

Received February 22, 1770.

XVIII. *An Investigation of the lateral Explosion, and of the Electricity communicated to the electrical Circuit, in a Discharge: By Joseph Priestley, LL. D. F. R. S.*

Read March 29, 1770. SEVERAL years before I made any experiments in electricity, except with a view to amuse myself, and my friends; I had observed, that in discharging jars, and particularly such as were filled with water, without any coating on the out side, I felt a slight shock; though it was plain that the hand, in which I held the discharging rod, made no part of the circuit.

Mr. Wilson, also, in his first experiments on the Leyden phial, observed, that bodies placed without the electric circuit would be affected with the shock, if they were only in contact with any part of it, or very near it. Analogous to this was his observation, that, if the circuit was not made of metals, or other very good conductors; the person who laid hold of them, in order to perform the experiment, felt a considerable shock, in that arm which was in contact with the circuit. See History of Electricity, p. 95.

Lastly, in the course of my experiments with large electrical batteries, I found the force of this lateral

lateral explosion (as I shall chuse to call it) to be very considerable : for I several times observed, that a chain which communicated with the outside of the battery, but which made no part of the circuit, made a black stain on a piece of white paper, on which it accidentally lay, almost as deep as the chain that formed the circuit. (History, p. 644.) And when, in order to judge, by my feeling, of the lateral force of electrical explosions ; I made it pass over a part of my naked arm, the hairs of the skin were all singed, and the *papillæ pyramidales* raised, not only along the path of the explosion, but also wherever any part of the chain had touched it, though not in the circuit. Ib. p. 686.

It was to ascertain the nature and effects of this lateral explosion, that the following experiments were made ; and in reciting them, I shall distinctly note the progress of my own thoughts in the course of the investigation, from a state of absolute uncertainty, to that of the fullest satisfaction ; and, I flatter myself, that some of the facts I shall exhibit, will give surprize and pleasure to those, who are best acquainted with, and most interested in, the history of electricity.

Not having the least doubt, but that if any electric spark passed between a body that was insulated, and another, the insulated body would appear, either to have received, or to have lost, electricity ; I imagined that nothing more was to be done, than to insulate bodies placed within the influence of the electric circuit, with pith balls hanging from them ; and, upon their diverging with the electric spark, immediately to observe, of what kind the electricity

they had contracted was ; and previous to the experiment, I conjectured it would be negative ; supposing that the discharge from the inside coating, in an interrupted circuit, was not able to supply the outside fast enough. And since, the larger the insulated body was, the greater quantity of the electric fluid it was capable of receiving, or parting with, and consequently the more sensible the effort would be ; I began with suspending on silken strings, a pasteboard tube, covered with tinfoil, seven feet long and four inches thick, with large knots at each end ; and a brass ball (at the end of an iron rod, which communicated with the outside of the jar) was placed within about a quarter of an inch of it ; while the discharge was made through an insulated interrupted circuit, no part of which was less than two feet from the insulated tube. On making the explosion, the spark appeared as I expected ; but, to my great surprize, I could not find that either positive or negative electricity was communicated to the insulated tube. Neither the pith balls, nor the finest threads diverged, or moved in the least, at or after the discharge ; though, every thing else remaining in the same state, the least sensible electricity communicated to this tube (a quantity so small as hardly to be visible, in the form of a spark, at the time of communication) made the balls and the threads separate to a great distance, and would have kept them in a state of divergency more than an hour. Lest a small degree of motion or divergency should escape my notice, while I was intent upon making the discharge, I had an assistant along with me, whose eye was

was upon the threads all the time I was making the experiment.

This experiment, as will easily be imagined, shook my whole hypothesis, and confounded all my ideas. I could not question the fact, having repeated the experiment, with precisely the same event, I believe, above fifty times, on account of having been hardly able to believe my own senses, or those of others. There was an evident electric spark, sometimes near half an inch in length, betwixt the bodies composing the electric circuit and the insulated tube, in such a state of the air, as I knew, by frequent trials, would have kept it electrified a long time, and yet there was no communication of electricity.

