



XXXVI. On the use of a secondary wire as a measure of the relative tension of electric currents

John W. Draper M.D.

To cite this article: John W. Draper M.D. (1839) XXXVI. On the use of a secondary wire as a measure of the relative tension of electric currents, Philosophical Magazine Series 3, 15:96, 266-279, DOI: [10.1080/14786443908649875](https://doi.org/10.1080/14786443908649875)

To link to this article: <http://dx.doi.org/10.1080/14786443908649875>



Published online: 01 Jun 2009.



Submit your article to this journal [↗](#)



Article views: 2



View related articles [↗](#)

But if all this be granted, those geologists who have examined Norfolk will admit that the denudation here alluded to must be that which gave to this district its actual valleys, and many of the leading features of its present geographical configuration. We are thus brought round to the conclusion that land in this country must have emerged from the sea after the deposition of the Norwich crag, and yet at a period anterior to that of the denudation just alluded to. But as we know of no denuding agency capable of excavating great valleys in a flat country like Norfolk, except the power of the ocean, operating either at the time of the submergence of land or that of its emergence from the waters, we must infer from all the facts and reasonings above set forth, that land, consisting of chalk covered by crag, was first laid dry before the origin of the sand-pipes, and then submerged again before it was finally raised and brought into its present situation.

For my own part I readily adopt the hypothesis of these oscillations of level, because I have found them indispensable to explain other geological appearances on the coast of Norfolk, not many leagues distant from Norwich, where there is independent evidence of the land having been first laid dry, after the deposition of the crag, so as to support a forest; then submerged again, so as to subside to the depth of 400 feet or more, the signs of the forest being buried under strata several hundred feet thick; and, lastly, of the same tract having been re-elevated, so as to bring the monuments of this remarkable succession of events into view. On this subject I shall shortly enlarge, when treating of the age and origin of "the Mud Cliffs" of Eastern Norfolk.

XXXVI. *On the Use of a Secondary Wire as a Measure of the Relative Tension of Electric Currents.* By JOHN W. DRAPER, M.D., Professor of Chemistry in the University of New York; late Prof. of Physical Science in Hampden Sydney College, Virginia*.

[With Figures: Plate I.]

IT is the object of this memoir to establish the following propositions:—

1st. That by means of a secondary wire, we may always determine the relative tension of electric currents.

2nd. That there is reason to doubt whether the processes usually supposed to affect the condition of an electric current,

* Communicated by the Author.

are ever attended with any such result; but that when changes have apparently taken place, it is probable that they may be directly traced either to a disturbance at the place of generation, or to the development of other currents of a different character, the primary current itself remaining unchanged.

3rd. That there are two different methods of accomplishing these disturbances, and thereby of raising the elastic force of a current: 1st, that tension may be augmented by the sacrifice of quantity; Volta's plan of a reduplicated series, and Henry's ribbon coil in its condition of equilibrium, being examples: 2nd, by the introduction of new affinities in the exciting cells; batteries charged with nitrosulphuric acid or sulphate of copper are examples.

4th. That the law which regulates the connexion of this diminution of quantity, or condensation, with the increase of tension, is the same as that which regulates the analogous phenomena of ponderable elastic fluids.

Incidentally, the examination of certain other points will be entered upon, for example, a brief consideration of Lenz's law of the conducting power of wires; this it will be shown holds not only in the case of Faradian currents, but in the direct currents from hydro-electric and thermo-electric pairs, as has been advanced by some philosophers but denied by others.

The terms tension, intensity, tensile effect, &c. have had very different significations attached to them. From this circumstance a great deal of confusion has arisen, and it is one of the causes of that diversity of opinion and contrariety of theory which obtain in the elementary parts of the science of electricity. For example, Dr. Faraday appears to use the words *tension* and *intensity* as synonyms, expressive, as it were, of elastic force,—chemical authors generally adopting the same signification: "The remoteness from the unexcited state, a condition expressed by the terms *tension* or *intensity*." "By tension or intensity is meant, the energy or effort with which the current is impelled." (Turner, Elem. Chem.)

