

tion due to the pancreatic disease. We have subdivided our population to define any bias created by these disparate populations, and our evaluation suggests no significant difference in outcome, although the mean follow-up is shorter. Abdominal pain, as a component of chronic pancreatitis, explains, we think, the higher rate of residual pain in this subgroup of patients.

The rate of return to normal levels of TB and AP may be prolonged. Although some reduction in abnormal levels after operation commonly was seen during the remaining hospitalization after operation, we typically required between 1 and 2 months for AP levels to return to normal values. This observation was particularly noted in the CP subgroup of patients. The CJ resolved by far the most rapidly.

Our data regarding needs for rehospitalization and length of stay must be considered rather carefully. An advantage of operative intervention that must not be minimized is the length of stay, which is considerably shorter than those noted for PTC treatment and biliary strictures, and rehospitalization and repeat imaging also are much less common in our population.<sup>3</sup>

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## Discussion

DR. KEITH D. LILLEMÖE (Baltimore, Maryland): Thank you, Dr. Thompson. I would like to thank Dr. Nealon for supplying me with the manuscript and asking me if I would discuss this fine paper. I would like to compliment him on a very excellent result, which I do believe provides a gold standard for which the other techniques must be compared.

I would like to focus on some of the points from the manuscript. First approximately half of the patients with postcholecystectomy injuries were recognized at the time of cholecystectomy, which is a higher percentage than what we have seen and what Bill Meyers has reported from Duke. I would ask him how those patients were managed when the injury was recognized and if indeed those patients had an attempted repair at that initial operation. It has been our observation that if an attempt at repair has been done and the patient develops a problem and ends up in our hands to be managed, that these tend to be higher injuries and probably more difficult to repair. Certainly if that is the case you deserve extra credit for the success rate that you report. I also notice in your report that the number of injuries at the bifurcation or higher were a rather small percentage. I would appreciate your comments about the management of these patients.

I would also like to ask what percent of your patients presented with a biliary fistula? The management of those patients is extremely difficult because you often have not only sepsis but also to control the leak and the contamination that is taking place in the peritoneal cavity and the associated inflammation. How do you time your repair in those patients?

I would like to compliment you, although this was not the purpose of the study, on your success rate in controlling pain in the difficult chronic pancreatitis patients. I was quite surprised that you observed no pain in the patients after repair of a postcholecystectomy injury. Certainly in a fair number of our patients who have had a successful repair with normal biochemical studies still do have some chronic right upper quadrant pain. Maybe in many of our patients, this may be related to ongoing litigation. But still, I would like your comments about this point.

Finally, I would like to caution you, although your mean follow-up in this group is 60 months, in the subgroup of patients with injuries following laparoscopic cholecystectomy, the follow-up is significantly less. Although I would agree that once these patients get out to be a couple of years, you can relax a little bit, I would hope that you would continue your diligent

follow-up of these patients and give us a subsequent report in a few years.

I would like to thank the Association for the opportunity to discuss the paper.

DR. THEODORE PAPPAS (Durham, North Carolina): Dr. Thompson, Ladies, and Gentlemen, I enjoyed the presentation, equally enjoyed the manuscript that I had an opportunity to review.

I would like to ask Dr. Nealon four questions.

The first has to do with the type of reconstruction that he chose. Obviously, for laparoscopic cholecystectomy injuries, Dr. Meyers and others have taught us that the Roux-en-Y reconstruction for these particularly high lesions are essential. In contrast, for patients with pancreatitis, Roux-en-Y reconstruction is not always necessary. How did you pick a Roux-en-Y for the benign chronic pancreatitis that you bypassed? There is a disquieting number of patients who re-present down the road with chronic pancreatitis with pain and other presentations that require another look with endoscopic retrograde cholangiopancreatography (ERCP) or other tests. How easy or hard is it to restudy these patients?

And, finally, with respect to that issue, how hard or easy is it to re-resect those patients in the occasional patient that comes back with pancreatitis that requires resection?

The second question I have has to do with the criteria for biliary bypass. Specifically, what criteria are you using? Obviously, ours is probably similar to yours, using a fixed stricture associated with an elevated alkaline phosphatase. The corollary is what happens when your colleagues in either vascular radiology or in ERCP decide to stent these patients? When do you try to convince them to convert those patients over to surgery? What criteria are you using on the repeated stent patients to decide on surgery?

