Lectures

ON

CERTAIN MUTUAL RELATIONS OF PHYSIOLOGY, PATHOLOGY, AND THE PRACTICE OF MEDICINE.

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LECTURE I. (Concluded.)

Physiology in its Relations to Pathology and the Practice of Medicine. Recent Physiological Researches. Cardiac Physiology in Relation to Cardiac Diseases. The Reflex Function of the Nervous System in Relation to Pathology, Diagnosis, and Therapeutics; its Bearings on Certain Organic and other Diseases, etc.

But, in attempting to show the relations between physiology and pathology, and the improvements in practical medicine which are due to the generalisations of these sciences, I should perhaps be placing too favourable a picture before you, were I to leave it as it now stands; for, unfortunately, there are other organs of the body, in advancing the knowledge of whose diseases, the elementary sciences have as yet done much less than in reference to those of the central organ of the circulation. The fault does not, however, rest in the fact that there exists disparity in the mutual relations of the physiology and pathology of these organs, and the therapeutics of their diseases, but simply from the imperfection of our elementary knowledge, and our consequent inability to follow, and rightly appreciate, the pathological changes which ensue in disease, and the symptoms they produce. As I have before remarked, wherever great difficulty attends mere physical investigation, there our knowledge of functional changes is imperfect, and, as a necessary consequence, our insight into diseased processes becomes less clear; and it is, perhaps, with reference to the nervous system, that we see this most forcibly exemplified; for, notwithstanding the researches and discoveries which have been made in reference to the nervous centres, our knowledge both of their structure and function, is much less accurate and precise than that which we possess of other portions of the body; and when we turn to the diseases of the nervous system, we must at once confess how much there is of obscurity in reference to their diagnosis, pathology, and treatment. From the peculiar character of the nervous tissue, and from its complex arrangement, there is unquestionably, as far as our present means go, an immense difficulty attending all inquiries into its mere anatomy; and I think it is very doubtful whether we shall ever gain as much aid from this source, in the investigation of the functions of the nervous system, as we do with reference to other organs. And we shall perhaps find that—although we derive much information from direct experiment, and from a careful comparison of the nervous centres throughout the whole animal kingdom—the most positive advances in neurophysiology will be made, by the observation of the symptoms which are produced in man, as the result

of pathological conditions, and especially of those rare and most important cases, where there is an accidental lesion of a portion of the nervous centres, and when, in fact, we have repeated, on man, the experiments which the physiologist performs on the lower animals. It is impossible to over-estimate the importance of such cases. They probably occur more frequently than we are aware of; but, unfortunately, the exact symptoms existing during life are not carefully noted; and even if a *post mortem* examination is made, it becomes valueless, inasmuch as the phenomena existing during life have not been observed. These cases are met with, for the most part, in the surgical wards of our hospitals. I venture to direct the attention of those who are engaged in hospital practice, and especially the house-surgeons of our public institutions, who have generally the first opportunity of seeing these cases, to this important subject. I would suggest to them the propriety of taking careful and accurate notes of the symptoms existing in all cases of injury about the head or spine; and, should the issue afford them an opportunity of making a *post mortem* examination, to note the particular portion of the brain or cord which is the seat of lesion, and the exact nature of the lesion. One important case (*Med.-Chir. Trans.*, vol. xlv), which occurred in the Northern Hospital, and which I was enabled, through the kindness of my colleague Mr. Hakes, to see, is probably familiar to some of you. It afforded a remarkable illustration of the untenable nature of a theory, long held by many eminent physiologists, and a valuable confirmation of the conclusions arrived at by more recent investigators.

Amongst the most important facts with which we have become acquainted of late years, in reference to the nervous system, is the discovery—for I think it must be ranked as such—of the reflex function of the nerves distributed to blood-vessels. Of the bearings of this principle on physiology and pathology we can speak in a very positive manner; and of its therapeutical relations, numerous instances might be mentioned. I think it probable that, as attention is more directed to this subject, we shall find it of far wider application than we at present see, both in reference to pathology and the treatment of disease.

