was published by the son in 1843. Dr. Brunetti and the phlebotomist were also living.

"The ovary, after having been preserved twenty-eight years in alcohol, is pyriform, is nine *centimètres* long and five in its greatest breadth (three inches and a half by two inches). The peculiar structure is searched for in vain, being entirely degenerated by scirrhus. The specimen has been taken from my private museum, and is now deposited in that of the illustrious Medico-Chirurgical Society of Bologna."

There are several interesting points in this narrative. The ligatures appear to have been left in the abdominal cavity; and it is certainly desirable that the specimen should be carefully examined. If it be really an ovary, it will certainly appear that the first case of ovariectomy in Europe was that by Emiliani of Faenza in 1815. I have written to Dr. Peruzzi, suggesting that the specimen should be examined by some competent morbid anatomist.

THE CONNECTION BETWEEN STOMACHIC AND LABYRINTHINE VERTIGO.*

By EDWARD WOAKES, M.D. Lond.,
Surgeon to the Throat Hospital.

A GREAT deal of attention has recently been devoted to what is commonly called Menière's disease, but, which, from the anatomical organ to which the symptoms are mainly referred, is also known as labyrinthine vertigo. The elaborate treatment of Dr. Gowers, Dr. Hughlings Jackson, and Dr. Ferrier compasses the present knowledge of the subject, so far as it relates to that form of vertigo resulting from the progressive invasion of the labyrinth from lesions of the middle ear.

But there is another class of vertiginous patients in whom the auditory apparatus is, up to the time of the attack, perfectly healthy, and may remain so after it has passed off, whose symptoms are, nevertheless, due to a more or less temporary lesion of the labyrinth, to which organ, in fact, the general consensus of opinion refers every such disturbance of equilibration as expresses itself in giddiness. It is to this class of cases I wish to invite your attention this evening, and, as in their investigation we shall get some insight into stomachic vertigo, the area of interest of the subject will be proportionately widened.

The form of vertigo to which I allude is that in which the patient, without being aware that he is otherwise than quite well, is suddenly attacked with giddiness and falls prostrate. This may never in that patient's experience be repeated, or the attack may recur after a shorter or longer period. The cases in question differentiate themselves from those treated of by the authors referred to in the fact, already stated, that the auditory apparatus is previously quite healthy, though they possess this feature in common with them: that there is no loss of consciousness. After the attack, the patient finds himself in one of the following conditions: his hearing may be normal, as heretofore; or he may be quite deaf on the side to which he fell; or there may remain some impairment of hearing only, with probably confused noises in the head.

As regards concomitant symptoms, nausea and a splitting headache may remain. There is also a painful sense of fear, a horror lest the attack should be repeated, and a dread of becoming epileptic; to which apprehension the members of our profession who suffer from the attacks are specially prone.

The following example, the subject of which was a medical man personally known to me, illustrates some of their features. When about thirty years of age, and being fully engaged in a very arduous country practice, he was one evening summoned to a patient, being already fatigued with the usual day's work. On reaching the house, he became giddy, and, as the door was opened, fell into the hall, a proceeding which, though perfectly conscious, he was quite unable to avoid. It is needless to say he was very much frightened by the attack, but was not otherwise affected by it. In the course of the following year, a second seizure occurred exactly corresponding in character to the former; but, for more than twenty years subsequently, there was no repetition of the attack, although he was for a lengthened period the subject of intense headaches, with tinnitus, for which he submitted to the ordinary method of depletion then in vogue.

The time at which these events happened was over thirty years ago. Menière's disease was, of course, unheard of; and Flourens had not yet experimented on the semicircular canals. It was not, therefore, extraordinary that this patient should be haunted with the dread of becoming epileptic.

