ELECTROPHYSIOLOGICA

BEING AN ATTEMPT TO SHOW HOW ELECTRICITY MAY DO MUCH OF WHAT IS COMMONLY BELIEVED TO BE THE SPECIAL WORK OF A VITAL PRINCIPLE

I

ON a white marble slab let into the front of a house in the Strada Felice at Bologna is an inscription showing that, in this house, then his temporary dwelling-place, at the beginning of September 1786, Galvani discovered animal electricity in the dead frog, and hailing this event as the well-spring of wonders for all ages (Luigi Galvani in questa casa di sua temporaria dimora al primi di Septembre dell' anno 1786, scoperse dalle morte rane La Ellettricita Animale—Fonte di maraviglie a tutti secoli). Animal electricity, well-spring of wonders for all ages! Yes, said I, as I copied these words a few weeks ago, and as I went into the house repeating them to myself. Yes, still said I, after seeing what was to be seen within the house. Within the house, indeed, there was much to excite the imagination, and to make me more ready to accept these words as the sober utterance of simple truth. Still the same were the common stairs leading from the open outer door to the landing on the first floor, with its two main doors, one on each side, each one opening to a distinct set of apartments, in one of which had lived the discoverer of animal electricity; and the only change of moment was one which served to call back more vividly the memorable past—a portrait in lithograph of Galvani himself hanging upon the wall facing the stair-head. Still the same was a third and smaller door, at which the portrait seemed to be looking, and beyond which were the stairs leading to the belvedere on the roof so common in Italian houses hereabouts. Still the same were these stairs, the lower flights of uneven bricks, the upper of ricketty woodwork, unmended, scarcely swept, since the time when Galvani went up and down them afire with the discovery made in the belvedere to which they led. Still the same was the belvedere itself—the same walls, blank on one side, pierced on the three others with arched openings, two at each end, three at the front, each opening being built up breast-high so as to form the parapet—the same roof overhead with its bare rafters and tiles—and, running across each opening a little below its arched top and parallel with the parapet, the very same iron bar upon which the frogs' limbs had been suspended by copper hooks in the experiment to which the inscription on the slab outside the house refers, and about which Galvani wrote:—"Ranas itaque consueto more paratas uncino ferreo earum spinali medulla perforata atque appensa, septembris initio (1786) die vesperascente supra parapetto horizontaliter collocavimus. Uncinus ferream laminam tangebat; en motus in rana spontanei, varii, haud infrequentes! Si digito uncinulum adversus ferream superficiem premeretur, quiescentes excitabantur, et toties ferme quoties hujusmodi pressio adhiberetur." So little change was there, indeed, that, forgetting the present altogether, I could only think of this experiment in which the existence of animal electricity was divined, and of those myriad other experiments to which it had led, and by which in the end the truth had been made manifest. So absorbed was I in these thoughts that I even forgot to look through the open arches of the belvedere at the blue Italian sky and the other beauties of the prospect. And when at length I came down, I was more than ever in the mind to assent unhesitatingly to the words, "la ellettricita animale, fonte di maraviglie a tutti secoli"—more than ever convinced that animal electricity would prove to be the key by which to unlock not a few of the secrets which are supposed to be exclusively in the keeping of life—more than ever resolved still to go on seeking for truth in the path along which I was urged to go by this conviction.

Nor was I long at a loss how to begin to carry out this resolution. I wanted to reiterate briefly and more clearly some of the things which I had said before respecting animal electricity, and the way in which this force may do a work ascribed to life in muscular action and nervous action; and at the same time to make use of certain new facts which were not a little calculated to confirm former conclusions. I wanted to show that the same workings of animal electricity may be detected in the condition called tone, and even in growth, and that these processes, no less than muscular action and nervous action, may have to be looked upon as electrical rather than as vital manifestations. A natural way of carrying out the resolution I had formed was, indeed, to do the work ready for me; and therefore the task I have now set myself is to do this work, beginning with an attempt to set forth a new theory of animal electricity, and then proceeding to say something in turn on the way in which this theory sheds light upon muscular action, nervous action, the maintenance of the state called tone, and the process of growth in cells and certain fibres—something calculated to show that in each of these cases animal electricity may have to do much of what is commonly believed to be the work of a vital principle.

