quality, as in gout and acid dyspepsia, and thus tend to induce more hurried and irregular contraction of the cardiac cavities.

Dr. Handfield Jones, who has considered this question in his interesting work on *Functional Nervous Disorders*, carries the explanation somewhat further by ascribing the phenomena to neurotic influence. He writes: "The associated symptoms point very decidedly to an implication of the vagi in the disorder; in five, the breathing was notably affected, and in two the stomach was deranged. My notion is, that the pathema is a paralytic neurosis of the vagi, or of their cardiac branches, essentially similar to a common neuralgia, e.g., sciatica."

Now, we know that division of the pneumogastrics, by removing their inhibitory or restraining influence, permits the sympathetic ganglia to run riot, and that enormously increased rapidity of the heart's action is the result; and Dr. Jones thus places the very reasonable theory before us that, from the influence of some weakening or paralyzing cause on the vagi or their centre in the cerebrum, the rein is partially removed, and the unrestrained vaso-motor ganglia, if we may use such an unscientific expression, take the bit between their teeth and bolt. And the analogy of physiological therapeutics also holds good here; for experiment has shown that the injection of atropine directly into an animal's veins causes an excessive rise in the number of cardiac pulsations, from its paralyzing power over the inhibitory branches of the pneumogastric distributed within the heart-muscle. Digitalis, on the other hand, produces a precisely opposite effect, for it causes a much lowered rate of pulse, even after section of the vagi, by acting on their cardiac nerves and the minute ganglia with which they are connected. Various other influences brought to bear on the innervation of the heart also induce increased rapidity of pulse; thus tubercular meningitis, in its later stages, when effusion at the base of the brain may be supposed to act on the centre whence the vagi spring, is characterised by an exceedingly quick pulse, and in children's disease generally we find an excessive excitability of the action of the heart. Thus, in pneumonia and in other of the inflammatory affections of early life, I have not unfrequently counted the pulse up to 200, even in cases which were not otherwise characterised by severe symptoms, but in which the mobility of constitution common to this period of life tends to the expenditure of nerve-force in various directions to the weakening and exhaustion of the great nerve-centres. We are also all familiar with the remarkable elevation of pulse following rapid exercise in ill-prepared persons: a brisk run, a stiff pull up a hill, will often run the cardiac pulsations up to what might well be considered a dangerous degree, and I have thus seen in my own case, during a severe ascent in Switzerland, undertaken under unfavourable conditions, the pulse beat at the rate of nearly 200.

In addition to these arguments in favour of the view that exhaustion of a certain nerve-force may cause well marked symptoms by enabling a distinct but usually subordinate function to come into play, we may appeal to the results of treatment. The successful issue of most of the cases I have mentioned was in all probability due to the administration of tonic and supporting drugs, among which digitalis always held a high place. We all know the property which this valuable medicine has in regulating and supporting cardiac contraction by its primary action on the inhibitory centres, as well as its secondary effect on the muscular structures of the heart itself. But, in this connection, my case affords a notable contrast to the others; for it was whilst the patient had been under the full influence of digitalis for twenty-five days that the violent palpitation came on. The remedy was here prescribed under the old-fashioned idea of reducing the labouring action of an enlarged and hypertrophied heart, and before modern research had shown that its use in such a case was not only unnecessary, but possibly injurious. The first result of its employment was to reduce the frequency of the pulse, and its second I fully believe to have been paralysis of the terminal cardiac filaments of the vagi, from that exhaustion which usually follows undue stimulation, and hence the remarkably rapid rate to which the sympathetic urged on the muscular contractions; and this is quite borne out by what is observed on experiment; for Nothnagel lays down the primary action of digitalis to be that of retarding the pulse, whilst larger or longer continued doses inevitably bring about a secondary stage of excitement and great rapidity. In Fox, therefore, it is probable that some temporary disorder of the secretions may have interfered with the due elimination of the drug, and so permitted that saturation of the system which is generally spoken of as indicating a so-called correlative effect. But it may be reasonably asked, Could not an exciting or stimulating influence directed towards the sympathetic nerve-supply of the heart explain more directly the phenomena of rapid pulse? This may be so, and Dr. Richardson was inclined to adopt such a view in Dr. Willett's case, where he believed the sudden emotional shock to have been the factor in enabling the accelerating action temporarily to overcome the controlling power

