P. S. The engine with which experiment No. 4 was tried, has lately had two inches added to the thickness of the piston, and the length of stroke reduced to seven inches. This I am told has proved injurious, but as my informant is not at present in this city, I cannot say to what extent.

M.

Some Experimental Researches to determine the nature of Capillary Attraction. By JOHN W. DRAPER.

FOR THE JOURNAL OF THE FRANKLIN INSTITUTE.

If we take a couple of leaden bullets, and pare away from each of them a small shaving, with a sharp penknife, so as to leave a bright, plane, metallic surface, on bringing them into contact, and gently pressing them together, they will be found to adhere very strongly—it requires some effort to separate them. The same takes place where two pieces of plate glass are used, and this phenomenon sometimes occurs on a large scale in manufactories of plate glass, where the pieces, after being dressed are piled on one another. The attractive force, acting constantly for a length of time, during which extensive barometric and thermometric changes occur in the atmosphere, the effect is to bring the pieces into such close contact, that they actually become united, and may be cut as one piece by a diamond.

We may likewise observe round the edge of a glass of water, that the liquid appears to stand higher wherever it touches the glass, than it does in the centre of its surface. From the former experiment, we see that solid bodies, upon an approximation almost to contact, attract each other; from the latter, that the attraction still exists even if one of the bodies be a liquid; for this rise of water, is plainly owing to the existence of an attractive force, between the water and the glass.

If we take a tube of small diameter, and dip one end of it into a cistern of water, we see the effect of this attraction in its most striking point of view. The liquid immediately rises in the tube, to a height proportionally greater as the tube is smaller. Glass tubes may be made of such excessive fineness, that their bore shall be much less than the diameter of a hair. Such tubes are called capillary tubes, and from the phenomenon taking place in them to the greatest extent, this attraction has been called capillary, or the phenomena of capillary tubes.

But, while some liquids thus rise in glass tubes, there are others in which a reverse effect takes place; mercury, or melted lead, or tin, falls. The circumstance of a rise or fall, appears to be dependant on the nature of the tube and liquid as regards each other; thus if a tube be indurated with grease, water will not rise in it, but is depressed. For different tubes of different diameters, the elevation or depression of a liquid, is in the inverse ratio of their diameters.

It is not known to whom we are indebted for the discovery of these singular facts, as they were well ascertained before the time of Newton—they are doubtless the result of the labours of some early experimenter. Boyle made experiments on the adhesion of polished marbles, at first attributing it to atmospheric pressure, but afterwards finding out his error, he relinquished that idea. At that time, the explanation given by philosophers of the rise of liquids in capillary tubes, was either founded on a supposition, that the atmospheric pressure *within* the tube was less than *without*, owing to the narrowness of the bore, or that there was a subtile fluid, which, flowing from the bottom to the top of the tubes, in a silent whirlpool, carried the liquids in its vortex; both these hypotheses were rejected by Newton, on considering that, in every case, the amount of rise, ought to be in an inverse ratio of the specific gravity of the liquid. So, whilst philosophers vainly tried to find, in agents exterior and invisible, the true causes of the phenomenon, this very cause existed in the tube they held in their hands, and depended on that kind of attraction designated by the name Attraction at Small Distances. Newton, after several erroneous guesses, came to this conclusion, whilst other philosophers were yet tardy to acknowledge the truth, willing rather to attribute all the phenomena to the unknown pressure of some spiritual fluid, or the vortices of some subtile matter. This was the last refuge of the vortices of Descartes, which after being banished from the celestial regions, sought to maintain themselves in those recesses of nature, where attraction, reproduced under another form, disputed the place with them. Succeeding philosophers have seen reason to attribute the whole class of phenomena to the same cause, and only differ as regards the mechanism by which all the effects are produced. (Haüy.)

With very little exception, nothing has been effected since that time to determine by experiment, the true nature of this attraction; and, indeed, I do not see how it was possible to come at the truth, without every aid that science, as it now stands, can afford. Capillary attraction having fallen into the hands of mathematicians, several of the most distinguished paid considerable attention to it. Of all these geometers, Clairaut alone came to one conclusion of vital importance, which I shall endeavour to indicate.

It has been already noticed, that whilst water and other liquids rise in tubes of glass, mercury experiences a corresponding depression. At first sight it might appear, that if the rise of water is in consequence of an attraction between the glass and that liquid, the depression of mercury might be attributed to a true repulsion, and this receives some colour from the fact, that liquids which can *wet* solid tubes, rise in them; but if they are unable to *wet* them, they fall. Thus water will wet glass, but quicksilver will not. Clairaut found, however, that both the rise and fall of liquids, are due to attraction, as compared with the adhesion of the fluids. He found, that if the mutual attraction of a solid and fluid, amount to less than half the cohesion of the latter, there will be a depression; if it be

equal to half, the liquid will stand level in the tube, and if it surpass the half, the liquid will rise.

As other theories have disappeared before that of Clairaut, it, in its turn, disappeared before that of the Marquis Laplace, and that of Dr. Young. I do not stay now to determine whether we are to consider the bounding meniscus as elastic surfaces, acting by their tension, which was the doctrine of Dr. Young, or whether, with Laplace, we are to attribute the rise or fall of liquids, to the attraction of a thin layer of liquid, immediately adjacent to the sides of the tube. The latter, I believe, will be found most agreeable to the general tenor of my experiments.

