

XV. *Sundry Papers relative to an Accident from Lightning at Purfleet, May 15, 1777.*

1. *A Letter from Mr. Boddington, Secretary to the Board of Ordnance, to Dr. Horsley, Sec. R. S. with Two Enclosures from Mr. Nickson, Store-keeper at Purfleet, giving an Account of the Accident.*

TO DR. HORSLEY, SEC. R. S.

S I R,

Office of Ordnance,  
May 31, 1777.

I AM directed by the lieutenant-general and the rest of the principal officers of the Ordnance, to transmit to you the copies of the reports and plan received from Purfleet, on occasion of some damage done by lightning; which reports and plan they desire you will please to lay before the Royal Society.

I am, &c.

JOHN BODDINGTON.

TO

TO SIR CHARLES FREDERICK, KNT. OF THE BATH.

HONOURABLE SIR,

Purfeer,  
May 16, 1777.

YESTERDAY afternoon we had much rain and distant thunder; but at six a very heavy cloud, in passing over the house, presented us with part of its contents, which struck the North-east corner of the house on one of the cramps that held the coping stones together, forced off about a square foot of that stone and one brick, and has displaced about a cube foot of brick-work underneath. It has not been yet discovered that any of the conductors have acted during the passage of that cloud, although the flash and report were both very great. One of my servants was out of doors by the coach-house at the same time, and narrowly escaped falling by the strength of the flash: the others were in the house, but were much frightened. My son says, that there is a dent in the cramp, on which the lightning fell, and I intend to preserve it as a curiosity. If the conductor on the house has acted, it is imperceptible as I am informed. I thought this account would be acceptable to you from,

Honourable sir, &c.

EDWARD NICKSON.

TO

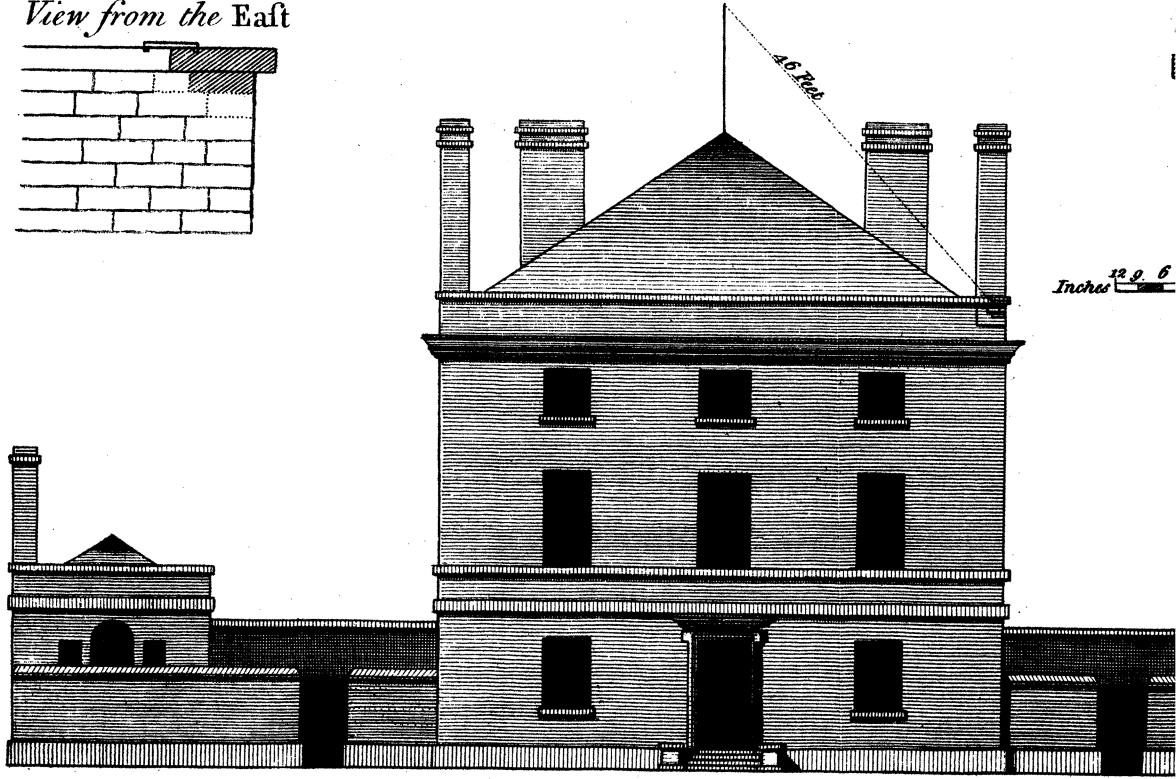
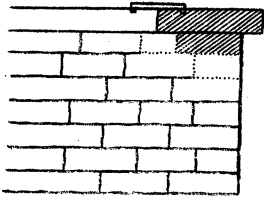
TO THE RIGHT HONOURABLE LORD AMHERST, LIEU-  
TENANT-GENERAL OF HIS MAJESTY'S ORDNANCE, &c.

MY LORD,

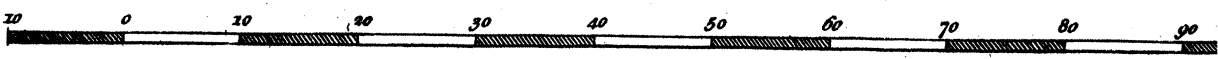
Office of Ordnance,  
Purfleet, May 20, 1777.

IN obedience to your lordship's commands of the 19th instant, directing me to report concerning the accident that happened to the Board's house by lightning on the 15th, I beg leave to acquaint your lordship, that on that day there had been much rain and distant thunder; but, about six o'clock in the afternoon, a very heavy cloud hung over the house for some time, which I looked at from the back-parlour window, and it being quite calm, made me suspect, that some of our conductors might find employment from it. I had not been long at the window before a violent flash of lightning and clap of thunder came together; and, as soon as the rain would permit any body to move about, one of the labourers brought me some pieces of stone and a brick, which were struck off from the coping on the parapet-wall of the building from the North-east corner. On my son's coming home, I desired him to go and view the top of the house. On his return he told me, that the lightning had struck one of the cramps that hold the coping together, and had made a dent in the lead of the cramp, and

*View from the East*



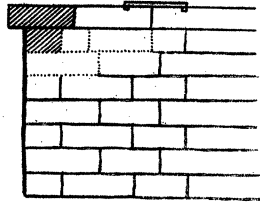
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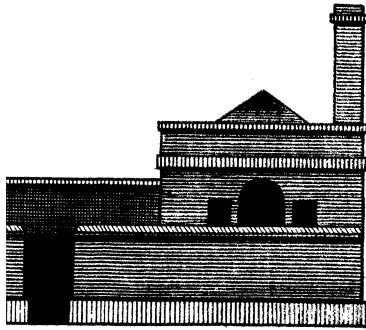
*East Front of the Board house at Purpe*



*View from the North*



120 60 90 1 2 3 Feet



90 100 120 150 Feet

*Surfleet.*

*Bastre Se.*

the stone adjoining to it, as if struck by a musquet-ball; that the quantity of stone thrown down might amount to about a square foot; and that it had disturbed about a cube foot of brick-work underneath: and, according to your lordship's commands, the distance from the point of the conductor on the house to the part struck has been measured by him this day, and amounts to forty-six feet.

All the conductors at this place are pointed, and it has not yet been discovered that any of them have acted on this occasion.

If your lordship should want any farther explanation, my son waits on you with a small drawing of the elevation of the East-front of the house, and of the part struck, which I hope will be satisfactory to your lordship and to the honourable board.

I am, &c.

EDWARD NICKSON.

2. *The Report of the Committee appointed by the Royal Society, for examining the Effect of Lightning, May 15, 1777, on the Parapet-wall of the House of the Board of Ordnance, at Purfleet in Effex.*

THE iron cramp on which the lightning fell was cemented into the coping stones on the parapet-wall, near the North-east corner, with lead: and on that lead, at one end of the cramp, there appeared to have been a small fusion; the end of the lead, and part of the adjoining stone, being indented about half an inch in diameter, and a quarter of an inch deep, as though a musquet-ball had been fired against them.

The iron cramp was situated over a plate of lead, and the ends of it, which were inserted in the stone, came within seven inches of that plate, which communicated with the gutter, and served as a fillet to it: this gutter was a part of the main conductor of the building.

When the lightning had quitted the iron cramp, and had passed through seven inches of stone, brick, and mortar, it fell upon the corner of the plate of lead above-mentioned, as appeared by the fusion of a very small portion of it discovered by pulling out the bricks, mor-

tar, &c. on purpose to examine into this particular. From this place no farther effect of the lightning could be traced; the metallic conductors to the earth having effectually performed their office. At the distance of seven feet and an half from the place stricken, a large leaden pipe went down from the gutter to a cistern of water in the yard.

The rain, Mr. NICKSON informed us, had fallen plentifully for some time before the stroke; so that the mortar, bricks, &c. did probably form an imperfect conductor for the distance of seven inches between the iron cramp in the coping stones and the filleting of lead above-mentioned.

At the termination of the iron cramp in the coping stone, a piece of the stone, with one brick, was stricken off; and a few other bricks were loosened, and removed less than half an inch from their places. The damage done to the parapet of the building is so inconsiderable, that it would scarce deserve notice, was it not an evident proof that the metallic communication with the earth hath, in this case, effectually prevented any farther injury.

The conducting rod on the ridge, near the center of the house, shewed no marks of its having been affected by the lightning in this case: and it is remarkable, that the surface of one of the hip-rafters, four inches and a

half in diameter, covered with lead (communicating with the gutter) and reaching within twenty-eight inches of the place stricken, seems not to have been at all affected.

The method we would recommend of preventing similar accidents to the parapet of this building for the future is the following. Let a channel of the same size with the cramps be made from cramp to cramp in the coping stones, quite round the building: let this channel be filled with lead, and let a metallic communication by plates about six inches broad be made from that lead in four places (one at each side or corner of the parapet) to the filleting of lead which is in contact with the gutter, which gutter is part of the main conductor to the building.

June 19, 1777.

W. HENLY.

T<sup>y</sup>. LANE.

E. NAIRNE.

J. PLANTA.

3. *Mr. WILSON'S Dissent from the above report.*

WHEN this important subject was first debated in the Purfleet committee of 1772, a passage was quoted from Dr. FRANKLIN'S philosophical publications, respecting the nature of such buildings as were secure from attacks by lightning.

The passage alluded to is this: " Buildings that have  
" their roofs covered with lead or other metal, and spouts  
" of metal continued from the roof into the ground to  
" carry off the water, are never hurt by lightning; as,  
" whenever it falls on such a building, it passes in the  
" metals, and not in the walls." FRANKLIN'S Exp.  
p. 481.

With this idea the building at Purfleet, called the Board-house, was considered by that committee to be in a similar situation, and consequently secure from such attacks, without having any other conductors than the leaden gutters, pipes, &c.

As the members of that committee then present seemed to be fully satisfied with that determination, I proposed that the magazines themselves should be put into the same circumstances; otherwise there would appear

pear to be an inconsistency in the different methods of securing those buildings.

My argument had no other effect than to occasion, at the next meeting of the committee, a resolution for fixing pointed conductors to all the buildings.

From this resolution I dissented, and gave in writing my reasons at large for differing in opinion, which are printed in your Transactions.

What has been the consequence since the conductors were put up?

Behold! this very Board-house, which was never attacked before by lightning, hath very lately been struck, and that within a few inches of the conductor; contrary to Dr. FRANKLIN'S assertion, which positively says, that in such circumstances the lightning passes in the metals, and not in the walls.

We may refine in our reasoning upon the philosophy of this event as much as we please; but let me tell you, gentlemen, there is no getting rid of the fact: which, according to my judgement, appears to be truly alarming. And as, I apprehend, the reputation of this learned Society is greatly concerned therein, we ought immediately to avail ourselves of this providential warning, and reject an apparatus which threatens us every hour with some unhappy consequences.

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It is with very great concern, that I am obliged to take notice, in this Society, of a house, which is of the first consequence in this kingdom, that hath pointed conductors also fixed upon it: I mean the KING'S, our most gracious Patron and Benefactor's. Who were the advisers of them I know not; but as they are there, I thought it my duty to mention them.

In considering the propriety of pointed conductors, I think it necessary to observe, that increasing the number of them in any given space does not by any means, in my opinion, lessen the risk of accidents by lightning; but on the contrary (at least in many cases) a greater number of such conductors will necessarily invite a larger quantity of lightning. At Purfleet there are several of those conductors; and by the storekeeper's letter sent to the Board of Ordnance, which was lately read before us, it appears, that he himself observed a very heavy cloud hanging over the house for some time before the stroke happened.

According to Dr. FRANKLIN'S idea, this event ought never to have happened; because he says, that pointed conductors will draw all the lightning out of the clouds, and carry it away into the earth silently.

This philosophy I never had any faith in, unless the quantity of lightning contained in the clouds happens to  
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be very little, and incapable of producing any fatal consequences.

I have now only to add, that I did not propose to have troubled this Society any more, had I not thought, upon this great occasion, it was my duty to stand forth, and give my opinion against the present report; as I know of no possible advantage to be derived from such conductors; at least none that are consistent with true philosophy, and a sincere regard to the welfare of society.

June 19, 1777.

B. WILSON.

4. *A Letter from the Board of Ordnance to Sir John Pringle, Bart. P. R. S. enclosing a Letter from Mr. Wilson to HIS MAJESTY, and an Account of his Experiments on the Nature and Use of Conductors.*

TO SIR JOHN PRINGLE, BART. P. R. S.

