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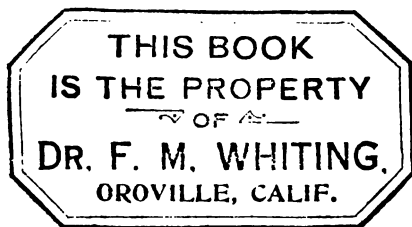


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Dawn
of the
Fourth Era in Surgery

and

OTHER SHORT ARTICLES PREVIOUSLY PUBLISHED

BY

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DAWN

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stopped to think, and receiving just what it deserved at the hands of reviewers, went to every corner of the world, while my book on the subject of appendicitis, sober with the weight of research work, written with every word and sentence in accurately studied position, went to the third edition only. This indicated what has long been suspected, that doctors really have a lot of human nature, and in order to mould professional thought it is not necessary to take life seriously, if at all.

The papers of this group are practically unchanged from the original, and that leaves some repetition which is inevitable, but I have been so thankful to men whose iteration finally brought me unwillingly to their points of view that I am leaving repetitious sentences, and no doubt will use them again in other contributions during the coming years.

Here and there a word has been added for the same reason that one drops an egg into a pot of coffee, and because of Mark Twain's

discovery that the difference between the right word and almost the right word is the difference between lightning and the lightning-bug. The launching of the fourth era of surgery gives a hull made up from my contribution published in the *Journal of the American Medical Association*, August 22, 1908, and from my discussion at the International Medical Congress at Budapest in 1910. The structure is now ready, however, for others to put in masts and machinery.

Wherever the words "normal involution of the appendix" occur in the original publications, a change has been made to "fibroid degeneration of the appendix," and just as soon as the book is published I shall be sorry that a number of other things were not changed also.

THE AUTHOR.

616 MADISON AVENUE, NEW YORK.

July, 1910.

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THE HAND OF IRON IN THE GLOVE OF RUBBER.*

In the Middle Ages there was an ogre on the other side of every hill.

People gradually overcame their fear of ogres, and began to get scared about witches.

After witches had been duly burned, surgeons were frightened about opening the peritoneal cavity. When I was a student, we were taught to have a chill whenever the subject was broached, and the text-books described various ways for avoiding the gruesome mishap.

After ogres, witches, and normal peritoneum had been disposed of, we began to enjoy a fear of pus in the peritoneal cavity.

* A paper read before the meeting of the Surgical Section of the New York Academy of Medicine, February 1, 1907, and published in the *Medical Record*, March. 9, 1907.

Surgeons passed through the transition period of washing their hands after operation instead of before, and arrived at the stage of aseptic preparation of the hands. Then, in logical sequence, we began to wear rubber gloves, and employed them in intraperitoneal work, even though infection were already present. What do you think of that?

Charlemagne baptized a lot of Mohammedans and then laughed up his sleeve when he saw them gravely bowing to the east after that, for he knew they were securely Christians. We put on rubber gloves and smiled at the thought of bacteria doing anything to the patient after we had conscientiously tried to remove infection, which could in fact be removed best by the patient himself. Worse than that, when we put on gloves for a boxing match with the patient's vitality, we rapidly placed him in an unfavorable position for self-defense. The use of rubber gloves made it necessary to use such long incisions that we could

work by sight, and this lowered the patient's vitality.

Long incisions are employed for killing bears, and we chose for saving weak patients the methods which are in use for killing bears.

Rubber gloves led to slow work, and that further reduced the natural resistance of patients. Tait without antiseptics or asepsis showed that his facile fingers could bring out better statistics than we can get with an iron-like hand in a glove of rubber. Tait was a thorn in the side for most of us. Nowadays we understand that he conserved the natural resistance of his patients, and turned the management of infections over to them, but in his day the only comfort that we could get was in the forlorn hope that he might be untruthful. Tait was a perennial insult to us, unless we could get even by making a retort.

Slow work means a longer period of anesthesia—a longer debauch with an intoxicant.

Have we not reached the stage of information where we can drop our fears of ogres, witches, normal peritoneums, and pus in the peritoneal cavity?

Do we not know that much of this pus is sterile, in spite of its odoriferous mercaptans and sulphur ethers, and that bacteria are chiefly at work in the tissues rather than in the pus? Is it not time for us to realize that we cannot get bacteria out of the tissues, but the patient is finely equipped for attending to the work if we do not disable him ourselves?

Simple, quick work which merely turns the tide of battle between bacterium and phagocyte is what the patient needs when he calls us to his aid, and simple, quick work is not facilitated by the use of rubber gloves.

The patient with an infection under way is a factory. His chief business is the manufacture of opsonins and other antibodies for handuffing bacteria, and phagocytes for disposing of them afterward. The hand of iron

in the glove of rubber throws the belts from the wheels in this factory.

We must not forget that even in such a disease as appendicitis, with pus in the peritoneal cavity, some patients recover without operation. How do they manage it? We know. We must not forget that we know how it is done, when we step in to improve upon nature's methods.

Let us not commit taxidermy upon valued citizens by stuffing them with gauze, or lower their natural physiological resistance with the kindly helpful hand reduced to second grade by a glove.

After the period of infection has passed, can we separate peritoneal adhesions in the best way by putting on a handicap, and allow the patient to escape with a mild attack of surgery? Not according to my observations.

There are places in which rubber gloves should be used. In opening an uninfected knee-joint, for instance.

The house staff at the hospital would

transfer too many proliferating colonies of bacteria, if rubber gloves were not worn on rounds when dressings are changed.

In some outside work, as in hernia operations, where we work by sight anyway, rubber gloves will give the best statistics, unless the slower work allows more bacteria to fall in from the air than would be carried in by the hands. That last point is an important one. Among human faculties the sense of proportion ranks just above moral sense in value. Psychologists may not agree with me, but they do not agree with each other. It is discrimination that we need more than rubber gloves. The subject allows of pretty good classification in a general way.

Rubber gloves may be useful: 1. In cases in which there is no infection or other disease to call out the patient's natural resistance to infection. 2. Where dressings are to be changed for several patients in succession, or where the surgeon operates upon an uninfected patient shortly after

operation upon an infected one. In the latter case, if the patient were to be consulted in the matter, he would probably ask to have his operation deferred until spring anyway.

Rubber gloves are not needed—or worse than that: 1. Where infection is already under way, and the patient is calling out his own protection. 2. Where a disease like cancer has already called out such a degree of protection that the oldtime war doctors could amputate a breast and get primary union under “well-waxed shoemaker’s thread” that had been held in the mouth or over the ear while operation was in progress. 3. Where no infection or other disease is present, but where slow or extensive operating necessitated by ungainly gloves will allow more bacteria to fall into the wound than would be carried in by well-prepared bare hands.

The argument has been made that the surgeon should wear rubber gloves for his own protection. For whom are we working?

The coming generations of surgeons, brought up on rubber gloves, will not do the wizard-like work that was done by some of the older operators, excepting in instances where discrimination has been the rule, and where the fingers have been taught better tricks than the eye ever knew.

Our surgery is not different from vegetable surgery. For recreation I hybridize hickories. It became necessary to find an expert grafter. High and low, all over the country search was made. Horticulturists all said the same thing. "Hickories cannot be grafted. Millions of dollars a year would come to us if we could do that grafting." Finally I found a man in Massachusetts who could catch 25 per cent. of hickory grafts, and a man in Texas who could catch 90 per cent. of them. Both said there was no trick about it at all. Nothing was needed but quick, neat work. Surgeons have to face precisely the same situation. A few will do with ease what most others say cannot be done at all. Rubber

gloves will debar surgeons from doing some of the most expert work that lies within the range of their capabilities.

I have no confidence in any man who does not believe that his own country excels in everything, no matter where he lives. From that standpoint of patriotism I want to believe that America has the best surgeons in the whole wide world, and that we are to be the first to drop rubber gloves wherever pathology and mechanics teach that handi-caps are not desirable. Men who understand the principles thoroughly put on rubber gloves thoughtlessly, just as the cautious mother answered, when her little son asked if he might go out on the street to see the comet, "Yes, dear—but don't get too near."

Rubber gloves are pretty, but let us not put on these badges of inferiority for every entertainment. Let us not join with wars and epidemics in doing honor to the shade of Malthus.

GALL SPIDER CASES.*

There are cobwebs in the attic of the abdomen.

There are hieroglyphics on the peritoneum of Darkest Abdomen, and physicians are to read them during the next decade with as great avidity as they read the hieroglyphics of appendicitis, once the key characters had been discovered.

Anatomists have noted that web-like adhesions were found in the bile tract region so frequently that they seemed to be almost a normal characteristic of the region. We used to feel the same way about adhesions in the cecal region. Byron Robinson called attention to the fact that adhesions

* A paper read at the meeting of the Wayne County Medical Society, Detroit, Michigan, May 1, 1905, and published in *American Medicine*, Vol. X, No. 3, pages 95-97, July 15, 1905.

were found in the bile tract region more frequently than elsewhere in the peritoneal cavity excepting in the pelvis in women; and that the cecal region stood third in order of abundance of adhesions.

We cleared up the history of pelvic adhesions first. Then we all turned our attention toward cecal adhesions, after the appendicitis paper of Fitz appeared in 1886.

Now we must all get to work upon the subject of bile tract adhesions.

The subject is one that has been full of surprises to me during the past two years. It is said that one finds whatever he is looking for in surgery. We have not been looking for the facts which are grouped about the cobwebs in the attic of the abdomen, any more than we looked for facts that were associated with cecal adhesions, previous to the year 1886.

How do we explain the presence of adhesions in special peritoneal areas?

In the pelvic cavity in women the presence of adhesions is explained largely by

the fact of open oviducts which allow ascending infection to spread over contiguous peritoneum.

In the cecal region we have a rudimentary organ which has lost some of its power of self-protection during the process of degeneration. When the inner coats of the appendix try to swell within the tight and narrow outer sheath, they become anemic by compression. We all know what happens to these anemic tissues that are vulnerable to the attacks of bowel bacteria, and the explanation for the presence of cecal adhesions belongs to history that is now several years old.

It remains for us, then, to explain the presence of abundant adhesions in the bile tract region.

Lartigau has shown us that bowel bacteria are carried to the liver by the afferent vessels of the portal system, and that on the way back to the bowel again they excite various infections of the bile tract mucosa. Ascending infection from the duo-

denum occurs also. Some of the infections are readily diagnosed when acute and violent manifestations attract attention to the region, but by far the greater number of cases get on with the humble diagnosis of "stomachache," or with a wrong diagnosis altogether. The acute disturbances associated with infections of the bile tract mucosa do not belong to the present subject for discussion. I mention the matter merely to bring in an explanation for the presence of the cobwebs which follow the work of the gall spiders. My feeling is that the thinness of the walls of the bile ducts and gall-bladder furnishes an important point. When there is infection on the mucosa side of the thin-walled structures, toxins penetrate the walls in sufficient force to cause toxic desquamation of endothelium on the peritoneal side. Endothelial cells are shed, plastic lymph exudes, coagulates, and is replaced by connective tissue. That seems to me to be a good enough explanation for the adhesions of

the bile tract region. Webs of adhesion are spun from the gall-bladder to the liver, from the gall-bladder and ducts to the pylorus, from the gall-bladder and ducts to the colon, from the gall-bladder and ducts to the peritoneum over the solar plexus. All of these webs pull upon structures that are normally free, and the result is that the disturbances in gall spider cases lead to as many kinds of diagnosis as we used to make in cases of appendicitis and its complications, or in loose kidney and its complications, or in gastric ulcer and its complications.

It is interesting to note the facility with which physicians make the diagnosis in gall spider cases, once their attention has been directed toward the subject. Before coming to Detroit to-day, I wrote Dr. Longyear, asking him to pick out a gall spider case for operation, so that we might have a clinical demonstration at the hospital. He answered that he was not sure of being able to make the diagnosis, but would try.

