Croonian Lectures

ON THE

LOCALISATION OF DISEASE.

DELIVERED BEFORE THE

ROYAL COLLEGE OF PHYSICIANS.

BY

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LECTURE I.

MR. PRESIDENT,—It is no disparagement to the unity of medical science, that its cultivation has always been carried on under two divergent aspects. Two *kinds* of the human intellect have at all times worked at the many problems that Nature places before us in morbid action; and while, in their extremes, both have often erred and will continue to err, both have, singly and conjointly, solved many questions of import to the advancement and benefit of mankind. At one time in the history of medicine we see the one, and at another period the other, prevailing. At the same time, the careful observer will discover in different countries tendencies belonging more to one or the other phase—tendencies that we may, with Mr. Buckle, attribute to climatic and other physical accidents of locality, but which, however accounted for, modify largely the practical conclusions of the healing art. In the language of the logician, the one phase would be termed the *à priori*, the other the *à posteriori*, method. The realist and the transcendental frame of mind might be the designation preferred by another school of metaphysicians; the distinction between the practical and the theoretical bias might convey my meaning better to another; but I shall probably be best understood if I state without circumlocution, that I allude to the types of mind displayed respectively among the cultivators of the two great branches of medicine, external and internal medicine, or surgery and physic. The one professes to deal essentially with what may be called direct, the other with circumstantial, evidence. The one is engaged with what is subjected to ocular demonstration and manual interference; the other seeks to unravel recondite processes *apparently* beyond the reach of physical investigation.

But, from the time of the great man from whose works the motto is taken that surrounds our coat of arms, to the present period, the most distinguished among our brethren have sought to realise the intimate relations that bind the two together; and it is not the least of the benefits conferred upon the science of medicine by the College of Physicians within the last few years, that it has officially recognised them by appointing, though not from its own body, examiners in surgery. It is, however, an error to suppose that this is an innovation. If an abuse or

neglect have been perpetrated at any time, it was committed when the College refused to have aught to do with surgery; for it was not so of old. Our great Fellow, Harvey, lectured on anatomy, in accordance with the directions of the will of Dr. Caldwell, who founded the Lumleian Lectures, *in usum lectoris artis seu scientiæ chirurgicæ*. Harvey styles himself, on the title-page of his *Exercitatio Anatomica de Motu Cordis et Sanguinis*, "Professor of Anatomy and Surgery to the College". Not a hundred years after the establishment of the Lumleian Lectures, property was devised to the College by Lady Sadlier to found, under the name of her first husband, Dr. Croone, a surgery lecture. It appears, then, that, so far from this College formerly ignoring surgery, it was willing to be the acknowledged promoter and teacher of this essential member of the healing art.

Little worthy as I deem myself to accept the office of Croonian lecturer, and to offer to so distinguished an assembly of the representatives of our profession any results of my own humble efforts in the cause of science, I conceive that I shall best meet the intentions of the founder of the Croonian Lectures by discussing one or more of those topics where the surgeon and the physician meet as on neutral ground. In fact, if we study the modern works of some of our most distinguished surgeons, from the *Principles of Surgery* by John Hunter to the *Lectures on Repair and Nutrition* or those on *Rest and Pain* by our cotemporaries, we find every where evidence to prove how completely surgery and physic dovetail into one another—how they supplement each other in all scientific aspects—how they offer mutual aid and explanation in matters of a directly practical character. It would seem almost at times as if the surgeon and physician changed places; the former looking for general constitutional states and constitutional means of treatment only to explain and remove local disorder; the latter seeking to localise all morbid action, to the exclusion of systemic disorder, and regarding the individual cell as the entity in which health or disease resides, without considering the source from which that cell draws its pabulum, and the controlling power under which it manifests its various actions. The reproach which Abernethy (*Surgical Observations*, p. 1) made to the profession, that "the effects of local disorder upon the constitution had been too little attended to," and that "the reciprocal operation of constitutional disorders upon local diseases had obtained still less attention," can scarcely be said to apply to a generation that has heard or read the lectures delivered of late years before the College of Surgeons. There is no longer, I apprehend, any question among the various sections of the profession as to the existence of a very intimate relation between local and general morbid phenomena, and the imperative necessity of attending to it. But what we all want now is a fuller and more precise development of the nature of the relation. As Mr. Grove has shown, with equal beauty and force, the correlation of the physical forces; as Dr. Carpenter, following in his footsteps, has sought, I think with much success, to display the relation of reciprocity existing between the forces concerned in the growth, development, and maintenance of the individual organism and those which are employed in the generative act (*Mutual Relations of Vital and Physical Forces*, *Phil. Trans.*, 1850); so the medical