S I R,

Office of Ordnance,  
Nov. 18, 1777.

MR. WILSON having laid before this Board a copy of the report made by him to His Majesty upon some experiments in consequence of the accident by lightning, in May last, to one of the buildings belonging to the royal magazine of gun-powder at Purfleet:

We

We beg leave to transmit to you a copy thereof, to be laid before the Royal Society; and, at the same time, we desire the favour of their instructions, if any thing more can be done, in order to the preservation of his Majesty's magazines.

We are, &c.

AMHERST.

CHARLES FREDERICK.

CHA<sup>s</sup>. COCKS.

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T O T H E K I N G .

S I R,

YOUR MAJESTY, in consequence of the accident from lightning that happened to one of the buildings at Purfleet in May last, having been graciously pleased to intimate the propriety of making some farther experiments, to ascertain the best method of preventing such accidents for the future; and having also condescended to be present at the exhibition of those experiments at the *Pantbeon*; I have presumed to address to YOUR MAJESTY

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this faithful and circumstantial account of what was there attempted, together with some observations thereupon, as an humble testimony of my duty and gratitude for the great honour conferred upon me.

How far I may have succeeded in these my zealous endeavours to ascertain the most proper construction for conductors is, with the greatest deference, submitted to YOUR MAJESTY and the public. And whatever consequences may be derived from these experiments, I am happy in the thought of having done every thing in my power, with the utmost candour and impartiality, to investigate truth, in a question of real advantage to science, and of such importance to the public, as seems, in my humble opinion, worthy the attention of the ablest philosophers.

I am,

SIR,

YOUR MAJESTY'S

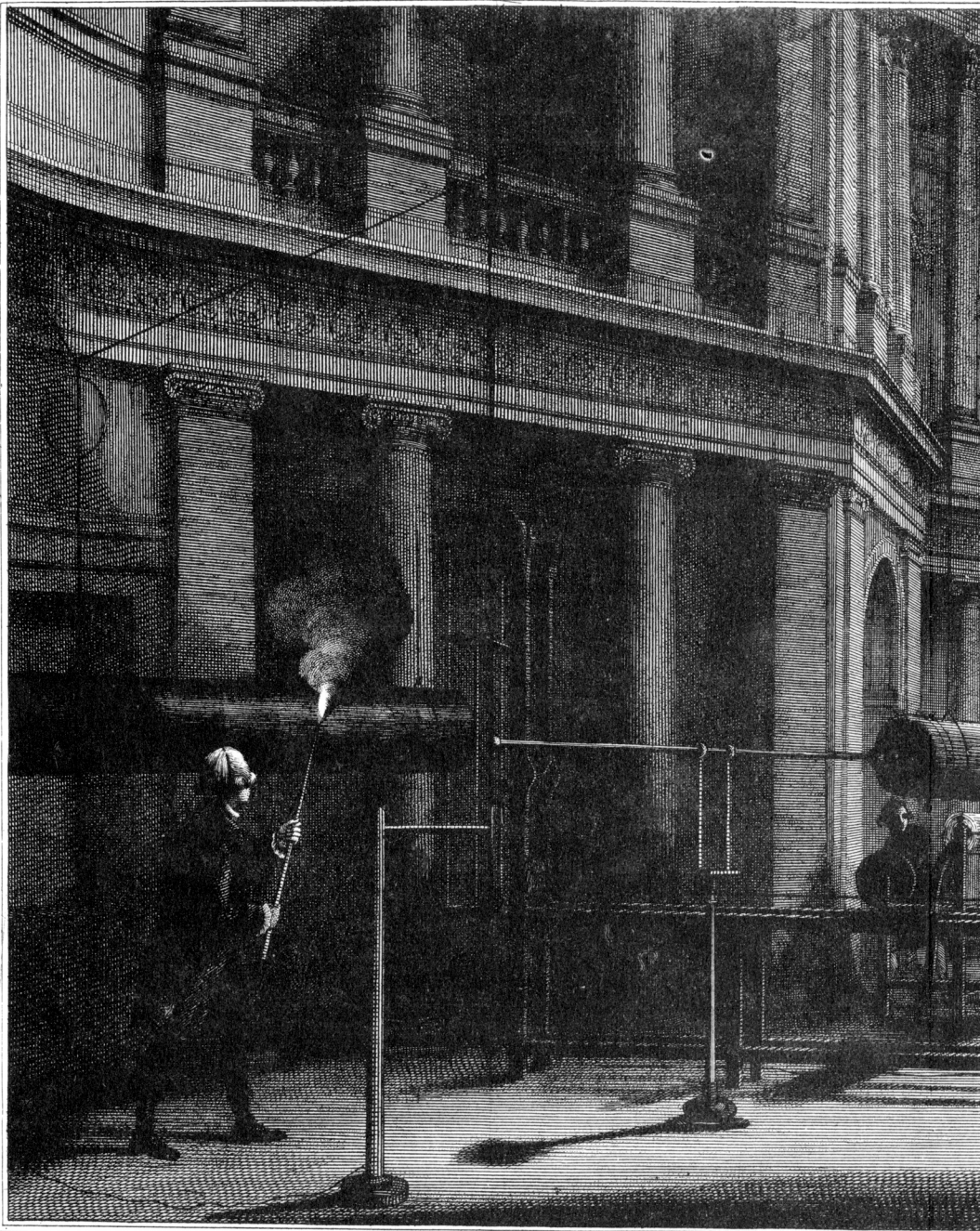
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most dutiful subject,

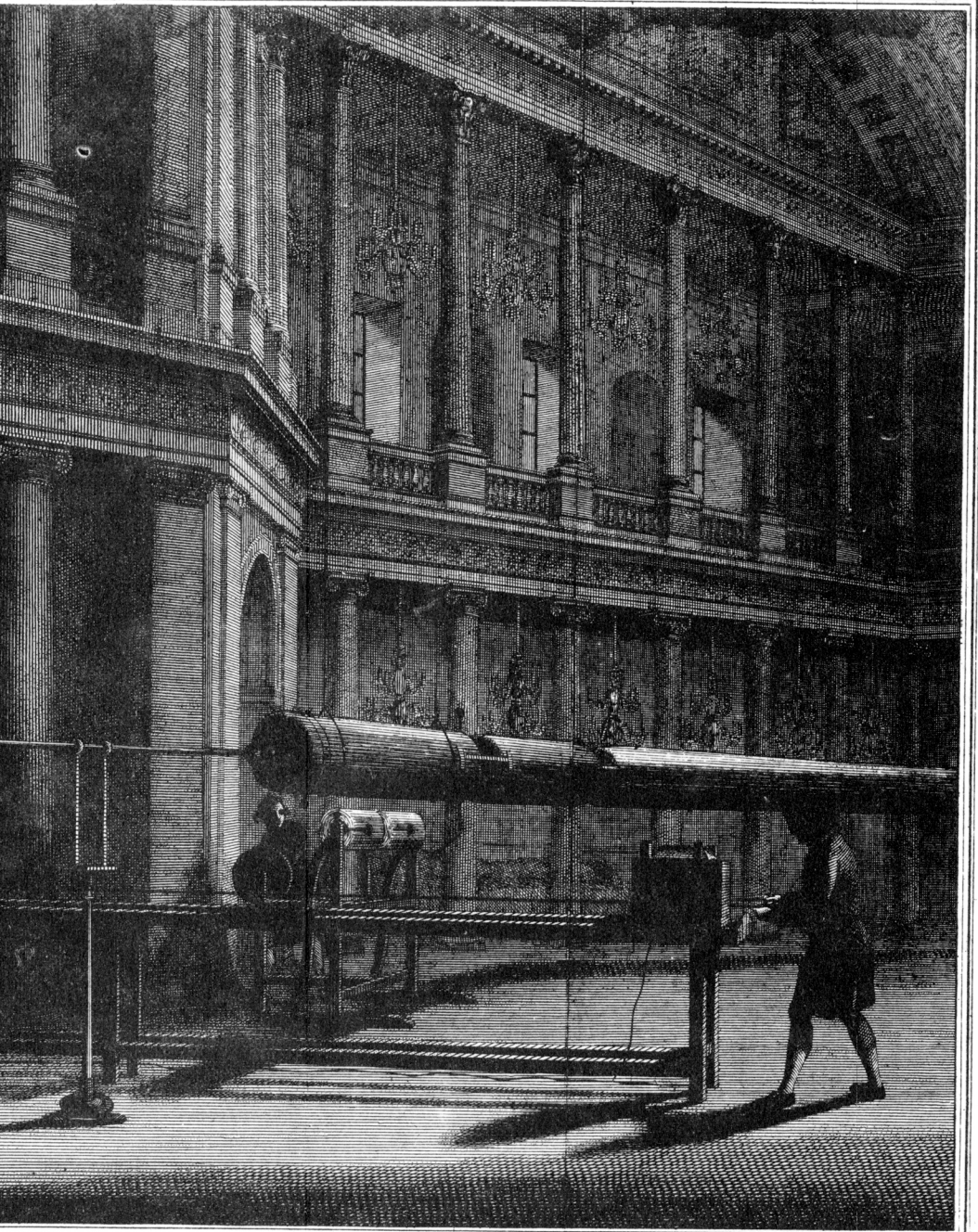
NOV. 12, 1777.

BENJAMIN WILSON.

*New*



*A View of the Apparatus and part of the Great*



*...s and part of the Great Cylinder in the Pantheon.*

*Engraved by J. Boffin.*

*New Experiments and Observations on the Nature and Use of Conductors, by BENJAMIN WILSON, F. R. S. of the Imperial Academy of Sciences at Petersburg, of the Royal Society at Upsal, and of the Academy of Institutes at Bologna.*

THE experiments I propose to give an account of in this paper, were made in consequence of the accident from lightning, which happened to one of the buildings belonging to his Majesty's magazine of gun-powder at Purfleet, on the 15th of May last.

Soon after that event, an official and particular account having been sent by the Board of Ordnance to the Royal Society, a committee of the members was immediately appointed, to examine the damage done to that building, and afterwards to make a report of the same.

When that report was laid before the Society, I thought it my duty, in particular, to stand forth, and offer some objections to the using pointed conductors at Purfleet, or indeed any where else.

This public proceeding was, I apprehended, the more necessary, as I had, upon a former occasion, in the year 1772, declared my *dissent* from the report then made by

the committee, who had recommended sharp-pointed conductors for that magazine, to be fixed ten feet higher than the respective buildings.

But notwithstanding I had read the paper alluded to above, I did not apprehend that my duty was fully discharged, without trying other methods of having so serious and interesting a subject farther inquired into.

I had the satisfaction, soon after, to meet with sufficient encouragement to induce me to consider of some experiments, which might make the subject in dispute more intelligible.

The plan I conceived to be the most proper for this purpose, was to have a scene represented by art, as nearly similar as might be, to that which was so lately exhibited at Purfleet by nature.

To carry a design of that kind into execution, it was necessary that attention should be given to the several circumstances concerned in the event at Purfleet.

The most material of those circumstances I apprehended to consist in having a substitute for a *thunder-cloud*, as it is vulgarly called, and large enough, or sufficiently long, to admit of being charged with a considerable quantity of the matter of lightning by artificial means; and likewise, that this substitute should admit of being easily moved, and with any velocity the experiment

ment required: or, at least, so as to equal the motion of a thunder-cloud.

An apparatus sufficiently large for these purposes could not conveniently be put in motion: therefore I proposed to get rid of this difficulty, by moving the building itself, instead of the substitute; as that would answer the same end.

In order to obtain a considerable charge of artificial lightning, I proposed to have one great cylinder covered with *tin-foil*, and a wire joined to one end of it, that should, when extended properly, consist of several hundred yards.

This idea leading to an expence too considerable for an individual, I presumed to hope for other assistance.

Upon an humble representation of these matters, his Majesty, who is always disposed to promote every pursuit which tends to the advancement of science and the good of the public, most graciously condescended to encourage the undertaking; and, by the favour of the right honorable and honorable Board of Ordnance, I was immediately enabled to carry the intended plan into execution.

Very soon after this encouragement, I procured correct drawings of the building called *The Board-house* at Purfleet: from these an exact model was made, excepting  
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the windows and doors, which were omitted, because they were immaterial upon this particular occasion.

In this model, a strict attention was paid to those parts of the building where metal had been introduced; such as the hips and gutters of the roof, and the several spouts to carry off the water. And as the North-east corner of the house was the part that suffered by lightning in May last, particular attention was paid to the two cramps at that corner, and the two spouts on the North side. These cramps, in the model, were made of small wire, that bore nearly an exact proportion to those in the building itself; not only in regard to length and thickness, but also their distance from each other, and from the turning up of the lead appertaining to the gutter.

The two spouts were represented each by a thick wire, the shorter of which communicated (at the bottom) with a cistern. This cistern resting upon two wooden pillars, or posts, about one foot and a half in length at Purfleet, the same circumstances and proportions were attended to, and made to correspond exactly in the model.

The other wire, in conformity to, and nearly in proportion with, the other spout at the Board-house, descending from the gutter for about seven inches, was there bent almost at right angles, and then continued on for twelve inches and three-quarters, in a line nearly  
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horizontal,

horizontal, till it reached within two inches (or little more) of the short wire: after which it was bent again almost at a right angle, and then lengthened out to the bottom of the model, from whence it communicated, by another wire, with a pump or well, in another part of the house.

This kind of communication was necessary, because, consisting of metal, it, in that respect, was similar to the communication at Purfleet.

