

**The history and present state of electricity, with original experiments /
[Joseph Priestley].**

Contributors

Priestley, Joseph, 1733-1804.

Publication/Creation

London : C. Bathurst & T. Lowndes, 1775.

Persistent URL

<https://wellcomecollection.org/works/bye9afdr>

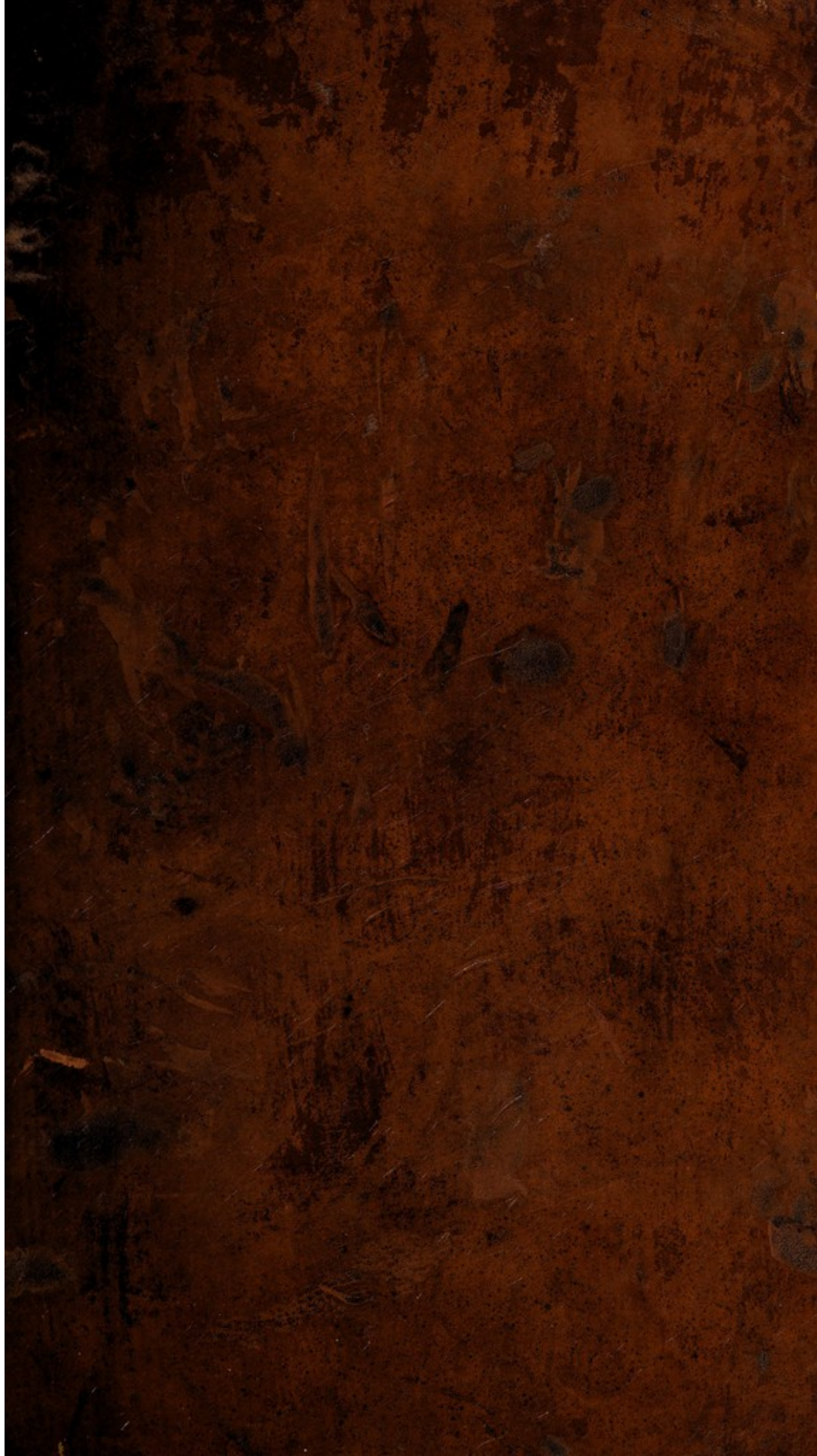
License and attribution

This work has been identified as being free of known restrictions under copyright law, including all related and neighbouring rights and is being made available under the Creative Commons, Public Domain Mark.

You can copy, modify, distribute and perform the work, even for commercial purposes, without asking permission.



Wellcome Collection
183 Euston Road
London NW1 2BE UK
T +44 (0)20 7611 8722
E library@wellcomecollection.org
<https://wellcomecollection.org>



5340

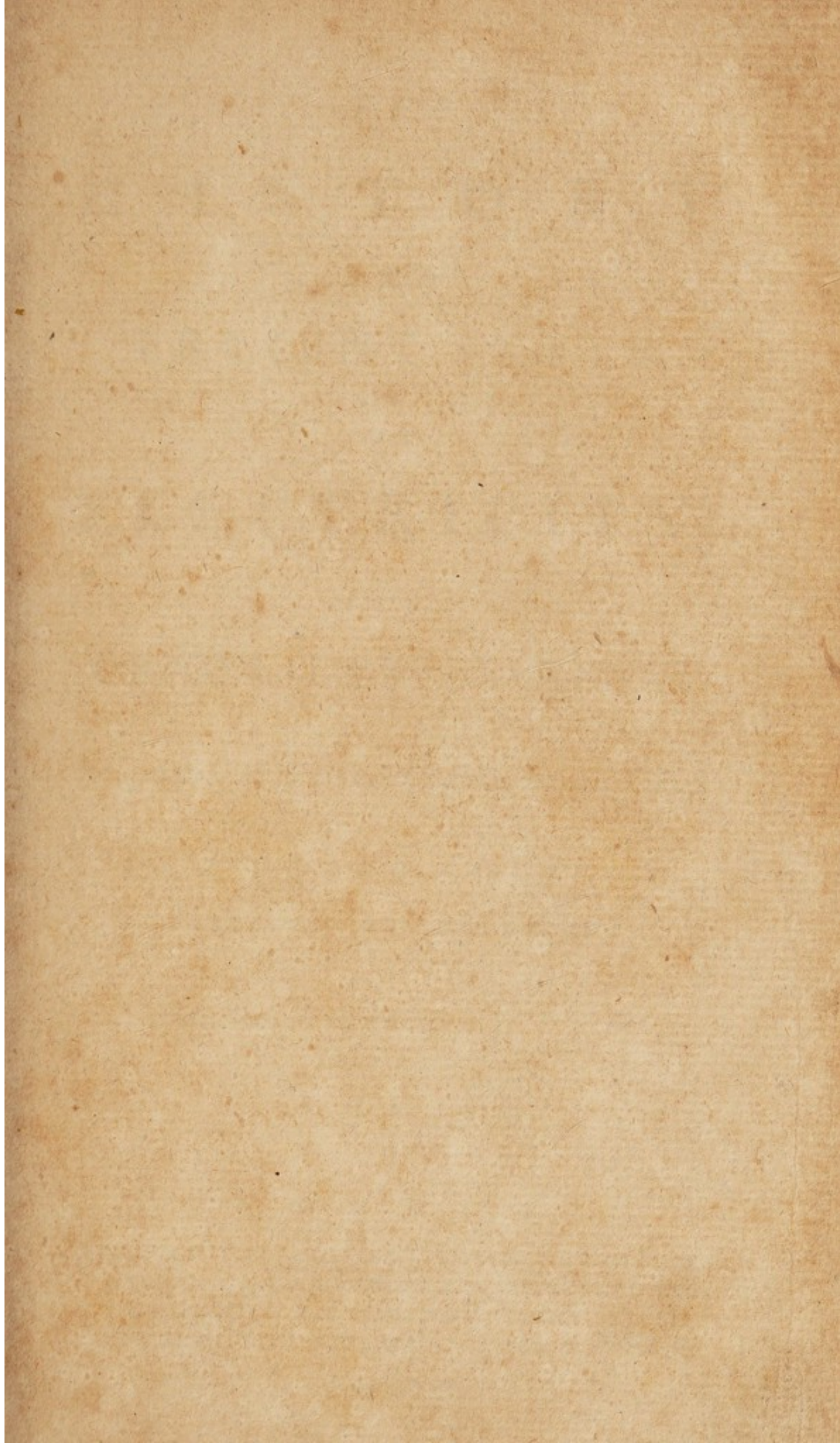
132

N. ix v

18

42175/B/2







70925

THE
HISTORY
AND
PRESENT STATE
OF
ELECTRICITY,

WITH
ORIGINAL EXPERIMENTS,
By JOSEPH PRIESTLEY, ESQ. F.R.S.

THE THIRD EDITION,
CORRECTED AND ENLARGED.

Causa latet, vis est notissima.

OVID.

VOL. I.

LONDON,

Printed for C. BATHURST, and T. LOWNDES, in Fleet-Street ;
J. RIVINGTON, and J. JOHNSON, in St. Paul's Church-Yard ;
S. CROWDER, G. ROBINSON, and R. BALDWIN, in Pater-
noster Row ; T. BECKET, and T. CADELL, in the Strand.

MDCCLXXV.

THE
HISTORY
AND
PRESENT STATE
OF
ELECTRICITY,
WITH



ORIGINAL EXHIBITS
BY JOSEPH H. WELLS, F.R.S.
A NEW EDITION
CORRECTED AND ENLARGED.
By
W. L. G. L. G.
LONDON
Printed by J. G. & J. G. in the Strand.
1841.

T O

THE RIGHT HONOURABLE

JAMES Earl of MORTON,

PRESIDENT OF THE ROYAL SOCIETY,

This HISTORY, &c.

I S,

WITH THE GREATEST RESPECT

INSCRIBED

By his LORDSHIP's

MOST OBEDIENT,

AND MOST HUMBLE

SERVANT,

JOSEPH PRIESTLEY.

T O

THE RIGHT HONOURABLE

JAMES EARL OF MORTON,

PRESIDENT OF THE ROYAL SOCIETY,

THIS HISTORY,

IS

WITH THE GREATEST RESPECT

INSCRIBED

BY HIS LORDSHIP

JOSEPH PRIESTLEY

AND JOHN HUNTER

SERVANTS

JOSEPH PRIESTLEY

T H E
P R E F A C E
T O T H E
F I R S T E D I T I O N.

I N writing the *History and Present State of Electricity*, I flatter myself that I shall give pleasure, as well to persons who have a taste for Natural Philosophy in general, as to electricians in particular; and I hope the work will be of some advantage to the science itself. Both these ends would certainly be answered in a considerable degree, were the execution at all answerable to the design.

THE History of Electricity is a field full of pleasing objects, according to all the genuine and universal principles of taste, deduced from a knowledge of human nature. Scenes like these, in which we see a gradual rise and progress in things, always exhibit a pleasing spectacle to the human mind. Nature, in all her delightful walks, abounds with such views, and they are in a more especial manner connected with every thing that relates to human life and happiness; things, in their own nature, the most interesting to us. Hence it is, that the power of association has annexed crowds of pleasing sensations to the contemplation of every object, in which this property is apparent.

THIS pleasure, likewise, bears a considerable resemblance to that of the sublime, which is one of the most exquisite of all those that affect the human imagination. For an object in which we see a perpetual progress and improvement is, as it were, continually rising in its magnitude; and moreover, when we see an actual increase, in a long period of time past, we cannot help forming an idea of an unlimited increase in futurity; which is a prospect really boundless, and sublime.

THE pleasures arising from views exhibited in *civil*, *natural*, and *philosophical* history, are, in certain respects, different from one another. Each has its advantages, and each its defects: and both their advantages and defects contribute to adapt them to different classes of readers.

CIVIL history presents us with views of the strongest passions and sentiments of the human mind, into which every man can easily and perfectly enter, and with such incidents, respecting happiness and misery, as we cannot help feeling, would alarm and affect us in a very sensible manner; and therefore, we are at present alarmed and affected by them to a considerable degree. Hence the pleasure we receive from civil history arises, chiefly from the exercise it affords our passions. The imagination is only entertained with scenes which occasionally start up, like interludes, or episodes, in the great drama, to which we are principally attentive. We are presented, indeed, with the prospect of
gradual

gradual improvement during the rise of great empires; but, as we read on, we are obliged to contemplate the disagreeable reverse. And the history of most states presents nothing but a tedious uniformity, without any striking events, to diversify and embellish the prospect. Besides, if a man have any sentiment of virtue and benevolence, he cannot help being shocked with a view of the vices and miseries of mankind; which, though they be not all, are certainly the most glaring and striking objects in the history of human affairs. An attention, indeed, to the conduct of divine Providence, which is ever bringing good out of evil, and gradually conducting things to a more perfect and glorious state, tends to throw a more agreeable light on the more gloomy parts of history; but it requires great strength of mind to comprehend those views; and, after all, the feelings of the heart too often overpower the conclusions of the head.

NATURAL history exhibits a boundless variety of scenes, and yet infinitely analogous to one another. A naturalist has, consequently, all the pleasure which the contemplation of uniformity and variety can give the mind; and this is one of the most copious sources of our intellectual pleasures. He is likewise entertained with a prospect of gradual improvement, while he sees every object in nature rising by due degrees to its maturity and perfection. And while new plants, new animals, and new fossils are perpetually

petually pouring in upon him, the most pleasing views of the unbounded power, wisdom, and goodness of God are constantly present to his mind. But he has no direct view of human sentiments and human actions; which, by means of their endless associations, greatly heighten and improve all the pleasures of taste.

THE history of philosophy enjoys, in some measure, the advantages both of civil and natural history, whereby it is relieved from what is most tedious and disgusting in both. Philosophy exhibits the powers of nature, discovered and directed by human art. It has, therefore, in some measure, the boundless variety with the amazing uniformity of the one, and likewise every thing that is pleasing and interesting in the other. And the idea of continual rise and improvement is conspicuous in the whole study, whether we be attentive to the part which nature, or that which men are acting in the great scene.

IT is here that we see the human understanding to its greatest advantage, grasping at the noblest objects, and increasing its own powers, by acquiring to itself the powers of nature, and directing them to the accomplishment of its own views; whereby the security, and happiness of mankind are daily improved. Human abilities are chiefly conspicuous in adapting means to ends, and in deducing one thing from another by the method of analogy; and where shall we find instances of greater sagacity, than in philosophers

THE PREFACE. v

sophers diversifying the situations of things, in order to give them an opportunity of showing their mutual relations, affections, and influences; deducing one truth and one discovery from another, and applying them all to the useful purposes of human life.

IF the exertion of human abilities, which cannot but form a delightful spectacle for the human imagination, give us pleasure, we enjoy it here in a higher degree than while we are contemplating the schemes of warriors, and the stratagems of their bloody art. Besides, the object of philosophical pursuits throws a pleasing idea upon the scenes they exhibit; whereas a reflection upon the real objects and views of most statesmen and conquerors cannot but take from the pleasure, which the idea of their sagacity, foresight, and comprehension would otherwise give to the virtuous and benevolent mind. Lastly, the investigation of the powers of nature, like the study of Natural History, is perpetually suggesting to us views of the divine perfections and providence, which are both pleasing to the imagination, and improving to the heart.

BUT though other kinds of history may, in some respects, vie with that of philosophy, nothing that comes under the denomination of history can exhibit instances of so fine a rise and improvement in things, as we see in the progress of the human mind, in philosophical investigations. To whatever height we have arrived in natural science, our be-

ginnings were very low, and our advances have been exceedingly gradual. And to look down from the eminence, and to see, and compare all those gradual advances in the ascent, cannot but give the greatest pleasure to those who are seated on the eminence, and who feel all the advantages of their elevated situation. And considering that we ourselves are, by no means, at the top of human science; that the mountain still ascends beyond our sight, and that we are, in fact, not much above the foot of it, a view of the manner in which the ascent has been made, cannot but animate us in our attempts to advance still higher, and suggest methods and expedients to assist us in our farther progress.

GREAT conquerors, we read, have been both animated, and also, in a great measure, formed by reading the exploits of former conquerors. Why may not the same effect be expected from the history of philosophy to philosophers? May not even more be expected in this case? The wars of many of those conquerors, who received this advantage from history, had no proper connection with former wars: they were only analogous to them. Whereas the whole business of philosophy, diversified as it is, is but one; it being one and the same great scheme, that all philosophers, of all ages and nations, have been conducting, from the beginning of the world; so that the work being the same, the labours of one are not only analogous to those
of

of another, but in an immediate manner subservient to them; and one philosopher succeeds another in the same field; as one Roman proconsul succeeded another in carrying on the same war, and pursuing the same conquests, in the same country. In this case, an intimate knowledge of what has been done before us cannot but greatly facilitate our future progress, if it be not absolutely necessary to it.

THESE histories are evidently much more necessary in an advanced state of science, than in the infancy of it. At present philosophical discoveries are so many, and the accounts of them are so dispersed, that it is not in the power of any man to come at the knowledge of all that has been done, as a foundation for his own inquiries. And this circumstance appears to me to have very much retarded the progress of discoveries.

NOT that I think philosophical discoveries are now at a stand. On the other hand, as quick advances seem to have been made of late years, as in any equal period of time past whatever. Nay, it appears to me, that the progress is really accelerated. But the increase of knowledge is like the increase of a city. The building of some of the first streets makes a great figure, is much talked of, and known to every body; whereas the addition of, perhaps, twice as much building, after it has been swelled to a considerable size, is not so much as taken notice of,

and may be really unknown to many of the inhabitants. If the additions which have been made to the buildings of the city of London, in any single year of late, had been made two or three centuries ago, it could not have escaped the observation of historians; whereas, now, they are so scattered, and the proportion they bear to the whole city is so small, that they are hardly noticed. For the same reason, the improvements that boys make at school, or that young gentlemen make at an academy, or the university, are more taken notice of than all the knowledge they acquire afterwards, though they continue their studies with the same assiduity and success.

THE history of experimental philosophy, in the manner in which it ought to be written, to be of much use, would be an immense work; but it were much to be wished, that persons who have leisure, and sufficient abilities, would undertake it in separate parts. I have executed it, in the best manner I have been able, for that branch which has been my own favourite amusement; and I shall think myself happy, if the attempt excite other persons to do the like for theirs.

I CANNOT help thinking myself to have been peculiarly fortunate, in undertaking the history of electricity, at the most proper time for writing it, when the materials were neither too few, nor too many to make a history; and when they were so scattered, as
to

to make the undertaking highly desirable, and the work peculiarly useful to Englishmen.

I LIKEWISE think myself exceedingly happy in my subject itself. Few branches of Natural Philosophy would, I think, make so good a subject for a history. Few can boast such a number of discoveries, disposed in so fine a series, all comprised in so short a space of time, and all so recent, the principal actors in the scene being still living.

WITH several of these principal actors it has been my singular honour and happiness to be acquainted; and it was their approbation of my plan, and their generous encouragement that induced me to undertake the work. With gratitude I acknowledge my obligations to Dr. Watson, Dr. Franklin, and Mr. Canton, for the books, and other materials with which they have supplied me, and for the readiness with which they have given me any information in their power to procure. In a more especial manner am I obliged to Mr. Canton, for those original communications of his, which will be found in this work, and which cannot fail to give a value to it, in the esteem of all the lovers of electricity. My grateful acknowledgements are also due to the Rev. Dr. Price, F. R. S. and to the Rev. Mr. Holt, our professor of Natural Philosophy at Warrington, for the attention they have given to the work, and for the many important services they have rendered me with respect to it.

x THE PREFACE.

To the gentlemen above mentioned the public is, likewise, indebted for whatever they may think of value in the *original experiments* which I have related of my own. It was from conversing with them that I was first led to entertain the thought of attempting any thing new in this way, and it was their example, and favourable attention to my experiments, that animated me in the pursuit of them. In short, without them, neither my experiments, nor this work would have had any existence.

THE historical part of this work, the reader, I hope will find to be full and circumstantial, and at the same succinct. Every new fact, or important circumstance, I have noted as it arose; but I have abridged all long details, and have carefully avoided all digressions and repetitions. For this purpose, I have perused every original author, to which I could have recourse; and every quotation in the margin points to the authority that I myself consulted, and from which the account in the text was actually taken. Where I could not procure the original authors, I was obliged to quote them at second hand, but the reference will always show where that has been done. That I might not misrepresent any writer, I have generally given the reader his own words, or the plainest translation I could make of them; and this I have done, not only in direct quotations, but where, by a change of person, I have made the language my own.

I MADE

T H E P R E F A C E. xi

I MADE it a rule to myself, and I think I have constantly adhered to it, to take no notice of the mistakes, misapprehensions, and altercations of electricians; except so far as, I apprehended, a knowledge of them might be useful to their successors. All the disputes which have no way contributed to the discovery of truth, I would gladly consign to eternal oblivion. Did it depend upon me, it should never be known to posterity, that there had ever been any such thing as envy, jealousy, or cavilling among the admirers of my favourite study. I have, as far as my best judgment could direct me, been just to the merits of all persons concerned. If any have made unjust claims, by arrogating to themselves the discoveries of others, I have silently restored them to the right owner, and generally without so much as giving a hint that any injustice had ever been committed. If I have, in any case, given a hint, I hope it will be thought, by the offending parties themselves, to be a very gentle one; and that it will be a *memento*, which will not be without its use.

I THINK I have kept clear of any mean partiality towards my own countrymen, and even my own acquaintance. If English authors are oftener quoted than foreign, it is because they were more easily procured; and I have found a difficulty I could not have expected, in procuring foreign publications upon this subject.

I FIND

I FIND it impossible to write a preface to this work, without discovering a little of the enthusiasm which I have contracted from an attention to it, by expressing my wishes, that more persons, of a studious and retired life, would admit this part of experimental philosophy into their studies. They would find it agreeably to diversify a course of study, by mixing something of action with speculation, and giving some employment to the hands and arms, as well as to the head. Electrical experiments are, of all others, the cleanest, and the most elegant, that the compass of philosophy exhibits. They are performed with the least trouble, there is an amazing variety in them, they furnish the most pleasing and surprising appearances for the entertainment of one's friends, and the expence of instruments may well be supplied, by a proportionable deduction from the purchase of books, which are generally read and laid aside, without yielding half the entertainment.

THE instruction we are able to get from books is, comparatively, soon exhausted; but philosophical instruments are an endless fund of knowledge. By philosophical instruments, however, I do not here mean the globes, the orrery, and others, which are only the means which ingenious men have hit upon, to explain their own conceptions of things to others; and which, therefore, like books, have no uses more extensive than the
views

views of human ingenuity ; but such as the air-pump, condensing engine, pyrometer, &c. (with which electrical machines are to be ranked) and which exhibit the operations of nature, that is of the God of nature himself, which are infinitely various. By the help of these machines, we are able to put an endless variety of things into an endless variety of situations, while nature herself is the agent that shows the result. Hereby the laws of her action are observed, and the most important discoveries may be made ; such as those who first contrived the instrument could have no idea of.

IN electricity, in particular, there is the greatest room to make new discoveries. It is a field but just opened, and requires no great stock of particular preparatory knowledge : so that any person who is tolerably well versed in experimental philosophy, may presently be upon a level with the most experienced electricians. Nay, this history shows, that several raw adventurers have made themselves as considerable, as some who have been, in other respects, the greatest philosophers. I need not tell my reader of how great weight this consideration is, to induce him to provide himself with an electrical apparatus. The pleasure arising from the most trifling discoveries of one's own, far exceeds what we receive from understanding the much more important discoveries of others ; and a mere reader has no chance of finding new truths, in comparison of him who now and then
amuses

amuses himself with philosophical experiments.

HUMAN happiness depends chiefly upon having some object to pursue, and upon the vigour with which our faculties are exerted in the pursuit. And, certainly, we must be much more interested in pursuits wholly our own, than when we are merely following the track of others. Besides, this pleasure has reinforcements from a variety of sources, which I shall not here undertake to trace; but which contribute to heighten the sensation, far beyond any thing else of this kind that can be experienced by a person of a speculative turn of mind.

IT is a great recommendation of the study of electricity, that it now appears to be, by no means, a small object. The electric fluid is no local, or occasional agent in the theatre of the world. Late discoveries show that its presence and effects are every where, and that it acts a principal part in the grandest and most interesting scenes of nature. It is not, like magnetism, confined to one kind of bodies, but every thing we know is a conductor or non-conductor of electricity. These are properties as essential and important as any they are possessed of, and can hardly fail to show themselves wherever the bodies are concerned.

HITHERTO philosophy has been chiefly conversant about the more sensible properties of bodies; electricity, together with chymistry, and the doctrine of light and colours, seems

seems to be giving us an inlet into their internal structure, on which all their sensible properties depend. By pursuing this new light, therefore, the bounds of natural science may possibly be extended, beyond what we can now form an idea of. New worlds may open to our view, and the glory of the great Sir Isaac Newton himself, and all his contemporaries, be eclipsed, by a new set of philosophers, in quite a new field of speculation. Could that great man revisit the earth, and view the experiments of the present race of electricians, he would be no less amazed than Roger Bacon, or Sir Francis, would have been at his. The electric shock itself, if it be considered attentively, will appear almost as surprising as any discovery that he made; and the man who could have made that discovery, by any reasoning *a priori*, would have been reckoned a most extraordinary genius: but electrical discoveries have been made so much by accident, that it is more the powers of nature, than of human genius, that excite our wonder with respect to them. But if the simple electric shock would have appeared so extraordinary to Sir Isaac Newton, what would he have said upon seeing the effects of a modern electrical battery, and an apparatus for drawing lightning from the clouds! What inexpressible pleasure would it give a modern electrician, were the thing possible, to entertain such a man as Sir Isaac for a few hours with his principal experiments!

To

To return from this excursion to the business of a preface: besides relating the history of electrical discoveries, in the order in which they were made, I thought it necessary, in order to make the work more useful, especially to young electricians, to subjoin a methodical treatise on the subject, containing the substance of the history in another form, with observations and instructions of my own. The particular uses of these parts of the work are expressed at large in the introductions to them. And, in the last place, I have given an account of such original experiments as I have been so fortunate as to hit upon myself.

I INTITLE the work the *History and Present State of Electricity*; and whether there be any more additions of the whole work or not, care will be taken to preserve the propriety of the title, by occasionally printing ADDITIONS, in the same size, as new discoveries are made; which will always be sold at a reasonable price to the purchasers of the book; or given *gratis*, if the bulk be inconsiderable.

CONSIDERING what respectable persons have already honoured this work with their valuable communications, I hope it will not be deemed arrogance in me, if I here advertise, that if any person shall make discoveries in electricity, which he would chuse to see recorded in this history, he will oblige me by a communication of them; and if they be really original, a proper place shall

cer-

certainly be assigned to them in the next edition, or paper of additions. And I hope that, if electricians in general would fall into this method, and make either a periodical, or occasional, but joint communication of their discoveries to the public, the greatest advantage would thence accrue to the science.

THE business of philosophy is so multiplied, that all the books of general philosophical transactions cannot be purchased by many persons, or read by any person. It is high time to *subdivide* the business, that every man may have an opportunity of seeing every thing that relates to his own favourite pursuit; and all the various branches of philosophy would find their account in this amicable separation. Thus the numerous branches of a large overgrown family, in the patriarchal ages, found it necessary to separate; and the convenience of the whole, and the strength, and increase of each branch were promoted by the separation. Let the youngest daughter of the science set the example to the rest, and show that she thinks herself considerable enough to make her appearance in the world without the company of her sisters.

BUT before this general separation, let each collect together every thing that belongs to her, and march off with her whole stock. To drop the allusion: let histories be written of all that has been done in every particular branch of science, and let the whole be seen

at one view. And when once the entire progress, and present state of every science shall be fully and fairly exhibited, I doubt not but we shall see a new and capital *æra* commence in the history of all the sciences. Such an easy, full, and comprehensive view of what has been done hitherto could not fail to give new life to philosophical inquiries. It would suggest an infinity of new experiments, and would undoubtedly greatly accelerate the progress of knowledge; which is at present retarded, as it were, by its own weight, and the mutual entanglement of its several parts.

I WILL just throw out a farther hint, of what, I think, might be favourable to the increase of philosophical knowledge. At present there are, in different countries in Europe, large incorporate societies, with funds for promoting philosophical knowledge in general. Let philosophers now begin to subdivide themselves, and enter into smaller combinations. Let the several companies make small funds, and appoint a director of experiments. Let every member have a right to appoint the trial of experiments in some proportion to the sum he subscribes, and let a periodical account be published of the result of them all, successful or unsuccessful. In this manner, the powers of all the members would be united and increased. Nothing would be left untried, which could be compassed at a moderate expence, and it being *one person's business* to attend to these experiments, they would be made, and reported without

without loss of time. Moreover, as all incorporations in these smaller societies should be avoided, they would be encouraged only in proportion as they were found to be useful; and success in smaller things would excite them to attempt greater.

I BY no means disapprove of large, general, and incorporated societies. They have their peculiar uses too; but we see by experience, that they are apt to grow too large, and their forms are too slow for the dispatch of the *minutiae* of business, in the present multifarious state of philosophy. Let recourse be had to rich incorporated societies, to defray the expence of experiments, to which the funds of smaller societies shall be unequal. Let their transactions contain a summary of the more important discoveries, collected from the smaller periodical publications. Let them, by rewards, and other methods, encourage those who distinguish themselves in the inferior societies; and thus give a general attention to the whole business of philosophy.

I WISH all the incorporated philosophical societies in Europe would join their funds (and I wish they were sufficient for the purpose) to fit out ships for the complete discovery of the face of the earth, and for many capital experiments which can only be made in such extensive voyages.

PRINCES will never do this great business to any purpose. The spirit of adventure seems to be totally extinct in the present race of merchants. This discovery is a grand deside-

ratum in science; and where may this pure and noble enthusiasm for such discoveries be expected but among philosophers, men uninfluenced by motives either of policy or gain? Let us think ourselves happy if princes give no obstruction to such designs. Let them fight for the countries when they are discovered, and let merchants scramble for the advantage that may be made of them. It will be an acquisition to philosophers if the seat of war be removed so far from the seat of science; and fresh room will be given to the exertion of genius in trade, when the old beaten track is deserted, when the old system of traffic is unhinged, and when new and more extensive plans of commerce take place. I congratulate the present race of philosophers on what is doing by the English court in this way*; for with whatever view expeditions into the South Seas are made, they cannot but be favourable to philosophy.

NATURAL PHILOSOPHY is a science which more especially requires the aid of wealth. Many others require nothing but what a man's own reflection may furnish him with. They who cultivate them find within themselves every thing they want. But experimental philosophy is not so independent. Nature will not be put out of her way, and suffer her materials to be thrown into all that variety of situations which philosophy requires, in order to discover her wonderful powers, with-

* Written in the year 1766.

out trouble and expence. Hence the patronage of the great is essential to the flourishing state of this science. Others may project great improvements, but they only have the power of carrying them into execution.

BESIDES, they are the higher classes of men which are most interested in the extension of all kinds of natural knowledge; as they are most able to avail themselves of any discoveries, which lead to the felicity and embellishment of human life. Almost all the elegancies of life are the produce of those polite arts, which could have had no existence without natural science, and which receive daily improvements from the same source. From the great and the opulent, therefore, these sciences have a natural claim for protection; and it is evidently their interest not to suffer promising inquiries to be suspended for want of the means of prosecuting them.

BUT other motives, besides this selfish one, may reasonably be supposed to attach persons in the higher ranks of life to the sciences; motives more exalted, and flowing from the most extensive benevolence. From Natural Philosophy have flowed all those great inventions, by means of which mankind in general are able to subsist with more ease, and in greater numbers upon the face of the earth. Hence arise the capital advantages of men above brutes, and of civilization above barbarity. And by these sciences also it is, that the views of the human mind itself are enlarged, and our common nature improved and ennobled. It is for the honour of the

species, therefore, that these sciences should be cultivated with the utmost attention.

AND of whom may these enlarged views, comprehensive of such great objects, be expected, but of those whom divine providence has raised above the rest of mankind. Being free from most of the cares peculiar to individuals, they may embrace the interests of the whole species, feel for the wants of mankind, and be concerned to support the dignity of human nature.

GLADLY would I indulge the hope, that we shall soon see these motives operating in a more extensive manner than they have hitherto done; that by the illustrious example of a few, a taste for natural science will be excited in many, in whom it will operate the most effectually to the advantage of science and of the world; and that all kinds of philosophical inquiries will, henceforward, be conducted with more spirit, and with more success than ever.

WERE I to pursue this subject, it would carry me far beyond the reasonable bounds of a preface. I shall therefore conclude with mentioning that sentiment, which ought to be uppermost in the mind of every philosopher, whatever be the immediate object of his pursuit; that speculation is only of use as it leads to *practice*, that the immediate use of natural science is the power it gives us over nature, by means of the knowledge we acquire of its laws; whereby human life is, in its present state, made more comfortable and happy; but that the greatest, and noblest use

use of philosophical speculation is the discipline of the heart, and the opportunity it affords of inculcating benevolent and pious sentiments upon the mind.

A PHILOSOPHER ought to be something greater, and better than another man. The contemplation of the works of God should give a sublimity to his virtue, should expand his benevolence, extinguish every thing mean, base, and selfish in his nature, give a dignity to all his sentiments, and teach him to aspire to the moral perfections of the great author of all things. What great and exalted beings would philosophers be, would they but let the objects about which they are conversant have their proper moral effect upon their minds! A life spent in the contemplation of the productions of divine power, wisdom, and goodness, would be a life of devotion. The more we see of the wonderful structure of the world, and of the laws of nature, the more clearly do we comprehend their admirable uses, to make all the percipient creation happy: a sentiment, which cannot but fill the heart with unbounded love, gratitude, and joy.

EVEN every thing painful and disagreeable in the world appears to a philosopher, upon a more attentive examination, to be excellently provided, as a remedy of some greater inconvenience, or a necessary means of a much greater happiness; so that, from this elevated point of view, he sees all temporary evils and inconveniences to vanish, in the glorious prospect of the greater good to which they are subservient. Hence he is able to venerate and

rejoice in God, not only in the bright sunshine, but also in the darkest shades of nature, whereas vulgar minds are apt to be disconcerted with the appearance of evil.

NOR is the cultivation of piety useful to us only as *men*, it is even useful to us as *philosophers*: and as true philosophy tends to promote piety, so a generous and manly piety is reciprocally, subservient to the purposes of philosophy; and this both in a direct and indirect manner. While we keep in view the great final cause of all the parts and the laws of nature, we have some clue, by which to trace the efficient cause. This is most of all obvious in that part of philosophy which respects the animal creation. As the great and excellent Dr. Hartley observes. “ Since this world is a
“ system of benevolence, and consequently its
“ author the object of unbounded love and a-
“ doration, benevolence and piety are our on-
“ ly true guides in our inquiries into it; the
“ only keys which will unlock the mysteries
“ of nature, and clues which lead through her
“ labyrinths. Of this all branches of natural
“ history, and natural philosophy afford abun-
“ dant instances. In all these inquiries, let the
“ inquirer take it for granted previously, that
“ every thing is right, and the best that can be
“ *ceteris manentibus*; that is, let him, with
“ a pious confidence, seek for benevolent
“ purposes, and he will be always directed to
“ the right road; and after a due continuance
“ in it, attain to some new and valuable truth;
“ whereas every other principle and motive of
“ examination, being foreign to the great plan
“ on

“on which the universe is constructed, must lead
“into endless mazes, errors, and perplexities*.”

WITH respect to the indirect use of piety, it must be observed, that the tranquility, and chearfulness of mind, which results from devotion forms an excellent temper for conducting philosophical inquiries; tending to make them both more pleasant, and more successful. The sentiments of religion and piety tend to cure the mind of envy, jealousy, conceit, and every other mean passion, which both disgrace the lovers of science, and retard the progress of it, by laying an undue bias upon the mind, and diverting it from the calm pursuit of truth.

LASTLY, let it be remembered, that a taste for science, pleasing, and even honourable as it is, is not one of the highest passions of our nature, that the pleasures it furnishes are even but one degree above those of sense, and therefore that temperance is requisite in all scientific pursuits. Besides the duties of every man's proper station in life, which ought ever to be held sacred and inviolate, the calls of piety, common friendship, and many other avocations ought generally to be heard before that of study. It is, therefore, only a small share of their leisure, that most men can be justified in giving to the pursuit of science; though this share is more or less, in proportion to a man's situation in life, his natural abilities, and the opportunity he has for conducting his inquiries.

I SHALL conclude with another passage from Dr. Hartley to this purpose. “Though the pur-

* Hartley's Observations on Man, Vol. ii. p. 245.

“ suit of truth be an entertainment and em-
 “ ployment suitable to our rational natures, and
 “ a duty to him who is the fountain of all
 “ knowledge and truth, yet we must make fre-
 “ quent intervals and interruptions; else the
 “ study of science, without a view to God and
 “ our duty, and from a vain desire of applause,
 “ will get possession of our hearts, engross
 “ them wholly, and by taking deeper root
 “ than the pursuit of vain amusements, be-
 “ come, in the end, a much more dangerous,
 “ and obstinate evil than that. Nothing can
 “ easily exceed the vain-glory, self-conceit,
 “ arrogance, emulation, and envy, that are
 “ found in the eminent professors of the sciences,
 “ Mathematics, Natural Philosophy, and even
 “ Divinity itself. Temperance in these studies
 “ is, therefore, evidently required, both in or-
 “ der to check the rise of such ill passions, and
 “ to give room for the cultivation of other es-
 “ sential parts of our natures. It is with these
 “ pleasures as with the sensible ones; our ap-
 “ petites must not be made the measure of
 “ our indulgence, but we ought to refer all
 “ to a higher rule.

“ BUT when the pursuit of truth is directed
 “ by this higher rule, and entered upon with a
 “ view to the glory of God, and the good of
 “ mankind, there is no employment more
 “ worthy of our natures, or more conducive to
 “ their purification and perfection *.”

* Hartley's Observations on Man, Vol. ii. p. 255, &c.

P R E F A C E

T O T H E

S E C O N D E D I T I O N.

TH E method I took to distinguish the books I had seen from those I had not seen, in the catalogue of electric authors, subjoined to the first edition of this work, has been attended with the advantage I promised myself from it; several persons, who were in possession of the books I had not seen, having communicated them to me; and I have carefully perused them, and digested their contents into this second edition. Far the greater part of these new authors, the reader will perceive by the catalogue, were German, and wrote in high Dutch, a language with which, I believe, the literati of this country are but little acquainted, which might be the reason why neither myself nor my friends had ever heard of them before. Though the new materials they have supplied cannot be said to be of the first importance, many of the articles are very curious, and I hope the reader, as well as myself, will think that they have well repaid me for my trouble in learning the language.

It is certainly much to be regretted that philosophers have not one common language; but neither the theory of language in general, nor the nature and analogies of things to be expressed

pressed by it are yet sufficiently understood, to enable us to contrive a new and philosophical one, which might be easily learnt, and would be completely adequate to all the purposes of science; and Latin is a language which persons of a philosophical turn of mind have seldom leisure to make themselves so much masters of, as to write in it with that elegance which the taste of the age requires. Besides, books written in Latin are but little read, at least, in England; and therefore could have no sale with us. These circumstances make it the more necessary, that there should be, in every country, persons possessed of a competent knowledge of foreign languages, who should be attentive to the progress of science abroad, and communicate to their countrymen all useful discoveries as they are made.

BESIDES the improvements in the history, and other parts of this work, the reader will find, in this edition, an addition of three intricate sections of original experiments. All that are of the least consequence are printed, and sold separately, for the benefit of those who purchased the first edition.

LEEDS, Jan. 1769.

ADVER-

P R E F A C E

T O T H E

T H I R D E D I T I O N.

TO the second edition of this work I made considerable additions, by an account of such discoveries as had been made in the interval between that and the first edition; and these *additions* I published separately for the use of those who had purchased the first edition. But in this edition I have inserted no account of any thing that was done after the publication of the second, because I reserve an account of them for a *Continuation of the History*, which, if God spare my life, I propose to write some years hence, when I hope there will be a greater stock of materials for it.

I HAVE, however, considerably improved this edition, by a fuller account of discoveries made by several foreigners, in consequence of becoming possessed of the original publications, whereas before I was obliged to content myself with quoting them at second hand. The reader will therefore find a much larger account of what was done by the Academicians del Cimento, by Mr. Du Fay, and some others. The alterations of the references, or the additions to them, will generally show where I have done this *.

* In the account of the experiments of Mr. Monnier, thinking proper to change the place of one of the articles, I neglected

To the account I have given of the reception of Dr. Franklin's system in France, I would add what I have since been informed of, viz. that Mr. Le Roi, secretary to the Royal Academy of Sciences, who has distinguished himself by his attention to various branches of philosophy, was the first who adopted this theory in that country, and became an open and strenuous advocate for it. He also demonstrated the principles of it by original experiments ; an account of which is contained in two valuable Memoirs published among those of the Royal Academy. In the former of these he proves, that there is an invariable distinction between the appearance of electric light at the points of metallic bodies as *connected with*, or *presented to* the prime conductor, or insulated rubber ; so that what is called the *pencil*, is uniformly the appearance when the pointed wire is electrified positively, and the *star* when it is negative. See Ac. Par. 1753. In another memoir for the year 1755, he shews, by using a globe of sulphur, that the resinous electricity of Mr. Du Fay is the very same thing with the negative power of Dr. Franklin. Though the same things had been demonstrated by others, and especially Father Beccaria, these philosophers made the discovery independent of each other, and therefore have equal merit.

ed to cancel the former account, which the reader will please to overlook, or expunge, from p. 145. The fuller account, from the original, is at p. 155. I mention this chiefly, that if any foreigner should translate from this edition, he may be admonished to omit the former paragraph.

WITHOUT entering into particulars, I shall take the liberty to acquaint the reader, that, in the sixtieth volume of the Philosophical Transactions, there are two papers of mine on electrical subjects, one on what I have called the *lateral explosion*, and the other on the *conducting power of charcoal*.

IN the former I shew that, in certain circumstances, an electric spark detaches itself from the circuit of an explosion to bodies placed near it, and returns to it again at the same instant. Through the air I have made this spark three-fourths of an inch in length, and in vacuo more than twelve inches.

IN the other paper I show, among other properties of charcoal, that its conducting power depends entirely on the degree of *heat* with which it is made. Some pieces do not conduct electricity at all, others as perfectly as silver or gold, and pieces of the former quality are always convertible into those of the latter, by the application of more heat.

ALSO, in my *observations on different kinds of air*, the reader will find, that I have demonstrated that the electric matter is, or contains phlogiston; by shewing that it affects all kinds of air as phlogiston does; particularly diminishing common air one fourth, and making it noxious, so as to make no effervescence with nitrous air.

IT may not be amiss to inform the reader, that the first translation of this work was into French, by a person who seems to have done it with no other view than to have an
 oppor-

opportunity of expressing his dislike of Dr. Franklin's system, and of myself as the abettor of it, and of defending that of Mr. Nollet. In the *notes*, which all other translators censure as in the highest degree illiberal, he represents me as being, beyond all bounds, partial to my own countrymen, and particularly unjust to the French; a charge from which I thought myself intirely exempt. I confess, however, that, inadvertently, I did give some handle to this censure in one passage, which I have therefore corrected in this edition. I have the pleasure to be informed that a new translation of this work, from this last edition, is undertaken by the excellent translator of Dr. Franklin's Philosophical writings, lately published. This gentleman will, I doubt not, do ample justice both to myself and to the subject.

CALNE, March 1775.

To the BINDER,

Place Plate I. *at the end of* Vol. I.

— Plate II. &c. *at the end of* Vol. II.

T H E
C O N T E N T S.

P A R T I.

THE HISTORY OF ELECTRICITY.

P E R I O D I.

*EXPERIMENTS and discoveries in electricity prior
to those of Mr. Hauksbee.* page 1

P E R I O D II.

The experiments and discoveries of Mr. Hauksbee. p. 19

P E R I O D III.

*The experiments and discoveries of Mr. Stephen Grey,
which were made prior to those of Mr. Du Fay,
and which bring the history of electricity to the year
1733.* p. 32

P E R I O D IV.

The experiments and discoveries of Mr. Du Fay. p. 54

P E R I O D V.

*The continuation, and conclusion of Mr. Grey's ex-
periments.* p. 69

P E R I O D VI.

The experiments of Dr. Defaguliers. p. 80

P E R I O D VII.

*Experiments of the Germans, and of Dr. Watson,
before the discovery of the Leyden phial in the year
1746.* p. 87

P E R I O D VIII.

The history of electricity, from the discovery of the Leyden phial in the year 1745 and 1746, to Dr. Franklin's discoveries page 102

SEC. I. *The history of the Leyden phial itself, till Dr. Franklin's discoveries relating to it.* ibid.

SEC. II. *The methods used by the French and English philosophers, to measure the distance to which the electric shock can be carried, and the velocity with which it passes.* p. 128

SEC. III. *Miscellaneous discoveries of Dr. Watson, and others, before the time of Dr. Franklin.* p. 140

SEC. IV. *Experiments on animal and other organized bodies in this period, and other experiments connected with them, made chiefly by the Abbé Nollet.* p. 167

SEC. V. *The history of the medicated tubes, and other communications of medicinal virtues by electricity, with their various refutations.* 179

P E R I O D IX.

The experiments and discoveries of Dr. Franklin. p. 191

SEC. I. *Dr. Franklin's discoveries concerning the Leyden phial, and others connected with them.* ibid.

SEC. II. *Dr. Franklin's discoveries concerning the similarity of lightning and electricity.* p. 204

SEC. III. *Miscellaneous discoveries of Dr. Franklin and his friends in America during the same period.* p. 222.

P E R I O D X.

The history of electricity, from the time that Dr. Franklin made his experiments in America, till the year 1766. p. 230

SEC.

- SEC. I. *Improvements in the electrical apparatus, with experiments and observations relating to it.*
page 232
- SEC. II. *Observations on the conducting power of various substances, and particularly Mr. Canton's experiments on air; and Signior Beccaria's on air and water.*
P. 241
- SEC. III. *Mr. Canton's experiments and discoveries relating to the surfaces of electric bodies, and others made in pursuance of them, or relating to the same subject; all tending to ascertain the distinction between the two electricities.*
P. 259
- SEC. IV. *Mr. Delaval's experiments relating to the two electricities, and his controversy with Mr. Canton upon that subject.*
P. 277.
- SEC. V. *Mr. Canton's experiments and discoveries relating to bodies immersed in electric atmospheres, with the discoveries of others, made by pursuing them.*
p. 286
- SEC. VI. *Mr. Symmer's experiments relating to the two electricities, and those made by Johannes Franciscus Cigna in pursuance of them.*
P. 308
- SEC. VII. *The history of the Leyden Phial continued.*
P. 334
- SEC. VIII. *Experiments and observations concerning electric light.*
P. 347
- SEC. IX. *The electricity of the Tourmalin.* p. 367
- SEC. X. *Discoveries that have been made since those of Dr. Franklin, with respect to the sameness of lightning and electricity.*
P. 381
- SEC. XI. *Observations on the general state of electricity in the atmosphere, and its more usual effects.*
P. 421.
- SEC.

SEC. XII. *The attempts that have been made to explain some of the more unusual appearances in the earth and heavens by electricity.* page 433

SEC. XIII. *Observations on the use of metallic conductors to secure buildings, &c. from the effects of lightning* p. 462

SEC. XIV. *Of medical electricity.* p. 472

SEC. XV. *Miscellaneous experiments and discoveries made within this period.* p. 490

THE
HISTORY
AND PRESENT STATE OF
ELECTRICITY.

PART I.
THE HISTORY OF ELECTRICITY.

PERIOD I.
EXPERIMENTS AND DISCOVERIES IN ELECTRICITY PRIOR TO THOSE OF MR. HAUKSBEE.

THE history of philosophy contains nothing earlier than the observation, that yellow amber, when rubbed, has the power of attracting light bodies. Thales of Miletus, the father of the Ionic philosophy, who flourished about six hundred years before Christ, was so much struck with this property of amber, that he imagined it was animated. But the first writer, who expressly mentions this substance, is Theophrastus, who flourished about the year 300 before Christ. He says, in his book concerning precious stones, sect. 53, that amber (which he supposes to be a native fossil) has the same property of attracting light bodies with the lyncurium; which, he observes, attracts not only straws, and small pieces of sticks, but even thin pieces
B of

2 THE DISCOVERIES

of copper and iron. What he says farther of the lyncurium will be related under the article of the *tourmalin*, which Dr. Watson has, in a manner, proved to be the same substance.

FROM *ηλεκτρον*, the Greek name for amber, is derived the term ELECTRICITY, which is now extended to signify not only the power of attracting light bodies inherent in amber, but other powers connected with it, in whatever bodies they are supposed to reside, or to whatever bodies they may be communicated.

THE attractive nature of amber is occasionally mentioned by Pliny, and other later naturalists; particularly by Gassendus, Kenelm Digby, and Sir Thomas Brown; but excepting the electricity of the substance called *jet**, the discovery of which was very late (though I have not been able to find its author) no advances were made in electricity till the subject was undertaken by William Gilbert, a native of Colchester, and a physician at London; who, in his excellent Latin treatise *de magnete*, published in the year 1600, relates a great variety of electrical experiments. Considering the time in which this author wrote, and how little was known of the subject before him, his discoveries may be justly deemed considerable, though they appear trifling when compared with those which have been made since his time.

To him we owe a great augmentation of

* Mr. Bosc is said to have shown, that the *agate* was very early known to have electric powers. Dantzick Memoirs, Vol. I. p. 179.

the list of electric bodies, as also of the bodies on which electrics can act; and he has carefully noted several capital circumstances relating to the manner of their action, though his theory of electricity was very imperfect, as might be expected.

AMBER and jet were, as I observed before, the only substances which, before the time of Gilbert, were known to have the property of attracting light bodies when rubbed; but he found the same property in the *diamond, sapphire, carbuncle, iris, amethyst, opal, vinctina, Bristol stone, beryl, and crystal*. He also observes that *glass*, especially that which is clear and transparent, has the same property; likewise all *factitious gems*, made of glass or crystal; *glass of antimony*, most *sparry substances*, and *belemnites*. Lastly, he concludes his catalogue of electric substances with *sulphur, mastic, sealing wax* made of gum lac tinged with various colours, *hard rosin, sal gem, tale*, and *roche alum*. Rosin, he said, possessed this property but in a small degree, and the three last mentioned substances, only when the air was clear and free from moisture.

ALL these substances, he observes, attracted not only straws, but all metals, all kinds of wood, stones, earth, water, oil; in short, whatever is solid, and the object of our senses. But he imagined that air, flame, bodies ignited, and all matter which was extremely rare was not subject to this attraction. Gross smoke, he found, was attracted very sensibly, but that which was attenuated very little.

4 THE DISCOVERIES

FRICITION, he says, is, in general, necessary to excite the virtue of these substances; though he had one large and smooth piece of amber which would act without friction. But with respect to this he probably deceived himself. The most effectual friction, he observed to be that which was light and quick; and he found that electrical appearances were strongest when the air was dry, and the wind north or east, at which time electric substances would act ten minutes after excitation. But he says, that a moist air, or a southerly wind almost annihilates the electric virtue. The same effect he also observed from the interposition of moisture of any kind, as from the breath, and many other substances, but not always from the interposition of sarsnet. He says that light and pure oil, sprinkled upon electrics, after excitation, did not obstruct their virtue; but that brandy, or spirit of wine, did. He also says, that crystal, talc, glass, and all other electrics lost their virtue after being burnt or roasted. But this was, in some measure, a mistake. The heat of the sun, collected by a burning glass, he says, is so far from exciting amber, and other electrics, that it impairs the virtue of them all; though, when electrics have been excited, they will retain their virtue longer in the sun-shine than in the shade.

MOST of the experiments of this author were made with long thin pieces of metal, and other substances, suspended freely on their centers, to the extremities of which he presented the electrics he had excited. His experiments

periments on water were made by presenting a round drop of it upon a dry substance to the excited electric; and it is remarkable, that he observed the same conical figure of the electrified drops which Mr. Grey afterwards discovered, and which will be related more at large in its proper place. Gilbert concluded, that air was not affected by electrical attraction, because the flame of a candle was not: for the flame, he says, would be disturbed if the air had the least motion given to it.

GILBERT imagined that electrical attraction was performed in the same manner as the attraction of cohesion. Two drops of water, he observed, rush together when they are brought into contact; and electrics, he says, are virtually brought into contact with the bodies they act upon, by means of their effluvia, excited by friction.

AMONG other differences between electric and magnetic attraction, some of which are very just, and others whimsical enough, he says, that magnetic bodies rush together mutually; whereas in electrical attraction it is only the electric that exerts any power. He observes also particularly, that in magnetism there is both attraction and repulsion, but in electricity only the former, and never the latter*.

SUCH were the discoveries of our countryman Gilbert, who may justly be called the father of modern electricity, though it be

* Gilbert de magnetē, Lib. 2. Cap. 2.

6 THE DISCOVERIES

true that he left his child in its very infancy.

LORD BACON, in his *Physiological Remains*, gives a catalogue of bodies attractive and not attractive; but it differs in nothing worth mentioning from that of Gilbert, and he does not seem to have made any observations of his own relating to the subject.

ABOUT 30 years after Gilbert, NICOLAUS CABÆUS, a Jesuit at Ferrara, repeated his experiments; and found that *white wax*, almost all the *gums*, and *crude gypsum* were to be ranked among electric bodies*.

THESE remarkable phenomena relating to amber, and other electric substances, did not escape the attention of the inquisitive and sagacious Mr. Boyle, who flourished about the year 1670. He made some addition to the catalogue of electric substances, and attended to some circumstances relating to electrical attraction, which had escaped the observation of philosophers who lived before him.

He found that the hard cake which remains after evaporating good turpentine was electrical, as also the hard mass which remains after distilling petroleum and spirit of nitre, glass of lead, the caput mortuum of amber, and the cornelian; but he could not find that property in the emerald, and he thought that glass possessed it but in a very low degree.

He found, that the electricity of all bodies capable of having it excited in them was in-

* Dantzick Memoirs, Vol. 1. p. 180.

creased by wiping, and warming them, previous to their being rubbed. By this means he made an electric body, no bigger than a pea, move a steel needle, which was freely poised, three minutes after he had left off rubbing it. He also found, that it was useful to have the surfaces of electric bodies made very smooth, except in the case of one diamond, on which he tried some experiments; which, though it was rough, was, he says, possessed of a stronger electric virtue than any polished one he had ever met with.

He observed that excited electrics would attract all kinds of bodies promiscuously, whether electric or not; that excited amber, for instance, would attract both powder of amber, and small pieces of it; differing, as he takes notice, from the property of the load-stone, which acts only on one kind of matter. He found, that his electrics would attract smoke very easily, and takes some pains to account for their not sensibly attracting flame, which Gilbert excepted from the bodies attracted by electricity.

THESE attractions, he found, did not depend upon the air: for he observed that they took place in vacuo. He suspended a piece of excited amber over a light body in a glass receiver, and saw, that when a vacuum was made, and the amber let down near the light body, it was attracted, as if it had been in the open air*. But S. Beccaria asserts, that

* Histoire de l'électricité.

8 THE DISCOVERIES

there is no electrical attraction in a perfect vacuum.

MR. BOYLE made an experiment to try whether an excited electric was acted upon by other bodies, as strongly as it acted upon them, and it succeeded: for, suspending his excited electric, he saw that it was sensibly moved by the approach of any other body. We should now be surprised that any person should not have concluded *a priori*, that if an electric body attracted other bodies, it must, in return, be attracted by them, action and reaction being universally equal to one another. But it must be considered, that this axiom was not so well understood in Mr. Boyle's time, nor till it was afterwards explained in its full latitude by Sir Isaac Newton*.

MR. BOYLE got a glimpse, as we may say, of the electric light: for he found that a curious diamond, which Mr. Clayton brought from Italy, gave light in the dark, when it was rubbed against any kind of stuff; and he found that, by the same treatment, it became electrical. He also observed the same property in several other diamonds†.

THESE experiments of Mr. Boyle's, we see, relate only to a few circumstances attending the simple property of electrical attraction. The nearest approach that he made to the discovery of electrical repulsion was his observing, that light bodies, as feathers, &c. would

* Boyle's Mechanical production of electricity.

† Secondat's history of electricity, p. 141.

cling to his fingers, and other substances, after they had been attracted by his electrics. He had seen but little of the electric light, and little imagined what astonishing effects would be afterwards produced by the same wonderful power, and how large a field he was opening for philosophical speculation in future times.

MR. BOYLE's theory of electrical attraction was, that the electric emitted a glutinous effluvium, which laid hold of small bodies in its way, and, in its return to the body which emitted it, carried them back with it. One James Hartman, whose account of amber is published in the Philosophical Transactions *, pretends to prove by experiment, that electrical attraction was really owing to the emission of glutinous particles. He took two electric substances, viz. pieces of colophonia, and from one of them made a distillation of a black balsam, and thereby deprived it of its attractive power. He says, that the electric, which was not distilled, retained its fatty substance, whereas the other was, by distillation, reduced to a mere *caput mortuum*, and retained no degree of its bituminous fat. In consequence of this hypothesis, he gives it as his opinion, that amber attracts light bodies more powerfully than other substances, because it emits oily and tenacious effluvia more copiously than they do.

CONTEMPORARY with Mr. Boyle was Otto

* Abridgement, Vol. 2. p. 473.

Guerieke, Burgomaster of Magdebourg, and the celebrated inventor of the air-pump, who is likewise intitled to a distinguished place among the first improvers of electricity.

THIS philosopher made his experiments with a globe of sulphur, made by melting that substance in a hollow globe of glass, and afterwards breaking the glass from off it. He little imagined that the glass globe itself, with or without the sulphur, would have answered his purpose as well. This globe of sulphur he mounted upon an axis, and whirled it in a wooden frame, rubbing it at the same time with his hand; and by this means he performed all the electrical experiments which were known before his time.

HIS was the discovery, that a body once attracted by an excited electric was repelled by it, and not attracted again till it had been touched by some other body. In this manner he kept a feather a long time suspended in the air above his sulphur globe; but he observed, that if he drove it near a linen thread, or the flame of a candle, it instantly retreated to the globe, without having been in contact with any sensible body.

NEITHER the sound, nor the light produced by the excitation of his globe, escaped the notice of this accurate philosopher, though he seems not to have observed them in a very great degree: for he was obliged to hold his ear near the globe to perceive the hissing sound of the electric fire; and he compares the light which it gave in the same circumstances

to that which is seen when sugar is pounded in the dark.

BUT the most remarkable experiments of this philosopher were two, which depend upon a property of the electric fluid that has not been illustrated till within these late years; viz. that bodies immersed in electric atmospheres are themselves electrified, and with an electricity opposite to that of the atmosphere. Threads suspended within a small distance of his excited globe, he observed to be often repelled by his finger brought near them, and that a feather repelled by the globe always turned the same face towards it, like the moon with respect to the earth. This last experiment seems to have been wholly overlooked by later electricians, though it is a very curious one, and may be made with so much ease*.

TO the members of the Academy *Del Cimento*, whose labours contributed very considerably to the advancement of various branches of natural knowledge, we are indebted for several observations on the subject of electricity.

THEY rank the electric bodies which they examined in the following order, according to the strength of their attractive power, *yellow amber, sealing wax, the rose diamond, and of the same strength with this the white sapphire, emerald, white topaz, spinelle, and ruby balleis*. After these they ranked all *trans-*

* Experimenta Magdeburgica, Lib. 4. Cap. 15.

parent gems, and next to the precious stones they placed *glass*, *crystal*, with *white and black amber*, the power of all which they say is very weak.

YELLOW amber appearing to them to have the greatest power of all electric substances, they made all their experiments with it. Among other things, they found that it attracted *smoke*, but not *flame*; and upon this occasion they observed the curious phenomenon of a *visible electric atmosphere*, which was afterwards re-discovered, and exhibited to more advantage by Dr. Franklin. For they say, that that part of the smoke which is attracted by the amber remains, and unites itself to it, like a small cloud, and as the amber cools, it rises in smoke again, and vanishes. At the same time they also observed a pretty curious effect of electrical repulsion; for they say that part of the smoke was thrown off from the amber, as from a looking-glass.

FLAME, they observed, was so far from being attracted by the amber, that, upon being presented to it, it presently deprived it of all its attractive power; for if, after it had taken up any thing, it was held to the flame, it would immediately let it go again.

THEY found that all *fluid substances* were sensible to the attractive power of amber, and among the rest even *mercury*; and that when the excited amber was presented to a large superficies of any liquor, it rose towards it in a small pointed eminence; an effect which, they say, is best observed in *oil* or *balsam*. This
observa-

observation, we shall find, was afterwards made again, and more particularly attended to by Mr. Grey *.

THESE gentlemen took a great deal of pains to try whether amber would attract in *vacuo*, but to no purpose, not being able to exclude the air, so as to rub the amber in *vacuo*, and apply it to the light body they had provided, with any effect; but neither could they make the amber act in the same confined situation even when the air was not at all excluded †.

THEY also found, with Mr. Boyle, that a piece of excited amber, suspended by a thread, was attracted by other bodies presented to it, just like a magnetical needle ‡.

LASTLY, these gentlemen found that when the excited amber was dipped in some liquors, it immediately lost its power, but not after being dipped in others. In this last class they enumerate several kinds of *oil*, *tallow*, *fat*, and *butter*, and it is now found that the conducting power of these substances is so small, that they are more properly classed among the non-conductors §.

I SHALL in the next place observe, that Mr. Boyle, Otto Guericke, and these gentlemen, made their experiments about the same time, and seemed to have derived no advantage whatever from each other's labours.

* Essays on Natural Experiments, translated by Walker, p. 128, &c.

† Ib. p. 43.

‡ Ib. p. 129.

§ Essays on Natural Experiments, translated by Walker, p. 131.

A MUCH finer appearance of electric light than that which Otto Guericke's sulphur globe exhibited was observed by Dr. Wall. The account of it is published in the Philosophical Transactions *.

MAKING experiments upon artificial phosphorus, which he took to be an animal oil coagulated with a mineral acid, he was led to conjecture that amber, which he supposed to be a mineral oil coagulated with a mineral volatile acid, might be a natural phosphorus; and with this view he began to make experiments upon it, the result of which, being very curious and surprising, it will be most agreeable to my readers to see in the very words of the observer himself.

“ I FOUND,” says he, “ by gently rubbing a well polished piece of amber with my hand, in the dark, that it produced a light: whereupon I got a pretty large piece of amber, which I caused to be made long and taper, and drawing it gently through my hand, being very dry, it afforded a considerable light.

“ I THEN used many kinds of soft animal substances, and found that none did so well as wool. And now new phenomena offered themselves: for, upon drawing the piece of amber swiftly through the woollen cloth, and squeezing it pretty hard with my hand, a prodigious number of little cracklings were heard, and every one of these

* Phil. Transf. abridged, Vol. 4. p. 275.

“ produced

“ produced a little flash of light; but when
“ the amber was drawn gently and slightly
“ through the cloth, it produced only a light
“ but no crackling; but by holding one’s
“ fingers at a little distance from the amber,
“ a large crackling is produced, with a great
“ flash of light succeeding it. And, what to
“ me is very surprising, upon its eruption, it
“ strikes the finger very sensibly, wheresoever
“ applied, with a push or a puff, like wind.
“ The crackling is full as loud as charcoal on
“ fire, and five or six cracklings, or more,
“ according to the quickness of placing the
“ finger, have been produced from one single
“ friction, light always succeeding each of
“ them.

“ Now I make no question, but upon
“ using a longer and larger piece of amber,
“ both the cracklings and light would be
“ much greater, because I never yet found
“ any crackling from the head of my cane,
“ though it is a pretty large one. This light
“ and crackling seems, in some degree, to re-
“ present thunder and lightning.”

AFTER reciting this experiment, he gives it as his opinion, that all, or most bodies, which have electricity, give light, and that it is the light which is the cause of their being electrical. He found that light could also be produced by rubbing jet, red sealing wax, made with gum lac and cinnabar, and the diamond. He even imagined he could distinguish true from false diamonds by this test.

NOTWITHSTANDING Dr. Wall made this
beautiful

beautiful discovery, as he imagined (for he seems not to have seen what Otto Guericke had written) of light proceeding from amber and other electric bodies, we see that he laboured under a great deal of confusion and misapprehension with respect to it. He says, that one thing appeared strange to him in the course of these experiments, which was, that though, upon friction with wool in the day time, the cracklings seemed to be full as many and as large, yet that, by all the trials which he made, very little light appeared, though in the darkest room. He says that the best time for making these experiments is when the sun is 18° below the horizon, and that when the sun was so low, though the moon shone ever so bright, the light was the same as in the darkest room, which made him chuse to call it a noctiluca.

It is remarkable that Dr. Wall compares the light and the crackling of his amber to thunder and lightning: so early was a similarity between the effects of electricity and lightning observed. But little was it imagined that the resemblance between them extended any farther than to appearances and effects. That the cause was the same in both, was a discovery reserved for Dr. Franklin, in a much later period.

THE great Sir Isaac Newton, though he by no means makes a principal figure in the history of electricity, yet made some electrical observations which engaged the attention of his philosophical friends; and which, independent

independent of their being made by him; do well deserve to be transmitted to posterity. They seem to shew, that he was the first who observed, that excited glass attracted light bodies on the side opposite to that on which it was rubbed.

HAVING laid upon the table a round piece of glass, about two inches broad, in a brass ring, so that the glass might be one eighth of an inch from the table, and there rubbing the glass briskly, little fragments of paper laid on the table, under the glass, began to be attracted, and move nimbly to and fro. After he had done rubbing the glass, the papers would continue a considerable time in various motions; sometimes leaping up to the glass, and resting there a while, then leaping down and resting there, and then leaping up and down again; and this sometimes in lines seemingly perpendicular to the table, sometimes in oblique ones; sometimes also leaping up in one arch, and leaping down in another, divers times together, without sensibly resting between; sometimes skipping in a bow from one part of the glass to another, without touching the table, and sometimes hanging by a corner, and turning often very nimbly, as if they had been carried about in the midst of a whirlwind, and being otherwise variously moved, every paper with a different motion. Upon his sliding his finger on the upper side of the glass, though neither the glass, nor the inclosed air below, were moved, yet he observed, that the papers, as they hung under

the glass, would receive some new motion, inclining this way or that, according as he moved his finger.

SOME of the motions, as that of hanging by a corner and twirling about, and that of leaping from one part of the glass to another without touching the table happened but seldom; but, he says, it made him take the more notice of them *.

AN account of this experiment Sir Isaac sent to the members of the R. Society in the year 1675, desiring it might be tried by them; and after some ineffectual attempts, and receiving further instructions how to make it, they at length succeeded, and the thanks of the society were formally returned to him †.

UPON repeating the experiment with some variety of circumstances, Sir Isaac observes, that rubbing variously, or with various things, altered the case. At one time he rubbed a glass four inches broad, and one fourth thick, with a napkin, twice as much as he used to do with his gown, and nothing would stir, and yet presently after, rubbing with something else, the motion soon began. After the glass had been much rubbed, he thought the motions were not so lasting, and the day following he found the motions fainter, and more difficult to be excited than at first ‡.

SIR ISAAC also mentions electricity in two queries annexed to his treatise on Optics, from

* Birch's Hist. of the R. Society, Vol. 3. p. 260, &c.

† Ib. 271.

‡ Ib. p. 270.

which we learn, that he imagined electric bodies when excited emitted an elastic fluid, which freely penetrated glass, and that the emission was performed by the vibratory motions of the parts of the excited bodies *.

P E R I O D II.

THE EXPERIMENTS AND DISCOVERIES OF
MR. HAUKS BEE.

AFTER Gilbert, Mr. Boyle, and Otto Guericke, Mr. Hauksbee, who wrote in the year 1709, distinguished himself by experiments and discoveries in electricity. He first observed the great electric power of glass, the light proceeding from it, and the noise occasioned by it, together with a variety of phenomena relating to electrical attraction and repulsion. He was indefatigable in making experiments, and there are few persons to whom we are more indebted for a real advancement of this branch of knowledge. This will appear from the following succinct account of his experiments, related not exactly in the order in which he has published them, but according to their connection. This method I have chosen, as best adapted to give the most distinct view of the whole.

I SHALL first relate the experiments he made concerning *electrical attraction and re-*

* Newton's Optics, octavo, p. 314, and 327.

pulsion, in many of which we shall see reason to admire his ingenious contrivances, and shall see that little was added to his observations, till the capital discovery of a *plus* and *minus* electricity by Dr. Watson and Dr. Franklin, and the farther illustration of that doctrine by Mr. Canton.

THE most curious of his experiments concerning electrical attraction and repulsion are those which shew the direction in which those powers are exerted.

HAVING tied threads round a wire hoop, and brought it near to an excited globe or cylinder, he observed, that the threads kept a constant direction towards the center of the globe, or towards some point in the axis of the cylinder, in every position of the hoop; that this effect would continue for about four minutes after the whirling of the globe ceased, and that the effect was the same whether the wire was held above or under the glass; or whether the glass was placed with its axis parallel, or perpendicular to the horizon.

He observed, that the threads pointing towards the center of the globe were attracted and repelled by a finger presented to them; that if the finger, or any other body, was brought very near the threads, they would be attracted; but that if it were brought to the distance of about an inch, they would be repelled, the reason of which difference he did not seem to understand*.

* Physico-Mechanical Experiments, p. 75.

HE tied threads to the axis of a globe and cylinder, and found that they diverged every way in straight lines from the place where they were tied, when the globe was whirled and rubbed. In both cases, he says, the threads would be repelled by the finger held on the opposite side of the glass, even without touching the glass; though they would sometimes suddenly jump towards it*. He observed farther, that by blowing with his mouth towards the glass, at three or four feet distance, the threads would have a very considerable motion given to them.

HE found that threads, hanging freely in an unexcited globe, at rest, would be moved by the approach of any excited electric at a considerable distance, except in moist weather; which failure he accounts for, by supposing the moisture on the surface of the glass prevented the free passage of the electric effluvia through it†.

THE varieties he observed on the appearances and properties of the *electric light*, are even more curious and surprising than his discoveries concerning electric attraction and repulsion; and it is something remarkable that Mr. Hauksbee's transition to electric light was like that of Dr. Wall, viz. from the light of phosphorus.

MR. HAUKS BEE first produced a considerable quantity of light by shaking quick-

* Physico-Mechanical Experiments, p. 78.

† lb. p. 160.

silver in a glass vessel, out of which the air was exhausted. Sometimes what he calls strange flashes of pale light were seen darting in a variety of directions, when the mercury was put in motion within an exhausted receiver*. But the discovery was probably accidental, and he did not seem, at that time, to know the reason of the appearance. He called this light the *mercurial phosphorus*, and did not consider the glass as any way concerned in producing it.

He also found, that this appearance of electric light (which he still calls the mercurial phosphorus) did not require a very perfect vacuum, nor even a near approach to it†. Nay he sometimes produced that appearance of light by shaking mercury in a vessel, in which the air was of the same density with the external atmosphere; but still he had no idea of the glass contributing to the phenomenon‡.

He observed a strong light in vacuo, and a small one in the open air, from rubbing amber upon woollen, but seems to have considered it as any hard body rubbing against a soft one||. He also observed a vivid purple, and afterwards a pale light produced by rubbing glass upon woollen in vacuo§. He says that every fresh glass first gave a purple, and then a pale light, and that woollen tintured

* Physico-Mechanical Experiments, p. 12.

† Ib. p. 14.

|| Ib. p. 26.

‡ Ib. p. 18.

§ Ib. p. 32.

with salt or spirits produced a new, strong, and fulgurating light*.

IN the following experiments we find his ideas of electric light much more distinct, and the appearances the same that are usually exhibited by our present electrical machines, the structure of which, we shall find to be nearly the same with those which he used.

HE provided himself with a machine in which he could whirl a glass globe; and he observed, when the air was extracted out of it, that, upon applying his hand to the globe, a strong light appeared on the inside, and, upon letting in the air, he observed the light on the outside also; but with some very considerable differences in its appearances, striking upon his fingers, and other bodies held near the globe. He also observed, upon this occasion, that one fourth of an atmosphere in the globe did very little diminish the light within. It is pleasing to observe, that the similar appearance in this experiment, and that with mercury in vacuo before-mentioned, made him suspect, though only suspect, that the light produced in the former case proceeded not from the mercury, but from the glass.

THE next experiment is of a very delicate and curious nature. It is not to be wondered at, that Mr. Hawksbee did not understand the circumstances which contributed to it, as

* Physico-Mechanical Experiments, p. 34.

the explication of it depends upon principles which were not discovered till a much later period by Mr. Canton.

HOLDING an exhausted globe within the effluvia of an excited one, he observed a light in the exhausted globe, which presently died away, if it was kept at rest; but which was revived; and continued very strong, if the exhausted globe was kept in motion. Presenting an exhausted tube to the effluvia of an excited globe, it produced what he calls an interrupted flashing light. He imagined that the exhausted globe was excited by the attraction of the effluvia from the other globe, so little did he understand the true cause of this curious experiment*. When he says that light is producible by the effluvia of one glass falling upon another, he adds; that electric (by which he means attractive) matter, is not to be brought forth by any such feeble strokes. He had before observed that, upon rubbing an exhausted tube, it discovered no attractive power, nor gave any light outwards, but only inwards.

He found that when the friction was performed in vacuo, no electricity (that is attraction) could be produced †; but that though the *attractive quality* required the presence both of the external and internal air, in order to its shewing itself, yet the *light* requires the presence of only one of them in order to its appearance; for that either a glass globe full

* Physico-Mechanical Experiments, p. 82.

† Ib. 242.

of air rubbed *in vacuo*, or with its air exhausted and rubbed *in pleno*, would produce a very considerable light*.

He says also, that those lights are less sensibly affected by the return of air, which are produced by the attrition of exhausted glass in pleno, than those which are produced by the attrition of glass full of air in vacuo; for that, in the former case, no great alteration was found in the light or colour, until a certain quantity of air was let into the inside of the exhausted glass; but that, in the latter case, both light and colour were sensibly changed, upon every admission of air to the outside of the full glass†.

The greatest electric light Mr. Hauksbee produced, was when he enclosed one exhausted cylinder within another not exhausted, and excited the outermost of them, putting them both in motion. Whether their motions conspired or not, he observed, made no difference. When the outer cylinder only was in motion, he says, the light was very considerable, and spread itself over the surface of the inner glass. What surprised him most was, that after both glasses had been in motion some time, during which his hand had been applied to the surface of the outer glass, the motion of both ceasing, and no light at all appearing; if he did but bring his hand again near the surface of the outer glass,

* Physico-Mechanical Experiments, p. 248.

† *ib.* p. 248.

there would be flashes of light, like lightning, produced in the inner glass; as if, he says, the effluvia from the outer glass had been pushed with more force upon it by means of the approaching hand*. This experiment was similar to those which he made with the excited and exhausted globe, and with the exhausted tube; and his reasoning upon it shews, that he was still far from being fully apprized of all the circumstances attending this fact.

THE next experiments which I shall relate of Mr. Hauksbee's, are those which shew the great copiousness, and extreme subtilty of electric light. They are really amazing, and have not yet been pursued in the manner they deserve.