This confusion of terms leads to a confusion of facts of a much more serious kind. English electricians uniformly state, that the magnetic needle deviating in the neighbourhood of a current, takes no note whatever of the intensity of that current. Continental writers, almost without exception, regard the deviation as a function of the intensity, and the statements therefore appear discordant. Whilst the effect is thus differently described, all agree as to the facts of the case. In what follows, the term *tension* will be used as expressive of the elastic force of the current, that power by which it is en-

abled to pass a resisting medium; the term *intensity* will be strictly confined to the acceptation in which writers on analytical mechanics use it. "By the intensity of a force, we understand its greater or lesser capacity to produce motion," (Boucharlat) and in the case before us, the intensity will be regarded as a function of the quantity and tension conjointly. Thus, the deviation of a magnetic needle does not indicate the tension, but the intensity, of a current.

Suppose now we had a current of electricity passing under a certain tension, along a channel of conduction, as a bar of large dimensions, and were suddenly to interpose in some part of its path a resisting obstacle, as, for example, a slender wire; it is obvious that a certain portion of the current would pass the barrier, a portion determined partly by the character and dimensions of the wire, and partly by the tension or elastic force of the current. Let the wire under all circumstances be the same, the absolute quantity of electricity be constant, but the tension thereof vary. Now, as the tension increases, the quantity that passes the resisting wire will also increase, and as the one diminishes so will the other too. Under these circumstances, the absolute quantity that passes will always be an increasing function of the tension, and as this quantity is under all circumstances measurable by the deviations of the magnetic needle, or by the voltameter, these instruments may be used to determine the tension, by making quantity indirectly the measure thereof.

If, therefore, we send a certain quantity of electricity, as 100 parts, to a resisting wire, and find that of these 50 parts can pass the obstruction, we may assume such a current to have a higher tension than one containing the same absolute quantity, of which only 30 could pass; and to have a much lower tension than one, of which 70, 80, or 90 parts could pass. In all these cases, the amount per cent. of the main current which passes the resisting wire, may be taken as the representative of the tension of that current.

This obstructing, resisting wire, I call a secondary wire.

But it is plain that this amount per cent. of which I am speaking, in introducing this fundamental proposition, is nothing more than the ratio which exists between the quantities passing the large and the little wires respectively. By dividing, therefore, the quantity that passes the secondary wire, by the quantity that passes the large wire, we shall have a numerical representative of the relative tension of the current under consideration.

Let us take an example: a single pair of plates developed

a current of electricity, which when measured at the torsion balance was found equal to 20 degrees; on subjecting this current to a secondary wire, 7 degrees passed it. Its tension might therefore be represented by

•3500.

A second pair was now added in conformity to the first, 31 parts passing; but when subjected to the secondary wire, 18 were indicated. The tension had now become

•5806 :

In the same way, by adding three more pairs, the tension rose to

•6346.

It must now be borne in mind, that the numerical determinations thus procured are entirely conventional; their absolute value depends upon the resistance of the secondary wire, and they therefore only express the relative condition of different currents.

As a considerable advantage will be gained, and much repetition avoided, by here indicating the mode adopted for procuring the following measures, I shall describe at once some modifications and additions which are necessary in the torsion balance, the instrument generally employed.

The voltmeter has of late come much into use in investigations of this sort, but when compared with the torsion balance, the latter is much more speedy and certain in its indications, and should generally be preferred. In point of fact, the indications of the two instruments are entirely of a different character; the magnetic needle shows the quantity of electricity that is passing in each indivisible portion of time, the voltmeter the quantity that has passed at the end of a finite time. In the conditions of the action of the one, time enters as an element, in the other it does not.

