An Address

ON

THE PATHOLOGY OF THE LIVING.

DELIVERED BEFORE THE ASHTON-UNDER-LYNE DIVISION
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MR. PRESIDENT AND GENTLEMEN,—When I had the honour of receiving your most cordial invitation to open the winter session of this Division of the British Medical Association, the thought came to me that I might perhaps be able to interest you by an endeavour to show in what manner our knowledge of the pathology and treatment of abdominal diseases had been modified and enlarged by the work of the surgeon. Those among us who can carry their minds back twenty years or thereabouts will remember that practically all the knowledge we then possessed of the diseases having their origin within the abdomen was based upon clinical observations of the symptoms and signs which the patient displayed during life, and the pathological evidence revealed upon the *post-mortem* table. At the present time, however, as a result of the enormous increase in the number of abdominal operations, a third and most fertile source of information has been disclosed to us. During the conduct of such an operation not only are the parts immediately concerned in the operation laid bare, but other organs also are exposed to our scrutiny and investigation, with the gradual result that we have been enabled to construct a pathology of the living as contrasted with the pathology of the dead. It is of the value of this pathology of the living and of its influence upon our powers of diagnosis and of treatment that I wish more especially to speak to-night.

I can well remember the time when the risks of an abdominal operation were very considerable. When I was first a resident in the Leeds Infirmary there was a long period in which approximately two-thirds of all the patients died after abdominal section had been performed upon them. The record of a year's work was not then a chapter of great surgical achievements; it was a martyrology. This was, in part, due to the large proportion which the desperate, the "too late," cases of obstruction and similar troubles bore to the total number of operations; in part, also, no doubt, to the fact that in surgery, as in all the arts, the hand of the beginner is heavy. It was in those days not an uncommon experience for the surgeon who recorded the discovery of certain pathological states during the operation to be held up to good-humoured derision by his colleagues, or the residents, when the *post-mortem* examination disclosed a state of affairs supposed to be entirely at variance with these. It never seemed to occur to us that the *post-mortem* evidence was perhaps of far less value and significance than that which had been furnished during the life of the patient. Yet it is an unquestioned fact that there is not seldom a greater change in the naked-eye changes of the parts within the abdomen after death than there is in the expression of the face. The features of a man in health change greatly when death comes, and these changes have their counterpart elsewhere.

The knowledge gleaned upon the operation table has shown that at least no small part of the *post-mortem* room pathology is in value insignificant as compared with the pathology of the living. This is, indeed, only what might reasonably be expected. If a patient is suffering from a certain disease, say of the stomach or gall bladder, it is of greater import and of greater service to us to see the exact pathological conditions present at the moment of his illness than it could be to see the same parts months or years afterwards, when unalterable changes, extensive advances, and perhaps a terminal infection, have been added to that early simple condition which first disturbed the patient's health. It is more important for us to know the pathological conditions which cause a patient's present sufferings—a pathological change which is, perhaps, remediable—than it is to know the fullest par-

ticulars of that unhindered morbid change which has at last caused death. Our chief purpose is to heal the living.

