XLI. *An Account of ſome new Experiments in Electricity, containing,* 1. *An Enquiry whether Vapour be a Conductor of Electricity.* 2. *Some Experiments, to aſcertain the Direction of the Electric Matter, in the Diſcharge of the* Leyden *Bottle: with a new Analyſis of the* Leyden *Bottle.* 3. *Experiments on the lateral Exploſion, in the Diſcharge of the* Leyden *Bottle.* 4. *The Deſcription, and Uſe, of a new Prime-conductor.* 5. *Miſcellaneous Experiments, made principally in the Years* 1771 *and* 1772. 6. *Experiments and Obſervations on the Electricity of Fogs, &c. in Purſuance of thoſe made by* Thomas Ronayne, *Eſq; with a Plan of an Electrical Journal, &c. By* William Henly, *F. R. S.*

SECTION FIRST.

An Enquiry whether VAPOUR be a CONDUCTOR of ELECTRICITY.

EXPERIMENT I.

Redde, May 5, 1774. I Inſulated a glaſs funnel [TAB. XIII. fig. 1.] into which the ſtreams, from a capillary tube, were directed by the electricity.

tricity. From this funnel, the electrified drops were received into a large inſulated earthen diſh; acroſs which lay a long wire; and from its end hung a pair of light cork-balls. On working the machine (after about ninety or an hundred turns of the winch, and when fifty or ſixty drops had fallen into the diſh) *the balls ſeparated*, and preſently diverged, to the diſtance of half an inch. Then taking off the electricity, from all the bodies concerned, I blew the column of water out of the capillary tube, replaced it in the bucket, pointing towards the funnel as before, and worked the machine again, to try whether the electricity, iſſuing from the ſyphon, and paſſing through the air, might not electrify all the bodies, ſo as to ſeparate the balls, without the jet of water; but no ſuch event happened. I then replaced it, with the jet falling into the funnel as before; when it ſucceeded. I then tried it a ſecond time, without the jet of water; and it failed. I thus repeated the experiment alternately, with, and without the jet, taking off the electricity of the apparatus carefully between the trials; till I was perfectly ſatisfied, that the jet of water, received into the funnel, and falling from thence into the inſulated diſh below, was the *medium* by which the balls, hanging from the end of the wire placed therein, became electrified. Hence I inferred, that vapour from boiling water, &c. muſt alſo be a conductor of electricity, though probably in a leſs degree, as being more diſſipated. Having ſince repeated this experiment by receiving the electrified jet *immediately*

diately into a large inſulated diſh, I obſerved the effect to be much greater.

EXPERIMENT II.

Having procured a tin veſſel, ſomewhat reſembling an eolipile, or a chymical retort; I placed it over a ſmall lamp, upon my prime-conductor [TAB. XIII. fig. 2.], and filled it about half-full of boiling water. The noſe of it was ſo ſituated, as to throw the electrified drops into an inſulated diſh, furniſhed with balls, as in the former experiments. After the water had been ſome time poured in, and I imagined enough had evaporated to have produced ſome drops in the neck; I examined the lip, to ſee whether any deſcended, but ſaw none. However, on giving the machine a turn or two, I was very agreeably ſurprized to ſee the electric ſtreams iſſue exactly as from a capillary tube; and a few drops having fallen into the diſh, the balls became electrical, and were attracted by my finger, at the diſtance of an half or three quarters of an inch. In a few turns more of the globe, they ſeparated half an inch. I then threw out the water; and, clearing the veſſel of its vapour, I remounted it upon its ſtand (pointing towards the diſh as before), to try whether the ſharp edge on the lip of the veſſel would not electrify the air, ſufficiently to ſeparate the balls, as the evaporated water had done. I turned the winch a long time for this purpoſe; but the balls never diverged at all. I then poured in the boiling water a ſecond time; and,

and, when the drops began to fall, the fourth turn ſeparated the balls; and the tenth cauſed them to diverge to the diſtance of half an inch; and in this ſtate of repulſion they continued a conſiderable time, after I had ceaſed to work the machine. I then took off the electricity with my finger, and again cleared the veſſel of its water, &c. and, having replaced it with the point as before, I worked the machine again as uſual. The air was now grown in ſome meaſure electrical; for, at the ſeventh or eighth turn, the balls began to ſeparate, and in forty turns they were about three eighths of an inch diſtant from each other. I then ceaſed to turn the winch any longer; but had no ſooner ſtopped, than the balls began to cloſe, and in a very few ſeconds they were in contact; whereas, in the former experiment (when the electrified drops were in the diſh), on my ceaſing to turn the globe, they ſhewed no ſign at all of converging; and, I imagine, would have remained ſeparate a long time, if I had not taken off their electricity with my finger. I apprehend, therefore, from this experiment, *that the vapour of hot water is a conductor of electricity.*

EXPERIMENT III.

I hung on a ſtring, as near to the cieling of the room as I could, a pair of pith-balls, which, on working the machine a conſiderable time, diverged three quarters of an inch, but no wider. Then ſticking into the conductor a *ſmoking deal-match,* and working the machine again, they preſently

preſently ſeparated to the diſtance of two inches. The match, when placed in the ſame ſituation, and not ſmoking, had no ſuch effect.

EXPERIMENT IV.

Having placed an earthen half-pint mug, upon a ſtand properly inſulated; I fixed to a large ball of braſs, which I had placed in the bottom of it; the end of a wire, ſix, or eight feet in length. The other end of the wire I connected with the prime conductor of a ſmall electrical machine [TAB. XIII. fig. 3.]. Over this mug, and as near to ceiling of the room as I could, I ſuſpended a pair of light cork balls. Then filling up the veſſel with boiling water, I began to work the machine; and in fifty, or ſixty turns of the winch, obſerved the balls to ſeparate three eighths, or half an inch, from each other. I then took off the electricity of the bodies; emptied the veſſel, and cleared it of the vapour; and having placed the apparatus in the ſame manner, I again worked the machine, for a longer time; but without effect. On replacing the boiling water, I ſucceeded as at firſt. At other times, when I have been able to ſeparate the balls by the air alone, to a ſmall diſtance; yet by pouring in the hot water, the vapour has preſently increaſed their divergence from one eighth, or three ſixteenths, to half an inch diſtance; or in that proportion, according to the ſtate of the atmoſphere with reſpect to dryneſs or moiſture. In ſhort I have repeated theſe kinds of experiments ſo often; and many times with ſo much ſucceſs; that I can have no doubt of vapour being a conductor of electricity.

EXPERIMENT V.

I inſulated the rubber of my machine, and hung a pair of Mr. CANTON's balls upon the prime-conductor. I then worked the machine, and having taken off a ſpark, or two, to draw off the electricity naturally inherent in the rubber, &c. I obſerved the divergence of the balls; which was very great; in ſo much that the ſtrings were bent: and on approaching the back of the rubber with a *ſmoking green wax taper*, juſt blown out, (the ſmoak of which was inſtantly attracted to it,) they diverged no wider (a). I then took off the balls, and placed my own electrometer in its ſtand, upon the prime-conductor [TAB. XIII. fig. 4]; and having taken off a ſpark, or two, as before; I again worked the machine, to obſerve the repellency of the index from the ſtem; and found it conſtantly to vibrate between five, and ten degrees, of the quadrant, which was divided into fifteen. I then brought the ſmoking taper, within four, or five inches, of the back of the rubber, as before; and obſerved, that on the attraction of the ſmoak to it, the index preſently began to riſe; and in a very ſhort time, got up to right angles. I repeated the experiment, ſeveral times, with the ſame ſucceſs. I then tried the experiment by bringing my finger to the ſame diſtance from the rubber, and pointing towards it; but this, in many trials, had not the leaſt effect. The taper likewiſe, when held at the ſame diſtance, *and not ſmoking*, had no effect at all. I am

(a) For this experiment, the back of the inſulated rubber ſhould be perfectly ſmooth: mine, is of wood; with the leather paſted down cloſely to it; ſo as to leave no points.

convinced

convinced therefore, that *the ſmoak* was the *medium*, which conveyed the electricity, from my hand, to the inſulated rubber.

EXPERIMENT VI.

I placed upon a ſtand, on my prime-conductor, a piece of ſmoking wax taper [TAB. XIII. fig 5.], when immediately, on working the machine, the ſmoak, from a large, and diffuſed volume, was much contracted; and its motion upwards, greatly accelerated. I then took off the electricity of the conductor, and held a pair of cork balls a quarter of an inch diameter, hung on threads two and a quarter inches long, (being the neareſt at hand,) perpendicularly, over the riſing ſmoak; and as high as I could poſſibly reach, ſtanding on a chair; this might raiſe the balls about five feet and an half above the prime-conductor; when (working the machine) in a few ſeconds the balls ſeparated to half an inch diſtance. I then removed the taper, but could not perceive that the balls were at all affected without it; but on replacing it, they ſeparated as before. I repeated the experiment ſeveral times, with, and without the taper, and the different effect, was conſtantly as above recited. I then ſet a tin ſaucer upon the ſtand, and placed upon the ſaucer, an half pint mug of boiling water [TAB. XIII. fig. 6.); and over this water, I preſented the balls, in the riſing vapour; as I had before done in the ſmoak. On working the machine a few ſeconds, the balls diverged to the diſtance of one twelfth part of an inch. On removing the water, and preſenting the balls as before, they ne-

ver ſeparated at all; though I worked the machine for a longer time; but on replacing the water, in a few ſeconds the balls diverged, as at firſt. Theſe experiments I repeated ſeveral times, and always with the ſame ſucceſs. The ſmoak, therefore, in the firſt experiment; and the vapour of the hot water, in this laſt; was certainly the *medium*, which conveyed the electricity, from the prime-conductor, to the balls: and I think I may now very ſafely pronounce, that SMOAK, *and the* VAPOUR *of hot water, are abſolutely conductors of electricity*; though ſmoak is a far better one than the vapour of hot water, and both of them are exceedingly bad ones.

Upon the queſtion, whether vapour be a conductor of electricity; I would obſerve, that Dr. FRANKLIN's curious experiment, of making a viſible atmoſphere, round an inſulated, electrified body; cannot be made, but in an exceedingly dry ſtate of the air. The reaſon is obvious; but in a very dry day, I have often ſucceeded in the experiment; and have ſhewed it to ſeveral of my friends, particularly Mr. MARSHAM, and Mr. NAIRNE.