man longs ardently to arrive at a satisfactory solution of the many enigmas presented to him when he examines the relation of the various phenomena resulting from the disturbance of the normal physico-vital conditions. Complicated as are the relations of the forces in health, their connexion and mutual action, when a disturbance takes place, becomes yet more intricate, because their manifestations are determined by influences as varied as the physical phenomena that are exhibited in the whole surrounding world. Nay, they are more varied; for we have not to lose sight of that hidden mystery which connects us with an invisible world, and which, in our religious life, our art-aspirations, our poetic imaginings, gives us the assurance that our existence is not altogether made up of material elements.

The preceding remarks are intended to indicate both the subject to which I propose to invite your attention, and the complicated difficulties with which it is surrounded. The question which I propose to examine, as to the *localisation of disease*, may be said to embrace the entire range of medicine; and, if I should be accused of presumption in dealing with materials so vast, I trust that my excuse may be found in the desire to promote the interests of our common profession, and to suggest for your consideration, rather than myself to develop, a topic worthy of the place and of the audience.

The reciprocal relation existing between local and general disorders is one that constantly obtrudes itself upon the student of disease; but though, since the times of Abernethy, whose remarks I have previously quoted to show that he fully appreciated it, many eminent writers have ventilated the remote, predisposing, and proximate causes of morbid action, and though our chemical, microscopical, and other modes of analysis have put our knowledge on a more precise basis than it ever yet rested upon, still I am not acquainted with any treatise specially devoted to the consideration of the question now before us. The different points of view from which different men regard the same subject, the difficulties surrounding many of the statistical data required, the ambiguity of language, are some of the impediments not actually inherent in the subject, and still materially obstructing the inquirer in his search.

Let me give an illustration of my meaning. A distinguished surgeon and author has recently published the results of his studies on the antecedents of cancer; and arrives at the conclusion that, so far from this disease being traceable to a morbid taint in the body, it is essentially one of a purely local origin, and, if anything, evidence in the first instance rather of an unusually robust and vigorous constitution, than of one bearing in it the seeds of a terrific and fatal malady. He regards the formation of cancer as due "to a capability of action beyond that which suffices, and is usually employed for the perfect construction" of the textures of those persons who are attacked with cancer. The author states, that "a theory which should attempt to account for the growth of disease out of health must, from the nature of the case, render the acceptance of the fact itself precarious." He is clearly in the right, if his premises justify the conclusions at which he arrives. The mere fact of their being in opposition to the previous teaching of schools would probably be an additional motive for promulgating them. With his facts and statements it is not my object to deal. I

quote his conclusions with a view to demonstrating that the difference between himself and his opponents may be resolved more readily into one of words than at first sight appears. It is not shown that external irritation, traumatic or otherwise, causes what is called a local disorder; but the morbid product is referred to a constitutional influence. This, however, the author does not regard as a morbid taint, but as an excess of strength and health, which results in well-marked local disease; and whether we term that a hyperplasia, or whether we regard it as a heterologous product, signifies nothing in the argument. After all, the author admits a constitutional predisposition; but he is unable to reconcile his experience of the progress of the disease with the ordinary terminology of science, which would term it morbid, though it disguise itself under the semblance of health.*

The illustration which I have given of the difficulties involved in language alone almost necessarily leads us to the question as to what may be termed the local and what the general phenomena of disease. Is the distinction a just one? Must one necessarily lead to the other? or may we have in the human body either a purely local or a purely general disease?

