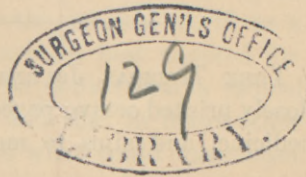


Kelsey (C. B.)

Excision
OF
Cancer of the Rectum,
An Analysis of One Hundred and
Forty Cases.
BY
CHARLES B. KELSEY, M. D.
REPRINT FROM
The New York Medical Journal.
December, 1880.



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AN ANALYSIS OF ONE HUNDRED AND FORTY
CASES OF EXCISION OF CANCER OF THE
RECTUM.

By CHARLES B. KELSEY, M. D.,

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THE treatment of cancer of the rectum by excision has not yet been accepted by the surgical world as a substitute for other measures, even in cases best adapted for the operation, although it can not be denied that a radical cure has sometimes been obtained, and that in many other cases life has been prolonged beyond what could have been hoped for by any other means of treatment. It is no less true that the operation is one of great danger, and that there are not lacking those whose experience has led them to believe that life was rather shortened than lengthened by it. By these it is claimed that in lumbar colotomy we have a safer method of relieving pain, and delaying the progress of the growth, and in both these ways prolonging life. American and British surgeons hold rather to this latter idea, while the French and the Germans favor excision.

Again, lumbar colotomy has been tried many times. It is

comparatively safe of performance, and in the vast majority of cases it gives a certain amount of relief; while in excision the surgeon is treading on newer and less certain ground. The operation itself is surrounded with dangers. He fears either immediate death for his patient, or a speedy return of the disease; and he turns to the more common treatment with the reliance that he is pretty sure to do *some* good to the sufferer, and leaves the newer ground for more venturesome operators.

There are several questions yet to be answered concerning excision of the rectum for cancer, and most of them can only be answered by experience. For the purpose of arriving at a knowledge of what experience has already taught in this matter, I have collected the reports of operations up to the present time as far as they are attainable. I can only regret, in passing, the incompleteness of the record in many of the published cases—an incompleteness depriving them of much of the value they would otherwise possess. It amounts to very little, in the consideration of a question such as this, to know that a patient of a certain age had some kind of cancerous disease in the rectum, that some sort of an operation was done, and that in a certain number of days or weeks the patient died or left the hospital with the wound more or less cicatrized and with more or less control over the fæces; and yet in a large proportion of the following cases this is the total amount of information to be derived.

I have, however, tried to glean all that could be gathered from the material in our hands, and submit it as it stands, subject to any modifications which a larger collection of better reported cases may make necessary.*

The questions for which a solution has been sought are chiefly these. What are the dangers, and what is the mortality of the operation? to what class of cases is it applicable? what are its results as a curative and as a palliative measure?

* Instead of copying cases in full or preparing a table (troublesome both to writer and publisher, and very likely to be tiresome to the reader), I shall give at the close a list of the operators, with reference to the place of record. I have been at no exhaustive search to compile a *complete* list of operations, but have gathered together those best known and most easily attainable.

how do these results compare with those of lumbar colotomy? and, finally, the results as to the control of the fecal evacuations.

Considering the operation, first, merely as a surgical procedure, we find that in twenty-two cases death followed as a direct result of the interference. The causes of death, in their order of frequency, were: peritonitis, ten; pelvic cellulitis and phlebitis, four; septicæmia, three; exhaustion, three; hæmorrhage, one; and erysipelas, one. Of these fatal results, four were due to accidents which may attend upon any surgical operation, viz., those due to erysipelas and exhaustion; and the others were from causes having their origin in the peculiar nature of this operation and the parts operated upon. Considering these alone, we find the three great dangers of the operation to be peritonitis, pelvic cellulitis, and septicæmia. The single death from hæmorrhage in one hundred and forty operations may, I think, fairly be dropped out of consideration, especially as the operation may, if desired, be rendered almost bloodless by the use of the *écraseur* or galvanic cautery.

