

agreeable one. It occurs, however, sometimes when there is no evidence of fits of any kind. Indeed Dr. Kirkes says: "Frequently a person smells something which is not present, and which other persons cannot smell; this is very frequent with nervous people, but it occasionally happens to everyone." He refers to a case in which the arachnoid was found after death to be beset with pieces of bone, and serofulous cysts were found in the hemispheres.

The above and the following remarks were made *apropos* of a case we saw at this hospital. The patient, a man past middle age, was subject to epileptiform attacks, which usually began by "a smell in his nose," although at other times he could not smell anything. Dr. Jackson said that he had under his care at the London Hospital another epileptic man of similar age, who has the same sort of subjective sensation when his fits are passing off. Now both these patients complain bitterly of mental failure, although they seem to be soberminded, quiet, and sensible men, and one of them keeps at his work. Some time ago a boy who had been under Dr. Jackson's care for fits beginning with the sort of sensation we have mentioned, and who was always odd in his manners, and evidently feeble-minded, became so unmanageable that he was sent to a lunatic asylum. Such a circumstance, however, is not rare in the history of patients who have epileptiform seizures without any disorder of smell, and therefore cannot have any great significance. Yet the case served to make Dr. Jackson think more on—what theoretical considerations had already suggested to him—viz., the possible relation betwixt defects of smell and symptoms of mental disorder.

Defects of smell and defects of mind may seem to be things which can have little to do with one another. But the olfactory bulb has, at the least, a geographical relation to a great part of what is believed to be an important division—the anterior lobe of the cerebrum—of the chief organ of intellectual life. This relation is quite as important in one way as that of the auditory nucleus to the centres for the chief functions of animal life is in another. Perhaps the mere geographical relationship of one olfactory bulb (possibly it might be better to say of the olfactory convolution) to the mass of the anterior lobe of the brain may not strike most people—especially those who think that the brain is a double organ—as a fact sufficiently important to encourage us to spend much time in searching for evidence of any kind of relationship betwixt smell and intellect.

In thinking, as physiologists, chiefly on the common relations of the various organs of sense (with their perceptive centres) to the hemisphere, we must, as physicians, pay equal attention to the wide differences in their geographical position. Although a much inferior sense, the physiological relationship of the centre for smell to the hemisphere is quite as significant as that of the centre for hearing, whilst its geographical relations are much more important. Still, clinical medicine shows that we must be very careful how we interpret the series of symptoms with which loss or defect of the special senses occurs, by reasoning either generally from the basis of a recognition of their relations as similar functional centres, or more narrowly from a consideration of the contiguity of their centres with other parts. Thus, in spite of the common relationship of the perceptive centres to the cerebrum, loss of hearing was rare and loss of sight not unfrequent in cases of disease of the hemisphere. Then as regards contiguity, the auditory nucleus is, Lockhart Clarke says, actually continuous with that of the vagus; and we know that experimental injury betwixt the origin of the auditory nerves (see particularly Roberts on Urinary Diseases) produces temporary glycosuria. Yet we hear nothing of disturbances of respiration nor of sugar in the urine in any cases of sudden deafness; possibly, however, the sugar is not sought for soon after the deafness comes on.

Dr. Hughlings Jackson thinks that, besides the physiological relation of the olfactory nervous system to the hemisphere—the nervous system of the nervous system,—and besides the geographical relations of the olfactory bulb to the anterior lobe as two masses of tissue, there is another relation which these two divisions of the nervous system may be considered to have to one another—viz., by their arterial supply. A single artery—the anterior cerebral—supplies part of the bulb, a great number of convolutions not only of the anterior lobe, but of a great part of the length of the inner surface of the hemisphere, and, what it is very important to bear in mind, the great commissure, the corpus callosum. Here Dr. Jackson entered more at length into the consideration of the differences between division of function and arterial region; and spoke especially of the kind of epileptiform seizures which he believed to depend on disorder of function of the parts in the

range of the middle cerebral artery. He thought it most likely that in those cases of sudden and temporary loss of consciousness in which convulsive movements were slight, or perhaps absent, the disorder of function was chiefly in the range of the anterior cerebral artery; and when there was convulsions, as regards the limbs, chiefly on one side, in the range of the middle cerebral. Dr. Jackson related and quoted several cases, and made many more remarks on this part of the subject—the hypothesis of disorder of function in arterial regions in epileptiform seizures.

