with the possibility of establishing a definite standard in the preparations, it can be of still greater service.

Of its use therapeutically, about which so much is said and written *pro* and *con*, we can say that recently, in a few cases which have for several months seemed to remain in *statu quo* under the usual hygienic methods, we have begun its use in accordance with the recommendations of its advocates. Our experience thus far, however, has been too meager to warrant any positive opinion. We can certainly say that we have seen no harm come from its use, and in one or two cases the improvement in symptoms has encouraged us to pursue our investigations still farther.

One word in reference to exercise. One great difficulty in a large sanatorium like that at Rutland is to provide sufficient and healthful amusement to keep the patients content. With men especially, many of them in vigorous condition, chafing at confinement, we allow, in carefully selected cases, a certain amount of freedom of exercise which on general principles some would think unwise. Believing that the moral effect of outdoor games will outweigh the possible deleterious effect of active exercise, we have allowed some of the patients who are approaching a state of arrest of disease to indulge at times in baseball and other games of a less active nature. It should be distinctly understood, however, that these are the exceptions to the general rule. While an occasional mishap may have occurred, we believe that much less evil has come from this than would be naturally supposed at first thought. At the same time there is a marked beneficial effect on the mental condition of the patients in allowing them this comparative freedom. Incidentally it should be said that in properly selected cases we thoroughly believe in the good effect of moderate calisthenic exercises for the chest, and disagree wholly with those who make the (to us) sweeping assertion that every tubercular chest should be kept in as absolute a state of immobility as possible, no matter how slight the amount of disease. That rest of the diseased organs, as far as it is possible to obtain it, is required for cases of very active trouble, we believe to be true; but we also maintain from experience that expansion of the lungs at regular intervals, where the disease is quiescent, is productive of excellent results.

What then can we say of the wisdom of establishing a state sanatorium after these six years of experience at Rutland? Have the results justified the original outlay, and can we urge other states to do likewise? In our opinion there can be but one answer to this question, and that in the strongest affirmative. No one who has studied the question of tuberculosis and who has carefully and conscientiously followed the working of methods for relief of the victims can fail to be struck forcibly by what has been already done at Rutland, first from the humanitarian point of view, and second from the utilitarian side of the question. As case after case comes before our eyes and we watch the gradual change in aspect from one of disease and invalidism to that of health and vigor, and contrast present conditions with the wretched hopelessness of former years, can we, if we have a spark of human sympathy in our breasts, fail to rejoice that we now can hold out the hope of renewed health to many of those who otherwise from lack of means would be practically doomed to die a lingering death?

From the humanitarian point of view alone we feel that the Rutland Sanatorium has given already suf-

ficient justification of its existence in spite of the inevitable discouragement and sense of failure which must come in a certain number of cases. When we consider the utilitarian side, can there be any doubt, when reading the large percentage of those who have returned to their homes and have become active wage-earners again, that the money expended has been fully repaid in the renewed vigor and usefulness of large numbers of our citizens?

It has been, moreover, often said, but the truth cannot be emphasized too much, that quite apart from the good done to the individual patient, stands the far-reaching educational effect of such institutions upon the community. This cannot be reckoned in figures, but that it is a great power for future good no rational person, if he has had even small experience, can deny.

Thus we give the results of our personal experience. That the Massachusetts institution may be but the small beginning of a similar movement all over our country in the fight against tuberculosis is the earnest wish of everyone connected with the Rutland Sanatorium.

WHEN NOT TO OPERATE FOR APPENDICITIS.*

JAMES E. MOORE, M.D.

MINNEAPOLIS.

The profession has spent a number of years discussing when to operate in appendicitis and has arrived at a definite conclusion concerning most cases, but there still remains a class of cases in which there is a decided difference of opinion.

In the past few years the extremists have been losing ground. It is some years now since I have met a medical man who claimed that medical treatment is the best for appendicitis and that operations should not be performed. On the other hand, I am not now personally familiar with the work of any surgeon who says that every case of appendicitis should be operated on no matter when the diagnosis is made, although several of my colleagues held that opinion a few years ago, and I was inclined to believe then that operation at all times and under all conditions should be the rule. At that time we had just learned that by operating on acute appendicitis early we could save nearly all cases. We then theorized that when an operation was so good in one class it must be in all classes. I now believe that our theory was based on false premises, because experience, which is always better than theory, has taught me that there are conditions under which we should not operate for appendicitis. Our extreme practices have resulted in good to our fellow man, although they were pretty hard on him for a time, because we have learned that in appendicitis, as in most things, there is a happy medium, and that there are times when not to operate.

