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DISCUSSIONS

DR. JOSEF FISCHER (Cincinnati, Ohio): I do not know of any other group that has experience with the various types of pouches and has analyzed them as carefully as this manuscript.

I would like to make several comments about three different areas that Dr. Sugerman has detailed and then to ask a question.

We emphatically support the retention of the transitional zone. And we continue to believe, and it appears that the data are gradually being accepted, that this is directly related to the preservation of nighttime continence.

It is interesting that the only patient in Dr. Sugerman's more recent series who must wear a pad is the patient in whom the dissection is down to the dentate line. In our early experience with Dr. Lester Martin, when we intentionally dissected down to the dentate line, we had a great deal more difficulty with continence than we do now.

If you look at the incidence of wearing a pad, I am not sure it is all related to the transitional zone. Our incidence of wearing a pad in a group of patients, which now numbers about 200, and we are part of the way through reviewing them with questionnaires and interviews and things like that, is about 7% in those patients without pouchitis.

We have a 6% incidence of pouchitis and we have a 7% incidence of patients without pouchitis who wear a pad, which is about the same as in this study.

Does the concern about leaving the transitional zone involve the kind of epithelium? Is it rectal epithelium or is it a different kind of epithelium? Our experience with many polyposis patients is not to have the regrowth of polyps, and I would wonder whether those patients are really transitioning into Gardner's syndrome, of whom we have seen a fair number with small bowel polyps and also gastric polyps.

I suspect that the length of rectal mucosa that we leave is about the same as Dr. Sugerman. We do not measure, but the distance between

the dentate line and the top of the columns, as we estimate it, is about 1.5 cm.

In the early part of the series, when we intentionally left 1 cm additional above where we thought the transitional zone ended, as does Dr. Sugerman, we had trouble with recurrent rectal disease. And 6 of the 12 patients in whom we intentionally left a good length of rectal mucosa have had difficulty, requiring continuous steroids, suppositories, and systemic steroids (one patient), so I do not think that is a very good idea.

The type of pouch is perhaps less important than the length of the sleeve of the exit, which, of course, is not an issue with the J-pouch because you anastomose the J pouch directly to the anus. We continue, because Cincinnati is a very traditional city, to use one form of operation, which is the S pouch. But the one thing we have done is that we have consistently shortened the length of the exit spout to about 5 mm and to make the pouch smaller. And the pouch is now about 10 cm on a side in the stretched state, the S pouch, which we believe contributes to a lower incidence of pouchitis, because the pouch empties completely.

The third question is that of diversion. If, in fact, it works out that the incidence of complications in the patients who are not diverted is lower than the patients who are diverted, then that would be a useful argument for doing some type of anastomosis and not protecting it.

Surgeons have very selective memories. As I was talking to Dr. Sugerman earlier, I said that I did not recall a patient with a leak following an ileostomy closure, but I probably would have repressed it anyway if I had. But we have had a certain number of complications from the ileostomy itself, including stenosis, a number of patients in whom we have had intestinal obstruction. And I had an occasion to revise an ileostomy at the peritoneal reflection last week in a patient 4 weeks after a diverting ileostomy.

The jury is still out on this. But if, in fact, the protective effect of the ileostomy and the leaks and the abscesses is less than that of the complications from the ileostomy or its closure, this is a very significant contribution.

I have only one question, Harvey, and that relates to the sphincter function if you do not divert these patients and they move their bowels quite early. One of the criteria that I use in closing the ileostomy is the softness of the area around the pouch anastomosis. We wait a little longer than most people, about 4 months. The reason I wait 4 months is because that is the time when, at least to my examination, the area around the anastomosis is quite soft.

How did these patients progress? Do they have difficulty early on in retaining their stools and then gradually improve quickly because of the scarring around it? Or is this something that has not been a problem for you?

DR. JOHN H. PEMBERTON (Rochester, Minnesota): In general the results of this operation have been good. This report today substantiates that observation. Our experience at Mayo with a rather large number of these patients also is good.

The questions posed by this paper are very important. Does the double-staple technique actually improve the functional results? This paper, and several others conclude that it does.

However we have reported in the literature several times that the stool frequency and rate of incontinence was largely similar to the reports you heard today. Our patients have about six stools during the day with one at night. And the rate of daytime and nighttime fecal spotting is less than 25% at 3 years. The incidence of gross incontinence is less than 1% at the same length of time after operation.