Then, as regards the association of defects of smell with symptoms of mental disorder, Dr. Jackson said that in Dr. Forbes Winslow's valuable work on *Obscure Diseases of the Brain*, there was a most important statement, made long before any idea on the relation had occurred to him (Dr. Jackson). Dr. Winslow says: "The insane, in the incipient stage of their malady, are often heard to complain of being exposed to the influence of most noxious and offensive smells." Dr. Lardner in his work on *Animal Physics*, writes that subjective olfactory sensations are not uncommon "with those afflicted with mental derangements," and that "insane persons often complain that fetid or fæcal matter has been mixed with their food."

Dr. Jackson related from his own practice and from medical records many cases of subjective sensations of smell. He had not, however, enough evidence on which to come to a conclusion, and added that although Dr. Winslow spoke so strongly of the association of defects of smell with insanity, he referred to the association of defects of the other special senses with the same malady.

The relations of temporary subjective sensations of light, colour, &c., were next considered, but chiefly in reference to epileptiform paroxysms. Dr. Jackson intends to return to a more full consideration of the whole bearings of the subject (we have now only given an outline of his remarks), and will be glad of any reference to cases and prior observations on the points we have mentioned. There is in the College Catalogue an account of two striking cases. A reference to these cases was made by Mr. Paget ten years ago in a clinical lecture, and from this lecture Dr. Jackson first learned anything definite on this curious symptom. The mental health of these two patients was, however, believed to be good. Dr. Jackson concluded by saying that his experience of cases of insanity was so limited that he had much difficulty in giving the proper value to the facts he had already collected, especially as they were, superficially at least, in apparent contradiction to one another. He is anxious to learn from psychologists what has been done in the matter, and especially if defects or loss of smell are met with in puerperal mania, which form of mania recurs under the same conditions as occasionally give rise to hemiplegia and chorea, both which diseases are due, the former sometimes, and, as Dr. Jackson thinks, the latter generally, to plugging of branches of the cerebral arteries.

Medical Societies.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

TUESDAY, MAY 8TH.

DR. ALDERSON, F.R.S., PRESIDENT.

PATHOLOGICAL AND SURGICAL OBSERVATIONS ON THE DISEASES OF THE EAR. (EIGHTH SERIES.)

ON DISCONNEXION OF THE INCUS AND STAPES; ITS EFFECT UPON THE FUNCTION OF HEARING, AND ITS TREATMENT.

BY JOSEPH TOYNBEE, F.R.S.,
CONSULTING AURAL SURGEON TO ST. MARY'S HOSPITAL, ETC.

THE author begins by some observations on the anatomy and physiology of the chain of bones. He gives an account of the tensor tympani ligament, whereby the membrana tympani and the chain of bones are kept in a naturally resilient state. And he then shows that the function of the chain of bones is twofold: (1) to transmit sonorous vibrations from the drum to the expansions of the auditory nerve; (2) to act as the analogue of the iris in the eye by adapting the labyrinth for the reception of sonorous vibrations having varying degrees of intensity. In proof of the first-named function, the experiments of MM. Sissajous and Dessains are cited, by which faint undulating lines were produced by a slender style attached to the base of the stapes during the vibration of the drum by sonorous undu-

lations. In proof of the second function of the drum, the fact was cited that during the act of listening the stapedius muscle relaxes the membrana tympani and the membrane of the fenestra rotunda; on the contrary, when a loud sound is expected, the tensor tympani muscle draws tense the membrana tympani and the membrane of the fenestra rotunda.

The pathological conditions alluded to in the paper are: (1) simple disconnexion of the incus and stapes; (2) disconnexion of the incus and stapes, the long process of the incus being absent.

1. The author shows that simple disconnexion of the stapes and incus, if attended with no other lesion, is not productive of any appreciable deafness, inasmuch as the tensor tympani ligament is able to keep the two bones in contact, and the action of the tympanic muscles is not interfered with. But if the membrana tympani or its ligament is relaxed, in addition to the disconnexion of the stapes and incus, then the function of hearing is interfered with, and often only to this extent, that the patient can hear only when the voluntary act of listening is performed—that is to say, when by voluntary muscular effort the incus is held in contact with the stapes. In this class of cases, gentle pressure on the outer surface of the drum by any resilient body restores the natural power of hearing, and the distress produced by the necessity of constant listening is quite overcome.