There are still a few exponents of this operative extreme, but they are condemned by their own statistics and their own discussions. Their statistics show a much higher mortality rate than those of surgeons who exercise some judgment in their selection of cases for operation. Surgery deals with human life and it can not be done by fixed rules. If we could say we must always operate, experience would be of little value. All that would be necessary would be to learn the rules. A surgeon, of all men, should avoid fixed rules

* Read before the Minnesota State Medical Society.

and should exercise reason and judgment in every case. He should base his opinion on his own experience, on the experience of others and on the conditions found in a given case, for no two cases are alike. A surgeon should be brave and should operate when he concludes that operation offers his patient the best chance, no matter how desperate that chance, but he has no license to be foolhardy because it is the patient's life that is at stake and not his own. One who operates for a record does not always have his patient's best interest at heart.

All surgeons and most physicians agree that chronic, recurring appendicitis should be operated on in the interim. There are still a few physicians who tell their patients after an acute attack that they are well and need no operation. How they can do so conscientiously is beyond comprehension, because they know that a goodly number of those patients do have recurrences, and some of them with fatal results, while on the other hand, operation in the interim by experienced surgeons is almost without mortality. Many surgeons of large experience can show an unbroken record. My first operation in the interim was performed Feb. 11, 1893, and since that time I have operated hundreds of times without a single death. One patient from Wapeton had chronic appendicitis when I operated on him, but he came to me with chronic obstruction of the bowels, for which a resection of several inches of bowel was made with a fatal result. Patients who have had an acute attack resulting in abscess are sometimes told that they need no operation because the appendix has sloughed away, but when the surgeon operates on these patients he always finds an appendix. It is sometimes abbreviated and shrunken, but still big enough to cause future attacks. At my first operation I was completely disgusted because after cutting my patient both lengthwise and crosswise I found, behind the ascending colon, an inoffensive looking, shriveled up remains of what had been an appendix. I felt that I had subjected my patient to a very grave operation without corresponding benefit. The outcome, however, was eminently satisfactory. Before the operation he had had frequent dangerous attacks of "perityphlitis" with abscess, and since the operation he has been in perfect health.

Because the interval operation is so safe it does not follow that it is always safe to wait for it, for the patient may die before reaching that period. It has been very aptly said that an interval operation is a good thing to recommend in the interval.

I wish to call particular attention to the fact that many people suffer from chronic appendicitis who have never had an acute attack. These patients should be all operated on because the operation is perfectly safe, and because they are almost certain sooner or later to suffer an acute attack with all its attending dangers.

Both physicians and surgeons agree that when a localized abscess is formed an operation is indicated.

All surgeons of large experience, and many physicians agree that under proper conditions every case of acute appendicitis coming under observation within the first forty-eight hours should be operated on at once. I hope and believe that the time is not far distant when this view will be universally accepted. Under proper conditions these operations can be performed with less than one per cent. mortality, and that is better than can be accomplished by any other method. When this rule is adopted by all appendicitis will have

lost most of its terrors, and there will be no ground for controversy except in cases where the proper conditions can not be complied with and where the diagnosis has not been promptly made. Fitz, Osler and other eminent internists are teaching this doctrine now, and it is to be hoped that their influence will convert the family physicians throughout the world, because they see these patients first. When a case goes beyond this stage it is the fault of the patient or his physician, and theirs is the responsibility. The patient's unwillingness to accept an operation does not relieve the physician of his responsibility unless he has given the patient his positive opinion in favor of an operation. It is not the province of the physician or surgeon to ask the patient what he wants, but to advise him what he needs. Fortunately, the laity are so well educated in this matter in most communities that operation is rarely refused when earnestly advised. In communities where early operations are not popular it is the fault of the physicians in that community who have been advising late operations only, and then when they thought the patient was about to die. The surgeon can not say to the physician "bring your appendicitis cases to me any time and I will cure them," but he does say most emphatically, "make your diagnosis early, bring your patient within forty-eight hours of the beginning of the attack and he can be cured ninety-nine times out of one hundred."