of the vagi. The only other point specially deserving of notice in my case was the observation that, during the rapid action of the heart, no trace of morbid *bruits* could be detected; and this was, no doubt, due to the feeble contraction of the cardiac muscular tissue not emptying the ventricle sufficiently fully or forcibly to bring about the physical conditions necessary for the production of murmur. And it was interesting, at the same time, to hear the clearness and distinctness of the first sound when unmasked by systolic *bruit*.

ORBITAL ANEURISMS.

By WALTER RIVINGTON, F.R.C.S. Eng.,
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THE value of Mr. Lansdown's extremely interesting case of "varicose aneurism of the left orbit cured by ligature of the diseased vessels" would be much increased if he would supply some additional information about the symptoms. The degree of exophthalmos; the nature of the *bruit*, whether continuous or intermittent; the presence or absence of thrill in the pulsating swelling; the presence or absence of noises in the head, and, if there were any, the date at which they were first heard by the patient; whether the *bruit* could be heard or not over the head, and, if so, whether it has now been completely removed; the presence or absence of chemosis, and of a fold of conjunctiva covering an everted lower eyelid; the presence or absence of paralysis of any or all of the orbital nerves; and the effects of pressure on the carotids—are all points of importance. Assuming for a moment that the exophthalmos was slight, that thrill was absent, that noises in the head were not heard at any time by the patient, that the *bruit* was only audible over the orbit, that there was no chemosis or conjunctival pad, and that paralysis of the orbital nerves was absent, the case would be one of much less severity than my own, in which all these symptoms were present, and in which it can scarcely be questioned that a communication existed between the carotid artery and the cavernous sinus, preceded and produced as it was by a very severe fracture of the skull, extending from the vertex along the right side and base of the skull. It would be of great advantage to determine whether such a lesion could or could not have been present in Mr. Lansdown's case. A fragment of the exploded soda-water bottle struck the patient at the inner part of the upper eyelid. Could it have penetrated so deeply as to wound the carotid in the cavernous sinus, as has happened in similar cases with other offensive objects? It is apparently beyond doubt, that the large vein which came off from the "aneurismal sac" was the dilated ophthalmic vein or one of its branches. Was not, then, the "aneurismal sac" a pouch of the vein, rather than a structure such as the description would denote; and the "feeding vessel" a vein, rather than an artery? If the affection was a varicose aneurism formed between the nasal artery and vein, it is difficult to understand, as Mr. Bader and Mr. Higgins found it difficult to understand, the congestion and dilatation of the retinal as well as of the superficial veins, and the projection of the eyeball.

The importance of establishing the diagnosis lies in the direction of the treatment adopted and recommended by Mr. Lansdown. If we grant the correctness of the diagnosis formed of a varicose aneurism between the nasal artery and vein, the treatment adopted remains both correct and successful; but it loses all, or nearly all, bearing upon the treatment of pulsatile swellings due to morbid conditions of blood-vessels behind the orbit. If we question the correctness of the diagnosis, and can render it not only probable, but almost certain, that there was either a small hole—possibly a very small hole—in the carotid, or a communication deeply within the orbit between the *ophthalmic* artery and vein, then the treatment pursued becomes of the highest interest. In point of fact, whichever view is taken, the treatment is unique. There is only one other similar case, so far as I know, in which the ligature of vessels within the orbit was attempted; but it was attempted without success. M. Passavant of Frankfort-on-the-Maine met with a case which resulted from a punctured wound of the orbit, caused by the forcible projection of a knitting-needle by the enraged sister of his patient: "Notumque furens quid femina possit." There were all the symptoms of an aneurismal tumour, and M. Passavant diagnosed an "aneurism of the lacrymal artery". An attempt was made to reach the supposed aneurism by resection of a part of the external wall of the orbit. No aneurism of the lacrymal artery was found; but at the apex of the orbit the finger encountered deeply, and on the inner side of the optic nerve, the pulsations of an aneurismal pouch. Attempts made to tie the pulsating sac were fruitless, and the operation was abandoned. The patient ultimately remained in the same state as before.

