

XI. *Electro-Physiological Researches.—First Memoir. The Muscular Current.*

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MY only resource for showing the Royal Society how grateful I feel for the distinction lately accorded to me, is the communication of some fresh researches on electro-physiological phenomena.

The exposition of these researches will form the subject of the present and subsequent memoirs.

From the commencement of my studies on this subject, my principal aim has always been to reduce the experiments of electro-physiology to the simplest possible form, so that they may be repeated without the aid of very expensive instruments, or such as require great skill and practice in the management.

It is for this reason that I have dwelt long upon the phenomena which the electric current occasions in its passage along the nerves of an animal recently killed. The galvanoscopic frog, the mode of preparing which, together with its use and all its details, I have described in my *Traité des Phénomènes Electro-Physiologiques des Animaux*, page 51, is indubitably a very delicate galvanoscope, and free from all error. By means of the galvanoscopic frog properly applied, it is easy to ascertain the direction of the current which traverses the nervous filament of the frog itself. There is only this to be observed, that it is essential to the occurrence of this indication to wait until the frog be sufficiently weakened; and that in spite of this precaution, in every series of experiments we find some one frog in which, although we always have the signs of the electric current, yet contraction fails to take place when the circuit is closed with the direct current, and when it is broken with the inverse.

A new method of employing the frog, which I shall presently describe, adapts itself better than the galvanoscopic frog to the demonstration of the existence and direction of the muscular current, and of the proper current of the frog, and therefore supersedes the necessity of a galvanometer. For this purpose the frog is prepared in the ordinary manner of GALVANI, that is to say, it is cut in half through the middle of the vertebral column, skinned, and the viscera removed. It is then easy, with the help of scissors (introducing them under the lumbar plexuses), to remove the greater part of the pelvis of the frog, leaving the above-mentioned plexuses intact;

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finally, the frog is divided into two parts by cutting the juncture of the two thigh-bones. In this way there remain two halves of the frog united together organically by their spinal nerves. When this frog is required to be used for the purpose of discovering the presence and direction of the muscular current, it should be disposed upon an insulating plane in such a manner that its two extremities or claws dip into two separate recipients. Joining these two recipients with strings of cotton or thread, soaked with the same liquid as that contained in the recipients themselves, or with a strip of paper similarly wetted, no sign of contraction is ever produced, therefore no current is in circulation. This last fact may easily be proved by closing the circuit with the two ends of the platinum wire of a delicate galvanometer, provided we can be perfectly certain of the homogeneity of the substance of the extremities of the wires. And it will be seen that it cannot be otherwise, on reflecting that each half of the frog constitutes an electro-motor element of the proper current, so that, in the above-described manner of using the prepared frog, there are always two equal currents circulating in contrary directions, and which consequently neither excite contractions, nor deflect the needle of the galvanometer.

There is nothing easier, and at the same time more decisive, than the confirmation of the existence of the muscular current and of its direction.

A pile consisting of thighs of frogs, or of muscles of other animals, should be prepared in the manner I have described in my work that I have quoted above. The two extremities of this pile (the internal surface of the muscle on the one hand, and the external on the other) should dip in distilled or spring water. When the frog is prepared as already described, and stretched upon the insulating plane, the extreme cavities of the pile are made to communicate with the two recipients in which the claws of the frog are immersed, by means of strings of thread or cotton soaked with water (Plate III. fig. 1.). The frog is then distinctly seen to contract both on closing and on breaking the circuit; but both limbs do not contract equally, since that which is traversed by the current, and which consequently is near that extremity of the pile which is formed by the internal surface of the muscle, contracts on closing the circuit with the direct current, while, on the contrary, the other limb, which is near the extremity of the pile formed by the external surface of the muscle, contracts on breaking the circuit. Simply by the aid of the frog so prepared, it is possible to confirm the principal laws of the muscular current, which I have already discovered with the galvanometer. Thus it happens that the contractions of the galvanoscopic frog increase proportionally with the number of elements; and they are the same for a pile formed of muscular elements deprived of all visible nervous filaments, as for a pile the muscular elements of which are intact. The same takes place operating with a pile composed of muscular elements taken from frogs killed by the action of narcotic poisons, carbonic acid, prussic acid, &c. Finally, with the galvanoscopic frog it is easy to discover the immense difference which exists between the signs of

Fig. 1.

