DYNAMICS OF NERVE AND MUSCLE.

By CHARLES B. RADCLIFFE, M.D., F.R.C.P.,

Physician to the Westminster Hospital, and to the National Hospital for the Paralysed and Epileptic.

In the article in last week's JOURNAL, in which my recent book entitled *Dynamics of Nerve and Muscle* is taken as the text, certain conclusions are drawn which ought not, as it seems to me, to be allowed to pass unchallenged.

The gist of the article is to object to a part of a theory of animal electricity which in substance is simply this—that the walls of the fibres and cells of living muscle and nerve-tissue during rest are charged in the way in which Leyden jars are charged, and that this charge is discharged during action, the discharge being analogous to that of the torpedo. It is argued that these walls are sufficiently bad conductors to justify the conclusion that they may act as dielectrics; it is argued that a charge of one kind of electricity may originate in the molecular reactions connected with nutrition and respiration, and that the development of the charge upon one surface of these dielectric walls may induce a charge of the other kind of electricity upon the other surface; and the opposite electrical condition of the sides and ends of the fibres are accounted for by supposing that the sides correspond to the outer charged surface, and the ends to the inner charged surface of the walls—the charge of the latter surface being conducted to the ends by the contents of the fibres. This in substance is the view to which exception is taken, and for which I am still disposed to do battle.

What may be regarded as the starting-point in my theorising is conceded, namely, this, that certain animal tissues—the sarcolemma and neurilemma in particular—are bad enough conductors to be capable of acting as dielectrics; and also this, that the electrical phenomena of living nerve and voluntary muscle during rest may be accounted for on the supposition that the neurilemma and sarcolemma are then in the predicament of charged Leyden jars. But it is objected that this view cannot apply to involuntary muscles of which the fibres are without sarcolemma; and it is further urged that the explanation given of the method of charging the fibres of nerve and muscle may be called in question, and that the discharge is taken for granted without sufficient proof of its existence. Objections are indeed raised which cannot be disregarded, and to which I therefore address myself *seriatim*, merely premising that I have lately made many new measurements of the resistance of animal tissues to electrical conduction, which show that this resistance, instead of being overrated, has been greatly underrated; that all soft animal tissues, when dry, really become insulators; and that perfectly fresh yellow elastic tissue, which enters largely into the composition of both sarcolemma and neurilemma, is a very bad conductor—the worst of all the animal tissues, apparently.

The objection that the Leyden jar hypothesis cannot apply to the fibres of involuntary muscle appears, at first sight, to have considerable weight in it. These fibres consist of a number of contractile cells, imbedded in an amorphous transparent substance. They have no proper sheath or sarcolemma like the fibres of voluntary muscle. But what of this? The cells, there is every reason to believe, are the contractile part of the fibre. Their walls, in the opinion of many, take the place of the sarcolemma, being in fact analogous to the sarcolemma; and certainly there is no reason why the contents of these cells should not be regarded as analogous to the contents of the sarcolemma. In point of fact, there is no good reason why these walls, if there be any walls, together with the amorphous transparent substance of the fibre outside them, should not be a crude, thick, less elaborated sarcolemma. Thick or thin walls will equally serve my purpose, provided they have the requisite non-conductibility to enable them to act as dielectrics. Nor need this view be looked upon as at all far-fetched; for it must be borne in mind that in many long voluntary muscles the fibres do not run through the muscle from end to end, but are made up of several fusiform fibres, overlapping more or less at their ends, and so really copying, in a sense, the arrangement of separate contractile cells which is met with in certain involuntary muscular fibres. Hence, after all, there need be no structural difficulty in supposing that the living fibres of involuntary muscles, like the living fibres of voluntary muscle, may, during rest, be charged like Leyden jars, as the theory supposes.