In this case, both the symptoms and the failure of the treatment are clearly explicable on the supposition of an arterio-venous communication in the cavernous sinus, and a resulting aneurismal varix of the ophthalmic vein. Might not Mr. Lansdown have ligatured the ophthalmic vein, and the cure have been effected by the coagulation of the blood in the deeper part of the vessel and the cavernous sinus? If so, there would certainly be an encouragement to the adoption of a like method in other cases. This mode of treatment had, indeed, occurred to me; but I was deterred from undertaking it by the reflection that it might be difficult to pass a ligature round the vein deeply enough to be of any service, and, if the vein happened to be wounded, the hæmorrhage might necessitate ligature of the carotid. Excision of the eyeball was proposed to me by the patient himself; but apart from the objection to remove an organ so important, as long as any reasonable prospect remained that vision might be regained, the operation was exposed to one of two disadvantages. Either it might fail to cure the condition causing the aneurismal varix of the ophthalmic vein, or it might be complicated with a wound of the vein and hæmorrhage of an embarrassing or even dangerous character, for which ligature of the carotid might be required. In reference to this last measure, I agree with Mr. Lansdown that it should not be undertaken indiscriminately. For a case of varicose aneurism of a small artery and vein like the nasal, it would certainly be out of place; and if we can find a less hazardous procedure applicable to other descriptions of morbid conditions of blood-vessels occasioning similar symptoms, by all means let it be adopted. As the account at present stands, ligature of the carotid for "intraorbital aneurism" has certainly been attended with marked success. Six deaths have occurred out of forty-seven cases of ligature, and only four deaths fairly attributable to the operation, two of the four being due to the advanced age of the patients. Is this experience delusive, or may future observers of orbital aneurisms be guided by it in the treatment of a class of affections whose pathological obscurity has not yet been wholly removed? At all events, it is of the greatest moment that we should be able to distinguish, if possible, between aneurismal conditions of the small vessels within the orbit and aneurismal conditions of the large vessels behind it; and it is in the hope of eliciting from Mr. Lansdown some features of differential value, that I have ventured to put the queries contained in the preceding remarks.

CLINICAL MEMORANDA.

THE TREATMENT OF CHLOROFORM POISONING.

DR. HARDIE's recently reported case has gone a long way towards destroying the proud *prestige* of ether, and towards showing that its superiority over other anesthetics is purely ethereal. We have still to search for a safe anæsthetic, but, in the meantime, human beings will continue to imbibe the more or less lethiferous vapours now in vogue, and it therefore becomes important that a concerted plan of action should be agreed upon when danger arises. As matters stand now, one man trusts to galvanism either of the heart or of the phrenic nerves, another puts his faith in artificial respiration, a third is content with flicking the skin with a wet towel in the hope of returning the expiring vital spark, another throws all his energies into opening the windows and pouring brandy down the victim's throat. Now, under the plan of inversion, introduced by Nélaton, recoveries have been effected after a much longer period of apparent death than under any of these; and it certainly is only right to give this method a fuller trial, and to resort to it on the first threatening of danger, and not after a number of other things have all been tried in turn and in vain, and when it would be as rational to attempt to resuscitate an Egyptian mummy.