While, on the one hand, "specialisation of function" is a characteristic of higher organisms, on the other we find that, as we mount in the scale of creation, the links which bind the various elements of the body into one become more numerous, and their mutual relations and affinities more intimate. Though the simple cell is equally the basis of all organisation, the differentiation of structure and function accompanying development is so great that it is not surprising to observe how late in the history of science the discovery of cell-growth was made. This differentiation establishes a new order of things; and we must be careful not to confound, as there sometimes appears a danger of our doing, phenomena occurring in and produced by the primordial cell, with processes occurring in the complicated arrangements resulting from many successive stages of growth and development. It is well said by the most eminent of living pathologists (Virchow, *Cellular Pathology*, Lect. 1), that "every animal presents itself as a sum of vital unities;" but these unities have no existence except in their combination into one whole, which constitutes the individual, and which is ruled and guided by the principle of Life.

Earlier in the lecture, I alluded to the possibility of physicians erring in the direction of localising or limiting too much the operations of morbid action.

* I may mention incidentally, that the interesting paper of Dr. Fenwick, in the last volume of the *Medico-Chirurgical Transactions*, promises a solution of the difficulties involved in the apparent contradictions between different writers on cancer. Dr. Fenwick investigates the morbid changes in the stomach and villi in persons who have died of cancer, and finds the former to be seriously diseased in 37 per cent.; while "in nineteen cases in which the intestines were examined, and in which the gastric tubes were normal, morbid changes were detected in eighteen, so that we have reason to suppose that few cases of cancer die in which serious anatomical change of a non-malignant nature is not present in some part of the digestive canal." If I understand Dr. Fenwick aright, he holds that the morbid condition of the gastro-intestinal mucous membrane is antecedent to the cancerous affection; and if this view should be confirmed by further observation, it may assist in reducing the phenomena of cancer to the simpler denomination of a definite nutritive disorder. At all events, Dr. Fenwick's researches indicate the direction which our labours must take in order to get some further clue to the ultimate cause of cancerous disease.

This would appear to be the tendency of at all events that school of pathology and physiology which lays overgreat stress upon the phenomena occurring in certain cells, or, as Virchow terms their congeries, cell-territories. The error in the opposite direction, and one characterising the transcendental schools of medicine, consists in overlooking and ignoring the operations of disease as manifested in individual parts of the economy, and seeking for them only in the system at large or in those functions and tissues which appear to be claimed as common property by all individual organs.

In order to obtain a correct reply to the inquiry as to the relation of local and general disease, we cannot do better than to examine what occurs in the healthy body. If we desire to understand the phenomena of respiration, we are compelled to take the lungs of the human being in their perfect development and examine their anatomical structure; and then, by analysis and experiment, ascertain the manner in which the air is received into them and how the interchanges are effected in the air-cells. A mere examination of the early embryonic stages of the lung-tissue would be useless for this purpose, though it might serve to elucidate points of obscurity left after the former exploration. In the same way, if we seek to know the uses of the liver, we do not first go to embryology to determine the manner in which bile is secreted, or how bile aids in the digestive process; but we examine the various phases of the viable human being, and employ the vivisections of animals of a high organisation to explain the anatomy and functions of that organ. In making these inquiries into the functions of individual organs, we are necessarily careful to include in them the relations of the vascular and nervous systems, and the bonds of union between them and the general organism.

In a similar manner, our researches into the perversions from the normal condition, the diseases of individual parts of the human body commencing with the site of injury, compel us to regard each individual organ, not so much in its elementary parts and in its earliest or embryonic conformation, but as a whole, capable of performing the duties assigned to it in the independent being. We have then to trace back the processes to the vascular and nervous system; and thus in both instances we may frequently, if not invariably, arrive at the conclusion that however much "cell-territories" or individual organs are at fault, we have an earlier fault of nutrition to deal with, not necessarily or primarily involving the particular organ in question.

This is in direct opposition to the tenor of Virchow's teaching, if I understand him aright; for he denies that the real seat of any dyscrasia can be in the blood, and holds that every dyscrasia is dependent upon a permanent supply of noxious ingredients from certain sources, which he places in definite diseased organs. I have not so read the phenomena of disease; and shall try in the short space of time at my command to give the reasons for the belief that is in me. Nor should I wish to be supposed to enter upon a controversy with Virchow, for whose views and researches I entertain the profoundest regard. I quote them, merely because among the scientific medical men of the age he holds perhaps the highest rank; and because they appear to me in the most stringent manner to put before us that

special tendency to localise morbid action that I have alluded to.

