NOTE

ON

THE "EMBOLIC THEORY" OF CHOREA.

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DR. BARNES writes, in this JOURNAL, December 9th, "The comparison of cases of chorea complicating pregnancy with chorea independent of that condition enabled me to disprove the embolic theory of the cause of the disease: a theory since destroyed on other evidence by Ogle and Dickinson, and rejected by Bristowe."

Many of the readers of this JOURNAL will doubtless feel inclined to adopt Dr. Barnes's opinion; for, so far from this hypothesis being, as Dr. Dickinson says, "somewhat attractive", it has, to the best of my observation, been considered by most medical men of little worth; many of those also who have some interest in it, will easily have their little faith destroyed by the rejection of it by such justly esteemed authorities as Ogle, Barnes, Wilks, Dickinson, and Bristowe.

As, next to Parkes, I am the earliest propounder of the hypothesis (*London Hospital Reports*, 1864,* vol. i, p. 459, *Lancet*,† November 26th, 1864, p. 606), I wish to say something in favour of it. It may be that I labour under twelve years of prejudice. I ask that Dr. Barnes's opinion as to the destruction of the embolic theory by Dr. Dickinson's researches should be accepted only after reading Dr. Dickinson's account of the pathological changes he found. It is right to grant that Dr. Dickinson has carefully distinguished the morbid changes after death from chorea from changes found in the brains of persons who have died of non-nervous diseases.‡ It shall be granted that they are, as Dr. Dickinson supposes, the real pathological changes causative of chorea; he points out, however, that "it is not possible but to connect them with those of a large group of nervous disorders, and prominently with those of diabetes". There are, he tells us, differences in the seats of the changes in chorea and in diabetes. Strange as it may seem, I think, as I said at the meeting after the reading of his paper, that the changes, "remarkably constant in kind and place" which he describes, are not unlike those admitted to result from the process of embolism. I would now say that they seem to me to be very like. Dr. Dickinson writes (*Med.-Chir. Trans.*, vol. 59, p. 15) of what he found in all his cases, that "they were all connected with vascular disturbance". This is a very important statement, in general harmony with the hypothesis of embolism, especially when what he says on localisation of the changes (*vide infra*) is taken into consideration.

Dr. Dickinson continues: "The injection was general to all the vessels; most marked in the arteries. When the sources of hæmorrhage could be determined, they were always arterial; the degenerations were usually periarterial, and the spots of sclerosis§ similarly placed." He goes on to say, "the first visible change would seem to be the injection or distension of the arteries, succeeded by extrusion of their contents to the irritation and injury of the surrounding tissue". Now, is not this considerably like the description of the effects of embolism as the process is stated by other physicians and pathologists? The usual descriptions, it must be borne in mind, are of cases in which blocking of large arterial branches has occurred; but the supporters of the embolic hypothesis of chorea suppose only the small arteries or capillaries to be the seats of embola. In my earliest statement of the hypothesis (1864), I wrote: "I think, from many circumstances, that embolism is a frequent cause of chorea. I do not say plugging

of the trunk of the middle cerebral, but probably of some of its ramuscles, which supply convolutions near to the corpus striatum (*Lond. Hosp. Reports*, vol. i, p. 459).* Hence, on this hypothesis, we should expect there to be marked differences of degree betwixt the appearances Dr. Dickinson has noted and those stated in text-books to be most characteristic of embolism. But in general the descriptions are much alike; there is, in each, increased quantity of blood in the vessels, "succeeded by extrusion of their contents". The vascular and periarthral changes of Dickinson seem to me to be like ordinary "red-softening" of the brain in miniature.

It is agreed on by all pathologists that cerebral embolism and embolism in other organs whose arteries are "terminal" leads nearly always to increased quantity of blood in the vascular district on the peripheral side the plugs. Wilks and Moxon write as follows: "The mechanical effects of the embolus are not quite what you would at first expect. One naturally thinks that if the artery of a part is stopped up, the blood-supply will be shut off, and the part become bloodless. But, when a part of an organ is embolised, the effect is that its vessels become congested—that is, if time be allowed" (*Lectures on Pathological Anatomy*, p. 172). I did not know this when, in 1864, and again in 1868, I stated the hypothesis of embolism in explanation of the clinical facts of chorea. I made the necessary corrections, *Med. Times and Gazette*, March 6th, 1869.