At a particular time of the year, a phenomenon takes place throughout all the vegetable world,—this is the rise of sap,—which is immediately connected with the budding, blossoming, and flourishing, of trees, plants, and flowers. In the months of April and May, if we cut asunder a vine branch, at one blow with a sharp axe, we shall perceive that the wood is nothing more than a vast collection of capillary tubes, from the wounded extremity of each of which, there runs a limpid water. By cutting the stump shorter and shorter down towards the ground, we shall find that it proceeds thence. On the 20th of April, I cut asunder at one blow with an axe, a vine of about one inch and a half in diameter, at about eighteen inches from the ground, and placed a vessel to receive the tears, covering the arrangement with branches, to protect it from the rays of the sun. The thermometer stood at 109.2° Fahr. in the sun, and 80.7° in the shade. The barometer 29.92, and the time of the day twenty minutes before ten in the morning. In the course of eight hours, the barometer had risen .1 inch, and the thermometer having fallen to 76° , the liquid was measured, and seventy ounces were found in the vessel. More would doubtless have been collected, but the extremities of the tubes were covered with a gelatinous matter, which choked them. This fluid had a vegetable taste. Through a powerful microscope, no animalculæ, or signs of life, could be found. There were, however, a number of semi-opaque bodies, some of a globular, and some of an ellipsoidal form, smaller in diameter than the capillary tubes. In a few days these appeared to have putrified, and then the whole liquid swarmed with life.

This phenomenon, the rise of sap in trees, from its generality and importance, has long been studied. The nearest approach to ascertaining its true nature, was made by Dutochet, who, relying on a certain galvanic experiment, and aided by the discovery of endosmosis which he had made, to which there will be occasion hereafter to refer, attributed it entirely to the action of electrical currents. The mechanism he designed was erroneous, but he was far in advance of any of his predecessors.

This was the state in which I found capillary attraction; my attention was first drawn to it during those tiresome moments of returning health, which follow an autumnal fever. Perhaps, if there be any merit in these experiments, it may hereafter be of service to some one to know, that they were begun in sickness, and in a land of

strangers,—they were pursued in all the calamity of family bereavement, and in the depths of forests, alike unused to music, to poetry, or to philosophy. Solitude, if it be conducive to the development of the intellect, and favourable to the exercise of thought, is likewise attended with many evils. Though no disturbance arises from the intrusion of the frivolous, yet the counsel and assistance of the wise are wanting, and, indeed, those advantages which are supposed to result from such tranquillity, are, for the most part, only fictitious appearances, which, like certain other apparitions, every one can discourse of, but no one can say he has seen.

My first experiments were made in regard to the rise of liquids in tubes, but it was soon found that this was too complicated a phenomenon, to offer any chance of detecting its causes. It might depend, as Laplace showed, on the positive or negative action of the bounding meniscus. Now the adhesion of plates of glass to the surface of different liquids, was allowed to depend on the same cause as the rise of those liquids in tubes of glass, and it was void of that complexity which enveloped the phenomena of capillary tubes. I first tried to assimilate the two, but quickly found that no positive conclusion could be arrived at in this way. For instance, there was a strong and variable attraction between mercury and glass, so that the pan of a balance required a very heavy load to detach them—but, mercury experiences a depression in tubes of glass.

Clairaut's theorem, however, will be found to reconcile these cases; it directly tends to prove that the force of attraction might even amount to more than the force required to separate a disk of glass from water, provided it could be proved, that the cohesion of mercury is double, or more than double that of water. I performed, again and again, the experiment of the adhesion of a glass plane to the surface of mercury, and found it was very difficult to make the valuation of the weight required to separate them, equal in any two experiments. Changes in barometric pressure, as determined by an excellent instrument, had no equivalent influence. Variations of temperature, seemed to exert a powerful effect on the adhesion, yet it was by no means uniform. The hygrometer sometimes afforded comparable experiments, and then again it gave conflicting results. However, after making a great number of these, and similar trials, I found that mercury chemically pure, which had sustained a long boiling, and disks of glass which had been kept for some time at a temperature approaching 500° Fahr. would, in a majority of cases, give identical results. But here again, if the glass were suffered to cool, if it was touched lightly by the finger, or if a particle of lead, or tin, or bismuth, was dissolved in the metal, the results were instantly discordant.

Hitherto I had had no theory or opinion to gratify, and I resolved to be led astray by no vain or crude notion, but making experiment the means, to regard truth as the *end*. I could not suppose these variations of adhesion by any means originated in alternations of the attractive force, but was obliged to suppose that they arose from disturbing causes, which acted at one period more effectually than

at another. To get rid of hygrometric humidity, I brought some mercury to the boiling point, and kept it so for some hours. It was then filtered, with the least possible exposure to atmospheric air, to separate any impurity which it might have contracted during ebullition. It was now suffered to cool down to 212° , and a clean plate glass disk laid on it, at the same temperature. The glass was insulated by a rod of gum lac, and the mercury contained in a cup supported by pillars of the same substance, an amalgamated wire proceeded from it, to the ball of a very sensible gold leaf electrometer; as long as the mercury and plate were in contact, the leaves of the electrometer hung parallel to each other, but on separating them, and it required considerable force to do so, a vast quantity of electricity was instantly developed, and the electroscope leaves were torn asunder by the violence of the repulsion. On carrying the plate of glass to another similar electroscope, by its insulating handle, the leaves diverged, and continued striking the sides of the instrument for several times in rapid succession. On examining the electrical state of the two substances, I found the mercury to be negative, and the glass disk positive. The electric equilibrium is therefore disturbed by the separation of glass from mercury. Whilst they are in contact there is indeed no development capable of being recognised by the electroscope, their electricity is at that moment disguised, and from theoretical consideration, it is easy to show, *that they must be strongly attracting one another.*

The next step was to consider whether other fluids, as water and alcohol, would act like mercury.

