

of sight behind the conjunctiva, we are in danger of wounding the sclerotic coat, as also of not entirely dividing the whole tendon of the affected muscle, the slightest fibre of which being left, is sure to continue what we wish to remedy. All that is necessary in the operation is, the round-edged scalpel and a blunt-nosed pair of common dissecting forceps: my experience bears me fully out in this assertion. The patient is placed with the head reclined back against the breast of some other person—I then take the forceps in the left hand, and seize a portion of the conjunctiva firmly about two lines distant from the outer edge of the cornea, and raising it well up, divide it by one incision with the scalpel in my right hand, the tendon of the muscle is now plain to be seen; I continue my hold with the forceps, and with a light stroke or two with the scalpel, divide the tendinous fibres of the muscle affected; the moment all are severed, the eye will immediately give way, and will be felt by the forceps hand—how infinitely simple this plan is to any one proposed, and how much less liable to mischief than even that of the celebrated Guerin; the operation is easy, the object plain to the view of the operator, and bungling indeed must he be to commit any mistake, with his eyes open, and the parts so fairly exposed. I was for some time reluctant to try my plan upon young children, from their unsteadiness, but I have since applied the plan to patients of seven years old with ease. A friend of mine, Mr. West, of Tonbridge, Kent, has nearly hit on a similar method, without any knowledge of my late operations; he uses the forceps in the same way, but substitutes a blunt-pointed pair of scissors, to divide the tendon after the first incision with the knife: with due deference to his skill, I still prefer the scalpel for all, and dislike the changing of instruments during the operation; and still maintain neither hook, probe, speculum, nor scissors, are at all necessary to its accomplishment. The simpler the operation, the fewer instruments and assistants required; the nearer we approach the true principles of surgery. Guerin acknowledges the operation requires rather more than a minute, whilst I have frequently done it in half that time. In the after treatment I avoid the usual plan of applying pads of linen, with cooling lotions, but direct the eye to have free access to the air, merely protecting it from strong currents by a shade, frequently to be washed with cold water; no inflammation follows; and I have not had a single case where the usual occupation has not been followed without interruption. When I look back on the various and numerous communications of your correspondents on this subject, I feel amused at the many awful responsibilities and frequent causes of failure enumerated, particularly in the very long articles of Mr. Duffin; I must confess the principle of making mole-hills into moun-

tains appears throughout his essays; how that gentleman could have failed so frequently, and find so many difficulties in the way of success to his operations, I cannot conceive; certainly had I met with a tithe of them, I should have given up operating long ago, under the impression of my incapability of performing it. I am prepared to bring my whole experience forward to prove, that to remedy the squint, all that is necessary is to divide the one affected muscle; and that in all cases I have yet seen of squint, no more than one muscle is ever affected. I have often been surprised how small a fibre of the tendon left undivided has still drawn the eye to its accustomed direction. If the whole of the tendon be fairly divided, there is no fear of the eye righting itself. I have in some few cases supposed the tendon entirely separated, but on searching I have found a small fibre left, *not the tendon of any other muscle*, which has remedied the evil on being divided. From the numerous cases I have had, I feel assured that in operations of this kind, where the squint has remained, it does not arise from the efforts of any other muscle, but simply from the whole tendon of the first muscle not being entirely severed. The curious proposition of operating on a sound eye to remedy the evil of its fellow, is too absurd to notice, to say nothing of the difficulty of procuring persons silly enough to allow their sound eyes to be tampered with under such circumstances. The novelty of this operation has given scope to a multitude of inventions and suggestions, many of which are both useless and injurious, and only tend to confuse the practitioner when the nine days' wonder shall have ceased. I have no doubt the scalpel and forceps will outlive all the schemes proposed, and then the operation will arrive at its ultimatum of true surgical principles.

CARIES OF THE TEMPORAL BONE.

To the Editor of THE LANCET.

SIR:—At a meeting of the Westminster Medical Society, Nov. 7, of which a brief report is given in your Journal, two cases of nearly a similar nature (caries of the temporal bone) were mentioned by the President, Mr. Streeter, as having occurred in my practice during the past year. The first I may certainly claim as my patient; but the other came under my care only a short time before his death. As the publication of cases resembling each other in their symptoms and results, when faithfully reported, cannot fail to be of service in directing us to a more correct knowledge and treatment of disease, I send you my notes of these two cases for insertion in your widely circulating Journal.

