

with it, as your Commissioner, copying the medical officer's report, proceeds to do.

Since the increase in the general death-rate was nearly all due to the epidemic of diarrhoea, it would be a waste of space to follow through this, but it appears to me peculiarly absurd for your correspondent to draw attention specially to the small fluctuations in the general death-rate to the end of the fourth week in September and then remark that "these figures clearly indicate that the exceptionally hot weather did aggravate the situation, and that the strike intensified the evil to an alarming and disastrous extent." The modest estimate is then made that for a period of two weeks a third of the deaths from diarrhoea were due to the strike. One cannot help wondering which two weeks these were, and how the "one-third" was arrived at; there is an uncanny precision about it that alarms one. The part of the strike which really interfered with scavenging for a few days occurred in the third week in August. Let it be granted that this aggravated the amount of infection and made more children ill with the disease, then these children have to sicken, pass through the course of the disease, and die before any effect is visible on the weekly death-rate. The very earliest week, therefore, in which the death-rate could show any effect due to the strike would be the fourth week in August, or more probably the first week in September. But in these weeks the epidemic had already commenced to show abatement: how then does your Commissioner arrive at his mathematical one-third of the deaths due to the strike? The only point in favour of an influence of the strike on the epidemic is the slow decline of the disease in September, but this is equally slow in Dublin where there was no complicating strike, and in both cases the most obvious reason is the mild character of the September weather.

To sum up, the epidemic was well on its way and already past its maximum before it could possibly have been influenced by the strike, and there is not one respectable shred of evidence that the strike had any influence upon it. Yet, as a consequence of this report, one finds the workmen accused in the lay press and in a vast mass of municipal election literature of massacring their own children. There are indications about that one of these days the medical profession itself may not be far from something closely resembling a strike, and much as many of us may rightly deprecate certain of the methods of the recent strike, we ought surely to have some meed of sympathy for men striking for a reasonable living wage for themselves and their wives and families. In any case, from the purely professional point of view such scares raised upon entirely inadequate evidence are, in my opinion, much to be deprecated.

I am, Sir, yours faithfully,

Liverpool, Oct. 30th, 1911.

BENJAMIN MOORE.

DR. CUTHBERT MORTON'S "PRINCIPLES OF ANATOMY."

To the Editor of THE LANCET.

SIR,—With reference to the review of my work, "Principles of Anatomy: The Abdomen Proper Described and Illustrated by Text and Plates," which appeared in THE LANCET of Oct. 21st, I should like to say that when a scientific work, bearing the visible imprint of years of hard and conscientious labour, is submitted to a scientific journal for review the reviewer is under an obligation to furnish the author and the publisher of the work, as well as the editor and the readers of the journal, with a scientific expert criticism, that is to say, an accurate and clear statement of what the work aims at, how far it attains its aim, and what scientific worth or practical value it possesses or is likely to possess either in its present or in any probable future form. Casual criticism is entirely out of place.

Your reviewer has noted some resemblance in appearance between the "Principles of Anatomy" and Snellen's test-types. I should like to draw attention to a certain resemblance in function also, for just as the keenness of a man's physical vision may be tested by learning from himself how much he is able to see in Snellen's test-types, so the keenness of a man's mental vision may often be gauged by learning from himself how much he has been able to see in a work which he has reviewed. Your reviewer has seen in the "Principles of Anatomy" only "14

drawings"; your readers will find 14 front-and-back plates with at least two drawings for each. He has seen only "outlines"; they will be struck by the details. He has seen that "the outlines all coincide, and when held up to the light in the special frame provided may be superimposed and viewed as transparencies"; they will perceive that outlines which all coincide may be superimposed but cannot be viewed as transparencies with profit. He has seen only "various organs in their relation to each other and to the surface of the body"; they will perceive that, *inter alia*, every organ of the abdomen proper can be studied in its surface anatomy, its parietal, visceral, and peritoneal relations, and its blood- and lymph-vascular and nervous supply. He has seen in some pages of the text an appearance "irresistibly suggestive of Snellen's test-types"; they will perceive that these pages are to be found only in the Table of Contents, where their "strange variety of type" serves a definite and useful purpose. He has seen, quoted, and criticised what "the author says of the purpose of the work"; they will perceive that in their original context on p. 6 of the Principles the words quoted are strictly relevant, referring as they do in the clearest and most unmistakeable manner solely to the presentation of the body by plates. The introductory lines of apology (which, as he truly remarks, have been culled from John Bunyan's Apology for the "Pilgrim's Progress") he has seen to be of sufficient importance to require in the review as much space as either the plates or the text, and he is even "tempted to wish that the author had quoted more fully from the Apology of the 'wonderful tinker,' and added the lines—

'If that thou wilt not read, let it alone;

Some love the meat, some love to pick the bone.'

Your readers will perceive that the "wonderful tinker" (and the author, too) has long anticipated both the verdict of your reviewer—

"Well, yet I am not fully satisfied,

That this your work will stand, when soundly tried,"—

and the author's reply—

"Be not too forward, therefore, to conclude,

That I want solidness,—that I am rude;

Nay, since your brethren pleased with it be,

Forbear to judge till you do further see.

If that thou wilt not read, LET IT ALONE;

Some love the meat, some love to pick the bone."

I am, Sir, yours faithfully,

WM. CUTHBERT MORTON, M.A. (Classical First),
M.D. Edin.,

Honorary Demonstrator of Anatomy in the University of Leeds.
Leeds, Oct. 28th, 1911.

THE NOMENCLATURE OF SPINAL DEFORMITIES.

To the Editor of THE LANCET.

SIR,—Mr. Paul B. Roth and myself disagree upon one very distinct point. He, following Lovett's deductions and the suggestion of W. Adams, believes that the spine is incapable of any simple lateral movement whatever, and that even in the normal subject rotation is coincident with the very beginning of side-bending. To him, therefore, the terms "lateral curvature" and "skoliosis" are synonymous. I hold a very firm opinion that lateral movement in the dorsal region is free until all play in the costo-vertebral articulations has been taken up and that rotation commences only when the tubercles of the ribs become firmly applied to the tips of the transverse processes. To me, therefore, the terms "lateral curvature" and "skoliosis" are *not* synonymous, for the first stage of the former may be quite free from any rotation whatever. The extent of this free stage varies with the type of patient, being very brief in the "rigid" type and much greater in the "loose-jointed" type. Its existence in the latter type has been noted again and again by old writers, who verified their observations by the use of some such instrument as Barwell's skoliosis gauge, and who adopted the term "lateral deviation" to fit this pre-skoliotic phase of lateral curvature.

However, Mr. Roth and myself can meet on common ground by putting aside the terms "skoliosis" and "lateral deviation" altogether, and by using the term "lateral curvature" to cover all phases of all cases. The classification he supports then works out as follows:—1. Postural lateral curvature. 2. Osseous lateral curvature in four stages—trace, moderate, severe, and extreme. The diagnosis between