The case that he selected was a typical one, as all of you observed at the operation. The patient gave a history of "stomach disorder since childhood." The diagnoses of "tapeworm" and of "malaria" had been made in the case, and the patient had been treated on the basis of wrong diagnosis for years. The diagnosis of "gall spiders" hit the bull's-eye. A few days ago a physician who came as a patient made a diagnosis of the same sort in his own case, after reading a reference to the subject. He had suffered for years, and commanded expert advice, without getting a diagnosis that was right, but his own final diagnosis was perfectly correct, as we proved by operation. Adhesions had bound the gall-bladder to the liver so firmly in his case that the gall-bladder had become partly imbedded in the liver, and there was an area of chronic local hepatitis in the vicinity.

Some of the diagnoses that had been made in gall spider cases in which I have operated recently were these: Torpid liver,

gall-stones, cancer of the gall-bladder, cancer of the stomach, nervous dyspepsia, gastralgia, ulcer of the stomach, chronic dyspepsia, chronic gastroenteritis, intestinal indigestion, malarial hepatitis. Some of the complications in these cases had been treated as diagnostic entities, notably, dilation of the stomach, mucous or membranous colitis, and chronic constipation.

Very many cases of obscure stomach or bowel disturbance that have been the subjects of unsatisfactory diagnosis by experts in the past will soon be readily classed among the gall spider cases. A great many of these cases must have escaped correct diagnosis in my own practice during the past twenty years. The patients must have gone on their way suffering, and holding me, and perhaps the profession generally, in light esteem.

Are there people who do not suffer much discomfort from the presence of cobwebs in the attic of the abdomen? It must be so, because the cases in which adhesions

are present make up such a large part of the whole community. I think that we can fairly make comparison on this point with cases of adhesion of the omentum to the site of midline incisions of the abdominal wall. Some patients are absolutely miserable on account of such adhesions, others have a little discomfort, and others still are not aware of the presence of omental adhesions at all. Then, again, there are a number of abdominal operations in which we seek to obtain peritoneal adhesions. We blow hot and we blow cold with peritoneal adhesions. Assuming that there are people who do not respond to the irritation caused by bile tract adhesions, we must, nevertheless, be sharply on guard to note the cases in which people are life-long invalids on account of such adhesions. The reason why there should be a greater proportion of sufferers from bile tract adhesions than from omental adhesions, is because more important structures are pulled upon by the bile tract adhesions, and because

there are very many more cases of bile tract adhesions than there are of omental adhesions.

Webs from the gall-bladder to the liver seem to cause the least degree of disturbance. Webs from the gall-bladder to the pylorus cause gastric dyspepsia, gastralgia, and intestinal indigestion commonly. I have operated in two cases in which they had caused dilation of the stomach. Constipation is a characteristic symptom in this pyloric group of webs, and it is in this group that we find cases simulating cancer of the stomach and ulcer of the stomach, but on the whole, we are to look for the lesser gastric annoyances rather than the grave gastric disasters among the gall spider cases that are seen in everyday office practice.

Webs extending between the gall-bladder and the colon are a frequent cause for chronic constipation. Webs from the gall-bladder to the peritoneum over the solar plexus cause the most confusing group of

symptoms, and in this group of cases the integrity of the heart is often called into question. In the solar plexus group of cases we have a most uncomfortable lot of patients, and general neurasthenia is common, aside from the nervous dyspepsia, mucous colic, and other evidences of profound and persistent disturbance of the great sympathetic ganglions.

In working out our diagnosis in gall spider cases there is a pair of points of definite importance, and I wish you all to work out the testimony offered by this pair of points in patients who come into the office to-morrow. Each point is situated about an inch and a half from the navel, on either side of the navel, and near the spinal column. I refer to the lumbar ganglions, the tenderness of which gives testimony of diagnostic value on finger-point pressure. The pair of points will serve to help us separate the gall spider cases from four other common sorts of cases in which stomach and bowel disturbance make

demonstration. I refer to (1) eyestrain; (2) fibroid degeneration of the appendix; (3) loose kidney, with or without enteroptosis; (4) pelvic irritations.

In making use of our pair of points, deep finger pressure is made upon the abdominal wall, about an inch and a half on either side of the navel, and the patient will easily tell whether one or both of the lumbar ganglions are hypersensitive or not. In typic cases we get the following testimony: 1. In gall spider cases, neither one of the lumbar ganglions is hypersensitive. 2. In eyestrain cases, neither one of the lumbar ganglions is hypersensitive. 3. In fibroid degeneration of the appendix cases, the right lumbar ganglion is very sensitive. 4. In loose kidney cases, the right lumbar ganglion is very sensitive. 5. In pelvic irritation cases, both lumbar ganglions are very sensitive. If you work out the value of this pair of points to-morrow in the office, the lesson will probably be one never to be forgotten.

One becomes impressed by the fact that the stomach specialist must be first an ophthalmologist, then an operating abdominal surgeon, and last of all a medical or dietetic therapist. I would not minimize the importance of the great work that has been done by gastroenterologists during the past few years; but when we know, for instance, that hyperchlorhydria may be present one week and absent the next, it is time to look to the spinal centers; and when we get as far as that it becomes time to find out what disturbs the spinal centers, and then to remove definitely removable causes for the disturbance.

Many kinds of therapeutic treatment, aside from operation, will relieve for awhile the symptoms caused by bile tract adhesions, and hydrotherapy is by no means appreciated as it should be in this class of cases.

Aside from the value of the pair of points in a diagnostic way, we can get pretty good testimony sometimes from another feature,

the feature of protective muscular spasm of the abdominal wall muscles. The rectus, oblique, and transversalis muscles are firmer than normal while making protective spasm, and we read the rule thus: (1) In gall spider cases there is transitory protective spasm; (2) in eyestrain cases there is no protective spasm; (3) with fibroid degeneration of the appendix there is persistent protective spasm of moderate degree; (4) with loose kidney there is marked persistent protective spasm, excepting in cases of general enteroptosis with flabby muscles or diastasis; (5) with chronic pelvic irritation there is no protective spasm.

One diagnostic feature of gall spider cases is a pretty persistent sense of discomfort "at the pit of the stomach." Sometimes it will disappear for a few days at a time, but generally the discomfort is present most of the time. Just as the fibroid appendix cases form the class in which we are "not quite sure whether the appendix is involved or not," so the gall spider cases

form the class in which we are "not quite sure whether gall-stones are present or not." Many times a fibroid appendix has been removed; the surgeon feared that he had removed a normal appendix, and yet the patient went on to make a remarkable gain in general health afterward. Many a surgeon has been nonplussed by operating for gall-stones; finding no gall-stones, and yet having the patient make a remarkable gain in general health; the fact that adhesions were separated incidentally having been overlooked as a factor. It is safer not to speak of gall-stones anyway. Speak of cholecystitis. If gall-stones are found incidentally, well and good.

Another diagnostic point in gall spider cases is the persistent tenderness on pressure over the bile tract area. Sometimes it almost disappears while the patient is on medical or dietetic treatment, but it reappears pretty promptly when the patient gets ready to change doctors on account of the irksomeness of treatment.

In gall spider cases there is more or less chronic cholecystitis, and Musser is the first physician to take a radical stand and to say that these cases should be turned over to the surgeon for removal of the gall-bladder, on the ground that the gall-bladder, like the appendix, "once infected is always infected." Removal of the gall-bladder takes away the chief focus of infection, but subsequent infective invasions of the common and the cystic bile ducts may lead to further web formation. Separation of the adhesions is, however, the main resource for relief in our gall spider cases.

Three of our remnants, or rudimentary organs, appendix, gall-bladder, and wisdom tooth, certainly make more trouble for us than any three normal organs in the body. The appendix has mechanical weakness, the gall-bladder has such thin walls that toxins apparently penetrate to the endothelial coat, and the wisdom tooth is deficient in calcification. In the surgical

treatment of gall spider cases I first remove the gall-bladder, snapping a pair of forceps on the cystic duct, ligating the duct proximally from the forceps, dividing the duct at a point midway between the ligature and the forceps, and tying in a small rubber tube drain. Adhesions are then separated from all neighboring peritoneum, and prevention of recurrence of adhesions is accomplished in one of two ways. The first way is one that I described some years ago, and consists in making a lymph-aristol coagulum cover all separated points. The second method, which I took up recently after making a series of experiments on rabbits, consists in covering separated areas with sterilized peritoneum from the ox (Cargile membrane). I am not sure as yet which method is best. Both are very satisfactory. Patients only a few hours out of ether, and with the disturbance caused by operation still present, will often say that they have a sense of comfort in the bile tract region that they have not felt for

months or for years previously. The subject is one toward which the whole profession may turn, in the expectation of finding a new and interesting field for diagnosis, and for giving relief to a class of sufferers who go the rounds of the profession with various undiagnosed stomach and bowel troubles.

BACK TO AN OLD IDEA, FOR IT INTRODUCES
A NEW PRINCIPLE IN SURGERY.*

About the middle of the last century a number of surgeons were calling attention to the desirability of rapid operating, and promulgating the idea that patients recovered more quickly when the attack of surgery had been of short duration.

The idea was based upon ordinary observation, rather than upon science, at that time.

Into the field came Pasteur and Lister. The attention of the whole surgical world was diverted toward questions of antiseptics and of asepsis. The patient himself was forgotten in our skilled maneuvers against

* President's address at the twentieth annual meeting of the American Association of Obstetricians and Gynecologists, Detroit, Mich., September 17-19, 1907, and published in the *American Journal of Obstetrics*, Vol. LVI, No. 5, 1907.

the bacterium. Tait stood out alone upon the plain in the midst of the whirlwind, and his statistics were too good to be generally accepted. He stood upon his *ipse dixit* rather than upon a basis of scientific explanation, which to-day can be given.

The dominant idea became that of preventing nature from growing her favorite colonies of bacteria at our expense, and we were to accomplish the task by our artifices. That is the dominant idea right now. It is crude and incomplete, and is shortly to be rounded out by the idea of conserving the natural immunity of the patient, and of holding his opsonic index up, even as the hands of Moses were held up. The patient himself is to be our best ally, and in our pride of achievement with artifices against the bacterium, we are not much longer to disregard such an ally as nature gives us in the patient.

That takes us back to the old idea of the middle of the last century, but it introduces a new principle which can be stated in

terms of classified knowledge—something which could not have been done at the time when the old idea was in practical application.

The surgical patient is a factory. The business of the factory is the manufacture of opsonins for disabling bacteria, and of phagocytes for destroying them. Our new idea is to stop disturbing this factory with surgical methods which interfere with its output of opsonins and phagocytes.

How are we to do this? By avoiding long debauch with intoxicating anesthetics. By not choosing, for patients who are to be saved, a type of incision that was popular with buccaneers. By not allowing the patient to be bitten to death by a pack of snapping artery forceps. By disturbing as little as possible viscera which ring up the central stations of the sympathetic ganglia whenever they are touched. By not wasting the patient's vital energy through unnecessary detail in conscientiously carrying out perfected technic. The cap of the climax

of the dominant idea was the introduction of the rubber glove. This last refinement of our art intensified the worst features of our methods.

I often say to the physician of a patient: "Now do you wish an ideal operation, or would you rather have me save your patient? It seems to me that the most important single feature in helping the business of the factory is rapid operating. Not hurried work, but expeditious completion of necessary steps, and to this point I will devote the chief part of my address.

In Philadelphia two surgeons of about the same capabilities have different statistics. A house surgeon serving under both was asked the reason for the difference in statistics and his reply was "about ten minutes."

I asked one of our professional anesthetists who works with from two to ten surgeons a day, what, in his opinion, was the most common fault among surgeons. His answer was, "Puttering and unnecessary attention to detail in technic." On that

same afternoon, working with another anesthesiologist, I quoted the first one, and asked if he agreed. He said, "Precisely; there are surgeons for whom I will not work, and I am engaged when they call for me. I have seen patients' chances for recovery lessened and have seen patients killed outright, in cases in which they could easily enough have recovered."