We are all familiar with the phenomena of reflex action of the so-called voluntary muscles, the relations of which to pathology and therapeutics were rendered familiar to the profession by the labours of Marshall Hall. The principle of reflex action, however, has been shown to have a more extended application than was formerly supposed. In fact, we now know that not only every voluntary or striped muscle may be the seat of reflex action, but every involuntary or unstriped one also. We know that the blood-vessels of the body may be made to contract in obedience to reflex action. In fact, we have learnt further, that reflex phenomena may exhibit themselves as acts of secretion, nutrition, and sensation.

It is a fact with which we are all acquainted, that, in many cases of hæmorrhage from the nose, the application of ice, or of any cold body, to the back of the neck, is followed by an arrest of the hæmorrhage.

Again, I believe I am right when I say, although here I cannot speak from actual experience, that the

application of cold to the surface of the abdomen will often restrain an uterine hæmorrhage.

Further, and of this I can speak from my own observation, if we place one hand into ice-cold water, the blood-vessels of the opposite hand will contract—not the vessels of the foot, nor the vessels of the trunk, but the vessels of the opposite hand—of that part, in fact, which is parallel to the part affected.

Now these are assuredly important phenomena—important not only physiologically, but in a pathological and therapeutic point of view.

But, before I make any practical application of the principle, let us stop for a moment to consider how the result is produced. When we arrest the bleeding from the nose by the application of the cold body to the neck, it is assuredly not by the direct effects of the cold that the contraction of the nasal blood-vessels is caused. The mere fact that the surfaces, although distant from each other, are yet continuous the one with the other, will not account for the phenomenon, any more than it will explain the contraction of the vessels of one hand when the other is submitted to the influence of extreme cold. The effects are unquestionably due to the impression made on the sensitive nerves of the skin, which impression, being conveyed to the spinal cord, results in a reflex act sent along the vaso-motor nerves.

This principle, then, established, I proceed to point out how it may show itself in acts of secretion, nutrition, and sensation. And first of secretion. It is well known that an irritation of the sensitive nerves of the face will produce an abundant flow of tears; that an irritation of the nerves of taste will cause a secretion of gastric juice; and that an irritation of the gastric nerves will give rise to a copious flow of saliva. Other instances might be mentioned, but these will suffice.

With regard to nutrition, there is abundant evidence to show that an impression made on one part of the body may give rise to a morbid process, an alteration in the nutrition, in a distant part. Injury of the sensitive nerves about the eye will often produce inflammation of the eye itself; and traumatic inflammation of one eye will often give rise to inflammation of the opposite eye. Again, how frequently do we find that a neuralgic affection of the fifth pair of nerves gives rise to a congestion or inflammation of the eye. These are instances in which an irritation of a peripheral nerve has produced, through reflex action, a morbid process; and I might adduce other examples. Again, in reference to sensation, the subject is one of no small practical importance. As injury, or disease, of a sensitive nerve may give rise to perverted nutrition, so may perverted nutrition give rise to pain, at some spot far distant from the seat of disease. Pain has been described, in the poetic language of Romberg, as “the cry of the nerve for healthy blood”; and although, in a given case, we can see no evidence of disease, either in the part which is the seat of pain or elsewhere, we must never forget the reflex effects produced by pathological processes. How otherwise can we explain the pain in the knee, with which surgeons are so familiar, in hip-joint disease? How otherwise, also, can we explain the varying seat of pain so frequently observed in some affections of the brain? Do we not frequently find that diseases of the brain, similar, as far as their morbid anatomy is

concerned, are in some persons for a long time attended with few or no symptoms, or symptoms of a particular kind; whilst in others the symptoms are much more urgent, or perhaps even of a different character altogether? Do not these facts add much to the obscurity of these disorders, and increase the difficulty of our diagnosis? And can we explain them on any other principle than that the perverted nutrition of the part involved in the disease has given rise, in different individuals, to different reflex manifestations? These reflex effects of disease are unquestionably of the greatest possible importance, especially in reference to obscure affections. How often do we see that a serious organic disease of the brain has gone on to an extent necessarily involving a fatal issue, without giving rise to any symptoms except of so mild a character as scarcely to attract the attention either of the patient or his medical attendant; and yet symptoms have no doubt occurred—symptoms not the direct production of the diseased parts, not involving any perverted function of the organ attacked, but indirectly produced in a distant part of the body, and showing themselves there as reflex acts of nutrition, secretion, or sensation.