The proper conditions on which I wish to lay great stress are hospital facilities and an experienced surgeon. Without these I believe early operations are contraindicated. Fortunately, this is a day of hospitals and surgeons, and there are very few who can not comply with these conditions. If every physician felt it his duty to operate whether he had had experience in abdominal surgery or not, or if it were considered proper to operate everywhere, the mortality would be greater than if no operations were performed.

As a rule the early diagnosis is easy and should be made by anyone competent to practice medicine. That familiar train of symptoms can scarcely be mistaken. Pain very commonly beginning in the epigastric or umbilical regions and gradually extending to the right iliac region; tenderness often all over the abdomen at first and gradually settling down to the region of the appendix; rigidity of the muscles on the right side; vomiting. There are a few cases, however, in which the symptoms are masked and in which a positive diagnosis is difficult or impossible. When the patient is very ill and what few symptoms there are suggest appendicitis, an exploratory opening should be made through the right rectus. The pernicious habit of administering an opiate for every case of bellyache has caused the loss of many lives by obscuring symptoms. Every attack of abdominal pain should suggest the possibility of appendicitis, or some other serious condition.

When appendicitis is complicated by typhoid fever, pneumonia, severe la grippe, acute nephritis, or other conditions which contraindicate an anesthetic, operation should be avoided as a rule, but even under these conditions a localized abscess should be opened under local anesthesia.

When a patient is evidently moribund he should not be subjected to operation, because it does him no good and it does the cause of surgery harm.

I believe that when a patient is not brought to the surgeon's notice until he is evidently convalescing from an attack, he should not be operated on, but

Nature should be allowed to complete her work, and that he should be carried over to the interim and then operated on.

Surgery can not be scientifically done by the calendar. We can not fix a rule that on a certain day we may operate for appendicitis, and on another day we must not. The nearest we come to it is at the beginning where the consensus of surgical opinion based now on a vast experience is that the first forty-eight hours of the attack is the safety period. Within this period the infection is usually limited to its original seat and it can be completely removed. The wound can be closed and all the dangers and annoyance of drainage avoided.

There is a time, beginning with the third day of an attack of appendicitis, when surgeons begin to differ as to whether we shall or shall not operate. We can not hope to settle this vexed question to-day, for it is doubtful if it ever will be definitely settled, but we can contribute our mite in the way of discussion. When I am called to see a case of appendicitis on the third, fourth or fifth day of the attack and the patient is evidently doing well, I do not operate because I have learned that when a patient is doing well at this time Nature is doing her part, and the rule is that the patient will either go on to convalescence without operation, or that a localized abscess will form, making an operation a safe procedure. The danger of a case going wrong under these conditions has been greatly exaggerated. Over zealous surgeons very greatly underestimate Nature's work in these cases. If we could operate as safely at this time as we can in the first two days I would gladly do so, but I tried it a few years ago and failed to improve on Nature's methods, and in this practice I have a host of good company. I am sure that with my improved technic I could do much better now than I did then, but I am too well content with Nature's work under these conditions to want to interfere. It is easy to understand the difference between the early and late cases. Under ordinary conditions an infection does not spread beyond its original seat within forty-eight hours, and the surgeon can remove it, making a complete operation, but after this period the infection has had time to spread and for the first few days Nature has not had time to complete her lines of defense. If the surgeon "butts in" at this time he interferes with Nature's work, and is very liable to fail to take her place successfully, because he can not surely remove the infection and he is liable to spread it.

When the attack has been going on for three, four or five days, and the patient is doing badly, the natural instinct of the surgeon is to interfere. He feels that Nature's efforts have failed and that the patient's only hope is in operation, but it has been demonstrated by Ochsner and his followers that immediate operation is not his only hope, and if statistics are worth anything they have demonstrated that his chances are better to wait. I am still at sea in these cases. I decide each case on its own merits to the best of my ability, and operate or wait as seems to me best at the time, and no matter what course I pursue I sometimes wish I had chosen the other one.

