strong impression on the pulmonary pneumogastric nerve sufficed to overturn. The relation of the pneumogastric to the function of equilibration is well known. Dr. Hughlings Jackson has pointed out that the connection of its nucleus with that of the auditory nerve in the medulla, may explain the frequent occurrence of vomiting in the paroxysms of Menière's disease; and Dr. Ferrier has suggested that there is probably a still higher association between the two nerves in the equilibration centre.

The vomiting which accompanies the disturbance of equilibrium, and which is to be attributed to the association with the function of the pneumogastric, is the source, as already stated, of much of the misconception as to the nature of these cases. A few years ago, had the question been asked, What are the commonest causes of paroxysmal vertigo? the invariable answer would have been "A disordered stomach or a diseased brain". It is probable that, even now, the exceptions to such an answer would be comparatively rare. The answer, at any rate, indicates the conditions from which the diagnosis of auditory-nerve vertigo has to be made; for gastric and cerebral disturbances are probably, next to labyrinthine affections, the most common causes of giddiness, and the paroxysms of Menière's vertigo are constantly ascribed to one or the other of those conditions.

The gastric associations are especially liable to mislead. The occurrence of vomiting, in the absence of other obtrusive cause of the vertigo, is held as proof of a causal derangement of the stomach. Sometimes, an attack of vertigo may not cause vomiting; it may merely disturb the gastric functions, just as the motion of a ship does with some persons in whom it does not cause seasickness. The dyspepsia which results is regarded as a sufficient cause for the giddiness. But, further, in persons the subjects of auditory vertigo, whose equilibrium nerves and centre are deranged, a primary gastric disturbance seems sometimes to excite a paroxysm of the special vertigo to which they are liable, and the gastric disturbance which thus causes the attack is naturally regarded as its only antecedent.

An illustration of this is afforded by the case of a gentleman, who suffers from frequent attacks of intense dyspepsia and vertigo; the former appearing to him to precede and cause the latter. The vertigo is sudden and violent; he has fallen with it, and would fall always if he were not careful. He cannot say to what side he falls; but on one occasion must have fallen to the right, as he grazed the right side of his cheek. He is almost completely deaf on the right side; a watch, in contact with the skull on that side, is quite inaudible. The hearing on the other side seems unimpaired. The deafness came on gradually about twelve years ago. He has been subject from boyhood to attacks of violent dyspepsia, with vomiting and prostration. In his youth they were as severe as they are now; but he never suffered from vertigo until the onset of his deafness. Since that time, the dyspepsia and vertigo have gone on together. The paroxysms of vertigo in this case appear distinctly to be excited on the attacks of gastric disturbance. They are produced by errors in diet, and the long liability to such dyspepsia indicates that it is not to be regarded as secondary to the aural affection. But the sequence of the symptom, the coincidence of the liability to vertigo and of the ear-disease, indicate clearly that the giddiness is ultimately due to the influence of the latter, although it is excited by the stomach-derangement.

[To be continued.]

A NEW THEORY OF THE ORIGIN OF TYPHOID FEVER.

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AT the present time, when the professional mind is carefully feeling after the origin of typhoid fever, I presume any probable theory that may be advanced to explain many of the facts surrounding this important question will in some degree be of use to, and therefore be welcomed by, those who make the subject their special study. It is with this hope that I venture to submit the following facts upon which I build my theory.

In attempting to discover the cause of this fever, from the fact of its universal prevalence in hot and cold climates, in town and country, in the houses of the rich and in the hovels of the poor, it is necessary to fix the blame upon a factor that is equally ubiquitous; and I would further add that, during the period in which my thoughts have been directed to the subject, the accumulation of different evidence bearing upon it has only tended, in my opinion, to increase the probability of the theory.