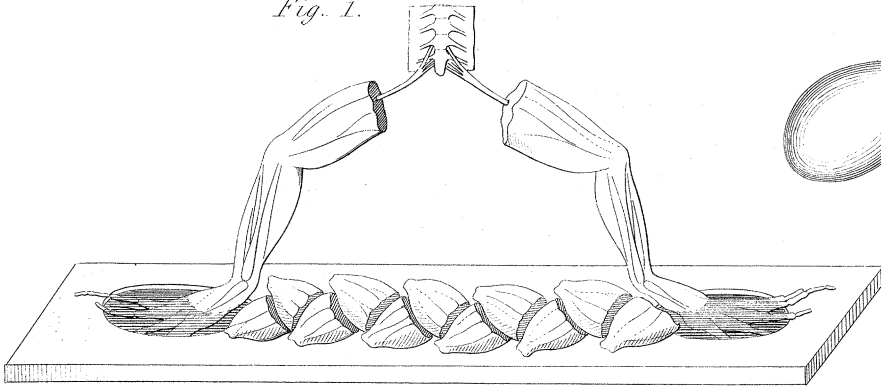


Fig. 2.

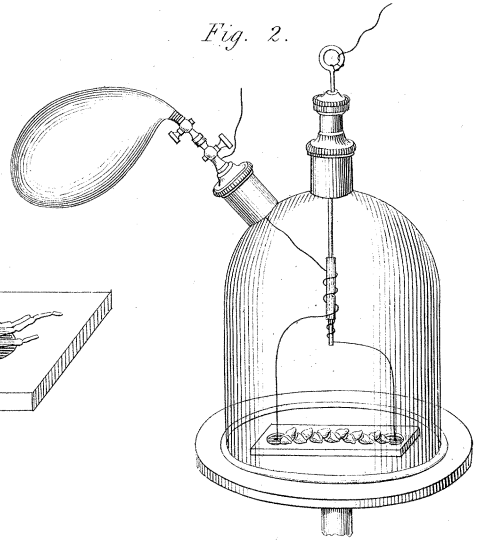


Fig. 3.

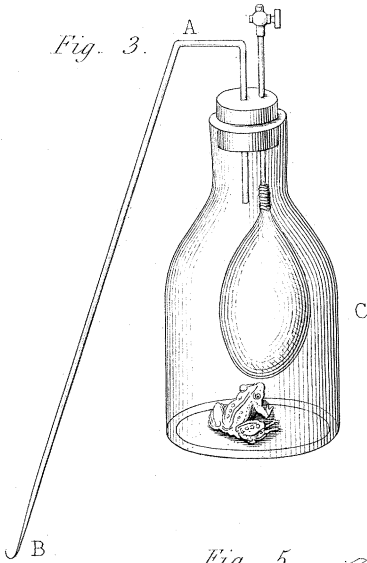


Fig. 4.

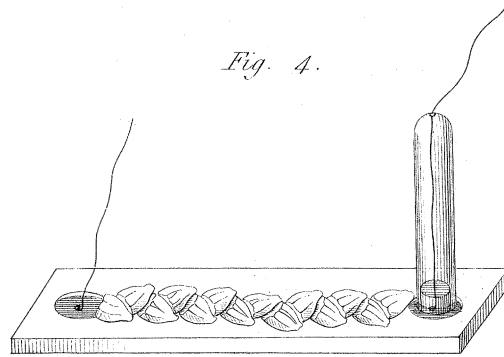


Fig. 5.

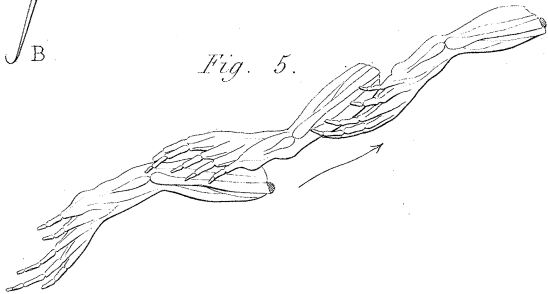


Fig. 6.

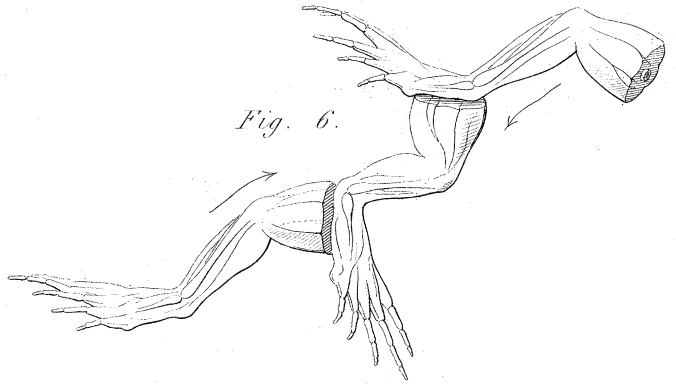
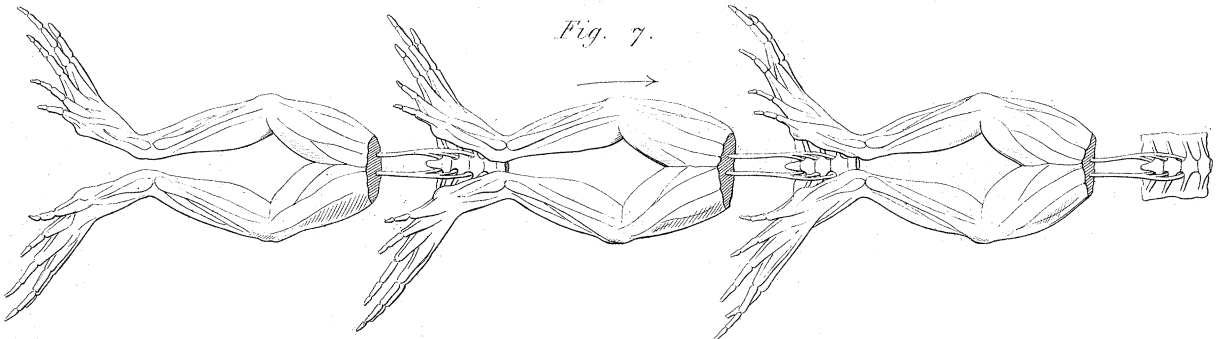