By applying a glass thread to the needle, the late Dr. Ritchie greatly improved the accuracy and general utility of the galvanometer; but even with that addition, unless certain precautions are taken, the instrument will not work satisfactorily; the motions of the needle are too versatile, and the tremulous state of vibration into which it may be thrown, are insuperable barriers to accuracy of measurement. A cylindrical trough filled with water is a perfect and admirable remedy for these difficulties.

Another difficulty, which is very generally overlooked, is the excentric position into which the thread is liable to be cast, when the upper micrometer has moved. The construction of the instrument requires, that the axis of motion of the upper micrometer, the axis of the glass thread, the axis of the spindle carrying the needles, and the vane, should be in the

same vertical straight line, through whatever arc the micrometer may have moved. Now it would be very difficult to accomplish this by any system of adjustments.

Whether the instrument is arranged with one or several needles, or whether it has a coil or merely a single strap, the vertical distance from the coil or strap, when the index is brought to zero, ought under no circumstances to vary.

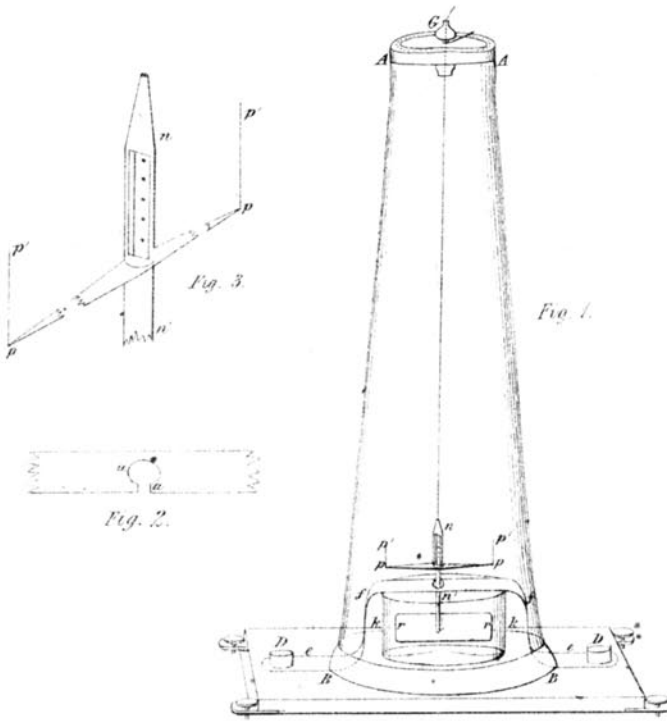
In a climate as hot as that in which the following experiments were made, one of the most unpleasant deviations depends on the thread wrenching in the wax, which is used to fasten it to the needles at one end, and to the micrometer at the other; when the wax softens, and the thread is moved through several degrees, it is not the free part alone that undergoes torsion, but also that which is in the wax, hence arises an error as respects the zero point. This I have always avoided, by ascertaining the zero at the beginning and close of each experiment.

After having had some experience with voltameters, deflecting galvanometers, &c. I am induced to describe the instrument used in these experiments, for it will enable those who are not accustomed to the torsion balance to execute measures very easily, which they might otherwise ineffectually attempt.