My method of doing it, is as follows: I place *the braſs caſe* of a ſteel-yard weight (about two and an half inches diameter,) upon a clean, dry, ſtand of ſealing wax: then having in readineſs, *a green wax taper, with a long ſnuff*, I give the inſulated body, a ſpark, from the knob of a poſitively charged bottle; or take one from it, by the knob of a negatively charged one (the appearance is the ſame in both caſes); then bringing the taper, juſt blown out, very gently towards the inſulated body, the

the ſmoak is inſtantly, and with a kind of violence, attracted to it; till it becomes compleatly covered with it. It remains in this ſituation for ſome ſeconds, when it begins to diſappear, at the bottom; and proceeds gradually, till it comes to the top; where it hangs quivering, like the departing flame of a lamp. It goes off in a long thin column, which rarefies, and diſperſes at the top, till it occupies a great ſpace. Obſerve that, in giving the ſpark, and bringing the taper towards the inſulated body, particular care muſt be taken, that the air be diſturbed as little as poſſible.

The *green wax taper*, on account of the verdegreaſe it contains, anſwers excellently, in this, and ſeveral other experiments, where ſmoak is required *without heat:* and I firſt uſed it, from a hint Dr. FRANKLIN was ſo obliging as to give me.

SECTION SECOND.

Of THE DIRECTION of the electric matter, in the diſcharge of the LEYDEN BOTTLE.

EXPERIMENT I.

LIGHT a ſmall wax taper, and place it, with the flame exactly between two braſs balls A and B, about two inches aſunder; properly introduced into the circuit [TAB. XIII. fig. 8.]. Then, having given a ſmall phial two or three turns of the globe, charging it poſitively, connect *the coating of it*, by a chain, with the wire of the ball A; and upon applying the knob of the phial, to the wire of the ball B, you will obſerve the flame to be plainly driven

driven from it; being often blown upon the ball A, ſo as to blacken it with the ſmoak. Then charge the phial negatively, and (the apparatus remaining as before) apply the knob of the phial as at firſt; and you will then perceive the flame to be blown quite in the contrary direction, viz. from A *towards*, and *often upon* B, as upon Dr. FRANKLIN's principles of the Leyden bottle, it ought to be. Obſerve that, in this experiment it is neceſſary to uſe the leaſt charge that can be given, juſt ſufficient to leap the interruption in the circuit; which experience will preſently determine; for if the charge be too great, the flame will be attracted as well as repelled, in the diſcharge of the phial; and then, nothing can be infered from the experiment.

EXPERIMENT II.

Charge a large jar *poſitively*, and inſulate it; then take a long curved wire, *pointed, at both ends*, and hold it by a glaſs handle, ſo as to bring one end of the wire, half an inch, from the knob, and the other end of it, to the ſame diſtance, from the coating of the jar. You will then obſerve a ſmall luminous ſpark, upon the point appoſed to the knob of the jar, and a fine pencil, diverging from the lower point, ſpreading upon the coating of the jar, which will preſently diſcharge it ſilently. Then charge your jar *negatively*; inſulate it, and apply the wire as before; and the appearances, at the points of the wire, will be directly reverſed; plainly demonſtrating the direction of the electricity in the diſcharge of the bottle.

Another

Another very convenient and eaſy method, of exhibiting the phænomena of the poſitive and negative electricity of the inſide, and outſide ſurfaces, of a charged Leyden bottle, is by ſlipping a cap of metal, furniſhed with a ball and wire, upon the outſide coating; and mounting it upon an electric ſtand, in an horizontal poſition; as TAB. XIV. fig. 3.; or if the bottom of the glaſs be turned much upward into the body of it, a piece of wood may be worked to its ſhape, and cemented thereto; and through the middle of this wood, a ſhort tube of metal may be inſerted, ſo as to admit the wire which is connected with the ball to paſs through it; and be brought into contact with the coating of the jar, at pleaſure (b). By this means, experiments may be made, at either end of the bottle with great facility; and other charged or exhauſted, bottles; excited ribbons; or other electrics: the curved pointed wire, &c. &c. may be readily applied; and give, or receive a ſpark; be attracted, or repelled; according to the kind of electricity in the two bodies, ſo applied towards each other. By hanging a chain round either of the wires, and connecting it with one end of the diſcharging rod; and bringing the other end of the rod, ſo as to leave a proper ſpace between *that* and the ball on the wire, at the oppoſite end of the bottle; the flame of a taper, &c. may be interpoſed; and ſhew the direction of the electricity in the diſcharge: or a cork-ball, *hung by ſilk*, may play between them, in the man-

(b) For many experiments, it needs only to be connected with the curved metal, or wood, in which the charged bottle is placed.

ner deſcribed by Dr. FRANKLIN. If the balls are taken off from the wires of the bottle; the wires being pointed, and one of them placed before the globe; or a prime-conductor, electrified *poſitively*: the phænomena of charging the Leyden bottle will be diſcovered by the different appearances, at the end of the wires; as at TAB. XIV. fig. 4. If the bottle be thus placed before a conductor, electrified *negatively*; or *the inſulated rubber* to a machine; the appearances, at the ends of the wires, will be reverſed: as upon Dr. FRANKLIN's principles they ought to be; and thus explain his theory of the Leyden phial.

But a more ſimple, and beautiful analyſis of the Leyden phial, hath not, I think, yet been exhibited; than the following. Let a bottle that will hold near a pint; having a long neck (about an inch in diameter,) be furniſhed with a ſmall plate at the top; with a valve properly ſecured, after the bottle is exhauſted: from which plate, a wire about one eighth of an inch in diameter, is to project a little below the neck; and terminate with a blunt end. The top is to be covered with a round braſs cap, firmly fixed thereon; and made air-tight. The bottom of the bottle ſhould be coated with tin-foil, which ſhould be continued *three inches up the ſide*. This bottle will charge and diſcharge ſeveral times in a minute; and the tin-foil coating, will prevent the ſhock from affecting the hand of the operator (c). The phænomena of charging the Leyden bottle, is elegantly explained by this contrivance; and is made viſible, by the end of the wire; on which, the appearances vary, according as the bottle is charged, viz. poſitively,

(c) The bottle being held below the edge of the coating.

or negatively; or as the conductor, from which it charged, is electrified. TAB. XIV. fig. 5. letter A. ſhews ſuch a bottle, charging *negatively*, at a conductor, loaded with *poſitive electricity*. Letter B. ſhews the ſame bottle, charging *poſitively*, at the ſame conductor.

TAB. XIV. fig. 6. letter C. ſhews the bottle charging *poſitively* at a conductor, *electrified negatively*; or at the inſulated rubber. Letter D. ſhews the ſame bottle, charging *negatively* at the ſame conductor.

SECTION THIRD.

Of the LATERAL EXPLOSION in the diſcharge of the LEYDEN BOTTLE.

EXPERIMENT I.

HAVING made a double circuit, the firſt by an iron bar, one inch and an half in diameter, and half an inch thick; the ſecond, by four feet and an half of ſmall chain; on diſcharging a jar, containing five hundred ſquare inches of coated ſurface, the electricity paſſed in both circuits, ſparks being viſible on the ſmall chain in many places. On making the diſcharge of three jars, containing together ſixteen ſquare feet of coated ſurface, through three different chains *at the ſame time*, TAB. XIII. fig. 7. bright ſparks were viſible in them all; and I have not the leaſt doubt, but it would have been viſible in as many more. The chains were of iron and braſs, of very different lengths; the ſhorteſt ten, or twelve inches, the

longeſt, many feet, in length. When thoſe jars were diſcharged through the iron bar before-mentioned, together with a ſmall chain, three quarters of a yard in length; the whole chain was illumined, and covered throughout with beautiful rays, like briſtles, or golden hair. Having placed a large jar in contact with my prime conductor, I affixed to the coating of it an iron chain, which I alſo connected with a plate of metal, on which I intended to make the diſcharge by my diſcharging-rod, TAB. XIII. fig. 9. This done, I hooked another chain, much longer, and of braſs, to the oppoſite ſide of the jar, and brought the end of it within eight inches and an half of the metal plate. In contact with this end, I laid a ſmall oak-ſtick, eight inches long, which I covered with ſaw-duſt of fir-wood. On making the diſcharge upon the plate, *both the chains* were luminous through their whole lengths; as was alſo the ſaw-duſt, which was covered by a ſtreak of light, making a very pleaſing appearance. I repeated the experiment ſeveral times. Perhaps, if I had uſed a bar of iron, inſtead of the chain firſt-ſpoken of, there might have been no light upon the ſecond chain, or upon the ſaw-duſt, eſpecially as the electricity had half an inch of air to paſs through, before it reached the end of the ſtick. But from this experiment may, I think, be inferred, the neceſſity of making the conductors, erected as a ſecurity to buildings, &c. from the damage of lightning, both of the beſt materials, and of a very ſufficient ſubſtance; and, for this purpoſe, perhaps nothing will be found ſo proper as

lead,

lead, which will remain in the earth many centuries without any conſiderable decay; and the tops of chimneys being covered with it (*d*), and furniſhed with a long, ſharp-pointed rod of copper, or iron pointed with copper, which I think ſhould extend at leaſt five, or ſix feet, above the top of the chimney, or higheſt part of the building; a communication ſhould be made from it by plates of lead, eight, or ten inches broad, with the lead, on the ridges, and gutters, and with the pipes which carry down the rain-water; which pipes ſhould be continued to the bottom of the building, and there made to communicate, by means of other leaden pipe, or a plate of it, as before-mentioned, with the water in a well, the moiſt earth, or the main pipe which ſerves the houſe with water.

SECTION FOURTH.

Deſcription and Uſe of a new PRIME-CONDUCTOR. Contrived by Mr. HENLY, and executed by Mr. EDWARD NAIRNE.

A. TAB. XIV. fig. 7. A glaſs-tube, eighteen inches long, and near two inches in diameter.

B. C. Balls of Braſs, with a ferule, two inches long, to each of them; which ferules are to be cemented to the ends of the tube, and made air-tight.