Adhering to the principle laid down in a former paper, we shall seek the clue to the symptoms in the source and vaso-motor relations

of the blood-vessels supplied to the region manifesting them: a proceeding which will show that this particular region, the labyrinth, possesses some very remote, and possibly unlooked for, alliances. At this point, it will be necessary to repeat what I have elsewhere had occasion to insist upon; viz., that the receptive part of the auditory apparatus receives its vascular supply from a totally distinct source—the vertebral artery—to that which is furnished to the conductive portion of the ear. This in itself is a suggestive fact, as the internal carotid, in its bony canal, is placed so close to the internal ear that one might naturally look to it as the source whence its vessels would be derived. As a matter of fact, however, neither the external nor internal carotid is in any way concerned with the circulation of the labyrinth. It is the vertebral artery and its relations we have to consider. The origin of this vessel, from the subclavian deep down in the neck, and its prolonged course upwards, guarded by the bony sheath formed by the vertebral foramina, acquire an increased importance when its relations to certain nerves are considered. In the first place, this position brings it into very close proximity with the inferior cervical ganglion, from which it derives a rich plexus of nerves, communicating, in their upward course, with the several cords which form the brachial plexus. It is important here to note that this lower cervical ganglion furnishes also the principal inhibitory nerve of the heart; viz., the inferior cardiac nerve. The experiments of Burdon Sanderson and others show that this nerve is capable of slowing the heart's action even to the extent of stopping it. We wish to point out its close relationship with the nerve which regulates the supply of blood to the labyrinth, both passing through the same sympathetic ganglion. Nor can it be without design that such an arrangement exists. Glancing for a moment at one aspect only of the functions of the semicircular canals, that, viz., by which, under circumstances of altered tension of their endolymph, they are capable of causing the individual to lose his equilibrium and fall to the ground, we get some insight into the object gained by associating the nerve which regulates this tension with that which tends to check the heart's action. The connection is such that an impression influencing the heart will affect the labyrinth. Thus a strong mental emotion arising centrally and propagated to the heart through its inhibitory nerve, which tends to stop its action, will also be deflected in the ganglion to the nerve regulating the blood-supply of the labyrinth, and, by suddenly changing the tension in this organ, will cause the subject to fall to the ground, so removing the mechanical impediment to the circulation which the upright posture implies. In this way, time is allowed for the excited influence of the heart to subside while the subject of it is placed perforce in the recumbent attitude.

Referring now to the communication between this ganglion and the brachial nerves besides those already mentioned, there are others proceeding directly from the ganglion to the brachial plexus. It is this arrangement doubtless which explains an occurrence noticed in gunshot-wounds of the plexus. It was first pointed out by Drs. Moorhouse, Mitchell, and Keen, in a treatise on gunshot-wounds of nerves published after the late American war. These observers record most succinctly that the subject of such a wound, whether received in the arm, axilla, or neck, immediately falls to the ground, without losing consciousness. This circumstance is quite unique; it has not been observed in connection with wounds of any other correspondingly non-vital part of the body. A certain amount of collapse attends these cases, from which it may be legitimately inferred that the shock is conveyed, not only in the direction of the labyrinth, but also through its inhibitory nerve to the heart.

Anyone who is conscious of the possession of what is popularly called the "funny bone" may, if he choose, verify these statements, at least to a certain extent, by giving it a blow considerably in excess of that which is sufficient to induce the well known tingling sensation in the fingers. It once happened to the writer to witness such an experiment in the instance of a lad who was struck on this spot by a hard tennis-ball thrown with considerable violence. Immediately he became giddy and confused in his head, and would have fallen but for the support of some railings, and altogether the extreme distress which he manifested appeared out of all proportion to the slowness of the cause. At the time of the occurrence, the symptoms were quite inexplicable by any known relations of the injured part. The observation of the American surgeons just quoted, as well as the anatomical continuity established through the inferior cervical ganglion between the brachial nerves and the labyrinthine circulation, afford, we venture to submit, a ready explanation of the phenomena to anyone who candidly examines them.

It will be apparent that we are taking for granted the physiological fact, that a shock communicated to the inhibitory nerve of a vessel temporarily withdraws its function, allowing it to become greatly distended with blood; it is this sudden distension which produces the pressure on the endolymph, and the consequent subversion of the

* Read before the Medical Society of London.

function of equilibration, which will be proportionate to the severity of the shock.