A point in reference to the value of *post-mortem* pathology is, it seems to me, too constantly overlooked. The evidence furnished in a series of years in the *post-mortem* room of a general hospital upon any subject is of enormous value; that no one disputes. But the value is necessarily a limited one. To take an example: In a recent discussion¹ Dr. Hale White briefly reviewed the cases in which gall stones had been found on *post-mortem* examination during twenty-five years at Guy's Hospital. Within this period 11,031 autopsies had been performed, and gall stones were found in 333 cases. There are probably few hospitals where so many *post-mortem* examinations are made as at Guy's, and it is doubtful if there is one where they are made with greater care or accuracy. Yet the first thought that occurs to one is that the accumulated experience of all these years at this great hospital is very meagre. The total number of cases in which gall stones or their complications are found is small, a great deal smaller, for instance, than the number of cases that have been operated upon by a few individual surgeons. In mere quantity, that is to say, the entire *post-mortem* experience of this great hospital in twenty-five years is less than the experience of a single surgeon during, say, five or ten years. In actual value the former cannot be compared with the latter. For however carefully the anamnesis of the dead patient may have been taken, his personal contribution to our knowledge is a final and completed thing, amenable to no revision. The surgeon, on the other hand, happily, is able to co-ordinate the details of the patient's history, amplified and more closely scrutinized, if need be, after the operation, with those morbid changes of which the operation has made him fully cognizant. If a single observer had been present at every autopsy at this hospital during all these years (an impossible assumption) his experience would have a less solid foundation in essential facts and observations than that of many surgeons who are now daily engaged in the treatment of patients suffering from this disease. It seems to me indisputable that the evidence which can be adduced from the *post-mortem* records of any hospital, however long the period chosen, in respect of gall-stone disease, of diseases of the stomach, or of the pancreas, or intestines including the appendix, is inconsiderable in quantity and insignificant in value when compared with that which is now available upon the operation table. (For it is, I think, reasonable to assert that patients do not commonly die in hospital as a result of the diseases from which they suffer protractedly during life. The occurrence of an autopsy in hospital upon a patient who has died of calculous obstruction of the common duct may be a rare event—one case in forty years at Guy's Hospital.) In my own work such a case is now seen upon the operation table on an average once in ten days. The death in hospital of a patient who has at any time suffered from gall-stone disease is evidently rare; for in twenty-five years at Guy's Hospital only 333 cases were seen. In six years I have myself seen a larger number than this of patients who were so seriously troubled by this disease that operative treatment was necessary, and Drs. W. J. Mayo and C. H. Mayo, of Rochester, have together operated upon over 1,500 cases. Again, I could quote more than one eminent physician who considers that death from hæmorrhage in cases of gastric or duodenal ulcer is extremely rare, because in all his hospital experience no such case had been known. Yet a little reflection will show that it is hardly to be expected that such cases would be admitted to hospital. Neither hæmorrhage nor perforation occurs except in rare instances when the patient is under treatment in hospital. Within the last twelve months I have seen three patients suffering from chronic ulcer, in two cases in the duodenum, in one in the stomach, who, while awaiting operation, bled to death before they could be admitted into hospital. It is, therefore, hardly necessary to point out that opinions, at times so confidently expressed, which are based only on *post-mortem* experience, which take no account of the sufferings or of the morbid changes in the living, and which show no acquaintance with the risks and results of operative treatment, can make no claim upon our acceptance nor any serious demand upon our consideration.

DISEASES OF THE STOMACH AND DUODENUM.

But my chief purpose is not merely to endeavour to adjust the relative values of the evidence derivable from the dead and from the living, but to attempt to show what are the contributions which have been made by the surgeon to the science of medicine by his study of the pathology of the living! Let me take the diseases of the stomach first. The work of the surgeon has, I think, shown that chronic ulcer of the stomach or of the duodenum is a far more common disease than was formerly believed, and that a very large number of the protracted or recurring cases of indigestion are due to its presence. It is no long time since most of the symptoms due to ulcer were attributed to vices of secretion, to excess or deficiency of hydrochloric acid in the gastric juice, and so forth. But exploration of the abdomen has shown that the part played by these factors in the type of case I have mentioned is so small as to be almost negligible. With regard to duodenal ulcer, it is hardly an exaggeration to say that nothing was known of its symptomatology, and very little could be done for its relief until the surgeon had realized its frequency, and had shown in what manner the patient could be cured. In the textbooks, or special medical works on the subject, duodenal ulcer is considered an infrequent disease, and one very difficult of recognition. Both Riegel² and Ewald³ give such meagre descriptions of duodenal ulcer that it is quite certain that the characteristic clinical picture of this malady is unknown to them, and the opportunity of co-ordinating the anamnesis with the living pathology in a case of duodenal ulcer has probably not occurred to either of them. The work of the surgeon has shown that duodenal ulcer is a common disease; I have myself operated upon 180 cases. In my last published paper I showed that the proportion of gastric to duodenal ulcers in my own cases was exactly as 2 to 1. Yet since then the proportions have altered, and recently I have operated oftener for duodenal than for gastric ulcer. Many of the ulcers which were supposed to be pyloric or in the stomach close to the pylorus we now know to be in the duodenum. To tell exactly whether an ulcer is gastric or duodenal is not always quite easy; a careful examination of the whole area must be made, and the venous ring which usually marks the site of the pylorus defined. By multiplying these careful examinations we have come to realize that duodenal ulcer, at least in the cases that come to the surgeon for treatment, is almost, if not quite, as frequent as gastric ulcer. And little by little the clinical picture has become complete, and a diagnosis of ulcer can now be made with reasonable certainty from the anamnesis alone. This is usually the story the patient tells: After food is taken the patient is free from pain, the period of an hour or two which follows a meal is the best time in the day. At a time varying from one and a half to four hours after the meal a sense of uneasiness is noted in the upper part of the abdomen. A burning, gnawing sensation develops, and there is a bitter taste in the mouth, with, it may be, eructations of food or gas, bitter and acid in taste. The pain, which gradually increases, may be relieved, often considerably, by belching or by pressure. As it increases in severity it strikes through to the back, to the right of the middle line, and it may radiate round to the right side of the chest. As all patients discover for themselves, the taking of food relieves the pain, so that many carry a biscuit in their pockets, or take milk, a dose of an alkaline medicine, or some form of food, as soon as the uneasiness develops. In several cases upon which I have operated the pain has been more severe than this—has been, in fact, indistinguishable from a mild form of hepatic colic; the patient describes the pain, which comes constantly two or three hours after food, as a "colic" or a "spasm." It is not improbable that a spasm of the pylorus is actually present, for such a condition may subsequently be seen during the course of an operation. The pain, it will be noticed, comes on at a time when the patient should be beginning to feel hungry for his next meal; for this reason the term "hunger-pain" which I suggested in a former paper, seems quite appropriate. The interval of relief after a meal varies chiefly according to the character of the food taken. The more substantial the food, the greater the interval of relief.