I wrote to two other professional anesthesiologists on the point. One of them replied: "I have observed beyond all question that patients do best when the surgeons do quick and, particularly, gentle work. These patients rarely have shock, and they recover easily from the effects of the anesthetic." The other answer was: "I certainly do believe that not only are the patients' chances for recovery lessened in certain cases, but whatever chances they had were lost." The public knows nothing of this sort of thing, and very many physicians are unfamiliar with its meaning. How is it that surgeons have come to this sort of criticism on the

part of professional anesthetists who are generally recognized as competent to criticize? It is through conscientious perfection of art which leaves the patient out of the structure. The play proceeds while the hero is too ill to be present. It was the introduction of anesthesia that first brought about a change to methods which are better for the patient in one way and worse in another way. The law of compensation worked out there as it does in most other fields of human activity. Men who had worked rapidly because of compassion alone, adopted slower methods after the introduction of anesthesia, and sometimes became so deliberate in their movements that the patient was injured without the surgeon's cognizance of the situation. Then came our study of the bacterium, and that still further opened up vistas of observation which left the patient out of the horizon.

It was during the middle of the last century that the fine idea of rapid operating started to grow into an imposing feature of

the landscape, but it was blown down before reaching prime by anesthesia and by antiseptics. Now it will sprout from the stump again. Among the very earliest writers, the ancient writers, there is no reference to rapid operating. Their works were mostly didactic and for novices, and not for trained men. To have counseled rapidity of operating would have entailed too much responsibility, and tyros would have been tempted to place speed before other requisites, perhaps. The relationship between the duration of an operation and the degree of shock is essentially a modern discovery. I have been surprised in looking over the literature of the subject to observe how modern a discovery it is, and how essentially English and American it is in development.

The old Romans made no reference to the matter of rapidity of operating. They divided surgeons into the *Vulnerarius* who treated wounds with dressings, and the *Carnifex* who operated for stone and hernia.

The Carnifex was barely tolerated by the Romans. The Greeks at that time could not do much operative surgery because they did not understand the ligation of arteries. Celsus is the first writer to the point, and he is opposed to rapid work, saying that the surgeon must not have compassion which will lead him to hurry. Celsus bears merely upon the point of "hurry" to condemn it, and most of us to-day will agree with that particular feature. There is a distinct difference to be made between hurry and safe rapidity.

Ambrose Paré, in the sixteenth century, speaks of rapidity. He defines surgery as "a quick motion of an intrepid hand, joined with experience."

The next great writer is Heister, in the seventeenth century. He quotes Celsus with approval and states that the operator "should use expedition, but not hurry."

Coming to the English school, Sir Astley Cooper does not mention rapidity, and states that self-possession and knowledge

of anatomy are of first importance. Sir Robert Liston praises skill, caution, dexterity, adroitness, and tempered boldness, but does not refer to time spent in operating.

A number of contemporary teachers such as Bell, Lizars, and others do not go into the matter of qualifications and requisites at all.

Velpeau's great work contains no reference to time saving, excepting when syncope (shock?) sets in, and he says that it may then be necessary to complete an operation quickly, or to finish it at a second stage.

Just after the introduction of anesthesia Skey awoke to the fact that shock might be inseparable from any operation, and that the duration of operations on anesthetized patients was introducing an element of danger, "for the duration may exceed the endurance of the patient."

Henry H. Smith goes out of his way to condemn rapidity *per se*, and believes that the day of rapid surgery is happily past.

He does say, however: "Safely at all events, quickly if you can."

Morand, a Paris surgeon, wrote a short article in 1772 on the subject of the old adage, "Tuto, cito, jucundo," and said that it applied to surgery quite as well as to medicine. "One should operate safely, quickly, and pleasantly." Bardeleben, in 1874, expresses the idea of Morand as though it were his own. The surgeons of the past century did not, as a rule, deal with the conduct of operations in general in such a way as to bring out the matter of rapidity of operating for the sake of preserving the patient's strength, but Bardeleben, in the seventh edition of his work (1874) says that "safety is the first consideration, but rapidity is a goal to be aimed at, and is sometimes indispensable. Cases occur in which protracted pain can lead to death, quite as well as does loss of blood. Rapidity need not be the same in all stages of operating." Elsewhere he again states that prolonged duration of operation may lead to exhaus-

tion of the patient. He does not mention shock, but speaks of syncope, which would probably be held to mean about the same thing to-day.

Treves, in his "Operative Surgery" (1892), gives pictures of the rapid operator of the past, conceiving him in the light of a player to the gallery, and giving him no credit at all for trying to preserve the patient's energy. He makes light of dexterity in surgery, as compared with the dexterity of the artisan, and says that "the days of brilliancy are past."

In this connection I would say that some of us who are interested in brilliantly preserving the natural immunity of the patient—holding it on a par with the principle of chemically and mechanically attacking bacteria—are just now being charged with playing to the gallery. I hear the work of certain colleagues referred to in this way, and presume that my own ideas naturally meet the same sort of reception. It is my feeling that other operators who try to work

rapidly do so with no intention of gaining personal *éclat*, for the matter does not really work out in that way at the present day. Certainly not to the point of substantial recognition. There is something else back of the motive which led physicians and surgeons in the past to make enthusiastic audiences for particularly rapid operators. It must have been that the audiences recognized some fundamental benefit for the patient; because underlying all of our professional feeling is the basic desire to see the sufferer helped—*Res est sacra miser*.

I believe that the attitude of a conservative profession when attacking new ideas is legitimate and commendable. It is our only protection against the introduction of a multitude of fanciful and harmful theories that are foisted upon us daily by earnest advocates of unwise plans and of imperfectly constructed methods. We are eager enough to accept new ideas when they are properly presented, and in a way to appeal to the reason; but we are all so busy in carrying

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out established ways for doing good that it is difficult to take time for a new stand and for comprehending a new principle. The bias shown by Treves and by Henry H. Smith toward rapid operators gives me the impression that they probably had in mind certain colleagues who won more or less applause for their ways of working.

Up to about forty years ago operative shock had no literature, but during the latter part of the last century a great many writers took up the subject. For the most part they describe shock as due to a multitude of personal and external factors, rather than to the direct effects of operative work. The latter effects were spoken of as "exhaustion." Exhaustion, not shock, was relative to the duration of an operation, according to most of the writers of the middle of the last century.

I am fully agreed with the ancient and modern writers who argue against hurry in operating, but we must cultivate as far as possible a rapidity of action which will

make every move count, and which will allow the average abdominal operation, for instance, to be completed in about fifteen minutes. Get in and get out! Personally I have not been able to do stomach and bowel resections or complete breast amputations in much less than thirty minutes, but appendicitis operations, in cases with many complications, are frequently completed in five minutes. We must drop many of the details of our beautifully constructed technic, which has for its object the removal or the destruction of bacteria, and we must come to know the face value at least of the natural immunity of the patient. We must conserve that immunity and not sacrifice it upon the altar of our art. Dawbarn poured milk, representing pus, into the abdominal cavity of a cadaver, and then set to work to determine how to get it all out. After a degree of incising, sponging, and flushing, sufficient to kill a bull, there was still plenty of milk left. How are we to read that object lesson? By perfecting methods for getting

that milk out? Oh, no! That would be in accordance with the dominant idea in surgery at the present moment, but it would be all wrong. The right way is to leave the patient in condition to take care of the milk himself. My own method would be to quickly open the peritoneal cavity with a pair of scissors, turn out the appendix and whatever pus happened to be close at hand, and put in a little wick drain. If pus flowed over normal peritoneum, exposed by the separation of adhesions, I would leave it, and expect that as an albuminous fluid it would furnish a certain amount of nutrition for the patient, even though its disgusting fetid sulphur ethers smelled like very dangerous material. Fifteen years ago I would have stood aghast at the mention of any such treatment. It would be subversive of all that I learned of wound treatment in expensive trips to Europe. Who taught us this new lesson? The physician who did not believe in operating for appendicitis. His attitude was immoral, and when asked to

defend himself he always ran away and hid behind bad surgeons, but he was of service to the profession because he taught us a lesson at the patient's expense. How did some patients with pus in the peritoneal cavity recover? Precisely as they do regularly after a quick and unideal operation. The only difference is this. In one case the patient fights it out all alone and unaided, and in the other case we step in and turn the tide of battle between phagocytes and bacteria. That is the principle—turning the tide of battle and letting the patient annihilate a running enemy with a rear fire of volleys of antibodies and phagocytes. By the popular method of sponging and flushing and filling the patient with gauze—committing taxidermy upon him—we fire directly into the ranks of our ally and disable him at the start. The only pus that I would leave in sight in quantity is the pus of protective staphylococcus albus infection, but a search for all of the pus in the peritoneal cavity ends like the search of the man who suspected the

presence of a leak in the gasoline barrel in the cellar and who went down with a candle to look for it.

I believe that under the new principle of conserving the natural immunity of the patient we can get our death rate in appendicitis down to a fraction of 1 per cent.; taking all cases as they come, refusing help to none, and operating upon all who are still breathing when we get to the house. Hotchkiss, in the *Medical News* for July 2, 1904, states that at one of his hospitals, where many emergency cases of appendicitis are received, the operative death rate for a series of years, under accepted methods of to-day, was 31 per cent. Hotchkiss then changed to methods which conserve the natural immunity of the patient, but which are not as yet acceptable to the profession, and there was no death rate at all in his next seventy-two consecutive operations for appendicitis, although the cases were of the same sort as those which had previously given a death rate of 31 per cent. Just

stop and think of that for a minute! After dropping it out of mind, think of it again. Every once in a little while let it come back into mind. The secretary of the Standard Oil Company told me that one of the most important things in this world was to know a good thing when we saw it. Is it necessary for one to quote such authority to doctors? Apparently! I know of other statistics quite like those of Hotchkiss, properly recorded, and available for all who wish them. The one example will suffice. Once in awhile we will have an appendicitis patient with mesenteric thrombosis, or with pylephlebitis, or with such violent peritonitis that we must lose him, but the proportion of such cases is trifling. One gets to feel that it is well-nigh impossible to lose any sort of an appendicitis patient, provided that accepted methods of treatment are dropped.

For the last thirty or forty years we have tried so conscientiously and so scientifically to help the patient by following up his bacteria that we ran past the patient himself

altogether. With our eyes upon the heavens, we have fallen over a fact upon the ground. A fact that was known to our teachers of earlier days. Now we must go back to the old idea of rapid operating, with its collateral features, because, by synecdoche, it stands for conserving the natural immunity of the patient. The first stage of surgery was heroic, the second was anatomic, the third was pathologic, and we are now about to enter upon the fourth or physiologic stage of surgery. Immunity is to be the watchword of the day, in surgery as well as in medicine. During the past thirty or forty years we forged far ahead of the internists, for our science was better than theirs. Now they are quietly slipping up to us, for their science is getting to be more comprehensive than ours. What, ho, watchman!

THE ADVANTAGES OF EXPEDITIOUS SURGICAL WORK.*

Previous to the days of anesthetics and of aseptic surgery, operators strove to complete their work in the shortest time possible, and the results of skill in this direction were appreciated. Under the influence of modern methods of protection for the patient, there came a tendency to devote more time to detail in operative technic, and this sometimes degenerated into "puttering." It is quite as important for the patient to retain his natural resistance after operation as it is for the surgeon to add artificial means for securing asepsis during operation. Mr. Tait was the first prominent exponent of this principle, but the special reasons for his success were not

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understood in his day. We now know that infections are met by the cell resistance of the individual, and the better the general resistance of the patient, the better his special cell resistance. A patient usually retains a great fund of natural resistance during the first fifteen minutes of an operation, no matter what is being done; but he is usually depressed after an hour of operative procedure, even though the work be simple in its nature. It seems well, therefore, for us to make an attempt to approach as nearly as possible to the fifteen-minute standard of time in most of our surgical work.

