ELECTROPHYSIOLOGICA:

SHOWING HOW ELECTRICITY MAY DO MUCH OF WHAT IS COMMONLY BELIEVED TO BE THE SPECIAL WORK OF A VITAL PRINCIPLE

III.

2. *In continuation of the question—How in muscular action electricity may do much of what is commonly believed to be the work of a vital principle.*

CONNECTED with the history of electrotonus as exhibited in these experiments* are also other facts which must not be overlooked in this attempt to trace out the workings of electricity in muscular action—facts which show that the departure of contractility and the arrival of rigor mortis are considerably retarded by both forms of electrotonus. Left to itself, the gastrocnemius of the frog loses its contractility and passes into the state of rigor mortis in a time varying with the season and from other causes from 6 to 12 hours; but not so when left to the action of electrotonus. In this latter case, indeed, the contractility may remain for 18, 24, or 36 hours—for a longer time in anelectrotonus than in cathelectrotonus—and even then there may still be no signs of rigor mortis. Once, where anelectrotonus was kept up steadily all the time, and where contractility lingered for 36 hours, the muscles were still limber at the end of 48 hours. No doubt, before exact conclusions can be drawn in these matters more experiments are wanted, many more; but it is not necessary to wait for these in order to be certain that the departure of contractility, and the arrival of rigor mortis, are considerably retarded by the action of both forms of electrotonus. And it is simply to the bare fact that attention is now directed.

What then? Do these facts bear upon what has gone before, and, if so, how?

The facts are obvious. In anelectrotonus and cathelectrotonus alike there are—suspension of the tetanus caused by feeble faradaic currents, elongation of muscle, exalted contractility, together with considerable retardation in the time at which contractility passes off and rigor mortis comes on. In anelectrotonus and cathelectrotonus the parts, muscle and nerve alike, are charged with a charge larger in amount than that which is natural to them—a positive charge in anelectrotonus, a negative in cathelectrotonus. The facts, indeed, are strangely in keeping with the premises. Only let it be supposed that the artificial charge acts upon the dielectric sheaths of the fibres as the natural charge has been supposed to act, but in the contrary direction, that is from without to within instead of from within to without, the charge imparted to the outside inducing the opposite charge on the inside, and all the rest follows. The artificial charge is larger in amount than the mutual charge, and hence the increased elongation of the muscular fibres, the compression arising from the natural attraction of the two opposite elements of the charge keeping up a state of elongation proportionate to the amount of the charge. Hence, also, the suspension of the tetanus by electrotonus, for if the charge elongates the fibres it is easy to see that another of its actions may be that of suspending or antagonising muscular action. And hence again the increased contractility, for, according to the premises, contraction, happening under these circumstances, will be greater because the elasticity of the muscle has freer play at the discharge. In these matters the artificial charge plays the same part as the natural charge, only more energetically, nothing more. And not less so, as it would seem, in the action exercised upon the passing off of contractility and coming on of rigor mortis. Contractility passes off and rigor mortis comes on in the ordinary course of things, because the muscle loses its natural electricity. Contractility passes off and rigor mortis comes on more slowly in electrotonus because the artificial charge associated with this state can take the place and do the work of the natural charge. This is all. Indeed, so far, the whole electrical history of muscle would seem to point to the view which led to the experiment with the elastic band, and to show that living muscle is kept in a state of elongation by the presence of an electrical charge, and that contraction is nothing more than the action of the fibres, by virtue of their elasticity, when liberated by discharge from the charge which kept them elongated previously—ordinary muscular contraction differing from rigor mortis in this only, that the charge which prevents contraction is suddenly withdrawn, and immediately replaced, in the former case, and gradually withdrawn, and not replaced, in the latter case.