I do not remember that I was ever more puzzled with any appearance in nature than I was with this; and, in the night following these experiments, endless were the schemes that occurred to me, of accounting for them, and the methods with which I proposed to diversify them the next morning, in order to find out the cause of this strange phenomenon. Accordingly, I was no sooner at liberty to attend to this experiment; but, repeating it with some difference in the disposition of the apparatus, I observed that, upon every discharge, a slight motion was given to the threads that hung from the insulated tube. Upon this the impossibility of an electric spark, neither giving nor taking any thing from an insulated body, contrary to my most attentive observation, and that of my assistants, I concluded that some motion must have been given to the threads the day before; especially when I found that, in these latter experiments,

the communicated electricity was always positive, the same with that of the inside of the jar; but the quantity of it was so small, that the most exquisite contrivance was necessary to ascertain the nature of it; for though, upon this occasion, the lateral spark was near a quarter of an inch in length, the threads on the insulated tube could only be made, by the explosion, to change their position, from leaning a little one way, to leaning as much the other, in the neighbourhood of an insulated brass rod, loaded with a small quantity of positive or negative electricity.

I could not help, however, being surprized, that so large a spark should give no more electricity to the insulated tube, than it appeared to have done; when, in other circumstances, a spark ten times less than this would have made a great and permanent alteration: yet, improbable as these circumstances were, I entertained no doubt at that time, but that these insulated bodies were electrified, either positively or negatively, according as the inside of the jar was positive or negative, by this lateral explosion, though the degree was exceeding small; and I continued in this persuasion the longer, as it happened to be a considerable time before I got another spark that communicated no sensible electricity. I cannot help taking notice, that if it had happened, that, in my first experiment, the insulated tube had always acquired or lost the least sensible electricity (and, as I afterwards found, there were many chances against the first result) I should have formed, and have acquiesced in, some sort of hypothesis, to account for the giving or receiving of electricity in those circumstances,

stances, and there the business would have ended ; but the seeming contrariety of these appearances obliged me to pursue them farther.

Not being able completely to satisfy myself with my last conclusion, attended with the difficulties above mentioned, I kept diversifying the experiments, and introducing every circumstance that I could imagine might possibly affect the result of them ; and among the rest, I made the following experiments, which quite unhinged me again, and left me as much at a loss as ever I had been before.

Having suspended a fine thread on an insulated brass rod, placed about  $\frac{1}{8}$  of an inch from another rod, which was likewise insulated, and one end of which was in contact with the coating of the jar ; and having electrified the rod that supported the pith balls, and placed a rod loaded with the same electricity near them : I observed that, upon every discharge, the balls, which before were repelled, were instantly attracted by the electrified rod ; and, that the result was invariably the same, whether they and the rod were loaded with positive or negative electricity ; and also whether the jar was charged positively or negatively. I repeated the experiment for several hours, without the least variation in the event ; which clearly proved, that, in these circumstances, the electricity of the rod that received the lateral explosion was discharged by it.

Afterwards, I repeated this experiment with some little variety, and found the electricity of the rod only lessened by the lateral explosion. These experiments, however, by no means favoured the supposition

tion of the uniform communication of electricity, either that of the inside or that of the outside of the jar; and, together with the former experiments, convinced me, that this lateral spark by no means produced the effect that might have been expected in communicating electricity. But, with the next set of experiments, the difficulty began a little to clear up, and continued to do so gradually, till I gained all the satisfaction I could wish for, with respect to this puzzling phenomenon.

The first time that I was able to vary the electricity of the insulated body placed near the electric circuit, or of the bodies that formed the circuit (which I now began to attend to), by any different adjustment of the apparatus, was on the following occasion.

Near to an iron rod, that touched the bottom of a jar charged positively, I placed another insulated rod, with a pair of pith balls hanging to it; and observed, that, when I attempted to make the discharge, through an imperfectly conducting circuit, (bringing *e, g*, part of the table into it), a strong spark passed between the insulated rod and the other that touched the jar, and immediately the balls separated as far as they possibly could; and, continuing in a repulsive state, appeared to be electrified negatively. But immediately completing the circuit with good conductors, and making the remainder of the explosion in a full spark; another spark passed between the two rods, and immediately the balls fell close together again; and sometimes would separate with the opposite, *i. e.* positive, electricity.

I could not, upon this occasion, make the lateral spark, in the full explosion, so great as in the im-

perfect discharge. I also observed, that the more interrupted the circuit was, the farther would the lateral explosion reach ; and that the electricity, which the full explosion communicated, was always positive when the jar was charged positively, and negative when it was charged negatively. The result of the imperfect discharge was always the reverse.

Insulating several bodies, and the jar too, charged positively, they all equally contracted positive electricity by the discharge.

In this state of the experiments, I had no idea of the possibility of the lateral spark not communicating electricity to the insulated body ; but I imagined that the kind of electricity communicated depended upon some circumstance in the disposition of the apparatus, that I was not sufficiently aware of.