Three years ago, I attended seven or eight cases of this disease in

a row of six cottages situated on the crest of a hill about two miles from this town. After careful inquiry, I arrived at the conclusion that the cause of this outbreak was to be attributed to the fact that the slaughter-house of a butcher was situated at the end of the row, into the common sewer of which the blood from his operations was allowed to flow, there to remain and putrefy. The waste-pipes from the sinks were directly connected with this drain without the intervention of any kind of trap, and the smell therefrom was often horrible. Here, the putrefaction of a highly albuminous liquid, blood, in the drain, and a direct communication between it and the interior of the cottages, seemed to give rise to the fever.

The following case, related by Dr. Domenichetti, medical officer of health, Louth Sanitary District (*Public Health* for July 1875), and quoted by Dr. Cornelius Fox (*BRITISH MEDICAL JOURNAL*, vol. ii, 1875, p. 376), appears to have had a similar origin; viz., the introduction of the products of decomposing blood to the system; in this case, by the water-supply. "A man aged 57 was seized with marked symptoms of typhoid fever, which terminated fatally in about a week, and, upon investigation, it appeared that the drinking water was at fault, though not suspected at the time, as it was derived from a blow-well, which, in these districts, is a term applied to water from the calcareous rock." "Upon examining the premises, I ascertained there was a stable-grating within a few yards of the well, and I requested the owner to have the drain opened. Upon this being done, the tin pipe through which the water was conducted from the underlying rock was found to be corroded through, and at its base there was an accumulation of refuse from pig-slaughtering, etc., quite sufficient to account for the contamination of the water. I should observe that no other case of typhoid fever had occurred near the spot for some time."

In the experiments instituted by M. V. Feltz, and communicated to the Académie des Sciences, upon the effect produced upon dogs by the injection of putrid blood, and alluded to in a contemporary (*Lancet*, vol. ii, 1875, p. 460), the symptoms produced were very analogous to those we see in typhoid fever.

"Putrid blood which had stood for several months was dried and desiccated in the air-bath and mixed with a certain quantity of distilled water, and injected into the crural vein of three powerful dogs. The animals immediately exhibited marked depression. After a period of incubation of from four to five days, febrile symptoms set in, accompanied by vomiting, loss of appetite, elevation of temperature, bilious and bloody diarrhoea, and biliary urine", and these symptoms were produced even when all trace of bacterial life had disappeared from the blood injected.

Pondering over the origin of the typhoid cases first mentioned in this paper, my thoughts were directed to the analogies existing between the symptoms present in typhoid fever and those which characterise certain cases of puerperal fever which appear to arise from the introduction of the product of decomposing blood to the system by a channel very much the same as in the case of the experiments on the dogs. In the cases to which I allude, diarrhoea is often one of the first appreciable symptoms, accompanied by high fever, the peculiar typhoid tongue, and, later on, hurried respiration, showing that marked tendency to affection of the lungs which is common to both.

I remember a case of this form of puerperal affection that occurred in a primipara after delivery by the forceps. Two days after delivery, she had a rigor accompanied by severe pain in the abdomen; she recovered after an illness of four weeks, during which time she suffered from symptoms remarkably like an ordinary typhoid attack in all the more important particulars. Unfortunately, as a general rule, cases attacked like this die in the course of a week or ten days, and the further clinical study of them is thus effectually prevented; but so much have some of them resembled ordinary typhoid fever cases of a very severe type that, if it were possible to have overlooked the puerperal condition, they might have been set down as cases of that fever. Of course, the puerperal cases are accompanied by local affections of the organs through which the poison travels before it finally reaches the general body of circulating blood, and this fact seems for a considerable time to have obscured the true nature of these cases by diverting attention from the ultimate general effects upon the system and fixing it too exclusively upon the local expressions of the infection.

The different channels by which the poison is admitted to the blood—in the puerperal case through the lymphatics or veins of the uterus, in the typhoid case through the stomach—would to my mind go far to account for any apparent variations in the symptoms. By the first method, the decomposing albuminous matter is admitted more directly, less changed by any chemical process, and in a more recently putrefied, and therefore in a more active condition, to the blood, and thus produces, as we might have expected, much severer effects than when it

is admitted by the second channel, as in the ordinary cases of typhoid fever, as it then has lain in the earth for some time and afterwards (as usually happens) been washed or percolated into the wells, and, after being received into the stomach, undergoes the action of the gastric juice before it effects an entrance to the blood.

