of creamy fluid. In looking at these cases it is evident that a few more months would have seen the whole of this morbid material disappear. This sometimes occurs, as in one of the best-marked cases of the disease which I have seen, the one referred to by Dr. Legg as presented to the Pathological Society by Dr. Eastes, and several other similar cases have been reported. I do not look upon this termination as surprising, but as inevitable, and I see no reason why the same should not occur in other organs if they were non-vital. The adrenals are not vital organs in the ordinary sense, seeing that persons live without them for many years, although it is equally true that when diseased they bring about death in a slow and insidious manner. We see the liver waste in the cirrhotic state, and, as regards the kidneys, we sometimes find them in Bright's disease reduced to their minimum size compatible with the existence of life. In the case of the thyroid, the glandular structure may altogether disappear, and as a result of inflammation of the testes every trace of the organ may have gone.

I would say therefore, in conclusion, that if Dr. Legg merely wishes to maintain that the affection of the adrenals so usually found in morbus Addisonii is not tubercular, I cannot but agree with him, having always insisted upon the same fact; but if his paper is written with the object of contravening the views which I have just stated, that the disease of the organs is essentially of one nature, and of that character which has mistakenly been called tubercular, then I am at variance with him. From a study of a large number of cases over many years I have formed the opinion which I have given, and I regard the cases which Dr. Legg has described as highly confirmatory of this view. His paper and remarks thereon are very interesting, and the truth can only be arrived at by a thorough criticism of this kind, but my conclusions drawn from his illustrations are, I am sorry to say, totally different from Dr. Legg's. He would say that, inasmuch as we sometimes see cases of Addison's disease where no trace of the adrenals can be found, the usual theory of their so-called tuberculous nature must be given up. I, on the contrary, would say that the total absence of the organs in very chronic cases proves to my mind indisputably that such process (so-called tubercular) and none other must have led to it.

I am, Sir, yours obediently,
Grosvenor-street, June, 1885. SAMUEL WILKS.

TREATMENT OF ACUTE PERITONITIS BY ABDOMINAL SECTION.

To the Editor of THE LANCET.

SIR,—The abstract of Mr. Treves' paper on this subject, given in THE LANCET of March 14th, gives doubtless a complete view of that gentleman's opinions and action in the particular case narrated, though it does not contain anything more than a slight allusion to a previous work on the subject; but it is quite enough to convince me that he has taken an insufficiently wide grasp of this important advance in abdominal surgery, and that he is not aware of the fulness of the reception which has already been accorded to the practice which he suggests as being proper for cases of acute general peritonitis. So far as I know, the first record of an operation performed deliberately on account of acute peritonitis was proposed and carried through by Dr. Wiltshire in 1868. An account of the case will be found at page 294 of the recent edition of my book on Diseases of the Ovaries, where I say: "I think sufficient praise can hardly be given to Dr. Wiltshire for his courage in performing the operation under such urgent conditions, and it is not too much to say that to his success in this case we owe a new departure in the practice of abdominal surgery by which operations under acute symptoms are undertaken, and apparently with results as satisfactory as those obtained in cases free from emergency." This has long since become the established practice, and I should think that some scores of cases are now on record where it has been completely and successfully adopted.

It is now some twelve years since I myself for the first time followed Dr. Wiltshire's example, and after I had done so I naturally asked myself the question, Why should I limit my operations to cases of peritonitis in which the disease is complicated by an ovarian tumour? Since that time I have never allowed a patient to die of peritonitis without opening the abdomen, if I could obtain the consent of my

colleagues and the patient's friends, and that consent has very rarely been withheld. The results of the great majority of these operations have been published in detail under various headings, because it has rarely been found that the cause of the peritonitis could not be ascertained, and under the head of the cause each case has been recorded, leaving only two instances in which the cause was not ascertained, and which were therefore recorded as operations for acute peritonitis. I have not made an accurate research into my record for the purpose of this paper; but I believe my statement is nearly correct when I say that I have opened the abdomen forty-four times on account of the presence of peritonitis, and that the operation has been completely successful in forty-one of the cases.