2. But if the membrana tympani or its ligament is much relaxed, then no voluntary effort can bring the stapes and incus into contact, and great deafness is the result. This deafness is also remedied by the application of an artificial membrana tympani, which, gently pressing upon the outer part of the chain of bones, keeps the incus and stapes in contact.

3. This disconnexion of the incus and stapes also occurs in conjunction with partial or complete loss of the long process of the incus, the membrana tympani being entire. The treatment in this class of cases consists in pressing inwards the membrana tympani so as to place its inner surface in contact with the head of the stapes, and to retain the two structures in contact.

The lesions above referred to also take place when the membrana tympani is perforate. When there is disconnexion of the incus and stapes, together with a thickening of the mucous membrane or the ligaments of the articulation, the treatment consists in keeping up gentle pressure upon the outer surface of the long process of the incus; when the long process of the incus is absent, the pressure must be upon the head of the stapes. In order to exercise gentle pressure upon the ossicles and still to allow the muscles to move the ossicles, the author has recently suggested a new kind of artificial drum, in the shape of a small globe of india-rubber containing air.

Mr. HARVEY said he considered the paper of much importance and interest, inasmuch as it would show the aural surgeon the means of preventing much of the mischief this part of the organ was subjected to after long-continued and neglected catarrhal disease. He (Mr. Harvey) had paid some attention to the subject before the Society; and from the experience thus derived he was not surprised to find the incus so often the seat of mischief, and so often displaced, owing to the anatomical connexion of this bone with the mastoid cells. His own examinations had been more especially directed to the ossicles and their capsular attachments, when attacked by rheumatism, gout, and certain forms of syphilis affecting the auditory functions as a consequence, which they often seriously imperil, and it not unfrequently happens that anchylosis supervenes. These lesions cannot be very accurately diagnosed at all times during life; yet he thought a sufficiently correct opinion might in many cases be made from which a rational mode of treatment could be derived, and which would be a source of gratification to the surgeon, and enable him to give much relief, and, in some cases, make a complete cure of the deafness. As regards the treatment of the cases brought forward by the author of the paper, where the incus was supposed to have been displaced, or, as suggested, dislocated from its attachments, he should like to be informed whether pressure alone had been adopted, or whether it had been combined with constitutional treatment. He thought two of the cases cited by the author pointed to such an amount of debility or relaxation of the structures as would have yielded to constitutional treatment—namely, that of giving an increased vitality to the system by tonics. The duration of time required for the cure was also a point of importance. There was another point of very grave consideration, and on which he would like to be more informed—whether the author had ever known epilepsy to have been brought into action by long-continued pressure on the os-

sicula; if so, he thought it would be preferable to pause rather than adopt such expedients as the author had suggested. He regretted that no history had been given of the preparations on the table by which more practical instruction would have been gained. Some useful suggestions thrown out, and many difficulties inherent in the subject-matter, might also have been cleared up.

Mr. BROOK said there was some advantage in the membrana tympani which had not been alluded to. Supposing the power of hearing to be feeble, it was possible, the membrana tympani being entire, to make the external auditory canal a reciprocating cavity by closing the external meatus. If the meatus be closed, and a tuning-fork be then applied to the head near the unclosed ear, it will be best heard by the closed one. Hence the importance of its collecting and communicating vibrations, independently of the pressure it exerts on the tympanum.

Mr. HINTON said that some cases which had come under his observation tended to support what might appear to be the weakest part of Mr. Toynbee's paper—namely, the evidence of relaxation or disconnexion of the ossicula when the membrana tympani was not perforated. In the cases referred to, of which he had seen several, the membrane was thin and relaxed, either altogether or in its posterior portion, and consequently had fallen inwards to such a degree that the head of the stapes projected beneath it. In some of these cases he had found that a stream of air passed into the tympanum, filling the cavity and raising up the membrane, considerably increasing the hearing power. But in others the opposite effect was produced: inflation of the tympanum diminished the hearing, which was restored either by sudden strong inspiration through the nose, or by pressing in a peculiar manner upon the meatus, both of which actions had the effect of placing the membrane visibly in contact with the stapes. In some instances in which an apparently similar collapse of the membrane existed on each side, the opposite effects were produced by inflation, showing that the condition of the parts contained without the tympanic cavity was different. When drawing or forcing the membrane inwards, in the mode above described, improved the hearing, the use of the artificial membrane had frequently been beneficial.

