

XXXVI. *Reasons for dissenting from the Report of the Committee appointed to consider of Mr. Wilson's Experiments; including Remarks on some Experiments exhibited by Mr. Nairne. By Dr. Musgrave, F. R. S.*

Read June 25, 1778. I DO not find that Dr. FRANKLIN, in any of the passages where he speaks of the efficacy of sharp-pointed conductors to prevent electrical explosions, has expressed any doubt of their being universally preferable for this purpose to those which have a blunt or spherical termination. The same observation may be made of the other gentlemen who are the advocates for his doctrine. It may therefore be assumed, that both he and they mean to assert an universal proposition, " That sharp points will, in all cases, draw off the electrical fluid silently within the distance at which rounded ends will explode; or, at least, that the former sort will in no case receive an explosion at a greater distance than the latter." I think it necessary to observe, that, though I dissent from this doctrine, I do not mean to assert the contrary universal proposition, but only to deny the universality of that asserted by Dr. FRANKLIN, which

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I apprehend to be sometimes true, and sometimes also false.

But before I attempt to specify the particular cases in which the sharp and the blunt terminations are respectively more liable to electrical explosions, it may be of use to shew (what many gentlemen seem not to be thoroughly aware of) that sharp points having the most perfect communication with the earth, are not wholly exempt from receiving them. My first authority shall be Dr. FRANKLIN himself. "Let a person," says he, p. 60. "standing on the floor, present the point of a needle at "twelve or more inches from it [the prime conductor], "and while the needle is so presented, the conductor "cannot be charged, the point drawing off the fire as "fast as it is thrown on by the electrical globe. Let it "be charged, and then present the point at the same distance, and it will *suddenly* be discharged." The word *suddenly* means, I suppose, that it will receive an explosion; that being the most natural and obvious proof of the *suddenness* of the discharge. The same thing is more directly asserted by Mr. HENLY, in vol. LXIV. of the Phil. Transf. p. 138. where he informs us, that in discharging three of his large jars, to the coating of which he had connected a wire nicely tapered to a point, the fire flew to the point, and the jars were discharged with a full and
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loud explosion. A third, and equally decisive, proof is furnished by Mr. NAIRNE's own experiments, though seemingly made with a contrary view. For when the double or interrupted conductor was used, and the second conductor fixed down by screws at about three inches distance from the first, the point presented to the contrary end of the second conductor was found to receive a strong and loud explosion, with a white light at the distance of at least three inches.

If we compare this experiment with another, very common one, exhibited at the same time by Mr. NAIRNE, the comparison will, perhaps, lead us to the discovery of a principle upon which electrical explosions very frequently depend. Though the point, in the circumstances above described, received so strong an explosion, yet when it was presented directly to the prime conductor, it received no explosion whatever at any distance, unless a succession of weak sparks, at the distance of about a quarter of an inch, can be called so. To what must this difference be attributed? Plainly to the different quantity of electric fluid accumulated on the prime conductor in the one and the other case. Where the point is presented to the prime conductor, from the time the machine begins to work, the property which is attributed to them; and which, in some cases, they really possess, of stealing

away the electricity silently; this property, I say, operating from the very beginning, prevents the electric fluid from being accumulated in the prime conductor, and of course the quantity of it will always be small. But when a double or interrupted conductor is used, the second conductor receives no electricity till the prime conductor is pretty highly charged, and, if put at the greatest striking distance, not till it is fully charged, and consequently the sharp point presented to the opposite end can carry away none of it till that time; when the whole quantity is thrown off at once. It should seem then, that the explosion in one case, and the non-explosion in the other, depended wholly upon the different quantities to be thrown off: whence it will follow, that though a small quantity of electricity will pass off silently upon a point, yet that this power is very limited; for that if a somewhat greater quantity be applied suddenly to a sharp point, it will not pass off silently, but create an explosion in proportion to its density.

The facts above related are a sufficient answer to that other experiment of Mr. NAIRNE's, in which he exhibited a sharp point, that when perfectly communicating with the earth drew off the electricity silently from the prime conductor; but received explosions freely, when the communication was broken by interposing little isthmuses of sealing wax. This experiment, it is true, demon-

demonstrates that a broken communication will occasion the sharp point to receive an explosion; and so far it must be owned to be conclusive. But if it was intended to suggest, that whenever sharp points do receive an explosion, it is owing to this circumstance; and, consequently, that Mr. WILSON's experiments at the Pantheon were unfairly made; in this view it has no weight, because we have already seen that an interrupted communication is not the only circumstance that will produce an explosion, for that increasing the quantity of electricity will have the same effect.