But, without trying any experiment, it might be predicted, that there are certain fluids so constituted, that the attractive force exerted between them and glass, may exceed the cohesion they have for themselves. Now suppose, on laying a disk of glass on the surface of water, the disguised electricity, caused an attraction between the two substances, greater than the cohesion of the water, the result will be, that the weaker force must yield to the stronger, and on lifting the glass up it will carry with it a thin sheet of water, held on its surface by the attractive force of the opposite states of electricity, which will be still disguised, and therefore the electroscope ought not to diverge. *All which is entirely conformable to experiment.*

As this position has a very near relation to the theory of Clairaut, I endeavoured to prove the truth of that experimentally. For this purpose, I took a glass disk of a certain size, and one of copper of the same dimensions, carefully and thoroughly amalgamated; it was washed in distilled water, and made quite dry. On one side, three small rings were fastened, for the purpose of suspending it horizontally from the arm of a balance. It was gently lowered on the surface of pure mercury, in a cup, and the weight required to overcome the adhesion, noted. This weight was regarded as measuring the cohesion of mercury. After each separation, the superfluous mercury was cleared from the disk; fresh mercury was used each time.

Exp't. 1.	54	} Weight required to overcome the cohesion of mercury.
2.	53.89	
3.	54	

The copper disk was now removed, and the glass substituted in its stead.

Exp't. 1.	24.9	} Weight required to separate the glass.
2.	25.8	
3.	25.72	

Now on comparing these six experiments, it would appear, that in no case did the attraction of mercury and glass, amount to half the cohesion of mercury. And as mercury is depressed in capillary tubes of glass, the numbers ought, by the theorem of Clairaut, to be such as were found by experiment.

But, lest seduced by experiments which might simulate an appearance of truth into an incorrect theory, I resolved on making an experiment, which should be a severe test. This was to measure, with as much exactness as possible, whether the adhesion took place in exact proportion to the electricity developed. For if one kept pace with the other, that would be the strongest proof that could possibly be had, that one was the cause, and the other the effect. In making such an experiment we have many disturbing causes in play.

If a disk of zinc is laid on a surface of mercury, it will be evident that a separation cannot take place, without the greater part of the electricity escaping before the contact is finished, owing to the conducting power of the zinc. Hence the resulting number determined by the torsion balance, is vastly inferior to what it would have been had the zinc not been a conductor. But if we substitute a disk of glass, in place of the zinc, owing to the nonconducting power of the glass, all the electricity developed remains attached to its surface. Hence, the apparent quantity of electricity developed by the contact of glass and mercury, is vastly superior to the quantity developed by any metal and mercury, whilst in truth it may be vastly inferior. This conclusion, to which I am thus led, receives strong support from the case of a galvanic battery, and a common electrical machine, in which the quantity of electricity developed by the former is much greater than that of the latter. If the contact could be broken before the electricity developed on the surface of the zinc was neutralized, by meeting with the opposite electricity on the mercury, then we might make correct estimates of the quantity of electricity developed by contact. Now, upon making the experiment, it will be found impossible to separate the touching surfaces from each other *at once*. Suppose, for instance, a disk of zinc was partly separated from the surface of mercury, if, by the strong attraction existing between the two bodies, or other causes, the parallelism of the touching surfaces was disturbed; or if a mere filamentary drop of metal, caused a momentary communication, all the free electricity of both surfaces, would in a moment be neutralized, and the resulting quan-

tity, either as measured by the torsion balance, or the gold leaf electroscope, is merely the residuum of the very last point of contact.

This observation applies to the fundamental experiment of Volta, of the development of electricity between a plate of zinc and one of copper. If the disks could be separated, before any of the contrary electricities had time to combine, much better proof would be had of Volta's assertion. A disk of iron, laid upon mercury, showed but little electricity; a disk of zinc still less; but the electricity developed by glass was apparent, owing to the nonconducting power of the glass. Instead of doubting whether electricity is excited by contact, it is much more unlikely that it is excited by friction, which, indeed, amounts to no more than a series of successive contacts. Some may, however, object to the whole theory of electricity developed by contact, and may quote the experiments of De la Rive as decisive of the question; but I would ask such, how are we to account for the development occasioned on the surface of a disk of glass, a substance not liable to chemical action? Besides, I am satisfied, from personal observation, that the chief experiment of De la Rive is erroneous. *He* may not have succeeded in producing any accumulation in his condenser, by the contact of zinc and copper in a jar of nitrogen, for the ablest philosophers cannot *always* succeed in so delicate an experiment, but, in many trials, I have done it, and am therefore satisfied in my own mind of the correctness of Volta's fundamental assertion.

With regard to the adhesion of plates to the surface of liquids, as keeping pace with the electricity developed, the terms in which that may be shown, may not be such as a rigorous measurement would give, but are such as the case admits. I took a disk of plate glass, of a highly polished surface, and on one side of it melted some gum lac, inclining the plate so as to spread the lac evenly over the surface. Whilst the lac was in fusion, a piece of plate glass was pressed down upon it, to give it a polished surface; but, as in separating it from the polishing glass, a splinter was torn from its surface, the numbers to be mentioned are rather too small for the lac. The disk was now suspended from the arm of a balance, to determine the adhesive force on the surface of pure mercury. First, with the vitreous side downwards, and then with the side covered with lac. The numbers were:

	Glass.	Lac.
Exp't. 1. Perfectly dry	33.75	29
2. Moistened by the breath,	40	30
3. Wet	45.87	45.50

The second and third experiments were made to determine whether, when the surface was wet, a greater or a less weight would be required to separate them. From theoretical considerations, it would appear that the disks ought to adhere more strongly when water intervenes; for the rupture does not take place in consequence of the attractive force of the mercury and glass being overcome, but in consequence of the cohesion of the watery particles being overba-

lanced; this is evident, as a film of water is to be found on both surfaces; but in the last two cases, the theory would likewise indicate that the numbers determined by the balance were increasing, while those determined by the electrometer should be decreasing; and even in the last case the electrometer ought to be insensible. It may likewise be remarked, as connected with this, that the gum lac adheres less forcibly to mercury than glass, and only increases in adhesion when exposed to the same source of moisture, in proportion as one to three. Yet, when both substances *are wet*, the force of adhesion becomes the same, for then it represents the cohesive power of water. This experiment gives a salutary caution, that in attempting to compare the force of adhesion, with the electricity developed, every part of the apparatus and substances employed, ought to be free from moisture. If the proof plane of Coulomb be used, it will likewise be necessary to have regard to the conducting power of the surface under trial; without this precaution erroneous results would be obtained, arising from unequal distribution over the surface after separation; this, in circular disks, might vary from unity at the centre to 2.9 at the circumference, if the conducting power were perfect.