Upon this view, also, it is possible to get a glimpse of the reason why contraction is more antagonised by anelectrotonus than by cathelectrotonus; and why contractility is slower in passing off, and rigor mortis slower in coming on, under the former state than under the latter. In anelectrotonus the artificial charge of the parts, muscle and nerve alike, is positive, and, being so, the sheaths are positive externally, and (by induction) negative internally, the manner of charging, which, there is reason to believe, is natural to the muscle. In cathelectrotonus, on the other hand, the opposite state of things obtains. Here the artificial change is negative, not positive. Here, consequently, the charging of the sheaths is negative on the outside and positive on the inside—a state of things which is not natural to the fibres, or which is only met with exceptionally, when these fibres are upon the point of passing into the state of rigor mortis. In anelctrotonus, therefore, the natural charge may co-operate with the artificial charge in a way in which it cannot do in cathelectrotonus; and which, without further comment, it is easy to see may explain in some degree why contraction is more antagonised by anelectrotonus than by cathelectrotonus; and why contractility passes off and rigor mortis comes on more slowly under the former condition than under the latter.

As I have shown elsewhere,* the whole electrical history of muscle is in keeping with this view. The charges obtained from the common friction machine act in the same way as those associated with electrotonus. Everywhere, the question is not of polarisation and of changes in direction of a continuous current, but simply of charge and discharge. Everywhere it is charge preventing, and discharge permitting, action. In a word, the whole electrical history of muscle would seem to show that electricity may have much to do in what is commonly believed to be the work of contractility and tonicity, and that the way in which this work is done is that which is here pointed out.

Against this view, however, sundry objections may be urged. It may be said that the phenomena of muscular action in muscles with sheathed fibres cannot be explained after this fashion. It may be said that the proof of charge during rest and discharge during action is little more than a matter of imagination. It may be said that the force of the natural electricity of muscle is inadequate as force. But, in reality, these objections, when fairly looked into, prove to be of little value.

No doubt the fibres of involuntary muscles differ from those of voluntary muscles in having no proper sheaths. Instead of having those sheaths, indeed, they are made up of cells, mostly fusiform in shape, imbedded in a sort of homogeneous plasm or matrix; and these cells, there is reason to believe, are the contractile elements of the fibres. Still it is not easy to allow the force of any objection arising in this fact, for may it not be that the walls of these contractile cells, which, like the sheaths of the fibres of voluntary muscle, in the main consist of the material of elastic tissue, behave in the way the sheath is supposed to behave under the charge and discharge, that a charge developed on the

* See NATURE, Jan. 11, 1872.

* "Dynamics of Nerve and Muscle." Macmillan.

inside of these walls induces the opposite charge on the outside, that the walls elongate under the compression arising from the mutual attraction of these charges, and shorten when this charge is discharged, because their elasticity is then left free to come into play? Nay, may it not be that this action of the cell membrane is not excluded in those long voluntary muscles in which the fibres seem to be made up of several cells or fibres over-wrapping at their ends, rather than of a single sheathed fibre? And, certainly, this idea is not contradicted by facts remaining in the background; for, as will be seen in due time, these go to show that the walls of all cells and fibres are affected electrically in the same way as that in which the sheath of the fibre of voluntary muscle is supposed to be affected. So that, after all, the phenomena of rest and action in sheathless muscular fibres may supply no valid objection to the view which has been taken of these phenomena as presented in muscular fibres with proper sheaths.