This leads me to say that Mr. Treves' case is open to a criticism on two points to which I desire to draw emphatic attention; because, though I give him most willingly all credit for the success of the case so far as it has gone, I think that success is due more to good luck than to good guidance. He tells us the patient was admitted to the hospital "on Jan. 21st, suffering from chronic pelvic peritonitis following severe gonorrhoea." Sufficient has been put upon record concerning this condition to make it perfectly certain that this patient was suffering from pyosalpinx; and if any further proof is needed, we have only to pass on two sentences, and find that the abscess was on the left side, where it always is if pus exists at all in the Fallopian tube. The case was clearly one of antecedent rupture of a suppurating tube, with the formation of a limited abscess, the secondary rupture of this abscess being the cause of the peritonitis. The first point of criticism is, therefore, that this patient ought to have been operated upon within three or four days of her admission to the hospital, and should not have been allowed to go on till Feb. 26th merely for the purpose of running the gauntlet of an attack of acute peritonitis. The second point of criticism is that Mr. Treves has not—at least he does not tell us that he has—removed the Fallopian tube. If the rent, which undoubtedly occurred in it, was sufficiently large and satisfactory to allow for a vent and the healing of the tube, then perhaps the patient may escape without further trouble. But the experience of those of us who have had a large number of these cases to deal with is that a case so incompletely treated as this will simply be the subject of further trouble by-and-by, when the removal of the tube will be a difficult and risky proceeding.

I am, Sir, your obedient servant,
Birmingham. LAWSON TAIT, F.R.C.S. Eng.

AURAL EXOSTOSES.

To the Editor of THE LANCET.

SIR,—In answer to Mr. Dalby, I beg to state that I have never claimed any originality for the operation for the removal of aural exostoses. On the contrary, I have always mentioned the name of Dr. Arthur Matthewson¹ of Brooklyn, New York, as being entitled to all the credit that is due in this matter. Dr. Matthewson some years ago kindly sent me his pamphlet, in which he carefully describes his first case (1876), and from this paper I got my idea of the operation.

Mr. Dalby says that from 1874 he has employed the drill, and in 1876, in THE LANCET, he states, "The operation consists in grinding the bone away, and the most satisfactory appliance for this purpose I find to be the drill which is in common use among dentists." But he has not published any cases for our guidance describing the details of the operation, and I was certainly under the impression that Dr. Matthewson's was the first case. That I am not singular in this opinion, I would refer to the authors² indicated below, who in this connexion never mention Mr. Dalby's name, for they doubtless have all of them missed so brief a notice of what is really a tedious and capital operation.

In the last edition of Mr. Dalby's work, published this year,

¹ THE LANCET, July 20th, 1878; Jan. 8th and 15th, 1881; Jan. 31st and April 1st, 1882; and May 30th, 1885; and British Medical Journal, Nov. 24th, 1883.

² Roosa: Treatise on Diseases of the Ear, sixth edition, p. 436. Barr: Diseases of the Ear, p. 213. Buck: Diagnosis and Treatment of Ear Diseases, p. 118. Macnaughton Jones: Aural Surgery, p. 12. McBride: A Guide to the Study of Ear Diseases, p. 46. Politzer's Text-book of Diseases of the Ear, p. 622 (Cassell). Burnett: A Treatise on the Ear, second edition, p. 306. Cumberbatch: St. Bartholomew's Hospital Reports, 1880. Pomeroy: Diseases of the Ear, p. 117.

he tells us nothing that adds to our knowledge, except, he says, that at the International Medical Congress held in London, Aug. 1881, "I called attention to the fact that in a very large proportion of these cases the subjects of the growths had been for many years addicted to sea-bathing." A fact that I was the first to demonstrate at least six months before.³

Mr. Dalby was present on one occasion when I operated, and in three of my cases of aural exostosis I had the advantage of a consultation with him.

I am, Sir, yours truly,

Lower Seymour-street, June, 1885.

GEORGE P. FIELD.

ON ACQUIRED IMMUNITY FROM INFECTIOUS DISEASE.

To the Editor of THE LANCET.

SIR,—While Dr. Sternberg, in his paper entitled "What is the Explanation of Acquired Immunity from Infectious Diseases?" in THE LANCET of April 11th and 18th of the current year, does not directly claim originality for the "vital resistance theory," a claim is at least implied by his statement that his work antedated that of Grawitz and Salmon. Dr. Sternberg's paper was read at the meeting of the Clinical Society of Maryland held on Dec. 5th, 1884. In it he bases his claim to priority upon a paper published in the *American Journal of the Medical Sciences*, April 1st, 1881. In the discussion which followed, I had the honour to show that at the annual meeting of the Medical and Chirurgical Faculty of Maryland held in April, 1875, I had read a paper (published in the Transactions of the Faculty for that year) in which the theory of immunity expressed by Dr. Sternberg in his paper was distinctly and explicitly propounded. This was publicly admitted by Dr. Sternberg at the meeting at which his own paper was presented. As Dr. Sternberg, while mentioning those who followed him, has neglected to refer, even in a footnote, to my publication antedating his own by six years, I beg that you will favour me by printing the enclosed extract from my paper, a reprint of which I have also the honour of sending.

I am, Sir, your obedient servant,

University of Maryland, Baltimore,

J. E. ATKINSON, M.D.

May 5th, 1885.

Extract.