(*d*) I mention covering the tops of chimneys with lead, as a protection to the upper courſes of bricks, from the effects of wind; and not as being of any eſſential ſervice to the conductor, any farther than as it may aſſiſt in fixing the pointed rod, which is to be elevated above it, more ſecurely.

One of the braſs plates, which are ſoldered to the ferules, hath a ſmall hole drilled through it, by which the air is to be exhauſted. It is covered by a ſtrong valve, properly ſecured, and concealed by the braſs ball B, or C.

D. E. Balls of braſs, about five eighths of an inch in diameter, fixed upon wires, which project two inches and an half from the braſs plates, at each end of the glaſs-tube.

F. A fine-pointed wire, to collect the electricity from the excited glaſs-globe, &c.

G. Supporters, of ſealing-wax; upon which the luminous conductor is to be mounted.

N. B. The dots in the tube are intended to repreſent the appearance of the electricity in it, in the experiments deſcribed in TAB. XIV. But, when a bottle, or a large jar, is diſcharged through the glaſs-conductor, it is uniformly filled with light.

The USE of the GLASS CONDUCTOR.

The glaſs-tube, thus furniſhed, and mounted, *being properly exhauſted, and perfectly dry*, will act in all reſpects like one of metal; and the electrometer, being placed upon the braſs ball B. will anſwer to the charge of a jar, or battery, exactly. But the principal uſe of this inſtrument, is *to aſcertain the direction of the electric matter, as it paſſes through it.* And this end, it completely anſwers in the manner following, viz. ſet it with the collecting-point F. before the globe, and place the knob

knob of an uncharged bottle nearly in contact with the braſs ball B. or hang a chain, &c. from thence to the table; and, on working the machine, the ball D. in the tube becomes entirely enveloped, in a denſe, white atmoſphere of electricity. If the point F. be brought nearly into contact with an inſulated rubber, and a communication be made from the ball B. to the table; the atmoſphere will be upon the ball E. in the tube. If a bottle, poſitively charged, be preſented as in the drawing TAB. XIV. fig. 9. the appearances in the tube will be as therein delineated. But, if a bottle, charged negatively, be thus applied, the atmoſphere will ſurround the ball E. in the tube, as in TAB. XIV. fig. 10.

CONJECTURES on theſe Phænomena.

It is ſuppoſed, that the impelling power of the globe, or the knob of a *poſitively charged bottle*, drives the particles of electricity *through the ſubſtance of the balls*, wire, &c. with which they are in contact, with great velocity, and in a kind of ſtraight line; but, the electricity having entered the *vacuum*, the repulſion of its particles immediately takes place, and the tube is inſtantly filled with light. The denſe white atmoſphere upon the oppoſite ball is ſuppoſed to proceed from the reſiſtance of the air in the tube; a ſmall portion of which, will, in this method of exhauſtion, inevitably remain in it. And, as every particle of electricity, is ſuppoſed to be in a ſtate of repulſion with reſpect to its next neighbour, the *vacuum* giving them free liberty of expanding themſelves,

or

or ſtanding at the greateſt diſtance from each other; it is imagined, that they actually do ſo, and will not enter the ball, and wire, at the oppoſite end of the conductor, *in a point, or ſmall ſpace,* (as they do in the open air); but that they entirely ſurround them, and enter *at all parts at the ſame time,* in order to their conveyance into thoſe bodies placed at the end of the braſs work to receive them.

If, inſtead of the braſs *balls* in the tube, *points* are uſed; or if *a point* be fixed at one end of the tube, and *a ball* at the other; the effect will be preciſely the ſame.——Note alſo, That the glaſs-conductor, for the purpoſe of making Dr. FRANKLIN's curious experiments, with a pointed and blunted wire, is far ſuperior to one of metal, the electric atmoſphere being ſo much better retained by it. By this eaſy and ſimple proceſs, may an ocular demonſtration, at all times, be given, in a dark room, and dry air, of the truth and propriety of Dr. FRANKLIN's hypotheſis of the Leyden bottle.

SECTION FIFTH.

Miſcellaneous Experiments, made principally in the Years 1771 and 1772.

EXPERIMENT I.

IF a black ſilk ribband, or a piece of black ſilk, be laid on a quire of paper, &c. on a table, and excited by drawing over its ſurface ſealing-wax, ſulphur,

ſulphur, amber, or a tube of glaſs with *the poliſh taken off by emery*; its electricity will be *poſitive*: whereas, if it be excited, ſingly, or together with a white ribband, by drawing them briſkly between the fingers, it is always *negative*.——Laying it on the paper, and drawing over its ſurface a rod, or tube of ſmooth glaſs, its electricity will alſo be negative.

EXPERIMENT II.

If a plate of glaſs, ten, or twelve inches in diameter, be excited, and placed upon the top of a box, from which a pair of light pith or cork-balls are ſuſpended, being mounted on a ſtand of ſealing-wax; the balls will ſeparate, and ſtand repelled from each other, being electrified poſitively, (in a dry air), upwards of four hours. When they come into contact, on removing the glaſs, they diverge again, and are negatively electrified; but, on replacing it, they cloſe. On removing it again, they ſeparate; and thus alternately as long as any electricity remains in it (e).

If the plate of glaſs be placed in a frame of wood, and a light pith or cork-ball be laid on its ſurface; on preſenting towards it the end of a finger, or the point of a pin, &c. the ball will recede from them, with a very briſk motion, and may thus be driven about upon the ſurface of the glaſs, like a feather in the air, by an excited tube, or the wire of a charged bottle. The cork-ball,

(e) For an explanation of theſe phænomena, ſee Mr. CANTON's experiments, Philoſophical Tranſactions, Vol. LVIII. Part I. N° 53.

being

being deprived of its electricity by the pin, &c. inſtantly flies to that part of the glaſs to which it is attracted the moſt forcibly.

EXPERIMENT III.

I hung on my prime-conductor a ſmall phial, two inches in diameter, coated three inches and a quarter from the bottom. From the coating of this phial, I ſuſpended two chains; the firſt, in contact with a heavy weight, placed upon a card, acroſs which, I had ruled lines, at equal diſtances, TAB. XIV. fig. 1. the ſecond chain formed a circuit, with leaden pipe, ſmall braſs wire, ſmall chain, &c. of one hundred and twenty feet in length. From the ball of my diſcharging rod, which reſted on another weight (ſee the figure), I alſo hung a chain, in contact with, and compleating, the circuit of one hundred and twenty feet before-mentioned; and obſerved, that, if my bottle was charged quite full, the electricity would, in the diſcharge, *paſs through the long circuit*, rather than over the ſurface of the card, when the weights were placed at nine ſixteenths of an inch aſunder: but, if I charged the bottle only about half-full, the electricity would, in the diſcharge, paſs through the long circuit, rather than over the ſurface of the card, though the weights were placed at the diſtance of only *three ſixteenths* of an inch.— Query, Can there be a greater proof of the ſmall reſiſtance made by *metal* to the paſſing of the electric matter, compared with card, wood, &c. and conſequently of the utility

lity of metallic conductors to buildings, ſhips, &c.? The ſame obſervation hath been repeatedly made, upon the effects of the natural electricity. And a remarkable inſtance hath lately happened, at the church of St. John, Weſtminſter; a very exact account of which hath been taken, by Dr. WATSON, F.R.S. and J. BANKS, ESQ. F.R.S. who, I hope, will at a convenient opportunity, favour the curious in theſe matters with their ingenious and intereſting remarks on it. The tower of this church, I am informed, hath no *pointed* metallic termination.

EXPERIMENT IV.

Having prepared a phial, in the manner directed by Mr. LANE, for making his curious experiment; by paſſing a wire through the bottom, and another through the cork, ſo as to bring the ends of the two, within half an inch of each other, about the middle of the bottle (which was filled with water) I found, as that gentleman obſerved, that a ſlight ſhock of electricity diſcharged through it, would break the bottle. But having put a very ſmall wire from the top, to the bottom of it, *through the water*; I diſcharged through it, three large jars, containing ſixteen ſquare feet of coated ſurface, when the whole of the ſmall wire was exploded; but the bottle remained unhurt. If therefore a metallic conductor (being too ſmall) ſhould happen to be deſtroyed by a ſtroke of lightning, yet the building, &c. to which it is affixed, will probably eſcape uninjured.

EXPERIMENT V.

When I ſtrongly electrify a large prime-conductor, three feet long, and twelve inches in diameter; if a perſon hold in his hand a braſs rod terminated by a ball, two inches in diameter, at the

diſtance of two inches, from the ſide of the conductor [TAB. XIV. fig. 2.], he will continue to draw ſuch ſtrong ſparks as will give him a ſenſible ſhock in both his legs: but if another perſon at the ſame time preſent the point of a lancet, or a wire five or ſix inches long, nicely tapered to a point, tipped with ſteel, towards the conductor; though at the diſtance of two feet, or ſomewhat more, this will draw off all its electricity *ſilently*; and not ſuffer a ſpark to paſs from thence to the braſs ball: it is alſo obſervable, that if the point of the wire, or lancet, be brought nearly into contact with the prime-conductor, yet no ſenſation is felt in the arm, &c. of the operator: hence, I think, appears clearly the preference due to *points*, rather than round balls, or blunted ends, for the termination of the conductors erected as a ſecurity to buildings, &c. from damage by lightning: for to me, it ſeems probable, that the ſharp point of the conductor will act upon the electric atmoſphere of the cloud, and perhaps gradually and ſilently continue to diminiſh the contents, before the cloud can approach near enough to ſtrike; and thus contribute to leſſen, if not actually prevent, a ſtroke (*f*). But ſhould the point be ſtruck, the conſequence I ſuppoſe will not be great, and a curious inſtance I have now before me, which I ſhall beg leave to quote as follows. "About nine o'clock we had a "dreadful ſtorm of thunder, lightning, and rain, "during which the main-maſt of one of the Dutch "Eaſt Indiamen was ſplit, and carried away by "the deck (*g*); the maintop-maſt and top gallant-

(*f*) Captain WINN's conductor, though two of the links were broken, effectually anſwered this intention.