I have tried to discover whether there was anything in the nature of the individual cases which were fatal to account for that result, and we find that in thirteen of them in which these data are given the situation and extent of the disease are described in eight as follows: (1.) The inferior extremity of the rectum was completely surrounded with hard, knobby tumors encroaching upon each other, causing a stricture through which there existed scarcely any passage, and extending so high that their upper limit could scarcely be determined by rectal touch. The recto-vaginal septum was involved in the disease to such an extent as to render the isolation of the latter impossible. The peritonæum was opened. (2.) The anus was surrounded with carcinomatous nodules; the disease reached four inches into the rectum, and the mass was adherent to the sacrum, coccyx, and posterior wall of the vagina. (3.) The disease completely surrounded the anus, reached three inches up the rectum, and the inguinal glands were involved. The peritonæum was opened. (4.) The operation involved three inches of the rectum, and the peritonæum was wounded. (5.) The disease extended beyond the reach of the finger, and only a

part could be removed. (6.) The rectum was completely surrounded for a distance of three inches. (7.) The disease began about six centimetres above the anus, was continuous on one side with a mass of the size of a hen's egg, which was fixed to the pelvic wall, and extended beyond the reach of the finger. The peritonæum was opened. (8.) Nine and one half centimetres were removed, and the peritonæum was opened.

These cases seem to point to a very evident relation between the extent of the operation attempted and the fatal result; and I do not hesitate to say that in such extensive disease as this, where the removal necessarily involves the danger of wounding the peritonæum, the operation is contra-indicated. It is true, not only that disease more extensive than any described in these fatal cases has been safely removed, but that the operation has been followed by a long period of health; and yet I can not regard such a result as other than exceptional, nor do I consider that the slight chance of obtaining it counterbalances the risk of immediate death. The surgeon is never compelled to this operation to relieve suffering. There are other and safer means always at command.

I shall not stop at this time to discuss the question as to how much of the anterior wall of the rectum is uncovered by peritonæum. There is an old rule for applying the trephine, that in every instance the operator should remember that some skulls are very much thinner than others, and he should act on the supposition that the particular point upon which he is operating is the thinnest part of the thinnest skull ever seen. Something of the same kind might be said of the peritonæum over the rectum; and everybody who has studied the anatomy of the part knows how various are the opinions of different authorities on this point. Nevertheless, a line of danger can be marked out, and that line is about three inches from the anus. It is true that more than this amount of the rectum has been removed without encountering the peritonæum, and it has been opened below this point; but I should not, for my own part, hesitate to try to remove three inches of the bowel for a cancer, and within a few weeks I have refused to attempt to extirpate in an otherwise suitable case, because the disease

passed this line. The index finger is a good guide. What is well within its reach in a hand of good length, it is safe to try to remove, provided it does not involve surrounding tissues to an extent which renders its complete removal impossible. Whatever may be said of the impunity with which the peritonæum may be opened in other parts of the body does not seem to apply here; for I have been able to find but three cases in which that accident was not followed by a fatal result.

Regarding the question of radical cure, we find difficulty in establishing exact dates, and have to take into consideration the reputation of the reporter. We find, however, that in one hundred cases (deducting those immediately fatal, and seventeen which passed out of observation immediately after operation) we have six cases of reported permanent cure, in which there had been no return for at least ten years. Three of these are reported by Volkmann, two by Velpeau, and one by March, of Albany.

In one case the patient was alive and well eleven years after the first operation, though there had been two returns and subsequent removals (Volkmann). These are perhaps the only cases which it would be proper to consider as permanently cured, though others might be included in that category. But, for the sake of exactness, I give the subsequent histories of the cases wherever they are mentioned.

