PROPOSITION X.

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

In all the experiments I have hitherto examined, l have never been able to discover, that the variation of the density of the atmosphere did any way alter the action of powder, although I have made several hundred shot in very different seasons : in particular, I have sometimes compared the trials, made at noon ; in the hottest summer sun, with those made in the freshness of the morning and evening, and I could not perceive any certain difference between them, and it was the same with trials made in the night and in winter ; although in this variety of seasons, the density of the atmosphere must have been very different: indeed, as we have seen that the same quantity of that fluid, in which the force of powder consists, is generated in a *vacuum,* and in common air, it is difficult to conceive how this force can be affected by the greater or lesser density of the atmosphere.

But though the density of the atmosphere: has no influence on the force of powder, yet its moisture has a great one; for the same quantity of powder, which in a dry season would communicate to a bullet a velocity of 1700 feet in 1", will not in damp weather communicate to the same bullet, placed in the same manner, a velocity of more, perhaps, than 12 or 13 feet in 1 ", or still less, if the powder be of a bad sort, and has been negligently kept. And this decrease of the force in damp powder appears by my experiments to be very unsteady and variable; so that 2 shot made with equal quantities of such powder, taken out of the same parcel, will differ considerably from each other, perhaps ten times more than if the powder was in good order; and as far as this uncertainty in its effects will permit me, I seem to collect; that a small charge loses a greater part of its force than a larger, provided each are equally damp. Another circumstance attending damp powder, is a remarkable foulness in the piece after firing, much beyond what arises from an equal quantity of dry powder.

Now all these effects arc easily to be accounted for, when it is known, that powder will imbibe moisture from the air ; for as a certain quantity of water, mixed with powder will prevent its firing at all, it follows, that every degree of moisture in powder, though insufficient to produce this effect, will yet abate the violence of its explosion, and will render the fire thereby produced less vehement than it would otherwise be ; whence a less quantity of fluid will be in this case generated, and the heat of that fluid and its elasticity is likewise less : consequently the action of damp powder must be, this two-fold account, be diminished according to the degree of moisture with which it is impregnated.

And as bad powder usually contains some common salt in it, by reason of the little care taken in the refining of the nitre, and as common salt imbibes moisture in a stronger degree than nitre ; it is not difficult to conceive, how bad powder should in a moist season be more impregnated with moisture than good, and should consequently lose more of its force.

The uncertainty in the effects of damp powder arises, I presume, from the different degrees of dryness it acquires in the piece; for as, after the first or second firing, the barrel grows warm, if the powder is contained any time in it, some part of its moisture will be thereby evaporated ; and as the heat of the barrel, and the time of the charge

continuing in it, are circumstances, which in their nature are very uncertain, it is not to be wondered at, that the evaporation, and consequently the addition of the powder, is likewise uncertain. 1 must remark, on this head, that, in the driest seasons, I have found the coldness of the barrel, and perhaps some little moisture condensed in its cavity, to have sensibly diminished the force of the powder in the first shot.

That small quantities of powder should have their action more diminished than larger quantities with the same degree of moisture, naturally follows from the smaller degree of heat, with which (as we have observed above) the explosion of small charges is attended ; since the same proportion of moisture must of necessity clog a weak fire more effectually, than it can do one which is more violent.

The remarkable foulness of the piece, from the firing of damp powder, which we have mentioned above; must likewise arise from the diminution of the activity of the fire in the explosion. For, when powder is of a proper temperature to fire readily and violently, the greatest part of its substance ought to be consumed to ashes, which will then be discernible in the form of a greyish substance on all bodies placed near, the mouth of the piece ; and the foulness of the piece is owing to those parts of the powder, which, either by their contiguity to the cold barrel, or their less inflammable composition, are but imperfectly burnt ; now since moist powder produces a less violent flame, in proportion to the moisture it imbibed ; it must follow, that a smaller part of the powder will in this case be perfectly consumed, and consequently a greater part will remain to contribute to the foulness of the barrel.