And surely the evidence supplied by the new quadrant electrometer is a sufficient contradiction to the objection that the charge during muscular rest and the discharge during muscular action are mere matters of imagination, for this evidence shows unequivocally that there is a charge during this state of rest and a discharge during this state of action. It is not a question of inference merely, such as it might be if the evidence supplied by the galvanometer were alone available; for here, as has been pointed out, the current during rest, and the comparative disappearance of this current during action, may in reality point to charge and discharge when traced to their causes: it is a question of simple fact. Moreover, the anatomical and physiological analogies existing between the muscular apparatus and the electrical apparatus in the torpedo and the phenomena of secondary contraction, make it more than probable that muscular action is accompanied by a discharge analogous to that of the torpedo. Like the nerves of the muscle, the nerves of the electric organs originate in the same track of the spinal cord, and terminate in the same manner. Like the muscles, the electric organs are paralysed by dividing their nerves. Like the muscles, the electric organs, after being thus paralysed, may be made to act by pinching the nerve below the line of section. Like the muscles, the electric organs are thrown into a state of involuntary action by strychnia. Like the muscles, the electric organs cannot go on acting without intervals of rest. And lastly, the nerves of the electric organs, like the nerves of the muscles, when somewhat exhausted, respond in the same curiously alternating way to the action of the "inverse" and "direct" current, if only discharge be taken as the equivalent of contraction. In a word, these analogies may be said almost to necessitate the conclusion to which Matheucci was led in regarding them, namely this—that muscular action is accompanied by a discharge of electricity analogous to that of the torpedo. And certainly this conclusion is borne out rather than contradicted by the phenomenon of secondary contraction which is exhibited in a prepared frog's leg, when, after laying its nerve upon the muscle of another such limb, contraction is produced in the latter limb; for here the only sufficient explanation would seem to be that offered by Becquerel, namely this—that contraction happens in the first limb because its nerve is acted upon by an electrical discharge developed in and around the muscles of the second limb during action—a discharge which may not indirectly show that there was a charge to be discharged during the previous state of rest. In a word, the evidence, direct and indirect, must surely suffice to show that the idea of charge during rest and discharge during action is something more than a mere matter of imagination.

Nor can it be fairly urged that the force of the natural electricity of the muscle is too feeble to produce the results attributed to it. On the contrary, after what has been said respecting the analogies between muscular action and the action of the electrical organs of the torpedo, it is quite fair to suppose that the force of the discharge in muscular action, instead of being feeble, may be equivalent to that of the torpedo; and that the reason why it cannot be detected in the same way may be that it is short-circuited, and so mainly out of reach, within the body.

3. *How in nervous action electricity may do much of what is commonly believed to be the work of a vital principle.*

There is good reason to believe* that the electrical law of nerve-fibre differs in no wise from that of muscular fibre.

There are also similarities between the principal structural elements of the nervous system from which it would appear that what holds good of one part of this system electrically may hold good of the other parts also. Nay more, there is in these facts reason for believing that what holds good of nerve-tissue generally may hold good of muscle also, for the typal element of nerve and muscle is evidently one and the same.

Looking at the different parts of the nervous system—ganglionic cells, and the peripheral nerve-organs—and at muscle-cells and fibres, it is easy to trace the same structural plan.

Central ganglionic cells, as seen in the ganglia of the sympathetic system, and in other small ganglia of the kind, consist of a round, oval, or pyriform mass of soft translucent, granular substance, with which two or more nerve-fibres communicate, and of an enclosing capsule formed of a transparent membrane with attached or embedded nuclei. The central granular substance, with which the nerve-fibres communicate, and the investing capsule, are unmistakeable in the ganglionic cells of the minute ganglia, but not so in the brain and spinal cord. In the brain and spinal cord there is the same central substance, but the proper cell wall is doubtful. Moreover, the central substance, instead of being a round, oval, or pyriform mass, with which the nerve-fibres are connected at one point only, branches out into several processes, which seem to be continuous with the nerve-fibres. At the same time, these cells and fibres are surrounded and supported by connective tissue, called reticulum by Kölliker, and neurologia by Virchow—a tissue which, as Dr. Sharpey points out, "is not merely an open mesh-work, but consists of fine laminæ formed of a close investment of finest fibrils, disposed as membranous partitions and tubular compartments for supporting and enclosing the nervous bundles;" so that, in the brain and spinal cord, as in the smaller ganglia, there is good reason for believing that the structure of the ganglionic cell is virtually the same, namely, a central granular mass, with which nerve-fibres are connected, and a membrane, with nuclei, investing this mass.