The next objection, which concerns the explanation I have offered of the way in which the fibres are charged, is only hinted at, but it must not be passed by without notice. I have said that this charge may have its origin in certain molecular reactions *outside* the walls of the fibres and cells: in the article under consideration it is hinted that this charging is as likely—nay, more likely—to happen from the *inside*. And so say I now. Indeed, for some time past I have held, as several per-

sons can testify, that the more probable view of the charging is this: 1. That the granular or fibrilliform substance within the fibres and cells, which substance represents more or less unchanged protoplasm, receives food and air, feeds and breathes, from without, not through every part of the walls, but only at the points occupied by the nuclei, which nuclei, indeed, may represent so much more or less unchanged protoplasm; 2. That an electrical charge is developed upon the *inner* surface of the walls by the molecular changes in the contents of the fibres and cells, which changes are brought about by nutrition and respiration—the opposite charge, which is not wanted, escaping to earth by the same opening, or openings, through which the fluids having to do with respiration and nutrition found entrance; and, 3. That in this way the walls of the fibres or cells are charged from within to without, the charge on the inner surface inducing through the dielectric walls the opposite charge on the outer surface of the walls. Either view is possible; but this view is, as it seems to me, not only simpler, but it also disposes of a difficulty which has been started elsewhere; namely, this, that I do not provide the conducting surfaces of the dielectric walls which are necessary to the charging on the Leyden-jar hypothesis; for it is manifest that the *inner* surface of the walls, whether this be a conductor or an insulator, will be charged by being directly in contact with the substance in the molecular reaction of which the charge originates; and that in this way the outer surface will be charged also, for, if one surface be charged, induction will secure the charging of the other surface, be that other surface a conductor or an insulator.

Nor do I consent to the statement that I have failed to supply sufficient proof of the discharge during action, for which I contend. In proof of this discharge I argued, along with Matteucci, from certain striking anatomical and physiological analogies between the muscular and electrical apparatus of the torpedo, and from the phenomenon of secondary or induced muscular action which Matteucci discovered; and I supported this argument by appealing to the parallel phenomenon of secondary or induced *nervous* action discovered by Du Bois-Reymond; and this argument, as it still appears to me, is perfectly valid.

In proof of this discharge, I instanced also the more or less complete disappearance of the muscle-current in muscular action, and of the nerve-current in nervous action—the great facts discovered by Du Bois-Reymond; and I argued that this disappearance *may* be a proof of the discharge of which Matteucci had inferred the existence from analogy; and these facts, as I take it, still justify this conclusion.

In proof of this discharge, I also showed—a fact which I have myself made out for the first time by means of Sir William Thomson's new quadrant electrometer—that in both nerve and muscle there is a disappearance more or less complete during action of the electrical charge demonstrably present during rest: and I argued, as I still do, that this is the very *experimentum crucis* of discharge, the instrument taking cognisance only, not of changes of current, but of the simple facts of charge and discharge.

In physiological matters, proof no doubt must always be more or less a matter of probability; but surely there must be here a fair amount of proof, not only that action in nerve and muscle may be accompanied by discharge of electricity, but also that this discharge is analogous to that of the torpedo!

What, then? Is this view of which I have been speaking, or that of Du Bois-Reymond, the more probable?

The primary electrical condition of nerve and muscle, according to Du Bois-Reymond, is not static, but current. The nerve and muscle are made up of what this physiologist has called *peripolar* molecules—molecules negative at the two poles and positive at the equator, or *vice versa*; and the nerve-current and muscle-current are derived portions of infinitely stronger currents ever circulating in closed circuits around the peripolar molecules. How these primary currents are kept up—how, that is, the two opposite electricities are separated and kept apart in the peripolar molecules—is not explained; how the weakening of the nerve-current and muscle-current, which happens in the action of nerve and muscle, is brought about, is not accounted for. The theory is incomplete in these respects; but this, perhaps, is of less moment, for no physiological work is supposed to be done by the electricity. On the other hand, there is nothing in the view of which I have been speaking which is not readily grasped by the imagination, and which does not as readily tally with the facts. In this view, the non-conducting properties of certain parts of the tissues, which are difficulties in the way of accepting the current-view, find their *raison d'être*. On this view, the disappearance of electricity during action in nerve and muscle, which is not accounted for on the current view, has its explanation, for it is only the sign of the discharge of the charge present during rest. On this view, the existence as well as the separation of the two opposite electricities in the fibres, which is not accounted for on the current view, is also explained, for the clue is simply the theory of the Leyden jar. And,

moreover, as I have endeavoured to show in the book supplying the text to the article upon which I am commenting, important consequences flow out of this theory, which consequences are themselves strong arguments in favour of the probability of this theory—consequences which show that electricity, instead of being a mere accident, may be an all-important agent in vital phenomena, an agent in many cases doing work which has been wrongly attributed to life.