Besides the two cramps mentioned above, another parapet was made to put on occasionally, which contained all the cramps: these were properly fixed therein, and at their proper distances from each other. And the Royal Society having thought proper, since the accident, to order that a metallic communication should be made between the cramps upon the parapet, quite round the building, as a better security from such accidents for the future, care was taken to make a similar connection with the wire cramps, by means of small slips of tin-foil that were pasted upon the parapet of this model.

On the top of the roof in the middle, conductors of different lengths and terminations were occasionally put, just as the experiments required.

The scale from which this model was made, when  
compared

compared with the house, was one-third of an inch to a foot.

In regard to the wood of which the model was made, I took care that it was well baked, and soaked, whilst hot, in drying oil, before the several parts were joined together, that it might be the more similar to the bricks and other materials of the building itself, in the power of resisting the passage of the fluid, whenever any attack thereof should be made. For brick, stone, dry lime, &c. had been observed many years ago by Mr. DELAVAL and others to resist the passage of this fluid very considerably.

In order to move this model with the velocity required, it was necessary to have a frame of wood, of such a length as would suffer the model, with the pointed conductor upon it, to be out of the reach or influence of the charge contained in the cylinder, both at its setting off, and when it had arrived at the end of its journey.

To this frame, two upright posts of wood, ten feet and a half long, were fixed at the farther end, and at a distance from each other equal to the width of the frame. Upon the top of these posts, and in the middle between them, were fixed two wheels of different diameters upon the same axis. The larger took the line

that was proposed to draw the model, and the lesser another line suspending two weights which regulated its motion. For after the heavier weight had descended so far as to bring the model directly under the substitute, it was then checked; but the lesser weight continuing to descend, the model moved forward with its acquired velocity, joined to the power of the lesser weight. And that the remaining motion might at last be overcome, without striking against the two posts, some narrow slips of cloth seven feet long, were nailed upon the frame, in those parts over which the model was to pass before it reached the end. This model moved like a sledge, by means of two slips of wood that were fixed at the bottom, which ran in two grooves that were cut along the frame from end to end. And the line which drew the model along, was fixed very near the center of resistance.

To construct the substitute for a cloud, I first joined together, in fifteen lengths, the broad rims of one hundred and twenty drums (merely to have them portable) by means of wood cut into long slips, which were fixed on the insides thereof: but, as those drums were not accurately of a size, the several joinings were covered over with cloth, and pasted down, to make the surface throughout more even. After this, the whole number were properly

covered with tin-foil, excepting eight; for these being brass, required to be covered only at their joinings. All these drums together formed a cylinder above one hundred and fifty-five feet in length, and above sixteen inches in diameter.

The whole cylinder was made in four separate parts; three of those parts could easily be made to communicate, or not, with each other: the fourth, being brass, was reserved for a different purpose. The several ends of those four parts were closed up with board, rounded off at the edges in every part, and covered with tin-foil likewise.

This great cylinder (consisting of the three parts) was suspended about five or six feet from the floor by silk lines; and formed a curve in the room, something like an horse-shoe; one end of which hung over the middle of the long frame, on which the model was proposed to move; the other (which I call the farther end) was joined occasionally to the end of a long wire, that was suspended through the whole space of the room. And lest the several atmospheres round this wire, in its charged state, might, in consequence of the unavoidable returns of the wire, interfere too much with each other, it was suspended in such a manner (by silk lines also) that each length was five or six feet from its neighbour: and those that were suspended nearest to the great cylinder hung at

the same distance from it. The remote end of this long wire hooked on occasionally at the end of the brass drums, which made a separate cylinder (the fourth part alluded to above) about ten feet in length: this was suspended likewise by silk lines, and about six feet from the floor; but in such a manner, that the farthest end thereof from the wire was within nine or ten feet of the great cylinder.

The long wire with the great cylinder and brass drums made the whole of the substitute for a thunder-cloud, when they were properly charged.

The machine, employed to charge this apparatus, consisted at first of two large glass cylinders that were turned by one wheel. But as the friction arising from the two together rendered it difficult to work them, and the advantage gained from both in the charge itself was found to be not so considerable as might reasonably be expected, one of them only was made use of in the following experiments. The place where this machine charged the great cylinder, was about ten or eleven feet from its nearest end. It was found expedient to be provided also with another machine; but this was employed only upon particular occasions, and was generally placed at the farther end of the great cylinder.

The floor of the room being of baked wood, it was necessary to have wires properly connected with the

cushions of both machines along the floor, where they were joined to another wire, which communicated with the well, in order to conduct the fluid more readily than the baked wood admitted of.

The whole of this apparatus, so contrived, was disposed in the great room of the *Pantheon*, by the favour of the proprietors, who, having heard that a large apartment was wanted, in which to shew before the Board of Ordnance and the Royal Society these experiments, were pleased to honour me with a very polite letter, offering the use of that elegant building for the purposes intended.

EXP. I. The model, with a pointed conductor upon it (which, in a degree of sharpness, was nearly equal to that of a common darning needle) being placed directly under the nearer end of the great cylinder, so that the distance between the point of the conductor and cylinder was little more than four inches, the machine was then put into motion. After two turns of the wheel, or thereabouts, a small stream of light appeared at a little interval, between the top of the longest thick wire which represented the bent spout, and its little cistern next to the gutter, where the metallic communication was purposely interrupted. This stream continued to be visible,

though the model was moved along the frame from its fixed station to more than the distance of forty-three inches.

EXP. II. When a conductor of the same length with the former, but rounded at the end, and no more than three-tenths of an inch in diameter, was put in the place of the pointed one, every other circumstance continuing the same, the small stream of light appeared again; but upon moving the model a little beyond the distance of sixteen inches, it totally disappeared.

FIRST OBSERVATION. By the first experiment it was manifest, that the point acted upon the charge all the time the model was moving through a space equal to forty-three inches; and consequently was, all that time, diminishing the charge in the great cylinder. On the other hand, the second experiment shewed, that the rounded end acted upon the charge only whilst the model moved through a space equal to sixteen inches. And from the two experiments compared, it appears, that a charged body is exhausted of more of the fluid by a pointed, than by a blunted, conductor.

EXP. III. If in the place of the rounded conductor a similar one was put, but about one-fifth of the length, whilst the model stood directly under the great cylinder



as before, the charge contained therein produced no appearance of light whatsoever.

EXP. IV. But when a pointed conductor of the same length with the last was put in its place, the small stream of light appeared, and continued visible all the time the model was moved through a space equal to eighteen inches and a half.

SECOND OBSERVATION. These last experiments, compared with the former two, shew that a rounded conductor, little more than one foot and a half above the highest part of a building, receives a far less quantity of the matter of lightning from a cloud fully charged therewith, than a pointed conductor placed ten feet above a building, circumstanced alike in every other respect. Nay, a pointed conductor of the same length with the short one that was rounded, appears, from these experiments, to collect a greater quantity of the fluid, than even the long conductor with a rounded end.

The difficulty of measuring exactly the effects, when the great cylinder was charged, was so great, that after a variety of endeavours to ascertain the quantity of the charge remaining, when different terminations and different lengths of conductors were employed, I was obliged at last to have recourse to the sense of feeling, uncertain as it is, in many cases, to determine the different effects

effects occasioned by the interposition of these different terminations.

EXP. V. On repeating the first experiment (that is, with the long pointed conductor upon the model) with ten turns of the wheel only, the charge remaining in the great cylinder was immediately received on the hand; the sensation it occasioned was little more than perceptible.

EXP. VI. But upon repeating the second experiment (that is with the long rounded conductor upon the model) with ten turns of the wheel also, the charge remaining was taken; but the sensation in this case was increased considerably.

EXP. VII. When the third experiment was repeated (that is with a rounded conductor five times shorter than the last) and with the same number of turns, the sensation was observed to be full as violent, as if no such metallic interposition had been presented to it.

EXP. VIII. Upon repeating the fourth experiment (that is, with the pointed conductor equal in length to that in the last experiment) and with ten turns of the wheel also, the sensation was not near so considerable; for it seemed something less than what was experienced with the long rounded conductor in the sixth experiment.

THIRD OBSERVATION. The several effects observed in the four last experiments agreeing so exactly with those in the four first, prove, at least so far, that rounded ends, in these cases, received the fluid less readily, and in less quantity, than points.

But, before other experiments are related, it may be proper to take notice of what passed at the short spout upon the model during the making of the four first experiments; because the consequences to be drawn from thence are very material.

EXP. IX. In the first experiment (when the long pointed conductor was put upon the model, and whilst the wheel was turning) a very small spark might be taken from the short spout; but if the hand, or a wire, was applied, which communicated with the well, and continued in contact with the short spout, or if it was connected with the long spout, the small stream of light seen before at the top of that spout now ceased.

EXP. X. But this was not the case when the third experiment was repeated (that is, with the short rounded conductor); for this, by reason of its greater distance from the cylinder, and the nature of its termination, did not draw a sufficient quantity of the charge from the great cylinder to cause the least appearance of a stream of light, as in the former case.

EXP.

EXP. XI. The effect, however, was different, when a pointed conductor of the same length as in the fourth experiment was made use of; for then the short spout was charged nearly in the same manner as in the first experiment, and the stream of light at the top of the bent spout disappeared the instant a communication was made from the short spout to the earth.

FOURTH OBSERVATION. The two posts of wood upon the ground, which supported the cistern at the bottom of the short spout, were therefore the true cause of these effects taking place in the short spout, by preventing a communication with the earth, and hindering the fluid, that was constantly charging the short spout, from discharging itself properly into the earth.

It may now be proper to take notice of other experiments, and also of two other circumstances that seem to be of considerable consequence in this inquiry.

One of these circumstances respects the motion of the model, instead of that of the cloud; and the other, the quantity of the fluid contained in the great cylinder when properly charged.

As to the former of these, it appears from observations, that clouds, in a very high storm, frequently move at the rate of eighty miles in an hour; and, with a moderate wind, about twenty miles<sup>(a)</sup>. Now when the clouds

(a) In the Phil. Transf. there is an account of a storm that was computed to move at the rate of three miles in a minute.

move at the rate of four or five miles an hour, the wind which occasions that motion is, I apprehend, in general, little more than what is usually said to be sensible: for it has been frequently observed, that a traveller on horseback must ride with a pretty good speed, to keep pace with the shadow from a cloud, when it chances to move in the direction of his journey, even though the wind at such a time can scarcely be said to blow.

Now a thunder-cloud, as it is usually called, moves, as I apprehend, in many cases at least, with a much greater velocity; and though there may be a few instances, where such clouds move with a less, I think, if the motion of them be put, in general, at four or five miles an hour, it ought to be considered, in this case, as a very moderate computation. For these reasons, the weight to draw the model was adjusted to that velocity.

In regard to the quantity of the fluid required to charge the great cylinder, I found, from many experiments, that twenty uniform, and rather brisk, turns of the wheel, were the most favourable for the following experiments; because half a turn in twenty made a far less difference in the charging, than half a turn of the same wheel, when eight or ten turns only were required, and with nearly the same uniform velocity.

In observing this last rule, proper allowance was made for the different states of the air, the state of the glass itself, and that of the cushion or rubber, which excited the fluid by a proper pressure upon the glass, joined to other circumstances. The most material of these circumstances consisted in the cushion's having a free communication with the earth and glass cylinder, in that part where the friction was applied; and also a similar communication between the great cylinder and the opposite side of the glass. Now, because three pointed wires conducted the fluid readily from the glass to the great cylinder, I apprehended it was proper to have three such points within the cushion itself to conduct the fluid as readily from the earth to the glass. For those points communicated by a wire with the well.

It will appear, that the effects produced by this artificial method of charging the cylinder must be very different from those produced in nature.

Suppose a thunder-cloud coming already charged, the lightning from this cloud strikes at Purfleet; the same cloud, passing afterwards over other places, will strike the earth again and again, without any apparent diminution of the quantity of lightning contained therein, as hath been frequently observed. But the case is not the same with the great artificial apparatus, because we not

only charge it by degrees; but when a stroke is taken from it, the greatest part of the charge by far is at that instant taken out of it: and therefore we are constantly under the necessity of renewing the charge before such another stroke can be taken.

Besides, thunder-clouds from their nature, and a variety of circumstances accompanying them, never assume the same shape and size: neither are they always at the same distance from each other. And yet we are told, that they have been observed to strike from one to another at different distances, just as they happen at the time to be circumstanced.

For all these reasons, I thought it was proper to make particular experiments in different cases, where some of the circumstances varied. And because a single cloud, after it hath struck any one object, sometimes continues to discharge vast quantities of lightning, I proposed to begin the experiments with the great cylinder only.

EXP. XII. The model (furnished with the wire of communication, and with the longest pointed conductor upon it) being properly placed upon the long frame, and held there in readiness to be drawn forward by the line and weight at the other end; the great cylinder was charged by twenty turns of the wheel. On letting go the model, and almost at the instant before the point came  
I  
under

under the center of the cylinder, it was suddenly struck with the matter of lightning, and frequently sooner. The least distance of the point from the great cylinder, when this stroke happened, measured nearly five inches. The quantity of charge that remained in the cylinder was very little to the sense of feeling, though taken immediately after the stroke happened.

EXP. XIII. On putting into the place of the pointed conductor one of the same length, that was rounded at the end, and without any other change of circumstances, the wheel having been turned the same number of times, I suffered the model to pass: the rounded end, in this case, was not struck. However, the instant after it had passed, the quantity of the charge that remained in the cylinder was taken in the same manner by the hand; on the doing of which, the sensation was more violent than in the last experiment.