Having thus seen how labyrinthine vertigo of a very severe type may be excited through these far off alliances of the semicircular canals, we shall be in a better position to understand the corresponding relations existing between the stomach, with its adjacent viscera, and the labyrinth. Before tracing these, it will be of service to refer to Trousseau's remarks on this subject, he being probably the first writer to call marked attention to *stomach vertigo* in his chapter entitled "*Vertigo e læso stomacho*", where he discusses it with his usual candour. No one, we venture to think, can carefully study the instances he adduces of this disease without sharing the conviction that the gastric lesion was only a subsidiary factor in the production of the symptoms he refers to it. That Trousseau had himself some suspicion of this is evidenced by his own statement. Thus, after describing the case of a lady in whom distressing giddiness, brought on by the bustle of the street, the passing of a carriage in rapid motion (causes exactly calculated to interfere with the organ of equilibration, when unduly susceptible), and who became worse by depletion and abstinence from generous diet, but was cured by tonics and restoratives, he remarks:—"I have frequently asked myself whether the treatment which, in these cases, I directed against the affection of the stomach was not, unknown to me, addressed to the nervous system; and whether I had not diagnosed a gastric affection rather than the effect of treatment than from the symptoms of the disease; whether I had not been led into an error in diagnosis by obtaining success for treatment usually employed with benefit in dyspepsia." (*Lectures on Clinical Medicine*: New Sydenham Society, vol. iii, p. 544.)

In this candid commentary, my own experience of so-called stomach-vertigo would lead me entirely to concur. A fair example is afforded in the following case. A gentleman, a member of the Society of Friends, consulted me, about five years ago, for symptoms closely corresponding with those enumerated in the foregoing quotation. He was about seventy-three years of age, and, though never robust, was otherwise in good health, except that any exertion brought on giddiness, and walking in the streets was attended with a feeling that he would fall forwards. His appetite was good, although the tongue was loaded; and it was observed he had no teeth, not even artificial ones; but it was ascertained that he ate meat with a relish. In view of the edentulous state of his jaws, and with Trousseau's teaching in recent recollection, he was prohibited from taking any solid food. Small doses of bromide of potassium were given at six hours' intervals. Under this treatment, he made a rapid recovery. After an interval of nearly a year, he returned to a mixed diet, and, in a short time, his old symptoms returned, but to a less extent than formerly. The same treatment was again had recourse to with a like result, and, in the period that has since elapsed, the rôle of circumstances just detailed has been enacted on several occasions. In fact, the sensitiveness of this patient's stomach to solid food appears to be proportionate to the motility of his equilibrating nervous centre. It is a suggestive feature of this case, as also of others that have occurred to me, that the progress towards recovery was invariably expedited by the bromide. Obviously, the point at issue is how to associate stomach-vertigo with the labyrinth. A very direct channel of communication is established between the pneumogastric nerve and the lower cervical ganglion by means of a fasciculus given off by the former about the point where the recurrent laryngeal leaves the trunk of the nerve. That the course of this branch is from the stomach to the ganglion will be apparent when it is remembered that vaso-motor fibres associated with cerebro-spinal nerves pursue an opposite course to the latter. When it is further remembered that nerves entering a ganglion break up and communicate with its corpuscles, by which means they contract new relationships with other nerves entering the ganglion, it will not be difficult to understand how the stomach is brought into relationship with the labyrinth. There can be no doubt, we think, that this correlation is effected in the inferior cervical ganglion through the medium of the communication from the pneumogastric to the ganglion, impressions passing along which become in it transferred to the inhibitory nerves furnished to the vertebral artery from the ganglion.

Having established this relationship anatomically, it seems impossible to resist observing the analogy between the shock propagated from a contused brachial nerve to the vertebral artery, with its concomitant vertigo, and the lesser shock caused to the nerves of the stomach by the lumps of unchewed meat swallowed by the toothless patient, and propagated thence to the same vessel, and attended with the same condition, viz., giddiness. *Mutatis mutandis*, the cause, the method, and the result are one and the same thing; the experiment is the same, the conditions only are varied. Hence it would seem that Nature had in the labyrinth erected a signal-box, in which a note of warning might be

sounded by the much-abused though tolerant viscus, the stomach, whenever the ill treatment it is subjected to threatens to compromise more vital parts of the organism. It would seem to teach us, what the acquired wisdom of advancing years so often fails to do, that the senile stomach is not the fit receptacle of unmastered meat; that it resents the slight shown it in this, and, of course, in many other ways: first of all by the warning attacks of giddiness, which, if not heeded, will shortly culminate in a fall. It was by such a catastrophe that the Duke of Wellington lost his life. The unscathed hero of a hundred fights failed to prove himself master of the situation, when the forces arrayed against him were a vigorous appetite and an acutely sympathetic labyrinthine circulation.