The question may now be asked in what way is the origin of these cases, apparently arising from some component of putrefying blood, connected with the vast number of typhoid fever cases which appear to arise from the pollution of drinking-water by the excrement of human beings? In this manner, by fixing upon the serum of the blood as the essential factor of the poison, we at once see how any severe case of diarrhoea would be sufficient to produce the disease, because the liquid evacuation of severe diarrhoea is principally composed of serum of blood, and it is drawn from a source and placed in a condition highly favourable to the development of the putrefactive process. If decomposing blood be capable of giving rise to typhoid fever, and if the decomposing liquid evacuations of diarrhoea have the same property, then serum of blood, which is the only component common to both, must be the poison factor.

By this process of reasoning, I had already arrived at the conclusion that severe cases of diarrhoea might give rise to typhoid fever in others, before the contribution of fever cases apparently owing this origin to the medical journals. Of this class, are those contributed by Dr. Low in the BRITISH MEDICAL JOURNAL for 1876, vol. i, page 659. Four cases occurred in an isolated house, where "the privy was found running over and so full that the seat could not be used". He goes on to describe a second group of cases, also four in number. "Two little boys attending a day-school had an attack of diarrhoea. One of the boys passed his evacuations in bed, but was able to go to school next morning. The mother of the boys employed a charwoman to assist her in washing the soiled bedclothes. An aunt of the boys also assisted. Within a week, these three persons were seized with symptoms of enteric fever, which developed rapidly and proved fatal to the mother and the aunt. All three were engaged in the cleaning of the soiled linen, and were nauseated by the foul smell of the evacuations." Dr. Low goes on to prove the exclusion of all possible sources of the specific infection from any prior case of the fever, either of the boys or of the women, and at length concludes: "In the absence of any proof to the contrary, we must admit that the disease began from the inhalation of the fetid stools of the boys; that the emanations from these stools poisoned the systems of the three individuals who inhaled them, and the filth-fever was generated."

In connection with this *de novo* origin of the fever, from the decomposing liquid evacuations of diarrhoea cases, it would seem something more than a coincidence that the belief is now nearly universal among practical physicians that, when the disease is propagated continuously from case to case, it is believed to be so propagated only by the medium of the stools. (*Vide* BRITISH MEDICAL JOURNAL, March 25th, 1876, page 383.) In my own management of these cases, I have never found any reason to regret my confidence in this belief. This would also tend to prove that an albuminous liquid is required to propagate and transmit the contagium of the disease to another healthy individual, or why should it not be equally communicated through the medium of the other excretions and secretions of the body?

This theory of typhoid fever, arising from the decomposition of the serous evacuations of severe diarrhoea, accounts in a more satisfactory manner than any other for the extraordinary prevalence of the fever at a certain period of the year. It is a fact of universal observation that enteric fever cases reach their maximum, in point of numbers, in the months of October, November, and December; and this "periodical disposition" to the disease is accounted for by Liebermeister, who believes that "the real cause of every epidemic and every isolated case of typhoid fever is only the specific poison of typhoid fever" (*Ziemssen's Cyclopadia*, vol. i, page 61), in the following manner. He says (*ibid.*, vol. i, page 65): "The curves representing the frequency of typhoid correspond to the curves of average temperature, only with this difference—the different points of the typhoid curve follow those of the temperature curve by an interval of some months"; and, in order to account for this discrepancy, he says it takes two or three months for the changes of temperature to penetrate to the breeding places of the typhoid germs.

But if it can be shown that typhoid fever may arise from the putrefactive decomposition of blood-serum, then the abundant prevalence of summer cholera, from the end of July to the beginning of September, affords plenty of material for the elaboration of the poison, which afterwards percolates into the wells or is washed by the autumnal rains into the sources of our water-supply.