FIFTH OBSERVATION. From the two last experiments it appears, that though every circumstance was the same, excepting the different terminations of the two conductors, yet the pointed one only was struck; notwithstanding they were both of the same length, and passed at equal distances from the cylinder. From whence we collect, that the quantity of lightning discharged from the great cylinder into the point, when an explosion happened,

was



was considerably greater than the quantity discharged into the rounded end, when there was no explosion.

At the first rise of a difference in opinion, respecting the proper termination and length for conductors, I was prevailed upon by some learned members of the Royal Society, in the year 1764, to publish my sentiments upon that subject. Accordingly, in a letter addressed to the Marquis of Rockingham <sup>(a)</sup> (after stating several reasons against the use of points, as I suppose they invited the lightning) I there recommended that conductors should not only be rounded at their ends, but be made considerably shorter than those which Dr. FRANKLIN contended for, and indeed should not exceed the highest part of the building.

In the following experiment, however, I did not place the pointed conductor below, nor upon a level with, the highest part of the building, but above it, even one-third of the length of that in the twelfth and thirteenth experiment.

EXP. XIV. The model being thus furnished, and every thing else put exactly into the same circumstances as in the thirteenth experiment, the great cylinder was charged by twenty turns of the wheel. Upon letting go the model, it passed the cylinder at the distance of seven inches, without being struck: but the charge that

(a) Phil. Transf. vol. LIV.

remained

remained in the cylinder at the instant after the model had passed it, was so considerable, that there appeared no material difference whether the model thus circumstanced was suffered to pass or not.

SIXTH OBSERVATION. This last experiment shews, that a thunder-cloud may pass a conductor so circumstanced without the latter being struck, or suffering the least injury; which it will not do in other circumstances, that is, when the conductor is pointed, and raised ten feet above the building.

EXP. XV. On repeating the fourteenth experiment, but with a rounded conductor, which was three-fourths of the whole length of that in the twelfth experiment (all other circumstances remaining the same) and after charging the cylinder by an equal number of turns, it passed also without being struck. In this case, the remaining charge in the cylinder was something less than in the last experiment.

SEVENTH OBSERVATION. This is a further instance of the advantage derived from having rounded terminations of a given length upon a building, compared with pointed ones, that are only two or three feet longer.

Having so far experienced the different effects of different terminations in the preceding experiments, it may be proper to mention another experiment, where the rounded

rounded end was struck. But as its distance from the cylinder, at that instant, was only one quarter of an inch less than the distance at which the point had been struck, I shall take no farther notice of it in this place; because, the several striking distances of those different terminations will (in another part of this paper) be correctly ascertained, in a different manner, by other experiments.

We are now to examine, whether the stroke by lightning, which happened at Purfleet, fell first upon the corner of the building where the cramps were affected, as hath been represented; or whether it did not fall upon the point of the conductor itself? And if it fell upon the point, how could it possibly affect those cramps, as they had no metallic communication with the main conductor which extended from the top to the bottom of the house, and of which the gutter below the parapet, that was nearest to the cramps, made a part?

It must be remembered, that though in attempting an experiment of this kind, we are provided with an apparatus, greater perhaps than was ever constructed before; yet, great as it is, it bears but a very small proportion to that which nature makes use of. On this account, we must expect but very faint appearances compared with those which are produced by a thunder-cloud. But, before we relate these appearances, it may be proper first to

see

see what effect the charge itself has upon the model, without any conductor upon it.

EXP. XVI. Upon charging the great cylinder as before, that is, by twenty turns of the wheel, and when the model, without any conductor upon it, was let go, there was no explosion in, or on, any part of it, during the time of its passing by the cylinder; notwithstanding the model itself was properly connected with metal from the top of the roof to the bottom of it, and afterwards to the well.

EXP. XVII. But when the experiment, with the long pointed conductor upon the model, was repeated, and after the wheel had been turned an equal number of times, the point was struck as it passed the cylinder; and, at the same instant also, a very small stream-like explosion appeared between the two cramps at the corner of the model, darting as it seemed from one to the other, in a direction that was rather particular. This stream-like appearance, I apprehended, was nothing more than the effect of a small explosion in consequence of the motion of the model.

Mr. WYATT, who is well acquainted with this part of Philosophy, and to whom great obligations are due from me for his very friendly assistance in the whole of this undertaking, having observed this appearance several

times, was curious to confirm the fact, lest the prejudices of vision, &c. might possibly deceive him.

The method he took upon this occasion, was to observe an appearance of light along that hip of the roof which was next to the corner struck; and whether the direction of that appearance was different from the direction of the other appearance he had observed at the corner between the two cramps. These appearances he endeavoured to ascertain by means of a paper tube, blacked on the inside, that was laid upon a stand of a proper height, so that, when he looked through it, a pin, being stuck upright at the further end thereof, coincided with the part in the model he was to examine; that is, when the model stood directly under the cylinder. Being thus circumstanced, and when the corner of the model (during its motion) passed the pin, he saw the direction of the light along the hip next the corner, and also between the cramps at the same time; and was positive that the direction of the light appeared to be downward from the roof, and, as he thought, horizontal between the two cramps. These appearances were observed by others afterwards, exactly as Mr. WYATT had before described them.

EIGHTH OBSERVATION. The fifteenth experiment shewed, that the corner of the model, where the two  
cramps

cramps were inserted, passed safely by the charged cylinder, without affording even the least luminous effect; and consequently proved, that in such circumstances the two cramps could not possibly be struck, because the charge in the cylinder remained the same, or very nearly so, after the model had passed.

NINTH OBSERVATION. But in the seventeenth experiment, the pointed conductor was fixed in its place upon the model, just like that at Purfleet; when not only the point was struck, as the model passed the cylinder; but, at the same instant, a small explosion was seen between the two cramps at the corner. That this light between the cramps arose from a different cause than what had been suggested by the second Purfleet committee, appeared from some circumstances accompanying that effect. For example, these cramps had no connection with the gutter or spouts next them, but were quite separate, and at the distance of six or seven inches from any metallic communication. And it is well known to philosophers, that lightning always passes where it meets with the least resistance; they also know, that the least resistance in the present instance must have been along the conductor at the top to the hips, gutters, and spout, and so on to the wire at the bottom to the well.

According to this law, the cramps themselves, then, were not properly circumstanced to receive the fluid as it passed to the earth, on account of the metallic communication, described above, being interrupted more than six inches.

Another cause therefore was necessary to explain the appearance; I mean that which is called the *lateral effect*; a term lately adopted, in consequence of an experiment I made near thirty years ago with the Leyden phial (which experiment has since been improved upon by Dr. PRIESTLEY): and though a charged glass (for such I call the Leyden phial) is by no means similar to the great cylinder when charged, for reasons that have been already published<sup>(b)</sup>; yet in the course of these experiments I apprehended the lateral effect produced with the charged glass might be considered in some respects as an illustration of the lateral effect which had been observed at the cramps.

But, not to rest this very material fact upon any doubtful opinion that may possibly be entertained respecting its existence, other experiments will be produced, that may explain this matter more satisfactorily.

(b) Farther Experiments and Observations upon Lightning, by BENJAMIN WILSON, published in 1774.

Before these experiments are related, it is proper to mention a material circumstance, which hath hitherto been unnoticed.

At the time the accident happened at Purfleet, a great quantity of rain fell, by which the walls of the Board-house, being made very wet, were disposed to admit more readily the lightning passing upon the surface, though yet not so readily as a covering of metal would have done. This particular circumstance of the rain will be attended to in its place.

But first it may be proper to shew the effects of a metallic communication.

To this end, I covered the top of the parapet quite round with tin-foil; and because the coping projected a little over the parapet, under which no rain could possibly get, I left a small interval, proportional to it, uncovered with that metal. This interval was on the opposite side of the model, which answered to the South-side of the building where the other spouts were fixed. Then from that interval I passed a broad slip of tin-foil down to the bottom of the model, where having fixed a small brass staple to hook a wire upon, I fastened the other end of it to the wire which communicated with the well. At the same time care was taken to fasten another wire in the same manner, which communicated with the opposite

site



site side of the model. This wire was intended to answer the purpose of a spout in the building at Purfleet towards the South.

Besides those precautions, there remained another circumstance to be attended to, respecting motion: for several gentlemen had observed, that clouds in a thunder-form are not always in motion; or at least not in the degree of motion which has been represented in the preceding experiment. To obviate that objection, I proposed to repeat those experiments when the model was at rest. In order to prepare the whole apparatus in the most proper manner for this purpose, it was necessary to attend to the circumstances that are observed in nature.

For when a cloud comes ready charged, and strikes another cloud with the matter of lightning, there must be a certain distance at which that effect must take place; and this we therefore call the greatest, or the striking distance. Now, to produce similar effects, our artificial apparatus must consist of two parts at least, to represent such clouds: and those parts must be so disposed as not to exceed the greatest distance at which they will strike, when the largest part, or substitute, is properly charged: nor must the distance between them be less, for reasons that will appear presently. And therefore, to make the experiment correspond with nature, it will require some trouble

trouble to adjust this distance between the two substitutes. Now the distance between them will depend upon the quantity of the charge given, and the method to determine that distance may be found by removing one substitute from the other so far as not to cause any previous or partial explosions before the great stroke happens; and when it does happen, it must not only strike between the substitutes, but, at the same instant, between the remote end of the less substitute and the object opposed to it.

EXP. XVIII. To each end of a slender substitute made of wood, about eleven feet in length, and something less than one inch in diameter, was fixed a ball of the same matter. The larger of these measured three inches in diameter, and the lesser one inch nine-tenths. The exact measure of these balls was attended to the more, in some of the experiments, because Mr. NAIRNE has given a description of an apparatus in the *Phil. Trans.* vol. LXIV. part I. p. 87 and 88. to which this is nearly similar. Having covered the whole of this slender substitute with tin-foil, it was then supported near the center by a slender frame of wood upon a pillar of glass, to adjust it to the height and distance required. The larger ball was then brought within one inch and a quarter (and sometimes at a greater distance) of another ball, one inch nine-tenths

tenths in diameter, that was covered with the same metal, and projected from the center of the nearer end of the great cylinder by a kind of stem made of wood, and covered with tin-foil also, which was six or seven inches long. Being so prepared, the model was set upon a table, directly under the ball at the remote end of the less substitute, with the pointed conductor upon it: and all the wires were properly connected, so as to make a free communication between the model and the well. Nothing now remained but to put the machine in motion; when, after ten turns of the wheel, the point upon the model was struck at the distance of four inches.

In the twelfth experiment, where the model was in motion, the point was struck at the distance of five inches nearly from the cylinder. This difference of distance in these two experiments seemed to arise chiefly from a difference in the states of the air, it being rather unfavourable when the eighteenth experiment was tried.

EXP. XIX. However, some time after, when the state of the air was very favourable, I repeated the last experiment, and observed that the point was struck at six inches and one quarter.

During this last experiment, a person, standing upon the wire of communication, placed his finger in contact with the pointed conductor (near the bottom) at the time

the

the stroke happened, when he received a blow, uncommonly violent, infomuch that he thought it exceeded greatly what he had ever experienced from the great cylinder only.

EXP. XX. I now repeated the eighteenth experiment; and attending only to the two cramps at the corner, there appeared, at the instant when the point was struck, a small spark or explosion between them: which clearly shewed, that the stream-like explosion, observed in the seventeenth experiment, was only this small spark accompanied with the circumstance of the motion of the model.

TENTH OBSERVATION. From what we have now experienced it appears, that thunder-clouds, even at rest, and that strike each other at a given distance with the matter of lightning, occasion the same phænomena nearly which a single cloud produces when motion is introduced.

EXP. XXI. When the distance between the two substitutes was made less in any degree than the greatest striking distance (in proportion to its diminution the circumstances were less similar to those in nature) it made a considerable difference in the effects; because the fluid in these cases passed more freely from the greater to the less substitute: and the freer it passed into the latter,

the nearer they approached to be one substitute. So that bringing the two substitutes into contact, occasioned the same phænomena that the great cylinder did alone; that is, the rounded end would cause an explosion at a considerable distance; and the point little or none, notwithstanding it was brought almost close to the substitute.

EXP. XXII. But if motion in this case was introduced, during the contact of the two substitutes, the point was struck at nine inches and an half distance from the ball. The motion employed upon this occasion was by the hand only, which held a proper stand with the point upon it; and this point communicated by a wire with the well.

ELEVENTH OBSERVATION. Now the nearer the two substitutes were brought together, the nearer they represented one cloud; and, consequently, as hath been observed before, the matter of lightning would pass from one to the other in these cases more readily, and without permitting so great an accumulation to take place as was experienced in the nineteenth experiment, when the separated substitutes struck at the greatest distance.

To occasion such a stroke, it is not only necessary to have an accumulation of the fluid, but that accumulation must be kept up, as it were, and increased suddenly, before a stroke in this case can possibly take place; as  
appeared

appeared manifestly by the motion introduced in the twenty-second experiment, where the two substitutes were united: for the resistance at a point being feeble, when in this experiment a point was suddenly brought towards the ball at the end of the great apparatus in charge, the whole of that charge must of consequence rush towards the point in an instant, to discharge itself into the earth. This, I apprehend, was the true reason why we obtained an explosion at the distance of nine inches and an half.

Having now gone through the experiments where points were introduced, we shall next relate the several experiments where other terminations were used. By this method of proceeding, we shall be able to form a proper judgement what kind of conductors are the most advantageous for securing buildings, &c.

EXP. XXIII. On repeating the eighteenth experiment, but with a rounded conductor upon the model, every other circumstance continuing the same, and the model at rest, the greatest distance at which it was struck (in consequence of ten or eleven turns) was not more than three quarters of an inch.

Now if we compare this distance with that at which the point was struck in the eighteenth experiment, the proportion will be found to be less than one to five.

