ABSTRACT OF A CLINICAL LECTURE

ON

THE TREATMENT OF PSOAS AND OTHER LARGE ABSCESES.

Delivered at St. Thomas's Hospital, London.

By JOHN SIMON, Esq., F.R.S.,

Surgeon to the Hospital.

IN reference to several cases of large chronic abscesses under his care Mr. Simon remarked, that the only real difference between psoas and most other abscesses due to diseased bone was, that its cause was deep within the body. If the diseased bone could be removed, the abscess would heal; but the bodies of the vertebrae were out of reach: the surgeon could only mitigate the symptoms, and leave the rest to nature. If the disease were only caries, a cure might result, with more or less angular curvature of the spine; but if necrosis were present there was no chance of a cure, the dead bone was not absorbed, its presence kept up a constant purulent discharge, and this led to anæmia, to albuminoid disease of the liver and kidneys, and finally to death from hectic and exhaustion.

In all these cases of chronic suppuration, the amount of constitutional and visceral damage is closely proportioned to the amount of the discharge: the amount of the discharge is proportionate to the extent of the abscess-cavity, and this depends, to a great extent, on the time it is suffered to extend. The great point in the treatment of these cases is, as far as possible, to prevent the formation of a large pus-secreting cavity. If, therefore, there be any suspicion of the existence of deep suppuration, keep a sharp look out, and open the abscess as soon as you can detect fluctuation, unless the proximity of large vessels, or of other important structures, affords strong reasons for delay.

In situations where the progress of the disease can be watched, as, for example, in abscess of the knee-joint, the difference in the result, according to whether you let out the matter early or not, is very great. If the pus be soon evacuated, there is a fair chance of saving the limb, and even of regaining some motion in the joint; but if the incision be postponed, the joint soon becomes utterly disorganised, burrowing sinuses form, and the neighbouring soft parts become deteriorated by infiltration.

There is, however, this serious difficulty in opening a psoas abscess. Perhaps it forms a large bulging tumour in the groin, yet the patient is fairly well; you cut into it, he at once becomes feverish, and in a fortnight is *in extremis*; then an ignorant person may reproach you with killing the patient. But, however well and strong the patient may appear, it is certain that this febrile condition will supervene sooner or later. It is inevitable. The longer it is postponed the worse it will be, since the cavity of the abscess will be larger. Be careful, then, always to explain to the friends of the patient, that the operation is a serious one, but that the consequence will be more serious the longer it is delayed. The severity of the consequent fever may, however, be greatly mitigated by treatment. Ten days ago, I opened a large dorsal abscess in a little girl, now under my care. I made a free incision, a very large quantity of thick pus escaped, and air was not excluded, yet the child has hitherto had no fever, and appears quite comfortable. All this time the cavity of the abscess is shrinking; and if the fever should now appear, it would be far less severe than it would have been had it occurred immediately after the operation. I owe this satisfactory state of things to the local application of cold; directly the pus was evacuated, an ice-bag was applied, and has been continued since. I have succeeded equally well in a large number of similar cases, and I can confidently recommend ice as an incomparable antiphlogistic.

Of course, if necrosed bone be present, the abscess will not entirely close; a mere sinus will, however, be left, which will not drain the patient to any considerable extent.

As I have said, I do not take extreme precautions to exclude air. At present I am inclined to reserve my judgment as to the value of the carbolic acid treatment, or at least as to the theory on which it is based; it is not yet proved that bacteria are the cause of unhealthy inflammation. Certainly air, as air, does not cause inflammation; and emptying an abscess by aspiration does not prevent the inflammatory process in its cavity. Recent experiments do, however, show that bacteria pass very readily in water, and attached to moist things; and common experience teaches us that infection is much more likely to be carried by sponges and surgical instruments than by mere air. From my own experience, I do not think that air, if only ordinarily pure and dry, is such a poison to surgical wounds as some assert; but, whatever your theory may be,

always carefully disinfect all surgical instruments, etc., with boiling water.