The third question deals specifically with the three to four patients in your series who presented with recurrent cholangitis or elevated alkaline phosphatases postoperatively. In our experience, those patients tend to be the ones that have recurrent strictures and, in long-term follow-up, may require reconstruction again. Do you have any specific data on those patients in long-term follow-up?

And, finally, a comment about apples and oranges. You have a couple of very, very different groups of patients that are presented in one series. You have a series of patients with reconstructions for laparoscopic cholecystectomy injuries, some of them high lap/chole injuries. These obviously are patients who will probably have the highest recurrence rates. Those are the smallest numbers with the shortest follow-up. In contrast, your longer follow-up and your larger series of numbers are the patients with chronic pancreatitis who have the best outcome with this operation. So how would you use these data if you break it down in making specific references to our colleagues in gastroneurology and vascular radiology?

Thank you for allowing me to discuss this very nice paper.

DR. WILLIAM C. MEYERS (Durham, North Carolina): Thank you. I just wanted to mention some of our data on 40 patients that we have followed more than 3 years after laparoscopic cholecystectomy injury. Most of these patients were high injuries.

Five of those patients had to undergo early reoperation, several of which were hepatic resections because of the extent of the highness of the injury on one side. But more than 3 years after the final operation, 96%, 38 of the 40, have normal liver function tests and normal ultrasounds. The other two actually have subsequently undergone either hepatic resection or a Roux-en-Y to a seventh duct with normal liver function tests, but have only been followed for a short term after that.

Of interest is that in these 40 patients, 12 have filed lawsuits; 28 did not. Of the ones who filed lawsuits, there was one in each category who had abnormal liver function tests. And the ones who filed lawsuits, 11 of the 12 developed vague symptoms about the time of filing of the lawsuits and 27 of the 28 patients who did not file lawsuits, there were no such vague symptoms.

I want to mention these data because of three things. One is that we certainly have patients who had undergone dilatation first and virtually all the patients who have undergone attempts at dilatation have subsequently come to surgery. And I think that dilatation, as a general rule, is not the right thing to do for a laparoscopic bile duct injury, except as a temporizing maneuver.

Second is that with persistence, you can attain awfully good results without transplantation, which we are reviewing on a national basis right now of the patients who have undergone transplantation for this problem. Transplantation probably is not necessary in most patients who undergo a definitive repair unless there is an associated vascular injury.

Thirdly, these are finally some data which support the difficulty interpreting complaints, particularly when there are medical/legal considerations.

DR. WILLIAM P. LONGMIRE, JR. (Los Angeles, California): President Thompson, Dr. Copeland, Ladies, and Gentlemen. I think it would be inappropriate to let a subject like this close without a certain amount of historical aspect mentioned. Having been involved with this problem from the time when practically none of the repairs ever worked—most of them were simple dilatations—up until the time we heard this excellent report today, where practically 99% of the patients were cured, I am afraid that I can claim responsibility for some of those antiquated reports that have been mentioned in which the results were not quite as excellent as what we have heard today.

As a matter of fact, results when we started in this business were so poor that it led us to propose a since-abandoned operation, for practical purposes, that of resecting part of the left lobe and draining the intrahepatic biliary system. As a matter of fact, Dr. Mike McArthur and I presented, a good many years ago, one of the rare indications for such a procedure, a patient who had had the complete hepaticoduodenal ligament divided—artery, portal vein, and bile duct. The artery and vein had been reanastomosed as well as the duct. All of these had failed, but it left the patient with really an arteriovenous fistula in the hilus of the liver. And, needless to say, an approach through the left lobe was preferable to re-entering that area.

I would like to follow up on some of the questions that have been proposed. Most of the cases that we were seeing at the time of our earlier reports were patients who had the injury at the confluence or above—a good many of them above the confluence, so you were left with a right and left hepatic duct to try to drain. This makes a lot of difference.

I have not had any experience since the days of laparoscopic cholecystectomy, but what I have heard from my colleagues would indicate that at least a fair number of these are distal injuries. And I think that that certainly makes all the difference in the world in the outcome, as has been suggested by other discussants.

The other thing I would point out is that I am sure that the detailed technique of the anastomosis is one extreme indicator of your subsequent results. The transplant surgeons have demonstrated to us the importance of the meticulous anastomosis between the duct and the biliary system.

So I would like to ask if there is time or maybe it would require an illustrated manuscript, really, to show us some of the details of their technique, but if there is time, maybe the authors would comment on more specific details that they use in their anastomosis and also the percentage of their cases that had high injuries.

