

REVIEW.

A Manual of General Pathology for Students and Practitioners. By WALTER SYDNEY LAZARUS-BARLOW, B.A., B.C., M.D., M.R.C.P. London: J. & A. Churchill. 1898.

THE above work will always prove instructive reading to the advanced student, the intelligent biologist, or the scientific practitioner. It is questionable, however, whether it will ever become very popular with ordinary students of medicine; and the reason for this is not far to seek. Where so much that is theoretical enters into the composition of a text-book, and where there is so much tendency, as there is in this, to generalise, a sense of bewilderment must inevitably be the outcome of its perusal, and more particularly if the necessary facts on which such theories and generalisations are based have not previously been mastered by the reader. Indeed, such a book, in the present unsettled state of the science of pathology, must be regarded somewhat in the light of an anachronism. Theories are to most people pleasurable, and the temptation to push beyond the facts on which they should be founded, especially in the case of a subject so transitional as the science of disease still is, often leads to a dogmatism which is bound to be of ephemeral existence. No doubt theory has done much for the sciences. What would the science of chemistry be without the flights of the imagination? Yet, where the theory has been lasting and has been shown in the long-run to be true, it has not been the pioneer but the handmaid of knowledge which has resulted from painstaking labour. It is questionable whether many of the theoretical speculations in the medicine of the past have been raised on such a substructure, whether they have been the natural outcome or extension of such ascertained data. The great fallacies in medicine have been occasioned by drawing conclusions from insufficient knowledge and airy and unstable observation. The element which survives and which has become a treasure to us has not been evolved by the methods of the speculative philosopher, but has been the fruit of patient experimental inquiry and well-grounded observation. Harvey's work will ever remain a monument of what the method of elucidation of the problems of biology ought to be, and the work which will live in futurity must run upon the same lines. A common understanding must be arrived at, verified, and strengthened by the test of the lapse of time, as to what is to be accepted as fact, and what is not, before we can enter the seductive realm of theory, or attempt to generalise. Have we reached this stage in pathology? Have not all the recent discoveries in this department of biology really been the result of a groping in the dark? Must we not confess that the science is so far inexact that we have not as yet discovered the guiding lines competent to found an inductive method upon? Take, for instance, perhaps the greatest discovery in medicine of recent times, that of the tubercle bacillus and its

properties. How was that accomplished? It might be replied—almost entirely by accident. No amount of reasoning from previously ascertained laws would ever have shown that it is an organism which is most fickle in its mode of growth, that it gives certain remarkable staining reactions, and that the laws regarding its inoculability differ from most other organisms of the same class. Hence I am always rather sceptical of the appropriateness, as yet, of such a work as the present, purporting as it does to reason from data of whose stability we are not always convinced. Notwithstanding this, however, the prevailing tone of the book is excellent; there is a charm, as there ought always to be in such a work, in perusing its pages.

The title is one with which I have not much sympathy. "General Pathology," as applied to the subject of "Pathology in General," is a term which in olden times had a specific enough meaning, but which with the altered aspect of affairs contingent upon the progress of the last fifty years, ought surely to be allowed to pass into disuse. The term "Institutes of Medicine" was at one time employed as practically synonymous with that of "Physiology," yet few would think of using it at the present day; nor do we speak of "General Physiology" as something apart from "Physiology" in the abstract. If pathology be the science of disease, then surely we should have some accepted understanding as to its scope, and have done with what is essentially a relic of a time when pathology constituted simply a branch of theoretical medicine. Just as physiology is looked upon as the summation of the facts, structural, functional, chemical, etc., of the normal organism, so it seems reasonable to conclude that pathology ought to be the summation of such facts, from like points of view, of the organism in a state of disease. There is no real difference between the two, for given an organism under the influence of a noxia, no matter what the nature of that may be, we can quite well study the physiology of such organism in terms of the modifications the noxia induces upon its structure and functions.

The author evidently regards "General Pathology" as synonymous with "Experimental Pathology." This was by no means the original meaning of the term, and is one which certainly will not be accepted by the majority of physicians. He is rather unfortunate (p. 5) in illustrating what is meant by general pathology, for, as he says, "Just as general pathology teaches us to concentrate our attention locally to the cure of simple ulcer" (say, on the leg), "so it teaches us that adoption of the same course is more than useless in the case of the syphilitic ulcer. For the simple ulcer is the prime disease, the syphilitic ulcer is merely symptomatic." I need hardly remind him that mere local treatment of a simple ulcer will in most cases prove inefficient, if general treatment, such as that of the relief of blood pressure in the affected limb, be omitted, and that local treatment of the syphilitic ulcer with, say, "black wash" has the most beneficial influence upon it. The ulcer of the leg is in most instances not a local affair, but the result of a diseased state of the vessels or of a low state of nutrition, quite comparable with that induced by the syphilitic poison.

