

Correspondence.

"Audi alteram partem."

THE LUMLEIAN LECTURES AND MEDICAL RESEARCH:

THE NEW SCHOOL.

To the Editor of THE LANCET.

SIR,—Controversy, above all with an old friend, is distasteful to me; there is something more to be said, but this shall be my last word.

It seems I was wrong in my definition of the "old" and "new" schools; it is not that the old is the clinical, and the new school that which, armed with the new and powerful methods of polygraph, X rays, electro-cardiogram, and so forth, has recently laid us under lasting obligations. Sir James Mackenzie retorts that "it is the hospital and laboratory workers" (the physiologists and pathologists) that "blind" as they are, have "dominated the profession" and given themselves insufferable airs. These are the "old" school. The new school, as with some candour he gives us to understand, is Sir James Mackenzie himself. As such, he declares, for instance, that "the medical profession" took up "recruiting, utterly unconscious of the fact that they did not possess the necessary knowledge." "Had the Royal College of Physicians itself taken up the duty" (i.e., every Member and Fellow except, of course, Sir James Mackenzie himself) "the same disastrous results would have followed because the necessary knowledge does not exist" (though the new school is not without some good notion of it). Alas! this accusation is but too true. We all, unless always for one of us, are far indeed from any certain and comprehensive understanding of that last and crowning miracle of creation, the human body. We have been, it is true, modestly thankful that we had penetrated a little way, and learned something, in two thousand years; but we are chastened by the verdict of the new school that our meddling with recruiting has been "disastrous." Not that the medical officers engaged, consisting of persons of various skill and experience, occupied on an unwonted task of enormous difficulty, and driven under terrific pressure, have made mistakes, this interpretation would be too generous to them; they would have been better away, their interference has been simply "disastrous." Their fault was sheer "ignorance of the amount of work a man in impaired health is capable of undertaking." Why, let us take a simpler case: In a girder of a bridge a flaw is found out accidentally—a flaw subject to stresses, frosts, and rust. The engineer is ordered to state definitely "the amount of work that impaired girder" (still *in situ*) "is capable of undertaking." As the girder is "failing" all the time he can but say that if Baldwin locomotives weighing 400 tons, with trucks of like capacity, are sent over the bridge at speeds of 60 miles an hour, it will collapse pretty soon; but that if light loads and low speeds be enforced, well, it may last ever so long. But the engineer then will appear as stupid and incompetent as the "old school" physician.

We were told that we blundered because we "cannot grasp the (new) movement" and "the significance of principles"; we cannot search rightly (as the new school does) for "the mechanism by which a symptom is produced," nor calculate the ultimate effect of the primary fault. Now, to meet the new school, I set out after my antique manner one or two only of the more important of these very "principles," in order to interpret this very "mechanism." Above all things, surely the heart is a pump and the circulation a hydraulic system. I quoted therefore one or two of the elementary hydraulic principles fundamentally implied, such as the cross sections of chambers and tubes and the behaviour of viscous fluids in linear or turbulent motion, with the respective losses of kinetic energy and so forth; plain laws of dynamics taught in all our schools. That they were correctly set out, and of no fine-spun or academic character, but in this matter of immediate and fundamental importance, I have since been assured by two or three distinguished physicists and physiologists. What is the comment of the new school on my answer to its own demand? Sir James Mackenzie says, "I frankly confess I don't understand him, and refrain

from criticising"! The new school "frankly" is indifferent to the elementary laws of the functions of the pump and vascular system of whose "mechanism" it contemptuously asserts that its opponents are stupidly ignorant.

Again, Sir James Mackenzie has "searched the writings" of home and foreign authors, and "failed completely to get from any of them a clear description of what is meant by heart failure." No wonder; by no literary ingenuity can so loose and colloquial a phrase be made to stand upright or to support any precise argument. He pays me the compliment of saying that he has thus searched even my poor essays, and in vain. I am writing now, away from all books, my own or other; but if Sir James Mackenzie will look again into my *Diseases of Arteries* (Vol. II.) he will find that in my opinion to say a man died of heart failure is about as precise as the old saw that "he died of shortness of breath." No engineer would use "girder failure" as a technical term.

While scoffing at us for inattention to the origins of maladies, Sir James Mackenzie himself still hovers only over the final phases of a cardiac fault, one, it may be, of long standing, when a local has begun to extend itself into a more general disturbance. In his last letter he repeats that the first sign to be looked for is a "sensation." "Every genuine sensation," he says (what is a non-genuine sensation?) "has a physical cause in its production" (e.g., palpitation, exhaustion, &c.). Is "physical" exclusive of psychical? If not, we are left with the information that every sensation is caused; no very bright illumination. But it is in part this very psychical element which makes "effort-tests" so fallible; until, indeed, "sensations" begin to stare us in the face without their help. Sir James Mackenzie adds that "long before physical signs are evident the patient is conscious that something is amiss." This is the sort of proposition that the new school lays down—a vague proposition as often wrong as right. One longs for a quarter of an hour of the Socratic method with the new school. As to the paragraph on palpitation, if its content is valuable to the new school, the old school to a man will cheerfully do without it. I am not writing flippantly. Sir James Mackenzie is a very able man and has no doubt profounder meanings in his mind. I criticise, and can criticise, only what is written.