I cannot omit the opportunity here offered me, of remarking the unfairness of the insinuations that have been thrown out to the prejudice of Mr. WILSON. Had there been any juggle in making his experiments, it would certainly have been detected by the committee appointed to examine them. And in case of such a detection, it was the duty of the committee to lay open the imposture both to the Society and the Public. Instead of which, instead of disputing or even doubting the fairness of them, they have in a manner admitted it, by only saying in their report, that they appear to be inconclusive. This, I say, is admitting the facts to be fairly stated: unless we could suppose their regard for Mr. WILSON, and tenderness for his reputation, had induced

them, after detecting the fallacy of his experiments, to pass it over in silence; of which improper partiality I do not know that they are so much as suspected. What therefore the committee, after a strict scrutiny of the matter, did not think themselves warranted to say, I take for granted they would not insinuate; and that therefore such insinuations can only arise from the levity of more obscure persons, puzzled perhaps by the seeming contradiction between Mr. WILSON's experiments and those of Mr. NAIRNE, and too impatient to investigate the real causes of that difference.

I am persuaded, however, that the known property of sharp points to carry off electricity silently, when the quantity is small, together with that other principle, which I apprehend I have here established, that they cease to do so when the quantity is large; that these two (taken together) will clear up the whole difficulty, and account for Mr. NAIRNE's experiments, without any impeachment to those of Mr. WILSON. I have already had occasion, in the course of this argument, to consider two of those experiments, of which therefore I shall say no more; but proceed, without further digression, to examine those that remain.

The first I shall mention is that, in which the prime conductor, being previously charged with electricity, a
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sharp point is presented to it within the attracting, and without the exploding distance, and then brought slowly on towards it. In this case no explosion follows; neither is there any reason to expect it should, because the quantity of electricity is gradually diminished by the approach of the point, so that when it comes within the striking distance there is not enough left to make an explosion.

It is equally easy to explain what happens when a transverse arm is hung so as to oscillate freely upon the prime conductor, and two equal cylinders of tin-foil are suspended, one at each end of this arm in perfect equilibrium with each other. The apparatus being in this state, if the machine be worked, the two cylinders will remain stationary, neither of them ascending or descending. They will also remain stationary, if a point be presented to one, and a rounded end to the other. In the first case they are electrified, but remain motionless, because there is no conducting body within the sphere of their action. In the second, the result is the same; because, making in fact part of the prime conductor, the point presented to one of them prevents any accumulation of electricity. When the point is withdrawn, and the rounded end suffered to remain, an accumulation takes place, because there is nothing now to steal it away; and the consequence is, that the cylinder descends towards the rounded end, and
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explodes as soon as that accumulation arrives at a certain period.

Thirdly, when an intermediate conductor is used, terminated at each end with a ball, and the middle of it resting in equilibrio upon a pivot, on which it has a free oscillation upwards and downwards; if in this state a point is placed under the end most distant from the prime conductor, the machine being then worked, the other end will approach so near the prime conductor, as that the stream of electricity will flow freely into it, as fast as it is produced by the action of the wheel. In this case there will be no explosion; and the reason is obvious, because the second conductor, when it approaches so near the first as to form an uninterrupted channel for the electric stream, becomes virtually a part of the first. Hence the point operates upon both together, just as it does when presented directly to the prime conductor, that is, it steals away the electricity by little and little, leaving not enough to give an explosion. When instead of the point a polished ball is placed under the same end as before, this being less disposed to receive the electric fluid, conveys away none of it; so that accumulating to a certain degree upon the prime conductor, it explodes upon the contiguous end of the second, which, having a free oscillation, flies up with the stroke, and carries the
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opposite end towards the ball, where, being saturated, it gives a snap; the recoil of which snap throws that end up, and the contrary end back towards the conductor, and so on alternately, as long as the machine continues working.

The event, however, is widely different when the second conductor, instead of having a free oscillation, is screwed down in one place, and at such a distance from the prime conductor, as not to receive the electric fluid till considerably accumulated. For then the sharp point, previously opposed to its other end, discharges it, as was before observed, not in a continued stream and silently, but at intervals, and with a strong explosion.