The matter came up for discussion recently in a little group of surgeons, and I agreed to have my next dozen operations timed. Not one of the operations was done in the shortest time in which I have done any one of them previously; other surgeons would have been still more expeditious, and two of the operations required much more than the average length of time, but, on the

whole, the list furnishes a very good illustration of the advantages of "getting in, and getting out" in surgical work. A common criticism is that important details may be neglected. That is a matter for the audience to decide, as all of these operations, except in the prostate case, were done in the presence of the class at the Post-Graduate Medical School. It is not desirable to operate against time, but it is worth while to make every move count, and to note, after all necessary things have been done, that the work was not unduly prolonged.

The following twelve cases are consecutive ones, with the exception of two or three minor procedures not worthy of classification.

The time recorded was the time from the beginning of an incision to the closing of the last suture; or, in cases without suturing, to the application of the dressing.

CASE I.—Mr. R. L. C., aged nineteen. Interval appendicitis; appendix entirely buried among dense adhesions.

Operation.—Gridiron incision one and one-half inches long, removal of appendix and suturing of separate tissue planes of the abdominal wall with catgut.

Time.—Sixteen and one-half minutes.

Result.—Primary union and patient out of bed on seventh day.

CASE II.—Dr. H. E. C., aged twenty-nine. Gangrenous appendix; old and new adhesions.

Operation.—Gridiron incision one and a half inches long, removal of appendix and separate suturing of tissue planes with catgut. No drainage.

Time.—Twelve minutes.

Result.—Primary union and patient out of bed on the seventh day.

CASE III.—Mrs. H. R., aged thirty-one. Extrauterine pregnancy; patient apparently dying. Intravenous saline solution of about 3,000 c.c. was given before operation until pulse could be counted.

Operation.—Midline abdominal incision, many pints of blood and clots removed, oviduct removed; no drainage; tissue planes separately sutured with catgut.

Time.—Twelve and one-half minutes.

Result.—Primary union and patient out of bed on the fifteenth day.

CASE IV.—Dr. J. O. T., aged sixty. Hypertrophy of prostate; bladder holding 20 ounces of residual urine; patient feeble.

Operation.—Removal of both lobes of prostate through perineal incision and insertion of Ferguson's drainage apparatus.

Time.—Five minutes for removal of the prostate and four minutes for application of drain and dressings.

Result.—Patient was out of bed on ninth day, but has pus casts with bacteria and will probably lose ground, unless flushing of the renal pelves with argyrol is to be effective.

Subsequent History.—Patient refused to have the pelves of the kidneys flushed through ureteral catheters daily, and he was placed on urotropin (hexamethylenamin). Excellent response was at once made, and he gradually improved to the point of being up and about the house, but died suddenly two months and a half from the date of operation. Overdose of morphine taken perhaps accidentally in the night.

CASE V.—Mr. F. K., aged twenty-three.

Osteoma to left superior maxilla involving vomer and ethmoid bones.

Operation.—Preliminary ligation of left common carotid artery and removal of superior maxilla and other bones through the classical incision.

Time.—About five minutes for the carotid operation, and about twenty-six minutes for the excision. Duration of anesthesia was forty-five minutes, as time was lost in getting out and preparing instruments, the need for which had not been anticipated. A most difficult operation of its class.

Result.—Primary union of all sutured structures. Patient was out of bed on third day after operation and is now having obturator applied by a dentist.

Subsequent History.—The obturator, which was made and fitted by Dr. F. L. Fossume, not only holds the cheek in normal form and allows of good articulation, but it carries teeth which restore the appearance and function of the lost maxilla.

CASE VI.—Dr. A. I. B., aged thirty-three. Interval appendicitis, with one short adhesion band.

Operation.—Gridiron incision one and a half inches long. Eucaïn anesthesia. Re-

moval of appendix, and closure of separate tissue planes with catgut.

Time.—Seven minutes.

Result.—Primary union. Patient was out of bed on third day, and gave a dinner to friends down town on the fifth day, against my knowledge or approval.

CASE VII.—Mrs. R. R. D., aged thirty-five. Extensive peritoneal adhesions, involving omentum, loops of bowel, and pelvic structures. Diastasis of rectus abdominis muscles following imperfect closure after a previous operation.

Operation.—Separation of all adhesions, and application of Cargile membrane for prevention of their recurrence. Dissection of sheaths of rectus muscles, and careful suturing of separate tissue planes. Patient was very adipose.

Time.—Twenty-three minutes.

Result.—Primary union excepting at pea-sized point. Patient was kept in a recumbent posture for eighteen days to avoid undue tension on abdominal wall.

CASE VIII.—Miss K. S., aged seventeen. Dysmenorrhea due to cirrhotic ovaries. Extensive active acne of forehead and shoulders. Preputial adhesions.

Operation.—Decapsulation of ovaries, through midline abdominal incision. Circumcision.

Time.—Thirteen and one-half minutes for both operations.

Result.—Primary union. Acne had disappeared by fifth day leaving small dry comedones. Patient was out of bed on fifteenth day.

Subsequent History.—Decapsulation of the ovaries has not relieved the dysmenorrhea, but the acne, which had been a distressing feature of the case for several years, is now apparently cured.

CASE IX.—Mrs. T. L., aged twenty-three. Extensive pelvic peritoneal adhesions. Perineum ruptured to second degree.

Operation.—Midline abdominal incision, separation of adhesions, conservative treatment of ovaries and tubes and application of Cargile membrane to prevent recurrence of adhesions. Repair of perineum by semilunar incision and suture of bulbocavernosus and levator ani muscles in median raphe.

Time.—Three and one-half minutes for the perineal operation, and nine minutes for the abdominal operation.

Result.—Primary union of abdomen and perineum. Patient was out of bed on seventeenth day.

CASE X.—Mrs. F. L. F., aged twenty-five. Extensive peritoneal adhesions of pelvis, including loops of bowel, omentum, and uterus and adnexa.

Operation.—Midline abdominal incision, separation of adhesions, and application of Cargile membrane; removal of left ragged adnexa; conservative treatment of right ragged adnexa.

Time.—Fifteen and one-fourth minutes.

Result.—Primary union. Patient was out of bed on the seventeenth day.

CASE XI.—Mrs. E. M., aged seventy-four. Carcinoma of right breast; patient was extremely adipose.

Operation.—Removal of breast, pectoral muscles, axillary and subclavicular glands and fat, which latter were firmly adherent to axillary vessels; buttonhole side drain for wick; the rest sutured.

Time.—Twenty-six minutes.

Result.—Primary union along the whole sutured line. Patient had been nervous about operation, and had been led by nurses and assistants to think that we had simply

applied "massage under an anesthetic" and did not discover that an operation had been performed until a week later when the dressings were changed. She wished to get up two or three days after the operation.

CASE XII.—Mrs. C. M., aged eighteen. Tuberculosis of diaphysis and epiphysis of left tibia.

Operation.—Excision of knee.

Time.—Twenty-one minutes.

Result.—Wound granulating safely, two weeks elapsed.

Subsequent History.—There is new tuberculous invasion at the site of the excision, and the leg will probably have to be amputated.

With the exception of the cases of excision of the maxilla and of the knee, the only instruments employed consisted usually of a pair of scissors, a needle, a single-hook retractor, and two pairs of artery forceps.

The object in using scissors in place of the classical scalpel is because there seems to be much less oozing of blood from small vessels, and because the number of instru-

ments is lessened. The argument that primary union may not follow the use of scissors is answered well enough by these cases without reference to further statistics.

MY CHANGES OF VIEW IN APPENDICITIS WORK.*

My first operation for appendicitis, or perityphlitis, as it was generally called at that time, was on March 15, 1888. The subject of operation was just beginning to be advocated strongly in New York by Sands, McBurney, Bull, Weir, and other leading surgeons. Discussion on the subject referred chiefly to the treatment of the abscesses of appendicitis, which Sands said were usually extraperitoneal. The accepted treatment at that time consisted chiefly in opening the abscesses, flushing the cavities, and employing large rubber or glass drains, together with more or less gauze packing. During the next four years

* A paper read at the meeting of the Society of Alumni of Bellevue Hospital, February 2, 1905, and published in the *Medical Record*, May 27, 1905.

I operated in a number of cases of appendicitis with abscess, and employed the commonly accepted methods of treatment. Results were pretty bad. Patients made slow recovery, with weak points left for the subsequent development of ventral hernia; or they died of acute peritonitis or of slow septicemia.

My first planned operation for removal of the appendix was on April 24, 1892, although a number of other surgeons had already done such planned operations. In 1892 I removed seven appendices. Four were gangrenous. Two were the site of acute exacerbations of infection, and one was a quiescent "interval appendix." Six of the patients lived. One died of ileus which developed suddenly on the sixth day after operation. Morphin was almost universally employed by the physicians and nurses at that time for controlling the symptoms subsequent to operation, and I began to forbid its use altogether in cases that were under my control. My views on

the subject of morphin in this connection have changed somewhat, and the drug is now given cautiously in certain cases in which there is such a degree of restlessness that the patient does not start off well after operation. Even in such cases morphin is a double-edged sword, because it puts the eliminating organs at rest along with the sensory nerves besides interfering with the natural protection factors.

During this year of 1892 I employed long incisions, peroxid of hydrogen flushing for abscesses of appendicitis, the wick drain composed of absorbent gauze rolled in gutta-percha tissue in cigarette fashion, catgut sutures for separate suturing of tissue planes, and more or less gauze packing. The stumps of amputated appendices were ligated and buried carefully, and a good deal of unnecessary surgery was forced by me upon some patients for the accomplishment of this step in technic.

Changes of view led to making incisions that were not "bold and free, and masterly

in the stroke," for it seemed to me there was a tendency to do too much surgical injury in these cases, and it was noted that short incision patients started off more quickly toward comfortable recovery. Peroxid of hydrogen flushing seemed to be the keynote to success in abscess cases, but later experience has shown that it is valuable chiefly in the way of neatness, and that we can safely leave pus to be disposed of by the wick drain and by the peritoneal lymphatics. There has been no change of view concerning the desirability of catgut suturing of separate tissue planes of the abdominal wall. The technic of burying the stump of the appendix has been abandoned since the publication of Seelig's article on the subject in the *Annals of Surgery* last year, and several minutes of time have been gained for each operation in consequence.

Gauze packing has been discarded altogether, on the ground that it is a foreign body, that it causes ileus by direct mechan-

ical effect, and excessive lymph exudation which leads to troublesome adhesion formation. Shock is caused by removal of gauze packing, and a serious defect is left in the abdominal wall in spite of provisional sutures.

When iodoform gauze is employed patients often suffer or die from iodoform poisoning, and unless the urine is examined for free iodine the symptoms of iodoform poisoning are commonly thought to be symptoms of septicemia which they simulate.

On discarding gauze packing it was at once observed that patients made remarkable recovery from operation, and the gauze packing had been worse than useless. This view was in direct opposition to the consensus of opinion among authorities at that time, and probably would not have been upheld in any court of law in the country during a period of several years.

In this year of 1892 I employed blunt dissection, but had sloughing of margins, of external oblique aponeurosis in two cases,

due to free separation of that structure, no doubt. The method was dropped, and not taken up again until the publication of McBurney's description of this method with better results. Since that time I have employed the "gridiron" method, not only in early infection and interval cases, but in practically all cases. It gives great protection against subsequent hernia formation.