Fig. 7.



the muscular current, occurring some time after death in piles composed of the same number of muscular elements, but taken from different animals, as frogs, eels, or pigeons. The signs of the current diminish in a rapid ratio as we ascend in the scale of animals upon which we operate.

Hitherto I have merely exhibited experiments which confirm my former conclusions. I have simply sought to show how that without a very delicate galvanoscope, and with the galvanoscopic frog alone, any person may be enabled to demonstrate the principal laws of the muscular current.

It has always been an object of peculiar interest to me, in the study of the muscular current, to establish fresh experimental proof, or to repeat and extend my former ones; all which have led me to conclude that the existence and intensity of the muscular current depend upon the existence and intensity of the changes of structure and composition, which constitute the nutrition of the muscle.

Nevertheless, before entering upon the exposition of my researches instituted in this view, I will describe some fresh facts, which, although they may be anticipated, notwithstanding, better serve to fix the origin of the muscular current.

With a pile of half thighs of frogs I have distinctly obtained the decomposition of iodide of potassium. To effect this, I soak a piece of paper in a solution of this substance, and upon it, about the distance of a line apart, I place the extremities of two platinum wires, the other ends of which dip in the fresh water which fills the extreme cavities of the pile. To facilitate and expedite the decomposition of the hydruret, I increase the extent of those portions of the platinum wires which are immersed in the cavities of the pile, by twisting the extremities into a coil; I am also in the habit of further wetting the paper soaked with the solution of hydruret of potassium, with a solution of starch paste, to which I add a few drops of chlorine. After the circuit has remained closed a few seconds, a blue or a yellow spot is formed around the wire which communicates with the cavity in which the external surface of the muscle is immersed.

With a pile of twenty elements of half thighs of frogs, I have likewise distinctly obtained signs of tension, by means of a tolerably delicate condenser. To this intent I put one extremity of the pile in communication with the ground, and the other with the plate of the condenser. I have frequently repeated the experiment, at one time establishing a communication between the internal surface of the muscle and the condenser, and the external surface with the ground; at another time I have reversed this order. I have likewise observed the phenomena which ensue on putting each of the extremities of the muscular pile in communication with one of the plates of the condenser. In every case the electroscope has constantly exhibited signs of a negative charge upon the internal surface of the muscle, and of a positive charge upon the external surface.

Again, I was desirous of ascertaining how the signs of the muscular current were affected on excluding the air from contact with the pile. For this purpose I em-

ployed the receiver commonly in use for the experiments of electricity *in vacuo*, or in other gases than air. The two platinum wires are fastened to the metallic rod which moves in the tube in the centre of the receiver; one of the wires, however, is insulated from the rod by a layer of gum-lac which intervenes where the wire encircles the rod. These two wires are twisted into the shape of a pitchfork, and so diverge from one another, that on pushing the rod down the two extremities enter the extreme cavities of the muscular pile (fig. 2.). I rapidly exhausted the air in the receiver, and then connected the platinum wires with the galvanometer. I have repeated the experiment while the air was exhausted, and immediately after admitting the air again into the receiver. The direction of the current has never varied, nor have I perceived any very great difference in the intensity and duration of the current in the different modes of conducting the experiment.

It will be readily understood from the description of the apparatus employed for operating in a vacuum, how easy it was to adapt it to experiments in other gases: I have used hydrogen and carbonic acid.

I think it important to describe minutely the results obtained in these different experiments. I began with a pile composed of twenty muscular elements, or half thighs of frogs, placing it under the receiver, and I filled the cavities of the pile with well-water, after ascertaining that there was no sign of a current on immersing the ends of the wire in the same liquid. In less than five minutes the frogs were killed and the pile prepared. In performing these comparative experiments, I have made use of frogs caught on the same day and in the same pool. In all these experiments the circuit was kept closed, and the duration of the current in the galvanometer observed for several hours successively. The following are the numbers obtained in the different experiments.