Facts of this kind should make us additionally cautious in our diagnosis of obscure affections, and should teach us not to regard, as a trifling matter, every superficial pain, or slight disturbance of function, the immediate bearings of which we cannot exactly see; for symptoms of this kind, unimportant as they may at first sight appear, may be no other than reflex demonstrations of some incipient deep-seated disease. And just as, in the early stages of degeneration of the kidneys, a slight anatomical change, a mere puffiness of the under eyelid, may point to the distant affection, whilst apparently health exists, so a slight perversion of physiological action may indicate the existence of an important organic malady.

Again, consider for a moment the pain in the arm and hand, and the altered circulation in the fingers, which so constantly come on in cases of fatty degeneration of the heart. This pain, and probably a large portion, if not the whole, of the pain of angina pectoris, is but a reflex manifestation of the internal disease, just as the pain in the knee is the reflex manifestation of the hip-joint disease; and the altered circulation in the hand can be ascribed to no other cause than a reflex contraction of the capillaries of the affected parts. We cannot attribute the phenomenon to the feeble action of the heart; for it is often confined to one or more fingers, and almost always to one hand. It is a symptom which always merits our careful consideration. In the late instance of death occurring to an eminent member of the legal profession, the symptom was strongly marked; in fact, it was the only prominent feature in the case. Its significance does not, however, seem to have been correctly appreciated until after the fatal result occurred.

I might further illustrate the importance of the principle of reflex pathology, in reference to various other morbid phenomena. Take, as an instance, the distressing symptoms which frequently arise, in connexion with thoracic aneurisms, before we have any physical evidence of the existence of these tumours. How often does it happen that we get a sudden accession of severe asthmatic, or laryngeal, symptoms—

a spasm involving the muscles of the larynx, or the whole, or a portion, of the muscular fibres of the bronchial tubes. Such cases most of us must be familiar with. The first impression is, that we have to deal with a severe asthmatic seizure, or a case of laryngitis. And how often has it happened that, acting on the latter supposition, the physician has directed his remedies to the relief of this supposed malady. And have not the most accomplished practitioners even thought of proceeding to the operation of tracheotomy, when perhaps the symptoms have suddenly become modified, and have thus indicated their true reflex nature?

How important is it that these facts should be well impressed on our minds, and that we should examine in all its relations this subject of reflex pathological action! It is perhaps not too much to say that, by a careful study of it, we might learn not only to diagnose with greater facility the more obscure affections with which we have to deal, when they are fully established, but, what is of greater importance still, to recognise them in their earlier stages, and at a time when remedial measures might be more successfully used.

Again, I might refer to the importance of a knowledge of this reflex principle in regard to some other diseases; and I would specially single out certain forms of paralysis which are occasionally met with. It is not every case of paraplegia which depends on organic disease of the spinal cord, or of the canal in which it is lodged. There is a paralysis which results from some peripheric irritation, which is altogether independent of any softening or inflammatory action going on in the cord, which will last as long as the exciting cause remains, but which admits often of very speedy cure, when once the exciting cause is removed. Graves relates the case of a sailor who was treated by Dr. Hutton for a stricture of the urethra. There were chills, dysuria, and loss of power over the lower limbs. "A very remarkable amendment took place in his back and lower extremities in a few days after the introduction of a catheter; in fact, it was almost sudden. The paraplegia was soon cured, together with the disease of the urinary canal."

I give this as an illustrative case; but I might refer to others of a similar kind, where paralysis has come on in conjunction with inflammation of the bladder—a case of which kind I have now under my care—abscess of the prostate, nephritis, or enteritis; and where the power over the limbs has rapidly returned without any specific treatment, but merely in consequence of the removal of the original disease.