The peripheral nerve organs, of which the principal forms are three in number—the end-bulbs, the touch-corpuscles, and the Pacinian bodies—agree in having (1) an inward part or core of soft, translucent, finely granular matter, in which one or more nerve-fibres end by bulbous, or knobbed extremities; and (2) an outer investing capsule of ordinary connective tissue, with nuclei. In the end-bulbs and touch corpuscles this capsule is simple; in the Pacinian body it is made up of many concentric layers, from forty to sixty in number, with nuclei, these layers, "encasing each other, like the coats of an onion, with a small quantity of pellucid fluid included between them," being strung together where the nerve passes through. The structural plan is still that of the ganglionic cell—a central mass of granular matter, with which nerve fibres are intimately connected, and an investing capsule, simple or complex, as the case may be; and this would seem to be the plan of all the peripheral parts of the nervous system without exception, for it is a question

* See NATURE, Jan. 4, 1872.

whether nerves do ever *terminate* in plexuses or meshes of any kind.

The fibre of voluntary muscle is said to consist of a large number of extremely fine filaments enclosed in a transparent, homogeneous, elastic (the composition agrees with that of elastic tissue), tubular sheath, called the sarcolemma or myolemma, in which are nuclei, called muscle-corpuscles. It might, however, be more correct to say that this fibre consists of a mass of soft granular matter (the granules being the *sarcous elements* of Bowman), agreeing in the main with the granular core of the ganglionic cells and peripheral nerve-organs, enclosed in the sheath which has been described; for the contents of the fibre, instead of splitting up longitudinally into filaments, may split up horizontally into discs—may split either way or any way, in fact, as they would do if they were made up, neither of fibrils nor discs, but of granules which may, as it happens, aggregate into fibrils or discs. The fibre of involuntary muscle, on the other hand, is made up of elongated fibre-cells, connected together by a homogeneous, transparent uniting medium, without any sarcolemma. Each of these fibre-cells has a core of finely granular matter, sometimes arranged so as to form imperfect fibrils, and of a distinct cell-membrane, with nuclei, the shape of the cell being fusiform, with ends sometimes pointed, sometimes truncated, sometimes simple, sometimes branched. The cell-membrane in reality takes the place of the sarcolemma, for each cell is nothing more or less than a rudimentary fibre. Indeed, in long voluntary muscles there are fibres which seem to partake somewhat of the character of voluntary and somewhat of the character of involuntary fibres—fibres which, instead of running continuously from one end of the muscle to the other, are made up of several elongated fusiform cells, overlapping each other at the ends, and which therefore may consist of cell-membrane and sarcolemma both. Nor is the connection of the nerves with the muscular fibres or cells peculiar. Beale and Kölliker think that the nerves belonging to voluntary muscle end in meshes of pale fibres outside the sarcolemma. Rouget, Kühne, and others are of opinion that this ending is in peculiar organs—motorial end-plates continuous with the axis-cylinder of the nerve, oval or irregular in shape, within the sarcolemma and between it and the proper muscular substance, the primitive nerve-sheath fusing with the sarcolemma, and one end-plate being devoted to each muscular fibre. And thus it may be that the muscular fibre or cell may agree in structure with the ganglionic cell, and the peripheral nerve organ, in having a soft granular core, with which one or more nerve-fibres are connected, and an investing membrane of connective tissue with one or more nuclei. It may be, indeed, that the muscular fibre and cell are only varieties of the peripheral nerve-organ.