Operating in atmospheric air with a pile of twenty elements of frogs, the deflection in my galvanometer was so strong that the needle reached  $90^{\circ}$ , oscillated, then gradually returned towards  $0^{\circ}$ . At about  $30^{\circ}$  the needle begins to be stationary, or at least it retrogrades considerably more slowly than at first. Every ten minutes I noted down the deviations, which are the following:  $15^{\circ}$ ,  $9^{\circ}$ ,  $5^{\circ}$ ,  $4^{\circ}$ ,  $3\frac{1}{2}^{\circ}$ ,  $3\frac{1}{2}^{\circ}$ ,  $3\frac{1}{2}^{\circ}$ ,  $3\frac{1}{2}^{\circ}$ . At the expiration of two hours the deflection was the same. The circuit was then broken, and the extremities of the wires immersed in pure water, the current produced was  $70^{\circ}$ , and its direction was contrary to the first when the circuit was closed with the muscular current. This was evidently the secondary current which at the commencement of my researches I showed to be the cause of the rapid decline of the muscular and proper current in those cases where the circuit is kept closed. I have twice repeated this same experiment with the same results.

I have constructed a third pile similar to the two already described, which I have put under the receiver mentioned above, and have exhausted the air to that degree of rarefaction that the mercurial column showed only one inch of pressure. I could not attain to a more complete degree of rarefaction on account of the aqueous vapour

which was continuously formed. I then closed the circuit, and the deflection at first was  $85^{\circ}$ ; as usual the needle oscillated, retrograded, and began to be stationary towards  $30^{\circ}$ . The following numbers show the degree of deflection at intervals of ten minutes, care having been taken to maintain constantly the degree of rarefaction unaltered. After the first ten minutes the deviation was  $15^{\circ}$ ,  $10^{\circ}$ ,  $7^{\circ}$ ,  $5^{\circ}$ ,  $5^{\circ}$ ,  $4^{\circ}$ , after two hours the deflection was fully  $3^{\circ}$ . On admitting the air into the receiver the needle advanced two or three degrees, then again returned to its deflection of  $3^{\circ}$ . This slight increase in the strength of the current occurs, as we shall presently see, whatever be the gas introduced into the receiver; it is also caused by working the air-pump while the circuit is closed, and this is probably owing to the liquid contained in the cavities of the pile being put in motion, so that the wires are more or less immersed. The lesser deviation obtained in this third experiment on first closing the circuit was very probably owing to the frog having been killed rather longer than the others, to the muscular elements immediately beginning to dry in rarefied air, and to some bubbles of air which adhere to the platinum wires when the pump is worked.

I next killed forty other frogs, and with them I composed two piles consisting of twenty elements each pile. I tried the effect of each of these piles separately in contact with the air; in one of them the first deflection was  $85^{\circ}$ , in the other  $88^{\circ}$ . I then put one of these piles under the receiver and exhausted the air; I closed the circuit, and the deflection was  $81^{\circ}$ . The needle oscillated, retrograded as usual, and remained fixed at  $15^{\circ}$  at the expiration of ten minutes. I now introduced a sufficient quantity of oxygen gas to fill the receiver. The needle oscillated slightly on admitting the gas, then continued retrograding, and after a lapse of thirty minutes it marked  $8^{\circ}$ , and at the end of another half-hour it showed  $4^{\circ}$ .

After this I placed the other pile under the receiver, exhausted the air, and closed the circuit. The first deviation of the needle was  $65^{\circ}$ . This difference is naturally owing to the length of time the frogs had been killed, that is, all the time taken up by my first experiment. Leaving the circuit closed, the needle as usual continued to retrograde, becoming stationary at  $4^{\circ}$ , as did the other pile immersed in oxygen. Finally, I re-admitted the air, and the deflection was not increased.

These experiments are sufficient to show that the muscular current neither varies in intensity nor in duration, after the death of the animal, from keeping the muscles from which it is obtained either in oxygen or in air reduced to a pressure equivalent to one inch of mercury.

I will now describe the results obtained from operating in the manner above described, in hydrogen gas. The singularity which this gas offers could not certainly have been anticipated before the experiment.

As usual I prepared a pile of twenty elements, half thighs of frogs; I placed it under the receiver, and previous to exhausting the air I completed the circle, and the deflection was  $85^{\circ}$ : the circuit was left closed. I then rapidly exhausted the air

and substituted hydrogen gas in its place. After the circuit had been closed ten minutes, the needle of the galvanometer remained stationary at  $15^{\circ}$ . But then instead of continuing to retrograde it began to advance, so that the deflection was  $50^{\circ}$  at the end of ten minutes. I broke the circle by drawing the rod up, and when the needle had returned to  $0^{\circ}$  I again completed the circuit; the needle now advanced to  $90^{\circ}$  in the usual direction of the muscular current, and remained fixed at  $55^{\circ}$ . From this point the needle of the galvanometer descended very slowly, being at  $40^{\circ}$  at the end of an hour. I then exhausted the hydrogen and re-admitted air, and the needle returned to  $12^{\circ}$ , continuing to recoil, but still more slowly than in the other experiments.