I do not propose to enter into the discussion as to whether this disease can have an abiogenetic origin, although the facts upon which

my theory is founded appear to favour that doctrine. Whether the fever arises only from the specific typhoid germs, which (according to the advocates of this theory) have a nearly omnipresent existence, and have the property of preserving their vitality in a dormant condition for many years, ready to spring into active and vigorous life when introduced into a proper nidus for their development; or whether the poison is manufactured from the ordinary germs existing in all the putrefactive processes which take place in certain animal fluids, or is elaborated by some subtle chemical change in the properties of the substance itself, does not signify so much to those who have to deal practically with the disease, so long as we can put our finger upon the factor, element, or pabulum without which these forces would be rendered permanently impotent.

It is from the conviction that this pabulum will be found in albuminous liquids, such as blood, blood-serum, and the liquid discharges from the bowels in diarrhoea, and that the poison of typhoid fever is elaborated from the putrefactive changes which occur in them after their expulsion from the body and subsequent exposure to the air, that I have ventured to draw the attention of the profession to what appears to me to be a probable explanation of the origin of this disease.

CASE OF SPINAL SCLEROSIS.*

By JOHN S. BARTRUM, F. R. C. S.

S. S. DIED September 1876, aged 73. She was a hearty vigorous woman, mother of nine children. Her mother was healthy; her father, though gouty, lived some years past eighty.

When about fifty-eight years old, she complained persistently of pains in the regions above the left shoulder and down the left arm; these, at the beginning, seemed to be rheumatic, and were relieved by friction; but very gradually the arm became tremulous and lost power, with sensations she termed "pins and needles". The same loss of power and subsultus extended to the left leg, thence to the right shoulder and right leg. She could move with difficulty, and not without help. Up to 1862, she, with help on each side, could walk a mile; she then had a long illness, after which she never walked. The voluntary muscles of the whole frame gradually lost their power, while many were in continuous tremulous action. For two or three years before her death, her state was very peculiar and interesting. She lay rigid and at full length on her back, the fingers stretched out, with tonic contractions of the tendons of the forearm and elbow, so continuous that it was impossible to feel the pulse at the wrist or elbow. The wrist, elbow, and shoulder joints were all fixed by muscular action, not rigid, for each could be forcibly bent. The toes were stretched forward and the foot extended on the leg, but the subsultus was not so marked as in the upper extremity. The thigh could be forcibly bent on the trunk, the rigidity being muscular, not articular. In the legs, when not well as usual, there were occasional spasmodic twitches and painful stretchings. In the earlier history of the case, there had been much uneasiness at the back of the neck, the muscles of which and of the back were in a state of equal tension, but not the muscles of the face, which was calm and could readily smile or laugh. The head was fixed as firmly as the limbs. Mrs. S., when put on her legs, was raised as a fixed trunk, exactly as is seen in acrobats, or as in a corpse when in the most marked stage of cadaveric rigidity. About 1868, her speech began to be difficult and voice faint, till, for some years preceding her death, the power of speaking was wholly lost, except such forcible expirations as "Yes" or "I won't". Those about her guessed her wishes, which she signified by the slightest expirations, not whispers; her assent or dissent being given by closing her eyelids or movement of the eyes. All voluntary power of the muscles of the cheeks or tongue was gone. There was no tremor of tongue or nystagmus.

Towards the end of her life, the mouth was always open, the tongue being within the mouth. She could not masticate at all, the slightest hard crumb causing imminent suffocation. This was, however, much modified by the flavour of the viands; she readily swallowed champagne, if still, but medicine could only be got down by teaspoonfuls. The slightest cough always caused anxiety lest the mucus should cause suffocation.

Sensation was everywhere perfect; a pinch or tickling of the legs caused spasmodic muscular action, and the reflex action on tickling the soles of the feet was quite painful. A ridge in her clothes was painful to her.

The digestion was good. The bowels and bladder acted naturally, and the nurse learned by intuition when it was needful to place her on

* Read before the Bath and Bristol Branch.