion of a bullet in its path through the air, we cannot consider it in the same light exactly as a top spinning, for it is at the same time endowed with a force of progression.* But let us consider the action of the arrow, which is only animated with this force of projection, and let us see how it is constructed, so that the resistance of the air should not act in an unfavorable manner against its action. First of all, nearly all its weight is concentrated at the point, so that its centre of gravity is close to it. At the opposite end feathers are placed, the heaviest of which does not affect the centre of gravity, but gives rise to an amount of resistance in the rear of the projectile, and which prevents its ever taking a motion of rotation perpendicular to its great axis, and keeps it in the direction of its projection. This difficulty which the arrow finds in changing its direction must concur in preventing its descending so rapidly as it would do were it only to obey the law of gravity, and must therefore render its trajectory more uniform. We need scarcely add that the lengthened form of the arrow has precisely for its object to render as weak as possible the resistance of the air to the force of projection. Let us, however, now come back to the grooves of M. Tamisier, and we shall find that they concur in giving to the bullet the two actions of the resistance of the air which we have demonstrated with respect to the top and arrow.

"Suppose that such a bullet describes the trajectory M, and A B be the position of its axis, it will be seen that the lower part

* These theories of the top and arrow are generally supposed to have been started by Captain Tamisier, but they were originated by Robins. See his *Mathem Tract*, vol. i., p. 331. Edit. 1761.

of the bullet re-establishes the air compressed, whilst the upper part finds itself in the rarefied air. That consequently the lower




part of the canelures is submitted to the direct action of the air's resistance, whilst their upper part totally escapes this action. The resultant of the air s resistance evidently tends to bring back the point of the moving body according to the trajectory; but as this action is produced by the pressure of an elastic fluid, it results that the point B, after having been an instant upon the trajectory, will fall below in virtue of the velocity acquired; but then the upper grooves finding themselves acted on by the action of the air's resistance, this action, joined to its weight, will force the point of the projectile upwards, which will descend to come up again, so that the projectile will have throughout its flight a vertical swing, which is seen distinctly enough in arrows."—(*Rifled Musket*, pp. 58—61.)

The flight of an arrow, as I have already remarked, offers a very imperfect illustration of the flight of a bullet. The vertical swing occasionally observable in an arrow is generally owing to the propelling force not passing directly through the centre of gravity. It also arises from the arrow not being properly balanced, or feathered; and other circumstances, altogether distinct from those which operate in the case of a rifle bullet. Steadiness of flight in an arrow depends, therefore, upon the nice adjustment of its parts. Steadiness of flight can only be given to a long bullet by imparting to it a sufficient rotary velocity about an axis situated in the direction of its flight.

Though I agree with Capt. White Jervis, that the circular grooves proposed by Capt. Tamisier fail in their object, I differ from him somewhat with regard to the effect which they must produce on the bullet's flight. In my opinion, the bullet, instead of acquiring a simple vertical movement, would describe a *cone* about the direction of its flight. The only effect which could be produced by canelures placed in the direction of those upon Captain Tamisier's bullet would be to increase the rapidity of the movement described by Professor Magnus, and in no way to compel the bullet to remain a tangent to the curve of flight.*

It is beyond dispute desirable that a shot should be caused to remain a tangent to the curve of flight; but this can only be effected in practice, by placing the centre of gravity in the fore end of the shot; and I am inclined to think that—the rotary velocity being proportionate—the axis will be disturbed in a manner also the least likely to affect the accuracy of the projectile, when the centre of gravity is in a forward position; since the fore part of a projectile so constructed (owing to the smaller cone† which it will describe about the direction of flight) would not acquire so great an inclination to the vertical plane.

When the centre of gravity is at or behind the centre of the projectile, the cone will be described about an axis parallel to the direction of projection, or nearly so; but when it is in the fore part, it will be about an axis which is nearly a tangent to its curve of flight; the whole pressure of the air, in the latter case, will therefore be less.

Professor Magnus supposes (from his experiments) that the axis of rotation of a long projectile always remains nearly a tangent to the curve throughout the whole flight; and that the apex is sometimes depressed even below the tangent to the curve of

* I think it questionable whether a shot fired from a rifled gun can be compelled to remain a tangent to the curve of flight by means alone of projections or grooves upon its rear end, placed in any fashion whatever. It appears to me probable, however, that by placing three or four projections on the rear end of a long projectile, in such a position that the action of the air shall take place in the direction of the rotary, instead of (as in Captain Tamisier's bullet) in that of the progressive movement, the *deflection* might be considerably diminished (see Frontispiece, Fig. 7, c c), especially with projectiles having the centre of gravity in a forward position. The surface of the projections would probably be too small to occasion much loss of rotary velocity, unless the projectile were fired with a very high elevation and velocity.