In 1893 I devised a plan for making a short route for the escape of bowel contents in cases in which fecal fistula was anticipated, by suturing the cecum to the abdominal wall at the site of the incision. It was thought this procedure would be useful also in preventing hernia formation. My views on that point have changed, and it no longer seems desirable to fix the cecum in an abnormal position. It was in fact harmful. Fecal fistulæ care for themselves pretty well if we employ skillful neglect in their treatment. Postoperative ventral hernia should be almost unheard of if we have right ideas about suturing the

divided structures. In 1893 I employed another resource that had been advocated as a protection against ventral hernia; the use of buried silkworm-gut sutures. Silkworm gut is not encapsulated kindly in the tissues like catgut, kangaroo tendon, silk or silver wire, and my knots began to work out, weeks, months, and years after they had been introduced. If I ever have a nightmare it will probably consist in a view of a row of influential patients bearing silkworm-gut knots on a charger. Aside from the use of gauze packing the most serious mistake that has ever come into my appendicitis work was the employment of silkworm gut for buried sutures. In 1893 my favorite incision had been made of a standard length of an inch and a half, for interval and early infection cases, no matter what the extent of adhesions. It was noted that patients after this incision were regularly out of bed by the seventh day after operation, and ready to leave the hospital by the tenth day. It was one of

my house surgeons who suggested that this was a week and a half, and others in a spirit of fun have added the instrument and a half and the dollar and a half to the formula, without my sanction. My views on the desirability of the short incision not only have not changed, but now it has been adopted for almost all sorts of work in abscess and peritonitis cases, as well as for interval and early infection cases. My belief is that one should take all of the room he requires for successful work, but he should get to require little room as quickly as the fingers can be trained to do better work when not confused by the eyes. One can easily extend the incision if necessary to do so. My friend Joseph Price says that when he gets among complicated adhesions he wants some one to tie a handkerchief about his eyes so that he cannot even see the surroundings of the room. In the language of the convert who shortened his prayers, "Them's my sentiments." In several hundred complicated appen-

dicitis cases I have had a mishap but once when working by touch, and in that case it simply amounted to severing the artery of the mesappendix, so that the incision had to be lengthened for the application of forceps. Adhesions were often very confusing in earlier days when it was necessary to work by sight, and there was danger of injury to the bowel, ureter, or iliac vessels, but no such danger appears in the statistics after one has learned to work by touch.

In 1893 I began to note the advantage to the patient of quick work, and gradually reduced the time of operation to an average of twenty minutes, then to fifteen minutes, and it is now common to have the time from the first incision to the last suture occupy not more than seven or eight minutes in complicated cases; sometimes less time. It has always been a rule to get the appendix out, on the ground that at least 10 per cent. of cases would carry concretions, and a much larger percentage of cases would

have late complications from mucous inclusions, if the appendix were left. Sometimes for a moribund patient, or when working among unfavorable surroundings, it is best to simply open abscesses, and to do expediency work expeditiously; but this has always seemed to me to be unfortunate, and my views have never leaned toward the policy of opening abscesses at one operation, and removing the appendix subsequently. I will not disagree with authorities who argue for this practice, but simply express personal views in the matter. Any successful method of any surgeon is right, however.

The question of operating upon moribund patients has always seemed to me to be one of morals rather than one of policy, and I have operated upon every patient who was still breathing when we got to the house or to the hospital. The pulse could not be counted in some of the cases, but it is surprising to note the effect of an intravenous saline infusion of fifteen hundred

cubic centimeters given in advance of operation, the effect of letting out a flood of toxins by a five-minute operation in many of these cases, and the effect of Murphy proctoclysis afterward. One might as well not operate at all in such cases if he contemplates spending thirty minutes at the work, and he must count upon having his chief death rate in this class of cases, at best.

The question of separating adhesions freely in a search for the appendix or for multiple abscesses is one in which my views have changed. At first I had the classical fear of the peritoneum and reasoned also that as adhesions were nature's protection it would be well to respect them. Later it was found necessary in many cases to separate adhesions freely in getting the appendix or in searching for multiple abscesses, and it was noted that no harm resulted from exposure of the normal peritoneum. It was evident that the local hyperleukocytosis and other elements of

protection had been called out sufficiently to guard the field, and for awhile it was my practice to break up all adhesions purposely. My final decision is that adhesions immediately re-form at separated points in acute infection cases, so that nothing is gained by separating them, but it does no harm to open freely into the normal peritoneal cavity in cases in which one is hunting for something among adhesions. After noting the degree to which the peritoneum was guarded by nature against infection I dropped the idea of flushing out the peritoneal cavity with saline solution in most of the cases in which the abdomen contained septic fluid. It seemed to me that almost everything that we had been doing conscientiously added to the severity of the operation and that the nearer we could come to leaving the patient alone the better. Even large masses of lymph coagulum remaining attached to the bowel are now left undisturbed. After dropping out of my practice the features which seemed to have

a special death rate of their own—gauze packing, iodoform gauze, long incisions, and the expenditure of time in unnecessary detail of work—I published a report on a series of one hundred consecutive unselected appendicitis operations, with a 2 per cent. death rate. Thirty-four of the patients had abscess and various stages of gangrene and peritonitis. Twelve of the patients had acute infection with gangrene of the inner coats of the appendix in some of the cases, but without external abscess formation. The rest of the patients in the list had quiescent forms of appendix trouble. Of the two patients who died, one was properly a case of gangrene of the ileum, several feet of which had slipped through an adhesion band from an old appendicitis, but the case had to be included because I removed the remains of the appendix incidentally. The other death was in one of the moribund patient operations.

The effect of the publication of this list of cases was to cut down my practice to a very

serious extent indeed, and one writer in the *Medical Record* for December 12, 1896, went so far as to say that such figures were "vainglorious cheats," and that operation must have been refused in many cases for the purpose of getting misleading statistics. As a matter of fact I had operated upon every appendicitis case to which I had been called during the period covered by the statistics, with the exception of two patients who were dead when we got to the house.

The methods that were advocated in my paper were generally held to be unsafe and dangerous. In the *Medical News* for July 2, 1904, Dr. L. W. Hotchkiss states that in one of the hospitals with which he is connected, and where many emergency cases are received, his death rate up to 1898 in appendicitis cases treated by the accepted methods of the day had been 31 per cent., but after taking up "unsafe and dangerous methods" he had not lost a single patient in his last seventy-two cases, although among the number there were twenty-six

gangrenous appendices, with or without perforation, and fifteen gangrenous appendices with advancing peritonitis. At the 1904 meeting of the American Medical Association, Dr. Ochsner of Chicago, advocating a "starvation method of treatment" in the class of cases giving the largest death rate, reported upon his last one thousand appendicitis operations, with a death rate of $2 \frac{1}{5}$ per cent., and since that time I have adopted Dr. Ochsner's treatment for the most part, but prefer to include a five-minute operation in the class of cases in which he would wait for the infection to become localized.

The question of removing normal appendices when they are at hand in the course of other operative work is one that I have never favored, on the ground that removal of the normal appendix delayed the operation and added a trifle of danger. Leave the appendix alone until it is infected, and then lose no time in having it inspected. This has always been my ground, and the

idea that I ever favored removal of normal appendices must have been a *reductio ad absurdum* from statistics.

In 1902, acting from experience, and upheld by the statistics of Dr. John G. Clark in pyosalpinx work, I began to close the abdomen without drainage in appendicitis cases in which considerable pus and other débris had been left in the peritoneal cavity. The object in closing completely was to get a stronger abdominal wall and to get the patients out of bed sooner. The practice was based upon belief in the power of the natural resistance factors of the patient to dispose of waste material by way of the peritoneal lymphatics.

As a result of the practice it was noted that primary union would occur in about half of the cases, but the patients would sometimes carry a temperature ranging up to 100° F. for days after they were up and about. In some of the cases secondary abscess would form, but it always pointed at the incision, so no harm was done. In

several cases in which primary union of the muscles of the abdomen was obtained, the adipose layer became infected and showed remarkable tardiness in granulating after the skin wound had been reopened. As a result of this experience I have gone back to the method of using a cigarette drain in all cases in which pus or septic débris have been left in the peritoneal cavity, but the wound is closed without drainage in cases in which gangrene and pus occur within the peritoneal coat of the appendix only.

The question about the time for operating in cases of appendicitis is one that has had some fluctuations. The dictum which I promulgated about 1890, "Operate as soon as the diagnosis of appendicitis is made," aroused a storm of opposition. It was intended for acute progressing cases. In cases of acute progressing appendicitis I was sometimes persuaded to wait until a business man of large affairs could consult with his lawyers and get a business of millions into

such form that others could manage it for awhile. Sometimes relatives of a school boy would telegraph us to wait until they could get to his bedside in a day or two. Occasionally I would be overbalanced in consultation with men whose opinions were highly respected. The results of such waiting have pointed straight back to the dictum. In this class of cases operate just as soon as things can be made ready, without regard for consultants, friends, relatives, diplomacy, finances, or anything at all, except the knowledge that the appendix is already being operated upon by bacteria.

In the class of cases in which the patient is suffering from the symptoms that go with fibroid degeneration of the appendix we may safely leave the matter to the decision of the patient, after stating the case clearly to him, and the patient himself may be allowed to assume the responsibility of saying when operation should be done.

In cases of acute appendicitis that are out of reach of competent surgical services,

the patient is much safer under ice, opium, and starvation treatment than he would be under the kindest hands that attempted to render untrained surgical services.

In the patients who are certainly better on the day when we first see them than they were on the previous day, a question arises that has not as yet been settled—the only appendicitis question remaining on which I am uncomfortable. If we attempt to carry out the dictum we shall operate upon some patients who would do better if we waited for a month or two. On the other hand some of these convalescing patients spring a surprise upon us at midnight, just as we are taking the train for Chicago, and when we are not prepared for the emergency work required in sudden exacerbation of infection. I have had various views about individual cases in which the patients were convalescing, but have been made to feel that bacteria have ways of their own that are manifested without warning. In patients who were sent to me

in the interval between attacks, but with a clear history of appendicitis reported by competent physicians, I formerly believed that it was best to remove the appendices. My views on this point have changed, and it now seems best to operate only when on palpation the appendix is found to be the definite seat of chronic infection, or of adhesions which cause symptoms. The reason for this change of view is because some of the quiet interval appendices were found to have lost their inner coats, and there was no danger of further infection. The question is one, however, which rests largely upon accurate palpation by satisfactory method.

McBURNNEY'S POINT AND ANOTHER POINT
IN APPENDIX DIAGNOSES.*

Draw a line from the right anterior superior spine of the ilium to the navel. On this line, an inch and a half from the anterior superior spine, we find McBurney's point. Tenderness on deep pressure at this point gives presumptive evidence of an irritative process in the vicinity of the appendix vermiformis, but tenderness on superficial pressure at this point may mean irritation of sensory nerves of the abdominal wall, and the irritation may be due to causes ranging from hysteria to toxemia. McBurney's point is of great diagnostic value to physicians who possess a considerable degree of collateral knowledge, aside from

* Presented at the Surgical Section of the New York Academy of Medicine, Dec. 5, 1907, and published in the *Journal of the American Medical Association*, January 25, 1908, Vol. L, p. 278.

their familiarity with the symptoms of appendicitis.

Instead of going out on the line to a point an inch and a half from the anterior superior spine of the ilium, come back on the line to a point situated an inch and a half from the navel. Here we find another tender point of definite diagnostic value. McBurney's point and the point here described are approximately six inches apart in the adult of average proportions. The point here described has reference to the right lumbar ganglia of the sympathetic nervous system, and notable tenderness of these ganglia has a diagnostic value which may briefly be placed in classification under the following heads; the statements are to be taken as referring to typic cases.

1. In the early stages of an acute infective process of the appendix the right lumbar ganglia are tender and the left lumbar ganglia are not tender. (The left lumbar ganglia may be described for diagnostic purposes as lying an inch and a half to the left



Fig. 1.—McBurney's point and another point in appendix diagnoses.

of the navel.) Under these circumstances the point here described is of secondary importance, while McBurney's point is of prime consequence.

2. A.—When an acute inflammatory process of the appendix has subsided, leaving a mucous inclusion or scar tissue, there may be no tenderness on pressure at McBurney's point, but there is tenderness at the point here described and no tenderness at the point of the left lumbar ganglia.

