

It may be added that high blood pressure and the cardiovascular changes incident to it are not necessary effects of advancing years. Many persons at the age of 70 or 80 have arteries as soft and elastic as those of a child. Nor are these changes the only ones met with in old age. The artery at the wrist may be thin and compressible when calcareous points, or plates, or rings can be felt in its walls; the blood pressure is not high and never has been. Again, there may be the special changes in large and small arteries due to syphilis, an account of which cannot now be given.

I am, Sirs, yours faithfully,

Brook-street, Feb. 3rd, 1903.

W. H. BROADBENT.

To the Editors of THE LANCET.

SIRS,—I venture to send you a line on this subject, partly because it is one to which I have contributed somewhat, but mainly because it is of supreme importance that we should have perfectly clear ideas of circulatory phenomena. Professor T. Clifford Allbutt was good enough to send me a proof copy of the interesting paper which he read recently before the Royal Medical and Chirurgical Society, and were it not for the correspondence which has already appeared in your pages I would not have thought it necessary to crave space for any remarks from me. The correspondence leads me, however, to ask Professor Allbutt if he would kindly make his position more clear to an ordinary mind such as my own. Professor Allbutt is such a master at literary garnishing that I am not quite sure I have succeeded in clearly determining the precise character of the viands he has displayed so attractively. The particular point regarding which I at present seek enlightenment is as to the precise place he gives to the condition of spasm contraction of arteries, for which I have proposed the term "hypertonus."

In a paper which appeared in THE LANCET in 1901¹ I drew attention to the part taken by this "hypertonus" in determining arterio-sclerosis and I so fully realise how important it is that the physician should understand what Professor Allbutt calls "the pathology of processes" that I wish to restate what I indicated then—namely, that certain conditions of blood lead to tightening up and thickening of vessel walls which is often mistaken for true structural alteration; that the recurrence of this "hypertonus" or spasm contraction leads ultimately to structural changes in the vessels; and that along with this "hypertonus" and subsequent structural change there may, or there may not, be raised blood pressure.

The conditions of the blood which lead to this "hypertonus" are, I think, sufficiently indicated by the word *impurity*. This is, if I understand aright, Sir R. Douglas Powell's position. Does Professor Allbutt accept this reaction of vessels? Or does he contend for some physical alteration in the blood which he terms *viscosity* and which, by impeding its flow, leads to heightened pressure, but to no active contractile reaction on the part of the vessel walls? Whatever the experimentalists may say, clinical observation surely puts it beyond doubt that impurity of the blood causes this contraction of vessels which in the vigorous is usually accompanied by heightened pressure. That, however, all tightening up of vessels should be spoken of, or thought of, as heightened pressure or raised *tension* is a fundamental error which we cannot too strenuously seek to correct. In seeking to know "the pathology of processes" it has appeared to me that this vessel response to impure blood was the first step. When it is accompanied by decidedly high pressure the thoughtful practitioner recognises it and hence the diet restrictions and the blue pill. Without the heightened pressure the condition is not usually recognised, and yet, if my contention be correct, herein lies the beginning of a process the recognition of which would prevent or delay those static manifestations which Professor Allbutt so picturesquely indicates.

I am, Sirs, yours faithfully,

Edinburgh, Jan. 31st, 1903.

WM. RUSSELL.

THE ANATOMY OF GLENARD'S DISEASE.

To the Editors of THE LANCET.

SIRS,—I have to thank you for your kindness in affording me an opportunity of perusing Dr. Arthur Keith's paper on the Anatomy of Glénard's Disease, which you were good enough to forward to me, and your annotation on which