† See chapter "On the Projectile," p. 84.

flight; but his description of the circumstances under which the projectiles were fired (in his experiments) is so vague, that it is impossible to form a correct opinion of the value of many of the results which he describes. He simply mentions that a very low velocity was given to the projectile. The position of the centre of gravity—the rotary velocity—the form of the projectiles and the angle at which they were fired—all of which considerably affect the movement of the projectile, remain altogether unnoticed. A description of these circumstances might have enabled us to account for some of his results, which, at present, are difficult to reconcile with the observations of others.

The motion, during its flight, of a projectile which has an insufficient rotary velocity imparted to it, presents a very different appearance to that of another which acquires a proper rotary velocity; and when, in the former case, the projectile has the centre of gravity behind its centre of figure, the apex will appear (especially when the rotary velocity and the angle of elevation are small) to be more depressed than that of another which has the centre of gravity in a forward position; since the cone, which the former describes about the line of direction, will be larger. If the apex of a properly formed projectile were ever really depressed below the tangent to the trajectory, the flight of long projectiles would not be so extended as we know it to be.

Therefore, although there can be no doubt that Professor Magnus's explanation of the effect of the air upon long rotating projectiles is substantially correct, as far as his experiments go, yet, inasmuch as those experiments are very imperfect, his description of the circumstances of the flight of a long projectile cannot be considered as applicable to such as are constructed of a proper form for obtaining the greatest range and accuracy, and which have a sufficient rotary velocity imparted to them.

He shows that the form (especially of the fore part) of the projectile considerably affects the manner in which the equilibrium of the axis of the projectile is disturbed by the air—but fails to describe the form which will be productive of the least disturbance; in fact, his experiments appear to have been made with one or two kinds of projectiles only, and he could not, therefore, give a decided opinion on this point. His experiments, however, possess a great value, since they show the actual manner in which a rotating projectile is deflected by the air; but he would have found many of the peculiar effects which he describes greatly modified, or altogether absent, if he had fired a properly formed projectile, with a sufficient rotary velocity.

The best formed projectile, for both range and accuracy (especially when the angle of elevation is high), will be such as have the centre of gravity sufficiently forward to maintain the axis nearly a tangent with the curve of flight; as, in this case, the retarding and disturbing force of the air will both be smaller.

When we consider the great pressure of the air, and that it must always act obliquely upon the projectile, it would appear impossible that the *derivation* can ever be completely overcome (except, perhaps, with shot fired with low velocities and a very quick turn), although with care it may be reduced in such a manner as to be easily allowed for with each elevation of the gun.

The spinning of a top is even a more unfortunate illustration to bring forward in support of Captain Tamisier's theory, than the flight of an arrow. A spinning top, which at first leans very much on one side, is not kept from falling, or induced to raise itself, by the resistance of the air; but acquires a vertical position *solely* from the act of rotation and *the friction of its point against the ground or surface upon which it is spinning;* and it *maintains* the vertical position in consequence of the stability acquired by the axis about which it is caused to rotate, which enables it to resist any attempt to disturb it;* and the greater the rotatory velocity, the greater will be the stability which the axis of the top will acquire, and the longer it will maintain a vertical position. The resistance of the air tends to

* The "Gyroscope" affords an admirable illustration of this.

destroy the rotary motion, and therefore to diminish the stability of the top. If we suppose a top spinning in a vacuum, it would maintain a vertical position for a much longer time than if spinning in the atmosphere; and if, in the former case, a disturbing force were to cause it to lean over to one side, it would recover its vertical position in precisely the same manner, and for the same reasons, that it would when spinning in the atmosphere.

If a top, spinning about an axis not quite vertical, were allowed to fall through a certain space to the ground, it would continue to rotate about an axis parallel to itself (supposing its centre of gravity to be at its centre) until it reached the ground, *when it would first begin to acquire a vertical position.*

In the event, however, of its falling upon a perfectly smooth surface, the friction would not be sufficient to allow of the axis of the top assuming a vertical position; but the part *below* the centre of gravity (the centre of gravity, and not the point, being, in this instance, at rest) would describe a cone about the axis, in a similar manner to the part *above* the centre of gravity; and the circle described by the point would be continually larger, as the rotary motion diminished. The movement of a projectile, when the rotary motion is not sufficiently rapid to insure a sufficient amount of stability to the longer axis of the projectile to enable it to resist the disturbing pressure of the air, is similar to this.

I am aware that this view of the question is contradictory to all elementary works on Natural Philosophy, and would therefore seem to require some further explanation; but a complete mathematical investigation of the subject would be too long, and of too abstruse a nature, for a work of this kind; suffice it to say, that the idea was originated by Mr. Sprague—a gentleman whose ability as a mathematician is unquestionable.