The subject, no doubt, is difficult enough, but its difficulties, in the main, disappear when once the threshold of the inquiry is crossed; and my object at present is to ask my readers not to be deterred from entering by any terrors connected with the “dweller on the threshold”, but to enter boldly, and then to do me the justice to weigh my argument as a whole, and not in part, and to reserve their judgment until they can find time to read what I have myself written on the subject.

EXPERIMENTS WITH CARBOLIC ACID.

By ANGUS MACKINTOSH, M.D., Callington.

IN the interest of science, as bearing specially on the effects of carbolic acid on organic substances, I desire to record a few experiments, with their results, which I carefully performed with carbolic acid in November last, with the view of proving further to my own satisfaction certain chemical and physiological changes that I have observed in treating ulcers and severe surgical injuries with the acid. The results at which I arrived were published last year; and, in consequence, Messrs. F. C. Calvert and Co., the well known chemists of Manchester, kindly forwarded to me several samples of the different preparations made by them, to be tried in small-pox, etc. I have now tested them in many cases of small-pox with almost unparalleled success, and also in various surgical operations; and, in justice to Messrs. Calvert and Co., must say that I consider their preparations immensely superior to any other of the acid yet given to the profession.

Having had reasons for years to question the accuracy and correctness of the germ-theory of disease, and of the *modus operandi* of the antiseptic system of treatment, so ably brought before the profession by my respected teacher, Professor Lister, I last year called the attention of the profession to the fact that the beneficial effects of carbolic acid on wounds, etc., were not to be attributed solely to the destruction of atmospheric germs, but mainly, if not completely, to the chemical and stimulating effects of it on the constituents of the blood and surrounding parts. The following experiments confirm the opinion then expressed.

CLASS I. Experiment 1.—On November 8th, 1870, I mixed a quarter of a pound of bullock's blood (fresh) with two ounces of F. C. Calvert and Co.'s carbolic acid No. 5 (proportion, 10 of water to 1 acid), and covered the whole so as to admit no air. The temperature was 50 deg. *Result*, July 10th, 1871: No signs of putrefaction.

Experiment 11.—On November 8th, 1870, I mixed a quarter of a pound of bullock's blood (fresh) with two ounces of Calvert's carbolic acid No. 5 (proportion, 10 of water to 1 acid), and left the whole uncovered and exposed to the air. The vessels used in this and the previous experiment were placed side by side in the same apartment. *Result*, July 10th, 1871: Not the slightest signs of or tendency to putrefaction.

Experiment 111.—On November 9th, 1870, I weighed two ounces of lean fresh beef, and placed it in a pot with two ounces of carbolic acid (Calvert's No. 5, 10 water to 1 acid), and covered the contents completely from the atmosphere. *Result*, July 12th, 1871: The meat was perfectly free from putrefaction.

Experiment IV.—On November 9th, 1870, I weighed two ounces of lean fresh beef, and placed it in a pot with two ounces of carbolic acid (Calvert's No. 5, 10 water to 1 acid), and left the contents exposed to the air in the same apartment with Nos. 1, 2, and 3. *Result*, July 10th, 1871: There was no offensive odour nor putrefaction.

CLASS II. Experiment v.—In January 1871, I excised from a man's left arm a non-malignant tumour weighing a pound and a half, and dressed the wound with thin muslin and Calvert's carbolic acid No. 5 (proportion, 30 of water to 1 of acid). The acid solution was dropped freely over the muslin many times during the day. The wound healed without suppuration or putrefaction.

Experiment VI.—I amputated an old man's middle finger in April 1871, and dressed it in the same way as No. 5, with thin muslin, dropping the acid solution freely over the muslin several times throughout the day. The result was a perfect cicatrix, without any appearance of pus, suppuration, or putrefaction.