Thank you very much, Mr. President. I enjoyed this presentation.

**PRESIDENT THOMPSON:** At the first meeting of the American Surgical Association that I attended, in the late Pleistocene, actually, there was a discussion of the management of acute problems with obstruction due to Crohn's disease. And in a very prolonged discussion, Dr. Ravdin finally got up and talked about an operation he had done in the middle of the night on President Eisenhower. After he finished, Dr. Bert Dunphy, who was closing the paper, said it was certainly nice to hear from a country surgeon talking about a couple of his patients. And we have just heard from Dr. Longmire who, I think in many ways carried the ball when everybody else was about to despair. And when I was fortunate enough to attend one of the many festivals celebrating Dr. Longmire's contributions to UCLA, Sherm Melinkoff, the great gastroenterologist at UCLA who became dean, characterized Dr. Longmire's career as having been spent operating in bloody concrete.

**DR. JOAQUIN S. ALDRETE** (Birmingham, Alabama): President Thompson, Dr. Copeland. I simply wish to reemphasize the message that Dr. Nealon is giving us. And that is that surgical repair of biliary strictures has been proven over the years to be highly effective. In these days, we frequently find reports of "successful transendoscopic stent placement" with 3 months' follow-up. I fully agree with his final comment and conclusion that the results with these new transendoscopic repair methods must be compared with the results obtained by open surgical repair methods after the patients have been observed for similar long periods of time.

In our own experience at the University of Alabama, I have accumulated a group of 20 patients that I have followed now for 10 years after repair of operative injuries of their bile ducts. All of them are well. We know that properly done, biliary-enteric anastomosis functions well for many years. In 1990, we presented to this Association 71 cases of choledochoduodenostomies that had been followed for at least 5 years. And currently half of them for 10 years or more. They all remain patent. There have been only two cases of cholangitis, therefore, another documentation of the effectiveness of the surgically constructed biliary-enteric anastomosis.

Certainly, the transendoscopic procedures have contributed some improvements, specifically, in the management of the tangential injuries to the bile ducts that occur at laparoscopic cholecystectomies.

However, for operative lesions producing total occlusion or transection, transendoscopic procedures have limited if any application.

Therefore, I again, completely agree with Dr. Nealon that when discussing the success or failure of any procedure to repair biliary strictures, we must talk in terms of long-term (5–10 years) results and that the new methods should be compared with the established methods which Dr. Longmire indicated many years ago. If one looks carefully, there is a lot of data that say that the long-term results obtained by these long-established operative methods are quite good.

Thank you.

**DR. WARD O. GRIFFIN, JR.** (Lexington, Kentucky): I have a question of Dr. Nealon. Of the seven patients who continued to have pain after the operation that you did who had pancreatitis, was the repair that you did on those seven patients the same, or were there a variety of repairs? Were the repairs done so the pancreatic duct was no longer exposed to bile? In other words, would a different type of repair, perhaps, have changed the situation?

I might say, too, as a corollary to what Dr. Aldrete just said, Dr. Longmire may have been operating in concrete, bloody concrete, a long time ago. Now with the stents, we are operating in bloody concrete, metal, or plastic.

**DR. WILLIAM H. NEALON** (Closing Discussion): I would first like to thank all of the discussants for their probing questions and generous comments.

First, for Dr. Lillemoe, he asked about specifically the patients with postcholecystectomy injury. And he mentions and Dr. Pappas mentions one of the weaknesses, if we are simply focusing on that subgroup of patients, that that is admittedly the group of patients with the smaller numbers and the relatively shorter follow-up. My mean follow-up for the common bile duct injuries is 32 months.

Accepting that, and accepting that, unfortunately, there is a phenomenon of late stricture, meaning we are certainly not out of the woods, I still hope that some of these data can serve as a reasonable piece of the armament for a surgeon trying to work with those individuals who may be offering an alternative method for management of these patients.

Dr. Lillemoe asked about the question of attempted repair. It is my feeling and the feeling, I believe, from Dr. Lillemoe's suggestions that repair of these injuries be left to a surgeon with more experience with complex biliary operations. The primary operating physician, in my opinion, is just fine leaving a drain and trying to get out. Any further damage will be managed by the subsequent operation by a biliary specialist.