B.—When the appendix is undergoing a normal involution process, with replacement of its lymphoid coats by fibrous tissue, digestive disturbances and various local neuralgias may be due to irritation of nerve filaments entrapped in the new fibrous tissue. There may be no tenderness at McBurney's point, but there is persistent tenderness at the point here described. There is no tenderness at the point of the left lumbar ganglia.

C.—When the appendix is congested without the presence of infection, as in many

cases of loose kidney, there may be little or no tenderness at McBurney's point, but there is persistent tenderness at the point here described. There is no tenderness at the point of the left lumbar ganglia.

Under circumstances A, B, and C the point which is here described rises to a position of primary importance, and McBurney's point falls to a position of no consequence or of secondary importance. The point here described will be found to be of value in everyday diagnostic work for differentiating between irritations of appendix origin and irritations of pelvic origin. In irritations of pelvic origin both right and left lumbar ganglia are tender. Take, for illustration, a case in which the appendix and the right Fallopian tube are bound together by adhesions. We are to decide whether certain symptoms proceed from the appendix or from the Fallopian tube. If the symptoms proceed from the appendix the point here described is tender, alone. If the symptoms proceed from Fallopian tube

both right and left lumbar ganglia are tender together. Irritation from a scar at the cervix uteri, or from hemorrhoids, will make both right and left lumbar ganglia tender together.

Taking cases as they run in daily practice, tenderness at the point here described alone means irritation proceeding from the appendix alone. Tenderness of both right and left lumbar ganglia together means irritation proceeding from some structure belonging to the pelvis.

Under circumstances A, B, and C, again, the question often arises as to whether an abdominal irritation proceeds from the appendix, from the pelvis, or from some point above the navel. If the irritation proceeds from some structure situated above the navel, neither the right nor the left lumbar ganglia are tender. A practical feature of the point here described is this: During the year many of the patients sent to me for operation for appendicitis are not operated on for appendicitis; they are either not

operated upon at all, or else attention is given to some organ other than the appendix. Very little time is required for fixing the point in mind. In the course of one day's office work one may examine several patients who will demonstrate the point.

To recapitulate: A patient comes in with the appendix in the form of a question mark. Right lumbar ganglia tender, alone—appendix trouble. Right and left lumbar ganglia tender, together—pelvic trouble. Neither right nor left lumbar ganglia tender—trouble somewhere cephalad from pelvis and appendix.

PROTECTIVE APPENDICITIS.*

The time has arrived when we may conveniently classify four kinds of appendicitis.

1. *Protective Appendicitis*.—An irritative lesion, occurring in the course of normal involution of the appendix, and dependent upon irritation of nerve filaments which persist in the contracting hyperplastic connective tissue which has replaced other normal structures of the appendix.

2. *Appendicitis with Intrinsic Infection*.—An infective lesion which seems to be dependent upon any cause leading to rapid swelling of the inner coats of the appendix

* A paper presented at the International Medical Congress at Budapest, September 2, 1909. The author presented points in discussion without reading the paper, which will appear in present form in the published transactions of the Congress, and published in the *Medical Record*, January 8, 1910.

within the close outer sheath, with consequent compression anemia, and bacterial attack upon the tissues rendered temporarily vulnerable by such compression anemia.

3. *Syncongestive Appendicitis*.—An irritative lesion, due to the presence of serous infiltrates in the tissues of the appendix, and occurring synchronously with similar congestion of neighboring tissues. Such congestion is found with so-called lithemic swelling of lymphoid structures of the bowel; with obstruction of the lymph and blood circulation through certain diseases of vital organs, and with loose right kidney, which is said to cause obstruction by pressure upon the superior mesenteric vein.

4. *Appendicitis with Extrinsic Infection*.—An infective lesion, due to bacterial approach from other structures lying outside of the appendix, and progressing slowly enough to allow the appendix tissues to develop a good degree of leukocyte protection as a rule. This form of appendicitis is found with tuberculosis of the peritoneum



Fig. 2.—An appendix which was quite normal in external appearance, but it felt harder than normal on palpation, and longitudinal section shows the inner coats to have been replaced by connective tissue, with total obliteration of the lumen. The patient had suffered from occasional attacks of pain in the appendix region, and from intestinal dyspepsia, both of which disappeared on removal of the appendix.

or with infections proceeding from the uterine adnexa, for instance.

We thus have appendicitis presenting itself in the form of two kinds of irritative lesion without infection, and two kinds of infective lesion.

The commonest lesion of the appendix vermiformis appears to be the irritative lesion which I formerly called fibroid degeneration of the appendix, but for which the term "protective appendicitis" is now proposed tentatively for the first time. The reason for this choice of nomenclature depends upon two facts: 1. Structures susceptible of acute infective processes are removed from the appendix by connective tissue replacement in the course of normal involution of the appendix, otherwise known as fibroid degeneration of the appendix. 2. I find nerve filaments persisting longer than most other structures in the appendix during the course of fibroid degeneration. These nerve filaments in the appendix are irritated by the contracting connective tissue,

just as they are irritated in other contracting connective tissue in other parts of the body. The irritation of nerve filaments, entrapped in the degenerating appendix, calls out a permanent local hyperleukocytosis which seems to protect such an appendix against bacterial attack.

Senn first called the attention of the profession to the lesion in question, in an article published in the *Journal of the American Medical Association* for March 24, 1894. He called the lesion "appendicitis obliterans," but did not separate it distinctly from the infective lesions. His nomenclature is not quite descriptive, because it leads to the conception that obliteration is due to the inflammation, whereas the contrary seems to be true, and the inflammation appears to be due to changes occurring in the course of an obliteration process.

Ribbert in 1902 was the next author to give a position of importance to the lesion. On page 319 of his "Lehrbuch der speciellen Pathologie," he describes it as a normal

involution of the appendix, and states his belief that the condition is provocative of infective processes. A study of the subject, with examination of what seems to be a sufficient number of specimens, leads me to the belief that we are to take at the present time an exactly opposite view, and to note in practice if the irritative lesion called by Senn "appendicitis obliterans," by Ribbert "normal involution of the appendix" and formerly by me "fibroid degeneration of the appendix," is not actually protective against infective processes, and properly to be classified as "protective appendicitis."

The symptoms of protective appendicitis are of two separate and distinct kinds: 1. Local discomfort in the appendix region caused by the presence of sensory nerve filaments entrapped in contracting connective tissue. This discomfort persists for years. Sometimes there is acute local pain and tenderness, but not enough to send the patient to bed. In fact, these

patients are particularly apt not to go to bed, on account of their general irritability. At other times there is merely a sensation of warmth in the appendix region, fluctuating in intensity from day to day—sometimes absent for short periods of time, and again attracting the attention of the patient for several days in succession. These patients are the ones who go the rounds of the profession asking whether they have appendicitis or not. Sometimes they are told that the discomfort is caused merely by the presence of gas in the bowel. At other times they are told that the condition is one of chronic appendicitis, and that operation should be performed for the removal of the appendix, on the ground that infection may suddenly supervene. We are to tell these patients, I think, that they are really protected against infective processes of the appendix, and that there is no need for removal of the appendix on the ground of impending danger. In some cases, symptoms of sensory and of functional disturbances seem to



Fig. 3.—Two appendices in which all structures had been practically replaced by connective tissue, but enough nerve filaments had remained to cause persistent intestinal dyspepsia, for which the patients had received treatment by authorities without permanent result, until the appendices were removed. Both patients then became well.

be altogether absent while the appendix is undergoing fibroid degeneration. 2. The second set of symptoms belonging to fibroid degeneration of the appendix has relation to sympathetic nerve filaments entrapped in contracting connective tissue. Sympathetic nerve filaments engaged in such tissues are irritated like the sensory nerves, but the demonstration appears to consist in a reflex irritation of the intimate ganglia of the bowel wall (Auerbach's plexuses and Meissner's plexuses). Disturbance of these plexuses appears to lead to chronic derangement of bowel function, and the patients form a considerable proportion of the class which is being treated for "chronic intestinal dyspepsia" with or without diagnosis referable to the appendix.

There are three chief points for diagnosis of the condition of protective appendicitis, when they are observed in connection with the subjective history. 1. The most important point is hypersensitiveness on deep

pressure at the site of the right group of lumbar ganglia, situated approximately an inch and a half to the right of the navel, and close to the lumbar vertebræ. If we make deep finger pressure at this point, finding a sensitiveness which is absent from the corresponding left side, we may look to the appendix as the original seat of irritation. Should there be hypersensitiveness at the sites of both right and left groups of lumbar ganglia, we may look to some pelvic structure instead of to the appendix for the seat of original irritation, and this constitutes a point of considerable consequence in some cases in which we are not sure whether the appendix or some pelvic structure is at fault. 2. The second diagnostic feature of importance consists in a distention of the cecum and ascending colon to the hepatic flexure, with an undue amount of gas. The feature is not always present, but is found regularly enough to receive attention. It is my belief that the distention is not due to the presence of gas primarily, but that con-

tinued irritation of the motor nerves of the bowel in the region of the appendix leads eventually to relaxation of the muscularis and the formation of a receptacle in which gas readily collects in quantity. 3. Third in order for making a diagnosis, would come the testimony elicited by palpation, and the finding of an appendix that is harder to the feel than a normal appendix. Some very competent diagnosticians have expressed a doubt about our ability to palpate the appendix accurately, but it is my belief that palpation of the appendix can be accomplished pretty regularly after one has acquired a certain method of procedure.

The treatment for protective appendicitis must depend upon the particular case, rather than upon any rules. Many patients who obtain the mental relief of finding that they are not in danger, will get on very well under medical and hygienic treatment aimed at the intestinal indigestion. Other patients will have such a degree of persistent

disturbance that it is best to advise removal of the appendix.

Statistics of removal of appendices which are undergoing fibroid degeneration will probably be more favorable than statistics of any other form of appendicitis, for the reason that chronic irritation has called out chronic leukocyte protection of the vicinity. This one fact seems to make it desirable for us to classify cases of appendicitis for statistical purposes better than has been done in the past. Cases of protective appendicitis have usually gone into statistics along with the other three kinds, and it is now time for us to make a more scientific elaboration in reports. The greatest danger from operation *per se* is probably in cases of syncongestive appendicitis, because resistance is apt to be lowered by the causes leading to serous infiltration. The next greatest danger from operation *per se* is perhaps when the perfectly normal appendix is removed, because we then open a point of infection in the midst of an unprepared field. On account of my



Fig. 4.—Cross-section of an appendix undergoing fibroid degeneration.

interest in this subject of fibroid degeneration of the appendix, some of my assistants, for purposes of brevity, got to speaking of "Morris appendices," and this nomenclature at a distance began to be applied to normal appendices, but I have always been opposed to the idea of removing normal appendices, on the ground that it required a higher degree of skill and caution than I cared to exercise for the purpose. In cases of infective appendicitis, it is now possible for the surgeon to have a series of one hundred consecutive operations in unselected cases, including those with varying stages of suppuration, gangrene, and peritonitis, without having a single death, if he follows the principles of the fourth or physiological era in surgery.* In cases of appendicitis with extrinsic infection, we are apt to have a field protected by hyperleukocytosis. These facts seem to make it desirable to classify four types of appendi-

* See *Journal of the American Medical Association*, August 22, 1908, pages 644-648.

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citis, and to make separate reports, particularly when dealing with fibroid degeneration of the appendix, *i.e.*, the irritative lesion of protective appendicitis.

PALPATION OF THE APPENDIX.*

This next patient was sent in yesterday, with a diagnosis of appendicitis. Last July, shortly after making a muscular effort, he was seized with abdominal pain and cramps, but there was little nausea and no vomiting. He was in bed for a few days, and there has been a persistent pain in the right inguinal region ever since—not a severe pain, but annoying.