SCHOLIUM. I .

We have asserted, as the basis of our reasoning in this proposition, that powder will imbibe moisture from the air in a humid state of the atmosphere; but it remains to assign the quantity it can thus imbibe, which we shall here endeavour to do from our own experiments.

A parcel of very good powder being placed on a white paper, which was pierced with a great number of fine holes, and the paper being held over the steam of hot water, I found, that in half a minute the powder was increased in weight by about $\frac{1}{30}$ part. Trying another parcel in the same manner; but continuing it longer in the steam, I found that the powder increased its weight by $\frac{1}{24}$ part ; but in this case some of the grains adhered together in small lumps, although the figure of the grains themselves was no ways changed.

To convince myself that the moisture of the atmosphere would likewise increase the weight of powder, I took about an ounce of powder, which had for some time been kept in a room, which had a fire in it every day, and I found, by drying it before the fire, that it lost above $\frac{1}{100}$ of its weight ; one third of which decrease in weight it had again acquired in less than two hours, by being removed to a different part of the room, at a distance from the fire. Now as the weather is often much moister than when I tried this experiment, and as in open air this moisture abounds much more than in a room where there is a fire, it cannot be doubted, but that sometimes the twentieth or thirtieth part of the substance of the best powder is water, which may be easily supposed to produce all the effects, we have observed and described in this proposition.

But, however, the moisture thus imbibed by powder from the air does not, as I have yet observed, render it less active, when it is dried again. The reader must have observed, in the experiments of the last proposition, how nearly those made with the same quantities of powder, and in the same circumstances, agree with each other. In these experiments, though made at different times in the course of three summer months, the dryness of the season prevented all the inequalities of this proposition. But trying the same powder in the winter, in a very damp season, I found, that though if it was used as in the summer, in its natural state ; without any drying, its effects were very irregular, and much short of those experiments ; yet if each charge was well dried, just before it was used, no diminution of its force could then be perceived, nor did it appear to act in any manner different from what it had done in the Preceding summer. Indeed if the powder be exposed to the greatest damps without any caution, or if common salt abounds in it, the moisture it imbibes may perhaps, be sufficient to dissolve some part of the nitre; which is a lasting damage, that no drying can retrieve. But when tolerable care is taken in preserving powder, and the nitre it is composed of has been well purged from common salt, it will retain its force much longer than is usually supposed. I have heard that powder which has been well kept, did not at the end of fifty years appear to be any ways injured; by its age.

Some care is necessary in the drying of damp powder; for there is a degree of heat, which though not sufficient to fire the powder, will yet melt the brimstone, and destroy the texture of the grains. − Nay more, there is a heat, with which the brimstone will flame and burn away gradually, and yet the powder will not explode ; of this any one may satisfy himself by heating a piece of iron red-hot; and then throwing a few grains of powder on it at different intervals, during the time of its cooling; for by this means he will find, that at a certain time the separate grains, that fall on the iron, will not explode, but will burn with a small blue flame for some space of time, the grain still remaining unconsumed. indeed, when it has begun to burp in this manner, it sometimes ends with exploding, but this more commonly happens when a number of grains lie near together ; for then, though each separate flame is not sufficient to explode its respective grain, yet the whole fire, made by them altogether, grows strong enough at last to end in a general explosion ; however, by attending to the proper temperature of the iron, and spreading the grains, I have often covered two or three square inches with a blue lambent flame, which has lasted a considerable time without any explosion; and examining the grains afterwards, I could not perceive that they had lost either their colour or their shape. Now· since these grains, when the brimstone is thus burnt, or even melted out of them, will no longer act as powder ; it is evident, that powder may be spoiled by being dried with too violent a heat.

From the great difference in the effects of moist and dry powder established in this proposition, it appears how very uncertain and irregular all those practical operations of Gunnery may prove, where this circumstance is not attended to; and how little confidence can be placed in any experiments where this cause of inequality could interfere.