(*g*) The ſtays, ſhrouds, &c. being all cut aſunder (Dr. SOLANDER informed me) as with a knife.

maſt,

" maſt, were ſhivered all to-pieces; ſhe had an " iron ſpindle at the main top-gallant-maſt-head, " which probably directed the ſtroke. This ſhip " lay not more than the diſtance of two cables " length from ours, and in all probability we ſhould " have ſhared the ſame fate, but for the electrical " chain which we had but juſt got up, and which " conducted the lightning over the ſide of the ſhip; " but though we eſcaped the lightning, the explo- " ſion ſhook us like an earthquake, the chain at " the ſame time appearing like a line of fire (b): a " centinel was in the action of charging his piece, " and the ſhock forced the muſket out of his hand, " and broke the rammer rod. Upon this occaſion " I cannot but earneſtly recommend chains of the " ſame kind to every ſhip, whatever be her deſti- " nation; and I hope that the fate of the Dutch- " man will be a warning to all who ſhall read " this narrative, againſt having an iron ſpin- " dle at the maſt-head."—— See Capt. Cook's voyage. This conductor was of copper wire, three ſixteenths of an inch in diameter; which I am inclined to think is rather too ſmall for the purpoſe; I am of opinion it ought to be a quarter of an inch at leaſt: and I have been informed by Dr. SOLANDER, that the point originally belonging to the conductor, had been ſtolen; and that *this*, on which the lightning fell, was of inferior workmanſhip, and not ſo ſharp; which was another great diſadvantage: perhaps if the wire of the chain had been larger, and

(b) It does not however appear, that the ſmall *hempen cord*, with which the links of the chain are cloſely connected, or even the ſtrings which connected them, had received the leaſt injury from the lightning.

the point more acute, the ſtroke would have been much leſſened; if not abſolutely prevented. If, inſtead of thoſe chains, *plates* of copper, three ſixteenths of an inch thick, and two inches in diameter, with the edges neatly rounded off, were inſerted in a groove, and continued down the main-top-gallant-maſt, the main-top-maſt, and part of the main-maſt, into the well-hole; a communication from the maſt, to the under-ſide of one of the decks, might be made with a plate, or rod of metal, flattened at each end; and from that rod, the conductor might be continued by plates of lead, or copper, on the under-ſide of the deck, and down both the outer-ſides of the ſhip, as low as the keel, if it be thought neceſſary: and this method (the conductor being always in readineſs, and kept perfectly in order) I ſhould apprehend would be preferable to the chains, which are now in uſe. Particular care ſhould be taken, to have all the plates, which form the conductor, as nearly as poſſible in contact with each other, and to fix a ſharp-pointed, ſlender rod of copper at its ſummit. And for the purpoſe of connecting the plates, inſerted in the main-top-gallant-maſt, the main-top-maſt, and the main-maſt; if an hoop of copper were fixed in a groove of its own thickneſs, at the top of the main-maſt; and another ſuch hoop at the upper end of the main-top-paſt; perhaps they might anſwer this end very conveniently (i). The learned and ingenious Dr. WATSON, F. R. S. hath, with

(i) If an objection ſhould be made to cutting grooves in the maſts, the plates of metal, which form the conductor, might readily be faſtened upon the ſurface, and very ſecurely.

great

great judgement and accuracy, collected from ancient hiſtory, the accounts therein recited, of electrical appearances, upon pointed bodies; as the ſpears of ſoldiers, &c. &c. (k) which have been very judiciouſly introduced by Dr. PRIESTLEY into his Hiſtory of Electricity (l): and I cannot but think, thoſe accounts, furniſh a very ſtrong argument, in favour of *pointed conductors:* for had the bodies here ſpoken of been terminated by blunted ends, or round knobs, it is probable that many of them inſtead of drawing off the lightning *ſilently*, would have been *ſtruck* with it; and this, being deemed a common occurrence, would have paſſed unnoticed, and conſequently never have been recorded in hiſtory.

If pointed bodies had really the property of drawing down ſtrokes of lightning upon themſelves, I think the pillar upon Fiſh-ſtreet Hill, commonly called the MONUMENT, could not long have eſcaped. This pillar is terminated by a baſin of metal, four feet and ſix inches in diameter. The baſin is ſurrounded by a great number of bended plates of metal, *ſharply pointed*, to repreſent flames of fire. From the baſin, to the floor of the gallery, are fixed perpendicularly in a circular order four thick bars of iron; and in thoſe bars are inſerted twenty-eight ſtrong hoops, and four ſegments of circles, of the ſame metal; which ſerve as ſteps from the gallery to the baſin. One of theſe bars (being one inch thick, and five inches broad) is connected with the iron rails of the

(k) See Philoſophical Tranſactions, vol. 48, part I. p. 210.

(l) See Hiſtory and Preſent State of Electricity, ſecond edition, p. 371.

ſtair-caſe,

ſtair-caſe, which reaches to the bottom of the building, and forms a ſubſtantial, regular conductor of metal the whole length. The monument was erected by Sir CHRISTOPHER WREN in remembrance of the fire of London, which happened in the year 1666. It was compleated by that great architect, in the year 1677; is, including the blazing urn at its ſummit, about two hundred and two feet in height, from the pavement; and hath never (ſo far as I have been able to learn) been ſtruck by lightning. The *antennæ* and legs of the graſshopper on the Royal Exchange in Cornhill; and the tongue and tail of the dragon on the ſpire of Bow church in Cheapſide, London, are alſo remarkable inſtances (*m*): indeed I have often thought it rather a favourable circumſtance, that moſt of the lofty public buildings in this metropolis which have metallic terminations, have generally been furniſhed with weather-fanes, which fanes commonly end in ſharp points: for had they been terminated with large round balls of metal, perhaps many more of them might long ſince have been demoliſhed. Here therefore I cannot but expreſs my earneſt wiſhes, that, on all future occaſions, where lofty public edifices are to be erected; a good pointed conductor for the lightning, may be conſidered by every architect, or ſurveyor, as an eſſential part of the edifice itſelf.

EXPERIMENT VI.

I attempted to aſcertain the conducting power, of different metals, in the manner following. I

(*m*) A great variety might be produced, but Dr. FRANKLIN hath himſelf rendered this unneceſſary.

took

took a thick piece of paſte-board, acroſs which I ruled lines, exactly an inch aſunder. Upon theſe lines, croſs-wiſe, I placed the wires; which I confined by heavy weights: the edges of which weights juſt touched the ruled lines; leaving exactly an inch of wire between them [ſee TAB. XIV. fig. 1.]. The kinds I tried were, *pure gold, ſilver, braſs, copper ſilvered*, and *iron*. They were all drawn through the ſame hole, except the iron, which was ſomewhat larger than the others. I proved them with two jars, containing eleven ſquare feet of coated ſurface; and adjuſted the charges, by an electrometer graduated in diviſions of one tenth of an inch each, the diameter of the ſcale being two inches. The reſult was as follows:

Pure Gold	was melted at	4	Diviſions.
Braſs		6	
Copper ſilvered		8	
Pure Silver		10	
Iron		10	

If I gave either of the wires a diviſion leſs than the number above ſpecified, it was not melted: if I gave either of them a diviſion more, it was *exploded*; the greater part vaniſhing in ſmoak: whereas theſe charges juſt burſt them into balls.

Should any gentleman chooſe to repeat this experiment, I would recommend it to him to be very particular in ſizing the wires; *to uſe a greater length*, to do it when they are all freſh drawn, to make the experiment in a very dry day, and in a room where there is no fire. With theſe precautions, probably there may be ſome difference in the reſult; and this method will perhaps give a

true account, of the conducting power, of the different kinds of metal.

Having lately been preſented, in the moſt polite manner, by the celebrated Dr. LEWIS, F. R. S. &c. with ſix ſpecimens of his *platina*; in as many different ſtates: I ſelected the largeſt grains, from one of the parcels which Dr. LEWIS informed me had been repeatedly expoſed to long-continued vehement fires; the moſt intenſe which he had been able to excite, or any veſſels he could procure would ſupport: and after a few ſmall globules (conſiſting doubtleſs in great part of heterogeneous metal) had melted out, repetitions of the operation produced no further change. It was afterward boiled ſucceſſively in oil of vitriol, aqua-fortis, and ſpirit of ſalt, in order to its further purification; and which indeed reduced it to a ſtate the moſt pure of any that excellent chemiſt had been able to produce. Having ruled a line with a blunt-ended wire, over the ſurface of a plate of white wax;

EXPERIMENT VII.

I preſſed in the grains of *platina* lightly, and in contact with each other; ſo as to form a regular line, half an inch long. At each end of the line of *platina*, and in contact with it, I placed a thick wire, with its ends nicely rounded off, and made perfectly ſmooth. I covered the *platina* with a piece of thick plate-glaſs; and then diſcharged through it, three jars containing ſixteen ſquare feet of coated ſurface: when I obtained many beautiful ſpherules of the *platina*. Several of them ſtuck to the wax, and glaſs; and others *imperfectly formed*, upon the edges, &c. of the grains: which proved

proved that the fuſion had been compleat. This experiment I made in the preſence of Mr. Ferguſon, F. R. S. Mr. Willlam Canton, Mr. Bell, and Mr. Marſham, who all acknowledged it perfectly ſatisfactory. Having mentioned the reſult of this experiment, and the method of making it, to Mr. NAIRNE; he hath ſince repeated it with equal ſucceſs.

Being informed, by Mr. William Canton, that his brother, Mr. Thomas Canton, had, in preparing *a dried cork* for an experiment in electricity, obſerved ſome appearances which induced him to believe, that the cork had been made electrical, by only cutting it with a penknife, and that on examination he found it really was ſo; I made the following experiment.

EXPERIMENT VIII.

I made a long cork perfectly dry, and held one end of it very near the fire, till it began to burn. At the ſame time, I held a ſmall, fine-toothed file, in the clear part of the fire, till that alſo, had become very dry, and rather hot. Then, having filed off the end of the cork, I applied it to a pair of neat, light pith-balls; when it attracted them both, and raiſed them perpendicularly, as high as the ſtrings would permit. Having electrified the balls by excited amber, the cork would increaſe their divergence from one, to near two inches; or it would repel them at an inch diſtance, ſo as to drive them one inch and an half out of the perpendicular. Electrifying the balls by excited glaſs, theſe appearances were directly reverſed. The cork therefore had parted with its electricity

to the file, and plainly acted as a negative electric.