1. *On a theory of animal electricity which seems to arise naturally out of the facts.*

A current, to which the name of muscle-current is given, may easily be detected in living muscle. It may be detected by applying the electrodes of the galvanometer, the one to the surface made up of the sides of the fibres, the other to that made up of either one of the two ends of the fibres, and also, though much less clearly, by examining either of these two surfaces singly, provided only the two points to which the electrodes are applied are at unequal distances from the central point of the surface. It may not be detected, if, instead of applying them in this manner, the electrodes are applied so as to connect either the two surfaces made up of the ends of the fibres, or two points equidistant from the central point of the surface made up of the sides, or of that formed by either one of the ends of these fibres. A current may or may not be detected under such circumstances, and when it is detected its direction is such as to show that the surface made up of the sides of the fibre is positive in relation to that made up of either one of the two ends, and that the former surface is more positive and the latter more negative as the distance increases from the line of junction between these two surfaces. In this way the galvanometer makes known the existence of points of similar and dissimilar electric tension in living muscle; and the only inference from the facts would seem to be that there is a current when the electrodes are applied so as to bring together points of dissimilar tension, but not otherwise. The facts are not to be questioned. The inferences arising from them can scarcely be mistaken.

This current is to be detected in living muscle, but not in muscle which has passed into the state of rigor mortis. As muscle loses its "irritability," indeed, it ceases to act upon the galvanometer, and no trace of the current is to be met with after the establishment of rigor mortis. As a rule, too, nothing is to be noticed except a gradual failure of current; but now and then (though not in the frog) there may be a reversal in direction in the last moments preceding the final disappearance.

When muscle passes from the state of rest into that of action, there is also a change in the muscle current to which the name of "negative variation" is given by its discoverer Du Bois-Reymond. Thus, when a galvanometer is connected with the gastrocnemius of a frog so as to respond to its muscle-current during the two states of rest and action in the muscle, the needle, which may have stood at 90°, or thereabouts, during the state of rest, is seen to fall back, and take up a position at 5° or nearer still to zero, during action. This change it is which is spoken of as "negative variation." It is a change indicating, not reversal of the current, but simple weakening; for the idea of reversal, which is readily suggested to the mind by the way in which the needle swings back past zero when the state of action is first set up, is at once corrected by the position which the needle takes up a moment or two later, and also by the fact that when the muscle-current of the *contracted* muscle is admitted into the coil of the galvanometer while the needle is resting at zero—when, that is, the experiment is not complicated by the muscle-current of the *relaxed* muscle being in the coil when the state of contraction is set up in the muscle—the needle is found to move in the same direction as that in which it moved under the current of the *relaxed* muscle, but not to the same distance from zero by a very great deal. So that, in fact, this "negative variation" of the muscle-current is nothing more than a sudden disappearance or failure of this current, and no good is gained by retaining a name which only serves to confuse and perplex.

Substituting the new quadrant electrometer of Sir William Thomson for the galvanometer, tensional changes are detected which are in every way parallel with the current changes which have been mentioned.

With this instrument, it is found that the surface made up of the sides of the fibres in living muscle, and that made up of either one of the two ends of these fibres, are in opposite electrical conditions, the ray of light marking the movement of the aluminium needle passing in the direction indicating positive electricity under the charge supplied by the former surface, and in the direction indicating negative electricity under the charge supplied by the latter surface—passing, that is to say, not in one direction only, as it would do if the needle were acted upon by charges differing, not in kind, but in degree only, but to the right in the one case and to the left in the other. It is found, indeed, not only that the surface made up of the sides of the fibres of living muscle is positive, and that made up of either end of these fibres negative; but also that the former surface is more positive and the latter more negative as the distance increases from the line of junction between these surfaces. With this instrument, too, it is found that these indications of free electricity fail *pari passu* with this failure of the "irritability" of the muscle, that they have disappeared altogether before the advent of rigor mortis, and also that there is a change which serves to point to discharge, more or less complete, when muscle passes from the state of rest into that of action. Thus—in illustration of this latter fact—if the ray of light on the scale stand at 30° under the charge supplied to the electrometer by either one of the two surfaces of living muscle during the state of rest, it will stand at 5° only, or still nearer to zero, under the charge supplied by the same surface during the state of action. The difference is always marked, and always of the same character; and, being so, the proof of discharge during action would seem to be as complete as may be, seeing that the instrument only takes cognizance of electrical changes of the nature of charge and discharge.

