

American Journal of the Medical Sciences of 1904, at p. 128, he writes: "If the lower sternal region—that is, the sternum contiguous to the heart—is first percussed in the standing and then again in the recumbent posture, one may appreciate a decided alteration in the percussion tone; in the former attitude it is resonant, or even hyper-resonant, in the latter it is dull or flat."

That is the exact reverse of my statement which Sir James Barr says is untrue and every one else seems to think so true that it must have been recorded in cuneiform. Clearly either Dr. Abrams or I must be badly wrong. Which?

I am, Sir, yours faithfully,

Exeter, June 18th.

W. GORDON.

SIR,—The facts alluded to in my previous letter as "long well known" should have been stated to be those bearing upon the mobility of the heart as a whole and of its right border, and in the matter of examination those bearing upon the accessibility of the right border to accurate localization, even without any such help as that of R. Godlee's prone percussion hammock or of the less extreme postures which from time to time have been suggested and used by other observers. But speaking without the support of an adequate bibliographical inquiry, it is my impression that, at the time of its publication, Dr. W. Gordon's valuable study was, in point of method and of clinical observation, not only original but novel.

With reference to the general question, to be plain in a matter fraught with responsibilities far beyond the academical, criticisms from cardiologists upon much that has been published as to the variations in the size and in the site of the heart may have been largely discouraged by a keen sense of their own inadequacy to an accurate solution of the problems involved, and by the less manifest sense of the inherent difficulties underlying many statements which have appeared, more particularly in connexion with cardiac dilatation.

For a genuine estimation of the size of the heart an accurate determination of its three diameters is indispensable. The best of percussions can only determine two of them, the *sagittal* diameter being beyond its reach. The credibility of startling conclusions based upon two diameters only is hopelessly weakened when one of these is passed *sub silentio*; and when the other, the *transverse* diameter, is estimated in linear units of enormous cubic significance, and, moreover, on a fluctuating basis of two standards, the actual diameter and the apparent diameter, the first of which is unhappily taught to be normally unascertainable, and the latter is a personal equation of those so taught. At the best the plexigraphy of the heart supplies an incomplete measurement. For what it may be worth, it could only be fit for strict discussion when the *vertical* diameter had been established on the most accurate determination of the two upper levels of the diaphragm, or practically speaking, of the liver (in the absence of ptosis), and when the *actual* transverse diameter had been accurately ascertained at the start as well as at the finish, irrespective of any postural obscuration of the right border and of any inconvenient degree of pulmonary overlapping.—I am, etc.,

London, S. W., June 19th.

WILLIAM EWART.

P.S.—The title selected for this correspondence opens a field too vast for me to attempt, but as Dr. Auld has mentioned me in connexion with the percussion of the spleen, it may be opportune to refer to a paper on "The Diagnostic Uses of Percussion of the Vertebral Spines, with general remarks on Pleximetric Bones and Viscera,"¹ in which the occurrence of a "boxy" percussion note of the spleen and also of the liver is described in its connexion with the question of diagnosis of suspected gas in the peritoneum.

ENVIRONMENT AND HEREDITY.

SIR,—The great importance of the issues raised in your article on environment and heredity, in the *JOURNAL* of June 4th, prompts me to take the earliest opportunity available of pointing out a possible source of error in the conclusions reached in the second pamphlet you then placed under review. The pamphlet emanates from the Francis Galton Laboratory, and is entitled, *A First Study*

¹ *Lancet*, vol. ii, 1898, p. 23.

of the Influence of Parental Alcoholism on the Physique and Ability of the Offspring. In order to determine the effect of parental alcoholism on the next generation, the necessary data are largely derived from an account of the children in the special schools in Manchester, supplied by Miss Mary Dendy. Miss Dendy has so frequently stated in public her conviction that alcohol has nothing to do with feeble-mindedness, that the results to be obtained from her statistics are more or less a foregone conclusion.

In the interesting introduction to the memoir, the difficulties of the problem of intemperance are pointed out, and also the importance of starting an inquiry without any bias as to its result. It is also explained that lay statistics are employed owing to the dearth of any similar statistics collected by medical men. It is a little difficult to reconcile the latter statement with the fact that the report of the Royal Commission on the Care and Control of the Feeble-minded, which was published nearly two years ago, and also such textbooks as Shuttleworth's and Tredgold's, contain definite information in regard to the connexion between alcoholism and congenital mental defect.

Miss Dendy's records show that only in about 8 per cent. of mentally defective children is a history of alcoholism to be found in the parents. They are, therefore, in marked contrast to the findings of nearly all medical investigators. Fletcher Beach and Shuttleworth found a history of parental intemperance in 16.38 per cent.; I found one in 41.6 per cent., and Tredgold did so in 46.5 per cent.; while other reliable authorities, such as Forel, have recorded even higher percentages. The higher figures found by Tredgold and myself are sometimes considered to be partly explained by the fact that each of us made a careful house-to-house visitation, seeing the parents and examining them if necessary; the earlier investigators, as a rule, had not done this. With these and other sets of figures available, it is a little unfortunate that in an investigation which seeks to be without bias that set should have been selected which comes almost at the extreme end of the scale. It is true that Looft in Norway found a history of alcoholism in the parents in only 3.7 per cent. of the mentally defective, but no other figures that I can find in the literature of the subject are at all comparable with Miss Dendy's.

It is important to remember that the memoir of the Eugenics Laboratory only traces the relation between alcoholism in the parents and intelligence in the child, and is, therefore, an incomplete study of the effect of alcoholism on the race. Those who have studied congenital mental defect have often noticed that the source of the mischief is to be found in the grandparents rather than in the parents; it is not an uncommon experience for the children of drunkards to show nothing unusual. When they have grown up, however, they sometimes tend to be eccentric, and fail to attain the success one would expect from their training and original position in life; they may even become teetotal cranks. Then, if any of their offspring turn out to be feeble-minded, as they sometimes do, it may be stoutly asserted that alcohol has nothing to do with the case. I have several times seen patients in whose families such a sequence of events could be traced. As a matter of fact, the relation of alcoholism to mental defect is a most difficult and intricate problem, especially as the intellectual weakness can seldom be attributed to a single factor, but is due rather to a combination of unfavourable elements. I made an exhaustive review of the subject in a paper on the relation of alcohol to feeble-mindedness, which was published in the *British Journal of Inebriety* for January, 1909. The conclusions I was then able to draw were that "the evidence is not clear that alcoholism, by itself, in the father will produce amentia; but it is quite plain that in combination with other bad factors it is a most unfavourable element, while maternal drinking, and drinking continued through more than one generation, are potent influences in mental degeneracy."—I am, etc.,

W. A. POTTS,

Late Medical Investigator to the Royal Commission on the Care and Control of the Feeble-minded.

Birmingham, June 19th.

SIR,—Will you allow me to make a short statement concerning the recently issued memoir by Miss Elderton *apropos* of the leader in your issue of June 4th? I fear