The appetite is generally good; in fact, often better than the normal if stenosis has not developed. It is not unusual for a patient to say, "I've a good appetite; I can take anything, and I never vomit." If he has given a history of pain, as I have described it, one may be confident that he has duodenal ulcer, without stenosis. Investigation by test meals will show no stasis and perhaps, but by no means always, some hyperacidity.

After a time—a few weeks, a month or two—the symptoms may gradually improve, and even disappear, to reassert themselves after a longer or shorter interval. The patient will then speak of having "attacks" of a certain duration, coming capriciously, leaving spontaneously. In the intervals of these attacks he may be perfectly well, suffer absolutely no discomfort, enjoy food, and gain weight. The attacks are more frequent and more severe in cold weather than in warm. A "chill" is often assigned as a cause of a certain attack, and attacks seem specially apt to come in times of stress and worry. The recognition of duodenal ulcer is most necessary, for it is, I feel sure, a far more serious disease than gastric ulcer, and it is, moreover, one which, in my judgement, should always be treated by operation. I often see and operate upon cases of duodenal ulcer which have been variously diagnosed as "chronic gastritis," "acid dyspepsia," "hyperchlorhydria," etc. But a slender acquaintance with the pathology of the living is all that is needed to connect the clinical history outlined above with a condition of chronic ulceration in the duodenum.

Another, and I think incomparably the most important, result of the surgical work upon the stomach, is concerned with the relationship between chronic ulcer of the stomach and cancer. It is a very remarkable coincidence that several surgeons of large experience have noticed that when the anamnesis of the cases of gastric cancer is studied a clear history of chronic ulcer in the stomach is given by more than half the patients. A medical friend of mine to whom I mentioned this fact wanted to know the evidence which led me to the conclusion that an ulcer had been the cause of these early symptoms. My reply is, that if a patient now consulted me, giving the same history of his present sufferings as the cancer patient gives me of his sufferings of ten or twenty years ago, I would advise surgical treatment in the confident expectation of being able to demonstrate a chronic ulcer as their cause. And here let me incidentally say that as a cause of symptoms the ulcer which cannot be demonstrated does not exist, in my opinion. If an ulcer justifies operation it is an ulcer which can be seen and felt and displayed, to the conviction of the onlooker. Unless a definite ulcer can be seen during an operation there is, in my judgement, no indication for the performance of gastro-enterostomy. If this operation is done for the relief of symptoms dependent upon no demonstrable organic cause the patient will have no relief, and the operation will be thereby discredited.