For farther illumination Sir James Mackenzie leaves the heart for gastric ulcer, of which, it seems, we are as ignorant as we are of heart diseases. Again he twits me personally with "ignorance of the cause and nature of heartburn"—a term surely even as loose as "heart failure" itself. Old women and others use it in various senses, but generally to signify, not regurgitation of the cleaner acid of the gastric juice, often in excess in gastric ulcer, but rather of the acrid products of the butyric fermentation; not a very abstruse process. However, in gastric ulcer "heartburn" is not a characteristic nor a very common feature—not, that is, in the ordinary use of the word. The proper pain of gastric ulcer arises when the peritoneal covering of the viscus is affected, as Dr. Hugh Anderson has explained.

Furthermore, Sir James Mackenzie writes: "The elementary knowledge for understanding pain is not yet known to the profession. We even do not know what tissues are capable of producing pain." This assertion reminds me of that in the previous letter which declared that no careful attempts had ever been made to study the behaviour of the heart in pregnancy. I then pointed out that, on the contrary, a great deal of particularly careful work had been devoted to this very subject; and now I note that this subject is silently dropped—with the "innocent murmurs." But I gladly testify that in this field, before the "new school" began, Sir James Mackenzie had done some early, original, and promising work, work which, I believe, he has laid aside for other disquisitions; but that merit cannot justify his lofty indifference to the no less valuable work of others in the sensory field, such as that of Dogiel, Henry Head, H. K. Anderson, Mott, and many another fellow neurologist. Dr. Anderson indeed has precisely demonstrated the very "tissue" which is asked for—namely, that in which visceral or other pains have their origin and seat.¹ I fear Sir James Mackenzie may again "frankly confess his ignorance" of these researches, but it is such isolation which allows an author to write without a blush: "No physician in the past had any conception of how to bring to light the information

¹ I am sorry that being away from home I cannot give the reference, but I have quoted his researches in my "Diseases of the Arteries."

behind a patient's sensations, nor knew how to set about acquiring that information"! "No physician in the past"; that is, until the new school arose!

I have animadverted more than once on the looseness of the terms and phrases of the new school, a looseness which betrays a like habit of dealing with thought, evidence, and doctrine. In a grave peroration Sir James Mackenzie, after advising the younger teachers not to lean on authority but to use their own faculties of observation and reason—a very salutary lesson but one which Sir James Mackenzie must have borrowed from the "old school"—appeals to them farther as follows: "Before you teach your students a simple fact, verify it from your personal experience, or search in the writings of your authorities for their reasons in making a statement." A "fact" is rather demonstrated than taught; alone it teaches nothing; nor is a fact a "statement"; nor again in the fact alone, uncoordinated with other facts, are there any reasons. A fact is no more a rational "statement" than a brick is a house. It is this lack of precision in terms in the new school which gives rise to much misunderstanding. And I am sorry to note a less excusable error. Sir James Mackenzie chides me for refusing for life insurance two brothers, otherwise eligible, on account of occasional extrasystoles in both. Now against this reading I had explicitly guarded myself by this clause—"or rather for stating the facts that led to their refusal." I well remember—for the coincidence was a curious one—telling these men that I had advised acceptance. It may surprise Sir James Mackenzie to hear that thirty or forty years ago the nature and significance of extrasystoles, thanks to observations of Burdon Sanderson, B. W. Richardson, Balfour, and many other physicians of the time, were as well, or as little, understood by the old school as they are now by the new.

Finally, Sir James Mackenzie knows, I hope, that no one has a greater respect for his work, and personal regard for himself than I have. We, all of us, owe much to him. If to my regret I shall have seemed to say anything inconsistent with this regard it is out of my regard for other men and other things also.

I am, Sir, yours faithfully,

CLIFFORD ALLBUTT.

Ullswater Hotel, Patterdale, Westmorland, August 26th, 1917.

PS.—In your issue of Sept. 1st is an interesting letter by Dr. E. W. Adams on the question of the adequacy of language to thought. That new thoughts may need new language or new adaptations of it, is true; but our first duty is to see that we use in a precise manner such language and terms as we possess.

To the Editor of THE LANCET.