These, then, are the facts which may be looked upon as fundamental. There are the facts brought to light by Du Bois-Reymond through the instrumentality of the galvanometer—the muscle-current, present in living muscle during the state of rest, suddenly disappearing when the state of rest changes for that of action, gradually disappearing as muscle loses its "irritability," and absent altogether in rigor mortis; there are the facts which I myself have been able to make out for the first time by means of the wonderfully sensitive new quadrant electrometer of Sir William Thomson—the two opposite charges of electricity, one positive, the other negative, present in living muscle during the state of rest, disappearing suddenly when this state changes for that of action, gradually disappearing before, and altogether absent in, rigor mortis. And this is all that need be said upon this subject at present.

And as in muscular so in nerve tissue, there is the current, in this case called the nerve-current, and there are the two opposite charges, positive and negative, this current and these charges being present during life, disappearing suddenly when the state of rest changes for that of action, disappearing gradually *pari passu* with the "irritability," and absent altogether at the time when rigor mortis has seized upon the muscles; and in truth every particular in the electrical history of the muscle is repeated with strict exactness in the electrical history of the nerve.

In these two tissues, muscle and nerve, there is no difficulty in arriving at a knowledge of these facts; in other tissues the case is different. In other tissues, indeed, all that can be said is that faint indications of electricity are to be detected during life only, and that in some of the fibrous structures there are differences between the surface made up of the sides of the fibres and that made up by either one of the two ends, which correspond to those met with in muscle and nerve.

These then being the fundamental points in the history of animal electricity, the question is as to their meaning. To what theory do they point?

In order to account for this muscle-current and nerve-current, Dr. Du Bois-Reymond supposes that the muscle-fibre and nerve-fibre (the same law applies absolutely to both) are made up of what he calls peripolar molecules—of molecules, that is to say, which are (with the exception of certain moments in which these electric relations may be reversed) negative at the two poles and positive in the equatorial belt between those poles. He supposes that the sides of the fibres are positive because the positive equatorial belts are turned in this direction, and that the two ends are negative because the negative poles of the molecules face towards the ends. He supposes also that the muscle-current and nerve-current are merely the outflowings of infinitely stronger currents ever circulating in closed circuits around the peripolar molecules of the muscle and nerve respectively. And this view no doubt has much to recommend it.

But another view may be taken of this matter—a view according to which this electrical condition of living muscle and nerve during rest is, not current, but static; and this view is that which recommends itself to my mind as in every way more simple, more comprehensive, and more to the point practically.

In taking this view the great resistance of the animal tissues to electrical conduction serves as the starting point. I assume that parts of these tissues may be bad enough conductors to allow them to act as *dielectrics*. I assume that the parts which are thus capable of acting as dielectrics are the sheaths of the fibres in muscle and nerve, or the cell-membrane of the contractile cells of those fibres in muscle which have no proper sheath. I assume that a charge, usually the negative, may originate in the molecular reactions of the contents of the sheath or cell-membrane, and that this charge, acting upon the inner surface of the sheath or cell-membrane, may *induce* the opposite charge upon the outer surface of the sheath or cell-membrane, and that in this way the sheath or cell-membrane during rest is virtually a charged Leyden-jar. I assume that this charge is discharged when the state of rest changes for that of action. I assume that the surface made up of the sides of the fibres in muscle and nerve is positive because positive electricity has been *induced* upon this surface, and that the surface made up of either cut-end of the fibre is negative, because the negative electricity, developed upon the inner surface of the sheath or cell-membrane, is *conducted* to these ends by the contents of the sheath or cell.