The nerve-fibres by which these several bodies—ganglionic cells, peripheral nerve organs of various kinds, and muscular fibres and cells—are connected together, are of two kinds, the tubular, which are white with dark borders, and those which are grey, pale, non-medullated or gelatinous. The white or tubular fibres, when quite fresh, appear perfectly homogeneous like threads of glass, but afterwards, when coagulation has taken place, they are found to consist of an axis, or primitive band, as it is called, a white medullary coating strongly refractive of light, and giving them the appearance of having dark borders, and an outer membranous sheath or tube, with nuclei in it, agreeing in composition with elastic tissue, and being analogous to the sarcolemma. The grey, pale, gelatinous fibres would seem to consist of the axis or primitive band of the others, with obscure sheaths in which are nuclei, but without medullary coating. They belong chiefly to the ganglionic system, but not exclusively; at all events the finer subdivisions of the white dark-bordered nerves of the other systems are found to have lost their dark borders, and to have become undistinguishable from those which have no dark borders naturally. In nerve-fibres, therefore, as in nerve-cells, there would seem to be a central core, and a membranous investment containing nuclei; and, all things considered, the connection of these fibres with ganglionic cells, with peripheral nerve-organs, and with muscular fibres and cells, would appear to be by one and the same method, the axis or primitive band being continuous with the central soft granular core of the central and peripheral elements of the nervous system, and of the muscular fibres and cells (for with so many points of analogy it is difficult not to believe with Rouget, Kühne, and others who agree with them in this matter), the primitive sheath, when there is one, being continuous with the membranous investment of this core, neurilemma, sarcolemma, or other, as the case may be.

Instead of being peculiar, therefore, the voluntary muscular fibre may be no more than a modified form not only of the contractile cell of the involuntary muscular fibre, but also of the nerve-fibre, and of the central and peripheral cell-elements of the nervous system. The same type of structures is to be traced out in each case. There is in each case the same central, granular, soft, substance, but slightly changed protoplasm probably, in the molecular change of which an electrical change may originate. There is in each case outside this central substance a membrane which may become charged leyden-jar-wise as the neurilemma and sarcolemma are supposed to be charged. And, therefore, it is not altogether begging the question to conclude that in each case one and the same electrical law may bear rule.

And certainly the adoption of this idea is calculated to elucidate much that is obscure in the structure and action of the nervous and muscular systems.

Upon this view a use is found for the contents and walls of the fibres and cells of which the nervous and muscular systems are made up. The contents are wanted for the generation of the charge; the walls are wanted for receiving and holding this charge. Their leyden-jar office, indeed, explains why it is that the nervous and muscular systems should be made up of cells and fibres.

Upon this view one use is found for the nucleus in the walls or sheath of cell or fibre. The nucleus may represent the spot at which the development of this wall or sheath is arrested—the spot at which the original, moist, *conducting* protoplasmic matter is not transformed by drying, or in some other way, into *non-conducting* wall or sheath, and, therefore, as I think, the nucleus may have a very definite function to fulfil. As I think, indeed, the case may be this: that the molecular changes in which the charge of the cell or fibre originates (those in the contents of the cell or fibre) depend upon the continual ingress of fresh and egress of used-up aërated matter; that this ingress and egress is, not through the wall or sheath anywhere or everywhere, but only through the nucleus; that the one charge not wanted for charging the inner surface of the wall or sheath may escape to earth through the nucleus; and that the channel of the discharge which happens when the cell or fibre passes from the state of rest into that of action may also be through the nucleus. Without such opening as may be supposed to exist in the nucleus, indeed, it is difficult to understand how the cell or fibre should be charged and discharged; and thus, upon the view in question, a use is found (not the only use, of course), for the nuclei present in the walls of the cells and in the sheaths of the fibres of the nervous and muscular systems.

Upon this view, too, the infinite number of these cells and fibres may in some degree be accounted for. For may it not be that each cell and fibre acts as a condenser to every other cell or fibre, so that a charge or discharge which is feeble without being multiplied becomes anything but feeble when multiplied? And may not this function of a condenser be the one function of the Pacinian bodies?

Other cells and fibres have other functions as well; these bodies may have this one function only. They may, in fact, be rudiments of the electric organs of the torpedo, with a sphere of action, not without the body, but within it. And this may be the reason why these bodies are placed on the trunks of nerves at points where it may be supposed that special means are wanted for keeping up the requisite degree of elastic tension, their use in this case being analogous to that of an ordinary leyden condenser in connection with a telegraph wire conveying a minimum amount of electricity.