Before I leave this article, I must mention a suspicion, I once entertained about this matter. − As water, when rarefied into vapour, is generally supposed to be near ten times more elastic than air equally heated, I imagined, that possibly the moisture imbibed by power might, in certain cases, be so proportioned to the quantity of powder, that it might

be converted into vapour by the explosion; and that thereby the force of the powder might be more increased by the addition of this very elastic vapour, than it was diminished by the damping of its flame. And I was the more induced to believe that this did sometimes happen, from the experiments of a late author, who tells us that the ranges of the same shot, fired from the same mortar, with equal charges of powder, were much greater in the freshness of the morning, than in the heat of the day. For I was well satisfied, that the mere density of the air (to which he seems to impute this variety) could not produce such different effects. However, upon a more accurate examination, I cannot find that any degree of moisture does at any time augment the force of powder; for, in all the numerous trials I have made, I never observed that force sensibly to exceed its mean quantity, except in two experiments ; and even those excesses, I had good reason to believe, were occasioned by some disorder in the machine. However, if the elasticity of watery vapour be as great as it is usually esteemed, (a point far from ascertained at present) it is not impossible, but something of this kind may take place in the firing of large quantities of powder.

REMARK

In this proposition, a very important fact is touched on by the author, why the powder at some time exerts a much smaller force than at another. The same is based on the amount of moisture which the powder has attracted from the air itself. Two circumstances may now occur, in the first place, how much moisture in any case is contained in the powder, and secondly, by how much will that force be reduced for every level of moisture mixed with it. The former can be detected by the weight. Because if one first puts in place a certain quantity of powder completely dried and accurately weighed, but afterwards the weight of the same quantity again is carefully weighed for any change in the air, the increase of the weight must always indicate how much moisture is contained in the powder. Because the humidity of the air can be quite accurately determined by the hygrometer, one could perform useful tests of this fact, if by each single different degree which the hygrometer indicates, a certain quantity of powder should be weighed. In this manner, one would know how much moisture the powder contained within itself in any state of the air, which is shown by the hygrometer itself.

But one would have for each change going on in the air, the powder exposed to the air for a considerable time so that the powder could always take just the degree of moisture in itself, which the air has. Because it is easy to consider that if the humidity of the air decreases, that which already has been drawn into the powder, cannot go so soon by evaporation. There would also be in the actual carrying out of such experiments, above all for one's reputation, still even more difficulties to be found later, but the same should not prevent similar experiments having a great benefit.

From that, as the author notes, the bad powder itself attracts far more of the humidity of the air than the good, one could also examine the quality of the powder in this way: for that one is undoubtedly the best, which takes up the least moisture.

Now if one in this manner had found how much moisture the powder had absorbed at any time, so one can understand easily, according to the method found by the author, to what extent the force would be diminished by that; and from such a form one would be

able to determine the effect of the powder alone simply from the theory, far more accurately than hitherto has been possible. Here the author quotes a most remarkable example, concerning the diminished power of the moist powder, that a bullet from wet powder would communicate only a speed of only1200 feet per second, since under the very same circumstances of dry powder, the speed of the ball discharged would be 1700 ft. per second. He did not in fact determine how much moisture was mixed in this case with the powder, but if we assume that the moisture be the thirtieth part of the whole weight, as one could say that if the powder were mixed with the thirtieth part water, its force will be reduced by $\frac{5}{17}$. If now further, it is not indeed to assume that the departure of the force of the powder would always to be proportional to the combined dampness, thus one can find from this, roughly how much the force of the powder would be diminished through another equal single [*i.e.* small] amount of dampness so combined with that.