In cases of carcinoma the removal of the stomach has furnished a few specimens which demonstrate the undoubted connexion between ulcer and cancer. The evidence of the development of cancer in chronic ulcer is from the clinical side as clear as it can be in the majority of cases; the pathological evidence is, of course, more difficult to obtain, because it must almost necessarily be based upon investigations of specimens removed during life. For by the time cancer of the stomach has proved fatal, and the specimen is obtained on the *post-mortem* table, the ravages of the disease are so extensive that all evidences of its origin in an ulcer may be wholly obliterated. But evidence is fast accumulating which will finally and completely dispel any doubts which are still felt as to the direct influence of ulcer in the genesis of cancer. The arguments in favour of the surgical treatment of all chronic ulcers of the stomach and duodenum are already sufficiently strong. No other treatment than that which the surgeon offers can do more than relieve the patient to some extent of his sufferings when once a chronic ulcer is established in the stomach; for if the ulcer lying near the pylorus heal, its cicatrix, while steadily undergoing contraction, is causing a constant mechanical interference with the movement of the stomach. Relief from the misery which this entails is offered only by mechanical means, and no operation in surgery gives more satisfactory results than gastro-enterostomy in

such circumstances. The mortality of this operation is very small; its results are extremely good. But the argument for surgical treatment becomes enormously strengthened when we are convinced of the fact that a large proportion, probably a majority, of the cases of cancer that develop in the stomach are due to the grafting upon the base of an old ulcer of a malignant process. Surgery, then, offers in cases of chronic ulcer not only a relief from present sufferings but an immunity from the possible onset of a disease of a far more terrible character. I hope I may be allowed to insert here, perhaps a little irrelevantly, a plea for the earlier submission to the surgeon of cases of cancer of the stomach. It is simply lamentable to look through the records of the cases of cancer that have been referred to me or to other surgeons for treatment. In the very great majority the disease is too far advanced for anything but a palliative operation, and in no inconsiderable number is the patient so utterly exhausted by his disease that the mere thought of operation is repellent. If only the patients who suffer from cancer of the stomach could be seen at an early stage by the surgeon there is no reason why the results of operative treatment should not be at least as good as they are in cases of mammary cancer.

It is, I submit, only by early exploration of possible cases of carcinoma of the stomach that the knowledge will be gained, by comparison of the symptoms with the pathological conditions then disclosed, which will equip us with the power of early positive recognition of this disease. At this moment the most expert clinician in the world cannot make an early diagnosis of cancer of the stomach. If, then, patients suffering from this disease are to have any hope of cure it is undeniable that the diagnosis must be made by inspection of the parts during life. I think there can hardly be a more promising occasion for the study of the pathology of the living than that afforded in the case of carcinoma of the stomach.

DISEASES OF THE BILIARY PASSAGES.

The work of the surgeon in the diseases of the gall bladder and bile ducts has also been most fruitful in knowledge. In the early days of gall-bladder surgery but little was known as to the signs and symptoms which arose from the presence of stones in any part of the bile tract. The symptoms which are taken to indicate the presence of stones were in fact symptoms called forth by the severe complications to which those stones gave rise. Jaundice, to mention but one symptom, was looked upon as a necessary manifestation before an unequivocal diagnosis of cholelithiasis was justified; yet jaundice is a very infrequent symptom of gall stones, and is not present in by any means all the cases in which a stone is lying even in the common bile duct. During the course of operations upon the stomach, appendix, intestines, or pelvic organs the gall bladder can be examined, and at times stones are found therein of which there was no previous knowledge. By close inquiry after the patient has recovered certain symptoms are elicited which can then with confidence be attributed to the gall-bladder disease of which positive knowledge is now possessed. By such methods, varied in kind, we have acquired the knowledge which is indispensable to an early diagnosis of stone. Accordingly we know that the most frequent manifestation of cholelithiasis is "indigestion," pain after food, coming on usually an hour or so after a meal, great discomfort or cramp in the epigastrium, a feeling of insufferable distension relieved by belching, great flatulence. The pain is sometimes colicky in character. If these symptoms are severe and recurrent, elicited by certain articles of diet and not by others, a diagnosis of gall stones may be safely made. Such a history is always obtained in those cases of advanced disease which are so frequently found on the operation table. In a case of choledochotomy operated upon on the day I write this paper the history given by the patient and by the medical man is that the first "attack" occurred two and a half years ago. In that attack the patient was jaundiced. When I elicited the anamnesis I asked directly, "How many years before this attack had you suffered from indigestion?" The reply came at once, "Oh, I have had *that* trouble for over thirty years." The history I drew from the patient of "that trouble" was the clearest evidence of the presence of stones in the gall bladder; and at the operation the small thickened,