Mr. SAVORY said the author had brought forward so much in his paper that it seemed almost ungracious to ask for more. He (Mr. Savory), however, failed to find a cause for the conditions the author described. It had occurred to him that some of the cases might have had a traumatic origin, and he asked Mr. Toynbee if this view might not throw some light on the particular way in which the disconnexion was brought about.

Mr. TOYNBEE, in reply, said that it was quite possible for the incus to be separated from the stapes as the result of a blow—indeed, one of the specimens indicated such a cause of dislocation; but, as a rule, the incus and stapes were disconnected by one of the three following causes:—(1) Relaxation of the tensor tympani ligament or of the membrana tympani itself. (2) Thickening of the tympanic mucous membrane with catarrh. (3) A loss of substance of the long process of the incus. Desirous of not encumbering the paper with pathological researches, he had deferred entering upon a consideration of the mode in which the pathological changes were effected, leaving the subject for a future paper. In reference to the difficulty of ascertaining the fact of the disconnexion of the incus and stapes when the membrana tympani is entire, Mr. Toynbee said that the affirmative could only be decided when the history, appearances, and symptoms concurred to favour the conclusion. In no case had any brain-symptoms been produced by the use of the artificial membrana tympani; indeed, it was highly improbable that the presence of the drum would produce such a result, as undue pressure on the chain of ossicles, and thence upon the vestibule, produces immediate increase of deafness. In many cases the artificial drum is dispensed with after a short time, as the chain of bones by its use appears to regain its normal resiliency; in other cases the artificial drum requires to be used at intervals. Considerable experience during many years had shown that the use of the artificial drum is attended with permanent advantage. Mr. Toynbee also said, in reply to Mr. Brooke, that in the early study of the subject he had reason to believe that the artificial drum acted beneficially by constituting the tympanum a closed cavity; and he was strengthened in this opinion by the beneficial effect following the use of a bubble of mucilage for the purpose of closing the aperture. Recent researches in pathology and in operative practice had, however, induced him to adopt the conclusions cited in the paper; and he was disposed

to believe that even the beneficial effect of the mucilage was due to the gentle pressure it exerted upon the chain of bones, thus conducing to render it continuous and resilient.

The PRESIDENT thought that much more would be gained by a close study of physiology as introductory to pathological investigation. He said that whilst we were fully acquainted with the mode in which light was impressed upon the retina and so communicated to the brain, notwithstanding that doubt still existed as to the true theory of the nature of light, yet, on the other hand, whilst the vibrations of the air were fully understood, very little that was positive had been ascertained as to the mode in which those vibrations were communicated to the brain.

Mr. TOYNBEE asked permission to say a few words in reply to the remarks of the President. In the first place, it was a mistake to suppose that all the ossicles could be lost without the production of deafness—the presence of the stapes was absolutely requisite to hearing. In the second place, it was only recently that the true action of the stapedius muscle upon the stapes was made out, and the analogy of the base or the stapes to a piston and the inner surface of the fenestra ovalis to a cylinder clearly shown. And thirdly, experiments and dissections had clearly demonstrated that the action of the tensor tympani muscle was to render tense the membrane of the fenestra rotunda by exercising traction on this muscle when the scala vestibuli of the cochlea was exposed, when the fluid in the scala was seen to move outwards; and, on the contrary, when the tendon of the stapedius muscle was pressed upon, the fluid was seen to recede again.

A CASE IN WHICH A NEW OPERATION FOR THE RADICAL CURE OF HERNIA WAS SUCCESSFULLY PERFORMED.

BY ARTHUR E. DURHAM, F.R.C.S.,

ASSISTANT SURGEON TO, AND LECTURER ON ANATOMY AT, GUY'S HOSPITAL.

Stephen H—, a sailor, twenty-six years of age, sought admission to Guy's Hospital for the purpose of being radically cured of an easily-reducible but very troublesome inguino-scrotal hernia on the right side. The hernia had been first noticed six years previously. It had gradually increased in size, and extended into the scrotum. Latterly it had given rise to such constant inconvenience and so much occasional pain that the patient had been quite unable to follow his occupation. He could not wear a truss, although he had repeatedly attempted to do so. At the period of admission to the hospital the scrotal portion of the hernia was about as large as a hen's egg, or rather larger. The bowel, of which it was evident the hernia mainly consisted, was easily returned into the abdomen, but a slight fulness of the inguinal canal persistently remained.