EXPERIMENT IX.

Having neatly rounded off the corners of a piece of thin talc, about three inches ſquare; I coated both the ſides of it, within three quarters of an inch of the edges, with tin-foil, which I alſo rounded off at the corners. The talc, thus prepared, I obſerved would readily charge, without wiping, or drying the uncoated part; and the force of the ſhock, in the diſcharge, was really aſtoniſhing.

Having been ſhewn, by my late truly ingenious friend Mr. CANTON, an electric ſpark, of a very beautiful *crimſon colour*, which always appeared as it was drawn *over*, or *through*, a piece of ſmooth wood, *at the top of the conductor-ſtand*, and which was ſuppoſed by ſome gentlemen, to be the light of electricity, *very thinly ſpread* upon the ſurface of the wood: I was exceedingly deſirous to know from what cauſe this phænomenon really proceeded; and for that purpoſe made the following experiment.

EXPERIMENT X.

I fixed between two balls, introduced into the circuit of an electric diſcharge, a piece of ſmooth wainſcot, about two inches in diameter, and a quarter of an inch thick; when, upon making the diſcharge of a pretty large jar, I obſerved the wainſcot to be nearly covered with the electric light, the outer parts, or edges of the light, were exceedingly thin, but the colour *very white*; as it was alſo in ſeveral

other experiments, made with the ſame intent. I then procured a circular piece of coloured box, which was glued to the top of the ſtand to my prime-conductor; when, drawing ſtrong ſparks *through this wood* (of whatever colour it was), I became clearly of opinion, that *the colour of the ſpark varied according to its depth in the wood*; viz. if it paſſed upon the ſurface, it was white; a little below it, yellow, or orange; ſtill lower, ſcarlet; and, deeper in the wood, crimſon.

It having been mentioned, by ſome gentlemen, as their opinion, that the matter of light, and the electric matter, were the ſame thing(*n*); I made the following experiment, in order to determine whether there was any foundation for ſuch an opinion or not.

EXPERIMENT XI.

I inſulated the rubber of my machine, and placed it in ſuch a ſituation, that the rays of the ſun, paſſing through the open window of my room, might fall immediately upon it; but this I obſerevd produced no electricity. I then collected the rays into a *focus*, by means of a good convex glaſs, and threw them upon the back of the rubber, till it was burned quite black; but this method was attended with no better ſucceſs. I then mounted one of Mr. CANTON's electrometers, furniſhed with very light balls, upon a ſtand of ſealing-wax; and, having electrified them negatively, by excited amber, ſo as to diverge a full inch, I again

(*n*) Query, If this were really the caſe, ſhould not electrical experiments ſucceed, in the moſt perfect manner, *in the cleareſt light of the ſun?* and would not the evening, or night, be an exceedingly improper time to attempt making them?

collected the rays of the ſun by the convex glaſs, and held it at ſuch a diſtance as to bring the *focus* exactly upon the end of the box, which was burnt very black, and the glue in the joints melted; but the balls were not in the leaſt affected.

Extract from Mr. BOYLE's Continuation of HAWKESBY's Phyſico-mechanical Experiments.

"I took a large piece of good amber; and, "having in a ſummer-morning (while the air was "yet freſh) tried that it would not, without being "excited, attract a light body I had expoſed to it, "I removed it into the ſun's beams, till they had "made it moderately hot; and then I found, as I "expected, that it had acquired an attractive "virtue, and that not only in one particular "place, as it is uſually obſerved when it is ex-"cited by rubbing, but in divers and diſtant "places at once; at any of which it would draw "to it the light body placed within a convenient "diſtance from it; ſo that, in this climate of "ours, a ſolid body may quickly acquire an at-"moſphere by the preſence of the ſun, and that "long before the warmeſt part of the day."

ANOTHER.

"I took a little, but thick, veſſel of glaſs, and "held it near the fire till it had got a *convenient* "*degree of heat*, which was not very great, "though it exceeded that of the amber. I found, "as I imagined, that the heat of fire had made

"even

"even this body attractive, as that of the ſun "had made the amber."

REMARK.

If Mr. BOYLE, when the amber was heated, preſſed it ever ſo lightly againſt his hand, in order to try its warmth, (though without the leaſt friction), *he excited it*; and, *without this*, it was not electrical, *neither would it become ſo in cooling*. If the amber was *too hot*, the heated air about it would *conduct*. Therefore he was obliged to find *a convenient degree of heat*. This aſſertion may be proved in the following manner.

EXPERIMENT XII.

Hold a piece of amber near the flame of a candle, till it becomes hot; then apply it to a ſuſpended thread, and *it will not attract it*, neither will it become electrical *in cooling*; but preſs it ever ſo lightly on your hand, in order to try its heat, though without the leaſt friction, and *(if it be not too hot)* it will be electrical, and attract it violently. Heat it again at the candle, and its electricity ſhall be taken quite away. Preſs it again gently on your finger, or hand, and the power will be reſtored. Apply it again to the candle, it is loſt. And thus alternately. Other electrics may probably act in the ſame manner; as *the flame of a candle*, or *hot air*, will conduct away the electricity of glaſs, almoſt inſtantaneouſly.

EXPERIMENT

EXPERIMENT XIII.

Shewing Mr. NAIRNE the above-mentioned experiments; when the amber had been well-heated, and being preſented to a ſuſpended thread, having ſhewn no ſign at all of electricity; I held it, between my thumb and fore-finger, very near the table, but not ſo as to touch it, that we might entirely avoid friction. He then blew againſt it thirty blaſts, with a pair of kitchen-bellows; when preſenting it to the thread, it attracted it, at the diſtance of one-eighth of an inch. He then blew againſt it, thirty blaſts more, as above deſcribed; when applying it again to the thread, we ſaw it attracted, at half an inch diſtance; and, on drawing back the amber, it drew the thread after it, ſix, or eight inches. We repeated the experiment three times, with the like ſucceſs; and are ſatisfied, that the amber was made electrical *by the friction of the particles of air againſt its ſurface*; *and not in the leaſt by heating only*. We afterwards excited the amber, when it muſt have been perfectly cold, but dry, by only blowing againſt it as before.

The ſame proceſs ſucceeds with glaſs.

SECTION SIXTH.

Experiments and Obſervations on the Electricity of FOGS, &c. in purſuance of thoſe made by THOMAS RONAYNE, Eſq;

1771, Nov. 14. Half paſt eight, A. M. I find a fog, not very thick, pretty ſtrongly electrified. The balls ſeparate full half an inch. They keep ſtationary, there being little or no wind.

Nov. 19.

Nov. 19. The air is pretty ſtrongly electrified; but the wind is ſo very troubleſome, that I cannot determine the kind with certainty.

Dec. 2. Half paſt eight, A. M. a fog, moderately thick, is ſtrongly electrified. The balls diverge half an inch; but, if they are brought near the building, they cloſe, and open again on removing them. The mercury in the thermometer, is fifteen degrees, above the freezing point.

Dec. 18. Half paſt four, P. M. a moderately thick fog is ſtrongly electrified, ſoon after its appearance. The balls diverge, full half an inch, and regularly cloſe, at the approach of excited wax. The wind is troubleſome; but the balls keep their diſtance, and at intervals very well admit trying the experiment.

1772, Jan. 5. A fog is ſtrongly electrified poſitively. The balls diverge full half an inch. The air is ſharp, and froſty.

Jan. 13. Nine o'clock, A. M. a fog, not very thick, is ſtrongly electrified poſitively. The mercury in the thermometer, is ſeven degrees and an half above the freezing point. There is little or no wind.

Jan. 18. Ten o'clock, A. M. The air is pretty ſtrongly electrified by a fall of ſnow.

Jan. 21. Nine o'clock, A. M. I find the air ſtrongly electrified, during a fall of thin ſleet, a mixture, of ſnow, and rain, very gentle. The balls ſeparate three quarters of an inch, and remain ſtationary, there being little or no wind. N. B. The electricity in the air is poſitive.

Jan. 29.

Jan. 29. Nine o'clock, A. M. a very thick fog, and ſharp froſt. The air is ſo ſtrongly electrified *poſitively*, that the balls ſeparate *full an inch and a quarter*. There is little or no wind, and they remain ſtationary; ſo that the experiment may be made without the leaſt danger of a miſtake.

Twelve o'clock, the balls diverge as wide as at nine.

Three o'clock, P. M. the balls are exceedingly diſturbed by the wind; but, blow as it will, they ſtill keep at a great diſtance from each other. It freezes very hard. A quarter paſt four: the ſame as at three o'clock.

Half paſt five. The balls are ſtationary, *at three quarters of an inch diſtance, from each other*. The fog increaſes; and the rods are perfectly wet, from end to end. It is now too dark for further accurate obſervation.

Jan. 30. Nine o'clock, A. M. I find the air ſtrongly electrified *poſitively*, in a ſlight froſt, and thick fog. The balls ſeparate full half an inch: they are diſturbed by the wind; but it does not cloſe them; and the experiment is tried eaſily. There hath been a ſmall ſhower of ſnow, which lies thinly ſpread upon the houſes; and I have often ſuſpected (as I do now), that *this forms points*, and conducts the electricity faſter. The electricity continued the whole day.

Feb. 4. Nine o'clock, A. M. A ſharp froſt, and thick fog. The air is ſtrongly electrified *poſitively*: the balls diverge full three quarters of an inch. Eleven o'clock, A. M. The balls are ſtationary,

ſtationary, *at upwards of an inch diſtance.* They cloſe at the approach of excited wax.

Half paſt two, P. M. The ſame as at eleven o'clock. Three o'clock. Over-caſt and cloudy: the balls are very ſtill, but ſhew ſcarce any ſign of electricity.

Feb. 11. Eight o'clock, A.M. A thick fog is ſenſibly electrified *poſitively.* The balls diverge a quarter, or three eighths, of an inch. Wind S. W. and troubleſome. Thermometer 38. Barometer, 29,94.