CASE 1.—Eliza Robinson, aged eleven

years, spare habit, dark hair and complexion, three years ago had measles, since which time she has been afflicted with an offensive discharge from the left ear, with frequent attacks of (what she called) ear-ache, always aggravated on taking cold. I was called to see her on the 18th of March last, when she had been attacked with a kind of epileptic fit, and remained for nearly two hours insensible. From that time her intellect became impaired, and, to use her mother's words, "she always afterwards appeared childish." She suffered intense pain on the left side of the head, and obtained only temporary relief from leeches, fomentations, blisters, and opiates. During the week previous to her death she had frequent rigors, and the last two days became quite insensible. She died in the month of April.

On examining the head twenty-four hours after death, a large abscess was found in the left hemisphere of the brain, containing about four or five ounces of most fetid and discoloured pus, which partially escaped on removing the calvarium; the petrous portion of the temporal bone was in a carious state, evidently of long standing.

The subject of the second case was a child of two years and two months old, which had been under the care of a gentleman of Pimlico, and only became my patient about a week before its death, in consequence of the parents removing into my neighbourhood. This little patient had whooping-cough twelve months ago, since which time it had been always ailing and afflicted with an offensive discharge from the left ear, and general wasting of the body. When I first saw the little sufferer, he was lying on the sofa in a half-comatose state; and it was evident, from the symptoms, that effusion into the brain had taken place; he would take some nourishment on being pressed to do so; had frequent attacks of screaming, and the last two days with convulsions, which terminated his sufferings on the 5th of November.

On removing the calvarium, the convolutions of the brain were remarkably flattened; and on separating the two hemispheres, the ventricles were so distended as to contain, at least, twelve ounces of transparent serum. In this case, as in the former one, the petrous portion of the temporal bone was in a highly carious state. The discharge from the ear had been for some months so offensive, as to render the room scarcely bearable.

My friend, Mr. Streeter, who assisted me in these examinations, referred me to some cases of a somewhat similar kind, mentioned in Dr. Johnson's "Medico-Chirurgical Journal" of Sept. 1, 1824, in giving an analyses of some papers on the pathology of the brain, published by Professor Lallemand, of Montpellier; and I have no doubt some of your readers will call to mind cases

which have occurred in their practice, where, had post-mortem examinations been made, the same appearances might have been expected; and from two cases having happened so recently in my practice, it may be inferred the disease is not rare. I remain, Sir, your obedient servant,

WM. HUGHES, Surgeon.

90, High Holborn, Dec. 1, 1840.

ON THE
NON-EXISTENCE OF INFECTION IN
GONORRHOEA.

To the Editor of THE LANCET.

SIR:—Seeing Dr. Trench, of Edinburgh, has done me the honour of singling me out from among your numerous correspondents to act as a foil for showing forth his powers of argument, I will cheerfully co-operate with him in that humble capacity, and my endeavours shall be that his novel mode of ratiocination may be seen through.

I think Dr. Trench must have read my paper in *THE LANCET* of the 5th September last, with very much less attention than it behoved him to do, as intending to make strictures upon it, otherwise I cannot conceive of his misrepresenting me in the manner he has done. He starts by an insinuation that I wish to set myself up for an inventor; a claim which I especially disown in the very first paragraph of my paper. He next accuses me of ignorance, because I do not quote authors whom he has read. In the first place, there are some of those he mentions which I have never read, others I have read, besides many more whom he does not name; but that he should make his own course of reading the standard of literary acquirements, savours, I think, somewhat of the Procrustian style of argument. Furthermore, I had no occasion to quote others, as my paper was intended to convey my own opinion, founded upon my own observations and experience. This latter, Dr. Trench dismisses rather uncereemoniously (as indeed he does all the other arguments which he cannot answer), by saying that I may have had deceptions practised upon me, and that I may never have met with a case of what he and others call gonorrhoea virulenta. This might have been true, had my observations been confined to some half-dozen cases carelessly seen in a month; but, unfortunately for his surmise, my observations have been carefully made during a space of upwards of sixteen years, and extend to several thousand cases; and therefore I think my opinion entitled to as much consideration as that of any author he has quoted, not even excepting James Cataneus, whose work, I thank my stars, I have never read, and I trust I never shall, if this is a fair specimen, where we are