When I am called to a distance to see a patient within the first few days of the attack, I always feel relieved when the attending physician tells me he wishes to consult with me as to the advisability of an operation, for there is always a chance for an honest difference of opinion. It is embarrassing to both surgeon and phy-

sician when the former is called to perform an operation and feels it his duty to advise against it.

The so-called "Ochsner method" has been so generally misunderstood and misapplied that, regardless of its real merit, it has done infinitely more harm than good. Dr. Ochsner is an earnest advocate of early operation for appendicitis. He does not recommend starvation and lavage for appendicitis, but he does recommend them for a spreading peritonitis due to a neglected appendicitis. The treatment, or rather the neglect, of early cases of appendicitis by starvation and lavage has caused the loss of many lives that might have been saved by early operation.

There remains for our consideration that grave class of cases in which the patient has a so-called general peritonitis. I say "so-called" advisedly because it has been demonstrated that a general peritonitis is, to put it mildly, a very rare condition. The patient's abdomen is distended, is hard and boggy to the touch, his pulse is high, he gives a history of having had a fluctuating temperature with possible chills and sweating; he is septic and has been so for some time. I can not understand the mental processes of men who report ten, twelve and sixteen cases of this class in which they have performed the radical operation with a mortality of 100 per cent., and who still advocate the radical operation. These patients do not all die when treated differently. I can still remember going from my own college to the University of New York to hear Professor Loomis deliver his famous lecture on the treatment of peritonitis by four grain doses of opium. His patients did not all die. A few years ago I did perform the radical operation on a very few of these cases, and they all died promptly. Since that experience I have been content to open abscesses locally, and to carry out to a greater or lesser extent the plan of treatment so admirably set forth by Dr. Ochsner, and I have succeeded in carrying a number of these patients over to a safe interval operation. That old dictum so forcibly laid down by the late Dr. Sayre, that the presence of pus always demands free incision, has been overworked. The mere presence of pus is not the point at issue. The question is whether the septic material is being absorbed so rapidly as to overcome the resisting powers of the patient. We all know that Nature does often take care of large quantities of pus, and this is particularly true in the peritoneal cavity. In face of this fact what right have we to perform an operation with such an appalling mortality? What surgeon is present who has not removed tubes from the pelvis which had contained pus for months and sometimes for years? What one among you knowing of the presence of pus in the tubes during the acute stage will not wait for a safe operative period? You often drain through the vagina during the acute stage, but you do not undertake the radical operation at that time.

During the past few months I have had but five cases of appendicitis of this extreme type, one referred by Dr. Cleveland of Osakis, Minn., two by Dr. Ridgway of Annandale, Minn., one by Dr. Cutts of Forest City, Minn., and one by Dr. Tarbell of Watertown, S. Dak. These patients were all treated expectantly, and are all alive today. Had I performed the radical operation on them, judging from my former experience, they would all have died. I am morally certain that some of them would.

In Dr. Cleveland's case no operation was performed for nearly three months, when I performed an interval

operation successfully. In one of Dr. Ridgway's cases several abscesses were opened locally and fecal fistulae formed. I operated later, removing the appendix and closing the fistulae successfully. His other patient, a seventeen-year-old girl, had a most extensive peritonitis, for which I drained through the vagina, evacuating immense quantities of pus. She is now in apparent good health and expects soon to have her appendix removed. Dr. Cutt's patient had pus seemingly in every corner of his abdomen. I opened first high up on the left side and later over the appendix. He was at last report, in April, in good health but had suffered one mild recurrence and still has a fistula. His foolish parents, who are responsible for his former bad condition, have so far prevented him from having his appendix removed. Dr. Tarbell's case is still in bed most of the time, but improving. I have opened several abscesses, and am waiting for the proper time to remove his appendix.

To recapitulate: I believe that there are times in appendicitis, as in all other surgical diseases, when operation is not the best treatment. I believe that the radical operation should not be performed, first, when the patient is evidently moribund; second, when the patient is evidently convalescing; third, when certain grave complications are present; fourth, in the midway cases beginning with the third day when the physician and surgeon are in doubt; fifth, in the extreme cases of suppurating peritonitis.