called forth the comment contained in my former letter. I must, however, add that my acquaintance with the original document has in no way modified the opinions expressed in my letter regarding the contents of the annotation in question or those contained in that which I afterwards forwarded to you on the subject of the editorial comment which you appended to my published letter. This latter comment, indeed, rather surprised me and—*emphatically*—calls for some reply, on the two-fold ground that it misrepresents me and misstates some very important facts in physiological anatomy. The critical rejoinder in question consists of three sentences. The first is: "Dr. Knott is criticising a paper which he has not read." I beg leave to state that Dr. Knott was doing nothing of the kind: he was criticising an annotation on a page of THE LANCET which lay open before him. This fact must be very obvious to everyone who has read my letter. The second sentence runs as follows: "His remarks as to the action of the diaphragm are mostly academically correct, but had he read the paper he would have gathered that several of his premisses are probably incorrect, particularly that which assumes the relative immobility of the central tendon." I do not propose to ask my critic to offer a scientific distinction between the conditions of being "*academically*" correct and *absolutely* correct, or to explain how a statement can be *academically* correct of which the logical premisses are "*probably* incorrect"; or how far the "*theory of probabilities*" can be utilised to influence opinion regarding the hard facts of physiological anatomy. It is my province to deal with the latter; and the only question in this domain referred to in the sentence which I have just quoted is that of "the relative immobility of the central tendon." The *arguments* of the quotation I take the liberty of relegating to the department of transcendental metaphysics, to which they appropriately belong. With regard to the movements of the cordiform tendon during inspiratory expansion of the chest, I had been already aware that one of the mysteries revealed by the x-ray illumination was the considerable downward displacement of this fibrous shelf during inspiration. I have seen the phenomenon very skilfully and effectively displayed in the course of a brilliant demonstration by an x-ray specialist. But I saw at the same time—what the accomplished physicist in question did not appear to have seen—that the descent in question was rather apparent than real, rather relative than absolute; for the *appearance* was produced almost wholly by the *ascent of the ribs and sternum*, which had, of course, the same effect on the objective field thus offered to the observer in an antero-posterior view of a translucent thorax. To my critic, and to all readers who are specially interested in the study of the action of the diaphragm, I would suggest the following experiment. There will be no danger, I think, of its being brought within the jurisdiction of the existing law against "vivisection" or even of those for the "prevention of cruelty to children." Let him *purchase* the friendly confidence of a lively "street Arab" of eight or nine, undress him, and place him standing on a table so that the denuded epigastric region will be (approximately) on a level with the visual organs of the observer. Let him then induce his youthful friend—under the appetising influence of a further present—to proceed to swallow, with a minimum expenditure of valuable time, three consecutive mouthfuls of dry oatmeal. The result will be a perfect display of the aberrant physiological phenomenon familiarly known as "hiccup." He will then have the best possible opportunity of seeing that with each *unmodified* contraction of the diaphragm the six lower ribs are drawn *upwards* and *inwards*, the degrees of displacement increasing from above downwards. This form of displacement is a mechanical necessity which could be foretold by any physiological anatomist, as the translation of every part of the disturbed chest-wall is towards the adjacent part of the circumference of the cordiform tendon of the diaphragm. It will also be seen how little real expansion of the chest takes place in this action. Indeed, without any such *objective* experiment, everybody's *subjective* recollections of the operation of *hiccuping* will have informed him how little tendency it produces to *inspiration* of the surrounding air—even if the glottis had been left wide open. The fact that the ribs are so extensively displaced in this movement is, I venture to suggest, an unanswerable reply to the misstatement that the cordiform tendon undergoes much downward change of position. *Both ends* of the muscular fibres cannot be *very much displaced* at the same time.

The third, and last, sentence of your criticism reads as

¹ THE LANCET, June 1st, 1901, p 1519.

follows: "As to the assertion that the lower ribs are drawn downwards and backwards by the simultaneous contraction of the diaphragm, intercostals, and serratus posticus inferior, it is sufficient to remark that clinicians who have opportunities of watching the isolated actions of muscles in disease are all agreed that the diaphragm *elevates* the lower ribs." The disingenuousness of the attempted argumentation of this sentence can hardly be surpassed, even in the records of legal, or theological, sophistication. Whose, I wonder, is "the *assertion* that the lower ribs are drawn downwards and backwards by the simultaneous contraction of the diaphragm, intercostals, and serratus posticus inferior"? Most assuredly it is not mine, as every reader of my letter can see. I have made the incontrovertible statement that *such is the action of the serratus posticus inferior, acting singly*. By so acting on the ribs it corrects the influence of the contracting diaphragm which, if left unopposed, would have the effect of *narrowing* "the basal section of the thorax, where most expansion is attainable." (I quote my own words.) It is, of course, superfluous to point out to those who have perused, and understood, my description of the action of the diaphragm that the testimony derived from "clinicians who have opportunities of watching the isolated actions of muscles in disease" was instruction wasted—so far as the present writer is concerned, as well as being conspicuously imperfect.

The contents of Dr. Keith's original paper call for comment under many other heads; but I will select only a few. On the first page, as the author discusses "the causes of the variability" of the level of the diaphragm after death, we are told that "the elasticity of the lungs draws the diaphragm up as far as it will yield. The high or low position of the diaphragm depends very little on the distension of the abdomen or of the bowel." Regarding these statements I must say that in the present state of physical and anatomical knowledge they read rather "funny." It is unnecessary, of course, to point out that the lungs, or their elasticity, have no *direct* connexion with the diaphragm. And every tyro in physiology knows, or surely should know, that the expansion of the lungs during inspiration is due to the atmospheric pressure—the regulation 15 lb. (14·7) to the square inch—on the *mucous* surfaces, when the active contraction of the muscles of inspiration has expanded the chest, by bearing a certain proportion of the same pressure from off the *cutaneous* (external) surface. The external weight lifted (or pushed) off the surface of the body is represented by that used by the internal limb of the gaseous balance in distension of the tubes and vesicles, as the pulmonary pleura follows the movements of the walls of the expanding chest. And when the inspiratory muscles have ceased to act the chest walls follow the contracting elastic tissue of the lung, till equilibrium of balance between the external and internal pressures is again reached. This atmospheric pressure acts with obvious directness on the circumferential chest-wall in squeezing it—in corset fashion—back to its original position; but it appears to be sometimes forgotten that the *atmospheric pressure is also the sole efficient agent in pushing up the diaphragm* during ordinary expiration, and more especially still in that which constitutes the last obvious movement of life.