Feb. 15. Half paſt ten, A.M. I find a thick fog, ſenſibly electrical. The balls diverge five-eighths of an inch, full. Preſently, after I had fixed my rod, there fell ſome ſmall drops of rain. *Upon the moment of its falling*, the balls increaſed their divergence near, or quite, a quarter of an inch. I never ſaw a fog more ſtrongly electrified when the weather was ſo warm, the mercury in my thermometer, in the open air, being ſeven and an half degrees above the freezing point. I ſuppoſe *higher in the atmoſphere* it is now uncommonly cold.

Fifty minutes paſt two, P. M. It ſnows very faſt. The air is now ſtrongly electrified *poſitively.* The balls ſeparate full three quarters of an inch. Wind S. W.

From the ſmall number of experiments I have been able to make, on the electricity of the atmoſphere, I cannot help being of opinion, that fogs are much more ſtrongly electrified *in*, or *immediately after, a froſt*, than at other times; and that the electricity in the fogs is often the ſtrongeſt,

ſoon after their appearance. I alſo now hold it for a certain rule, that, whenever there appears a thick fog, *and the air is at the ſame time ſharp and froſty*, that fog, is ſtrongly electrified *poſitively*. Though rain may not be *an immediate*, yet I am inclined to think it is by no means a very remote conſequence of electricity in the atmoſphere; and, from the trifling obſervations I have had an opportunity to make on that ſubject, I have not failed to find, that, in two or three days, after I have diſcovered the air to be ſtrongly electrified, (eſpecially if that electricity continued for as long a time), we have had rain, or other falling weather, and I incline to believe, more plentifully, in proportion to the ſtrength, and continuance, of the electricity; if not rain, ſnow, &c. according to the ſtate of the atmoſphere, with reſpect to heat and cold. If electricity be not a cauſe, I think it at leaſt *a prognoſtic*, of falling weather. But, for further ſatisfaction in this particular, I would recommend it to any gentleman curious in theſe enquiries, and having leiſure, to keep an electrical journal, upon a plan of the following kind.

Let a large book be provided, and ruled in the manner of a bill-book, uſed by tradeſmen. The columns ſo ruled may contain a collection of obſervations in the following order: Date and day; hour, latitude and longitude, or place; divergence of the balls; kind of electricity; variation of the needle; dip of the needle; barometer, thermometer, hygrometer, wind, weather, occaſional obſervations; to which may be added, the rain-gage, wind-gage, &c. Theſe things being carefully noted,

and

and kites being frequently raiſed, to the greateſt heights poſſible, *together with a thermometer* (o), in different ſtates of the weather, would probably ſoon throw new light upon this ſubject, and perhaps produce diſcoveries, of which we now have not the leaſt idea. From my experiments, and obſervations, on the electricity of fogs; I once imagined, that, whenever I ſaw a very thick one, and the air was at the ſame time ſharp, and froſty; that fog, was ſtrongly electrified poſitively: but I have met with ſeveral exceptions to this rule, for on Dec. 24, 25, 26, 27, 28, 29, 1772, in all which days there were thick fogs, thermometer from 36 to 33, wind conſtantly N. E. I could perceive no ſenſible electricity. Hence I conjectured, that though I could diſcover none *in my ſituation*, yet higher in the atmoſphere it might probably be found in plenty, and this conjecture was preſently afterward verified by Mr. NAIRNE, who obſerved the air to be electrical, when he ſtood in the golden gallery of St. Paul's cathedral, which is about two hundred and eighty feet in height, though he could diſcover none in the ſtone gallery, which is conſiderably lower: and the ſame obſervation hath ſince been made by others; and by myſelf, when I found the divergence of the balls to be increaſed, if I projected the rod, (from which they were ſuſpended) through one of the lights, in the lanthorn; which is ſtill higher. And it is with ſome pleaſure that I have ſince obſerved that Dr. LIND, and Mr. BRYDONE, have made the

(o) If Lord CHARLES CAVENDISH's can be raiſed properly, perhaps that may be preferable to any other.

ſame remark in raiſing their electrical kites (p). I have made many obſervations on the electricity of the atmoſphere, of which I kept a journal more than a year: but I have no deſire to publiſh it; as I hope the curious in theſe matters will be favoured with a much more accurate one, by gentlemen better qualified to undertake it.

It may not perhaps be improper to obſerve, that in the courſe of my experiments upon the electricity of fogs, I have frequently obſerved the balls to diverge *full two inches*: but this never happened except in a thick one; when the wind was S. W., and the mercury in the thermometer, under 40. In the inſtances I met with, where I could diſcoyer no electricity in ſuch a fog, though the mercury ſtood at 35 or 36, the wind was always N. E. The apparatus I uſed, conſiſted of a light rod about ſeven feet long, furniſhed with a box containing a pair of light cork balls, hung by linen threads, ſeven inches long. This rod was placed in a piece of wood, (over the top of one of the higheſt windows in the houſe, moſt remote from other buildings) properly fitted to receive it. The end of the rod, from which the balls were ſuſpended, was elevated to an angle of about forty-five degrees. Another rod, of equal length, was provided with a tin ſocket, into which went a long, ſubſtantial, ſtick of hard ſealing wax, or ſhell lac; which being excited, and projected out at the window, was brought near the balls; and thus readily, determined the kind of electricity in the atmoſphere.

(p) See Dr. PRIESTLEY's Hiſtory of Electricity, ſecond edition, p. 333. Experiments by Mr. DE ROMAS.

But in *an open ſituation*, ſuch an apparatus is unneceſſary, as one of Mr. CANTON's electrometers *having light balls*, ſucceeds very well, when held at the diſtance of about two feet from the body: the back of the obſerver being turned towards the wind. This method alſo ſerves to determine the kind of it, when there is a much larger quantity of electricity in the atmoſphere; and which would electrify the balls ſo ſtrongly, if held at the end of a long rod, high in the air; that excited wax, &c. being brought under them, would not produce the leaſt alteration in their divergence.

Notwithſtanding the balls hanging from the end of my rod, *in the open air*, have in ſome fogs diverged full two inches; yet I have never been able to make a pair of very light ones, *hanging from an inſulated conductor, in my room*, diverge in the leaſt, by means of the electricity collected from ſuch a fog, by a long fiſhing-rod, round which a fine pointed wire was twiſted, and made to communicate with the conductor. On mentioning this circumſtance to my worthy friend Dr. FRANKLIN; he deſired me to try whether having electrified the air *in one room*, I could by introducing the end of ſuch an inſulated rod into that air, make the balls diverge, when hanging at the oppoſite end of it, *in another room*. I have ſince tried the experiment in two rooms, *ſeparated by a paſſage, nine feet long*, in the following manner: I drew off the charge of a large jar, without ſucceſs; but having recharged it, and drawn it off a ſecond time; the balls hanging from the end of the rod, (upwards of twenty feet long) in the other room, diverged a

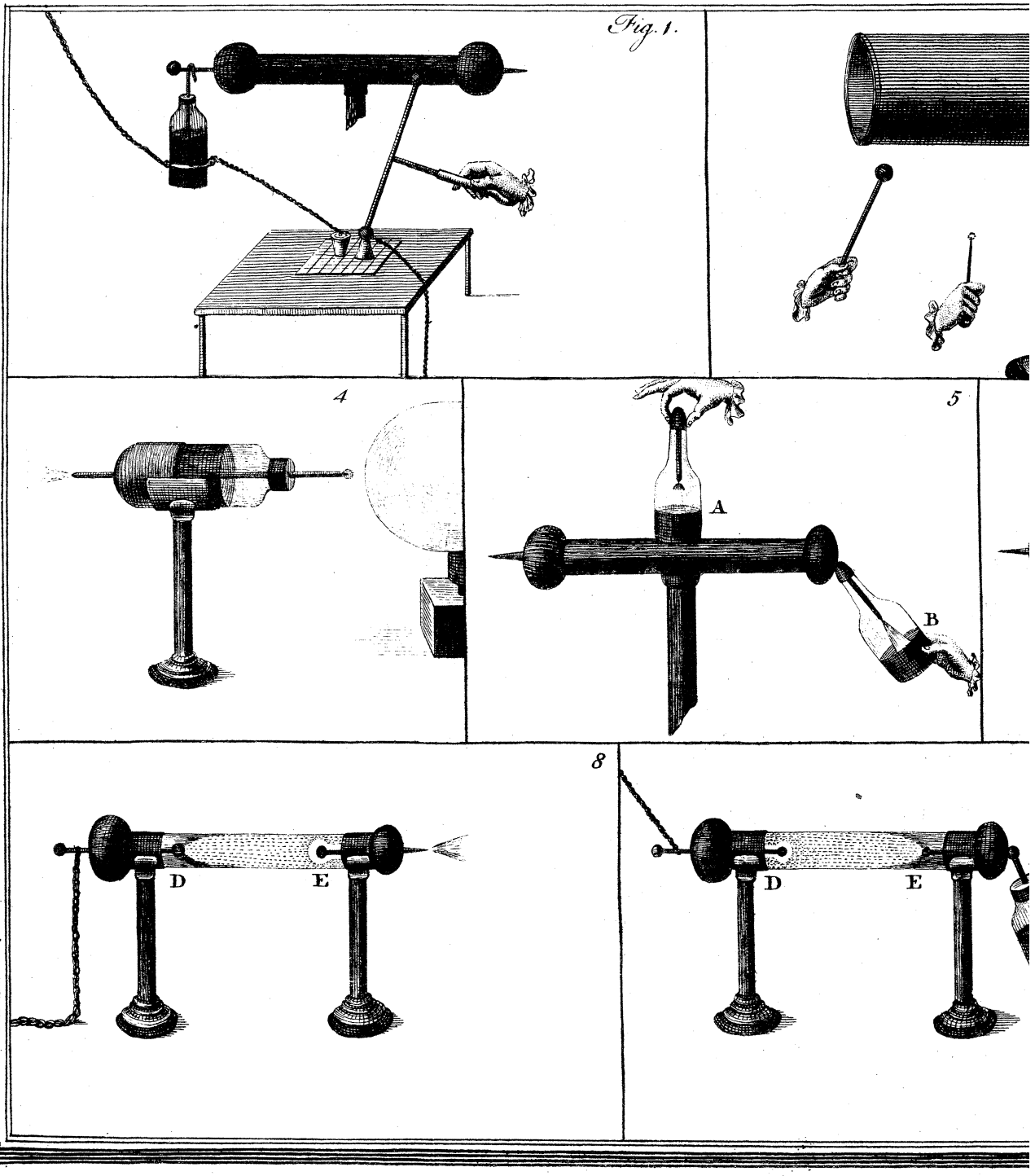
full

full inch; and I ſuppoſe the effect would have been greater, but there being a large fire in the room where the experiment was made, on opening the door, in order to introduce the pointed wire, which was twiſted round the inſulated rod, I apprehend, much of the electrified air was diſplaced, by the outer air preſſing in upon it; and driving it to the chimney, &c.