Having constructed disks of sulphur, beeswax, gum lac, sealing-wax, and crown glass, of exactly the same shape, weight, and dimensions, with the side that was to repose on the surface of pure mercury as highly polished as possible, I determined the adhesive force of each of these; the resulting numbers, the mean of several experiments, were as follows.

Crown Glass	55
Gum Lac	50
Sealing-wax	50
Sulphur	45
Beeswax	20

The mercury experimented upon, had been three times distilled with care; a very dry day was selected, and after all moisture had been expelled by heat, the experiments were made in a vessel containing chloride of calcium. I satisfied myself of the absence of all electricity, by the test of an exceedingly sensible gold leaf electrometer. The results afforded by a torsion balance were—

Glass	18
Lac	16
Sealing-wax	15½
Sulphur	11
Beeswax	7

Although these do not bear the same proportion to each other, as the numbers in the preceding table, yet they follow exactly the same order. But fearing there might be some mistake arising from the construction of my torsion balance, I took a long magnetic needle, and furnishing it with a gilt pith ball, insulated by a filament of gum lac, I noticed the effect caused by the movable ball and proof plane.

The numbers reduced to the standard of glass, in the last experiment were—

Glass	.	.	.	.	.	18
Lac	.	.	.	.	.	16.2
Sealing-wax	.	.	.	.	.	15.82
Sulphur	.	.	.	.	.	12.1
Bees-wax	.	.	.	.	.	7

I afterwards found that the same results might be obtained, in a rougher manner, by observing the distances at which each of the excited disks would affect a gold leaf electroscope.

The numbers thus found would probably agree better with those obtained by adhesion, if they were properly corrected. In the first place, the conducting power being variable in the different substances, will derange the result, and the expression for those in which it is greatest, ought to increase. There is another source of error, for which I see no remedy,—the attractive force exerted between glass and air; there can be no doubt, that air actually *wets* glass, and this is a circumstance of some importance to chemists. I have noticed in analysis of gaseous matter, traces of the presence of atmospheric air, which I am certain could be derived from no other source but this.

But if it cannot be rigidly proved that electricity is the cause of capillary attraction, by an exact correspondence of rigorous measures, such an opinion could be supported by no slight argument, if it was found that electricity exercised an apparent control over all the phenomena of capillary action.

Reasoning on the principle laid down, I succeeded in ascertaining in a distinct and positive manner, that electricity has complete control over chemical action, and that it can disturb the position of liquids in tubes, with the same ease, and quite as powerfully, as it can disturb the position of the magnetic needle. It might at first sight be supposed, that this could be effected by electrifying mercury, or water, in a capillary tube, furnished with a cistern, by means of a common machine; but a moment's consideration would show that this is an error. Owing to the expansive force of the electric fluid, it will, as is theoretically shown, occupy only the extreme part of the arrangement, and from this circumstance no particular result could be reasonably expected. Hence, though water be electrified in this manner, until it gives out sparks an inch or two long, it will remain in its original position in the tubes, even though the finger be approached very close to it. It is immaterial whether the electricity be positive or negative, or whether mercury or water be employed.

But whilst electricity of tension only affects the surface of bodies, electricity produced by the galvanic battery, occupies itself with their constituent atoms. It is from this source we must look for the effect. Now, if water be placed in a tube by itself, and the terminal wires of a galvanic battery being immersed in its extremities, it showed no disposition to be affected, the reason would be obvious; the attraction of capillarity is between the water and the glass tube, whilst

the electric current passes down *through the water*, without at all interfering, so as to alter the position of the suspended fluid. From this it is evident, that the tube must communicate with one pole of the battery, and the suspended liquid with the other. Further, the tube and the liquid must not only both be capable of conducting electricity, but it is also essential, that their conducting power should not be the same, or the electricity will pass from one to the other with too great velocity to cause any disturbance. To meet all these requisitions, and likewise to observe the reaction upon the capillary tube, I took a tube of glass, one-tenth of an inch in diameter, and wetted it thoroughly with water; I then placed it in a cup containing mercury and distilled water. The water rose in the tube; and on lowering it into the mercury, the mercury experienced its usual depression. The apparatus might now be regarded as a tube of water, containing mercury depressed in it by capillary attraction. I now made this tube of water positive by uniting it with a battery of fourteen Wollaston plates, and on making the mercury in the cup negative, the mercury in the tube instantly rose.

The only form in which the experiment will succeed, is when the positive pole dips into the *water* of the capillary tube, *and not into the mercury*. It is immaterial whether the negative pole dips into the mercury, or the circuit be completed elsewhere.

Some might incline to suppose, that as the rise takes place from the negative to the positive wire, it might receive ample explanation from the fact, that the electric current passing in the same direction, carries it mechanically with it. If the effect depended on this cause, the position of mercury ought to be deranged, by the passage of a stream of electricity from a common machine, but this is not the case; for strong explosions from a Leyden phial, may be passed in either direction through the tube, even at the risk of bursting it, and the position of the mercury will still remain unchanged. But the most decisive experiment is this,—if a positive platinum wire pass down the axis of a capillary tube into the water, and the tube and its wire be very cautiously elevated, whilst the mercury communicates as before with the negative wire, *at a particular position*, the water will experience an instantaneous depression, and will fall in the tube the whole length of the platinum wire. The particular position is, when the immersed extremity of the tube has just quitted the surface of the mercury, and the effect arises from an increased attraction between the mercury and the water. When a copper wire from the galvanic battery dips into mercury, it becomes wetted, and the mercury rises with a wave-like motion. But as in most other molecular actions, the phenomena are very complicated, the mercury having risen on the wire, capillary attraction, strictly speaking, is at an end; the mercury overcoming the attraction of the wire for itself. Change of capacity for caloric is the immediate result of change of composition, and heat is either absorbed or developed.