SIR,—Two years' experience of cardiac work at a base overseas has shown that the question a medical officer has most often to answer in a "heart" case is a practical one: "Is the man 'fit' or 'unfit' for active service?" The "effort test" by Army physical drill, as recommended by the workers at the Military Hospital at Hampstead, was utilised to aid in answering this important question. During the whole of this experience the question of the clinical significance of a systolic murmur at the apex of the heart was constantly under consideration. The difficulty of estimating the myocardial efficiency by the response to effort, under circumstances where men may, even unconsciously, exaggerate their complaints, increases the complexity of the problem.

Sir Clifford Allbutt, in his letter to THE LANCET of Aug. 4th, which purports to represent the "old" school, begs the question by discussing "endocardial murmurs." The assessment of the importance of a systolic murmur is only possible when we know the cause to which it is due. To describe a bruit as an endocardial murmur in no way explains the pathology of its origin. The teaching of Mackenzie that we should endeavour to ascertain the real cause of any murmur heard, is always illustrated by his conception of the cardiac complications of rheumatic fever. Here the student in the "new" school learns the cause to be an infective process, often of slow progression, as demonstrated by the study of the infected individual throughout the rest of his life. It becomes difficult to disentangle "old" and "new" school ideas, but together they represent the tide of scientific progress. A study, with the continuity of observation that Sir James Mackenzie desires, of the "soil," as represented by the infected patient, must bear fruit equally with the researches that are conducted upon the "seed." The more

so as to-day the clinical side of the study of many chronic infective processes is just as difficult and obscure as is their bacteriological aspect.

I am, Sir, yours faithfully,

JOHN E. MACILWAINE.

College-gardens, Belfast, August 17th, 1917.

THE GENESIS OF PHOBIAS IN CHILDHOOD.

To the Editor of THE LANCET.

SIR,—In your issue of August 18th Captain W. H. R. Rivers has recorded very fully an interesting case of claustrophobia. By careful direction of the thoughts of the sufferer, the remembrance of a certain terrifying incident in his childhood was regained. As a little child he had been shut up in a closed space, unable to escape the attentions of a growling dog. A complete cure followed when the tardy recollection of this incident had provided the explanation of the origin of the phobia. I suppose it is possible that in the sensitive mind of a child a single severe shock of this kind may produce effects which persist long after the recollection of the incident has faded from memory. I have had a patient, 50 years of age, who from fear of falling had never been able to go down stairs without holding on to the banisters. In his case, however, family tradition assigned the infirmity to a fall in early childhood, and since he had long accepted this explanation, though without recollection of it himself, I was unable to exercise "psycho-analysis" or effect a cure by a demonstration of the causative incident.

Whether such forgotten experiences are or are not a common cause of neuroses I do not pretend to say, but I think that everyone who is a student of childhood as well as of Freudism, in determining the cause of a phobia, would lay the greater stress, not upon any one shocking or terrifying experience, but upon the unwise attitude of parents and nurses in focussing the child's attention upon the fear, in sapping his confidence by showing their own apprehensions and communicating them to the suggestible child. The child of himself is not afraid to fall. It is the parents who make him afraid by treating the fall as a disaster. The child of himself is not afraid of being left alone. It is the parents who undermine his confidence by keeping him always under observation from a nameless dread of what may happen to him.

Captain Rivers's analysis of his case of claustrophobia ignores this all-important point. The malign influence was doubtless here a too great solicitude for an ailing child. An atmosphere of unrest and apprehension in the nursery exerts a depressing influence upon the mental equilibrium, the physical constitution, and the whole vitality of the child, and this influence is exerted not on a single occasion only, but throughout all the years of childhood. The restoring of the incident to memory and the subsequent freedom from symptoms indicate no more than that the extreme suggestibility which was his undoing during an ill-managed childhood is still to be found in the patient. If this sufferer from claustrophobia had had full recollection of the incident, or if the family had persistently repeated the story to him, would his own study of the teaching of Freud, which we are told had been close, have sufficed to effect a cure without the stimulating influence of Captain Rivers's personality? I think not.—I am, Sir, yours faithfully,

London, W., August 28th, 1917.

H. CHARLES CAMERON.

THE RELATION OF TRENCH FEVER TO OTHER CONDITIONS.

To the Editor of THE LANCET.

SIR,—The following preliminary statement on the relations existing between trench fever, "P.U.O.," myalgia, "rheumatism," "influenza," "D.A.H." (or "soldier's heart"), and other obscure conditions accounting for an enormous percentage of the morbidity in the British Armies, appears to us to be of sufficient interest to be brought at once to the notice of the profession. The observations on which our conclusions are based were carried out on a large number of cases grouped as indicated above. We submit:—

1. That it is of the utmost importance to search carefully for the sources of toxin which may have precipitated breakdown in this group of cases.