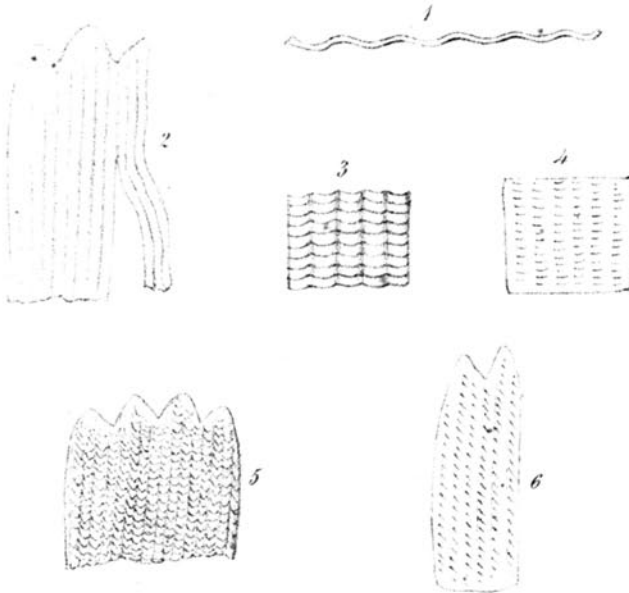
AA, BB (Fig. 1. Plate I.) is a glass jar, 16 inches high, open at both ends; at A A it is $2\frac{1}{2}$ inches in diameter, at B B, 6 inches; it rests upon a piece of wood 8 inches by 10. A strap of stout sheet copper, *effe*, 1 inch wide and 15 long, is bent into the form indicated; its extremities at *ee* being let into the wood, and bearing mercury boxes, D D. The central part of this strap from *f* to *f*, is placed horizontally, and has a circular aperture and side gap, as is shown in fig. 2. *aa*, through which the spindle carrying the needle can be passed, and works.

The upper extremity of the jar A A, is accommodated with a divided circle, in the centre of which the key G works: this key is ground like a stopcock to a slightly conical figure, it therefore revolves very truly without any shake; it is drilled longitudinally to admit the passage of the glass thread, which is secured in it by means of a perforated straw and a drop of sealing-wax.

The other extremity of the thread enters a little tubular perforation in the ivory axis *nn'*, and is also secured therein by wax. Only one needle is used, it is lozenge-shaped and is $4\frac{1}{2}$ inches long. Besides carrying this needle, the ivory axis extends an inch and a half below it, and in a slit at its lower extremity, confines a parallelogram of stout tinfoil, *rr*, an



Prof. Draper's Improved Torsion Balance.



J. Baer, Lith.

Scales of Butterfly Wings, observed by the Rev E. Craig.

inch wide and $2\frac{3}{4}$ long. When in use, this vane of tinfoil works in a glass cup, $\frac{1}{4}$ inch in diameter, which is filled with water.

One of the chief improvements in the instrument is connected with the needle, and the axis on which it works. The latter is a small cylinder of ivory; it has two flat faces filed upon it, corresponding to the direction of the needle. On each of these faces, as is represented in fig. 3, is drawn a vertical line, and a little to the right of it are placed five dots. The polar extremities of the needle are accommodated with two upright wires pp' , pp' , an inch long, which serve as indexes; and at a distance of 10 or 15 inches, in the magnetic meridian, a plate of metal, not shown in the figure, with a small hole in its centre, is placed to be used as a sight. When an observation is to be made, the experimenter adjusts this sight in front of the instrument, either on its north or south side; and on looking through it, as soon as the needle moves, he sees the index pp' traverse *before the scale on the axis*. There is no shake or vibration, even though any one should cross the floor or jar the table, for the index and the scale equally participating in all these disturbances, the motion is almost as steady as that of a shadow on a sun-dial; the vane of tinfoil does not in the least interfere with the accuracy of indication, but effectually stops the oscillations, and the utmost accuracy may be obtained, by previously giving the index pp' a slight bend out of the vertical line, and using the five dots as a diagonal vernier.

In the following memoir it will be seen, that the terms primary and secondary wire are occasionally used; the former in a somewhat extended sense. I mean by it not only the thick polar wires that come from the electromotor, those which were used being one-fifth of an inch thick, but include the electromotor itself, no matter what its character may be—if a hydro-arrangement, the plates, exciting liquid, &c. The secondary wires are simply long or slender wires to obstruct the current; of these I have occasionally used two, the first 47 inches long, the second 290: they are of copper, one foot of which weighs 10.65 grs., and are covered with silk.