Finally, I must qualify my advice with a caution: remember that fluctuation is not always due to pus. Open early all acute or chronic abscesses, but never cut into collections of blood or synovia. A bruise, in ill-conditioned subjects, may be followed by extensive extravasation of blood, causing a fluctuating tumour, which, if deep in the limb, might easily be mistaken for an abscess. If these extravasations be let alone, and treated with cold applications, they disappear, though they may take a long time about it; but an incision into one is generally followed by grave constitutional symptoms. If well-marked signs of inflammation appear, you must treat the swelling as an abscess; otherwise never open one.

When you are dealing with chronic suppuration, always look out for the chronic cause. The tendency of inflammation is to subside, unless there be a stimulus of some sort present. A man was admitted here some time ago, with a deep wound in the gluteal region, caused by falling on a spike; the wound did not heal, and after some weeks, on careful examination, a piece of his trouser was detected at the bottom. So, again, there is a boy with disease of the knee-joint, in my ward, whose leg has been saved entirely by attention to position. By extension of the limb, and pushing back the femur, we have greatly reduced the inflammation; and whereas the child was before rapidly becoming worse, he is now as rapidly mending. Always treat such displacements in young subjects early and carefully; mere dislocation of the parts will keep up irritation and suppuration, without the presence of any dead bone.

ON THYROTOMY.

By T. HOLMES, M.A., F.R.C.S.,

Surgeon to St. George's Hospital; Professor of Surgery in the Royal College of Surgeons of England, etc.

I HAVE read with much interest Dr. Morell Mackenzie's paper on Thyrotomy. My personal experience of this operation has been at present limited to the single case which I have reported, and on which Dr. Mackenzie makes some observations. There is, I think, much that is just in those observations. But, as I have very recently had the opportunity of seeing the patient (now a young woman), I can rectify some errors in Dr. Mackenzie's view of the case, which were, in great part, caused by its being necessarily reported in a rather imperfect stage, as I had temporarily lost sight of her in her childhood. Let me, in the first place, notice the remark that Dr. Mackenzie makes on my account of the bleeding during the operation. I thought it desirable to state that the parts over the larynx were vascular, and that the division of the cartilage was followed by very free bleeding, in order that any surgeon who might undertake the operation from my description might be prepared for bleeding, which, if he were not so prepared, might be dangerous. I had no intention of saying that, under the circumstances and with the excellent assistance which I had (from my friend Mr. Pick), there was anything alarming in the hemorrhage during this operation. A tube being placed in the lower part of the wound, and the bleeding being kept under for a few minutes by the pressure of a sponge, no further trouble was experienced. Again, although the patient was not able at first to dispense with the tube, she has now been without it for several months, and I have lately performed an operation for closing the fistula left after its removal. There is no recurrence of the disease whatever, and, but for the loss of voice, she is perfectly well.

But now I come to the essential point, on which I am constrained to accept Dr. Mackenzie's criticism—viz., that it would have been better not to perform this operation at all. When the patient was first under my care, she was only a child aged 9; and, although exceedingly patient and tractable, willing to give all possible assistance in laryngoscopy to an extent very unusual with children; yet the parts about the fauces were so small, and when touched by the laryngeal mirror poured forth such abundant secretion, that none of those who examined her (among whom was Professor Czermak); could bring the larynx into view. The symptoms, however, left me in little doubt that there was a growth in the immediate neighbourhood of the vocal cords; and I hoped, by removing this growth, to be able to dispense with the tracheal tube, and restore the child at once to the power of speech. In this I was disappointed; and the result abundantly proves, as Dr. Mackenzie says, that it would have been better for her if I had abstained. Laryngoscopic examination is now easy, and shows that the true vocal cord on the side operated upon is somewhat distorted, so that the glottis cannot close. If the child had been left to wear the tube, without further interference, after the operation undertaken to relieve the spasmodic dyspnoea, until laryngoscopic examination had become possible,

