XV. *Electro-Physiological Researches.—Seventh and last Series. Upon the relation between the intensity of the Electric Current, and that of the corresponding physiological effect. By Signor* CARLO MATTEUCCI, *Professor in the University of Pisa, &c. &c. Communicated by* MICHAEL FARADAY, *Esq., F.R.S., &c. &c.*

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IN bringing to a conclusion my series of researches on electro-physiology, I shall dwell in the present memoir on such of them as have frequently engaged my attention, and which I have lately again enlarged upon as bearing directly upon the highest, and I would even say, most physical point of the science of electro-physiology.

We admit as clearly demonstrated by experiment that electro-magnetic, as well as electro-chemical action, give the measure of the electric current; in other words, that different quantities of electricity produce chemical and magnetic effects proportional to these quantities. What then is the nature of this relation between the quantity of electricity and the contraction thereby excited, when transmitted through the nerve of an animal either living or killed as recently as possible? In an early memoir, published three years ago in the Ann. de Chem. et de Phys., and in a communication which I had the honour of making to the British Association at York, I described my first experiments on this subject. Nevertheless I have always been desirous of being able to renew these experiments with far more perfect instruments than I possessed at that time.

All the apparatus which I have employed were executed by M. BREGUET with his accustomed skill and talent. The principal among them (Plate XII. fig. 1) has been already described in my Fourth Series. In the course of the present memoir I will give the description of the other instruments.

The following is the general disposition of the experiment. A frog prepared rapidly is reduced to a thigh with the leg, the lumbar nerve, and a morsel of spinal marrow. An electric current, or a discharge from the Leyden jar, is passed through a certain length of this nerve. The electro-physiological effect is the contraction of the limb, which at the expiration of a given time is raised to a certain height. Let a determined quantity of electricity now be passed, then the half, a third, a fourth of that quantity, and so on, and a measure be taken of the corresponding electro-physiological effect; that is to say, of the height to which the limb is raised, and within what time.

It will be seen from this that there is no difficulty in putting forward the subject of the research, but the practical difficulty is very great. It is needless to describe in what this difficulty consists, it being sufficiently apparent in itself. I have therefore confined my efforts to the discovery of the relation between the electro-physiological effects, and the quantities of electricity which produce them, finding it almost impossible to arrive at approximative measures of these absolute effects.

On passing an electric current from different piles along the nerve of a frog disposed in the apparatus described in my Fourth Series, fig. 1, and noting the movements of the index in the different cases, it will be perceived that the limb is raised to the same height for very different currents. Thus I have found that a current from a GROVE's pile of six couples of plates, and one from six of a FARADAY's pile with and without a powerful magnet in the circuit, and finally a current from only one couple of WHEATSTONE's plates, give the same number of degrees for the movement of the index of my apparatus. It is clear from the above that all these currents are too strong for the effects to be compared together; it would be precisely the same case if we were to pretend to measure different currents with a galvanometer the needle of which is propelled to 90° by the most feeble of these currents. The real object, therefore, to be ascertained was the feeblest current which produced the greatest possible medium contraction. I succeeded in the following manner in discovering this. I employed a small WHEATSTONE's pair of plates, of constant force, introducing within the circuit a cylinder of distilled water. This water is contained in a glass tube a centimètre in diameter, and bent in the form of the letter U. By plunging the metallic conducting wires more or less deeply in this tube, I vary at will the resistance of the circuit. The following are all the details of the experiment. The WHEATSTONE pair of plates was exactly like that described in the memoir of that skilful experimenter; the circuit is composed of a copper wire covered with silk, and half a millimètre in diameter, which serves to close the circuit interrupted by the cylinder of distilled water. The extremities of the wire which dips in the cylinder were of platinum; the length of the copper wire in all about three mètres. The circuit was closed and broken by rapidly plunging one end of the wire, held in the hand, into a capsule of mercury. Both the pile and my apparatus were perfectly insulated, and the copper wire which served to close and break the circuit was varnished at that part which was held in the hand. The depth to which the current, which I call limited, penetrated within the column of water was about eighty-eight centimètres (in the case of very vivacious frogs rapidly prepared) during the first fifteen or twenty contractions. In order to render these experiments as comparable one with another as is possible in this kind of research, it is necessary that the circuit be broken always at equal intervals of time. I leave the circuit closed as short a time as possible, and invariably allow from fifteen to twenty seconds between one experiment and the other. I always employ the direct current for reasons well-known.

