

and intestinal disorders are common causes of the anginous paroxysm. But these are not the only causes of attacks, and I indicated how emotion and physical effort also produced attacks through the vasomotor centre, which had therefore to be clinically regarded as hypersensitive. I showed that that hypersensitiveness might be influenced from the alimentary tract by means of diet, or by the correction of even slight disorders of that tract. This is, to my mind, a perfectly sound clinical chain. Did space permit I might elaborate this subject from the standpoint of excessive proteid feeding or of the intestinal putrefaction of flesh foods. Before Professor Chittenden published his remarkable researches I was clinically satisfied that excess of proteid led, either directly or indirectly, to arterial changes, and since the publication of his work it has been easy to understand the prevalence of these changes. The recurrence, the maintenance, and the exaggeration of arterial contraction, determined by alimentary and digestive conditions, leads to the anatomical changes which characterize arterio-sclerosis as that term ought to be applied.¹ By arterio-sclerosis I mean the common thickening of the arterial wall, which is due to thickening of the internal and middle coats, without atheromatous degeneration or calcareous infiltration. This distinction is not mere pedantry, for Sir James Barr himself recognizes that atheromatous and calcareous changes in vessels interfere with their contractility, while in the condition to which I confine the term "arterio-sclerosis" that is not the case. The conditions are so totally different that one is amazed the confusion should have existed so long. Atheroma, as a matter of fact, is a relatively rare condition in the radial artery; and if Sir James Barr will examine after death thickened radial arteries, including those he has diagnosed as atheromatous, he will be as much surprised as I was, and as I am still from time to time, and I shall have had the honour of making a valuable convert.

Only one other point remains for me to deal with, and that refers to the cerebral and coronary arteries. If physiology says we are not to reckon clinically on the possibility of contraction of these vessels because their connexion with the vasomotor centre has not been demonstrated, I should unhesitatingly decline to accept the dictum.

Physiology has its own important place, but if I am told "that when clinical medicine does not harmonize with physiology, so much the worse for medicine," I find an answer in Professor Chittenden's work, dealt with so tactfully and lucidly in a leading article in the JOURNAL of February 17th.

Personally I have always found physiologists glad to have the assistance of clinical observation; were it otherwise, I should change Sir James Barr's words, and say "that when physiology does not harmonize with clinical facts, so much the worse for physiology."

Apologizing for occupying so much of your valuable space,—I am, etc.,

Edinburgh, Feb. 18th.

WILLIAM RUSSELL.

THE RELATIVE BRAIN-WEIGHT OF MAN AND WOMAN.

SIR,—When discussion degenerates into recriminative details and quotations, it loses its interest for the ordinary reader. I will therefore be very brief.

Professor Pearson does not really answer—I do not think he can satisfactorily answer—either of the two points I made in my former letter. In his attempt to meet the second, the analogies are too imperfect. He abjures Mr. Fison, as I do; but it seemed natural to me to conclude, from the confidence of his assertion, that he was a myrmidon of the Professor, and knew something of what he was talking about.

Professor Pearson, however, thinks he has reason to complain of my having stated in *l'Anthropologie* that, according to him, "*il n'existerait aucune corrélation entre la capacité crânienne et le développement de l'intelligence.*" I am sorry if I have ever misrepresented him. It is a little difficult sometimes to distinguish between him and his coadjutors in their joint labours; and in this case I suppose it was Dr. Alice Lee whom I ought to have quoted. In a paper in the *Philosophical Transactions* she (or he) said:

I think we may conclude:

1. That there is no marked correlation between skull capacity and intellectual power in the case of either sex alone.

2. That brain-weight must have a very considerable correlation with skull capacity; and, therefore, our data present nothing to encourage the belief that there is a relation between brain-weight and brain-power.

Though my quotation may not have been absolutely accurate, I cannot see that it was otherwise than practically correct.

I am sorry that Professor Pearson has so poor an opinion of my anthropological work; but it somewhat consoles me to recollect that Broca and Virchow, and Topinard, all appreciated it, and in some cases even built upon my foundations. Therewith I am content.—I am, etc.,

Bradford-on-Avon, Feb. 17th.

JOHN BEDDOE.

SIR,—My sole reason for asking permission to continue this discussion is that medical men may fully appreciate the importance of the discovery which Professor Pearson has brought before the readers of the BRITISH MEDICAL JOURNAL. Medical men have frequently sought to explain the fact that the average man has a brain about 100 grams (roughly one-thirteenth of the total brain) heavier than the average woman; it has been left for the biometricians to discover the only explanation which is entirely satisfactory to Professor Pearson. His last letter now permits the explanation to be put very simply—the average man's superior brain-weight is due to the greater size of his head. It is "a valid scientific deduction," I hope, to infer that owing to the greater size of the average man's head he requires 100 grams of brain matter more than the average woman to fill his skull. In brief, one-thirteenth of the brain-weight of men with mediocre heads, such as Professor Pearson's and my own, is present simply by way of padding, to allow our brains to fit our skulls. In the case of men like Byron and Cromwell, whose brains exceed the average by 800 or 900 grams, the padding must have been enormous; while in the case of men like Tiedemann the anatomist, who fell about 100 grams short of the average for his countrymen, the padding must have been less than normal. This discovery of the biometricians will cause us to revolutionize our conceptions of brain growth. The cranium is the capsule of the brain; we have hitherto supposed that the size of a capsule depended on the size of the organ; as regards that organ, the brain, we must now suppose that its size is determined by the growth of its capsule. In a hydrocephalic child, for instance, we must give up the idea that the head grows because the brain has become dropsical and regard the collection of fluid as due to the extreme growth of its skull. In short, the brain is made to fit a skull which is a size too large for it much in the same way as a hat is adapted to a head which is too small for it, by the use of padding.

Owing to the wonderful stimulus given to the imagination by the constant use of symbols in place of the reality, biometricians have raised round themselves a delightful Alice-in-Wonderland atmosphere, where heads expand by the use of one multiple regression formula and contract by the application of another, and I hope they will not feel surprised by my application of their discovery to the reality.

I guessed, when Professor Pearson brought this matter forward under the pretext of a misrepresentation of mine, that his ultimate object was to interest medical men in his efforts to place physical anthropology on a sound scientific basis—an object in which he has my entire sympathy. For that object two things are necessary—a rational method and rational data. Professor Pearson has got the method, but owing to our ignorance of the manner of growth and of the mechanism of the human body that method has to be applied to irrational data, which give necessarily such an irrational result as the one just cited—that the size of the brain depends on the size of the skull.—I am, etc.,

London, E., Feb. 20th.

A. KEITH.

ON VENTRIFIXATION.

SIR,—Whether the recent discussion on ventrifixation at the Obstetrical Society of London showed British gynaecology at its best is not, perhaps, worth

¹ *Lancet*, June 1st, 1901, and *Edinburgh Medico-Chirurgical Transactions*, vol. xix, 1900-1.