Croonian Lectures

ON THE

LOCALISATION OF DISEASE.

DELIVERED BEFORE THE

ROYAL COLLEGE OF PHYSICIANS.

BY

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LECTURE II. (*Continued.*)

PERMIT me now to turn to the consideration of another form of morbid action, in which the stomach itself, the prime organ of digestion, becomes the seat of disease. Let us for a few minutes consider simple ulceration of the stomach, and inquire whether that may be regarded as a primary local affection. At first sight it presents all the appearance of meriting that appellation. There is pain and tenderness at the epigastrium, well marked according to the exact locality of the ulcer; there is more or less pain after food, often amounting to intense suffering; there is vomiting, and the vomiting may be accompanied by hæmatemesis. The patient, from not being duly nourished, becomes emaciated and more and more exhausted, and death may end the scene. But, if we inquire a little more closely, we shall find antecedent indisposition of greater or less duration—if the patient be a young woman, the probability is that for one or more months the catamenia have been arrested or scanty, there has been a languid circulation, cold feet and chilblains, often more or less dry cough, and more or less absence of appetite and general debility; or there has been general lassitude, with inability to take proper food and an indisposition to attend to the ordinary duties of life, which sympathising friends attribute to indolence or want of energy and resolution, and which sends the sufferer to the chemist's shop or the cellaret, but does not seem to justify calling in the physician. Have we not all again and again been consulted about such local disease, which necessitated a prolonged sick regimen and medication, or which had already attained to such a point, that our indignation could scarce express itself in terms sufficiently polite to convey our opinion of the utter and almost wicked neglect of God's laws that had been incurred, sins, as the clerical apologist would say, not of commission but of omission? Fortunate are those in whom the destruction of tissue, the organic disease, is discovered before irreparable mischief is done; but more fortunate still are they who at the first symptom of indisposition receive and follow the advice that may anticipate new growth or destruction of tissue. There is a difference in the duration of the antecedent traceable symptoms of disease, according to the greater or less carelessness of a sanitary regimen; but they all—I speak under correction—agree in the broad fact, that long before the physician can diagnose ulcer of the stomach, there are palpable proofs of deranged digestion and irregular general nutrition.

Permit me, not to weary you with my personal experience, to lay before you a part of one of Dr. Brinton's cases. (*On the Pathology, Symptoms, and Treatment of Ulcer of the Stomach.* By William Brinton, M.D. 1857. P. 157.) E. S., an unmarried female servant, aged 22 years, had enjoyed good health as a child. She menstruated at the usual age, and continued to do so regularly till she came under observation on Nov. 13th, 1855. The flux had, however, become more and more scanty during the last five or six months, though it was only at the last menstrual period prior to her application at the hospital that its decrease amounted to complete amenorrhœa. About two years before her admission, she began to suffer from symptoms of severe indigestion; especially from capricious (generally deficient) appetite; pain in the region of the stomach and between the shoulders, usually coming on about an hour after eating; and irregular (mostly) sluggish action of the bowels. After about twenty months of these symptoms, vomiting, which had previously occurred but rarely, gradually became very frequent. And, at the end of about four months more, it increased to such a degree as scarcely to allow any meal to be completely retained by the stomach. This state was accompanied by a great increase in the severity of the pain, as well as by a considerable alteration in its character. Latterly, though it came on shortly after meals as an intense pain, it was almost continuous during the whole day as a dull burning soreness. With regard to the further progress of the case, it may be satisfactory to know that, thanks to the scientific treatment of an able physician, the patient entirely recovered, in spite of the long continuance and the severity of the symptoms. What more immediately concerns us is, that the details of her case, with reference to the antecedents of gastric ulcer, apply with but slight variation to all cases of the kind, and afford an indication of the importance of early rectification of all errors of nutrition tending to localise themselves in the stomach. We ought further to inquire why, if there is so close a resemblance between the fundamental conditions upon which all morbid action is based, there is a greater tendency to the localisation of disease sometimes in one, sometimes in another organ. The inquiry is a legitimate one; and I allude to it to show that I do not ignore its interest and practical importance. But to entertain the question as it merits, would demand much more time and space than are allowed me. I therefore return to my original argument.