Let us now assume, that a $\frac{1}{n}^{\text{th}}$ part of the weight of the powder is represented by the combined humidity, so $\frac{1}{30}^{\text{th}}$ itself must be in proportion to $\frac{1}{n}$ as $\frac{5}{17}$ to the loss of the force of the powder sought, which therefore would be $\frac{150}{17n}$ or almost $= \frac{9}{n}$. If also only the one hundredth part of the dampness is combined with the powder, so the same force must lose around the $\frac{1}{11}^{\text{th}}$ part, which loss nevertheless would be far greater, as that shown by the experiment cited by the author. Since there, as the author related, such powder, which as it was situated in a warm room for a long time, lost no more than a $\frac{1}{100}^{\text{th}}$ part of its weight by the drying, so itself there would presumably often be found to have an even much greater difference [in weight] to the powder, from that in which the before mentioned experiment was performed, which regardless seems still to agree quite well overall with the author's theory. Therefore it is concluded, that, if only a one hundredth part of dampness is combined with the powder, its strength will not be appreciably diminished by that. One can see easily also, from what has been said already, that the different weight and density of the air causes no marked difference in the action of the powder. One can indeed think that there, a heavy air [*i.e.* a greater air pressure] presses more strongly against the bullet, than if the same were driven out through the barrel by the lighter powder, and the speed in the first case must become reduced more than in the second. Only, as we have pointed above, no significant noticeable difference arises in the speed of the bullet, even if one leaves the counter-pressure of the air completely out of consideration, the different strength thereof causes an appreciable less marked effect.

ANMERKUNG

In diesem Satz wird von dem Verfasser ein sehr wichtiger Umstand berühret, weswegen das Pulver zu einer Zeit eine weit geringere Gewalt ausübet, als zu einer anderen. Derselbe beruhet auf der Menge der Feuchtigkeit, welche das Pulver aus der Luft an sich gezogen. Hierbey kommen nun zwey Umstände vor, erstlich wie viel Feuchtigkeit das Pulver in einem jeglichen Fall in sich enthalte, und zweytens um wie viel die Gewalt desselben von einem jeglichen Grad der damit vermischten Feuchtigkeit vermindert werde. Das erstere läßt sich durch das Gewicht erkennen. Denn wenn man erst lich eine gewisse Quantität Pulver vollkommen trocknet und genau abwiegt, hernach aber bey einer jeden Veränderung der Luft das Gewicht derselben Quantität wiederum sorgfältig bemerket, so muß der Zuwachs des Gewichts immer anzeigen, wie viel Feuchtigkeit in dem Pulver enthalten ist. Weil die Feuchtigkeit der Luft durch das Hygrometrum ziemlich genau bestimmet werden kann, so könnte man über diesen Umstand nützliche Versuche anstellen, wenn man bey einem jeglieben verschiedenen Grad, den das Hygrometrum weiset, eine gewisse Quantität Pulver abwiegen sollte. Auf diese Art würde man erkennen, wie viel Feuchtigkeit das Pulver in einem jeglichen Zustand der Luft, welcher durch das Hygrometrum augezeiget wird, in sich enthalte. Man müßte aber bey einer jeden Veränderung, so in der Luft vorgeht, das Pulver eine geraume Zeit der Luft ausgesetzt lassen, damit dasselbe immer eben den Grad der Feuchtigkeit an sich nehmen könnte, welche die Luft hat. Denn es ist leicht zu erachten, daß wenn die Feuchtigkeit der Luft abnimmt, diejenige, welche sich schon vorher in das Pulver gezogen, nicht so bald wiederum durch die Ausdünstung davon gehen könne. Es würden sich auch bey der würklichen Anstellung solcher Versuche, allem Ansehen nach, noch mehr Schwierigkeiten einfinden, dennoch aber würden dieselben nicht hindern, daß dergleichen Experimente keinen sehr grossen Nutzen haben sollten. Da auch, wie der Autor bemerket, das schlechte Pulver weit mehr als das gute von der Feuchtigkeit der Luft an sich ziehet, so könnte man auch auf diese Art die Güte des Pulvers untersuchen: indem dasjenige unstreitig das beste ist, welches am wenigsten Feuchtigkeit zu sich nimmt.