HE lined more than half of the inside of a glass globe with sealing wax, and having exhausted the globe, he put it in motion; when, applying his hand to excite it, he saw the shape and figure of all the parts of his hand distinctly and perfectly, on the concave superficies of the wax within. It was as if there had only been pure glass, and no wax interposed between his eye and his hand. The lining of wax, where it was spread the thinnest, would but just allow the sight of a candle through it in the dark; but in some places the wax was, at least, one eighth of an inch thick; yet even in those places, the light and the figure of his hand were as distinguishable through it, as any where else. Nay

† Physico-Mechanical Experiments, p. 87.

though,

though, in some places, the sealing wax did not adhere so closely to the glass as in others, yet the light on these appeared just as on the rest *.

THESE experiments succeeded equally with pitch instead of sealing wax. And he observed, that when the air was let into the glass, every part of it, the lined part and the unlined, seemed to attract with equal vigour †. Melted flowers of sulphur had no such effect, but common sulphur answered as well as sealing wax, or pitch. In both these last experiments the sulphur was found to have been separated from the glass ‡.

USING a large quantity of common sulphur in the same manner, the light in the inside was four times as great, but the figure of his fingers was not so distinguishable as in the former cases. He likewise observed, that on the part near the axis, where the substance of the sulphur was the greatest, no light was produced; which he attributed, chiefly, to the slowness of the motion in that place §.

UPON the admission of a small quantity of air into the globe, thus partially lined with sealing wax, the light wholly disappeared on the part covered with the wax, but not on the other.

HE also observed, that when all the air was let in, and the hoop of threads before-

* Physico-Mechanical Experiments, p. 168.

† lb. 269.

‡ lb. 274.

§ lb. 275.

mentioned held over the glass, the threads were attracted at greater distances by the part which was coated with the wax than by the other: when all the air was exhausted, he says, the wax would attract bodies placed near the out-side of the glass; that even in this case, the threads preserved their central direction, though not so vigorously as when the air was let in; but that they would not be attracted at all, when there was no wax on the inside of the exhausted globe.

MR. HAUKEBEE was not unattentive to the *sound* made by the emission of the electric effluvia, or to the manner in which it affected the sense of *feeling*. He observed, that when an excited tube of glass attracted various bodies, and threw light upon them, as they were held near it, a noise, which he calls a *snapping*, was likewise heard. He also says, that the rubbed tube, held near the face, gave a feeling, as if fine hairs had been drawn over it; and when he repeated the experiment of whirling and rubbing the glass globe, he observed the light to proceed from it with some noise, and to make a kind of pressure upon the finger, when it was held within half an inch of it*.

NOR was Mr. Haukeesbee's attention confined to the electric power of *glass*. He made experiments with a globe of *sealing wax*, in the center of which was a globe of wood; from which he concluded, that the electricity

* Physico-Mechanical Experiments, p. 65.

of sealing wax was, in general the same with that of glass, but different from it in degree. He could not make any light adhere to his finger when presented to the excited sealing wax, any more than when it was presented to an exhausted and excited globe of glass.

He provided himself, in like manner, with a globe of sulphur, and another of rosin with a mixture of brick dust, but the sulphur could hardly be excited at all; whereas the rosin acted more powerfully than the sealing wax had done. This he ascribed to its being used while it was warm: for, in the same warm state, it attracted leaf brass without any attrition at all*.

He says, that the excited rosin gave no light in the dark, and the sulphur but little†.

With respect to the power of electricity in general, he observed, that a slight friction was sufficient to excite it, and that a greater pressure, or a more violent motion did not considerably increase it‡. He says, that all the phenomena of electricity were improved by warmth; and diminished by moisture; which he attributed to the resistance that the aqueous particles gave to the effluvia; and, like Mr. Boyle, and others before him, he was confirmed in this hypothesis, by finding, that the interposition of linen cloth prevented any effects from being observed beyond it.

* Physico-Mechanical Experiments, p. 154.

† Ib. p. 156.

‡ Ib. p. 54.

HE also observed, that when the tube was filled with other matter than air, as with dry writing sand (which he actually tried) the attractive power of the effluvia was considerably abated; but he did not know what kind of bodies would produce that effect. He himself observes, that he found the electric virtue of a solid cylinder of glass to be, not indeed quite so strong as that of a hollow tube, but more permanent*.

THAT Mr. Hauksbee, after all, had no clear idea of the distinction of bodies into electrics and non-electrics, appears from some of his last experiments, in which he attempted to produce electric appearances from metals, and from the reasons he gives for his want of success in those attempts. "From
 " these experiments," he says, " I may safely
 " conclude, that if there be any such
 " quality as light to be excited from a brass
 " body, under the forementioned circumstances," viz. of whirling and rubbing,
 " all the attrition of the several bodies I have
 " used for that purpose, have been too weak
 " to force it from it. And indeed, considering the closeness of the parts of metals,
 " and with what firmness they adhere, entangle, and attract one another, a small
 " degree of attrition is not sufficient to put
 " their parts into such a motion as to produce an electrical quality, which quality,
 " under the forementioned circumstances, I

* Physico-Mechanical Experiments, p. 64.

“ take to be the appearance of light in such a
 “ medium.”

CONSIDERING what great success Mr. Haukebee had with his globe of glass, and his machine to give motion to it, it is surprising that the use of it should have been so long discontinued after his death. To this circumstance we may perhaps, in a great measure, ascribe the slow progress that was afterwards made in electrical discoveries. Mr. Haukebee's successors confined themselves to the use of tubes. I suppose because they were lighter, more portable, and more easily managed in the experiments which they chiefly attended to: but the use of the globe would certainly have put them much sooner in the way of making the capital discoveries, which were afterwards made in electricity.

P E R I O D III.

THE EXPERIMENTS AND DISCOVERIES OF
MR. STEPHEN GREY, WHICH WERE
MADE PRIOR TO THOSE OF MONSIEUR DU
FAY, AND WHICH BRING THE HISTORY
OF ELECTRICITY TO THE YEAR 1733.

NOTWITHSTANDING the important discoveries of Mr. Hauksbee, and the promising appearance they made, as an opening to farther discoveries, we find, after him, a great chasm in the history of electricity, an interruption of discoveries, and, as far as we can learn, of experiments too, for the space of near twenty years; and at a time when philosophical knowledge of every other kind was making the most rapid progress, under the auspices of the great Sir Isaac Newton. But the attention of this great man happened to be engaged by other subjects, and this very circumstance might be the reason why the attention of other philosophers were also diverted from electricity.

AFTER this long interval, commences a new æra in the history of electricity; in which we shall have the works of another labourer in this new field of philosophy to contemplate, viz. Mr. Stephen Grey, a pensioner at the Charter House. No person who ever applied to this study was more assiduous in making experiments, or had his heart more intirely
in

in the work. This will appear by the prodigious number of experiments he made, and some considerable discoveries with which his perseverance was crowned; as well as by the self-deceptions, to which his passionate fondness for new discoveries exposed him.

BEFORE the year 1728, Mr. Stephen Grey had often observed, in electrical experiments made with a glass tube, and a down feather tied to the end of a small stick, that, after its fibres had been drawn towards the tube, they would, upon the tube's being withdrawn, cling to the stick, as if it had been an electric body, or as if there had been some electricity communicated to the stick, or to the feather. This put him upon thinking, whether, if the feather were drawn through his fingers, it might not produce the same effect, by acquiring some degree of electricity. This experiment succeeded accordingly, upon his first trial; the small downy fibres of the feather being attracted by his finger, when held near it; and sometimes the upper part of the feather with its stem would be attracted also.

It will be obvious to every electrician, that the success of this experiment depended upon other principles, than those to which he had a view in making it. Proceeding, however, in the same manner, he found the following substances to be all electric; *hair, silk, linen, woollen, paper, leather, wood, parchment, and ox gut* in which leaf gold had been beaten. He made all these substances very warm, and some of them quite hot before he rubbed them.

He found light emitted in the dark by the silk and the linen, but more especially by a piece of white *pressing paper*, which is of the same nature with card paper. Not only did this substance, when made as hot as his fingers could bear, yield a light; but, when his fingers were held near it, a light issued from them also, attended with a crackling noise, like that produced by a glass tube, though not at so great a distance from the fingers *.

THE preceding experiments bring us to the eve of a very considerable discovery in electricity, viz. the communication of that power from native electrics, to bodies, in which it is not capable of being excited; and also to a more accurate distinction of electrics from non-electrics. I shall relate the manner in which these important discoveries were made pretty fully, but, at the same time, as succinctly as possible.

IN the month of February, 1729, Mr. Grey, after some fruitless attempts to make metals attractive, by heating, rubbing, and hammering, recollected a suspicion which he had some years entertained; that, as a tube communicated its light to various bodies when it was rubbed in the dark, it might possibly, at the same time, communicate an electricity to them, by which had hitherto been understood only the power of attracting light bodies. For this purpose he provided himself with a tube three feet five inches long, and

* Philosophical Transactions abridged, Vol. viii. p. 9.

near one inch and two tenths in diameter; and to each end was fitted a cork, to keep the dust out when the tube was not in use.

THE first experiments he made upon this occasion were intended to try, if he could find any difference in its attraction when the tube was stopped at both ends by the corks, and when left entirely open; but he could perceive no sensible difference. It was, however, in the course of this experiment that, holding a down feather over against the upper end of the tube, he found that it would fly to the cork, being attracted and repelled by it, as well as by the tube itself. He then held the feather over against the flat end of the cork, and observed, that it was attracted and repelled many times together; at which, he says, he was much surpris'd, and concluded, that there was certainly an attractive virtue communicated to the cork by the excited tube.

He then fixed an ivory ball upon a stick of fir, about four inches long; when, thrusting the other end into the cork, he found, that the ball attracted and repelled the feather, even with more vigour than the cork had done, repeating its attractions and repulsions many times successively. He afterwards fixed the ball upon long sticks, and upon pieces of brass and iron wire, with the same success; but he observed, that the feather was never so strongly attracted by the wire, though it were held very near the tube, as by the ball at the end of it.

WHEN a wire of any considerable length was used, its vibrations, caused by the action of rubbing the tube, made it troublesome to manage. This put Mr Grey upon thinking whether, if the ball were hung to a packthread, and suspended by a loop on the tube, the electricity would not be carried down the line to the ball; and he found it to succeed according to his expectation. In this manner he suspended various bodies to his tube, and found all of them to be capable of receiving electricity in the same manner.

AFTER trying these experiments with the longest light canes and reeds that he could conveniently use, he ascended a balcony twenty-six feet high; and, fastening a string to his tube, he found, that the ball at the end of it would attract light bodies in the court below.

He then ascended to greater heights, and by putting his long canes in the end of his tube, and fastening a long string to the end of the canes, he contrived to convey the electricity to much greater distances than he had done before; till, being able to carry it no farther perpendicularly, he next attempted to carry it horizontally; and from these attempts arose a discovery, of which he was not in the least aware when he began them.

IN his first trial he made a loop at each end of a packthread, by means of which he suspended it, at one end, on a nail driven into a beam, the other end hanging downwards. Through the loop which hung down, he put
the

the line to which his ivory ball was fastened, fixing the other end of it by a loop on his tube; so that one part of the line, along which the electricity was to be conveyed, viz. that to which the ball was fastened, hung perpendicular, the rest of the line lay horizontal. After this preparation, he put the leaf brass under the ivory ball, and rubbed the tube, but not the least sign of attraction was perceived. Upon this he concluded, that when the electric virtue came to the loop of the packthread, which was suspended on the beam, it went up the same to the beam; so that none, or very little, of it went down to the ball; and he could not, at that time, think of any method to prevent it.

On June the 30th, 1729, Mr. Grey paid a visit to Mr. Wheeler, to give him a specimen of his experiments; when, after having made them from the greatest heights which the house would admit, Mr. Wheeler was desirous of trying whether they could not carry the electric virtue to a greater distance horizontally. Mr. Grey then told him of the fruitless attempt he had made to convey it in that direction: upon which Mr. Wheeler proposed to suspend the line to be electrified by another of *silk*, instead of *packthread*; and Mr. Grey told him, it might do better, on account of its smallness; as less of the virtue would probably pass off by it than had done by the thick hempen line, which he had used before. With this expedient, they succeeded far beyond their expectations.

THE first experiment they made after this expedient occurred to them, was in a matted gallery at Mr. Wheeler's house, July 2d, 1729, about ten o'clock in the morning, as Mr. Grey, after his usual manner, has minutely recorded it. About four feet from the end of the gallery, they fastened a line across the place. The middle part of this line was silk, the rest pack thread. They then laid the line to which the ivory ball was hung, by which the electric virtue was to be conveyed to it from the tube, and which was eighty feet and a half in length, across this silken line, so that the ball hung about nine feet below it. The other end of the line was, by a loop, fastened to the tube, which they excited at the other end of the room. After this preparation, they put the leaf brass under the ivory ball, and, upon rubbing the tube, it was attracted, and kept suspended for some time.

THE gallery not permitting them to go any greater lengths with a single line of communication, they contrived to return the line, making the whole length of it almost twice that of the gallery, or about one hundred and forty-seven feet, which answered very well. But, suspecting that the attraction would be stronger without doubling or returning the line, they made use of a line one hundred and twenty-four feet long, running in one direction in the barn; and, as they expected, they found the attraction stronger than when

when the line had been returned in the gallery.

JULY the 3d, proceeding to make more returns of the line, the silk which supported it happened to break, not being able to bear the weight of it, when shaken with the motion that was given to it by rubbing the tube. Upon this they endeavoured to support it by a small iron wire, instead of the silken string; but this also breaking, they made use of a brass wire a little thicker. But this brass wire, though it supported the line of communication very well, did not answer the purpose of these young electricians: for, upon rubbing the tube, no electricity was perceived at the end of the line. It had all gone off by the brass wire which supported it. They had recourse to brass wires, as being stronger than their silken lines, and no thicker; for the same reason that they had before used silken lines in preference to hempen strings; because they could have them stronger, and at the same time smaller. But the result of this experiment convinced them, that the success of it depended upon their supporting lines being *silk*, and not, as they had imagined, upon their being *small*. For the electric virtue went off as effectually by the small brass wire, as it had done by the thick hempen cord.

BEING obliged, therefore, to return to their silken lines, they contrived them to support very great lengths of the hempen line of communication; and actually conveyed the electric virtue seven hundred and sixty-five feet,

nor did they perceive that the effect was sensibly diminished by the distance*.

In the same manner in which *silk* was found to be a non-conductor, it is probable that, about the same time, *hair*, *rosin*, *glass*, and perhaps some other electric substances, were found to have the same property, though the discovery be no where particularly mentioned; for we shall presently find Mr. Grey making use of them, to insulate the bodies which he electrified.

AFTER this, Mr. Grey and his friend amused themselves with trying how *large* surfaces might be impregnated with the electric effluvia; electrifying a large map, table cloth, &c. They also carried the electric virtue several ways at the same time, and to a considerable distance each way.

THE magnetic effluvia, they found, did not in the least interfere with the electric; for when they had electrified the load stone, with a key hanging to it, they both attracted leaf brass like other substances.

SOME time after this, Mr. Wheeler, in the absence of Mr. Grey, electrified a red-hot poker, and found the attraction to be the same as when it was cold. He also suspended a live chicken upon the tube by the legs, and found the breast of it strongly electrical†,

* Phil. Trans. abridged, Vol. vii. p. 15.

† Ib. p. 16.

IN August, 1729, Mr. Grey advanced one step farther in his electrical operations. He found that he could convey the electric virtue from the tube to the line of communication without touching it, and that holding the excited tube near it was sufficient. Repeating his former experiments with this variation, in conjunction with Mr. Wheeler, and among others, carrying the electric virtue several ways at the same time without touching the line, they always observed, that the attraction was strongest at the place which was most remote from the tube; a fact which they might have observed, if they had attended to it, in their former experiments*.

SOME time in the same month, Mr. Wheeler and Mr. Grey in conjunction made some experiments, in order to try whether the electric attraction was in proportion to the quantity of matter in bodies; and with this view they electrified a solid cube of oak, and another of the same dimensions which was hollow; but they could not perceive any difference in their attractive power; though it was Mr. Grey's opinion, that the electric effluvia passed through all the parts of the solid cube†.

ON the 13th of August in the same year, Mr. Grey made another improvement in his electrical apparatus, by finding that he could electrify a *rod*, as well as a *thread*, without

* Phil. Transf. abridged, Vol. vii. p. 17.

† *Ib.* p. 17.

inserting any part of it into his excited tube. He took a large pole twenty-seven feet long, two inches and one half in diameter at one end, and one inch and one half at the other. It was a sort of wood which is called horse-beach, and had its rind on. This pole he suspended horizontally by hair lines, and at the small end of the pole he hung a cork, by means of a packthread about one foot long, and put a small leaden ball upon the cork, to keep the packthread extended. Then the leaf brass being put under the cork, the tube rubbed, and held near the larger end of the pole, the cork ball at the opposite end attracted the leaf brass strongly, to the height of an inch or more. Mr. Grey also observed, that though the leaf brass was attracted by any part of the pole, it was not near so strongly as by the cork *.

ABOUT the beginning of September, Mr. Grey made experiments, to shew that the electric effluvia might be carried in a circle, as well as along lines, and be communicated from one circle to another; and also that it might be done whether the circles were vertical, or horizontal.

ABOUT the latter end of autumn, or the beginning of winter 1729, Mr. Grey resumed his inquiries after other electrical bodies, and found many more to have the same property, but he mentions only the dry leaves of several trees; from whence he concluded, that

* Phil. Trans. abridged, Vol. vii. p. 18.

the leaves of all vegetables had that attractive virtue*.

WE are now advanced to a new scene of Mr. Grey's electrical experiments, viz. upon *fluids*, and upon *animal bodies*. Having no other method of trying whether any substances could have the electric virtue communicated to them, but by making them raise light bodies placed upon a stand under them, it may easily be imagined, that he could not well contrive to put a fluid body into that situation. The only thing that Mr. Grey could do in this way, was to make use of a bubble, in which form a fluid is capable of being held in a state of suspension. Accordingly on March 23d and 25th, 1730, he dissolved soap in Thames water, and suspending a tobacco pipe, he blew a bubble at the head of it; and, bringing the excited tube near the small end, he found the bubble to attract leaf brass to the height of two, and of four inches†.

APRIL the 8th, 1730, Mr. Grey suspended a boy on hair lines in a horizontal position, just as all electricians had, before, been used to suspend their hempen lines of communication, and their wooden rods; then, bringing the excited tube near his feet, he found that the leaf brass was attracted by his head with much vigour, so as to rise to the height of eight, and sometimes of ten inches. When the leaf brass was put under his feet, and the

* Phil. Trans. abridged, Vol. vii. p. 19.

† Ibid.

tube brought near his head, the attraction was small; and when the leaf brass was brought under his head, and the tube held over it, there was no attraction at all. Mr. Grey does not attempt to assign any reason for these appearances. It was not till many years after this time, that the influence of *points* in receiving and emitting the electric effluvia was observed. While the boy was suspended, Mr. Grey amused himself with making the electricity operate in several parts of his body, at the same time; and at the end of long rods, which he made him hold in his hands, and in diversifying the experiment several other ways*.

It is curious to observe the inference which Mr. Grey makes from these experiments. By them, says he, we see, that animals receive a greater quantity of electric fluid than other bodies; and that it may be conveyed from them several ways at the same time, to considerable distances. He had no idea that the bodies of animals receive electricity only by means of the moisture that is in them, and that this hempen line of communication, and his wooden rods could not have been electrified at all, if they had been perfectly dry.

In all these experiments Mr. Grey observed, that the leaf brass was attracted to a much greater height from the top of a narrow stand than from the table; and, at least, three times

* Phil. Transf. abridged, Vol. vii. p. 20.

higher than when it was laid on the floor of the room.

ABOUT this time Mr. Grey communicated to the Royal Society his suspicion, that bodies attracted more or less according to their *colour*, though the substance was the same, and the weight and size equal. He says, he found red, orange, and yellow attracted at least three or four times stronger than green, blue, or purple; but he forbore communicating a more particular account of them, till he had tried a more accurate method which, he says he had thought of, to make the experiments. The communication, however, was never made. The thing itself was only a deception, as will be shewn in some subsequent experiments made by Monsieur du Fay*.

MR. GREY, having found that he could communicate electricity to a bubble of soap and water, was encouraged to attempt communicating it to water itself. In order to this, he electrified a wooden dish full of water, placed on a cake of rosin, or a pane of glass, and observed, that if a small piece of thread, a narrow slip of thin paper, or a piece of sheet glass was held over the water, in an horizontal position, at the distance of an inch or something more, they would be attracted to the surface of the water and then repelled; but he imagined that these attractions and repulsions were not repeated so often as they would have been, if the body had been solid.

* Phil. Trans. abridged, Vol. vii. p. 22.

BUT he afterwards contrived to shew the effect of electricity upon water in another and more effectual manner. As this experiment was very curious and exhibited an appearance which was quite new to the electricians of those times, I shall relate the particulars of it very fully, and generally in Mr. Grey's own words*.

HE filled a small cup with water higher than the brim, and when he had held an excited tube over it, at the distance of about an inch or more, he says, that if it were a large tube, there would first arise a little mountain of water from the top of it, of a conical form; from the vertex of which there proceeded a light, very visible when the experiment was performed in a dark room, and a snapping noise, almost like that which was made when the finger was held near the tube, but not quite so loud, and of a more flat sound. Upon this, says he, immediately the mountain, if I may so call it, falls into the rest of the water, and puts it into a tremulous and waving motion.

WHEN he repeated this experiment in the sun shine, he perceived that very small particles of water were thrown from the top of the mountain; and that, sometimes, there would arise a fine stream of water from the vertex of the cone, in the manner of a fountain, from which there issued a fine steam or vapour, whose particles were so small as not

* Phil. Trans. abridged, Vol. vii. p. 23.

to be seen; yet, he says, he was certain it must be so; since the under side of the tube was wet, as he found when he came to rub it afterwards. He adds, that he had since found, that though there does not always arise that cylinder of water, yet that there is always a stream of invisible particles thrown on the tube, and sometimes to that degree as to be visible on it.

When some of the larger cups were used (his sizes were from three fourths to one tenth of an inch in diameter) which, he says were to be filled as high as could be done without running over; the middle part of the surface, which was flat, would be depressed, upon the approach of the tube, into a concave, and the parts towards the edge be raised; and that when the tube was held over against the side of the water, little conical protuberances of water issued out from it horizontally; and, after the crackling noise, returned to the rest of the water; and that sometimes small particles would be thrown off from it, as from the small portions of it above mentioned.

THIS last experiment he repeated with hot water, and found that it was attracted much more strongly, and at a much greater distance than before. The steam arising from the vertex was in this case visible, and the tube was sprinkled with large drops of water.

HE tried these experiments, in the same manner, with quicksilver, which was likewise raised up, but, by reason of its gravity, not to so great a height as the water: but, he says,
that

that the snapping noise was louder, and lasted much longer than it did with the water *.

It is not easy to know what to make of the next set of experiments which engaged the attention of Mr. Grey, or how far he deceived himself in the results of them. He fancied that he had discovered a *perpetual attractive power* in all electric bodies which did not require heating, rubbing, or any kind of attrition to be excited. The following experiments, he imagined proved the discovery.

He took nineteen different substances, which were either rosin, gum lac, shell lac, bees wax, sulphur, pitch, or two or three of these differently compounded. These he melted in a spherical iron ladle, except the sulphur, which was best done in a glass vessel. When these were taken out of the ladle, and their spherical surfaces hardened, he says, they would not attract till the heat was abated, or till they came to a certain degree of warmth; that then there was a small attraction, which increased till the substance was cold, when it was very considerable †.

THE manner in which he preserved these substances in a state of attraction, was by wrapping them in any thing which would keep them from the external air. At first, for the smaller bodies, he used white paper, and for the larger ones, white flannel; but he afterwards found that black worsted stockings

* Phil. Trans. abridged, Vol. vii p. 24.

† Ibid.

would do as well. Being thus cloathed, he put them into a large firm box; where they remained, till he had occasion to make use of them.

He observed these bodies for thirty days, and found that they continued to act as vigorously as on the first or second day; and that they retained their power till the time of his writing; when some of them had been prepared above four months.

He makes the most particular mention of a large cone of stone sulphur, covered with a drinking glass in which it was made; and says, that whenever the glass was taken off, it would attract as strongly as the sulphur, which was kept covered in the box. In fair weather, the glass would attract also, but not so strongly as the sulphur, which never failed to attract, let the wind or the weather be ever so variable; as would all the other bodies, only in wet weather, the attraction was not so great as in fair weather.

He also mentions a cake of melted sulphur, which he kept without any cover, in the same place with the body abovementioned, and where the sun did not shine upon them; and says, that it continued to attract till the time of his writing; but that its attraction was not one tenth part of that of the cone of sulphur which was covered.

THESE attractions he tried by a fine thread hanging from the end of a stick. He held the electric body in one hand, and the stick in the other; and could perceive the attrac-

tion at as great a distance as he could hold them.

AT the time of his writing, he was upon the subject of permanent electricity in glass, but had not then completed his experiments*.

GREAT light will be thrown upon these experiments of Mr. Grey by some that will be hereafter related of Mr. Wilcke. It is probable that the glass vessel in these experiments was possessed of one electricity, and the sulphur, &c. of the other. But the two electricities were not discovered till afterwards.

WE are now come to a different set of electrical experiments, made by Mr. Grey and Mr. Wheeler in conjunction, similar to some of Mr. Hauksbee's.

IN the first place, Mr. Grey made some experiments, which, probably unknown to him, had been made before by Mr. Boyle, on excited glass, and several other bodies *in vacuo*, and found that they would attract at very near the same distance as *in pleno*. To determine this, he suspended the excited substance in a receiver of an air-pump, and when it was exhausted, he let the electric down to a proper distance from some light bodies, placed on a stand below. The event was, as near as could be judged, the same in *vacuo* as in the open air, if the experiment was made in the same receiver, and if the electric was brought to the light bodies at the same distance of time from the act of excitation†.

* Phil. Trans. abridged, Vol. vi. p. 27.

† Ibid.

ABOUT the latter end of August 1732, Mr. Grey and Mr. Wheeler suspended, from the top of a receiver, a white thread, which hung down to the middle of it. Then exhausting the receiver, and rubbing it, the thread was attracted vigorously. When it was at rest, and hung perpendicularly, the excited tube attracted it; and when the tube was taken away slowly, the thread returned to its perpendicular situation; but the tube being removed hastily, the thread jumped to the opposite side of the receiver. This last effect followed, if the hand was hastily removed from the receiver; and at first it appeared, in both cases, unaccountable to them; but upon farther consideration, they concluded, that it proceeded from the motion of the air, made by the tube, or the hand, which shook off the attraction on that side, and not on the other*. They also found, that an excited tube would attract the thread through another receiver, which was put over that in which it was suspended. And, sometime after, Mr. Wheeler found that the thread was attracted through five receivers put one over another, and all exhausted: he even thought that, in this case, the attraction was rather stronger than when a single receiver was used. N. B. The more effectually to keep any thing of moisture out of the receivers, which would have been of bad consequence in this experiment, instead of wet leather, he made use of a cement made

* Phil. Trans. abridged, Vol. vii. p. 56. † Ibid. p. 97.

of wax and turpentine, which Mr. Boyle used in his experiments *.

These two gentlemen, about the same time, made a curious experiment which, they say, shewed that attraction is communicated thro' opaque, as well as transparent bodies, not in vacuo. But a little knowledge of metal, as a conductor of electricity, would have saved them the trouble they gave themselves. They took a large hand-bell, and taking out the clapper, they suspended a cork, besmeared with honey, from the top of it; and set it on a piece of glass, on which they had put some leaf brass. The excited tube was then brought near several parts of the bell; and, upon taking it up, several pieces of leaf brass were found sticking to the cork, and others were removed from the places in which they had been left, having, probably, been attracted by the bell †.

We see by how small steps advances were made in the science of electricity, by some experiments made by Mr. Grey, 16th June 1731, and which he has thought worth recording; though they contain hardly any thing which we should think new, notwithstanding the discoveries appeared pretty considerable to him.

He electrified a boy standing on cakes of rosin, as strongly as he had before electrified him when suspended on hair lines. He afterwards electrified a boy suspended on hair lines,

* Phil. Trans. abridged, Vol. vii. p. 97.

† Ibid. 96.

by means of a line of communication from another boy who was electrified, at some feet distance from him. He varied this experiment with rods and boys several ways; and concluded from it, that the electric virtue might not only be carried from the tube, by a rod, or line, to distant bodies, but that the same rod, or line, would communicate that virtue to another rod, or line, at a distance from it; and that, by this other rod, or line, the attractive force might be carried to still more distant bodies. This experiment shews that Mr. Grey had not properly considered the line of communication, and the body electrified by it, as one and the same thing, in an electrical view, differing only in form, as they were both alike conductors of electricity.

IN December following, Mr. Grey carried this experiment something farther, by conveying electricity to bodies which did not touch the line of communication, making it pass through the center of hoops standing on glass. One of his hoops was twenty, another forty inches in diameter*.

* Phil. Trans. abridged, Vol. vii. p. 100.

P E R I O D IV.

THE EXPERIMENTS AND DISCOVERIES OF
MR. DU FAY.

HITHERTO the spirit of electricity seems to have been confined to England; but, about this time, we find that it had passed the seas, and that ingenious foreigners were ambitious of distinguishing themselves, and acquiring reputation in this new field of glory. Mr. Du Fay, intendant of the French king's gardens, and member of the academy of sciences at Paris, assiduously repeated the experiments above-mentioned of Mr. Grey, and likewise added to the common stock many new ones of his own. To him we are also indebted for the observation of several general properties of electricity, or rules concerning the method of its action, which had not been taken notice of before, and which reduced to fewer propositions what had been discovered concerning it. These experiments were comprised in eight memoirs, inserted in the History of the Academy of Sciences for the years 1733, 1734 and 1737; and an account of some of them also makes an article in the Philosophical Transactions, dated December 27, 1733. The first of his memoirs contain a history of electricity, brought down to the year 1732*.

* Dantzick Memoirs, Vol. i. p. 195.

HE found that all bodies, except metallic, soft, and fluid ones, might be made electric, by first heating them, more or less, and then rubbing them on any sort of cloth. He also excepts those substances which grow soft by heat, as gum; or which dissolve in water, as glue. He also remarked, that the hardest stone and marble required more chafing and heating than other bodies, and that the same rule obtains with regard to woods; so that box, *lignum vitæ*, and other kinds of very hard wood must be chafed almost to a degree of burning; whereas fir, lime-tree, and cork require but a moderate heat*. Among the more perfect electrics he enumerates all *vitifications*, the Venetian and Muscovite *talc*, the *phosphorus of Berne*, *gypsum*, *transparent selenites*, and in general all *transparent stones*, of whatsoever kind†. Of *salts* he tried only *alum*, and *sugar candy*, both of which he found to be electrical, after warming and rubbing; and he supposed that all the rest would be found to have the same properties, if due precautions were observed in making the experiments. He also observed the electricity of all kinds of *hair*, *silk*, *wool*, and *cotton*, and especially, what appeared to him very extraordinary, the powerful electricity of the *back of a dog*, and still more that of a *cat*‡.

HE found that not only damp air, but also great heat was prejudicial to electricity, and

* Phil. Trans. abridged, Vol. viii. p. 393.

† Ac. Par. 1733. M. p. 105.

‡ Ib. p. 107.

that his experiments often failed in the warmest hours of a moderately hot day *. Also when he heated several bodies, in order to excite their electric power, he found that in some the hottest, and in others the coolest parts only were electrical †.

HE says, that, pursuing Mr. Grey's experiments, to make water receive electricity, he found, that all bodies, without exception, whether solid or fluid, were capable of it, when they were placed on glass, or sealing-wax slightly warmed, or only dried, and the excited tube was brought near them. He particularly mentions his having made the experiments with ice, lighted wood, red hot iron ‡, coal, and every thing that happened to be at hand at the time; and to his great surprize, remarked, that such bodies as were of themselves the least electric, had the greatest degree of electricity communicated to them by the approach of the excited tube §.

To determine whether the transparency of glass was the cause of the transmission of the electric effluvia through that substance (for no one at that time suspected that bodies could be affected by electricity through glass in consequence of its being actually permeable to the electric matter) he used sealing wax, and found that light bodies were affected through that

* Dantzick Memoirs, Vol. i. p. 211.

† Ac. Par. 1733, M. p. 109.

‡ Nollet's Recherches, p. 212

§ Ac. Par. 1733. M. p. 115.

opaque substance, as much as through glass *.

HE refutes Mr. Grey's assertion concerning the different electricity of differently coloured bodies, and shews that it proceeded not from the colour as a *colour*, but from the substance which was employed in dying it †.

IN order to determine whether simple *colour* had any influence in electricity, he introduced a beam of the sun's light into a darkened room, and made his experiments on bodies illuminated with the different primitive colours; and he found that, in no respect whatever, did the different colours make any difference in the power of receiving, communicating, or destroying electricity ‡.

HAVING communicated the electricity of the tube, by means of a packthread, after Mr. Grey's manner, he observed, that the experiment succeeded better for wetting the line; and though he made the experiment at the distance of one thousand two hundred and fifty-six feet, when the wind was high, the line making eight returns, and passing through two different walks of a garden, that the electric virtue was still communicated §. He also made use of glass tubes, sometimes lined with sealing wax, and found that it answered as well as silk lines, and was often more conveniently applied ||.

Our ingenious electrician varied this experiment by suspending two cords with their

* Ac. Par. 1737, M. p. 338. † Ibid. 1733. M. p. 337.

‡ Ib. p. 334. § Phil. Transf. abridged, Vol. viii. p. 355.

|| Ac. Par. 1733. M. p. 345.

ends opposite to one another, and found that when they were placed at no greater distance than an inch, the electricity was communicated without any sensible diminution from the one to the other; but at the distance of a foot it was hardly sensible. When the cords were placed at the distance of three inches, he found that a lighted candle held between them did not prevent the transmission of electricity, nor did the blast of a pair of bellows *. Bringing wood and other conducting substances, suspended by silk, to this electrified cord, he found, as he had concluded *a priori*, that it lost only part of its electricity; the whole quantity being equally distributed between them †.

The electric spark from a living body, which makes a principal part of the diversion of gentlemen and ladies, who come to see experiments in electricity, was first observed by Mr. du Fay, accompanied at that time, as in most of his experiments, by the Abbé Nollet, who, afterwards, we shall find, did himself obtain a distinguished name among electricians.

MR. DU FAY, having got himself suspended on silk lines, as Mr. Grey had done the child mentioned above, observed, that, as soon as he was electrified, if another person approached him, and brought his hand within an inch, or thereabouts, of his face, legs, hands, or cloaths, there immediately issued

* Ac. Par. 1733. M. p. 350.

† Ibid. p. 352.
from

from his body one or more pricking shoots, attended with a crackling noise. He says this experiment occasioned to the person who brought his hand near him, as well as to himself, a little pain, resembling that of the sudden prick of a pin, or the burning from a spark of fire; and that it was felt as sensibly through his cloaths, as on his bare face, or hands. He also observes, that, in the dark, those snappings were so many sparks of fire*.

THE Abbé Nollet says he shall never forget the surprize, which the first electrical spark which was ever drawn from the human body excited, both in Mr. du Fay, and in himself†.

HE says that those snappings and sparks were not excited, if a bit of wood, of cloth, or of any other substance than a living human body, was brought near him; except metal, which produced very nearly the same effect as the human body. He was not aware, that it was owing to points, or partial dryness, in the substances which he mentions, that they did not take a full and strong spark. He seems also to have been under some deception, when he imagined that the flesh of dead animals gave only an uniform light, without any snapping, or sparks§.

FROM this circumstance, however, he, at that time concluded, that the bodies of liv-

* Phil. Transf. Vol. viii. p. 395. Ac. Par. 1733. M. p. 353.

† Leçons de Physique, Vol. vi. p. 408.

§ Phil. Transf. abridged, Vol. viii. p. 395. Ac. Par. 1734. M. 714.

ing animals, (and also metals) were surrounded with an atmosphere of vapour, which was actually set on fire by electric light *.

He observed, that a cat emitted an electric spark, which evidently gave her pain, when the finger was brought to any part of her body, after she had been stroked, while she was sitting on a silk cushion. This must have appeared very extraordinary, before it was known, that the electric matter passed from the hand to the cat, in the act of rubbing †.

With the electric sparks, he imagined he could have fired inflammable substances; and he made several attempts with tinder and gunpowder, but without success: he found no appearance of real fire. This was a discovery reserved for the Germans ‡.

THE two next capital observations of Mr. Du Fay I shall repeat in his own words, because they are important and curious; and yet the former of them is little more than what Otto Guericke had observed before him. “ I
“ discovered,” he says, “ a very simple prin-
“ ciple, which accounts for a great part of
“ the irregularities, and, if I may use the
“ term, of the caprices, that seem to accom-
“ pany most of the experiments in electricity.
“ This principle is, that electric bodies at-
“ tract all those which are not so, and repel
“ them as soon as they are become electric,
“ by the vicinity or contact of the electric
“ body. Thus leaf gold is first attracted by

* Dantzick Memoirs, Vol. i. p. 215, 230.

† Ibid. p. 216.

‡ Ibid. p. 229.

“ the tube; acquires electricity by approach-
 “ ing it, and, consequently, is immediately
 “ repelled by it; nor is it re-attracted, while
 “ it retains its electric quality. But if, while
 “ it is thus sustained in the air, it chance to
 “ light on some other body, it straightway
 “ loses its electricity, and consequently is re-
 “ attracted by the tube; which, after having
 “ given it a new electricity, repels it a second
 “ time; and this repulsion continues as long
 “ as the tube keeps its power. Upon apply-
 “ ing this principle to various experiments of
 “ electricity, one will be surprised at the
 “ number of obscure and puzzling facts
 “ which it clears up.” By the help of this
 principle, he, particularly, endeavours to ex-
 plain several of Mr. Hauksbee’s experi-
 ments *.

“ CHANCE, he says, has thrown in my
 “ way another principle more universal and
 “ remarkable than the preceding one; and
 “ which casts a new light upon the subject of
 “ electricity. The principle is, that there
 “ are *two kinds of electricity*, very different
 “ from one another; one of which I call
 “ *vitreous*, the other *resinous* electricity. The
 “ first is that of glass, rock-crystal, precious
 “ stones, hair of animals, wool, and many
 “ other bodies. The second is that of am-
 “ ber, copal, gum lac, silk, thread, paper,
 “ and a vast number of other substances.
 “ The characteristics of these two electricities

* Phil. Trans. abridged, Vol. viii. p. 396.

“ are, that they repel themselves, and at-
 “ tract each other. Thus a body of the vi-
 “ treous electricity repels all other bodies
 “ possessed of the vitreous, and on the con-
 “ trary, attracts all those of the resinous
 “ electricity. The resinous, also, repels the
 “ resinous, and attracts the vitreous. From
 “ this principle, one may easily deduce the
 “ explanation of a great number of other
 “ phenomena; and it is probable, that this
 “ truth will lead us to the discovery of many
 “ other things.”

THIS very capital discovery was, as the ingenious author acknowledges, perfectly accidental, having been made in consequence of casually observing (which, he says, was to his great surprize) that, having caused a piece of leaf gold to be repelled, and suspended in the air, by an excited glass tube, and meaning likewise to chase it about the room by a piece of excited gum copal, instead of being repelled by it, as it was by the glass tube, it was eagerly attracted. The same was the case with sealing-wax, and the other substances enumerated above. He also observed, that when a piece of leaf gold was electrified by excited sealing-wax, &c. it was constantly attracted by excited glass, but repelled by excited sealing-wax, &c. *

OUR ingenious electrician was, however, too hasty in concluding, as he did, that the two electricities which he had discovered were

* Ac. Par. 1733, M. p. 627.

altogether independent of the substance with which the electrics were rubbed. All the difference, he says, produced by a change of the rubber, was that of *more or less* of the same kind *. We shall find that, in a much later period, the contrary was discovered to be true by Mr. Canton.

IN order to know, immediately, to which of the two classes of electricity any body belonged, he made a silk thread electrical, and brought it to the body, when it was excited. If it repelled the thread, he concluded it was of the same electricity with it, viz. resinous; if it attracted it, he concluded it was vitreous †. He had also other ingenious methods to ascertain the same thing ‡.

HE also observed, that communicated electricity had the same property as the excited. For having electrified, by the glass tube, balls of wood or ivory; he found them to repel the bodies which the tube repelled, and to attract those which the tube attracted. If they had the resinous electricity communicated to them, they observed the same rule, by attracting those bodies which had the vitreous electricity communicated to them, and repelling those which had received the resinous. But, he observes, the experiment would not succeed, if the bodies were not made equally electrical; for, if one of them was weakly electrical, it would be attracted by that which

* Ac. Par. 1733, M. p. 639.

† Phil. Trans. abridged, Vol. viii. p. 397.

‡ Ac. Par. 1733, M. p. 639.

was much more strongly electrical, of whatever quality it was.

THIS discovery of the two electricities was certainly a capital one, but was, notwithstanding, left very imperfect by Mr. Du Fay. We shall see that Dr. Franklin found, that, in all probability, the vitreous electricity is positive, or a redundancy of electric matter; and the resinous, negative, or a want of it; and that Mr. Canton has discovered, that it depends upon the surface of the electric bodies, and of the rubber, whether the electricity be positive or negative.

THE doctrine of two different electricities, produced by exciting different substances, considerable as the discovery of it was, seems to have been dropped after Mr. Du Fay, and those effects ascribed to other causes; which is an instance that science sometimes goes backwards.

MR. DU FAY himself seems, at last, to have adopted the opinion, which generally prevailed to the time of Dr. Franklin; that the two electricities differed only in degree, and that the stronger attracted the weaker: not considering that, upon this principle, bodies possessed of the two electricities ought to attract one another less forcibly, than if one of them had not been electrified at all, which is contrary to fact.

IT will be seen that, many years after, Mr. Kinnerley of Philadelphia, a friend of Dr. Franklin's, being at Boston in New England, made some experiments which again shewed

shewed the difference of the two electricities. He communicated those experiments to Dr. Franklin, who repeated and explained them*.

MR. DU FAY was the first person who endeavoured to excite a tube in which air was condensed, and, to his great surprize, found the attempt ineffectual. Suspecting this might be owing to moisture, which he might have forced into the tube, in using his condensing instrument, he cemented a large copper eolipile to his tube, and compressed the air in it, by putting the eolipile upon the fire. After this, he turned a cock, which he had placed to prevent the return of the compressed air, and disengaged the tube from the eolipile; but he still found the excitation to be impossible†. The Abbé Nollet, who assisted at most of this gentleman's experiments, declares himself not satisfied even with this precaution; thinking that the non-excitation of the tube might still be owing to the moisture, which always exists in the air, and the particles of which must be drawn nearer together by condensation‡. In answer to this objection, Mr. Boulanger says, that a small glass full of water poured into a tube, and immediately thrown out again, will not destroy the excitability of the glass near so much as the condensed air§.

MR. DU FAY found no difference in the excitation of a glass tube whether it was filled with warm *sand* or not; but when the tube was

* See his Letters.

† Ac. Par. 1734. M. p. 489.

‡ Nollet's Recherches, p. 258.

§ Boulanger, p. 132.

cool it was not so easily excited. The excitation was more obstructed by *bran*, and much more by *water*, warm or cold; though, he says, that the electricity was not quite destroyed by it *.

MR. DU FAY took a good deal of pains to ascertain the effect of electricity *in vacuo*, but his conclusion can hardly be depended upon. For, he says, that *glass*, and other substances possessed of the same kind of electricity, are hardly capable of being excited *in vacuo*; whereas *amber*, and the substances of that class, are excited as easily, and as vigorously *in vacuo*, as in the open air. The *vacuum* he made seems to have been as good as can be made by the better kind of our air-pumps †.

This philosopher was the first who observed that electric substances attract the dew more than conductors. He observed that a glass vessel, placed on a metal cup, and set in the open air all day, will often be wet when the metal is dry. S. Beccaria accounts for this fact, by supposing that alterations in the electricity of the air easily produce correspondent alterations in the electricity of metals, in which the electric fluid moves with the utmost ease, but not in glass. Whenever, therefore, the state of the electric fluid in the air is altered, the glass is electrified *plus* or *minus*, and therefore attracts the vapours in the air ‡.

* Ac. Par. 1733. M. p. 341.

† Ac. Par. 1734. M. p. 489.

‡ Beccaria del elettricismo naturale et artificiale, p. 179.

It must be observed, that Mr. Granville Wheeler, in the autumn of the year 1732, made several curious experiments, relating to the repulsive force of electricity. These he repeated to Mr. Grey in the summer following, and designed to communicate them, through his hands, to the Royal Society; but, deferring the execution of it from time to time, he was informed that Mr. Du Fay had taken notice of the same solution of the repulsive force. Upon this he laid aside all thoughts of communicating his discovery to the public: but, finding that his experiments were different from those of Mr. Du Fay, he was persuaded to publish them in the Philosophical Transactions for the year 1739.

THE experiments were made by threads of various kinds, and other substances, hanging down from silk lines, and generally made to repel one another by the approach of an excited tube. The result of them all he comprised in the three following propositions. 1st. That bodies made electrical, by communication with an excited electric, are in a state of repulsion with respect to such excited bodies. 2dly. That two, or more bodies, made electrical by communicating with an excited electric, are in a state of repulsion with respect to one another. 3dly. Excited electrics do themselves repel one another*.

ONE of his experiments, to prove the second of these propositions, deserves to be

* Phil. Trans. abridged, Vol. viii. p. 411.

mentioned for its curiosity. He tied a number of silk threads together, by a knot at each extremity; when, upon electrifying them, the threads repelled one another, and the whole bundle was swelled out into a beautiful spherical figure; so that he could with pleasure, he says, observe the knot at the bottom rising upwards, as the electricity and mutual repulsion of the threads increased; and he could not help imagining his bundle of silks to resemble a bundle of muscular fibres.

By way of corollary to the same proposition, he observes, that it suggests, more plainly than any other known experiment, a reason for the dissolution of bodies in menstrua; viz. that the particles of the solvend, having imbibed particles of the menstrua, so as to be saturated with them, the saturated particles become repulsive of one another, separate, and fly to pieces *.

* Philosophical Transactions abridged, Vol. viii. p. 410.

P E R I O D V.

THE CONTINUATION AND CONCLUSION OF
MR. GREY'S EXPERIMENTS.

MR. GREY, upon refuming his experiments, expreffes great fatisfaction, that his former obfervations had been confirmed by fo judicious a philofopher as Mr. Du Fay; who, he acknowledges, had made feveral new ones of his own, particularly that important *luciferous one*, as he calls it, recited above; which, put him upon making the experiments which follow, and which were made in the months of July and Auguft 1734*.

As Mr. Du Fay had faid, that the snapping and the fparks, he had mentioned, were ftrongly excited by a piece of metal, prefented to the perfon fufpended on filk lines; Mr. Grey concluded, that if the perfon and metal fhould change places, the effect would be the fame. He, accordingly, fufpended feveral pieces of metal on filk lines, beginning with the common utensils, which were at hand, as the poker, tongs, fire fhovel, &c. and found, that, when they were electrified, they gave fparks, in the fame manner as the human body had done in like circumftances.

* Phil. Tranf. abridged, Vol. viii. p. 397.

This was the origin of *metallic conductors*, which are in use to this day *.

MR. GREY did not, at the time above-mentioned, think of making his experiments in the dark, in order to see the light proceeding from the iron; not imagining, that electricity, communicated to metals would have produced such surprising phenomena, as, he says, he afterwards found it to do.

CONTINUING his experiments at Mr. Wheeler's, they found, that the flesh of dead animals exhibited, very nearly, the same appearances as that of living animals, contrary to the assertion of Mr. Du Fay.

BUT what most surprised Mr. Grey, and the gentlemen then present, in the experiments he made upon that occasion, was the

* Phil. Trans. abridged, Vol. viii. p. 398.

In order the more conveniently to communicate electricity to the iron bar, Mr. Du Fay (who adopted the contrivance from Mr. Grey) fastened to the end of it a bundle of *linen threads*, to which he applied his excited tube. He was led to prefer the thread for this purpose, in consequence of having found that, of the flexible substances, *wool, silk, cotton, or linen*; the last was most attracted by an excited electric. He suspended them all to the same bar, and brought an excited glass tube to an equal distance from them all at the same time, and observed that they were attracted in the order in which I have mentioned them, *wool* the least, and *linen* the most. Ac. Par. 1737. M. p. 137. These linen threads of Mr. Du Fay kept their ground in a much improved state of the electrical apparatus, when globes were substituted in the place of tubes, but now *small wires* are universally used in preference to them.—Mr. Du Fay also, in order to determine what *metal* was the most proper for this purpose, procured equal cylinders of gold, silver, copper, brass, lead, iron, and tin, and having placed them so as to constitute one cylinder, he drew sparks from each of them in their turn, when the whole was electrified; but he could not, in any respect, perceive the least difference in them. Ac. Par. 1737. M. p. 132.

phenomenon above referred to, and what he nows calls *a cone*, or *pencil of electric light*; such as is commonly seen to issue from an electrified point. As this was the first time that this phenomenon, which is now so common, was distinctly seen, I shall relate the experiment, of which it was the result, at large.

MR. GREY, and his friends, provided themselves with an iron rod four feet long, and half an inch in diameter, pointed at each end, but not sharp. Suspending this iron rod upon silk lines in the night; and applying the excited tube to one end of it, they perceived not only a light upon that end, but another issuing from the opposite end, at the same time. This light extended itself, in the form of a cone, whose vertex was at the end of the rod: and Mr. Grey says, that he and his company could plainly see, that it consisted of separate threads, or rays of light, diverging from the point of the rod, the exterior rays being incurvated. This light appeared at every stroke they gave the tube.

THEY likewise observed, that this light was always attended with a small hissing noise, which, they imagined, began at the end next the tube, increasing in loudness till it came to the opposite end. He says, however, that this noise could not be heard, but by persons who stood near the rod, and attended to it*.

* Phil. Trans. abridged, Vol. viii. p. 398.

MR. GREY, repeating those experiments in the September following, after his return to London, observed an appearance, which he says, surpris'd him very much. After the tube had been applied to the iron rod, as before, when the light, which had been seen at both ends, had disappeared; it was visible again upon bringing his hand near the end of the rod; and, upon repeating this motion of his hand, the same phenomenon appeared for five or six times successively; only the rays were, at each time, shorter than the other. He also observed, that these lights, which were emitted by the tube upon the approach of his hand, were, like the others, attended with a hissing noise.

HE took notice, that the light which appeared on the end next the tube, when it was held oblique to the axis of the rod, had its rays bending towards it; and that, all the time he was rubbing the tube, those flashes of light appeared upon every motion of his hand up or down the tube, but that the largest flashes were produced by the motion of his hand downwards*.

WHEN he used two or three rods, laying them either in a right line, or so as to make any angle with each other, and applied the tube to any one of their ends; he observed, that the farthest end of the farther rod exhibited the same phenomena as one single rod†.

* Phil. Trans. abridged, Vol. viii. p. 399.

† Ib. p. 400.

Using a rod pointed only at one end, he observed, that the other end gave but one single snap, but that it was much louder than the greatest of those which were given by the point of the rod; also that the pain, resembling pricking or burning, was more strongly felt, and that the light was brighter and more contracted.

CONNECTING a pewter plate with the iron rod, and filling the plate with water, he observed the same light, the same pushing of the finger, as he calls it, and the same snapping, as when the experiment was made with the empty plate. And when the experiment was made with water, in the day-light, it appeared to rise in a little hill, under the finger which was presented to it; and, after the snapping noise, fell down again, putting the water into a waving motion near the place where it had risen.

THESE effects were the same with those which he had before observed to proceed from the immediate action of the tube, but by these experiments, he says, he found (what, no doubt, appeared a real advance in the science to him) that an actual flame of fire, together with an explosion, and ebullition of cold water, might be produced by communicative electricity. What he adds is so remarkable, that I shall repeat his own words. “ And although these
“ effects are at present but *in minimis*, it is
“ probable, in time, there may be found out
“ a way to collect a greater quantity of the
“ electric fire, and consequently to increase
“ the

“ the force of that power; which, by several
 “ of these experiments, *si licet magnis com-*
 “ *ponere parva*; seems to be of the same
 “ nature with that of thunder and light-
 “ ning*.”

How exactly has this prophecy been fulfilled in the discoveries of the Leyden electricians, and Dr. Franklin; the former having discovered the amazing accumulation of the electric power, in what is called the Leyden phial; and the latter having proved the matter of lightning to be the very same with that of electricity; though Mr. Grey might possibly mention thunder and lightning only by way of common comparison.

ON February the 18th, 1735, Mr. Grey, repeating the experiments of the iron rods with wooden ones, found all the effects to be similar, but much weaker; as it is now well known must have been the case; wood being so imperfect a conductor, and only in proportion to the moisture it contains.

AT the same time, he relates, that the repeating the electrification of water, he found, that the phenomena before mentioned were produced, not only by holding the tube near the water, but when it was removed, and the finger afterwards brought near it†.

MAY the 6th of the same year, he again suspended a boy on silk, and found that this boy was able to communicate the electric fire,

* Phil. Transf. abridged, Vol. viii. p. 401.

† Ibid. p. 402.

first to one, and then to several persons standing upon electric bodies.

MR. GREY seems still to have imagined, that electricity depended, in some measure, upon colour. The boy suspended on blue lines, he says, retained his power of attraction fifty minutes; on scarlet lines, twenty-five minutes; and on orange coloured lines, twenty-one minutes. By these experiments, he says, we see the efficacy of electricity on bodies suspended upon lines of the same substance, but of different colour *.

BUT the greatest deception which this ingenious gentleman seems to have lain under, was occasioned by the experiments which he made with balls of iron, to observe the revolution of light bodies about them. The paragraph relating to these experiments, being the last which Mr. Grey wrote, I shall give it at length as a curiosity.

“ I HAVE lately made,” says he, “ several
“ new experiments upon the projectile and
“ pendulous motion of small bodies by electricity; by which small bodies may be
“ made to move about large ones, either in
“ circles, or in ellipses; and those either
“ concentric, or eccentric to the center of the
“ larger body, about which they move, so as
“ to make many revolutions about them.
“ And this motion will constantly be the
“ same way that the planets move about the
“ sun, viz. from the right hand to the left,

* Phil. Trans. abridged, Vol. viii. p. 403.

“ or from west to east. But these little planets, if I may so call them, move much faster in their *apogee*, than in the *perigee* parts of their orbits; which is directly contrary to the motion of the planets about the sun *.”

THESE experiments Mr. Grey had thought of but a very little while before his last illness, and had not time to complete them; but the progress he had made in them he revealed, on the day before his death, to Dr. Mortimer, then secretary to the Royal Society. He said they struck him with new surprise every time he repeated them, and hoped that, if God would spare his life a little longer, he should, from what these phenomena pointed out, bring his electrical experiments to the greatest perfection. He did not doubt but, in a short time, he should be able to astonish the world with a new sort of planetarium, never before thought of; and that, from these experiments, might be established a certain theory, to account for the motions of the grand planetarium of the universe. These experiments, fallacious as they are, deserve to be briefly recited, together with those which were made in consequence of them after Mr. Grey's death. I shall relate them in Mr. Grey's own words, as they were delivered to Dr. Mortimer, on his death-bed.

PLACE a small iron globe, said he, of an inch, or an inch and a half in diameter, on

* Phil. Transf. abridged, Vol. viii. p. 404.

the middle of a circular cake of rosin, seven or eight inches in diameter, gently excited; and then a light body suspended by a very fine thread, five or six inches long, held in the hand over the center of the table, will, of itself, begin to move in a circle round the iron globe, and constantly from west to east. If the globe be placed at any distance from the center of the circular cake, it will describe an ellipse, which will have the same eccentricity, as the distance of the globe from the center of the cake.

If the cake of rosin be of an elliptical form, and the iron globe be placed in the center of it, the light body will describe an elliptical orbit, of the same eccentricity with the form of the cake.

If the iron globe be placed in, or near, one of the foci of the elliptical cake, the light body will move much swifter in the apogee, than in the perigee of its orbit.

If the iron globe be fixed on a pedestal, an inch from the table, and a glass hoop, or a portion of a hollow glass cylinder, excited, be placed round it; the light body will move as in the circumstances mentioned above, and with the same varieties.

He said, moreover, that the light body would make the same revolutions, only smaller, round the iron globe placed on the bare table, without any electrical body to support it; but he acknowledged he had not found the experiment succeed, if the thread was supported by any thing but a human hand;

hand; though, he fancied, it would have succeeded, if it had been supported by any animal substance, living or dead*.

MR. GREY went on to recite to Dr. Mortimer other experiments still more fallacious; which, out of regard to his memory, I shall forbear to quote. Let the chimeras of this great electrician teach his followers, in the same, and still but newly opened field of philosophy, a proper degree of caution in their reasonings from induction. Let not the example, however, discourage any person from trying what may appear improbable; but let it induce a man to delay the publication of his discoveries, till they have been perfectly ascertained, and performed in the presence of others. In experiments of great delicacy, a strong imagination will have great influence even upon the external senses; of which we shall have frequent instances in the course of this history.

DR. MORTIMER himself seems to have been deceived by these experiments of Mr. Grey. He says, that, in trying them after his death, he found, that the light body would make revolutions round bodies of various shapes and substances, as well as round the iron globe; and that he had actually tried the experiment, with a globe of black marble, a silver standish, a small chip box, and a large cork†.

* Phil. Transf. abridged, Vol. viii. p. 404, 405.

† Ibid. p. 405.

THESE experiments of Mr. Grey were tried by Mr. Wheeler, and other gentlemen, at the Royal Society's house, and with a great variety of circumstances; but no conclusion could be drawn from what they at that time observed. Mr. Wheeler himself took a great deal of pains to verify them, with various success; and at last he gave it as his opinion, that a desire to produce the motion from west to east was the secret cause, that determined the pendulous body to move in that direction, by means of some impression from Mr. Grey's hand, as well as his own; though he was, at the same time, persuaded, that he was not sensible of giving any motion to his hand himself*.

MR. DU FAY, in the Memoirs of the Academy of Sciences for the year 1737, acknowledges that these experiments of Mr. Grey and Dr. Mortimer did not succeed with him. But says, they were the only ones of Mr. Grey that had not; and, with a temper becoming a philosopher, adds, that he doth not, therefore, say, that they never had succeeded; but seems rather willing to attribute the failure with himself, to his omitting some circumstance, not mentioned by those gentlemen, though, unknown to them, it might be of principal consequence to the experiment †. He was afterwards informed of the result of the last experiment in England on this subject, agreeing with his own ‡.

* Phil. Trans. abridged, Vol. viii. p. 418.

† Dantzick Memoirs, Vol. i. p. 226.

‡ Ac. Par. 1737. M. p. 436.

P E R I O D VI.

THE EXPERIMENTS OF DR. DESAGULIERS.

WE are now come to the labours of that indefatigable experimental philosopher Dr. DESAGULIERS, in this new field of science. The reason which he gives why he had avoided entertaining the Royal Society upon this subject before, and why he had not pursued it so far as he might have done; considering, as he says, that he could excite as strong an electricity in glass, by rubbing with his hand, as any body could, is worth mentioning for its curiosity, and for the light that it throws upon the temper and manner of Mr. Grey. He says, that he was unwilling to interfere with the late Mr. Stephen Grey, who had wholly turned his thoughts to electricity; but was of a temper to give it entirely over, if he imagined that any thing was done in opposition to him *.

DR. DESAGULIERS begins with observing very sensibly (and the observation is still true) that the phenomena of electricity are so odd, that, though we have a great many experiments upon that subject, we have not yet been able, from their comparison, to settle such a theory as will lead us to the cause of that property in bodies, or even to judge of

* Phil. Trans. abridged, Vol. viii. p. 419.

all its effects, or find out what useful influence electricity has in nature; though certainly, from what we have seen of it, we may conjecture that it must be of great use, because it is so extensive.

HIS first experiments, of which an account is given in the Philosophical Transactions, dated July, 1739, were made with a hempen string, extended upon cat-gut. To the end of the hempen string, he suspended various substances; and says, that all those which he tried, amongst which were several *electrics per se*, as sulphur, glass, &c. without exception, received electricity*.

HE changed one of the cat-gut strings, on which his hempen line of communication was extended, and put various other substances in its place, to try what bodies would transmit electricity to the suspended body, and what would not; and from the result of his experiments, partly concluded, that bodies in which electricity could not be excited intercepted the electric effluvia; and that those in which electricity could be excited, did not intercept it, but permitted it to go on to the extremity of the hempen string. But still he had no just idea, that, except metals, it was the moisture in the bodies he tried which intercepted the electric effluvia; and his ideas of the manner in which they were intercepted were very imperfect.

* Phil. Trans. abridged, Vol. viii. p. 420.

To Dr. Defaguliers we are indebted for some *technical terms* which have been extremely useful to all electricians to this day, and which will probably remain in use as long as the subject is studied. He first applied the term *conductor* to that body to which the excited tube conveys its electricity; which term has since been extended to all bodies that are capable of receiving that virtue. And he calls those bodies in which electricity may be excited, by heating or rubbing, *electrics per se*.

In the writings of this author we find many *axioms* relating to electrical experiments, some of which are expressed in a more clear, and distinct manner than they had been before; but the real improvements which he made, were very few and immaterial.

On several occasions, and particularly in a paper delivered to the Royal Society, in the month of January 1741, he lays down, among others, the following general rules, which seem to be more accurate than any which had been delivered before upon the subject*.

“ An electric per se will not receive electricity from another electric per se, in which it has been excited, so as to run along its whole length: but will only receive it a little way, being, as it were, saturated with it.

* Phil. Trans. abridged, Vol. viii. p. 430.

“ AN electric per se will not lose all its
 “ electricity at once, but only the electricity
 “ of those parts near which a non-electric
 “ has been brought. It, consequently, loses
 “ its electricity the sooner, the more of those
 “ bodies are near it. Thus, in moist weather,
 “ the excited tube holds its virtue but a little
 “ while, because it acts upon the moist va-
 “ pours which float in the air. And if the
 “ excited tube be applied to leaf gold laid
 “ upon a stand, it will act upon it much
 “ longer, and more strongly, than if the
 “ same quantity of leaf gold be laid upon a
 “ table, which has more non-electric surface
 “ than the stand*.” This, however, seems
 not to be the whole reason; for if the leaf
 gold were laid upon a broad surface of glass,
 it would not be acted upon so powerfully, as
 if it were placed upon a narrow stand of any
 kind of matter.

“ A NON-ELECTRIC, when it has received
 “ electricity, loses it all at once, upon the
 “ approach of another non-electric.” This
 however could only be the case when the ap-
 proaching electric was not insulated, but had
 a communication with the earth. It must
 also be brought into contact with the electrified
 body.

“ ANIMAL substances are non electrics by
 “ reason of the fluids they contain †.

“ EXCITED electricity exerts itself in a
 “ sphere round the electric per se, or

* Phil. Trans. abridged, Vol. viii p. 427.

† Ibid p. 429.

“ rather in a cylinder, if the body be cylindrical *.”

Few of the many experiments which were made by Dr. Defaguliers (accounts of which were published in the Philosophical Transactions) had, as I observed before, any thing new in them. Those which were the most so are the following.

ENDEAVOURING to communicate electricity to a burning tallow candle, he observed, that the candle attracted the thread of trial, but not within two or three inches of the flame; but that, as soon as the candle was blown out, the thread was attracted by every part of it, and even by the wick, when the fire was quite extinguished. He electrified a wax candle in the same manner, and the experiment succeeded as well, only the electricity came not so near the flame in the wax, as in the tallow candle.

He says, that only warming a glass receiver, without any rubbing, would cause the threads of a down feather, tied to an upright skewer to extend themselves, as soon as it was put over the feather; and that sometimes rosin and wax would exert their electricity by being only exposed to the open air.

He observed that if a hollow glass tube, supporting the line of communication, were moistened by blowing through it, it would intercept the electricity.

* Phil. Trans. abridged, Vol. viii. p. 431.

HE says, that when an excited tube has repelled a feather, it will attract it again, after being suddenly dipped into water, but in fair weather it will not attract it unless it hath been dipped pretty deep into the water, a foot of its length at least; whereas, in moist weather, an inch or two will suffice *.

HE shewed the attraction of water by an excited tube, in a better manner than it had been shewn before, viz. by bringing the tube to a stream issuing from a condensing fountain: which, thereupon, was evidently bent towards it.

DR. DESAGULIERS seems to have been the first who expressly said, that pure *air* might be ranked amongst electrics per se, and that cold air in frosty weather, when vapours rise least of all, is preferable, for electrical purposes, to warm air in summer, when the heat raises the vapours †. He also supposed that the electricity of the air was of the vitreous kind; and he accounted for the electricity appearing on the inside only of an exhausted glass vessel, by its going where it met with the least resistance from so electrical a body as the air ‡.

HE endeavoured to account for the fixing of air by the steams of sulphur, according to the experiment of Dr Hales; by supposing that the particles of sulphur, and those of air, being possessed of different kinds of electricity, attracted one another, whereby their repulsive

* Phil. Trans. abridged, Vol. viii. p. 429.

† Ibid. p. 437.

‡ Ibid. 438.

power was destroyed. He also proposed the following conjecture concerning the rise of vapour. The air at the surface of water being electrical, particles of water, he thought, jumped to it, then, becoming themselves electrical, they repelled both the air and one another, and consequently ascended into the higher regions of the atmosphere*.

THE last paper of Dr. Desaguliers in the Philosophical Transactions, upon the subject of electricity, is dated June 24th, 1742, in which year he published a dissertation on electricity, by which he gained the prize of the academy at Bourdeaux. This prize was a medal of the value of 300 livres, proposed, at the request of Monsieur Harpez de la Force, for the best essay on electricity, and shews how much this subject engaged the attention of philosophers at that time†. The dissertation is well drawn up, and comprises all that was known of the subject till that period.

* Phil. Trans. abridged, Vol. viii. p. 437.

† Dantzick Memoirs, Vol. i. p. 261.

P E R I O D VII.

EXPERIMENTS OF THE GERMANS, AND
OF DR. WATSON, BEFORE THE DISCO-
VERY OF THE LEYDEN PHIAL IN THE
YEAR 1746.

ABOUT the time that Dr. Defaguliers had concluded his experiments in England, viz. 1742, several ingenious Germans began to apply themselves to the same studies with great assiduity, and their labours were crowned with considerable success.

To the Germans we are indebted for many capital improvements in our electrical apparatus within this period, without which, the business would have gone on very slowly and heavily; but, by the help of their contrivances, we shall see that astonishing effects were soon produced.

MR. BOZE, a professor of philosophy at Wittemburgh, substituted the *globe* for the tube, which had been used ever since the time of Hauksbee*. He likewise added a *prime conductor*, which consisted of a tube of iron or tin, at first supported by a man standing upon cakes of rosin, and afterwards sus-

* According to other accounts, Christian Augustus Haufen, professor of mathematics at Leipzig, was the first who revived the use of Hauksbee's glass globe, and Mr. Boze, who was excited to make experiments in electricity by the example of Mr. Haufen, borrowed this capital improvement from him. Dantzick Memoirs, Vol. i. p. 278, 279.

pended on silk horizontally before the globe *.

To prevent the tube from doing any harm to the globe, he put a bundle of thread into the end which was next to it, and which was left open for that purpose. This expedient, besides occasioning various pleasant phenomena, was observed to make the force of the conductor much stronger †.

THE use of the globe was immediately adopted in the university of Leipzig, where Mr. Winckler, the professor of languages, substituted a *cushion* instead of the hand, which had before been employed to excite the globe. But the best rubber for the globe, as well as the tube, was, long after this, still thought, by all electricians, to be the human hand, dry, and free from moisture ‡.

MR. P. GORDON, a Scotch Benedictine monk; and professor of Philosophy at Erford, was the first who used a *cylinder* instead of a globe. His cylinders were eight inches long, and four inches in diameter. They were made to turn with a bow, and the whole instrument was portable. Instead of a cake of rosin, he insulated by means of a frame, furnished with net work of silk §.

THE apparatus, likewise, of many of the German electricians was very *various*, and expensive. Mr. Winckler, in a paper read at the Royal Society, March 21st, 1745 ||, de-

* Histoire de l'électricité, p. 27.

† Phil. Trans. abridged, Vol. x. p. 271. ‡ Ibid. 272.

§ Histoire, p. 31.

|| Phil. Trans. abridged, Vol. x. p. 273.

scribes a machine for rubbing tubes, and another for rubbing globes, and compares the effects of them both. He observes, that the sparks which are produced from glass vessels drawn to and fro were larger, and more vehemently pungent, provided that those vessels were of the same magnitude with the globes; but that the flux of effluvia was not so constant as from the globes. Mr. Winckler also invented a machine, which he describes at large in his works, by means of which he could give his globe six hundred and eighty turns in a minute *. This gentleman likewise contrived to rub glass, and china vessels, in the inside; and he says they acted as strongly on bodies placed on the outside of them, as when they were rubbed on the outside †.

THE German electricians generally used more globes than one at a time, and imagined they found the effects proportionable, though this fact was called in question by Dr. Watson, and others; and Mr. Nollet preferred globes made blue with zaffre ‡, which were carefully tried, and rejected by Dr. Watson afterwards §.

SUCH a prodigious power of electricity could they excite from these globes, whirled by a large wheel, and rubbed with woollen cloth, or a dry hand (for we find both these methods were in use among them about this

* Histoire, p. 32.

† Dantzick Memoirs, Vol. i. p. 400.

‡ In subsequent trials the Abbé himself found no advantage in those blue globes. Ac. Par. 1745. M. p. 162.

§ Phil. Trans. abridged, Vol. x. p. 277.

time) that, if we may credit their own accounts, the blood could be drawn from the finger by an electric spark; the skin would burst, and a wound appear, as if made by a caustic. They say, that if several globes or tubes were used, the motion of the heart and arteries of the electrified person would be very sensibly increased; and that, if a vein were opened under the operation, the blood, issuing from it, would appear like lucid phosphorus, and run out faster than when the man was not electrified. Analogous to this last experiment, Mr. Gordon observed, that water, running from an artificial fountain electrified, was scattered in luminous drops, that a larger quantity of water was thrown out in a given time than when the fountain was not electrified*; and that electrified water evaporated faster than water not electrified, when exposed in similar glass vessels†. Part of this account we know might be true, but some part must have been exaggerated. It is certain that Mr. Gordon increased the electric sparks to such a degree, that they were felt from a man's head to his foot, so that a person could hardly take them without falling down with giddiness‡, and small birds were killed by them§. This he effected by conveying electricity, with iron wires, to the distance of 200 ells from the place of excitation.

* Phil. Trans. abridged, Vol. x. p. 277.

† Dantzick Memoirs, Vol. ii. p. 357.

‡ Ibid. p. 358.

§ Nollet's Recherches, p. 172.

He also found that the sparks were stronger when the wire was thick than when it was small *.

MR. WAITZ made his glass tubes act stronger by rubbing them with a waxed cloth and a little oil. He also found, that glass made with very little pot-ash, acted much better than that in the composition of which much of it was used; but it required longer time, and more heat to vitrify it. He got some glass made on purpose to ascertain the fact †.

By various experiments of attraction and repulsion, which Mr. Waitz made in rubbing a dog (which he had made thoroughly dry for that purpose) he proved that the flashes of light, which sometimes appear when animals are stroked, are electrical. This had been supposed, but was not accurately ascertained before ‡. It was this gentleman who gained the prize of 50 ducats, proposed, in the year 1744, by the Academy of Sciences at Berlin, for the best dissertation on the subject of electricity. It was published along with three others, which were offered at the same time, and thought worthy of that honour §.

THE thing that strikes us most in their experiments, performed by these machines, is their setting fire to inflammable substances. This they were, probably, led to attempt, from observing the vivid appearance of elec-

* Dantzick Memoirs, Vol. ii. p. 359.

† Ibid. p. 381.

‡ Ibid. p. 385.

§ Ibid. p. 380.

tric light, the burning pain that was felt by a smart stroke from the conductor, and the many analogies the electric fluid evidently bore to phosphorus and common fire.

THE first person who succeeded in this attempt was Dr. Ludolf of Berlin, towards the beginning the year 1744; who kindled, with sparks excited by the friction of a glass tube, the etherial spirit of Frobenius. This he did at the opening of the Royal Academy, and in the presence of some hundreds of persons. He performed the experiment by electric sparks proceeding from an iron conductor. Mr. Winckler did the same in the May following, by a spark from his own finger; and kindled, not only the highly rectified spirit above-mentioned, but French brandy, corn spirits, and other spirits still weaker, by previously heating them. He also says, that oil, pitch, and sealing-wax might be lighted by electric sparks, provided those substances were first heated to a degree next to kindling*. To these it must be added, that Mr. Gralath fired the smoke of a candle just blown out, and lighted it again†; and that Mr. Boze fired gunpowder, melting it in a spoon, and first the vapour that rose from it.

THE German electricians, likewise, constructed a machine, by which they could give friction to a glass cylinder in vacuo. By these means they contrived to electrify a wire which terminated in the open air, and there shewed

* Phil. Trans. abridged, Vol. x. p. 271.

† Dantzick Memoirs, Vol. ii. p. 438.

a considerable electric power. They also electrified that end which was in the open air, and made the other end which was in vacuo exert its electricity*.

THE same Germans also mention an experiment, which, if pursued, would have led them to discover, that the friction of the glass globe did not produce, but only collect the electric matter. But that was a discovery reserved, as we shall find, for Dr. Watson. It seems that both Mr. Boze and Mr. Allamand had suspended the machine, and the man who worked it, upon silk; and observed, that, not only the conductor, but also the man, and the machine gave signs of electricity; though they did not attend accurately to all the circumstances of that curious fact, which did not at all answer their expectations. For, imagining that part of the electric power was continually going off to the ground by the machine, they supposed that the effect of insulating it, would have been a stronger electricity†.

IN this period it was that Ludolf the younger demonstrated, that the luminous barometer is made perfectly electrical by the motion of the quicksilver; first attracting, and then repelling bits of paper, &c. suspended by the side of the tube, when it was enclosed in another out of which the air was extracted‡. Before this experiment, those ef-

* Phil. Trans. abridged, Vol. x. p. 275.

† Wilson's Essay, preface, p. 14. Watson's Sequel, p. 34.

‡ Dantzick Memoirs, Vol. iii. p. 495.

fects had been ascribed to the air *. Profef-
 for Hamberger and Mr. Waitz had discover-
 ed that the motion of quicksilver in a glass
 vessel, out of which the air was extracted,
 had the power of moving light bodies; and
 Mr. Allamand likewise found, that it made
 no difference whether the vessel had air in it
 or not †.

ABOUT the same time also, Mr. Boze took
 a great deal of pains to determine, whether
 the weight of bodies would be affected by
 electricity, but he could not find that it
 was.

THE electrical *star*, made by turning
 swiftly round an electrified piece of tin,
 cut with points equidistant from the center;
 and also the *electrical bells*, which will be
 described hereafter among the surprising and
 diverting experiments performed by the help
 of electricity, were of German invention ‡.
 The star was contrived by Mr. Gordon, and
 by turning the points a little obliquely, he
 was surprised to find it began to move of it-
 self §. Lastly, to these it may be added, that
 Mr. Winckler contrived a wheel to move by
 electricity; that Mr. Boze conveyed electrici-
 ty from one man to another by a jet of wa-
 ter, when they were both placed upon cakes of
 rosin, at the distance of six paces; and that
 Mr. Gordon even fired spirits by a jet of water ||.