But this and the eighteenth experiment were repeated many times afterwards, for many days together, and when the state of the air at each trial was very different; on which accounts the results of those experiments varied as follows.

Sharp point.

$2\frac{1}{4}$  inches.

4

$3\frac{1}{4}$

$5\frac{1}{2}$

$6\frac{1}{2}$

2

Rounded end.

$\frac{5}{8}$  of an inch.

$\frac{1}{2}$

$\frac{7}{8}$

$\frac{4}{10}$

$\frac{1}{2}$

$\frac{1}{10}$

Immediately after relating the nineteenth experiment, mention was made of the sensation or blow that was received by a person who had brought his finger into contact with the pointed conductor at the time the stroke happened. The experiment was repeated here with the rounded end; but the sensation or blow received (as well as the explosion) was considerably weaker in this case.

EXP. XXIV. When the twenty-third experiment was repeated, and the rounded end struck at three quarters of an inch, the same kind of spark appeared between the two cramps as in the twentieth experiment when the point was used; but in this experiment with the rounded

rounded end the spark at the cramps appeared considerably less to every observer.

EXP. XXV. Upon repeating the twenty-first experiment where the two substitutes were brought into contact, every other circumstance remaining the same, the rounded end was struck at the same distance nearly as when a spark was taken by a larger metal ball (suppose three inches in diameter) from any part of the great cylinder when equally charged: for in this case the two substitutes, being in contact, made in reality but one great substitute.

EXP. XXVI. I now repeated the twenty-second experiment, where motion was introduced; and without any other change of circumstances than putting in the place of the point the rounded end. Upon this occasion, as well as upon the former, the same person moved the stand with the rounded end upon it, and with the same velocity (as near as he and others present could judge) but not before the connected substitutes were fully charged by an equal number of turns. The instant that the rounded end approached within a certain distance of the ball at the end of the less substitute, it struck; but the explosion seemed inferior to that which the point occasioned at the distance of nine inches and an half. In this experiment the distance between the rounded end and  
ball



ball was not more than six inches and an half. From which it appears, that even in those circumstances the point was struck at a greater distance than the rounded end in the proportion of nine and an half to six and an half.

EXP. XXVII. It has been observed in a former part of this paper, that a great quantity of rain fell when the accident happened at Purfleet; as this circumstance seemed to be material, it was proper to put the model into a similar situation; and therefore, after removing the tin-foil upon the parapet, I washed the model all over with a sponge; and, whilst it continued in this state, the machine was put into motion: after ten turns of the wheel, the point was struck at five inches distance. In consequence of this, a small explosion (more vivid than in the seventeenth experiment) appeared, not only between the cramps; but also another was seen, still more vivid, at the inner corner of the parapet, nearest to the cramps, darting, as it seemed, from the gutter up to the cramps.

EXP. XXVIII. Upon pasting tin-foil upon the top of the parapet quite round the model, as had been done before, and moistening the inner part of the parapet down to the gutter, the stroke was again received by the point, when the same appearances were observed as in the last experiment.

EXP. XXIX. I then made a metallic communication between the top of the parapet down to the gutter : after this the experiment was repeated. And though the point was struck at five inches distance, there was no spark whatsoever, either between the cramps or at the inner corner next to the gutter.

TWELFTH OBSERVATION. From these last experiments it appeared, that rain contributed to make the *lateral* effect greater at the corner, by forming a better communication between the cramps and the gutter, than the dry materials of brick and stone admitted of. But it also appeared that, when the communication between the gutter and cramps was rendered more perfect by a slip of tin-foil that was interposed between them, that *lateral* effect ceased at the cramps; because a still freer passage was made for the fluid to discharge itself through; not only along the slip of metal communicating with the gutters and the tin-foil quite round the parapet, but also along the tin-foil down the side of the model, from whence it was conveyed by the wire to the well.

EXP. XXX. An objection having been made, that the wire communicating from the bottom of the model to the well, as it consisted of several distinct parts, occasioned a resistance at each of the junctions, and therefore constituted only an imperfect conductor; in order to try  
the

the validity of this objection, a new communication was now made from the model to the well, consisting of one entire wire, pointed at the end, with which I repeated the experiment of passing the model; and finding no sensible difference from the former results, I then, in order to apply a rounded end to this perfect communication, had the pointed conductor, which was previously made use of, foldered on to the end of the wire instead of the other point; and to this conductor so foldered I occasionally applied the small ball which was used in the former experiments, and which has hitherto been called the rounded end. It was made to fit upon the point of the foldered conductor by means of a socket, so as to render the communication perfect. With these different terminations, and thus circumstanced, all the experiments were repeated; and it was found, that the results were rather more in favour of the doctrine hitherto advanced, than before the communication was made so perfect.

It having been farther objected, that the motion of the model employed in these experiments was considerably greater than the motion of a thunder-cloud, I made the following experiment.

EXP. XXXI. The weight which moved the model in the preceding experiment was gradually reduced till it was nearly balanced by the friction; and when the  
motion

motion was rendered so slow as seven feet seven inches in seven seconds, it was very little accelerated; and in this state the great cylinder being charged, the model was suffered to pass: and though the velocity was less than three quarters of a mile in an hour, the point was struck. This experiment was repeated several times, with the same success, in the presence of several gentlemen.

Having made two other experiments, respecting the different terminations of conductors, and in very different circumstances from any that have been yet related, I shall here give an account of them.

EXP. XXXII. When the great ball of the less substitute was placed at the greatest striking distance from the ball at the end of the great cylinder, I fixed a needle into the under side of the remote end of the less substitute, with the point downwards: opposite to this point, and upon the same stand described in the twenty-second experiment, was fixed another needle; so that the two points were opposed to each other. The space between them was varied from time to time, in order to find the greatest distance at which the lower one could be struck. Upon charging the great cylinder it appeared, that the greatest distance in this case was five inches and a quarter.

EXP. XXXIII. Upon repeating this experiment, whilst every circumstance remained the same, excepting that, instead of the point below, a rounded end was put in its place, and after charging the cylinder again, it appeared, that the greatest distance, at which the lightning (from the needle) struck the rounded end, was not more than two inches and three quarters. And the largeness of the spark, as likewise the loudness of the explosion, appeared to be less considerable than in the thirty-second experiment.

The reason for fixing the needle at the end of the less substitute with the point downward, was to represent a fragment or jagged part of a cloud, which sometimes hangs down towards the earth; and, as Dr. FRANKLIN and others have supposed, serves as a kind of stepping-stone for the lightning to pass more readily, and in a silent manner, from a charged cloud to a pointed conductor underneath it. But we see, from the two last experiments, the lightning does not pass in a silent manner, because the point below, as well as the rounded end, was always struck, and the former at twice the distance nearly of that of the latter.

That two points opposed to each other, in the manner described above, should ever occasion a stroke of lightning, may perhaps appear strange to those who are not  
very

very well acquainted with this subject. However, I have related it not only as it is a fact of a very curious kind, but as the consequences which may be drawn from it seem to be considerable.

During the course of this inquiry, having occasion to try some experiments in the dark, I observed a curious circumstance, which seemed to shew, that a point had a far greater influence upon the charged substitute, in certain circumstances, than a rounded end had when it was placed in the same situation.

EXP. XXXIV. The circumstance alluded to was an appearance of light upon the brass ball (for so I call it, to be more clear in the description) that was fixed at the end of the great cylinder, when the copper ball (for so I call *that* for the same reason) of the less substitute was opposed to it at the greatest striking distance, as in the eighteenth experiment, every other circumstance remaining the same; and whilst the model, with its pointed conductor, stood upon the table directly under the tin ball that was fixed at the remote end of the less substitute: for soon after seven or eight turns of the wheel, a light began to appear on the brass ball, and continued to increase in brightness till the moment it burst forth in an explosion towards the copper ball. The part of the brass

on which the light appeared was that next to the copper ball: and the general appearance of it was round, and sometimes more than half an inch in diameter. It did not send forth rays or streams that were luminous; neither did it extend beyond the surface; or the distance to which it did extend was so inconsiderable, as to seem incapable of being ascertained, even at the instant before the explosion, when it was most vivid; though at that time there did appear something like a small swell towards the centre, as if it was making an effort to get out. Whenever the wheel was stopped suddenly, or the motion of it decreased, the light retired on the instant, and totally vanished: but when the motion of the wheel was renewed for a little time, the light returned as before. The whole time that this appearance continued, was never more than five or six seconds: reckoning from the moment it was first seen, to the instant when the explosion happened. The distance between the point and the tin ball measured three inches and a quarter. There was no such appearance on the copper ball; nor is it easy to conceive how there ever should, when all the circumstances are taken into consideration.

EXP. XXXV. Upon repeating this experiment with a rounded end, instead of a point, and at the same distance from the tin ball, notwithstanding every other circumstance

stance continued the same, there was no such appearance.

EXP. XXXVI. But when the rounded end was moved considerably nearer, that is, within six-tenths of an inch, a light was visible; but then it was faint, and not more than one-tenth of an inch in diameter, even at the instant before the explosion happened.

THIRTEENTH OBSERVATION. By the first of these experiments it appears, that the influence which the point had upon the whole of the fluid contained in the great cylinder, was such as to cause a general tendency of it towards the less substitute; but, on account of the resistance which seemed to operate at the surface of the brass ball, it was there stopped, and by degrees accumulated, till such time as the accumulation was great enough to overcome that resistance. Now, according to this manner of reasoning, the point did not draw the fluid out of the great cylinder silently; but when the accumulation had got to a sufficient degree, a sudden explosion ensued, more or less violent, according to the circumstances which accompanied the experiment.

FOURTEENTH OBSERVATION. From the other experiment it appears, that the rounded end had not so great an influence as the point upon the charge in the cylinder; because we were obliged to bring it five times nearer



nearer before any light could be perceived at all; and even then it was so faint and inconsiderable in its diameter (rather less than one tenth of an inch) compared with the other light produced by the influence of the point, that it manifestly confirmed the truth of the last observation.

These facts being in my opinion so clear and satisfactory in regard to the great object we have had in view, I think that any farther experiments respecting the nature and use of conductors are unnecessary. I shall therefore proceed to make some general deductions from what has been already related.

It seems to be clear, that in all experiments made with pointed and rounded conductors (provided the circumstances be the same in both) the rounded ones are by far the safer of the two, whether the lightning proceed from one or more clouds; that those are still more safe, which (instead of being, as Dr. FRANKLIN recommends, ten feet high) are very little, if at all, above the highest part of the building itself; and that this safety arises from the greater resistance exerted at the larger surface.

The luminous appearance at the end of the brass ball, occasioned by the point in the thirty-fourth experiment, manifestly shewed that there was an accumulation of the fluid within that part of the ball, in consequence of some  
resistance:

resistance: for when the resistance at the surface of the brass ball was at last overcome by the influence the point had upon the charge, the explosion took place immediately; and that, not only between the two substitutes, but also between the end of the less substitute and the point.

A cloud, therefore, that happens to be charged, and within the striking distance of another cloud which is not charged, and also equally within the influence of a pointed conductor, must necessarily produce similar effects with those mentioned in the thirty-fourth experiment.

On the other hand, clouds that are circumstanced like those above, and not within the influence of a rounded conductor, will pass quietly over such a termination, and without any explosion.

Nor can any one, at all acquainted with subjects of this kind, want to be reminded, how far the effects of experiments, made within the limited power of such an apparatus, must differ in degree from those which are exhibited in the great and wonderful phenomena of nature.

If I could grant to those who object to the motion employed in some of these experiments, that a cloud charged with lightning, and motionless, may impend

over a building; it must nevertheless be allowed, that clouds are generally observed to move with considerable velocity; so that when the velocity is from three quarters of a mile or something less, to four or five miles or more in an hour, the pointed conductor is always struck by the charge contained in the great cylinder. And the experiments which have been made by the addition of the less substitute shew also, that no security can be expected from a pointed conductor when a thunder-cloud is even entirely at rest.

#### UPON ACCELERATION, AND ITS EFFECTS.

From considering the extraordinary effects which have sometimes been produced upon gross matter by lightning, and the distance there frequently is between thunder-clouds and the earth, when such effects take place, I suspected that those effects might in some degree be owing to an increase of the velocity of the fluid which produced them.

To try whether this was really so, it seemed necessary to have an apparatus of a far greater length than the great cylinder: I therefore made use occasionally of the long wire which has been already described.

EXP.

EXP. XXXVII. Upon connecting one end of this long wire with the farther end of the great cylinder, and the other with one end of the brass drums; I found, that about six uniform turns of the wheel, with a moderate velocity, were required to cause the appearance of a small stream of light at the top of the spout described in the first experiment, when the model, with the pointed conductor upon it, stood directly under the great cylinder, but at the distance of five inches.

EXP. XXXVIII. When the great cylinder was unconnected with the long wire and brass drums, and whilst the model, with the same conductor upon it, remained in its place; about two turns, with the same velocity, were sufficient to charge the great cylinder, so as to cause a similar appearance at the spout.

EXP. XXXIX. On separating the great cylinder from a fourteenth part of it (which fourteenth part for the present I call the little cylinder) the model and machine continuing in their places, it was found, that half a turn of the wheel was sufficient to charge the little cylinder, so as to cause the like appearance at the spout.

FIFTEENTH OBSERVATION. Now these differences in the numbers of turns required for causing similar appearances, when the several charges were given in the thirty-seventh and thirty-eighth experiments, could not

arise from a difference in the quantity of metallic matter contained in the respective substitutes; because the tin-foil which covered the great cylinder (independent of the nails and wood it contained) was found to be near three times heavier than the weight of the whole wire.