Now let us palpate the appendix. The patient is a large man, with strong, muscular abdominal walls, and it will require close attention of the finger tips in order to get testimony from below in advance of the operation. If we observe the methods of physicians who say that the normal appen-

* A clinical lecture at the New York Post-Graduate Medical School, November 2, 1904, and published in *St. Louis Medical Review*, January 7, 1905.

dix cannot be palpated, it is evident that they are honest in their belief. The work must be done by a method. We first press lightly upon the abdominal wall with three fingers held flat, in order to determine if there is unnatural resistance of the muscular wall. If we poke at the abdominal wall with one or two fingers almost any abdominal wall will object. Three fingers held flat and applied gently are not so likely to bring out a response of protection. In this case we find the abdominal wall unduly rigid, and that means that something is wrong within.

The next step consists in getting landmarks, and the first landmark to be sought is the ascending colon. This is palpated by pressing the three fingers gently beneath the right rectus muscle, on a plane above the navel, and then drawing the fingers away in such a manner that the colon must slip out from beneath them. Finding this landmark, we follow it downward until the cecum is reached. The search for the

appendix begins at this point. In a patient of this size the force required for indenting the abdominal wall with the fingers of one hand would completely destroy the delicacy of touch required, and so we resort to a trick. The trick consists in gently indenting the abdominal wall with the feeling fingers, at the site of the appendix, and then with the other hand on the other side of the abdomen, making pressure back and forth in such a way that the cecum is carried back and forth beneath the feeling fingers. In this case I cannot as yet make out the appendix, so we resort to another trick. The feeling fingers are pressed beneath the cecum in such a way that the latter structure will be lifted, and made to pass over their tips, when the pressing fingers are again employed. We now find the appendix. It is a very small one for a man of this size. It is barely two inches and three-quarters long, soft, and held by its mesentery in the form of a rather close letter S. Pressure upon the appendix does not result in caus-

ing muscular objection. I would not operate upon this man for removal of the appendix, although he comes to the hospital for that purpose. Let us see what else can be wrong. We observe that the patient has a testicle on the right side that is barely within the scrotum. It belongs to the undescended testicle class. Associated with undescended testicle there is apt to be an open peritoneal pouch, which is conducive to hernia formation. It is quite possible that this patient's trouble is due to a beginning hernia. In any event the presence of the undescended testicle gives us an excuse for operating for the purpose of doing Bevan's operation. The incision shows what we suspected, that there is an open peritoneal pouch, and adherent in the pouch a film of omentum. This can explain all the symptoms.

Let us enlarge the opening sufficiently for an inspection of the appendix. As it is brought up you observe that it is not over two inches and a half long, soft, and held

in the form of a snug letter S by its mes-appendix.

Some of the members of the class ask me to remove it. I am opposed to removal of normal appendices, even when they are exposed in the course of other work, on the ground that such work delays the other operation and introduces one of the little elements of danger—not large; but all surgeons know that it is little things that furnish the death rate in these days of close margins on statistics, and the question is at any rate a moral one. Leave the appendix alone until it is infected, and then lose no time in having it inspected. That is one of my favorite sayings.

Some of the members of the class are showing antipathy so persistently toward this appendix that I will break the rule and take it out. This requires enlargement of the abdominal incision, one of the things to be avoided. As the mesappendix is cut away, an artery close to the cecum, and not included in the ligature, suddenly gives way,

and as the structures draw back it requires ten minutes of our time before the vessel is secured. This has introduced the only complication in the whole operation for curing the hernia and relieving an undescended testicle. The only complication that was troublesome in the whole operation resulted from the insistence of the class on having a normal appendix removed, and it is a valuable lesson.

Leaving the normal appendix is sometimes injurious to the operator. Some time ago a patient was brought in with a tense abdominal wall, and a diagnosis of appendicitis. She related a history that was flawless in its perfection. The abdomen was so sensitive that palpation was not allowed. I operated and found a normal appendix. The girl had hysteria of the type which delights in deceiving doctors. I am on guard against such cases, but this girl had deceived both me and her family physician who brought her in. When we looked at the appendix and realized the

situation her physician said, "Take out her appendix, anyway," and when I objected, he said that he would tell her that I had taken the appendix out, but would not tell her that it had been put back again. The girl was "cured of her appendicitis" and returned home in a fortnight. A year later she went to Buffalo for some imperative ovarian operation, and told about my previous removal of the appendix. The operator was glad to have an opportunity to examine the remains of the stump, but ran across a surprise which was passed about quietly for a while.



THE DAWN OF THE FOURTH OR PHYSIOLOGIC ERA IN SURGERY.*

It seems to me that the object lesson of the results of conserving the patient's natural resistance, in cases of appendicitis with peritonitis, has opened the vista of a new epoch in surgery.

In the days of Hippocrates surgery was heroic. That represents the first era. Then came Andreas Vesalius and the anatomists, and we had the second or anatomic era in surgery. Pasteur and Lister introduced the third, or pathologic, era. The pathologic era is the one now prevailing the world over. The dominant idea is to prevent the

* Read in the Section on Surgery and Anatomy of the American Medical Association, at the Fifty-ninth Annual Session, held at Chicago, June, 1908, and published in the *Journal of the American Medical Association*, Aug. 22, 1908, Vol. LI, pp. 644-648.

development of bacteria in wounds and to remove the products of infection by means of our art.

Appendicitis has been so refractory in response to the perfection of the art of the pathologic era that when the rubber glove appeared, representing the last degree of refinement of the art, it seemed to have upset the whole system. The use of the rubber glove necessitates comparatively slow work, the employment of long incisions and work by sight. Geologists tell us that the constant accumulation of snow and ice at the antarctic pole may cause a sudden changing of the axis of rotation of the earth. The rubber glove was the last snowstorm of the pathologic era of surgery, and the sun is now to shine on what is perhaps the most fertile area ever exposed to light.

Our faces are now turned toward Metchnikoff and Wright, with their descriptions of phagocytes and opsonins, and of the natural protective forces of the patient. We are at the dawn of the fourth, or physiologic, era

in surgery. We are to conserve the natural resistance of the patient and to turn him over to his phagocytes and antibodies as helpfully as we can. We are to leave the patient in his best condition for manufacturing phagocytes, opsonins, and other antibodies, through the shortest possible method of anesthesia and the least degree of surgery which will suffice to turn the tide of battle between bacterium and leukocyte.

That is the new principle—turning the tide of battle only, and leaving the patient with his physiology as nearly intact as possible. The first object lesson in support of the new idea was perhaps furnished by the physician who did not believe in operations for appendicitis, and whose patients sometimes recovered, even though they had pus in the peritoneal cavity. These cases required explanation, and we now have the explanation. The patient gave personal attention to the bacteria and to the products of infection.

Another object lesson was furnished by

Clark, when he closed the abdominal cavity completely without drainage after pyosalpinx operations, and gave us unexpected statistics.

A notable object lesson was furnished by Ochsner, who awakened us with the results of his starvation method of preparing appendicitis patients for operation. An important object lesson was furnished by the work of Tait; we did not understand its meaning and it made us very uncomfortable. Some of us tried to escape the discomfort by saying that he must be untruthful. Others felt that a man in a responsible position cannot afford to be untruthful.

To the generalizing type of mind the facts of some patients' recovering without operation; the work of Clark, Ochsner, and Tait and the results of neglect of the principles of the third era of surgery in appendicitis cases with pus; it became obvious that we were all missing some great principle that was plainly being overlooked.

One of the most engaging object lessons consists of the statistics of cases of infective appendicitis, managed according to the principles of the new era, in which we conserve the natural resistance of the patient so well that he can manufacture phagocytes, opsonins, and other antibodies freely, and can manage infections with these resources better than we did it with our highly developed art of removing products of infection.

Guided by the principles of the third or pathologic era we so damaged the patient that he could not manufacture phagocytes and antibodies freely. Guided by the principles of the fourth or physiologic era, it is now possible for many surgeons to operate upon a series of one hundred consecutive, unselected cases of acute infective appendicitis without having a single death; and in the very same class of cases, which a few years ago gave us a death rate of 20 or 30 per cent. Let the patient have the shortest possible period of anesthesia; the shortest incision through which a surgeon can do

his work; and let the work be completed in the most rapid manner compatible with the degree of safe dexterity. Do not stop to do ideal and fanciful work with the stump of the appendix. Get the appendix out in any practical way, so long as it is done quickly, without regard for artistic technic. Pay little attention to separation of adhesions, excepting such separation as may be absolutely necessary for removal of the appendix, or for opening multiple abscess cavities. Pay no attention to pus which gets upon normal peritoneum while we are at work. The peritoneum will care for it better than we can with our wiping or washing. Put in the smallest capillary drain which will suffice to lead products of infection out of the peritoneal cavity. Do all of this in five minutes, if possible. Get in and get out. Do not waste time in trying to remove fetid pus. It will be particularly difficult for the surgeons of Europe to put aside traditions of the art of the third or pathologic era of surgery which they have conscien-

tiously developed to the highest point permitted by human intelligence, but let me at this congress give the watchword for successful treatment of cases of infective appendicitis with peritonitis and with pus in the peritoneal cavity. This is the watchword: "Run while pus is running! *Pollice presso!* Do not stay to kill the patient!"

When you have stopped running turn to look back, and behold the dawn of the fourth era of surgery lighting up the horizon.

THE IDEA OF GROSS CLEANLINESS IN SURGERY, AND ITS HARMFUL RESULTS.*

Modern surgeons are apt to smile at the term "laudable pus," and yet the earlier surgeons were not so far wrong in their nomenclature after all. Along with the teaching of scientific cleanliness in the schools at present, there is developed the idea that wound discharges in general are unclean, and are to be removed. It is an idea of gross cleanliness that would appeal to one's kitchen maid, if he had the fortune to possess such a maid, and the application of the idea is full of harmful consequences. Trained nurses and the assistants on the hospital staff are so imbued with the idea of keeping wounds clean that it is difficult to teach them the advantages

* A paper read at the meeting of the Keuka Lake Medical Association and published in the *Medical Record*.

of skillful neglect. Let us consider in detail three classes of cases: Those in which we are to have epithelial repair, those undergoing connective-tissue repair, and cases in which we are to have endothelial repair.

A case representative of the first class would be one of an open incised wound of the arm, with granulation well under way. New hyaline epithelium is shooting out from the epithelial borders rapidly on all sides, but the new cells are so delicate that their area is barely visible to the naked eye. The surface of the wound is bathed in pus. If this pus is wiped away whole rows of young epithelial cells are disarranged, repair is delayed, and nature has to help out with an undue amount of connective tissue, resulting in a larger and harder scar. If the pus is washed away with an antiseptic, the corrosive action of the antiseptic damages the new epithelium. Even if the pus is removed by simply pouring on sterilized water, damage results. This is because even plain water is corrosive, the

chief reason for this being that it quickly absorbs salts by exosmosis from the new cells. A ready demonstration of the corrosive action of ordinary water is furnished by putting a couple of drops upon the conjunctiva. The conjunctiva smarts and becomes red, and there is a profuse lacrimal secretion, several minutes often elapsing before the irritation ceases. If we use a physiologic salt solution there is no irritation, and new cells would be damaged only by the application of the solution with sufficient force to produce mechanical injury. It may be well to remember, by the way, that the six-tenths of 1 per cent. salt solution that is in common use is isotonic for frog's blood, but that the nine-tenths of 1 per cent. solution is isotonic for man.

Hydrogen dioxide is the most destructive of all the agents commonly used for cleansing purposes.

If we place cotton or gauze ever so gently upon a surface undergoing epithelial

repair, the dressing is harmful because new cells are caught in the mesh and torn away when the dressing is changed. A wound undergoing epithelial repair, then, is to be carefully protected against cleaning, and it must have some protecting medium like silver foil, Cargile membrane, or Lister's silk between the new cells and the absorbent dressing. There is no objection, to be sure, against keeping the skin in the vicinity of the wound as neat as one pleases, but not the wound itself.