I repeated this same experiment with precisely the same results; that is to say, the needle descended as usual before the hydrogen was let into the receiver; after the introduction of the gas it advanced to  $50^{\circ}$ . I then produced a vacuum and again admitted the air, and the needle again descended, still however keeping up a greater deflection than there would have been if the hydrogen had never been used. I have been able, several times with the same pile, to observe the alternations of the effects produced by air and by hydrogen gas. Prolonging the contact of the hydrogen for some time, I have constantly observed that when that gas was exhausted and atmospheric air substituted for it, the final deflection of the needle, taking into account the time elapsed since the preparation of the pile, and *cæteris paribus*, was much greater than it would have been if no hydrogen had been introduced into the receiver. Finally, I will mention another experiment performed by closing the circuit with the usual muscular pile after having filled the recipient with hydrogen. The first deflection was the usual one, and then followed  $47^{\circ}$ ,  $41^{\circ}$ ,  $40^{\circ}$ ,  $38^{\circ}$ ,  $37^{\circ}$ ,  $35^{\circ}$ ,  $34^{\circ}$ ,  $32^{\circ}$ ,  $31^{\circ}$ ,  $30^{\circ}$ . These signs of deflection were noted down at intervals of ten minutes. After six hours the needle was stationary at  $25^{\circ}$ . When hydrogen is not used, that is in atmospheric air, *in vacuo*, or in oxygen, the deflection does not exceed  $5^{\circ}$  with the usual pile of twenty elements, after the circuit has remained closed an hour.

Although I could not attribute this singular difference produced by the presence of hydrogen upon the muscular pile to the action of this gas upon the source of electricity in the muscles, nevertheless it appeared to me important to discover the cause of this difference. I very quickly prepared forty elements (half thighs of frogs), using every possible precaution to have all those circumstances equal in all the conditions which are known to influence the muscular current. Thus, two individuals prepare the frogs at the same time, each frog is divided into halves, and two separate heaps are made, from each of which twenty elements, or half thighs, are selected. I leave twenty of these elements exposed to the air, and the others in an atmosphere of hydrogen by means of the receiver.

At the expiration of forty minutes I take these twenty elements out of the hydrogen, and compose the pile, and I do the same with the other elements left in contact with

the atmosphere. I oppose the two piles to one another in the manner I have described in my work quoted above, for the purpose of discovering the differential current. This done, I determine the intensity of the muscular current in each of the piles, taking note of the first deflection, and of that which the needle indicates after the circuit has been closed ten minutes. In another experiment performed in exactly the same manner, I confronted the current of one muscular pile, the elements of which had been in hydrogen, with another, the elements of which had been in air highly rarefied. Finally, before putting the muscular elements in hydrogen gas and in rarefied air, I measured the muscular currents of each of these piles, in order to compare them with those which were to follow. I should be too long if I were to report all the numbers of the different experiments which I performed. The conclusion to be drawn from them all is extremely simple. Hydrogen gas does not act differently upon the muscular elements from oxygen, and atmospheric or rarefied air, or in other words, these different gaseous media do not exert any influence upon the intensity and duration of the muscular current.

It only remained for me to determine precisely the cause of the singular effect exhibited by hydrogen; and as I could not doubt that this effect was owing to the action of hydrogen gas upon the secondary polarities evolved upon the platinum, I tried the following experiment. I introduced a platinum wire into a tube of glass, and with the blow-pipe I soldered it to the upper and closed end of the tube. I constructed a pile of twenty elements, and closed the circuit, as seen in fig. 4. The tube filled with water is inverted in the liquid contained in the extreme cavity of the pile, in which the outer surface of the muscle is immersed. When the circuit was closed the first deflection was  $90^\circ$ , then the needle retrograded as usual. When the deflection was at  $20^\circ$ , I introduced hydrogen into the tube A, and the needle instantly began to rise to  $25^\circ$ ,  $30^\circ$ ,  $40^\circ$ ,  $50^\circ$ . I allowed the hydrogen to escape, filling the tube again with water and closing the circuit, and after ten minutes the needle was at  $5^\circ$ . Again, I filled the tube with hydrogen and the needle rose to  $20^\circ$ ,  $25^\circ$ ,  $30^\circ$ . Finally, I transferred the hydrogen tube to the other cavity in which the inner surface of the muscle was immersed, and instead of advancing the needle only fell more rapidly.