Neither could these differences be owing to a difference in the quantity of surface of the respective substitutes; because the surface of the great cylinder was found to be ten times greater than the surface of the wire.

Those several differences must therefore depend upon some other cause; and, as a true knowledge of this cause may be of some moment in the present inquiry, we must endeavour to find it out by experiments and observations.

To this end it may be necessary, that our inquiry should begin from an early part of this subject, so that we may proceed regularly, step by step, as nature directs. By pursuing this method (though it is proposed to be done very generally on account of the length of this paper) we may possibly arrive at the knowledge of the cause in question, and shorten the road to the main object in view; I mean, the effects of comparative velocities.

From the nature of this subtle and elastic fluid, and its being diffused throughout the whole earth as well as

the air furrounding it, the leaft violence exerted muft neceffarily difturb it.

And although experience hath taught us how to vary the natural quantity of this fluid in many fubftances, in confequence of violence; yet the fame experience hath likewife taught us, that we cannot increafe that quantity in any particular fubftance, without taking it from the general ftock contained in the earth or air furrounding it: and therefore, when this fluid is fo increafed, it may be properly faid to be in an unnatural ftate; and whilst it remains fo, muft (from its elaftic principle) be always endeavouring to recover its natural one.

But experience hath alfo taught us, that metal, for example, hath a property of receiving the fluid more readily, whenever it is difturbed, than moft other fubftances: for which reafon a notion hath prevailed with many, that this property of metal arifes from a power of attraction, which they fuppoze it poffeffes in a greater degree than any other fubftance.

If this philofophy were true, it would follow, that the fame power which attracted the fluid into the metal ought to keep it there: for it cannot be fuppozed to attract the fluid at one time, and then let it go at another; this would be abfurd, and contrary to experience. We muft therefore try to find a better reafon why metal is poffeffed

with the property of receiving this fluid more readily than other substances.

EXP. XL. When the great cylinder, with the wire and brass drums, were charged with a very small quantity of this fluid, by the wheel being turned something less than a quarter round, there was, the moment after, a visible explosion, and a sensible effect perceived at the remote end of the wire. When half a turn was given, these effects were greater; and, after a whole turn, the quantity of the fluid accumulated in this great apparatus was increased considerably.

SIXTEENTH OBSERVATION. Now, something must have hindered the fluid from getting out of the cylinder and wire all the time they were charging, otherwise we should not have been able to have caused the least accumulation; for, from the nature of this fluid, there cannot be any accumulation without some resistance to occasion it. And whatever the nature of that resistance may be, experiments shew, that there are certain bounds prescribed to its power of acting, and which in particular circumstances seem to be very easily surmounted.

From Sir ISAAC NEWTON's observations<sup>(c)</sup>, and a great variety of experiments made since his time, we collect, that this principle of resistance is probably exerted at, or

(c) NEWTON's Optics, p. 240, 241, and 372.

very near, the surfaces of bodies, and extends only to very small distances from them.

It seems then, that it is by this kind of resistance at the surface of bodies, that the fluid is prevented from escaping out of the great cylinder and wire, whilst it is accumulating within them: and therefore, when we begin to charge the great apparatus at the nearer end, the moment any part of the charge arrives at the farther end of the wire, it is prevented (in some degree at least) from escaping, in consequence of the resistance it meets with at that end. And, if we continue to make the charge greater, the charge itself, during its increase, must also resist every farther effort which any ways tends to make it greater, with a force probably proportional to the quantity accumulated.

EXP. XLI. When the great cylinder and wire with the drums were fully charged, and a person, standing upon the wire which communicated with the well, suddenly approached the brass drums with his hand, an explosion ensued, which indeed was neither so large, nor did it take place at so great a distance, as might have been expected<sup>(d)</sup>; nevertheless, the person received a violent sen-

(d) The circumstances attending the explosion in this experiment were certainly owing to the long wire being not entire, but consisting of several pieces twisted together, the ends of which, being very many, must have caused a considerable part of the fluid to escape, and so have weakened the general effect.



fation, not unlike that produced by the Leyden phial, as it affected his body quite through, from the hand that took the discharge to the feet that stood upon the wire.

EXP. XLII. Upon repeating the above experiment, with the great cylinder only, and when it was fully charged, the explosion appeared stronger, and the distance it struck at greater, than in the other case; and yet the sensation received was not near so violent as when the long wire was connected with it.

EXP. XLIII. When the little cylinder by itself was fully charged, the effects were very inconsiderable, compared with those from the great cylinder: for, in this case, the person standing upon the wire of communication was affected in his hand only, and that no farther than the wrist. These three experiments were repeated many times by different persons, and the results were nearly the same.

SEVENTEENTH OBSERVATION. When all the circumstances in the two last experiments are considered, we may safely conclude, that the difference in the sensation, produced by the two cylinders, could arise from no other cause than a difference in their lengths; the one being fourteen times longer than the other, and both in other respects nearly similar; and since the sensation perceived  
in

in the thirty-eighth experiment, where the long wire was employed, was considerably greater than when the great cylinder alone was charged, we seem to have sufficient reason to apprehend that the effects of every charge, as to sensation, will be proportional to the length of the body charged; provided the charge (or accumulation) be uniform from end to end in every experiment.

Apprehending that, if some of the circumstances employed in producing the charge were varied, we might possibly obtain a greater charge than we had yet found, I made the following experiments.

EXP. XLIV. Instead of one machine to charge the great apparatus I made use of two. The glass cylinders belonging to each were of the same length and diameter nearly. One of those machines was continued in its usual place, which was not far from the nearer end of the great cylinder. The other stood at the farther end of the brass drums. After connecting the long wire with the great cylinder and brass drums, in the manner before described, the wheels of both machines were put into motion, with equal and uniform velocities: and after six turns of each wheel (for I could not prevail upon any one present at the time to take a higher charge), and, after waiting above eight seconds, a person suddenly

ently approached the brass drums with his hand; immediately an explosion took place, and a disagreeable sensation was perceived. The discharge was then made at the nearer end of the great cylinder, and there seemed to be no difference in the effect.

EXP. XLV. Upon repeating the experiment with one machine only, and after the same number of turns of the wheel with the same velocity, and after waiting above eight seconds also, the same person suddenly caused an explosion with the same hand. But the sensation in consequence of it was very different from the last experiment; for he declared it was little more (if that) than half as violent. These last experiments were also repeated several times by another person, who gave the same account of the results<sup>(e)</sup>.

EXP. XLVI. I now charged the long wire only and fully, and with one machine: the explosion, in this case, appeared not very large, but of a reddish hue; and the distance it struck at was not more than one inch and an half; however, the sensation across the body was at that instant

(e) On re-considering these two last experiments I find, that instead of giving six turns to the wheel, in the last experiment, I ought to have given twelve, in order to make any comparison between the effects of the two experiments: I therefore simply relate the experiments as they were made, without making any deductions from them.

sharp (as it was expressed) and violent, but not quite so disagreeable as when the great cylinder was connected with it, and similarly charged.

EXP. XLVII. Having procured an equal quantity of the same kind of wire, and of the same diameter, with that which was suspended and tried in the last experiment, it was placed in the form of coils upon a board, fixed on the top of a long stand of glass, without having any connection whatsoever with the great apparatus, or any part of it. These coils were then fully charged by the power of one of those machines only. The sensation they afforded, in consequence of causing sparks, was very inconsiderable, compared with what had been observed in the last experiment.

EXP. XLVIII. The several coils of wire employed in the last experiment, as likewise seven hundred yards more in coils also, were joined together at their several ends. These coils being then strung upon silk lines, were drawn out into a form resembling that of a screw, and separated from each other in such a manner all along as to occupy one hundred yards of silk line. The several diameters of these coils, at a mean, were about fifteen inches. As I had so short a time in which to prepare and suspend them properly, the disadvantage of their touching and intersecting each other in many places

could not be prevented; however, I found that the sensation caused, after charging this wire, was nearly equal to that which had been experienced from the long wire in the forty-sixth experiment.

EXP. XLIX. Upon joining the farther end of these coils to one end of the long wire, so that the whole length was in this experiment about three thousand nine hundred yards, and afterwards charging the nearer end of the coils, and without the great cylinder (it being at that time taken down) the sensation complained of, by two indifferent persons, was twice as violent as the sensation perceived by the same persons when the long wire alone was charged.

It may be now proper to make some general observations respecting the *explosion* itself, and the quantity of the fluid discharged in consequence thereof.

After many experiments we found, that when the great apparatus was fully charged, and the motion of the wheel suddenly stopped, it appeared, that a single explosion at either end of it, instantly (as to *sense*) discharged the fluid contained therein; but never so effectually as to leave no remainder: for the quantity which did remain (upon a second application immediately afterwards) was generally sufficient to cause a second explosion

sion perceptible to the sense of feeling, as well as to that of sight.

Now, before the great explosion was caused, the fluid accumulated in the apparatus must have been diffused equally through it, in consequence of its elastic principle; and, being so circumstanced, a sudden application of the hand, or any other substance, which would open a door for the passage of the fluid into the earth, was found to discharge the greater part of that fluid: and whatever part thereof was so discharged, the most distant particles seemed to have arrived at the point where the explosion took place, at the same time with those that were the nearest to it; because, immediately after the explosion, there was very little of the fluid remaining in the apparatus.

If then the discharge of the fluid be, as it seems to be, very nearly instantaneous, the particles of it must move with velocities, and consequently with forces, very nearly proportional to those distances.

From this consideration I apprehend it will appear, why the sensation upon the discharge from the long wire in the forty-sixth experiment was more violent: and upon recollecting the thirty-seventh, thirty-eighth, and thirty-ninth experiments, and the observations I made upon them, I am inclined to believe, that the effect

depends more upon the length of the metallic body, than upon the quantity of its matter or surface.

It was upon the idea of the velocity of the fluid being thus increased, that I apprehended gun-powder might be fired without the least appearance of a spark. The success of this experiment was an inducement to try KUNKELL's phosphorus, which was made by Dr. HIGGINS. The moment this inflammable substance was brought very near the surface of the brass drums, it burst into a blaze; and common tinder, applied in the same manner, was set on fire the instant when it was brought so near as to touch the metal: but there was not the least appearance of a spark in any of these experiments.

The method taken to fire the gun-powder was this: upon a staff of baked wood a stem of brass was fixed, which terminated in an iron point at the top. This point was put into the end of a small tube of Indian paper, made somewhat in the form of a cartridge, about one inch and a quarter long, and about two-tenths of an inch in diameter. When this cartridge was filled with common gun-powder (unbruised) the wire of communication with the well was then fastened to the bottom of the brass stem. Being so circumstanced, and whilst the charge in the great cylinder and wire was continually kept up by the motion of the wheel, the top of the cartridge was brought  
so

so near to the drums as frequently to touch the metal. In this situation, a small faint luminous stream was observed between the top of the cartridge and the metal drum..

Sometimes this stream would set fire to the gun-powder at the instant of the application; at others, it would require half a minute or more before it took effect. But this difference in time might probably arise from some difference in the circumstances, for any the least moisture in the silk lines, the powder, or in the paper itself, was unfavourable to the experiment.

This new method of firing gun-powder by a luminous stream of the matter of lightning surely merits the most serious attention; and more especially in those cases where pointed conductors are fixed to secure magazines of gun-powder from such accidents.

In repeating the last experiment, there were one or two instances where the powder was fired without making use of the long wire; but never with the great cylinder alone: for we were obliged to connect it with the brass drums by means of a wire ten or twelve feet long.

It was, however, a considerable time (ten minutes or more) before the experiment succeeded even with this last apparatus. This was not the case with tinder; for it  
fired



fired pretty readily, and sometimes with the great cylinder alone.

EXP. L. Before the apparatus was taken down in the Pantheon, attempts were made several days to fire gun-powder, but without success, except in one instance, which was attended with some difficulty. This failure seemed to be owing to a variety of causes, the chief of which appeared to be moisture that affected the silk lines, and perhaps the powder itself. I might add also the dust, which in so long a time had settled upon the lines, and rendered them in some degree incapable of resisting the passage of the fluid. However, when the coiled wires upon the silk lines were properly joined to the long wire, as in the forty-ninth experiment, it was found that gun-powder could be fired very readily. This is another proof of the increase of velocity by increasing the length of the wire.

The explosion of gun-powder, in the particular manner related above, without the assistance of the Leyden charge, and even without a spark, is an effect which could not easily be deduced from reasoning upon any experiments hitherto made; for though gun-powder has been frequently fired with the Leyden charge, yet there is a difference in the two cases, which makes it necessary to take some farther notice of that experiment.

When

When the Leyden phial is charged we find, by experiment, that the charge is confined in a small compass, or very much condensed, near one surface of the glass. It is also true, that the opposite surface of the same glass is as much rarified, or (according to Dr. FRANKLIN, who was the first that observed it) in a *minus* state. Now, according to this rule, the greater the surface of the glass is, and of an even and limited thickness, the greater charge may be given; and the greater the charge, the greater must be the effect (whenever the discharge is properly made) to restore the natural state of the glass on the two opposite sides. In order therefore to fire gun-powder with this kind of apparatus, experience hath taught us hitherto, that the powder should be confined, for example, in a tube or cartridge; and that this cartridge should be placed in such a manner as to make part of the circuit necessary for the discharge of the fluid from one surface of the glass, and carry it through the cartridge to the other surface. But it is also necessary, before the powder can be fired, to have part of the wire which forms the circuit in contact with the powder; or, to have one end of the wire, which makes part of the circuit, forced a little way into one end of the cartridge; and another end of a wire, which also makes part of the circuit, forced a little way into the other end of the cartridge; but so as that the

two ends of the wire within the cartridge may be at a proper distance from each other; which distance will depend upon the strength of the charge. When every thing is thus adjusted, and the circuit properly made, the gun-powder is generally fired; I say generally, because it sometimes happens, that the charge is not great enough to produce the effect required. For this effect does not appear to proceed from the spark or the explosion produced by the fluid (because flame of a certain density will not fire gun-powder); but it must be from the ends of the two wires, or from one of them at least, within the cartridge, being rendered hot enough to fire the gun-powder, in consequence of the very great quantity of the fluid, and the velocity with which it passed through the wire at that moment: it being well known that wire, even of a considerable thickness, has been frequently made red-hot, and even melted, by the Leyden charge.