I have said, that, in the arrangement pointed out, a rise takes place; I would not, however, be understood to mean that such is always the case. That rise may be converted into a fall, and still all the

reasoning will apply. All that results from the theoretical action of a battery, is an increased pressure. This, under the control of disturbing causes, will produce a motion, but the direction of that motion is entirely governed by circumstances.

Before proceeding to the converse of this experiment, to show the depression of liquids, I may observe, that all these changes of position are accompanied by certain and definite changes of figure of the bounding surface. It appears, from the theory of Laplace, that an increase of attractive force ought to be accompanied by such a change of figure; which is the result of an equilibrium of the variable attractive force, and the cohesive power of the liquid. Reasoning upon these principles, it would appear that as the attraction between water and mercury increases towards equality, with the value of half the cohesive force of mercury, the bounding surface becomes of a less curved figure, and a rise takes place. From being convex upwards, as the surface approaches horizontality it becomes plane, and finally ends in being concave. That a rise ought to take place is shown by Laplace; for suppose the chord of the arc given by any section of the meniscus, to remain constant, the curvature of the arc becomes more and more sensible, forming a greater part of the circumference, whose radius becomes smaller and smaller, at the same time, the number of molecules contained on the bounding surface increase, and by a necessary consequence, the action of the meniscus itself increases; we may therefore assume, that, if a variation of the attractive force produces a change of figure, change of figure *may* be a legitimate indication of variation in attracting force.

In reversing the former experiment, so as to cause wetting liquids to rise higher, and mercury to be depressed beneath its usual level, regard must be had to disturbing causes, which may arise from the chemical nature of the liquids. A descent of mercury may be caused by altering the connexion of the polar wires. The mercury which, in the former experiment, was made negative, is now to be made positive, but as soon as this is done it becomes covered with a coating of oxide, which blocks up the tubes, and prevents any decisive action. Liquids capable of dissolving the oxide must be employed, and with sulphuric acid the rise and fall take place, on altering the communication. The simultaneous ascent of mercury, and descent of water, is instructively shown thus:—Place two plane plates of glass parallel to each other, so that there may be communication from their sides, with mercury and water in a reservoir. Make the water positive, by means of a platinum wire extending into the space between the glasses; on making the mercury negative, it immediately rises between the plates, and the water falls through the open sides, carrying with it any small particles of dust, or other light substance, placed there to show the path of the current. On breaking the communication, the currents return, flowing backwards, and things remain as before making the experiment.

It might here be objected, that no motion whatever ought to take place, from the elements laid down, the water having just as much tendency to descend as the mercury to rise; for, from the very nature

of disguised electricity, the positive and negative electricities are in the exact ratio for neutralising each other, which ratio is that of equality; an increased pressure only ought to take place.

This objection is easily obviated, when we remember that water and mercury differ in many respects; for instance, the inertia of the one is greater than that of the other; their action on the glass which included them, is very different, and all these things are to be considered as modifying the effect. On electrifying two substances, these trivial circumstances exercise a powerful influence, and may even determine a motion in direct opposition to what it ought to be. To chemists this is no new doctrine; these very forces, acting on this very electricity, determine so many chemical changes in direct opposition to affinity, else how can we give an explanation, that iron turnings decompose water at a red heat, and at the very same temperature, peroxide of iron is decomposed by hydrogen gas.

Aided by these considerations, I have contrived an arrangement to illustrate them experimentally. It consists of an inverted syphon, one of whose legs may be about one-tenth inch in diameter, and the other one-half inch. Mercury is to be poured into the syphon, until it rises in the smaller leg; pour upon it, in that leg, sulphuric acid; make the acid positive, and the mercury negative, and instead of a rise, a fall takes place. Change the communication, and there is a rise.

It may be remarked, that this alternation of rise and fall is decisive of the idea, that motion is produced by the mechanical action of the current; if that were the case, the motion in both instances should be alike, if the direction of the current was the same in both cases. I took the unequal legged syphon, filled with mercury and sulphuric acid, and caused the current to pass in its course along the wire of a galvanometer. The sulphuric acid was now connected by a wire passing from it, to one of the galvanometer cups; the mercury in the wide leg, had a thick amalgamated wire dipped into it, which proceeded from the negative pole of the battery. On completing the communication by a similar wire from the positive pole, the galvanometer needles traversed west. On reversing the communication, the mercury rose, and the needles traversed east. On substituting spring water for the sulphuric acid, and making the water positive, it instantly fell, vibrating about its lowest point of descent, and when it rose, vibrating about its highest. It is to be remarked, in making this experiment, that when the mercury is rising, the watery tube moves with a waving motion, between the glass tube and the mercury, and if the battery be in sufficiently vivid action, and the tube small enough, it will pass down, in direct opposition to the laws of hydrostatics, by several inches of mercury, and make its appearance in the other leg of the syphon.

I now took a syphon, whose legs were of equal diameter, and having made the water positive, and the mercury negative, there was a rise, the needle traversing west. On reversing, and making the water negative, the mercury blackened the tube, but did not move, the needles going east. Now, connecting this and the last experiment,

we find in one a current deflecting a needle westward, causing a rise, in the other, a fall. This I take to be proof sufficient that the mere motion of a current, dragging the mercury in its path, is quite unequal to produce the phenomena.