The book is modelled very much on the principle of Cohnheim's "Lectures on General Pathology," and many of the subjects discussed, such as those of bacteriology, the pathology of the circulation, inflammation, dropsy, organisation, and nutrition are of primary interest.

The chapter on vegetable micro-organisms, although sketchy, touches upon

some of the most vital points bound up with bacteria and disease. The author seems to think that the cause of growth of the tetanus bacillus on a wound, anærobie as the organism proves to be in artificial cultures, is the immediate proximity of other organisms which use up oxygen. Without denying this influence, it is hard to see how an anærobic organism lying, say, in an open wound does not receive as much oxygen as any of the other organisms which are associated with it; and the difficulty becomes, to say the least of it, perplexing, if it be the case, as he assumes, that "no anærobic micro-organism is capable of multiplying under aërobic conditions," and that "even if more than the minutest trace of oxygen is present, the anærobie completely refuses to grow" (p. 20).

In the chapter on "The Pathology of the Circulation," etc., a certain ambiguity is apparent (p. 48), which the author doubtless did not intend to convey to the reader's mind regarding the nature of inflammatory lymph. The impression conveyed to the ordinary reader would be that the fibrin of inflammatory lymph is something quite different from blood fibrin. For my own part I fail to see in what the difference consists. The plasmatic liquid from which it is precipitated exudes primarily from the blood vessels, and coagulates or not according to circumstances. No doubt it contains in most cases an excess of colourless corpuscles, but not always so. It is sometimes almost pure fibrin, and presents the same fibrillated appearance as fibrin taken directly from the blood. That it is a chemical precipitate, due to the reaction of one chemical constituent upon another, does not seem to be sufficiently insisted upon, and its relationship to organisation is not sufficiently defined. The use of the term "lymph" in a pathological sense, as contrasted with that of "normal lymph," is not perfectly clear, and when he states (p. 54) that "lymph becomes organised, *i.e.* becomes converted into fibrous tissue," he takes us back to the pathology of last century. This, however, is quite unintentional, as is apparent from the context.

In writing on valvular disease of the heart (p. 58) he refers to the adhesion of adjacent flaps of a valve, as if the occurrence of such a phenomenon were an admitted fact. I would ask, in the first place, whether the author has ever seen indubitable evidence of this, and, in the second, whether he considers such a thing at all likely. Is it probable that two surfaces in constant movement, and separated by the liquid and ever changing blood, would incline to become adherent? If so, then the laws regulating adhesion, as applied to the valves of the heart, must differ from those regulating adhesion of any other surfaces of the body with which we are acquainted. Would, for instance, two flaps of a wound tend to adhere, if placed under like conditions? We know, on the contrary, that to induce adhesion of such surfaces, the closest apposition and undisturbed rest are necessary. Pericardial or pleural adhesions take place only after the liquid separating the two layers of the sac has been absorbed, and when the two lymph-coated surfaces are brought into contact. In the case of the valves of the heart a constant separation of the surfaces is occurring, a condition which is directly inimical to the establishment of adhesion.

If there is one matter more than another which has been the subject of misstatement in cardiac pathology, it is that of the effect of valvular disease upon the size and thickness of the walls of the cavities. Nothing but actual measurements, carried out over a long series of instances, will suffice to correct

