done as to render any further use of this instrument unnecessary, and that after some years the urethra can be proved to be structurally and functionally normal. In the majority of these cases the perineum has been opened for stricture complicated with abscess and extravasation of urine. It must, however, be borne in mind that a perineal section is seldom resorted to until the urethra is largely impregnated, so to speak, with old cicatricial tissue of a contractile nature, which, notwithstanding the longitudinal splice of sound tissue introduced by the operation, must remain behind to contract and to mar to some extent the result obtained. The conditions which the operation must necessarily fulfil are complete division of the stricture and thorough urine drainage.

In wounds of the urethra, made either accidentally or in the course of surgical operations, which by the nature of circumstances have to be treated without due regard for urine drainage, means should be more systematically taken to prevent the development of urine fever, as well as the formation of a dense contractile cicatrix. In promoting these objects much may be done by local irrigation and by internal administration of drugs eliminated by the urine.

Lastly, we must remember that the prevention of stricture is within our scope. If, as I have urged, stricture is the result of urine leakage following inflammation in one instance, whilst in another it is brought about by the constant contact of the excretion with an internal wound, then the importance in the one case of irrigation and in the other of free urine drainage is at once evident. It is in connexion with ruptures of the urethra that we see exemplified the most disastrous effects of permitting a wound to heal subject to the irritation that constant contact with pent-up urine is capable of exercising, and I have not the least doubt that those do best which are treated by perineal section and drainage without reference to the question of extravasation, the wound healing with a scar that shows but little tendency to subsequent contraction. And what applies to the healing of wounds inflicted accidentally upon the urethra internally, applies equally to others similarly inflicted on the canal for surgical purposes. The testimony that I have gathered from the examination of many hundred cases is certainly not favourable either to the permanency or the character of the relief that internal urethrotomy usually affords. Amongst the worst cases of stricture that I have thus met with have been those treated by an internal section. If there is any force in the observations I have brought before the Society this evening, I do not think there should be any difficulty in explaining how this happens, and of recognising the importance of applying to the treatment of wounds of all kinds involving the urethra those principles which are the basis of Listerism—namely, drainage and cleanliness. There is no part of the human body which by disregard of these conditions is more likely to be attended with disastrous consequences, whether we have regard to the present or to the future. In conclusion, gentlemen, most speakers commence their discourse with a text; I prefer concluding with one: Urine can spoil tissue as well as blood.

ON THE ELECTRICAL BASIS OF VITAL MOTION.

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IN muscles, and in the nerves belonging to them, while alive and at rest, are unmistakable signs of electrical charge and current which are not met with after death. These signs, which are exactly similar in nerve and muscle, differ in a very marked manner in the opposite states of rest and action. When the state of rest changes into that of action the charge is discharged, and the current undergoes a change which is spoken of as "*negative variation*," and which ought to be spoken of as *simple diminution of current*. Using a galvanometer with a very high degree of resistance in the coil, it is easy to detect the current of nerve and muscle, and to follow the "*negative variation*" which happens when the state of rest is made to change into that of action, but not so when the galvanometer has a coil with a very low degree of resistance in it. Indeed, in this latter case there is—

what there never is when the galvanometer with a high degree of resistance in the coil is used—*discharge like that of the torpedo* (Matteucci) and *contraction*, which contraction is also witnessed when the connexion between the ends and sides of the fibres is made by a short piece of wire, or any good conductor. And so it is that I am ready to believe that the whole case may be greatly simplified by looking upon the charge and discharge as the only phenomena which have any claim to be regarded as *primary*; the current and its "*negative variation*" being in reality no more than the accidental result of the discharge being passed through a galvanometer with a degree of resistance in the coil sufficient to bridle down the discharge into the quieter pace of the current, which in reality is only a slow and almost silent discharge.

Again and again, by using the new quadrant electrometer of Sir William Thomson or the electrometer of Professor Lipmann, the reality of the charge and discharge is made very apparent. Again and again, by using the highly resisting galvanometer of Professor Du Bois-Reymond or Sir William Thomson, I have been able to verify all that Professor Du Bois-Reymond has said about the current and the "*negative variation*" of the current. In a detached piece of nerve or muscle the current passes from the sides to either of the two ends of the fibres, and, as may be expected in such a case, the former surface is found to be charged positively, and the latter negatively. And this state of things admits of explanation, if only it be assumed that the sheaths of the fibres, which are more or less *dielectric*, because they are composed of tissue more or less akin to *ligamentum nuchæ*, which is highly dielectric, are charged in a double fashion, as Leyden jars are charged; that ordinarily the exterior of the sheaths is +, and the interior -; that in a detached piece of nerve or muscle the sides of the fibres are +, and the ends -, because the negatively electrified interiors of the sheaths make themselves felt at the opened-out ends; and that the *state of relaxation in muscle* may be produced by the repellant action of the charge upon the muscular molecules; that the *state of contraction in muscle* may be owing to the discharge, which happens then, having given over these molecules to the action of the attractive force which is inherent in their physical constitution.