All that I assume, indeed, may be readily illustrated upon a small cylinder of wood, left bare at its two ends, and having its sides covered with a coating which may be charged as a Leyden-jar is charged—a threefold coating, formed of an inner and outer layer of tinfoil, with an intermediate layer of gutta-percha sheeting, the latter layer projecting a little towards the two ends of the cylinder, so as to secure the necessary insulation of the inner and outer metallic surfaces; for by charging the inner layer of foil with negative electricity, this cylinder, which may be regarded as a model of a muscular fibre, is found to be, not only positive at the sides and negative at the two ends, but more positive at the sides and more negative at each end as the distance increases from the line of junction between the sides and ends. With this model thus charged, indeed, it is easy to imitate all the phenomena of the nerve-current and muscle-current, provided the electrodes of the galvanometer be applied in a suitable manner, and the charge kept up. With this model thus charged, it is also easy to imitate all the tensional phenomena of nerve and muscle which are made known by the electrometer. And thus the nerve-current and muscle-current, instead of being out-flowings of infinitely stronger currents ever circulating around peripolar molecules, may be secondary phenomena only, the accidental result of certain points of dissimilar electric tension upon the surface of the fibres of muscle and nerve being brought into relation by means of the galvanometer or the electrometer, as the case may be.

In this view, I have assumed that certain parts of nerve and muscle were sufficiently bad conductors to enable them to act as dielectrics, but I had not, it is easy to see, the firmest ground for this assumption. It was certain that these tissues were bad conductors; it was not certain that they were bad enough conductors for my purpose. Here, then, was occasion for new work—for work which must be done before I could hope to gain a secure footing for my theory; and this, therefore, was the task I set myself a few months ago, and about which I have now to say something.

In this work I have made use of a Wheatstone's Bridge having on each side resistance coils of the value respectively of 10, 100, and 1,000 B. A. units, of a set of resistance coils capable of measuring up to 1,000,000 of the same units, and of a battery consisting of six medium-sized Bunsen's cells. With this apparatus I have measured the resistance of muscle, tendon, yellow elastic ligament, brain, and spinal cord, the portion measured in each case being a parallelogram an inch in length by $\frac{1}{20}$ of an inch in breadth, formed by making a slice with a Valentin's knife, of which the blades were $\frac{1}{20}$ of an inch apart, and then cutting a strip from the slice by moving the knife, with its blades still separated to the same degree, at right angles to its surface. In order to eliminate the resistance due to secondary polarity, I measured each of these bodies at ·25, ·50, and ·75 of the inch, as well as at the full inch, the fact being, as was pointed out by Sir Charles Wheatstone in his first great paper on the means of measuring electrical resistance, that while the resistance of a conductor increases with its length, the resistance due to secondary polarity remains the same everywhere. Thus, *at* ·25 it is impossible to say how much of the resistance met with belongs to the body itself, and how much to secondary polarity; but not so *after* ·25, *at* ·50, or ·75, or 1·0; for the resistance belonging to secondary polarity being the same at ·50, ·75, and 1·, as at ·25, it follows that by deducting the resistance at ·25 from the resistance at ·50, ·75, and 1·0 the difference at each of these points will represent the resistance of the body itself between ·25 and that particular point.

Of these measurements those which I made last of all will serve as well as any others for the text of what I have now to say, and these are as follows:—

	Inch.		B. A. units.
Muscle (ox)	at ·25	=	17,000
	·50	=	27,000
	·75	=	36,000
	1·0	=	46,000
Tendon (ox)	at ·25	=	19,000
	·50	=	43,000
	·75	=	69,000
	1·0	=	99,000
Yellow elastic ligament (ox) . .	at ·25	=	160,000
	·50	=	300,000
	·75	=	820,000
	1·0	=	1,000,000 and more.
Brain (ox)	at ·25	=	11,500
	·50	=	16,100
	·75	=	23,000
	1·0	=	32,000
Spinal cord (ox)	at ·25	=	8,300
	·50	=	14,200
	·75	=	17,500
	1·0	=	22,500

I had made several measurements before these, corresponding more or less closely with them in results, and I was proceeding to make others, with a view to arrive at some common mean of numbers, when I found that the resistance went on continually altering, every moment becoming higher and higher, until in the end it was beyond the reach of my means of measurement.