Perhaps some may imagine that a reasonable account may be rendered of this rise and fall, on chemical and hydrostatic principles. In the case where mercury rises in straight tubes, hydrogen gas is freely developed from its surface, and though most of the volatile metals are prone to form combinations with hydrogen, as tellurium, potassium, and arsenic, it, nevertheless, does not follow that these compounds must be gaseous: for instance, the solid compound of hydrogen and potassium of Gay Lussac and Thenard, and a similar compound of hydrogen and arsenic, formed by Davy. If, it may be said, an union of this nature took place between mercury and hydrogen, and the resulting compound was soluble in metallic mercury, it would, in all probability, be specifically lighter than mercury, as is the case with the ammoniacal amalgam; and on its formation it would disturb the hydrostatic equilibrium in the syphon, and a rise must take place to compensate for such disturbance. In the case where the mercury falls, why may not the hydrogen escape, and the oxygen unite either with the water or the mercury. Now from Dulong's experiments it appears, that the oxide of hydrogen is soluble to a certain extent in mercury itself, and peroxide of mercury is of greater specific gravity than water. Either of these suppositions, would account for a movement in the branches of the syphon.

In the first case, where a compound of mercury and hydrogen is assumed to be the cause of a rise, I would merely remark, that we have no warrant for supposing any such compound can be formed, and generally to the whole objection I make this reply,—The rise takes place *in a moment*, the instant the current passes, so that there is not sufficient *time* for any compound to be formed, at least not to an extent sufficient to disturb the hydrostatic equilibrium. Again, in the case of a rise in a tube of pure water, it is evident, after the current has ceased to pass, the mercury ought either to remain suspended, or slowly to disengage hydrogen whilst sinking. Now neither of these results agrees with observation; the moment the current ceases, the hydrogen drops, without disengaging the minutest bubble of gas. In repeating this experiment, care must be had that the water is pure, or at least only rendered a sufficient conductor, by substances which contain no alkaline matter; for instance, if chloride of sodium be present, the resulting amalgam will disengage hydrogen for some time after the current has ceased to pass; but even in this case, sufficient evidence may be had against this objection, for the sinking of the mercury is almost as instantaneous as its rise, and it is not until after *it has fallen*, that the disengagement of hydrogen is perceived. Again, if oxide of mercury be the cause of the fall, it should be remembered that the fall ought to be permanent. And as to peroxide of hydrogen, it is a substance so notoriously liable to decomposition, and requiring so large a volume of oxygen for its forma-

tion, that I cannot for a moment conceive any possibility of its presence at all affecting the experiment.

Again, it may be doubted, whether the current itself, or some peculiar quality of it, is the cause of these phenomena; or whether they do not arise from the electro dynamic action of the wire upon currents traversing the mercury.

On approaching a strong bar magnet in any position with regard to the apparatus, no disturbance ensues, and the poles of a powerful horse-shoe are equally inert. A battery consisting of a few large plates, though it will make the galvanometer needles turn violently round, will by no means cause that change of level, which a number of smaller plates will occasion. It therefore requires that peculiar voltaic arrangement, which is exactly suited to chemical effects. A larger number of moderate sized plates, has the most energetic action. Hence another important inference may be drawn—if, as we see the same arrangement controls capillary attraction, that is most effectual in disturbing affinity of chemical action; and if chemical attraction is identical with electricity, then there is great reason to suppose capillary attraction is referable to the same cause.

The evidence which it was my intention to bring forward, to show what strong suspicions there were for supposing that capillary attraction is due to electricity, may be regarded as complete; but a number of arguments might be adduced in addition. I would ask, what better proof have we of the truth of any scientific theory; we see that all the phenomena of capillary action may be legitimately accounted for by the laws of electricity; we see that the measure of the one is the measure of the other; that when the one increases or decreases, so does the other, in the same proportion, and that the one has that control over the other, that a cause might be expected to have over an effect. But lest, deceived by specious appearances, we should form an erroneous opinion, let us see how new facts will elucidate the matter, holding ourselves ready to forsake any theory, however plausible it may appear, the moment we find one fact that contradicts it. In doing this, I have to require a distinction to be made between things that *cannot* be explained on these principles, and those that I am unable to explain. For in treating on an intricate subject like this, where the actions of that unaccountable agent *ELECTRICITY*, are deeply involved, it would be vain entirely to rely on my humble ability. There was a saying of old, that it required a clever charioteer, to turn his horses in a narrow chamber,—how then shall I hope to guide such an ungovernable steed, and grope the way in darkness?

I believe it was Sir H. Davy who first noticed the convulsions of mercury, when electrified under water. In a watch glass, I placed about an ounce of mercury, and poured on its surface water, rendered a better conductor by a drop of sulphuric acid. The mercury was now made negative, and on dipping the positive pole of the battery into the water, a rotary motion was produced, exactly similar to that which would be exhibited by a current passing from a blow-pipe over the surface of the water. Now, it is to be remarked, that the arrangement is the same as when mercury rises in a straight capillary

tube; the mercury in both cases is to be in connexion with the negative pole. On reversing, and making the mercury positive, no motion is produced in either case.

As the battery I made use of in all these experiments, consisting of fourteen three inch Wollaston plates, was much too powerful when charged in the ordinary manner, I poured the exciting fluid from the cells, and replaced it by water rendered slightly saline by common salt. The action of the battery was now much moderated, and I observed, on repeating the experiment, that on making an arrangement as described, for producing currents, the mercury suddenly became more globose. On breaking the communication, it gradually dilated again, becoming a very oblate spheroid, much flattened at the upper part. At the time of its greatest convexity, a slight current was seen, carrying light bodies in its vortices, and betraying the energetic gyrations, which the particles of the mercury were accomplishing, whilst the little globule was detained, in such a state of force. On reversing there was a motion produced. It is easy to show that this change of figure proceeds from a change in attractive force of the bodies in contact. I made this experiment in another manner; I took a glass tube of such a diameter, that it would contain a little acidulated water, and a globule of mercury, without the spherical figure of the latter being deformed. In the bottom of the tube, a platinum wire was sealed; this the mercury reposed upon, and it likewise served to communicate with the negative end of the battery; on passing the current an instant deformation of figure took place. The upper surface flattened, and the mercury touched the tube all round, in a complete ring. Its surface was ploughed by gentle currents, and in the thin aqueous tube thus formed, decomposition rapidly took place.