shrunken gall bladder, full of stones and buried in adhesions, was clearly to be held responsible for those faults for which a perfectly healthy stomach had so long been blamed. Jaundice, as I have said, is a very uncommon symptom in cholelithiasis. In my own cases jaundice at any period has been noticed in less than 25 per cent. Jaundice, which Courvoisier called the "cardinal symptom" of calculous obstruction of the common bile duct, may now and again be wanting. A few months ago I removed a stone as large as a billiard chalk from the common duct of a man who was not jaundiced and who never had been jaundiced, and I find that of 31 cases of choledochotomy performed by me during this year, in 7 jaundice was not present at the time of the operation. Yet in 6 of these cases the diagnosis of common duct obstruction was confidently made, because of the rapid succession of attacks of pain, accompanied by shivering and sweating (a rigor, in fact), in a patient who was rapidly losing flesh.

It is a disgrace to our diagnostic acumen to admit it, but it is nevertheless the fact that the impaction of a stone in the common duct is not a rare event. When we have thoroughly learnt our work we shall be able to recognize the presence of gall stones before this serious and tardy complication of an otherwise simple disease has developed. In almost all common duct operations the adhesions are very numerous, and the gall bladder shows evidence of severe and long-standing disease—chronic cholecystitis, with thickening and sclerosis of the walls of the gall bladder, to a degree which may make the organ difficult to discover, or even impossible to recognize; fistulae may be present between the gall bladder and the intestine, the common duct may be so dilated as readily to admit two or three fingers. When the duct is opened it may contain many stones, pus, or offensive bile, and the hepatic ducts may be filled with stones or black tenacious mud. Stones which I have removed have been as large as a walnut, and in one case over 550 stones were numbered, and several thousands were left uncounted. Yet we are gravely told on high authority that a patient with a stone in the duct should not be submitted to operation because he is likely to recover if left alone. This opinion is based upon the fact that in forty years only one case of common duct stone was found on the *post-mortem* table at Guy's Hospital. To deduce such an opinion from such a fact seems to me to be a shining example of a certain inaccuracy of thought which may come from a too exclusive devotion to the pathology of the dead. The most wholesome corrective for such an error is the close study of the pathology of the living. And unless an opinion is based in some measure upon the knowledge so acquired it must always be accepted with some abatement.

We are often told that gall stones may exist for years in the gall bladder without causing symptoms. I do not believe for one moment that this statement is accurate; it is only another of those legacies of error which were first bequeathed to us in the days when none but the dead could disclose the secrets of pathology, and which have since been handed down reverently from one generation to another. The truth is rather that those inaugural symptoms which are caused by the stones as they lie in the bladder are not generally recognized. Because jaundice has not been observed, the complaints of the patients are put down to "neuralgia of the stomach," or are complacently ascribed to some other equally vague "disease." Whenever gall stones are discovered accidentally during the performance of an abdominal operation, such as hysterectomy, a history of symptoms attributable to them can in my experience always be obtained, though it is perhaps rare for an organic cause for them to have been previously recognized. So when a necropsy discloses gall stones it is assumed, with no reason, that because jaundice or other positive symptoms are not recorded the stones have done nothing to excite recognition. I feel confident that it is not accurate to say the gall stones frequently cause no symptoms; it is, on the contrary, quite certain that they frequently, if not constantly, cause symptoms which we are not educated to recognize. From the dead no account can be obtained of their previous sufferings, but from the patients who by accident disclose their stones to the eye of the surgeon much has been, and far more may be learnt. From them we may glean much knowledge of the pathology of the living.

DISEASES OF THE PANCREAS.

