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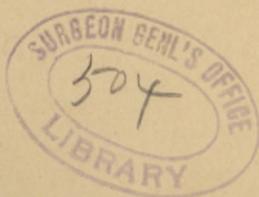
The Question of Early Catharsis
after Cœliotomy.

BY

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THE QUESTION OF EARLY CATHARSIS AFTER
CÆLIOTOMY.*

By JOSEPH BRETTAUER, M.D.,

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A glance at our latest text-books makes it sufficiently evident that the question of catharsis after cœliotomy is by no means settled. Though the older method of one week's artificial constipation is nowadays scarcely practiced, nevertheless there are still important differences between the two methods of treatment in vogue: in the one, the bowels are not moved till about the fourth day; in the other, catharsis is induced at the earliest possible moment, even within twelve hours after operation. The undoubtedly excellent results attained by either method seem to indicate that it is not the time at which catharsis is induced, but rather a wholly different factor, recognized by every one as the *conditio sine qua non*—namely, perfect asepsis—to which the good results must be ascribed.

Though I can not agree with those who attribute to this question a rôle of the first magnitude, I venture to bring this subject before you because a few cases do occur in which the question is of great, perhaps even vital, importance, and because several considerations present themselves to me in this connection to which I should like to call your attention.

The great majority of our cases are uncomplicated, and do not demand immediate operation. We have sufficient time to prepare the patient and secure thorough evacuation of the bowels. Thus prepared, with empty stomach and quiet intestines, our patients are operated for ovarian cysts, fibroids, and some diseases of the annexa. The operation is simple, requires but little time, and the patient is brought to bed in good condition. The first few hours the patient may be disturbed by some after-effects of the anæsthetic, but, as a rule, after twenty-four hours we find the following condition: Temperature normal, or perhaps slightly raised; pulse, 80 to 100; abdomen flat and painless on palpation; tongue clear and moist; urine

* Read at a meeting of the New York Obstetrical Society, held December 19, 1893



normal in quantity or possibly a little concentrated; subjective symptoms on the whole good. The nourishment taken during this time almost *nil*; gases have not yet escaped.

Does there exist at this moment any indication whatever for interference with the natural course by starting peristaltic action? I should say, Certainly not.

In the normal course of these cases we notice after forty-eight hours, exceptionally somewhat sooner, the escape of gas *per anum*—a welcome sign of intestinal permeability and beginning peristalsis. If, now, at this time, any untoward symptoms, such as a feeling of fullness or tympanites, develop, an enema of soapsuds or olive oil suffices in the vast majority of cases to bring on thorough evacuation and to relieve the patient.

This is the course I look for after uncomplicated operations, and in them I follow the golden rule: "Let well enough alone."

Sometimes we find on the second day an elevation of temperature from one to three degrees, accompanied by slight tympanites and an acceleration of the pulse. These symptoms are regarded by many operators as an urgent signal for immediate catharsis, on the ground that thorough evacuation of the bowels may prevent septic infection or abort the process already started. Are we justified in considering the symptoms I have referred to as necessarily the signs of septic infection, or can we not find another more probable explanation for them? Most of us, unhappily, have not yet attained the lofty standpoint of a recent writer, who says: "Of peritonitis following an operation I know nothing, except, in a general way, that it is treated by salines." Nevertheless, our results sufficiently prove that acute septic infection is extremely rare, except in those cases in which the field of operation is infected beforehand.

We have in some cases a considerable exudation of fluid from blood-vessels and cut lymphatics, caused by the irritation of our operative procedures. The exuded fluid necessarily undergoes some chemical changes, and it appears to me far more reasonable, in the light of our present knowledge, to attribute the symptoms in question to the absorption of chemical products in this aseptic lymph. That in the majority of these cases we are not confronted by a septic infection is perfectly clear from the further course they take when left without any treatment whatever; within twenty-four or forty-eight hours after the development of the suspicious symptoms the temperature has fallen, the pulse-rate is reduced, and the tympanites is relieved by a simple enema.