I have had 11 of 31 of those patients with cholecystectomy-related injuries with attempt at repair, including 2 who have had sort of a dangling Roux-en-Y limb in the right upper quadrant after the recognition that they had no idea what to do there had been made.

Dr. Lillemoe also talks about the timing, and I cannot emphasize enough my belief that a rush to operative management

of this lesion is a terrible mistake. One of the advantages we have with these alternative methods, including subhepatic drainage of the bile collection, for example, is the possibility of totally stabilizing the patient, making sure that your operative procedure is entirely elective, making sure that the patient has no residual sepsis, really is nutritionally sound, all of the things that you would like to have anytime you do an operation. And in my opinion, that is what these alternative measures have provided us.

Dr. Lillemoe talks about the pain, and Dr. Meyers mentioned the same thing. I am absolutely aware of this phenomenon. I do not know exactly how many have had a lawyer but, unfortunately, a high number of the patients with common duct injuries have lawyers.

I am making the determination for pain in this group as being the group that is still requiring narcotic analgesics for pain and really have fairly significant pain. Unfortunately, I am aware, however, of the phenomenon that Dr. Lillemoe mentioned.

Dr. Pappas discussed the type of reconstruction, and I have to admit I have a bias toward the Roux-en-Y reconstruction. I am not as happy about the choledocoduodenostomy, for example, although I think it is a perfectly acceptable technique. I do not happen to use it often.

The question of being able to re-evaluate these patients. I do actually still have continuity of the bile duct with the chronic pancreatitis patients, so I am able with endoscopic retrograde cholangiopancreatography (ERCP) to visualize the common bile duct reasonably well even after these bypasses.

It is a challenge, however, in a patient who has had a puestow type procedure pancreaticojejunostomy, that evaluating those ducts is not as easy. Either percutaneous or transpapillary access to the biliary tree should be achievable in the majority.

Our criteria for biliary bypass are exactly those that you have recommended, which is a dilated duct and a persistently elevated alkaline phosphatase.

I have suggested that this group of patients with chronic pancreatitis who have had combined pancreatic duct and bile duct decompression have a slightly lower rate of recurrence of pain compared with another group of chronic pancreatitis patients who have undergone pancreatic duct diversion alone. I am achieving a slightly higher level of pain relief if I am simultaneously doing a biliary decompression.

The question of either endoscopic or invasive radiographic instrumentation of the dilated bile duct in chronic pancreatitis

has been remarkable for the lack of long-term data. They may completely muddy the situation. Stents are temporary. And if they are not replaced at a 3-month interval, which is the normal standard, they are going to cause cholangitis. I believe they have no role in this disease.

In my institution no enthusiasm exists for this measure. We have had two such patients in this series who have come in with stents in the bile duct. All I can say is I think it is a mistake.

Dr. Pappas mentioned the disparate groups.

Dr. Meyers, thank you for your comments. And I mentioned my own personal thoughts about lawsuits and the symptoms. I also share with you your feeling that dilatation of biliary duct strictures is unsuccessful in most cases and, if anything, may serve as a way to subject the patient to repeated procedures by the interventional radiologists of a patient who finally, will be managed by the surgeons.

Dr. Longmire, I am honored, and I thank you for your comments. I hope you would focus on the fact that I mentioned that these types of strictures were primarily limited to the most skilled surgeons, and I would, obviously, include you in that list.

The question of distal *versus* proximal injury may reflect a trend unique to the laparoscopic era. We actually have had more of our patients with either common hepatic or higher or more proximal injuries.

I also find that even when the radiographic evidence of injuries may be in the common hepatic duct, my finding at operation is extension of the stricture to a more proximal level because of inflammation and fibrosis.

In other words, my likelihood of having to perform a diversion in the bifurcation or beyond the bifurcation has been quite high. We had 13 out of 31 patients with postoperative injuries after cholecystectomy and 7 of 16 patients who had a variety of nonoperative causes for their stricture with reconstructions that were in the hilum of the liver where bringing celiac and falciform plates down was necessary.

That is a real breakthrough, the ability to anatomically access those plates and bring them down into the field. The one great advantage to that is that that plane remains unperturbed.

Dr. Aldrete, I appreciate your comments, and it sounds like you have had some excellent success with your own patients. And, finally, Dr. Griffin, I thank you for your comments. With regard to the seven patients with pain, they all had the same form of reconstruction and I have interpreted those as representing recurrence of pain from chronic pancreatitis.

I thank the Association for the privilege of the floor.