Nor does this view fail to elucidate in some degree the way in which nerves tell upon muscle and react upon each other. Let the contents of the muscular fibre or cell be connected with the contents of the corresponding ganglionic cell by the axis cylinder of the nerve, and a charge or discharge in the nerve centre must tell upon the muscular tissue, just as in the case of two leyden jars with their inner coatings connected by a conductor, the charge or discharge of the one involves corresponding changes in the other. Let the case be that of a sensory peripheral cell and a central ganglionic cell, similarly connected, and a charge or discharge in the former will involve a charge or discharge in the latter, the discharge producing sensation. The case is simply that of a leyden battery, with all possible space economised by making the conductors, where they may, do the work of the jars. The case is plain as regards the charge, for the molecular charges are ever at work by which it is kept up and renewed; and the case is not altogether obscure even as regards the discharge, for it may well be that discharge happens when the charge increases until it overleaps the barrier of insulation presented in the dielectric walls of the fibres and cells—a result which, for want possibly of a sufficiently insulating barrier somewhere, happens more easily than it ought to do in the case of involuntary nervous action, such as is seen in convulsion, neuralgia, and the rest.

Viewed in this way, too, it is easy to see that the nervous system may do its work, not by discharge only, but by charge also. It is easy to see that the discharge may be all that is wanted to cause contraction; indeed, according to the premises, all that is wanted for this purpose is that the charge which kept the muscular fibre elongated should be discharged, and the fibre so left to the play of its own natural elasticity. It is easy to see, also, that discharge may be the mechanical agent which may call the various nerve-centres into action—by shaking the veil which separates the visible from the invisible in the higher mental processes, perhaps. And for charge no less than discharge it is also easy to see that there may be a definite work to do—a work of which the end is, not to cause action in the muscles and in the various nerve-centres, but to prevent it. Indeed, after what has been said, it is to be supposed that all nerves, through their electricity, have during rest an action which Pflüger supposes to be peculiar to certain nerves only, and to which he gives the name of *inhibitory*.

And here opens out a question of paramount interest.

It has been seen that the electric law of nerve and muscle is one and the same. It has been seen that the state of contraction in muscle is antagonised by the presence of a charge of electricity in muscle—that a state of actual elongation is produced by the action of this charge. It has been seen, not only that the state of contraction is antagonised and a state of elongation set up by the presence of the natural charge of electricity in muscle, but that more marked changes of the same kind are produced by the action of an artificial charge of electricity, provided this charge be greater in amount than the natural charge. The facts, indeed, are calculated to justify the notion that the degree of elongation produced by the conjoint action of the charge belonging to the muscle itself and the charge imparted to the muscle from its nervous system is greater than that produced by the action of the former charge singly; or, in other words, that the charge imparted to the muscle by its nervous system may cause a degree of elongation in the muscle which is over and above that caused by the charge belonging to the muscle itself—which surplus may have much to do in explaining rhythmical action in hollow muscles.