In conclusion I wish to state that some of these opinions concerning these late cases are in all probability not final with me, and that should I have the privilege of reading to you again on this topic a year or more hence, the chances are that I shall have modified them. I hope to die before I reach a point where I am no longer open to conviction, for I agree with Emerson that a man who never changes his mind probably has no mind to change.

THE CIRCUMSTANCES AND TREATMENT OF BRIGHT'S DISEASE.

ALFRED C. CROFTAN, M.D.
CHICAGO.

THE MODERN CONCEPTION OF BRIGHT'S DISEASE.

The determining feature of Bright's disease is high arterial tension, with resulting cardiovascular changes. The latter lead to nutritional disorders in various parts of the body, and particularly in those organs that are supplied by end-arteries, viz., the kidneys, retina and brain.

The old idea that in Bright's disease the kidneys are always primarily involved, and that as a result of renal inadequacy certain toxic products are retained that produce the cardiovascular changes and the eye and brain symptoms, is untenable, for many cases of Bright's disease occur in which the kidneys are not involved until very late in the course of the disease, long after cardiac hypertrophy, retinitis, albuminuria and brain symptoms have appeared.

True, there are many infections and intoxications of an acute character in which the kidneys are primarily affected. Most of these nephritides, however, run their course without any involvement of the heart, the arteries, the retina or the brain. If occasionally, late in their course, degenerative changes about the heart and arteries, as well as in many other organs of the body, occur, then these secondary events must be considered due to

the general toxemia, that is, to the inevitable result of advanced degrees of renal insufficiency.

These, however, are not cases of Bright's disease. The underlying causes and, above all, the sequence of events are different.

It is unfortunate that the name Bright's disease is retained at all in our medical nomenclature, for what we understood by this term and what Richard Bright originally described are two very different things.

It is still more unfortunate that the terms Bright's disease and nephritis are so commonly employed synonymously. This is radically wrong, for Bright's disease in the modern sense is a systemic disorder that usually produces nephritis, but does not invariably do so, and nephritis, while often due to Bright's disease, may also be due to a great many other causes. It is not tautology, therefore, by any means, to speak of the nephritis of Bright's disease.

THE CAUSES OF BRIGHT'S DISEASE.

The high arterial pressure that ushers in every case of Bright's disease, in all probability, is produced by circulating toxins. The latter, so far as we know, are derived from the abnormal degradation of albumins either in the intestine or in the tissues at large.

A number of pressure-raising principles have been isolated from putrid bowel contents. We also know that many of the intermediary products of metabolism can raise blood tension.

A deranged bowel function always leads to the abnormal disassimilation of albumins, chiefly because putrefactive bacteria gain unopposed sway. The toxic albuminoids and alkaloids generated in this way flood the liver channels, where they should normally be arrested or disinfected. For a while the hepatic cells can withstand the stream of toxic matter that circulates around and through them and can properly exercise their disinfecting properties; but an overwhelming mass of putrefactive material flooding them at one time, or small quantities of putrid excrement irritating them continuously, must needs impair their function and render them inadequate to protect the organism as a whole from poisoning. When this occurs, intestinal toxins filter through into the circulation beyond, and there can exercise their deleterious effects on the heart, on the arteries, and also on those organs that chiefly supplement the disinfecting function of the liver by eliminating poisons, namely, the kidneys.

Hepatic insufficiency produced in this way also leads to the incomplete elaboration of the aforementioned intermediary products of metabolism that reach the liver in the general circulation as poisonous purin bases, ammonium salts, etc., and should leave the liver more highly oxidized, as innocuous uric acid, urea, etc. When the liver cells are inadequate to produce this conversion, then these intermediary bodies are returned unchanged to the general circulation, and thus cause auto-intoxication. That some of these bodies can produce the cardiovascular changes of Bright's disease and some of the renal changes was shown by me in 1900.¹

Another important result of hepatic insufficiency is perversion of the physical and chemical character of the bile. Instead of flushing the bile channels in a broad stream, the bile sluggishly oozes through the hepatic capillaries. The thick and viscid character of the bile favors diapedesis of poisonous bile ingredients from the

1. Croftan: "The Role of the Alloxyuric Bases in the Production of the Cardiovascular Changes of Nephritis." *Am. Jour. Med. Sciences*, February, 1900.