In one case the patient was alive and well six years after the operation; in two others, five; in four others, four; in three, three; and in five, two years or a little over. Nine are stated to have been alive and well, with no sign of return, at a year or a little over after the operation, and sixteen were reported in the same condition at times varying from two months to one year. In eight the return is stated to have occurred in distant parts—one in the liver, causing death eight years after the operation, and another seven years after. In the six other cases there was a return, either in the neighboring lymphatics or in the internal organs, at a time varying from one year to sixteen months. This gives a total of twenty-four cases, out of one hundred in which the result is known, in which life was prolonged from one to six years with no re-

turn of the disease; three which proved fatal after six years; and six in which there had been no return ten years after the operation. On the other hand, deducting two cases stated to have proved fatal by a return of the disease, but at a time not given, and seven cases of Labbé's in which the average time of return is said to have been ten months—in thirty-five cases where this fact is mentioned there was a local return in nine within three months, in nine within six months, in eleven within one year, in four within a year and a half, and in three within two years.

But, to arrive at a just conclusion as to the value of the operation, we must study these figures in a different light. It is claimed in favor of lumbar colotomy that the operation of excision, even when a good immediate result is obtained, may shorten life by hastening the return and final progress of the disease. Unfortunately, it is difficult to tell in any particular case how long a patient would have lived, had the disease been left to its course; but, accepting as a basis for comparison Allingham's estimate of the average duration of life in cancer of the rectum as two years or less, we are justified in concluding that in all cases where life was prolonged more than one year and a half after the time of operation (the operation generally being done late in the disease), this length of life may fairly be attributed to the surgical interference. We find, not counting the permanent cures above stated, twenty-two such cases.

This estimate is manifestly a small one, for a study of the cases makes it evident that many who did not live eighteen months after the operation yet gained a considerable length of comfortable existence; and there is nothing to prove that in any case the operation hastened the natural course of the disease. We can only guess in any given case what the duration of life would have been had the disease not been interfered with, and, in cases where the estimate has exceeded the reality, it still remains to be proved that the operation is to be held accountable. There is a marked absence of anything in the reported cases which would go to uphold this supposition. On the contrary, we find in almost every case that attention is called to the great improvement in general health,

the loss of pain, and the increase in strength. Patients go away believing themselves radically cured, return to their employments, and are reported by the French surgeons as "*parfaitement guéries,*" a few weeks after the operation.

And this leads me to call attention to another point—the operation of excision as a palliative measure. In cases properly chosen, where the disease is not so extensive as to render its removal one of the capital surgical operations, we know of nothing better, and this fact can not fail to be deeply impressed upon the reader of these cases. The statement that all suffering was relieved is almost invariable.

I have carefully searched the record of cases in which a return of the disease within six months of the time of operation is reported, to discover whether, here also, there was any marked relation between this result and the nature or extent of the disease at the time of operation; but it is especially at this point that the table fails us. A proper answer to this question involves not only a careful report of the extent of the disease, but a microscopic study of its character, and such data are given only in a relatively small proportion of cases. I believe, however, that the cases show a marked relation between the rapidity of the growth before operation and the speedy return after removal.

We can trace no connection between the time of the return and the extent of the disease removed when the removal has been complete; and the microscopic reports are too few for general conclusions to be drawn from them. I know of no writers, except Stimson and Holmer, who have made a careful study of the specimens excised, and have given the results; and, so far as the clinical reports of the German operators go, they would seem to give support to their practice of removing everything involved, no matter how extensive, in the hope that the local return may be long delayed.

One other point which has been held to weigh against this operation is the alleged incontinence of *fæces* sure to follow it. In studying these cases, it strikes one curiously to read in Gross's "Surgery" that this result is sure to follow excision of a portion of the rectum sooner or later. In forty-five cases in which the condition in this respect is noted, there is stated to

have been complete control over the evacuations in seventeen, control except in case of diarrhœa in nine, a fair amount of control (enough to prevent accidents, provided the patient were able to attend to the call of nature as soon as it was felt) in ten, and complete incontinence in nine only.