* Histoire, p. 89.

† Dantick Memoirs, Vol. ii. p. 426.

‡ Nollot's Recherches, p. 187.

§ Dantick Memoirs, Vol. ii. p. 317, 358.

|| Phil. Trans. abridged, Vol. x. p. 276.

MR. GOTTFRIED HEINRICK GRUMMERT, of Biala in Poland, made a curious experiment upon electric light, which, as we shall see, was afterwards made and pursued to great advantage by Dr. Watson and Mr. Canton. In order to observe whether an exhausted tube would give light when it was electrified, as well as when it was excited, he presented one, eight inches long, and a third of an inch wide, to the electrified conductor, and was surprised to find the light dart very vividly the whole length of the tube. He also observed, that, some time after the tube had been presented to the conductor, and exposed to nothing but the air, it gave light again, without being brought to any electrified body. This light *in vacuo* Mr. Grummert proposed to make use of in mines and places, where common fires, and other lights cannot be had, and for this purpose he mentions several methods of increasing this light *.

I SHALL conclude this account of the discoveries of the German Philosophers in this period with a very curious one of professor Kruger, concerning the change made in the colour of bodies by the electric effluvia. In order to try, whether there was any thing of sulphur in these effluvia, he exposed the red leaves of wild poppies to the electric spark, and found that they were presently changed to white. He was not able to produce any change in yellow colours, nor in

* Dantzick Memoirs, Vol. i. p. 417.

blue immediately; but found that when they had lain a day or two, after being exposed to this operation they became white. In these experiments the leaves were fastened with white wax to plates of tin *.

SUCH a general attention was excited to electricity by these curious discoveries, that, in the year 1745, electrical experiments were exhibited, in Germany and Holland, for money, as a show; and public advertisements appeared in the news-papers for that purpose †.

The firing of the effluvia of bodies, which was first done in Germany, was soon after repeated in England, and among others by Dr. Miles; who, as appears by a paper of his, read at the Royal Society, March the 7th, 1745, kindled phosphorus by the application of the excited tube itself to it without the intervention of any conductor ‡.

THIS gentleman's tube happening to be in excellent order upon this occasion, he observed, and was perhaps the first who observed, *pencils of rays*, which he calls *coruscations*, darting from the tube, without the aid of any conductor approaching it. Of these coruscations he gave a drawing, which answers pretty exactly to the appearance of such pencils as are now very common, particularly since Mr. Canton has taught us the use of the amalgama, by which a tube may be excited

* Dantzick Memoirs, Vol. iii. p. 393.

† Ibid. Vol. ii. p. 399.

‡ Phil. Trans. abridged, Vol. x. p. 272.

much more strongly than it could have been before *.

BUT the most distinguished name in this period of the history of electricity, is that of Dr. WATSON. He was one of the first among the English who took up, and improved upon, the discoveries made by the Germans; and to his ingenuity, and intense application, we owe many curious improvements and discoveries in electricity. His first letters to the Royal Society on this subject are dated between March 28th, and October 24th, 1745.

DR. WATSON's attention to the subject of electricity seems first, or principally, to have been engaged by the accounts of the Germans having fired spirit of wine by it. In this experiment he succeeded; and, moreover, found that he was able to fire, not only the ethereal spirit of Frobenius, and rectified spirit of wine, but even common proof spirit. He also fired air made inflammable by a chemical process †. He even fired both spirit of wine, and inflammable air, by a drop of cold water, thickened with a mucilage made with the seed of flea-wort, and even with ice ‡. He also fired these substances with a hot poker electrified, when it would not fire them in any other state §. He fired gunpowder and discharged a musket by the power of electricity, when the gunpowder had been ground with a little camphor, or a few drops of some

* Phil. Trans. abridged, Vol. x. p. 272.

† Ibid. p. 286.

‡ Ibid. p. 290.

§ Ibid. p. 288.

inflammable chemical oil *. Lastly, it was a discovery of Dr. Watson's, that these substances were capable of being fired by what he calls *the repulsive power of electricity*; which was performed by the electrified person holding the spoon which contained the substance to be fired, and another person, not electrified, bringing his finger to it †. Before this time, the substance to be fired had always been held by a person not electrified.

IN his attempts to fire electrics per se, as turpentine, and balsam of capivi, by this repulsive power, he thought he confuted an opinion which had prevailed among many persons, that electricity only floated on the surfaces of bodies, for he found that the fume of these substances could not be fired by a spark fetched from the spoon which contained them. This spark must therefore pass through the electric, from the surface of the spoon below, which was in contact with the electrified conductor.

ELECTRIFYING a number of pieces of fine spun glass, and other pieces of wire, of the same length and thickness, he was agreeably amused by observing, that the threads of glass jumped to the electrified body, and adhered to it without any snapping; whereas the wires jumped up and down very fast, giving a snap, and a small flame, every time ‡.

* Phil. Transf. abridged, Vol. x. p. 289.

† Ibid. p. 281.

‡ Ibid. p. 286.

IN a paper read at the Royal Society, February 6th, 1746; he observed, that electric sparks appeared different in colour and form, according to the substances from which they proceeded; that the fire appeared much redder from rough bodies, as rusty iron, &c. than from polished bodies, though they were ever so sharp, as from polished scissars, &c. He judged that the different appearance was owing rather to the different reflection of the electric light from the surface of the bodies from which it was emitted, than to any difference in the fire itself*.

He also observed, that electricity suffered no refraction in pervading glass; having found, by exact observations, that its direction was always in right lines, even through glasses of different forms, included one within another, and large spaces left between each glass†; that if books or other non-electrics were laid upon glass and interposed between the excited electric and light bodies, the direction of the virtue was still in right lines, and seemed instantly to pass through both the books and the glass. In these experiments he constantly observed, that the electric attraction through glass was much more powerful when the glass was made warm than when it was cold‡. He sometimes found electricity to pervade, though in small quantities, electrics of above four inches thick§.

* Phil. Trans. abridged, Vol. x. p. 200.

† Ibid. p. 292.

§ Ibid. p. 295.

† Ibid. p. 291.

HE says, that in electrifying substances of great extent, the attractive power was first observed at that part of it which was most remote from the excited electric.

HE made some experiments which showed, that the fire of electricity was affected, neither by the presence, nor the absence of other fire. One of his experiments was made with a chemical mixture, thirty degrees below the freezing point of Fahrenheit's thermometer; from which, when electrified, the flashes were as powerful, and the strokes as smart, as from red hot iron *.

IN a sequel to the above experiments, read the 30th of October 1746, Dr. Watson mentions his having lined a glass globe to a considerable thickness with a mixture of wax and rosin; but he found no difference between that and the other globes †.

HE also made various experiments with a number of globes, whirled at the same time, and having one common conductor; and concluded from them, that the power of electricity was increased by the number and size of the globes, to a certain degree, but by no means in proportion to their number and size. Yet the Doctor allows a very great increase, in an inference he makes from these very experiments. As bodies to be electrified, he says, will only contain a certain quantity of electricity; when that quantity is acquired, *which is soonest done by a number of globes*, the

* Phil. Trans. abridged, Vol. x, p. 293.

† Ibid. p. 295.

furcharge is dissipated as fast as it is excited. So that, it is plain, more fire was collected by the number of globes, though the form of the conductor he made use of was such as could not retain it. The great power of his four globes united, is manifest from his own account of them. For, he says, that when two pewter plates were held, one in the hand of an electrified person, and the other in the hand of one who stood upon the floor; the flashes of pure and bright flame were so large, and succeeded each other so fast, that, when the room was darkened, he could distinctly see the faces of thirteen persons who stood round the room*.

LASTLY, the Doctor found, that the smoke of original electrics was a conductor of electricity, and also that flame would conduct the whole of it undiminished; by observing that two persons, standing upon electrics, could communicate the virtue to each other, with nothing interposed but the smoke in the one case, and flame in the other†.

IT was in this period that Mr. Du Tour discovered that flame would destroy electricity; as he informed the Abbé Nollet, in a letter dated 21st August 1745. The same was also discovered by Mr. Waitz.

* Phil. Transf. abridged, Vol. x. p. 295.

† Ibid. p. 296.

P E R I O D VIII.

THE HISTORY OF ELECTRICITY, FROM
THE DISCOVERY OF THE LEYDEN PHIAL
IN THE YEARS 1745 AND 1746, TO DR.
FRANKLIN'S DISCOVERIES.

S E C T I O N I.

THE HISTORY OF THE LEYDEN PHIAL
ITSELF, TILL DR. FRANKLIN'S DISCO-
VERIES RELATING TO IT.

THE end of the year 1745, and the beginning of 1746 were famous for the most surprising discovery that has yet been made in the whole business of electricity, which was the wonderful accumulation of its power in glass, called at first the LEYDEN PHIAL; because made by Mr. Cuneus a native of Leyden, as he was repeating some experiments which he had seen with Messrs. Muschenbroeck, and Allamand, professors in the university of that city *. But the person who first made this great discovery, was Mr. Von Kleist, dean of the cathedral in Camin; who, on the 4th of November 1745, sent an account of it to Dr. Lieberkuhn at Berlin. This account, as taken by Mr. Gralath out

* Dalibard's *Histoire abrégée*, p. 33.

of the register of the academy at Berlin, to which it had been communicated, is as follows. “ When a nail, or a piece of thick
 “ brass wire, &c. is put into a small apothecary’s phial and electrified, remarkable
 “ effects follow : but the phial must be very
 “ dry, or warm. I commonly rub it over
 “ before-hand with a finger, on which I put
 “ some pounded chalk. If a little mercury
 “ or a few drops of spirit of wine, be put
 “ into it, the experiment succeeds the better.
 “ As soon as this phial and nail are removed
 “ from the electrifying glass, or the prime
 “ conductor, to which it hath been exposed,
 “ is taken away, it throws out a pencil of
 “ flame so long, that, with this burning machine in my hand, I have taken above sixty
 “ steps, in walking about my room. When
 “ it is electrified strongly, I can take it into
 “ another room, and there fire spirits of wine
 “ with it. If while it is electrifying, I put
 “ my finger, or a piece of gold, which I
 “ hold in my hand, to the nail, I receive a
 “ shock which stuns my arms and shoulders.

“ A TIN tube, or a man, placed upon
 “ electrics, is electrified much stronger by this
 “ means than in the common way. When
 “ I present this phial and nail to a tin tube,
 “ which I have, fifteen feet long, nothing but
 “ experience can make a person believe how
 “ strongly it is electrified. I am persuaded,
 “ he adds, that, in this manner, Mr. Boze
 “ would not have taken a second electrical
 “ kiss. Two thin glasses have been broken

“ by the shock of it. It appears to me very
 “ extraordinary, that when this phial and nail
 “ are in contact with either conducting or
 “ non-conducting matter, the strong shock
 “ does not follow. I have cemented it to
 “ wood, metal, glass, sealing-wax, &c. when
 “ I have electrified without any great effect.
 “ The human body, therefore, must contri-
 “ bute something to it. This opinion is con-
 “ firmed by my observing, that, unless I hold
 “ the phial in my hand, I cannot fire spirits
 “ of wine with it *.”

NOTWITHSTANDING Mr. Kleist immediately communicated an account of this famous experiment (which indeed it is evident he has but imperfectly described) to Mr. Winckler at Leipfick, Mr. Swietniki of Dantzick, Mr. Kruger of Hall, and to the professors of the academy of Lignitz, as well as to Dr. Lieberkuhn of Berlin above-mentioned, they all returned him word, that the experiment did not succeed with them. Mr. Gralath of Dantzick, was the first with whom it answered; but this was not till after several fruitless trials, and receiving farther instructions from the inventor †.

THE Abbé Nollet had information of this discovery, and, in consequence of it, says, in a letter to Mr. Samuel Wolfe, of the Society of Dantzick, dated March 9th, 1746, that the experiment at Leyden was upon principles similar to this made with a phial half full of

* Dantzick Memoirs, Vol. i. p. 407.

† Ibid. p. 411.

water, and a nail dipped in it; and that this discovery would have been called the Dantzick experiment, if it had not happened to have got the name of that of Leyden*.

THE views which led to this discovery in Holland were, as I have been informed, as follows. Professor Muschenbroeck and his friends, observing that electrified bodies, exposed to the common atmosphere, which is always replete with conducting particles of various kinds, soon lost their electricity, and were capable of retaining but a small quantity of it, imagined, that, were the electrified bodies terminated on all sides by original electrics, they might be capable of receiving a stronger power, and retaining it a longer time. Glass being the most convenient electric for this purpose, and water the most convenient non-electric, they first made these experiments with water, in glass bottles: but no considerable discovery was made, till Mr. Cuneus, happening to hold his glass vessel in one hand, containing water, which had a communication with the prime conductor, by means of a wire; and, with the other hand, disengaging it from the conductor (when he imagined the water had received as much electricity as the machine could give it) was surprised by a sudden shock in his arms and breast, which he had not in the least expected from the experiment.

* Dantzick Memoirs, Vol. i. p. 409.

MR. ALLAMAND and Mr. Muschenbroeck were the first who repeated and published an account of this experiment in Holland, the Abbé Nollet and Monsieur Monnier in France, and Messrs. Gralath and Rugger in Germany*.

It is extremely curious to observe the descriptions which philosophers, who first felt the electrical shock, give of it; especially as we are sure we can give ourselves the same sensation, and thereby compare their descriptions with the reality. Terror and surprize certainly contributed not a little to the exaggerated accounts they gave of it; and, could we not have repeated the experiment, we should have formed a very different idea of it from what it really is, even when given in greater strength than those who first felt this electrical shock were able to give it. It will amuse my readers if I give them an example or two.

MR. MUSCHENBROECK, who tried the experiment with a very thin glass bowl, says, in a letter to Mr. Reaumur, which he wrote soon after the experiment, that he felt himself struck in his arms, shoulder and breast, so that he lost his breath, and was two days before he recovered from the effects of the blow and the terror. He adds, that he would not take a second shock for the kingdom of France†.

* Dantzick Memoirs, Vol ii. p. 433.

† Histoire, de l'électricité, p. 30.

THE first time Mr. Allamand made this experiment (which was only with a common beer glass) he says, that he lost the use of his breath for some moments; and then felt so intense a pain all along his right arm, that he at first apprehended ill consequences from it, though it soon after went off without any inconvenience *. But the most remarkable account is that of Mr. Winckler of Leipzig. He says, that the first time he tried the Leyden experiment, he found great convulsions by it in his body; and that it put his blood into great agitation; so that he was afraid of an ardent fever, and was obliged to use refrigerating medicines. He also felt an heaviness in his head, as if a stone lay upon it. Twice, he says, it gave him a bleeding at his nose, to which he was not inclined. His wife (whose curiosity, it seems, was stronger than her fears) received the shock only twice, and found herself so weak, that she could hardly walk; and, a week after, upon recovering courage to receive another shock, she bled at the nose after taking it only once †.

WE are not, however, to infer from these instances, that all the electricians were struck with this panic. Few, I believe, would have joined with the cowardly professor, who said that he would not take a second for the kingdom of France. Far different from these were the sentiments of the magnanimous Mr. Boze, who with a truly philosophical heroism, worthy

* Phil. Transf. abridged, Vol. x. p. 321.

† Ibid. p. 327.

of the renowned Empedocles, said he wished he might die by the electric shock, that the account of his death might furnish an article for the memoirs of the French Academy of Sciences *. But it is not given to every electrician to die in so glorious a manner as the justly envied Richman.

It was this astonishing experiment that gave eclat to electricity. From this time it became the subject of general conversation. Every body was eager to see, and, notwithstanding the terrible account that was reported of it, to *feel* the experiment; and in the same year in which it was discovered, numbers of persons, in almost every country in Europe, got a livelihood by going about and showing it.

WHILE the vulgar of every age, sex, and rank were viewing this prodigy of nature and philosophy with wonder and amazement, we are not surprized to find all the electricians of Europe immediately employed in repeating this great experiment, and attending to the circumstances of it. Mr. Allamand remarked, that, when he first tried it, he stood simply upon the floor, and not upon cakes of rosin. He said, that it did not succeed with all kinds of glass; for that, though he had tried several, he had perfect success with none but that of Bohemia, and that he had tried English glasses without any effect at all †. Professor Muschenbroeck at that time only

* Histoire, p. 164.

† Phil. Trans. abridged, Vol. x. p. 321.

observed,

observed, that the glass must not be all wet on the outside.

It is no wonder that so few of the properties of glass charged with electrical fire were known at first, notwithstanding the attention that was immediately given to the subject by all the electricians in Europe. The experiment is, to this day, justly viewed with astonishment by the most profound electricians: for, though some remarkable phenomena of it have been excellently accounted for by Dr. Franklin, and others, much remains to be done; and, in many respects, the circumstances attending it are still inexplicable. What will result from more attention being given to it, time only can show.

To begin the farther illustration of this discovery with such of the phenomena as were observed in Germany, where it was made. Mr. Galath made the shock much stronger by using a glass vessel five inches in diameter, with a narrow neck, ten inches long; by substituting an iron wire with a knob of tin for the iron nail, and water for spirit of wine*. He first found, that the same shock could be communicated to a number of persons, who took hold of one another's hands; if the person at one extremity of the line they made, touched the outside of the phial, and he at the other touched a wire communicating with the inside. In this manner, on the 10th of April, 1746, he gave

* Dantzick Memoirs, Vol. ii. p. 411.

a shock to twenty persons; and he says, he did not doubt, but it might be given to a thousand*. When these persons were connected by pieces of metal, and did not hold one another's hands, they found the same shock, but not when they held wood, and other imperfect conductors†. This gentleman, also, gave a shock, by means of long wires, to a person standing in a garden, while he himself, who directed the experiment, had the machine in a part of the house at a considerable distance‡; and he was the first who made what we now call an *electrical battery*; for he increased the shock by charging several phials at the same time§. Lastly he observed that if the phial had the least crack in it, it could never give a shock; and also that when a phial was discharged it acquired a small charge by standing, without receiving any thing from the machine, so as to give a small shock||. This is what we now call the *residuum* of a charge, and is properly that part of the charge that lay on the uncoated part of the phial, which doth not let go all its electricity at once; so that it is, afterwards, gradually diffused to the coating.

MR. WINCKLER found out the method of making the discharge of the phial without feeling the shock himself, by not bringing his own body into the direct circuit¶. He also gave the shock when several ells of run-

* Dantzick Memoirs, Vol. ii. p. 439.

† Ibid. p. 440.

‡ Ibid.

§ Ibid. p. 552.

|| Ibid. Vol. i. p. 514, 516.

¶ Ibid. Vol. ii. p. 459.

ning water, in his garden, made part of the circuit. This he did on the 28th of July 1746, about the same time that Monsieur Monnier performed the same experiment in France*. And, lastly, he found, that the more globes were used, and the larger they were, the stronger was the shock†. This must be the consequence of increasing the power of excitation.

WHEN Mr. Jallabert made the Leyden experiment with hot water, the phial broke by a spontaneous discharge, and a circular piece, two lines and a half in diameter, was thrown from the place of the rupture, against a wall, which was at the distance of five feet. The vessel, he says, had no crack, or other injury‡.

DR. WATSON, who gives an account of this famous experiment in the Philosophical Transactions, observes, that it succeeded best when the phial, which contained the water, was of the thinnest glass, and the water warmer than the ambient air. He says he tried the effect of increasing the quantity of water in glass vessels of different sizes, as far as four gallons, without in the least increasing the stroke. He also observed, that the force of the stroke did not increase in proportion to the size of the globe, or the number of globes employed upon the occasion; for that he had been as forcibly struck with a phial

* Dantzick Memoirs, Vol. iii. p. 504.

† Ibid. p. 526.

‡ Jallabert's Experiences, p. 128.

charged by means of a globe of seven inches in diameter, as from one of sixteen, or from three of ten; and that, at Hamburgh, a sphere had been employed of a Flemish ell in diameter, without the expected increase of power. But, in both these observations, there must have been some mistake. He found; that if mercury was used instead of water, the stroke was by no means increased in proportion to its specific gravity. He also first observed, that several men, touching each other; and standing upon electrics, were all shocked, though only one touched the gun barrel; but that no more fire was visible from them all, than if one had only discharged it.

SEVERAL of these observations show how imperfectly this great experiment was understood, for some time after it was first made. Dr. Watson, however, observed a circumstance attending the charging of the phial, which, if pursued, would have led him to the discovery, which was afterwards made by Dr. Franklin. He says, that “when the phial is well electrified, and you apply your hand thereto, you see the fire flash from the outside of the glass, wherever you touch it, and it crackles in your hand *.”

He also observed, that when a single wire only was fastened round a phial, properly filled with warm water, and charged; upon the instant of its explosion, the electrical coruscations were seen to dart from the wire,

* Philosophical Transactions abridged, Vol. x. p. 298.

and to illuminate the water contained in the phial.

SEVERAL other very important circumstances, relating to the discharge of the phial, were observed by Dr. Watson. He found that the stroke was, *cæteris paribus*, as the points of contact of the non-electrics on the outside of the glass. And upon showing Dr. Bevis the experiments which proved this assertion, the Doctor suggested a more clear and satisfactory method of proving it, and which has been the means of accumulating and increasing the force of charged glass, far beyond what was expected from the first discovery of it. This method was, *coating* the outside of the phial, very near to the neck, with sheet-lead, or tinfoil. When a bottle was prepared in this manner, and nearly filled with water, they observed, that a person who only held in his hand a small wire communicating with that coating, felt as strong a shock as he would have felt, if his hand had been in actual contact with every part of the phial touched by the coating*.

DR. WATSON also discovered, that the electrical power, in the discharge of the phial, darts *rectissimo cursu*, as he styles it, between the gun-barrel and the phial; and, though it is not strictly true, that the shock goes the nearest way, yet it does so *cæteris paribus*, which alone was a considerable discovery for that time. He observed, that, in a company

* Phil. Transf. abridged, Vol. x. p. 299.

joining hands, a person touching two other persons in the circle, who did themselves touch one another, felt nothing of the shock, his body making no necessary part of the circle; and also, if a man, holding a wire, which communicated with the outside of the phial, as it hung upon the conductor, should touch the conductor with it; the explosion was made, but the man felt nothing*.

IN a paper read at the Royal Society, January 21st, 1748, Dr. Watson mentions another discovery relating to the Leyden phial, which Dr. Bevis suggested, and he completed. Having been before fully satisfied, that the shock from the phial was not in proportion to the quantity of matter contained in the glass, but was increased by it, and likewise by the number of points of non-electric contact on the outside of the glass; he procured three jars, into which he put round leaden shot, and, joining their wires and coating, discharged them all as one jar. Upon this he observed, that the electrical explosion from two or three of those jars was not double or treble to that from one of them; but that the explosion from three was much louder than that from two, and the explosion from two much louder than that from one†.

THIS experiment had induced him to imagine, that the explosion from those jars was owing to the great quantity of non-electric matter contained in them. And whilst he was

* Phil. Transf. abridged, Vol. x. p. 301.

† Ibid, p. 374.

considering of some certain method of assuring himself whether the fact was so, Dr. Bevis informed him, that he had found the electrical explosion to be as great from covering the sides of a pane of glass, within about an inch of the edge (which was a curious improvement of Mr. Smeaton's), as it could have been from an half pint phial of water. Upon this Dr. Watson coated large jars with leaf silver, both inside and outside, within an inch of the top, and from the great explosion he produced, when so little non-electric matter was contained in them, he was of opinion, that the effect of the Leyden bottle was greatly increased by, if it was not principally owing to, not so much the quantity of non-electric matter contained in the glass, as the number of points of non-electric contact within the glass, and the density of the matter of which those points consisted; provided the matter was, in its own nature, a ready conductor of electricity. He also observed, that the explosion was greater from hot water inclosed in glasses, than from cold; and from his coated jars warmed, than when cold*.

THE Doctor observed, that when the circle for the discharge was not through perfect conductors, the explosion was made slowly, and not all at once. This law, he says, was invariable, but he was not able to account for it. But to prove that the electricity passed with its whole force through the circle of

* Phil. Trans. abridged, Vol. x. p. 377.

non-electrics, he made a circuit consisting of iron bars, and spoons filled with spirits between each bar (but at some small distance from them), and, upon the explosion, all the spoons were on fire at once. This was the first time, as he observes, that spirits were fired without either the spirits, or the non-electric on which they were placed, being insulated, or put upon original electrics. And yet, he says, though we know, from its effects, that the electricity goes through the whole circuit of non-electrics, with all its vigour, its progress is so quick, as not to affect, by attracting or otherwise, any light bodies disposed very near the non-electrics, through which it must necessarily pass *.

IT is curious to observe in what manner Dr. Watson explained the shock of the Leyden phial, about the time that he first made the experiment with it. He had then been led (by a course of experiments which will be mentioned hereafter) to the notion both of the *afflux*, and *efflux* of electric matter in all electrical experiments. To apply this principle to the case in hand, he supposed, that the man who felt the shock parted with as much of the fire from his body, as was accumulated in the water and the gun-barrel; and that he felt the effect in both arms, from the fire which was in his body, rushing through one arm to the gun barrel, and through the other to the phial. He imagined also, that as much

* Phil. Transf. abridged, Vol. x. p. 378.

fire as the man parted with was instantly replaced from the floor of the room, and that with a violence equal to the manner in which he lost it. It also appears, from Dr. Watson's remarks on some subsequent experiments of Mr. Monnier, that he then imagined, that though a considerable quantity of the electric matter pervaded the glass (as he thought was seen upon presenting a non-electric body to it, when it stood upon the glass stand, and without which it could not be charged at all) yet, that the loss of the electric matter this way was not equal to what came in by the wire; the thinness of the glass permitting it not wholly, but partially to stop the electricity*.

AFTERWARDS, when (from a course of experiments, which will also be recited in their proper place) Dr. Watson changed his opinion about this afflux and efflux of electric matter, with a generosity and frankness becoming every inquirer after truth, he retracted this hypothesis; and, in refutation of it, he farther adds, that the charged phial will explode with equal violence, if the hoop of the wire be bent, so as to come near the coating of the phial, without any other non-electric body being near, from which such a quantity could be supplied. He had also observed, that if a man stood upon glass, and discharged the phial, he felt the same shock as if he had stood upon the floor. I shall sub-

* Phil. Trans. abridged, Vol. x. p. 348.

join a remarkable paragraph of the Doctor himself upon this occasion, as I think it very applicable even to us, in this more advanced state of the science.

“ I TAKE notice of these,” says the Doctor, “ in as much as, notwithstanding the
 “ very great progress which has been made
 “ in our improvements in this part of natural
 “ philosophy, within these few years; posterity will regard us as only in our *noviciate*;
 “ and therefore it behoves us, as far as we
 “ can be justified therein by experiment, to
 “ correct any conclusions we may have
 “ drawn, if others yet more probable present
 “ themselves *.” The Doctor has lived to see, not only that *posterity* would consider him and his assistants at that time, as in their noviciate; but he *himself*, already in the course of a few years, looks upon both himself and them in the same light. And, considering the quick advances still making in this science, it is to be hoped he may still live to see, even the electricians of the present year to have been only in their noviciate.

HAVING seen what was done by Dr. Watson towards explaining the electric shock, before it was undertaken by Dr. Franklin; let us see what obligations we are under to other English electricians, and particularly Mr. Wilson.

MR. WILSON says that, as early as the year 1746, he discovered a method of giving

* Phil. Transf. abridged, Vol. x. p. 373.

the shock to any particular part of the body without affecting the rest *. He increased the strength of the shock by plunging the phial in water, thereby giving it a coating of water on the outside, as high as it was filled on the inside †.

IN a letter to Mr. Smeaton, dated Dublin October 6th, 1746, he mentions his having made some experiments, in order to discover the law of accumulation of the electric matter in the Leyden bottle; and found, that it was always in proportion to the thinness of the glass, the surface of the glass, and that of the non-electrics in contact with the inside and outside thereof. The experiments, he says, were made with water a little warmed, which was poured into the bottle, while the outside was immersed in a vessel filled with water, but a little colder; leaving three inches, or thereabout, uncovered, which was preserved dry and free from dust. An account of this experiment he wrote to Mr. Folkes, and it was read before the Royal Society, October 23d, 1746, as appears by their minutes of that day, though the original was lost or mislaid.

ANOTHER curious experiment Mr. Wilson made, in order to prove an hypothesis, which he conceived very early, of the influence of a subtle medium surrounding all bodies, and resisting the entrance or exit of the electric fluid. To determine this, he made

* Wilson's Essay, p. 88.

† Ibid. p. 71.

the Leyden experiment with a chain, and considered each link of it as having two surfaces, at least; so that lengthening or shortening the chain, in each experiment, would occasion different resistances; and the event, he says, proved accordingly. When he made the discharge with one wire only, he found the resistance to be less than when a chain was used. But to leave no room for doubt, he caused the chain to be stretched with a weight, that the links might be brought nearer into contact, and the event was the same as when a single wire had been used *.

Two circuits being made, one consisting of the arms of a man, and the other of the links of a chain; he found, that the fire would take the arms of the man; but that if the chain were stretched, it would take the chain. No person, he says, who has not made the experiment, would imagine, with how much force the chain must be stretched before the experiment will answer, and the electric fluid pass through it without producing a spark at any of the links; that is, before the links can be brought into absolute contact with one another, their own weight being by no means sufficient †.

MR. WILSON observed, that if one part of the Leyden phial was ground very thin, and covered with sealing-wax till it was charged, and then had the sealing-wax taken off, and a conductor communicating with the earth

* Letter to Hoadley. † Wilson and Hoadley, p. 65.

touched the thin part, the charge would be dissipated in nearly half the time that it otherwise would have been *.

HE 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. To shew this to the most advantage, he set a charged phial upon a glass stand, and placed several pieces of brass upon the stand, one of them in contact with the chain that formed the circuit, and others a twentieth of an inch from it, or from one another; and, upon making the discharge, there was a spark visible between each of them †.

ANALOGOUS, in some respects, to this, was Mr. Wilson's 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.

HE also observed, that when the phial was coated, within and without, with metals, the first explosion bore the greatest proportion to the subsequent ones, the whole charge being dissipated almost at once; whereas, when water was used, the subsequent explosions were more in number, and more considerable; and that when the phial was charged with nothing but a wire inserted into it, the first explosion

* Wilson's Essay, p. 74.

† Ibid. p. 90.

and the subsequent ones were still more nearly equal.

MR. WILSON once happening to break a small wire by the convulsive shock given to his arms by the Leyden phial, he fastened to his hands, well guarded with leather, a large wire, of the thickness of a slender knitting needle, and placed himself in such a manner, that it would necessarily be stretched, if his arm should be convulsed again. He accordingly discharged the phial, and this wire was broken, like the former*.

MR. GEORGE GRAHAM shewed how several circuits for the discharge of the Leyden phial might be made at the same time, and the fire be made to pass through them all. He made a number of persons take hold of a plate of metal, communicating with the outside of the phial; and all together, likewise, laid hold of a brass rod with which the discharge was made; when they were all shocked at the same time, and in the same degree †.

LASTLY, Mr. Canton found, that if a charged phial was placed upon electrics, the wire and the coating would give a spark or two alternately; and that, by continuing this operation, the phial would be discharged ‡. This discovery, which is the first that I find recorded of this excellent philosopher, to whom the science of electricity owes so much, has a near affinity to the great discovery of

* Wilson's Essay, p. 84.

† Ibid. p. 128.

‡ Ibid. p. 64.

Dr. Franklin; but he did not then observe, that those alternate sparks proceeded from the two contrary electricities. This history will furnish many more instances of persons being on the eve of great discoveries, without actually making them. Both Mr. Galath and Mr. Richman observed, in several cases, a stronger spark between two bodies, when both of them were electrified, than when only one of them was in that state; but neither of them suspected that the electricities were of a different kind *.

WE have seen what observations the English philosophers had made upon the Leyden experiment before the time of Dr. Franklin; let us now take a view of what was done by electricians in other parts of the world, within the same period.

As Mr. Muschenbroeck's letter to Mr. Reaumur, concerning the experiment of the phial, came at a time when many learned men were employed about electricity, the Abbé Nollet, and Mr. De Monnier, gentlemen of the academy, zealous to search into so extraordinary a phenomenon, divesting themselves of the fear with which the professor's letter had justly inspired them, made the experiment upon themselves, and, in like manner, said they found the commotion very terrible. The report of it instantly spread through the court and the city, from whence all ranks of men crowded to see this new

* Wilcke's Preface to the German Version of Franklin's Letters.

kind of thunder, and to experience the effect of it*.

THE Abbé Nollet was the first who made experiments upon the phial in France, and the result of many of them was the same with what Dr. Watson had discovered, for which reason I shall not recite them here. They may all be seen at one view in his *Leçons de physique*, p. 481. The circumstances which the English philosophers had not attended to, are the following.

THE Abbé received a shock from a bottle out of which the air had been exhausted, and into which the end of his conductor had been inserted. As he was amusing himself with observing the beautiful irradiations of the electric light in the vessel, it immediately occurred to him, that, being so strongly electrified, it could not fail to give a shock similar to that of the Leyden phial, and without any farther reflection, laying one hand on the vessel, and bringing the other to the conductor, his conjecture was verified, but in a manner that gave him more pain than he could have wished. The blow he received was greater, he says, than he ever felt from the Leyden experiment in any other form†.

IN the same place he observes, that he never considered the water in the phial as of any use, but to convey the electric matter into the inside of the glass; and that he ascribed the force of the glass in giving a shock, to

* Nollet's *Leçons de Physique*, p. 452. Ac. Par. 1746. M.
p. 5.

† *Recherches*, p. 426.

that property of it, whereby it retained it more strongly than conductors do, and was not so easily divested of it as they are. The Abbé also gave the shock with porcelain, and observed, that some persons were much more sensible to it than others in whatever part of the circuit they were placed *.

MR. MONNIER is said, by Mr. Buffon, to have been the first who discovered that the Leyden phial would retain its electricity a considerable time after it was charged, and to have found it do so for thirty-six hours, in time of frost. He frequently electrified his phial at home, and brought it in his hand, through many streets, from the college of Harcourt to his apartments in the King's garden, without any considerable diminution of its efficacy †.

In France as well as in Germany experiments were made to try how many persons might feel the shock of the same phial. The Abbé Nollet, whose name is famous in electricity, gave it to one hundred and eighty of the guards, in the King's presence; and at the grand convent of the Carthusians in Paris, the whole community formed a line of nine hundred toises, by means of iron wires between every two persons (which far exceeded the line of one hundred and eighty of the guards) and the whole company upon the discharge of the phial, gave a sudden spring, at

* Ac. Par. 1746, M. p. 11, 26.

† Phil. Trans. abridged, Vol. x. p. 333.

the same instant of time, and all felt the shock equally*.

MR. NOLLET also tried the effect of the electric shock upon two birds, one of which was a sparrow, and the other a chaffinch, which, as far as I can find, were the first brute animals of any kind that ever received it. The consequence was, that upon the first shock, they were both instantaneously struck motionless, and, as it were, lifeless, though for a time only; for they recovered some few minutes after. Upon the second shock, the sparrow was struck dead, and, upon examination, was found livid without, as if it had been killed with a flash of lightning; most of the blood vessels in the body being burst by the shock. The chaffinch revived as before†. Fishes were also killed with the electric shock, by the Abbé, and others.

THE circumstance of the blood vessels of the sparrow being burst is pretty singular. I have seen no such effect, when smaller animals have been killed by a shock fifty times as great as, it is probable, the Abbé used upon this occasion.

THE Abbé Nollet, as well as Mr. Jallabert, mentions the bursting of glass vessels by the electric explosion. They were pierced, he says, with round holes, three or four lines in diameter‡.

IT seems that the French philosophers, as well as the English, had observed, that, if the

* Phil. Trans. abridged, Vol. x. p. 335. † Ibid p. 336.
Ac. Par. 1746. M. p. 32. ‡ Nollet's Lettres, Vol. i. p. 42.

phial stood upon glass, it could not be charged, except a person's hand, or some other non-electric substance were brought near to it. Upon this they imagined the fire streamed out of the hand, and passed through the substance of the phial into the water *. This fact surprised them very much, as it well might. Mr. Monnier observed, that a light body would be attracted by a charged phial, as it stood upon the table, and produced a spark if any person touched the wire; but that the light body must be suspended by a non-electric substance †. He likewise found, that when the charged phial stood upon glass, it might be handled with all safety ‡. These experiments seem not to have been made with proper circumspection: for by an attention to these very circumstances, Dr. Franklin was afterwards led to the great discovery of the different quality of the electricity, on different sides of the glass.

* Phil. Trans. abridged, Vol. x. p. 334.

† Ac. Par. 1746, M. p. 684.

‡ Phil. Trans. abridged, Vol. x. p. 337.

SECTION II.

THE METHODS USED BY THE FRENCH AND ENGLISH PHILOSOPHERS, TO MEASURE THE DISTANCE TO WHICH THE ELECTRIC SHOCK CAN BE CARRIED, AND THE VELOCITY WITH WHICH IT PASSES.

WE are now come to an ampler field of electrical experiments, in which we shall be spectators, not of what might be exhibited in a private room, and by a few operators, but where we shall find an amazing apparatus necessary, and a great number of assistants in the management of it; as well as the greatest judgment, and the most unwearied patience in the conduct of it.

THE French philosophers were the first to appear in this field, but they excited the English to go far beyond them in these great undertakings. It has been said already, that a circuit was made of nine hundred toises, consisting of men holding iron wires betwixt each two, through which the electric shock was sensibly felt. At another time, they made the shock pass through a wire two thousand toises in length, that is near a Paris league, or about two English miles and a half; though part of the wires dragged upon wet grass, went over charmil hedges, or palisades, and over ground newly ploughed up. Into another chain they took the water of the
basin

basin in the Thuilleries, the surface of which was about an acre, and the phial was discharged through it*. Mr. Monnier, who made this experiment, mentioning the quantity of surface in the basin of water, as if it was of consequence to the experiment, and saying it was *electrified*, (tho' all he meant by this was, that it received and transmitted the electric charge †.) Mr. Galath made several experiments, which prove, that bodies which form the circuit of the shock are not properly electrified ‡.

MR. MONNIER the younger, also endeavoured to determine the velocity of the electric matter; and for this purpose, made the shock pass through an iron wire of nine hundred and fifty toises in length, but he could not observe, that it spent a quarter of a second in passing it. He also found, that when a wire of one thousand three hundred and nineteen feet, with its extremities brought near together, was electrified; the electricity ceased at one end, the moment it was taken off at the other. This fact refuted the opinion of those who maintained, that it was the force of the electrical *shock*, which threw the electric matter with so great velocity §.

These attempts of the French gave occasion to the greater, the more accurate, and the more numerous experiments of the English.

* Phil. Trans. abridged, Vol. x. p. 336.

† Ac. Par. 1746. M. p. 678.

‡ Dantzick Memoirs, Vol. iii. p. 552.

§ Ac. Par. 1746, M. p. 686.

The names of the English gentlemen, animated with a truly philosophical spirit, and who were indefatigable in this business, deserve to be transmitted to posterity in every work of this nature.

THE principal agent in this great scene was Dr. Watson. He planned and directed all the operations, and never failed to be present at every experiment. His chief assistants were Martin Folkes, Esq. president of the Royal Society, Lord Charles Cavendish, Dr. Bevis, Mr. Graham, Dr. Birch, Mr. Peter Daval, Mr. Trembley, Mr. Ellicott, Mr. Robins, and Mr. Short. Many other persons, and some of distinction, gave their attendance occasionally.

DR. WATSON, who wrote the history of their proceedings, in order to lay them before the Royal Society, begins with observing (what was verified in all their experiments) that the electric shock is not, strictly speaking, conducted in the shortest manner possible, unless the bodies through which it passes conduct equally well; for that, if they conduct unequally, the circuit is always formed through the best conductors, though the length of it be ever so great.

THE first attempt these gentlemen made, was to convey the electric shock across the river Thames, making use of the water of the river for one part of the chain of communication. This they accomplished on the 14th and 18th of July 1747, by fastening a wire all along Westminster bridge, at a considerable
6 height

height above the water. One end of this wire communicated with the coating of a charged phial, the other being held by an observer, who, in his other hand, held an iron rod, which he dipped into the river. On the opposite side of the river, stood a gentleman, who, likewise, dipped an iron rod in the river, with one hand; and in the other held a wire, the extremity of which might be brought into contact with the wire of the phial.

UPON making the discharge, the shock was felt by the observers on both sides the river, but more sensibly by those who were stationed on the same side with the machine; part of the electric fire having gone from the wire down the moist stones of the bridge, thereby making several shorter circuits to the phial; but still all passing through the gentlemen who were stationed on the same side with the machine. This was, in a manner, demonstrated by some persons feeling a sensible shock in their arms and feet, who only happened to touch the wire, at the time of one of the discharges, when they were standing upon the wet steps which led to the river. In one of the discharges made upon this occasion, spirits were kindled by the fire which had gone through the river*.

UPON this, and the subsequent occasions, the gentlemen made use of wires, in pre-

* Phil. Trans. abridged, Vol. x. p. 394.

ference to chains, for this, among other reasons, that the electricity which was conducted by chains was not so strong, as that which was conducted by wires. This, as they well observed, was occasioned by the junctures of the links not being sufficiently close, as appeared by the snapping and flashing at every juncture, where there was the least separation. These lesser snappings, being numerous in the whole length of a chain, very sensibly lessened the great discharge at the gun-barrel.

THEIR next attempt was to force the electrical shock to make a circuit of two miles, at the New River at Stoke Newington. This they performed on the 24th of July 1747, at two places; at one of which the distance by land was eight hundred feet, and by water two thousand: in the other, the distance by land was two thousand eight hundred feet, and by water eight thousand. The disposition of the apparatus was similar to what they before used at Westminster bridge, and the effect answered their utmost expectations. But, as in both cases, the observers at both extremities of the chain, which terminated in the water, felt the shock, as well when they stood with their rods fixed into the earth twenty feet from the water, as when they were put into the river; it occasioned a doubt, whether the electric circuit was formed through the windings of the river, or a much shorter way, by the ground of the meadow: for the experiment

ment plainly shewed, that the meadow-ground, with the grass on it, conducted the electricity very well.

By subsequent experiments, they were fully convinced, that the electricity had not, in this case, been conveyed by the water of the river, which was two miles in length, but by land, where the distance was only one mile; in which space, however, the electric matter must necessarily have passed over the New River twice, have gone through several gravels pits, and a large stubble field*.

JULY 28th, they repeated the experiment, at the same place, with the following variation of circumstances. The iron wire was, in its whole length, supported by dry sticks, and the observers stood upon original electrics; the effect of which was, that they felt the shock much more sensibly than when the conducting wire had lain upon the ground, and when the observers had likewise stood upon the ground, as in the former experiment.

AFTERWARDS, every thing else remaining as before, the observers were directed, instead of dipping their rods into the water, to put them into the ground, each one hundred and fifty feet from the water. They were both smartly struck, though they were distant from each other above five hundred feet†.

* Phil. Transf. abridged, Vol. x. p. 360.

† Ibid. p. 357.

THE same gentlemen, pleased with the success of their former experiments undertook another, the object of which was, to determine, whether the electric virtue could be conveyed through dry ground; and, at the same time, to carry it through water to a greater distance than they had done before. For this purpose, they pitched upon High-bury-barn beyond Islington, where they carried it into execution on the 5th of August 1747. They chose a station for their machine, almost equally distant from two other stations for observers upon the New River; which were somewhat more than a mile asunder by land, and two miles by water. They had found the streets of London, when dry, to conduct very strongly, for about forty yards; and the dry road at Newington about the same distance. The event of this trial answered their expectations. The electric fire made the circuit of the water, when both the wires and the observers were supported upon original electrics, and the rods dipped into the river. They also both felt the shock, when one of the observers was placed in a dry gravelly pit, about three hundred yards nearer the machine than the former station, and one hundred yards distant from the river; from which the gentlemen were satisfied, that the dry gravelly ground had conducted the electricity as strongly as water.

FROM the shocks which the observers received in their bodies, when the electric power was conducted upon dry sticks, they were

were of opinion, that, from the difference of distance simply considered, the force of the shock, as far as they had yet experienced, was very little, if at all impaired. When the observers stood upon electrics, and touched the water, or the ground, with the iron rods, the shock was always felt in their arms or wrists; when they stood upon the ground with their iron rods, they felt the shock in their elbows, wrists, and ancles; and when they stood upon the ground without rods, the shock was always felt in the elbow and wrist of that hand which held the conducting wire, and in both ancles *.

THE last attempt of this kind which these gentlemen made, and which required all their sagacity and address in the conduct of it, was to try whether the electric shock was perceptible at twice the distance to which they had before carried it, in ground perfectly dry, and where no water was near; and also to distinguish, if possible, the respective velocity of electricity and sound.

FOR this purpose, they fixed upon Shooter's-hill, and made their first experiments on the 14th of August 1747, a time, when, as it happened, but one shower of rain had fallen during five preceding weeks. The wire communicating with the iron rod, which made the discharge, was six thousand seven hundred and thirty-two feet in length, and was supported all the way upon baked sticks;

* Philosophical Transactions abridged, Vol. x. p. 360.

as was also the wire which communicated with the coating of the phial, which was three thousand eight hundred and sixty-eight feet long, and the observers were distant from each other two miles. The result of the explosion demonstrated, to the satisfaction of the gentlemen present, that the circuit performed by the electric matter was four miles, viz. two miles of wire, and two of dry ground, the space between the extremities of the wires; a distance which, without trial, as they justly observed, was too great to be credited. A gun was discharged at the instant of the explosion, and the observers had stop watches in their hands, to note the moment when they felt the shock: but, as far as they could distinguish, the time in which the electric matter performed that vast circuit might have been instantaneous *.

IN all the explosions where the circuit was made of considerable length, it was observed, that though the phial was very well charged, yet that the snap at the gun barrel, made by the explosion, was not near so loud as when the circuit was formed in a room; so that a by-stander, says Dr. Watson, though versed in those operations, would not imagine, from seeing the flash, and hearing the report, that the stroke, at the extremity of the conducting wire, could have been considerable; the contrary whereof, when the wires were properly managed, he says, always happened.

* Phil. Transf. abridged, Vol. x. p. 363.

STILL the gentlemen, unwearied in these pursuits, were desirous, if possible, to ascertain the absolute velocity of electricity at a certain distance; because though, in the last experiment, the time of it's progress was certainly very small, if any, they were desirous of knowing, small as that time might be, whether it was measureable, and Dr. Watson had contrived an excellent method for that purpose.

ACCORDINGLY, on the 5th of August 1748, the gentlemen met once more, and the last time, at Shooter's-hill; when it was agreed to make an electric circuit of two miles, by several turnings of the wire, in the same field. The middle of this circuit, they contrived to be in the same room with the machine, where an observer took in each hand one of the extremities of the wires, each of which was a mile in length. In this excellent disposition of the apparatus, in which the time between the explosion and the shock might have been observed to the greatest exactness, the phial was discharged several times; but the observer always felt himself shocked at the very instant of making the explosion. Upon this the gentlemen were fully satisfied, that through the whole length of this wire, which was 12276 feet, the velocity of the electric matter was instantaneous*.

THESE experiments excited the admiration of all foreign electricians. Professor Muschen-

* Phil. Transf. abridged, Vol. x. p. 368.

broeck, who was greatly satisfied with the extent and success of them, said, in a letter to Dr. Watson, upon the occasion, *Magnificentissimis tuis experimentis superasti conatus omnium.*

It is said by some, that the last of these experiments go upon a wrong supposition, and therefore can be of no use; it being supposed that the very same particles of the electric fluid, which were thrown on one side of the charged glass, actually made the whole circuit of the intervening conductors, and arrived at the opposite side: whereas Dr. Franklin's theory only requires that the deficiency on one side of the glass be supplied from the neighbouring conductors; which may, in return, receive as much as they parted with, from the side of the glass that was overcharged. So that, to be a little more particular, the redundancy of electric matter on the charged side of a pane of glass, only passes into the bodies which form that part of the circuit which is contiguous to it, driving forward that part of the fluid which was natural to them; till, at length, the fluid which resided in those conductors which formed the last part of the circuit, passes into the exhausted side of the glass.

BUT should this be the case (though in great discharges it supposes the natural quantity of electricity in bodies to be very considerable) and should Dr. Watson, and other philosophers at that time, have conceived otherwise; it does not follow, that the experiments could
possibly

possibly determine *nothing*: for there still remains something to be measured, viz. the time required for the successive dislodging the electric fluid in the whole length of the circuit.

WERE the whole mass of the electric matter contained in all the intervening conductors absolutely solid, no motion could be made at one extremity, without producing an instantaneous motion at the other; just as if one end of a rod be struck, the motion is instantly communicated to the other end. But this cannot be the case in an elastic medium, the parts of which yield to one another. In this case, the motion is communicated in a real succession, like a vibration, running the whole length of the circuit; which must therefore take up time, and be measurable. The motion of sound may be measured, though no particle of the vibrating air be finally displaced. These great experiments of Dr. Watson, therefore, had a real object, only it appeared to be too small to be ascertained by them.

SECTION III.

MISCELLANEOUS DISCOVERIES OF DR.
WATSON, AND OTHERS, BEFORE THE
TIME OF DR. FRANKLIN.

THE first of these discoveries in order of time, and in importance second to none (except that of the shock itself, and Dr. Franklin's discovery of the different electricity of the opposite sides of the charged glass) was that of Dr. Watson, proving, that the glass tubes and globes did not contain the electric power in themselves, but only served as *first movers*, and *determiners*, as he calls it, of that power.

HE was first led to this discovery by observing, that, upon rubbing the glass tube, while he was standing upon cakes of wax (in order, as he expected, to prevent any of the electric power from discharging itself through his body upon the floor) the power was, contrary to his expectation, so much lessened, that no snapping could be observed upon another person's touching any part of his body; but that if a person not electrified held his hand near the tube, while it was rubbed, the snapping was very sensible *.

THE event was the same when the globe was whirled in similar circumstances. For if

* Phil. Trans. abridged, Vol. x. p. 303.

the man who turned the wheel, and who, together with the machine was suspended upon silk, touched the floor with one foot, the electric fire appeared upon the conductor; but if he kept himself free from any communication with the floor, no fire was produced.

DR. WATSON by this, and the following experiments in conjunction, discovered, what he calls, the complete circulation of the electric matter. He observed, that only a spark or two would issue from his hand to the insulated machine, unless he, at the same time, formed a communication between the conductor and the floor; but that then there was a constant and copious flux of the electric matter to the machine.

OBSERVING, that while his hand was in contact with the conductor, the man who turned this insulated machine gave sparks, which would fire inflammable substances, and perform other electrical experiments which were usually performed at the conductor; he naturally imagined, that the fire issued from the man, for the very same reason that all electricians had before imagined that it came from the conductor; and seeing that the man gave no fire unless there was a communication between the floor and the conductor, he concluded that, in this case, the fire was supplied by that communication, so that the course of the electricity was inverted, as he expresses it *.

* Phil. Transf. abridged, Vol. x. p. 305.

It was not then suspected, that the eye could not distinguish in what direction an electric spark proceeds. Electricians naturally imagined that all electric powers, and consequently the electric fluid, which they supposed to be the cause of these powers, existed in the excited electric, whatever it was; and that whatever powers were exerted by electrified bodies, proceeded from a real communication of electric matter to them. Accordingly, when Dr. Watson found that, by cutting off the communication of the electric with the floor, all electrical operations were stopped, he concluded, that the electric fluid was collected from the floor to the rubber, and thence conveyed to the globe. For the same reason, seeing the rubber, or the man who had a communication with it, give no sparks but when the conductor was connected with the floor, he would as naturally conclude that the globe was supplied from the conductor, as he had before concluded that it was supplied from the rubber.

COMPARING both these experiments together, Dr. Watson was led to infer, that, in all electrical operations, there was both an *afflux* of electric matter to the globe, and the conductor, and likewise an *efflux* of the same electric matter from them*.

FINDING that a piece of leaf silver was suspended between a plate electrified by the conductor, and another communicating with the

* Phil. Trans. abridged, Vol. x. p. 311.

floor, he reasons from it in the following manner. “ No body can be suspended in
 “ equilibrio but by the joint action of two
 “ different directions of power: so here, the
 “ blast of electric ether from the excited
 “ plate blows the silver towards the plate un-
 “ excited, and this last, in its turn, by the
 “ blast of electric ether from the floor setting
 “ through it, drives the silver towards the
 “ plate electrified. We find from hence,
 “ likewise, that the draught of electric ether
 “ from the floor is always in proportion to
 “ the quantity thrown by the globe over the
 “ gun barrel, or the equilibrium by which
 “ the silver is suspended could not be main-
 “ tained *.”

DR. WATSON observes, that the Abbé Nollet, two years before he made this communication, had given it as his opinion (though without any experiment which proved it) that the electric matter did not only proceed from the electrified bodies, but from all others about them, to a certain distance †.

SOME time after this, Dr. Watson observes, in a paper read at the Royal Society, January 21st, 1748, that Dr. Bevis had carried his experiment, to prove that rubbing the tube or the globe, only conveyed, and did not produce the electric matter, farther than he had done. For he had observed, above a year before, that placing one man upon electrics, to rub the tube or globe, and another

* Phil. Trans. abridged, Vol. x. p. 310.

† Ibid. p. 315.

also upon electrics to touch them, as the conductor; both the man who rubbed, and the man who touched the excited glass would give a spark; and farther, that if they touched one another, the snapping was much greater than if either of them touched a person standing upon the floor. Upon this the Doctor seems to have corrected his former opinion of the afflux and efflux of electric matter: for he accounts for this fact by supposing, that as much electricity as was taken from the person rubbing was given to him who touched the conductor, being conveyed by the globe. By this means the electricity of the former of these persons, he observes, was more rare than it naturally was, and that of the latter more dense; so that the density of electricity between these two persons differed more than that between either of them, and another person standing upon the floor. In this manner did Dr. Watson discover, what Dr. Franklin observed, about the same time, in America, and called the *plus* and *minus* in electricity*.

DR. WATSON observed that the flame at the end of an electrified wire was sensible to the hand, as a cool blast of wind, and that when light substances were attracted and repelled between an electrified plate and one communicating with the floor, the succession of these alternate attractions and repulsions was extremely quick, so that sometimes the

* Phil. Trans. abridged, Vol. x. p. 369.

eye could hardly keep pace with it; and that when a glass globe, of about an inch in diameter, very light and finely blown, was put upon a plate of metal, and another plate hung on the conductor over it, the strokes from the alternate attractions and repulsions were almost too quick for the ear. From this last experiment he likewise deduced an argument to prove the extreme velocity with which these glass globes were attracted and repelled. He says, that if they were let fall from the height of six feet or more upon a wooden floor, or even a plate of metal, they were rarely broken; but that by the attraction and repulsion of them between these plates, though at the distance of no more than one sixth of an inch, they were frequently beaten to pieces*.

THE Doctor also proved, that the electric matter passed through the substances of the metal of communication, and not over the surface of it, by covering a wire with a mixture of wax and rosin, and discharging a phial through it.

I MUST here observe, that Mr. Monnier the younger discovered, that electricity is not communicated to homogeneous bodies in proportion to their masses or quantity of matter, but rather in proportion to their surfaces; and yet that all equal surfaces do not receive equal quantities of electricity, but that those receive the most which are most extended in length; that a

* Phil. Transf. abridged, Vol. x. p. 309.

square sheet of lead, for instance, received a much less quantity of electricity than a small strip of the same metal with a surface equal to that of a square sheet*.

MR. WILSON, whose curious observations on the Leyden phial have been mentioned in a former section, claims no small share of honour in this. As early as the latter end of the year 1746, he made the same discovery that Dr. Watson had done, viz. that the electric fluid did not come from the globe, but from the earth itself, and from all other non-electric bodies about the apparatus. He suggested a method of proving this in a letter to Mr. Elliott from Chester; and mentions his having completed the experiment himself soon after, in a letter to Mr. Smeaton, from Dublin.

HAVING conceived that the difference between electric and non-electric bodies was owing to the different resistance which a *subtle medium*, as he calls it, on the surfaces of all bodies gave to the passage of the electric fluid; and conceiving that heat would rarify this medium, and thereby convert electrics into non-electrics, he made some experiments which confirmed him in that supposition. He found that one person might communicate electricity to another, notwithstanding the intervention of a considerable quantity of red-hot glass. He also made other experiments of a similar nature, as discharging phials by means of hot glass, hot amber,

* Phil. Trans. abridged, Vol. x. p. 338.

and various other heated electrics. These, however, as Mr. Canton afterwards observed, might be owing to the hot air upon the surfaces of those bodies, which he found to transmit electricity very well. But another experiment, which Mr. Wilson made upon melted rosin, does not seem liable to that objection. He poured the melted rosin into a phial, and found that he could give shocks with it; but he observed, that these shocks diminished as the rosin grew cold, and that when it was quite cold, they entirely ceased*.

MR. WILSON mentions a curious experiment (of which, however, he does not say that he was the inventor) which he made with paper vanes stuck in a cork, and suspended by a magnet. These, he says, if they were brought near the point of any body proceeding from the prime conductor, would turn round very swiftly, but would not turn at all in vacuo. This blast he thought was occasioned by the issuing of the electric matter out of the point, which caused a current in the air; but he did not try what would be the consequence of presenting the vanes to a point which received the electric fluid†.

LASTLY, Mr. Wilson observed, that if a needle were presented to a piece of down hanging to the conductor, it would cling close to it; but that, upon presenting any thing that was blunt, it would be repelled again; and says that Mr. Canton made

* Wilson's Essay, p. 143.

† Ibid. p. 141.

several curious experiments of the same kind*.

MR. SMEATON, within this period, observed, that if a man who was insulated pressed against the globe with the flat part of his hand, while another person, standing on the floor, did the same, in order to excite it, the person who was insulated would hardly be electrified at all; but that, if he only laid his fingers lightly on the globe, he would be electrified very strongly†. The same ingenious person also observed, that upon heating the middle of a large bar of iron to a glowing heat, and electrifying it, the electric power of the part that was heated was as strong as that of the cold part‡.

For several curious discoveries relating to electricity, made within this period, we are indebted to Dr. Miles. In a paper read at the Royal Society, January 25th, 1746, he says, that having excited a stick of black sealing-wax with white and brown paper, or clean dried flannel, he was able to kindle common lamp-spirits with it. Comparing the stick of wax with the glass tube, he observed a remarkable difference between the appearance of fire from both, though he did not understand the reason of it. He says he found the luminous effluvia to proceed in a much greater quantity from the top of his finger to the stick of wax, than they did to the glass. He several times observed a small

* Wilson's Essay, p. 141.

† Ibid. p. 153.

‡ Ibid. p. 129.

globular spot of fire to appear first on his finger, from which issued regular streams towards the wax, in the form of a comet's tail. This is now well known to be the constant appearance of the electric fire between an un-electrified body and an electric excited negatively *.

DR MILES found a stick of sulphur to perform very well; but not at all, when he had put an iron rammer in the center of it, to strengthen it. It is remarkable, that after setting this stick upright in a cupboard, it lost all its electric virtue, and could never afterwards be excited in the least degree. This effect the Doctor attributed to its being put up without any cover.

DR. MILES also mentions his having got a tube of green glass, which he could never excite but with great difficulty, and then but to a small degree †.

THE same ingenious gentleman, some time after, made an experiment upon pieces of leaf brass in a bottle hermetically sealed. To these he found he could give motion by the approach of the excited tube, in the same manner as if they had been in the open air; but one appearance struck him, of which he by no means gives a satisfactory account. He observed that when he removed the tube from the exhausted glass slowly, no commotion was seen in the leaf brass, but a very brisk one

* Wilson's Essay, p. 317.

† Phil. Trans. abridged, Vol. x. p. 320.

upon removing it suddenly. Indeed this fact could not have been understood but by comparing it with other facts depending upon the same principle, and which were not discovered till some years after *.

FROM England, to which, as an Englishman, I would give the preference only in matters of absolute indifference, I pass over to France, where, next to those made in England, the most important discoveries, and greatest number of them were made in the period of which I am treating. And, without all dispute, the greatest name in France, in this or any other period, except that of Mr. Du Fay, his friend and associate, is that of the Abbé Nollet.

THE favourite observation of Mr. Nollet, on which he built his darling theory of affluences and effluences was, that bodies not insulated, plunged in electric atmospheres, shewed signs of electricity. He observed a sensible blast from the hand of a person not electrified, in the above mentioned circumstances, also the attraction and repulsion of light bodies by them, the appearance of flame, the diminution of their weight by increased evaporation and perspiration, and almost every other appearance and effect of electricity. Moreover observing that his globe contracted a foulness while it was whirling, even when rubbed with a clean hand, he had the curiosity to collect a quantity of the mat-

* Phil. Trans. abridged, Vol. x. p. 326.

ter which formed that foulness; and finding that, when it was put into the fire, it had the smell of burnt hair, he concluded that it was an animal substance; and that it had been carried by the affluent electricity from his own body to the globe*.

THE only mistake of this ingenious philosopher in these experiments, and which was the source of many others, which, in the end, greatly bewildered and perplexed him, was, that the electricity of the body, which was plunged in the atmosphere of an electrified body, was of the same nature with that of the electrified body. Had he but preserved the distinction, which Mr. Du Fay had discovered, between the two electricities, and imagined that the body electrified, and that which was plunged in its atmosphere were possessed of these two different and opposite electricities, he might have been led to the great discoveries made by Mr. Canton, Dr. Franklin, and Mr. Wilcke; which, we shall find, arose from that single observation; and he would have avoided a great deal of debate and contention, which has not ended to his advantage.

THIS partial discovery of Mr. Nollet is by no means the only one of his, that the history of electricity presents in this period. He made several experiments on pointed bodies, and observed, that those which had the smallest points soonest threw out brushes of

* Nollet's Recherches, p. 142.

electric light, but did not show other signs of electricity so strong as bodies that were not pointed*.

HE took a great deal of pains in making experiments, in order to determine the degree in which different substances conducted the electric fluid; and found that the smoke of gum lac, turpentine, karabé, and sulphur did not carry away the electricity of an excited tube so soon as the smoke of linen, wood, and more especially the steam of water, and the effluvia of burning tallow, and of other fatty substances. In short, he found, that vapours which were not watery did very little, or no injury to electrical experiments, provided the tube was not exposed to them near the fire which caused them. A smoky room did not prevent his performing experiments, at least in any great degree; nor were odoriferous effluvia at all prejudicial to them †.

SEVERAL curious observations were made by the Abbé upon heat, and heated bodies. He found, that a piece of iron glowing hot, so as to throw off ignited particles, did not leave the smallest trace of electricity in an excited tube, to which it had been brought within five or six inches, and only held there two or three seconds; but it ceased to affect the tube at the same distance before it ceased to be red, and had no influence at all long before it was cold. The electricity of the tube, in this instance, was probably conveyed through

* Recherches, p. 146.

† Ibid. p. 194, &c.

the air heated by the iron; as it can hardly be supposed, that the iron emitted any effluvia capable of producing that effect *.

HE found that the excited tube lost nothing of its electricity in the focus of a burning mirror. That the flame of a candle, or the near approach of it, would destroy electricity had been known before: he observed, that the flame was sensibly disturbed by the approach of the excited tube, and he mentions Mr. Du Tour, and the Abbé Needham's having found, that the interposition of the thinnest piece of glass, or of any other substance, between the candle and the tube prevented the dissipation of the electricity. From this fact it was inferred, that the dissipation was owing to some effluvia proceeding from the candle †.

CONTINUING his observations on what increased or impeded electrical experiments, he found, that a light body, placed on a non-electric stand, moved more briskly upon the approach of an electrified body, than when it was placed upon an electric stand ‡. Several electrical experiments, he observed, succeeded best when there was a number of spectators present, and when they drew near, and stood close together to see his experiments; provided they did not occasion so great a perspiration as made his glasses moist §. This observation we shall find accounted for hereafter by Mr. Wilcke.

* Recherches, p. 216.

† Ibid. p. 219.

‡ Ibid. p. 122.

§ Ibid. p. 123.

THE Abbé moistened, with water or spirit of wine, a slender and pointed bar of iron, and thought that the blast from the point of it was more sensible than when it was not moistened; which he attributed to the electric fluid carrying away with it some of the particles of the water, and of the spirit of wine*.

SOME few observations the Abbé made on the difference between excited and communicated electricity, and between the electricity of glass and that of sulphur. He observed, that the electricity of an excited globe or tube, caused an odd sensation upon the face, as if a spider's web were drawn over it; whereas that effect was seldom produced by communicated electricity. Excited electricity, he also says, might be perceived by the smell, at more than a foot distance, when communicated electricity could not †.

HE melted sulphur in a glass globe, by turning it over a chafing dish of burning coals; when he observed, that small pieces of sulphur, before they were melted, were attracted and repelled by the glass within, at the same time that the ashes of the coals were attracted without ‡. Holding a piece of excited sulphur in one hand, with a piece of down sticking to it, and ready to fly off, the down, he says, would cling fast to the sulphur, upon presenting to it an excited glass, tube, which he held in his other hand §.

* Recherches, p. 140.

† Ibid. p. 136.

‡ Ibid. p. 124.

§ Ibid. p. 124.

I SHALL, in the last place, recite the Abbé Nollet's experiments made in vacuo. He found that glass, and other electrics, might be excited in vacuo, but not so strongly as in the open air*. He observed that there was a remarkable difference between the appearance of the electric light in vacuo, and in the open air; being much more diffuse, and unbroken in vacuo†. Inserting the extremity of his conductor into an exhausted glass vessel, he observed the vessel to be full of light, whenever he brought his hand to it; that the light was considerably increased when he spread his hand over it; and that when a spark was taken from the conductor, the whole vessel seemed to be full of light. He also observed, that small pieces of metal, inclosed in the vessel, adhered close to the glass; but detached themselves from it, on the approach of the finger, or of any conductor on the outside.

M. MONNIER attempted, in an ingenious method, to be certain whether the quantity of electricity communicated to a body was in proportion to its solid or superficial contents. He first found that an anvil which weighed two hundred pounds, gave but an inconsiderable spark, while the spark from a speaking-trumpet of tin, which weighed but ten pounds, but was eight or nine feet long, was almost equal to the shock of the Leyden phial. He then observed, that a ball of

* Recherches, p. 236.

† Ibid. p. 243.

lead four inches in diameter, gave a spark of the same force with another from a thin piece of lead of the same superficies, in the form of a hoop. And, lastly, he took a thin and long piece of lead, and observed, that when it was electrified in its whole length, it gave a very strong spark, but a very small one when it was rolled into a lump. But, because a square piece of lead did not give a spark equal to one from a piece of the same quantity of surface, but of greater length, he concluded that, though electrification is stronger in proportion to the surfaces of electrified bodies, yet that, of equal surfaces, that which is drawn out to the greatest length will have the advantage*.

THERE are a few other names of electricians in France, whose experiments and observations, made within this period, deserve to be mentioned. Of these is Mr. Boulanger. He took great pains to determine the degree in which different substances are capable of being excited. The experiments, he says, were made with the greatest care: and though the state of the science did not admit of this business being determined with greater accuracy, it may not be disagreeable to see the result of them; which he has comprised in the following table, beginning with those that are least excitable in every column.

* Ac. Par. 1746, M. p. 693.

FIRST COLUMN.

Ebony.
Guaiacum.
Box wood.
Sandal wood.
Oak.
Elm.
Ash.
Linden tree.
Rose.
Willow.
Ozier.
Cork.
Dry wood of all kinds.
All dry plants.

SECOND COLUMN.

Shells of all kinds.
Whalebone.
Bones.
Ivory.
Horn.
Scales.
Parchment.
Hair.
Wool.
Feathers.
Cotton.
Silk.

THIRD COLUMN.

Alum.
Sugar Candy.

The Phosphorus of
Berne.

Yellow and white wax.
Japan varnish.
Sandarac.
Mastich.
Amber.
Jet.
Pitch.
Gum copal.
Gum lac.
Colophonia.
Sulphur.
Sealing-wax.

All salts which have
sufficient consist-
ence.

All resins.

FOURTH COLUMN.

Loadstone.
Hand-stone.
Marble of all colours.
Slate.
Free-stone.
Granite.
Porphyry.
Jasper.
Varnished earth.
Cornelians.
Agates.
All opaque precious
stones.
Porcelaine.

FIFTH

FIFTH COLUMN.

Hyacinth.	Venice and Muscovy talc.
Opal.	Coloured diamonds, especially yellow.
Emerald.	White diamonds, especially the brilliant.
Amethyst.	
Topaz.	All transparent precious stones.
Ruby.	Glass, and all vitrifications without excepting those of metals.
Sapphire.	
Cat's eye.	
Peridote.	
Granite.	
Rock crystal.	

THE inference which this author draws from this catalogue is, that the most brittle, and the most transparent substances, are always the most electric; and he has recourse to an awkward hypothesis to account for the marcasites not being excitable at all, notwithstanding they are both brittle and transparent. He says it is owing to condensed air contained in those substances, which is known to prevent excitation *.

THE same author says, that mineral waters are much more sensibly affected with electricity than common water; that black ribbons are much sooner attracted than those of other colours; and, next to them, the brown, and deep red †.

* Boulanger, p. 74.

† Ibid. p. 124.

MR. LE CAT, a physician at Rouen, who has distinguished himself by several performances in the learned world, suspended several pieces of leaf gold at his conductor, and observed that they hung at different distances, according to their sizes, the smaller pieces placing themselves nearer the conductor, and the larger receding farther from it. This he compares to the distances at which the planets make their revolutions round the sun, and he supposed the cause to be the same in both. The same author very particularly compares the electric shock, which had just been discovered, to thunder *.

GERMANY affords but few articles for the electrical history of this period; one of them, however, is curious, and well deserves to be transmitted to posterity. Mr. Gordon of Erford excited the electricity of a cat so strongly, that, when it was communicated by iron chains, it fired spirit of wine †.

It has been mentioned before, that several gentlemen in Germany, as well as in England, had found, that if the man who rubbed the globe stood upon electrics, sparks were perceived upon touching him; but Mr. Klingenshierna, a Swede, and Mr. Stroema, were the first who properly electrified by the rubber; and their experiments were published in the Acts of the Royal Academy of Sciences at Stockholm for the year 1747 ‡.

* Histoire, p. 84—85.

† Nollot's Recherches, p. 98.

‡ Wilcke, p. 112.

MR. JALLABERT, professor of philosophy at Geneva, found that a coating of pitch did not prevent the conductor from being electrified, which proved that the electric fluid enters the substance of metals. He also proved, that ice was a conductor of electricity, by making the Leyden experiment with a bottle in which water was frozen *.

THE amazing and extensive effects of electricity now began to make philosophers look for it where it had not been suspected before. The first account that is given of woollen garments being observed to exhibit signs of electricity, when they were put off, after the flashes of light they gave were known to be owing to electricity, was sent to the Royal Society by Mr. Coke of the Isle of Wight, who says, that a lady of his acquaintance observed it; and that it was also at last found, that it was only new flannel, and after some time wearing, which gave that appearance, and that this property was lost when it was washed †.

THE same appearance, he observes, upon another occasion, was most conspicuous in frosty weather; in which season he takes notice, that there is generally, not only a greater purity of the air, and absence of moisture, but that all hairy and horny substances (for hairs, as he says, are only small horns) are more elastic, and consequently susceptible of, and more capable of exciting strong vi-

* Histoire, p. 95, 96.

† Phil. Trans. abridged, Vol. x. p. 343.

brations.

brations. He says, that the flannel being tendered damp with sea water, and afterwards dry, would heighten the electric appearances*.

BUT though this was the first appearance of the kind that was observed, after it was known to arise from electricity, similar appearances had been several times noted before. Bartholin, who flourished in 1650, wrote a book *De luce animalium*, in which he supposes, that unctuous effluvia had a great share in those appearances. The same writer says, that Theodore Beza might be seen by a light proceeding from his eye-brows; and that sparks would flash from the body of Charles Gonzaga, Duke of Mantua, upon being gently rubbed. But he does not say whether he had any particular hairy, or scaly superficies to his skin †.

DR. SIMPSON, who published a philosophical discourse on fermentation, dedicated to the Royal Society in 1675, also takes notice of the light proceeding from animals on friction, or peccation as he calls it, and instances in the combing of a woman's head, the currying of a horse, and the stroking of a cat's back ‡.

MR. CLAYTON also, in a letter to Mr. Boyle, dated June 23d, 1684, at James-town in Virginia, gives him an account of a strange accident, as he calls it, which happened to one Mrs. Sewall, whose wearing apparel emit-

* Phil. Trans. abridged, Vol. x. p. 344. † Ibid. p. 344.

‡ Ibid. p. 357.

ted a flashing of sparks, which were seen by several persons. The like happened to Lady Baltimore her mother-in-law *.

I SHALL conclude this section with what I can find, in this period, about increasing the power of electricity, and measuring its effects.

MR. MONNIER the younger, whose name has been frequently mentioned in the course of this history, used glass spheroids instead of globes, and endeavoured to increase his electrical power by using several of these spheroids at a time; but he found, upon trial, that they did not answer his expectations; and was thence disposed to conclude, that there might be a *ne plus ultra* in the intensity of electricity, as well as in the heat communicated to boiling water †.

THE power of glass in electrifying being found to be so great, it is no wonder that philosophers should endeavour to find what kind of glass was capable of being excited to the greatest degree. Among other proposals we find a very memorable one communicated to the Royal Society, April 6th, 1749, by Mr. Boze. He says, that a glass ball which has often been employed in violent distillations, and other chemical operations sends forth electricity incomparably more strong, than any glass which had never been exposed to so violent a fire. This article is the more curious, as it shews us how much philosophers at this

* Phil. Trans. abridged, Vol. x. p. 278.

† Ibid. p. 330.

time piqued themselves upon discoveries in electricity. He asserts his being the first person who ever mentioned this notable circumstance, as he calls it, and desires Dr. Watson, to whom he communicated it, to let him have the honour of that improvement in the Philosophical Transactions*.

IT was within this period that Dr. Watson contrived to improve the strength of electricity by moistening the rubber of his globe, though he was not aware of all the reasons for it. He observed that the man who stood on the floor, to excite the globe by his hand, did it more strongly than a cushion. This, he says, he could not conceive to be owing to any other difference, than to his hand being more moist, and consequently more readily conducting the electricity from the floor; wherefore he ordered his machine, and even his cushion, to be made damp; and then found that the electricity was as strong as when the globe was rubbed by the hand†.

A GENTLEMAN at Chartres in France, greatly increased the effects of electricity by means of moisture, for asserting which he is very much ridiculed by the author of *Histoire de l'Electricité*.

MR. WILSON says, that if the cushion (which he made of leather) was gilt with silver, brass, or copper, it would do very well; and that the silk line on which the conductor

* Phil. Transf. abridged, Vol. x, p. 329.

† Ibid. p. 312.

hung should be red or yellow*. The table, he says, should stand on moist ground, or a wire pass from the machine to the moist ground †.