A pair of balls hanging from my hand, near the end of the rod, *in the electrified air*, diverged one inch and an half: but being held near the other end of the rod, *in the unelectrified air*; they diverged only half an inch. I then inſulated the rubber of my machine, and ſtuck a long ſharp-pointed needle in the back of it. Then hanging a chain from my prime-conductor to the table, I began to turn the winch; when the air of the room, the end of the rod, &c. were preſently affected: and the balls at the oppoſite end of it, in the other room, ſoon ſeparated conſiderably more than an inch.

I cannot prevail on myſelf to quit this pleaſing, yet difficult ſubject, without expreſſing a wiſh, that an electrical machine may be conſtructed, to work from ten, to fifty large cylinders, furniſhed with a prime-conductor, batteries, and other apparatus, proportionably large; the whole to be incloſed by brick walls, having ſlews quite round it, (like an hot-houſe, for botanic purpoſes) to keep the air in the room in a due temperature for experiment. With ſuch a machine as this, properly managed; new, unexpected, and wonderful diſcoveries might be

be made: and, to uſe the words of my learned and ingenious friend Dr. PRIESTLEY, they are not philoſophers who think no advantages could be gained by it. Electrical bells ſhould alſo be ſet up, to give notice of electricity, in the atmoſphere; and by a pair of light pith or cork balls, hanging by linen threads from the apparatus, the kind, continuance, and changes of the electricity ſhould be carefully noted. It would alſo be proper to put out occaſionally, a long ſtick, or the ſtrongeſt parts of a fiſhing-rod, having a box, with a pair of light cork balls hanging from thence, at the end of it, which would diſcover much ſmaller degrees of electricity in the air, than is ſufficient to ring bells; and by theſe, the kind of it may be readily aſcertained. Such a courſe of experiments as I have recommended in this paper, would ſoon throw new light upon the ſubject of electricity; hitherto, I believe, but little underſtood; though ſo intereſting to mankind, and ſo highly deſerving the niceſt inveſtigation, of the moſt curious enquirers into nature. But theſe purſuits can be properly attended to, by thoſe only, who are gentlemen of fortune, and leiſure: and could ſuch be prevailed on to undertake them, I have not the leaſt doubt but the exquiſite knowledge of *this ſecret part* of the operations of nature, which they would ſoon attain in the practice, would prove an ample reward of their labours; an honour to their country, and perhaps a benefit to the whole human race.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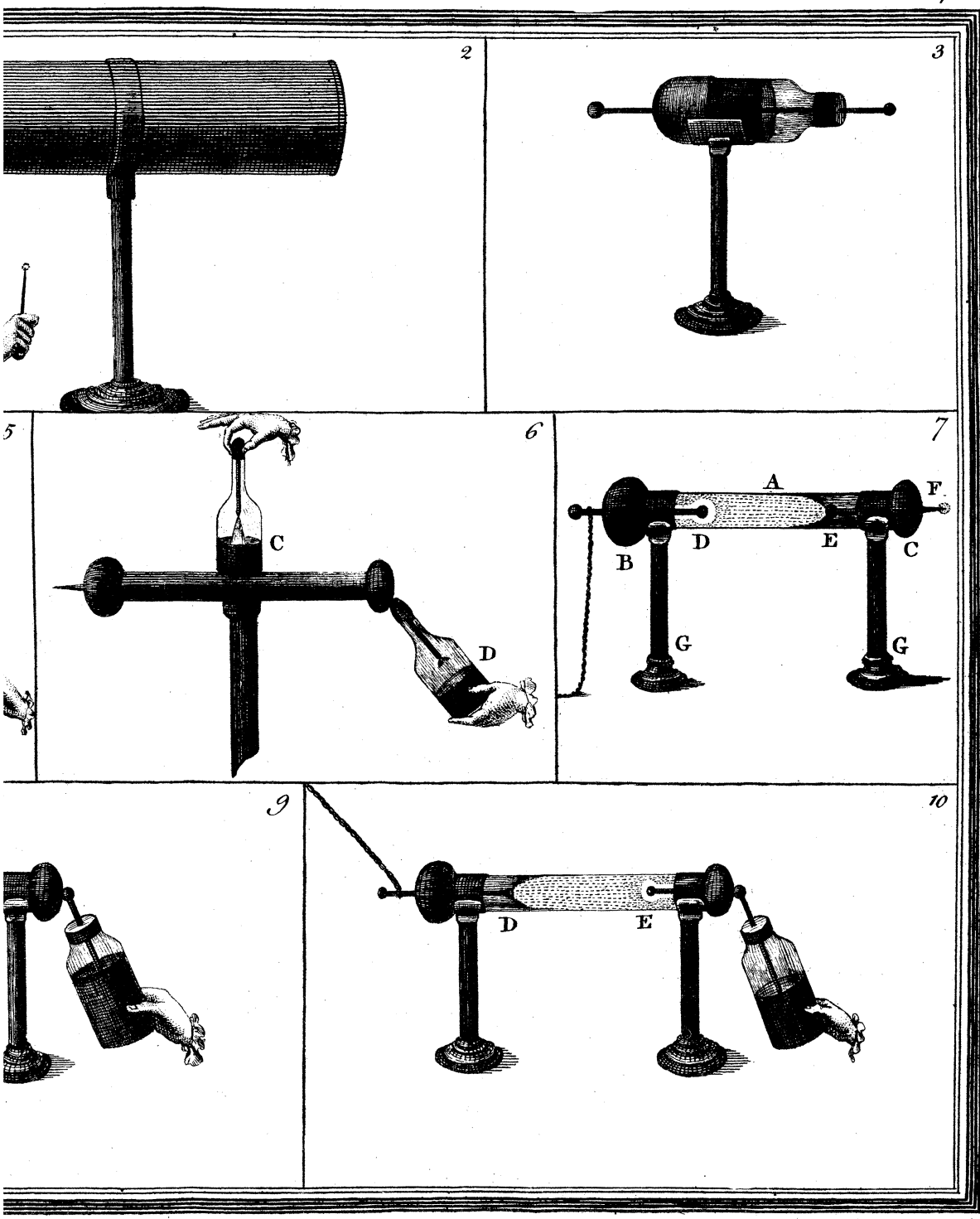

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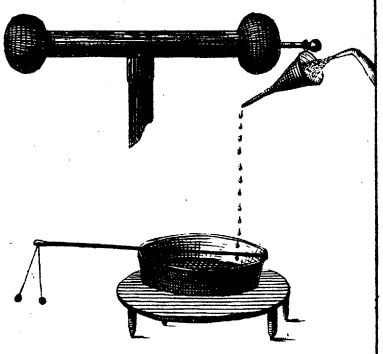

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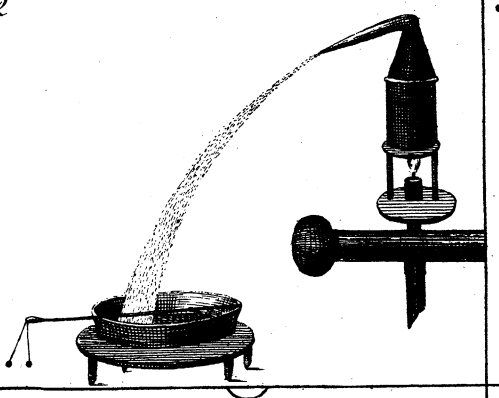

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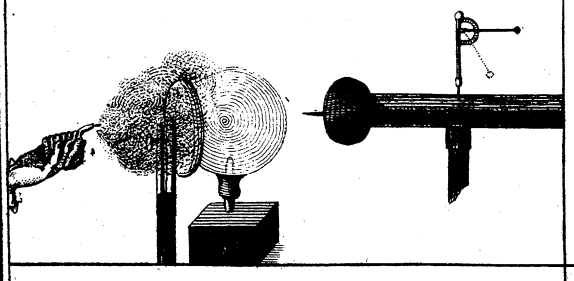

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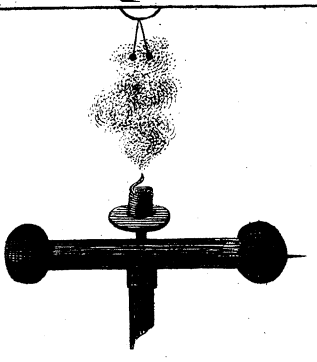

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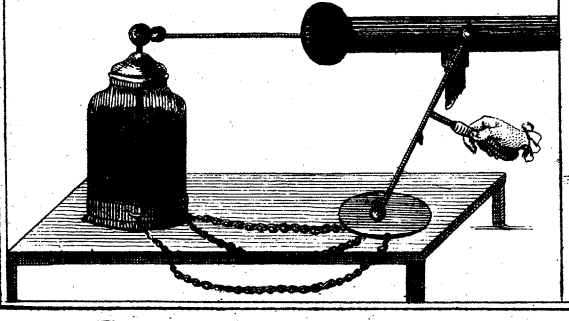

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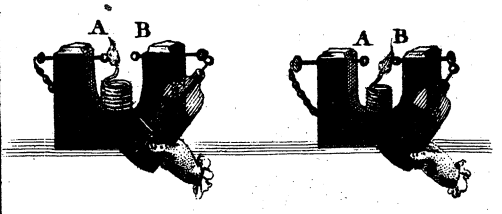