Again, cases of the following kind must be referred to the same cause. After exposure to wet or cold, or both, loss of power over the limbs comes on, without the existence of any of those symptoms which characterise organic disease of the cord or its membranes. I have had two such cases under my care, and one quite recently. In both instances, there had been exposure to wet and cold; and this was followed by a paralysis, in one patient of the lower extremities only, in the other of all the limbs. There were no symptoms of softening, or inflammation, of the cord or its membranes, nor of disease of the vertebræ. Both cases yielded in a few weeks to simple treatment.

There is one other disease to which I would briefly

refer, on which the principle of reflex action has undoubtedly very important bearings. I mean epilepsy. It is probable that the more we know of the nature of this disease, the more we shall see that it is one which will never admit of distinct, specific, and definite cure; but that, depending on various constitutional conditions, each case will require its own peculiar treatment, directed more to the general state of the individual than to any specific morbid condition of the nervous system.

The application of the principle of reflex action of voluntary muscles, to the phenomena of an epileptic seizure, was undoubtedly an important step in explanation of these phenomena; but it by no means served to explain the sudden loss of consciousness in the affection, nor why in one individual a local irritation, either palpable or obscure, should give rise to the clonic convulsions which characterise the affection, whilst in the majority of individuals no such conditions arise.

It would occupy too much time to consider the question of the exact condition of the brain in the epileptic seizure; but, passing over this point, there are some pathological views of the disease which appear to me worthy of attention, especially as I have observed beneficial results follow a treatment directed in accordance with their indications.

Admitting that the epileptic fit is a phenomenon of a reflex character, it is obvious that there must be some centre from which this reflex action proceeds. Now without dwelling at length on the subject, it seems probable that this centre is the medulla oblongata—that in epileptic patients there is some peculiar condition of this portion of the nervous system, which is the cause of the seizures. It has been shown by the pathological researches of Dr. Schroeder van der Kolk, that, in old-standing cases of epilepsy, a *post mortem* examination has revealed a vascular condition of the medulla oblongata. Even if we admit that, in the earlier stages of the disease, there is no increased vascularity, and that the vascularity in these old cases is the result of the repeated attacks from which the patients have suffered, yet the fact of such vascularity existing, shows the tendency for the medulla to become implicated; and we may fairly allow, that supposing the medulla to be the centre from which the reflex action, giving rise to the convulsions, proceeds, any increased vascularity, or alteration in its nutrition, might increase its excitability.

As I have already remarked, I am convinced, from my own observations of the disease, that no particular line of practice will be universally applicable to it; but guided by the pathological views to which I have just alluded, I have certainly found a treatment, which appears to have been very successful in the hands of Schroeder van der Kolk, followed by more satisfactory results than any other I have employed. This consists in the use of such general measures as the nature of the case may indicate, the great point being to keep the blood in a healthy condition, and to regulate the various functions of the body; and further, in the occasional application—especially in some cases where pain is complained of in the back of the head—of one or two leeches, or the use of dry cupping, or of counterirritation in some form, to the nape of the neck.

But what has appeared of more real value than any of these local measures, is the systematic and

prolonged use of a tepid or cold douche to the back of the head and neck, continued for from five minutes to a quarter of an hour every night.

I have thus endeavoured to point out some of the relations of the reflex function of the nervous system, more especially as regards the vaso-motor nerves; and I have shown that we can, through the medium of the function, act on distant parts; that by irritating the skin of one part of the body, we can produce a contraction of the blood-vessels of another part. On this principle, I believe, hinges, in great measure, the value of counterirritation.

For a long time I could not satisfactorily account for the effects of counterirritation; but I now think that a knowledge of the principle of reflex action of the nerves of the blood-vessels enables us, in great measure, to explain the phenomena, and affords us a rational basis on which to ground our practice. Just as, by the application of ice to the neck, or hand, we can produce a contraction of distant capillaries, so, I believe, by the application of other counter-irritants, we may produce a like contraction of vessels, and perhaps the more so, if those vessels are morbidly dilated.