the removal of the tumour would probably have been easily effected from the mouth, and with much less danger both to life and to voice. I may add that I took all possible care to avoid any interference with the tissue of the true vocal cord in the operation. In fact, the tumour was attached, not to the true, but to the false vocal cord. The distortion of the cord is, I suspect, the result of the cicatrisation which followed on the removal of the tumour. If it is objected that the same cicatrisation might have followed after removal by the laryngeal *écraseur*, I would reply that I think this is less likely, from the very circumstance that the base of the tumour would probably have been less easy to reach. In my zeal to extirpate the disease, after having removed the chief mass, I cut away another portion which still projected. If I ever repeat the operation, I would not do this. The stalks of these polypi, like those of uterine polypi, may doubtless be safely left to wither away, after the portion which produces symptoms has been removed. With this precaution, possibly the mere removal of the polypus from the larynx may not involve more danger to the voice in the one method than in the other. But surely the division of the whole larynx, from top to bottom, cannot be effected without risk to the integrity of its mechanism. If there is no risk that some unlucky deviation of the knife may injure the cords mechanically, is there no risk that their structure or muscular mechanism may be injured by the resulting inflammation and cicatrisation? I can hardly bring myself to believe this.

As to the danger to life, I will not interfere in the controversy between Dr. Mackenzie and Mr. Durham on this head, as judged by the results of practice hitherto. But, I think, no one can witness the operation without admitting that it is a very serious surgical proceeding, and that it ought to be reserved for cases of proved necessity. If I offended against this rule in the case, under consideration, I can only plead want of experience. In another case I would follow Dr. Mackenzie's advice—viz., to leave the tube in until a full view of the larynx can be obtained, and I would only perform thyrotomy after the failure of a properly conducted attempt at removal by the mouth. The use of a tracheal cannula for a few years does not interpose any serious obstacle to the closure of the wound after its removal, and would not prevent the complete restoration of the voice, while any injury done to the vocal cord must render this latter result hopeless.

I am surprised to hear from Dr. Mackenzie that the view obtained of the larynx on division of the thyroid cartilage is not ample, and that the opening resulting from the wound is smaller than that of the glottis. I have made no experiments on this head; but anyone could easily satisfy himself of the accuracy of this assertion, by a few observations on the dead subject. In my case (having carefully extended my incision fairly into the thyro-hyoid membrane), I obtained a most ample view of the interior of the larynx by a far larger opening, as it seemed to us at the time, than the glottis. Perhaps this may differ at different periods of life, and in different conditions of the cartilages.

ON THE ANATOMICAL INVESTIGATION OF EPILEPSY AND EPILEPTIFORM CONVULSIONS.

By J. HUGHLINGS JACKSON, M.D., F.R.C.P.,

Physician to the Hospital for the Epileptic and Paralysed, and to the London Hospital; etc.

IN his valuable paper in the JOURNAL of April 26th, p. 457, Dr. Ferrier has drawn attention to some investigations* I have made concerning the bearing of cases of convulsion on the localisation of movements in the cerebral hemispheres. It is very satisfactory to me to find that the results he has obtained from the new method of investigation—the artificial rousing up of the functions of particular parts of the encephalon in lower animals by direct faradisation—agree with the general conclusions I have come to from observing cases of paralysis, convulsion, chorea, etc., in man. The importance of his novel facts, and those of Fritsch and Hitzig to which he refers, for anatomy and physiology, is obviously exceedingly great. But for what is called the *pathology* of convulsions in man, they have a remarkable value.

What is above called pathology, would, however, be more conveniently named anatomy or physiology. Such convulsions, as I shall mention later in this article, are really experiments on parts of the human brain analogous to those Ferrier has made on the brains of dogs, cats, and rabbits—they are experiments made by disease. Indeed, very much of our clinical work is a study of quasi-anatomical and physiological experiments. For example, a large part of our investigations into cases of hemiplegia, convulsion, chorea, etc., is so. I think