The difference occasioned by the column of eighty-eight centimètres of water, and which showed that I had arrived at the limited current, was very manifest in my apparatus; thus at a depth of a few centimètres less I had the highest indication, the same as that given when the stratum of water was not included in the circuit. With the column of water eighty-eight centimètres in depth, the indication was lessened by from 2° to 4°, and with a column of from ninety to ninety-five centimètres the diminution was very sensible.

In the very numerous experiments which I have instituted on this subject, I have always, in each in particular, sought for the length of the stratum of water which gave the limited current. I repeat that it was very easy to effect this by approximating the wires more or less in the water. It remains now to be considered by what means the half, a third, a fourth of the quantity of electricity was made to pass into the same nerve. After several trials I returned to my first plan as being the least inexact possible. When I wish to pass half the quantity of electricity through the nerve, I interpose a second nerve between the forceps and the thigh ; to pass a third of the current I add two nerves, and so on. Theoretically, and with regard to the great resistance of the circuit, it may be admitted as true that the half of the current passes into each of the two nerves, and a third into each of the three nerves, the same ratio being observable according as the subdivision of the current proceeds; but having respect also to the bad conductibility of the nerve, which is certainly, from its nature* and dimensions, at least as great as that of the stratum of water, it might be imagined that in increasing the number of nerves the total current would be also augmented, and by that means rather more than the half of the current which passed into one nerve would pass in the case of the two nerves; in the case of the three nerves, somewhat more than a third of the current for each nerve ; and so on. By introducing a galvanometer within the circuit, we are able to ascertain the very slight augmentation produced by the nerves successively added†.

In order that these nerves might be similar in all points, as nearly as possible, they were always taken from frogs prepared at the same time as the frog which served for the experiment. Care must be taken in placing them side by side not to allow them to touch one another. I have invariably observed that when this happened, and that the nerve of the frog within the apparatus was enveloped by the nerves added subsequently, there were no longer any signs of contraction on passing the current, though these were immediately manifest when the same number of nerves were prevented from contact with one another. I look upon this result as a further proof that the cause of contraction from the passage of the electric current, is the discharge which takes place at the commencement and at the termination of the circuit.

The following is the march of the phenomena in these experiments. If we begin to add the second and third, &c. nerve while the current is still tolerably far from

* A hempen thread soaked in water, and a nervous filament as nearly as possible of the same dimensions, offer degrees of resistance which are in the following ratio, twelve for the moist thread, and fifteen for the nerve. The nerve must be very fresh to afford this resistance : it naturally increases in proportion as it becomes dry.

† In performing these experiments, I have found that if the stratum of water was more resisting than the nerve, there was no perceptible increase in the current from adding two, three, or four nerves to the circuit instead of one. In my experiment the resistance of the column of water is greater than of the nerve.

its limit, we invariably observe that the contraction remains the same, or nearly so, as when there was no other nerve than that of the frog contained in the apparatus. On the contrary, if these experiments were not performed until the current was less than the limited current, then, after the second nerve had been added, there was no longer any contraction that could be measured.

It is, I repeat, by taking our departure from the limited current that we must determine in every case, performing the experiments with all the precautions described, and always being careful to wait until the natural contractions of the frog have ceased; and in this manner we may attain to numerical results which will admit of being compared together.

Before stating these numbers I must speak of a research which it was necessary to make in order to render these results as exact as possible. I allude to the duration of these contractions, both great and slight. I have measured these with the apparatus (figs. 2, 3, 4), which appears to me analogous in principle to that upon which the instruments invented by Messrs. WHEATSTONE and BREGUET for measuring the velocity of projectiles are founded.

This is a magnet (*a*, fig. 4), the anchor (*b*) of which is moveable, and at each movement strikes against the knob of a chronometer (*c*). The small weight (*o*, figs. 2, 3) which the frog raises in contracting is constructed in such a manner as to complete or break the circuit of the magnet. When the contraction takes place the circuit is broken, and it is re-established an instant afterwards when the contraction ceases. It then is broken again, and so continues to alternate. It will be seen from this that the interval of time elapsed between two successive contractions executed without any time being lost between them, will be marked by the chronometer.

I have found that in this manner the interval is constant for the first ten or fifteen seconds, during which it is $0''\cdot25$; after which it becomes longer, and remains $0''\cdot33$ for ten or fifteen seconds longer, and afterwards it becomes $0''\cdot41$, $0''\cdot58$, &c.