It is not improbable that this desire, which is but an expression in a new form of the ancient bias of all "practical" men as distinguished from "theorists", resulted from the antagonism brought about by a different school, which made dyscrasia their shibboleth. Dr. White of Newcastle, in a recent address, reminds us that medical men have a great tendency to bestow all their attentions upon individual organs, and that there is generally a sort of epidemic attachment for one or the other organ of the body to the exclusion of the rest.

"How strange," he says, "is the tendency that medicine has at certain periods to throw on special organs every good or every evil. One time the brain, at others the spine, the stomach, the liver, the womb in females, and even the teeth! Latterly, the skin has been gaining an ascendancy, and its habitual stimulation by temperatures unknown to Nature in her hottest moods has been deemed advisable." (BRITISH MEDICAL JOURNAL, Dec. 2nd, 1865.)

The fault that Dr. White points out, is one that an enlarged capacity for estimating the varied conditions of the body and the multiple influences to which it is subjected will obviate and correct. Only we must be careful, in our estimation of the importance of a physical basis for our conclusions, not to be led into ignoring the fact that there are many things which we never shall be able to demonstrate to the eye, which are yet proved to exist by evidence that we dare not reject. Mr. Simon, for instance, in his lucid and suggestive *Lectures on General Pathology*, details his experience with regard to the influence of nerve-lesions bringing on lesions at a distance from the seat of injury. Although it would be probably impossible in these cases to show the actual lesion of the intervening nerve-tissue, it would be unreasonable to refuse to admit the justice of the conclusion, borne out, as it is, both by previous physiological experiment and frequent experience of the clinical observer.

"Several years ago," he says, "I was consulted at King's College Hospital by a man who, some months previously, had torn his ulnar nerve at the inner condyle. His two fingers had become swollen and livid from vascular injection. Two years ago," he continues, "I had a female patient here, with disease in a large portion of the lumbar and sciatic plexus of nerves on one side, causing paralysis and anæsthesia of the limb; neuralgia was referred specially to the vicinity of the knee, and at this spot ulceration had occurred."

I forbear from continuing the quotation, which bears upon the occurrence of pneumonia as a consequence of nerve-lesion, because I shall go more fully into this subject at a later period; but I give the preceding extract from a surgical author, because it offers an analogue to what, I apprehend, is constantly occurring within the body, only not so readily appreciable. A lesion is observed in a certain locality or "cell-territory", which, taken by itself, is unintelligible, but, if viewed as a change of nutrition dependent upon the propagation of a morbid condition from elsewhere, is brought into harmony with our general knowledge of the functions of the body. Many years ago, a case fell under my observation, which I may mention as a still more striking illustration of a morbid influence propa-

gated to a great distance, though probably no amount of microscopical or other analysis would have been able to demonstrate an actual change either in the blood or in any tissue intervening between the original site of injury and the more recent lesion. A cooper had, many years before coming under observation, severely cut his right hand, in consequence of which the ring and little finger became permanently bent and contracted. In order to restore the symmetry of the parts, but certainly, as we may fairly say, from a morbid irritation propagated from the right hand, after a time the corresponding fingers of the left hand became fixed and contracted similarly to their fellows. Here was certainly no congestion or ulceration, as in Simon's cases; but there was not the less a serious injury, for which no legitimate mode of reasoning could have assigned a ground in the parts themselves.

[To be continued.]