These observations naturally lead us to infer that the doubts which instigated Trousseau to ask himself the question, whether "the treatment which in these cases he directed against the affection of the stomach was not, unknown to him, addressed to the nervous system", were well founded. The fact is it was directed to both. By resting and restoring the digestive apparatus, he appeased those waves of excited vaso-motor sympathy existing between the viscus and the important tissue tracts supplied by the vertebral artery. That these various and distant tissue-tracts are really correlated through the medium of the ganglion in question receives confirmative evidence from the effects of certain drugs. Thus tobacco will produce giddiness, tinnitus, nausea, præcordial distress, and *aching in the arms*, amongst other conditions, indicating its progressive influence over the centres of the sympathetic system. Quinine, again, in large doses, produces giddiness, tinnitus, and occasionally, in extreme cinchonism, such trophic changes in the upper extremity that the cuticle will be shed like a glove. Both these drugs, therefore, may be viewed as having a paralysing influence over the inhibition of the lower cervical ganglion. The bromides, and notably hydrobromic acid, have an opposite effect, because they annihilate the aural symptoms of quinine, cure labyrinthine tinnitus; coupled with suitable diet, they remove stomachic vertigo, and have proved highly serviceable in a case of associated hiccough. They have, moreover, a now well established therapeutic relation to epilepsy. But the fact that the treatment of epilepsy, however conducted, is greatly influenced by the diet, that the absolute withdrawal of solid food immensely expedites recovery—a circumstance which I have repeatedly proved—this fact, we say, pointing to a direct influence between the innervation of the stomach and the condition of tone of the vertebral artery, is strongly confirmative of the reality of the correlation advocated in this paper.

From the foregoing remarks, it will be seen that we have, in the infracervical ganglion, an organ for connecting the upper extremities—the heart, the stomach, and upper portions of the digestive apparatus—with the labyrinth. These relationships, in all probability, by no means exhaust its range of influence; but they suffice the objects of this discussion. The ganglion in question brings these widely separated regions into very intimate sympathy by virtue of its regulatory power over the blood-supply of the labyrinth, and of its afferent and efferent branches to the organs referred to. Returning to the case of the toothless patient, who suffered from *vertigo* whenever he partook of solid food, we take the following to be the course of events in this, as probably in all cases of giddiness which originate in the stomach. The ingestion of the irritant gives rise to an impression, which is conveyed along the channel already indicated as forming a communication between the pneumogastric nerve and the infracervical ganglion, whence it is reflected to the vertebral artery in the shape of a wave of diminished inhibition. This is equivalent to an increased flow of blood to the labyrinth, with corresponding pressure on the endolymph. This pressure, physiologically interpreted, means giddiness, so far as it relates to the semicircular canals; and when, as usually happens, the circulation of the cochlea also becomes hyperæmic, there is concomitant tinnitus of a more or less pulsating character. If the vascular dilatation admit of effusion of serum, deafness is superadded; and if the exudation partake of the more solid particles of the blood, so that a clot is formed, the deafness may be permanent. In the milder occurrences, the impression will be transitory, passing away with removal of the exciting cause. It is to be noted that the labyrinth is placed at the extreme periphery of the tract supplied by the vertebral artery; it will, therefore, be the first to appreciate the consequences of the wave of suspended inhibition communicated to it from the ganglion. The phenomena it occasions, under the circumstances, are of such a marked character that they cannot fail of attracting the patient's notice; whether the impression originates in the heart, the upper extremities, or the digestive organs, the first note of warning is thus usually struck in the labyrinth. We are, therefore, justified in regarding this organ as possessing a sentinel-like office to warn the subject of changes of blood-supply about to happen throughout the whole of the important regions supplied by the vertebral artery.