The following considerations, kindly furnished me by that gentleman, will be sufficient to show those who are acquainted with the mathematical Theory of the Composition of Rotary Velocities, that the friction of the point of the top upon the surface on which the top is spinning, really causes the axis of the top to assume a vertical position. Supposing a top to be spinning about an axis inclined to the vertical, upon a smooth surface, the action of gravity has a tendency to make the top rotate about a horizontal axis; or, in other words—to fall. It is well known, however, that, in consequence of the rotation of the top, it will not fall, but the new rotation which would be produced by gravity combines with the original one, and the effect is that the axis of the top, about which it still continues to rotate, will slowly describe a cone about the vertical line,—the velocity with which the axis of the top revolves being less, as the velocity of rotation of the top is greater. Now, if the surface on which the top is spinning is quite *smooth*, it will offer no resistance in a lateral direction to the motion of the point of the top, and therefore, in accordance with the elementary and well-known laws of motion of a solid body, the centre of gravity of the top will remain at rest, while the point of the top will describe a circle on the surface, and the apex of the top will describe another circle in a parallel plane. If, however, the surface is rough, the friction introduces another force acting on the top, which will give it a tendency to rotate about a new axis, *perpendicular to the axis of the top, and in the same vertical plane*. This new rotation will combine with the former rotation of the top; and it will be seen, by considering the directions in which the various rotations take place, that the resulting rotation will be about an axis less inclined to the vertical than the axis of the top, so that that axis will gradually assume a vertical position. In consequence of this, the apex of the top, instead of describing a circle, will describe a diminishing spiral, till at last the axis of the top becomes vertical—the point of the top remaining at rest, or nearly so, during the motion.

The explanation of Captain White Jervis, as set forth in the foregoing extract of his work, is quite erroneous. In fact, he has overlooked the circumstance that, if his theory were correct, no sooner would the axis of the top begin to move towards the vertical, than the resistance of the air on that side would become greater, and the top would be driven back to its former position, or still further.

(C.)

## IMPROVEMENTS IN ORDNANCE.

THAT the reader may form some idea of the progress which has been made in the use and the construction of cannon during the last two centuries, I have inserted a description of the different cannon in use, and a table of the ranges obtained with some of them, in the year 1646.

From this it will be seen that the chief improvement consists in the attainment of a slight increase in the effect, with a shorter and lighter description of gun. This advantage is principally owing to the improvement made, since the above period, in the manufacture of gunpowder; which (from its larger initial or explosive force) will now give a shot, when fired from a short modern gun, a velocity which, formerly, was only to be acquired with a gun of great length, and the use of a much larger charge. And although the range of balls of equal weight remains, as will be seen, much the same, yet the means by which these results are obtained have been very much improved.

The extracts which follow are taken from a little work, to which reference has already been made, *The Gunner's Glasse*, by W. Eldred, who, for sixty years, was Master Gunner at Dover Castle. It should be observed that the powder in use at the period when these ranges were taken, was "corn" powder, a (then) new description of gunpowder of greatly increased strength; being, as remarked by Eldred (*Gunner's Glasse*, p. 21), "twice as strong" as that which was used before his time. The powder in use previously to this period, and even then partly so, was called "serpentine" powder, and was of inferior strength, and not granulated. Before the "corn" powder came into use, cannon were of enormous length; the demi-culverin, or basilisk, at Dover Castle, temp. Queen Elizabeth, is nearly 24 feet (or 60 calibres) long; and, at Deal, there was, in Eldred's time, a "brasse" demi-culverin 16 feet long.

The powder of the present day is "twice as strong" as that used in Eldred's time—the charges, in consequence, are now much smaller, and the guns, although comparatively stronger at the breech, of reduced length.

Table No. 1, is descriptive of the different pieces of ordnance in use two hundred years ago; No. 2 contains a more minute description of several pieces of which the ranges are given; and No. 3 is a table of the ranges, or "randoms" (as Eldred calls them), of these pieces.

These Tables are not transcribed exactly in the order in which they stand in Eldred's book, but are taken from different parts of the work. I have also taken the liberty of making various small alterations—such as reckoning the ranges in yards, instead of by miles and scores—in order to render the tables more simple.

It will be observed that the *windage* was very large, and that the same quantity (¼ inch) was used with each description of gun.