"It has been stated that all the parts of the tissues of the body whose molecular motion was such that the consequence of their union with the contagious particles was to degrade them until they were unfit for their normal functions were thrown off from the body, and that there were left behind only such portions as were able to repel the contagia, or, having united with them, were able, while undergoing a certain modification themselves, to also modify the life of the contagia, so that the union would result in matter, somewhat altered it is true, but not to such an extent as to interfere with the performance of proper normal function. The body will thus have gotten rid of all matter capable of being degraded by the contagium of the given specific fever. In the course of the subsequent nutrition of this individual, there must be a constant and almost unvarying supply of new material introduced from without. It has been seen that within the system, and endowed with life, this material is termed protoplasm. Now the mere fact of proteinaceous compounds gaining access to the circulation does not confer upon them the properties of this protoplasm. On the contrary, it must be that, as these matters enter the circulation, they may not attain the dignity of protoplasm until life shall have been conferred upon them by their incorporation with the protoplasm already existing in the body, with the buds and gemmules split off from the larger masses, and which, thus nourished, increase in size and develop the properties of their progenitors. Understanding, then, that these buds and gemmules are all descended from the protoplasm that has already resisted or been modified by the especial contagium particle, it is perfectly evident that they and their descendants must possess the same qualities of resistance. At the same time, it will not be difficult to imagine that this condition, being one opposed to the perfectly normal life of the cells of the body, may after a time, in consequence of a steady and persistent struggle to regain the condition enjoyed before infection, be

receded from, and the individual again acquire his susceptibility to the same eruptive fever that he has already experienced. In the same manner may we account for the fact that the occurrence of the diseases in the parent does not necessarily or usually confer immunity upon the offspring; that a certain protection is conferred we know, since the eruptive fevers almost always produce their most dire effects upon virgin populations. The history of epidemics in the Pacific Islands exemplifies this. It is even probable that several diseases, the causes of dreadful mortality in centuries gone by, have disappeared in consequence of an immunity gradually inherited. In spite of this, however, it is very probable that in the reproductive processes the inheritance of perfectly normal molecular motion tends to reassert its superiority over that of a recently acquired modified motion, and that it returns to the mode of life that appertained to the parent previous to the occurrence of the eruptive fever, just as the parent would return, and sometimes does, were his vital processes as active."

HEALTH ASSURANCE ASSOCIATIONS.

To the Editor of THE LANCET.

SIR,—As imitation is the sincerest flattery, I suppose I ought to feel flattered by the fact referred to in your leading article of the 30th ult., that no fewer than three of these associations have been started within the last few months. I do not feel flattered however, I am simply grieved—not because of the fact that these societies have adopted my scheme as far as they can, and one of them has even appropriated the name which I have given to the system without the slightest acknowledgment, but at the misapplication of the scheme, and at the further loss and damage which the profession in particular is sure to suffer if these associations are to be allowed to grow.

On what grounds are my objections based? On these:—The formation of an "Association" for the working of health assurance is not only absolutely unnecessary but positively mischievous—first, by increasing the cost of working the system; and, secondly, by taking the control of the fees out of the hands of the medical practitioner. These are two of the evils of the provident dispensaries, and in these new associations they will be greatly magnified.

First, the cost of working. Where are the expenses of rent of offices, salaries of the secretary and clerks, commissions of agents, and other disbursements to come from? Is it not clear that if the fee charged to an assurer for medical attendance be 20s. per annum, not more than 15s. will find its way into the pockets of the medical attendant? Why should medical fees be thus divided and the profession or the public robbed?

Secondly, the doctor's loss of control over his fees. That this is an evil only too real, the medical officers of provident dispensaries and the various amalgamated friendly societies can bear witness; but the evil is trifling in amount compared with what it will be if the promoters of these aggressive companies are to have their way. One would have thought that the profession had already learned a lesson on this point which would have made it jealous of its rights and duties for the future; but it seems the lesson has not yet been severe enough. I have elsewhere shown that the one remedy *par excellence* for the evils of clubs and many other evils besides is health assurance—not health assurance "associations." For the working of life assurance, and the various schemes of assuring against the losses of accident and sickness, associations are really necessary; but a serious blunder is committed if it be assumed that health assurance can be advantageously worked in the same way. Nor does the blunder affect the medical profession alone. One of the great advantages of health assurance is that the patient is brought into an intimate and friendly relation with his client, in whose health he becomes interested from the moment when the first fee is paid, and he has many and regular opportunities—at the periods of payment for example—of seeing the assurer and his dwelling, observing his state of health, advising him thereon, and detecting the beginnings of disease. Is this a picture of the state of things likely to obtain under a middleman, *alias* an "association"? If the medical profession persists in its policy of indifference and *laissez-faire*—which has already proved so disastrous—in matters so nearly concerning its best interests

³ THE LANCET, Jan. 8th, 1881.