It is as a result of the work of the surgeon upon the biliary apparatus that our knowledge of many of the diseases of the pancreas has been born. It was in 1896 that Riedel⁴ gave the first detailed description of chronic pancreatitis. He had noticed that in certain operations performed for cholelithiasis the head of the pancreas was considerably enlarged and was very indurated; its condition was, in fact, very similar to that found in cases of primary malignant disease. Riedel described in full 3 cases; in 2 the patients recovered after operation, and the pancreatic enlargement gradually disappeared; in the third case the patient died, and the microscopic examination of the gland showed that the tumour was due not to carcinoma, but to chronic interstitial inflammation. But until Mayo Robson's paper in 1900⁵ this subject did not attract general attention, nor was the clinical importance of chronic pancreatitis adequately recognized. It was therein demonstrated that many cases formerly regarded as examples of primary carcinoma of the gland were in fact cases of chronic inflammatory induration due to the irritation and infection of gall stones at some time present in the common duct. The discrimination of carcinoma from chronic pancreatitis became at once a matter of the highest importance, for it was clearly recognised that the inflammatory condition was amenable to surgical treatment, whereas cancer of the gland was, and still is, a hopelessly incurable disease. The differential diagnosis remained in all cases difficult, and in some impossible, until Cammidge discovered that the urine afforded important evidence in this connexion. The value of Cammidge's test in cases of pancreatic disease has caused much discussion, and I can only here record the fact that in many doubtful and difficult cases, both before and after operation, it has given me great help.

The fact that the common bile duct is always in close proximity to the head of the pancreas, and that in approximately two-thirds of all cases it actually runs within the substance of the gland, and that the mucous membranes of the common duct and of the pancreatic duct are continuous at the diverticulum of Vater, account for the frequency of the association of pancreatic disease with an infection of the common bile duct due to stone. The inflammation of the pancreas will persist so long as the irritation and infection are continuously excited by a gall stone; but—and herein lies a serious factor—when once the pancreatitis has progressed beyond a certain stage, it may persist even after the stone which first caused it has passed or has been removed. Gifford Nash⁶ and others have related cases in which glycosuria due to pancreatic disease has subsided as soon as the infection of the bile passages was relieved by drainage. In a case of my own, however, a case seen with Dr. Stuart Settle, chronic pancreatitis due to stones in the common duct was found; the stones were removed, and the patient made a good recovery, and remained well for more than a year. Glycosuria then developed, and the patient died of diabetic coma. The sclerosis of the gland which followed upon the chronic inflammation had doubtless involved the islands of Langerhans. Chronic pancreatitis in its early forms no doubt passes often unrecognized; it is relieved by the treatment of the cholangitis which has caused it. In the later stages, however, when surgical intervention has been too long delayed, the condition is one which may prove of the utmost gravity.

DISEASES OF THE LARGE INTESTINE.

There are several conditions involving the small intestine to which I should have wished to refer, but time and space forbid. I will ask your attention for one moment, however, to a remarkable result of a study of the living pathology in the large intestine. In a paper read last year before the Clinical Society of London⁷ I drew attention to certain simple conditions, some hitherto undescribed, in which the mimicry of malignant disease in the large bowel was complete. In operating for supposed carcinoma of the colon, resection of the growth is, if possible, performed. The examination of the specimen subsequently may reveal no evidence whatever of a malignant growth. A hyperplastic tuberculous tumour, a dense inflammatory deposit in the serous covering, or the formation of many false diverticula with inflammation in and around them,

may be found. Such conditions are clinically not usually to be distinguished from malignant growths, and, but for their removal by the surgeon, might have waited long for recognition. Yet they are all innocent conditions, which, when removed, do not recur. Since the case of mimicry of carcinoma by the development of false diverticula, which is recorded in my paper, I have performed colectomy on a second case, and have seen a third in which a fistula had formed between the bowel and the bladder. Indeed the formation of a vesico-intestinal fistula seems to be one of the tendencies of a perforated false diverticulum; a search through the literature has shown that it is far more common than was supposed. In cases where a hard growth in the intestine is accompanied by the passage of flatus and faeces by the urethra, a diagnosis of carcinoma seems irresistible, yet the probability is that "the growth" would be simple, and that the cause of the fistula would be a false diverticulum, which had burrowed its way through all the coats of the bowel, and thence through the wall of the bladder which had become adherent. These mimics of malignant disease are found, of course, not seldom in the stomach, but their existence in the large intestine is not generally recognized.

TUBERCULOUS PERITONITIS.