Clearly, as Dr. Sugerman states in his manuscript, the only way to determine if function is improved after ileoanal anastomosis will be to conduct a prospective randomized trial.

If such a trial shows that the double-staple technique indeed does improve continence, then the second question must be addressed: what implications are there for patients with retained rectal mucosa.

Dr. Sugerman does, unlike some people in the literature, face this issue squarely. All but one of his patients had residual disease. One might, therefore, make a reasonable case that the operation presented was actually an ileorectostomy and not an ileoanal anastomosis. This, of course, brings up a whole new set of controversies that really go right to the heart of the rationale for doing the ileoanal operation in the first place. I must stress that we do not have to worry about the second question until a trial can determine whether there is really functional improvement.

Regarding diverting ileostomy, it would be great if we could eliminate an ileostomy from this operation. We, however, have covered nearly all of our patients with an ileostomy because a stoma-related complication is bothersome but repairable, while pelvic complications such as pelvic sepsis, and its sequelae are not.

How would you advise us, then, on choosing the right kind of patient for a single-stage procedure? Finally, I worry about incorporating the posterior vaginal wall in stapled low anterior resections. This, of course, is a very low stapled resection. How do you prevent this?

DR. WILLIAM P. J. PEETE (Durham, North Carolina): I want to focus my comments on the one patient Dr. Sugerman had with congenital polyposis. In 1983 Dr. William Waddell and Richard Lowry reported the resolution of polyps in the colon of four patients who had Gardner's disease treated with Sulindac.

In 1989 the expert in this Society, Dr. Elmo Cerise, and Gary Ganzer joined with them to report seven more patients. Now they had seven patients with Gardner's disease, four patients with familial polyposis coli, seven patients who had partial colectomy and ileoproctostomy and four patients who had the polyps treated in an unresected colon. As long as the patients took the Sulindac there was complete resolution of the polyps.

About this time last year I was presented with a patient from West Virginia who had more than 200 polyps identified in his colon by colonoscopy examination, one at the hepatic flexure was carcinoma *in situ*. I thought it appropriate to remove his abdominal colon and carry out an ileo low sigmoidostomy. He was discharged 7 days later on 200 mg of Clinoril a day. A month later he was found to have 27 polyps in his rectum. I have followed him serially for more than 1 year. Most of the polyps disappeared by April.

When I scoped him 2 weeks ago, there were no polyps in his rectal segment and he was having one to two bowel movements a day.

The next patient is different. In 1988 a 52-year-old man had a 7 × 6 × 5 cm villus adenoma removed from his rectum with subsequent low anastomosis. The margins were free. However, in April of this year, I found that he had a 2 × 3 cm recurrent villous adenoma near his suture line and a 9-mm polyp below the splenic flexure.

This slide shows a biopsy done in April of 1990. The patient was told of this work with Clinoril and, facing a possible proctectomy and colectomy, and knowing that he had a benign lesion, at least according to that biopsy, which shows villus adenoma, he agreed to take Clinoril, 200 mg, twice a day.

Two months later you see the possible effect of blocking prostaglandin, diminishing DNA and perhaps tumor growth factors. The tumor at that time was one half the size it had been. I follow him every 2 months.

In November he had a repeat biopsy. He still has tumor but less.

This interesting patient makes one wonder what effect Clinoril may have on other polyps of the colon. His 9-mm polyp near the splenic flexure has not been apparent to me on endoscopy since June. Therefore it is possible and perhaps probable that we have an effective chemical prevention for carcinoma in patients with polyposis coli whether most of the colon has been removed or whether only that portion in the abdomen has been removed, possibly for other polyps.

The main side effects of Clinoril are well known.

Clinoril is moderately expensive. Two patients stopped taking it with polyp growth; then resolution when it was restarted. Even so, in the treatment of patients with polyposis coli, a total colectomy with ileostomy or near-total colectomy with very low anastomosis is probably unnecessary. Work and time will tell. Whatever route of treatment is followed, careful follow-up is essential.

DR. HARVEY SUGERMAN (Closing discussion): With regard to the anal transition zone, I kept pleading with our pathologist to tell me if this was anal transition zone and not actual columnar epithelium, but they refused to cooperate. In all of the specimens they confirmed the fact that this was columnar epithelium.

The length of the tissue between the anastomosis and the dentate line improved with time. So as we became more experienced with the technique, we got closer to the dentate line. But as mentioned, there is a point to which you should not go, and I think you do sacrifice continence and control when you get down to the dentate line.