There is another possible explanation for these symptoms—a factor on which some surgeons base their advocacy of the use of purgatives. All of us have met with cases in which an elevation of temperature was due to constipation, and disappeared after evacuation had taken place. It is probable that in such cases the temperature is caused by absorption of products of decomposition of the intestinal contents, the so-called “auto-infection.” Certainly, there are some abnormal or diseased conditions of the intestinal mucous membrane in which this is possible, but, as we are at present considering only cases with normal digestive organs, we are justified in not regarding this as a likely source for these symptoms.

But, granted that septic infection has taken place, how are we to explain the action claimed for the purgatives? None of the writers on the subject, so far as I know, have entered to any extent into a theory of this alleged action of salines, except Dr. Boise, of Michigan, who says* (page 6) that they relieve the congestion of mesenteric vessels, “causing them to replenish themselves from the fluids in the peritoneal cavity, thus promoting absorption and drainage”; and, again (page 7), the introduction of salines will “establish the flow of fluid toward the intestinal canal. . . . Thus, good intestinal drainage is established, the effusion of serum into the cavity is checked, the culture medium of pyogenic germs is removed, and septic peritonitis is prevented.”

Now, in the first place, exudation is not necessarily present in cases of septic infection. Secondly, if the vessels are made to absorb an infected exudation, in accordance with this theory, they are doing just what it is desirable that they should not do. The danger in septic infection depends solely on the absorption of the toxins produced by the germs in the peritonæum, and if we were able to prevent this absorption absolutely, the infection would be harmless. Thirdly, even though the fluids may be taken up by the vessels or transuded into the intestinal canal, the source of the danger, the germs—the manufacturers of the poison—would remain behind, and therefore no permanent advantage would be attained.

It seems to me clear, for the reasons which I have given, that the prevalent faith in the beneficial action of early catharsis in cases of supposed or threatening peritonitis is not justified. On the contrary, I believe that by this treatment the great advantage of keeping a pos-

* The After-treatment of Coeliotomy. *Ann. of Gynecol. and Pædiat.*, October, 1893, vol. vii.

sible peritonitis circumscribed within its limits is sacrificed, and thus more harm than good may be done.

The second, and not less important, reason for which early catharsis is advocated is that by this means the formation of peritoneal adhesions is prevented. A final explanation of the cause of peritoneal adhesions has not yet been formulated, and a definite conclusion will be arrived at only as the result of experimental studies. The many experiments which have already been made (by Sanger, Dembrowsky, Kelterborn, Kustner, Thomson, and others) have yielded the most diverse results. One fact is certain: that adhesions occur not less when the intestines are in active motion than when they are at rest. In this matter I share the opinion of Reichel,* who regards the various measures taken for the prevention of adhesions as useless refinements, which, missing the real cause of the formation of adhesions, may even be harmful. The observation of Reichel that normal peristalsis suffices to overcome newly formed adhesions, and of Kustner,† who, on reopening the abdomen fourteen months after an operation, found no trace of adhesions which he had previously seen, rather indicate that we attach far too much importance to this subject.

I have thus far attempted to show that the advantages claimed for early catharsis rest on no scientific foundation, without entering into a consideration of the positive harm which the unqualified and invariable employment of early purgation may accomplish.

In the great majority of cases it is a matter of no consequence whether the bowels are moved a little sooner or later. But in those cases in which early catharsis has been unsuccessfully attempted, in which symptoms develop, analogous to those which occur more commonly after severe and complicated operations—inconstant vomiting, intense distention, etc.—in those cases the further exhibition of purgatives is, in my opinion, decidedly harmful. It is to this point that I should like to see the discussion of the society especially directed.

Permit me to present the following illustrative cases:

CASE I.‡—Mrs. K., abdominal hysterectomy, performed by Dr. Cleveland at the New York Cancer Hospital; vaginal opening into the peritoneal cavity tamponed with iodoform gauze; thirty-six hours

* Paul Reichel, Zur Pathologie des Ileus und Pseudoileus. *Deutsche Zeitschrift fur Chirurgie*, 1893.

† *Centbl. fur Gyn.*, 1890, No. 24.