—such, for instance, as the medulla oblongata and the contiguous nerve-centres at the base of the brain. Hence epilepsy or apoplexy may be about to supervene; the *petit mal* of the former disease being the signal note of the labyrinth, betokening the condition of its circulation, and suggesting the search for sources of suspended nerve inhibition.

In the disturbance of some one or more of the multitudinous sympathies, thus imperfectly sketched, will be found an explanation of the occurrence of those *simple attacks* of labyrinthine vertigo, which come on *without any previously existing ear-disease*, the desire to understand which furnished the key-note of this paper.

AN EXPERIMENT ON THE DISINFECTION OF ENTERIC EXCRETA.

By JOHN DOUGALL, M.D., F.F.P.S.G.,

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IN the sixth Public Health Report of the Medical Officer of the Privy Council for 1875, is an extremely able account by Dr. Baxter of an experimental study of certain disinfectants. Mr. Simon, in his preface, states that the investigation was carefully planned by Dr. Baxter and Dr. Burdon Sanderson. It may be mentioned that, in the experiments, three infective poisons, viz., that of glanders, that of vaccine, and that of intensified or infective inflammation, were submitted to the action of four alleged disinfectants; viz., permanganate of potash, sulphurous acid, chlorine, and carbolic acid. At the end of the report, the principal inferences which seemed to flow from the results of the investigation are embodied in a series of eleven propositions. In the fourth proposition, it is stated that, "when permanganate of potash is used to disinfect a virulent liquid containing much organic matter, or any compounds capable of decomposing the permanganate, there is no security for the effectual fulfilment of disinfection, short of the presence of undecomposed permanganate in the liquid after all chemical action has had time to subside".

As this conclusion, which is founded on a great number of very interesting experiments, seemed a safe basis for testing further, in an easy and very practical manner, the disinfecting power of the permanganate, I resolved to try its strength in the form of Condyl's fluid with enteric excreta, this fluid being the state in which the permanganate is almost exclusively used by the public. The experiment was conducted as follows. To a newly passed, characteristic, ochry-looking enteric stool, quantity unknown, a portion of a known quantity of Condyl's fluid was added. The mixture was then stirred and set aside for a short time, and, when the pink colour had changed to a brown, more of the fluid was added, and the process repeated till the pink colour was found permanent after the lapse of twelve hours; in other words, chemical action in the mixture had subsided, as indicated by the presence of the undecomposed pinkish permanganate liquid. The mixture was now measured, and the quantity of Condyl's fluid added being known, this was subtracted from the whole, thus showing the amount of faecal matter acted on. To find the exact relation between the quantity of Condyl's fluid deoxidised and the amount of enteric faeces disinfected, was now a simple matter of calculation. Avoiding fractions, it amounted to this (and I confess the result astonished me): one ounce of enteric faeces had deoxidised not less than ten ounces of Condyl's fluid; in other words, there is no security that enteric faecal matter is effectually disinfected by Condyl's fluid, unless the bulk of the fluid used be ten times as great as the bulk of the enteric faeces to be disinfected. In the same manner, I experimented with a fluid ounce of enteric urine, and here the result was, that one ounce of the urine deoxidised at least two ounces of Condyl's fluid.

Now, supposing a typhoid patient passing twelve ounces of faecal matter and twenty ounces of urine during each twenty-four hours, say for a week, which, it will be conceded, are not excessive quantities, and supposing the Condyl's fluid sold to the public in eight-ounce bottles at one shilling each is used, it follows that two hundred and eighty ounces of Condyl's fluid are required to oxidise or disinfect the week's urine, which, at one shilling per eight ounces, amounts to £1 15s.; and that eight hundred and forty ounces are required to oxidise or disinfect the week's faeces, which, at one shilling per eight ounces, amounts to £5 5s.; in all, £7 per week, or at the rate of £364 *per annum*. Supposing a hospital with thirty enteric patients, on an average, constantly under treatment, on these data, it would take £10,920 worth of Condyl's fluid to disinfect their yearly excretions.