I am not at a loss when I try to understand how the state of charge may be brought about. On the contrary, the charge, as it seems to me, may be looked upon as the natural result, not of any electrical action confined to the muscular system, but of an electrical action extending to the whole solar system, and not limited to it. At spring-tide and at neap-tide, when the action of the sun and moon are most differentiated, there are variations in terrestrial heat which show very plainly that the moon as well as the sun is directly concerned in producing these variations. There are variations in terrestrial magnetism at the same time which as clearly tell the same story. And there are cotemporaneous variations of the same sort in atmospheric potential, only as yet not so clearly made out as the variations in heat and magnetism. And the deficiency in the evidence here is supplemented by the presence of the belt of terrestrial magnetism, and by the requirements of the doctrine of the correlation of the physical forces: by the presence of the belt, because the very existence of magnetism is inconceivable apart from electricity; by the requirements of the doctrine, because these oblige me to regard heat and light, and electricity and magnetism, and the other forms of physical forces, as mere modes of one and the same force. Indeed, there is, as I have attempted to show elsewhere,¹ good reason to believe that the earth is continually acted upon by electricity which has its source in an electrical interaction which is always going on between the sun and moon on the one hand and the earth on the other, and that this electricity is manifested in the form of currents across the earth from the sun and moon, the case being really one in which it is easy to believe that the dielectric sheaths of the fibres in nerve and muscle may be charged, as Leyden jars are charged, by these currents, and that this charge will be instantly renewed after discharge. Nor can it be said that these currents are not powerful enough to do all this, for, as atmospheric potential is equal to several hundred volts at a short distance above the earth, and to still more than this as this distance increases, and as the directive power of terrestrial magnetism

¹ Behind the Tides. London: Macmillan and Co. 8vo. 1888.

is not altogether annihilated by the most powerful artificial magnetism, it is only fair to conclude that the solar and lunar currents are powerful enough to do all the work they have to do in charging the sheaths of the fibres in nerve and muscle, as Leyden jars are charged, and in instantly renewing this charge after discharge.

Nor have I to argue differently when I turn from the charge and discharge to the current and "negative variation" of this current, which enter so conspicuously into the electrical history of living muscle and motor nerve. Many most beautiful experiments by Professor Du Bois-Reymond show that the "negative variation" of the current is no more than a diminution of this current. The needle of the galvanometer, which had diverged to the check under the action of the current of the resting nerve or muscle, swings back and takes up a position at no great distance from zero, when the state of rest is made to change into that of action. The needle yields to the change which is spoken of as "negative variation," and which ought to be spoken of as simple diminution of current, brought about by the discharge of charge which happens then; and, in fact, there is nothing in the history of the current, as made out so clearly by Professor Du Bois-Reymond, which does not tally with the history of the charge to which I am directing attention.

Nor is any contradiction to this view to be found in cadaveric rigidity and in the absence of relaxation and contraction in nerve. In ordinary contraction, the fibres are given over temporarily to the attractive force which is inherent in the physical constitution of their molecules by the discharge which happens then; in rigor mortis the fibres are given over permanently by the final departure of this charge. Ordinary contraction is a step towards death; rigor mortis is the arrival at death. As I take it, so it is, and no more. And it may be wrong to speak of the apparent absence of relaxation and contraction in nerve as being real absence. It is very possible that the difference between muscle and nerve in this respect may be no greater than that which exists between hyaline amoebæ, which do shrink and expand, and granular amoebæ, which have nearly or altogether ceased (they were hyaline at first, and very mobile) to expand and shrink. And here, too, it may be well to remember how a living nerve shrinks when it is divided, for this fact may point, not only to this shrinking as being in some degree analogous to contraction, but also to a process in some degree analogous to relaxation as having existed before the shrinking.

In dealing with the workings of artificial electricity upon nerve and muscle, I might go on to show that these are resolvable into those of charge and discharge: the charge favouring the state of rest, unless it be that the positive and negative elements of the artificial charge are opposed to the positive and negative elements of the natural charge; the discharge causing the state of rest to change into that of action by dislodging the natural change. I might go on to show that in electrotonus, in the case of the "inverse and direct currents," and in other cases as well, it is not the current which is acting, but the charge associated with the current, which charge, owing to the animal tissues being very bad conductors, is very decidedly + on the side of the + pole, and as decidedly - on the side of the - pole. I might also go on to show that the discharges of statical electricity and the instantaneous currents of high tension attending upon the beginning and ending of the constant current, which currents are strictly analogous to discharges, bring about a change from a state of rest to that of action by discharging the charge which antagonises the state of action. I have not to do with a new story; I have only to do with a new version of the story to which I have been directing attention. Want of space preventing me, I cannot make good these statements here; indeed, under the circumstances, all I can do here is to refer to a work of mine which was published about five years ago,² and say that what is not given here will be found there.