A further illustration of the advantages of a study of the living pathology is afforded in respect of tuberculous peritonitis. Of the three varieties of this disease, the ascitic, the fibrous, and the suppurative, the former alone lends itself to successful surgical treatment. The first case in which surgical treatment was adopted was the historical one operated upon by Sir Spencer Wells in 1862. The patient was a female, aged 22, who was believed to have an ovarian tumour. She had been twice tapped, on one occasion 18 pints having been drawn off; the fluid reaccumulated, and operation was advised. The abdomen was opened and myriads of tubercles were found studding the peritoneum. The fluid was emptied away and the abdomen closed. The patient recovered after a sharp attack of peritonitis.

After this it rapidly became the custom to operate upon many cases of tuberculous peritonitis with effusion. It was thought to be enough to open the peritoneal cavity, to empty it, and to close the abdominal wound without drainage. After such treatment the patient's recovery was much hastened, and the success of the operation was often very remarkable. A variety of explanations was offered as to the reason for the undoubted effect of so simple a measure. But a closer inquiry into the histories of patients treated in this way showed that the ultimate result was not fully satisfactory; in a certain number of cases the peritoneal affection recurred, and a second or even a third or a fourth operation had to be done. As a result chiefly of the work of J. B. Murphy and W. J. Mayo, we have been brought to realize that the simple emptying of the peritoneal cavity is not enough. They have shown that tuberculous peritonitis is always secondary to a local infection (which however may itself be secondary to disease elsewhere), and that the operation must include, if permanent success is to be ensured, the removal of this local source of infection. This source, which can usually, though not always, be demonstrated, may be the appendix, the pelvic organs in the female, or the intestine, and it is curious that in my last three cases an example of each of these primary infections was encountered. After the fluid is emptied away a search for the primary focus of disease is made, and this must be removed. The results of such treatment are much better than ever before.

I think it is evident that the opportunity for an early pathological examination in these cases of ascitic tuberculous disease is capable of revealing the origin of the infection in the appendix or the Fallopian tubes, whereas a later examination upon the *post-mortem* table could only show the universal havoc wrought by a disease that had been allowed to go on unchecked to a fatal termination. The pathology of the dead in such a case is little or no help to the treatment of the living. But by operating at an early stage the pathological processes are observed at a time when they are so limited in extent as to be removable.

In this very imperfect recital of a few of the results which have come from the work of the surgeon I hope I have said something to convince you that the study of

morbid conditions within the abdomen during the progress of an operation has materially increased our capacity to make a more certain and an earlier diagnosis, and has accordingly equipped us with more efficient therapeutic power. It is, I submit, by a close study of the anamnesis followed by a careful investigation of the parts implicated in the disease, *during the life of the patient*, that the surest foundations for accurate diagnostic power can be built. The surgeon, after hearing the detailed story of an illness has not to wait until death comes to the patient before he can lay bare those pathological processes which have given rise to all the symptoms. He can see and handle the organ or organs affected at the time they are exciting the sufferings of the patient; not at the time, months or years later, when all bounds have been overstepped by the unchecked extension of the disease, in parts laid waste by a late infection. At the time when symptoms are being caused, the pathological changes are open to examination; that is the advantage which comes from a study of the pathology of the living. So far as abdominal diseases are concerned, he is the best diagnostician who spends much of his time in the operation theatre. The lessons there to be learnt are far greater in number and far outweigh in value those that can be learnt in the *post-mortem* room, in so far as they bear any reference to the treatment of the living.

I would, therefore, urge upon all those engaged in practice the desirability of following their patients to the operation table whenever opportunity occurs. The lessons there to be learnt will in practice be of a value beyond all reckoning, and interest in the daily work will be thereby quickened to an unaccustomed degree.

REFERENCES.

¹ *Clinical Journal*, 1907, xxx, 273. ² *Diseases of Stomach*, Saunders's translation, p. 614. ³ *Diseases of the Digestive System*, 1907, p. 198. ⁴ *Berl. klin. Woch.*, 1896, xxxiii, 1 and 25. ⁵ *Lancet*, 1900, ii, 235. ⁶ *Ibid.*, 1902, ii, 1192. ⁷ *Edinburgh Medical Journal*, 1907, i, 228.

THE SACCULAR THEORY OF HERNIA.*

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BEFORE dealing with the subject of this communication, I wish to express my sincere regret that death has recently removed from our ranks one who by his writings has done so much to broaden our views and advance our knowledge regarding the cause and the treatment of hernia. Needless to say I refer to the late Jonathan Macready, whose *Treatise on Ruptures*, the outcome of a wide clinical experience, has long been regarded as the standard work on this subject in the English language.