DISCOVERY OF TRICHINA. Dr. Cobbold thus sums up his views on this disputed point: "1. Professor Owen first described and named the flesh-worm (*Trichina spiralis*). He first interpreted the true nematoid nature of the parasite. From a zoological point of view, he is the discoverer of trichina. 2. Mr. Paget, according to the testimony of Owen, 'at the same time satisfactorily determined the existence of the entozoon, with the assistance of Mr. Brown and Mr. John Bennett, at the British Museum.' Probably, they actually saw the worm before Owen. The Professor's candid statement not having been repeated in the subsequently written article 'Entozoa' in Todd's Cyclopædia, has called forth the remark by Pagenstecher (*loc. cit.*, p. 6), 'Hier nennt sich Owen als Entdecker und verschweigt Paget ganz.' 3. Mr. Wormald had 'more than once' previously noticed the characteristic specks 'in subjects dissected at St. Bartholomew's Hospital. He transmitted the individual specimens by which Owen made the discovery. 4. Mr. Hilton was the first to suggest the parasitic and animal nature of the specks observed in human muscle. He anticipated Wormald in his observation of the 'gritty' particles in dissecting-room subjects, and described the bodies as 'probably depending upon the formation of very small cysticerci.' 5. According to Dr. Hodgkin 'the first observation of these little bodies was made in 1828' by Mr. H. Peacock. The latter made a dried preparation of the *sterno-hyoideus* muscle to display the specks. That preparation is the oldest in existence, and may be seen in Guy's museum. 6. Henle, Diesing, Küchenmeister, Davaine, and Thudichum have pointed to a notice by Tiedemann as probably, or possibly, indicating a prior observation of the specks. Leuckart rejects the evidence. Pagenstecher appears to be in doubt as to the nature of the bodies in question. I entertain a persuasion that they were calcified trichina-capsules. 7. Herbst was the first to rear muscle-fleshworms, or encapsuled trichinae in animals by experiment (1850); and Virchow was probably the first to rear and recognise sexually mature intestinal trichinae by experimenting upon a dog. (*Deutsche Klinik*, 1859, s. 430.) 8. Leuckart was the first to offer a full and correct solution as to the source and mode of genesis of the fleshworm (1860). He disproved the erroneous views of Küchenmeister. 9. Zenker opened up a new epoch in the history of trichinal discoveries. He first observed the young in the act of migration, and was the first to demonstrate that these parasites were capable of giving rise to a violent disease in the human body."

Illustrations

OF

HOSPITAL PRACTICE:

METROPOLITAN AND PROVINCIAL.

BIRMINGHAM GENERAL HOSPITAL.

CASE OF PNEUMOTHORAX OCCURRING AT THE VERY COMMENCEMENT OF TUBERCULAR DEPOSIT IN THE LUNG.

Under the care of JAMES RUSSELL, M.D.

THE case which I am about to relate illustrates in a very interesting manner the accidents of disease. It exhibits a malady which ordinarily belongs to advanced disease of the lungs, presenting itself in the very earliest stage, and hurrying on a fatal termination, which would have been postponed, perhaps, for years under ordinary circumstances.

Excluding the exceptional cases wherein pneumothorax occurs in connexion with a perfectly healthy lung—an occurrence most remarkably exemplified by Dr. Gairdner in the opening of his article on this subject (*Clinical Medicine*)—the rupture of the pleura occasioning the pneumothorax is usually deferred until at least the second stage of phthisis, with which disease it is associated in the immense majority of cases; Dr. Walshe (*Diseases of the Lungs*, 1860, p. 307) says, probably in 90 per cent. Instances, however, are by no means wanting in which, as in the present case, from the accident of position in the pulmonary disease, and from absence of adhesion between the lung and the wall of the chest, rupture has coincided with the very commencement of tubercular or other disease. Thus Dr. Gull has related a case in the *Medico-Chirurgical Transactions*, in which two very small secondary abscesses, situated at the surface of the lungs, burst into the pleural sac and occasioned pneumothorax; and Dr. Gairdner (p. 406, note) mentions a similar case, in which a number of very small bronchial abscesses had formed after typhoid fever. Louis (*On Phthisis*, Syd. Soc., p. 336) gives a case of pneumothorax occurring only fifteen days after the first symptoms of tubercular disease; and in another case (323), although extensive disease of the lung was present (from the description, probably cirrhosis), the pneumothorax resulted from "a yellow round spot, one line in diameter, corresponding to the situation of a softened tubercle, partially evacuated and burst into the pleura." Alluding to instances of pneumothorax of very early occurrence, Louis says (p. 342): "Cases of this kind" (referring to the one alluded to above) "are fortunately very rare; and this is the only one I have myself observed in the course of twenty years, and out of upwards of six hundred phthisical subjects whose bodies I have opened."

In my present case, the entire absence of any history of chest-disease until a very recent period, the healthy condition of the opposite lung so far as physical examination went, and the character of the expectoration, all coincided with the appearances after death, and led to the formation during life of a correct opinion as to the nature and extent of the morbid changes.

An interesting feature in the case—the obscurity of the early symptoms attending the rupture—supports an observation made by different authors, to the effect that, although the symptoms which mark the occurrence of perforation of the lung are usually