DR. WATSON also found, that though no electricity could be produced by rubbing the globe with original electrics perfectly dry, yet that they answered very well when they had been made moist; the water imbibed by those substances serving as a canal of communication to the electricity between the hand, or the cushion, and the globe; in the same manner as the air, replete with vapours in damp weather, prevents the accumulation of the electric matter in any considerable degree, by conducting it as fast as it is excited to the nearest non-electrics. He observed, on the contrary, that most vegetable substances, though made as dry as possible, furnished electricity, but small in quantities. He excited electricity not only from linen, cotton, &c. but even from sheet lead and a deal board ‡.

THE Abbé Nollet says, that he found oil of turpentine upon a piece of woollen cloth excited glass very powerfully, but that the least water mixed with it prevented the excitation §.

MR. BOULANGER says, that if two cylinders be made of the same kind of glass, and of the same fashion, one of them transparent, and the other tinged with any colour, the transparent cylinder will be excited more easily

* Wilson's Essay, p. 5, 6.

† Ibid. p. 8.

‡ Ibid. 380.

§ Recherches, p. 168.

than the coloured one *. He acknowledges, however, that sometimes the most transparent, and the most brittle glass is capable of acquiring but little electricity †. In another place he says, that a cylinder of three or four lines in thickness will acquire a stronger, and a more lasting electricity than a cylinder of one line thick ‡. He also says, that a person's two hands, or one cushion, is better than more §.

ABOUT the same time that Dr. Watson made his first experiments upon the Leyden phial, Mr. Canton discovered a method by which the quantity of electricity accumulated in the phial might be measured to a good degree of exactness. He took the charged phial in his hand, and made it give a spark to an insulated conductor, which spark he took off with his other hand. This operation he repeated till the whole was discharged, and he estimated the height of the charge by the number of the sparks. This is a pretty certain and exact method of knowing how high a phial *has been* charged; but what electricians chiefly want is a method of ascertaining how high a phial *is* charged, or the exact force of the charge while it is contained in the glass.

SOMETHING of this kind was done by Mr. Ellicott, in the same year 1746. He proposed to estimate the strength of common

* Poulanger, p. 64.

† Ibid. p. 135.

‡ Ibid. p. 164.

§ Ibid. p. 136.

electrification, by its power to raise a weight in one scale of a balance, while the other should be held over the electrified body, and pulled to it by its attractive power*. Mr. Galath also constructed an electrometer upon the same principle†.

THE Abbé Nollet applied the threads that Mr. Grey and Du Fay had used in electrical experiments, to shew the degree of electricity. He hung two of them together, and observed the angle of their divergence, by means of the rays of the sun, or the light of a candle, and their shadow upon a board placed behind them. Mr. Waitz also thought of the same kind of electrometer, with this improvement, that he loaded the ends of the threads with small weights‡.

* Boulanger, p. 324.

† Dantzick Memoirs, Vol. i. p. 525.

‡ Histoire, p. 58.

SECTION IV.

EXPERIMENTS ON ANIMAL, AND OTHER ORGANIZED BODIES IN THIS PERIOD; AND OTHER EXPERIMENTS CONNECTED WITH THEM, MADE CHIEFLY BY THE ABBE NOLLET.

HITHERTO the effect of electricity upon human bodies had not been attended to, farther than the mere shock of the Leyden phial. But we shall now see a curious set of experiments on this subject exhibited by the Abbé Nollet. The English philosophers, who led the way in almost every other application of electricity, were among the last to try its effects upon animals, and other organized bodies. The only article that I can find upon this subject, before the discoveries of the Abbé Nollet, is one of Mr. Trembley's; who says that several persons had observed, that while they were electrified, their pulse beat a little faster than before. He says, that he himself, after having been electrified a long time together, had felt an odd sensation all over his body, and that some persons had felt very sharp pains after being electrified *.

* Phil. Transf. abridged, Vol. x. p. 321.

THE ingenious Abbé Nollet begins his experiments with the evaporation of fluids by electricity. They were made with the greatest attention, and the following observations were the result of them.

“ 1. ELECTRICITY augments the natural
“ evaporation of fluids; since, excepting mer-
“ cury, which is too heavy, and the oil of
“ olives, which is too viscous, all the others
“ which were tried suffered a diminution
“ which could not be ascribed to any other
“ cause than electricity.

“ 2. ELECTRICITY augments the evapo-
“ ration of those fluids the most, which are
“ most subject to evaporate of themselves. For
“ the volatile spirit of sal ammoniac suffered a
“ greater loss than spirit of wine, or turpen-
“ tine; these two more than common water;
“ and water more than vinegar, or the solu-
“ tion of nitre.

“ 3. ELECTRICITY has a greater effect
“ upon fluids when the vessels which contain
“ them are non-electrics; the effects always
“ seeming to be a little greater when the
“ vessels were of metal, than when they were
“ of glass.

“ 4. THIS increased evaporation was more
“ considerable when the vessel which contain-
“ ed the liquor was more open, but the ef-
“ fects did not increase in proportion to their
“ apertures. For when these liquors were
“ electrified in vessels, whose aperture was
“ four inches in diameter, though they pre-
“ sented to the air a surface sixteen times
“ larger

“ larger than when they were contained in
 “ vessels whose aperture was one inch in dia-
 “ meter, they were, nevertheless, far from
 “ suffering a diminution proportioned to that
 “ difference.

“ 5. ELECTRIFICATION does not make
 “ any liquors evaporate through the pores,
 “ either of metal, or of glass ; since after ex-
 “ periments which were continued ten hours,
 “ there was found no diminution of their
 “ weight, when the vessels in which they were
 “ contained were well stopped *.”

AFTER having made experiments on fluids, the Abbé began another course on solids of various kinds, the result of which was, that they lost weight only in proportion to the moisture they contained, and the openness of their pores †.

THE Abbé also extended his experiments to other sensible qualities of bodies, as their smell, their taste, and chemical properties ; but found no change in any of them, after a strong and continued electrification of a variety of substances. Electrification did not affect the power of the magnet, and neither retarded nor accelerated the heating or cooling of bodies ‡.

HE then proceeded to the electrification of capillary tubes, full of water ; it having been observed by Mr. Boze, who communicated the observation to Mr. Nollet ||, that the water would issue in a constant stream when they

* Nollet's Recherches, p. 327.

† Ibid. p. 335.

‡ Ibid. p. 341.

|| Ibid. p. 343.

were electrified; whereas it would only drop very slowly without that operation. Every person, at first sight, would judge that the stream was accelerated, and that the electrified vessel would soon be empty: but this accurate philosopher was unwilling to rely on first appearances, and therefore resolved to ascertain the fact, by measuring the time, and the quantity of liquor running out. And, in order to know if the acceleration, supposing there were any, was uniform, during the whole time of the running out, he made use of vessels of different capacities, terminating in pipes of different bores, from three lines in diameter, to the smallest capillaries.

As the Abbé did not find it so easy a matter to draw a safe conclusion in this case as might at first be imagined, he gives us in gross the following result of above an hundred experiments*.

“ 1. THE electrified stream, though it
 “ divides, and carries the liquid farther, is
 “ neither sensibly accelerated nor retarded,
 “ when the pipe through which it issues is
 “ not less than a line in diameter.

“ 2. UNDER this diameter, if the tube is
 “ wide enough to let the liquid run in a con-
 “ tinued stream, electricity accelerates it a
 “ little; but less than a person would ima-
 “ gine, if he judged by the number of jets
 “ which are formed, and by the distance to
 “ which they go.

* Recherches, p. 327. Phil. Trans. abridged, Vol. x. p. 382.

“ 3. If the tube be a capillary one, from
 “ which the water only drops naturally, the
 “ electrified jet not only becomes a continu-
 “ ed stream, and even divided into several
 “ streams, but is also considerably accelerat-
 “ ed; and the smaller the capillary tube
 “ is, the greater, in proportion, is this ac-
 “ celeration.

“ 4. So great, is the effect of the electric
 “ virtue, that it drives the water in a con-
 “ stant stream out of a very small capillary
 “ tube, out of which it had not before been
 “ able even to drop.”

THE most unaccountable of these experi-
 ments, as the ingenious Abbé acknowledges,
 are those which suppose a retardation of the
 electrified current, and he long doubted the
 fact; but a great number of experiments,
 carefully noted in his journal, obliged him to
 admit it, though still with hesitation, and to
 account for it in the best manner he could;
 which, indeed, is not very satisfactory*.

THE beautiful appearance of these streams
 of electrified water, when the experiment
 was exhibited in the dark, is particularly de-
 scribed by this author, after Messrs. Boze and
 Gordon, who first observed it†.

THESE last experiments served as a basis to
 the Abbé's future inquiries. He considered
 all organized bodies as assemblages of capil-
 lary tubes, filled with a fluid that tends to run
 through them, and often to issue out of them.

* Recherches, p. 351.

† Ibid. p. 354.

In consequence of this idea, he imagined, that the electric virtue might possibly communicate some motion to the sap of vegetables, and also augment the insensible perspiration of animals. He began with the following experiments, the result of which confirmed his supposition*.

He electrified for four or five hours together, fruits, green plants, and sponges, dipped in water which he had carefully weighed; and found that, after the experiment, all those bodies were remarkably lighter than others of the same kind, weighed with them, both before and after the experiment, and kept in the same place and temper†.

THE electrification of growing vegetables was first begun in Britain. Mr. Maimbray at Edinburgh electrified two myrtle trees, during the whole month of October 1746; when they put forth small branches and blossoms sooner than other shrubs of the same kind, which had not been electrified. Mr. Nollet, hearing of this experiment, was encouraged to try it himself‡.

He took two garden pots, filled with the same earth, and sowed with the same seeds. He kept them constantly in the same place, and took the same care of them; except that one of the two was electrified fifteen days together, for two or three, and sometimes four hours a day. The consequence was, that the

* Recherches, p. 355.

† Phil. Trans. abridged, Vol. x. p. 383.

‡ Recherches, 356.

electrified pot always shewed the sprouts of its seeds two or three days sooner than the other. It also threw out a greater number of shoots, and those longer in a given time; which made him believe, that the electric virtue helped to open and display the germs, and thereby to facilitate the growth of plants. This, however, our cautious philosopher only calls a conjecture, which required farther confirmation. The season, he says, was then too far advanced to allow him to make as many experiments as he could have wished, but he says the next course of experiments had greater certainty, and they are not less interesting *.

THE same experiments were carrying on about the same time by Mr. Jallabert, Mr. Boze, and the Abbé Menon, principal of the college of Bueil at Angers, who all drew the same conclusions from them †.

THE Abbé chose several pairs of animals of different kinds, cats, pigeons, chaffinches, sparrows, &c. All these he put into separate wooden cages, and weighed them. One of each pair he electrified for five or six hours together, and then weighed them again. The result was, that the electrified cat was commonly sixty-five or seventy grains lighter than the other, the pigeon from thirty-five to thirty-eight grains, the chaffinch or sparrow six or seven grains. In order to have nothing to charge upon the difference that

* Recherches, p. 358, &c. Phil. Trans. abridged, Vol. x. p. 383.

† Recherches, p. 357.

might arise from the temperament of the individuals he happened to pitch upon, he repeated the same experiments, by electrifying that animal of each pair which had not been electrified before; and, notwithstanding some small varieties the electrified animal was constantly lighter than the other in proportion*.

AFTER these experiments, he had no doubt but that electricity increased the insensible perspiration of animals, but it was not certain whether this increase was in the ratio of their bulks, or in that of their surfaces. The Abbé's opinion was, that it was neither in the one, nor the other, strictly speaking, but in a ratio much more nearly approaching the latter than the former; so that he imagined, there was no room to apprehend, that a human person electrified would lose near a fiftieth part of his weight, as it appeared to him to have happened to one sort of bird; nor 140th part, as to the pigeon, &c. All that he had then observed upon that head was, that a young man or woman, between the ages of twenty and thirty, from being electrified five hours together, had lost several ounces of their weight, more than they were wont to lose when they were not electrified†.

THE Abbé observes, that no inconvenience whatever was felt by the persons who submitted to be electrified in this manner. They

* Recherches, p. 366, &c.

† Phil. Transf. abridged, Vol. x. p. 324. Recherches, p. 382.

only found themselves a little exhausted, and had got a better appetite. He adds, that none of them found themselves sensibly warmer, and that he could not perceive that their pulse was encreased *.

THESE last experiments on human bodies, he justly observes, are difficult to pursue with exactness, because the cloathing, which cannot strictly be compared to the hairs or feathers of animals, retains a considerable share of the perspired matter, and prevents our forming a good judgment of the whole effect of the electric virtue.

THE foregoing experiments, he says, convinced him of the reality of the *effluent* matter, carrying away with it the perspirable parts of bodies, and what could be evaporated from their surfaces. And he was convinced of the *affluent* matter, by observing all those effects produced, if, instead of electrifying bodies themselves, they were only brought near a large body which was electrified. He moistened a thick sponge in water, and cut it into two pieces, and then weighed the parts separately, and placing the whole near a large electrified body; he found that, after an electrification of five or six hours, that part of the sponge which was nearer to the electrified body had lost more weight than the other. From this fact he concluded, that if any part of an animal body was presented to a large electrified substance, it would perspire more

* Recherches, p. 389.

than the other, and that perhaps obstructions might by this means be removed from the pores of it *.

THE experiments above recited of Mr. Nollet by no means satisfied the English philosophers, and particularly Mr. Ellicott, who made experiments to refute the theory which the author had deduced from them. He observed that the syphon, though electrified, would only deliver the water by drops, if the basin in which the water was contained was electrified too. But this does not invalidate Mr. Nollet's curious experiments upon the subject of evaporation and perspiration. For when an animal body is electrified, there is always non-electric matter enough in the atmosphere, to answer the purpose of the unelectrified basin, in the experiment of the capillary tube; thereby to cause a continual exhalation of the perspirable matter from the pores of the skin. Besides, the capillary tube will, in fact, unite the water in a constant stream, when it has only the open air to throw it into. In all debates upon subjects in natural philosophy, facts ought only to be opposed to facts. The veracity of the Abbé Nollet is not to be called in question; though it must be acknowledged, that, in his later writings, at a time when his favourite system was in danger, he makes mistakes with respect to the facts that nearly affect it.

* Phil. Trans. abridged, Vol. x. p 385.

To account for the appearance of light, which seems, in some cases, to issue from a non-electric body presented to an excited electric, and which Mr. Nollet thought to be the affluent matter, Mr. Ellicott supposes that it was the light which had come from the electric. In accounting for the suspension of leaf gold between an electrified and an unelectrified plate, Mr. Ellicott's theory made it necessary to suppose (what Dr. Franklin afterwards found not to be fact) that the leaf gold will always be suspended nearer the unelectrified than the electrified plate.

IN his answer to Mr. Nollet, Mr. Ellicott also endeavours to account for the electric matter issuing from a point at the extremity of the conductor, more sensibly than if it had terminated round or flat. He says that the effluvia, in rushing from the globe along the conductor, as they approached the point, were brought nearer together, and therefore were denser there than in any part of the rod. Consequently, he says, if the light be owing to the density and velocity of the effluvia, it will be visible at the point, and nowhere else. This, as far as I can find, was the first attempt to account for this phenomenon; but it by no means accounts for the whole virtue of the conductor being dissipated from such points. Indeed, it is no wonder that the influence of points which are but imperfectly understood even at this day, furnished too difficult a problem so many years ago*.

* Phil. Trans. abridged, Vol. x. p. 393.

It will, now, be universally acknowledged, that there was very great merit in these experiments of the Abbé Nollet, made upon animal and other organized bodies. He opened a new and noble field of electrical discoveries, and he pursued them with great attention, perseverance, and expence. This last circumstance, I suppose, may have been the reason why his experiments have not, as far as I can find, been resumed and pursued by any electrician since his time, though there seems to be great room to improve upon what he began. The only method in which they can be conducted to any purpose, would be by the help of a machine for perpetual electrification, to go by wind or water; which would, likewise, serve for many other capital experiments in electricity. This application of electricity, in particular, may perhaps be of more use in medicine, than any other mode in which it has hitherto been administered.

MR. JALLABERT of Geneva carried the experiments on plants farther than the Abbé Nollet had done; and, by electrifying bottles in which the plants were growing in water, and placing in the same exposure other bottles, containing plants of the same kind; he proved, in the clearest manner, that the electrified plants always grew faster, and had finer stems, leaves, and flowers than those which were not electrified, and consumed more of their water*.

* Beccaria dell' elettricismo naturale et artificiale, p. 125.

S E C T I O N V.

THE HISTORY OF THE MEDICATED TUBES,
AND OTHER COMMUNICATIONS OF MEDICINAL VIRTUES BY ELECTRICITY, WITH
THEIR VARIOUS REFUTATIONS.

I N the course of this history we have seen frequent instances of self-deception, for want of persons attending to all the essential circumstances of facts; but nothing we have yet seen equals what was exhibited in the years 1747 and 1748. Mr. Grey's deceptions were chiefly owing to his mistaking the cause of real appearances; but in this case we can hardly help thinking, that, not only the imagination and judgment, but even all the external senses of philosophers must have been imposed upon. It was asserted by Signior Pivati at Venice (who has all the merit of these extraordinary discoveries), and, after him, by Mr. Verati at Bologna, Mr. Bianchi at Turin, and Mr. Winckler at Leipfick, that if odorous substances were confined in glass vessels, and the vessels excited, the odours and other medicinal virtues would transpire through the glass, infect the atmosphere of the conductor, and communicate the virtue to all persons in contact with it; also that those substances, held in the hands of persons electrified, would communicate their virtues to them; so that medicines might be made to

operate without being taken into the stomach. They even pretended to have wrought many cures by the help of electricity applied this way. Some of the more curious of these pretended experiments deserve to be recorded, for the entertainment and instruction of posterity.

THE forementioned Signior Johannes Francisco Pivati, a person of eminence at Venice, says, in an Italian epistle, printed at Venice with all the usual licences, in the year 1747, that a manifest example of the virtue of electricity was shown in the balsam of Peru, which was so concealed in a glass cylinder, that, before the excitation of it, not the least smell could by any means be discovered. A man who, having a pain in his side, had applied hyssop to it, by the advice of a physician, approached the cylinder thus prepared, and was electrified by it. The consequence was, that when he went home, and fell asleep, he sweated, and the power of the balsam was so dispersed, that even his cloaths, the bed, and chamber, all smelled of it. When he had refreshed himself by his sleep, he combed his head, and found the balsam to have penetrated his hair; so that the very comb was perfumed*.

The next day, Signior Pivati says, he electrified a man in health in the same manner, who knew nothing of what had been done before. On his going into company half an

* Phil. Transf. abridged, Vol. x. p. 400.

hour afterwards, he found a gradual warmth diffusing itself through his whole body, and he grew more lively and chearful than usual. His companion was surpris'd at an odour, and could not imagine whence it proceeded, but he himself perceived that the fume arose from his own body, at which he also was much surpris'd, not having the least suspicion that it was owing to the operation which had been performed upon him by Signior Pivati*.

MR. WINCKLER of Leipfick, being struck with so extraordinary a relation, says, that he was desirous of trying the power of electricity on certain substances in the same manner, and that he found the event to confirm what had been related†.

HE put some pounded sulphur into a glass sphere, so well covered and stopp'd, that, on turning it over the fire, there was not the least smell of sulphur perceived. When the sphere was cold he electrified it; and, immediately, a sulphureous vapour issued from it, and, on continuing the electricity, fill'd the air, so as to be smelted at the distance of more than ten feet. He call'd in a friend well vers'd in electricity, professor Haubold, and several others, as witnesses and judges of this fact; but they were presently driven away by the stench of the sulphur. He staid a little longer himself in this sulphureous atmosphere, and was so impregnated thereby, that his body, cloaths, and breath retained

* Phil. Trans. abridged, Vol. x. p. 401.

† Ibid.

the odour even the next day. On repeating this experiment in the presence of a person who was conversant with the effects of sulphur, the signs of an inflamed blood were visible in his mouth on the third day*.

AFTER this he tried the effect of a more agreeable smell, and filled the sphere with cinnamon. When he had heated this as before, the smell of cinnamon was soon perceived by the company, and the whole room was in a short time so perfumed by it, that it immediately affected the noses of all who came in, and the odour remained the next day.

HE tried the balsam of Peru with the like success, when his above mentioned friend (whose testimony, he says, he did not care to be without) after he had received the power of the balsam, smelled so strong of it, that going abroad to supper, he was often asked by the company what perfume he had about him. The next day, when Mr. Winckler was drinking tea, he says, he found an unusual sweet taste, owing to the fumes of the balsam that still remained in his mouth†.

IN a few days, when the sphere had lost all the scent of the balsam, they let a chain out of the chamber window, and extended it through the open air, into another room detached from the former. Here they suspended the chain on silken lines, and gave it into the hand of a man, who also stood on extended silken lines, and knew nothing of their

* Phil. Trans. abridged, Vol. x. p. 401.

† Ibid.

purpose. When the electricity had been excited for some time, the man was asked whether he smelled any thing; and, on snuffing up his nose, he said he did. Being asked again what smell it was, he said he did not know. When the electrification had been continued for about a quarter of an hour, the room smelled so strong of it, that the man, who knew nothing of the balsam, said his nose was filled with a sweet smell, like that of some sort of balsam. After sleeping in a house, a considerable distance from the room where the experiment was tried, he rose very chearfully in the morning, and found a more pleasant taste than ordinary in his tea*.

I SHALL only give an account of two instances of the effect of medicine applied in this manner. The assistance of Signior Pivati, the celebrated inventor of this improvement in electricity, was implored by a young gentleman, who was miserably afflicted by a quantity of corrupted matter collected in his foot, that eluded all the attempts of the physicians. Signior Pivati filled a glass cylinder with proper materials; and, having electrified it, drew sparks from the part affected, and continued the operation for some minutes. When the patient went to bed, he had a good night, and a mitigation of his pain. When he awaked in the morning, he found a small red tubercle on his foot, which only itched, as if a cold humour had flowed through the

* Phil. Transf. abridged, Vol. x. p. 401.

inner part of his foot. He sweated every night for eight days together, and at the end of that time was perfectly well.

AFTER this, Signior Donadoni, bishop of Sebenico, came to Signior Pivati, attended by his physician and some friends. His lordship was at that time seventy-five years old, and had been afflicted with pains in his hands and feet for several years. The gout had so affected his fingers, that he was not able to move them; and his legs, so that he could not bend his knees. In this deplorable situation, the poor old bishop intreated Signior Pivati to try the effects of electricity on him. The electrician undertook it, and proceeded after the following manner. He filled a glass cylinder with discutient medicines, and managed it so, that the electric virtue might enter into the patient, who presently felt some unusual commotions in his fingers; and the action of electricity had been continued but two minutes, when his lordship opened and shut both his hands, gave a hearty squeeze to one of his attendants, got up, walked, smote his hands together, helped himself to a chair, and sat down, wondering at his own strength, and hardly knowing whether it was not a dream. At length he walked out of the chamber down stairs, without any assistance, and with all the alacrity of a young man*.

* Philosophical Transactions abridged, Vol. x. p. 403.

A VARIETY of facts of this nature being published, and seemingly well attested, engaged all the electricians of Europe to repeat these experiments; but none of them could succeed but those mentioned above. An excellent remark of Mr. Baker's, who advised trying all these experiments, notwithstanding their seeming very improbable, deserves to be quoted here. "Romantic as these things
 " may seem, they should not be absolutely
 " condemned without a fair trial, since we
 " all, I believe remember the time, when
 " those phenomena in electricity, which are
 " now the most common and familiar to us,
 " would have been thought deserving as little
 " credit as the cases under consideration may
 " seem to do, had accounts of them been sent
 " to us from Rome, Venice, or Bologna,
 " and had we never experienced them our-
 " selves*."

To see these wonders, and to be assured of their truth or fallacy, Mr. Nollet, who was deeply interested in every thing that related to his favourite study, and who set no bounds to his labour or expences, in the pursuit of truth, even passed the Alps, and travelled into Italy, where he visited all the gentlemen who had published any account of these experiments. But though he engaged them to repeat their experiments in his presence, and upon himself; and though he made it his business to get all the best information he could

* Phil. Trans. abridged, Vol. x. p. 406.

concerning them, he returned, convinced that the accounts of cures had been much exaggerated; that in no one instance had odours been found to transpire through the pores of excited glafs; and that no drugs had ever communicated their virtues to persons who only held them in their hands while electrified.

He had no doubt, however, but that, by continued electrification, without drugs, several persons had found considerable relief in various disorders; particularly, that a paralytic person had been cured at Geneva, and that a person deaf of one ear, a footman who had a violent pain in his head, and a woman who had a disorder in her eyes, were cured at Bologna *.

THE English philosophers showed no less attention to this subject than the Abbé Nollet. The Royal Society had received an account from Mr. Winckler of his experiments, to prove the transfusion of odoriferous matter through the pores of excited glafs; and none of them succeeding here, the secretary was desired to write to Mr. Winckler, in the name of the society, desiring him to transmit to them, not only a circumstantial account of his manner of making the experiments, but likewise some globes and tubes, fitted up by himself, for that purpose.

THESE vessels, and directions how to use them, Mr. Winckler actually sent, and the

* Phil. Trans. abridged, Vol. x. p. 413, &c.

experiments were made with every possible precaution, at the house of Dr. Watson (the most interested and active person in the kingdom in every thing relating to electricity) on the 12th of June 1751. There were present Martin Folkes, president of the Royal Society, Nicholas Mann, Esq. vice president, Dr. Mortimer, and Peter Daval, Esq. secretaries, Mr. Canton a fellow, and Mr. Shroder, a gentleman of distinction, well known to, and corresponding with Mr. Winckler. But, notwithstanding all the pains these gentlemen took, pursuing, with the utmost exactness, the directions of Mr. Winckler, and also using methods of their own, which they thought still better adapted to force the effluvia through the glass, they were unsuccessful. They were not able to verify Mr. Winckler's experiments even in one single instance*.

BUT perhaps the most satisfactory refutation both of this pretended transfusion of odours, and the medicinal effects of electricity above mentioned, was made at Venice, the very place where this medical electricity took its rise. The experiments were made by Dr. Bianchini, professor of medicine, in the presence of a great number of witnesses, many of them prejudiced in favour of the pretended discoveries; but they were all forced to be convinced of their futility, by the evidence of facts, and by experiments made with the greatest care and accuracy†.

* Phil. Trans. abridged, Vol. xlvii. p. 321.

† Ibid. Vol. xlviii. p. 399.

AFTER the publication of these accounts properly attested, every unprejudiced person was satisfied, that the pretended discoveries from Italy and Leipfick, which had raised the expectation of all the electricians in Europe, had no foundation in fact; and that no method had yet been discovered whereby the power of medicine could by electricity be made to insinuate itself into the human body *.

LASTLY, I would observe, that Dr. Franklin also showed, by several experiments, the impossibility of mixing the effluvia or virtue of medicines with the electric fluid †.

IN some respects similar to the experiments with the *medicated tubes* (as those mentioned above were usually called) was that of professor Boze, which he termed the *beatification*; and which, for a long time, employed other electricians to repeat after him, but to no purpose. His description of this famous experiment was, that if, in electrifying, large globes were employed, and the electrified person were placed upon large cakes of pitch, a lambent flame would by degrees arise from the pitch, and spread itself round his feet; and that from thence it would be propagated to his knees and body, till, at last, it ascended to his head; that then, by continuing the electrification, the person's head would be surrounded by a glory, such a one, in some

* Phil. Trans. abridged, Vol. xlviii. p. 406.

† Franklin's Letters, p. 82.

measure, as is represented by painters in their ornamenting the heads of saints *.

THIS experiment, as well as that of the medicated tubes, set all the electricians in Europe to work, and put them to a great deal of expence ; but none of them could succeed, so as to produce an appearance any thing like that described by Mr. Boze. No person took more pains in this business than Dr. Watson. He himself underwent the operation several times, supported by solid electrics three feet high. Upon being electrified very strongly, he found, as he says several other persons also did, a tingling upon the skin of his head, and in many parts of his body, or such a sensation as would be felt from a vast number of insects crawling over him at the same time ; and he constantly observed the sensation to be the greatest in those parts of his body which were nearest to any non-electric, but still no light appeared upon his head, though the experiment was several times made in the dark, and with some continuance †.

AT length the Doctor, wearied with these fruitless attempts, wrote to the professor, and his answer showed that the whole had been a mere trick. He candidly acknowledged, that he had made use of a suit of armour, which was decked with many bullions of steel, some pointed like nails, some like wedges, and some pyramidal ; and that when the electrization was very vigorous, the edges of the

* Phil. Trans. abridged, Vol. x. p. 411.

† Ibid.

helmet

helmet would dart forth rays, something like those which are painted on the heads of saints. And this was all his boasted beatification*.

THIS same Mr. Boze, who seems to have had a singular affectation of something mysterious and marvellous in his experiments, in a letter to the Royal Society at London, said that he had been able, by electricity only, to invert the poles of a natural magnet, to destroy their virtue, and to restore it again, but he did not describe his method†. Considering that no person in England could succeed in this attempt, and that we are now able to do it but imperfectly, it is hardly probable that he did it at all.

THERE seems to have been some deception in an experiment which the worthy and excellent Dr. Hales communicated to the Royal Society this year, when he says, that he observed the electric spark from warm iron to be of a bright light colour; from warm copper, green; and from a warm egg, of a yellowish flame colour. These experiments, he said, seemed to argue, that some particles of those different bodies were carried off in the electric flashes, whence those different colours were exhibited‡.

I SHALL conclude this section, which might justly be intitled the *marvellous*, with

* Phil. Trans. abridged, Vol. x. p. 413.

† Wilson's Essay, p. 219.

‡ Phil. Trans. abridged, Vol. x. p. 406.

mentioning the surprising effect of an electric spark in setting fire to a fustian frock, on a son of Mr. Robert Roche, when he was electrified for some disorder. I do not question the fact. The experiment was repeated, and it answered again as well as at the first time, when it was merely accidental. The paper containing this account was read at the Royal Society on the 29th of May 1743*.

P E R I O D IX.

THE EXPERIMENTS AND DISCOVERIES OF
DR. FRANKLIN.

S E C T I O N I.

DR. FRANKLIN'S DISCOVERIES CONCERN-
ING THE LEYDEN PHIAL, AND OTHERS
CONNECTED WITH THEM.

WE have hitherto seen what had been done in electricity by the English philosophers, and those on the continent of Europe, till about the year 1750; but our attention is now strongly called to what was doing on the continent of America; where Dr. Franklin and his friends were as assiduous in trying experiments, and as successful in

* Phil. Trans. abridged, Vol. x. p. 406.

making discoveries, as any of their brethren in Europe. For this purpose, we must look back a few years. As Dr. Franklin's discoveries were made intirely independent of any in Europe, I was unwilling to interrupt the former general account, by introducing them in their proper year. For the same reason, I imagine, it will be generally more agreeable, to see, at one view, what was done in America for some considerable space of time, without interrupting this account with what was doing, in the mean time, in Europe. I shall, therefore, digest, in the best manner I can, the three first publications of Dr. Franklin, entitled *New Experiments and Observations on Electricity, made at Philadelphia in America*; communicated in several letters to Peter Collinson, Esq. of London, fellow of the Royal Society; the first of which is dated July 28th, 1747, and the last April 18th, 1754.

NOTHING was ever written upon the subject of electricity which was more generally read and admired in all parts of Europe than these letters. There is hardly any European language into which they have not been translated; and, as if this were not sufficient to make them properly known, a translation of them has lately been made into Latin. It is not easy to say, whether we are most pleased with the simplicity and perspicuity with which these letters are written, the modesty with which the author proposes every hypothesis of his own, or the noble frankness with which he relates his mistakes, when
they

they were corrected by subsequent experiments.

THOUGH the English have not been backward in acknowledging the great merit of this philosopher, he has had the singular good fortune to be, perhaps even more celebrated abroad than at home; so that to form a just idea of the great and deserved reputation of Dr. Franklin, we must read the foreign publications on the subject of electricity; in many of which the terms *Franklinism*, *Franklinist*, and the *Franklinian system* occur in almost every page. In consequence of this, Dr. Franklin's principles bid fair to be handed down to posterity, as expressive of the true principles of electricity; just as the Newtonian philosophy is of the true system of nature in general.

THE zeal of Dr. Franklin's friends, and his reputation, were considerably increased by the opposition which the Abbé Nollet made to his theory. The Abbé, however, never had any considerable seconds in the controversy, and those he had, I am informed, have all deserted him.

THE rise of Dr. Franklin's fame in France was first occasioned by a bad translation of his letters falling into the hands of Mr. Buffon, intendant of the French King's gardens, and author of the Natural History for which he is famous. This gentleman, having successfully repeated Dr. Franklin's experiments, engaged a friend of his, Mr. Dalibard, to revise the translation; which was afterwards published,

ed, with a short history of electricity prefixed to it, and met with a very favourable reception from all ranks of people. What contributed not a little to the success of this publication, and to bring Dr. Franklin's principles into vogue in France, was a friend of Mr. Dalibard's exhibiting Dr. Franklin's experiments for money. All the world, in a manner, flocked to see these new experiments, and all returned full of admiration for the inventor of them*.

DR. FRANKLIN had discovered, as well as Dr. Watson, that the electric matter was not created, but collected by friction, from the neighbouring non-electric bodies. He had observed, that it was impossible for a man to electrify himself, though he stood upon glass or wax; for that the tube could communicate to him no more electricity than it had received from him in the act of excitation. He had observed, that if two persons stood upon wax, one of which rubbed the tube, and the other took the fire from it, they would both appear to be electrified; that if they touched one another after that operation, a stronger spark would be perceived between them, than when any other person touched either of them; and that such a spark would take away the electricity of both†.

THESE experiments led the Doctor to think, that the electric fluid was conveyed from the person who rubbed the tube to him who touch-

* Nollet's Letters, Vol. i. p. 4.

† Franklin's Letters, p. 14.

ed it; which introduced some terms in electricity that had not been used before, but have continued in use ever since. The person who touched the tube was said, by Dr. Franklin, to be electrified *positively*, or *plus*; being supposed to receive an additional quantity of electric fire: whereas the person who rubbed the tube was said to be electrified *negatively*, or *minus*; being supposed to have lost a part of his natural quantity of the electric fluid*.

THIS observation was necessary to explain the capital discovery which Dr. Franklin made with respect to the manner of charging the Leyden phial; which is, that when one side of the glass is electrified positively, or *plus*, the other side is electrified negatively, or *minus*; so that whatever quantity of fire is thrown upon one side of the glass, the same is thrown out of the other; and there is really no more electric fire in the phial after it is charged than before; all that can be done by charging, being to take from one side, and convey to the other. Dr. Franklin also observed, that glass was impervious to electricity, and that, therefore, since the equilibrium could not be restored to the charged phial by any internal communication, it must be done by conductors externally, joining the inside and the outside†.

THESE capital discoveries he made by observing, that when a phial was charged, a cork ball suspended on silk would be attracted

* Franklin's Letters, p. 15.

† Ibid. p. 3.

by the outside coating, when it was repelled by a wire communicating with the inside; and that it would be repelled by the outside, when it was attracted by the inside*. But the truth of this maxim appeared more evident when he brought the knob of the wire communicating with the outside coating within a few inches of the wire communicating with the inside coating, and suspended a cork ball between them; for, in that case, the ball was attracted by them alternately, till the phial was discharged†.

THE European electricians had observed, that a phial could not be charged unless some conductor was in contact with the outside; but Dr. Franklin made the observation more general, and also was able, by the principle above mentioned, to give a better account of it. As no more electric fire, he says, can be thrown into the inside of a phial when all is driven from the outside; so, in a phial not yet charged, none can be thrown into the inside when none can be got from the outside. He also showed, by a beautiful experiment, that, when the phial was charged, one side lost exactly as much as the other gained, in restoring the equilibrium. Hanging a small linen thread near the coating of an electrified phial, he observed, that every time he brought his finger near the wire, the thread was attracted by the coating. For as the fire was taken from the inside by touching the

* Franklin's Letters, p. 4.

† Ibid. p. 5.

wire, the outside drew in an equal quantity by the thread*.

HE proved that, in discharging the phial, the giving from one side was exactly equal to the receiving by the other, by placing a person upon electrics, and making him discharge the phial through his body; when he observed, that no electricity remained in him after the discharge†. He also hung cork balls upon an insulated conductor at the time of the discharge of a phial hanging to it; and observed, that if they did not repel before the explosion, they did not repel at the time, nor after‡. But the experiment which most completely proved, that the coating on one side received just as much as was emitted from the discharge of the other, was the following.

HE insulated his rubber, and then, hanging a phial to his conductor, he found it could not be charged, even though his hand was held constantly to it; because, though the electric fire might leave the outside of the phial, there was none collected by the rubber to be conveyed into the inside. He then took away his hand from the phial, and forming a communication, by a wire from the outside coating to the insulated rubber, he found that it was charged with ease. In this case, it was plain, that the very same fire which left the outside coating was conveyed by the way of the rubber, the globe, the conductor,

* Franklin's Letters, p. 5.

† Ibid p. 8.

‡ Ibid. p. 84.

and the wire of the phial, into the inside*.

DR. FRANKLIN's new theory of charging the Leyden phial led him to observe a greater variety of facts, relating both to charging and discharging it, than other philosophers had attended to. He found that the phial would be electrified as strongly if it were held by the hook, and the coating applied to the globe or tube, as if it were held by the coating, and the hook applied; and, consequently, that there would be the same explosion, and shock, if the electrified phial were held in one hand by the hook, and the coating touched with the other, as when held by the coating, and touched at the hook. To take the charged phial by the hook with safety, and not diminish its force, he observes, that it must first be set down upon electrics per se†.

DR. FRANKLIN observed, that if a man held in his hand two phials, the one fully electrified, and the other not at all, and brought their hooks together, he would have but half a shock: for the phials would both remain only half electrified, the one being half charged, and the other half discharged‡.

If two phials were charged both through their hooks, a cork ball suspended on silk, and hanging between them, would first be attracted, and then repelled by both; but if they were electrified, the one through the

* Franklin's Letters, p. 83.

† Ibid. p. 21.

‡ Ibid. p. 19.

wire, and the other through the coating, the ball would play vigorously between them both, till they were nearly discharged*. The Doctor did not, at that time, take notice, that if the phials were both charged through their coatings (by which both the hooks would have been electrified minus) the ball would be repelled by them both, as when they were electrified plus. And when he, afterwards, observed that two bodies electrified minus repelled one another, he seems to have been surpris'd at the appearance, and acknowledged that he could not satisfactorily account for it†.

It was known to every electrician, that a globe or tube wet on the inside would afford little or no fire; but no good reason was given for it, before Dr. Franklin attempted its explanation by the help of his general maxim. He says, that when a tube lined with any non-electric is rubbed, what is collected from the hand by the downward stroke enters the pores of the glass, driving an equal quantity out of the inner surface, into the non-electric lining; and that the hand, in passing up to take a second stroke, takes out again what had been thrown into the outward surface, the inner surface at the same time receiving back again what it had given to the non-electric lining; so that the particles of the electric fluid go in and out of their pores, upon every stroke given to the tube‡.

* Franklin's Letters, p. 21.

† Ibid. p. 34.

‡ Ibid. p. 7 6.

IF, in these circumstances, a wire was put into the tube, he observed, that, if one person touched the wire, while another was rubbing the tube, and took care to withdraw his finger as soon as he had taken the spark, which had been made to fly from the inside, it would be charged *.

IF the tube was exhausted of air, he observes, that a non-electric lining in contact with the wire was not necessary; for that, in vacuo, the electric fire would fly freely from the inner surface, without a non-electric conductor †.

UPON the same principle he accounts for the effects of an excited electric being perceived through the glass in the vacuum beyond it. The tube and its excited atmosphere, being brought near a glass vessel, repels the electric fluid from the inner surface of the glass; and this fluid, issuing from the inner surface acts upon light bodies in the vacuum, both in its passage from the glass, and likewise in its return to it, when the excited electric on the outside is withdrawn ‡.

THIS maxim, that whatever the phial takes in at one surface it loses at the other, led Dr. Franklin to think of charging several phials together with the same trouble, by connecting the outside of one with the inside of another; whereby the fluid that was driven out of the first would be received by the second,

* Franklin's Letters, p. 77.

† Ibid.

‡ Ibid. p. 78.

and what was driven out of the second would be received by the third, &c. By this means he found, that a great number of bottles might be charged with the same labour as one only; and that they might be charged equally high, were it not that every bottle receives the new fire, and loses its old with some reluctance, or rather gives some small resistance to the charging. This circumstance, he says, in a number of bottles, becomes more equal to the charging power, and so repels the fire back again on the globe sooner than a single bottle would do*.

UPON this principle Dr. Franklin constructed an *electrical battery*, consisting of eleven panes of large sash glass, coated on each side, and so connected, that charging one of them would charge them all. Then having a contrivance to bring all the giving sides in contact with one wire, and all the receiving sides with another, he united the force of all the plates, and discharged them all at once†.

WHEN Dr. Franklin first began his experiments upon the Leyden phial, he imagined that the electric fire was all crowded into the substance of the non-electric in contact with the glass; but he afterwards found, that its power of giving a shock lay in the glass itself, and not in the coating, by the following ingenious analysis of the bottle.

In order to find where the strength of the charged bottle lay, he placed it upon glass;

* Franklin's Letters, p. 12.

† Ibid. p. 26.

then first took out the cork and the wire, and finding the virtue was not in them; he touched the outside coating with one hand, and put the finger of the other into the mouth of the bottle; when the shock was felt quite as strong as if the cork and wire had been in it. He then charged the phial again, and pouring out the water into an empty bottle insulated, expected that if the force resided in the water it would give the shock, but he found it gave none. He then judged that the electric fire must either have been lost in decanting, or must remain in the bottle; and the latter he found to be true; for, filling the charged bottle with fresh water, he found the shock, and was satisfied that the power of giving it resided in the glass itself*.

THE Doctor made the same experiment with panes of glass, laying the coating on lightly, and changing it, as he had before changed the water in the bottle, and the result was the same in both†. This experiment is more satisfactory than the former; because, when the water is poured out of the phial, there still remains a thin coating of the fluid, which might be thought to contain the power of giving a shock.

THAT the electric fire resided in the glass, was also farther evident from this consideration, that when glass was gilt, the discharging of it would make a round hole, tearing off a part of the gilding, which, the Doctor

* Franklin's Letters p. 24.

† Ibid. p. 25.

thought, could only have been done by the fire coming out of the glass through the gilding. He also says, that when the gilding was varnished even with turpentine, this varnish, though dry and hard, would be burned by the spark driven through it, yielding a strong smell, and a visible smoke. Also, that when a spark was driven through paper, it would be blackened by the smoke, which sometimes penetrated several of the leaves, and that part of the gilding which had been torn off was found forcibly driven into the hole made in the paper by the stroke. He also observed, that when a thin bottle was broken by a charge, the glass was broken inwards, at the same time that the gilding was broken outwards *.

LASTLY, Dr. Franklin discovered, that several substances which would conduct electricity in general, would not conduct the shock of a charged phial. A wet packthread, for instance, though it transmitted electricity very well, sometimes failed to conduct a shock; as also did a cake of ice. Dry earth too rammed into a glass tube intirely failed to conduct a shock, and indeed would convey electricity but very imperfectly †.

* Franklin's Letters, p. 32.

† Ibid. p. 33.

SECTION II.

DR. FRANKLIN'S DISCOVERIES CONCERNING THE SIMILARITY OF LIGHTNING AND ELECTRICITY.

THE greatest discovery which Dr. Franklin made concerning electricity, and which has been of the greatest practical use to mankind, was that of the perfect similarity between electricity and lightning. The analogy between these two powers had not been wholly unobserved by philosophers, and especially by electricians, before the publication of Dr. Franklin's discovery. It was so obvious, that it had struck several persons. I shall give one instance, in the sagacious Abbé Nollet.

THE Abbé says*, “ If any one should
 “ take upon him to prove, from a well con-
 “ nected comparison of phenomena, that
 “ thunder is, in the hands of nature, what
 “ electricity is in ours, that the wonders
 “ which we now exhibit at our pleasure are
 “ little imitations of those great effects which
 “ frighten us, and that the whole depends
 “ upon the same mechanism; if it is to be de-
 “ monstrated, that a cloud, prepared by the
 “ action of the winds, by heat, by a mix-
 “ ture of exhalations, &c. is opposite to a

* Leçons de Physique, Vol. iv. p. 34.

“ terrestrial object; that this is the electrized
 “ body, and, at a certain proximity from
 “ that which is not; I avow that this idea, if
 “ it was well supported, would give me a
 “ great deal of pleasure; and in support of
 “ it, how many specious reasons present
 “ themselves to a man who is well acquaint-
 “ ed with electricity. The universality of the
 “ electric matter, the readiness of its action,
 “ its inflammability, and its activity in giv-
 “ ing fire to other bodies, its property of
 “ striking bodies externally and internally,
 “ even to their smallest parts, the remarkable
 “ example we have of this effect in the ex-
 “ periment of Leyden, the idea which we
 “ might truly adopt in supposing a greater
 “ degree of electric power, &c. all these
 “ points of analogy, which I have been some
 “ time meditating, begin to make me be-
 “ lieve, that one might, by taking electrici-
 “ ty for the model, form to one’s self, in re-
 “ lation to thunder and lightning, more per-
 “ fect and more probable ideas than what
 “ have been offered hitherto, &c.”

MR. WINCKLER also enumerated many particulars, in which electricity and lightning resemble one another *.

BUT though the Abbé, and others, had been struck with the obvious analogy between lightning and electricity, they went no farther than these arguments *a priori*. It was Dr. Franklin who first proposed a method of

* Dantzick Memoirs, Vol. iii. p. 528.

verifying this hypothesis, entertaining the bold thought, as the Abbé Nollet expresses it, of bringing lightning from the heavens, of thinking that pointed iron rods, fixed in the air, when the atmosphere was loaded with lightning, might draw from it the matter of the thunderbolt, and discharge it without noise or danger into the immense body of the earth, where it would remain as it were absorbed.

MOREOVER, though Dr. Franklin's directions were first begun to be put in execution in France, he himself completed the demonstration of his own problem, before he heard of what had been done elsewhere: and he extended his experiments so far as actually to imitate almost all the known effects of lightning by electricity, and to perform every electrical experiment by lightning.

BUT before I relate any of Dr. Franklin's experiments concerning lightning, I must take notice of what he observed concerning the power of *pointed bodies*, by means of which he was enabled to carry his great designs into execution. For he was properly the first who observed the intire and wonderful effect of pointed bodies, both in drawing, and throwing off the electric fire.

IT was a small step towards discovering the effect of pointed bodies, that Carolus Augustus van Bergen, professor of medicine at Frankfurt on the Oder, observed that sparks taken from a polished body were stronger than those from a rough one. He could fire spirit easily

with a polished conductor, but with difficulty by means of one not polished*.

MR. JALLABERT was perhaps the first who observed that a body, pointed at one end, and round at another, produced different appearances upon the same body, according as the pointed, or round end was presented to it. But as Mr. Nollet, in whose presence he made the experiment, says, the effect was not constant, and nothing was inferred from it†. And the Abbé acknowledges, that Dr. Franklin was the first who showed the property of pointed bodies, in drawing off electricity more effectually, and at greater distances than other bodies could do it‡.

HE electrified an iron shot, three or four inches in diameter, and observed, that it would not attract a thread, when the point of a needle was presented to it; but that this was not the case, unless the pointed body had a communication with the earth; for, presenting the same pointed body, stuck on a piece of sealing-wax, it had not that effect; though the moment the pointed body was touched with his finger, the electricity of the ball to which it was suspended was discharged. The

* Dantzick Memoirs, Vol. ii. p. 378.

† Lettres, Vol. i. p. 130.

‡ Recherches, p. 132. Dr. Franklin, in the new edition of his Letters, p. 5, says, that the power of points to throw off the electric fire, was communicated to him by his friend Mr. Thomas Hopkinson, who electrified an iron ball of three or four inches diameter, with a needle fastened to it, expecting to draw a stronger spark from the point, as from a kind of focus, but was surprised to find little or none.

converse of this he proved, by finding it impossible to electrify the iron shot when a sharp needle lay upon it*.

By observing points of different degrees of acuteness, Dr. Franklin corrected the conclusion of Mr. Ellicott, and other English electricians, that a pointed body, as a piece of leaf gold, would always be suspended nearer to the plate which was unelectrified than that which was electrified, if it were put between them. For the Doctor observed, that it always removed farthest from that plate to which its sharpest point was presented, whether it was electrified or not; and if one of the points was very blunt, and the other very sharp, it would be suspended in the air by its blunt end, near the electrified body, without any unelectrified plate being held below it at all†.

DR. FRANKLIN endeavoured to account for this effect of pointed bodies, by supposing that the base on which the electric fluid at the point of an electrified body rested, being small, the attraction by which the fluid was held to the body was slight; and that, for the same reason, the resistance to the entrance of the fluid was proportionably weaker in that place than where the surface was flat‡. But he himself candidly owns, that he was not quite satisfied with this hypothesis. Whatever we think of Dr. Franklin's theory of the influence of pointed conductors in

* Franklin's Letters, p. 56, &c.

† Ibid. p. 67.

‡ Ibid. p. 56.

drawing and throwing off the electric fluid, the world is greatly indebted to him for the practical use he made of this doctrine *.

THE manner in which Dr. Franklin first conceived the practicability of drawing lightning from the clouds may be seen in an extract which he has given us from his memorandums, November 7th, 1749. After enumerating all the known points of resemblance between lightning and electricity, he concludes with saying, "The electric fluid is attracted by points. We do not know whether this property be in lightning, but since they agree in all the particulars in which we can already compare them, it is not probable that they agree likewise in this. Let the experiment be made." Every circumstance relating to a discovery of so much importance as this, is interesting and pleasing †.

DR. FRANKLIN begins his account of the similitude of the electric fluid and lightning by cautioning his readers not to be staggered at the great difference of the effects in point of degree; since that is no argument of any disparity in their nature. It is no wonder, says he, if the effects of the one should be so much greater than those of the other. For if two gun barrels electrified will strike at two inches distance, and make a loud report, at how great a distance will ten thousand acres of electrified cloud strike, and give

* Franklin's Letters, p. 62.

† Letters, New Edit. p. 323.

its fire, and how loud must be that crack *!

I SHALL digest all Dr. Franklin's observations concerning lightning under the several points of resemblance which he observed between it and electricity, mentioned these points of similarity in the order in which he himself remarked them; only bringing into one place the observations which may happen to lie in different parts of his letters, when they relate to the same subject.

1. FLASHES of lightning, he begins with observing, are generally seen crooked, and waving in the air. The same, says he, is the electric spark always, when it is drawn from an irregular body at some distance †. He might have added, when it is drawn by an irregular body, or through a space in which the best conductors are disposed in an irregular manner, which is always the case in the heterogeneous atmosphere of our globe.

2. LIGHTNING strikes the highest and most pointed objects in its way preferably to others, as high hills, and trees, towers, spires, masts of ships, points of spears, &c. In like manner, all pointed conductors receive or throw off the electric fluid more readily than those which are terminated by flat surfaces ‡.

3. LIGHTNING is observed to take the readiest and best conductor. So does electri-

* Franklin's Letters, p. 44.

† Ibid. p. 46.

‡ Ibid. p. 47.

city in the discharge of the Leyden phial. For this reason, the Doctor supposes that it would be safer, during a thunder storm, to have one's cloaths wet than dry, as the lightning might then, in a great measure, be transmitted to the ground, by the water, on the outside of the body. It is found, says he, that a wet rat cannot be killed by the explosion of the electrical bottle, but that a dry rat may *.

4. LIGHTNING burns. So does electricity. Dr. Franklin says, that he could kindle with it hard dry rosin, spirits unwarmed, and even wood. He says, that he fired gunpowder, by only ramming it hard in a cartridge, into each end of which pointed wires were introduced, and brought within half an inch of one another, and discharging a shock through them †.

5. LIGHTNING sometimes dissolves metals. So does electricity, though the Doctor was mistaken when he imagined it was by a cold fusion, as will appear in its proper place. The method in which Dr. Franklin made electricity melt metals was by putting thin pieces of them between two panes of glass bound fast together, and sending an electric shock through them. Sometimes the piece of glass, by which they were confined, would be shattered to pieces by the discharge, and be broken into a kind of coarse sand, which once happened with pieces of thick looking-glass; but if they remained whole, the piece

* Franklin's Letters, p. 47.

† Ibid. p. 48, 92.

of metal would be missing in several places where it had lain between them, and instead of it, a metallic stain would be seen on both the glasses, the stains on the under and upper glass being exactly similar in the minutest stroke *.

A PIECE of leaf gold treated in this manner appeared not only to have been melted, but, as the Doctor thought, even vitrified, or otherwise so driven into the pores of the glass, as to be protected by it from the action of the strongest aqua regis. Sometimes he observed that the metallic stains would spread a little wider than the breadth of the thin pieces of metal. True gold, he observed, made a darker stain, somewhat reddish, and silver a greenish stain †.

MR. WILSON supposes that, in this experiment, the gold was not driven into the pores of the glass, but only into so near a contact with the surface of the glass, as to be held there by an exceedingly great force; such an one, he says, as is exerted at the surface of all bodies whatever ‡.

6. LIGHTNING rends some bodies. The same does electricity §. The Doctor observes, that the electric spark would strike a hole through a quire of paper. When wood, bricks, stone, &c. are rent by lightning, he takes notice, that the splinters will fly off on that side where there is the least resistance.

* Franklin's Letters, p. 48, 65.

† Hoadley and Wilson, p. 68.

§ Franklin's Letters, p. 49.

‡ Ibid. p. 68.

In like manner, he says, when a hole is struck through a piece of pasteboard by an electrified jar, if the surfaces of the pasteboard are not confined and compressed, there will be a bur raised all round the hole on both sides of the pasteboard ; but that if one side be confined, so that the bur cannot be raised on that side, it will all be raised on the other side, which way soever the fluid was directed. For the bur round the outside of the hole is the effect of the explosion, which is made every way from the center of the electric stream, and not an effect of its direction *.

7. LIGHTNING has often been known to strike people blind. And a pigeon, after a violent shock of electricity, by which the Doctor intended to have killed it, was observed to have been struck blind likewise †.

8. IN a thunder storm at Stretham, described by Dr. Miles ‡, the lightning stripped off some paint which had covered a gilded molding of a pannel of wainscot, without hurting the rest of the paint. Dr. Franklin imitated this, by pasting a slip of paper over the filleting of gold on the cover of a book, and sending an electric flash through it. The paper was torn off from end to end, with such force, that it was broken in several places ; and in others there was brought away part of the grain of the Turkey leather in which the book was bound. This convinced

* Franklin's Letters, p. 124.

† Ibid. p. 63.

‡ Phil. Transf. abridged, Vol. xlv, p. 387.

the Doctor, that if it had been paint, it would have been stripped off in the same manner with that on the wainscot at Stret-ham *.

9. LIGHTNING destroys animal life. Animals have likewise been killed by the shock of electricity. The largest animals which Dr. Franklin and his friends had been able to kill were a hen, and a turkey which weighed about ten pounds †.

10. MAGNETS have been observed to lose their virtue, or to have their poles reversed by lightning. The same did Dr. Franklin by electricity. By electricity he frequently gave polarity to needles, and reversed them at pleasure. A shock from four large jars, sent through a fine sewing needle, he says, gave it polarity, so that it would traverse when laid on water. What is most remarkable in these electrical experiments upon magnets is, that if the needle, when it was struck, lay East and West, the end which was entered by the electric blast pointed North, but that if it lay North and South, the end which lay towards the North, would continue to point North, whether the fire entered at that end or the contrary; though he imagined, that a stronger stroke would have reversed the poles even in that situation, an effect which had been known to have been produced by lightning. He also observed, that the polarity was strongest when the needle was struck ly-

* Phil. Trans. abridged, Vol. xlv. p. 64.

† Franklin's Letters, p. 86, 153.

ing North and South, and weakest when it lay East and West. He takes notice that, in these experiments, the needle, in some cases, would be finely blued, like the spring of a watch, by the electric flame; in which case the colour given by a flash from two jars only might be wiped off, but that a flash from four jars fixed it, and frequently melted the needles. The jars which the Doctor used held seven or eight gallons, and were coated and lined with tinfoil *.

To demonstrate, in the completest manner possible, the sameness of the electric fluid with the matter of lightning, Dr. Franklin, astonishing as it must have appeared, contrived actually to bring lightning from the heavens, by means of an electrical kite, which he raised when a storm of thunder was perceived to be coming on. This kite had a pointed wire fixed upon it, by which it drew the lightning from the clouds. This lightning descended by the hempen string, and was received by a key tied to the extremity of it; that part of the string which was held in his hand being of silk, that the electric virtue might stop when it came to the key. He found that the string would conduct electricity even when nearly dry, but that when it was wet, it would conduct it quite freely; so that it would stream out plentifully from the key, at the approach of a person's finger †.

* Franklin's Letters, p. 90, &c.

† Ibid. p. 106.

AT this key he charged phials, and from electric fire thus obtained, he kindled spirits, and performed all other electrical experiments which are usually exhibited by an excited globe or tube.

As every circumstance relating to so capital a discovery as this (the greatest, perhaps, that has been made in the whole compass of philosophy, since the time of Sir Isaac Newton) cannot but give pleasure to all my readers, I shall endeavour to gratify them with the communication of a few particulars which I have from the best authority.

THE Doctor, after having published his method of verifying his hypothesis concerning the sameness of electricity with the matter lightning, was waiting for the erection of a spire in Philadelphia to carry his views into execution; not imagining that a pointed rod, of a moderate height, could answer the purpose; when it occurred to him, that, by means of a common kite, he could have a readier and better access to the regions of thunder than by any spire whatever. Preparing, therefore, a large silk handkerchief, and two cross sticks, of a proper length, on which to extend it, he took the opportunity of the first approaching thunder storm to take a walk into a field, in which there was a shed convenient for his purpose. But dreading the ridicule which too commonly attends unsuccessful attempts in science, he communicated his intended experiment to no body but

but his son, who assisted him in raising the kite.

THE kite being raised, a considerable time elapsed before there was any appearance of its being electrified. One very promising cloud had passed over it without any effect; when, at length, just as he was beginning to despair of his contrivance, he observed some loose threads of the hempen string to stand erect, and to avoid one another, just as if they had been suspended on a common conductor. Struck with this promising appearance, he immediately presented his knuckle to the key, and (let the reader judge of the exquisite pleasure he must have felt at that moment) the discovery was complete. He perceived a very evident electric spark. Others succeeded, even before the string was wet, so as to put the matter past all dispute, and when the rain had wetted the string, he collected electric fire very copiously. This happened in June 1752, a month after the electricians in France had verified the same theory, but before he had heard of any thing that they had done.

BESIDES this kite, Dr. Franklin had afterwards an insulated iron rod to draw the lightning into his house, in order to make experiments whenever there should be a considerable quantity of it in the atmosphere; and that he might not lose any opportunity of that nature, he connected two bells with this apparatus, which gave him notice,

by their ringing, whenever his rod was electrified *.

THE Doctor being able, in this manner, to draw the lightning into his house, and make experiments with it at his leisure; and being certain that it was in all respects of the same nature with electricity, he was desirous to know if it was of the positive or negative kind. The first time he succeeded in making an experiment for this purpose was the 12th of April 1753, when it appeared that the lightning was negative. Having found that the clouds electrified negatively in eight successive thunder gusts, he concluded they were always electrified negatively, and formed a theory to account for it. But he afterwards found he had concluded too soon. For, on the sixth of June following, he met with one cloud which was electrified positively; upon which he corrected his former theory, but did not seem able perfectly to satisfy himself with any other. The Doctor sometimes found the clouds would change from positive to negative electricity several times in the course of one thunder gust, and he once observed the air to be strongly electrified during a fall of snow, when there was no thunder at all †.

BUT the grand practical use which Dr. Franklin made of his discovery of the sameness of electricity and lightning, was to se-

* Franklin's Letters, p. 112.

† Ibid. p. 112, &c.

cure buildings from being damaged by lightning, a thing of vast consequence in all parts of the world, but more especially in several parts of North America, where thunder storms are more frequent, and their effects, in that dry air, more dreadful, than they are ever known to be with us.

THIS great end Dr. Franklin accomplished by so easy a method, and by so cheap, and seemingly trifling apparatus, as fixing a pointed metalline rod higher than any part of the building, and communicating with the ground, or rather the nearest water. This wire the lightning was sure to seize upon, preferably to any other part of the building; whereby this dangerous power would be safely conducted to the earth, and dissipated, without doing any harm to the building*.

DR. FRANKLIN was of opinion, that a wire of a quarter of an inch in thickness would be sufficient to conduct a greater quantity of lightning than was ever actually discharged from the clouds in one stroke. He found, that the gilding of a book was sufficient to conduct the charge of five large jars, and thought that it would probably have conducted the charge of many more. He also found by experiment, that if a wire was destroyed by an explosion, it was yet sufficient to conduct that particular stroke, though it was thereby rendered incapable of conducting another†.

* Franklin's Letters, p. 62, 124. † Ibid. p. 124, 125.

THE Doctor also supposed, that pointed rods erected on edifices might likewise often prevent a stroke of lightning in the following manner. He says, that an eye so situated as to view horizontally the underside of a thunder cloud, will see it very ragged, with a number of separate fragments, or petty clouds, one under another, the lowest sometimes not far from the earth. These, as so many stepping stones, assist in conducting a stroke between a cloud and a building. To represent these by an experiment, he directs us to take two or three locks of fine loose cotton and connect one of them with the prime conductor, by a fine thread of two inches (which may be spun out of the same lock) another to that, and a third to the second, by like threads. He then bids us to turn the globe, and says we shall see these locks extending themselves towards the table (as the lower small clouds do towards the earth) but, that, on presenting a sharp point, erect under the lowest, it will shrink up to the second, the second to the first, and all together to the prime conductor, where they will continue as long as the point continues under them. A most ingenious and beautiful experiment! May not, he adds, in like manner, the small electrified clouds, whose equilibrium with the earth is soon restored by the point, rise up to the main body, and by that means occasion so large a vacancy, as that the grand cloud cannot strike in that place*.

* Franklin's Letters, p. 121, &c.

MR. WILCKE, in his remarks on Dr. Franklin's letters, says, that on the 20th of August 1758, he saw this supposition verified; as he was viewing a large fringed cloud, strongly electrified, passing over a forest of tall fir trees. The ragged and depending parts of the large cloud were first attracted lower, and then suddenly rose higher, and joined the large cloud*.

He was also an eye witness of two clouds lying one over the other, approaching, and flashing into one another. The lightning spread itself over all the parts of the blacker cloud, which was negative, and which immediately began to dissolve in rain†.

DR. FRANKLIN advises persons who are apprehensive of danger from lightning, to sit in the middle of a room (provided it be not under a metal lustre suspended by a chain) sitting on one chair, and laying their feet on another. It is still safer, he says, to bring two or three mattresses, or beds, into the middle of the room, and folding them double, to place the chairs upon them, for they not being so good conductors as the walls, the lightning will not chuse to pass through them; but the safest place of all is in a hammock hung in silken cords, at an equal distance from all the sides of the room, p. 484. I would add, that the place of most absolute safety must be the cellar, and especially the middle of it; for when a person is lower than

* Wilcke's Translation, p. 351.

† Franklin's Letters, p. 259.

the surface of the earth, the lightning must strike it before it can possibly reach him. In the fields, the place of safety is, within a few yards of a tree, but not quite near it.

S E C T I O N III.

MISCELLANEOUS DISCOVERIES OF DR.
FRANKLIN, AND HIS FRIENDS IN
AMERICA, DURING THE SAME PERIOD.

DR. FRANKLIN, retaining the common opinion, that electrified bodies have real atmospheres of the electric fluid (consisting of particles at some distance from the surface of the body, but always going along with it) observed that these atmospheres and the air did not seem to exclude one another; though, he says, this be difficult to conceive, considering that they are generally supposed to repel one another.

An electric atmosphere, he says, raised round a thick wire, inserted into a phial, drives out none of the air it contained; nor on withdrawing that atmosphere, will any air rush in, as he found by a very curious experiment, accurately made; whence he also concluded, that the elasticity of the air was not affected by it *.

THE experiment, as the Doctor informs me, was made with a small glass syphon,

* Franklin's Letters, p. 98.

one leg passing through the cork into the bottle. The other leg had in it a drop of red ink, which readily moved on the least change of heat or cold in the air contained in the phial; but not at all on the air's being electrified.

He also made an experiment which would seem to prove the immobility, as we may say, of these atmospheres by any external force, if they have any existence at all; but others may think it is rather an argument against their existence. He electrified a large cork ball fastened to the end of a silk string three feet long; and, taking the other end in his hand, he whirled it round, like a sling, a hundred times in the open air, with the swiftest motion he could possibly give it; and observed, that it still retained its electric atmosphere, though it must have passed through eight hundred yards of air *.

To show that a body, in different circumstances of dilatation and contraction, is capable of receiving, or retaining more or less of the electric fluid on its surface, he made the following curious experiment. He electrified a silver cann, in which there were about three yards of brass chain, one end of which he could raise to what height he pleased, by means of a pulley and a silken cord. He suspended a lock of cotton by a silken string from the ceiling of the room, making it hang near the cup; and observed, that every time he drew up the

* Franklin's Letters, p. 97.

chain, the cotton approached nearer to the cup, and as constantly receded from it when the chain was let down. From this experiment it was evident, he says, that the atmosphere about the cup was diminished by raising the chain, and increased by lowering it; and that the atmosphere of the chain must have been drawn from that of the cup when it was raised, and have returned to it again when it was let down*.

To make electric atmospheres in some measure visible, the Doctor used to drop rosin on hot iron plates held under bodies electrified; and, in a still room, the smoke would ascend, and form visible atmospheres round the bodies, making them look very beautiful. In trying in what circumstances, the repellency between an electrified iron ball, and a small cork ball would be altered, he observed, that the smoke of rosin did not destroy their repellency, but was attracted both by the iron and the cork†.

THE Doctor observed, that silver exposed to the electric spark would acquire a blue stain, and that iron would seem corroded by it; but he could never perceive any impression made on gold, brass, or tin. The spots on the silver or iron were always the same, whether they received the spark from lead, brass, gold, or silver; and the smell of the electric fire was the same, through whatever bodies it was conveyed‡.

* Franklin's Letters, p. 121.

† Ibid. p. 81, 98.

‡ Ibid. p. 55.

WHILE we are attending to what was done by Dr. Franklin at Philadelphia, we must by no means overlook what was done by Mr. Kinnerley, the Doctor's friend, while at Boston in New England. Some of his observations, of which an account is given in the Doctor's letters, are very curious; and some later accounts, which he himself has transmitted to England, seem to promise, that, if he continue his electrical inquiries, his name, after that of his friend, will be second to few in the history of electricity.

HE first distinguished himself by re-discovering Mr. Du Fay's two contrary electricities of glass and sulphur, with which both he and Dr. Franklin were at that time wholly unacquainted. But Mr. Kinnerley had a great advantage over Mr. Du Fay; for making his experiments in a more advanced state of the science, he saw immediately, that the two contrary electricities of glass and sulphur were the very same positive and negative electricities, which had just been discovered by Dr. Watson and Dr. Franklin.

HE observed, that a cork ball, electrified by a conductor from excited glass, would be attracted by excited amber and sulphur, and repelled by excited glass and china; that electrifying the ball with the wire of a charged phial, it would be repelled by excited glass, but attracted by excited sulphur; and that when he electrified it by sulphur or amber, till it became repelled by them, it would be attracted by the wire of the phial, and repelled by its

Q

coating.

coating. These experiments surpris'd him very much, but by analogy he was led to infer, *a priori*, the following paradoxes, as he calls them, which were afterwards verified by Dr. Franklin at his request *.

“ 1. IF a glass globe be placed at one end
“ of a prime conductor, and a sulphur one at
“ the other, both being equally in good or-
“ der, and in equal motion, not a spark of
“ fire can be obtained from the conductor,
“ but one globe will draw out as fast as the
“ other gives in.

“ 2. IF a phial be suspended on the con-
“ ductor with a chain from its coating to the
“ table, and only one of the globes be made
“ use of at a time, twenty turns of the wheel,
“ for instance, will charge it; after which,
“ as many turns of the other wheel will
“ discharge it; and as many more will charge
“ it again.

“ 3. THE globes being both in motion,
“ each having a separate conductor, with a
“ phial suspended on one of them, and the
“ chain fastened to the other; the phial will
“ become charged, one globe charging posi-
“ tively, and the other negatively.

“ 4. THE phial being thus charged, hang
“ it in like manner, on the other conductor.
“ Set both wheels a-going again, and the
“ same number of turns that charged it be-
“ fore will now discharge it, and the same
“ number repeated will charge it again.

* Franklin's Letters, p. 99.

“ 5. When

“ 5. WHEN each globe communicates with
 “ the same prime conductor, having a chain
 “ hanging from it to the table, one of them,
 “ when in motion (but which I cannot say)
 “ will draw fire up through the cushion, and
 “ discharge it, through the chain; and the
 “ other will draw it up through the chain,
 “ and discharge it through the cushion*.”

WHEN Mr. Kinnerseley was advising his friend to try the experiments with the sulphur globe, he cautions him not to make use of chalk on the cushion, telling him that some fine powdered sulphur would do better. And he expresses his hope that if the Doctor should find the two globes to charge the prime conductor differently, he would be able to discover some method of determining which it was that charged positively.

DR. FRANKLIN, when these experiments and conjectures were proposed to him, had no idea of their having any real foundation; but imagined, that the different attractions and repulsions observed by Mr. Kinnerseley proceeded rather from the greater or smaller quantities of the electric fire, obtained from different bodies, than from its being either of a different kind, or having a different direction. But finding, upon trial, that the principal of Mr. Kinnerseley's suppositions were verified by fact, he had no doubt of the rest †.

* Franklin's Letters, p. 100.

† Ibid. p. 102, 103.

IN answer to the doubt of Mr. Kinnerfley, whether the glass, or the sulphur electrified positively, the Doctor gave it as his opinion, that the glass globe charged positively, and the sulphur negatively, for the following reasons.

1. BECAUSE, though the sulphur globe seemed to work equally well with the glass one, yet it could never occasion so large, and so distant a spark between his finger and conductor as when the glass globe was used. But what he adds to confirm this proof does not seem to be satisfactory. He supposes that bodies of a certain bigness cannot so easily part with the quantity of electric fluid which they have, and hold attracted within their substance, as they can receive an additional quantity upon their surface, by way of atmosphere; and that therefore so much could not be drawn out of the conductor, as might be thrown on it*.

2. He observed that the stream or brush of fire, appearing at the end of the wire connected with the conductor, was long, large, and much diverging when the glass globe was used, and made a snapping or rattling noise; but that when the sulphur globe was used, it was short, small, and made a hissing noise. He also observed, that just the reverse of both these cases happened when he held the same wire in his hand, and the globes were worked alternately. The brush was large,

* Franklin's Letters, p. 104.

long, diverging, and snapping or rattling, when the sulphur globe was turned; but short, small, and hissing, when the glass globe was turned. When the brush was long, large, and much diverging, it seemed to the Doctor, that the body to which it joined was throwing the fire out, and when the contrary appeared, it seemed to be drinking in *.

3. HE observed, that when he held his knuckle before the sulphur globe, while it was turning, the stream of fire between his knuckle and the globe seemed to spread on its surface, as if it flowed from the finger, but before the glass globe it was otherwise.

4. He observed that the cool wind (or what was called so) which is felt as coming from an electrified point, was much more sensible when the glass globe, than when the sulphur one was used. But these, though the best arguments which the senses can furnish, of the course of the electric fluid, the Doctor acknowledges were but hasty thoughts. Indeed, considering that the velocity of the electric fluid has been found, by experiment, to be nearly instantaneous, in a circuit of many miles, it cannot be supposed that the eye should be able to distinguish which way it goes in the space of one or two inches †.

I SHALL conclude this article with observing that the experiments, which the Doctor made with globes of glass and sulphur, are

* Franklin's Letters, p. 104.

† Ibid. p. 105.

much more easily exhibited by the conductor and insulated rubber of either of them, all the effects being the reverse of each other.

I MUST now, for the present, take leave of this ingenious writer and his friends, after having brought the history of their labours to the year 1754, and must return to see what was doing on the continent of Europe for two or three years preceding this date, while we left it to go over to America.

P E R I O D X.

THE HISTORY OF ELECTRICITY, FROM THE TIME THAT DR. FRANKLIN MADE HIS EXPERIMENTS IN AMERICA, TILL THE YEAR 1766.

WE are now entering upon the last period into which the history of electricity divides itself, in which the great variety of matter presented to our view must oblige an historian to have recourse to the strictest method; for, otherwise, the narration would be extremely perplexed and disgusting. As this period contains the events of a larger space of time than most of the others, yet without any convenient resting place; as the business of electricity has been considerably multiplied in it, and a greater number of labourers have been employed in gathering in the harvest of discoveries, the seeds of which were sown by Dr. Watson, Dr. Franklin, and others,

others, in the preceding periods; I am obliged to subdivide this into more distinct parts, but I hope they will not be found to be more than were necessary, in order to prevent confusion.

HOWEVER, this circumstance, of the great quantity and variety of materials furnished in this period, in proportion as it tends to embarrass an historian, and exercise his talent for proper distribution and arrangement, is a striking demonstration of a truth, which must give the greatest pleasure to all the lovers of electricity and Natural Philosophy. If the progress continue the same in another period, of equal length, if the harvest of discoveries continue to be more plentiful, and the labourers proportionably more numerous; what a glorious scene shall we see unfolded, what a fund of entertainment is there in store for us, and what important benefits may be derived to mankind!

SECTION I.

IMPROVEMENTS IN THE ELECTRICAL APPARATUS, WITH EXPERIMENTS AND OBSERVATIONS RELATING TO IT.

AS our electrical apparatus has been much improved within this period, I shall first recite what has occurred to me upon this subject; particularly the methods which have, from time to time, been communicated of increasing the power of electricity, by different circumstances of excitation.

So early as the year 1751, upon occasion of trying Mr. Winckler's experiments, notice is taken of Mr. Canton's method of rubbing tubes with silk prepared with linseed oil. These he had found, by the experience of some considerable time, to produce the greatest effect upon tubes, but he had not found that they were proportionably useful in rubbing globes*.

UPON another occasion, Mr. Canton observes, that by means of this rubber, a solid cylinder of glass, which had been set before the fire till it was quite dry, might be excited as easily as a glass tube, so as to act like one in every respect; that even the first stroke would make it strongly electrical†.

* Phil. Trans. abridged, Vol. xlvii, p. 239.

† Ibid. Vol. xlviii. pt. ii. p. 784.

BUT the greatest improvement which Mr. Canton discovered for increasing the power of electricity, was by rubbing on the cushion of the globe, or on the oiled silk rubber of the tube, a small quantity of an amalgam of mercury and tin, with a very little chalk or whiting. By this means, a globe or tube may be excited to a very great degree with very little friction, especially if the rubber be made more damp or dry as occasion may require *.

MR. WILCKE says, that a glass tube excited with a woollen cloth, on which some white wax or oil has been put, will throw out flames with a great noise in the dark †. These flames, he says, he never knew to be thrown from a globe, except sometimes when they were first used ‡.