Anatomical proximity justifies my passing from the stomach to the peritoneum. Time was, when peritonitis, idiopathic peritonitis, met us at every turn. What does morbid anatomy, skilfully read by our distinguished fellow, Dr. Habershon, teach us? Dr. Habershon states that he has ascertained by the analysis of 500 cases of peritonitis which occurred at Guy's Hospital, among 3752 inspections in twenty-five years, that not one could be correctly regarded as solely in the peritoneal serous membrane; he finds that the cases referred to are divisible into three classes: 1, those produced by the extension of disease from adjoining viscera, or excited by direct injury; 2, peritonitis connected with blood changes, as albuminuria, pyæmia, puerperal fever, erysipelas; and 3, peritonitis caused by nutritive changes of the system, such as struma or cancer. (See Dr. Haber-

shon's paper on the Etiology and Treatment of Peritonitis in *Med.-Chir. Transactions*, 1860.) It follows that, if peritonitis be not altogether a myth, it is but a rare occurrence; not, I believe, altogether unknown, but still so seldom seen as to justify a warning to the beginner to expect almost any other abdominal complaint than this. And what, again, is the pathological import of Dr. Habershon's researches, but that, as in pneumonia, as in gastric ulcer, so here, too, the nosological and the real entity are two different things, and that, to constitute peritonitis, we must have a series of more or less marked antecedent disorders; and that the peritonitis is the climax of nutritive derangements certainly not to be sought for primarily in the serous investment of the intestines? In the summary of conclusions with which Dr. Habershon favours us at the end of his paper, he dwells mainly on questions of treatment; but his first paragraph is so applicable to my general argument, and has so close a relation to observations that I have previously made, that I cannot resist quoting it. Dr. Habershon says that he considers his analysis of the Guy's Hospital *post mortem* examinations to establish, "that peritonitis is never idiopathic in its origin, and that we do not find any such instance as acute disease of the peritoneum coming on from mere exposure to cold. *In such supposed cases, the cold merely tends to render acute an already existing morbid state.*" I do not, I think, misinterpret this sentence by translating it into the language of the present argument, and saying that Dr. Habershon regards the peritonitis as a local expression of a general and antecedent derangement involving the elements of nutrition; but for this paraphrase I, of course, alone am responsible.

If we now turn our attention to the diseases affecting the parenchymatous organs situated in the abdominal cavity, what does modern medicine teach us with regard to the liver—to the kidneys—to the pancreas—to the spleen? As the attention of observers is directed to one or other organ, is there not a tendency at first to regard it as the *fons et origo mali*, its morbid conditions as essential and primary affections, and all other diseases as consecutive and dependent? What follows? As morbid anatomy gives precision to our knowledge, and we trace the relations between effects and causes, we arrive more and more at results analogous to those exhibited by Dr. Habershon in reference to peritonitis; the importance of the local disease falls away more and more, and the general condition of nutrition and metamorphosis of tissue advances to the foreground, enabling us to show a concatenation of conditions which did not appear at the first commencement of the inquiry. How many are the idiopathic affections of either of these organs? How many of the nosological entities of our class-books can we retain when we study them at the bedside, and with the light that modern physiology, chemistry, and pathology, shed upon them? In the pathology of the ancients, and particularly of Galen (we find Frerichs remarking), the liver and the portal system served as the starting point of manifold disturbances. (Frerichs, *Diseases of the Liver*; Dr. Murchison's Translation.) There were described, not only a host of anatomical and functional lesions of the organ itself, such as inflammation, abscess, obstruction of the ducts, and the different conditions resulting from intemperance, but a large proportion of constitutional diseases were

referred to the same source. Abnormal conditions of the liver were regarded as the principal cause of changes in the constitution of the blood. Gradually, however, by the labours of successive inquirers, from the times of Galen to those of Frerichs and other contemporaries, the physiology and pathology of this important organ have been placed on a more correct basis. Our knowledge of the normal functions of the liver appears to be more precise than its application to disease, owing to difficulties in applying our knowledge of its physiology to the investigation of its pathology, to which Frerichs forcibly alludes. But still, the more we extend our inquiries, one conclusion always appears to force itself upon us more and more, that any given disease of the organ has not an existence *per se*; but, to be fully explained and accounted for, must be traced back to some earlier state of perverted nutrition, arrested and vitiated metamorphosis of the tissues, acting perhaps through the nervous system, but more probably through the blood, upon the hepatic tissue. Let us see what can be learnt on this point in studying the idiopathic disease of the liver—*κατ' ἐξοχὴν*—if there be one, hepatitis. It is scarcely known to us in its acute form in our temperate regions. I cannot do better than quote from one of the most careful scrutinies of medical statistics bearing upon tropical disease, a model for other inquirers, Dr. Morehead's *Clinical Researches on Disease in India*. After discussing the influence of intemperance in giving rise to the affection in such preponderating numbers in Europeans, he asks:

"Is hepatitis, with a tendency to run on to abscess, peculiarly related to the cachexia engendered by continued and lengthened influence of elevated temperatures? I believe," he replies, "it is so. It is very probable that future research will show, more distinctly than any data which I possess, that those who are exhausted and enfeebled by continued heat and its associated debilitating conditions are very prone to hepatitis; and that in such individuals the inflammation is very frequently excited by exposure to external cold; I mean, to such depression of temperature as suffices to influence bodies whose heat-generating power is low. But there is another question to propose in regard to heat. Is it ever the exciting cause of hepatitis, as it assuredly sometimes is of cerebral disease? The occurrence in the hot months of the year of hepatitis in plethoric Europeans, lately arrived in India, and with excreting functions deranged by free living, is probably sometimes best explained on the supposition that tropical heat is occasionally an exciting cause of hepatitis."

You perceive that in both these cases, the cold in the former, the heat in the latter, act, according to Dr. Morehead's opinion, only as exciting causes to the disease, in the same way that similar agencies aid in producing catarrh, as spoken of at the beginning of this lecture. The real efficient cause in either case is the derangement of nutrition, the accident of climate and habit determining the organ which is to suffer. It would lead us too far to go into an analysis of the circumstances associated with the production of the chronic diseases of the same organ—cirrhosis, fatty degeneration, hypertrophy, or waxy degeneration. If I have made out anything of a case in favour of acute hepatitis being essentially a disease consequent upon malassimilation or perverted metamorphosis of tissue, *à fortiori*, the argument applies to the morbid conditions just mentioned,

which, by the common consent of the profession, are already regarded as typical of deep-seated malnutrition. If the blood is unable to maintain the nutrition or the functions of the part, whatever it may be, inflammation or other disease, as Mr. Paget (*Surgical Pathology*, vol. 1, pp. 15 and 312) well shows, are certain to follow. The question of acuteness or chronicity will depend upon the intensity of the perversion in the pabulum, the vital force of the patient, and the severity of the exciting cause.

Permit me now to direct your attention to the diseases of the kidney, with a view to a further elucidation of the argument. What number of diseases do we meet with in this important emunctory, which can truly be said to be idiopathic—to arise within itself? We hear of nephritis, as we hear of inflammations of every other part of the body; and probably, in the course of a long practical experience of disease, one or more cases occur within our own direct observation, which we cannot fully account for by the facts ascertainable in regard to antecedent derangements. But what is the bearing of the whole teaching of modern clinical research, from the time when Bright first threw the light of his genius upon so many previously unintelligible processes? The more we pursue the path of rigid induction, which, since Bright, has so well been trodden by Christison, by Johnson, by Frerichs, by Parkes, the more we satisfy ourselves that any given disease of the kidney is but an expression of anterior malnutrition pervading the system and residing essentially in the blood. That this view is more generally adopted than would appear at first sight, appears from the heading under which the most recent authority on medicine, Dr. Aitken, discusses renal disease. He entitles his section that is devoted to that subject, "Diseases in which Lesions tend to be localised in the Kidneys." This implies that he regards the disease, the primary morbid state, as something external to the organ in question; and that its localisation is a secondary result—the former the essential, the latter, if I may so say, the accidental feature. If we go to statistics for precise numerical information, the aspect of affairs is not materially altered, unless we choose to assert that it is only the secondary diseases of the kidney that find their way to the *post mortem* table. Dr. Chambers remarks on the difficulty with which the kidneys take on the inflammatory state, inasmuch as 2161 autopsies at St. George's Hospital yielded only forty-five instances. A summary of these cases shows inflammation of the kidney to have been due in twelve cases to pyæmic infection; in one case, to suddenly checked skin-disease; in five cases, to injury of the spine; in two, to lithotripsy; to a neighbouring abscess, in one; to diseased bladder, urethra, prostate, etc., in twelve; to the first stage of Bright's disease, in eleven; and to the presence of scrofulous tubercle, in one case. (*Decennium Pathologicum, British and Foreign Medico-Chirurgical Review*, April 1853.) This large field of observation, then, offers not one case in which there was not well marked antecedent morbid action in the system. If this is the conclusion forced upon us regarding nephritis, it is scarcely necessary to discuss any chronic renal affections, which, by common consent, are all regarded as the "effect defective" coming by the cause of vitiated blood. Is it otherwise with any functional disorder