Take the case of a hollow muscle—a capillary vessel, for example. This vessel has its special nervous system, vasomotor nerves, efferent and afferent, vasomotor centre; and the question is as to how this system acts upon the vessel. May it be that a charge of electricity is continually being developed upon the cell-walls and fibre-sheaths of this system by the action of the oxygen of the blood and other causes upon the contents of the cells or fibres; and that this development goes on until, the bounds of insulation being overpassed, discharge happens? May it be that the muscular fibres forming the walls of the vessel elongate, and in so doing cause the vessel to dilate as long as this charge is imparted to them? May it be that the vessel passes from the state of dilatation into that of contraction when the discharge of this charge happens, in consequence of the muscular fibres being then liberated from the condition of extra-elongation caused by the charge imparted to them from the nerves, and so left to the play of their natural elasticity? May it be that thus there are diastolic and systolic changes in the vessel by which the blood is alternately drawn into and driven out of the vessel, changes which may supply the key to the mystery of "capillary force"? Nay, more; may it not be that the diastolic and systolic movements of the heart itself may have to be explained in the same way? To all these questions I answer, unhesitatingly, yes, it may be so. Indeed, after what has been said, the only explanation which seems to be called for concerns the movements of the auricles of the heart, and this is easily given: for, as it seems to me, the auricles must be looked upon chiefly as cisterns formed of dilated veins, and their movements chiefly as passive consequences of the movements of the ventricles, the systole of the auricles being little more than the passive falling-in of the auricular walls upon the blood being suddenly sucked away by the ventricular diastole, the diastole of the auricles being little more than the passive bulging-out of the auricular walls, caused at one and the same time by the stream of blood which is ever flowing in from the valveless openings of the great veins, and by a forcing back of this stream, consequent upon the sudden closure and recoil of the auriculo-ventricular valves at the moment of the ventricular systole. In this way the seemingly diastolic and systolic movements of the auricles must alternate with the true diastole and systole of the ventricles, and, at the same time, the absence of valves at the opening of the great veins into the auricles is accounted for—an absence altogether inexplicable if the auricular systole had to play the *active* part in the circulation which is played by the ventricular systole. And much to the same effect may be said of rhythmical movements in other hollow muscles, the chief difference between one such movement and another being perhaps this—that contraction follows upon dilatation more slowly in consequence of the cell-walls and fibre-sheaths of the special nervous systems being constructed differently as regards the capacity for quick charging and discharging; but these hints must suffice for what might be said upon this subject.

Nor can it be urged as an objection to this view of nervous action—the only objection which may be urged, so far as I know—that the state of action in nerve-fibre is unattended by the contraction which attends upon action in muscular fibre. The electrical law of nerve and muscle being one and the same, it might be expected, perhaps, that this particular difference should not exist; but this difficulty, if it be one, is soon disposed of. Thus, the success of the experiment with the elastic band depends upon the band being of a certain thickness, and upon the weights being so adjusted as to balance without overbalancing its elasticity. Failing these conditions charge and discharge may not tell in causing elongation and contraction. And, therefore, the absence of perceptible elongation and contraction in the nerve-fibre under the charge and discharge may be simply owing to the fact that the thickness and stretching of the neurilemma have not been adjusted for the production of these results. Besides, it is by no means certain that there are not in some nerve-fibres slight changes which are strictly parallel to the elongation and contraction witnessed in muscular fibres.

In a word, there seems to be good reason for believing that in nerve as in muscle electricity may have to do much of what is commonly regarded as the special work of an inherent vital principle.

4. *How in maintaining the "tone of the system" electricity may have to do much of what is commonly regarded as the special work of a vital principle.*

After what has been said little remains to be added under this head. The conclusion arrived at is that each perfect fibre and cell of living muscle and nerve (and, by implication, every living fibre and cell), is a charged leyden-jar while at rest. It is that the membranous portion of the fibre or cell is at this time compressed by the mutual attraction of the two opposite charges disposed leyden-jar-wise upon its two surfaces. It is that the effect of this compression is to elongate the fibre or cell by squeezing out this membrane lengthwise. What then? May it be that this compression, this squeezing out, is sufficient to account for what is called the "tone of the system"? This state, no doubt, is indefinite enough, but it becomes more definite when viewed in this way—so definite, in fact, that here also, in the maintenance of the "tone of the system" that is to say, electricity may have to do much of what is commonly believed to be the work of a vital principle.