On Jan. 19th, chloroform having been fully administered, and the hernia reduced as completely as possible, Mr. Durham proceeded to operate in the following manner. An incision about two inches and a half in length was made through the skin and superficial fascia, in a direction at right angles to Poupart's ligament, and just over the inner border of the internal or deep abdominal ring. The tendon of the external abdominal oblique muscle was next divided in a similar direction, but to a somewhat less extent, and in a situation slightly further from the median line of the body. The lower fibres of the internal abdominal oblique or cremaster were then separated longitudinally, and the internal spermatic fascia or fascia propria of the hernia was exposed. A slight incision having been made in the lower and deeper part of this fascia, an aneurism needle was carefully insinuated through the areolar tissue, and by its means a ligature was placed between the sac of the hernia and the important structures of the spermatic cord, and carried through the upper and deeper part of the fascia. The fascia and sac were then drawn gently downwards and towards the median line of the body, and the ligature was tied tightly as high up and as far outwards as possible; in fact, as nearly as could be judged, exactly at the internal or deep ring. The ligature thus included the greater part of the circumference of the fascia propria or internal spermatic fascia just where it becomes continuous with the fascia transversalis, the whole circumference of the sac just at its junction with the general peritoneal lining of the abdominal parietes, and within the sac a small plug-like portion of omentum. In passing the aneurism needle, a slight puncture was unintentionally made into the sac. This puncture, however, when subsequently dilated, afforded the opportunity of ascertaining that the sac did not communicate with the tunica vaginalis testis, but that it contained a small piece of irreducible omentum; this was drawn out and cut off below the

ligature. Finally the wound was closed above and below by sutures, which were passed through the sac. The ends of the ligature were left coming out through the middle of the wound.

The after progress of the case was most satisfactory. The patient was kept absolutely in the recumbent position for more than six weeks. From first to last he never had a single bad symptom worth mentioning. There was never any abdominal tenderness, constitutional disturbance, or other indication of general peritonitis; nor was there ever much pain about the site of the operation. The upper and lower parts of the wound healed by first intention. The ligature came away on the eighteenth day, and complete closure of the wound speedily followed. On the 29th March, the patient, having to a certain extent recovered his strength, went into the country. Before leaving the hospital he was examined by all the members of the surgical staff as well as by many visitors. There could not be discovered the slightest hernial protrusion, nor any abnormal impulse on coughing. The inguinal canal from the internal ring seemed perfectly blocked by the obliterated sac and new material.

This case, although a solitary one, may be considered to prove—first, that the method of operation described is practicable; and, secondly, that it is not necessarily attended by danger. The author would add that he believes it to be more scientific in principle than any other method yet adopted, and he hopes therefore it may prove more successful in practice.

Mr. SPENCER WELLS said that the proceeding described to the Society as *new* reminded him very strongly of operations performed centuries ago for the radical cure of reducible hernia. But, as the object of the older operators was to apply a ligature around the neck of the sac, or to obliterate it by scarification or by the introduction of foreign bodies within it, or to destroy it by caustics or the actual cautery, their proceedings proved to be so extremely dangerous, and so often unsuccessful, that when he (Mr. Wells) brought Wutzer's method before the Society in 1854 all operations for the radical cure of hernia were very generally condemned by the profession. Since 1854 Wutzer's operation had been very frequently practised here, and had often proved unsuccessful. But this was because it had been performed in cases for which it was unsuitable. Where the rings have not been much widened and the canal not much shortened it is a very successful operation, and is almost free from danger. Unfortunately, it is only applicable to a very small proportion of cases. Mr. Wood's operation is of much wider application, but it is unquestionably a more serious, not to say dangerous, proceeding. The operation performed by Mr. Durham appeared to be even more hazardous. Free division of skin, fascia, and muscle, and the application of a ligature, even if the sac were not accidentally opened, though not in this case followed by any bad symptom, might certainly be expected to lead to danger in a certain proportion of cases. And it became a grave question whether any very serious operation could be justifiable in cases of reducible hernia. If the hernia could be well supported by a truss, no operation attended with danger to life could be sanctioned. When a truss could not be borne, then Wutzer's operation in cases to which it was adapted, or Wood's operation when the rings were wide and canal short, seemed to offer at least an equal prospect of success, and to be much less hazardous than the operation performed by Mr. Durham.