CLINICAL MEMORANDA.

MOSQUITOES AND FILARIAE.

I REQUEST permission to make a correction of a rather serious error that has, not altogether unnaturally, occurred in the annotation that has appeared in the JOURNAL on this subject.

The statement that "the mosquito is the means of spreading the *filaria sanguinolenta* amongst human beings" is absolutely incorrect. It was through the courtesy of the Pathological Society that I was permitted, on the occasion referred to, to combat the error in question, and to explain that the human parasite taken up by the mosquito had no genetic relation whatsoever with *filaria sanguinolenta*.

At the Linnæan Society, on the 7th instant, I communicated the details of Dr. Manson's discovery, originally announced on January 12th. In the discussion that followed, a general acceptance of the correctness of Manson's views was accorded by Dr. Allen Thomson, Sir Joseph Fayrer, Mr. Curling, Mr. Ray Lankester, Dr. Collingwood, and also by Mr. MacLachlan, the well-known entomologist. The President of the British Association acknowledged, for his part, that he thought that the authors of the two papers before the Society had fairly proved their case; and Sir Joseph Fayrer characterised the subject as one of unparalleled interest and importance in relation to public health in the tropics.

If there were one point more than another upon which I insisted at the Pathological Society, it was on the necessity of not mixing up the *filaria sanguinolenta*—an entozoon of the dog, known to Redi nearly two centuries ago—with the *filaria sanguinis hominis* of Lewis, the *filaria immitis* of Leidy, and the *hamatozoon subulatum* of Leisegang. Unfortunately, as if the nomenclature of these helminths were not already sufficiently complicated, we have Dr. Beale giving the weight of his great authority as a microscopist to an error which I felt it my duty to expose on the night in question, and which, I regret to see, has now even gained the sanction of the valued columns of the JOURNAL. In the last page of the fourth edition of the *Microscope in Medicine*, the author writes as follows: "Dr. Cobbold seems not to be aware that, in his work on *The Pathological Significance of Nematode Hamatozoa*, published in 1874, Dr. Lewis gives an account of the mature *filaria sanguinolenta*, with drawings of the male as well as of the female worm." If anyone will read the context in Dr. Beale's work (p. 505), they will at once perceive that Dr. Beale's object is to defend Lewis's position on the question of priority in reference to the discovery of the sexually mature worm. Dr. Beale's imagination has led him to fancy that the *filaria sanguinolenta* is the same worm as Lewis's *filaria sanguinis hominis* and my *filaria Bancrofti*; and then he has sought to make it appear that I am ignorant of Lewis's memoir—a little work a copy of which was sent to me by the distinguished author himself, and which, I may add, is scarcely ever out of my sight.

The members of the Pathological Society will testify to the earnestness of the appeal I made in reference to the necessity of precision and accuracy in dealing with the numerous forms of hæmatozoa of man and animals. The number of errors that have been published on this head is very considerable. I do not allude to mere questions of microscopic size and so forth, concerning which all of us are liable to err; but when it comes to the confusing of species which have absolutely no connection with one another, and not even any relation as regards their bearers, then I feel it is impossible to remain silent.

T. SPENCER COBBOLD, M.D., F.R.S.

THE ACTION OF OPIUM AND ATROPIA.

THE publication in the JOURNAL of the case of opium-poisoning successfully treated by atropine, and Dr. John Harley's letter of warning, induce me to send a brief account of a case in which the physiological action of these drugs received a *vice versâ* illustration.

Early one morning in November last, shortly after midnight, I was summoned out to a lady who had been seized with angina pectoris, of which complaint she had had several attacks during the past few months. On my arrival, I found her sitting up in bed, with her head and body bent forward between her knees, in agonies of pain; the extremities cold, the pulse feeble, and the pupils dilated. She had also frequent retching, and was incessantly pushing forward her tongue and asking for "water, water". I was told she had vomited several times; but what she had thrown up last had alone been kept, and I found this to consist merely of bile and mucus without any peculiar smell. Having brought with me my hypodermic syringe and a solution of morphia, I gave her at once an injection of about a quarter of a grain of morphia. Almost immediately the pain and retching ceased, the