Let us pause, to make a few useful applications of the knowledge we have acquired. Nearly one hundred and fifty years ago, Huygens saw with astonishment, that in a tube of a few lines in diameter, mercury might be made to stand at the height of seventy inches. This phenomenon is constant, when the interior of the tube is quite dry, and the mercury desiccated by long boiling. It receives an explanation on these principles. When the mercury is made to descend, in a dark room, to its usual barometric height, the vacuum is pervaded by a lambent electrical flame. This phenomenon takes place, to a greater or less extent, in all barometers; those that are most free from moisture and gaseous matter, being most liable to it. In tubes of a large diameter, it is manifested by an adhesion of mercury to the tube, which must be overcome by tapping the case of the instrument, or other mechanical means. I took a tube four-tenths of an inch in the bore, and having made it perfectly clean, I exposed it to as strong a heat as it could bear without altering its figure; whilst it was hot, one end was sealed, and the other bent like a syphon barometer. Mercury which had been distilled three times, at a low temperature, and then made to boil violently for a few minutes, was poured into it; it was again boiled for rather more than four hours. The adhesive force of the mercury to this tube was remarkable; it could never be

brought to stand at the same altitude, in two consecutive experiments. The slightest motion filled the tube with a pale electrical flame.

On the same principle may be explained an experiment, made by P. Abat, which has not a little puzzled philosophers. In a syphon, whose branches are of equal diameter, pour some mercury, so that its height in both branches may be equal. Make the mercury rise in one of the legs by inclining the syphon, and then slowly return it to an upright position; the mercury will now stand higher in the branch in which it rose, the hydrostatic equilibrium being destroyed. From the principles laid down it is plain this should be the case. When the syphon is brought to its greatest inclination, the tendency of the electricity developed by the contact of glass and mercury, is to produce a species of adhesion, and if the motion in restoring the syphon to its former position be gradual, that adhesion has time to take effect.

Dutrochet, I believe, for I have not yet seen his original work, saw that endosmosis might receive an explanation from the action of electrical currents, founded on a galvanic experiment, made many years ago. Circumstanced as I am with regard to his writings, I cannot criticise them. If, however, he supposes endosmosis is due to electrical currents, I submit that it is an error. For it appears, that a very simple explanation may be given. The liquid, whatever it may be, that has the greatest attraction for the bladder, or other porous substance, passes through by common capillarity; as soon as it reaches the upper surface of the system of tubes, it unites with the other liquid; in the case of alcohol and water, for example, the water passes through, and unites with the alcohol: this compound has not the same force of capillarity as the water alone, consequently the water still continues to rise, its upper surface being in a manner removed, by uniting with the alcohol, just as the sap continues to rise in trees, as the upper end of each filamentary column is removed, by evaporation from the surface of the leaves.

Setting out with these ideas, I attempted to determine the matter experimentally. I stretched a disk of bladder over a light metallic ring, and suspended it horizontally in equilibrio from the arm of a balance. The liquids experimented on, were taken from the same phial as had been used for verifying the endosmosis—they were, distilled water, proof alcohol, and a mixture of each. I expected that by ascertaining the force required to detach the bladder from them, I should know their comparative cohesion. From other circumstances I knew that the ratio of the electricity developed by alcohol and bladder, and by water and bladder, was something less than that of six to ten.

Exp't.		Alc. Water. Mix.		
1.	{ Force required to lift a disk of bladder from	20.1	33.4	23
2.		20.1	33.1	23
3.		19.8	33	23

It appears that the water passes through bladder with a force represented by 33; whilst the alcohol only attempts the passage with a force represented by 20; of course, the greater pressure prevails, and

the water passes. On arriving at the other side of the bladder, it forms a compound with alcohol, and the backward pressure now amounts to 23. The water, therefore, still continues to rise. I have used these numbers, in a rough manner; they do not accurately express the forces, but are a sufficient approximation for the purpose of illustration.

Guided by these principles, it appeared to me, that a material improvement might be made on the common galvanic battery, if the electricity developed during the mutual diffusion of liquids into each other, was added to that developed by two metals, acted on by a chemical menstruum. To put this into practice, I made an arrangement in a glass tube, half an inch in diameter, consisting of two metallic coils, one of copper and one of zinc, to each of these a copper wire was soldered. The zinc coil was at the bottom of the tube, through which its copper wire passes, being made water tight with gum lac. The copper coil was situated at the upper extremity, and about one inch distant from the zinc. Now on putting a strong solution of muriate of soda in the bottom of the tube, and dilute muriatic acid, of less specific gravity, at the top, during the act of diffusion of these liquids into each other, a strong current of electricity, it was presumed, would pass along the connecting wires. It was with no small pleasure I observed a full corroboration of the truth of the principles on which I was reasoning; for on introducing the poles into a drop of water, instantaneous decomposition ensued with considerable energy.

A plate of zinc, one-eighth of an inch in width, and half an inch long, fitted with a similar copper plate, by the aid of the solution above referred to, is perfectly adequate to the decomposition of water. A battery of half a dozen such tubes, bears a favourable comparison with one of fifteen Wollaston plates on the old construction.