## No. 1.—A Table of the Height and Weight of Pieces.

| The Names of the Pieces. | The Calibre or Height of the Bore. | The Weight of the Shot. | The Length of the Piece. | The Weight of the Piece. |
|---|---|---|---|---|
| A Rabonet.......... | 1¼ in. | ¾ lb. | 3 ft. | 120 lb. |
| A Falconet.......... | 2 in. | 1¼ lb. | 4 ft. | 210 lb. |
| A Falcon........... | 2¾ in. | 2¼ lb. | 6 ft. | 700 lb. |
| A Minion........... | 3 in. | 4 lb. | 8 ft | 1500 lb. |
| A Saker............ | 3½ in. | 5¼ lb. | 9½ ft. | 2500 lb. |
| A Demy-Culverin.... | 4¼ in. | 9 lb. | 10 ft. | 3600 lb. |
| A Whole Culverin, .. | 5 in. | 15 lb. | 11 ft. | 4000 lb. |
| A Demy-Cannon..... | 6 in. | 27 lb. | 12 ft. | 6000 lb. |
| A Whole Cannon.... | 7 in. | 47 lb. | 10 ft. | 7000 lb. |
| A Cannon Royall.... | 8 in. | 63 lb. | 8 ft. | 8000 lb. |

## No. 2.

| | Falcon. | | | Sacar. | | Demi-Culverin. | | Whole Culverin. | |
|---|---|---|---|---|---|---|---|---|---|
| | Brass. 1 | Brass 2 | Iron. 3 | Brass. 1 | Iron. 2 | Iron. 1 | Iron. 2 | Iron. 1 | Brass 2 |
| The height of the Bore | 2¾ in. | 2¾ | 2¾ | 3½ | 3½ | 4½ | 4¼ | 5¼ | 4¾ |
| The height of the Shot | 2½ in. | 2½ | 2½ | 3¼ | 3¼ | 4¼ | 4 | 5 | 4½ |
| The weight of the Shot | 2 lb. | 2 lb. | 2 lb. | 5 lb. | 4 lb. 12oz. | 10 lb. | 8 lb. 11oz. | 17 lb. 5oz. | 16 |
| The weight of the Powder . . . | 2 lb. | 2 lb. | 2¼ lb. | 4 lb. | 4 lb. | 7 lb. | 7 lb. | 9 lb. | 8 lb. |
| The length of the Cartridge . . | 7 in. | 7 in. | 8 in. | 10 in. | 10 in. | 17 in. | 17 in. | 19 in. | 18in |
| The length of the Dispart . . | 2¼ in. | 2 in. | 1¾ | 3¼ | 3¼ | 3¼ | 3¾ | 3¼ | 2¾ |
| Fortified in the Chamber . . . | 2¾ in. | 2 in. | 3 in. | 3½ | 5½ | 5¼ | 6 in. | 6¼ | 5¾ |
| The length of the Piece . . | 6ft. 1in. | 6.6 | 6.6 | 9 ft. | 9.9 | 10.2 | 10.8 | 10.9 | 16ft. |
| The weight of the Piece . . | 700 | 800 | 1100 | 1600 | 2700 | 3500 | 4000 | 4600 | 4200 |

## No. 3.—A Table of Randoms, or Ranges.

| Angle of Elevation. | Falcon. | Saker. | Demi-Culverin. | Whole Culverin. |
|---|---|---|---|---|
| | Yards. | Yards. | Yards. | Yards. |
| Level............ | 320 | 360 | 400 | 460 |
| ¼............... | .... | 405 | 450 | 515 |
| ½............... | .... | 450 | 500 | 570 |
| ¾............... | ... | 495 | 550 | 625 |
| 1................ | 480 | 440 | 600 | 630 |
| 2................ | 640 | 720 | 800 | 900 |
| 3................ | 800 | 910 | 1000 | 1120 |
| 4................ | 960 | 1090 | 1200 | 1330 |
| 5................ | 1120 | 1270 | 1400 | 1460 |
| 6................ | 1280 | 1450 | 1600 | 1770 |
| 7............... | 1440 | 1630 | 1800 | 1990 |
| 8................ | 1620 | 1810 | 2000 | 2210 |
| 9................ | 1760 | 1990 | 2200 | 2430 |
| 10............... | 1920 | 2170 | 2400 | 2650 |

*The Gunner's Glasse* is well worth the perusal of all who take an interest in the subject of gunnery, and would be an excellent work for military libraries, both for the purpose of showing the state of gunnery two centuries back, and, "though it appears a little out of fashion," for the good advice which it contains; the author being evidently one who was accustomed to "put his trust in Providence, and keep his powder dry."

The second part of the book need not necessarily be reprinted, as it contains only the opinions of Diego Ufano, Captain of the Artillery of Antwerp Castle, with reference to matters in connection with the warfare of the period. The first part (about 110 pages) might be transcribed and reprinted, with the woodcuts, at a cost of about £30 or £40. The only copy which I have yet seen is in the British Museum.

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