The last of Mr. NAIRNE's experiments, and the only one yet unconsidered, is that of the sharp point, which, being fixed to a kind of inverted pendulum, oscillated with great velocity under the prime conductor, without receiving any explosion. Now from this experiment I do not comprehend how any general conclusion can possibly be drawn. It has been already shewn, from the acknowledgement of Dr. FRANKLIN, and the experiments of Mr. HENLY and Mr. NAIRNE, that electricity, accumulated to a certain degree, will explode upon a point. If, therefore, in any particular instance it does not explode, what can we infer from it, but that the accumulation in

every such instance was not sufficiently great; which may happen either from the smallness of the apparatus, or from want of care in making the experiment.

And now if we look back upon Mr. NAIRNE's experiments (which, by the by, have not all of them the merit of novelty) we shall find them to be nothing more than different exemplifications of this well-known principle, that sharp points giving less resistance to the ingress of the electric fluid will draw it off at a greater distance than blunt or spherical terminations, and where the quantity is small will draw it off silently. This, I say, is the whole amount of his experiments; the only one of them in which the electric fluid had time to accumulate, being attended with a different event from the rest, and producing, as might reasonably be expected, a strong explosion.

It is not, however, this single property of sharp-pointed conductors, which must decide the question. We have already seen, that there are two properties inseparable from them, both of which must be taken into the account, before we can determine the propriety of affixing them to buildings, particularly powder magazines, as preservatives from lightning: first, their greater propensity to admit the electric fluid, in consequence of which they act upon electrified bodies at a greater distance than
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rounded ends will; and, secondly, their incapacity to draw away more than a certain quantity of electricity without an explosion.

The first quality enables them, when electricity is accumulated gradually, or when they are brought gradually towards the electrified body, to steal away the fluid by little and little, till there is not enough left to give an explosion. And hence, in common experiments, the point, placed at a greater distance than the ball, will prevent the electricity from exploding, as it otherwise would do, upon the latter. But if we combine this quality with the second, the superior propensity to admit, with the incapacity in certain circumstances of discharging silently, it will be evident, *à priori*, that the phenomena must in such cases be reversed, just as they appear to be in Mr. WILSON's experiments; that the point must strike at a greater distance, and the rounded end at a lesser.

What puts this matter beyond a doubt is, that when the double or interrupted conductor is used, the experiment may be so managed, as that the ball shall receive an explosion at a greater distance than the point, or the point at a greater distance than the ball, at the pleasure of the operator. If care be taken, at the beginning of the experiment, to set the second conductor at the greatest

distance from the first, compatible with its giving a full and smart explosion, the point in that case will receive the explosion at a much greater distance than the ball. I was myself once present at an experiment, when the difference was as 6 to 1, that is, the ball would receive no explosion at a greater distance than $\frac{7}{8}$ ths of an inch, when the point received it at six times that distance. On the contrary, if the second conductor be put considerably within the distance above described, the ball will receive an explosion much farther off than the point. Upon repeating both these experiments lately with a small machine, I found the result to be as follows. When the distance between the first and second conductor was $1\frac{5}{8}$ th of an inch, the point was struck at $2\frac{1}{16}$ th of an inch; but a ball of $1\frac{5}{8}$ ths of an inch in diameter would not take the stroke at more than one inch. But when the distance between the conductors was only $\frac{5}{8}$ ths of an inch, the point could not be struck at more than $\frac{7}{8}$ ths, whereas a ball of the same diameter as before was struck at 7 inches and $\frac{3}{4}$, and a lesser ball $\frac{3}{16}$ ths of an inch in diameter at 6 inches and $\frac{3}{16}$ ths. I have been told also, but have not yet had time to verify it, that a medium distance may be found, at which if the second conductor be set, the point and ball presented to the other end will be exactly upon a par with respect to the exploding distance.

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These phenomena, to persons who have not carefully considered them, must appear so extraordinary, that unless the cause of the diversity is explained, they will perhaps be led to suspect some unfairness in making the experiment. The truth, however, is this; that when the two conductors are set at the greater of the two distances, the absolute quantity of electricity collected before the explosion is exactly the same in each experiment; and therefore the distances of the ball and point from the second conductor being equal, and the greatest at which either of them will be struck, the explosion will go to the point, as being more susceptible, and giving less resistance than the ball. But in the second supposed case, when the second conductor is set considerably within the former distance, the quantity of electricity which explodes upon the point and the ball is not the same; the point in this case exerting its known property of stealing away the electricity silently, which the ball from its greater resistance is incapable of doing. The consequence is, that the quantity accumulated to give an explosion upon the ball is greater than that which explodes upon the point, and being greater will very naturally explode to a greater distance.