That hyperæmia occurs from blocking of vessels is now an accepted doctrine. The facts and theories of the process were first stated, I believe, in this country, by Bastian, with regard to "red softening", in his article on Softening of the Brain in Reynolds's *System*. Prevost and Cotard's experimental researches on red softening are known now to everybody. The hæmorrhagic part of the process of embolism can, as Wilks and Moxon have said, be occasionally seen in the eye. Some years ago, Brudenell Carter showed a well marked case of this kind at the Medical Society of London, by the aid of his demonstrating ophthalmoscope. After the reading of Dr. Dickinson's paper, I handed round a drawing by Knapp, in which were depicted hæmorrhages in the field of an obstructed branch of the arteria centralis retinae, and along with it another drawing from Prevost and Cotard's work, showing extravasations in a dog's brain following artificial embolism by tobacco-seeds.

The following is from *Lectures on Pathological Anatomy*, by Wilks and Moxon, p. 172. It is from the part where they are not speaking of embolism of any particular organ, but of the embolic process in general; it is to be read in connection with the former quotation from their work. "The stagnation of the blood sufficiently explains its inability to maintain the contractility of the vessel. But it is not only the wall of the vessel that suffers, there is a general lowering of the nutrition of the part; the overgorged dilated vessels allow the escape of serum, or by bursting, or by transudation, allow blood-corpuscles to escape, and thus arises œdema or apoplexy of the part."

Of course, no medical man would suppose that the irregular movements in chorea depend on hæmorrhages or on "sclerosis" (disintegrated nerve-tissue). No one supposes that epileptiform seizures in cases of tumours of the brain are the direct results of the tumours, or of the disintegrated brain round about them. All movements, healthy or morbid, must depend on nervous discharges. Disintegrated nerve-tissue cannot discharge, for really the disintegrated matter has ceased to be nerve-tissue. I suppose the excessive movements occurring either in chorea or in epilepsy or epileptiform seizures are produced by discharges of grey matter, which, except for great instability from over-nutrition (not better nutrition), is healthy. There are discoverable two conditions of the motor organs in most cases of chorea: a negative one, paresis; a positive one, over-movement. The "sclerosis" which Dr. Dickinson speaks of (which is "disintegrated nerve-tissue") may account for the paresis of the parts which are the subjects of the choreal movements; but these points of sclerosis can only, I submit, be indirect evidence of the seats of the unstable cells on discharges of which the irregular movements themselves depend. We cannot expect to discover, with our present means of research, the alterations in grey matter on which excessive discharges depend. "Experience teaches us," says Niemeyer, "that the lesions from which abnormally active impulses proceed are insusceptible of anatomical demonstration." Abnormally increased expenditure of energy, at any rate, implies an abnormally increased "storing up" of energy; that is, in the case before us, abnormally increased nutrition of nerve-cells by some pathological process bringing more nutrient material.† My opinion is, that the direct pathological state leading to instability of grey matter pro-

* Soon afterwards (1865), Dr. Broadbent arrived quite independently at the same conclusion. Kirkes's hypothesis has had strong support also from Dr. Tuckwell, Dr. Russell of Birmingham, and a few other physicians.

† I refer to the remarks reported in the *Lancet* to take the opportunity of once more drawing attention to a clerical error therein: for "limited to softening of the brain," read "limited softening of the brain". See other parts of the present article for qualification of this amended statement.

‡ Wilks and Moxon write (*Lectures on Pathological Anatomy*, p. 614): "After thus enumerating the minute changes which have been described as associated with insanity, we must warn you that there is nothing peculiar in their kind. There is no one of these changes which may not be found in ordinary brains after middle age, or even younger" (italics in original). I quote this to show that we must be careful how to attach special importance to microscopical changes in the brain.

§ Dr. Dickinson does not, he tells us, mean to imply by "sclerosis" induration from increase of connective tissue, but a condition "which is apparently one of degeneration of the nervous elements rather than increase of the connective". This sclerosis is, indeed, miniature cerebral softening.