A case representative of the second class, one undergoing connective-tissue repair, would be furnished by a fecal fistula. Repair is conducted by the heaping up of new connective-tissue cells in delicate granulation tissue, and gradual contraction of the wall of the sinus. If we think that the fistula must be kept clean in accordance with the idea of obtaining gross cleanliness, the water that is used may distend the contracting wall. If it is plain sterilized water, or water containing antiseptics, it will

corrode the delicate granulations and delay repair. Such a fistula which would close spontaneously in a few weeks, can be kept open for months by conscientious and well-meant attention on the part of physicians and nurses.

Of the third class of cases, in which we are dealing with endothelial repair, an example would be furnished by a case of perforative appendicitis with extensive peritoneal infection; and these cases furnish a pretty large death rate in response to the application of the idea of obtaining gross cleanliness. An active warfare is in progress between the bacteria and the leukocytes. The bacteria are winning. If we open the abdomen quickly, remove the appendix, and do not stop to make too careful cleansing of the peritoneum, the patient retains his natural resistance, the tide of battle is turned, and the endothelium of the peritoneum attends to final cleansing, and to repair of its own defects. If we believe, however, that it is necessary to get the peri-

toneum as visibly clean as possible by washing and wiping, and by putting in gauze masses for drainage, the patient is deprived of much of his natural resistance by the severity of the surgery, and he cannot quickly manufacture leukocytes and carry on excretion and repair. It is very easy indeed to produce death in such an appendicitis case by conscientious attention, and I believe that more than one of my earlier patients was lost in this way.

It is more or less harmful to one's reputation to avoid giving way to the idea of gross cleanliness, and to employ skillful neglect instead, for such treatment is not generally approved by the nurses or the members of the family, or by many physicians who call it carelessness; but the results are so quickly in evidence that one is likely to be impressed by the fact that previously he has been trying all along to do too much.

THE CHOICE OF PROCEDURE IN CASES OF
LOOSE KIDNEY.*

Too much operating has been done for loose kidney. Too little operating has been done for loose kidney. How can we blow hot and blow cold on this subject? How shall we proceed to advise our patients, and what shall be the choice of procedure in cases selected for operation?

Ten years ago when the subject of loose kidney became a prominent one, many operations were done indiscriminately, on the mere discovery that a kidney ran out of its range in any given case. A parallel history belongs to other ventures into new fields, in surgery, in mining, in banking, in law.

* Read at the meeting of the Surgical Section of the New York Academy of Medicine, December 7, 1906, and published in *American Journal of Surgery*, January, 1907.

Many of the loose kidney patients of earlier days have to look upon their operations in an altruistic way, and to be grateful only for the opportunity which they had for furnishing negative testimony for the benefit of the profession, and incidentally for the public.

It is now the time for us to halt and look over the field with critical eye, and with judgment based upon full and ripe experience.

Herbert Spencer tells us that men judge from a standpoint of personal experience only. In my own personal experience some of the most brilliant results have been in loose kidney patients. On the other hand, there are cases that I would like to try over again just once, or to leave entirely alone. There have been fewer and fewer of the latter class, but once in a while there is one still. This must represent the experience of other surgeons who are consulted many times a year in loose kidney cases, but the points that I would make are chiefly for surgeons

who are just entering the field, and who are fully equipped for everything excepting for bearing with good grace the feeling that they have done an unnecessary operation. It is hard for conscientious men to equip themselves for that. The experience is to come most often to those who can least afford it.

Loose kidney is so common that we can find it in perhaps 10 per cent. of all of the women who come into the office. Are we to operate for loose kidney upon 10 per cent. of all the women who come into the office? The medical consultant thinks not.

What are we going to do about it then?

When one first goes to China all Chinese look alike to him, but after he has been there for awhile he will at least get to know the men from the women. In the same way one gets to classify loose kidney cases elaborately, and to do this well requires such a range of general medical information that the subject gets to be rather engaging. As a rule, it is well to ask these patients to allow consultation with authorities upon various

phases of their cases. It is better for the surgeon to assume the position of one who is to make it easier for the family physician to manage the case, rather than to promise the patient a quick and easy route out of difficulties.

The following classification of cases seems to me to be a good one for a working basis:

1. Loose kidney present, but causing no disturbance.

Note: We must prove by clinical tests that the condition is causing no disturbance, and not depend upon the off-hand opinion that one sometimes gets from pretty good authorities.

2. Patients with minor psychoses associated with, but not dependent upon the presence of loose kidney.

Note: These cases are traps. The patients are the ones who brought discredit upon ovarian surgery. They are the invalids who drive the good pastor of the church out of religion into the real estate business.

3. Patients with minor or major psychoses precipitated or intensified by the presence of loose kidney.

Note: Consultation between unprejudiced surgeons and unprejudiced neurologists is demanded in this class of cases in order to separate the cases from those of Class 2. I have had two patients who were cured of cyclical mania by fixation of their loose kidneys. The result was not due to the effect of the operation *per se*, as sufficient time has elapsed to prove that point. Recovery was probably due to relief from the toxemia of intestinal disturbance rather than to relief from any direct or reflex cerebral impression made by the loose kidneys. In dealing with these patients it is best to state that the operation may be useless.

4. Patients with various reflex gastrointestinal disturbances, depending upon the influence of loose kidney through the large sympathetic ganglia.

Note: These patients are the ones who

make the most brilliant response to surgical treatment. We have to rule out carefully by exclusion a number of causes which produce the same symptoms. The most commonly overlooked causes for reflex gastrointestinal disturbance are perhaps (a) eye strain, (b) bile tract adhesions—"gall spider cases," (c) fibroid degeneration of the appendix. These causes are so common that we must at least rule them out methodically in practically every loose kidney case that comes into the office. It is particularly difficult to rule out the fibroid appendix because of the reason given by Edebohls, who showed that loose kidney is often the cause for congestion of the appendix. Palpation will usually allow us to differentiate between Edebohls' appendix and the one I describe. Edebohls' appendix is plump and persistently tender. The one which I describe is hard, seldom plump, and not persistently tender.

5. Patients with direct local mechanical results from the influence of loose kidney.



Fig. 5.

Note: Bright's disease, of the type described by Delafield as congestive nephritis, is common, and is probably due to torsion of the renal vein. Renal colic is caused by torsion or angulation of the ureter. Renal neuralgia is due to torsion or tension of the sensory nerves that are involved. Cases of this fifth class give the cleanest group of cures after kidney fixation.

6. Patients in whom loose kidney is only a part of a panptosis of the abdominal viscera.

Note: Fixation of the kidney is futile or sometimes very injurious, unless we correct the other visceral ptoses at the same time, and repair the rectus muscle diastasis. The operation is too severe to be recommended until abdominal supporters have been well tried. Some of these patients get on pretty comfortably with supporting apparatus, but others come to us finally with the statement that they prefer to have their supporters inside of the abdomen instead of outside.

What treatment shall we propose for our loose kidney cases?

Abdominal supporters first, in all of the panptosis cases.

Abdominal supporters first, in all of the cases where there is doubt about whether loose kidney is hero in the play of symptoms. Some of these patients will get so much relief that they do not ask for operation. Others will improve just enough to show that we are on the right line, but that operation will do the work best. I had one male loose kidney patient who had been treated for "stricture of the esophagus" in one city, for "gastric ulcer" in another city, and upon various other diagnoses in other cities. He was a man of means and could afford many diagnoses. External support of the kidney relieved him from the effects of all these diagnoses. He gained forty pounds in weight, and took on such an amount of new kidney fat that it limited the range of the kidney, and we did not have to operate.

In cases selected for operation, which method of work is to be chosen?

I have tried all of the type methods with the exception of the new one of Longyear, who shortens the nephrocolic ligament. The point gained by that operation seems to be covered by the combination which I now employ, and which gives me a feeling of perfect comfort and satisfaction. After some years of experience, it is now a pleasure to operate for loose kidney, in the right sort of cases. The operation combines the idea of Goelet, who suspends the kidney by a kangaroo tendon suture entering the lower pole of the organ, carried alongside the dorsum for half the length of the kidney, then across to the other side, then down and out near the point of entrance. The two loose ends are made fast to any convenient muscular attachment. This hangs the kidney right where we want it. The next point in the combination includes the ideas of Senn and of Longyear. The lower pole of the kidney is packed with iodoformized

gauze in such a way that the retroperitoneal pouch is distended. Gauze in a narrow strip is employed, and one end of the strip is left outside of the wound. The sheath of the quadratus lumborum muscle alone is sutured, and that allows good approximation of deeper structures. The skin is sutured with the exception of just enough room for the loose end of gauze. It is, perhaps, not necessary to state that the fatty capsule of the kidney is trimmed and that the fibrous capsule is split along the dorsum in order to give relief from the immediate inflammatory tension. At the end of a week the iodiformed gauze is removed, and no further attention is required excepting change of outer dressings for neatness. The patients are allowed to get out of bed at the end of fourteen days, as a rule.

What does this combination accomplish? The suture hangs up the kidney in a simple way. The gauze acts as a drain at first, and then causes the formation of granulation tissue, so that the secondary adhesion

obliterates the retroperitoneal pouch and fixes the nephrocolic ligament.

The only instruments that I use for the operation are a pair of scissors and a needle. It is not necessary to tie any vessels. Avoidance of deep suturing obviates the danger of catching the lumbogluteal nerve where it would get pinched. The entire operation can be done in fifteen minutes, and I have fixed both kidneys in that length of time without working hurriedly at any moment. Patients are in much better condition after any sort of expeditious operation than they are if we putter. My position on the subject of rapid operating has been misunderstood. My idea is simply to conserve the natural resistance of the patient. We get better results from expeditious operating, provided that no detail is neglected. This combination operation is a speedy one, and leaves no detail neglected.

MANAGEMENT OF THE ADIPOSE LAYER IN SUTURING.*

Text-books and teachers sometimes describe methods for giving special support for the adipose layer, by means of sutures which enter that layer. All sutures which enter the adipose layer set free a certain amount of fat, and this free liquid fat burrows in accordance with the principles of hydrostatics. The burrowing free fat leaves channels which must be closed by reparative processes, in a tissue which is badly equipped for carrying on repair.

The principle of depending upon atmospheric pressure for keeping adipose tissue surfaces in contact is not universally employed by surgeons. The wet leather disk used by boys as a toy for lifting stones demonstrates the principle.

* Reprinted from *Surgery, Gynecology and Obstetrics*, April, 1907, page 538.

The method which I have employed for many years in abdominal work is as follows.

1. A continuous suture of very small catgut for the peritoneum, in order to cause the least degree of irritation which would lead to plastic exudation and subsequent peritoneal adhesions.

2. A continuous suture of small or large catgut for the posterior and anterior sheaths of the abdominal muscles, allowing atmospheric pressure to close out "dead spaces" evenly in the muscle tissue.

3. A continuous catgut suture through the skin only, carefully avoiding the adipose layer. If the adipose faces of the wound are pressed snugly together before the last knot is tied, they will adhere so firmly and evenly that it will be difficult to separate them twenty-four hours later, if for any reason it becomes necessary to reopen the abdominal incision.

I have sutured many abdominal walls in which the adipose layer was more than two inches in thickness—sometimes nearly

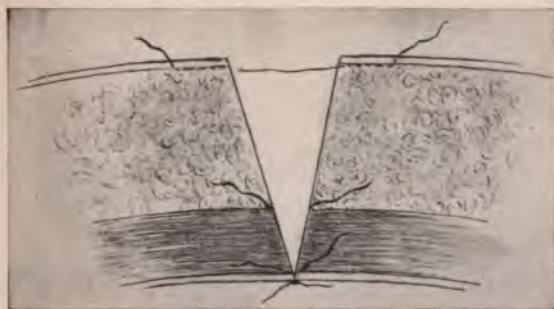


Fig. 6.



double that thickness—without having the wound break down. In my early experience, when special supports for the adipose layer were applied in or through that layer, the wound often broke down in four or five days.

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