Though the following case presents no novel feature, and, indeed, is altogether less striking than the cases reported by Dr. Campbell, Dr. Marion Sims, and Sir John Rose Cormack, still I think it worth recording, because it serves as well as another to redraw attention to this important subject, and because it may stimulate those who have opportunities experimentally to determine, first, the condition of the brain in cases of chloroform-poisoning; and, second, the comparative value of artificial respiration in the horizontal and the inverted position. A boy, about eleven years of age, who had previously taken chloroform without any bad symptom, was narcotised in one of the surgical out-patient rooms of the Manchester Infirmary for the purpose of having a large nævus ligatured. Before completing the operation, my attention was drawn to the patient, when I noticed that respiration had ceased; his teeth were closed, there was frothy mucus at the angles of the mouth, the lips were blue, the pulse could not be felt at the wrist, nor, on placing the ear to his chest, could any action of the

heart be detected. Several students were present, and, at my request, one of them held the boy up by his heels, while I commenced to employ artificial respiration on Silvester's plan, his tongue being meanwhile drawn forward by a pair of forceps. No note was taken then of the time that elapsed before a natural respiration was made; and, therefore, no perfectly accurate record can now be given. We are, indeed, little able to calculate the flight of time under such circumstances with precision; and, conscious of the tendency we have under excitement to exaggerate its duration, I did not myself give any estimate; but, on referring to those students who were mere on-lookers, I found that none of them gauged the time at less than six minutes before the first feeble respiration showed there was still life in the lad. Little by little, the respirations increased in strength, he vomited, and, in a few minutes more, was all right again. It is, of course, easy enough to sling up a slip of a lad in this way, but, if we decide to Nélatonise all our patients who show dangerous symptoms, it may chance that some "Manningtree ox" of a man may tax our capacity; would it not, therefore, be desirable, to furnish every operating theatre with a hoist by which the weightiest patients might be at once elevated to the necessary altitude, and kept there till the danger has passed away?—S. MESSENGER BRADLEY, Manchester.

THE ORIGIN OF TYPHOID FEVER.

IN recent numbers of the JOURNAL, attention having been drawn to this subject by Mr. Whitgreave, and more recently by Dr. Hinchliffe, I turned over my cases, and find two which, bearing on the matter, I would place on record. Mrs. B. stated to me that the only manner in which she could account for her having contracted the fever was, that on one hot day, while walking in the Belgrave Road after shopping, as the dustmen were emptying the dustbin, and were in the act of turning the basket over into the cart, she happened to pass by; a whiff of foul air from some decomposing offal seemed to strike her, and the horrid smell never left her nose, nor its taste her mouth, till her convalescence from the fever. The offal, she says, was either a dead rat or cat.

The next patient was a lady, who, finding a most unpleasant smell about the front of her house, for which no one could account, searched everywhere till she came to the dustbin, on opening the door of which, the foul emanations from a decomposing rabbit met her, and were so offensive, that she vomited, immediately sickened, and was for a long time laid low with typhoid, and a tardy convalescence. I am at present attending in a house where an elder member is suffering with typhoid, the younger persons being all attacked with scarlatina; but, whether both these fevers have been generated from the same poison—different structures, and parts of the body being affected by the poison, as it may tend to have an affinity for the intestines, etc., a condition of determination which obtains in typhoid; or, for the throat, skin, etc., which produces scarlatina—I am not in a position yet to decide, although, I believe, such may be the case.

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SURGICAL MEMORANDA.

ON A SLIGHT MODIFICATION IN THE OPERATION FOR CLOSING FISSURES OF THE SOFT PALATE.

THOSE who are in the habit of operating upon these cases, are aware how important is the passing of the first suture, and that its disposition with regard to the edges of the cleft and to the limit of the extent of the fissures, are of the first moment to the subsequent steps. Now it very frequently happens that the processes of freshening the edges of the cleft, and of dividing the muscles, in the hands of inexperienced operators, render the subsequent passing of these sutures a matter of considerable difficulty, even although the patient be chloroformed, and the parts distended by means of the gag.

I have found that the primary introduction of a silver wire suture in the following manner is a great auxiliary to success in the more important steps of the operation, before the edges be freshened, and

* One other suggestion seems worth making to those who have opportunities of experimenting *in corpore vili*. It has recently been shown that chloral hydrate, in its decomposition in the system, produces carbonic oxide, which is presumably the cause of death in fatal cases. May not chloroform, in some measure, produce anæsthesia by the evolution of the same product? and, if so, would not the rational remedy be to have a bag of oxygen gas at hand, and to employ this agent, instead of atmospheric air, when resorting to artificial respiration?