It may be thought that there is no parallel between the sudden contraction of the vessels of the nose, or hand, from the application of ice, and the more gradual effects produced on an inflamed organ by a blister or other irritant; but it must be borne in mind that, in the one instance, the cause is sudden, and produced in all its intensity at once, whilst, in the other, the cause acts more slowly, and its effects are likely to be more gradual.

Further, the vessels in the one case are in a condition of health, whilst in the other they have lost their tonicity and cannot suddenly recover it. Again, although the contraction of the vessels of an inflamed part is an important element in the cure of an inflammation, it is probably only a part; and the reflex action may not be confined to the mere diminution of the calibre of the vessels, but may influence, in other ways, the nutrition of the parts.

Before I conclude my remarks on the relations of reflex action, I wish to direct your attention very briefly to a subject which has long occupied the attention of the profession, and which, at the present time, is undergoing re-discussion. I mean apnœa. And the main points to which I wish to refer are: first, the distinctions we should draw between the apnœa of the still-born, and the apnœa occurring after respiration has been fully established; and secondly, the difference in details of treatment applicable to each.

It is somewhat remarkable that, although the distinctions may have struck those who have given attention to the subject, they have never been practically insisted on, and the same measures for effecting the entrance of air into the lungs of an infant, who has never breathed, have been, and still are, recommended, as in the case of an adult.

It is well known that I have, both in this institution and elsewhere, advocated the use of Marshall Hall's ready method in cases of suspended animation, when we meet with it after respiration has been fully established; and from the numerous instances of recovery which have been recorded, in the case of still-born children, where this same method has been used, its applicability to such cases may appear to be well established.

A little consideration, however, will, I think, serve to show us that, in these latter cases, the method can be of no avail as a direct means of getting air into the lungs. The great feature on which the value of the ready method rests, is this: *that by first compressing the chest, we drive air out of the lungs, and thereby bring into play the elasticity of the thoracic walls, by which means air is drawn into the lungs.*

By alternate compression and relaxation, a constant current of air to, and from, the lungs may be produced; but the *first* step in the process is the expulsion of air from the lungs. Now, in the still-born infants' lung there is *no* air; the air-sacs, the bronchial tubes, and the trachea, are unexpanded; in fact, they are in a perfectly solid state. It is quite obvious, therefore, that no air can be forced out of the lung, and consequently, that the primary step in the ready method cannot be accomplished. It is true, that if respiration have been established, however feebly, the method may be brought into play with benefit, but respiration cannot be commenced by this process.

The case is, however, far different in all instances where the lungs have been previously in action. Here the essential condition for the performance of the ready method exists. Air is present in the lungs.

It appears to me, therefore, that the apnœa of the still-born child and that of the adult differ very materially; and in their treatment, although the same principles may be applicable, yet they must be modified to meet the requirements of each case. In the infant, respiration has to be primarily excited; in the adult, it has to be restored. In the infant, setting aside the question of direct inflation of the lungs, a proceeding of very doubtful value, all efforts should be directed to produce such an impression on the cutaneous nerves as will result in a reflex act of inspiration. In fact, we have to imitate the process of nature; the first inspiration is the result of the impression of air on the surface of the body. If this fail, the child is still-born, and impressions of a more powerful kind must be resorted to. The alternate use of hot and cold water to the skin, the brisk application of the palm of the hand to the back, friction of the chest, even rolling the body, may produce the desired result, a reflex inspiratory act.

And does not actual experience confirm these remarks? I have frequently been told by those who have had to resuscitate still-born children, that the ready method has appeared to them of little or no use; whereas, means calculated to excite reflex action have been most efficacious. It is well that whatever plan we adopt, we should have distinctly before us the objects at which we aim. Those who are familiar with the condition of the infant's lungs before respiration has occurred, must, I think, feel convinced that, setting aside direct inflation, nothing can be of any avail to get air into them, except the expansion of the chest-walls by muscular action.

MATICO. In consequence of the war in America, where matico is in great repute as a vulnerary, and where, for that reason, it is unhappily just now in great request, true official matico—the leaves of *Artanthe Elongata*—has become a scarce article in this country.