Mr. HOLTHOUSE wished to know the particular reasons for performing this operation for the radical cure of hernia, and what were its advantages over other operations having a similar object. He thought Mr. Wells was in error in believing that it was the same operation which had been done some centuries ago. In the cases to which Mr. Wells referred the sac had been tied at the external ring, and thus a pouch was left open above the ligature, into which a hernia could descend. In Mr. Durham's operation the sac was tied at the internal ring. Mr. Holthouse thought the new operation was inferior to Mr. Wood's, partly because it was less safe, and partly because it dealt only with the hernial sac, and made no provision for drawing together the sides of the canal. Mr. Wood's operation, he added, blends the sac with the walls of the canal, which thus offers a firm resistance against future protrusion.

Mr. CHRISTOPHER HEATH, without wishing to detract from the credit of a successful case, could not but regard the good result in Mr. Durham's operation as in great measure due to the accidental presence of a piece of omentum within the sac, and which, therefore, served to plug the canal. This occurrence could not be looked for in every case, and even if the sac were opened, as was unintentionally done by the operator

in his case, it would be scarcely possible to draw omentum down with safety. Mr. Heath thought three months too short a time to show the ultimate success of the operation, and was not surprised to hear that after the inflammation excited and the formation of an abscess there was reported to be considerable thickening of the parts. He knew from his own experience and that of others in Wood's operation that very considerable induration disappeared after a time, and he feared that then the peritoneum would again become stretched and the hernia be reproduced. Another point worthy of consideration, he thought, was the advisability of dividing the muscles to the extent recommended by the author. He feared that it would lead to permanent weakening of the abdominal parietes, as was seen after the operation for ligature of the iliac arteries.

Mr. HULKE asked if the omentum was included intentionally?

Mr. KINGDON said that it was not the fatality of former operations which had led to their disuetude, as Mr. Wells had just remarked, but their inefficacy. The former, as well as the present, operations were based upon a false assumption—viz., that there was power in the adhesions thereby instituted permanently to prevent a recurrence of hernia. There was not the slightest evidence in favour of such an assumption, but the recorded evidence of Dupuytren and Scarpa was against it. Sooner or later, as Mr. Heath had stated, the adhesions were sure to yield.

Mr. DURHAM, in reply, said that although he had searched diligently he had failed to find on record the details of any such operation as was described in his paper just read. He therefore ventured to consider his method a new one. With regard to the ancient operations referred to by Mr. Spencer Wells, the descriptions given were for the most part so vague and indefinite as to afford no exact information as to the precise particulars of the methods adopted. In those instances in which the descriptions were more exact, the operations were manifestly coarse and clumsy, and necessarily involved the very sources of danger (such as sloughing of the hernial sac, wasting of the testicle, &c. &c.) which he (Mr. Durham) had been most careful to avoid. The operations most similar to his own were those of Langenbeck and Schmucker. Those surgeons, however, applied their ligature at the external, not at the internal ring. The most that could have been accomplished by such a proceeding, even if successful so far, must have been the conversion of a scrotal hernia into a bubonocoele. No very great gain this. Mr. Durham had tried a new method in this case simply because it did not appear to him worth while to try either of the methods at the present time or recently in vogue—viz., Wutzer's and Wood's. Wutzer's operation had been amply proved by experience to be unsuccessful—nay, worse than unsuccessful. And, indeed, it was so unscientific in principle that the wonder was it should ever have been adopted to any extent. Mr. Wood's method was certainly very ingenious, but his own account of the results of his numerous operations did not seem to him (Mr. Durham) very encouraging. Of the sixty patients whose cases are detailed in Mr. Wood's book, between forty and fifty (a very large majority) were discharged wearing trusses, and in only six or seven of the fully-recorded cases did it appear to have been considered right to dispense with such mechanical supports. Could a hernia be said to be radically cured in the full and proper sense of the word if the patient were still obliged to wear a truss? Further, in less skilful hands than his own Mr. Wood's method had been shown to be by no means free from danger. With regard to the possible and probable risks of his method, Mr. Durham believed that the danger of meddling with the peritoneum, particularly with such portion as forms the hernial sac, had been very greatly exaggerated. He certainly should hardly have expected Mr. Spencer Wells, of all men, to be so very fastidious and fearful about touching a little extension from that general abdominal lining which he (Mr. Spencer Wells) was in the habit of cutting into and mopping out so freely, fearlessly, and with so much impunity. The argument derived from the statistics of operations for strangulated hernia was very bad. That a large proportion of cases in which the sac was opened proved fatal was indisputably true; but the explanation was obvious. Those cases in which it was necessary to open the sac were the most dangerous, for they were those in which the strangulation was most severe or had continued longest, and, therefore, those most likely to prove fatal. There was no evidence to show that the mere opening of the sac increased the danger of the operation; indeed, in a great many cases of recovery if the sac had not been opened the hernia could not have been