Wenn man nun auf diese Art gefunden hat, wie viel Feuchtigkeit das Pulver zu einer jeden Zeit in sich enthält, so kann man nach der von dem Autore erfundenen Methode leicht untersuchen, um wie viel dadurch die Gewalt verringert wird; und solchergestalt würde man im Stande seyn, die Würkung des Pulvers bloß allein aus der Theorie weit genauer zu bestimmen, als bißhero möglich gewesen. Der Verfasser führet hier, um die verminderte Kraft des feuchten Pulvers darzuthun, ein merkwürdiges Exempel an, daß der Kugel von feuchten Pulver nur eine Geschwindigkeit von 1200 Schuh in einer Secunde mitgetheilet worden, da unter eben denselben Umständen von trockenem Pulver die Geschwindigkeit der Kugel 1700 Schuh in einer Secunde ausgetragen. Er bestimmt zwar nicht, wie viel Feuchtigkeit in diesem Fall mit dem Pulver vermischet gewesen; wenn wir aber annehmen: daß die Feuchtigkeit den dreißigsten Theil des ganzen Gewichts betragen, so könnte man sagen, daß wenn das Pulver mit dem dreyßigsten Theil Wasser vermischet wird, seine Gewalt um $\frac{5}{17}$ vermindert werde. Wenn nun ferner, welches zwar nicht zu vermuthen ist, der Abgang der Gewalt des Pulvers immer der

damit vermischten Feuchtigkeit proportional wäre, so könnte man hieraus finden, um wie viel die Gewalt des Pulvers durch eine jegliche andere Menge Feuchtigkeit, so damit vermischet ist, vermindert werde.

Denn laßt uns setzen, daß die mit dem Pulver gemischte Feuchtigkeit den $\frac{1}{n}$ Theil des Gewichts austrage, so müßte sich $\frac{1}{30}$ zu $\frac{1}{n}$ verhalten wie $\frac{15}{17}$ zu dem gesuchten Verlust der Gewalt des Pulvers, welcher folglich $\frac{150}{17n}$ oder beynahe $=\frac{9}{n}$ seyn würde. Wenn also nur der hundertste Theil Feuchtigkeit mit dem Pulver vermischet ist, so müste die Kraft desselben um den $\frac{1}{11}$ Theil abnehmen, welcher Verlust gleichwohl weit grösser seyn würde, als die von dem Autore angeführten Experimente anzeigen. Denn da, wie der Autor berichtet, solches Pulver, welches lange Zeit in einem warmen Zimmer gelegen, dennoch bey der Trocknung noch den $\frac{1}{100}$ Theil von seinem Gewichte verlohren, so muß sich vermuthlich öfters ein eben so grosser Unterscheid bey dem Pulver, womit die vorhergemeldeten Experimente gemacht worden, befunden haben, welche doch diesem ungeachtet mit des Autoris Theorie ziemlich genau überein zu kommen scheinen. Dahero zu schliessen ist, daß, wenn nur der hundertste Theil Feuchtigkeit mit dem Pulver vermischet ist, seine Gewalt dadurch nicht merklich vermindert werde.

Man siehet aus demjenigen, was bißher angeführet worden, auch leicht, daß die verschiedene Schwehre und Dichtigkeit der Luft in der Würkung des Pulvers keine merkliche Veränderung verursachen könne. Man könnte zwar denken, daß, da eine schwehre Luft stärker gegen die kugel, indem dieselbe von dem Pulver durch den Lauf heraus getrieben wird, drücket, als eine leichtere, die Geschwindigkeit im erstern Fall mehr vermindert werden müßte, als in dem letztern. Allein, da wir oben gewiesen haben, daß kein merklicher Unterscheid in der Geschwindigkeit der Kugel heraus kommt, wenn man auch den Gegendruck der Luft völlig aus der Acht läßt, so kann die verschiedene Schwehre desselben um so viel weniger einen merklichen Unterscheid verursachen.