A very different story from this would seem to be told by the wild workings of vital motion which are seen in different forms of convulsion. The common impression is that the convulsion is caused by "a rush of blood to the head," by a bloodshot state of some motorial nerve centre, by a state of "morbid irritability" in that centre which seems to imply exalted vitality, by a state of + and not by a state of -, but there is no real foundation for any such notion.

In epilepsy, as Delasneuve was the first to point out, the very first symptom is a death-like pallor of the countenance; and so it is in epileptiform convulsion of any sort.

In the case of death by bleeding, privation of blood and convulsion stand together in the light of cause and effect, and, so far as I can see, there is no escape from the conclusion that the blood must have told upon the muscles *before* the convulsion rather than *in* it, that the blood may have helped to keep off the convulsion rather than to bring it on. Nor can it be said that the state of the nervous system is opposed to that of the circulation, for as coma is the accompaniment of convulsion, it seems to follow that the nerve centres are then very ill-supplied with "nervous influences" as well as with electricity.

And as in the convulsion of bleeding, so in the convulsion of suffocation the facts do not admit of being read in a different way. The convulsion in this case has been referred to the stimulation of the black blood with which the arteries are then filled to the full; but, in reality, it is as easy to refer it to the absence of arterial blood. And there is this to be said in favour of this view, that it brings the convulsion of bleeding and the convulsion of suffocation into the same category.

And as in the last of these two cases of convulsion, so in epilepsy. In it convulsion, coma, and suffocation still go hand in hand. Nor does the full and hard pulse at the height of the paroxysm in epilepsy lead to a different conclusion. This is the pulse, not of *red* blood, but of *black* blood. The pulse is that of suffocation, and the state of the circulation in suffocation is—what? It is *not* one in which, as many believe, the veins go on filling at the expense of its arteries until the arteries are emptied and the veins ready to burst. It is one, on the contrary, in which the arteries go on filling at the expense of the veins, and in which the pulse becomes fuller and firmer as the colour of the blood, which is plainly visible through the coats of the vessels, changes from red to black in the arteries, partly because venous blood goes on circulating for some time, and partly because black blood does not find its way out of the arteries as readily as red blood. Evidence to this effect is supplied abundantly in the numerous experiments of John Reid³ and Professor Draper,⁴ and also in the simple experiment which is repeated whenever arteriotomy is performed when an epileptic fit is at its height; for in this case it is seen, not only that the blood is *black*, but also that it jets out far beyond the point to which red blood is ever projected. Indeed, the more the matter is looked into, the more it becomes evident that the unusually full and hard pulse at the height of the fit is nothing more than the pulse of black blood which naturally belongs to the state of suffocation.

Nor do the facts with which I have to do make me ready to believe that the connexion between convulsion and suffocation is to be explained by supposing that *carbonic acid* is the stimulus by which a sleeping vital property of irritability in nerve and muscle is made to waken up. It is certain that retained excreta, renal and biliary in particular, may lead to epileptiform convulsions; it is by no means improbable that carbonic acid may act in the same manner, but it does not follow that this manner is that to which I have just referred. On the contrary, it may be that this manner is not unlike that of strychnia, which, as Dr. George Harley points out,⁵ is very unfriendly to the arterialisation of the blood. And there is this to be said in favour of this view, that want of arterialisation is in accordance with the premisses, which, to say the least, is more than can be said in favour of the notion that carbonic acid provokes convulsion by acting as a stimulus to a vital property of irritability in certain motorial nerve centres.

All that has been said goes to show that coma, which is the accompaniment of the convulsion about which I have been speaking, has nothing to do with a state of over-action in the circulation. The state of the circulation is marked by ghastly pallor at the very commencement of the fit, and by unequivocal signs of suffocation afterwards. There is a great want of arterial blood during the time of coma. It is as it is with convulsion. And it may well be that the coma is dependent, not upon contraction of the capillaries consequent upon stimulation of the nerve centres having to do with the capillaries, but simply upon want of arterial blood, the cause of the coma and the convulsion being, in very deed, one and the same.

² Vital Motion as a mode of Physical Motion. Second edition. London: Macmillan and Co. 8vo. 1882.

³ Phys., Anat., and Path. Researches. 8vo. Edin., 1848.