of the same gland? Do we not investigate the constitution of the urine in acute and chronic disorder, generally with the view to determining "changes which go on in the body"—whether those changes point more to the nervous centres, to the stomach, to the liver—but still essentially changes involving primarily nutrition, assimilation, and metamorphosis? The excretion of urea has long been regarded as the product of the disintegration of the nitrogenous tissues of the body, the nitrogen forming nearly one-half (46.667 per cent.) of the elements constituting urea. The quantity of urea eliminated varies in health according to the food, exercise, and general habits of the individual; but, if the Rev. Dr. Haughton's conclusions are adopted, it affords a direct measure of the amount of work performed by an individual. Dr. Haughton holds that the average tissue-change of the body in health is at the rate of two grains of urea during the twenty-four hours, for every pound weight, assuming that the body is at rest; but that the increase in the urea discharged is in the exact ratio of the amount and kind of work done. (See papers on the Natural Constituents of the Urine of Man, by the Rev. S. Haughton, F.R.S., *Dublin Medical Journal*, 1859 and 1860; also Parkes on *Urine*, p. 109.) In this variation of an important excretion we have one of the palpable securities for the preservation of health under the great variety of influences to which the human body is exposed. I am not in a position to assert the absolute correctness of Dr. Haughton's data. One objection raised by Dr. Parkes is, that he assumes all the nitrogen used in the work of the body to be eliminated by the kidneys. But, even if there be sources of error, it is impossible not to accept the results of Dr. Haughton's elaborate researches as at least approximately true. We should be the more disposed to do so, if we consider that they represent in a scientific manner the daily experience of mankind as to the relation between expenditure and force, and that they further indicate that correlation of the vital forces to which I have previously drawn attention. Thus Dr. Haughton calculates that the force which is equivalent to raising one hundred tons one foot high in twenty-four hours leads to a disintegration of tissue equal to 38.69 grains of urea. If a man weighing 150 lbs. perform an ordinary day's work, which is equivalent to raising about 258 tons one foot high, he will pass 100 grains of urea in addition to the 300 grains which were derived from the necessary metamorphosis of tissue. Whether this be absolutely correct, or only an approximation to the truth, there can be no doubt that, in the considerations presented by these and allied experiments, we possess some of the most important of all indications to a real appreciation of morbid phenomena. They offer us some of the earliest indications of variations in the relation between force and expenditure which constitute disease. If, by the use of the thermometer and allied methods, we are enabled to watch the actual course of vital phenomena, and with the test-tube and scales determine the results to which they lead, we have at once a measure of health and a means of recognising the disturbance of the balance between the forces that constitutes a deviation from the normal standard, or disease. At the same time, the balance between the different forces in the body affords (as we shall have occasion to see later on) the most correct indication

for the means by which health may be preserved and restored.

The last remarks have been suggested by the renal excretion, which is more readily used as a means of testing an individual's condition than other secretions and excretions; but it is evident that neither our physiology nor our pathology will be perfectly reliable, so long as we have not obtained the means of accurately estimating all the changes taking place in the body at the same time. It is scarcely conceivable, that human capacity will ever achieve so vast and glorious a result. For the present, we must be content with a distant view of our goal, and with a careful inquiry whether we have discovered and are pursuing the right path that leads to it, though our strength may be insufficient for the race.

[To be continued.]

Original Communications.

NOTES AND OBSERVATIONS ON DISEASES OF THE HEART AND LUNGS.

By T. SHAPTER, M.D., F.R.C.P., Senior Physician to the Devon and Exeter Hospital, etc.

THE medical observer, who has had an extended practical acquaintance with diseases of the heart, must often have been struck with the fact that certain analogous departures from the normal action of this organ are, in some cases, associated with fatal tendencies, in others are proved to be consistent with a good average state of health; while others, again, yielding to the most careful physical examination but few, or even no, indications of disease, may prove to be unexpectedly fatal.