‡ *Transactions of the N. Y. Obst. Soc.*, 1893, page 300.

after operation routine treatment was followed and several doses of salt were given without effect other than to produce vomiting. Enemata of soapsuds, olive oil, turpentine, and high rectal tube were then employed and calomel given, but no evacuation was produced, not even gases being passed. The condition of the patient grew steadily worse, vomiting became more frequent, meteorism increased, and the patient died twelve hours later with the signs of intestinal obstruction. Post-mortem examination revealed no sign of septic peritonitis; the entire intestinal canal was enormously distended; the sigmoid flexure was empty and adherent to the gauze tampon, but no obstruction was apparent; all other organs somewhat anæmic but apparently normal.

CASE II.*—Cœliotomy by Dr. Cleveland at the New York Cancer Hospital. Extra-uterine pregnancy. Extirpation of the sac difficult and tedious; the large cavity packed with gauze. Although during the first three days the temperature rose to 101.5° to 102° , the pulse was 120, and slight abdominal distention present, no attempt was made to move the bowels, but morphine was given quite freely. On the fourth day, when the temperature showed a tendency to decline, a dose of calomel followed by Epsom salts produced several copious evacuations. Patient recovered with an abdominal sinus.

CASE III.†—Operation by Dr. Boise for inflammatory disease of the appendages. The case seems to me of such importance that I take the liberty of quoting it to some extent, at least, in the words of the author. Pus was found in the right tube. The operation was a difficult and tedious one; no drainage was used. "The patient rallied well but was troubled very much with vomiting and severe abdominal pain. The bowels gradually became distended with gas, and the stomach refused to be quieted. Although the temperature was not high and the character of the pulse good, I feared peritonitis, and attempted to move the bowels by means of sulphate of magnesia in repeated doses, but with no effect except aggravation of the stomach symptoms. Enemata were then resorted to—turpentine and castor oil, oxgall, large quantities of water administered through the long tube, concentrated solution of salt, etc.—but with no result. There was, however, constant regurgitation from the stomach, with some abdominal pain and increasing tympanites. The patient was seen by Dr. Etheridge, of Chicago, who concurred in the diagnosis of

* *Transactions of the N. Y. Obst. Soc.*, 1893, page 103.

† *N. Y. Jour. of Gyn. and Obst.*, 1893, No. 2, page 118.

intestinal paralysis and predicted death within twenty-four hours. All active measures were stopped and a quarter of a grain of morphine administered hypodermically. This was followed by cessation of pain and vomiting, with rest to the patient. The administration of another enema after the lapse of several hours was followed by copious free and offensive discharges—so free and frequent as to be entirely involuntary. From this time the patient recovered, but developed a pelvic abscess on the left side, which was opened through the abdominal wall. . . .”

CASE IV.—Mrs. G. Dilatation of the cervix by a practitioner in this city was followed by pelvic peritonitis, in which condition she came under my care at the Mount Sinai Hospital. After the general symptoms had nearly subsided I removed with considerable difficulty the diseased appendages, and packed the cavity with gauze on account of the presence of very offensive pus in the left tube. The patient did well until the end of the second day, when she began to vomit and showed some tympanites. Temperature, 102° ; pulse, 125. In the habit of inducing catharsis at this period, especially in the presence of such alarming symptoms, the acting house surgeon ordered the bowels to be moved by salts, or, if they were not well taken, by enema. These measures were without effect other than to aggravate the condition of the patient; meteorism became intense, and vomiting of greenish fluid incessant. In this condition I found the patient forty-eight hours after operation. I at once stopped all attempts at moving the bowels, and ordered an ice-coil to the abdomen and a third of a grain of morphine hypodermically. Nourishment and stimulants were given by rectum in very small but repeated doses. Twenty-four hours later the alarming symptoms had in great part subsided. Vomiting had nearly ceased, the distention was reduced, the abdomen was softer and nearly painless on palpation. During the following day improvement was steady, gases passed spontaneously, and an enema of castor oil and glycerin produced a good movement. The patient recovered with an abdominal sinus, for which she was recently operated by Dr. Mundé.

It is clear that all of these cases present more or less marked degrees of intestinal paralysis, or, as some call it, “pseudo-ileus,” manifested, to use the words of Dr. Boise, as a refusal of the muscular fibers of the intestines to act under the ordinary stimuli.