The effect then of the hydrogen gas is to act upon the oxygen which tends to be evolved upon the platinum which transmits the muscular current, which passes, as is well known in the muscular element itself, from the interior of the muscle to the surface. It is a case analogous to the gas-pile of GROVE. These researches seem to prove that the cause of the rapid diminution, both of the muscular and proper current, from the circuit being kept closed, lies in the secondary polarities of the platinum extremities, which generate a current that circulates in a direction contrary to that of the muscular pile. I have yet to mention the experiments attempted by putting the muscular elements in contact with carbonic acid for different lengths of time. I have performed three experiments of this nature, in each of which I have confronted



the pile, the muscular elements of which had been in carbonic acid, with another pile similar to the former, but which had merely been left in contact with the air. After having opposed the two piles to one another, I have closed the circuit without ever having found that any difference was induced by the carbonic acid.

Summing up the results of the various and repeated experiments attempted in the view of discovering the influence of some gaseous media (air, air greatly rarefied, oxygen, hydrogen and carbonic acid) upon the intensity and duration of the muscular current, we must conclude that this influence is either null or insensible, or in other words, that the immediate causes of the development of electricity in the muscles reside in the muscular substance, independently of the gaseous medium in which it is placed.

In order better to determine the relation between the signs of the muscular current and the organico-vital conditions of the muscle, since the last few months of the past year I have directed my attention towards ascertaining the intensity of the muscular current generated by a pile of half thighs of frogs, always made up of twenty elements. I did this for the purpose of comparing the influence of the temperature of the medium in which the frogs had lived, with the muscular current which they gave: I will describe once for all and in a few words, my mode of experimenting. From November 1844 to the end of March 1845, I have constantly sent twice a week to have frogs caught from the same marsh. As soon as the frogs arrived at Pisa, they underwent the usual mode of preparation for determining the intensity of their muscular current. A certain number of these same frogs were put into a glass recipient without water, and kept in a little room, the constant temperature of which was  $+16^{\circ}$  C. ( $60^{\circ}8$  F.) A similar number of the frogs were placed in a like recipient, but exposed, on the terrace of the meteorological observatory, to the temperature of the atmosphere. Lastly, four frogs taken from the mass were put into the bottle furnished with a tube (fig. 3.), the tube AB of this bottle dipped in mercury. The apparatus thus disposed was likewise left exposed to the temperature of the atmosphere upon the terrace. In eight hours' time the tube C was opened, and inflating the bladder, a portion of the air was collected, and the quantity of carbonic acid produced by the four frogs in that time, and in those given conditions, determined. It is impossible for me to relate all the experiments performed for the space of five months, twice a week; comparing the frogs just arrived fresh from the marshes, with those kept for twelve and twenty-four hours at a temperature of  $+16^{\circ}$  C. I shall merely state some of the numbers given by these experiments; in order to render the results of so many experiments, all concordant, more evident. The temperature of the air, or of the medium in which they live, the activity of the respiratory function, the intensity and duration of the muscular current, are quantities which vary from one another proportionally; the temperature of the medium in which the frog lives cannot be raised without occasioning an increase of activity in its respiration, and a corresponding increase in the intensity and duration of the muscular current.

Four frogs exposed to a temperature varying from  $0^{\circ}$  to  $4^{\circ}$ , produced in twenty-four hours 0.5 cubic centimeters of carbonic acid; four other frogs placed in the same recipient and in the same conditions, but at  $+16^{\circ}$  C, gave 0.3 cubic centimeters of carbonic acid. The celebrated experiments of EDWARDS give double this number at a temperature of  $+27^{\circ}$  C. We now come to the signs of the muscular current. In the coldest days of the past winter twenty elements gave a deflection of  $32^{\circ}$ , then in succession, as the temperature of the air increased,  $38^{\circ}$ ,  $48^{\circ}$ ,  $50^{\circ}$ ,  $56^{\circ}$ ,  $60^{\circ}$ ,  $66^{\circ}$ . These indications correspond to the gradual increase of the temperature from  $0^{\circ}$  to  $8^{\circ}$ . Finally, in the present month, the thermometer rising to  $+15^{\circ}$  in the shade, the intensity of the muscular current was expressed by the numbers  $80^{\circ}$ ,  $85^{\circ}$ ,  $90^{\circ}$ . It is needless for me to repeat that these numbers refer to the first deviation which takes place on closing the circuit of the pile of twenty half thighs of frogs, always disposed in the same manner, and with their extremities immersed in the two cavities of the board, filled with spring water.

I attained to the same result, comparing the intensity of the muscular current produced by frogs recently caught in the cold season, with that obtained from frogs which had been left for the space of twenty-four hours to forty-eight hours exposed to a temperature of  $+16^{\circ}$  C. The following are some of the many numbers noted down, for the sake of demonstrating, even in this case, the relation between the intensity of the muscular current and the temperature in which the frogs have lived. A pile composed of frogs caught in the coldest part of the season have produced a deflection of  $32^{\circ}$ . The deflection caused by other similar frogs which were kept for two days in a warmer temperature, was  $38^{\circ}$ . In another case the muscular current rose from  $30^{\circ}$  to  $48^{\circ}$ , in another from  $50^{\circ}$  to  $64^{\circ}$ , in another from  $66^{\circ}$  to  $85^{\circ}$ .