In continuation of this research, on the power that one substance possesses of penetrating into the pores of another, as nearly measured by their power of cohesion, I was led to repeat the experiments formerly made by Guyton De Morveau, on the adhesion of disks of metal, to the surface of quicksilver. The results were—

1 Gold	6 Zinc
2 Silver	7 Copper
3 Tin	8 Antimony
4 Lead	9 Iron
5 Bismuth	

Which is likewise the order of their affinity,—another proof that chemical affinity and capillary action, are to be attributed to the same cause.

In repeating these experiments, I had occasion to notice incidentally, the production of heat, the moment mercury was touched by gold. This remark was made many years ago by Boyle, as may be observed in his essay on the Mechanical Origin of Heat and Cold.

It might be predicted, from the experiments of Pouillet, that gases must exhibit the same phenomenon. A piece of red hot charcoal, when I had cooled it in mercury, and then immersed it in an atmosphere of ammoniacal gas, had its temperature suddenly raised. When it was surrounded by a mixture of atmospherical air and hydrogen gas, and

the temperature slowly raised, the heat developed by the charcoal was such, that it set fire to the gaseous mixture, acting in the same manner, though not so energetically, as spongy platinum would have done.

This principle offers an explanation of the decomposition of the peroxide of hydrogen, and persulphuretted hydrogen, by several substances which have no chemical action on them. These compounds only exist at certain low temperatures, for a heat much short of 212° Fah., entirely decomposes them. The metals, and metallic oxides, likewise act in a very energetic manner; I am now speaking of the peroxides, which do not pass to a state of higher oxydation, during the action. On dropping any of these into a tube, containing peroxide of hydrogen, an instantaneous explosion ensues, and the tube becomes hot. The decomposition takes place, in consequence of the heat disengaged by capillary action; at least, the heat which must of necessity be developed from that cause, would be sufficient to decompose the layer of peroxide of hydrogen, immediately reposing on the oxide, even if no other disturbance was in action. And agreeably to this, we find, that in many cases oxide of silver will be reduced itself, whilst it is reducing peroxide of hydrogen.

There is a point, in connexion with this, to which I would refer. A liquid cannot repose on the surface of a solid, without the disengagement of caloric. A gas cannot pass through a capillary tube without changing its temperature. Hence, in the phenomenon of endosmosis, of liquids and gases, the membranous partition is of necessity obliged to undergo a change of temperature. For this reason it is well worth the inquiry of physiologists, whether animal heat may not in a measure be traced to this source. It is impossible that such changes as take place during respiration, should proceed without exaltation of temperature, but it is equally impossible, that the phenomenon of endosmosis, taking place at every breath we draw, should proceed without exaltation of temperature in the membranous septa of the lungs, and the whole vital apparatus. From Dulong's researches it appears that, supposing all the oxygen that has passed into the state of carbonic acid, has really combined with carbon during respiration, and supposing all the oxygen which has disappeared has combined with hydrogen to form water, and that the sum of these two quantities of caloric, accurately represents the whole quantity of heat produced by the chemical changes of respiration, the amount is too small by one-tenth, and often by two-tenths, of the actual quantity.

I might now proceed to add something concerning those chemical decompositions, which may be effected by the mere action of capillarity, as exemplified in the cases of deutoxide of nitrogen, atmospherical air, and pure ammonia; and likewise to show how much these processes are controlled by the condensation of those gases. I might likewise speak of those molecular motions, which silently take place in the innermost recesses of solid bodies, and indicate the laws of the movement and equilibrium of those systems of atoms. For we are not to suppose, because the texture of solid bodies appears to be the same for many years, that there is no movement of their consti-

tuent parts. I might proceed to consider the magnitude and shape of those atoms, and how one system of them may be made to disturb the motions of another; the distance there is between atom and atom, and how it is affected by their orbital paths, and vibratory oscillations. I might show how the disguised electricity, produced by near approximation, acts on these elementary systems as a centripetal force, and how the caloric which is produced under these circumstances acts as a counterbalance to prevent their fall on each other. But so long as hypothetical considerations are involved in these researches, and until the geometrical investigation is corroborated by extensive recourse to experiment, we cannot hope to come to an undoubted conclusion.

If we look once more on the mass of evidence which appears, we shall find, that all the phenomena of capillary action may be explained according to the laws of electricity,—that electricity is produced in some of the chief cases of capillary attraction, its development keeping pace with the intensity of attraction. We shall find, that whatever disturbs the production of the one, disturbs the action of the other. That the formulas of Clairaut, developed many years ago, receive, upon this doctrine, experimental corroboration—and that the attractive force may be measured by the torsion balance. If further proof were wanted, we should find it in the complete control the galvanic battery has over the height of a column, whether of mercury or water, suspended in tubes, making it rise and fall at pleasure; and, what ought to be an argument to a chemist, this is done by that arrangement which effects that affinity, which is said to be due to electricity. The endosmosis of Dutrochet is explained, not hypothetically, but by recourse to the balance; and to show that we may employ this theory to advantage, it has proved a guide for discovering arrangements in galvanic batteries of unusual intensity. It is found that the electrical excitements of different metals and mercury, as determined by their affinity, keep pace with their capillary action. The singular evolution of heat produced by molecular action, affords another passing proof to that type—the polar connecting wire of a voltaic battery. The physiological application of this fact, is striking and important; and if any thing be wanting to complete the chain of evidence, it may hereafter be found in the power that capillary attraction possesses over chemical affinity.

Christiansville, Va., July, 1834.

FRANKLIN INSTITUTE.

Quarterly Meeting.

The forty-second quarterly meeting of the Institute, was held at their Hall July 17, 1834.

Mr. JNO. C. CRESSON, was appointed Chairman, and

Mr. SAMUEL HUFFY, Recording Secretary, P. T.

The minutes of the last quarterly meeting were read and approved.

Donations of books, models, and minerals, were received from Messrs. M. W. Baldwin, M. D. Lewis, Timothy Abbott, jr., Edward