Experiment VII.—A miner had both arms and face burnt in consequence of an explosion whilst blasting. The face was treated with linseed-oil and linseed-meal poultices, and afterwards with oxide of zinc

ointment. A small quantity of pus could be detected. The arms were treated with carbolic acid and linseed-oil, 1 to 20. The muslin (the only covering) was always kept wet with the oil and acid. The result was a complete cure, without any pus or putrefaction.

These experiments, in combination with those which I published last year, have completely convinced me that the detailed plan of dressing wounds recommended by Professor Lister is absolutely unnecessary for the object of healing wounds by the first intention or without the formation of pus; and that the germ-theory of disease is, correctly speaking, an error.

In the first class of these experiments, I treated the blood and beef with exactly the same quantity of the acid solution, in the same proportion; and sealed one so as to exclude the contents completely from the atmosphere, and left the other exposed to the atmosphere. Under these opposite conditions, both, in the course of eight months, presented almost the same appearance, and no appreciable distinction could be recognised between their odours. There were no marks or signs of putrefaction in either.

The second class of experiments also gave the same result, and shows that only careful attention and regular application are required to produce always similar effects. I used the muslin cloth, so as to allow a free current of air to all parts of the wounds. I firmly believe that the real difference in healing with or without suppuration is chiefly, if not altogether, owing to regularity in the application; and that the beneficial effects of the acid are to be principally attributed to its chemical and stimulating influence on the constituents of the blood and other parts concerned.

NOTES ON THE TREATMENT OF INSANE PATIENTS IN PRIVATE PRACTICE.*

By D. YELLOWLEES, M.D., Medical Superintendent of the Glamorgan County Asylum.

IF we could obtain prophetic knowledge as to the effects of remedies, or could understand a case as fully at its commencement as after we had watched its progress, this unattainable knowledge would often modify our treatment. Possessing unusual opportunities for observing insanity in all its phases, and for watching the progress of cases of which others have seen only the commencement, it has occurred to me that this special experience, in so far as it relates to home-treatment, might be usefully summarised. I hope no one will construe my remarks as reflecting on the treatment of others, or will suppose that I wish to assume the office of an instructor: nothing can be further from my intention. I know well the extreme difficulty—often the impossibility—of properly carrying out the treatment of the insane at their own homes; and it is with a view to lessen, if possible, this difficulty, that I submit the following notes.

Insane patients treated at home are apt to get—

1. *Too much narcotic medicine and too little general treatment.* This is often the case, and very naturally. The mental excitement is the most prominent symptom, the one which most alarms the friends, and for the abatement of which they are supremely anxious. Even to the practitioner, the brain-disturbance is apt to eclipse the other symptoms, and to receive undue or almost exclusive attention; whereas treatment may be urgently required for the amenorrhœa, the dyspepsia, the constipation, or other coexisting disorder, of which the brain-disturbance is probably in great measure a result. Relieving a sluggish liver, or restoring the functions of a neglected skin, often allays excitement better than any quantity of sedatives. When sedatives are necessary—and they are often most necessary and beneficial—chloral hydrate, bromide of potassium, or bromide with cannabis Indica, should generally be first used; they often answer admirably, and the injurious general effects of more direct narcotics are avoided. I am satisfied that the recovery of curable cases has often been retarded, and the condition of hopeless cases aggravated, by the undue use of opium. On the other hand, it is often of great value.

2. *Too much depression and too little support.* This is a frequent phase of home-treatment. Antimony, alone or with morphia, is often a favourite remedy; and the friends are always anxious to aid in keeping the patient low. This may, perhaps, be wise treatment in some cases; but it is very certain that brain-excitement is usually a result of *asthenia*, and requires support far oftener than depression. I know no form of excitement that is aggravated by an ample supply of the most nourishing and wholesome food. In every case it is an essential element of proper treatment, and in many cases a liberal addition of stimulants is the best sedative we can use.

* Read at the annual meeting of the South Wales and Monmouthshire Branch, July 5th.