I would observe, however, that if the frogs are kept too long exposed to a warm temperature, and deprived of the medium in which they generally live and are nourished, instead of the increase of the muscular current, produced by an increase of temperature taking place, a considerable diminution of the current follows in comparison with that of the frogs recently caught. Experience has also shown me (that which was easy to foresee), that in proportion to the elevation of the temperature in the frogs originally, so much the sooner the effect of the want of nutrition was manifest.

I think it important to describe a few experiments which establish the relation between the intensity of the muscular current and the activity of the respiratory function. I have only been enabled to try these experiments upon frogs, from their great tenacity to life. I have repeated the following experiment several times: I skinned ten frogs completely, and put them into a glass recipient near to another in which were ten other frogs intact. The frogs thus flayed live for six, eight, and even ten hours, and are even quite lively. The muscular current produced by the skinned frogs was always considerably weaker than that of the frogs in their natural

state. In four experiments made upon the skinned frogs, left in this state from two to eight hours, the first deflections were from  $80^{\circ}$  to  $85^{\circ}$ , and the needle first remained stationary at  $18^{\circ}$ , then continued to fall slowly. In four other experiments made upon frogs in their natural state, the first deflection was  $90^{\circ}$ , and the needle was stationary at  $25^{\circ}$ , then fell as usual.

I have performed a few experiments upon the muscular current of warm-blooded animals. In one of these experiments, communicated by M. DUMAS to the Academy of Sciences at Paris, and in which I composed the muscular pile, with live pigeons, I succeeded in obtaining the signs of the muscular current. The result of that experiment was to show me that the intensity of this current increased in proportion to the rank the animal operated on occupies in the scale of animals, while the persistence of the current diminished in the same proportion. Operating with great rapidity upon chickens and pigeons, I have been able to demonstrate the truth of this, using for my experiments the thighs of the above-mentioned animals. Comparing an equal number of elements, whether of fowls or pigeons, with the same number of elements taken from frogs, the current, at first, is as intense, and in the greater number of cases more so than that of the frogs. Reflecting a moment on the greater length and resistance of the circuit of the pile of fowls and pigeons, the greater intensity of the muscular current in warm-blooded animals than in frogs, will be manifestly proved. This advantage, however, persists but for a very short space of time: a pile of eight elements of half thighs of pigeons or fowls, at the expiration of an hour, gives either no sign at all, or an almost imperceptible sign of a muscular current in the most delicate galvanometer I possess. This is far from being the case with the same number of half thighs of frogs, which continue for eight hours and more to manifest signs of the same current.

Nor is this difference owing to an unequal evaporation, whether from the internal or external surface of the muscle. I have very frequently bathed the surface of the muscle with pure water, and made a fresh surface, by cutting it away with a razor, and reconstructing the pile: I have never found more than a slight increase in the first deflection, and none whatever on waiting until the needle became fixed.

In order still better to confirm some of my former experiments for showing the influence of the sanguineous circulation upon the intensity and duration of the signs of the muscular current, I compared the current produced by twenty elements, or half thighs of frogs, in their natural state up to the moment of conducting the experiment, with twenty others, from which the heart had been taken away, but which still preserved considerable power of motion for a great length of time; by this means I have established the conclusions to which I had arrived, proving that the muscular current is very much weakened by the defect of the sanguineous circulation. But the following experiment, better than all those referred to formerly, will demonstrate the truth of this last conclusion. From a quantity of frogs all caught in the same

pool, I chose twenty, which I put into water that had boiled for two hours. I covered the glass cylinder in which this water was contained, with a plate of glass which I luted to the cylinder. To prevent the water from again taking up air, I covered the surface with oil. The temperature of the water was  $+15^{\circ}$  C. The frogs appeared more vivacious at the commencement of the experiment, from their continual movement from the surface of the water to the bottom, and *vice versa*, but it was not long before this vivacity ceased, and in about an hour they were all at the bottom of the cylinder, showing signs of suffering, and with but little motion. In two hours all motion entirely ceased and the frogs seemed dead. I have repeated this experiment twice with the same result, the only difference discoverable was in the time, which varies inversely as the temperature of the medium. The signs of the muscular current are considerably weakened in a pile composed from frogs which have been labouring under asphyxia for some time. Thus, while a pile composed of frogs which had not been submitted to any previous injury, caused a first deflection of  $90^{\circ}$ , and the needle remained fixed between  $25^{\circ}$  and  $30^{\circ}$ , with a pile equally composed of twenty elements, but which had been taken from frogs in a state of asphyxia, the first deflection was not more than from  $50^{\circ}$  to  $60^{\circ}$ , and the needle stopped at from  $10^{\circ}$  to  $12^{\circ}$ . This difference is very striking, and the influence of a normal state of the sanguineous circulation and respiration could not be more clearly demonstrated. I will mention here a singular appearance of the muscles in frogs asphyxiated: the muscles are almost white, and acquire a slightly red tint from exposure to the air. I was desirous to renew and verify the singular action of sulphuretted hydrogen. The following numbers, the result of various experiments, show that the muscular current is very much weakened in frogs killed by this gas. Twenty elements, or half thighs of frogs killed in the usual manner, gave a deflection of  $56^{\circ}$ . Another twenty elements taken from the same mass gave  $44^{\circ}$ , another similar pile  $41^{\circ}$ .