OUR electrical apparatus has been much augmented within this period by the discovery of Father Windelinus Ammerfin of Switzerland, who, in a Latin treatise, published at Lucern, in the year 1754, has shewn us, that wood properly dried, till it becomes very brown, is a non-conductor of electricity. He recommends boiling the wood in linseed oil, or covering it over with varnish, after being dried, to prevent any return of moisture into its pores; and adds, that wood, so treated, seems to afford stronger appearances of electricity than even glass. He himself made use of common wooden measures, such as are

* Phil. Trans. Vol. lii. pt. ii. p. 461.

† Wilcke, p. 124.

‡ Ibid. p. 126.

usually found in granaries, first boiled in oil, and afterwards mounted, so as to be turned by a wheel *.

It appears from the Philosophical Transactions, says Mr. Wilson, so early as the year 1747, that Dr. Watson having occasion to support a long wire, in an experiment made near Shooter's Hill, with a view to determine the velocity of electric fluid, used stakes of dry wood, which he told him, were baked, to prevent the electric fluid from escaping into the ground †.

A MORE extraordinary method of procuring electricity than by baked wood, was one that Signior Beccaria made use of. He put a dry and warm cat's skin upon his glass globe, and rubbing it with his hand, excited a very powerful electricity ‡.

THESE wooden cylinders electrify positively or negatively as the rubber is silk or flannel, but much more powerfully when negative than when positive, owing to the roughness which there generally is upon their surfaces, and therefore make an agreeable variety in an electrical apparatus. But the oldest and most usual method of procuring negative electricity was by globes of sulphur. These Mr. Le Roi made by putting a coating of sulphur upon a globe of glass, and then smoothing it with an hot iron; but Mr. Nollet preferred melting the sulphur in the inside

* Phil. Trans. Vol. lii. pt. i. p. 342.

† Ibid. Vol. li. pt. ii. p. 896.

‡ Lettere dell' Eletticismo, p. 58.

of the glass globe, and then breaking the glass from off it, because this method made a much finer polish *.

ONE globe he made of a mixture of sulphur and pounded glass, but he found that it had the same effect as if it had been all of sulphur. He says that, when one part of this globe was excited, the whole surface became electrical †.

BUT since Mr. Canton's discovery of the negative power of rough glass, some philosophers have made use of glass globes made rough by emery; and the usual method of taking off their polish was by rubbing them as they turned upon their axis; but Mr. Speedler, a mathematical instrument maker at Copenhagen, observes, in his letters upon the subject of electricity, that glass globes, made rough by drawing the stone, or emery, from pole to pole, have a much greater virtue; this method of taking off the polish giving them a greater roughness with respect to the rubber ‡.

BUT a better, and a readier method than all these of producing negative electricity, is by insulating the rubber of a smooth globe, and connecting it with an insulated prime conductor, while the common conductor hath a communication with the ground. The rubber, if well insulated, is sure to produce a negative electricity, equal in power to the positive of the same globe. Mr. Dalibard directs a

* Nollet's Letters, Vol. ii. p. 121.

† Ibid. p. 125, 127.

‡ Wilcke, p. 57.

great number of precautions, in order to electrify well at the rubber, and to prevent it from receiving any electric fire in its state of insulation *.

MR. BERGMAN of Upsal, says, that very often, when his glass globes could not be excited to a sufficient degree of strength, he lined them with a thin coating of sulphur, and that then they gave a much stronger positive electricity than before †.

IN Italy, and other places, Mr. Nollet informs us, it is the custom of electricians to put a coating of pitch, or other resinous matter on the inside of their globes, which they pretend, makes them always work well ‡.

WE are obliged to the Abbé Nollet for some observations on the electrical powers of different kinds of glass, in the sixth volume of his *Leçons de physique* printed in the year 1764.

IT is not every sort of glass, says he, that is equally electrizable. There are some sorts which are not so at all, or hardly at all; such, for example, is that of which they make plates of glass at St. Gobin in Picardy. I have tried it, says he, an hundred times, in the form of plates, tubes, and globes, and in all kinds of weather, but have scarce been ever able to draw from it the least sensible sign of electricity.

* Dalibard's Franklin, p. 110.

† Phil. Trans. Vol. lli. pt. ii. p. 485.

‡ Lettres, Vol. ii. p. 122.

THE glass of which panes for windows are made, and which is also used for drinking-glasses, when it is newly manufactured, is excited with great difficulty. I have often, says he, repeatedly rubbed tubes, and other pieces, even in the glass-house where they were made, but without success; and it has not been till after some months, and sometimes years, that I could bring them to act.

It is certain, and he says he has constantly observed, that glass becomes more fit for electrical experiments by force of rubbing and that sometimes it has required some months to bring globes and tubes to act well.

He did not think that these facts could be accounted for either by the different degrees of transparency, or the different colours of glass. This, indeed, was evident from some globes acquiring electricity from use which had it not originally. The glass of which bottles are made at Severs served him very well, whereas globes of white glass did not become tolerable till after having been used a certain time.

He could not tell positively why certain kinds of glass were electrizable or not by rubbing, but he suspected, that it was principally owing to the degree of its hardness and vitrification. He was induced to think so, because he found that the glass at the French manufactory at St. Gobin, and at Cherbourg (the hardest, the most compact, and the best vitrified of all the kinds of glass in France)

was

was the most difficult to be electrized; whereas the crystal glass of England, that of Bohemia, &c. which are much softer, were the best of all for experiments in electricity. He says, moreover, that he had procured imperfect glasses, which had not been long enough in the furnace to be clear; and that, though they were of the same composition as plates of glass, which, he observed before, were not easily electrized, yet that these were excited very sensibly.

He says that a globe of ten or twelve inches diameter, and which makes about four revolutions in a second of time, will receive a convenient rubbing; but that we must not expect that if the globe be one half, or one fourth part greater or less, the effects will be increased or diminished in proportion*.

UPON the subject of insulating bodies, he observes, that when the cakes of sulphur, resin, sealing-wax, and bees-wax are made use of for this purpose, they ought to be well cooled before they are used: for, he says, he has constantly observed, that when they are newly made, they are not so proper to insulate bodies, as they generally are at the end of some months†.

It will be proper, under this head, to acquaint young electricians, that globes have been several times known to burst during the act of excitation, and that the fragments have been thrown with great violence in every di-

* *Leçons de Physique*, Vol. vi. p. 273—276.

† *Ibid.* p. 299.

rection, so as to be very dangerous for the by-standers. This accident happened to Mr. Sabatelli in Italy, Mr. Nollet in France, Mr. Beraud at Lyons, Mr. Boze at Wittemburgh, Mr. Le Cat at Rouen, and Mr. Robein at Rennes.

THE air in the inside of Mr. Sabatelli's globe had no communication with the external air, but that of the Abbé Nollet had. This last, which was of English flint, which had been used two years, and which was more than a line thick, burst like a bomb in the hands of a servant who was rubbing it; and the fragments (the largest of which were not more than an inch in diameter) were dispersed on all sides, to a considerable distance. The Abbé says, that all the globes which were burst in that manner exploded after five or six turns of the wheel; and he ascribes this effect to the action of the electric matter, making the particles of the glass vibrate in a manner he could not conceive*.

WHEN Mr. Beraud's globe burst (and he was the first to whom this accident was ever known to happen) he was making some experiments in the dark, on the 8th of February 1750; when a noise was first heard, as of something rending to pieces; then followed the explosion, and when the lights were brought in, it was observed, that those places of the floor which were opposite to the equatorial diameter of the globe were strewed with

* Nollet's Letters, Vol. i. p. 19.

smaller pieces, and in greater numbers than those which were opposite to other parts of it. This globe had been cracked, but it had been in constant use in that state above a year, and the crack had extended itself from the pole to the equator. The proprietor ascribed the accident to the vibration of the particles of the glass, and thought that the crack had some way impeded those vibrations*.

WHEN Mr. Boze's globe broke, he says that the whole of it appeared in the act of breaking, like a flaming coal; a circumstance which we shall see accounted for hereafter by Mr. Wilcke†.

MR. BOULANGER says, that glass globes have sometimes burst like bombs, and have wounded many persons, and that their fragments have even penetrated several inches into a wall‡. He also says, that if globes burst in whirling by the gun-barrel's touching them, they burst with the same violence, the splinters often entering into the wall§.

THE Abbé Nollet had a globe of sulphur which also burst, as he was rubbing it with his naked hands, after two or three turns of the wheel, having first cracked inwardly. It broke into very small pieces, which flew to a great distance; and into a fine dust, of which part flew against his naked breast; where it entered the skin so deep, that it could not be got off without the edge of a knife||.

* Histoire, p. 87.

† Wilcke, p. 124.

‡ Boulanger, p. 23.

§ Ibid. p. 144.

|| Nollet's Letters, Vol. ii. p. 220.

SECTION II.

OBSERVATIONS ON THE CONDUCTING POWER
OF VARIOUS SUBSTANCES, AND PAR-
TICULARLY MR. CANTON'S EXPERI-
MENTS ON AIR; AND SIGNIOR BECCA-
RIA'S ON AIR, AND WATER.

ONE of the principal *desiderata* in the science of electricity, is to ascertain wherein consists the distinction between those bodies which are conductors, and those which are non-conductors of the electric fluid. All that has been done relating to this question, till the present time, amounts to nothing more than observations, how near these two classes of bodies approach one another; and before the period of which I am now treating, these observations were few, general, and superficial. But I shall now present my reader with several very curious and accurate experiments, which, though they do not give us intire satisfaction with respect to the great *desideratum* above mentioned; yet throw some light upon the subject. They show that substances which had been considered as perfect conductors, or non-conductors, are so only to a certain degree; and that, probably, all the known parts of nature have, in some measure, the properties of both.

THESE experiments were made by two persons, whom, in the style of history, I
R may

may justly call two of the greatest *heroes* of this part of my work, viz. Mr. CANTON, whose discoveries in electricity are far more numerous, and more considerable than those of any other person, within this period, in England; and Signior BECCARIA, one of the most eminent of all the electricians abroad.

THAT air was capable of receiving electricity by communication, and of retaining it when received, had not been discovered by any person before Mr. Canton; but, by the help of one of his exquisite contrivances, he was able to ascertain that delicate circumstance, and even measure the degree of it, if it was in the least considerable.

He got a pair of balls, turned in a lathe, out of the dry pith of elder. These he put into a narrow box, with a sliding cover, so disposed that the threads (which were of the finest linen) were kept straight in the box. Holding this box by the extremity of the cover, the balls would hang freely from a pin in the inside. These balls hung at a sufficient distance from buildings, trees, &c. easily show the electricity of the atmosphere. They also determine whether the electricity of the clouds and the air be positive, by the decrease; or negative, by the increase of their repulsion, at the approach of excited amber or sealing-wax.

By the help of this instrument, he observed, that it was possible to electrify the air of a room near the apparatus; and even
the

the air of the whole room in which it was, to a considerable degree, and he was able to do it both positively and negatively.

IN a paper read at the Royal Society, December the 6th, 1753, he observes, that the common air of a room might be electrified to a considerable degree, so as not to part with its electricity for some time. Having rendered the air of his room very dry, by means of a fire, he electrified a tin tube (with a pair of balls suspended at one of its extremities) to a great degree; when it appeared, that the neighbouring air was likewise electrified. For, having touched the tube with his finger, or any other conductor, the balls, notwithstanding, continued to repel one another, though not at so great a distance as before*. But he observes that their repulsion would decrease as they were moved towards the floor, wainscot, or any of the furniture; and that they would touch each other when brought within a small distance of any conductor. Some degree of this electric power, he has known to continue in the air above an hour after the rubbing of the tube, when the weather had been very dry.

To electrify the air, or the moisture contained in it, negatively, Mr. Canton supported, by silk stretched between two chairs (placed back to back, at the distance of about three feet), a tin tube with a fine sewing needle at

* Phil. Trans. Vol. xlix. pt. i. p. 300.

one end of it; and rubbed sulphur, sealing-wax, or a rough glass tube as near as he could to the other end, for three or four minutes; after which he found the air to be negatively electrical, and that it would continue so a considerable time after the apparatus was removed into another room *.

IN a paper dated November the 11th, 1754, he says, that dry air, at a great distance from the earth, if in an electric state, will continue so till it meets with some conductor, is probable from the following experiment. An excited glass tube, with its natural polish, being placed upright in the middle of a room (by putting one end of it into an hole, made for that purpose, in a block of wood) would, generally, lose its electricity in less than five minutes, by attracting to it a sufficient quantity of moisture, to conduct the electric fluid from all parts of its surface to the floor; but if, immediately after it was excited, it was placed, in the same manner, before a good fire, at the distance of about two feet, where no moisture would adhere to its surface, it would continue electrical a whole day, and how much longer he knew not †.

SINCE the publication of the first edition of this work, Mr. Canton has hit upon another, much readier, and more powerful method of communicating electricity to the air than that described above. This he gives me

* Phil. Transf. Vol. xlviii. pt. ii. p. 784.

† Ibid.

leave to publish, and it appears to me to be of such a nature, as that it may very possibly lead to farther discoveries concerning the electricity of the atmosphere, and the phenomena depending upon it. "Take," says he, "a charged phial in one hand, and a lighted candle, insulated, in the other; and, going into any room, bring the wire of the phial very near to the flame of the candle, and hold it there about half a minute; then carry the phial and candle out of the room, and return with the pith balls, suspended, and held at arm's length. The balls will begin to separate on entering the room, and will stand an inch and half, or two inches a part, when brought near the middle of it."

SIGNIOR BECCARIA, who had no knowledge of what Mr. Canton had done, made the same discovery of the communication of electricity to the air, and diversified the experiment in a much more pleasing and satisfactory manner. He proves, that the air, which is contiguous to an electrified body, acquires, by degrees, the same electricity; that this electricity of the air counteracts that of the body, and lessens its effects, and that as the air acquires, so it also parts with this electricity very slowly.

He began his experiments by hanging linen threads upon an electrified chain, and observing, that they diverged the most after a few turns of his globe. After that, they came nearer together, notwithstanding he

kept turning the globe and the excitation was as powerful as ever *.

WHEN he had kept the chain electrified a considerable time, and then discontinued the friction, the threads collapsed by degrees, till they hung parallel. After this, they began to diverge again, without any fresh electrification; and, if the air was still, this second divergence would continue an hour, or more.

THIS divergence was lessened by the electrification of the chain. For if the globe was turned again, the threads would first become parallel, and then begin to diverge again as before. Thus the second divergence of the threads took place, when the chain was deprived of its electricity, and when that which the air had acquired began to show itself.

WHILE the threads were beginning to diverge with the electricity of the air, if he touched the chain, and thereby took off what remained of its electricity, the threads would separate farther. Thus the more the electricity of the chain was lessened, the more did the electricity of the air appear.

WHILE the threads were in their second divergence, he hung two other threads, shorter than the former, by another silk thread to the chain; and when all the electricity of the chain was taken quite away, they would separate, like the former threads.

* Lettere dell' Eletticismo, p. 87.

IF he presented other threads to the former, in their second divergence, they would all avoid one another *.

IN this complete and elegant manner did Signior Beccaria demonstrate, that air actually receives electricity by communication, and loses it by degrees; and that the electricity of the air counteracts that of the body which conveys electricity to it.

SIGNIOR BECCARIA also made a variety of other experiments, which demonstrate other mutual affections of the air and the electric fluid; particularly some that prove their mutual repulsion; and that the electric fluid, in passing through any portion of air, makes a temporary vacuum.

HE brought the ends of two wires within a small distance of one another, in a glass tube, one end of which was closed, and the other immersed in water; and observed, that the water sunk in the tube, every time that a spark passed from the one to the other, the electric fluid having repelled the air †.

HE made the electric explosion a great number of times, in the same air, inclosed in a glass tube, in order to ascertain whether the elasticity of the air was affected by it; but he could not find any alteration. After the operation, he broke the tube under water, but neither did any air make its escape, nor any water force its way into the tube. The expe-

* Lettere dell' Eletticismo, p. 90.

† Eletticismo artificiale e naturale, p. 110.

riment was made with all the precaution, with respect to heat and cold, that the nature of the case required*.

SIGNIOR BECCARIA'S experiments on *water*, showing its imperfection as a conductor, are more surprising than those he made upon air, showing its imperfection in the contrary respect. They prove that water conducts electricity according to its quantity, and that a small quantity of water makes a very great resistance to the passage of the electric fluid.

HE made tubes, full of water, part of the electric circuit, and observed, that when they were very small, they would not transmit a shock, but that the shock increased as wider tubes were used†.

BUT what astonishes us most in Signior Beccaria's experiments with water, is his making the electric spark visible in it, notwithstanding its being a real conductor of electricity. Nothing, however, can prove more clearly how imperfect a conductor it is.

HE inserted wires, so as nearly to meet, in small tubes filled with water; and, discharging shocks through them, the electric spark was visible between their points, as if no water had been in the place. The tubes were generally broken to pieces, and the fragments driven to a considerable distance. This was evidently occasioned by the repulsion of the water, and its incompressibility, it not

* *Elettricismo artificiale e naturale*, p. 81.

† *Ibid.* p. 113.

being able to give way far enough within itself, and the force with which it was repelled being very great*.

THE force with which small quantities of water are thus repelled by the electric fluid, he says, is prodigious. By means of a charge of four hundred square inches, he broke a glass tube two lines thick, when the pieces were driven to the distance of twenty feet. Nay he sometimes broke tubes eight or ten lines thick, and the fragments were driven to greater distances in proportion†.

HE found the effect of the electric spark upon water greater than the effect of a spark of common fire on gunpowder; and says he does not doubt, but that, if a method could be found of managing them equally well, a cannon charged with water would be more dreadful than one charged with gunpowder. He actually charged a glass tube with water, and put a small ball into it, when it was discharged with great force, so as to bury itself in some clay which he placed to receive it‡.

THIS resistance which small quantities of water make to the electric matter, he imagined, was greater than the resistance made to it by air§. And yet he thought it was

* *Elettricismo artificiale e naturale*, p. 114.

† *Lettre dell' elettricismo*, p. 74.

‡ *Ibid.* p. 75, 76. Mr. LULLIN says, he produced much greater effects than these, by making the electric spark visible in oil instead of water. Oil being a much worse conductor, the spark in it would be larger. *Dissertatio Physica*, p. 26.

§ *Elettricismo artificiale, &c.* p. 115.

possible, that, in this case, the electric matter might not act upon the water immediately, but upon the fixed air that was in it. For when the tubes were not broken, he observed that a great number of air bubbles were let loose, through the whole mass of the water, rose to the top, and mixed with the common atmosphere *.

He also imagined that the electric fluid acted upon the fixed air in all bodies, though no experiment could make it sensible †.

On the contrary, he supposed that the action of the electric matter tended to fix elastic air, by exciting a sulphureous matter, which Dr. Hales shows to have that property ‡. But the experiment above-mentioned, of the electric spark taken in a closed tube, doth not favour this supposition.

When a small drop of water was put between the points of two wires, and a large shock passed through them, the water was equally dispersed on the inside of a glass sphere, in which they were all inclosed. In the same manner, he conjectures, that the action of the electric matter promotes the evaporation of water §.

Discharging a shock through a quantity of water, poured on a flat surface, where some parts of the circuit were purposely left almost dry; those parts became quite dry sooner than they would have been, if no shock had passed through them ||.

* *Elettricismo artificiale*, &c. p. 116.

† *Ibid.* p. 83.

‡ *Ibid.*

§ *Ibid.* p. 117.

|| *Ibid.* p. 121.

UPON this principle he accounts for the supposed bursting of the blood vessels in small birds killed by the electric shock*. And when a muscle contracts by the shock, he supposes it is owing to the dilatation of the fluids their fibres contain, as the electric matter passes through them.

So imperfect a conductor of electricity is mere water, that, he thought, a green leaf conducted a shock better than an equal thickness of water†. If this be true, and vegetable fluids conduct electricity better than water, it will confirm a conjecture which Dr. Franklin told me he had drawn from some experiments that he had not properly pursued, viz. that animal fluids conducted electricity better than water. He tried milk many years ago; and Mr. Kinnerfley, and others in America, have since tried blood and urine, and also the sinews of animals newly killed; and they were all found to be exceedingly good conductors, remarkably better than water.

SIGNIOR BECCARIA also found, that even *metal* was not a perfect conductor of electricity, but made some resistance to the passage of the electric fluid. This he ascertained, by measuring the time that it was retarded, in its passing through long and small wires, notwithstanding the experiments which had been made before, that seemed to prove the contrary.

* Eletticismo artificiale, &c. p. 128. † Ibid. p. 135.

HE suspended a wire of five hundred Paris feet, in a large building, and, by means of a pendulum which vibrated half seconds, observed, that light bodies placed under a ball of gilt paper, at one end, did not move, till, at least, one vibration of this pendulum, after he had applied the wire of a charged phial to the other.

TRYING the same with a hempen cord, he could count six, or more vibrations before they would stir; but when he had wetted the cord, they were moved after two or three vibrations*. He does not, however, absolutely say that, the electric fluid must have taken up all this time in its progress, as it might require a certain quantity of the fluid, before it could raise the light bodies. But he did imagine, that it moved with more velocity, in proportion as the bodies into which it passed had more or less of the fluid before†. And he was confirmed in this opinion by several phenomena of the atmosphere, which will be related in their proper place, particularly by seeing, very evidently, the progress of a quantity of electric matter in the air, as it advanced to strike his kite.

To these experiments of Signior Beccaria on the conducting powers of air and water, I shall subjoin another curious set of the same author, showing the manner in which the smoke of rosin and of colophonia is affected by the approach of an electrified

* *Elettricismo artificiale*, &c. p. 51.

† *Ibid.*

body, as they have a very near affinity to this subject.

REPEATING Dr. Franklin's experiments to make electric atmospheres visible with the fume of colophonia, which he preferred, for this purpose, to rosin; he observed several curious circumstances, which had escaped the notice of that ingenious philosopher.

HE heated the colophonia on a coal, which he held in a spoon under an electrified cube of metal; and observed, that when part of the smoke ascended to the cube, another part covered the handle of the spoon, and spread to his hand *.

THE smoke lay higher on the flat parts of the cube than on the edges, and corners.

IF a spark was taken from the conductor, the smoke was thrown into an agitation, but presently resumed its former situation.

THE cube with its atmosphere gave larger, and longer sparks, than a cube not surrounded with one.

A LARGER spark might be taken from it by the spoon, than by any other body.

HAVING insulated the spoon, he observed, that hardly any of the smoke went to the cube; and that what happened to go near it was not affected by it, any more than it would have been by any other body. He put his finger to the spoon, and the former phenomena returned. Taking it off again,

* Eletticismo artificiale, p. 72.

the smoke that had settled on the cube soon dispersed *.

UNDER this head of the electricity of various substances, it will not be improper to mention an experiment made by Mr. Henry Eeles of Lismore in Ireland, which, he thought, proved that steam, and exhalations of all kinds, are electrical. The paper containing this account was read at the Royal Society, April the 23d, 1755.

HE electrified a piece of down, suspended on the middle of a long silk string, and made steam and smoke of several kinds pass under it, and through it; and observed, that its electricity was not in the least diminished, as he thought it would have been, if the vapour had been non-electric, and consequently had taken away with it part of the electric matter with which the down was loaded. He observed that the effect was the same, whether the down was electrified with glass or wax, which he thought was not easy to be accounted for †.

To this experiment Dr. Darwin of Litchfield, in a letter addressed to the Royal Society, and read May the 5th, 1757, answers; that many electrified bodies, and particularly all light, dry, animal, and vegetable substances, will not easily part with their electricity, though they be touched, for a considerable time, with conductors. He touched

* *Elettricismo artificiale*, p. 73, 74.

† *Phil. Trans.* Vol. xlix. pt. i. p. 153.

a feather,

a feather, electrified like that of Mr. Eeles, nine times with his finger, and still found it electrified. A cork ball was touched seven times in ten seconds before it was exhausted*.

MR. KINNERSLEY of Philadelphia, in a letter dated March 1761, informs his friend and correspondent Dr. Franklin, then in England, that he could not electrify any thing by means of *steam* from electrified boiling water; from whence he concluded, that, contrary to what had been before supposed by himself and his friend, steam was so far from rising electrified, that it left its share of common electricity behind†.

To try the effects of electricity upon air, Mr. Kinnersley contrived an excellent instrument, which he calls *an electrical air thermometer*. It consisted of a glass tube, about eleven inches long, and one inch in diameter, made air tight, closed with brass caps at each end, and a small tube, open at both ends, let down through the upper plate, into some water at the bottom of the wider tube. Within this vessel he placed two wires, one descending from the brass cap at the upper end, and the other ascending from the brass cap at the lower end; through which he could discharge a jar, or transmit a spark, &c. and at the same time see the expansion of the air in the vessel, by the rise of the water, in the small tube. With this instrument he made the following experiments, related in a let-

* Phil. Trans. Vol. 1. pt. i. p. 252.

† Ibid. Vol. liii. pt. i. p. 84.

ter to Dr. Franklin, dated March the 12th, 1761.

HE set the thermometer on an electric stand, with the chain fixed to the prime conductor, and kept it well electrified a considerable time; but this produced no considerable effect: from whence he inferred, that the electric fire, when in a state of rest, had no more heat than the air, and other matter wherein it resides.

WHEN the two wires within the vessel were in contact, a large charge of electricity, from above thirty square feet of coated glass, produced no rarefaction in the air; which showed, that the wires were not heated by the fire passing through them.

WHEN the wires were about two inches asunder, the charge of a three pint bottle, darting from one to the other, rarefied the air very evidently; which shewed, that the electric fire produced heat in itself, as Mr. Kinnerley says, as well as in the air, by its rapid motion.

THE charge of a jar which contained about five gallons and a half, darting from wire to wire, would cause a prodigious expansion in the air; and the charge of his battery of thirty square feet of coated glass would raise the water in the small tube quite to the top. Upon the coalescing of the air, the column of water, by its gravity, instantly subsided, till it was in equilibrio with the rarified air. It then gradually descended, as the air cooled, and settled where it stood before. By carefully

fully observing at what height the descending water first stopped, the degree of rarefaction, he says, might be discovered, which, in great explosions, was very considerable.

It is obvious to remark, that the first sudden rise of the water of Mr. Kinnerfsley's thermometer, upon an explosion being made in the vessel which contained it, is not to be ascribed to the rarefaction of the air by heat, but to the quantity of air actually displaced by the electrical flash. It is only when that first sudden rise is subsided, as Mr. Kinnerfsley himself observes, that the degree of its rarefaction by the heat can be estimated, viz. by the height at which the water then stands above the common level.

DR. FRANKLIN had said, that *ice* failed to conduct a shock of electricity, and Mr. Bergman, in a letter to Mr. Wilson, read at the Royal Society November the 20th, 1760, shows (what Signior Beccaria had done before) that a small quantity of water failed as much as the ice had done with Dr. Franklin, who seems to have made use of an icicle which, Mr. Bergman thought, was not large enough for the purpose. From hence he suspected, that large quantities of ice would transmit a shock of electricity as perfectly as a large quantity of water*.

HOWEVER, he seems, afterwards, to have changed his sentiments with respect to ice: for, in a subsequent paper, read at the Royal

* Phil. Transf. Vol. li. pt. ii. p. 908.

Society March the 18th, 1762, when he had remarked that snow would not conduct the electric shock, he says, he believes, if he could procure plates of ice of a proper thickness, he could charge them in the same manner as glass *.

JOHANNES FRANCISCUS CIGNA was so fully persuaded of the non-conducting power of ice, that he made use of it in an experiment, designed to ascertain whether electric substances did, according to Dr. Franklin's hypothesis, contain more of the electric matter than other bodies. He inclosed a quantity of ice in a glass vessel, and when he thought he had converted it from an electric to a non-electric by melting; he tried whether it was electrified; but, though he found no appearance of its having acquired any more of the fluid than it ought to have in its new state, he does not seem to have given up his opinion †.

IN the last part of this work the reader will find some experiments, which, it is imagined, will ascertain the class of bodies in which ice ought to be ranked, by proving its conducting power to be, at least, nearly equal to that of water.

* Phil. Transf. Vol lli. pt. ii. p. 485.

† Memoirs of the Academy at Turin, for the year 1765, p. 47.

S E C T I O N III.

MR. CANTON'S EXPERIMENTS AND DISCOVERIES RELATING TO THE SURFACES OF ELECTRIC BODIES, AND OTHERS MADE IN PURSUANCE OF THEM, OR RELATING TO THE SAME SUBJECT; ALL TENDING TO ASCERTAIN THE DISTINCTION BETWEEN THE TWO ELECTRICITIES.

TILL this last period of the history, the same electricity had always been produced by the same electric. The friction of glass had always produced a positive, and the friction of sealing-wax, &c. had always produced a negative electricity. These were thought to be essential, and unchangeable properties of those substances; and hence the one was by many called the vitreous, and the other the resinous electricity; and to electrify negatively, that is produce a resinous electricity, by means of glass; or to electrify positively, that is, produce a vitreous electricity, by means of sealing-wax, &c. would have been thought as great a paradox, as to electrify at all by the friction of brass or iron. For though it was not known why the electric matter should flow from the rubber to the excited glass, or to the rubber from excited sealing-wax, the fact had been inviolable; and nothing is even mentioned to have happened, in the course of any experiments,

ments, that could lead a person to suspect the possibility of the contrary.

WHAT then must have been the surprise of electricians, to find that these different powers of glass and sulphur were so far from being invariable, that they were even interchangeable; and that the same glass tube could be made to assume the powers of both! And what must have been their satisfaction to find the circumstance on which the convertibility of those opposite powers depended, completely ascertained. This surprise and pleasure was given them by Mr. Canton, who showed that it depended only on the rubber, and the surface of the glass, whether it electrified positively or negatively.

IN what manner, by what train of thought, or by what accident he was led to this discovery, this excellent philosopher has not been pleased to inform us; but it is certainly a discovery which, in an eminent manner, distinguishes this period of my history. It throws great light upon the doctrine of positive and negative electricity, and led the way to other discoveries which throw still more light upon it.

THIS subject of the two electricities seems to have engaged the attention of electricians in a more particular manner, in the whole course of this period, and ever since the discovery of Dr. Franklin, that the electricity of the two surfaces of charged glass are always contrary to one another. Accordingly, the reader will find several sections in this period of

of the history relating to it; but he will find that though much has been done, much yet remains to be done; and that we are still far from thoroughly understanding the nature of the two electricities, with their dependence upon and relation to one another.

PREVIOUS to the communication of the discovery itself, Mr. Canton observes, that sealing-wax might have positive electricity superinduced upon it. He excited a stick of sealing-wax about two feet and a half in length, and an inch in diameter; and, holding it by the middle, he drew an excited glass tube several times over one part of it, without touching the other. The consequence was, that that half which had been exposed to the action of the excited glass was positive, and the other half negative: for the former half destroyed the repelling power of balls electrified by glass, while the other half increased it*.

THE experiments, which prove that the appearances of positive and negative electricity depend upon the surface of the electrics, and that of the rubber, were made in the latter end of December 1753.

HAVING rubbed a glass tube with a piece of thin sheet lead, and flour of emery mixed with water, till its transparency was intirely destroyed, he excited it (when it was made perfectly clean and dry) with new flannel, and found it act in all respects like excited

* Phil. Trans. Vol. xlviii. pt. i. p. 356.

fulphur or sealing-wax. The electric fire seemed to issue from the knuckle, or end of the finger, and to spread itself on the surface of the tube, in a very beautiful manner.

IF this rough or unpolished tube was excited by a piece of dry oiled silk, especially when rubbed over with a little chalk or whiting, it would act like a glass tube with its natural polish. In this case the electric fire appeared only at the knuckle, or the end of the finger, where it seemed to be very much condensed, before it entered.

BUT if the rough tube was greased all over with tallow from a candle, and as much as possible of it wiped off with a napkin, then the oiled silk would receive a kind of polish by rubbing it; and, after a few strokes, would make the tube act in the same manner as when excited at first by flannel.

THE oiled silk, when covered with chalk or whiting, would make the greased rough tube act again like a polished one; but if the friction was continued till the rubber became smooth, the electric power would be changed to that of sulphur, sealing-wax, &c.

THUS, says he, may the positive and negative powers of electricity be produced at pleasure, by altering the surfaces of the tube and rubber, according as the one or the other is most affected by the friction between them. For if the polish be taken off one half of a tube, the different powers may be excited with the same rubber at a single stroke; and, he adds, the rubber is found to move much

easier over the rough, than over the polished part of it.

THAT polished glass electrified positively, and rough glass rubbed with flannel negatively, seemed plain from the appearance of the light between the knuckle, or end of the finger, and the respective tubes. But this, Mr. Canton thought, was farther confirmed by observing, that a polished glass tube, when excited by smooth oiled silk, if the hand was kept three inches, at least, from the top of the rubber, would, at every stroke, appear to throw out a great number of diverging pencils of electric fire; but that none were ever seen to accompany the rubbing of sulphur, sealing-wax, &c. nor was he ever able to make any sensible alteration in the air of a room merely by the friction of those bodies; whereas the glass tube, when excited so as to emit pencils, would, in a few minutes, electrify the air, to such a degree, that, after the tube was carried away, a pair of balls, about the size of the smallest peas, turned out of cork, or the pith of elder, and hung to a wire by linen threads of six inches long, would repel each other to the distance of an inch and an half, when held at arm's length in the middle of the room *.

AFTER these experiments of Mr. Canton, Mr. Wilson made several, which throw a little more light upon this curious subject; but it is difficult to draw any general conclu-

* Phil. Transf. Vol. xlviii. pt. ii. p. 782.

sion from them, and his own is not sufficiently determinate. It is, that two electrics being rubbed together, the body whose substance is hardest, and electric power strongest, will always be *plus*, and the softest and weakest *minus* *. Rubbing the tourmalin and amber together, he produced a *plus* electricity on both sides of the stone, and a *minus* on the amber; but rubbing the tourmalin and diamond together, both sides of the tourmalin were electrified *minus*, and the diamond *plus*.

THESE experiments, which, he thought, proved this proposition, encouraged him to try what would be the effect of rubbing or forcing air against different electrics, and the effects were very considerable. In these experiments he only made use of a common pair of bellows, and his first experiment was upon the tourmalin. This substance he brought near the end of the pipe, and found, that after it had received about twenty blasts, it was electrified *plus* on both sides. Air, therefore, seemed to be less electric than the tourmalin.

INTO the place of the tourmalin, he brought a pane of glass, and blew against it the same number of times as in the former experiment; and when he had examined both sides, he found that they were electrified *plus* also, but less than the tourmalin.

AMBER, treated in the same manner, was electrified less than the glass.

* Phil. Transf. Vol. li. pt. i. p. 331.

HE next had recourse to a smith's bellows. The difference which these occasioned was only a much stronger electricity in the tourmalin. Amber was still weaker than the glass; and the glass weaker than the tourmalin.

HAVING in view the medium (which, I have observed, he laid great stress upon, as constituting the difference between electrics and non-electrics) he considered that heat would rarify it on the surfaces of the particles of air; by which means, air, having its resistance lessened, would more readily part with the electric fluid, and, of consequence, electrify more powerfully.

THE pipe of the bellows being made red hot, he blew against the tourmalin, twelve times only, which was eight times less than in the former experiment with cold air. In this experiment the tourmalin was electrified *plus* on both sides, but to a considerable degree more than was done in the former experiments. The hot air had the same effect upon glass, but electrified it less than the tourmalin; and amber, though, like the other bodies, it received an increase of power by the same treatment, was electrified the least of all.

FROM the air electrifying more powerfully when it was hot than when it was cold, and the tourmalin being electrified more than glass, and glass more than amber, as appeared by the last experiments, we seem, says Mr. Wilson, to have obtained a proof, that
the

the whole atmosphere is constantly promoting a flow of the electric fluid, by the alternate changes of heat and cold; and farther, that air is not only less electric than the tourmalin, but less than glass, or even amber*.

IN another paper, read at the Royal Society November the 13th, 1760, Mr. Wilson recites some curious experiments, which, he says, shew that a *plus* electricity may be produced by means of a *minus* electricity.

HAVING electrified the inside of a large Leyden bottle *plus*, by means of a conducting wire from an excited glass globe; he set it on a stand of prepared wood, and took away the conducting wire, after which the mouth of the bottle was closed with a stopple of glass. Then the pointed end of an ivory conductor was brought opposite to the middle of the bottle, and about two inches from it. Upon doing this, the balls were electrified *minus*; and the more so as the ivory was moved nearer the bottle, in an horizontal direction.

BUT, on removing the ivory to a greater distance, the *minus* electricity decreased; and, at a certain distance, there was not any sign of it remaining; but when the distance was increased to about eighteen inches from the bottle, a *plus* electricity appeared, which continued even after the ivory was removed entirely away†.

WITH a cylinder of baked wood he electrified the balls hanging to the ivory *minus*, at

* Phil. Transf. Vol. li. pt. i. p. 332, &c.

† Ibid. pt. ii. p. 899, &c.

the distance of four feet or more, by holding the cylinder over the middle of the ivory, and continuing it there; and, on moving it nearer, they were more strongly electrified *minus*; but the same cylinder, on removing it back again to the distance of two or three feet, or more, electrified the balls *plus*.

WHEN another conductor of metal, without edges or points, was used, instead of the ivory, and without any thing hanging from it, the same cylinder held over the metal (as was done in the last experiment over the ivory, at the distance of two feet) produced a *plus* electricity; and this was rendered weaker as the cylinder was moved nearer; but by lessening the distance to about one foot, the *minus* electricity took place. In these cases Mr. Wilson thought, that the *plus* appearance arose from the earth, air, or other neighbouring bodies.

WHEN the preceding experiments were first made, he was a little embarrassed, by the uncertain appearances of a *plus* electricity at one time, and a *minus* at another, in the same experiment; but, by repeated trials and observations, he found, that a *plus* or *minus* electricity may be produced at pleasure, by carefully attending to the three following circumstances; viz. the form of the bodies, their sudden or gradual removal, and the degrees of electrifying.

MR. WILSON, after this, proceeds to mention some other circumstances of a very nice nature, where, the slightest and almost imperceptible

perceptible differences in the position or in the course of the friction of two bodies produce, in either of them, the *plus* electricity at one time, and the *minus* at another. Such, says he, are the effects of this subtle and active fluid, when the experiments are carefully made; and therefore they require the most scrupulous attention to trace out the causes which occasion them.

SEALING-WAX and silver were the bodies used in the two first experiments, but many other substances seemed to perform as well. The sealing-wax was clean, and undisturbed by any friction whatever, but that of the air surrounding it, and had been so for some hours. The silver was fixed to a piece of prepared wood, which was also preserved from friction for the same length of time. Then, taking one of those substances in each hand, the silver being at the end of the wood the farthest from the hand, he laid the smoothest part of the silver upon the sealing-wax, and moved it along the surface gently, once only, and with a very slight pressure, after which the silver was electrified *plus*, and the wax *minus*.

ON repeating the experiment with equal care, and in the same manner, except that the smooth side of the silver was a little inclined, so that the edge of it pressed against the wax; the silver, after moving it as before, was electrified *minus*, and the wax *plus*, contrary to what was observed in the last experiment.

THESE

THESE opposite effects, occasioned by the different applications of the *flatted part* or *edge* of the silver, seemed to arise from an alteration made in the surface of the wax, by destroying the polish in one case, and not in the other; and in this respect resembled the polished and rough glass mentioned before.

UPON making use of prepared wood instead of wax, and employing different degrees of pressure in the friction, with the same edge of the silver, he produced the like appearances; the least pressure causing a *plus*, and the greatest pressure a *minus* appearance in the silver.

A FLAT piece of steel well polished, and the edges rounded off, afforded the same appearances, by only applying the flat surface to the wood, but it required more pressing to produce the *minus* effect in this case than it did in the former, where the edge was concerned.

WHETHER the reason offered above for explaining these last curious appearances be true or not, Mr. Wilson did not venture to affirm, for want of farther experiments; but thus much he thought might be safely advanced, that we have learned to produce at pleasure a *plus* or *minus* electricity from the same bodies, by attending to the manner of their application and friction *.

MR. BERGMAN, in a letter to Mr. Wilson, read at the Royal Society February the 23d,

* Phil. Transf. Vol. li. pt. ii. p. 899, &c.

1764, gives an account of some curious experiments of his, which, in conjunction with those of Mr. Canton above mentioned, concerning surfaces, may throw considerable light upon the doctrine of positive and negative electricity.

THE experiments were made with two ribbons of silk, one of which was extended in a frame, while Mr. Bergman held the other in his hand. He observed, that if the two ribbons were the same with respect to texture, colour, superficies, and in every thing else, as far as could be judged; and if he drew the whole length of the ribbon which he held in his hand over one part of that which was extended in the frame, that in his hand contracted the positive electricity, and that in the frame a negative. If he drew one part of that which he held in his hand over the whole length of the other, the effects were reversed.

IF the ribbon in his hand was of a different colour from that in the frame (provided it was not black) the event was the same.

IF the ribbon in his hand was black, it was always negative, which ever way it was rubbed, except that in the frame was black too; for then, if the whole length of it was rubbed, it was electrified positively.

IN endeavouring to account for these effects, he observes, that the ribbon which was most rubbed was made *smoother*, and *warmer* than the other; and was of opinion, that though smoothness did dispose bodies to be excited positively,

positively, yet that other circumstances were also to be taken into consideration; having found that when he held in his hand a ribbon, which, by much friction, was made very smooth, and drew it over one part of another ribbon, which was rough, and had never been used before, that the rough ribbon was, nevertheless, positive. From this experiment he concluded, that this effect was, in some measure, owing to the colour; and, in pursuing this thought farther, he was led to the following experiments.

IF the ribbon in his hand was well warmed, though it was drawn over one part of that in the frame, it became electrified negatively, and that in the frame positively. He made these experiments with the same success upon ribbons of silk of various colours, blue, green, red, white, &c.

IF the ribbon in the frame was black, it never contracted a positive electricity, though that in his hand had been much heated, except this were black too. From these experiments, he thought he might safely conclude, that heat did dispose some substances, at least, to a negative state; and he thought that the want of attention to this circumstance might have occasioned mistakes in the event of some experiments, especially those concerning island crystal.

FROM the whole he concludes, that there is a certain fixed order with respect to negative and positive electricity, in which all bodies may be placed, while other circumstances

stances remain the same. Let A, B, C, D, E, be certain substances, each of which, when rubbed with one which is antecedent to it, is negative, but with a subsequent positive. In this case, the less distance there is between the bodies that are rubbed, the weaker, *cæt. par.* will be the electricity; wherefore it will be stronger between A and E, than it will be between A and B. Heat, he says, disposes bodies to a negative electricity, but if the distance above-mentioned be considerable, it may not be able to *overcome*, though it may *weaken* that electricity, as is evident from the ribbon of black silk. When a glass globe grows warm in whirling, we are sensible that its electric power is diminished. Is it not owing, says he, to this circumstance, that by heat it is more disposed to negative electricity, by which means the distance above-mentioned between the glass and the rubber is lessened *?

UPON the subject of this section, I must introduce to the acquaintance of my reader two eminent electricians whose discoveries will give him the greatest satisfaction; I mean Mr. WILCKE, and Mr. ÆPINUS, the former of Rostock in Lower Saxony, and the latter of Peterburgh: a circumstance which gives me an occasion of congratulating all the lovers of the sciences, and particularly of electricity, on the extensive spread of their studies. What joy would it have given Mr.

* Phil. Transf. Vol. liv. p. 86.

Hauksbee, or Mr. Grey, to have foreseen that two such admirable treatises on the subject of electricity, as those of the persons above-mentioned, would come from countries so remote from the place of its rise !

MR. WILCKE relates many curious experiments concerning the generation of what he calls *spontaneous electricity*, produced by the liquefaction of electric substances, which, compared with those of Mr. Canton, throw great light upon the doctrine of positive and negative electricity.

HE melted sulphur in an earthen vessel, which he placed upon conductors ; then, letting them cool, he took out the sulphur, and found it strongly electrical ; but it was not so when it had stood to cool upon electric substances.

HE melted sulphur in glass vessels, whereby they both acquired a strong electricity in the circumstances above mentioned, whether they were placed upon electrics or not ; but a stronger in the former case than in the latter ; and they acquired a stronger virtue still, if the glass vessel into which they were poured was coated with metal. In these cases, the glass was always positive, and the sulphur negative. It was particularly remarkable, that the sulphur acquired no electricity till it began to cool and contract, and was the strongest when in the state of greatest contraction ; whereas the electricity of the glass was, at the same time, the weakest ; and was the strongest of all when the sulphur was shaken

T

out,

out, before it began to contract, and acquired any negative electricity.

PURSUING these experiments, he found that melted sealing-wax, poured into glass, acquired a negative electricity, but poured into sulphur it acquired a positive electricity, and left the sulphur negative. Sulphur poured into baked wood became negative. Sealing-wax also poured into wood was negative, and the wood consequently positive; but sulphur poured into sulphur, or into rough glass, acquired no electricity at all*.

EXPERIMENTS similar to these were also made by Mr. *Æpinus*. He poured melted sulphur into metal cups, and observed that when the sulphur was cold, the cup and the sulphur together showed no signs of electricity, but showed very strong signs of it the moment they were separated. The electricity always disappeared when the sulphur was replaced in the cup, and revived upon being taken out again. The cup had acquired a negative, and the sulphur a positive electricity; but if the electricity of either of them had been taken off while they were separate, they would both, when united, show signs of that electricity which had not been taken off. This electricity, he observes, was only on the surface of the sulphur†.

MR. WILCKE has, likewise, recited several curious experiments, which he made on the friction of various substances, which like-

* Wilcke, p. 44, &c.

† *Æpini Tentamen*, p. 66. 70.

TWO ELECTRICITIES. 275

wife throw considerable light on the same subject.

SULPHUR and glass rubbed together produced a strong electricity, positive in the glass, and negative in the sulphur.

SULPHUR and sealing-wax being rubbed together, the wax became positive, and the sulphur negative.

WOOD rubbed with cloth was always negative.

WOOD rubbed against smooth glass became negative, but against rough glass positive.

SULPHUR rubbed against metals was always positive, and this was the only case in which it was so; but being rubbed against lead it became negative, and the metal positive; lead appearing, thereby, to be not so good a conductor as the other metals.

AFTER these experiments, Mr. Wilcke gives the following catalogue of the principal substances with which electrical experiments are made, in the order in which they are disposed to acquire positive or negative electricity; any of the substances becoming positively electrical when rubbed with any that follow it in the list, and negative when rubbed with any that precede it.

Smooth glass.	White wax.
Woollen cloth.	Rough glass.
Quills.	Lead.
Wood.	Sulphur.
Paper.	Other metals *.
Sealing-wax.	

* Wilcke, p. 54, &c.

IN all experiments made to determine the order of these substances, Mr. Wilcke says, that great care is necessary, to distinguish original electricity from that which is communicated, or the consequence of friction*.

MR. WILCKE says that smooth glass is in all cases positive, and thence infers that it attracts the electric fluid the most of all known substances; but Mr. Canton tells me he has found, that the smoothest glass will acquire a negative electricity by being drawn over the back of a cat.

OF the same nature with these experiments of Mr. Wilcke are the following of *Æpinus*. He pressed close together two pieces of looking-glass, each containing some square inches; and observed, that when they were separated, and not suffered to communicate with any conductor, they each acquired a strong electricity, the one positive, and the other negative. When they were put together again, the electricity of both disappeared, but not if either of them had been deprived of their electricity when they were asunder; for in that case, the two when united, had the electricity of the other. The same experiment, he says, may be made with glass and sulphur, or with any other electrics, or with any electric and a piece of metal†.

* Wilcke, p. 69.

† *Æpini Tentamen*, p. 65.

SECTION IV.

MR. DELAVAL'S EXPERIMENTS RELATING
TO THE TWO ELECTRICITIES, AND HIS
CONTROVERSY WITH MR. CANTON UPON
THAT SUBJECT.

MR. CANTON, in the course of experiments related in the preceding section, clearly proved, that the production of either of the two electricities depends intirely upon the surface of the excited electric with respect to the rubber, and showed, that the very same glass tube would produce either of them at pleasure; yet, notwithstanding this demonstration, Mr. Delaval, several years afterwards, proposed another theory of the two electricities, which seems to be more ingenious than solid; as it goes upon the old supposition of the different powers depending intirely upon the different substances themselves. The account of this theory was read at the Royal Society, March the 22d, 1759. It necessarily occasioned some controversy with Mr. Canton, in the course of which some new experiments were made, and some new facts discovered; on account of which I shall, with the utmost impartiality, report all that was advanced on both sides.

MR. DELAVAL observed, that there are two of the pure chymical principles of bodies, viz. *earth* and *sulphur*, which are each pos-

ferred of a different kind of electricity; one of which we might call a *plus* electricity, the other a *minus*; and thought that it might be expected, that, in a body compounded of both, the opposite powers of those ingredients would counterbalance, and destroy the effect of each other; and therefore, that bodies in which the negative and positive powers were equal, would be neutral, or non-electrics. Such a substance he took metal to be, consisting of calx and sulphur; metals not being calcinable without a degree of heat sufficient to dissipate all their sulphur; as is evident from their not being reducible again to their metallic form, without the admixture of some unctuous matter. The same dissipation of sulphur, he says, must take place in animal and vegetable substances, before they become white ashes. Transparent stones he considered as little more than pure earth, free from the least mixture of oil; judging of others by the chymical resolution of crystal.

To confirm this theory, Mr. Delaval made experiments with dry powders of calcined metals, viz. cerufs, lead ashes, minium, calx of antimony, &c. inclosing them in long glass tubes, and endeavouring to transmit the electric virtue through them, and always finding it impossible. Animal and vegetable substances, when reduced to ashes, were alike impermeable to electricity, as also the rust of metals.

HE was first led to these experiments, and to this hypothesis, by finding that dry mould would not conduct electricity. This he also tried with dry Portland stone, some of which he had cut into plates nearly as thin as window glass. These he heated to a proper degree, and coated them on both sides with metal, in order to make the Leyden experiment. When the stone was hot enough to singe paper, it conducted as perfectly as when cold; but on cooling a little, it began not to conduct, and afforded small shocks; which gradually increased in strength for about ten minutes, at which time it was about its most perfect state, and remained so near a quarter of an hour. After that time, the shocks gradually decreased, as the stone grew cooler; till, at last, they ceased, and the stone returned to its conducting state again, but this state appeared before the stone was quite cold.

EXPERIMENTS of this kind succeeded with all bodies abounding with calx, or earth, as stones, earth, dry clay, wood when rotten, or burned in the fire till the surface becomes black. Among other substances he tried a common tobacco-pipe, part of which, near the middle, he heated to a proper degree, and then applied one end of it to an electrical bar, while the other was held in the hand; and he observed, that the electric fluid passed no farther along the pipe than to the heated part*.

* Phil. Transf. Vol. li. pt. i. p. 83.

FROM these experiments Mr. Delaval inferred, that stones and other earthy substances were convertible, by several methods, and particularly by different degrees of heat, from non-electrics to electrics. But finding, afterwards, that it was the opinion of some persons (Mr. Canton was the person chiefly hinted at) that this change did not immediately, but only consequentially depend on heat, by evaporating the moisture, which would return again when the substance cooled; he observes, in a paper read at the Royal Society, December the 17th, 1761, that the tobacco-pipe lost its electricity before it was cold, and therefore before it could have imbibed moisture sufficient to destroy its electricity; and besides, that the substance employed in the experiment was not of that kind of bodies which is apt suddenly to draw moisture from the air.

To account for Mr. Delaval's experiments, Mr. Canton supposes, in a paper read to the Royal Society, February 4th, 1762, that stone, tobacco-pipe, wood, &c. will conduct when cold by the moisture they contain in that state; that when their moisture is evaporated by heat they become non-conductors; and that when they are made very hot, the hot air at, or near their surfaces will conduct, and the bodies will appear to be conductors again. Hot air, he says, may easily be proved to be a conductor of electricity, by bringing a red hot-iron poker, but for a moment, within three or four inches of a small electrified

fied body ; when it would be perceived, that its electric power would be almost, if not entirely destroyed ; and by bringing excited amber within an inch of the flame of a candle, when it would lose its electricity before it had acquired any sensible degree of heat.

To confirm this, he mentions his having observed, that the tourmalin, Brasil topaz, and Brasil emerald, would give much stronger signs of electricity when cooling, after they had been held about a minute within two inches of an almost surrounding fire, where the air is a conductor, then they ever will after heating them in boiling water. He adds, that if both sides of those stones be equally heated, in a less degree than will make the surrounding air a conductor, the electricity of each side, whether *plus* or *minus*, would continue so all the time the stone was both heating and cooling, but would increase while it was heating, and decrease while it was cooling ; whereas, if the heat was sufficient to make the surrounding air conduct the electric fluid from the positive side of the stone to the negative side of it, while it was heating, the electricity of each side would increase while the stone was cooling, and be contrary to what it was while the stone was heating.

As to the tobacco-pipe, Mr. Canton says, that it not only attracts the moisture of the air, but absorbs it. Hence a tobacco-pipe, after it begins to cool, will become a conductor again sooner than wood. And that it imbibes moisture faster than wood, he says, is evident,

evident, because when wetted, it will not continue wet so long as wood, imbibing the moisture presently.

THAT tobacco-pipe does not become a conductor by a particular degree of heat, without evaporating its moisture, is evident, he says, from the following experiments.

IF three or four inches of one end of a tobacco-pipe, of more than a foot in length, be made red hot, without sensibly heating the other end, this pipe will prove a ready conductor, through the hot air surrounding one part of it, and the moisture contained in the other; although some part of it must have the degree of heat of a non-conductor. But if the whole pipe be made red hot, and suffered to cool till it has only superficial moisture enough to make it a good conductor, and then three or four inches of one end be again made hot, it will become a non-conductor.

IF a nail be placed at, or near each end of a longish solid piece of any of the absorbent bodies above-mentioned, so as the point of each nail may be about half the thickness of the body within its surface; this body, by heat, may be made a non-conductor externally or superficially, while it remains a good conductor internally. For the electric fluid will readily pass from one nail to the other, through the middle of the body, when it will not pass on its surface, and even when the internal parts of the body are in an equal degree of heat with the external, as they must soon be after it begins to cool. But if the
same

same body be exposed, for a short time, to a greater degree of heat than before, or if it be kept longer in the same heat, it will become a non-conductor intirely *.

Mr. DELAVAL, in confirmation of particular bodies requiring particular degrees of heat to render them electric or non-electric, independent of moisture, mentions a substance which, he says, is affected by heat in an opposite manner to the former instances; since the degrees of heat, necessary to render the other substances electric, makes this non-electric.

THE substance was *island crystal* (which is well known for its singular property of a double refraction) on a piece of which he made the following observations. 1. After this piece of crystal had been rubbed, when the heat of the air was moderate, it showed signs of electricity, though not very strong ones. 2. If the heat was increased, so as to be a little greater than that of the hand, it destroyed its electric power intirely. 3. By cooling the stone again, the electric power was restored.

HE immersed this piece of crystal into a vessel filled with quicksilver, and surrounded with ice, where it remained near two hours, when the weather was very cold; and observed, that, upon taking it out with a pair of tongs (that it might not be altered by the

heat of his hands) and rubbing it again, it was more strongly electric than he had at any other time experienced; but that, on placing it a few minutes upon the hearth, at some distance from the fire, its electric property was again destroyed, for that rubbing would not occasion any signs of it.

THUS, says he, we see two different kinds of fixed bodies, the one of which acquires an electric property with the same heat with which another loses it; while a third set of substances, as glass, &c. retain their electricity through both the degrees of heat necessary to the other two.

SOME pieces of island crystal, which he had procured from different places, had not the property of losing their electricity by a moderate heat. He had, in particular, a piece of that crystal, one part of which, when greatly heated, became non-electric, while the other part, with the same heat, or even with a much greater one, remained perfectly electric.

HE found several other earthy substances, whose electricity was destroyed by different degrees of heat.

FROM considering that the degree of heat, at which the island crystal first mentioned was in its most perfect electric state, was less than the usual heat of the air, and that a small increase of that heat rendered it non-electric; he did not think it improbable, that many substances, which are not known to be electric,

tric, might prove so, if exposed to a greater degree of cold than they have been hitherto examined in*.

To these observations Mr. Canton replies, that having formerly observed that the friction between mercury and glass in vacuo would not only produce the light of electricity, as in the luminous barometer, or within an evacuated glass ball, but would also electrify the glass on the outside, he immersed a piece of dry glass in a basin of mercury; and found, that by taking it out, the mercury was electrified *minus*, and the glass *plus*, to a considerable degree. He also found, that amber, sealing-wax, and island crystal, when taken out of mercury, were all electrified positively. How then, says he, does it appear, that the electricity which was observed in rubbing the last mentioned substance, after it was taken out of mercury surrounded by ice, was owing to cold, and not to the friction between it and the mercury in taking it out. Island crystal when warm is a non-conductor, and all non-conductors may be excited with proper rubbers†.

MR. BERGMAN of Upsal, in a letter to Mr. Wilson, read at the Royal Society, April the 14th, 1761, says, that he had tried the experiments of Mr. Delaval with island crystal, but that the event had always been contrary to

* Phil. Transf. Vol. lii. pt. i. p. 354, &c.

† Ibid. pt. ii. p. 461.

what Mr. Delaval had reported. Trying different pieces of crystal, he found one which instead of having its virtue increased by cooling, was sensibly increased by heating. Afterwards trying all the rest which he had by him, whether Swedish crystal, or island, he found the effect to be the same. From this he inferred, that the crystals which he had were of a quite different kind from that of Mr. Delaval*.

S E C T I O N V.

MR. CANTON'S EXPERIMENTS AND DISCOVERIES RELATING TO BODIES IMMERGED IN ELECTRIC ATMOSPHERES, WITH THE DISCOVERIES OF OTHERS, MADE BY PURSUING THEM.

IN this section I shall present my reader with the finest series of experiments that the whole history of electricity can exhibit, and in which we shall see displayed the genius and address of four of the most eminent electricians in this whole period; viz. Mr. Canton and Dr. Franklin, Englishmen; and Messrs. Wilcke and Æpinus, foreigners. Mr. Canton had the honour to take the lead, and he made all the essential experiments. Doctor Franklin professedly pursued them; and though *all his strength he put not forth on*

* Phil. Transf. Vol. liii. pt. i. p. 98.

this occasion, he diversified the experiments, and made some improvement in the method of accounting for them. But Messrs. Wilcke and Æpinus in conjunction carried the experiments vastly farther, and completed the discovery; which is, certainly, one of the greatest that has been made since the time of Dr. Franklin. I say the time of Dr. Franklin, though he himself be one of the persons concerned; for by the *time of Dr. Franklin* will always be understood the time in which he made his capital discoveries in America. This will always be a distinguished epocha in the history of electricity, from which all his own future discoveries will be dated.

THE original experiments in this section, when Mr. Canton first published them, in his usual concise, though perspicuous manner, without any preamble, to inform us how he was led to them, exhibit such a variety of attractions and repulsions of electrified bodies in different circumstances, as looked like the power of magic; and were they conducted with a little art, I do not know any electrical experiments (made without light, or noise) more proper for a deception of this kind. But when they are attentively considered, they demonstrate a remarkable property of all electrified bodies, which has often been referred to in the course of this history, but which had not been attended to before; nor indeed do I apprehend that it was fully understood, till it was explained in all its extent by Mr. Wilcke and Æpinus. It is, that

that the electric fluid, when there is a redundancy of it in any body, repels the electric fluid, in any other body, when they are brought within the sphere of each other's influence, and drives it into the remote parts of the body; or quite out of it, if there be any outlet for that purpose. In other words, bodies immersed in electric atmospheres always become possessed of the electricity, contrary to that of the body, in whose atmosphere they are immersed. This principle pursued led them to the method of charging a plate of air, like a plate of glass, and to make the most perfect imitation of the phenomena of thunder and lightning.

THE paper, containing an account of Mr. Canton's experiments, was read at the Royal Society, December the 6th, 1753.

MR. CANTON suspended cork balls, one pair by linen threads, and another pair by silk; then holding the excited tube at a considerable distance from the balls with the linen thread, they separated; and, upon drawing it away, they immediately came together: but he was obliged to bring the excited tube much nearer to the balls hanging by silk threads, before they would separate; though when the tube was withdrawn, they continued separate for some time.

As the balls in the former of these experiments were not insulated, Mr. Canton observes, that they could not properly be said to be electrified; but that when they hung within the atmosphere of the excited tube, they
might

might attract and condense the electric fluid round about them, and be separated by the repulsion of its particles. He conjectures also, that the balls, at this time, contain less than their common share of the electric fluid, on account of the repelling power of that which surrounds them, though some may be continually entering and passing through the threads. And if that be the case, he says, the reason is plain why the balls hung by silk in the second experiment must be in a much more dense part of the atmosphere of the tube before they will repel each other. He adds, that at the approach of an excited stick of wax to the balls, in the first experiment, the electric fire is supposed to come through the threads into the balls, and to be condensed there, in its passage towards the wax; since, according to Dr. Franklin, excited glass emits the electric fluid, and excited wax receives it.

WHEN two balls, suspended by linen threads upon an insulated tin tube, were electrified positively, and had separated; he observed, that the approach of the excited tube would make them come nearer together; if brought to a certain distance, they would touch; and if brought nearer, they would separate again.

IN the return of the tube, they would approach each other, till they touched, and then repel as at first. If the tin tube was electrified by wax, or the wire of a charged phial; the balls would be affected in the same man-

ner at the approach of excited wax, or the wire of the phial. If the cork balls were electrified by glass, their replusion would be increased at the approach of an excited stick of wax. And the effect would be the same, if the excited glass was brought towards them, when they had been electrified by wax.

THE bringing the excited glass to the end, or edge of the tin tube, in the former of these experiments, is by Mr. Canton supposed to electrify it positively, or to add to the electric fire it before contained; and therefore some will be running off through the balls, and they will repel each other. But at the approach of excited glass, which likewise emits the electric fluid, the discharge of it from the balls will be diminished, or part will be driven back, by a force acting in a contrary direction, and they will come nearer together. If the tube be held at such a distance from the balls, that the excess of the density of the fluid round about them above the common quantity in air, be equal to the excess of the density of that within them above the common quantity contained in cork, their repulsion will be quite destroyed. But if the tube be brought nearer, the fluid without being more dense than that within the balls, it will be attracted by them, and they will recede from each other again.

MR. CANTON farther observes, that when the apparatus has lost part of its natural store of this fluid, by the approach of excited wax to one end of it, or is electrified negatively,

ly, the electric fire is attracted and imbibed by the balls, to supply the deficiency; and that more plentifully at the approach of excited glass, or a body positively electrified, than before; whence the distance between the balls will be increased, as the fluid surrounding them is augmented. And, in general, whether by the approach or recess of any body, if the difference between the density of the internal and external fluid be increased, or diminished; the repulsion of the balls will be increased, or diminished accordingly.

He observed, that when the insulated tin tube was not electrified, if the excited glass was brought towards the middle of it, the balls hanging at the end would repel each other, and the more so as the excited tube was brought nearer. When it had been held a few seconds, at the distance of about six inches, and withdrawn, the balls would approach each other till they touched; and, separating again, as the tube was removed farther, would continue to repel when the tube was taken quite away. This last repulsion would be increased by the approach of excited glass, and diminished by that of excited wax; just as if the apparatus had been electrified by wax, after the manner described in the last experiment.

He insulated two tin tubes, which may be distinguished by calling them A and B, so as to be in a line with each other, and half an inch asunder, and at the remote end of each suspended a pair of cork balls. Then, upon

bringing the excited glass tube towards the middle of A, and holding it a short time at the distance of a few inches, he observed each pair of balls to separate. Upon withdrawing the tube, the balls of A would come together, and then repel each other again, but those of B would hardly be affected. By the approach of excited glass the repulsion of the balls of A would be increased, and those of B diminished*.

IN the former of these experiments, Mr. Canton supposes the common stock of electric matter in the tin tube to be attenuated about the middle, and to be condensed at the ends, by the repelling power of the atmosphere of the excited glass tube, when held near it. And perhaps, he says, the tin tube may lose some of its natural quantity of the electric fluid before it receives any from the glass, as that fluid will more readily run off from the ends or edges of it than enter at the middle; and accordingly, when the glass tube is withdrawn, and the fluid is again equally diffused through the apparatus, it is found to be electrified negatively; since excited glass brought under the balls will increase their repulsion.

IN the latter of the experiments, Mr. Canton supposes that part of the fluid driven out of one tin tube enters the other, which is found to be electrified positively, by the decreasing of the repulsion of its balls at the approach of excited glass.

* Phil. Trans. Vol. xlviii. pt. i. p. 350.

IT will readily be seen that, at the time these experiments were made, Mr. Canton retained the common idea of electric atmospheres; whereas it will appear by the experiments of Messrs. Wilcke and Æpinus (which in fact contain nothing more than those of Mr. Canton) that they tend to refute the common opinion, and are much easier explained upon the supposition, that the portion of fluid belonging to any electrified body is constantly held in contact, or very nearly in contact, with the body; but acts upon the electricity of other bodies at a certain distance.

DR. FRANKLIN pursued, or rather diversified the experiments of Mr. Canton, but retaining, likewise, the common opinion of electric atmospheres, he thought that the phenomena were more easily explained upon the supposition, that these atmospheres, being brought near each other, did not easily mix, and unite into one atmosphere, but remained separate, and repelled each other; and moreover, that an electric atmosphere, would not only repel another electric atmosphere, but also the electric fluid contained in the substance of a body approaching it, and, without joining or mixing with it, force it into the other parts of the body that contained it.

THOUGH it must be difficult to assign a reason why the particles of one atmosphere should repel the particles of another atmosphere, or of the fluid contained in another body with more force than they repel one another, or the particles of the fluid contained

in the body to which they belong, since the matter is the same in both; yet this idea of the mutual repulsion of electric atmospheres, could it once be supposed, will certainly and clearly account for all the facts; and the theory pleases on account of its simplicity. But the same appearances will be accounted for, in a manner as simple and intelligible, upon the supposition, that the portion of electric fluid belonging to each body, being strongly attracted by the body, is held in close contact with it; but that it acts by repulsion upon the electric fluid belonging to other bodies, at a distance from them; and that the electric fluid doth not actually pass out of one body into another, till it have first repelled the fluid out of the other body, and then be more strongly attracted by the other body, than by its own; which has already got more than its natural share.

THE paper containing an account of these experiments of Dr. Franklin was read at the Royal Society, December the 18th, 1755. His apparatus was different from that of Mr. Canton, but still he exhibited the same effects proceeding from the same cause. He fixed a tassel of fifteen or twenty threads, each three inches long, at one end of his prime conductor, which was five feet long and four inches in diameter, supported by silk lines. The threads were a little damp, but not wet.

IN these circumstances, an excited tube brought near the end of the prime conductor, opposite to the threads, so as to give it

some sparks, made the threads diverge, each thread having thereby acquired its separate electric atmosphere.

IN this state, the approach of the excited tube, without giving any sparks, made the threads diverge more; but, being withdrawn, they closed as much; the atmosphere of the conductor being driven by that of the tube into the threads, and returning again upon withdrawing the tube, which had then left no part of its atmosphere behind it.

THE excited tube brought under the diverging threads made them close a little, having driven part of their atmospheres into the conductor. Upon being withdrawn, they diverged as much; that portion of their atmospheres which they had lost returning again from the conductor, and the tube having left no part of its own.

THE excited tube, held at the distance of five or six inches from the end of the conductor opposite to the threads, made them separate, and, upon being withdrawn, they came together again: but if, in their state of separation, a spark was taken from the conductor near them, they would close; and, upon removing the tube, would separate. The tube, in both cases, left no part of its atmosphere behind it. It only drove the natural quantity of electricity contained in the conductor towards the threads; and part of that being taken away by the spark, the tube would leave the conductor and threads negative, in

which case, they would repel one another, as if they had been electrified positively.

IN this situation, if the excited tube was brought near the conductor, they would close again; the atmosphere of the tube forcing that of the conductor into the threads, to supply the place of what they had lost: but, upon withdrawing the tube, they would open again; the tube, as before, taking its whole atmosphere away with it. When the excited tube was brought under the threads, diverging with negative electricity, they diverged more; the atmosphere of the tube driving away more of the atmospheres of the threads, and giving them none in its place.

LASTLY the Doctor brought the excited tube near the prime conductor, when it was not electrified; and when the threads were, thereby, made to diverge, he brought his finger near them, and observed, that they receded from it. This appearance had been taken notice of by Mr. Hauksbee, and others. Dr. Franklin accounts for it by supposing, that when his finger was plunged into the atmosphere of the glass tube, part of its natural electricity was driven back, through his hand and body, so as to leave the finger negatively electrified, as well as the threads; in which case they must necessarily repel one another. To confirm this hypothesis, he held a slender lock of cotton, two or three inches long, near the prime conductor, electrified by excited glass, which made the cotton stretch itself to-
wards

towards the conductor; and observed, that, in this state, it would recede from the finger of his other hand, at the same time that it was attracted by a wire of a bottle charged positively*.

THESE experiments of Dr. Franklin, made in pursuance of those of Mr. Canton, were confirmed, as I observed before, and carried much farther by Messrs. Wilcke and Æpinus.

MR. WILCKE observes, that a small body immersed in any electric atmosphere, if it be touched by no other body, and be withdrawn before it be repelled, scarce ever shows any sign of electricity; if any, it is of the same kind with that of the body into whose atmosphere it was plunged†. If any body, communicating with the ground, be brought to this light body, while it remains immersed in the atmosphere of the electrified body, it is first attracted, and then repelled by it. If a point be presented to this light body, and afterwards withdrawn, it will be found to have acquired an electricity opposite to that of the electrified body. From this he concludes, that parts of non-electric bodies, plunged in electric atmospheres, acquire an electricity opposite to that of the atmosphere in which they are plunged‡.

He placed two large insulated conductors with their ends opposite to one another, and a cork ball suspended on silk between them; and observed, that, upon the application of

* Phil. Transf. Vol. xlix. pt. i. p. 300.

† Wilcke, p. 73.

‡ Ibid. p. 77.

the excited glass tube to one end of either of them, the cork ball would play between them very fast; and if the tube were held a while at the same distance, would be at rest. Upon withdrawing the tube, the motion of the cork ball began again, and, at length, ceased gradually as before. If the conductors were removed from one another, while they were within the atmosphere of the tube, they would, upon being brought together again, give a spark. This experiment confirmed the demonstration, that the part of a body which is immersed in the atmosphere of an electrified body acquires the contrary electricity*.

BUT the most complete demonstration of this general maxim is an experiment of Mr. Æpinus. He placed a small weight upon one end of a large metallic conductor, and, by means of a silk string, removed it from the conductor, while the end on which it rested was immersed in the atmosphere of an electrified body; and found that it had actually acquired a different electricity from that of the atmosphere. If the end of the conductor, opposite to that on which the moveable weight was placed, was made to communicate with the earth, still that part of it which was near the excited electric was affected with the opposite electricity. Placing the moveable weight on the opposite end of the conductor, when it was insulated, he found that it had sometimes

* Wilcke, p. 78.

acquired an electricity contrary to that of the excited electric, sometimes the same electricity, though weak, and sometimes no electricity at all *.

THE same ingenious philosopher considered that the same principle must extend to glass, and all other electrics; since they, as well as conductors, contain a certain quantity of the electric fluid, in their natural state. To verify this, he took a glass tube, and electrified one end of it positively. The consequence was, that four or five inches of that end were positive; but beyond that there were two inches negative; and beyond that the tube was again positive, though weakly so. This experiment he repeated very often with the same success; as also when, instead of glass, he used a solid stick of sulphur. To account for this fact, he supposed, that the electricity communicated to the end of the tube repelled the natural quantity of the fluid in the glass to some distance. This natural quantity retiring from its former situation, he supposes to become condensed, and consequently to repel another quantity of the fluid natural to the glass from its place; and that thus the whole rod would be alternately positive and negative. The author asserts, that it was from theory only that he was led to this curious experiment, the fact exactly corresponding to what he had before deduced, as the necessary consequence of Dr. Franklin's

* *Æpini Tentamen*, p. 129.

principles of negative and positive electricity *.

THE hint of these experiments Mr. Æpinus received from those above mentioned of Mr. Wilcke; and these gentlemen, residing at the same time at Berlin, pursued these curious experiments jointly, till they were led by them to discover a method of charging a plate of air in the same manner as plates of glass had usually been charged, and to throw still more light upon the theory of the famous Leyden experiment.

IN the above mentioned experiments, these gentlemen observed, that the negative state of one of the bodies depended on the opposite state of the other, which was known to be exactly the case of the two sides of a charged pane of glass; and the reason of the non-communication of the same electricity was evidently the impermeability of the glass to the electric fluid in the one case, and the impermeability of the air in the other. Upon this hint they made several attempts to give the electric shock by means of air; and at length succeeded, by suspending large boards of wood covered with tin, with the flat sides parallel to one another, and at some inches asunder. For they found, that upon electrifying one of the boards positively, the other was always negative, agreeable to the former experiment: but the discovery was made complete and indisputable by a person's touch-

* Æpini Tentamen, p. 192.

ing one of the plates with one hand, and bringing his other hand to the other plate; for he then received a shock through his body, exactly like that of the Leyden experiment *.

WITH this plate of air, as we may call it, they made a variety of curious experiments. The two metal plates, being in opposite states, strongly attracted one another, and would have rushed together, if they had not been kept asunder by strings. Sometimes the electricity of both would be discharged by a strong spark between them, as when a pane of glass bursts with too great a charge. A finger put between them promoted the discharge, and felt the shock. If an eminence was made on either of the plates, the self-discharge would always be made through it, and a pointed body fixed upon either of them prevented their being charged at all.

THE state of these two plates, they excellently observe, justly represents the state of the clouds and the earth during a thunder storm; the clouds being always in one state, and the earth in the opposite; while the body of air between them answers the same purpose as the small plate of air between the boards, or the plate of glass between the two metallic coatings in the Leyden experiment. The phenomenon of lightning is the bursting of the plate of air by a spontaneous discharge, which is always made through emi-

* Wilcke, p. 97.

nences, and the bodies through which the discharge is made are violently shocked*.

THIS principle, they likewise thought, would explain an observation of the Abbé Nollet, that electricity was often observed to be peculiarly strong, when the room was full of company, and more particularly, when numbers of them drew near together, to see the experiments. The conductor was then in one state, and the company in another; so that, constituting a large surface, when any of them took a spark, as he thereby discharged the electricity of all the company, he would feel it more sensibly than if he had stood single†.

THIS discovery, of the method of giving the electric shock by means of a plate of air, may be reckoned one of the greatest discoveries in the science of electricity since those of Dr. Franklin. It is beautiful to observe how this fine discovery took its rise from the experiments of Mr. Canton. Mr. Canton's experiments were pursued by Dr. Franklin, and those of Dr. Franklin, pursued by these gentlemen, produced the discovery. It is one and the same principle that, in different circumstances, accounts for this beautiful series of experiments.

THIS experiment of charging a plate of air is likewise related by Mr. Æpinus, who says that he was led to the discovery, by rea-

* Wilcke, p. 101.

† Ibid. p. 96, &c.

soning from the consequences of Dr. Franklin's theory.