I might safely have rested the matter upon this ground; but another proof, equally decisive, having since
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occurred, it is but doing justice to my argument to insert it here. In the experiments made with a view to settle this dispute, and published by Mr. HENLY about four years ago, there is one, the fifth of that set, which it is difficult to reconcile with the doctrine here laid down, that electricity strongly accumulated, and moving with great velocity, will explode upon a sharp point rather than a ball.

He describes it thus: " Having insulated the jar, and
 " connected *by chains* with the external coating, on one
 " side a knob, and on the other side a sharp-pointed wire,
 " both being insulated and standing five inches from
 " each other, I placed a large copper ball, eight inches
 " in diameter (insulated also) so as to stand exactly at
 " half an inch distance both from the knob and the
 " point. The jar being fully charged, I delivered it upon
 " the copper ball by my discharging rod, whence it
 " leaped to the knob, which was three quarters of an
 " inch in diameter, and the jar was discharged by a loud
 " and full explosion, and the chain was very luminous."
 Phil. Transf. vol. LXIV. p. 136.

It must be obvious to any careful electrician, and Mr. WILSON had in his answer observed, that an experiment thus loosely and unphilosophically made, was not greatly to be relied upon; because two chains being made use of,

one to connect the ball, and another to connect the sharp point with the coating of the phial, a different number of links, or different degree of tightness in the two chains, would produce a difference in the result: for it being a known property of electricity to pass most readily where it has the fewest and the smallest intervals to leap over, the explosion would naturally pass that way, where the links were drawn tightest; or if both chains were left loose, as from the plate they appear to have been, then where they were fewest in number, instead of being determined by the circumstances of bluntness and sharpness.

It was however possible, that Mr. HENLY might be right in attributing it to the sharpness of the point; and therefore, in order to settle this doubt, I desired that the experiment might be tried over again in somewhat different circumstances. Mr. CAVALLO accordingly tried it in the following manner. Upon an insulated stand he placed a ball, about $\frac{6}{10}$ ths of an inch in diameter, and a sharp point, directly opposite to the place where a Leyden phial, when charged, was to be set down. Both of these, the ball and point, were connected to his discharging rod by copper wire. He then took the phial, which held about a quart, and having charged it, set it down before the ball and point, took his discharging rod, and
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completed the circuit. After the explosion the distances were measured. In the experiments which I saw the phenomena were as follows: in the first, the distance of the ball was $\frac{1.9}{2.0}$ of an inch, that of the point $\frac{2.6}{2.0}$; in the second, distance of the ball $\frac{1.6}{2.0}$, of the point $\frac{2.7}{2.0}$; in the third, distance of the ball $\frac{1.5}{2.0}$, of the point $\frac{2.6}{2.0}$; in the fourth, distance of the ball $\frac{1.8}{2.0}$, of the point $\frac{2.9}{2.0}$. In all these experiments the point, though considerably farther off from the coating of the phial, was struck in preference to the ball. In a fifth experiment, when the point stood at double the distance, the ball was struck, and not the point.

The result of these experiments being so widely different from the result of that made by Mr. HENLY, is a clear proof that he formed his conclusions too hastily, having attributed to sharpness and bluntness a phenomenon caused by the unequal resistances of the chains. As this experiment may be made with almost any machine, those who do not chuse to repeat it will have no right to plead the want of a sufficient apparatus, and must look out for some other reason to evade the force of it.

I come now to consider more particularly the practical question, whether the sharp-pointed or the blunt conductors are most proper to be affixed to buildings, as
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preservatives from lightning. And here it is necessary to observe, that buildings may be exposed to a stroke of lightning in several different ways. The lightning which, to avoid prolixity, I shall only speak of as positive electricity: the lightning, I say, may accumulate directly over the building; or it may be brought towards the building by a small cloud fetching it in several successive trips from a large cloud at some distance; or a large electrified cloud may be carried rapidly towards it by the wind: a circumstance this by no means rare, there being no less than four instances of it upon record in the *Phil. Trans.* vol. XLIX. p. 16. and p. 309. vol. LXI. p. 72. and vol. LXIV. p. 351. In the first of these supposed cases a sharp-pointed conductor might possibly drain the cloud of its lightning as fast as it began to accumulate, and thereby prevent any explosion whatever. In the second, as the cloud, by supposition, not being driven in one direction by the wind, could not move with any remarkable velocity, it is reasonable to imagine, that in this case also there might be no explosion; and that the electricity of the larger cloud might be gradually exhausted. But if, according to the third supposition, a cloud of great extent and highly electrified should be driven with great velocity in such a direction, so as to pass directly over the sharp-pointed conductor, there can

be no doubt but that such a point, from its superior readiness to admit electricity, would take the explosion at a much greater distance than a rounded end, and in proportion to the difference of that striking distance would do mischief instead of good.