5. *How in certain processes of growth electricity may do much of what is commonly regarded as the special work of a vital principle.*

A cell or fibre is at first a mass of protoplasm without any investing membrane. Later, this membrane makes its appearance, and how is this? Is it that the surface of the protoplasmic mass, except at the part or parts where the nucleus is afterwards met with, hardens by desiccating, or dying, or changing in some other way, and, so hardening, acquires dielectric properties? Is it that the molecular changes ever going on in the protoplasmic matter beneath this crust, develope a charge on the inside of this crust, which, acting inductively, leads to the development of the opposite charge on the outside? Is it that the compression arising from the mutual attraction of these opposite charges, causes the crust to stretch out every way, and so separate from the underlying protoplasmic mass, leaving thereby in some instances a vacuole, which may be filled with a thin liquid or even air? Is this the way in which the sarcolemma and neurilemma, the cell-walls, and all membranes more or less analogous to them, may be formed? After what has been said such an idea is by no means improbable. Nay, such an idea may be looked upon as the natural consequence of the premises. And if so, then electricity may have to do much of what is commonly believed to be the work of a vital principle in these phenomena of growth, as well as in the various processes which have been already passed in review, and upon which so much has been said as to leave only room now for these bare hints of what might be said upon the subject.

C. B. RADCLIFFE

MERCURY PHOTOGRAPHS

AN entirely novel method of photographic printing has just been discovered by M. Merget of Lyons. Although akin in some respects to the daguerreotype process, it differs essentially therefrom in the fact that exposure to light is not necessary to the formation of every separate image. It is difficult indeed just now to apply any distinguishing name to M. Merget's invention, for the methods hitherto discovered—and the number of these has, we all know, increased of late beyond all calculation—are all of them divisible into two very distinct classes. Thus we have those processes broadly termed chemical, in which every print is secured by the aid of light, as for instance, the nitrate of silver and carbon methods; and those again where a matrix, or printing block, having been prepared, the copies are struck off in the ordinary lithographic or printing press; photographs prepared in this latter manner are usually termed photo-mechanieal prints. M. Merget's invention partakes singularly enough of the nature of both classes; for while the prints are undoubtedly formed by chemical action, the question of light is of no moment at all, and the manipulations involved are to some extent of a mechanical nature.

The experiments of Faraday upon the diffusion of gases will be remembered by many, and it was the results arrived at by that distinguished philosopher that incited M. Merget, the Professor of Physics at the Faculté des Sciences of Lyons, to take up the investigation he has so successfully carried through. Faraday had already found out that the vapour of mercury acted very sensibly upon gold-leaf, and the first task undertaken by M. Merget was to discover whether this same action also took place upon other metals or their compounds. The investigation, it should be stated, was designed to be of a purely theoretical nature, and was not undertaken, in the first instance at any rate, with a view of working out any practical processes such as may eventually result from the research. The principal points discovered by M. Merget may be thus summarised:—

1. The vaporisation of mercury is a continuous phenomenon; that is to say, the metal emits vapour at all times, even at a very low temperature, and when in a solidified form.

2. Mercury vapour may be condensed upon certain substances, such as carbon, platinum, &c., without these latter being chemically affected.

3. Mercury vapour will pass with exceeding facility through porous bodies, such as wood, porcelain, &c.

4. The salts of all precious metals when in solution are very sensitive to the action of mercury vapour, which has the effect of rapidly reducing them.

The most sensitive to mercury of the precious metal salts are nitrate of silver and the soluble chlorides of gold, palladium, and iridium, and paper prepared with any of these forms at once a most delicate test for the volatile metal; but the solutions must contain some hygrometric body to prevent complete desiccation, so that the surface coated with them will always remain in a moist condition. To demonstrate how exceedingly sensitive this test-paper is to mercury, we may state that its contact with any body containing but a slight trace of amalgam suffices to darken the surface, while it is affirmed that any workman who has been employed for some time in a looking-glass or other similar factory, may produce an impression of his hand by simply laying the same upon a sensitive surface of this kind, the minute traces of mercury in the pores of the skin being amply sufficient to bring about a reduction of the salt, and to produce consequently an imprint of the fingers. In the same way a section of wood exposed to mercury vapours, and afterwards pressed in contact with a sheet of sensitive paper, prints off upon the surface all the rings and markings it possesses, the mercury being deposited in the pores of the wood in a more or less condensed form.