To enter into a complete account of the physiology of peristalsis would carry me beyond the limits of this paper. I wish, however, briefly to mention a few points. The intestines are at rest when

they are empty ; peristaltic action is ordinarily started by the presence of chyme or fæcal matter. There are, of course, a number of minor causes, which act as stimuli. Brunton has shown experimentally that excess of venous blood stimulates peristalsis, whereas excess in the arterial flow inhibits intestinal movements.

The fact that irritation of the sympathetic fibers in the peritonæum is followed by contraction of the arterioles, and therefore by diminution in the arterial supply in the mesenterium, affords one explanation for the paralysis ; on the other hand, the circumstance that all long and difficult operations in the peritoneal cavity necessitate severe manipulation of the intestines, separation of adhesions, and long exposure to the atmosphere, affords us another ready explanation for the occurrence of overstimulation with consequent exhaustion of the ganglia, and paralysis of the muscles supplied by them.

Of course, for our purpose it is a matter of indifference whether the ganglia in the intestinal walls themselves or the more central sympathetic fibers are responsible for this condition.

If what I have just said is correct—if, as is generally assumed, the intestinal paralysis is caused by overstimulation of the nerve mechanism of the intestinal muscles, can there be any scientific justification for the persistence in the attempts to move the bowels—that is, to further stimulate the already exhausted nerve centers ? It seems to me far more rational to stimulate the *inhibitory* centers by means of small doses of opium in those cases in which the conditions described are favorable for the development of intestinal paralysis ; and later, if paralysis has occurred, to paralyze these centers by large doses of opium. This action of opium, analogous to the action of small and large doses of digitalis on the heart, has been demonstrated by Nothnagel.*

Reasonable as this mechanical theory of the origin of intestinal paralysis appears, I can not give it my unqualified support, but prefer another explanation, at least for the majority of cases. If the mechanical insults sufficed, we should expect to meet with intestinal paralysis far more frequently than it occurs—to meet it, in fact, after every case involving severe manipulations of the intestines. As a matter of fact, intestinal paralysis is, on the whole, a rare occurrence. I am inclined to think that it always depends on an infection with germs whose identity has as yet perhaps escaped us, but whose metabolic products exercise a paralytic action on the intestinal muscle fibers.

* Nothnagel, *Beiträge zur Physiologie und Pathologie des Darmes*, Berlin, 1884.

We all know that it is frequently impossible to differentiate acute septic peritonitis from mere intestinal paralysis, which, indeed, is always a symptom of the infectious process.

In the second place, in my experience, we encounter intestinal paralysis most frequently in those cases in which the conditions for septic infection are present before operation. Dr. Coe,* for instance, has expressed the opinion that iodoform-gauze packing, which some of us use in these cases, favors the development of paralysis. I would rather suggest the view that it is just in those cases in which we have reason to fear an infection that we make use of Mikulicz's method; in other words, that it is not the gauze packing that we use in septic cases, but the sepsis itself which is responsible for the paralysis.

In the third place, in the cases which I have cited, and I am sure they are fairly typical of this class of cases, there was the most positive evidence of infection—pelvic abscess and abdominal sinus—except in Case I, in which the evidence was negative so far as the gross appearances were concerned; but we know from experiments, especially Reichel's,† that a septic process in the peritonæum does not necessarily produce macroscopic lesions.

Permit me, in conclusion, to sum up briefly the points which I have endeavored to establish in this paper:

1. In the vast majority of our cases of cœliotomy it is a matter of absolutely no consequence if the bowels are moved early or not. Our good results depend solely upon perfection in the operative technique and asepsis.

2. In those cases in which early attempts to move the bowels have failed, persistent efforts to produce catharsis should not be made, but, on the contrary, a dose of opium (morphine) is indicated.

3. In those cases in which we have employed iodoform gauze to exclude the field of operation from the general peritoneal cavity, to prevent infection, it is, in my opinion, decidedly bad practice to attempt early catharsis; but we should rather wait until the sign of the restitution of normal peristalsis has been given—that is, the spontaneous escape of gases per rectum.

45 EAST SIXTY-FOURTH STREET.

* Dr. H. C. Coe, *The Ætiology and Pathology of Pelvic Adhesions*. *New York Journal of Gynecology*, 1893, p. 387.

† Reichel, *loc. cit.*