Dr. LIND, a gentleman who is well acquainted with this subject, favoured me with a sight of a very curious experiment, which seems to shew more clearly what has been advanced above, respecting the immediate cause of the gun-powder's taking fire in the Leyden experiment. He procured for the purpose some extraordinary fine threads or shavings of steel; one of which was so disposed, where gun-powder was properly lodged and confined, as

to

to be in contact with the powder. After having charged a small phial, which did not exceed a pint in measure, with only a few turns of the wheel, he made the discharge, and fired the gun-powder. The advantage he gained by having so small a thread of metal in this experiment was, that it could be made red-hot with a less charge than what is necessary when a thicker wire is made use of; and the smallness of the charge he employed to fire the powder was a certain proof that the steel thread had been made red-hot.

Upon the whole we find, that this method of firing gun-powder is totally different from the other, where the great apparatus was employed; because the faint luminous stream, observed at the brass drums where the gun-powder was applied, I found was absolutely incapable of making the point of metal within the cartridge red-hot. Besides, there did not appear to be any explosion whatsoever between the apparatus and the powder at the instant it was fired.

Before I conclude this paper, it may not be amiss if I take notice of certain atmospheres, which bodies have round them when they are properly charged with this elastic fluid; because the nature of them may not perhaps be perfectly understood by every one who attends to enquiries of this kind.

Every charge, from the nature of the fluid which produces it, must, while such charge remains, continually act upon the air surrounding it; and of consequence upon the fluid also, which stands diffused therein, and in the intervals between its parts by its repulsive principle. This appears to be true, not only from theory but experiment.

The state of the fluid in the air nearest to the cylinder that is charged, must therefore be in an opposite state to that which is in the cylinder; that is, in the one case it will be condensed, and in the other rarified.

Now, because the power arising from the charge within the cylinder, which caused the rarefaction beyond its outward surface, is limited; the distance to which that rarefaction extends, must be limited also: and beyond that distance the fluid must be condensed more or less, according to the power of the charge which caused the rarefaction.

Whenever therefore we are disposed to open a door to let the charge out of the cylinder, the rarified fluid near the outside of the cylinder must necessarily promote the discharge at that instant.

As to the distance to which this rarefaction extends, it will always depend upon the strength of the charge, and  
the

the state of the air in the place where the experiment is made.

When I suspended the 1600 yards of wire in the Pantheon, the several lengths thereof were purposely hung (as hath been observed before) at five or six feet distance from each other, entirely upon this idea; lest the respective atmospheres might interfere with each other, or with the charge contained in the wire, and by that means disturb the experiment.

It is now time to put an end to this inquiry; in which, however defective I may have been in ability, neither attention nor impartiality have been wanting; and although it may be true that, even before these experiments were tried, I was inclined more to one side of the question than the other, it was because I many years ago grounded my persuasions upon the philosophy of Sir ISAAC NEWTON. Without those persuasions, and the advantage of that most gracious encouragement which is never wanting to honest and candid endeavours in the pursuit of philosophical truth, I should hardly have felt sufficient zeal to engage in so considerable an undertaking: nor indeed, without powerful assistance, could the great object of this inquiry have been in any degree attained. For my own part, I can boast of very little more in this investigation than patience and industry.

I shall only add, that if any one, who may be disposed to try the preceding experiments, will use an apparatus of sufficient dimensions, and faithfully attend to every circumstance as I have done, I have no doubt they will find the same results; but they must also apply their observation to much smaller circumstances than have been here specified, as the intervention of a single hair, a fibre of down, or even a little vapour arising from perspiration or otherwise, will, where great exactness is required, sometimes prevent the success of an experiment; and by that means mislead the observer, or afford a subterfuge for a mistaken hypothesis.

Great Ruffel-street, Bloomsbury,  
November 12, 1777.

*N. B.* The different distances at which the point and ball were struck being expressed in fractions, p. 278. I thought it would be better to put these distances in whole numbers, that the ratio between them may more clearly appear.

Sharp point.	Rounded end.
90	25
160	20
130	35
220	16
260	20
80	4

Forty of those parts are equal to one inch.

*Measure-*

*Measurements, &c. of the great Apparatus and Machinery.*

	Feet.	In.	lbs.
Length of the wire suspended, which consisted of many pieces that were connected by twisting the several ends together, - - -	4800		
Weight of the wire, - - -			30
Twenty-one pieces of this wire, laid parallel and close to each other, measured exactly, - - -		1	
Surface of the whole wire in square feet,	57		
Length of coiled wire, which was suspended also, but in a form somewhat resembling a screw and of the same thickness with the wire above,	6900		
Length of great cylinder, including the brass drums above, - -	155		
Diameter of this cylinder, - - -	1	4	
Weight of tin-foil, which covered 112 drums, - - -			87
Surface of 112 drums in square feet (including the six ends) measured,	592		

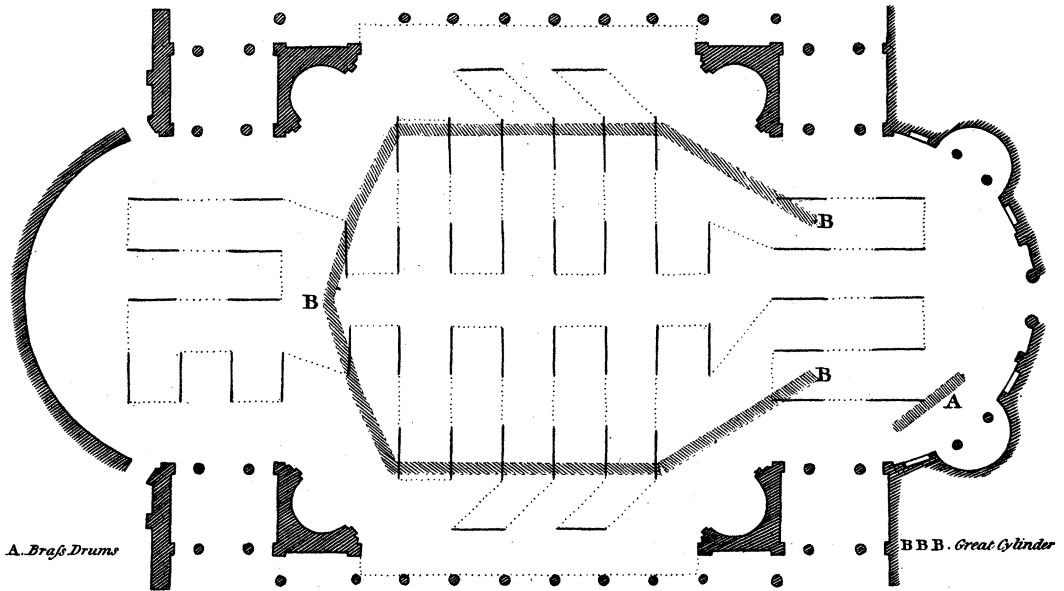
Space



	Feet.	In.	lbs.
Space through which the conductor } upon the model passed from its } beginning to move, to the instant } nearly of its being struck, - } Heighth of the frame from the floor, } Breadth of the frame, - - } Heighth of the two posts, on the top } of which were fixed two wheels, } Diameter of the great wheel, - } Diameter of the smaller wheel, fixed } upon the same axis, - } Length of frame on which the model } moved, - - - } Diameter of the pulley that was fixed } between the two posts, which deli- } vered the line from the model to } the greater wheel, the center of } which pulley was two inches and } an half above the center of resis- } tance in the model, - }	7	7	
	3	1½	
	1	3	
	10	6	
	1	11	
		11¾	
	18	2	
		5	
Greater weight to draw the model,			32
Smaller weight, - - -			4
Weight of the model, - - -			12

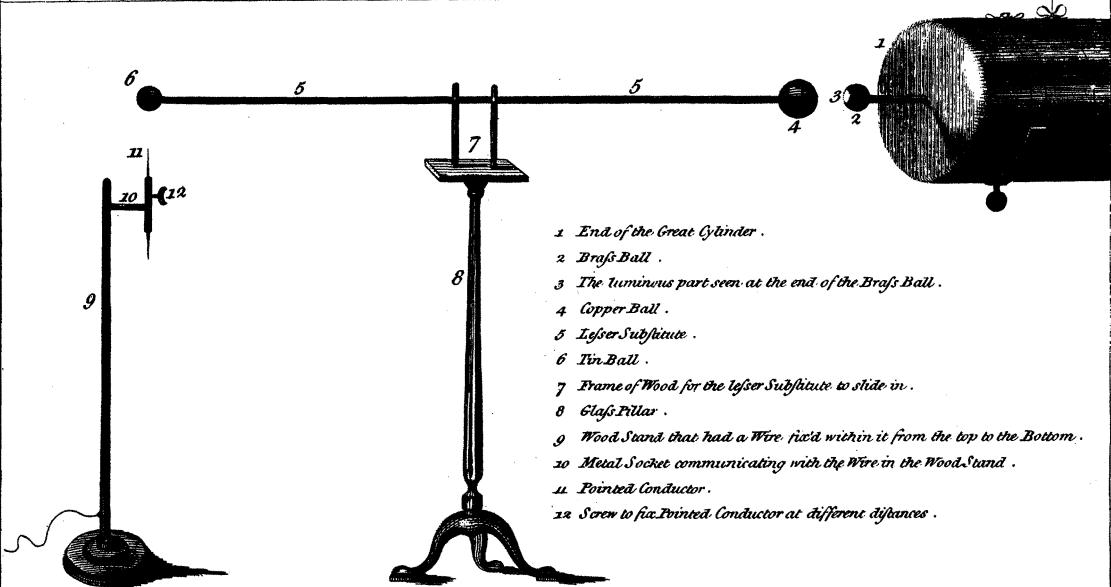
Upon

*Disposition of the long Wire, Brags Drums & Great Cylinder, as suspended in the Pantheon.*



*The dotted lines represent the lower, the black lines the upper horizontal parts of the Long Wire.*

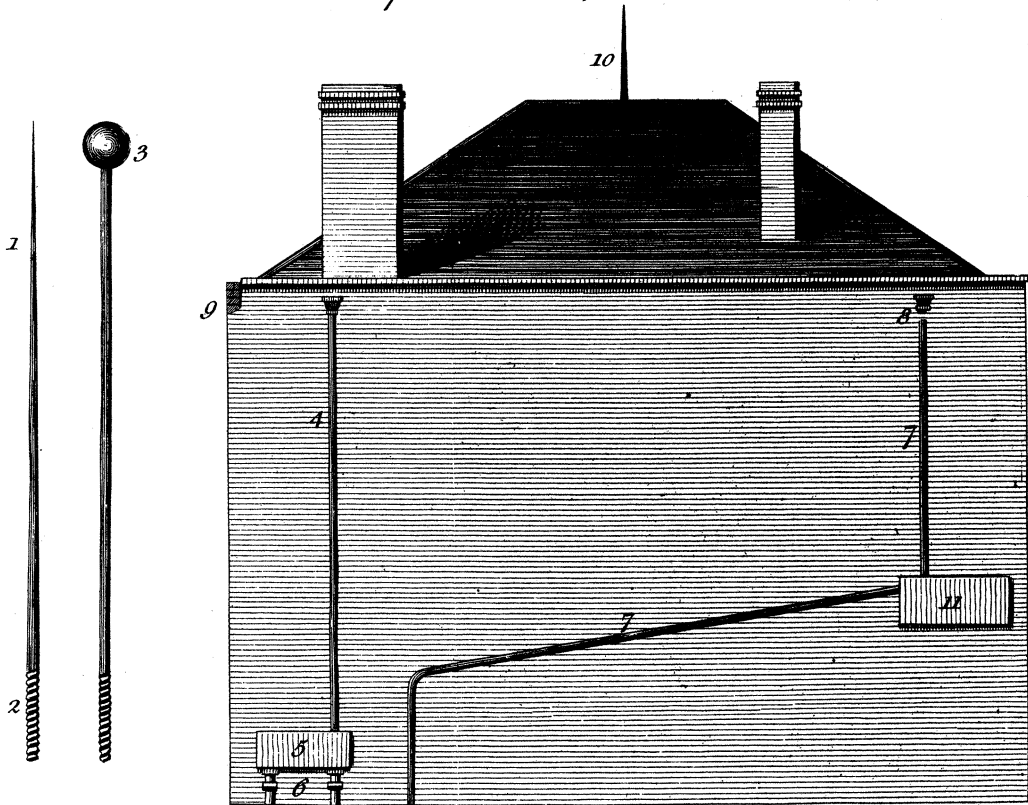
*The perpendicular parts could not be represented in this plate and must therefore be supplied by the Readers Imagination.*



- 1 End of the Great Cylinder.
- 2 Brags Ball.
- 3 The luminous part seen at the end of the Brags Ball.
- 4 Copper Ball.
- 5 Lighter Substitute.
- 6 Tin Ball.
- 7 Frame of Wood for the lighter Substitute to slide in.
- 8 Glass Pillar.
- 9 Wood Stand that had a Wire fixed within it from the top to the Bottom.
- 10 Metal Socket communicating with the Wire in the Wood Stand.
- 11 Pointed Conductor.
- 12 Screw to fix Pointed Conductor at different distances.