At length recollecting, that this last experiment resembled, in some respects, that curious one of professor Richman, mentioned in the History of Electricity, p. 272, in which it appeared, that when the coating of either side of a plate of glass communicated with the ground, the opposite electricity of the other side was more vigorous ; I was satisfied that the negative electricity of the bodies that formed the circuit in the imperfect discharge, was produced by the greater difficulty with which the outside of the jar was supplied, than the inside was discharged ; so that the outside was comparatively in a state of insulation, and therefore would communicate negative electricity to all bodies within its reach. And from this I was led to conclude, that, provided the jar was insulated, and the inside

was



was made to part with its electricity with more difficulty than the outside received it, the bodies that formed the circuit would contract positive electricity; and the result answered exactly my expectations.

I also concluded, that, making the interruption in the middle of the circuit, since, in this case, the inside would give, and the outside receive, with equal difficulty, the bodies in the circuit, placed between the place of interruption and the inside of the jar, would be charged positively; and those placed between the place of interruption and the outside, would be charged negatively; and this also was verified by experiment.

In this state of things, I found, that I could give the insulated circuit what kind of electricity I pleased, provided there was any kind of interruption in some part of the circuit; and conjecturing that the electricity of bodies placed near the circuit would be the same with that of the bodies that composed it, I sometimes placed the rod that supported the pith balls near the circuit, and sometimes introduced it into the circuit; and found, that, in both cases, it contracted the same electricity. This tended to confirm me in my supposition, that the lateral explosion was always attended with a giving or receiving of electricity, according to the nature of the circuit, and the place where it was situated; and I again overlooked the disproportion between the cause and the effect.

Presently after this, it occurred to me, that what may be called the redundant electricity of the outside or inside of the jar, separates from that which is in the glass, and constitutes the charge, must have  
some

some concern in this event; and the supposition was verified by fact. For, insulating a jar, charged positively, I observed, that when I touched the outside coating last (as is commonly done in setting it down) and made the discharge through good conductors, they were all electrified positively; and bodies placed near the circuit were the same. On the contrary, if, after placing the jar upon the stand, I touched the knob of the wire, communicating with the inside, so as to take off all its redundant electricity; both the circuit and the neighbouring bodies contracted negative electricity.

I had at this time quite forgot that *Æpinus* had made the same observation, on discharging a plate of air, mentioned in the *History of Electricity*, p. 273; but, considering what he says on that subject, I find he was mistaken with respect to the reason of this experiment not succeeding with Dr. Franklin and others, who had always asserted, that the electric circuit contracts no electricity at all by a discharge. For he says, that the surfaces with which the doctor tried the experiment, were not large enough to make the effect sensible; and that the distance of the metal plates was likewise too small, as, he says, it necessarily must be in the charging of glass: whereas I could give the insulated circuit as sensible electricity with a common jar, as he could with his plate of air; and much more depends upon the height of the charge, which must have been inconsiderable in the plate of air, than the quantity of surface; which, however, may be increased at pleasure, by multiplying jars in batteries.

I found, however, afterwards, that much depended upon the quantity of surface in the coating, and the bodies connected with them, as containing more of that redundant electricity, the effect of which was seen in the last mentioned experiment. For when I discharged the jar, standing in contact with the prime conductor, the tendency to the communication of positive electricity was so great, that, in that situation, the insulated circuit contracted strong positive electricity, when, every thing else remaining the same, except removing it from the conductor, and then making the discharge, it contracted no electricity at all.

Being now perfectly master of the electricity of the circuit in electrical explosions, and being able, in two different methods, to give which of the two electricities I pleased; I imagined that, if I could so balance them, as to communicate neither, there would be no lateral spark, as in the abovementioned experiments; but in this I was absolutely mistaken. For,

In the first place, when, after setting the charged jar upon the stand, I took off, as near as I could guess, one half of the redundant electricity of the inside, and left both sides equally electrified (as appeared by the equal attraction of the pith balls to them both), the discharge of the jar through a circuit of good conductors did not, indeed, communicate the least sensible electricity to the circuit, but the lateral explosion was almost as manifest as before. The pith balls, hung upon the rod that received it, never separated.