Thus, in the strip of spinal cord, the resistance at ·25 inch, which at first was 8,300, was 180,000 in five hours, and more than 1,000,000 twelve hours later.

Thus, the resistance of the strip of brain, which at first was 11,500 at ·25 inch, was 25,000 five hours later, and upwards of 1,000,000 after the still further lapse of a dozen hours.

And so, likewise, with muscle, and tendon, and yellow elastic ligament, there was a corresponding increase of resistance when the measurement was repeated at these different times after the first trial.

Nor was this the only proof of a change of this sort; for on repeating these measurements on the same specimens some days later, after they had become thoroughly dried up, I found that the very shortest length which could be got for measurement—a length so short, that the two electrodes conveying the measuring current were all but touching—gave a higher resistance than that which could be gauged by the means at my disposal.

These, then, being the facts, it was evidently useless to go on searching for any numbers which could express anything like a common mean of resistance. It was evident, indeed, that the soft tissues, one and all, apart from moisture, were to be looked upon as insulators, rather than as conductors. Nay, it was possible that they might be insulators rather than conductors even in the fresh state; for it is quite supposable that in this fresh state the walls of the fibres and cells forming these tissues may be virtually dry, with moisture on each side, not with moisture percolating from side to side, and that the degree of resistance presented by these tissues, when fresh, is not that which would be encountered if the current passed *across* these walls, but that which is encountered by the current in passing along their outer moistened surface. It is quite supposable that the measuring current may not pass *across* the walls of the cells and fibres at all, but may glide over and between them only. All this is supposable; and therefore, the facts being as they are, I am, as I conceive, at liberty to assume that the walls of fibres and cells are sufficiently non-conducting to justify me in adopting the theory which I have ventured to propose—a theory, according to which, the electrical condition of muscle and nerve during rest is, not current, but static—the sheath of the fibre, or membrane, taking its place, being always charged as a Leyden-jar is charged, except during the time of action, when there is a discharge of this charge—a theory which, to say the least, has a less visionary foundation than that which rests on peripolar molecules seeing that it rests upon structural facts which cannot be called in question—a theory also which, as will be seen in due time, has this in its favour,—that it will simplify not a little several important problems in physiology.

C. B. RADCLIFFE

ICE-MAKING IN THE TROPICS

THE most marked example of the influence of radiation of heat on temperature is its influence on the production of artificial ice by the natives of India.

The fields in which the ice is made are low, flat, and open; and the ice is produced in large quantities when the temperature of the air is 16° or 20° F. above the freezing point; and the plan followed is an interesting example of accurate observation applied to practical purposes by a people now ignorant of science. The same process has been employed from time immemorial in India with scientific accuracy; and while the theory was explained by Dr. Wells,* the practical application was not so well understood; and this first led me to investigate the subject in India.†

The following method is employed by the natives of Bengal for making ice at the town of Hooghly near Calcutta, in fields freely exposed to the sky, and formed of a black loam soil upon a substratum of sand.

The natives commence their preparations by marking out a rectangular piece of ground 120 feet long by 20 broad, in an easterly and westerly direction, from which the soil is removed to the depth of two feet. This excavation is smoothed, and is allowed to remain exposed to the sun to dry, when rice straw in small sheaves is laid in an oblique direction in the hollow, with loose straw upon the top, to the depth of a foot and a half, leaving its surface half a foot below that of the ground. Numerous beds of this kind are formed, with narrow pathways between them, in which large earthen water-jars are sunk in the ground for the convenience of having water near, to fill the shallow unglazed earthen vessels in which it is to be frozen. These dishes are 9 inches in diameter at the top, diminishing to $4\frac{3}{10}$ inches at the bottom, $1\frac{3}{10}$ deep, and $\frac{3}{10}$ of an inch in thickness; and are so porous as to become moist throughout when water is put into them.

