

as a matter of course, always admitted to the scarlet fever block, since the question of the presence of diphtheria bacilli in the throats of cases of scarlet fever on admission prior to 1901 was never even thought of in connexion with the administration of most hospitals, much less systematically sought for by bacteriological examination of throat swabbings. Prior to the inauguration of this scientific control of the new admissions post-scarlatinal diphtheria frequently occurred, as it did also during the interim which elapsed before laboratory details were complete under the new régime.

There can be no doubt that too much money has been spent in many places on very elaborate fever hospital buildings where the money would have been better spent in securing the full-time services of an expert fever hospital medical superintendent with bacteriological knowledge, experience, and equipment, who would so have instructed his nurses as to secure enviable results in a hospital of a more modest design than many municipal hospitals in existence. I advocated the use of glass partitions in wards in a paper on "Fever Nursing" in *The Hospital* in 1901, but ripper opinion is outlined in the last sentence quoted above from the later paper in 1909.

I am, Sir, yours faithfully,

Norwich, Feb. 19th, 1910.

J. T. C. NASH.

TUMOURS OF BILATERAL ORIGIN.

To the Editor of THE LANCET.

SIR,—In reply to your annotation on my writing *re* bilateral tumours in THE LANCET of Feb. 19th, p. 515, I would like to offer a few comments.

In the first place, let me remark that truth unsearched for seldom comes to light, and that facts often appear incredible only because we are ill informed. Even in the learned professions most people nowadays think very little and very seldom; yet all have strong beliefs, opinions, &c., which are determined for them mainly by the bias of the moment, which is often hardly distinguishable from the folly of the moment. The sense of organic continuity which would often suffice to correct these divagations is only too often lacking. Medicine, which is neither an exact science nor a perfect art, has for this reason always been specially prone to this kind of infirmity. Hence Celsus long ago remarked: "It is a good thing to be free from the bias of an education exclusively professional."

The present unsatisfactory condition of medical knowledge is mainly due to impediments of this kind, for faulty bias prevents pathologists and practitioners from assimilating the enormous array of new facts, data, and general conclusions which are now available for progress but unutilised. Owing to the frequency of such lapses the history of medicine teems with might-have-beens. If the best results are to be attained the profession must coöperate and sympathise with its pioneers; for the greatest discoveries fail to develop and fructify when the spirit of the age is apathetic, ignorant, bigoted, "opaque of nature and of soul," money-grubbing, or otherwise out of joint. Moreover, it cannot be too strongly insisted that the science which investigates the facts and laws of disease is something quite different from the arts applied to cure the same, and from the fads and fancies of the latter it must be completely emancipated.

It is to the crosslights derived from the neighbouring sciences, especially from the biological sciences, that we must look for the advancement of medical science rather than to clinical experience. Above all, it is to the biological sciences that we must look for true methods of research rather than to clinical experience, which of itself inevitably tends to empiricism, stagnation, and retrogression. For instance, biologists having shown that life is the continual adjustment of internal arrangements to external arrangements, it follows that disease, which is but an unusual manifestation of life, must be similarly conditioned. Strange to relate, this cardinal law of pathology, which is of universal application, finds hardly any recognition in the writings of modern pathologists. Hence the whole of pathogenesis needs to be rewritten from this standpoint; for the conditions of existence constitute the chief factor in the causation of disease, whatever may be the nature of the spark which precipitates the explosion. The first attempt to apply this method to the investigation of disease is embodied

in my recently published book on the Natural History of Cancer, which has thus introduced a new note into pathology.

Now, in the matter of tumour pathogenesis, professional bias has taken the form of a blind, unreasonable, and invincible prejudice in favour of the microbic origin of tumours, which will not tolerate a single suggestion in the contrary sense. The violence and fury of this fanaticism has lately been brought home to me by the stream of abuse which its votaries have poured on my book, because I have declined to believe in their fetish; and this although in my book I have given far more attention to their theories, facts, and experiments than they intrinsically merit, out of deference to their being in what may be called the order of the day.

I fancy some dim echo of this miserable bias must quite unconsciously have affected your correspondent, who has so politely criticised my recent writing and belauded it with manifold dubitations. With regard to the bilateral origin of malignant tumours he says: "There is always a possible objection that the growth on one side appearing first has given rise to the growth which appeared later on the opposite side." Just so; but, as far as I could determine, all cases of this kind have been rigorously excluded from my list. For instance, in Cohnheim's case of bilateral rhabdomyosarcoma of each kidney, the appearances were such that it seems to me unreasonable to doubt the independent origin of each tumour; and this is typical of the cases comprised in my list. But, even supposing that error has occasionally crept in, the number, variety, and authenticity of the instances of the bilateral outbreak of malignant disease there cited is too considerable to be thus explained away, even in the absence of collateral evidence pointing in the same direction, of which, however, there is an abundant store on hand, although, for considerations of brevity, I have not drawn upon it.

Then, concerning the bilaterality of fibromas of nerves, your correspondent says: "We fail to see much force in this example." But surely this multiplicity—when dissemination can be excluded, as it can in these cases—is a remarkable and very striking peculiarity, considering the solitariness of most tumours; and it is just such occurrences that prove tumour formation to be "something more than a solitary local aberration." Moreover, in this affection it has been repeatedly shown that the multiple tumour formation takes place in certain spots where the development of the neurilemma is imperfect, owing to antenatal developmental disturbance; and it is just in these localities that malignant disease is apt to supervene. I submit that such indications, which are by no means rare, are not without a certain significance for the pathogenesis of tumours; and, in nearly all cancers, the impress of the prenatal stamp can be found when it is thoroughly looked for.

I am, Sir, yours faithfully,

Feb. 19th, 1910.

W. ROGER WILLIAMS.

THE STANDARDISATION OF AXIAL NOTATIONS.

To the Editor of THE LANCET.

SIR,—Dr. W. Ettles in his friendly criticism of my method of the axial notation of cylindrical lenses in THE LANCET of Feb. 26th, p. 602, has overlooked the main point. The question involved is not that of deciding upon or endorsing a "best" method, but of establishing a standard which is known to ophthalmic surgeons and opticians throughout the world. The fact that the Optical Society has adopted the protractor as the basis of its method does not explain why the horizontal has always been made the basal line from which the axes of cylindrical lenses have been delineated. But as between the compass and the protractor, which is the better, more universally known instrument? The answer must be—the compass. The compass, therefore, is undeniably the best instrument upon which to base a standard. The so-called "standard" of the axial notation of cylindrical lenses in this country is not a "standard" recognised by foreign ophthalmic surgeons and opticians. Let us suppose, for example, that a prescription for cylindrical lenses has to be cabled to America. It would be of little purpose sending a message such as the following: R.E. + 3 cyl. axis 30°, L.E.