But perhaps it will be said, that every stroke of lightning falling upon a sharp point is previously diminished by that point, and therefore may more easily be transmitted through the conductor, than when it falls undiminished upon a rounded end. Upon this supposition I must observe, that it not only contradicts Mr. WILSON's experiments at the Pantheon, but also Mr. HENLY's experiment already referred to in this paper, where the fire flew to a very taper point, and melted the end with a strong and loud explosion. So also the sharp-pointed conductors affixed in America to the houses of Mr. WEST, Mr. RAVEN, and Mr. MAYNE, do not *seem* to have diminished the force of the explosion, if we may judge from the violence of its effects as related at large in Dr. FRANKLIN's works. It should seem, therefore, that the power of diminishing a stroke, like that of preventing it, is only contingent, and depends, as we said before, upon the degree of velocity with which the lightning moves.

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The sum of the whole is, that conductors, terminated by sharp points, are sometimes advantageous, and at other times prejudicial. Now as the purpose for which conductors are fixed upon buildings is, not to protect them from one particular sort of clouds only, but, if possible, from all, it cannot surely be adviseable to use that kind of conductors, which if they diminish danger on one hand, will increase it on the other. It is the duty of a pilot to keep out of the way of rocks; but it is also incumbent upon him, in avoiding the rock, not to take so large a compass as to run his ship upon a quicksand.

When I say that sharp-pointed conductors may in some cases diminish danger, I speak of them, perhaps, rather too favourably: for their power of stealing away the electric fluid being confined to cases where the accumulation is small, it follows, that they only operate where their operation is not wanted. The cases against which we wish principally to provide, are the explosions of extensive and highly electrified clouds; and here we have seen, that blunted ends, as acting to a much smaller distance, are entitled to the preference.

If it be admitted, that sharp-pointed conductors are attended with any, the slightest degree of danger, how much must that danger be augmented by carrying them

high up into the air, by fixing them upon every angle of a building, and making them project in every direction? Ought this to be advised while there is still a doubt of the possibility of their doing mischief? And can the committee, therefore, be perfectly justified for giving such a decided preference to the use of sharp conductors, in defiance of numerous experiments, not one of which they have attempted to controvert?

I have now done with the report of the committee, and shall next proceed to enquire whether, as some gentlemen apprehend, the termination of conductors is a matter of indifference: but this I must reserve for the subject of a future paper.

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I think it necessary to apprise the reader, that the foregoing remarks were drawn up before Mr. NAIRNE'S paper appeared, and are therefore to be considered as relating only to his experiments.

I must at the same time observe, that, in stating the different circumstances in which lightning may be collected so as to affect a building, I have supposed a case which possibly may never exist. I have said, that lightning may accumulate directly over a building, and admitted, that in that case the cloud might be drained of it by a pointed conductor. Now we have no evidence, that the accumulation of lightning is confined to a single cloud, or small circumscribed spot in the heavens. On the contrary, the numerous explosions, which in most thunder storms happen nearly at one and the same instant, rather lead us to imagine that a great part of the horizon is at those times full of lightning, and therefore incapable of being drained. I would therefore wish to have my supposition understood as a mere imaginary supposition, for the sake of rendering the argument more perspicuous, and not as the admission of a real fact.

Lastly, I beg leave to correct an expression I have used with respect to pointed conductors, that they only operate where their operation is not wanted. Now this is not accurately true: for if by operating upon a quantity of electricity too small in itself to do mischief, they prevent its growing to a great and dangerous quantity, this would, as far as it goes, be a very considerable advantage.

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I ought therefore to have said only, that pointed conductors afford no protection where the danger is great and imminent, and only obviate that which is distant and problematical; and that these last are not the cases against which we principally wish to provide.

March 18, 1779.