By varying the length of the stratum of water in three different experiments and with the frog still vivacious, my apparatus showed contractions differing from 6° to 10° up to 28° or 32°. The duration of the interval was perceptibly the same in all ($0''\cdot30$ to $0''\cdot33$).

Finally, it remained to measure the duration of the two different acts which are produced in the frog's limb in the interval between one contraction and the next, that is to say the limb is raised and contracts, then ceases to contract and falls.

I employed to ascertain this the same method which, I believe, the celebrated WATT adopted in the first instance for determining the velocity of the pistons in his machines. A fine point was attached to the little shank fastened to the leg of the frog, which point scraped, during the contraction, against a rapidly revolving smoked disc, of which the rotations were perfectly uniform.

The trace which the point leaves on the disc during its elevation may serve to indicate the duration of this elevation when it is known how long it takes to perform one revolution of the disc. The disc I used performed forty-eight revolutions in $1''$.

I have found from a great number of experiments, that the duration of the true contraction was much shorter than that of the descent or return of the limb, and that the difference became so much the greater as the muscle became fatigued.

I estimate the real contraction in my experiments at less than $\frac{1}{100}$th of a second. This duration was perceptibly the same for both great and slight contractions.

The following table shows the numbers found in a great variety of experiments conducted in the manner described, the number of nerves being varied.

I will only give a certain number of my experiments without making any selection therefrom.

	Number of nerves.	Degrees of contraction.		
First experiment	3	6	8	8
	2	10	10	10
	1	24	22	14
Second experiment	3	11		
	2	22		
	1	30		
Third experiment	2	12	14	
	1	26	28	
Fourth experiment	3	8	6	
	2	14	16	
	1	26	26	
Fifth experiment	3	6		
	2	14		
	1	24		
Sixth experiment	2	20	16	
	1	32	28	
Seventh experiment	1	6	6	
	2	3	4	
Eighth experiment	2	12		
	1	20		
Ninth experiment	2	6		
	1	10		
Tenth experiment	3	3		
	2	10		
	1	20		

Examining attentively the numbers which I have just exposed, and taking into consideration the reflections made above, and those principally which establish the fact, that the total current increases perceptibly with the number of nerves, it appears to me that we ought to conclude from these experiments that it is sufficiently demonstrated, and true as far as this kind of experiment admits of demonstration, that the electro-physiological effect is proportional to the intensity of the current.

Pisa, February 1847.

POSTSCRIPT.

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Extract from a Letter from Professor Matteucci to Mr. Bowman, dated Pisa.

(*Translation.*)

"As I do not propose to resume my electro-physiological researches, at least for some time to come, I should feel obliged by your adding the following fact, which is a very striking one, to the memoirs just presented to the Royal Society.

"You know the law of the electro-physiological action of the current on mixed nerves: you know that this law is very different in the case of the simple nerves of the anterior roots, as I found in conjunction with LONGET.

"Now, if you render a rabbit or a dog insensible by the inspiration of sulphuric ether, and, while in that state, pass the direct current along one sciatic nerve, and the inverse along the other, you will have the following phenomena:—

"1st. If the animal is not *totally* insensible, some cries of pain at the commencement of the *direct* current, which continue more or less while it is passing, but *no contraction*: the contraction with this current only appears on *interrupting* the current.

"2nd. With the *inverse* current, no cry or sign of pain on completing the circuit, if the animal is thoroughly etherized, and slight cries if not quite etherized: *contraction* of the muscles in *closing*, *none* on *opening* the circuit.

"These are the phenomena of the anterior roots.

"3rd. Now cut the nerves at their insertion into the spinal marrow: instantly the phenomena are reversed, and we have the ordinary play of the mixed nerves; that is to say, the *contraction* with the *direct* current takes place on *closing*, and that with the *inverse* current on *opening* the circuit.

"In the mixed nerves, therefore, the phenomena are complicated by the presence of the sensitive fibres.

"This conclusion appears to me very important, and I much regret that I am unable to tell you that the same things occur with the frog. Etherized frogs give the ordinary phenomena of mixed nerves, as heretofore.

"But who knows how the two kinds of fibres mingle in the nerves of different animals? Who is so ignorant as to believe that, in such matters, he can know all, and all at once?"