In the next place, I repeated this experiment by balancing the two different methods of communicating electricity to the circuit, one against the other. For, not insulating the jar, but setting it on the table, which gave the circuit and the bodies contiguous to it an advantage for contracting positive electricity by the discharge; but, at the same time, making an interruption in the circuit (by introducing part of the table into it, which tended to give them negative electricity); I could easily manage it so, that the circuit contracted neither the one nor the other; and yet, as in the former case, the lateral explosion was as considerable as ever. The balls never separated.

To vary the experiment, I placed an insulated brass ball, two inches in diameter, round and smooth, so as not easily to part with any electricity it had got, in the place of the rod that supported the pith balls; and having found a situation in which no electricity was communicated to the circuit, I observed that none was communicated to it, though, to all appearance, it received a spark of about  $\frac{1}{4}$  of an inch in length. At least, if it had contracted any, it was so little, as to make it very problematical; whether a pith ball, or a fine thread, was moved by it, or not: whereas, when I gave it the smallest sensible spark in any other manner, it would attract those light bodies for a long time together.

The interruption of the circuit I made use of in this experiment, was not by means of any part of the table, but only about a yard of brass chain introduced into it, and disposed between the inside of the jar and that part of the circuit, near which the

insulated ball was placed. N. B. The ball must not be placed near the jar itself; for, in that situation, I found, that, though it was very smooth, and perfectly spherical, yet it could not be placed very near the outside of the jar standing on the table, without contracting negative electricity, in a very small space of time.

These experiments threw me back into my former state of perplexity, with respect to the lateral spark; since, when the two electricities of the circuit were exactly balanced, it was very little diminished, and yet the body that received it was not in the least sensibly electrified. But, upon reflection, I concluded, that this lateral spark must be of the nature of an explosion, and consequently, that an electric spark must enter, and pass out again, within so short a space of time, as not to be distinguished, and leave no sensible effect whatever: for though, in this case, part of the electric matter natural to the body must be repelled, to make room for the foreign electricity, its restoration to its natural state was so quick, that no other motion could correspond to it.

This hypothesis is favoured by the observation, that it is the very same thing whether a body be introduced into a circuit, or placed near it, with respect to contracting electricity; that is, whether the electric charge enter the body at one place, and go out at another, or whether it be received and emitted at the same place.

This lateral explosion is an effect similar to a partial circuit, in which, part of the electric matter that forms the charge in an explosion, goes one way, while the rest of the charge goes another; the only difference

difference is, that this detached part of the charge leaves the common track, and returns to it again, in the very same place.

Several remarkable partial circuits occurred in the course of my experiments before, particularly one, mentioned in the History of Electricity, p. 692, in which, part only of the explosion passed in the shortest way, while another part of it took a circuit, consisting of the same materials, thirty times as long; and another, mentioned, p. 691, where one circuit was made through a thick rod of metal, and another, at the same time, through the open air.

That there is an admission and an explosion of the electric matter, in this lateral explosion, seems evident, from this circumstance, that it is far more considerable when the body that receives it is large, than when it is small. In the former case, there is room for the electric matter, natural to the body, to retire, upon the admission of the foreign electricity belonging to the charge; whereas, in the latter case, there is not room for it. When I placed a small brass ball, of about a quarter of an inch in diameter, near the circuit, I could not perceive that it was at all affected by any lateral explosion; and the spark was very inconsiderable, when I placed a needle, about two inches in length, to receive it; but when I connected the large tube above mentioned, by means of a pretty thick iron wire, to any body whatever, that was placed in the neighbourhood of the circuit, I have (with a jar of only half a square foot of coating glass) made the lateral explosion, an inch or more in length, consisting of a very full and bright spark of electric fire. Insulated bodies, of  
about

about eight or nine feet in length, seem to admit as large a lateral explosion as any body whatever is capable of: for, connecting them with the ground, by means of the best conductors (which gave the electric matter in the bodies, the freest recess possible) I could never make this explosion much more considerable, using the same jar, and all other circumstances the same.

It is a manifest advantage in these experiments, that the lateral explosion be not taken from the coating of the jar itself, or from any part of the circuit, very near to it. I have found that, *cæteris paribus*, it is the most considerable taken at the extremity of a brass rod, of one foot, or a foot and a half long, the other end of which is contiguous to the jar. It is analogous to this, that the longest spark is taken, not from the body of the prime conductor itself, but at the extremity of a long rod inserted into it. The electric matter seems to acquire a kind of *impetus* by the length of the medium, through which it passes. But I found that the *maximum*, in this case, did not exceed, or rather, that it did not quite reach, three feet; for, making use of a thick iron rod, eight or nine feet long, the lateral explosion, taken at the extremity of it, was about the same, as when it was taken at the end of a rod four inches from the jar; and not half so considerable as when taken at the extremity of a rod one foot long. This, I imagined, might be owing to the obstruction which the electric fluid meets with in passing even through metals; which appears, by my former experiments, to be much more considerable than was generally imagined.