On page 2 the reader is told that he "must look upon the diaphragm as a fan-shaped digastric muscle." This appears to be Dr. Keith's own special discovery. "The central tendon corresponds to the intermediate tendon of such a muscle as the digastric or rectus abdominis." To this physico-metaphysical statement I take leave to take special exception. The preposterous statement that the diaphragm is a digastric muscle cannot for a moment be entertained by any skilled physiological anatomist. It would be less intolerable, indeed, to speak of it as a myrio-gastric muscle, but even such description would be very unsatisfactory. It is rather difficult to comprehend how anyone acquainted with the diaphragm can hold the view that the action of each individual portion is to draw its circumferential attachment towards the spine. Everybody who knows its structure knows that the lateral fibres, on either side, pass from the ribs towards the central tendon and that the only possible effect of their contraction—in addition to reducing the upward convexity—is to draw these yielding bones towards that tendon; that is to say, upwards and inwards. The fibres attached to the *central leaflet*, of course, have necessarily the collective action of drawing the xiphoid cartilage and adjacent cartilages of the lower ribs towards the spine. But this movement may almost be

regarded as incidental. The modern geometer "squares the circle" by treating every *infinitely small* segment of the circumference as a straight line. By this—"quam proxime" correct—assumption he resolves the space into a series of infinitely narrow *parallel* rectangles. The calculation is then easy. But his rectangles must be parallel. The diaphragm undoubtedly may, for fanciful purposes, be looked upon as a structure composed of an infinite number of infinitely small rectangular digastric muscles. The rectangular bundles are, however, not parallel, and cannot possibly be made so; and, accordingly, the mathematical methods of "summation of series" cannot be made to apply.

I more especially wonder how it is that in this unusually original thesis Dr. Keith has ignored the incontrovertible fact that if the diaphragm acted according to his view—by drawing all its distal attachments towards the spine—it would very seriously limit the space behind the (almost vertical) posterior fibres of the muscle, which is precisely the region where, under existing arrangements, the lungs undergo the greatest proportion of expansion. A glance at Fig. 1 of his own paper will show this. I must also point out the fact that the greater firmness of the chest-wall in this region renders the wearing of corsets more tolerable than could otherwise be the case.

In the discussion of the "action on the ribs" we are told in regard to that on the abdominal ends of the six lower ribs: "this of the three directions in which it expands (? expands) the force of each contraction is the most difficult to understand." And the writer proceeds to ask, "How is it that the ends of those diaphragmatic ribs do not move in towards the origin of the diaphragm from the lumbar region of the spine?" In this connexion I beg leave to state that both difficulties are wholly of Dr. Keith's own creation. If the diaphragm were, as he states, a digastric muscle, the phenomenon would be simply impossible to understand, even after the most careful perusal of his preposterous explanation. But as the diaphragm is *not* a digastric muscle, endowed with only antero-posterior activity, the difficulty only exists in his own imagination.

There are other points in this very original paper of Dr. Keith's which may very easily be made to yield to criticism, but it is probably better to add nothing further to an already too lengthy communication.

I am, Sirs, yours faithfully,

JOHN KNOTT, M.D. Dub.

THE RAPID TREATMENT OF CONGENITAL HIP-MISPLACEMENT.

To the Editors of THE LANCET.

SIRS,—The profession having now had time to cool down from the excitement caused by the demonstrations of Professor Lorenz—a little study and calm reflection will show it that miracles must not be expected. Already a long list of failures and very serious accidents have been recorded, and as the public, which is ignorant of such matters, will be clamouring for marvellous cures it is necessary that practitioners should be on their guard when advising, and though an effort has been made by medical and other papers to caution the public that complete cures are not to be expected and that the plan is not free from considerable risk it is unlikely that this will reach the very extensive circle to which the operation is known. Hence much may rest with the advice medical men give, though the public is prone to take matters into its own hands when such promising results are expected. Should Professor Lorenz ultimately succeed in demonstrating anything approaching a complete cure he will be a great benefactor to humanity and I shall try to be the first to congratulate him. In the meantime it seems to me that the odds are heavy against forcible reposition as they were against the open cutting methods and for the following reasons. The child walks late, usually about the end of the second year, and though she falls about the parents think she will grow out of the trouble, so that these cases are often not brought for treatment until the child has walked some time. Hence the head and neck of the femur are more or less deformed, the acetabulum remains very incomplete, the ligaments are overstretched, and some of the muscles are shortened and others lengthened. How is it possible to correct all this by any method? It is quite right to try to do our best to remedy the deformity but to run considerable risk in so doing, unless