It may be assumed that those more obvious physical phenomena which the heart reveals, whether to the touch, to careful percussion, or to the attentive ear, as differing from what is usual in health, are due to departures, perchance temporary only, from the normal condition of this organ; and it may also be concluded, where these phenomena are found to be persistent, that they are due to alteration, as the case may be, in its structure or bulk, or to disarrangements of its valvular completeness.

These latter generally entail, sooner or later, by their disturbance of the animal economy, a fatal termination. Nevertheless, conspicuous and very remarkable exceptions occasionally present themselves, and the issue of life or death does not appear to be influenced by the inferred condition of the heart; while, on the other hand, the former may present themselves as very dangerous, and perhaps immediately fatal affections, as may also those cases occasionally met with, and which appear to be due to no structural alterations appreciable by careful examination, or, at any rate, to such as neither alter the size of the heart nor impede its blood-flow.

It is proposed, by reference to the notes of a considerable number of cases, to review some of the affections belonging to these several categories, and chiefly with the view of considering and determining their relative fatal tendencies. But, before doing this, I should wish to say some few words respecting the action of the heart in its relations to the circulation of the blood, and of the sources of the normal and abnormal sounds emitted thereby.

As regards the heart, it has appeared to me that the view of its action has been unnecessarily—I would even say erroneously—complicated by the general statement that the function of the auricle is that of a “propeller” of the blood into the ventricle. The simpler view that I would hazard is, that the ventricle is essentially the organ that, on the one hand, by expansion sucks in the blood, and, on the other, by contraction expels it; that the proper function of the auricle is, on the one hand, to contain a ready supply of blood for the expanding ventricle, and, on the other, a safety-pouch to counteract or equalise the backward strain caused by the contraction of the auriculo-ventricular valves.

I have elsewhere (*Medical Gazette*, Dec. 5th, 1857) ventured to state that, as regards the heart, the circulation of the blood is more simply effected than is usually considered to be the case; that, in fact, the heart proper is ventricular, and ventricular only; and that the circulation of the blood through the heart mainly depends upon, firstly, its ventricular exhaustion or suction power, and then, secondly, on its ventricular contraction, whereby it is induced with the function of propulsion.

The auricle, on the other hand, is rather to be considered as an appendage proper to the venous system, and not as appertaining or necessary to the ventricular heart; and therefore (without ignoring the influence, as a propeller of the blood, of the capillaries and of the veins generally, feeble though this influence may be, as also of the auricle, as portion of the venous system), auricular contraction is not an active agent in the movement of the blood.

The above conclusions are grounded on the fact, that the distension or greatest size of the auricle is not induced by a sudden influx of blood from the cavæ, but on the contraction of the ventricle, when there arises a retrograde pressure against the onward flow of the blood, caused immediately by the closure of the flaps of the mitral (or tricuspid) valve as these are propelled into the auriculo-ventricular axis; hence the blood then contained within the auricle is ponded or regurgitated back, and the greater distension of the auricle is effected. The relaxing of the valve, on the expansion of the ventricle, whereby the exhausting or suction power of the heart is exercised, immediately relieves the auricle of this, its fuller amount of tension. Its normal condition of a full, but not distended, vessel is then restored. In this state it continues during its period of rest to receive blood from the cavæ, and to deliver it into the expanding ventricle, till this latter, being again filled, again contracts upon its contents.

In the above we see exemplified, as regards the auricle, the phenomena of elasticity, rather than those of active muscular contractility; while the ventricle distinctly exemplifies the latter.

The normal state of the auricle is that of an elastic tube or vessel, capable, within certain limits, of expanding or contracting on the fluid it is called on to contain. Ordinarily full, it partially collapses as the blood is first drawn from it into the ventricle, on the expanding of this latter, immediately after its period of contraction has been concluded. The ordinary and, so to say, passive condition of the auricle is again disturbed at the moment of the closure of the auriculo-ventricular valves, when it becomes obviously distended, from containing, relatively, an increased amount of fluid. The adaptation of the auricle to these series of changes may fairly be assumed to be due mainly, if not entirely, to its elasticity, and not through the agency of any active inherent contractile power.

The very careful and valuable experiments of