And lastly, the measures are sometimes arranged in a form such as this,

$$\left. \begin{array}{l} 100 \\ 50 \end{array} \right\} .5000$$

in which the large or upper number represents the quantity passing the primary wire, the under or smaller number the quantity passing the secondary wire; and the decimal on the right hand of the bracket, being the quotient of the former numbers, is, as will presently be shown, the representative of the tension.

We have now to examine the foregoing proposition more minutely. Let us call the primary wire, being that which is in connexion with the electromotoric source, A; and the secondary or resisting wire, B. Now how does B act towards currents when they are of variable character? There is no current, no matter how low its tension may be, that will not pass along B to a certain extent: this is abundantly proved by such a wire transmitting a thermal current, of the lowest tension and amount. But at the other extremity of the scale, is there a limiting point? Can a wire conduct electricity of a certain tension, only to a certain amount? I think not, for a wire of small diameter was found upon trial to conduct a thermal current to the extent at one time of 20, and then of 284 parts, the tension in both cases being the same; and if it would do this in the case of currents whose tension is so very low, the same might be looked for in hydro-currents; here, however, when the quantity reaches a certain point, the ignition of the wire ensues, and its physical character is changed. Sir Humphry Davy's experiments lead to the same conclusion, (Phil. Mag. Dec. 1821) nor does there appear to be any limit to the conducting power of a wire, either for high or for low tension. If a wire carries a certain amount of electricity, an increase of quantity or of tension will enable it to carry more, and the converse. To this important point I shall presently return.

As it thus appears that any increase of the quantity which A transmits, involves also an increase of that which passes B, a second question arises, What is the ratio that will be observed in the two cases? If the quantity passing A be doubled, will the quantity passing B be doubled also? This is a very important problem, for if the ratio above-mentioned holds, it would show that an observation by the secondary wire will give the tension independent of the absolute quantity. Let a represent the quantity traversing A, and b the quantity traversing B. Now, if the tension remains constant, and the quantity only is variable, the ratio

$$\frac{b}{a}$$

is always constant, and is entirely independent of the value of a .

This I have endeavoured to prove experimentally. I took a hydro-electric pair of copper and zinc, each of the plates exposing about two square feet of surface, and dipped them to different depths in dilute sulphuric acid. The following table exhibits one of these results.

TABLE A.

Primary Wire.	Secon. Wire.	Calculated.
49	34	34
37	26	25.6
24	17	16.6
13	9.50	9.0

and therefore we infer that the foregoing ratio holds.

Currents of very low tension give proofs of the same fact. A thermal pair of platina and palladium passed 44 through the primary, and 19.50 through the secondary wire; and when by increasing the temperature 236 passed through the primary, 115 went through the secondary wire. In a pair of palladium and silver, 165 and 1130 being passed successively through the primary, 43 and 313 went through the secondary wire. In a pair of iron and platina, 170 and 249 being successively sent through the primary, 79 and 112 respectively passed through the secondary wire.

But let us further suppose, that the quantity of electricity passing at different times through the primary wire A is constant, its tension alone undergoing an increase. If A formerly conducted all that was presented to it, it will under this new condition of things of course still do the same. Such however will not be the case with B, for a greater quantity is now enabled to pass it than before, and the ratio $\frac{b}{a}$ will give a

greater value; we shall therefore in this case have a measure of the tension. But if the tension still keeps increasing, b will continually approach to equality with a ; and when the tension is infinitely high, these quantities are accurately equal to each other; or in other words, when the elastic force of a current is infinitely high, its tension is unity.

If, on the other hand, the tension becomes lower and lower, b continually decreases, and finally might be found equal to zero. The value of the ratio then becomes zero; and therefore at the two extremes, or where the tension is unity, and where it is zero, the secondary wire, so far from ceasing to act, still truly indicates the condition of the current.