In directing attention to what Mr. Hamilton Russell has named the saccular theory of hernia, I am perfectly well aware that there is nothing really new in the arguments I propose to advance in favour of this theory, but I do believe there is something really true in the statements I am about to make in support of it.

Inguinal, femoral, and umbilical hernia are so much more commonly met with than the other varieties of abdominal hernia that I propose to confine my remarks to them, but wish it to be clearly understood that I believe the saccular theory offers the only satisfactory explanation of the occurrence of all abdominal herniae which have not a definite traumatic origin.

OBLIQUE INGUINAL HERNIA.

Though inguinal hernia is a very common complaint, has a wide geographical distribution and a very ancient origin—in fact, it is far from improbable that an inguinal hernia first appeared in one of the children or grandchildren of Adam—we are still far from agreed as to its exact causation, and, consequently, still further from agreement as to the best way of curing it.

Every medical man has been taught—and probably medical students are still taught—that there are two distinct types of oblique inguinal hernia: hernia occurring during infancy and childhood, in which the sac is of congenital origin, and the hernia of adult life, in which the sac is said to be almost invariably acquired. This is the orthodox faith maintained by the writers of all surgical

textbooks and generally accepted by the medical profession throughout the world.

Though every man should long hesitate before changing his faith, it is well, in surgical matters at all events, not to rely too implicitly on authority, but to think for oneself, and from time to time to carefully review the evidence upon which our faith rests, and to consider the rules which should guide our surgical conduct.

If the evidence on which the orthodox teaching respecting the etiology of inguinal hernia rests be thus considered, it will, I think, appear somewhat remarkable that the theory of an acquired inguinal hernia sac should have met with such general and lasting approval.

As we have to distinguish between an acquired and a congenital hernia sac, it will be well at the outset to understand clearly in what respect they resemble or differ the one from the other.

The difference between a typically acquired sac, such as occurs when a ventral hernia follows upon a laparotomy, and a typical congenital sac, as seen in the inguinal region when the bowel descends into a sac formed by the unobliterated processus vaginalis testis, is, however, sufficiently obvious to render further comment in this respect unnecessary.

It has long been recognized that the frequent occurrence of inguinal hernia during infancy is due to the presence of a preformed sac, a defective obliteration of the processus vaginalis testis, and the type of sac most commonly met with in infants and young children is exactly the same type as that most commonly met with in adult life, namely, a sac entirely separate from the tunica vaginalis. I wish to emphasize this point because there appears to be an impression in the minds of many medical men that the usual type of hernia sac in infants is one in which the bowel descends into the tunica vaginalis. So far from this being the case, a hernia involving the tunica vaginalis is only met with in about 18 per cent. of all cases.

From clinical experience, then, we know that during infancy and childhood two distinct varieties of the same type of congenital hernia sac are seen: the complete sac which involves the tunica vaginalis, and the incomplete sac which is entirely separate from the tunica vaginalis, the latter being much the more common of the two. In the adult, also, two distinct varieties of the same type of hernia sac are seen: the complete sac which involves the tunica vaginalis, and the incomplete, the latter being much the more common of the two. The general relationship of the sac, whether complete or incomplete, to its coverings is remarkably constant, and similar both during infancy and adult life. In the diagram, *a* and *A* are both obviously

Fig. 1.

of congenital origin; *b*, the typical hernia sac of infancy, is also undoubtedly of congenital origin; *B*, the typical hernia sac of adult life, it is whose origin excites so much interest. It is to this type of sac, the sac of the so-called acquired hernia, that I wish particularly to direct attention. It surely has been the experience of all hospital surgeons to operate for inguinal hernia, possibly a strangulated hernia, which has suddenly and for the first time appeared, and at the operation to discover that the sac includes the tunica vaginalis. In such a case, though the hernia appeared for the first time during adult life, there can be no doubt that a preformed sac had been present since birth.

I have on several occasions operated on adults in whom an inguinal hernia had appeared for the first time and become strangulated. The sac was entirely separated from the tunica vaginalis, had a relatively long narrow neck, and there was no extravasation of blood outside the sac or other indication to suggest that the hernia sac had

* A paper read in the Section of Surgery at the Annual Meeting of the British Medical Association at Exeter, 1907.