Admitting the fact, how can it be accounted for? For my own part, I have studied this question till I find it much easier to prove that there should not be incontinence after destruction of the sphincters than to explain why there should be. I believe the sphincters play a very secondary rôle in the physiology of defecation, and yet a great degree of incontinence has been seen to follow their simple division in cases of fistula. The anatomical arrangement which was first described by Nélaton under the name of a "third sphincter," and which is supposed to take the place of the others, is not all a myth, neither is it what its name would indicate to one having in mind the muscular band closing the anus; and Houston's folds of mucous membrane, though sometimes heavy enough to obstruct the passage of fæces, can not be relied upon. In the reports of cases there are many in which it is stated that the patients were able to live comfortably by giving immediate attention to the desire to evacuate; and it is perhaps the *power to resist* a desire to evacuate the bowels, rather than a *constant resistance* to the passage of fæces which have accumulated in the rectum, which best expresses the function of the sphincters in defecation.

The periodically recurring descent of fæces into the rectum causes the desire to evacuate. In health, we are able to resist that desire, and after a certain time it may pass away and may not return till a corresponding hour of the succeeding day. Its passing away is probably due to the retreat of the fæces into the sigmoid flexure. When the sphincter is destroyed, we still have the warning of the descent of fæces, but the power of control or resistance after such descent is gone.

This is emphatically coming back to O'Beirne's "new views of the process of defecation"; but the views which he published in 1833 are essentially those which Foster publishes in 1880, and they are founded on the facts (?) that the rectum is normally empty, and that, except in cases of chronic con-

stipation, where the rectum has become unnaturally distended, no fæces will be found pressing against the sphincter or below the sigmoid flexure.

The fæces, as they pass along the colon, are lodged for a time in the sacculi, and finally accumulate in the sigmoid flexure, where they are supported by the sacrum and perhaps also by the bladder. Defecation is the result of a voluntary effort at first, but yet is actually accomplished by a mechanism beyond the control of the will. The voluntary part of the act is shown in two ways: first by inhibiting the action of the lumbar nerve center which controls the sphincter, and thus relaxing its normal tonic contraction; and secondly, by the voluntary pressure on the colon by the abdominal muscles. But neither of these is sufficient to empty the sigmoid flexure, and they are therefore joined to a third involuntary element in the act—an increase in the peristaltic movements of the flexure itself. This, however, is not exactly the order of action. The sigmoid flexure becomes full, and the pressure of the fæces excites in it an increased peristaltic action, by which its contents are pressed onward into the previously empty rectum and come to press upon the sphincter. The sphincter is then relaxed by the voluntary inhibiting of its spinal nerve center, and the pressure of the abdominal muscles is brought to bear upon the descending colon; by which, and by the increased peristalsis, the sigmoid flexure and the rectum are entirely emptied.

Accepting these views of the physiology of defecation, it is not difficult to understand why there should not be complete incontinence after destruction of more or less of the lower end of the rectum and anus; and there are other facts, such as the presence of the valves, the crooked course of the rectum, and its normal contraction due to its heavy muscular layer, the consistence of the fæcal mass itself, and its tendency to lie quietly in its place till expelled by the activity of the involuntary muscular fiber of the gut, which tend to the same result. The question admits of much discussion, but the fact remains, that, as a palliative measure, lumbar colotomy has no advantages over excision on the ground of comfort and cleanliness.

From a study of the cases we are justified, then, in drawing the following conclusions :

1. The fatal results which have thus far been recorded as following this operation nearly all occurred in cases where from the extent of the disease such a result was not improbable.

2. When the disease reaches above three inches, or involves neighboring parts to such an extent as to render its entire removal without injury to the peritonæum questionable, the operation is contra-indicated.

3. Although there have been a few cases of cure, such a result is so rare as not to justify the exposure of the patient to the risk of immediate death which attends the attempt to remove extensive cancerous disease.

4. The operation is chiefly valuable as a palliative measure, and as such it is applicable to cases where the disease has not made extensive progress.

5. As a palliative measure in proper cases, it compares favorably with the results of lumbar colotomy, both in prolonging life and in relieving pain.

6. The operation is not followed by an annoying incontinence of fæces, except in a small proportion of cases.

7. The operation is not a substitute for lumbar colotomy in cases where the disease has reached more than three inches from the anus.

8. There is no proof that the operative interference shortens life by hastening the progress of the disease.

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