⁴ Lectures on the Physiology of the Circulation. Amer. Med. Monthly, April, 1860.

⁵ THE LANCET, June and July, 1856.

And all that may be said goes to show that convulsion or spasm has nothing to do with inflammation or active determination of blood, or congestion or fever. Epilepsy is often suspended for a time by the patient having been severely burned during a fit. It seems as if there was something in the inflammation and fever set up by the burn which is uncongenial to the fit; nay, it must be so, for, as a rule, fits which happened frequently before the burn keep away until the process of healing is complete and the circulation has returned to its natural condition, which is always somewhat under the mark as to activity. Nor can spasm be looked upon as the characteristic symptom of acute spinal meningitis. It is, indeed, quite a mistake to look upon spasm as being thus characteristic, for when the history of this disorder is carefully looked into it is found (I am speaking now of the disorder when inflammation is actually developed) that the so-called spasm is little or no more than a voluntary or semi-voluntary fixing of the spinal column, of which the object is to prevent the very severe pain caused by moving the exquisitely tender spinal cord. Only let the patient keep quite still, and he is little likely to have much pain, or any symptom which may be looked upon as spasmodic. I can appeal to evidence in support of this statement, but not here. Indeed, here, for want of space, all I can do is to say that it is so, not only in the convulsion or spasm about which I have been speaking, but also in any analogous phenomenon, physiological or pathological, and that much is said in "Vital Motion as a mode of Physical Motion" to make good this statement. Indeed, instead of having to do with the vital property of irritability as the master-key to life as manifested in muscle and motor nerve, I have only to do with electricity. And I am, I think, not the loser in adopting this course. I am not the loser as regards the vital property of irritability, for this property is no more than an *occult* property akin to the *virtus dormitiva of opium*. Now, the vital property of irritability which is looked upon as the cause of life in nerve and muscle is of the same parentage as the *virtus dormitiva of opium*, about which Molière makes fun.⁶ It is equally an *occult* quality. It is equally an empty dream of the schoolmen of the middle ages, which has no foundation in fact, and about which the wonder is that it should have outlived the *virtus dormitiva* so very long. Indeed, do what I may, I cannot account for this anomalous state of things in these days when the reign of facts is supreme. And I am not the loser so far as the electrical hypothesis is concerned, for, by falling back upon charge and discharge, and supposing that the charge brings about relaxation by keeping the muscular molecules in a state of mutual repulsion, and that the discharge brings about contraction by allowing the attractive force which is adherent in the physical constitution of the molecules to come into play, I pass at once from the unknown and unknowable to the known, from mysterious metaphysics to plain physics, from the realm of fancy into that of fact. Indeed, so far from being the loser by adopting this view, I am, as I take it, in every way the gainer.

HYPERTROPHY AND ITS COUNTERFEITS.

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(Concluded from page 13.)

BEFORE dismissing overgrowth, we ought to consider a variety of the process which is difficult to define, but is better illustrated by an example. The roach represented in Fig. 5 presents a large tumour growing from its side. The scales covering the tumour are in many places more than three times larger than those on the healthy parts of the fish; in the sketch a normal and abnormal scale are drawn of natural sizes for comparison. Notwithstanding the great difference in the sizes of the scales, the amount of material in each is equal. As the tumour increased in size the matrix of the scales expanded and produced scales of extreme tenuity. The specimen is preserved in the museum of the Royal College of Surgeons. A somewhat similar condition may be studied occasionally in man, in connexion with the subungual exostosis. As the exostosis grows it expands the matrix of the nail, and the

surface of the neoplasm becomes covered with a corneous cap. This expansion of the nail matrix is not constant. The same applies to the enormously distended skull in hydrocephalus. In these crania the cubic capacity may exceed the normal some two or three times, yet the total amount of osseous material in these skulls is often less than in a healthy skull of corresponding age. These specimens illustrate clearly enough that enlargements of this character should not be confounded with hypertrophy.

FIG. 5.

A roach with a spindle-celled sarcoma growing from its side. The lower figure represents the natural size of the scale, whilst the upper one shows the large size of the scales over the tumour.

The following case, communicated to me by Mr. R. E. Holding, is interesting as illustrating the disastrous effects of overgrowths. The horns of a Norfolk ox had grown unduly, and become so displaced that they crossed each other beneath the mouth in such a manner that the jaws

FIG. 6.

Abnormal position and overgrowth of the horns of a Norfolk ox.

could only be opened for a distance of two inches. The animal could only feed itself with difficulty. It was fattened for the butcher by having its food administered by an attendant. The position of the horns is well shown in the accompanying engraving. (Fig. 6.) The constant

⁶ Molière: La Malade Imaginaire, 3ième intermède.