Whilst, therefore, A conducts freely the whole current, B will measure its tension under all circumstances; but in point of practice, we can never make the adjustment here hypothetically indicated, or so arrange a wire A, that it shall conduct *all* the electricity presented to it. Let us therefore here inquire, how this variable condition of *both* wires will affect the result. Let the tension (t), so change by any amount as to

Phil. Mag. S. 3. Vol. 15. No. 96. Oct. 1839. T

become (nt) , then a corresponding change will happen in a and b , admitting the principle that the quantities passing through A and B are increasing functions of (t) . If then (t) becomes (nt) , a will become (na) . Now if the equation

$$\frac{b}{a} = t$$

holds; $b = at$; but when the change impressed on (t) has happened, b will be equal to the conjoint values of (na) and (nt) ; and if these values be substituted in the former ratio, the result is still equal to (nt) ; so that whatever may be the change impressed on (t) the formula $\frac{b}{a} = t$

will always indicate it.

Having thus settled, by the foregoing simple reasoning, the fundamental doctrine of investigation, I next proceed to apply it to the analysis of the different processes, by which a change of tension is supposed to be impressed on an electric current; and this leads to the consideration of the second proposition:—

“That there is reason to doubt, whether the processes usually supposed to affect the condition of an electric current, are ever attended with any such result; but that when changes have apparently taken place, it is probable that they may be directly traced either to a disturbance at the place of generation, or to the development of other currents of a different character, the primary current itself remaining unchanged.”

It is popularly supposed, that if we pass an electric current through a wire of certain length, coiled upon itself, a kind of inductive influence will be exerted, so that the current shall become more and more intense as it goes. Or, if two currents are simultaneously passed into a double helix, they will mutually fortify each other.

(a.) A wire covered with silk, 48 feet long, and arranged as one circular arc, had a current passed through it, which produced a deviation of 35 degrees. The same wire was then coiled round a piece of wood, so as to make 155 circumvolutions; the deviation was still 35; and therefore no change was impressed on the current.

(b.) A thermal current was passed through a straight wire with the following result:—

$$\left. \begin{array}{l} 42 \\ 22 \end{array} \right\} \cdot 5238.$$

The wire was then coiled into a helix, the current passed through it, and measured; a powerful bar magnet was next introduced into the helix, and then a rod of soft iron. But in

Measure of the relative Tension of Electric Currents. 275

all these cases the measured numbers were absolutely the same as before. Therefore there is no change impressed on a thermal current, either in relation to quantity or tension, by making it pass along a coiled wire, or by acting on it with a magnet or a bar of soft iron.

(c.) The same experiments were made with a hydro-electric current, and they gave the same results.

(d.) The above-mentioned (b) thermal current was passed along one of the wires of a double helix, and through the other wire a hydro-current was passed, from a single pair of plates; but the tension and quantity remained the same as before. On sending a current of still greater intensity, viz. from a voltaic series of five pair of plates, the same result was still obtained; the hydro-current had power enough to decompose water.

(e.) On altering the polar communications, and thereby changing the course of the current, no change whatever in the primary current, either as to quantity or tension, was observed.

It is well known, that by using a long wire as a discharger of a single pair of plates, a spark will be obtained of a much more brilliant character than when the current passes through a shorter wire; it is upon this fact that the flat spiral ribbon coil is constructed. Many electricians have supposed that the results obtained by this beautiful contrivance were partly due to the inducing action of the successive spires, but chiefly to a long and easy conducting channel being open to the current, which gathers momentum in its passage. I have already shown that there is no *permanent* action of induction in the case of a coiled wire,—an observation applying equally to an elongated helix and to a flat spiral. Let us now determine whether the increased tension is due to momentum.

A copper wire, 46 feet long and $\frac{1}{16}$ inch in diameter, being arranged as the discharger of a single pair of plates, a brilliant spark was seen to pass; but with a wire of the same diameter and a foot long, the spark was barely perceptible. The quantity and tension in each case was now determined.

TABLE B.