these errors. Loose statements, very often founded upon the mere casual recollection of the observer, will certainly conduce to error, and have done so in the past. In treating of the subject of hypertrophy and dilatation of the left ventricle it seems to me that sufficient prominence is not given to the factor which of all others is calculated and actually does occasion the greatest amount of these lesions, namely, the regurgitant influence of an incompetent aortic. The deleterious influence of this defect upon the ventricle is owing to the rebound of the aortic blood upon the inside of the ventricle at a time when it is relaxed and is capable of expansion. So great is this that, when in an animal the valve is suddenly broken down, actual aneurysm or rupture of the wall may ensue. The belief, also, that when a heart once becomes dilated from irreparable valvular defect it may again contract to normal size (p. 69) is open to question, and the general arguments bearing upon the causes of hypertrophy would have to be reconsidered. What is the overwork, for instance, which the heart has to perform in, say, a widely regurgitant aortic? Is it entirely that of driving onwards a greater mass of blood? Or, is part of the overwork not comprised in keeping up the tone of the heart, and preventing distension beyond a certain limit? In free aortic regurgitation, the dilatation is primarily an effect of the rebound of the arterial blood upon the ventricle while in a state of diastole. The orifice being wide, the blood will be driven out of the ventricle with facility equal to or even greater than that of health. Why, then, does the distension cease at a particular period? Why does it not go on progressing up to a point when actual rupture might take place? Is it not a possibility, nay a very great probability, that the increase of the muscular fibre prevents this, that towards the end of diastole the heart fibre, in beginning to contract, prevents unduly great and progressive distension, and that this constitutes a great part of the overwork to be performed? In stricture of the urethra, or other form of obstruction in the lower urinary viæ, the bladder hypertrophies, and the usual explanation given is that increased propulsive power is required to eject the urine. This has always seemed to me to be questionable doctrine. In fact, it is a matter of opinion whether the muscular fibre of the bladder, either under normal or abnormal conditions, has much to do with micturition. A woman's bladder is practically as large immediately after micturition as before it; a sound at least will pass into the cavity to just about the same distance. The bladder hypertrophied from such a cause as we have supposed, a stricture or other obstruction, is also a dilated bladder; and it seems reasonable to suppose that part if not the whole of the overwork discharged by the muscular fibre is that of supporting the immensely increased mass of urine within it and thus preventing rupture. Among the fallacies connected with the subject of hypertrophy of the heart is that of supposing that the left auricle hypertrophies readily in valvular disease of the left side. As a matter of fact, hypertrophy of either the right or left auricle is of rare occurrence in any valvular disease, and for confirmation of this statement it is only necessary to pass in review a sufficient number of examples. The subject is referred to (p. 71), and the author, although granting that in mitral stenosis the auricular wall is thicker than it should be, rightly draws attention to the fact that the hypertrophy is only moderate.

The statement (p. 75) that "so far as the lungs are concerned any morbid condition which impedes the circulation through them leads to changes in the right ventricle," and that these changes are comprised in dilatation and hyper-

trophy, would have to be modified. Many extremely obstructive diseases of the lungs are unaccompanied by either dilatation or hypertrophy. I may mention tubercular phthisis, fibroid lung, stonemason's lung, by way of example. In these it is exceptional to find either the one or the other. Even the ancient bogie, "emphysema with bronchitis," referred to by the author as so productive of these conditions, will be found on the average to cause far less of either the one or the other, or both, than is generally supposed.

The miserable old mixty-maxy jumble with reference to infarctions of the lung, kidney, spleen, etc. etc. is given fully, and with few modifications. I shall not enter upon its discussion—I have a liver.

Before leaving the subject of the heart and blood vessels, let me ask the author whether, as a matter of personal observation, he has found an increase in the amount of fibrous tissue in the liver, kidney, or spleen, dependent upon a pure venous congestion from valvular disease. I certainly have not, and others have had a like experience. The affirmation of such an occurrence is one of the pet sayings which loosely pass muster for gospel in affairs pathological, and without question.

The chapters on inflammation and dropsy are interesting and are well written. The remarks upon œdema embody virtually the substance of the author's papers on this subject in the *Journal of Physiology*.

The assertion (p. 515) that "the iodine reaction" with amyloid "is lost at once if the tissue affected with the lardaceous change is treated with alcohol" must surely have been an overlook, and the statement probably does not express what the author intended to convey. One of the most elementary facts, which every tyro in pathology learns, is that amyloid organs may be kept an almost indefinite time in alcohol, and still retain their power of giving the amyloid reaction with iodine unimpaired.

The subjects of atrophy and hypertrophy are unsatisfactory, and would require to be thought over and cut down. Before discussing the causes of a condition, surely it is advisable to state what is meant by that condition, and when the reader finds that "in general terms, it is said when a part is smaller than normal that it is atrophied," the statement does not inspire confidence in what may follow. Either define the condition in terms scientific, or say nothing about it. Then ensues the usual string of jargon associated with these two morbid conditions, "true atrophy," "true hypertrophy," "physiological hypertrophy," "pathological hypertrophy," and all the rest of it. Ribbert's "tissue tension" theory of growth and hypertrophy is discussed, but without reference to earlier works upon the subject.

I have drawn attention to what I consider some of the questionable points in the book, but in a suggestive rather than in a captious spirit. As I remarked in the beginning of this review, the treatment of the subject matter is fascinating reading. I have only further to add that it shows a keen perception of the salient points in the subjects discussed, and reflects no small credit on the industry of the author. If I still further add that the style in which the book is written is easy and flowing, and that the typographical errors are uncommonly few for a first edition, I trust that the author will consider that I have not treated him from a biassed point of view.

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