Upon the whole, this remarkable experiment seems to be made to the most advantage in the following circumstances. Let the jar stand upon the table; let a thick brass rod, insulated, stand contiguous to the coating; and, near the extremity of this rod, place the body that is to receive the explosion. This body must be six or seven feet in length, and, perhaps, some inches in thickness, or be connected with a body of those dimensions. Lastly, let the explosion be made with the discharging rod resting upon the table, close to a chain, the extremity of which reaches within about an inch and a half of the coating of the jar. In this case, the operator will hardly fail of getting a lateral explosion of an inch in length; which shall enter and leave the insulated body, without making any sensible alteration in the electricity natural to it.

With large jars, containing three or four square feet of coated glass, bearing a very high charge, I make no doubt but that this experiment might be made to much more advantage; but, at the time that I was engaged in this investigation, I happened not to have any such jar, and therefore only used one that contained half a square foot of coated glass.

If the interruption in the circuit, which is almost necessary in these experiments, be made by introducing a length of chain into it, rather than by making part of the explosion pass along the tube, there is a medium in the length of chain, that answers better than either a longer or a shorter circuit. In a long interrupted circuit, the electric matter seems

to



to lose the *impetus* which it discovers in a short one.

In all these cases, the electric charge seems to remain for a moment in the parts of the interrupted circuit; and therefore instantaneously rushes, in all directions, as well towards bodies that are not placed along its passage to the jar, as those that are; but, when the same charge occupies a larger circuit, it has more room to expand itself, and is not so strongly impelled to desert it. I found, however, by repeated trials, that when I made use of three yards of brass chain in the circuit, there was a distance to which the lateral explosion would not reach. The same distance it also would not reach, when the circuit consisted of only one brass rod; but it reached it with great ease, when only half a yard of chain was used, even without any other interruption in the circuit. But it reached to a much greater distance, when the chain was very short, and the interruption was greater in other respects.

I had imagined, that, since the body which had received the lateral explosion contained, for a moment, more than its natural quantity; that, if it were acutely pointed, some would escape, and that, upon the return of the explosion, the body would be exhausted; but I found no such effect, though I affixed fine needles to the bodies I made use of. The lightest pith balls, placed near the extremities of these needles, were not in the least affected by the explosion.

When I placed a number of brass balls, one behind another, the lateral explosion passed through them all, being visible in the intervals between each  
of

of them, and returned the same way, leaving them all in the same state in which it found them; and a great number of lateral explosions might be taken at the same time, in different parts of the circuit, some of them, very near one another.

It made no difference, whether the lateral explosion was received on a flat smooth surface, or the points of fine needles. In both cases, the spark was equally long and vivid.

I had no sooner completed these experiments on the lateral explosion; but I had a curiosity to see what kind of an appearance it would make *in vacuo*; since no other phenomenon in electricity resembles it. In all other cases, the electric matter rushes in one single direction; whereas, in this, it goes and returns in the same path, and, as far as can be distinguished, at the same instant of time; so that all the difference of the two electricities, which are so conspicuous *in vacuo*, must here be confounded. Accordingly, I found, though my pump was not in good order, that I could perceive this explosion *in vacuo*, at the ends of rods, placed several inches asunder; and when they were brought within about two inches, they seemed to be joined by a thin blue or purple light, quite uniform in its appearance. As these rods were made to approach, this light grew denser; but still exhibited no such variety, as is observed between the bodies that give and receive electricity, in the common experiments *in vacuo*.

I was pretty soon convinced, that uncoated jars could not be used to any more advantage in these experiments, than those that were coated; since the want of coating only operated as an interruption in

the circuit, occasioning a difficulty in the admission of the charge on the outside of the jar. And, in all cases, the greater this difficulty of passage was made, provided the discharge was made at once, the more considerable was the lateral explosion, and the greater shock was given to the hand that held the discharging rod; which shock was nothing more than one of these lateral explosions, issuing from the rod as part of the circuit.

I shall conclude the account of these experiments with observing, that they may possibly be of some use in measuring the conducting power of different substances; since, the greater is the interruption in the electric circuit, occasioned by the badness of its conducting power, the more considerable, *cæteris paribus*, is the lateral explosion.