*Apparatus employed in the 34.<sup>th</sup> experiment.*

*The North Elevation of the House in which the Board of Ordnance meet call'd, for that reason, the Board House.*



1. Size of pointed Conductor as on the Model.
2. The part Inserted at the top of the Roof.
3. Size of rounded end, as on the Model.
4. Short Spout.
5. Cestern.
6. Two Posts.

7. Long Spout.
8. Little discontinuation of Metal not in the House but left in the Model for the purpose of making Experiments.
9. The Corner damag'd by Lightning.
10. Pointed Conductor. 11. Cestern.

		Feet.	In.	lbs.	
Upon a scale of one-third of an inch to a foot.	}	Length of the model, -	1	$9\frac{1}{4}$	
		Breadth, - -	1	$4\frac{1}{6}$	
		Height to the top of the parapet,	1	$1\frac{1}{12}$	
		Roof above the parapet, -		$4\frac{2}{3}$	
		Chimnies exceeded the highest } part of the roof, about }		$\frac{1}{6}$	
		Pointed conductor when fixed } upon the roof, - }		$3\frac{1}{3}$	
		Length of short spout, -		10	
		Length of long spout in its } bent state, - - }	1	$10\frac{3}{4}$	
		The two posts or pillars to sup- } port the cistern (which cif- } tern was lined with lead) }		$\frac{5}{8}$	

5. *A Report of the Committee, appointed by the Royal Society, to consider of the most effectual method of securing the Powder Magazines at Purfleet against the Effects of lightning; in compliance with the Request of the Board of Ordnance.*

IT being referred to us to consider, whether any thing more than what was formerly directed by the committee of the Royal Society in the year 1772 can be done,

done, for the preservation of His Majesty's magazines at Purfleet: we, having attentively examined the experiments and observations of Mr. WILSON, contained in a paper referred to the Society by the Board of Ordnance; and having maturely considered the subject at large, submit it as our opinion.

1. That it is very improbable, that the powder magazines, guarded in the manner in which they are at present, should receive any damage from lightning.

2. That they would be still less liable to be injured if three other elevated pointed rods, similar to those already erected, were to be fixed upon the roof of each of the five magazines, between the extreme rods, at equal distances from each other, with three strips of lead, about one foot in breadth, strongly connected with them, and carried down the roof, from the ridge to the eaves, on each side of the building; thence two of them to be continued into the earth, and to terminate at the bottom of wells; one of which should be dug for that purpose nearly in the middle of each of the intervals between the magazines, deep enough to contain at least four feet of water. The middle strip should be connected with the iron rod over the door, hereafter to be mentioned.

We also advise, that other high pointed rods be erected; one at each of the four corners, and one over each

each of the metal doors in the middle of the sides; which latter should be bent, so as to avoid the doors, in the same manner as those which are already placed upon the outward side of the outermost magazines. All which rods should be continued into the earth, and be made to communicate with the bottom of the water of the nearest wells, by means of leaden pipes, closely connected with these iron rods. Likewise, that strips of lead be put upon the coping of the end walls, and be made to communicate with the rods to be placed at the several corners as above directed.

3. But that the greatest degree of security would be attained by covering the whole roof, and the tops of the end walls of each of the five magazines, with lead; erecting all the additional conducting rods above directed; and forming a communication between the leaden covering of the roof, and the bottom of the wells as before mentioned.

As to the other buildings belonging to the magazines we recommend:

1. That a pointed rod, similar to the rest, be erected at each end of the proof-house, and be united with the lead already there: also, that the lead on the roofs of the two low buildings, destined for the reception of the empty powder casks, &c. be connected with the wells by means

of one strip of lead in the middle of each building, of the same breadth as those above mentioned.

2. That a pointed rod of copper, about three quarters of an inch in diameter, be erected on each of the four chimnies of the Board-house, reaching five feet above them; and be connected, by strips of lead, with the other lead upon the roof of the building.

We do also advise in general: 1. That the lead and rods, upon the several buildings, be respectively connected with the nearest wells, by the shortest metallic communication that can conveniently be formed; in particular, that the two leaden spouts of the Board-house, which do not reach the ground, but terminate in cisterns, be connected, by strips of lead, with those spouts that do already communicate with water. 2. That the different pieces of which the iron rods may be composed, be strongly screwed together in close joints, having a thin plate of lead between them, as directed by the first committee. 3. That these rods be firmly fixed, and closely connected with the lead upon the roofs. 4. That all the strips and pieces of lead be well fastened and foldered together, so as to make a perfect metallic communication with the bottom of the wells. 5. That the iron rods be painted, except in those places where they are to be in contact with the lead; that they be all ten feet high;

and that they be terminated with pieces of copper eighteen inches long, like those already erected: and, 6. that these copper terminations be very finely tapered, and as acutely pointed as possible.

We give these directions, being persuaded, that elevated rods are preferable to low conductors terminated in rounded ends, knobs, or balls of metal; and conceiving, that the experiments and reasons, made and alledged to the contrary by Mr. WILSON, are inconclusive.

March 12, 1778.

J. PRINGLE, P. R. S.

W. WATSON.

H. CAVENDISH.

W. HENLY.

S. HORSLEY.

T. LANE.

MAHON.

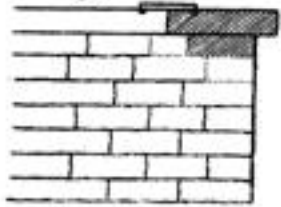
EDW<sup>D</sup>. NAIRNE.

JOSEPH PRIESTLEY.





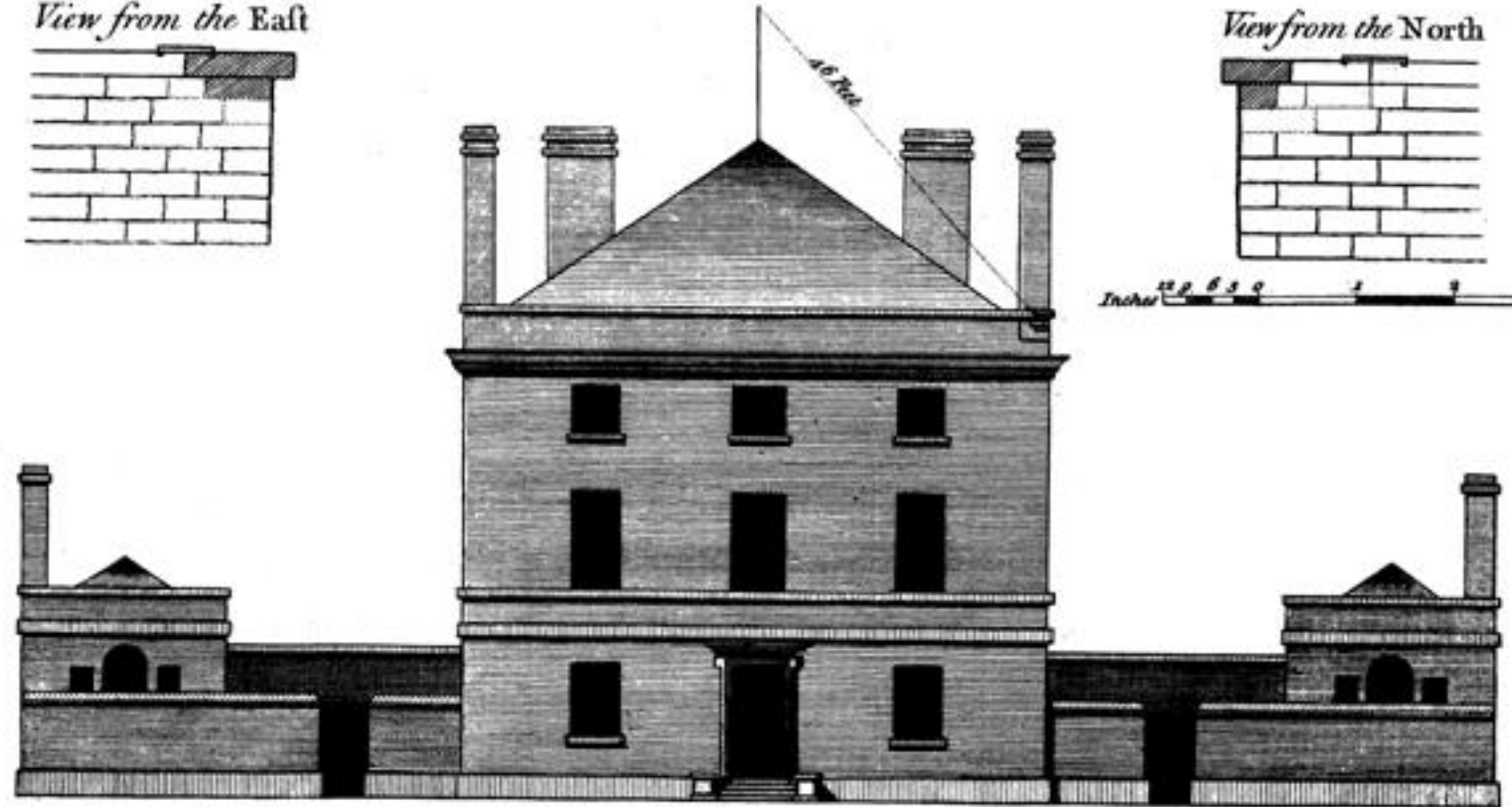
View from the East



View from the North

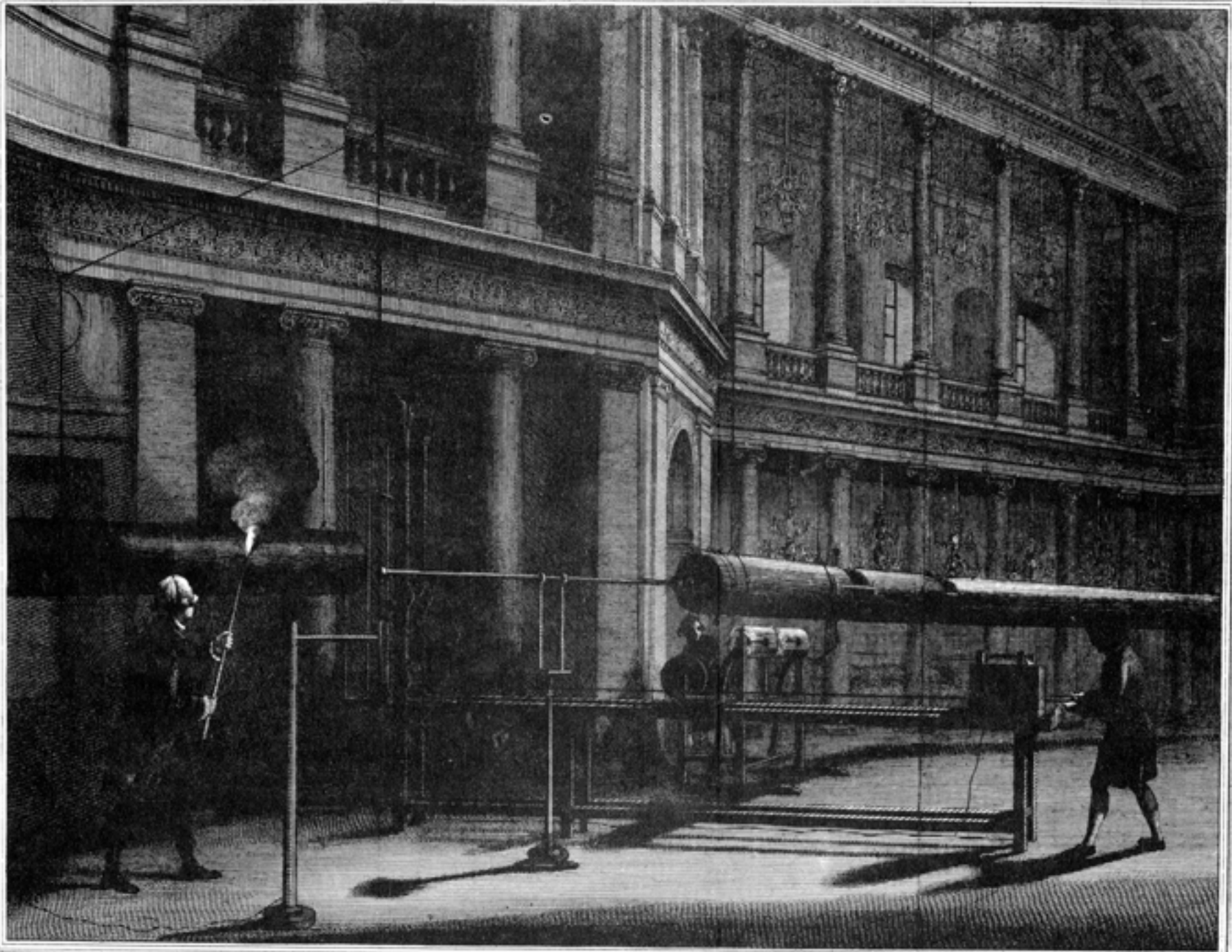


Inches 12 2 6 3 9 1 2 3 Feet



20 0 10 20 30 40 50 60 70 80 90 100 110 120 Feet

East Front of the Board house at Purfleet.



*A View of the Apparatus and part of the Great Cylinder in the Pantheon.*

*Designed by J. B. B. B.*