I will now sum up the results obtained from these different experiments. In the first place, the intensity and duration of the muscular current are independent of the nature of the gas which envelopes the muscular pile. Secondly, this current, as I have already shown from the commencement of my researches, is altogether independent of the cerebro-spinal nervous system, and the circumstances which exercise a marked influence upon its intensity are respiration and the sanguineous circulation. Thirdly, those poisons which seem to act directly upon the nervous system, have no influence upon the muscular current; among these I would mention hydrocyanic acid, morphine and strychnine. Fourthly, sulphuretted hydrogen has a marked influence in diminishing the intensity of the muscular current. Fifthly, the intensity of the muscular current varies according to the temperature in which the frogs have lived a certain time; it is needless to observe that this result is not discoverable except in those animals which like the frog necessarily take their temperature from that of the medium in which they live. Sixthly, the intensity of the muscular current

increases in proportion to the rank the animals occupy in the scale of beings, while the duration of this current after the death of the animal is in an exactly inverse ratio.

Comparing these conclusions with those generally admitted by physiologists, and drawn from a great number of experiments on the vital properties of muscles, it is impossible not to perceive that the property of the muscles, immediately connected with the muscular current, is that which HALLER calls irritability, and which at the present day I believe physiologists designate by the name of organic contractility, or simply contractility.

With regard to the manner of representing the origin of the muscular current, I only find in my present experiments a confirmation of the opinion I set forth in my preceding ones. The chemical action which goes on in the nutrition of the muscle, principally that which takes place in the contact of the arterial blood with the muscular fibre, is in all probability the source of this electricity in the muscles. I will not here repeat all the many and minute researches, together with the precautions I took, in the numberless experiments attempted in order to exclude every possible cause of a current foreign to the muscle; and nobody who has taken the pains to follow out the description of my experiments, can retain any doubt that the origin of the muscular current is in the muscle endued with a certain degree of vitality. The experiments referred to, making the pile act in vacuum, in hydrogen, in oxygen, and in carbonic acid, prove with full evidence that it is not the action of the gas upon the inner surface of the muscle which occasions the current. To remove all possible doubt, I have taken the precaution of preparing the half thighs of frogs with gilded scissors or with pieces of glass made sharp at the edges; the muscular current was the same, as in fact it ought to have been, as indeed it should have been.

It might be said, reasoning according to a theory the value of which is well-appreciated at the present day, that the cause of the muscular current resides in the contact between the inner and the outer part of the muscle, or in other words, of two heterogeneous bodies. Let this then be the simplest interpretation of all the facts discovered by myself; it is sufficient for me to have well-established that this contact of heterogeneous parts of the muscle generates electricity in the conditions discovered, and such as they are described in the results referred to above.

With regard to my own opinion, it appears more satisfactory to say that the development of electricity takes place in the muscle during life from the chemical action between the arterial blood and the muscular fibre; that the two electric states evolved in the muscle neutralize each other, at the same points from which they are evolved, in the natural conditions of the muscle; and that in the muscular pile imagined by myself, a portion of this electricity is put in circulation just as it would be in a pile composed of acid and alkali, separated from each other by a simply conducting body.

I will conclude this memoir upon the muscular current, with the description of an

experiment which appears to me to tend to establish the origin of the muscular current according to these views.

I prepared a great number of little cones, of about the size of half a frog's thigh, cutting the very thin membrane of the cæcum intestine into triangular pieces, and folding them up, and gumming them together upon a little wooden form of a conical shape. When these cones were dry, I prepared some fibrine by beating up some bullock's blood, the bullock being killed at that moment. I immediately fitted the cones with this fibrine steeped in blood, and composed a pile of twenty elements precisely similar to the pile of half thighs. This pile did not exhibit the slightest signs of a current in the most sensible of my galvanometers. Nor is it to be imagined that there was any want of conductivity in the pile just described ; in fact, I added four thighs of frogs prepared to the above twenty elements, and I obtained from this pile a deflection but slightly differing from that which the frogs gave, acting alone. This fact evidently proves that simple heterogeneity of the animal parts is not sufficient to produce the muscular current ; this heterogeneity should be such as exists in the living muscle.