Short wire.....	39 12 } .3076
Long wire.....	13 8 } .6153

Hence, by the use of a long wire we greatly increase the tension of an electric current. A second experiment, in which a

wire $\frac{1}{8}$, and a third, in which a wire $\frac{1}{3}$ of an inch in diameter, were used, gave analogous results. In neither of these cases, however, did the tension rise so high as in the former; it was lower as the diameter of the wire was greater.

This increase of tension follows the increase of the length of the wire, as the following measures show.

TABLE C.

Ex.		Quantity.	Tension.
1.	Current from a single pair of plates	79	·4177
2.	———— a long wire introduced	44	·5909
3.	———— a second ditto, added	27	·7222
4.	———— third	21	·7619
5.	———— fourth	15	·8333

Thus, by successively increasing the aggregate length of the discharging wire, the tension continually increased, commencing at ·4177, and finally becoming ·8333. Similar experiments with other wires gave similar results.

Now is this remarkable rise of tension due to a momentum which the current acquires on the wire? Or does it arise from the fact, that the wire acts simply as an obstacle, reacting thereby on the electromotoric plates, the increase of tension being due to them, not it? This is easily determined; for if the rise of tension be due to the plates and not to the wire, a short wire, slender enough to obstruct the current to the same extent, ought to act equally as well as the long wire.

This experiment, the result of which leads to the true theory of voltaic combinations, I shall carefully describe.

I took a copper wire, 46 feet long and $\frac{1}{8}$ inch in diameter, and found that it stopped a certain portion of the current coming from a single pair of plates. The micrometer of the balance was now turned, and the needle brought accurately to zero. Then I cut off from another slender copper wire, such a length (2 feet 10 inches) as to obstruct the current to the same extent as the long wire, the needle being brought when it was interposed in the path of the current to zero. The secondary coil was now introduced; it of course stopped off a certain portion of the current; but the micrometer was again adjusted, until the needle was brought to zero. And now the long wire being introduced, and the slender one taken away, the needle came again to zero. But I suppose, if the long wire had impressed more tension on the current than the slender one, either by momentum or otherwise, more electricity should have passed the secondary wire when it was used, which is not the case.

Again, I took a copper wire, 242 feet long and $\frac{1}{16}$ inch in diameter, and adjusted to it a fine iron wire as before: the extremities of this wire were tinned; it was $12\frac{1}{2}$ inches long. Either of these wires being used as a discharger, brought the needle to the same point of the scale. On using the secondary wire and the long wire together, I adjusted the needle accurately to zero, and then passing the current through the fine wire and secondary wire, it came again to zero. And this was repeated often, and so near was the adjustment, that when an assistant turned first one and then the other wire on, it could not be told which was in action, or whether the current had come along the long or the short wire. A long wire therefore impresses no sort of change on a current, but merely serves as an obstacle; for in the first case we had one wire sixteen times longer than the other, and in this we have a wire more than 230 longer than the one with which it is compared, yet the tension has increased only to the same amount in both.

And the same results were obtained by the voltmeter.

The current that flows in a simple closed voltaic circle may be resisted in two ways: 1st, the length of the wire connecting the plates may be increased, as in the foregoing experiments; 2nd, the connecting wire remaining of constant length, the *distance of the plates* may be increased: the result is the same in both cases, a rise of tension.

TABLE D.

Ex.	Distance of the plates in inches.	Quantity.	Tension.
1. 2.75	111	.7297
2. 4.50	50	.7600
3. 9.00	27	.8888

So that, whether we obstruct the current by lengthening the connecting wire, or by increasing the distance of the plates, the general effect is the same, the tension immediately rises; that increase of tension being due to the plates themselves, and not to the channel of conduction. This brings us to the third proposition:—

“ That there are two different methods of accomplishing these disturbances, and thereby of raising the elastic force of a current. 1st, That tension may be augmented by the sacrifice of quantity; Volta’s plan of a reduplicated series, and Henry’s ribbon coil in its condition of equilibrium, being examples: 2nd, By the introduction of new affinities in the ex-

citing cells; batteries charged with nitrosulphuric acid or sulphate of copper are examples."