This is different from the Mayo Clinic data, as most of the published series do describe better fecal control when some residual tissue above the dentate line is retained. And, as I will mention again, a randomized study is mandated to answer the questions prompted by this study.

In terms of the pouch type, we, too, had three 10-cm limbs with our S pouches, but our average spout was about 2 cm. This seemed to stretch sometimes because of tension since in many of these patients with the S pouches it was done because we needed more length. And we get the greatest length with the S pouch.

With regard to the ileostomy, yea or nay, it is two operations and two hospitalizations under the best of circumstances. And, in addition, you have a tradeoff between the complications, and there are complications in all series with the ileostomy itself as well as without the ileostomy.

One of the crucial questions, as already posed, is whether the disastrous complications that could occur if you do the operation without an ileostomy are life threatening or can be picked up early enough that you can save not only the pouch but, most importantly, the patient's life.

So it is important to be aggressive and operate early, with a diverting ileostomy; if a leak occurs if you do this operation without an ileostomy.

With regard to sphincter function without diversion, that is one of the negative aspects of the operation. They seem to go slightly more often at the beginning as when they've had a diverting ileostomy that you close, because at the beginning the stool tends to be more watery and burning. It may be a bit better after they have had an ileostomy, since they have had some time for the ileum to compensate. But the problem is that these patients have not had an ileostomy to know how unpleasant an ileostomy can be. And that, in a way, is a negative aspect. Because when they are going a lot at the beginning, they can not relate this to having had an ileostomy and say 'I'm willing to accept this so that with time it's going to get better.' But in our experience there has not been much difference whether the patients have or have not had an ileostomy in terms of their initial control.

With regard to whether this is an ileorectostomy rather than an ileoanal

procedure, I think that is only a matter of semantics. Is this the bottom of the rectum or the top of the anus? I think it is both. And the key issue is that this is not the ileoproctostomies that used to be constructed.

If you look at the published series with regard to the ileorectal anastomoses, they average about a 6% incidence of adenocarcinoma. And this is preserving somewhere between 10 and 15 cm of the rectum for that procedure.

The risk of cancer is projected actuarially to be 15% cumulative in a period of 30 years. So this must be significantly lower in our patients with 1.8 cm residual tissue. It will be there, and it will be a concern.

One of the other issues, therefore, is follow-up, and that is a genuine problem. Follow-up is easy because all you need to do is anoscope the patient. The problem is that many of these patients are feeling so good and are so asymptomatic that they do not want to come back.

I have concerns about the staple procedure for the polyposis patients. I am concerned that we may be leaving some residual tissue that could form polyps, and I think that those patients may be at somewhat greater risk. Again it would be easy to examine them, it would be easy to fulgurate them, it would be easy to excise them. But the question is patient follow-up and the cooperation of your patients.

The other issue is rectal bleeding. You may have bleeding because of residual disease; you may have bleeding because of pouchitis; or you could have bleeding because of the development of a carcinoma. And it will take many years before we know what that potential risk really is.

With regard to the risk of a rectovaginal fistula, we have not had a

single rectovaginal fistula in this group. It is important in doing the operation that you dissect the rectum all the way down to the levator sling, circumferentially so that you are not getting the vagina when you come in with the pin on the EEA instrument.

With regard, finally, to the polyposis issue of Clinoril or Sulindac, to use the generic name, I have not seen any data on that. I find the concept interesting.

There are other complications with Sulindac. It induces severe pancreatitis. And in fact, the father-in-law of our Chief of Urology developed fulminant pancreatitis secondary to Sulindac. The appropriate procedure is to do a subtotal colectomy, proctectomy, and ileoanal procedure in one way or another for these patients, and then you do not have to worry about their polyposis problem.

Finally we must have a randomized prospective trial to evaluate stool control function with either leaving the residual mucosa behind or using it as a stapled *versus* the hand-sewn technique at the dentate line. And, second, there is a need for a randomized prospective trial to do the operation with and without an ileostomy. It's safe to do it with the staple technique and without an ileostomy. As you saw from our data, our complications were rare, but the one major complication we had was in a severely malnourished patient.

We had 13 patients on Prednisone, 8 for fulminant colitis, 3 of whom were severely malnourished. One of those three is the one who developed the leak. If a patient has a very low serum albumin, it is appropriate to be more conservative.