it is important to bear in mind that much of our clinical study of disease is *not* pathology; from not doing so, our notions on the "causes" of epilepsy, chorea, etc., are confused. Thus the word "cause" is used by medical men in different senses. It is used for the seat of a lesion, as when it is said that "hemiplegia is caused by disease of the corpus striatum;" for the functional nature of the lesion, as when it is said that "epilepsy is caused by increased excitability of the medulla oblongata;" and for the pathological process when it is asserted that "loss of speech is caused by local softening." But, strictly, the *three* lines of inquiry should be pursued; the causation is to be studied triply in *each* case of nervous disease. It is an Anatomical inquiry to seek the Organ or part damaged (the seat of disease, the Localisation as it is more technically called). It is a Physiological inquiry to search into the defective working of the nervous tissue of the organ damaged—the Functional affection. It is a Pathological inquiry to trace the processes by which the Nutrition of nervous tissue is altered. (Strictly, I suppose, we should speak of pathological anatomy, pathological physiology, and pathological nutrition?) Reversing the order, and putting these three things in the most abstract form, there are abnormalities in the absorption, in the expenditure, and in the distribution of force.

In the study of epilepsy and of epileptiform seizures, it is of very great importance to keep these several lines of investigation distinct; often it is not possible. The importance of doing so is that we shall more easily trace the fundamental resemblances of different symptoms in spite of their superficial differences. For example, a convulsion affecting one side, hemiplegia and hemichorea are alike in that the same muscles are affected, and, therefore, inferentially alike in that the same internal region is damaged; again a convulsion, a sudden stench in the nose are alike in that both depend on the same functional alteration in nerve-tissue, they are alike as disorders of function; thirdly, epistaxis, hæmorrhage in the retina and cerebral hæmorrhage are alike in that they are the accidental results of the same pathological process in arteries—results of the same abnormality of nutrition. A further advantage of thus differentiating our investigation is that we shall learn more exactly *where* our knowledge is deficient.

In this paper I shall speak of the first line of inquiry,—Anatomical, and almost solely with the intention of showing the bearings of Ferrier's researches on the methodical study of the *seat of the lesion* in epilepsies. In future articles I shall consider the Physiology of Epilepsies and their Pathology. As in this article the illustrations I give are from cases of *convulsions*, I can defer the definition I have to give of the term *epilepsy* as I use it.

It is to be hoped that Dr. Ferrier will make careful comparisons of the effects of the local discharges he artificially induces in animals and those artificial or, at any rate, abnormal discharges induced by local disease in human beings. Of course the inference is not to be drawn that the phenomena in the two cases are alike. The differences in the external conformation of animals imply differences in the normal functions of their nervous centres.* These differences will assert themselves, even under excessive and unnatural excitation, whether it be by faradisation or by disease as surely as they assert themselves during healthy activity. But, although this is what one would expect *a priori*, Dr. Ferrier's experiments have the great value of *demonstrating* special differences in different animals. He concludes that "striking differences corresponding with the habits of the animal are to be found in the differentiation of the centres." "Thus," he continues, "the centres for the tail in dogs, the paw in cats, and the lips and mouth in rabbits, are highly differentiated and pronounced." In fact we have in Dr. Ferrier's researches a starting point for a "Comparative Physiology" of the Convulsions. For, so far as comparative anatomists have ascertained homological cerebral structures, so far, it is to be hoped, will he be able to develop the homological functions.

Before we pass to speak of convulsion in man, it is necessary to state certain principles as to the constitution of nervous centres.

The nervous centres represent movements, not muscles; chords, not notes. This is evident from the effects of destroying lesions of the corpus striatum. From a *small lesion* of this body there does not result paralysis of a *small part* of the arm, nor of any such group of muscles as flexors, or extensors; there results *partial paralysis of the whole arm*, the most special parts of it suffering most. There is loss of a certain *number of movements* of the limb. Let us take a more striking example: in cases of *very grave* lesion of the corpus striatum (that is, of a centre far above the supposed deep origins of the ocular motor-nerves), there is, besides palsy of the face, arm, and leg, an ocular palsy. Now this palsy is not of the sixth nerve, nor of the third nerve, nor of

* "On the Anatomical and Physiological Localisation of Movements in the Brain," *Lancet*, Jan. 18, Feb. 1, and Feb. 15, 1873; also "Study of Convulsions," *St. Andrew's Reports*, vol. iii, 1870.

* I would here refer the reader to an article I contributed to this JOURNAL in October, 1869, p. 371, in which I have considered certain symptoms the result of disease or experimental injury of the brain in dogs, etc., in relation to corresponding symptoms in man.