A single pair of plates, under the influence of a long wire, or the spiral coil, presents a remarkable analogy to Volta's pairs arranged in reduplicated series. In point of fact, they may be considered as scarcely differing from each other either in mode of action, or in effect. The study of the single pair under this condition, reveals at once the theory of the voltaic battery.

If we inspect tables B, C, D, we are at once furnished with the fundamental fact which is the basis of explanation. When we compare together the tension and quantity of the electricity flowing in the primary wire, we are struck with the fact, that whenever the one has increased the other has diminished. *No matter what the other conditions may be, whether the communication is made by a long wire or a short one, whether the plates are near or far apart; whenever the quantity is diminished the tension increases, and whenever the quantity increases the tension is diminished.*

The remarkable analogy of the ponderable elastic fluids, which when their volume is diminished, or in other words condensation takes place, experience an increase of tension or elastic force, is here too broadly indicated to be mistaken.

When I first saw that removing the plates to a greater distance apart determined a given rise in the elastic force of the current, for a time it appeared to me that Dr. Faraday's theory of the tension being due to the affinity of the zinc for oxygen must certainly be incorrect. A more extensive acquaintance with the facts has reversed that opinion. If the tension be determined by the affinity of the metal for oxygen, which must be a constant force, how comes it to pass that moving the plates to a greater distance apart can cause it to increase? This apparent paradox when properly understood forms a fine illustration of the truth of the doctrine advanced in the 5th, 7th, and 8th series of that philosopher's researches. In what follows I shall therefore regard those doctrines as established.

Let us take a given pair of plates, and connect them together by a slender wire. We find that the quantity that the plates generate is diminished, and its tension is increased; but that this has not happened either by gain of momentum or inductive influence in the channel of communication, and we are compelled to refer the effect to the resistance of the wire, placing the plates and the electrolyte between them in a state of force. If this be the action of a resisting medium, we might suppose that by continually increasing it, we should

continually increase the tension, and when it became infinitely great, the tension would be so too. But what is the true action of a slender wire, connecting in this way a pair of plates? A certain amount of electricity passes along it, but not the *whole quantity* that the plates could generate in a *given time*: yet we cannot but suppose that *all* that does pass comes from the *whole surface exposed*, and not from a *fractional portion* thereof. The water and zinc are ready to generate, and as it were attempting to drive a fresh quantity of electricity through the wire; and accordingly, as the quantity that actually passes becomes a greater and greater portion of what the system actually tends to put in motion, the tension becomes less and less. The tension would therefore become zero, if the whole circle wires, plates, and electrolyte could carry all that the zinc and water could generate. The limit prescribed to its diminution is the conducting power of the electrolyte, which is the worst conductor of the system.

This hypothetical condition, of a tension ranging near zero, is most nearly approximated to in a thermal pair.

Suppose now that everything remains the same, as respects wires, electrolyte, distance of plates, &c., except that the dimensions of both plates are doubled. Shall we increase the tension? No; for although the surface in action is doubled, and the absolute quantity which the system could generate is doubled, yet the quantity that passes both the primary and secondary wire is also doubled: the ratio $\frac{b}{a}$ is therefore the

same as before. For this reason, increasing the magnitude of the plates, increases the quantity only, and not the tension.

Under all these circumstances, therefore, the tension depends on the ratio of the quantity that does pass the combination, to the quantity that the system tends to put in motion.

[To be continued.]

XXXVII. *On the Configuration of the Scales of Butterflies' Wings, as exhibited in the Microscope.* By the Rev. EDWARD CRAIG, M.A., F.R.S.E.

[With Figures: Plate I.]

To the Editors of the Philosophical Magazine and Journal.
GENTLEMEN,

IF the following observations are not rendered unnecessary by earlier correspondents, they are at your service for insertion in the Journal.