FROM these experiments he was also led to form a more distinct idea of the impermeability of glass to the electric fluid. For since a plate of air might be charged as well as a plate of glass, that property, whatever it be, must be common to them both; and could not, as Dr. Franklin once supposed, be any thing peculiar to the internal structure of glass. Impermeability, he, therefore, infers, must be common to all electrics; and since they can all receive electricity by communication to a certain degree, it must consist in the difficulty and slowness with which the electric fluid moves in their pores; whereas, in perfect conductors, it meets with no obstruction at all*.

IT was chiefly this course of experiments, also, that led Mr. *Æpinus* to deny the existence of electric atmospheres, consisting of effluvia from electrified bodies.

HE seems, however, to consider this as a bold opinion; since he herein differs, as he says, from all the electricians who had written before him, and even from Dr. Franklin himself. Though the common opinion, he says, is by no means countenanced by the general principles of this theory, which suppose the electric fluid to move with difficulty through every electric substance like the air.

To those who might say, that an electric atmosphere is a thing obvious to the senses,

* *Æpini Tentamen*, p. 82.

and no matter of theory; since it may be felt like a spider's web upon the hands or face; he replies, that this feeling, together with the sulphureous smell of electrified bodies, are only sensations excited by the action of the fluid in the electrified bodies upon the electric fluid in the nostrils, or the hand; or upon those parts of the body themselves in an unelectrified state; and that they are not felt by a person who is not possessed of the same kind and degree of electricity.

HE, therefore, thinks there never was any sufficient reason to admit those atmospheres; and declares, that whenever he uses the word, he means no more by it than the *sphere of action* of the electricity belonging to any body. Or, he says, the neighbouring air, which is electrified by it, may be so called.

BUT that these atmospheres have little effect in electrical experiments, he says, is evident from this circumstance; that if it be blown upon with a pair of bellows, the electricity of the body which it surrounds is not sensibly diminished. The electric fluid, he supposes, to reside wholly in the electrified body, and from thence to exert its attraction or repulsion to a certain distance*.

THE subject of electric atmospheres had not escaped the attention of the accurate Signior Beccaria, who was probably prior to Mr. Æpinus in supposing, that electrified bodies have no other atmosphere than the electricity commu-

* Æpini Tentamen, p. 257.

nicated to the neighbouring air, and which goes with the air, and not with the electrified bodies, agreeable to that curious discovery of his mentioned above.

He also mentions an experiment, which, he thinks, directly proves, that all the electricity communicated to any body adheres to its surface, and does not spread into the air. He electrified a large conductor of gilt paper, in which the gilding was, in several places, taken off quite round; and observed that whenever he discharged it, by taking a spark at the end, other sparks were visible at all the interruptions; the charge of the more remote parts having come off through the substance of the metal, and not along the air; as the greatest part of it, at least, might have done, if it had lodged there *.

It is now also Mr. Canton's opinion, that electric atmospheres are not made of effluvia from excited or electrified bodies, but that they are only an alteration of the state of the electric fluid contained in, or belonging to the air surrounding them, to a certain distance; that excited glass, for instance, repels the electric fluid from it, and consequently, beyond that distance makes it more dense; whereas excited wax attracts the electric fluid existing in the air nearer to it, making it rarer than it was before.

This will be best understood by a figure. Let A (Pl. I. fig. 1.) represent unexcited glass

* *Elettricismo artificiale*, p. 54.

or wax. B excited glass, and C excited wax; and let the dots on each side of A represent a line of particles of the electric fluid at their proper distance in a natural state.

LET B and C be carried about where you will in the air, B will make an atmosphere equally dense, and C an atmosphere equally rare, while the quantity of the electric fluid each of them contains is the same as at first. When any part of a conductor comes within the atmosphere of B, the electric fluid it naturally contains will be repelled by the dense atmosphere, and will recede from it. But if any part of a conductor be brought within the atmosphere of C, the electric fluid it naturally contains will be attracted by the rare atmosphere, and move towards it. And thus may the electric fluid contained in any body be condensed or rarefied; and if the body be a conductor, it may be condensed or rarified in any part of it, and some may be easily drawn out of, or an additional quantity put into it.

IT was observed before, that an experiment of Dr. Franklin, which he thought proves that electric atmospheres did not exclude the air, might justly make us suspect the existence of those atmospheres, since the electric matter is known to repel the air. Another experiment of the same nature was made by Dr. Darwin of Litchfield, who sent an account of it to the Royal Society, which was read May the 5th, 1757. He got a glass tube, open at one end, and having a ball at the other. This ball and half of the tube he coated; and when he had
inverted

inverted it, and dipped a considerable part of it into a vessel containing oil of turpentine, he introduced a wire into it, and charged it; and observed, that the oil did not at all appear to subside. From this he concluded, that the electric atmosphere, flowing round the wire and the coating the tube, above the oil, did not displace the air, but existed in its pores*.

AN experiment similar to that of Dr. Franklin and that of Dr. Darwin was made by Signior Beccaria. He took a coated phial, and when he had inserted into it a small glass tube, bent horizontally when it came out of the phial; he closed it with cement, and presented light ashes to the extremity of the tube, the orifice of which was very fine; and always found, that the ashes were blown off, when a spark was taken into the phial, but they returned towards the end of the tube afterwards†. It is probable, that the metal not being sufficiently in contact with the inside coating, a spark was made in the inside, which expelled the air, and caused the motion in the ashes. The fairest method of trying it would be with a phial, in which the metal that received the fire from the conductor, should be a production of the inward coating.

* Phil. Trans. Vol. I. pt. i. p. 351.

† Lettere dell' elettricismo, p. 79.

S E C T I O N VI.

MR. SYMMER'S EXPERIMENTS RELATING
TO THE TWO ELECTRICITIES, AND THOSE
MADE BY JOHANNES FRANCISCUS
CIGNA IN PURSUANCE OF THEM.

IT had hitherto been universally supposed, that all the phenomena of electricity were produced by the action of one electric fluid. Even Mr. Du Fay, at the time that he imagined he had discovered another electric fluid, distinct from that of glass, and peculiar to rosin, &c. thought, however, that it was quite independent of the other, and that their operations were never combined. Dr. Watson, and Dr. Franklin thought it was very evident, that the difference between the two electricities consisted in the one being a redundancy, and the other a deficiency of the same matter. And all the experiments that had been made concerning the two electricities seemed to confirm this hypothesis. At length, however, Mr. Symmer produces a great number of curious experiments, relating to the same subject; and infers from them the probable existence of *two electric fluids*, not independent, but always co-existent, and counteracting one another.

THE first set of his experiments are very remarkable, but he does little more than relate naked facts. They were diversified, and
2 pursued

purfued much farther by Mr. Cigna, of Turin, who has alfo explained them upon the principles of Dr. Franklin's theory; though he was of opinion, that no experiments that had yet been made were decifive in favour of either of the two hypothefes. Few hiftories of experiments are more entertaining than the firft of thefe of Mr. Symmer; the fubfequent experiments are lefs fatisfactory. The papers relating to them all were read at the Royal Society in the year 1759*.

THIS gentleman had for fome time obferved, that upon putting off his ftockings, in an evening, they made a crackling or snapping noife, and that, in the dark, he could perceive them to emit fparks of fire. He had no doubt but that this proceeded from the principle of electricity, and, after a great number of obfervations, to determine on what circumftances thofe ftong electrical appearances depended, he found, at length, that it was the combination of white and black that produced the electricity; and that the appearances were the ftrongeft when he wore a white and black filk ftocking upon the fame leg†. Thefe, however, difcovered no fign of electricity while they were upon the

* Phil. Tranf. Vol. li. pt. i. p. 340.

† The Abbé Noller, in repeating thefe experiments of Mr. Symmer, found, that it was not abfolutely neceffary, that one of the ftockings fhould be black, for that, if one of them was only dipped in a decoction of gall-nuts, which doth not dye them black, but is only a preparative to it, it would have the fame effect. Noller's Lettres, Vol. iii. p. 42.

leg, or hand (for he found that his hand was sufficient) though they were drawn backwards and forwards upon it several times. Nor when taken from the hand, and presented to an electrometer (i. e. Mr. Canton's balls) did they appear to have acquired any more than a very small degree of electricity; but the moment they were separated, they were found, both of them, to be highly electrified, the white positively, and the black negatively.

BOTH the stockings, when held at a distance from one another, appeared inflated to such a degree, that, when highly electrified, they exhibited the intire shape of the leg; and when two black, or two white stockings were held together, they would repel one another, so as to form an angle, seemingly, of thirty or thirty-five degrees.

WHEN a white and black stocking were presented to each other; they would be mutually attracted; and, if permitted, would rush together with surprising violence. In their approach their inflation gradually subsided, and their attraction of foreign objects diminished, but their attraction of one another increased. When they actually met, they grew flat, and joined as close together, as if they had been so many folds of silk. When they were separated, their electricity did not seem to have been in the least impaired by the shock of meeting; for they would be again inflated, attract, repel, and rush together as before.

WHEN

TWO ELECTRICITIES. 311

WHEN this experiment was performed with two black stockings in one hand, and two white ones in the other, it exhibited a curious spectacle. The repulsion of those of the same colour and the attraction of those of different colours, threw them into an agitation which was not unentertaining, and made them catch each at the opposite colour, at a greater distance than could have been expected.

WHEN the stockings were separated from one another, they would lose their power very soon, much like the excited tube; but when they were together, they would retain it an hour or two, or longer, if the air was favourable to electricity. The sharpest metallic point could not deprive them of it; and when they were one within the other, no means he could think of could procure the least perceivable discharge of the electricity. In this respect, Mr. Symmer thought there was a considerable resemblance between the black and the white stocking, when put within one another, and the Leyden phial.

WHAT was still more remarkable in these experiments with the white and black stockings, was the power of electrical cohesion which they exhibited. Mr. Symmer perceived that the white and black stockings, when electrified, and allowed to come together, not only joined extremely close, but actually stuck to each other. By means of a balance, he found, that in order to separate them, it required from one to twelve ounces. Another

time they raised seventeen ounces, which was twenty times the weight of that stocking which supported them, and this in a direction parallel to its surface.

WHEN one of the stockings was turned inside out, and put within the other, it required twenty ounces to separate them, though when they were applied to each other externally, ten ounces were sufficient.

GETTING the black stockings new dyed, and the white ones washed, and whitened in the fumes of sulphur; and then putting them one within the other, with their rough sides together, it required three pounds three ounces to separate them. And he had reason to think that the sulphur contributed nothing to the experiment.

TRYING this experiment with stockings of a more substantial make, he found the effects more considerable. When the white stocking was put within the black one, so that the outside of the white was contiguous to the inside of the black, they raised nine pounds wanting a few ounces, which was fifty times the weight of the stocking. When the white stocking was turned inside out, and put within the black one, so that their rough surfaces were contiguous, they raised fifteen pounds one pennyweight and a half, which was ninety-two times the weight of the stocking.

HAVING cut off the ends of the thread, and the tufts of silk, which had been left in the inside of the stockings, the cohesion was considerably diminished. Pressing them together

gether between his hands contributed much to strengthen it *.

WHEN the white and black stocking were in cohesion, and another pair, more highly electrified were separated from one another, and presented to the former, their cohesion would be dissolved; and each stocking of the second pair would catch hold of, and carry away with it, that of its opposite colour. If the degree of electricity of both pairs were equal, the cohesion of the former pair would be weakened, but not dissolved; and all the four would cohere, forming one mass. If the second pair were but weakly electrified, the cohesion of the first pair would be but little impaired, and the cohesion of the whole mass would be small in proportion.

MR. SYMMER also observed, that white and black silk, when electrified, not only cohered with each other, but would also adhere to bodies with broad, and even with polished surfaces, though those bodies were not electrified. This he discovered accidentally, having, without design, thrown a stocking out of his hand, which stuck to the paper hangings of the room. He repeated the experiment, and found it would continue hanging near an hour.

HAVING stuck up the black and white stockings in this manner, he came with another pair of stockings highly electrified; and applying the white to the black, and the

* Phil. Transf. Vol. li. pt. i. p. 393.

black to the white, he carried them off from the wall, each of them hanging to that which had been brought to it.

THE same experiments held with the painted boards of the room, and likewise with the looking-glass, to the smooth surface of which both the white and the black silk appeared to adhere more tenaciously than to either of the former*.

A FEW observations, similar to some of these of Mr. Symmer, were made by Signior *Alessandro Amadeo Vaudonia*, a friend of Signior Beccaria. He put a beaver shirt between two others, which he wore in extreme cold weather; and whenever he put off the uppermost shirt, which he did every day, he found it adhered to the beaver shirt, and, on the separation, electric sparks were visible between them. Whenever he put off the beaver shirt, it adhered still more to the under shirt, and when held at a considerable distance from it, would rush to it. These attractions would be repeated many times, but they grew more languid by degrees, till they intirely ceased. Signior Beccaria, upon hearing of this experiment, repeated it with some variation, and found it to answer on himself†.

THE cohesion of the two stockings induced Mr. Symmer to try the force of electrical cohesion in electrified panes of glass. For this purpose, he got two panes of common window glass, the thinnest and the smoothest that he

* Phil. Trans. Vol. li. pt. i. p. 366.

† Dell' elettricismo artificiale, p. 197.

could find, and coated one of the sides of each with tinfoil, leaving a space uncovered near the edges. He then put the uncovered sides together, and charging them both as one pane, he found, as he expected, that their cohesion was considerably strong : but he had no apparatus to measure the strength of it. He then turned the plates upside down, and found that the same operation which had before charged them, did now uncharge them, according to the analogy of the Leyden phial.

PLACING two panes of glass, each of them coated on both sides, one upon the other, he found that they were both charged separately, and that there was no cohesion between them.

IN pursuance of these last mentioned experiments of Mr. Symmer, and another made at Pekin (which will be recited presently) Signior Beccaria made the following, which are very curious ; but which, after the example of the author, I shall relate without attempting an explanation.

HAVING charged a coated plate of glass, he slipped the coating from off the negative side, and applied another, uncoated and uncharged plate of glass close to it. After this, putting a coating upon the uncharged glass (so that the whole resembled one coated plate, consisting of two *laminæ*) he formed a communication between the coatings. The consequence was an explosion, and a cohesion of the plates.

IF he separated the plates before the explosion, after they had been in conjunction some time, the charged plate was positive on both sides, and the uncharged plate negative on both sides. But if he separated them after the explosion, the charged plate was negative on both sides, and the uncharged plate positive on both sides.

IF after the explosion he separated and joined them again alternately, a small circle of paper, placed under the uncharged plate, adhered to it upon every separation, and was thrown off again upon every conjunction. This he could repeat even five hundred times, with once charging the plate. This was the experiment that he says was made by some Jesuits at Pekin, in the year 1755, and being sent to the Academy of Sciences at Peterburgh, was published in their Memoirs, vol. viii. p. 276.

IF, in these experiments, the charged plate was inverted, and the positive side applied to the uncharged plate, all the effects were exactly the reverse of the former. If it was inverted ever so often, after remaining some time in contact with the uncharged glass, it would produce a change in the electricity. In the dark, a light was always seen upon the separation of these plates.

LAYING the two plates together, like one plate, and coating the outsides of them, he charged them both together, and, at the distance of about four feet, he distinguished six of the *coloured rings*, which Newton describes in his book of Optics, all parallel to one another,

other, and nearly parallel to the edge of the coating. At the angles of the coatings the rings spread to a greater distance; where the coatings did not quite touch the glass, the rings bent inwards; and where the coatings adhered very close, they retired farther from them. Upon discharging these two plates, the coloured rings vanished, and the electric cohesion (which Mr. Symmer had observed in this case) ceased with them.

UPON separating these plates before the explosion, that which had received the positive electricity was positive on both sides, and the other negative on both sides. If they were separated after the explosion, each of them (as in the former experiment) was affected in a manner just the reverse of this. Upon inverting these plates, that which was the thinner appeared to be possessed of the stronger electricity, and (like the charged plate in the former experiment) brought the other to correspond to it.

CHARGING the two plates separately, and taking off two of the coatings, so as to place the two positive or the two negative sides together, there was no cohesion or explosion. But joining a positive and a negative side, they cohered, and a communication being formed on the outside, there was an explosion, which increased the cohesion. Making the above-mentioned experiments with these plates, he says, they acted just as the two that were charged at the same time*.

* Phil. Transf. Vol. lvii. p. 458.

MR. SYMMER concludes his account of these experiments with declaring it be his opinion, that there are two electric fluids, or emanations of two distinct electric powers, essentially different from each other; that electricity does not consist in the afflux and efflux of these fluids, but in the accumulation of the one or the other of them in bodies electrified; or, in other words, it consists in the possession of a larger portion of one or the other power, than is requisite to maintain an even balance within the body; and lastly, that, according as the one or the other power prevails, the body is electrified in the one or the other manner. Nor will this principle, says he, of two distinct electrical powers be found, upon due consideration, to disagree with the general system of nature. It is one of the fundamental laws of nature, that action and reaction are inseparable and equal; and, when we look round, we find that every power which is exerted in the material world meets with a counteracting power, which controuls and regulates its effects, so as to answer the wise purposes of providence*.

MR. SYMMER also alledges, in proof of his two distinct powers of electricity, the experiment which Dr. Franklin has related, of piercing a quire of paper with an electric shock. He thought that the bur which was raised on both sides of the paper was produced by two fluids, moving in two different direc-

* Phil. Transf. Vol. li. pt. i. p. 389.

tions. To show the manner in which this stroke was made more evidently, he mentions two other similar experiments, in which the circumstances of the stroke were a little varied.

A PIECE of paper, covered on one side with Dutch gilding, and which had been left accidentally between two leaves, in a quire of paper in which the former experiment had been made, was found to have the impression of two strokes upon it, about a quarter of an inch from each other; the gilding being stripped off, and the paper left bare for a little space in both places. In the center of one of these places was a little round hole, in the other only an indenture or impression, such as might have been made with the point of a bodkin.

THESE observations Mr. Symmer communicated to Dr. Franklin, who, notwithstanding Mr. Symmer was endeavouring to establish a theory of electricity contrary to his own, with the generosity natural to him, assisted him with his apparatus in making another experiment in pursuance of that mentioned above.

IN the middle of a paper book, of the thickness of a quire, Mr. Symmer put a slip of tinfoil; and in another, of the same thickness, he put two slips of the same sort of foil, including the two middle leaves of the book between them. Upon striking the two different books, the effects were answerable to what he expected. In the first, the leaves on
each

each side of the foil were pierced, while the foil itself remained unpierced; but, at the same time, he could perceive an impression had been made on each of its surfaces, at a little distance from one another; and such impressions were still more visible on the paper, and might be traced, as pointing different ways. In the second, all the leaves of the book were pierced, excepting the two that were between the slips of foil; and in these two, instead of holes, the two impressions in contrary directions were visible.

MR. SYMMER afterwards got an electrical apparatus of his own, formed on the model of that of Dr. Franklin, with which he frequently repeated the experiments above-mentioned, the result of all which he comprises in the three following observations.

I. WHEN a quire of paper, without any thing between the leaves, is pierced with a stroke of electricity, the two different powers keep in the same track, and make but one hole in their passage through the paper: not but that the power from above, or that from below, sometimes darts into the paper at two or more different points, making so many holes, which, however, generally unite before they go through the paper. They seem to pass each other about the middle of the quire, for there the edges are most visibly bent different ways; whereas, in the leaves near the outside of the quire, the holes very often carry more the appearance of the passage of a power issuing out, and exploding

ploding into the air, than of one darting into the paper.

2. WHEN any thin metallic substance, such as gilt leaf, or tinfoil, is put between the leaves of the quire, and the whole is struck, in that case, the counteracting powers deviate from the direct tract, and leaving the path which they would in common have taken through the paper, only make their way in different lines to the metallic body, and strike it in two different points, distant from one another about a quarter of an inch, more or less; the distance appearing to be the least when the power is greatest: and whether they pierce it, or only make impressions upon it, in either case they leave evident marks of motion from two different parts, and in two contrary directions. It is this deviation from a common course, and the separation of the lines of direction consequent upon it, says he, that affords a proof of the exertion of two distinct and counteracting powers.

3. WHEN two slips of tinfoil are put into the middle of the quire, including two or more leaves between them, if the electricity be moderately strong, the counteracting powers only strike against the slips, and leave their impression there. When it is stronger, one of the slips is generally pierced, but seldom both; and from what he had observed in such cases, he says it should seem, as if the power which issued from the outside of the phial acted more strongly than that which proceeded from within, for the lower slip was most com-

monly pierced. But this, he adds, may be owing to the greater space which the power from within has to move through before it strikes the paper*.

IN the same paper, Mr. Symmer furnishes a remarkable instance of the power of an hypothesis in drawing facts to itself, in making proofs out of facts which are very ambiguous, and in making a person overlook those circumstances in an experiment which are unfavourable to his views.

WHEN a phial is electrified but a little, Mr. Symmer says, if we touch the coating of it with a finger of one hand, and, at the same time, bring a finger of the other hand to the wire, we shall receive a pretty smart blow upon the tip of each of the fingers, the sensation of which reaches no farther. If the phial be electrified a degree higher, we shall feel a stronger blow, reaching to the wrists, but no farther. When, again, it is electrified to a still higher degree, a severer blow will be received, but will not be felt beyond the elbows. Lastly, when the phial is strongly charged, the stroke may be perceived in the wrists, and elbows; but the principal shock is felt in the breast, as if a blow from each side met there. This plain and simple experiment, says Mr. Symmer, seems obviously to suggest to observation the existence of two distinct powers, acting in contrary directions; and, I believe, says he, it would be held as a

* Phil. Transf. Vol. li. pt. i. p. 377, &c.

sufficient proof by any person, who should try the experiment, with a view to determine the question simply from his own perceptions *.

It is a sufficient answer to this remark of Mr. Symmer, that if twenty people join hands, they may all be made to feel the shock in their wrists, or their elbows, without having their breasts affected in the least. And can it be supposed, that the two currents of electric fire could come at all their wrists or elbows, without passing through their breasts? According to Mr. Symmer's hypothesis, it should seem, that, in a large circle, those persons only who stood near the phial, on either hand, should feel *a small shock*; that a few persons more, at each extremity of the circle, should feel one something stronger; and that it could only be a very strong shock, which could at all affect the person who stood in the middle; and that then he should be affected the least of any person in the company. But all these consequences are contrary to fact.

THIS hypothesis of Mr. Symmer, notwithstanding he has failed in his application of it to the experiments above mentioned, has attracted the notice of several electricians, both at home and abroad; and some persons seem inclined to adopt it, in preference to Dr. Franklin's theory. I shall therefore consider it more at large, when I come to treat of *theories*

* Phil. Trans. Vol. li. pt. i. p. 373, &c.

professedly; till which time, I take leave of this ingenious philosopher, and his two electric fluids.

THE experiments of Mr. Symmer excited the attention of Mr. Cigna, and led him to a course of experiments, which throws still more light, both upon the doctrine of the two electricities and the Leyden phial. They are also a farther illustration of the discovery of Mr. Canton, improved by Messrs. Wilcke and Æpinus, of the mutual repellency of similar electric atmospheres.

HE took two white silk ribbons, just dried at the fire, and having extended them upon a smooth plain, either a conductor or a non-conductor, he drew over them the sharp edge of an ivory ruler, and found, that both the ribbons had acquired electricity enough to adhere to the plain; though, while they remained upon the plain, they shewed no other sign of it. If they were both taken off from the plain together, they attracted one another, the upper having acquired the resinous and more powerful, and the lower the vitreous and weaker electricity. If they were taken up separately, they repelled one another, having both acquired the resinous electricity*.

IN this separation of both the ribbons from the plain, as also in their separation, afterwards, from one another, electric sparks were visible between them; but if they were

* Memoirs of the Academy at Turin for the year 1765, p. 31.

again put upon the plain, or joined together, no light appeared upon their second separation, without another friction of the ruler. Also, when, by being taken off separately, they had been made to repel one another, if they were laid on the plain again, and taken off together, they would not attract; and if, by being taken off together, they had first been made to attract one another, and were laid on the plain a second time, and then taken off separately, they would not repel, without another friction.

WHEN, by the operation above mentioned, they had acquired the same electricity, if they were placed, not upon the smooth body on which they had been rubbed, but on a rough one, and a conductor, as hemp or cotton, not very dry; they would, upon being separated, show contrary electricities; which, when they were joined together, would disappear as before*.

IF they had been made to repel one another, and were afterwards placed one upon the other, on the rough surface above mentioned, they would, in a few minutes, attract one another; the lower of the two ribbons having changed its resinous into a vitreous electricity.

IF the two white ribbons received their friction upon the rough surface, they always acquired contrary electricities; the upper of the two having the resinous, and the lower

* Memoirs of the Academy at Turin for the year 1765, P. 33.

the vitreous, in whatever manner they were taken off.

THE same thing that was done by a rough surface was done by any pointed conductor. If two ribbons, for instance, were made to repel, and hang parallel to one another; and the point of a needle were drawn opposite to one of them, along its whole length, they would presently rush together; the electricity of that ribbon to which the needle was presented being changed into the contrary*.

IN the same manner in which one of the ribbons changed its electricity, a ribbon not electrified would acquire electricity, viz. by putting it upon a rough surface, and laying an electrified ribbon upon it; or by holding it parallel to an electrified ribbon, and presenting a pointed conductor to it.

HE placed a ribbon not quite dry under another that was well dried at the fire, upon a smooth plain; and when he had given them the usual friction with his ruler, he found that, in what manner soever they were removed from the plain, the upper of them had acquired the resinous, and the lower the vitreous electricity†.

IF both the ribbons were black, all the above mentioned experiments succeeded, in the same manner as if they had been white‡.

* Memoirs of the Academy at Turin for the year 1765, p. 34. † Ibid, p. 35. ‡ Ibid.

IF, instead of his ivory ruler, he made use of any skin, or of a piece of smooth glass, the event was the same; but if he made use of a stick of sulphur, the electricities were, in all cases, the reverse of what they were before; the ribbon which was rubbed having always acquired the vitreous electricity.

WHEN he made use of paper, either gilt or not gilt, the results were uncertain.

WHEN the ribbons were wrapped in paper, gilt or not gilt, and the friction was made upon the paper, laid upon the plain above mentioned, the ribbons acquired, both of them, the resinous electricity *.

IF the ribbons were one black, and the other white, which ever of them was laid uppermost, and in whatever manner the friction was made, the black generally acquired the resinous, and the white the vitreous electricity †.

HE observed, however, the following constant event; that whenever the texture of the upper piece of silk was loose, yielding, and retiform, like that of a stocking, so that it could move, and be rubbed against the lower, and the rubber was of such a nature as to impart but little electricity to glass; the electricity which the upper piece of silk acquired, did not depend upon the rubber, but upon the body it was laid upon; in which case the black was always resinous, and the white vi-

* Memoirs of the Academy at Turin for the year 1765, p. 36.

† Ibid. p. 38.

treous. But when the silk was of a close texture, hard and rigid, and when the rubber was such as imparted a great degree of electricity to glass, the electricity of the upper piece did not depend upon the lower, but upon the rubber. Thus a white silk stocking, rubbed with gilt paper upon glass, became resinous, and the glass vitreous; but if a piece of silk, of a firmer texture, was laid upon a plate of glass, it always acquired the vitreous electricity, and the glass the resinous, if it was rubbed with sulphur; and for the most part, if it was rubbed with gilt paper*. So that the silk that was rubbed received its electricity, sometimes from the rubber, and sometimes from the substance placed under it; according as it received greater friction from the one or the other, or in proportion as one or the other was more proper to give electricity to glass.

ANOTHER set of experiments, which the same Mr. Cigna made, illustrate the adhesion of Mr. Symmer's electrical stockings to bodies with smooth surfaces. He insulated a plate of lead, and bringing an electrified ribbon near it, observed that it was attracted very feebly. Bringing his finger to the lead, a spark issued out of it, upon which it attracted the ribbon vigorously, and both together shewed no signs of electricity. Upon the separation of the ribbon, they again both ap-

* Memoirs of the Academy at Turin, for the year 1765, p. 40.

peared to be electrified, and a spark was perceived between the plate and the finger *.

LAYING two plates of glass upon a smooth conductor communicating with the ground, and rubbing them in the same manner as the ribbons had been rubbed, they likewise acquired electricity, and adhered firmly, both to one another, and to the conductor. If it were a plate of lead, not very thick, it would be supported by the attraction. When they were together, they showed no other signs of electricity †.

WHEN the two plates of glass were separated from the conductor, and kept together, they showed, on both sides, a vitreous electricity; and the conductor, if it had been insulated, was seen to have contracted a resinous electricity.

THE two plates of glass themselves, when separated, were possessed of the two electricities; the upper of the vitreous and stronger, and the lower of the resinous and weaker.

WITH a rough conductor, whether they were originally rubbed upon it, or brought to it, after they had been rubbed upon a smooth one, they scarce contracted any electricity; though, when they were separated from one another, they were affected as before.

UPON this principle, Mr. Cigna endeavours to account for the non-excitation of a globe

* Memoirs of the Academy at Turin, for the year 1765, p. 43.

† Ibid. p. 52.

or tube from which the air is exhausted, or which is lined with conducting substances. In this case, he says, the vitreous electricity on the external surface of the glass is balanced by the resinous in the inward coating, or in the vacuum which serves instead of a coating; and therefore it is in the situation of the plates of glass while they lie upon the conductor above-mentioned: but when the inward coating is taken away, the electricity appears on the outside, without any fresh excitation, as when the plates were removed from the conductor *.

WHEN he laid a number of ribbons of the same colour upon the smooth conductor, and drew his ruler over them; he found, that when he took them up singly, they all gave sparks, at the place where they were separated, as the last ribbon did with the smooth plate, and had all acquired the resinous electricity †.

IF they were all taken from the plate together, they cohered in one mass, which, on both sides, appeared to be resinous. If they were laid upon the rough conductor, in the same order (whereby the opposite electricities were brought to an equilibrium) and they were all separated singly, beginning with the lowest, sparks appeared as before; but all the ribbons had acquired the vitreous electricity, except the uppermost, which retained

* Memoirs of the Academy at Turin, for the year 1765, p. 54.

† Ibid. p. 61.

the resinous electricity it had received from the friction *.

IF they received the friction upon the rough conductor, and were all taken up at once (in order to have a bundle in which the opposite electricities were balanced) all the intermediate ribbons acquired the electricity, either of the highest or the lowest ribbon, according as the separation was begun with the highest, or the lowest.

IF two ribbons were separated from the bundle at the same time, they clung together; and, in that state, showed no sign of electricity, as one of them alone would have done. When they were separated, and the different electricities were manifest, the electricity was observed to reside in the outermost, and was opposite to that by which they had both adhered to the bundle, but much weaker †.

HE placed a number of ribbons upon a plate of metal, which received electricity from the globe, while he held a pointed body to the other side of the ribbons. The consequence was, that all the ribbons became possessed of the electricity opposite to that of the plate, or of the same, according as they were taken off; except the most remote, which always kept an electricity opposite to that of the plate.

FROM these experiments he infers, that as electricity is propagated from the outermost

* Memoirs of the Academy at Turin, for the year 1765, p. 61.

† Ibid.

ribbon to those underneath it, or else from the plate below to those next above it, when they are separated, so likewise when the coating is separated from a charged pane of glass, it likewise deposits its electricity upon the superficies of the glass, the phenomena being the same in both. For when he put metal coatings on the side of a plate of glass, without any cement, they adhered firmly to the glass when it was charged, and a light appeared upon their being separated from it, as in the case of the ribbons *.

WHEN he coated a number of ribbons in the same manner, and charged them, the coatings adhered firmly to the ribbons; but he could never separate one of them, but (in consequence of the loose texture of the silk) a spark would go to the opposite coating, which immediately fell off, the whole being then discharged †.

BUT he thought the coatings did not deposit all their electricity on the plate, when they were taken off; for though, when both were taken off, the electricities of the two sides still balanced one another (because each retained the same diminished quantity) yet, when one superficies of the glass, or of the ribbons, received its electricity from friction, and the other only from the opposite coating, he observed, that the electricities which balanced one another while the coating was on, were no longer balanced when it was taken off;

* Memoirs of the Academy at Turin, for the year 1765, p. 63.

† Ibid. p. 64.

the electricity of the surface which was rubbed then prevailing, because the conducting coating had, upon its separation, taken part of its electricity along with it*.

To confirm this, he adds another experiment. He charged a pane of glass, coated on one side, while the other received electricity by a pointed conductor from the machine: he likewise inverted the plate, and made the coated side communicate with the prime conductor, while a pointed piece of metal was presented to the opposite side; and, in both cases, found, that while the coating remained, the two electricities balanced one another; but that when the coating was slipped off, the electricity of the opposite side prevailed, so as to be apparent on both sides of the plate†.

* Memoirs of the Academy at Turin for the year 1765, p. 65.

† Ibid.

SECTION VII.

THE HISTORY OF THE LEYDEN PHIAL
CONTINUED.

GREAT as were the discoveries of Dr. Franklin concerning the Leyden phial, he left some curious particulars for this period of the history of electricity; and the subject is by no means exhausted. Many of the properties of this *wonderful bottle*, as the Doctor calls it, are still unexplained. But as more and more light is perpetually thrown upon it, let us hope that, at length, we shall thoroughly understand this great experiment. The greatest discovery concerning the properties of the Leyden phial, in this period, hath already been related in the account of Messrs. Wilcke's and *Æpinus's* method of giving the shock by means of a plate of air; and other observations have, likewise, been occasionally mentioned, in places where their connection required them to be inserted. This section, however, will contain several experiments of a miscellaneous nature, which are well worth notice.

IMMEDIATELY upon the discovery of the shock given by glass, all electricians attempted to charge other electric substances, but none of them succeeded before Signior *Beccaria*. He found that a very smooth plate of sealing-wax, made by pouring that substance, when

when melted, upon an oiled marble table, would receive a considerable charge*.

AFTER trying several other electrics, in the same manner, he found that a mixture of pitch and colophonia was charged less than sealing-wax, but more than sulphur, and a great deal more than pitch alone†.

BUT the most curious experiment of this philosopher, relating to this subject, was made with a view to ascertain the real direction of the electric fluid in a discharge. He suspended a coated plate of glass by a silk thread, and having charged it, and kept it perfectly still; he observed that no motion was given to it, when the discharge was made by a crooked wire approaching both the sides at the same time. The experiment, in fact, proved the re-action of the glass upon the electric matter; whereby the plate were kept still, notwithstanding the fluid rushed with great violence from one side to the other. He compares the glass to an ivory ball placed between two others, which keeps its place, when, by an impulse given to one of them, the opposite ball flies off‡.

MR. HARTMAN of Hanover has published an account of a curious experiment, which seems to show the progressive motion of the electrical explosion. He passes a shock through a great number of cannon balls, sometimes to the number of forty, placed near one another, upon small drinking-glasses;

* Lettere dell' elettricismo, p. 64.

† Ibid. p. 66.

‡ Ibid. p. 72.

when all the sparks are seen at the same moment of time, and all the snappings make but one report. But when he substitutes eggs instead of the balls of metal, the progress of the explosion is visible, every two giving a snap and a flash separately. This experiment requires weather very favourable to electricity, and, he says has generally succeeded the best with only ten or twelve eggs *. This author has not expressly said in which direction the sparks ran; but as he adopts Dr. Franklin's hypothesis, it may be presumed, that he imagined them to go from the positive to the negative side of the charged glass.

A VERY ingenious experiment has also been made by Mr. Amadeus Lullin of Geneva, in order to ascertain the direction of the electric fluid in explosions, and which he thinks comes nearer to the *experimentum crucis* than any other. He placed a common card in the circuit of the electrical explosion, while the wire which communicated with the positive side of the jar lay on one side of it, and that which communicated with the negative side lay on the other, their extremities not being placed opposite to one another, but at some distance. Things being thus circumstanced, he observed, that upon making the discharge, the card was constantly pierced close to the extremity of the wire which communicated with the negative side of the jar,

* Abhandlung, p. 3, &c.

as if the electric fluid, rushing from the positive side of the jar into the wire which communicated with it, and issuing from the extremity of it, was driven by its own impulse along the surface of the card, and did not pierce it, till it came opposite to the extremity of the other wire, which communicated with the negative side, by which it was strongly attracted*.

A VERY curious and elegant experiment on the Leyden phial was made by professor Richman of Peterburgh, whose unfortunate death will be related in this history.

HE coated both sides of a pane of glass, within two or three inches of the edge, and fastened linen threads to the upper part of the coating, on both sides; which, when the plate was not charged, hung down in contact with the coating; but setting the plate upright, and charging it, he observed, that when neither of the sides was touched by his finger, or any other conductor communicating with the earth, both the threads were repelled from the coating, and stood at an equal distance from it; but when he brought his finger, or any other conductor, to one of the sides, the thread hanging to that side fell nearer to the coating, while the thread on the opposite side receded as much; and that when his finger was brought into contact with one of the sides, the thread on that side fell into contact with it likewise, while the

* Dissertatio Physica, p. 24.

thread on the opposite side receded to twice the distance at which it hung originally; so that the two threads always hung so as to make the same angle with one another *.

ÆPINUS shows, that it is not strictly true, that an insulated person, discharging the Leyden phial through his own body, contracts no electricity. Electrifying a large plate of air, he observed, that if the nearer plate, (by which I suppose he means that which he first touched) was electrified positively, he acquired a positive electricity by the discharge; but if it were negative, he acquired a negative electricity. He supposes that the reason why the experiment did not succeed with Dr. Franklin, was, that the surfaces with which he 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 it necessarily must be in charging of glass †.

MR. CIGNA has invented a new method of charging a phial, upon the principle discovered by Mr. Canton and Mr. Wilcke, viz. that the electricity of one body repels that of another, especially if it have a flat surface, and gives it the contrary electricity.

HE insulates a smooth plate of lead, and while he brings an electrified body, as a stocking, to it, he takes a spark with the wire of a phial from the opposite side; and removing the stocking, he takes another spark with

* Æpini Tentamen, p. 335.

† Ibid. p. 27.

his finger, or any conductor communicating with the ground. Bringing the stocking nearer the plate a second time, he takes a second spark, with the wire of the phial, as before; and, removing it again, takes another, in the same manner, with his finger. This operation he continues, till the phial is charged; which, in favourable weather, may be done with very little diminution of the electricity of the stocking *.

If, instead of taking a second spark with his finger, he had taken it with the wire of another phial, that would have been charged likewise, with no additional labour, and with an electricity opposite to that of the other phial. If the second spark were taken with the coating of the same phial, the charging would be accelerated, but the operation would be troublesome to manage.

The theory of this new method of charging a phial is very easy, upon the principle referred to above. The approach of the electricity in the stocking, not being able to enter the broad smooth surface of the metal, drives the electric fluid out of that part of the plate which is opposite to it, to the other side, which, being thereby overcharged, will part with its superfluity to the wire of the phial. The stocking being taken away, the plate will have less than its natural share of the electric fluid, and will therefore readily take

* Memoirs of the Academy at Turin for the year 1765, P. 49.

a spark, either from the finger, or the wire of another phial *.

THE same ingenious philosopher makes a considerable difference between the electric fluid which gives the shock, and that on which some other phenomena of coated glass depend. The former, which is far the greatest quantity, he supposes to reside, either in the coating itself, or on the surface of the glass; whereas the other, he imagines, to have entered the pores, and affected the substance of the glass itself.

HE laid two plates of glass, well dried, one upon the other, as one piece; the lower of them being coated on the outside; and, when they were insulated, he alternately rubbed the uppermost plate with one hand, and took a spark from the coating of the lower with the other, till they were charged; when the coating, and both the plates adhered firmly together. Giving a coating to the other side, and making a communication between that and the other coating, the usual explosion was made. But the plates, though thus discharged, still cohered; and though, while they were in this state, they showed no other sign of electricity; yet, when they were separated, they were each of them found to be possessed of an electricity opposite to that of the other.

IF the two plates were separated before they were discharged, and the coating of

* Memoirs of the Academy at Turin for the year 1765, p. 51.

each were touched, a spark came from each; and when put together, they would cohere as before, but were incapacitated for giving a shock *.

He, therefore, compares the electricity which gives the shock to the electricity of the metal plate in the former experiment; which is lost with taking one spark, as the silk is removed from it, and is different from the electricity by which the two plates of glass cohere. The one is dispersed at once, but the other slowly; the one existing, as he supposes, in the conductors, or upon the surfaces of the electrics, and the other in the substance itself †.

AMONG experiments relating to the electric shock, we ought to mention what has been observed within this period of its amazing force in melting wires, and producing other surprising effects.

THAT even artificial electricity, says Dr. Watson, in a paper read at the Royal Society June 28th, 1764, when in too great a quantity, and hurried on too fast, through a fine iron wire, has a remarkable effect upon the wire, appears from a very curious experiment of Mr. Kinnerfley. This gentleman, in the presence of Dr. Franklin, made a large case of bottles explode at once through a fine iron wire. The wire at first appeared red hot, and then fell into drops, which burned

* Memoirs of the Academy at Turin for the year 1765, p. 55.

† Ibid. p. 56.

themselves into the surface of his table or floor. These drops cooled in a spherical figure like very small shot, of which Dr. Franklin transmitted some to Mr. Canton, who repeated the experiment. This proves the fusion to have been very complete; as nothing less than the most perfect fluidity could give this figure to melted iron.

MR. CANTON, in a note subscribed to the same paper, observes, that the diameter of a piece of Mr. Kinnerley's wire, which he received from Dr. Franklin, was one part in 182 of an inch. He adds, that artificial lightning, from a case of thirty-five bottles, would entirely destroy brass wire, of one part in 330 of an inch. At the time of the stroke, he says, a great number of sparks, like those from a flint and steel, would fly upwards, and laterally, from the place where the wire was laid, and lose their light, in the day-time, at the distance of about two or three inches. After the explosion, a mark appeared on the table, the whole length of the wire, and some very round particles of brass were discovered by a magnifier near the mark, but no part of the wire itself could be found *.

SIGNIOR BECCARIA was able to melt small slips of metal, without inclosing, or covering them with pieces of glass. But he thought that the same colour was impressed upon glass by all the metals; and imagined that this

*Phil. Transf. Vol. liv. p. 208.

circumstance was a trace of the fundamental principles being the same in them all*.

MR. DALIBARD observed, that, when a large pane of glass discharged itself, the polish was taken off at the place of the discharge, and that the track it left behind it was usually, as he expresses it, in the *zig zag* form. With the piece of glass with which he made these discharges he pierced 160 leaves of paper. It contained 1200 square inches†.

MR. WILCKE says, that, if a small piece of metal be hung in a small silken string, opposite to a part of a glass jar, made thin for the purpose; upon the spontaneous discharge through that place, the piece of metal will be driven off to the distance of five or six inches‡.

MR. WINKLER fired the seeds of clubmoss (*lycopodium*) by discharging a phial through a quantity of them. He also fired the *aurum fulminans* placed upon a piece of parchment, which was torn to pieces by the explosion§.

By the electric shock, Signior Beccaria could melt borax and glass. But the most remarkable of his experiments with the electric shock are those by which he *revivified metals*. This he did by making the explosion between two pieces of the calces. In this manner he

*E lettricismo artificiale, &c. p. 134, 135.

† Histoire abrégée, p. 84.

‡ Remarks on Franklin's Letters, p. 266.

§ Phil. Trans. Vol. xxxviii. pt. ii. p. 773.

revivified several of the metals, and among others, zink. He even produced real quicksilver from cinnabar*. In this case of revivification, he always observed streaks of black beyond the coloured metallic stains, owing, as he imagined, to the phlogiston driven thither from the parts that were vitrified, when the other part revivified the calx †.

ANOTHER curious experiment he made with the electric shock, by discharging it through some brass dust, sprinkled between two plates of sealing-wax. The whole was perfectly luminous and transparent ‡. An experiment which throws some light upon one of Mr. Hauksbee's.

WITH the electric shock he also made that capital experiment, on which he lays so much stress in his theory of thunder storms, and by which he proves, that the electric matter forces into its passage all light conducting substances; by means of which it is enabled to pass through a quantity of resisting medium, which it could not otherwise do. He put a narrow piece of leaf silver between two plates of wax, laying it across the plates, but so as not quite to reach one of the sides. The discharge being made through this strip of metal, by bringing a wire opposite to the silver, at the place where it was discontinued; the silver was found melted, and part of it dispersed all along the track that the electric matter took, between the plates

* Lettere dell' elettricismo, p. 282. † Ibid. p. 255.

‡ Ibid. p. 257.

of wax, from the silver to the wire*. An accident gave him occasion to observe another fact of a similar nature. He once, inadvertently, received the charge of a small jar, through some smoke of spirit of nitre; when a hole was made in his thumb, where the fire entered; and which he thought could only have been made by the nitre, which was carried along with the electric fluid†.

VIOLENTLY as the electric explosion generally affects the human body, we have accounts of some persons who could not be made to feel it; particularly three or four mentioned by Mr. Muschenbroeck, among whom was a young woman‡. I have also been told, that this was the case with a person near Leeds, who was at the same time a little paralytic.

I SHALL close the history of the Leyden phial for this period with the accounts of some extremely curious facts, which Mr. Canton gives me leave to publish relating to this subject. They certainly deserve the utmost attention of philosophers, and may probably throw some light upon the electricity of the tourmalin.

HE procured some thin glass balls, of about an inch and a half in diameter, with stems or tubes of eight or nine inches in length, and electrified them, some positively on the inside, and others negatively, after the manner of charging the Leyden phial,

* Lettere dell' elettricismo, p. 248.

† Ibid. p. 249.

‡ Monthly Review, October 1767, p. 250.

and then sealed them hermetically. Soon after, he applied the naked balls to his electrometer, and could not discover the least sign of their being electrical: but holding them to the fire, at the distance of five or six inches, they became strongly electrical, in a very short time, and more so when they were cooling. These balls would, every time they were heated, give the electric fire to, or take it from other bodies, according to the *plus* or *minus* state of it within them. Heating them frequently, he found, would sensibly diminish their power; but keeping one of them under water a week did not appear in the least to impair it. That which he kept under water was charged the 22d of September 1760, was several times heated before it was kept in water, and had been heated frequently afterwards; and yet it still retained its virtue to a considerable degree, on the 31st of October following, when he sent an account of it to Dr. Franklin. The breaking two of his balls accidentally gave him an opportunity of measuring their thickness, which he found to be between seven and eight parts in 1000 of an inch.

THE balls mention in the account above, which was written six years ago, still* retain their virtue, but in a less degree.

MR. LULLIN also found, that a glass tube charged, and hermetically sealed, would show signs of electricity when it was heated†.

* In 1769.

† *Dissertatio Physica*, p. 32.

S E C T I O N VIII.

EXPERIMENTS AND OBSERVATIONS CONCERNING ELECTRIC LIGHT.

MY reader has been informed of the necessity I was under of dividing the business of this period of my history into several parts. He has already seen titles which he could not have expected from the divisions of the preceding periods, but he would perhaps least of all expect a distinct section upon electric light; and yet the experiments and observations which have been made, immediately relating to this subject, are so many, that they deserve a place by themselves. And I would rather err by making too many subdivisions, than too few; because, above all things, I would wish to preserve perspicuity, which is chiefly injured by crowding together things dissimilar.

MANY experiments had been made very early, by Mr. Hauksbee and others, on electricity, and particularly electric light, in vacuo; but so little was, at that time, known of the nature of electricity in general, that, comparatively, little use could be made of those experiments. Very fortunately, Dr. Watson happened to turn his thoughts that way, after the great discovery of the accumulation of electricity in the Leyden phial; and by this means he discovered, that our atmosphere, when

when dry, is the agent by which, with the assistance of other electrics per se, we were enabled to accumulate electricity upon non-electrics (he might have added electrics too), that is, to communicate to them a greater quantity of electricity than those bodies naturally have. That upon the removal of the air, the electric fluid pervaded the vacuum to a considerable distance, and manifested its effects upon any non-electric substances by which it was terminated.

THIS he demonstrated by one of the most beautiful experiments which the whole compass of electricity yet exhibits. He exhausted a glass cylinder, three feet in length, and three inches in diameter, with a contrivance to let down a brass plate, as far as he pleased, into it; in order to make it approach another plate, fixed near the bottom of the vessel.

THIS cylinder, thus prepared, he insulated, and observed, that when the upper plate was electrified, the electric matter would pass from one plate to another, at the greatest distance to which the brass plates could be drawn; and that the brass plate at the bottom of the cylinder was strongly electrified, as if a wire had connected it with the prime conductor. It was a most delightful spectacle, he says, when the room was darkened, to see the electric matter in its passage through this vacuum; to observe, not as in the open air, small brushes or pencils of rays, an inch or two in length, but coruscations of the whole length
of

of the tube, and of a bright silver hue. These did not immediately diverge, as in the open air, but frequently, from a base apparently flat, divided themselves into less and less ramifications, and resembled very much the most lively coruscations of the aurora borealis.

SOMETIMES he observed, that when the tube had been exhausted in the most perfect manner, the electric fluid was seen to pass between the brass plates in one continued stream, of the same dimensions throughout its whole length; which he thought demonstrated, that the cause of that very powerful mutual repulsion of the particles of electric fire, which is seen in the open air, is more owing to the resistance of the air, than to any natural tendency of the electricity itself. For, in the open air we observe that these brushes, when the electricity is strong, diverge so much, as to form almost a spherical figure*.

He made this vacuum part of a circuit necessary to make the discharge of a phial; and, at the instant of the explosion, there was seen a mass of very bright embodied fire, jumping from one of the brass plates in the tube to the other. But this did not take place when one of the plates was farther distant from the other than ten inches. If the distance was greater, the fire began to diverge, and lose part of its force: and this force diminished in proportion to its divergency,

* Phil. Transf. Vol. xlvii. p. 367.

which was nearly as the distance of the two plates.

To find a more perfect vacuum for the passage of the electric fluid, he had recourse to an excellent invention of Lord Charles Cavendish; who, by means of a long bent tube of glass, filled with mercury, and inverted, made all the bended part of it (which was above the mercury) the most perfect vacuum that can be made. This vacuum Dr. Watson insulated, and one of the basons of the mercury being made to communicate with the conductor, when some non-electric substance touched the other, the electric matter pervaded the vacuum in a continued arch of lambent flame, and, as far as the eye could follow it, without the least divergency.

CONNECTING one of the basons with the machine, which was insulated, the fire was seen pervading the vacuum in a contrary direction. And this he considered as the *experimentum crucis* of two principles which he had advanced before, viz. that electricity is furnished to the conductor, not by the excited electric, but from the non-electrics in contact with the rubber; and that we are able to take from, or add to that quantity of electricity, which is naturally inherent in bodies.

He also observed, that if, in the fore-mentioned circumstances, the hand of a person standing upon the floor was brought near the side of the glass, the coruscations would dart themselves that way in a great variety of forms, extremely curious to behold.

BUT

BUT the Doctor found, that even this vacuum did not conduct so perfectly as metals, or water ; because a person standing upon the floor, and applying his finger to the upper brass plate, received a smart stroke. This he conceived to arise from the electricity of the brass being so much more rarefied than that of the body of the man who applied his finger *.

MR. WILSON engaged Mr. Smeaton, the inventor of a new and more perfect kind of air-pump, to make some electrical experiments in vacuo. The following is the account of them that he transmitted to Mr. Wilson. They are, in several respects, similar to those made by Dr. Watson, and yet are attended with a considerable variety of new circumstances.

A GLASS vessel, about one foot in length, and eight inches in its greatest diameter, open at both ends, had one of its ends closed by a brass ferule, which constituted one of the centers on which it turned ; the other end was closed with a metal plate. In the center of this plate was a square stem, which was applied to the arbor of a lath, by which the glass was turned round. On one side of this last plate was fixed a cork, by means of which the glass was screwed upon the air-pump.

UPON rarefying the air within the glass about 500 times, and afterwards turning the

* Phil. Transf. Vol. xlvii. p. 373.

glafs in the lath, whilft, at the fame time, it was rubbed with his hand; a confiderable quantity of lambent flame, variegated with all the colours of the rainbow, appeared within the glafs, under the hand. This light was pretty fteady in every refpect, except that every part of it was perpetually changing colour.

WHEN a little air was let into the glafs, the light appeared more vivid, and in a greater quantity, but was not fo fteady: for it would frequently break out into a kind of corufcations, like lightning, and fly all about within the glafs. When a little more air was let in, the flafhing was continual, and ftreams of blueifh light feemed to iffue from under his hand, within the glafs, in a thoufand forms, with great rapidity; and appeared like a cascade of fire. Sometimes it feemed to fhoot out into the forms of trees, mofs, &c.

WHEN more air was let in, the quantity of light was diminifhed, and the ftreams compofing the flafhes narrower. The glafs now required a greater velocity, and harder friction. Thefe circumftances increafed as more air was let in; fo that, by fuch time as the glafs was one third full of air, thefe corufcations quite vanifhed, and a much fmall quantity of light appeared partly within, and partly without the glafs. And when all the air was let in, the light appeared wholly without the glafs, and much lefs in quantity than when the glafs was in part exhaufted*.

* Wilfon's Eflay, p. 216.

MR. CANTON, in repeating Dr. Watson's experiment with the Torricellian vacuum, observed one circumstance attending it, which throws great light upon the Leyden phial. He observed, that when the excited tube was brought near one of the basons of this machine (insulated) a light was seen through more than half of the vacuum; which soon vanished, if the tube was not brought nearer, but which appeared again as it moved farther off; and that this appearance might be repeated several times, without exciting the tube afresh.

THIS experiment he considered as a kind of ocular demonstration of the truth of Dr. Franklin's hypothesis, that when the electric fluid was condensed on one side of the glass, it was repelled from the other, if it met with no resistance. Thus, at the approach of the excited tube, he supposed the fire to be repelled from the inside of the glass surrounding the vacuum, and to be carried off through the columns of mercury, but to return again as the tube was withdrawn *.

THIS curious experiment Mr. Canton, as he informed me, showed and explained to Mr. Wilson; who afterwards expatiated upon it, in a book published by him and Dr. Hoadley in conjunction, intitled *Observations*

* Phil. Transf. Vol. xlviii. pt. i. p. 356. It has been seen p. 95, that this observation, of the return of the electric light *in vacuo*, was made before the discovery of the Leyden phial by Mr. Grummet of Biala in Poland; but this was unknown both to Dr. Watson and Mr. Canton.

on a Series of Electrical Experiments; in a note of which, p. 28, he says, "Mr. Canton has taken notice of this vanishing and returning of the light."

MR. CANTON has since diversified this beautiful experiment, by bringing the excited tube to another glass tube, exhausted, and hermetically sealed; by which means he exhibits the perfect appearance of an aurora borealis. The flame from one of its extremities, which is in a manner coated by the hand which holds it, will dart to the other extremity, at uncertain intervals of time, for near a quarter of an hour together, without repeating the application of the excited tube.

WHEN it was generally agreed among electricians, that what had been called vitreous and resinous electricity were in reality a redundancy of the electric fluid in one case, and a deficiency in the other; and when, in consequence of this supposition, the one was called positive, and the other negative electricity; there still remained some doubt which of the two was positive, and which negative. Mr. Wilson, in a paper read at the Royal Society, December the 6th, 1759, recites an experiment which, he thought, put the matter beyond all dispute, and absolutely determined, that what had been called vitreous was really positive, and what had been called resinous was negative; as, indeed, had generally been supposed, though, as Mr. Wilson thought, without sufficient reason, notwithstanding what had been advanced by Dr. Franklin,

Franklin, and Mr. Canton upon that subject.

REPEATING the beautiful experiment mentioned before, as first contrived by Lord Charles Cavendish, he says he attended to a circumstance which seemed to have been overlooked by Dr. Watson, who published the account of it. This was a singular appearance of light upon one of the surfaces of the quicksilver. To observe this remarkable appearance to more advantage, Mr. Wilson let a small quantity of air into the tube, by which means four columns of quicksilver were obtained, and consequently six visible surfaces, in one of the legs of the inverted tube. He then electrified the mercury in the other leg, while the mercury on the opposite side had a communication with the earth, when, the room being dark, the stream of electric light was visible through the whole length of the vacuum, and its general appearance was of a seeming uniform density; except at the upper surfaces of each column, where about one tenth of an inch above the surface, the light was always considerably brighter; whereas the under surfaces exhibited no such appearance, the light being rather less bright in those places than in the general appearance of the whole illuminated vacuum.

THIS luminous appearance Mr. Wilson ascribed to the resistance the fluid met with at the upper surface of the quicksilver, in endeavouring to get into it. He therefore inferred that excited glass electrified bodies po-

sively, or gave them a greater quantity of the electric fluid than they had.

ELECTRIFYING, in the same place, with a cylinder of rosin, instead of glass, the luminous appearances were all on the under surfaces of the columns of quicksilver; from which he inferred, that rosin electrified bodies negatively, depriving them of part of the electric fluid which they naturally had; or, as he expresses it, occasioning a current of electric fluid to set the contrary way.

THESE luminous appearances, Mr. Wilson also considered, as a strong confirmation of the existence of a *medium*, at or near the surfaces of bodies, which hindered the entrance or exit of the electric fluid. A doctrine which Mr. Wilson had advanced, and laid great stress upon on several other occasions*.

THE arguments which to Mr. Wilson appeared conclusive, in proof of what is commonly supposed, that glass electrifies *plus*, and sulphur, &c. *minus*, did not appear so to Mr. Æpinus; though he acknowledges that the knobs of light in the vacuum did, in common with many other appearances, prove a real difference between the two electricities; and thought that it was very easy to conceive, that when an elastic fluid issues from a body, it should be denser near the surface from whence it issues, than where it finds more liberty to expand itself. He might have added, that this might have been expected, from

* Phil. Transf. Vol. li. pt. i. p. 308.

the mutual attraction which is supposed to subsist between the electric fluid, and other bodies. But Æpinus did not expressly mention this circumstance. Mr. Wilson, therefore, makes light of the objection; and adds, that when he related the experiment with the bent tube, in his letter to Dr. Heberden, he omitted some phenomena attending the fact, which greatly favoured the doctrine he advanced. If, says he, when glass is electrified, and applied to the first column, we suffer the electric fluid to pass along the tube in small quantities only, and at short intervals, little luminous streams will be seen to move from the first to the second column of quicksilver, and consequently from the glass. The like appearances happen, but in a contrary direction, when rosin or amber is made use of, and applied to the same column. Glass, therefore, he concludes, electrifies *plus*, or fills bodies with more of this fluid than belongs to them naturally, and rosin, &c. *minus* *.

This controversy took its rise, in some measure, from a deception: for Mr. Canton informs me, and gives me leave to inform the public, that the light which Mr. Wilson takes notice of, as appearing on one surface of the mercury in the double barometer of Lord Charles Cavendish, and which Mr. Wilson takes to be a proof of the existence of a medium on the surface of bodies, which hinders the entrance or exit of the electric fluid to

* Phil. Trans. Vol. liii. p. 438. p. 441.

some degree, he found to be caused by nothing but common air. For if the Torricellian vacuum be properly made, no difference of light can be seen on the surfaces of the columns of mercury; but if as much air be let into the vacuum, as will make each column of mercury a quarter of an inch shorter than that of a good barometer, the light will appear as Mr. Wilson has described it. When Mr. Wilson supposed that Dr. Watson, when he made the experiment of the Torricellian vacuum, did not attend to the singular appearance of light on one of the surfaces of the mercury, he little suspected that if the vacuum Dr. Watson made was free from air, there was no such singular appearance of light to be attended to. Air, Mr. Canton adds, must be condensed near the surface of all bodies that attract it; and will, therefore, be some hindrance to the exit, or entrance of the electric fluid, except the bodies be very sharply pointed.

SOME curious observations relating to electric light were made by Mr. Wilcke. Rubbing two pieces of glass together in the dark, he observed a vivid phosphoreal light: which, however, threw out no rays, but adhered to the place where it was excited. It was attended with a strong phosphoreal smell, but with no attraction or repulsion. From this experiment he inferred, that friction alone would not excite electricity, so as to be accumulated upon any body; and that to produce this effect, the bodies rubbed together must

must be of different natures, with respect to their attracting the electric fluid. He, moreover, imagined, that all examples of phosphoreal light, without attraction, were owing to the same excitation of electricity, without the accumulation of it. Such he imagined to be the case of light emitted by the Bolognian stone, cadmea fornacum, rotten wood, pounded sugar, and glass of all kinds *.

A TUBE excited with a woollen cloth, on which white wax or oil had been put, he says, threw out flames; each of which when examined, appeared to rise out of a little protuberance of fire. The flame was one, and very narrow at the bottom, but farther from the tube it divided into several ramifications; which always leaned to those parts of the tube which were the least excited, or to conductors in the neighbourhood †.

He says that, upon presenting a finger or other non-electric to an excited negative electric, a cone of light is formed; the base of which is at the finger, or other non-electric, and the apex at the electric, on the surface of which it spreads to a considerable distance all round ‡.

SOMETIMES, he had seen fiery particles thrown laterally from an irregular electric spark, which shone like stars, and were very like those which are produced by the collision of flint and steel §.

* Wilcke, p. 123, 124.

† Ibid. p. 125.

‡ Ibid. p. 127.

§ Ibid. p. 130.

SUSPENDING various balls from his conductor, and presenting others to them, which were sometimes of glass and sometimes of metal, and varying them in every manner possible, he always found (except when two metal balls were used) that the light between them formed a cone, the base of which was always on the body which was positive, and the apex on that which was negative. He says that this criterion is sufficient to distinguish the two electricities from one another.

HE observes that, at the apex of a cone issuing from pointed bodies, electrified positively, there is a cylindrical spark, out of which lucid rays, like a river, are darted. These rays, he says, form a lucid cone, the apex of which is turned towards the point from which the fire proceeds. Sometimes from the apex, or at some distance from it, there is a lucid point, which, he says, Haufenius calls *the fire of the second kind*, out of which flew streams of fire. The streams never issue from the electrified body itself, but always from this lucid point. He says, moreover, that this lucid point at the extremity of an electrified body, and which throws out lucid rays, forms the distinctive character of the positive cone *.

A NEGATIVE cone, he says, is small, consisting of very slender filaments, which immediately adhere to the point at which the light

* Wilcke, p. 132.

enters, or to its sides; and, if accurately examined, seems to form little cones, the bases of which rest upon the body.

WHEN he afterwards comes to consider the cause of negative cones of light, he owns himself to be at a great loss how to do it.

MR. WILCKE put English phosphorus upon a pointed body, which, in the dark, rendered the whole visible; and when he suspended this pointed body perpendicularly, the phosphoreal vapours were seen to ascend; but upon electrifying it, as it hung in the same direction, the vapours were carried downwards, and formed a very long cone, extending out of the middle of the cone of electric light, which was seen perfectly distinct from it. When the electrification was discontinued, the phosphoreal vapour ascended as at first. From this depression of the phosphoreal effluvia Mr. Wilcke infers the efflux of the electric fluid from the point, and upon the surface, and not only through the substance of the pointed body. It is pity that he did not try this curious experiment with pointed bodies electrified negatively. He would certainly have found the same depression of the phosphoreal effluvia, and would, probably, have retracted his conclusion concerning this proof of the efflux*.

MR. WILCKE also thought it to be a proof, that the electric matter did not only flow out of the substance of electrified bodies, but up-

* Wilcke, p. 134.

on the surface of them, that a metallic ring, projecting ever so little beyond the point of a wire on which it had been put, prevents the appearance of the lucid point.

THE last observation which I shall recite of Mr. Wilcke concerning electric light is, that if a point not electrified be opposed to a point electrified positively, the cones of light, which, in other circumstances, would appear upon both of them, disappear; but that if a positive cone be opposed to a negative cone, they both preserve their own characteristic properties*.

SIGNIOR BECCARIA was of opinion, that the direction of the electric fluid may be determined from the phenomena of pointed bodies. The *pencil* (by which he means the electric fire at a point electrified positively) he says, contracts as it approaches a flat piece of metal not electrified; whereas the *star* (by which he means the electric fire at a point electrified negatively) expands in the same circumstances, and has a small cavity near the point towards the large superficies. The pencil is attended with a snapping noise, the star makes little or no noise. He hardly gives any reason for the first of these phenomena; he only says, that such is the necessary consequence of a fluid issuing out of, or entering into a point. But the greater noise made by the pencil, he thought was made by the impulse given by the electric matter to the air,

* Wilcke, p. 140.

causing it to vibrate: and this must be greater when the fluid is thrown from the point into the air, than when it comes through different portions of the air, and meets in one point*.

WHEN two points are opposed to one another, he says, the phenomena are much the same in both†.

SIGNIOR BECCARIA observed that hollow glass vessels, of a certain thinness, exhausted of air, gave a light when they were broken in the dark. By a beautiful train of experiments, he found, at length, that the luminous appearance was not occasioned by the breaking of the glass, but by the dashing of the external air against the inside, when it was broke. He covered one of these exhausted vessels with a receiver, and letting the air suddenly on the outside of it, observed the very same light. This he calls his *new invented phosphorus*‡.

THIS excellent philosopher produced a most beautiful appearance of electric light in the following manner. He conveyed positive electricity to a brass ball, suspended by a wire, within an exhausted receiver, when, upon presenting to it another ball *in vacuo*, the lower hemisphere of the former was most beautifully illuminated, with a visible electric atmosphere. When he conveyed negative electricity to the ball, the same beautiful illumination was observed on the ball presented to it. This experiment, he says, is a very deli-

* Elettricismo artificiale, p. 63.

† Ibid.

‡ Lettere dell' elettricismo, p. 365, &c.

cate one, requiring great patience and dexterity, in adjusting the distances, &c. in order to make it succeed perfectly*.

THAT electric light is more subtle and penetrating, if one may say so, than light produced in any other way is manifest from several experiments, particularly the remarkable one of Mr. Hauksbee; but none prove it so clearly as some made by the ingenious Mr. Lane, who gives me leave to mention these.

WHEN he had, for some different purpose, made the electric shock pass over the surface of a piece of marble, in the dark; he observed, that the part over which the fire had passed was luminous, and retained that appearance for some time. No such effect of the electric shock having ever been observed before, he repeated the experiment with a great variety of circumstances, and found it always answered with all calcareous substances, whether animal or mineral, and especially if they had been burnt into lime. And, as far as he had tried, many more substances would retain this light than would not do it; among others several vegetable substances would do it, particularly white paper. Tiles and brick were luminous, but not tobacco-pipe clay, though well burnt.

THAT gypseous substances, when calcined, were luminous, appeared from bits of

* Phil. Transf. Vol. lvi. p. 107.

images made of plaister of Paris; and of this class, he says, is the famous Bolognian stone. But many bodies, he found, were luminous after the electric stroke, which were not apparently so, when exposed to the rays of the sun.

HE made these curious experiments by placing the chains, or wires that led from the conductor to the outward coating of his jar, within one, two, or three inches (according to the strength of the charge) from one another, on the surface of the body to be tried, and discharging a shock through them. If the stone was thin, he found, that if one chain was placed at the top, and the other at the bottom, it would appear luminous on both sides after the explosion.

MR. CANTON, to whom these experiments were communicated, clearly proved, that it was the *light* only that the substances retained, and nothing peculiar to electricity; and, moreover, after frequent trials, discovered a composition, which retains both common light, and that of electricity, much more strongly than either the Bolognian stone, or any other known substance whatever. With this new phosphorus he makes a great number of most beautiful experiments. The flash made by the discharge of a common jar, within an inch of a circular piece of it, of about two inches and a half in diameter, will illuminate it so much, that the figures on a watch plate may be easily distinguished by it
in

in a darkened room, and it will retain the light half an hour *.

I SHALL close this section of experiments and observations on electric light, with an account of a remarkable appearance which occurred to Mr. Hartman. When he had been making experiments four or five hours together, in a small room, and after going out of it, returned soon, with a lighted candle in his hand, walking pretty swiftly; he perceived a small flame to follow him, at the distance of about a step and a half, but it vanished when he stopped to examine it. He was a good deal alarmed at the appearance at first, but afterwards imagined it to be occasioned by the ascension of the sulphur, which had been thrown into the air by the violent continued electrification †.

* See a particular account of this composition, and experiments with it, in my *History of Discoveries relating to Vision, Light, and Colours*, p. 371.

† Abhandlung, p. 135.

S E C T I O N IX.

THE ELECTRICITY OF THE TOURMALIN.

THIS period of my history furnishes an entirely new subject of electrical inquiries; which, if properly pursued, may throw great light upon the most general properties of electricity. This is the *Tourmalin*: though, it must be acknowledged, the experiments which have hitherto been made upon this fossil stand like exceptions to all that was before known of the subject.

THE tourmalin, as Dr. Watson supposes, was known to the antients under the name of the *lyncurium*. All that Theophrastus says concerning the lyncurium agrees with the tourmalin, and with no other fossil that we are acquainted with. He says, that it was used for seals, that it was very hard, that it was endued with an attracting power like amber, and that it was said, particularly by Diocles, to attract not only straws, and small pieces of wood, but also copper and iron, if beaten very thin; that it was pellucid, of a deep red colour, and required no small labour to polish it. The account which passed current among the ancients concerning the origin of this stone was fabulous, which made Pliny think that all that was said of it was fabulous too.

THIS

THIS stone, though not much attended to by European philosophers, till very lately, is common in several parts of the East Indies, and more particularly in the island of Ceylon, where it is called by the natives *tournamal*. In this island the Dutch became acquainted with it, and by them it is called *aschentrikker*, from its property of attracting ashes, when it is thrown into the fire.

THE first account we have had, of late years, concerning this extraordinary stone is in the History of the Royal Academy of Sciences at Paris for the year 1717; where we are told, that Mr. Lemery exhibited a stone, which was not common, and came from Ceylon. This stone, he said, attracted and repelled small light bodies, such as ashes, filings of iron, bits of paper, &c.

LINNÆUS, in his *Flora Zeylonica*, mentions this stone under the name of *lapis electricus*, and takes notice of Mr. Lemery's experiments.

NOTWITHSTANDING this, no farther mention was made of this stone and its effects till some years after; when the Duc De Noya, in his letter to Mr. Buffon, presented to the Royal Society, informed us, that when he was at Naples, in the year 1743, the Count Pichetti, secretary to the king, assured him, that, during his stay at Constantinople, he had seen a small stone called *tourmalin*, which attracted and repelled ashes. This account the Duc De Noya had quite forgotten, but being
in

in Holland in the year 1758, he saw, and purchased two of those stones. With these, in company with Messrs. Daubenton and Adamson, he made a great number of experiments, of which he favoured the public with a particular account*.

BUT prior to the Duc De Noya's experiments, Mr. Lechman had acquainted Mr. *Æpinus* with the attractive power of the tourmalin, and furnished him with two of them, on which he made many experiments; the result of which he published in the History of the Academy of Sciences and Belles Lettres at Berlin for the year 1756. The substance of the memoir is as follows.

THE tourmalin has always, at the same time, a positive and a negative electricity; one of its sides being in one state, and the other in the opposite; and this does not depend on the external form of the stone. These electricities he could excite in the strongest degree, by plunging the stone in boiling-water.

IF one side of the tourmalin be heated more than the other (as if it be laid upon a hot cake of metal) each of the sides acquire an electricity opposite to that which is natural to it; but if left to itself, it will return to its natural state.

IF one of the sides of the tourmalin be rubbed, while the other is in contact with some conductor communicating with the ground;

* Phil. Transf. Vol. li. pt. i. p. 396.

the rubbed side is always positive, and the other negative. If neither side be in contact with a conductor, both become positive. If, in the former of these cases, the tourmalin be rubbed, so as to acquire a sensible heat, and the side which is naturally positive be made negative, it will upon standing to cool, return to its natural state; but if it have acquired no sensible heat, it will not return to its natural state while any kind of electricity remains. If it be heated, even when it is rubbed and insulated, (in which case both sides become positive) it will still return to its natural state upon cooling.

THE Duc De Noya mentions these experiments of Mr. Æpinus, but does not admit of a *plus* and *minus* electricity belonging to the tourmalin when heated. On the contrary, he says, that both the sides are electrified *plus*, but one of them more than the other, and that it was the difference between those degrees which led Mr. Æpinus into his mistake*.

THE tourmalin was introduced to the notice of the English philosophers by Dr. Heberden, who fortunately recollecting to have seen one of them, many years before, in the possession of Dr. Sharp at Cambridge (and it was the only one known in England at that time) procured it for Mr. Wilson; who, though it was but a small one, repeated with it most of the experiments of Mr. Æpinus, so far as to

* Phil. Transf. Vol. lviii. pt. i. p. 315.

satisfy himself that his opinion of its positive and negative power was well founded.

AFTERWARDS, Dr. Heberden, ever desirous of extending the bounds of science, procured some of these stones from Holland, and put them into the hands of those persons who were likely to make the best use of them, particularly Mr. Wilson and Mr. Canton; in whose hands they were not lodged in vain, as will appear by the brief account I shall subjoin of their experiments upon them.

MR. WILSON'S observations are too many, and too particular to be all inserted in this work. The result of them was, in the main, the same with that of Mr. *Æpinus*, establishing the opinion of the two different powers of this stone; but, contrary to Mr. *Æpinus*, he asserts, that when the sides of the tourmalin are unequally heated, it exhibits that species of electricity which is natural to the hotter side, that is, the tourmalin is *plus* on both sides, when the *plus* side is the hotter; and *minus* on both sides, when the *minus* side is the hotter.

UPON this Mr. *Æpinus* repeated all his former experiments, and still found the result of them agreeable to his former conclusion, and contrary to that of Mr. Wilson. Mr. Wilson also repeated his, without any variation in the event, and imagined the difference between him and Mr. *Æpinus* might arise from the different sizes of the tourmalins they made use of, or from their different manner of making the experiments. And it is evident, from

the description of both their apparatuses, that of Mr. Wilson was much better calculated for the purpose of accurate experiments than that of Mr. *Æpinus*. Mr. Wilson also used a greater variety of methods of communicating heat to his tourmalins. He both plunged them in boiling water, held them to the flame of a candle, and exposed them to heated insulated electrics *.

THOUGH the detail of all Mr. Wilson's experiments would be, as I observed before, much too long for my purpose, I cannot help relating one of them, which was made with the last mentioned method of treating. He heated one end of a glass tube red-hot, and when he had exposed what both he and Mr. *Æpinus* call the negative side of the tourmalin to it; he observed that about three inches of the heated part of the glass were electrified *minus*, though the glass beyond that was electrified *plus*, and continued so even after the glass was cold, the electric fluid having passed from the tourmalin to the glass; since these were the same appearances that were produced by presenting an excited tube to the heated glass.

HE then applied the *plus* side of the tourmalin to the same heated glass, and found that the tube was electrified *minus*, above a foot in length, without the least appearance of a *plus* electricity beyond the *minus* one, as in the other experiment; and this *minus* electricity

* Phil. Trans. Vol. liii. p. 436, &c.

appeared when the tube was nearly cold. In this case he judged that the electric fluid had passed from the glass to the tourmalin.

MR. WILSON imagined that the tourmalin, as well as glass, was permeable to the electric fluid, and that the resistance to its entering the substance of it was less on what he calls the negative than on the positive side. These conclusions he drew from the two following experiments. Rubbing the positive side of the stone slightly, he found both sides electrified *plus*, but rubbing the negative side in the same manner, both sides were electrified *plus* more strongly than before *.

SEVERAL experiments led Mr. Wilson to conclude that the tourmalin resisted the exit and entrance of the electric fluid considerably less than glass, or even than amber; and upon the whole he infers, that the tourmalin differs in nothing from other electric bodies but in acquiring electricity by heat †.