* I there quoted a summary of Dr. Kirkes's opinions from Dr. Handfield Jones's Report of Medicine in the New Sydenham Society's Year-Book for 1863.

† Reynolds, in his work on *Epilepsy*, writes: "The proximate cause of convulsion is an abnormal increase in the nutritive changes of the nervous centres."

ducing choreal movements is increased quantity of blood in the periphery of the capillary district embolised. The local periarterial softening or sclerosis account for the choreal paresis. Of course, this opinion as to the nature of the change in unstable cells is hypothetical; but so is every one's opinion as to the nature of "lesions insusceptible of anatomical demonstration".

Let me state Dr. Dickinson's opinion on the relations he supposes to exist betwixt the "muscular excitements" and the changes he discovered. (I italicise some words.) "The result chiefly in muscular excitement, rather than in paralysis or loss of sensation (though it is to be observed that a lesser degree of both is frequently present), may be associated with the character of the lesions, which are points of irritation rather than planes of section, and as such calculated to produce irritative rather than paralytic effects; not so much to cut off as unnaturally to excite nervous function."

The localisation of the changes has an important bearing on the interpretation of their pathology. Dr. Dickinson found them in the spinal cord; that is, in a part which those who hold the hypothesis of embolism believe to be unconcerned in the production of the irregular movements of chorea. So far, it will be supposed that Dr. Dickinson has the best of the argument.* But disease of any kind in the cord cannot account for the movements of the face. The elaborateness of the movements in chorea is, I submit, strong warrant for the inference that the changes causing them must be seated higher up. As previously stated from the very first, I thought their seat as high as the convolutions.† But it may be said that, whilst the lesions in the cord account for some of the movements, the lesions Dr. Dickinson discovered in the brain also account for the others. It has been said by one writer, as bearing on the seats of changes causing chorea, that there are centres of co-ordination all along the cerebro-spinal system. But the various choreal movements, very unlike in that different parts of the body are affected, are alike in the character of great elaborateness. No one denies that every motor nervous centre is a centre for the co-ordination of movements; but nervous centres, besides differing in combining different kinds of impressions and movements, differ also in the degree of complexity to which (be the movements of what part of the body they may) the process of co-ordination reaches. The question, then, is, *Do such elaborate movements as those of chorea imply abnormal developments of co-ordination effected by the spinal cord?*

But, as regards the brain, Dr. Dickinson discovered the changes he describes mostly in that region which, on the embolic theory, is believed to be the part to blame for the irregular movements—I suppose the convolutions near the corpus striatum (see quotation already given from *London Hospital Reports*); the corpus striatum and optic thalamus, according to Broadbent.

Dr. Dickinson writes: "The lesions are, indeed, determined in position by the course of arteries in the brain, notably by the middle cerebral—*favoured routes of emboli*—and the perforating branches which pass from these to the corpora striata." (No italics in original.)

But it remains that Dr. Dickinson discovered no plugs in the arteries; and this will very justly appear to most medical men strong evidence against the hypothesis which I and a few other physicians endeavour to prove; for, even if it be admitted that the changes resemble those produced by embolism, many will agree with Dr. Dickinson that they arise from some non-mechanical cause. Dr. Dickinson thinks they are "produced by causes mainly of two kinds: one a morbid, probably a humoral, influence, which may affect the nervous centres as it affects other organs and tissues; the other, irritation in some mode, usually mental, but sometimes what is called reflex, which especially belongs to and disturbs the nervous system, and affects persons differently according to the inherent mobility of their nature." (*Op. cit.*, p. 38.)

I have myself stated prominently that embola were not discovered by Gowers and Ferrier in some brains from choreal patients examined for me. I believe too strongly that most evidence is in favour of the hypothesis to wish to conceal any facts which may be thought to decide against it. The damaging observations of two men who are equally eminent as physicians and pathologists must not be concealed. Wilks and Moxon, who have examined many fatal cases of chorea, have never seen any microscopic embola; and in one case, in which hemiplegia from embolism had occurred, there were no "discoverable emboli in the small vessels". They have discovered no constant

morbid appearances in the nervous system after death from chorea. But Dr. Dickinson has discovered constant morbid appearances, and appearances not very unlike those producible by embola, regard being had to size of arteries plugged. Then we must remember that others have found embola. Tuckwell has found them. Bastian found plugging in three cases by, he believes, the process of thrombosis. It may be, I would admit here, that the hypothesis of embolism will be displaced by Bastian's hypothesis of thrombosis as an explanation of many cases of chorea. Dr. Bastian does not give it as the pathology of all cases of chorea.