During the day the loose straw in the beds above the sheaves is occasionally turned up, so that the whole may be kept dry, and the water-jars between the beds are filled with soft pure water from the neighbouring pools. Towards evening the shallow earthen dishes are arranged in rows upon the straw, and by means of small earthen pots, tied to the extremities of long bamboo rods, each is filled about a third with water. The quantity, however, varies according to the expectation of ice—which is known by the clearness of the sky, and the steadiness with which the wind blows from the N.N.W. When favourable, about eight ounces of water is put into each dish, and when less is expected, from two to four ounces is the usual quantity; but, in all cases, more water is put into the dishes nearest the western end of the beds, as the sun first falls on that part, and the ice is thus more easily removed, from its solution being quicker.

There are about 4,590 plates in each of the beds last made, and if we allow five ounces for each dish, which presents a surface of about 4 inches square, there will be an aggregate of 239 gallons, and a surface of 1,530 square feet of water in each bed.

In the cold season, when the temperature of the air at the ice-fields is under 50° F., and there are gentle airs from the northern and western direction, ice forms in the course of the night in each of the shallow dishes. Persons are stationed to observe when a small film appears upon the water in the dishes, when the contents of several are mixed together, and thrown over the other dishes. This operation increases the congealing process; as a state of calmness has been discovered by the natives to diminish the quantity of ice produced. When the sky is quite clear, with gentle steady airs from the N.N.W., which proceed from the hills of considerable elevation near Bheerboom, about 100 miles from Hooghly, the freezing commences before or about midnight, and continues to advance until morning, when the thickest ice is formed. I have seen it seven-tenths of an inch in thickness, and in a few very favourable nights the whole of the water is frozen, when it is called by the natives solid ice. When it commences to congeal between two and three o'clock in the morning, thinner ice is expected, called paper-ice; and when about four or five o'clock in the morning the thinnest is obtained, called flower-ice.

Upwards of two hundred and fifty persons, of all ages, are actively employed in securing the ice for some hours every morning that ice is procured, and this forms one of the most animated scenes to be witnessed in Bengal. In a favourable night upwards of 10 cwt. of ice will be obtained from one bed, and from twenty beds upwards of 10 tons.

When the wind attains a southerly or easterly direction, no ice is formed, from its not being sufficiently dry; not even though the temperature of the air be lower than when it is made with the wind more from a northern or western point. The N.N.W. is the most favourable direction of wind for making ice, and this diminishes in power as it approaches the due north, or west. In the latter case more latitude is allowed than from the N.N.W. to the north. So great is the influence of the direction of wind on the ice, that when it changes in the course of a night from the N.N.W. to a less favourable direction, the change not only prevents the formation of more ice, but dissolves what may have been formed. On such occasions a mist is seen hovering over the ice-beds, from the moisture over them, and the quantity condensed by the cold wind. A mist in like manner forms over deep tanks during favourable nights for making ice.

Another important circumstance in the production of ice is the amount of wind. When it approaches a breeze no ice is formed. This is explained by such rapid currents of air removing the cold air, before any accumulation of ice has taken place in the ice-beds. It is for these reasons that the thickest ice is expected when during the day a breeze has blown from the N.W., which thoroughly dries the ground.

The ice-dishes present a large moist external surface to the dry northerly evening air, which cools the water in them, so that, when at 61°, it will in a few minutes fall to 56°, or even lower. But the moisture which exudes through the dish is quickly frozen, when the evaporation from the external surface no longer continues radiative; a more powerful agent then produces the ice in the dishes.

The quantity of dry straw in the ice-beds forms a large mass of a bad conductor of heat, which penetrates but a short way into it during the day; and as soon as the sun descends below the horizon, this large and powerfully-radiating surface is brought into action, and affects the water in the thin porous vessels, themselves powerful radiators. The cold thus produced is further increased by the damp night air descending to the earth's surface, and by the removal of the heating cause, which deposits a portion of its moisture upon the now powerfully radiating, and therefore cold surface of the straw, the water, and the large moist surface of the dishes. When better radiators of heat were substituted, as glazed, white, or metallic dishes, the cold was greater, and the ice was thicker, and the dishes were heavier in the morning than the common dishes. Any accumulation of heat on their surface from the deposit of moisture is prevented by the cold dry north-west airs which slowly pass over the

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* Essay on Dew, 1814.

† Experimental Essay: Jour. As. Society, Calcutta, vol. ii, p. 80.