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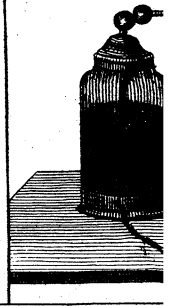

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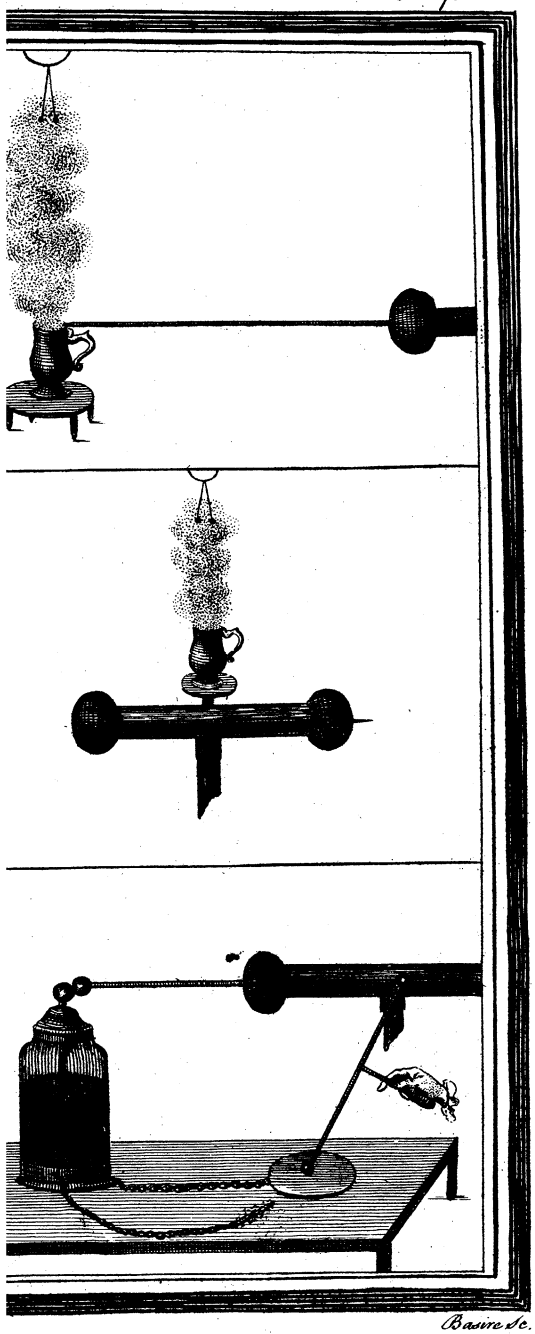

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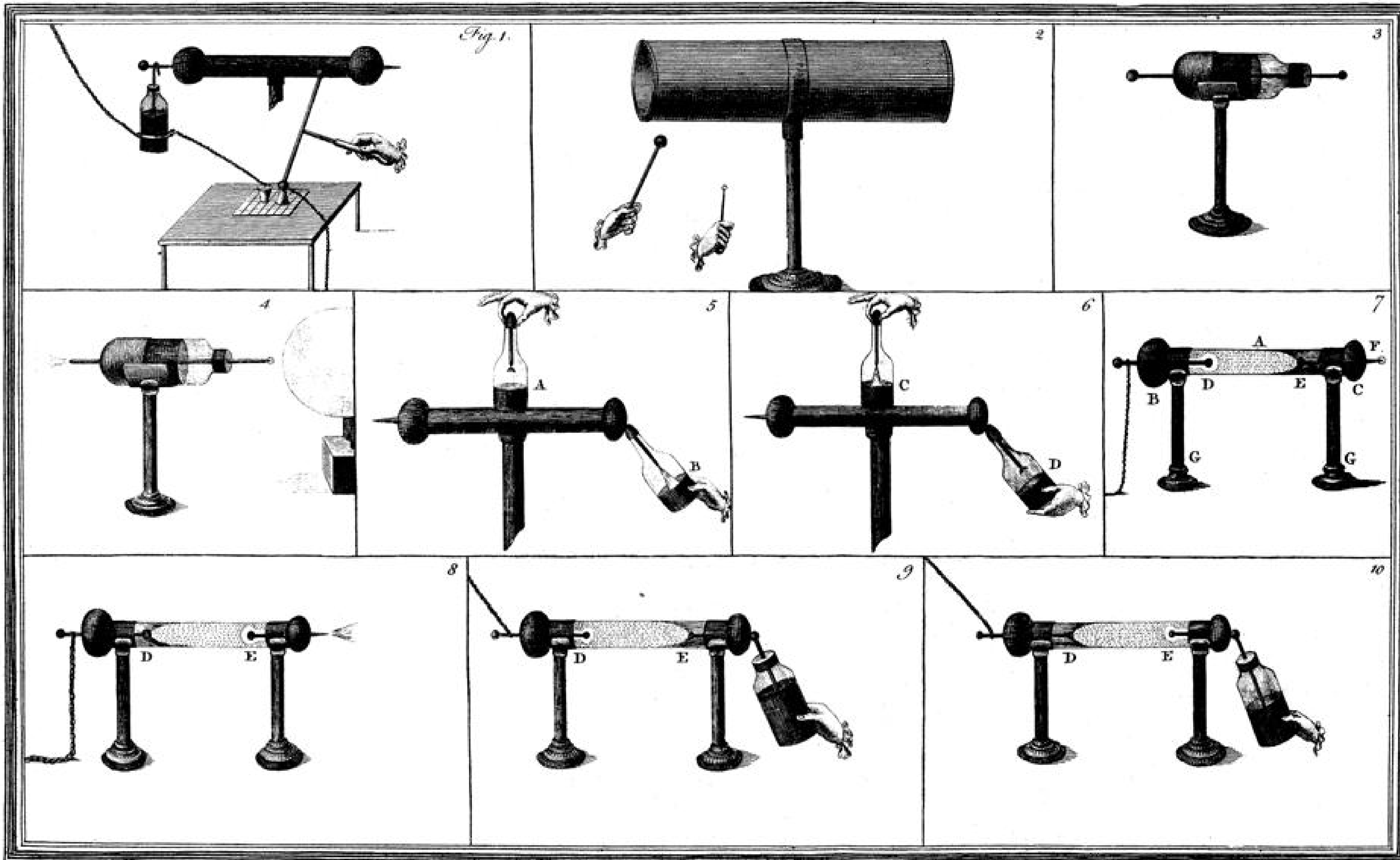

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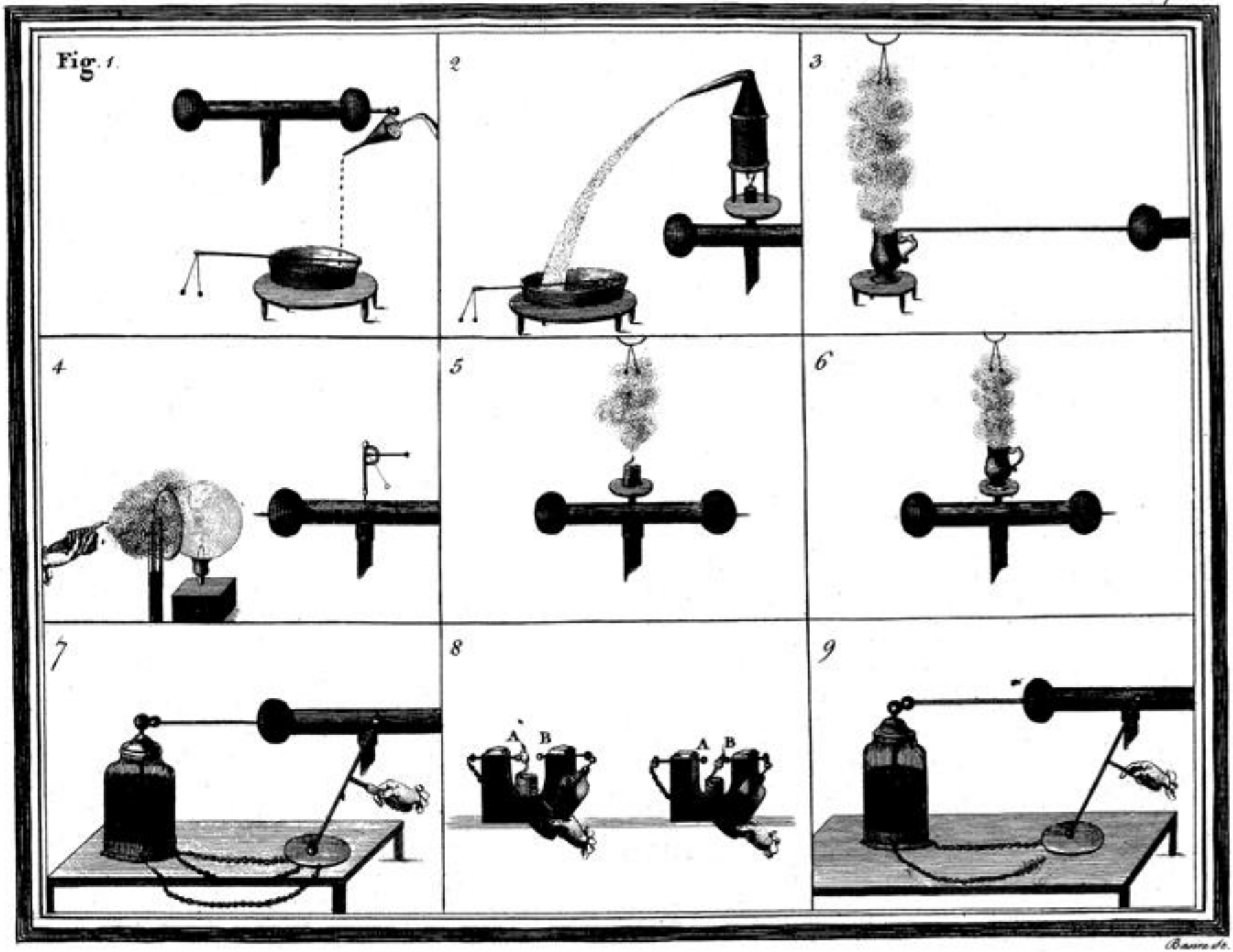
Fig. 1.
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