I hope to be able soon to gather together the observations I have made since 1864 on this disease, and then I will as faithfully as I can consider in detail the arguments of Barnes, Dickinson, Bristowe, and others, against the views which I and some other physicians take.

COINCIDENCE OR CORRELATION? A NOTE ON THE EMBOLIC THEORY OF CHOREA.

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I HAVE at the present time under my care in the London Hospital a little girl ten years of age, suffering from chorea. The circumstances of her case are sufficiently remarkable to be deserving of special consideration. They seem to me to have an important bearing on the embolic theory of chorea.

The little patient's mother had been under my care for two or three years on account of a valvular affection of the heart—mitral stenosis. She was a stock case with me to point out to students the distinguishing characteristics of a presystolic mitral murmur. It was an exceedingly well marked example of this murmur; and, being combined with a systolic murmur at the same spot, it enabled me not only to point out the means of discriminating between it and the latter, but to prove that the so-called presystolic murmur did in fact precede the systole. The presystolic murmur was accompanied by a characteristic thrill which ended abruptly with the ventricular impulse. After ebbing and flowing for some time, the mother was suddenly stricken down. I was called to see her two or three days after her seizure, and found her semicomatose and completely paralysed on the right side of her body. I gave a most unfavourable prognosis as to her recovery, and warned her relatives that, if she regained consciousness, she would very likely be speechless. Six days from the time of her seizure, the mother died. Previous to death, she regained a certain degree of consciousness, so as to give some slight signs of recognition to those about her; and she moved the left arm and leg. She never spoke or uttered any intelligible sound. She was a Jewess, and no *post mortem* examination could be obtained.

As to the cause of the hemiplegia, although there was no necropsy, I think there could be no reasonable doubt. The age of the patient (she was thirty-one years old), the fact that her tissues showed no degenerative changes, the existence of valvular disease of the heart, and the sudden accession of the cerebral symptoms, all point in the clearest manner to embolism. The presence of right-sided palsy showed that the embolon had plugged the middle cerebral artery. The severity of the symptoms would make it equally clear that the embolon detached from the diseased mitral valve was of such size as to plug the trunk of the artery.

Ten days after the mother's death, her daughter, the patient now under my care, was observed to have some twitches of her face and limbs. A few days later, she was brought to me. She is a bright intelligent little girl, of dark olive complexion, and of a Spanish-gypsy type. In appearance, she is a miniature portrait of her mother. So striking is the likeness, that several gentlemen in the habit of attending my class immediately recognised her parentage. I found she had choreal movements of the left half of her body. The face, arm, and leg were affected. There were no movements of the right side. On examining her chest, I detected a *bruit* over the mitral region. The heart's action was so rapid and irregular that the murmur could not be accurately timed by the carotid pulse; but, from its character, I judged it to be presystolic. A few days later, when the child had been resting quietly in bed, the following notes were dictated. "The cardiac impulse not distinctly visible, but it is felt in the fourth and fifth interspaces, unaccompanied by any thrill. Cardiac dullness bounded by a line just internal to the left nipple, by right margin of sternum and by third rib. An inch below and just internal to the nipple-line are two murmurs. As timed by the carotid pulse, they are presystolic and systolic. The systolic murmur is conducted slightly towards the axilla, but the presystolic is easily lost." At another

* About ten years before I stated my opinion as to localisation in chorea, Dr. Russell Reynolds had fixed on the corpus striatum as the seat of the disease in chorea; so, then, I have not to establish any claims to priority. As to the pathology, Kirkes came before me, and recently, as to localisation, Reynolds. Having regard to the great elaborateness of the movements in chorea, I still think it most probable that the convolutions are the parts diseased.

† Hitzig and Ferrier's conclusions are in harmony with that opinion.