returned, and the patients must have died. In the present case there never was the slightest indication of any dangerous symptom whatever. There was some of the "severe inflammation," and nothing of the "extensive abscess" which Mr. Heath had suggested. In conclusion, it appeared to Mr. Durham that he had done intentionally what Mr. Wood only did accidentally—viz., close the hernial sac at the internal ring. On the other hand, Mr. Wood attempted to accomplish intentionally what he (Mr. Durham) allowed in this case to become accomplished as it might—viz., constriction of the inguinal canal and external abdominal ring. In certain cases the association of the two methods would probably be much more successful than either by itself, and such association he should without hesitation adopt.

CLITORIDECTOMY.

To the Editor of THE LANCET.

SIR,—Seeing that you have opened your columns to the discussion of the subject of Clitoridectomy, by the admission of a letter from Mr. H. Gage Moore, of Ipswich, in which mention is made of the London Surgical Home, I cannot, as registrar and assistant-surgeon to that institution, allow such a communication to pass unnoticed.

I have been connected with the Surgical Home for well-nigh a year, and during that time I have diligently watched Mr. Baker Brown's treatment of the particular cases referred to, and, after a sufficiently long period of probation, I have to confess my adherence to his views, while I also venture the affirmation that if any man will truly investigate the subject and give to it an impartial and unbiased consideration he will arrive at the same conclusion. I have yet to learn that Mr. Brown professes to cure every case of epilepsy, and in its several phases; and if some of the cases are unsuccessful does it follow that therefore this special mode of treatment is to be discarded?

Sir, the refutation of such an argument, of which the like is furnished as matter of history in the cases of vaccination and ovariectomy, would neither be profitable to your readers nor worth my trouble.

Mr. Gage Moore has referred to one of those cases in which a relapse has occurred after the patient's return home, and after having received apparently marked benefit from the treatment to which she was subjected in the Surgical Home.

On the other hand I have to present the following case:—S. F., æt. 41, admitted into the "Home" December, 1861; epileptic fits for twelve years. She was a dressmaker, and had so frequently, on her way to and from business, fallen in the streets, that she had been carried into almost every hospital in London, and a large number of open surgeries. The operation was performed on the 19th, and she left on January 20th, 1862, quite well. Here is an abstract of a letter received from her last week:—"Miss S. F. begs to say that her general health is good, and that she has not had the slightest symptom of a fit since she left the Home, and that is nearly four years and six months. S. F. must not omit to say that her sister, who had not seen her for some years, can scarcely believe it to be the same S. F. of years past."

Mr. Moore is pleased to call this operation a "questionable" one. Will Mr. Moore tell me the difference, in this point of view, between applications of lunar caustic or actual cautery, and extirpation of the organ? And the former is practised by several men of great eminence in the profession. The moral aspect of the question has been too much harped upon to the exclusion of the scientific.

I may here remark that nothing is done at the "Home" privately, but a ready entrance is granted to anyone who chooses to visit it. Many have come to learn, and have profited thereby.

I have only further to add that the weight of an objection depends very much on the amount of experience, in the particular instance, which guides the objector. I know not how much Mr. Moore brings to bear on this subject.

Apologising for the length of this communication,

I am, Sir, your obedient servant,

June 12th, 1866.

GEO. GRANVILLE BANTOCK, M.D.

THE Council of the Society of Arts held a conversazione at the South Kensington Museum on Wednesday evening. More than 3000 ladies and gentlemen were present.