EXAMINING a great number of tourmalins, he found that a line drawn from the *plus* part, through the center of the stone, would always pass through the *minus* part.

ALMOST all these tourmalins he greased over, and whilst they were warm enough to preserve the grease liquid, he tried each tourmalin separately, but found no alteration in the virtue of the stone, except that it was a little weakened; though it is well known, he says, that moisture of any sort readily conducts

* Phil. Transf. Vol. li. pt. i. p. 327.

† Ibid. p. 329.

the electric fluid. If, therefore, the tourmalin had not a fixed kind of electricity, the *plus* and *minus* electricity, observable on the two sides of the stone, must, by this treatment, have united and destroyed each other. From this circumstance Mr. Wilson concluded, that the tourmalin suffered the electric fluid to pass through it only in one direction, and that in this it bore some analogy to the loadstone; having, as it were, two electric poles, which are not easily destroyed or altered*. But he was not aware how imperfect a conductor oil or grease of any kind is.

THIS induced him to try whether, like the loadstone, the tourmalin would lose its virtue after being made red-hot; but, though he kept two of them in a strong fire for half an hour, he could not perceive the least alteration in them: but plunging one in water, while it was red-hot entirely destroyed its virtue, and gave it the appearance of having been shivered in many parts without breaking†. He observes that when a tourmalin, which he had from Dr. Morton, was held between the eye and the light, and viewed in the direction through which the electric fluid is found to pass, it appears of a darker colour considerable than when it is viewed at right angles to the former direction. This appearance, he says, obtains in many other tourmalins, especially when they happen to be as conveniently shaped‡.

* Phil. Transf. Vol. li. pt. i. p. 337, 338.

† Ibid. p. 338

‡ Ibid. Vol. liii. p. 448.

NOTWITHSTANDING the attention given to this subject by Mr. *Æpinus* and Mr. *Wilson*, the most important discovery relating to the electricity of the tourmalin was reserved for Mr. *Canton*; who, in a paper read at the Royal Society in the same month with that above-mentioned of Mr. *Wilson*, viz. December 1759, observes, that the tourmalin emits and absorbs the electric fluid only by the increase or diminution of its heat. For if the tourmalin, he says, be placed on a plain piece of heated glass or metal, so that each side of it, by being perpendicular to the surface of the heated body, may be equally heated; it will, while heating, have the electricity of one of its sides positive, and that of the other negative. This will, likewise, be the case when it is taken out of boiling water and suffered to cool; but the side which was positive while it was heating will be negative while it is cooling, and the side which was negative will be positive.

IN this paper Mr. *Canton* refers to the *Gentleman's Magazine* for the month of September before, in which he had published the result of some experiments he had made on a tourmalin which he had procured from Holland. The propositions he there lays down are so few, and comprise the principal part of what is known upon this subject in so concise and elegant a manner, that I shall recite them all in this place.

1. WHEN the tourmalin is not electrical or attractive, heating it, without friction, will

make it so; and the electricity of one side of it (distinguished by A) will be positive, and that of the other side (B) will be negative.

2. THE tourmalin not being electrical will become so by cooling; but with this difference, that the side A will be negative, and the side B positive.

3. IF the tourmalin, in a non-electrical state, be heated, and suffered to cool again, without either of its sides being touched; A will be positive, and B negative, the whole time of the increase and decrease of its heat.

4. EITHER side of the tourmalin will be positive by friction, and both may be made so at the same time.

THESE, says he, are the principal laws of the electricity of this wonderful stone; and he adds, if air be supposed to be endued with similar properties, i. e. of becoming electrical by the increase or diminution of its heat (as is probable, if its state before and after a thunder storm be attended to) thunder clouds both positive and negative, as well as thunder gusts, may easily be account for.

THESE capital discoveries were made before Mr. Canton had received of Dr. Heberden the tourmalins above mentioned. When those came to his hands he was enabled to make several new and curious experiments, which I have leave to publish.

HE put one of the tourmalins, which was of the common colour into the flame from a blow-pipe and burnt it white; when he found
that

that its electrical property was intirely destroyed. The electricity of another was only in part destroyed by the fire. Two other tourmalins he joined together, when they were made soft by fire, without destroying their electrical property. The virtue of another was improved by its being melted at one end; and he found (contrary to what Mr. Wilson had observed of another tourmalin, which he treated in the same manner) that one tourmalin retained its electrical property after it had been frequently made red-hot, and, in that state, put into cold water.

BUT the most curious of his experiments were made upon a large irregular tourmalin, about half an inch in length, which he cut into three pieces; taking one part from the positive, and another from the negative end. Trying these pieces separately, he found the outer side of the piece which he cut from the end that was negative when cooling was likewise negative when cooling, and that the outer side of that piece which was cut from the end that was positive when cooling was likewise positive when cooling; the opposite sides of both pieces being, agreeable to the general law of the electricity of the tourmalin, in a contrary state.

THE middle part of the same tourmalin was affected just as it had been when it was intire; the positive end remained positive, and the negative end continued negative. The same he had also observed of two other tourmalins,

malins, each about the size of this, which were also cut out of a large one.

ON January the 8th, 1762, Mr. Canton took the Doctor's large tourmalin (which Mr. Wilson has given a description of in the fifty-first volume of the Philosophical Transactions, p. 316.) and having placed a small tin cup of boiling-water on one end of his electrometer, which was supported by warm glass, while the pith balls were suspended at the other end; he dropped it into the water, and observed, that during the whole time of its being heated, and also while it was cooling in the water, the balls were not at all electrified.

TILL the year 1760 it had been supposed, that, of all electric substances, the tourmalin alone possessed the property of being excited by heating and cooling; but in the beginning of that year, Mr. Canton having had an opportunity of examining a variety of gems, by the favour of Mr. Nicolas Crisp a jeweller in Bow Church-yard, first found the *Brazil topaz* to have the electrical properties of the tourmalin. The largest he met with he put into the hands of Dr. Heberden, who returned it November the 27th, 1760, and sent with it the tourmalins above mentioned.

IN September 1761, Mr. Wilson (who had been informed of Mr. Canton's discovery) met with several other gems, of different sizes and colours, that resembled the tourmalin with respect to electrical experiments. The most beautiful of them were something like the
ruby,

ruby, others were more pale, and one inclining to an orange colour. In point of hardness and lustre, they were nearly the same with the topaz.

FROM all his experiments upon these gems he thought it was abundantly evident, that the direction of the fluid did not depend upon the external figure of the gem, but upon some particular internal make and constitution of it. And that there is some such natural disposition in all gems affording these appearances may be collected, he says, from another curious specimen of the tourmalin kind, which is green, and formed of long slender crystals with severals sides; many of which are found sticking together, and are brought from South America.

THESE gems, numbers of which were furnished him by Mr. Emanuel Mendes Da Costa, he not only found to be like the tourmalin with respect to electric appearances; but that the direction of the electric fluid moving therein was always along the grain or shootings of the crystals, one end of it being electrified *plus*, and the other end *minus*. And that the fluid is more disposed to pass in that direction than in another, he thought, might be farther collected from what has been observed on the grain of the loadstone by Dr. Knight; who found that though the magnetic poles of the natural loadstone might be varied in any direction, yet that the same loadstone admitted of being made much more magnetical along the grain than across it.

FROM these experiments and observations Mr. Wilson inferred by analogy, that the electric fluid, flowing through all these stones and gems, moves in that direction in which the grain happens to lie; and that the reason of this is, that the resistance which the fluid meets with in passing through the gem is less in that direction than in any other *.

IN a subsequent paper of Mr. Wilson's, read at the Royal Society December the 23d, 1763, and March 1764, he recites several curious experiments on the effects of removing the tourmalin from one room to another, in which there was some difference of heat; the result of which exactly confirms Mr. Canton's discovery, that the side which is positive when heating is negative when cooling, and *vice versa*. Upon a very nice examination, and during some favourable circumstances, Mr. Wilson says he has observed the tourmalin to be feebly electrified, when the thermometer varied up and down only one degree †.

* Phil. Transf. Vol. lii. pt. ii. p. 443.

† Ibid. p. 457.

SECTION X.

DISCOVERIES THAT HAVE BEEN MADE SINCE
THOSE OF DR. FRANKLIN, WITH RE-
SPECT TO THE SAMENESS OF LIGHTNING
AND ELECTRICITY.

THE year 1752 makes an æra in electricity no less famous than the year 1746, in which the Leyden phial was discovered. In the year 1752, was verified the hypothesis of Dr. Franklin, of the identity of the matter of lightning and of the electric fluid; and his great project of trying the experiment by real lightning actually brought down from the clouds, was carried into execution.

THE French philosophers were the first to distinguish themselves upon this memorable occasion, and the most active persons in the scene were Mr. Dalibard and Delor, both zealous partisans (as Mr. Nollet calls them) of Dr. Franklin. The former prepared his apparatus at Marly La Ville, situated five or six leagues from Paris, the other at his own house, on some of the highest ground in that capital. Mr. Dalibard's machine consisted of an iron rod forty feet long, the lower extremity of which was brought into a centry box, where the rain could not come; while, on the outside, it was fastened to three wooden posts by long silken strings defended from the rain.

rian. This machine happened to be the first that was favoured with a visit from this ethereal fire. The philosopher himself happened not to be at home at that time ; but, in his absence, he entrusted the care of his apparatus to one Coiffier a joiner, a man who had served fourteen years among the dragoons, and on whose understanding and courage he could depend. This artisan had all proper instructions given him, both how to make observations, and also to guard himself from any harm there might be from them ; besides being expressly ordered to get some of his neighbours to be present, and particularly to send for the curate of Marly, whenever there should be any appearance of the approach of a thunder storm. At length the long expected event arrived.

ON Wednesday the 10th of May 1752, between two and three o'clock in the afternoon, Coiffier heard a pretty loud clap of thunder. Immediately he flies to the machine, takes a phial furnished with a brass wire, and presenting one end of the wire to the rod, he sees a small bright spark issue from it, and hears the snapping that it made. Taking a second spark stronger than the former, and attended with a louder report, he calls his neighbours, and sends for the curate. The curate runs with all his might, and the parishioners, seeing the precipitation of their spiritual guide, imagine that poor Coiffier had been killed with lightning. The alarm spreads through the village, and the hail which came on did not prevent

vent the flock from following their shepherd. The honest ecclesiastic arriving at the machine, and seeing there was no danger, took the wire into his own hand, and immediately drew several strong sparks, which were most evidently of an electrical nature, and completed the discovery for which the machine was erected.

THE thunder cloud was not more than a quarter of an hour in passing over the zenith of the machine, and there was no thunder heard besides that single clap. As soon as the storm was over, and no more sparks could be drawn from the bar, the curate wrote a letter to Mr. Dalibard, containing an account of this remarkable experiment, and sent it immediately by the hands of Coiffier himself.

HE says, that he drew sparks from the bar of a blue colour, an inch and a half in length, and which smelled strong of sulphur. He repeated the experiment at least six times in the space of about four minutes, in the presence of many persons, each experiment taking up the time, as he, in the stile of a priest expresses himself, of a *pater* and an *ave*. In the course of these experiments he received a stroke on his arm, a little above the elbow, but he could not tell whether it came from the brass wire inserted into the phial, or from the iron bar. He did not attend to the stroke at the time he received it, but, the pain continuing, he uncovered his arm when he went home, in the presence of Coiffier; and a mark was perceived round his arm, such as might have

have been made by a blow with the wire on his naked skin; and afterwards several persons, who knew nothing of what had happened, said that they perceived a smell of sulphur when he came near them.

COIFFIER told Mr. Dalibard, that for about a quarter of an hour before the curate arrived, he had, in the presence of five or six persons, taken much stronger sparks than those which the curate mentioned *.

EIGHT days after, Mr. Delor saw the same thing at his own house, although only a cloud passed over, without either thunder or lightning †.

THE same experiments were afterwards repeated by Mr. Delor, at the request of the king of France, who, it is said, saw them with the greatest satisfaction, and expressed a just sense of the merit of Dr. Franklin. These applauses of the king excited in Mr. De Buffon, Dalibard, and Delor, a desire of verifying Dr. Franklin's hypothesis more completely, and of pursuing his speculations upon the subject.

MR. DELOR's apparatus in Paris consisted of a bar of iron ninety-nine feet high, and answered rather better than that of Mr. Dalibard, which, as was observed before, was only forty feet high. But as the quantity of electricity which they could procure from the clouds, in these first experiments, was but small, they added to this apparatus what they

* Dalibard's Franklin, Vol. ii. p. 109, &c.

† Noller's Letters, Vol. i. p. 9.

called a *magazine* of electricity, consisting of many bars of iron insulated, and communicating with the pointed iron rod. This magazine contained more of the electric matter, and gave a more sensible spark, upon the approach of the finger than the pointed bar.

A MAGAZINE of this kind Mr. Mazeas had in an upper room of his house, into which he brought the lightning, by means of a wooden pole projecting out of his window, at the extremity of which a glass tube, filled with rosin, received a pointed iron rod, twelve feet long. But all this while the electrics, which they made use of to support these iron rods, were too much exposed to the open air, and consequently were liable to be wet, which would infallibly defeat their experiments.

THE most accurate experiments, made with these imperfect instruments, were those of Mr. Monnier. He was convinced that the high situation in which the bar of iron had commonly been placed was not absolutely necessary for this purpose: for he observed a common speaking-trumpet, suspended upon silk five or six feet from the ground, to exhibit very evident signs of electricity. He also found that a man placed upon cakes of rosin, and holding in his hand a wooden pole, about eighteen feet long, about which an iron wire was twisted, was so well electrified when it thundered, that very lively sparks were drawn from him; and that another man, standing upon non-electrics, in the middle of

a garden, and only holding up one of his hands in the air, attracted, with the other hand, shavings of wood which were held to him.

He says, that he observed a continual diminution of the electricity when the rain came on, though the thunder was still very loud, and though the cake of rosin which supported his conductor was not wet. But he afterwards found, that this was not universally true.

He once observed, that when the conducting wire was surrounded with drops of rain, only some of them were electrified, as was evident from the conical figure they had, while the rest remained round as before. It was also perceived, that the electrified and non-electrified drops generally succeeded each other alternately; which made Mr. Monnier call to mind a singular phenomenon, which happened some years before to five peasants who were passing through a corn field, near Frankfort upon the Oder, during a thunder storm; when the lightning killed the first, the third, and the fifth of them, without injuring the second or the fourth *.

It was not owing to any want of attention to this subject that the English philosophers were not the first to verify the theory of Dr. Franklin. They happened to have few opportunities of trying experiments. In the few they had, they were disappointed by

* Phil. Transf. Vol. xlvii. p. 551.

the rain wetting their apparatus, which was not better constructed than that of the French.

AT length success crowned the assiduity and happier contrivance of Mr. Canton, who, to the lower end of his conducting wire, had had the precaution to fasten a tin cover, to keep the rain from the glass tube which supported it. By this means, on the 20th of July 1752, he got sparks at the distance of half an inch, but the whole appearance ceased in the space of two minutes *.

MR. WILSON, who took great pains in these pursuits, as he did in every thing else relating to electricity, perceiving several electrical snaps, on the 12th of August following, from no other apparatus than an iron curtain rod; one end of which he put into the neck of a glass phial, which he held in his hand, while the other was made to point into the air.

DR. BEVIS also, at this time, viz. on the 12th of August, observed nearly the same appearances that Mr. Canton had done before †.

MR. CANTON afterwards, resuming his observations on lightning, with the assiduity and accuracy with which he observes every thing, found, by a great number of experiments, that some clouds were in a positive, and some in a negative state of electricity. And that, by this means, the electricity of

* Phil. Transf. Vol. xlvii. p. 568.

† Ibid. p. 569.

his conductor would sometimes change from one state to the other five or six times in less than half an hour*.

THIS observation of Mr. Canton, on the different electricity of the clouds was made, and the account of it published in England, before it was known that Dr. Franklin had made the same discovery in America.

WHEN the air was dry, he observed, that the apparatus would continue electrified for ten minutes, or a quarter of an hour after the clouds had passed the zenith, and sometimes till they were more than half way towards the horizon; that rain, especially when the drops were large, generally brought down the electric fire; and hail in summer, he believed, never failed. The last observation he had made before the time of his writing this paper, when his apparatus had been electrified by a fall of thawing snow. This was on the 12th of November 1753, which, he says, was the 26th day, and the sixty-first time, it had been electrified since it was first set up, viz. about the middle of the preceding May.

ONLY two thunder storms had happened at London during that whole summer, and Mr. Canton's apparatus was so strongly electrified by one of them, that the ringing of the bells (which he suspended to his apparatus, to signify when the electrification was begun, and which were frequently rung so loud as to be heard in every room in the

* Phil. Transf. Vol. xlviii. pt. i. p. 356.

house) was stifled by the almost constant stream of dense electric fire between each bell and the brass ball, which would not suffer it to strike.

UPON a farther occasion, he observes, that in the succeeding months of January, February, and March, his apparatus was electrified no less than twenty-five times, both positively and negatively, by snow as well as by hail and rain; and almost to as great a degree when Fahrenheit's thermometer was between twenty-eight and thirty-four, as he had ever known it in summer, except in a thunderstorm*.

MR. CANTON concludes his paper with proposing the two following queries. 1. May not air suddenly rarefied give electric fire to, and air suddenly condensed receive electric fire from clouds and vapours passing through it? 2. Is not the aurora borealis the flashing of electric fire from positive towards negative clouds at a great distance, through the upper part of the atmosphere, where the resistance is least†?

MR. CANTON not only observed the different states of positive and negative electricity in the clouds, but also noted the proportion that the one bore to the other for a considerable time. In the first period he had observed the clouds had been positively electrical 83 times, and negative 101. In this period he

* Phil. Transf. Vol. xlviii. pt. ii. p. 785.

† Ibid. pt. i. p. 358.

had punctually set down how often the powers had shifted, and the whole time that the apparatus continued to be electrified, but he had intirely neglected to note the time that each power lasted. But this last circumstance he afterwards carefully attended to for about two months, viz. from the 28th of June to the 23d of August 1754; and found the apparatus to be electrified positively thirty-one times, which taken together lasted three hours thirty-five minutes; and negatively forty-five times, the whole duration of which was ten hours thirty-nine minutes. He also observed that the positive power was generally the stronger. This account he wrote the 31st of August 1754.

THESE observations, which Mr. Canton gives me leave to make public, are extremely curious, and must have required great attention; but they are hardly sufficient to authorize any general conclusion.

ONE of the effects of lightning and electricity is the melting of metals. This was first thought to be a cold fusion; but that opinion is refuted, in a very sensible manner, by Dr. Knight, in a paper read at the Royal Society, November 22d, 1759. He observes, that the instances most generally given of cold fusion are two, viz. that of a sword being melted in its scabbard, and that of money being melted in a bag, both the scabbard and the bag remaining unhurt.

A GREAT number of authors, he says, have mentioned both the facts, but without giving

giving either their own testimony, or that of any one else for the truth of them, or describing any of the other concomitant circumstances. And it seemed to him very possible, that lightning might produce effects similar to those above mentioned, without our being obliged to have recourse to a cold fusion to account for them.

If, says he, the edge or external surface of a sword had been melted, whilst the main part of the blade remained entire, it would have afforded sufficient ground to assert, in general terms, that the sword was melted, and yet the scabbard might have remained unhurt; because either the edge or surface of a sword might be instantly melted by lightning and cooled so suddenly, as to make no impression of burning upon the scabbard. Metals, as well as other bodies, he observes, will both heat and cool sooner in proportion as they are thin and slender; that very small wire will instantly become red hot, and even melt, and run into a round globule in the flame of a common candle; though it is no sooner removed out of the flame, but it is instantly cold. He therefore concludes, that the edge of a sword, or even its surface, might be instantly melted by lightning; and being in contact with, or rather still united to the rest of the blade, which might be cold, it would part with its heat too suddenly to produce any appearance of burning.

He was confirmed in this reasoning, by examining some fragments and particles of

wire melted by lightning, which were sent him by Mr. Mountaine. Amongst them appeared globules of various sizes, which had undergone very different degrees of fusion. The largest of them had not been fluid enough to put on a spherical figure, but they approached nearer to it, in proportion as they were smaller; so that, in the smallest granulæ, the fusion was most perfect, the globules being very round and smooth. Their sizes continued diminishing till they became invisible to the naked eye, and some of them, when viewed with a microscope, required a third or fourth magnifier to see them distinctly.

SOME of the bits of wire were rough and scaly, like burnt iron, and were swelled in those places where they were beginning to melt. Others continued straight, and of an equable thickness; but their outward surface seemed to have undergone a perfect fusion, so that there were two or more pieces adhering together, as if joined by a thin folder.

IN the Philosophical Transactions, Dr. Knight says, there are two or three relations which seemed at first to favour a cold fusion, but when duly considered prove nothing conclusive*.

BUT that there is really no such thing as cold fusion, either by electricity or lightning, was most clearly demonstrated by Mr. Kinnersley, in a letter to Dr. Franklin, dated Philadelphia, March 12th, 1761.

* Phil. Transf. Vol. li. pt. i. p. 294, &c.

HE suspended a piece of small brass wire, about twenty-four inches long, with a pound weight at the lower end; and, by sending through it the charge of a case of bottles, containing above thirty feet of coated glass, he discovered what he calls a new method of wire-drawing. The wire was red-hot, the whole length well annealed, and above an inch longer than before. A second charge melted it so that it parted near the middle, and measured, when the ends were put together, four inches longer than at first.

THIS experiment, he says, was proposed to him by Dr. Franklin, in order to find whether the electricity, in passing through the wire, would so relax the cohesion of its constituent particles, as that the weight might produce a separation; but neither of them had the least suspicion that any heat would be produced.

THAT he might have no doubt of the wire being *hot*, as well as *red*, he repeated the experiment on another piece of the same wire, encompassed with a goose quill, filled with loose grains of gun-powder; which took fire, as readily as if it had been touched with a red-hot poker. Also tinder, tied to another piece of the wire, kindled by it; but when he tried a wire about twice as big, he could produce no such effects.

HENCE, says he, it appears, that the electric fire, though it has no sensible heat, when in a state of rest, will, by its violent motion, and the resistance it meets with, produce heat
in

in other bodies, when passing through them, provided they be small enough. A great quantity will pass through a large wire, without producing any sensible heat; when the same quantity, passing through a very small one, being there confined to a narrower passage, the particles crowding closer together, and meeting with a greater resistance, will make it red-hot, and even melt it.

HENCE, he concludes, that lightning does not melt metal by a cold fusion, as Dr. Franklin and himself had formerly supposed; but that, when it passed through the blade of a sword, if the quantity was not very great, it might heat the point so as to melt it, while the broadest and the thickest part might not be sensibly warmer than before.

WHEN trees and houses are set on fire by the dreadful quantity, which a cloud, or sometimes the earth discharges, must not the heat, says he, by which the wood is first kindled, be generated by the lightning's violent motion through the resisting combustible matter?

IF lightning, by its rapid motion, produced heat in itself as well as in other bodies (which Mr. Kinnerley imagined was evident from some experiments made with electrical thermometer, mentioned before) he thought that its sometimes singeing the hair of animals killed by it might easily be account for; and that the reason of its not always doing so might be, that the quantity, though sufficient to kill a large animal, might not be great enough

enough, or not have met with resistance enough, to become by its motion burning-hot.

WE find, says he, that dwelling-houses struck with lightning, are seldom set on fire by it; but that when it passes through barns, with hay or straw in them, or store-houses containing large quantities of hemp, or such like matter, they seldom, if ever, escape a conflagration. This, he thought, might be owing to such combustibles being apt to kindle with a less degree of heat than was necessary to kindle wood*.

ALL that was done by the French and English electricians, with respect to lightning and electricity, fell far short of what was done by Signior BECCARIA at Turin. His attention to the various states of the atmosphere, his assiduity in making experiments, his apparatus for making them, the extent of his views in making them, the minute exactness with which he has recorded them, and his judgment in applying them to a general theory, far exceeded every thing that had been done by philosophers before him, or that has been done by any person since. And though I shall give considerable scope to my account of his experiments and observations, I shall be able to give my reader but a faint idea of the extent, variety, and value of his labours in this great field.

* Phil. Transf. Vol. liii. p. 92, &c.

HE made use both of kites and pointed rods, and of a great variety of both at the same time, and in different places. Some of the strings of his kites had wires in them, and others had none. Some of them flew to a prodigious height, and others but low; and he had a great number of assistants, to note the nature, time, and degree of appearances, according as his views required.

To keep his kites constantly insulated, and at the same time to give them more or less string, and for many other purposes, he had the string rolled upon a reel, which was supported by pillars of glass; and his conductor had a communication with the axis of the reel*.

To distinguish the positive and negative state of the clouds, when the electricity was vigorous, with more certainty, and with more safety than it could be done by presenting an excited stick of glass, or sealing-wax to threads diverging from his conductor; he inclosed a pointed wire and a flat piece of lead opposite to it within a cylindrical glass vessel, wrapped in pasteboard, so that the inside could have no communication with the external light. Into this cover, and opposite to the point of the wire, he inserted a very long tube of pasteboard; through which he could look from a considerable distance, and see the form of the electric light at the end of the

* Lettere dell' elettricismo, p. 112.

wire; which is the surest indication of its quality*.

FROM Signior Beccaria's extremely exact and circumstantial account of the external appearances of thunder clouds, which he prefixes to his observations on their probable causes, I shall draw a general outline of the most remarkable particulars, in the usual progress of a thunder storm.

THE first appearance of a thunder storm (which generally happens when there is little or no wind) is one dense cloud, or more, increasing very fast in size, and rising into the higher regions of the air. The lower surface is black, and nearly level; but the upper finely arched, and well defined. Many of these clouds often seem piled one upon another, all arched in the same manner; but they keep continually uniting, swelling, and extending their arches.

AT the time of the rising of this cloud, the atmosphere is generally full of a great number of separate clouds, motionless, and of odd and whimsical shapes. All these, upon the appearance of the thunder cloud, draw towards it, and become more uniform in their shapes as they approach; till, coming very near the thunder cloud, their limbs mutually stretch towards one another; they immediately coalesce, and together make one uniform mass. These he calls *adscititious* clouds, from their coming in, to enlarge the size of the

* Lettere dell' elettricismo, p. 107.

thunder cloud. But, sometimes the thunder cloud will swell, and increase very fast without the conjunction of any adscititious clouds, the vapours in the atmosphere forming themselves into clouds wherever it passes. Some of the adscititious clouds appear like white fringes, at the skirts of the thunder cloud, or under the body of it, but they keep continually growing darker and darker, as they approach to unite with it.

WHEN the thunder cloud is grown to a great size, its lower surface is often ragged, particular parts being detached towards the earth, but still connected with the rest. Sometimes the lower surface swells into various large protuberances bending uniformly towards the earth. And sometimes one whole side of the cloud will have an inclination to the earth, and the extremity of it will nearly touch the earth*. When the eye is under the thunder cloud, after it is grown larger, and well formed, it is seen to sink lower, and to darken prodigiously; at the same time that a number of small adscititious clouds (the origin of which can never be perceived) are seen in a rapid motion, driving about in very uncertain directions under it. While these clouds are agitated with the most rapid motions, the rain generally falls in the greatest plenty, and if the agitation be exceedingly great, it commonly hails†.

* Lettere dell' elettricismo, p. 151.

† Ibid, p. 155.

WHILE the thunder cloud is swelling, and extending its branches over a large tract of country, the lightning is seen to dart from one part of it to another, and often to illuminate its whole mass. When the cloud has acquired a sufficient extent, the lightning strikes between the cloud and the earth, in two opposite places, the path of the lightning lying through the whole body of the cloud and its branches. The longer this lightning continues, the rarer does the cloud grow, and the less dark is its appearance; till, at length, it breaks in different places, and shows a clear sky. When the thunder cloud is thus dispersed, those parts which occupy the upper regions of the atmosphere are equally spread, and very thin; and those that are underneath are black, but thin too: and they vanish gradually, without being driven away by any wind*.

HAVING seen what this philosopher observed abroad, and in the air, let us see what he took notice of at his apparatus within doors. This never failed to be electrified upon every approach of a thunder cloud, or any of its branches; and the stream of fire from it was generally perpetual, while it was directly over the apparatus†.

THAT thunder clouds were sometimes in a positive as well as negative state of electricity, Signior Beccaria had discovered, before he heard of its having been observed by Dr.

* Lettere dell' elettricismo, p. 146. 176.

† Ibid. p. 167.

Franklin, or any other person *. The same cloud, in passing over his observatory, electrified his apparatus, sometimes positively, and sometimes negatively †. The electricity continued longer of the same kind, in proportion as the thunder cloud was simple, and uniform in its direction; but when the lightning changed its place, there commonly happened a change in the electricity of his apparatus. It would change suddenly after a very violent flash of lightning, but the change would be gradual when the lightning was moderate, and the progress of the thunder cloud slow.

IT was an immediate inference from his observations of the lightning abroad, and his apparatus within, that the quantity of electric matter, in an usual storm of thunder, is almost inconceivably great, considering how many pointed bodies, as trees, spires, &c. are perpetually drawing it off, and what a prodigious quantity is repeatedly discharged to, or from the earth ‡.

AFTER this summary view of appearances, I shall, in the same succinct manner explain the hypothesis by which this excellent philosopher accounts for them, and some other principal and well known phenomena of thunder storms.

CONSIDERING the vast quantity of electric fire that appears in the most simple thunder storms, he thinks it impossible that any cloud, or number of clouds should ever contain it

* Lettere dell' elettricismo, p. 138.

† Ibid. p. 172.

‡ Ibid. p. 180.

all, so as either to discharge or receive it. Besides, during the progress and increase of the storm, though the lightning frequently struck to the earth, the same clouds were the next moment ready to make a still greater discharge, and his apparatus continued to be as much affected as ever. The clouds must, consequently, have received at one place, the moment that a discharge was made from them in another *. In many cases, the electricity of his apparatus, and consequently of the clouds, would instantly change from one kind to another several times; an effect which cannot be accounted for by any simple discharge, or recruit. Both must have taken place in a very quick succession †.

THE extent of the clouds doth not lessen this difficulty: for, be it ever so great, still the quantity ought to be lessened by every discharge: and, besides, the points, by which the silent discharges are made, are in proportion to the extent of the clouds ‡. Nor is the difficulty lessened by supposing that fresh clouds bring recruits; for besides that the clouds are not ripe for the principal storm, till all the clouds, to a great distance, have actually coalesced, and formed one uniform mass, those recruits bear no sort of proportion to the discharge, and whatever it was, it would soon be exhausted.

THE fact, therefore, must be, that the electric matter is continually darting from the

* Lettere dell' elettricismo, p. 183. 188.

† Ibid. p. 220.

‡ Ibid. p. 185.

clouds in one place, at the same time that it is discharged from the earth in another. And it is a necessary consequence from the whole, that the clouds serve as conductors to convey the electric fluid from those places of the earth which are overloaded with it, to those which are exhausted of it *.

To ascertain this fact in the most complete manner, he proposes that two observatories be fixed, about two leagues asunder, in the usual path of the thunder clouds; and that observations be made, whether the apparatus be not often positive at one place, when it is negative at the other †.

THAT great quantities of electric matter do sometimes rush out of particular parts of the earth, and rise through the air, into the higher regions of the atmosphere, he thinks is evident from the great quantities of sand, ashes, and other light substances, which have often been carried up into the air, and scattered uniformly over a large tract of country ‡. No other known efficient cause of this phenomenon can be assigned, except the wind; and it has been observed when there was no wind stirring; and the light bodies have even been carried against the wind §. He supposes, therefore, that these light bodies are raised by a large quantity of electric matter issuing out of the earth, where it was overcharged with it, and (by that property of it which he had demonstrated) attracting, and

* Lettere dell' elettricismo, p. 193.

† Ibid. p. 194.

‡ Ibid. p. 199.

§ Ibid. p. 225.

carrying with it every substance that could serve as a conductor in its passage. All these bodies, being possessed of an equal quantity of the electric fluid, will be dispersed equally in the air, and consequently over that part of the earth where the fluid was wanting, and whither they serve to convey it *. Had these bodies been raised by the wind, they would have been dispersed at random, and in heaps.

THIS comparatively rare phenomenon (but of which he had been more than once a spectator) he thinks exhibits both a perfect image, and a demonstration, of the manner in which the vapours of the atmosphere are raised to form thunder clouds. The same electric matter, wherever it issues, attracts to it, and carries up into the higher regions of the air, the watery particles that are dispersed in the atmosphere. The electric matter ascends to the higher regions of the atmosphere, being solicited by the less resistance it finds there than in the common mass of the earth; which, at those times, is generally very dry, and consequently highly electric. The uniformity with which thunder clouds spreads themselves, and swell into arches, must be owing to their being affected by some cause which, like the electric matter, diffuses itself uniformly wherever it acts, and to the resistance they meet with in ascending through the air †. As a proof of this, steam, rising from an electrified eolipile, diffuses itself with the same uni-

* Lettere dell' elettricismo, p. 202.

† Ibid. p. 205.

formity, and in similar arches, extending itself towards any conducting substance*.

THE same cause which first raised a cloud, from vapours dispersed in the atmosphere, draws to it those that are already formed, and continues to form new ones, till the whole collected mass extends so far as to reach a part of the earth where there is a deficiency of the electric fluid †. Thither too, will those clouds, replete with electricity, be strongly attracted, and there will the electric matter discharge itself upon the earth. A channel of communication being, in this manner, found, a fresh supply of electric matter will be raised from the overloaded part, and will continue to be conveyed by the medium of the clouds, till the equilibrium of the fluid between the two places of the earth be restored. When the clouds are attracted in their passage by those parts of the earth where there is a deficiency of the fluid, those detached fragments are formed, and also those uniform depending protuberances, which will be shown to be, in some cases, the cause of water-spouts and hurricanes ‡.

THAT the electric matter, which forms and animates the thunder clouds, issues from places far below the surface of the earth, and that it buries itself there, is probable from the deep holes that have, in many places, been made by lightning §. Flashes of lightning have, also, been seen to arise from subterrane-

* Lettere dell' elettricismo, p. 206.

† Ibid. p. 212.

‡ Ibid. p. 214.

§ Ibid. p. 227.

ous cavities, and from wells *. Violent inundations have accompanied thunder storms, not occasioned by rain, but by water bursting from the bowels of the earth, from which it must have been dislodged by some internal concussion. Deep wells have been known to fill faster in thunder storms †, and others have constantly grown turbid at the approach of thunder ‡.

THIS very rise, as well as the whole progress of thunder clouds, has sometimes been, in a manner, visible. Exhalations have been frequently seen to rise from particular caverns, attended with a rumbling noise, and to ascend into the higher regions of the air, with all the phenomena of thunder storms described above, according to the description of persons who lived long before the connection between electricity and lightning was suspected §.

THE greatest difficulty attending this theory of the origin of thunder storms relates to the collection, and insulation of electric matter within the body of the earth. With respect to the former, he has nothing particular to say. Some operations in nature are certainly attended with a loss of the equilibrium in the electric fluid, but no person has yet assigned a more probable cause of the redundancy of the electric matter which, in fact, often abounds in the clouds, than what we may suppose

* Lettere dell' elettricismo, p. 228.

† Ibid. p. 233.

‡ Ibid. p. 360.

§ Ibid. p. 231.

possible to take place in the bowels of the earth. And supposing the loss of the equilibrium possible, the same cause that produced the effect would prevent the restoring of it; so that not being able to force a way, at least one sufficiently ready, through the body of the earth, it would issue at the most convenient vent into the higher regions of the air, as the better passage. His electrical apparatus, though communicating with the earth, has frequently, in violent thunder storms, given evident sparks to his finger*.

IN the enumeration of the effects of thunder storms, he observes that a wind always blows from the place from which the thunder cloud proceeds; that this is agreeable to the observations of all mariners, and that the wind is more or less violent in proportion to the suddenness of the appearance of the thunder cloud, the rapidity of its expansion, and the velocity with which the adscititious clouds join it. The sudden condensation of such a prodigious quantity of vapours must displace the air, and repel it on all sides†.

HE, in some measure, imitated even this effect of thunder, at least produced a circulation of all the air in his room, by the continued electrification of his chain‡.

AMONG other effects of lightning, he mentions the case of a man rendered exceedingly stiff, presently after he was struck dead in a storm of thunder. But the most remarkable

* Lettere dell' elettricismo, p. 236.

† Ibid. p. 329, 340.

‡ Ibid. p. 343.

circumstance, in this case, was the lightning (choosing the best conductor) having struck one particular vein, near his neck, and followed it through its minutest ramifications ; so that the figure of it appeared through the skin, finer than any pencil could have drawn it *.

HE cautions persons not to depend upon the neighbourhood of a higher, or, in all cases, a better conductor than their own body ; since, according to his repeated observations, the lightning by no means descends in one undivided track ; but bodies of various kinds conduct their share of it, at the same time, in proportion to their quantity and conducting power †.

A GREAT number of observations, relating to the descent of lightning, confirm his theory of the manner of its ascent : for, in many cases, it throws before it the parts of conducting bodies, and distributes them along the resisting medium through which it must force its passage ‡.

UPON this principle it is, that the longest flashes of lightning seem to be made, by its forcing into its way part of the vapours in the air §. One of the principal reasons why those flashes make so long a rumbling, is their being occasioned by the vast length of a vacuum, made by the passage of the electric matter. For though the air collapses the mo-

* Lettere dell' elettricismo, p. 242.

† Ibid. p. 246.

‡ Ibid. p. 247.

§ Ibid. p. 851.

ment after it has passed, and the vibration (on which the sound depends) commences at the same moment, through the whole length of the track; yet, if the flash was directed towards the person who hears the report, the vibrations excited at the nearer end of the track will reach his ear much sooner than those excited at the more remote end; and the sound will, without any repercussion or echo, continue till all the vibrations have successively reached him *.

I MUST introduce in this place a very curious experiment and observation of Mr. Lullin, concerning the production of electricity in the clouds. He made a long insulated pole project from the side of one of the Alps; and, on the 29th of June 1766, observed, that when small clouds of vapour, raised by the heat of the sun, rose near the foot of the mountain, and ascended along the side of it; if they touched the extremity of the pole only, it was electrified; but if the whole pole, and consequently part of the hill on which it stood, was likewise involved, it was not electrified. From this he concludes, that the electricity of the clouds is produced by their passing through the air while the sun shines upon them. But to which of these two circumstances, namely the motion through the air, or the action of the sun's rays, this was owing, he could not determine, though he made several experiments for that purpose †.

* *Lettre dell' elettricismo*, p. 252.

† *Dissertatio physica*, p. 42.

ONE of the most remarkable effects of lightning is that it gives polarity to the magnetic needle, and to all bodies that have any thing of iron in them, as bricks, &c. and by observing which way the poles of these bodies lye, it may be known, with the utmost certainty, in what direction the stroke passed *. In one case S. Beccaria actually ascertained the direction of the lightning in this manner †.

SINCE a sudden stroke of lightning gives polarity to magnets, he conjectures that a regular and constant circulation of the whole mass of the fluid, from North to South, may be the original cause of magnetism in general ‡. This is a truly great thought; and, if just, will introduce greater simplicity into our conceptions of the laws of nature.

THAT this ethereal current is insensible to us, is no proof of its non-existence, since we ourselves are involved in it. He had seen birds fly so near a thunder cloud, as he was sure they would not have done, if they had been affected by its atmosphere §.

THIS current he would not suppose to arise from one source, but from several, in the northern hemisphere of the earth. The aberration of the common center of all these currents from the North point may be the cause of the variation of the needle, the period of this declination of the center of the currents may be the period of the variation, and the obliquity with which the currents strike into the

* Lettere dell' elettricismo, p. 262.

† Ibid. p. 263.

‡ Ibid. p. 268.

§ Ibid.

earth may be the cause of the dipping of the needle, and also why bars of iron more easily receive the magnetic virtue in one particular direction *.

HE thinks that the *Aurora Borealis* may be this electric matter performing its circulation, in such a state of the atmosphere as renders it visible, or approaching nearer to the earth than usual. Accordingly very vivid appearances of this kind have been observed to occasion a fluctuation in the magnetic needle †.

STONES and bricks struck by lightning are often vitrified. He supposes that some stones in the earth, having been struck in this manner, first gave occasion to the vulgar opinion of the thunder bolt ‡.

SIGNIOR BECCARIA was very sensible that heat contributes much to the phenomena of thunder, lightning, and rain; but he could not find, by any experiment, that it tended to promote electricity. He, therefore, rather thought that heat operated, in this case, by exhaling the moisture of the air, and thereby cutting off the communication of the electric fluid between one place and another, particularly between the earth and the higher regions of the air, whereby its effects were more visible §.

HAVING entertained my reader with the observations of this great Italian genius, I must once more conduct him to France, where he will see several experiments well worth his

* Lettere dell' elettricismo, p. 269.

† Ibid. p. 272.

‡ Ibid. p. 263.

§ Ibid. p. 359.

notice. In this country we have seen that Dr. Franklin's theory of the identity of electricity and the matter of lightning was first verified, and we shall now see it verified in the grandest and most conspicuous manner.

THE greatest quantity of electricity that was ever brought from the clouds, by any apparatus prepared for that purpose, was by Mr. De Romas, assessor to the presideal of Nerac. This gentleman was the first who made use of a wire interwoven in the hempen cord of an electrical kite, which he made seven feet and a half high, and three feet wide, so as to have eighteen square feet of surface. This cord was found to conduct the electricity of the clouds more powerfully than a hempen cord would do, even though it was wetted; and, being terminated by a cord of dry silk it enabled the observer (by a proper management of his apparatus) to make whatever experiments he thought proper, without danger to himself.

By the help of this kite, on the 7th of June 1753, about one in the afternoon, when it was raised 550 feet from the ground, and had taken 780 feet of string, making an angle of near forty-five degrees with the horizon; he drew sparks from his conductor three inches long and a quarter of an inch thick, the snapping of which was heard about 200 paces. Whilst he was taking these sparks, he felt, as it were, a cobweb on his face, though he was above three feet from the string of the kite; after which he did not think it safe to stand
so

so near, and called aloud to all the company to retire, as he did himself about two feet.

THINKING himself now secure enough, and not being incommoded by any body very near him, he took notice of what passed among the clouds which were immediately over the kite; but could perceive no lightning either there or any where else, nor scarce the least noise of thunder, and there was no rain at all. The wind was West, and pretty strong, which raised the kite 100 feet higher, at least, than in the other experiments.

AFTERWARDS, casting his eyes on the tin tube, which was fastened to the string of the kite, and about three feet from the ground, he saw three straws, one of which was about one foot long, a second four or five inches, and the third three or four inches, all standing erect, and performing a circular dance, like puppets, under the tin tube, without touching one another.

THIS little spectacle, which much delighted several of the company, lasted about a quarter of an hour; after which, some drops of rain falling, he again perceived the sensation of the cobweb on his face, and at the same time heard a continual rustling noise, like that of a small forge bellows. This was a farther warning of the increase of electricity; and from the first instant that Mr. De Romas perceived the dancing straws, he thought it not adviseable to take any more sparks even with all his precautions; and he
again

again intreated the company to spread themselves to a still greater distance.

IMMEDIATELY after this came on the last act of the entertainment, which Mr. De Romas acknowledged made him tremble. The longest straw was attracted by the tin tube, upon which followed three explosions, the noise of which greatly resembled that of thunder. Some of the company compared it to the explosion of rockets, and others to the violent crashing of large earthen jars against a pavement. It is certain that it was heard into the heart of the city, notwithstanding the various noises there.

THE fire that was seen at the instant of the explosion had the shape of a spindle eight inches long and five lines in diameter. But the most astonishing and diverting circumstance was produced by the straw, which had occasioned the explosion, following the string of the kite. Some of the company saw it at forty-five or fifty fathoms distance, attracted and repelled alternately, with this remarkable circumstance, that every time it was attracted by the string, flashes of fire were seen, and cracks were heard, though not so loud as at the time of the former explosion.

IT is remarkable, that, from the time of the explosion to the end of the experiments, no lightning at all was seen, nor scarce any thunder heard. A smell of sulphur was perceived, much like that of the luminous electric effluvia issuing out of the end of an electrified bar of metal. Round the string ap-
peared

peared a luminous cylinder of light, three or four inches in diameter; and this being in the day-time Mr. De Romas did not question but that, if it had been in the night, that electric atmosphere would have appeared to be four or five feet in diameter. Lastly, after the experiments were over, a hole was discovered in the ground, perpendicularly under the tin tube, an inch deep, and half an inch wide, which was probably made by the large flashes that accompanied the explosions.

AN end was put to these remarkable experiments by the falling of the kite, the wind being shifted into the East, and rain mixed with hail coming on in great plenty. Whilst the kite was falling, the string came foul of a penthouse; and it was no sooner disengaged, than the person who held it felt such a stroke in his hands, and such a commotion through his whole body, as obliged him instantly to let it go; and the string, falling on the feet of some other persons, gave them a shock also, though much more tolerable*.

THE quantity of electric matter brought by this kite from the clouds at another time is really astonishing. On the 26th of August 1756, the streams of fire issuing from it were observed to be an inch thick, and ten feet long. This amazing flash of lightning, the effect of which on buildings or animal bodies, would perhaps have been equally destructive with any that are mentioned in history, was

* Gent. Magaz. for August 1756, p. 378.

safely conducted by the cord of the kite to a non-electric body placed near it, and the report was equal to that of a pistol.

MR. ROMAS had the curiosity to place a pigeon in a cage of glass, in a little edifice, which he had purposely placed, so as that it should be demolished by the lightning brought down by his kite. The edifice was, accordingly, shattered to pieces, but the cage and the pigeon were not struck*.

THE Abbé Nollet, who gives this account, adds, that if a stroke of this kind had gone through the body of Mr. De Romas, the unfortunate professor Richman had not probably been the only martyr to electricity, and advises, that great caution be used in conducting such dangerous experiments†.

WHEN we consider how many severe shocks the most cautious and judicious electricians often receive through inadvertence, we shall not be surprised, that when philosophers first began to collect and make experiments upon real lightning, it should sometimes have proved a little untractable in their hands, and that they were obliged to give one another frequent cautions how to proceed with it.

THE Abbé Nollet, as early as the year 1752, advises that these experiments be made with circumspection; as he had been informed, by letters from Florence and Bologna, that those who had made them there had had

* Nollet's Letters, Vol. ii. p. 239.

† Phil. Transf. Vol. lii. pt. i. p. 342.

their curiosity more than satisfied by the violent shocks they had sustained in drawing sparks from an iron bar electrified by thunder. One of his correspondents informed him, that once, as he was endeavouring to fasten a small chain, with a copper ball at one of its extremities, to a great chain, which communicated with the bar at the top of the building (in order to draw off the electric sparks by means of the oscillations of this ball) there came a flash of lightning, which he did not see, but which affected the chain with a noise like that of wild fire. At that instant, the electricity communicated itself to the chain of the copper ball, and gave the observer so violent a commotion, that the ball fell out of his hands, and he was struck backwards four or five paces. He had never been so much shocked by the experiment of Leyden *.

MR. ROMAS received a severe stroke when he first raised his kite: and Mr. Dalibard says, that Mr. Monnier, a physician of St. Germain en Laye, member of the Academy of Sciences at Paris, and Mr. Bértier of the Oratory at Montmorency, a correspondent of the Academy, were both struck down by strokes of lightning, as they were taking sparks from their apparatus †.

BUT the greatest sufferer by experiments with lightning, since mankind have introduced so dangerous a subject of their inquiries, was professor Richman of Petersburg

* Phil. Trans. Vol. xlviii. pt. i. p. 205.

† Dalibard's Franklin, Vol. ii. p. 129.

before mentioned. He was struck dead, on the 6th of August 1753, by a flash of lightning drawn by his apparatus into his own room, as he was attending to an experiment he was making with it. There were two accounts of this fatal accident communicated to the Royal Society, one by Dr. Watson who had it from the best authority*; and the other translated from the High Dutch†. From both these the following is extracted.

THE professor had provided himself with an instrument which he called an *electrical gnomon*, the use of which was to measure the strength of electricity. It consisted of a rod of metal terminating in a small glass vessel, into which (for what reason I do not know) he put some brass filings. At the top of this rod, a thread was fastened, which hung down by the side of the rod when it was not electrified; but when it was, it avoided the rod, and stood at a distance from it, making an angle at the place where it was fastened. To measure this angle, he had the arch of a quadrant fastened to the bottom of the iron rod.

HE was observing the effect of the electricity of the clouds, at the approach of a thunder storm, upon this gnomon; and, of course, standing with his head inclined towards it, accompanied by Mr. Solokow (an engraver, whom he frequently took with him, to be a

* Phil. Transf. Vol. xlviii. pt. ii. p. 765.

† Ibid. Vol. xlix. pt. i. p. 61.

joint observer of his electrical experiments, in order to represent them the better in cuts) when this gentleman, who was standing close to his elbow, observed a globe of blue fire, as he called it, as big as his fist, jump from the rod of the gnomon towards the head of the professor, which was, at that instant, at about a foot distance from the rod. This flash killed Mr. Richman, but Mr. Solokow could give no account of the particular manner in which he was immediately affected by it: for, at the same time that the professor was struck, there arose a sort of steam, or vapour, which intirely benumbed him, and made him sink down upon the ground; so that he could not remember even to have heard the clap of thunder, which was very loud.

THE globe of fire was attended with a report as loud as that of a pistol: a wire, which brought the electricity to his metal rod, was broken to pieces, and its fragments thrown upon Mr. Solokow's cloaths. Half of the glass vessel in which the rod of the gnomon stood was broken off, and the filings of metal that were in it were thrown about the room.

UPON examining the effects of the lightning in the professor's chamber, they found the door-case half split through, and the door torn off, and thrown into the room*. They opened a vein of the breathless body twice, but no blood followed, and endeavoured to re-

* Phil. Transf. Vol. xlviii. pt. ii. p. 763.

cover sensation by violent chafing, but in vain. Upon turning the corpse with the face downwards, during the rubbing, an inconsiderable quantity of blood ran out of the mouth. There appeared a red spot on the forehead, from which spirted some drops of blood through the pores, without wounding the skin. The shoe belonging to the left foot was burst open, and, uncovering the foot at that place, they found a blue mark; from which it was concluded, that the electrical force of the thunder, having entered the head, made its way out again at that foot.

UPON the body, particularly on the left side, were several red and blue spots, resembling leather shrunk by being burnt. Many more blue spots were afterwards visible over the whole body, and in particular over the back. That upon the forehead changed to a brownish red, but the hair of the head was not singed, notwithstanding the spot touched some of it. In the place where the shoe was unripped, the stocking was intire; as was the coat every where, the waistcoat only being singed on the foreflap, where it joined the hinder; but there appeared on the back of Mr. Solokow's coat long narrow streaks, as if red hot wires had burned off the nap, and which could not be well accounted for.

WHEN the body was opened the next day, twenty-four hours after he was struck, the cranium was very intire, having no fissure,

nor cross opening; the brain as found as it possibly could be, but the transparent pellicles of the windpipe were excessively tender, gave way, and easily rent. There was some extravasated blood in it, as likewise in the cavities below the lungs; those of the breast being quite found, but those towards the back of a brownish black colour, and filled with more of the above mentioned blood: otherwise, none of the entrails were touched; but the throat, the glands, and the thin intestines were all inflamed. The singed leather-coloured spots penetrated the skin only. Twice twenty-four hours being elapsed, the body was so far corrupted that it was with difficulty they got it into a coffin*.

* Phil. Trans. Vol. xlix. pt. i. p. 67.

SECTION XI.

OBSERVATIONS ON THE GENERAL STATE OF
ELECTRICITY IN THE ATMOSPHERE, AND
ITS MORE USUAL EFFECTS.

ELECTRICIANS, after observing the great quantity of electric matter with which the clouds are charged during a thunder storm, began to attend to the lesser quantities of it which might be contained in the common state of the atmosphere, and the more usual effects of this great and general agent in nature. Mr. Monnier, whose observations of the electricity of the air during a thunder storm have been already mentioned, was the first who found that there was very often, and perhaps always, a quantity of electric matter in the atmosphere, when there was no appearance of thunder. This he confirmed by decisive experiments, made at St. Germain en Laye, and published in a memoir read at the Royal Academy of Sciences at Paris November the 15th, 1752*.

BUT more accurate experiments upon the electricity of the air were made by the Abbé Mazeas, at Chateau de Maintenon, during the months of June, July, and October 1753, and communicated to the Royal Society, in a letter to Dr. Stephen Hales.

* Phil. Transf. Vol. xlviii. pt. i. p. 203.

THE Abbé's apparatus consisted of an iron rod 370 feet long, raised ninety feet above the horizon. It came down from a very high room in the castle, where it was fastened to a silken cord six feet long; and it was carried from thence to the steeple of the town, where it was likewise fastened to another silken cord of eight feet long, and sheltered from rain. And a large key was suspended, by the end of this wire, in order to receive the electric fluid.

ON the 17th of June, when he began his experiments, the electricity of the air was sensibly felt every day, from sun rise till seven or eight in the evening, except in moist weather, when he could perceive no signs of electricity. In dry weather, the wire attracted minute bodies at no greater distance than three or four lines. He repeated the experiment carefully every day, and constantly observed, that, in weather void of storms, the electricity of a piece of sealing-wax of two inches long was above twice as strong as that of the air. This observation inclined him to conclude, that in weather of equal driness the electricity of the air was always equal.

IT did not appear to him that hurricanes and tempests increased the electricity of the air, when they were not accompanied with thunder; for that, during three days of a very violent continual wind, in the month of July, he was obliged to put some dust within four or five lines of the conductor, before any sensible attraction could be perceived.

THE direction of the winds, whether East, West, North, or South, made no sensible alteration in the electricity of the air, except when they were moist.

IN the driest nights of that summer, he could discover no signs of electricity in the air; but it returned in the morning when the sun began to appear above the horizon, and vanished again in the evening, about half an hour after sun-set.

THE strongest common electricity of the atmosphere, during that summer, was perceived in the month of July, on a very dry day, the heavens being very clear, and the sun extremely hot. The distance of ten or twelve lines was then sufficient for the approach of the dust to the conductor, in order to see the particles rise in a vertical direction, like the filings of iron on the approach of a magnet.

ON the 27th of June, at two in the afternoon, he perceived some stormy clouds rising above the horizon, and immediately went up to his apparatus; and, having applied the dust to the key, it was attracted with a force which increased in proportion as the clouds reached the zenith. When they had come nearly over the wire, the dust was so impetuously repelled, as to be entirely scattered from the paper. He drew considerable sparks from it, though there was neither thunder nor lightning. When the stormy clouds were in the zenith of his wire, he observed that the electricity was increased to such a degree, that

even the filken thread attracted light bodies at the distance of seven or eight inches.

THESE stormy clouds remained about two hours above the horizon, without either thunder or lightning; nor did a very heavy rain diminish the electricity, except about the end, when the clouds began to be dissipated*.

MR. KINNERSLEY observed, that when the air was in its driest state, there was always a considerable quantity of electricity in it, and which might be easily drawn from it. Let a person, he says, in a negative state, standing out of doors, in the dark, when the air is dry, hold, with his arm extended, a long sharp needle, pointing upwards, and he will soon be convinced that electricity may be drawn out of the air; not indeed very plentifully, for, being a bad conductor, it seems loth to part with it, yet some will evidently be collected. The air near the person's body, having less than the natural quantity, will have none to spare; but his arm being extended, as above, some will be collected from the remoter air, and will appear luminous as it converges to the point of the needle.

LET a person electrified negatively, he says, present the point of a needle horizontally, to a cork ball suspended by silk, and the ball will be attracted towards the point, till it has parted with so much of its natural quantity of electricity, as to be in a negative state, in the

* Phil. Trans. Vpl. xlviii. pt. i. p. 377, &c.

same degree with the person who holds the needle; then it will recede from the point, being, as he supposes, attracted the contrary way by the electricity of greater density in the air behind it. But as this opinion, he pleasantly says, seems to deviate from *electrical orthodoxy*, he would be glad to see these phenomena better accounted for by the superior and more penetrating genius of his friend Dr. Franklin, to whom he is writing.

WHETHER the electricity in the air, in clear dry weather, be of the same density at the height of 200 or 300 yards, as near the surface of the earth, he thought might be satisfactorily determined by Dr. Franklin's old experiment of the kite.

THE twine, he says, should have throughout a very small wire in it, and the ends of the wire, where the several lengths are united, ought to be tied down with a waxed thread, to prevent their acting in the manner of points.

WHEN he wrote this letter, he had tried the experiment twice, when the air was as dry as it ever is in that country, and so clear, that not a cloud had been seen, and found the twine each time in a small degree electrified positively*.

THE preceding observations of Mr. Monnier, Mr. Mazeas, and Mr. Kinnerfley, fall far short of the extent and accuracy of those of Signior Beccaria; whose observations on

* Phil. Transf. Vol. liii. pt. i. p. 87.

the general state of electricity in the atmosphere I have reserved for the last place of the section, because they are the most considerable though they were all made independent of, and, many of them, prior to those mentioned before.

He observed that, during very high winds, his apparatus gave no signs of being electrified *. Indeed he found that in three different states of the atmosphere, he could find no electricity in the air. 1. In windy and clear weather. 2. When the sky was covered with distinct and black clouds, that had a slow motion. 3. In moist weather, not actually raining †. In a clear sky, when the weather was calm, he always perceived signs of a moderate electricity, but interrupted. In rainy weather, without lightning, his apparatus was always electrified a little time before the rain fell, and during the time of the rain, but it ceased to be affected a little before the rain was over.

THE higher his rods reached, or his kites flew, the stronger signs they gave of their being electrified ‡. Also longer strings or cords, extended and insulated in the open air, acquired electricity sooner than those which were shorter. A cord 1500 Paris feet long, stretched over the river Po, was as strongly electrified during a shower, without thunder, as a metallic rod, to bring lightning

* Lettere dell' elettricismo, p. 106.

† Ibid. p. 166.

‡ Ibid. p. 114.

into his house, had been in any thunder storm *.

HAVING two rods for bringing the lightning into his house, 140 feet asunder, he observed, that if he took a spark from the higher of them, the spark from the other, which was thirty feet lower, was at that instant lessened; but, what is remarkable, is that its power revived again, though he kept his hand upon the former †.

HE imagined that the electricity communicated to the air might sometimes furnish small sparks to his apparatus; since the air parts with the electricity it has received very slowly, and therefore the equilibrium of the electric matter in the air will not be restored so soon as in the earth and clouds ‡.

AMONG the effects of a moderate electricity in the atmosphere, Signior Beccaria reckons *rain, hail, and snow*.

CLOUDS that bring rain, he thought, were produced in the same manner as thunder clouds, only by a more moderate electricity. He describes them at large, and the resemblance which all their phenomena bear to those of thunder clouds is indeed very striking §.

HE notes several circumstances attending rain without lightning, which make it very probable, that it is produced by the same cause as when it is accompanied with lightning. Light has been seen among the clouds by

* Lettere dell' elettricismo, p. 165.

† Ibid. p. 175.

‡ Ibid. p. 347.

§ Ibid. p. 284.

night in rainy weather; and even by day rainy clouds are sometimes seen to have a brightness evidently independent of the sun *. The uniformity with which the clouds are spread, and with which the rain falls, he thought were evidences of an uniform cause like that of electricity †. The intensity of electricity in his apparatus generally corresponded very nearly, to the quantity of rain that fell in the same time ‡. Nor is any thing to be inferred to the contrary of this supposition from the apparatus not being always electrified during rain. It has sometimes failed during thunder. Indeed it follows from his general theory, that the electricity of his apparatus could not always correspond to the electricity of the clouds; since it must in some measure depend upon the situation of the observatory, with respect to those parts of the earth or clouds which are giving or taking electric fire. This was confirmed by an observation which he made upon one thunder cloud, which passed over his observatory. At its approach his apparatus was electrified positively, when it was directly over him all signs of electricity ceased, and when it was passed, his apparatus was electrified negatively §. This observation very much favours his general theory of thunder clouds.

SOMETIMES all the phenomena of thunder, lightning, hail, rain, snow, and wind, have

* Lettere dell' elettricismo, p. 288.

† Ibid. p. 299.

‡ Ibid. p. 307.

§ Ibid. p. 310.

been observed at one time; which shows the connection they all have with some common cause*.

SIGNIOR BECCARIA, therefore, supposes that, previous to rain, a quantity of electric matter escapes out of the earth, in some place where there was a redundancy of it; and, in its ascent to the higher regions of the air, collects and conducts into its path a great quantity of vapours. The same cause that collects, will condense them more and more: till, in the places of the nearest intervals, they come almost into contact, so as to form small drops; which uniting with others as they fall, come down, in rain. The rain will be heavier in proportion as the electricity is more vigorous, and the cloud approaches more nearly to a thunder cloud†.

HE imitated the appearance of clouds that bring rain by insulating himself between the rubber and conductor of his electrical machine, and with one hand dropping *colophonia* into a spoon fastened to the conductor, and holding a burning coal, while his other hand communicated with the rubber. In these circumstances the smoke spread along his arm, and, by degrees, all over his body, till it came to the other hand that communicated with the rubber. The lower surface of this smoke was every where parallel to his cloaths, and the upper surface was swelled and arched like clouds replete with thunder and rain‡. In

* Lettere dell' elettricismo, p. 290. 345.

† Ibid. p. 305.

‡ Ibid. p. 294.

this manner, he supposes, the clouds that bring rain diffuse themselves from over those parts of the earth which abound with electric fire, to those parts that are exhausted of it; and, by letting fall their rain, restore the equilibrium between them.

SIGNIOR BECCARIA thought that the electricity communicated to the air, which both receives and parts with it slowly, would account for the retention of vapours in a clear sky; for small disjointed clouds, not dispersed into rain; for the smaller and lighter clouds in the higher regions of the air, which are but little affected by electricity; and also for the darker, heavy, and fluggish clouds in the lower regions, which retain more of it *. The degree of electricity which he could communicate to the air of his room, notwithstanding its being in contact with the floor, the walls, &c. made this appear to him both possible and probable †.

He even imagined, that some alteration in the weight of the air might be made by this electricity of it ‡. He observed his barometer to fall a little immediately upon a flash of lightning; but he acknowledges that this circumstance is no sufficient foundation to suppose that electricity will account for *much* variation of the height of the barometer §. But he thought that the phenomena of rain favoured the supposition, that the electric matter in the air did, in some measure, lessen its pres-

* Lettere dell' elettricismo, p. 348, 349.

† Ibid. p. 350.

‡ Ibid.

§ Ibid. p. 353.

sure. For when the electric matter is actually in the air, collecting and condensing the vapours, the barometer is lowest. When the communication is made between the earth and the clouds by the rain, the quicksilver begins to rise; the electric matter, which supported part of the pressure, being discharged. And this, he shows, will be the case whether the electricity in the air be positive or negative *.

HAIL, this ingenious philosopher supposes to be formed in the higher regions of the air, where the cold is intense, and where the electric matter is very copious. In these circumstances, a great number of particles of water are brought near together, where they are frozen, and in their descent collect other particles: so that the density of the substance of the hail-stone grows less and less from the center; this being formed first, in the higher regions, and the surface being collected in the lower. Agreeable to this, it is observed, that, in mountains, hail-stones, as well as drops of rain, are very small; there being but small space through which they can fall, and thereby increase their bulk. Drops of rain and hail agree also in this circumstance, that the more intense is the electricity that forms them, the larger they are †. Motion is known to promote freezing, and so the rapid motion of the electrified clouds may promote that effect in the air ‡.

* Lettere dell' elettricismo, p. 354.

† Ibid. p. 318,

‡ Ibid. p. 314.

CLOUDS of snow differ in nothing from clouds of rain, but in the circumstance of cold, which freezes them. Both the regular diffusion of snow, and the regularity in the structure of the parts of which it consists (particularly some figures of snow or hail, which he calls *rosette*, and which fall about Turin) show the clouds of snow to be actuated by some uniform cause, like electricity*. He even endeavours, very particularly, to show in what manner certain configurations of snow are made, by the uniform action of electricity†. All these conjectures about the cause of hail and snow were confirmed by observing, that his apparatus never failed to be electrified by snow, as well as by rain.

A MORE intense electricity unites the particles of hail more closely than the more moderate electricity does those of snow. In like manner, we see thunder clouds more dense than those which merely bring rain, and the drops of rain are larger in proportion, though they often fall not from so great a height‡.

I SHALL conclude this section with observing, that professor Winthrop found his apparatus to be strongly electrified for several hours, while the snow, which fell the day before (and which had not electrified his apparatus while it was falling) was driven about by a high wind the same he had observed twice before. Franklin's Letters, new edition, p. 494.

* Lettere dell' elettricismo, p. 320. 322. 325.

† Ibid. p. 325. 331. 333.

‡ Ibid. p. 328.

S E C T I O N XII.

THE ATTEMPTS THAT HAVE BEEN MADE
TO EXPLAIN SOME OF THE MORE UN-
USUAL APPEARANCES IN THE EARTH
AND HEAVENS BY ELECTRICITY.

IN the two preceding sections of this period, relating to the electricity of the atmosphere, the experiments and observations of Signior Beccaria have made a principal figure; and the materials I have collected from him make a no less considerable part of this. They who may have thought he indulged too much to imagination before, will think him absolutely extravagant here; but his extravagancies, if they be such, are those of a great genius; and had he a thousand more such extravagancies, I should, with pleasure, have followed him through them all.

THE meteor, usually called a *falling star*, has hitherto puzzled all philosophers. Signior Beccaria makes it pretty evident, that it is an electrical appearance; and the fact which he relates as a proof of it, is exceedingly curious and remarkable.

As he was one time sitting with a friend in the open air, an hour after sun-set, they saw what is called a falling star directing its course towards them, and apparently growing larger and larger, till it disappeared not far

F f

from

from them; when it left their faces, hands, and cloaths, with the earth, and all the neighbouring objects, suddenly illuminated, with a diffused and lambent light, attended with no noise at all. While they were starting up, standing, and looking at one another, surprised at the appearance, a servant came running to them out of a neighbouring garden, and asked them if they had seen nothing; for that he had seen a light shine suddenly in the garden, and especially upon the streams which he was throwing to water it*.

ALL these appearances were evidently electrical; and Signior Beccaria was confirmed in his conjecture, that electricity was the cause of them, by the quantity of electric matter which, as was mentioned before, he had seen gradually advancing towards his kite; for that, he says, had very much the appearance of a falling star. Sometimes also he saw a kind of *glory* round the kite, which followed it when it changed its place, but left some light, for a small space of time, in the place which it had quitted†.

THAT appearances, which bear evident marks of electricity, have a very sensible progressive motion, is demonstrated from a variety of meteorological observations. I shall relate one made by Mr. Chalmers, when he was on board the Montague under the command of Admiral Chambers. The account of it

* Lettere dell' elettricismo, p. 111.

† Ibid. p. 130.

was read at the Royal Society, March the 22d, 1749.

ON the 4th of November 1749, in lat. $42^{\circ} 48'$ long. $9^{\circ} 3'$ he was taking an observation on the quarter-deck, about ten minutes before twelve, when one of the quarter-masters desired he would look to the windward; upon which he observed a large ball of blue fire rolling on the surface of the water, at about three miles distance from them. They immediately lowered their top-fails, &c. but it came down upon them so fast, that before they could raise the main-tack, they observed the ball to raise almost perpendicular, and not above forty or fifty yards from the main-chains; when it went off with an explosion as if hundreds of cannon had been fired at one time, and left so great a smell of brimstone, that the ship seemed to be nothing but sulphur. After the noise was over, which, he believed, did not last longer than half a second, they found their main top-mast shattered into above a hundred pieces, and the main-mast rent quite down to the heel. There were some of the spikes which nail the fish of the main-mast drawn with such force out of the mast, and they stuck so fast in the main-deck, that the carpenter was obliged to take an iron crow to get them out. There were five men knocked down, and one of them greatly burnt by the explosion. They believed, that when the ball, which appeared to them to be of the bigness of a large mill-stone, rose, it took the middle of the main top-mast, as the

head of the mast above the hounds was not splintered. They had a hard gale of wind from the N. by W. to the N. N. E. for two days before the accident, with a great deal of rain and hail, and a large sea. From the northward they had no thunder or lightning, neither before nor after the explosion. The ball came down from the North-East, and went to the South-West.

THAT the *Aurora Borealis* is an electrical phenomenon was, I believe never disputed, from the time that lightning was proved to be one. To the circumstances of resemblance which had before been taken notice of between this phenomenon and electricity; Signior Beccaria adds, that when the *Aurora Borealis* has extended lower than usual into the atmosphere, various sounds, as of rumbling, and hissing, have been heard*.

MR. BERGMAN says, he has often observed the magnetic needle to be disturbed by a high aurora borealis, but that he could never procure any electricity from them, either with pointed metallic rods, or by means of a kite †.

MR. CANTON (besides his conjecture, mentioned before, p. 389, that the aurora borealis may be the flashing of electric fire from positive towards negative clouds at a great distance, through the upper part of the atmosphere, where the resistance is least) supposes that the aurora borealis, which happens

* Eletticismo artificiale e naturale, p. 221.

† Phil. Trans. Vol. lii. pt. ii. p. 485.

at the time that the needle is disturbed by the heat of the earth, is the electricity of the heated air above it; and this, he says, will appear chiefly in the northern regions, as the alteration in the heat of the air in those parts will be the greatest. This hypothesis, he adds, will not seem improbable, if it be considered, that electricity is now known to be the cause of thunder and lightning, that it has been extracted from the air at the time of an aurora borealis; that the inhabitants of the northern countries observe the aurora to be remarkably strong, when a sudden thaw happens after severe cold weather; and that the curious in these matters are now acquainted with a substance that will, without friction, both emit and absorb the electric fluid, only by the increase or diminution of its heat; meaning the tourmalin, in which he had discovered that property*.

IN a paper, dated November the 11th, 1754, he says he has sometimes known the air to be electrical in clear weather, but never at night, except when there has appeared an aurora borealis, and then but to a small degree, which he had several opportunities of observing that year. How far positive and negative electricity in the air, with a proper quantity of moisture between, to serve as a conductor, will account for this, and other meteors, sometimes seen in a serene sky, he leaves to be inquired into†.

* Phil. Transf. Vol. li. pt. i. p. 403.

† Ibid. Vol. xlviii. pt. ii. p. 784.

SIGNIOR BECCARIA takes some pains to show that *water spouts* have an electrical origin. To make this more evident, he first describes the circumstances attending their appearance, which are the following.

THEY generally appear in calm weather. The sea seems to boil, and send up a smoke under them, rising in a hill towards the spout. At the same time, persons who have been near them have heard a rumbling noise. The form of a water spout is that of a speaking-trumpet, the wider end being in the clouds, and the narrower end towards the sea. The size is various, even in the same spout. The colour is sometimes inclining to white, and sometimes to black. Their position is sometimes perpendicular to the sea, sometimes oblique; and sometimes the spout itself is in the form of a curve. Their continuance is very various, some disappearing as soon as formed, and some continuing a considerable time. One that he had heard of continued a whole hour. But they often vanish, and presently appear again in the same place*.

THE very same things that water spouts are at sea are some kinds of *whirlwinds* and *hurricanes* by land. They have been known to tear up trees, to throw down buildings, make caverns in the earth; and, in all these cases, to scatter earth, bricks, stones, timber, &c. to a great distance in every direction†. Great

* Eletticismo artificiale e naturale, p. 206, &c.

† Ibid. p. 210.

quantities of water have been left, or raised by them, so as to make a kind of deluge; and they have always been attended with a prodigious rumbling noise.

THAT these phenomena depend upon electricity cannot but appear very probable from the nature of several of them; but the conjecture is made more probable from the following additional circumstances. They generally appear in months peculiarly subject to thunder storms, and are commonly preceded, accompanied, or followed by lightning, rain, or hail; the previous state of the air being similar. Whitish or yellowish flashes of light have sometimes been seen moving with prodigious swiftness about them. And, lastly, the manner in which they terminate exactly resembles what might be expected from the prolongation of one of the uniform protuberances of electrified clouds, mentioned before, towards the sea; the water and the cloud mutually attracting one another: for they suddenly contract themselves, and disperse almost at once; the cloud rising, and the water of the sea under it falling to its level. But the most remarkable circumstance, and the most favourable to the supposition of their depending upon electricity is, that they have been dispersed by presenting to them sharp pointed knives or swords. This, at least, is the constant practice of mariners, in many parts of the world where these water spouts abound; and he was assured by several of them, that

the method has often been undoubtedly effectual *.

THE analogy between the phenomena of water spouts and electricity, he says, may be made visible, by hanging a drop of water to a wire communicating with the prime conductor, and placing a vessel of water under it. In these circumstances, the drop assumes all the various appearances of a water spout, both in its rise, form, and manner of disappearing. Nothing is wanting but the smoke, which may require a great force of electricity to become visible.

MR. WILCKE also considers the water spout as a kind of great electrical cone, raised between the cloud strongly electrified, and the sea or the earth †, and he relates a very remarkable appearance which occurred to himself, and which strongly confirms his supposition. On the 20th of July 1758, at three o'clock in the afternoon, he observed a great quantity of dust rising from the ground, and covering a field, and part of the town in which he then was. There was no wind, and the dust moved gently towards the East, where there appeared a great black cloud, which, when it was near his zenith, electrified his apparatus positively, and to as great a degree as ever he had observed it to be done by natural electricity. This cloud passed his zenith, and went gradually towards the West, the dust then following it, and continuing to

* *Elettricismo artificiale e naturale*, p. 213.

† Wilcke, p. 142.

rise higher and higher till it composed a thick pillar, in the form of a sugar-loaf, and at length seemed to be in contact with the cloud. At some distance from this, there came, in the same path, another great cloud, together with a long stream of smaller clouds, moving faster than the preceding. These clouds electrified his apparatus negatively, and when they came near the positive cloud, a flash of lightning was seen to dart through the cloud of dust, the positive cloud, the large negative cloud, and as far as the eye could distinguish, the whole train of smaller negative clouds which followed it. Upon this, the negative clouds spread very much, and dissolved in rain, and the air was presently clear of all the dust. The whole appearance lasted not above half an hour*.

To Signior Beccaria's theory of water spouts and hurricanes, I shall add a description of a hurricane in the West Indies, from the *Account of the European Settlements in America*, part of which is transcribed from the Philosophical Transactions. Both were evidently written without the most distant view to any philosophical theory, and least of all that of electricity; and yet those who are disposed to favour this hypothesis may perceive several circumstances, which tend to strengthen it. I need not point them out.

“ It is in the rainy season, principally in the month of August, more rarely in July

* Remarks on Dr. Franklin's Letters, p. 348.

“ and September, that they are assaulted by
“ *hurricanes*, the most terrible calamity to
“ which they are subject from the climate.
“ This destroys, at one stroke, the labour of
“ many years, and frustrates the most exalt-
“ ed hopes of the planter; and often just at
“ the moment when he thinks himself out of
“ the reach of fortune. It is a sudden and
“ violent storm of wind, rain, thunder, and
“ lightning; attended with a furious swelling
“ of the sea, and sometimes with an earth-
“ quake; in short, with every circumstance
“ which the elements can assemble that is ter-
“ rible and destructive.

“ FIRST they see, as a prelude to the en-
“ suing havock, whole fields of sugar canes
“ whirled into the air, and scattered over the
“ face of the country. The strongest trees of
“ the forest are torn up by the roots, and
“ driven about like stubble. Their wind-
“ mills are swept away in a moment. Their
“ works, their fixtures, the ponderous copper
“ boilers and stills, of several hundred weight,
“ are wrenched from the ground, and batter-
“ ed to pieces. Their houses are no protec-
“ tion: the roofs are torn off at one blast,
“ whilst the rain, which in an hour rises five
“ feet, rushes in upon them with an irresistible
“ violence.

“ THERE are signs, which the Indians of
“ these islands taught our planters, by which
“ they can prognosticate the approach of a
“ hurricane. It comes on either in the quar-
“ ters, or at the full or change of the moon.

If

“ If it will come on at the full moon, you be-
 “ ing at the change, observe these signs.
 “ That day you will see the sky very turbu-
 “ lent. You will observe the sun more red
 “ than at other times. You will perceive a
 “ dead calm, and the hills clear of all those
 “ clouds and mists which usually hover about
 “ them. In the clefts of the earth, and in
 “ the wells, you will hear a hollow rumbling
 “ sound, like the rushing of a great wind. At
 “ night the stars seem much larger than usual,
 “ and surrounded with a sort of burs. The
 “ North-west sky has a black and menacing
 “ look, and the sea emits a strong smell, and
 “ rises into vast waves, often without any
 “ wind. The wind itself now forsakes its
 “ usual steady Easterly stream, and shifts
 “ about to the West; from whence it some-
 “ times blows, with intermissions, violent-
 “ ly and irregularly, for about two hours
 “ at a time. You have the same signs at the
 “ full of the moon. The moon itself is sur-
 “ rounded with a great bur, and sometimes
 “ the sun has the same appearance *.”

THE first person who advanced that *earth-*
quakes were probably caused by electricity,
 was Dr. STUKELEY, upon occasion of the
 earthquakes at London, on February the 8th,
 and on March the 8th, 1749; and another
 which affected various other parts of England,
 the center being about Daventry in Northamp-

* Account of the European Settlements in America, Vol. ii.
 p. 96, &c. Phil. Trans. abridged, Vol. ii. p. 106.

tonshire, on the 30th of September 1750. The papers which the Doctor delivered to the Royal Society on these occasions, and which were read, March the 22d, 1749, and December the 6th, 1750, well deserve the attention of all philosophers and electricians. I shall here give the substance of both; only abridging, and differently arranging the materials of them.

THAT earthquakes are not owing to subterraneous winds, fires, vapours, or any thing that occasions an explosion, and heaves up the ground, he thought might easily be concluded from a variety of circumstances. In the first place, he thought there was no evidence of any remarkable cavernous structure of the earth; but that, on the contrary, there is rather reason to presume, that it is, in a great measure, solid; so as to leave little room for internal changes and fermentations within its substance; nor do coal-pits, he says, when on fire, ever produce any thing resembling an earthquake.

IN the second earthquake at London, there was no such thing as fire, vapour, smoke, smell, or an eruption of any kind observed, though the shock affected a circuit of thirty miles in diameter. This consideration alone, of the extent of surface shaken by an earthquake, he thought was sufficient to overthrow the supposition of its being owing to the expansion of any subterraneous vapours. For it could not possibly be imagined, that so immense a force, as could act upon that compass
of

of ground instantaneously should never break the surface of it, so as to be discoverable to the sight or smell; when small fire balls, bursting in the air, have instantly propagated a sulphureous smell all around them, to the distance of several miles.

BESIDES, the operation of this great fermentation, and production of elastic vapours, &c. ought to be many days in continuance, and not instantaneous; and the evaporation of such a quantity of inflammable matter would require a long space of time.

HE thought that if vapours and subterraneous fermentations, explosions, and eruptions were the cause of earthquakes, they would absolutely ruin the whole system of springs and fountains wherever they had once been: which is quite contrary to fact, even where they have been frequently repeated. Mentioning the great earthquake which happened A. D. 17, when no less than thirteen great cities of Asia Minor were destroyed in one night, and which may be reckoned to have shaken a mass of earth 300 miles in diameter, he asks, How can we possibly conceive the action of any subterraneous vapours to produce such an effect so instantaneously? How came it to pass, that the whole country of Asia Minor was not at the same time destroyed, its mountains reversed, its fountains and springs broken up, and ruined for ever, and the course of its rivers quite changed? Whereas, nothing suffered but the cities. There was no kind of altera-

alteration in the surface of the country, which, indeed, remains the same to this day.

To make the hypothesis of subterraneous vapours, &c. being the cause of earthquakes the more improbable, he observes, that any subterraneous power, sufficient to move a surface of earth thirty miles in diameter, as in the earthquakes which happened at London, must be lodged at least fifteen or twenty miles below the surface of the earth, and therefore must move an inverted cone of solid earth, whose basis is thirty miles in diameter, and axis fifteen or twenty miles; an effect which, he says, no natural power could produce.

UPON the same principle, the subterraneous cause of the earthquake in Asia Minor must have moved a cone of earth of 300 miles in base, and 200 in the axis; which, he says, all the gun-powder which has ever been made since the invention of it would not have been able to stir, much less any vapours, which could be supposed to be generated so far below the surface.

IT is not upon the principles of any subterraneous explosion that we can, in the least, account for the manner in which ships, far from any land, are affected during an earthquake; which seem as if they struck upon a rock, or as if something thumped against their bottoms. Even the fishes are affected by an earthquake. The stroke, therefore, must be occasioned by something that could communicate motion with unspeakably greater velocity than

than any heaving of the earth under the sea, by the elasticity of generated vapours. This could only produce a gradual swell, and could never give such an impulse to the water, as would make it feel like a stone.

COMPARING all these circumstances, Dr. Stukeley says, he had always thought, that an earthquake was an electrical shock, of the same nature with those which are now become familiar in electrical experiments. And this hypothesis he thought was confirmed by the phenomena preceding and attending earthquakes, particularly those which occasioned this publication.

THE weather, for five or six months before the first of these earthquakes, had been dry and warm to an extraordinary degree, the wind generally South and South-West, and that without rain; so that the earth must have been in a state of electricity ready for that particular vibration in which electrification consists. On this account, he observes, that the Northern regions of the world are but little subject to earthquakes in comparison with the Southern, where the warmth and dryness of the air, so necessary to electricity, are common. All the flat country of Lincolnshire before the earthquake in September, though, underneath it is a watery bog, yet, through the whole preceding summer and autumn (as they can have no natural springs in such a level), the drought had been so great on the surface of the earth, that the inhabitants were obliged to drive their cattle several miles to water.

water. This, he says, shows how fit the dry surface was for an electrical vibration; and also, which is of great importance, that earthquakes reach but very little below the surface of the earth.

BEFORE the earthquake at London, all vegetables had been uncommonly forward. At the end of February, in that year, all sorts of garden stuff, fruits, flowers, and trees were observed to be as forward as, in other years, about the middle of April; and electricity is well known to quicken vegetation.

THE aurora borealis had been very frequent about the same time, and had been twice repeated just before the earthquake, of such colours as had never been seen before. It had also removed to the South, contrary to what is common in England; so that some Italians, and people from other places where earthquakes are frequent, observing these lights, and the peculiar temperature of the air, did actually foretell the earthquake. For a fortnight before the earthquake in September, the weather was serene, mild, and calm; and, one evening, there was a deep red Aurora Borealis, covering the cope of heaven, very terrible to behold.

THE whole year had been exceedingly remarkable for fire-balls, thunder, lightning, and coruscations, almost throughout all England. Fire balls were more than once seen in Ireland and Lincolnshire, and particularly observed. And all these kinds of meteors, the Doctor says, are rightly judged to proceed

ceed from the electrical state of the atmosphere.

IN these previous circumstances of the state of the earth and air, nothing, he says, is wanting to produce the wonderful effect of an earthquake, but the touch of some non-electric body, which must necessarily be had *ab extra*, from the region of the air, or atmosphere. Hence, he infers that, if a non-electric cloud discharge its contents upon any part of the earth in that highly electrical state, an earthquake must necessarily ensue. As the discharge from an excited tube produces a commotion in the human body, so the discharge of electric matter from the compass of many miles of solid earth must needs be an earthquake, and the snap from the contact be the horrid uncouth noise attending it.

THE DOCTOR had been informed, by those who were up and abroad the night preceding the earthquake, and early in the morning, that coruscations in the air were extremely frequent; and that, a little before the earthquake, a large and black cloud suddenly covered the atmosphere, which probably occasioned the shock, by the discharge of a shower. Dr. Childrey, he says, observes, that earthquakes are always preceded by rain, and sudden tempests of rain in times of great drought.

A SOUND was observed to roll from the river Thames towards Temple Bar, before the houses ceased to nod, just as the electrical snap precedes the shock. This noise, an ob-

server said, was much greater than any he had ever heard. Others, who write upon earthquakes, commonly observe, that the noise precedes the shock: whereas it must have been quite the contrary, if the concussion had depended upon a subterraneous eruption. This noise attending earthquakes, the Doctor thought, could not be accounted for, but upon the principles of electricity. The earthquake in September was attended with a rushing noise, as if houses were falling, and people, in some places, were so universally frightened, as to run out of their houses, imagining that their own, and those of their neighbours were tumbling on their heads. In some villages, the people, being at divine service, were much alarmed with the noise; which they said, beyond all comparison, exceeded all the thunder they had ever heard.

THE flames and sulphureous smells, which are sometimes observed during earthquakes, the Doctor thought were more easily accounted for, from the supposition of their being electrical phenomena, than from their being occasioned by the eruption of any thing from the bowels of the earth.

THE impression made by an earthquake upon land and water, to the greatest distances is, as was observed before, instantaneous, which could only be effected by electricity. In the earthquake in September, the concussion was felt through a space of 100 miles in length, and forty in breadth; and, as far as could be judged, at the same instant of time.

That

That this tract of ground, which amounted to 4000 square miles in surface, should be thrown into such agitation in a moment, is such a prodigy, the Doctor says, as we could never believe, or conceive, did we not know it to be fact from our senses. But if we seek the solution of it, we cannot think any natural power equal to it but that of electricity, which acknowledges no sensible transition of time, no bounds.

THE little damage generally done by earthquakes, the Doctor thought to be an argument of their being occasioned by a simple vibration, or tremulous motion of the surface of the earth, by an electrical snap. This vibration, he says, impressed on the water, meeting with the solid bottoms of ships and lighters, occasions that thump which is said to be felt by them: yet, of the millions of ordinary houses, over which it passed, not one fell. A consideration which sufficiently points out what sort of a motion this was not; also what sort of a motion it was, and whence derived; not a convulsion in the bowels of the earth, but an uniform vibration along its surface, like that of a musical string, or what we put a drinking-glass into, by rubbing one's finger on the edge; which yet, being brought to a certain pitch, breaks the glass; undoubtedly, he adds, an electrical repulsion of its parts.

THAT earthquakes are electrical phenomena, is farther evident, he says, from their chiefly affecting the sea-coast, places along rivers, and, I may add, eminences. The

earthquake in September spread mostly to the North and South, which the Doctor says is the direction of the Spalding river, whereby it was conveyed to the sea shore, where it was particularly sensible; thence up Boston channel, and so up Boston river to Lincoln. The greatest part of this earthquake displayed its effects along, and between the two rivers Welland and Avon, and that from their sources down to their mouths. It likewise reached the river Witham, which directed the electrical stream that way also to Lincoln; for which reason, meeting the same coming from Boston, it was most sensibly felt there. It reached, likewise, to the Trent at Nottingham, which conveyed it to Newark.

THE first electrical stroke in this earthquake seemed to the Doctor to have been made on the high ground about Daventry, in Northamptonshire. From thence it descended chiefly Eastward, and along the river Welland, from Harborough to Stamford, Spalding, and the sea; and along the rivers Avon and Nen to Northampton, Peterborough, Wisbich, and the sea. It spread itself all over the vast level of the isle of Ely, promoted by a great number of canals, natural and artificial, made for draining that country. It was still conducted Eastward, by Mildenhall river in Suffolk, to Bury, and the parts adjacent. All these circumstances duly considered were to him a confirmation of the doctrine he advanced on this subject.

LASTLY,

LASTLY, the Doctor adds, as a farther argument in favour of his hypothesis, that pains in the back, rheumatic, hyſteric, and nervous caſes; head-aches, cholics, &c. were felt by many people of weak conſtitutions, for a day or two after the earthquake; juſt as they would after electrification; and, to ſome, theſe diſorders proved fatal.

IN what manner the earth and atmosphere are put into that electrical and vibratory ſtate, which prepares them to give or receive that ſnap and ſhock, which we call an earthquake, and whence it is that this electric matter comes, the Doctor does not pretend to ſay, but thinks it as difficult to account for as magnetiſm, gravitation, muſcular motion, and many other ſecrets in nature*.

To theſe obſervations of Dr. Stukeley, I ſhall add ſome circumſtances which were obſerved by Dr. Hales, in the earthquake at London, on March the 8th, 1749, as tending to ſtrengthen the hypothesis of its being cauſed by electricity; though the Doctor, who relates them, thought that the electric appearances were only occaſioned by the great agitation which the electric fluid was put into, by the ſhock of ſo great a maſs of the earth.

AT the time of the earthquake, about twenty minutes before ſix in the morning, the Doctor, being awake in bed, on a ground floor, at a houſe near the church of St. Mar-

* Phil. Tranſ. abridged, Vol. x. p. 526. 535. and p. 541. 551.

tin's in the Fields, very sensibly felt his bed heave, and heard an obscure rushing noise in the house, which ended in a loud explosion up in the air, like that of a small cannon. The whole duration, from the beginning to the end, seeming to be about four seconds.

THIS great noise, the Doctor conjectured, was owing to the rushing, or sudden expansion of the electric fluid at the top of St. Martin's spire, where all the electric effluvia, which ascended along the large body of the tower, being strongly condensed, and accelerated at the point of the weathercock, as they rushed off, made so much the louder expansive explosion.

THE Doctor farther says, that the soldiers, who were upon duty in St. James's park, and other persons who were then up, saw a blackish cloud, and a considerable lightning, just before the earthquake began *.

MR. HARTMAN is of opinion that electricity is the cause of earthquakes, and gives a succinct enumeration of all the circumstances which favour this hypothesis †.

MY reader, who has seen to how great an extent Signior Beccaria has already carried the principles of electricity, will have no doubt but that he supposes *earthquakes* to be derived from that cause. And indeed, without any knowledge of what Dr. Stukeley had done, he did suppose them to be electrical phenomena; but, contrary to the Doctor, imagined the

* Phil. Trans. abridged, Vol. x. p. 540, 541.

† Abhandlung, p. 148.

electric matter which occasioned them to be lodged deep in the bowels of the earth, agreeable to his hypothesis concerning the origin of lightning.

It is certain that if Signior Beccaria's account of the origin of thunder clouds be admitted, there will be little difficulty in admitting farther, that *earthquakes* are to be reckoned among the effects of electricity. For if the equilibrium of the electric matter can, by any means, be lost in the bowels of the earth; so that the best method of restoring it shall be by the fluid bursting its way into the air, and traversing several miles of the atmosphere to come to the place where it is wanted; it may easily be imagined, that violent concussions may be given to the earth, by the sudden passage of this powerful agent. And several circumstances attending earthquakes he thought rendered this hypothesis by no means improbable.

VOLCANOS are known to have a near connection with earthquakes; and flashes of light, exactly resembling lightning, have frequently been seen to rush from the top of Mount Vesuvius, at the time that ashes and other light matter have been carried out of it into the air, and been dispersed uniformly over a large tract of country. Of these he produces a great number of instances, from the best authority*.

* Lettere dell' elettricismo, p. 226. 362, &c.

A RUMBLING noise, like thunder, is generally heard during an earthquake. At such times, also, flashes of light have been seen rising out of the ground, and darting up into the air. Real lightning hath sometimes occasioned small shakings of the earth, at least has been attended by them. But the strongest circumstance of resemblance which he observed is the same that Dr. Stukeley lays so much stress on, viz. the amazing swiftness with which the earth is shaken in earthquakes. An earthquake, says he, is by no means a gradual heaving, as we might have expected from other causes, but an instantaneous concussion, so that the fluidity of the water is no security against the blow. The very ships, many leagues off the coast, feel as if they struck against a rock.

THIS admirable philosopher, having imitated all the great phenomena of natural electricity in his own room, doth not let the earthquake escape him. He says, that if two pieces of glass, inclosing a thin piece of metal, be held in the hand, while a large shock is sent through them, a strong vibration, or concussion will be felt; which sometimes, as in Dr. Franklin's experiments, breaks them to pieces.

SIGNIOR BECCARIA thinks, that there are traces of electrical operations in the earthquake, that happened at Julian's attempt to rebuild the temple of Jerusalem*.

* Lettere dell' elettricismo, p. 363.

THAT the electric fluid is sometimes collected in the bowels of the earth, he thought was probable from the appearance of *ignes fatui* in mines, which sometimes happens, and is very probably an electrical phenomenon *.

WHICH of these two philosophers have advanced the more probable opinion concerning the seat of the electric matter which occasions earthquakes, I shall not pretend to decide. I shall only observe that, perhaps, a more probable general hypothesis than either of them may be formed out of them both. Suppose the electric matter to be, some way or other, accumulated on one part of the surface of the earth, and, on account of the dryness of the season, not easily to diffuse itself; it may, as Signior Beccaria supposes, force itself a way into the higher regions of the air, forming clouds in its passage out of the vapours which float in the atmosphere, and occasion a sudden shower, which may farther promote the passage of the fluid. The whole surface, thus unloaded, will receive a concussion, like any other conducting substance on parting with, or receiving a quantity of the electric fluid. The rushing noise will, likewise, sweep over the whole extent of the country. And, upon this supposition, also, the fluid, in its discharge from the country, will naturally follow the course of the rivers, and also take the

* Dell' elettricismo artificiale e naturale, p. 223.

advantage of any eminences, to facilitate its ascent into the higher regions of the air.

I SHALL close this account of the theory of lightning and other phenomena of the atmosphere, with an enumeration of the principal appearances of natural electricity observed by the ancients, and which were never understood before the discovery of Dr. Franklin. It will be very easy for me to do this, as I find them already collected to my hands by Dr. Watson *.

A LUMINOUS appearance, which must have been of an electrical nature, is mentioned by Plutarch in his life of Lyfander. He considered it as a meteor.

PLINY, in his second book of Natural History, calls those appearances *stars*, and tells us, that they settled not only upon the masts, and other parts of ships, but also upon men's heads. *Exsistunt*, says that historian, *stellæ et in mari terrisque. Vidi nocturnis militum vigiliis inhærere pilis pro vallo fulgorem effigie ea: et antennis navigantium aliisque navium partibus, ceu vocali quodam sono insistent, ut volucres, sedem ex sede mutant* — *Geminæ autem salutare et prosperi cursus præ-nunciæ; quarum adventu, fugari diram illam ac minacem appellatamque Helenam ferunt. Et ob id Polluci et Castori id numen assignant, eosque in mari deos invocant. Hominum quoque capiti vespertinis horis, magno præfagio circumfulgent.*

* Phil. Transf. Vol. xlviii. pt. i. p. 210.

But, adds he, these things are *incerta ratione et in naturæ majestate abdita.*

“ STARS make their appearance both at
 “ land and sea. I have seen a light in that
 “ form on the spears of soldiers, keeping
 “ watch by night upon the ramparts. They
 “ are seen also on the sail-yards, and other
 “ parts of ships, making an audible sound,
 “ and frequently changing their places. Two
 “ of these lights forebode good weather, and
 “ a prosperous voyage; and drive away *one*
 “ that appears single, and wears a threatening
 “ aspect. This the sailors call Helen, but
 “ the *two* they call Castor and Pollux, and
 “ invoke them as Gods. These lights do
 “ sometimes, about the evening, rest on
 “ men’s heads, and are a great and good
 “ omen. But these are among the awful
 “ mysteries of nature.”

SENECA in his Natural Questions, chap. i. takes notice of the same phenomenon. *Gylip-
 po, Syracusas petenti visa est stella supra ipsam
 lancem constitisse. In Romanorum castris visa
 sunt ardere pila, ignibus scilicet in illis de-
 lapsis.*

“ A STAR settled on the lance of Gylip-
 “ pus, as he was sailing to Syracuse: and
 “ spears have seemed to be on fire in the Ro-
 “ man camp.”

IN Cæsar, de Bello Africano, cap. vi. edit. Amstel. 1686, we find them attending a violent storm. *Per id tempus fere Cæsaris exercitui res accidit incredibilis auditu; nempe*
Vir-

Virgiliarum signo confecto, circiter vigilia secunda noctis, nimbus cum saxea grandine subito est coortus ingens. Eadem nocte legionis V. pilorum cacumina sua sponte arserunt.

“ About that time, there was a very extraordinary appearance in the army of Cæsar.
 “ In the month of February, about the second watch of the night, there suddenly arose a thick cloud, followed by a shower of stones;
 “ and the same night, the points of the spears belonging to the fifth legion seemed to take fire.”

LIVY, cap. xxxii. mentions two similar facts. *In Sicilia militibus aliquot spicula, in Sardinia muro circumeunti vigilias equiti, scipionem, quem in manu tenuerat, arsisse; et litora crebris ignibus fulsisse.*

“ THE spears of some soldiers in Sicily,
 “ and a walking-stick, which a horseman in Sardinia was holding in his hand, seemed to be on fire. The shores were also luminous with frequent fires.”

THESE appearances are called, both by the French and Spaniards, inhabiting the coasts of the Mediterranean, St. Helme's, or St. Telme's fires; by the Italians the fires of St. Peter, and St. Nicholas; and are frequently taken notice of by the writers of voyages.

IF some late accounts from France, adds the Doctor, are to be depended upon, this phenomenon has been observed at Plauzet for time immemorial, and Mr. Binon, the Curé

Curé of the place says, that for twenty-seven years, which he has resided there, in great storms accompanied with black clouds, and frequent lightning, the three pointed extremities of the of the cross of the steeple of that place appeared furrounded with a body of flame; and that when this phenomenon has been seen, the storm was no longer to be dreaded, and calm weather returned soon after.

MODERN history furnishes a great number of examples of flames appearing at the extremities of pointed metallic bodies projecting into the air. Little notice was taken of these, while the cause of them was unknown; but since their near affinity with lightning has been discovered, they have been more attended to, and collected.

SECTION XIII.

OBSERVATIONS ON THE USE OF METALLIC
CONDUCTORS TO SECURE BUILDINGS,
&c. FROM THE EFFECTS OF LIGHTNING.

THE former sections of this period relate chiefly to the theory of electricity. In the two next I shall consider what has been done towards reducing this science into practice. And, in the first place, I shall recite the observations that have been made respecting the use of metallic conductors, to secure buildings from lightning, as having the nearest connection with the subject of the sections immediately preceding.

DR. FRANKLIN'S proposal to preserve buildings from the dreadful effects of lightning was by no means a matter of mere theory. Several striking facts, which occurred within the period of which I am treating, demonstrate its utility.

INNUMERABLE observations show how readily metallic rods actually conduct lightning, and how small a substance of metal is sufficient to discharge great quantities of it. Mr. Calendrini, who afterwards applied to Dr. Watson, to be informed of the best methods of securing powder magazines, says that he himself was an eye-witness of the effect of a flash of lightning, where he observed it had struck the wire of a bell, and had been completely

pletely conducted by it, from one room of a house to another, through a very small hole in the partition. This observation was prior to the discoveries of Dr. Franklin, but was recollected and recorded afterwards*.

DR. FRANKLIN himself, in a letter to Mr. Dalibard, dated Philadelphia, June the 29th, 1755, relating what had been shown him of the effects of lightning on the church of Newbury in New England, observes, that a wire not bigger than a common knitting needle, did in fact conduct a flash of lightning, without injuring any part of the building as far as it went, though the force of it was so great, that from the termination of the wire down to the ground, the steeple was exceedingly rent and damaged, some of the stones, even in the foundation, being torn out, and thrown to the distance of twenty or thirty feet. No part of the wire, however, could be found, except about two inches at each extremity, the rest being exploded, and its particles dissipated in smoke and air, as the Doctor says, like gunpowder by a common fire. It had only left a black smutty track upon the plaister of the wall along which it ran, three or four inches broad, darkest in the middle, and fainter towards the edges. From the circumstances of this fact it was very evident, that, had the wire been continued to the foot of the building, the whole shock would have been conducted without the least

* Phil. Trans. Vol. liv. pt. i. p. 203.

injury to it, though the wire would have been destroyed *.

BUT the most complete demonstration of the real use of Dr. Franklin's method of securing buildings from the effects of lightning, is Mr. Kinnerley's account of what happened to the house of Mr. West, a merchant of Philadelphia in Pensilvania, which was guarded by an apparatus constructed according to the directions of Dr. Franklin. It consisted of an iron rod, which extended about nine feet and an half above a stack of chimnies, to which it was fixed. It was more than half an inch in diameter in the thickest part, and went tapering to the upper end, in which there was a hole that received a brass wire about three lines thick and ten inches long, terminating in a very acute point: the lower part of the apparatus joined to an iron stake, driven four or five feet into the ground.

MR. WEST judging, by the dreadful flash of lightning and instant crack of thunder, that the conductor had been struck, got it examined; when it appeared, that the top of the pointed rod was melted, and the small brass wire reduced to seven inches and a half in length, with its top very blunt. The slenderest part of the wire he suspected, had been dissipated in smoke; but some of it, where the wire was a little thicker, being only melted by the lightning, sunk down (while in a fluid state) and formed a rough irregular cap, lower

* Phil. Transf. Vol. xlix. pt. i. p. 309.

on one side than on the other, round the upper end of what remained, and became intimately united with it. It is remarkable, that, notwithstanding the iron stake, in which the apparatus terminated, was driven three or four feet into the ground, yet the earth did not conduct the lightning so fast, but that, in thunder storms the lightning would be seen diffused near the stake two or three yards over the pavement, though at that time very wet with rain *.

IN order to secure ships from sustaining damage by lightning, Dr. Watson, in a letter to Lord Anson, read at the Royal Society, December the 16th, 1762, advises, that a rod of copper, about the thickness of a goose quill, be connected with the spindles and iron work of the mast; and, being continued down to the deck, be from thence, in any convenient direction, so disposed, as always to touch the sea water †.

WITH respect to powder magazines, Dr. Watson advised Mr. Calandrini above mentioned, that the apparatus to conduct the lightning from them be detached from the buildings themselves, and conveyed to the next water.

WHAT lately happened to St. Bride's church in London is a sufficient proof of the utility of metallic conductors for lightning. Dr. Watson, who published an account of this fact in the Philosophical Transactions, ob-

* Phil. Transf. Vol. liii. pt. i. p. 96.

† Ibid. Vol. liii. pt. i. p. 633.

serves, that the lightning first took a weather-cock, which was fixed at the top of the steeple; and was conducted without injuring the metal, or any thing else, as low as where the large iron bar or spindle which supported it (and which came down several feet into the top of the steeple) terminated. There, the metallic communication ceasing, part of the lightning exploded, cracked, and shattered the obelisk, which terminated the spire of the steeple, in its whole diameter, and threw off at that place several large pieces of Portland stone, of which the steeple was built. Here it likewise removed a stone from its place, but not far enough to be thrown down. From thence the lightning seemed to have rushed upon two horizontal iron bars, which were placed within the building cross each other, to give additional strength to the obelisk, almost at the base of it, and not much above the upper story. At the end of one of these iron bars, on the East and North-East side, it exploded again, and threw off a considerable quantity of stone. Almost all the damage done to the steeple, except near the top, was confined to the East and North-East side, and generally, where the ends of the iron bars had been inserted into the stone, or placed under it; and, in some places, by its violence in the stone, its passage might be traced from one iron bar to another.

It is very remarkable, that, to lessen the quantity of stone in this beautiful steeple, cramps of iron had been employed in several
parts

parts of it; and upon these, stones of no great thickness had been placed, both by way of ornament, and to cover the cramped joint. In several places these stones had, on account of their covering the iron, been quite blown off, and thrown away. A great number of stones, some of them large ones, were thrown from the steeple, three of which fell upon the roof of the church, and did great damage to it; and one of them broke through the large timbers which formed the roof, and lodged in the gallery.

UPON the whole, the steeple was found, on a survey, to be so much damaged in several of its parts, that eighty-five feet were taken down, in order to restore it substantially; and the manner in which this steeple was damaged completely indicated, as Dr. Watson observes, the great danger of insulated masses of metal from lightning; and, on the contrary, evinced the utility and importance of masses of metal continued, and properly conducted, in defending them from its direful effects. The iron and lead employed in this steeple, in order to strengthen and preserve it, did almost occasion its destruction; though, after it was struck by the lightning, had it not been for these materials keeping the remaining parts together, a great part of the steeple must have fallen.

THIS building suffered the more, on account of the thunder storm having been preceded by several very warm days. The nights had scarce furnished any dew, the air

was quite dry, and in a state perfectly unfit to part with its highly accumulated electricity, without violent efforts. This great dryness had made the stones of St. Bride's steeple, and all other buildings under the like circumstances, far less fit, than if they had been in a moist state, to conduct the lightning, and prevent the mischief. For, though the thunder storm ended in a heavy shower of rain, none, except a few very large drops, fell till after the church was struck. And Dr. Watson had no doubt, but that the succeeding rain prevented many accidents of a similar kind, by bringing down, with every drop of it, part of the electric matter, and thereby restoring the equilibrium between the earth and clouds.

It is frequently observed, he says, that, in attending to the apparatus for collecting the electricity of the clouds, though the sky is much darkened, and there have been several claps of thunder, at no great distance, yet the apparatus will scarcely be affected by it; but that, as soon as the rain begins and falls upon so much of the apparatus as is placed in the open air, the bells belonging to it will ring, and the electrical snaps succeed each other in a very extraordinary manner. This, as he observes, demonstrates, that every drop of rain brings down part of the electric matter of a thunder cloud and dissipates it in the earth and water, thereby preventing the mischiefs of its violent and sudden explosion. Hence when the heavens have a menacing appearance, a shower of rain is much to be wished for.

FROM all these considerations, Dr. Watſon had no doubt, but that the miſchief done to St. Bride's ſteeple was owing to the efforts of the lightning, after it had poſſeſſed the apparatus of the weathercock, endeavouring to force itſelf a paſſage from thence to the iron work employed in the ſteeple. As this muſt be done *per ſaltum*, as he expreſſes it, there being no regular metallic communication, it was no wonder, when its force was vehement, that it rent every thing which was not metallic that obſtructed its eaſy paſſage; and that, in this particular inſtance, the ravages increaſed, as the lightning, to a certain diſtance, came down the ſteeple.

THE Doctor adviſes that, in order to have ocular demonſtration when theſe metallic conductors do really diſcharge the lightning, they be diſcontinued for an inch or two, in ſome place convenient for obſervation; in which caſe the fire will be ſeen to jump from one extremity of the wire to the other. If any danger be apprehended from this diſcontinuance of the metallic conductor, he ſays that a looſe chain may be ready to hang on, and complete the communication *.

MR. DELAVAL, who alſo gives an account of the ſame accident, obſerved, that, in every part of the building that was damaged, the lightning had acted as an elaſtic fluid, endeavouring to expand itſelf where it was accumulated in the metal; and that the effects

* Phil. Tranſ. Vol. liv. p. 201, &c.

were exactly similar to those which would have been produced by gunpowder pent up in the same places, and exploded.

IN the same paper Mr. Delaval gives it as his opinion, that a wire, or very small rod of metal, did not seem to have been a canal sufficiently large to conduct so great a quantity of lightning as struck this steeple; especially if any part of it, or of the metal communicating with it, was inclosed in the stone work, in which case, he thought, the application of it would tend to increase its bad effects, by conducting it to parts of the building which it might otherwise not have reached.

UPON the whole, he thought that a conductor of metal, less than six or eight inches in breadth, and a quarter of an inch in thickness (or an equal quantity of metal in any other form that might be found more convenient) cannot with safety be depended on, where buildings are exposed to the reception of a great quantity of lightning*.

MR. WILSON, in a paper written upon the same occasion, advises, that pointed bars or rods of metal be avoided in all conductors of lightning.

As the lightning, he says, must visit us some way or other, from necessity, there can be no reason to invite it at all; but, on the contrary, when it happens to attack our buildings, we ought only so to contrive our apparatus, as to be able to carry the lightning

* Phil. Transf. Vol. liv. p. 234.

away again, by such suitable conductors, as will very little, if at all, promote any increase of its quantity.

To attain this desirable end, in some degree at least, he proposes, that the several buildings remain as they are at the top; that is, without having any metal above them, either pointed or not, by way of conductor; but that on the inside of the highest part of the building, within a foot or two of the top, a rounded bar of metal be fixed, and continued down along the side of the wall, to any kind of moisture in the ground*.

SIGNIOR BECCARIA whose observations and experience with respect to lightning give a weight to his opinion superior to that of any other man whatever, seems to think very differently from Mr. Wilson on this subject. He says that no metallic apparatus can attract more lightning than it can conduct. And so far is he from thinking one conductor, rounded at the top, and a foot or two under the roof sufficient; that if the building be of any extent, he advises to have several of the usual form; that is, pointed, and higher than the building. One conductor he thought sufficient for one tower, steeple, or ship; but he thought, two necessary for the wing of a building 200 feet long, one at each extremity; three for two such wings, the third being fixed in the middle; and four for a square palace of the same front, one at each corner†.

* Phil. Trans. Vol. liv. p. 249.

† Lettere dell' elettricismo, p. 278.

My readers at a distance from London will hardly believe me, when I inform them, that the elegant spire which has been the subject of a great part of this section, and which has been twice damaged by lightning (for it is now very probable, that a damage it received in the year 1750, was owing to the same cause) is now repaired, without any metallic conductor, to guard it in case of a third stroke.

S E C T I O N XIV.

OF MEDICAL ELECTRICITY

THE subject of medical electricity falls almost wholly within the period of which I am now treating. For, though some effects of electricity upon animal bodies had been noted by the Abbé Nollet, and a few diseased persons had said they had received benefit from being electrified; yet very little had been done this way, and physicians had scarcely attended to it, till within this period; whereas electricity is now become a considerable article in the *materia medica*.

THE first account I have met with of the application of electricity to medical purposes is of Mr. C. Kratzenstein, professor of medicine at Halle; who, in the year 1744, cured a woman of a contracted little finger in a quarter of an hour. He also so far relieved a person who had two lame fingers, by once electrifying them, that he could play upon
the

the harpsicord, which he had before been disabled from doing. He also observed, that a man's pulse, which had beat eighty in a second before he was electrified, immediately after beat eighty-eight, and was presently increased to ninety-six *.

THERE is, another celebrated instance of the cure of a palsy before this period; which is that performed by Mr. Jallabert, professor of philosophy and mathematics at Geneva, on a lock-smith of the age of fifty-two †, whose right arm had been paralytic fifteen years, occasioned by a blow of a hammer. He was brought to Mr. Jallabert on the 26th of December 1747, and was almost completely cured by the 28th of February 1748. In this interval he was frequently electrified, sparks being taken from the arm, and sometimes the electric shock sent through it ‡. Mr. Jallabert's own account of this cure is very circumstantial. But it appears from the Abbé Nollet's account of his second journey to Italy, that this person relapsed to the condition in which Mr. Jallabert found him. See the French translation of this book, vol. ii. p. 396.

THE report of this cure performed at Geneva engaged Mr. Sauvages of the Academy in Montpellier to attempt the cure of paralytics, in which he had considerable success. In one case it occasioned a salivation, and in

* Dantzick Memoirs, Vol. i. p. 294.

† Jallabert's Experiences, 143.

‡ Histoire, pt. iii. p. 36.

another a profuse sweat. Many paralytics, however, were electrified without any success. Indeed the prodigious concourse of patients of all kinds, which the report of these cures brought together, was so great, that few of them could be electrified, except very imperfectly. For two or three months together, twenty different patients were electrified every day. It is not surprising to find, that the neighbouring populace considered these cures as an affair of witchcraft, and that the operators were obliged to have recourse to their priests to undeceive them *. In the course of these experiments it was found, by very accurate observations made with a pendulum, that electrification increases the circulation of the blood about one sixth.

ONE of the first who attended to electricity in a medical way was Dr. Bohadtch a Bohemian ; who, in a treatise upon medical electricity, communicated to the Royal Society, gave it as his opinion, after the result of much experience, that of all distempers the *hemiplegia* seemed to be the most proper object of electricity. He also thought it might be of use in intermitting fevers †.

THE palsy having happened to be the first disorder in which electricity gave relief, there was a considerable number of cases published pretty early, in which paralytics were said to have found benefit from this new method of treatment. In the year 1757, Mr. Patric

* Histoire, p. 97.

† Phil. Transf. Vol. xlvii. p. 351.

Brydone performed a complete cure of a hemiplegia, and, indeed, an almost universal paralytic affection, in about three days. The patient was a woman, aged thirty-three, and the palsy was of about two years continuance *. And John Godfrey Teske, very nearly cured a young man, of the age of twenty, of a paralytic arm, of which he had not had the least use from the age of five years †.

THE Abbé Nollet's experiments upon paralytics had no permanent good effect ‡. He observes, however, that, during fifteen or sixteen years that he had electrified all sorts of persons, he had known no one bad effect to have arisen from it to any of them §.

DR. HART, in a letter to Dr. Watson, dated Salop, March the 20th, 1756, mentions a cure performed by electricity upon a woman of twenty-three years of age, whose hand and wrist had for some time been rendered useless by a violent contraction of the muscles. She was not sensible of the first shock that was given her; but, as the shocks were repeated, the sensation increased, till she was perfectly well. She was also cured a second time, after a relapse occasioned by a cold ||.

BUT perhaps the most remarkable case that has yet occurred of the use of electricity in curing a disorder of this kind, and indeed of any that is incident to the human body was

* Phil. Transf. Vol. l. pt. i. p. 392.

† Ibid. Vol. li. pt. i. p. 179.

‡ Recherches, p. 412.

§ Ibid. p. 416.

|| Phil. Transf. Vol. xlix. pt. ii. p. 558.

of that dreadful disorder, an universal *tetanus*. It is related by Dr. Watson in the Philosophical Transactions; and the account was read at the Royal Society the 10th of February 1763. The patient was a girl belonging to the Foundling hospital, about seven years of age, who was first seized with a disorder occasioned by the worms, and at length by an universal rigidity of her muscles; so that her whole body felt more like that of a dead animal than a living one. She had continued in this dismal condition above a month, and about the middle of November 1762, after all the usual medicines had failed, Dr. Watson began to electrify her; and continued to do it by intervals, till the end of January following; when every muscle of her body was perfectly flexible, and subservient to her will, so that she could not only stand upright, but could walk, and even run like other children of her age*.

DR. EDWARD SPRY relates a complete cure which he made of a locked jaw and paralysis, in the case of a girl of eighteen years of age. Small shocks were given to the muscles particularly affected†.

THAT electricity may be hurtful, and even in some cases in which analogy would lead us to promise ourselves it might be of use, is evident from many cases, and particularly from one related by Dr. Hart of Shrewsbury, in a letter to Dr. Watson, which was read at the Royal Society, November the 14th, 1754.

* Phil. Transf. Vol. liii. p. 10.

† Ibid. Vol. lvii. p. 88.

A YOUNG girl about sixteen, whose right arm was paralytic, and greatly wasted in comparison of the other, on being electrified twice, became universally paralytic, and remained so above a fortnight; when the new palsy was removed by proper medicines, though the first diseased arm remained as before.

However Dr. Hart, notwithstanding this bad accident, had a mind to try electricity again. The girl submitted to it, but after having been electrified about three or four days, she became a second time universally paralytic, and even lost her voice, and the use of her tongue, so that it was with great difficulty she could swallow. She was relieved of this additional palsy a second time by a proper course of medicines, continued about four months; but was discharged out of the hospital as incurable of her first palsy. It is said that the Doctor would have tried electricity a third time; but the girl, being more nearly concerned in the experiment than her physician, thought proper to decline it*.

DR. FRANKLIN's account of the effects of electricity, in the manner in which he applied it, is by no means favourable to its use in such cases. He says, in a letter to Sir John Pringle, read at the Royal Society, January the 12th, 1758, that some years before, when the news-papers made mention of great cures performed in Italy and Germany by electri-

* Phil. Transf. Vol. xlviii. pt. ii. p. 785.

city, a number of paralytics were brought to him, from different parts of Pennsylvania and the neighbouring provinces, to be electrified, and that he performed the operation at their request. His method was, first to place the patient in a chair, or upon an electrical stool, and draw a number of large strong sparks from all parts of the affected limb or side. He then fully charged two six-gallon glass jars, and sent the united shock of them through the affected limb or limbs, repeating the stroke commonly three times each day.

THE first thing he observed was an immediate greater sensible warmth in the affected limbs, which had received the stroke, than in the others; and the next morning the patients usually said, that, in the night, they had felt a prickling sensation in the flesh of the paralytic limbs; and would sometimes show a number of small red spots, which they supposed were occasioned by those prickings. The limbs too were found more capable of voluntary motion, and seemed to receive strength. A man, for instance, who could not, the first day, lift his lame hand from off his knee, would the next day raise it four or five inches; the third day higher; and on the fifth was able, but with a feeble languid motion, to take off his hat. These appearances, the Doctor says, gave great spirits to the patients, and made them hope for a perfect cure; but he did not remember that he ever saw any amendment after the fifth day; which

the patients perceiving, and finding the shocks pretty severe, became discouraged, went home, and in a short time relapsed; so that he never knew any permanent advantage from electricity in palsies.

PERHAPS, says he, some permanent advantage might be obtained, if the electric shocks had been accompanied with proper medicine and regimen, under the direction of a skilful physician. He thought too, that many small shocks might have been more proper than the few great ones which he gave; since, in an account from Scotland, a case was mentioned in which 200 shocks from a phial were given daily, and a perfect cure had been made*.

THAT there is an intimate connection between the state of electricity in the air and the human body, is evident from several facts, particularly a very remarkable one related by the Abbé Mazeas, in a letter to Dr. Hales. He was electrifying a person who was subject to epileptic fits, by his apparatus to make observations upon the electricity of the common atmosphere. At first this person bore the sparks very well, but in two or three minutes the Abbé, perceiving his countenance to change, begged he would retire, lest any accident should happen; and he was no sooner returned home, than his senses failed him, and he was seized with a most violent fit. His convulsions were taken off with spirits of

* Phil. Trans. Vol. l. pt. ii. p. 481.

hartshorn, but his reason did not return in an hour and an half. He went up and down the stairs like one who walks in his sleep, without speaking to, or knowing any person, settling his papers, taking snuff, and offering chairs to all who came in. When he was spoken to, he pronounced inarticulate words, which had no connection.

WHEN this poor man recovered his reason, he fell into another fit; and his friends told the Abbé, he was more affected with that distemper when it thundered than at any other time; and if it ever happened, which it rarely did, that he then escaped, his eyes, his countenance, and the confusion of his expressions, sufficiently demonstrated the weakness of his reason.

THE next day, the Abbé learned from the person himself, that the fear of thunder was not the cause of his disease; but that, however, he found a fatal connection between that phenomenon and his distemper. He added, that when the fit seized him, he perceived a vapour rising in his breast, with so much rapidity, that he lost all his senses before he could call for help*.

MR. WILSON cured a woman of a deafness of seventeen years standing. He also observes, that she had a very great cold when she began to be electrified; but that the inflammation ceased the first time, and the cold was quite gone when the operation had been

* Phil. Transf. Vol. xlviii. pt. i. p. 383.

performed again the second day. But he acknowledges, that he had tried the same experiment upon six other deaf persons without any success *.

THE same person observes, that one gentleman, near seventy years old, could never be made to receive a shock except in his wrists. He says that he himself could once have borne very great shocks without inconvenience, but that he could not bear them at the time that he wrote.

MEDICAL electricity is very much obliged to the labours and observations of Mr. Lovet, lay-clerk of the cathedral church at Worcester, who has for many years been indefatigable in the application of electricity to a great variety of diseases. His success has been very considerable, and all the cases he has published seem to be well authenticated.

ACCORDING to Mr. Lovet, electricity is almost a specific in all cases of violent pains, of however long continuance, in every part of the body; as in obstinate head-achs, the sciatica, the cramp, and disorders resembling the gout. He had no trials of the proper gout, but only on those who were slightly attacked, and who received immediate relief.

THE tooth-ach, he says, is generally cured instantly, and he scarce ever remembered any one who complained of its raging a minute after the operation †.

* Wilson's Essay, p. 207.

† Lovet's Essay, p. 112.

It has seldom failed, he says, to cure rigidities, or a wasting of the muscles, and hysterical disorders, particularly if they be attended with coldness in the feet. According to him, it cures inflammations, it has stopped a mortification, cured a fistula lachrymalis, and dispersed extravasated blood *. He also says it has been of excellent use in bringing to a suppuration, or in dispersing without suppuration, obstinate swellings of various kinds, even those that were scrophulous. In his hands it cured the falling sickness, and several kinds of fits, though the patients had been subject to them for many years; and one cure he mentions of a hemiplegia †. Lastly, he relates a well attested case, from Mr. Floyer, surgeon at Dorchester, of a complete cure of what seemed to be a *gutta serena*. The same Mr. Floyer, he also says, cured with it two young women of obstructions, one of whom had taken medicines a year to no purpose ‡.

In the rheumatism, Mr. Lovet candidly confesses, it has failed; but he says it was seldom in the case of young persons, if they were taken in time.

THE manner in which electricity operated in these cures, Mr. Lovet imagined to be, by removing secret obstructions, which are probably the cause of those disorders. In all his practice he never knew an instance of harm being done by it, and thinks that, in all the cases in which it has done harm, the manner

* Lovet's Essay, p. 76.

† Ibid. p. 119.

‡ Ibid. p. 101.

of administering it has been injudicious. In general, he thinks the shocks have been made too great. This he imagined to have been the case of the patient before mentioned of Dr. Hart, who was made more paralytic by electric shocks. Mr. Lovet advises to begin, in general, with simple electrification, especially in hysterical cases; then to proceed to taking sparks, and lastly to giving moderate shocks, but hardly ever any that are violent, or painful.

THE account of the application of electricity by Dr. Zetzel of Upsal, which may be seen in Mr. Lovet's treatise, agrees in the main with the result of his own practice; and where there is any difference between them, Mr. Lovet thinks there are evident marks of unfairness in the Swedish account. And a subsequent account from Sweden mentions several cures being made in those very cases, in which Dr. Zetzel says that no relief was to be had from electricity.

THE Rev. Mr. J. Wesley has followed Mr. Lovet in the same useful course of medical electricity, and recommends the use of it to his numerous followers, and to all people. Happy it is when an ascendancy over the minds of men is employed to purposes favourable to the increase of knowledge, and to the best interests of mankind. Mr. Wesley's account of cures performed by electricity agrees very well with that of Mr. Lovet, whom he often quotes. He adds, that he has scarce ever known an instance in which shocks all

over the body have failed to cure a quotidian or tertian ague*. He mentions cases of blindness cured or relieved by it; and says that he has known hearing given by it to a man that was born deaf†. He mentions cures in cases of bruises, running sores, the dropsy, gravel in the kidneys (causing the patient to part with it) a palsy in the tongue, and lastly in the genuine consumption. But Mr. Boissier says it is of disservice in phthical complaints‡.

MR. WESLEY candidly says, he has not known any instance of the cure of an hemiplegia; and though many paralytics have been helped by electricity, he scarcely thinks that any palsy of a year's standing has been thoroughly cured by it. He asserts, however, that he has never yet known any person, man, woman, or child, sick or well, who has found (what Mr. Wilson says, that some persons complained of) an unusual pain some days after the shock. Mr. Wesley had only known that the rheumatic pains, which were afterwards perfectly cured, had increased on the first or second application§.

MR. WESLEY directs the same method of administration with Mr. Lovet. In deep hysterical cases, he advises that the patients be simply electrified, sitting on cakes of rosin, at least half an hour morning and evening;

* Wesley's Desideratum, p. 3.

† Ibid. p. 48.

‡ Carmichael Tentamen inaugurale medicum de Paralyfi, p. 34. ex Act. Ups.

§ Wesley's Desideratum, p. 50.

when, after some time, small sparks may be taken from them, and afterwards shocks given to them, more or less strong, as their disorder requires; which, he says, has seldom failed of the desired effect*.

THIS account of the medical use of electricity by Mr Lovet and Mr. Wesley is certainly liable to an objection, which will always lie against the accounts of those persons who, not being of the faculty, cannot be supposed capable of distinguishing with accuracy either the nature of the disorders, or the consequences of a seeming cure. But, on the other hand, this very circumstance of their ignorance of the nature of disorders, and consequently of the best method of applying electricity to them, supplies the strongest argument in favour of its innocence at least. If in such unskilful hands it has produced so much good, and so little harm; how much more good, and how much less harm would it probably have produced in more skilful hands.

BUT whatever weight there be in this objection against the last mentioned writers, it certainly cannot be urged against Antonius de Haen, one of the most eminent physicians of the present age; who, after six years uninterrupted use of it, reckons it among the most valuable assistances of the medical art; and expressly says, that though it has often been applied in vain, it has often afforded relief

* Wesley's Desideratum, p. 56.

where no other application would have been effectual. But I shall recite in a summary manner, from his *Ratio Medendi*, the result of all his observations on this subject.

WITH respect to partial palsies, in particular, he says, it never did the least harm; that one or two persons who had received no benefit from it in six intire months, were yet much relieved by persevering in the use of it. That some persons discontinuing it, after having received some benefit, relapsed again; but afterwards, by recurring to the use of electricity, recovered, though more slowly than before. Some persons, he says, were relieved who had been paralytic one, three, six, nine, and twelve years, and some longer; but that in one or two of these cases, the patients had received less relief, and more slowly than was usual in recent cases. In some cases, he says, a most unexpected benefit had been found by those who had been paralytic in their tongues, eyes, fingers, and other particular limbs. A paralysis and trembling of the limbs, from whatever cause it arose, he says, never failed to be relieved by it; and he relates one instance of a perfect cure being performed in a remarkable case of this nature, after receiving ten shocks *.

DE HAEN's custom was to apply the operation for half an hour together at least. He seems to have used gentle shocks, and he joined to electricity, the use of other remedies,

* *Ratio Medendi*, Vol. i. p. 234, 199.

which,

which, however, would not have been effectual without it *.

ST. VITUS's dance, he says, never failed to be cured by electricity †. He always observed it to promote a more copious discharge of the menses, and to relieve in cases of obstruction; but, for this reason, he advises that it be not administered to women with child. He found it of use in some cases of deafness, but it entirely failed in its application to a gutta serena, and to a strumous neck ‡.

LASTLY, he relates a remarkable case, communicated to him by Mr. Velse at the Hague, of the cure of a mucous apoplexy §.

To the cases which have been mentioned occasionally, in which harm may be apprehended from electrification, may perhaps be added the venereal disease in which Mr. Verratti advises, that electrification be by all means avoided ||.

I SHALL conclude this account of medical electricity with observing, that there are two general effects of electricity on the human body, of which, it should seem, that physicians might greatly avail themselves. These are, that it promotes insensible perspiration, and glandular secretion. The former is effected by simple electrification, and the latter

* Ratio Medendi, Vol. i. p. 233.

† Ibid. p. 389.

‡ Ibid. Vol. ii. p. 200.

§ Ibid.

|| Carmichael Tentamen, p. 34.

by taking sparks from the glands, or the parts contiguous to them; on which it acts like a stimulus. Of the former, instances have been produced in the experiments of the Abbé Nollet, and a few have been given occasionally of the latter.

To these I shall now add, that Linnæus observed, that when electric sparks have been drawn from the ear, it has instantly promoted a more copious secretion of ear-wax; and that it has also been observed, that, when the eye, or the parts about the eye, have been electrified, the tears have flowed copiously. But the most remarkable case that I have met with, is, of its promoting the secretion of that matter which forms the hair; whereby hair has been actually restored to a part that had long been bald*.

HITHERTO electricity has been generally applied to the human body either in the method of drawing sparks, as it is called, or of giving shocks. But these operations are both violent, and though the strong concussion may suit some cases, it may be of disservice in others, where a moderate simple electrification might have been of service.

THE great objection to this method is the tediousness and expence of the application. But an electrical machine might be contrived to go by wind or water, and a

* Carmichael Tentamen, p. 33.

convenient room might be annexed to it; in which a floor might be raised upon electrics, and a person might sit down, read, sleep, or even walk about during the electrification. It were to be wished, that some physician of understanding and spirit would provide himself with such a machine and room. No harm could possibly be apprehended from electricity, applied in this gentle and insensible manner, and good effects are, at least, possible, if not highly probable. It would certainly be more for the honour of the faculty, that the practice should be introduced in this manner, than that it be left to some rich Valetudinarian, who may take it into his head, that such an operation may be of service to him.

SECTION XV.

MISCELLANEOUS EXPERIMENTS AND DISCOVERIES MADE WITHIN THIS PERIOD.

HAVING distributed into distinct sections all the subjects, under which I had collected materials enow to form a separate account; I have reserved for the last place, those smaller articles, which could neither with propriety be introduced under the former heads, nor were large enough to make a section themselves.

IT has been a great controversy among electricians, whether glass be permeable to the electric fluid. Mr. Wilson appeared in favour of the permeability, and, in a paper read at the Royal Society, December the 6th, 1759, produced the following experiments to support his opinion; notwithstanding he, even afterwards, acknowledged, in a paper read at the Royal Society, November the 13th, 1760; that, in the Leyden experiment, Dr. Franklin had proved that the fluid did not go through the glass *.

HE took a very large pane of glass, a little warmed; and holding it upright by one edge, while the opposite edge rested upon wax, he rubbed the middle part of the surface

* Phil. Transf. Vol. li. pt. ii. p. 896.

with

with his finger, and found both sides electrified *plus* *.

UPON this I cannot help observing that it ought to be so on Dr. Franklin's principles. If one side be rubbed by the finger, it acquires from the finger some of the electric fluid. This, being spread on the glass as far as the rubbing extended, repels an equal quantity of that contained in the other side of the glass, and drives it out on that side, where it stands as an atmosphere, so that both sides are found *plus*. If the unrubbed side were in contact with a conductor communicating with the earth, the electric fluid would be carried away, and then that side would be left *apparently* in the natural state. If the electric fluid found on the unrubbed side was really part of that which had been communicated by and from the finger, and so had actually *permeated* the glass, it might, when conducted away, be continually replaced by fresh permeating fluid communicated in the same manner: But if the effect is continually diminishing, while the supposed cause, repeated, continues the same, there seems reason to doubt the supposed relation between that cause and the effect. For it appears difficult to conceive how some electric fluid, having passed through a permeable body, should make it more difficult for other particles of the same electric fluid to follow, till, at length, none could pass at all.

* Phil. Transf. Vol. li. pt. i. p. 314.

MR. WILSON also says, that, holding [the same pane of glass within two feet of the prime conductor, which was electrified *plus*, that part of the glass which was opposite to the conductor became electrified *minus* on both sides; but, in a few minutes, the *minus* electricity disappeared, and the *plus* continuing, diffused itself into the place of the other, so that now the whole was electrified *plus*.

THE experiment so far succeeding, induced him to make use of a less piece of glass, that the whole might be electrified *minus*. These advances, he says, led him to observe the power of electrifying that small piece of glass at different distances.

HE exposed the same small piece of glass to the prime conductor, at the distance of two feet, and observed a *minus* electricity at both surfaces.

As he moved the glass nearer, to a certain distance, it was more sensibly electrified *minus*; and after that, on moving it still nearer, the *minus* appearance was less and less sensible; till it came within the distance of about one inch, and then it was electrified *plus* on both sides.

THIS *plus* electricity in the glass, he found, might be changed to a *minus* again, by removing the glass, and holding it for a time at a greater distance; which he thought to be a proof of the repulsive power of that fluid *.

HAVING by him a pane of glass, one side of which was rough and the other smooth, he

* Phil. Transf. Vol. li. pt. i. p. 328.

rubbed it slightly on one side; upon doing which, both sides were electrified *minus*.

ON this I must also take the liberty to observe that, as the electric fluid contained in glass in its natural state, is kept equal in both sides by the common repulsion; if the quantity in one side is diminished, the fluid in the other side, being less repelled, retires inward, and leaves that surface also *minus*.

SLIGHT changes, *plus* or *minus*, may be made in either surface, that have not strength to act on the other side, by repulsion, or by abating repulsion, through the glass; and so *plus* electricity may be given to one surface, and *minus* to the other in some degree. Both sides may also be made *plus*, and both *minus*, by rubbing, or by communication, without any necessity of supposing the glass *permeable*.

AND yet it is probable that some glass, from having a greater mixture of non-electric matters in its composition, may be permeable, when cold, in some small degree, as all glass is found to be when warmed.

MR. WILSON treated the other side of this pane of glass in the same manner, after which the *minus* electricity was changed into a *plus* on both sides.

THOUGH Dr. Franklin was of opinion, that glass when cold is not permeable to electricity, he had made no experiments upon it when hot; but Mr. Kinnerfley, a friend of his, made one, which seemed to prove, that it was very differently affected in this respect, in the different states of hot and cold. He found,
that

that a coated Florence flask (made of very thin glass, and full of air bubbles) containing boiling water, could not be electrified. The electricity, he says, passed as readily through it as through metal. The charge of a three pint bottle went freely through it, without injuring the flask in the least. When it became cold, he could charge it as before. This effect he attributed to the dilatation of the pores of glass by heat *.

ALL Mr. Wilson's experiments to prove the permeability of glass were repeated by Mr. Bergman of Upsal; and, as he says, with success †.

MR. ÆPINUS, however, was by no means satisfied with Mr. Wilson's experiments concerning the permeability of glass; and yet he brings no other fact in answer to his arguments, but a very common one, which shows that a glass tube both receives and loses its electricity very slowly; so that he only asserts a *difficulty*, and a *slowness* in the electric fluid passing through electric substances, as was mentioned before; and consequently Mr. Wilson seems to have an advantage in the controversy: for, as he says, passing through, though ever so slowly, is a real passing through ‡.

MR. ÆPINUS has shown, by a curious experiment, that though a metallic conductor and a cork ball be both electrified positively,

* Phil. Transf. Vol. liii. pt. i. p. 85.

† Ibid. Vol. lii. pt. ii. p. 485.

‡ Ibid. Vol. liii. p. 443.

so as to repel one another; yet, that, if the ball be forcibly brought within two, three, or four lines of the conductor, it will be attracted by it; and that it will be repelled again, if it be forcibly pushed beyond that limit of attraction. If the ball be confined to move within the same small distance, a moderate electrification of the conductor will repel the ball to its utmost limit; but a stronger electrification of the conductor will cause it to be attracted. He, therefore, limits the general maxim, that bodies possessing the same kind of electricity repel one another; and asserts, that this will be the case, only when the quantity of electric fluid belonging to them both, as one body is greater, or less than that which is natural to them*. This experiment deserves particular attention.

SIGNIOR BECCARIA, who has contributed so largely to several former sections in this period, furnishes a few articles which well deserve a place in this.

He thought it was evident, that the electric fluid tended to move in a right line, because a longer spark may be taken in a direct line, from the end of a long conductor, than can be taken from the same place in any other direction. But he thought it was still more evident, from observing, both in the air, and in vacuo; that, presenting the finger, or a brass ball, at a proper distance, and in a certain angle with the conductor (which experi-

* *Æpini Tentamen*, p. 146.

ence will soon find) the electric spark will make an exact curve, to which the conductor produced will be a tangent: as if the electric matter was acted upon by two different forces, one its own acquired velocity, urging it forward in a right line; the other the attraction of the body presented to it, which throws it out of the right line*.

IN his observations on pointed bodies, he says, that two pointed bodies, equally sharp, in their approach to an electrified conductor, will appear luminous only at half the distance at which one of them would have done †.

THE same ingenious philosopher reports a curious, but cruel experiment which he made on a live cock. He detached the belly of one of the muscles from the thigh of the animal, leaving the extremities in their proper insertions, and then discharging a shock through it. At the instant of the stroke, the leg was violently distended, and the muscle greatly inflated; the motion beginning at the tendon, and the extension of it resembling the opening of a lady's fan. No pricking with a pin could make it act so strongly ‡.

I MUST not omit to mention, in this chapter of miscellaneous experiments, what the Dutch writers have reported concerning the gymnotus, a fish peculiar to Surinam, which very much resembles what naturalists relate concerning the torpedo. Mr. Muschenbroeck

* *Elettricismo artificiale*, p. 56.

† *Ibid.* p. 67.

‡ *Lettere dell' elettricismo*, p. 129.

says, the gymnotus is possessed of a kind of natural electricity, but different from the common electricity, in that persons who touch it in water are shocked, and stunned by it, so as to be in danger of drowning. The fish has been taken, and put into a vessel; when experiments were made upon it at leisure; and it was found, that it might be touched with all safety with a stick of sealing-wax; but if it was touched with the naked finger, or with a piece of metal, and especially a gold ring, held in the fingers, the arm was shocked as high as the elbow. If it was touched with the foot, the sensation reached as high as the knee, and the pain was as great as if the part had been struck with something hard. This kind of electricity is the same by night or by day, when the wind is in every direction, when the fish was put in vessels of any materials, and whether it was in water or out of water. Every part of the body of the fish is capable of giving this shock, but more especially the tail. The sensation is the strongest when the fish is in motion, and it is transmitted to a great distance; so that if persons in a ship happen to dip their fingers or feet in the sea, when the fish is swimming at the distance of fifteen feet from them they are affected by it. Other fishes, put into the same vessel with it, presently died; but it is itself killed by the lobster. The gymnotus is found in the upper part of the river of Surinam, particularly the

rocky part of it. It feeds upon all kinds of fish, and will even eat bread. This author proposes as a query whether the sensation communicated by the torpedo does not depend upon a similar electricity; since Monsieur Reaumur says, that when it is touched, the hand, arm, and shoulder are seized with a sudden stupor, which lasts for some time; and is unlike any other sensation *.

THIS gymnotus, I suppose, is a different fish from the *Anguille tremblante*, the *trembling eel*, which is also a native of Surinam, and lives in marshy places, from whence it cannot be drawn, except when it is intoxicated. It cannot be touched with the hand, or with a stick, without feeling a terrible stun, which reaches as high as the shoulder. If it be trod upon with shoes, the legs and thighs are affected in a similar manner. Fourteen persons joining hands, and the first of them touching it with a stick, they were all shocked violently. It is conjectured that this power of giving a shock resides in two muscles, which are particularly prominent and conspicuous †.

IT is to be regretted, that none of the persons who have made experiments on these fishes should have endeavoured to ascertain whether they were capable of exhibiting the phenomena of attraction and repulsion, or

* Muschenbroeck's Introd. ad philosoph. naturalem, No. 901—909.

† P. Fermin's Nat. Hist. of Surinam, p. 59.

the appearance of electric light, as experiments of this kind are of principal consequence, and must have been very easy to make.

MR. HAMILTON, professor of Philosophy at the university of Dublin, made a curious experiment with a wire, five or six inches long, finely pointed at each end. To the middle of this wire he fitted a brass cap, which rested on the point of a needle communicating with the conductor. Half an inch at each extremity of this wire he bent, in opposite directions, perpendicular to the rest of the wire, and in the plane of the horizon. The consequence of electrifying this apparatus was, that the wire would turn round with very great velocity; moving, as he says, always in a direction contrary to that in which the electric fluid issues from its point, without having any conducting substance near it, except the air. He also observes, that if this wire were made to turn the contrary way, it would stop, and turn as before*.

THE same experiment was also made by Mr. Kinnerley of Boston, with this addition, that he electrified the wire negatively; and observed, to his great surprize, that it still turned the same way. This he endeavoured to account for by supposing that, in the former case, the points, having more electricity than

* Phil. Transf. Vol. li. pt. ii. p. 905.

the air, were attracted by it; in the latter case, the air, having more than the points, was attracted by them *.

It may, by some, be thought that this pointed wire turning the same way, whether it be electrified negatively or positively, is a proof that the electric fluid issues out at the points in both cases alike, and by the reaction of the air is, together with the points, driven backwards; contrary to what ought to have been the case if the electric fluid had really issued out of the point in one case, and entered it in the other. But it will be found by experiment, that an eolipile, with its stem bent like the wire above mentioned, and suspended on its center of gravity by a fine thread, will move in the same direction, whether it be throwing steam out at the orifice; or, after it is exhausted, and cooling, it be drawing the air or water in.

With respect to the power of points, it has been observed by Mr. Villette of Liege, that a needle, concealed in a glass tube, which projected an inch beyond it, takes a stronger spark from a prime conductor than a man's finger; also that when the points of needles are covered with tallow, bees-wax, sulphur, &c. they take peculiarly strong sparks. He adds, that, when sulphur is used, and the sparks are taken obliquely, they are sometimes of a beautiful citron colour †.

* Phil Trans. Vol. liii. pt. i. p. 86.

† Noller's Letters, Vol. iii. p. 212.

MR. LULLIN has made an additional observation to Monsieur du Fay's, concerning the different manner in which conducting and non-conducting substances are affected, when they are exposed to the dew. He says, that if a plate of glass be extended upon filken threads, and exposed to the open air all night, and a plate of metal, less than the glass, be laid upon it, in the morning, the metal will be dry, and likewise the glass, on both sides, exactly under the metal; but that the edges of the glass, where the metal did not reach, will be wet on both sides*.

I SHALL close this section of miscellaneous articles, and the whole history of electricity, with a succinct account of some of the chief particulars in which the analogy between electricity and magnetism consists; very nearly as it was drawn up, in an abridgement of Mr. Æpinus, and communicated to me for this purpose by Dr. Price.

1. As a rod of iron held near a magnet will have several successive poles; so will a glass tube touched by an excited tube have a succession of positive and negative parts.

2. BODIES positively and negatively electrical, when in contact, will unite to one another; as will magnets, when they are laid with their opposite poles to one another.

* Nollet's Letters, Vol. iii. p. 54.

3. GLASS is a substance of a nature similar to hardened steel. The positive and negative sides of the former answer to the attracting and repelling ends of the latter, when magnetical.

4. As it is difficult to move the electric fluid in the pores of the former; so, likewise, it is difficult to move the magnetic fluid in the pores of the latter.

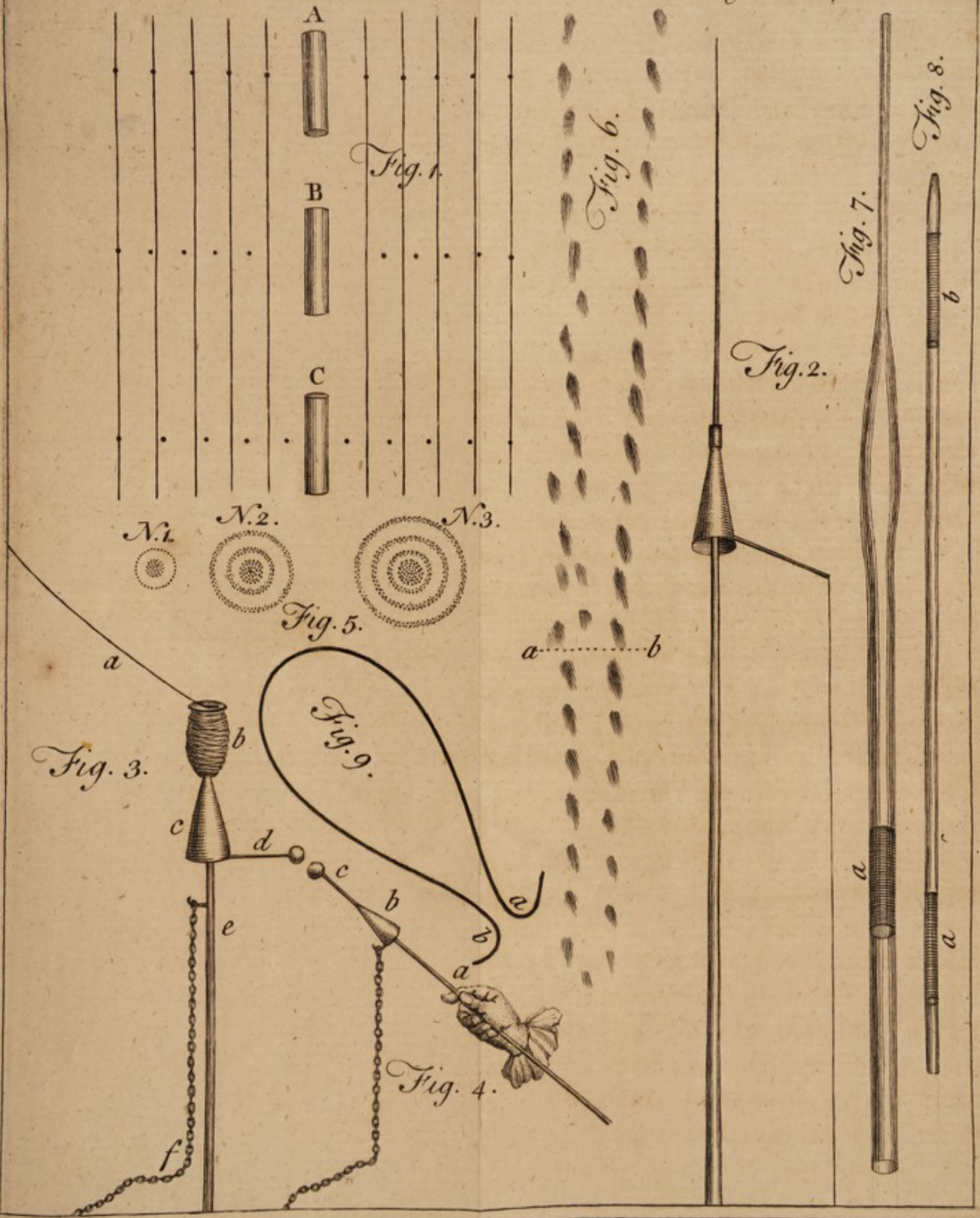
5. As there can be no condensation of the electric fluid in the former, without a rarefaction; so, in the latter, if there be a condensation, or positive magnetism in one end of a bar, there must be an evacuation, or negative magnetism in the other end.

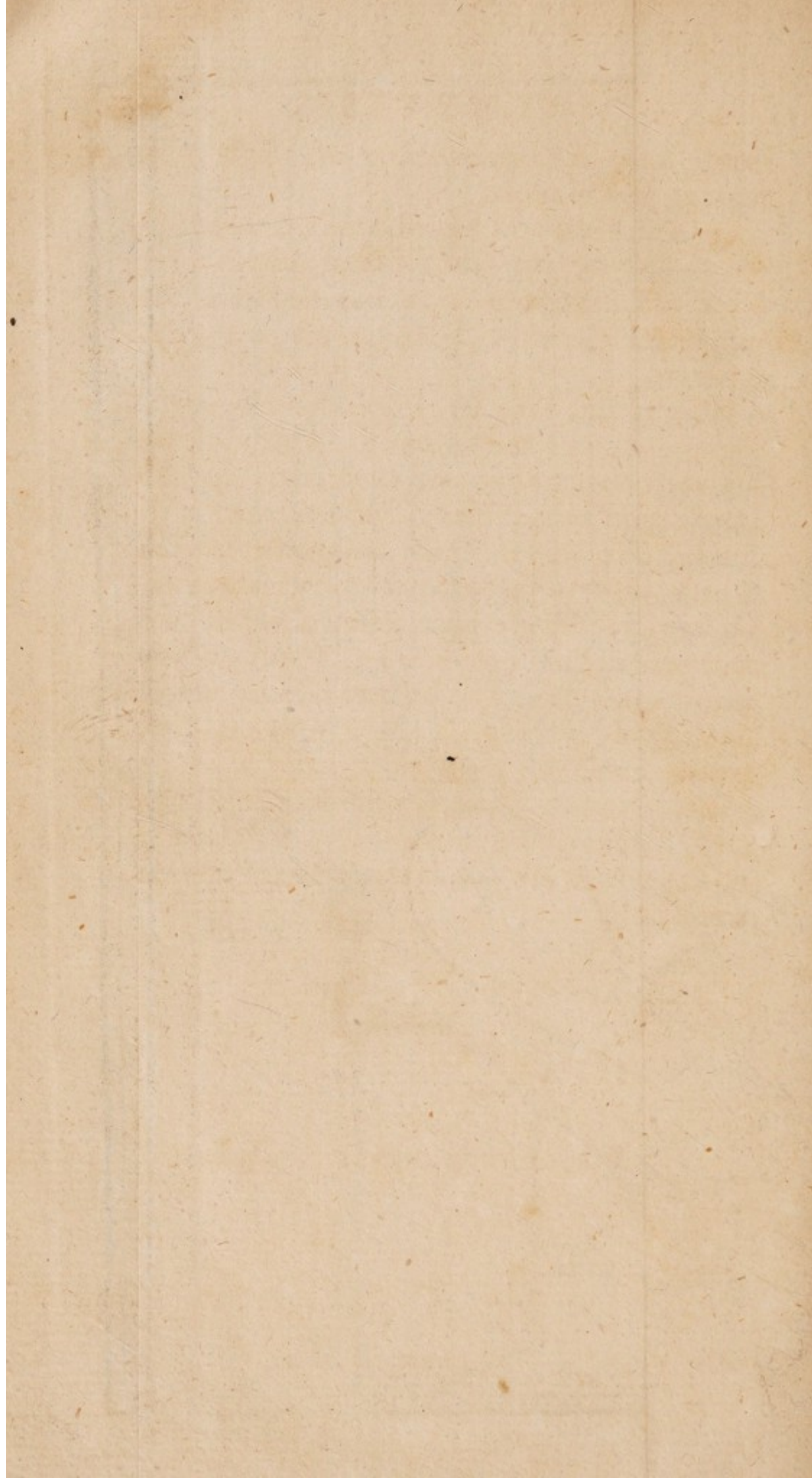
6. STEEL corresponds to electrics per se, and iron in some measure, to conductors of electricity.

7. STEEL is less susceptible of the magnetic virtue, but when it has acquired it, it retains it more strongly than iron; just as electrics per se will not so easily receive the electric fluid, but, when it is forced into them, will retain it more strongly than conductors.

8. MR. ÆPINUS adds, and reckons it one of his discoveries, that an electrified body does not act on other bodies, except they are themselves electrified; just as a magnet will not act on any other substances, except they are themselves possessed of the magnetic virtue. So that an electrified body attracts and repels another body, only in consequence of rendering it first of all electrical; as a magnet attracts
iron,

Hist of Electricity





iron, only in consequence of, first of all making it a magnet.

9. MR. CANTON has also found, that if the tourmalin be cut into several pieces, each piece will have a positive and negative side, just as the pieces of a broken magnet would have.

THUS far, says Dr. Price, there is an analogy, and, in some instances, a striking one, between magnetism and electricity, upon the supposition that the cause of magnetism is a fluid. But there is no magnetic substance which answers perfectly to the conductors of electricity. There is no afflux or efflux of the magnetical fluid ever visible. The equilibrium in a magnet cannot be instantaneously restored, by forming a communication between the opposite ends with iron, as it may in charged glass. Nor are there any substances positively or negatively magnetical only, as there are bodies which are positively or negatively electrical only.

