Short Esophagus

Analysis of Predictors and Clinical Implications

Otávio L. Gastal, MD; Jeffrey A. Hagen, MD; Jeffrey H. Peters, MD; Guilherme M. R. Campos, MD; Majid Hashemi, MD; Jörg Theisen, MD; Cedric G. Bremner, MD; Tom R. DeMeester, MD

Hypothesis: Preoperative assessment can identify the predictors of esophageal shortening in patients with gastroesophageal reflux disease.

Design and Setting: Patient comparison study in a university-based tertiary care center.

Patients: A total of 236 patients with gastroesophageal reflux disease underwent primary antireflux procedures. Sixty-five patients were suspected of having a short esophagus and underwent a transthoracic approach. In 37 patients, a lengthening procedure was necessary to avoid tension on the repair. The remaining 28 patients were thought—after complete esophageal mobilization—to have sufficient length for a repair without needing a gastroplasty. An abdominal approach (laparoscopic Nissen fundoplication) was performed on 171 patients judged to have normal esophageal length.

Main Outcome Measures: Univariate and multivariate analyses of preoperative variables were performed to identify predictors of a short esophagus.

Results: On univariate analysis, manometric esophageal length below the fifth percentile of normal was associated with esophageal shortening. On multivariate analysis, only the presence of an esophageal stricture predicted the need for a Collis gastroplasty (odds ratio, 7.5). The presence of Barrett’s esophagus of 3 cm or greater identified patients in whom the transthoracic esophageal mobilization alone was sufficient (odds ratio, 3.4).

Conclusions: The presence of a stricture was associated with esophageal shortening sufficient to require a gastroplasty. Transthoracic esophageal mobilization alone was usually sufficient to perform a safe repair without tension in patients with a Barrett’s esophagus of 3 cm or greater.


The increasing popularity of laparoscopic antireflux repairs with associated shorter recovery time and greater patient acceptability has increased the number of patients referred for surgical therapy. The 2 most common causes of technical failure in these patients are the low placement of the fundoplication and herniation of the fundoplication into the chest. Such complications are likely to occur when the esophagus is short or when mobilization is inadequate.

Esophageal shortening occurs most commonly in patients with advanced reflux disease due to acid-related inflammatory changes in the muscular wall of the esophagus. On healing, this results in contracture. Although severe degrees of esophageal shortening are obvious on a contrast esophagogram, subtler degrees of acquired shortening are difficult to detect. As a result, acquired esophageal shortening remains a potential cause of treatment failure with the laparoscopic approach. The purpose of this study was to identify clinical markers of a short esophagus in patients with gastroesophageal reflux disease (GERD) to avoid failure of laparoscopic antireflux surgery.

Results: Of the 236 patients with GERD and adequate esophageal peristalsis who underwent fundoplication, 37 (15.7%) had objective evidence of esophageal shortening. In these patients, esophageal lengthening was required, despite complete esophageal mobilization at thoracotomy, to reduce the gastroesophageal junction without tension. In an additional 28 patients (11.9%), extensive esophageal mobilization by the transthoracic approach was sufficient. The remaining 171 patients were judged clinically to have an esophagus of normal length. All of these...
patients were found to have adequate esophageal length at the time of operation and had a laparoscopic Nissen fundoplication performed. Patient characteristics in the 3 treatment groups are shown in Table 1.

Average manometric esophageal length for the 3 treatment groups and in normal subjects is shown in Figure 2. All reflux patients had a shorter esophagus on average than normal subjects. Furthermore, patients who underwent a transthoracic procedure had a shorter mean esophageal length than the laparoscopic Nissen group and normal subjects. However, individual variability in the treatment groups did not allow the identi-
fication of a threshold value below which a short esophagus can be reliably determined to exist.

The prevalence of hiatal hernia, Barrett’s esophagus, and strictures in the treatment groups is shown in **Table 2**. On multivariate analysis of patients who had a transthoracic procedure, the presence of an esophageal stricture identified those in whom a gastroplasty was likely to be required after esophageal mobilization (odds ratio, 7.5). Transthoracic esophageal mobilization alone appeared to be sufficient for patients with a long segment (≥3 cm) of Barrett’s esophagus (odds ratio, 3.4) (**Table 3**). The combined presence of a hiatal hernia of 5 cm or greater and an esophageal stricture was strongly associated with the need for a gastroplasty (**P** < .001). This was not so for the combination of a 5-cm or longer hiatal hernia and a 3-cm or longer Barrett’s esophagus.

An excellent or good outcome was achieved in 89% of patients in the gastroplasty group, 93% in the esophageal mobilization group, and 91% in the laparoscopic group. There were no significant differences in outcome between treatment groups.

In patients with fair or poor outcome, objective documentation of the cause of failure was identified in 9 of 15 in the laparoscopic Nissen group and in all 8 patients in the transthoracic groups. Of the 9 patients who underwent laparoscopic Nissen fundoplication with a fair or poor outcome who also underwent diagnostic studies, 6 had technical failure of the fundoplication (wrap disruption with or without herniation). In contrast, there were no technical failures in the 8 patients with unsatisfactory outcome in the transthoracic groups. Rather, 5 had persistent difficulty with stricture, 2 had excessive bloating and diarrhea, and 1 had postthoracotomy pain. The likelihood of technical failure as the cause of an unsatisfactory outcome was significantly different (6 of 9 vs 0 of 8; **P** = .01).

**Acquired esophageal shortening**, when unrecognized, remains a threat to the success of transabdominal or laparoscopic antireflux procedures. Although severe esophageal shortening may be easily detected on a contrast esophagram, the diagnosis of milder degrees of esophageal shortening remains subjective. In an attempt to define objective criteria for the preoperative identification of the short esophagus, we used the need for a gastroplasty for esophageal lengthening as the defining criteria for the existence of a short esophagus. The results of our study suggest that a combination of preoperative findings can be used to identify patients at risk for the presence of esophageal shortening in whom a transthoracic procedure is advisable.

Overall, 28% of our study population had evidence suggesting esophageal shortening. In 16% of patients, there was unequivocal esophageal shortening, despite complete esophageal mobilization at thoracotomy; in 12%, tension-free reduction of the hiatal hernia could be accomplished after thorough transthoracic esophageal mobilization. A similar high prevalence of esophageal shortening was reported by Swanstrom et al11 in patients with GERD who underwent laparoscopic antireflux surgery. Since a short esophagus may be the most important factor resulting in the 2 most common causes of failure,13 preoperative recognition of a short esophagus is critical to the successful outcome of laparoscopic fundoplication.

Pearson et al8 first suggested that patients with esophageal strictures may also have longitudinal shortening of
Esophageal shortening is a common complication of GERD, and a careful preoperative investigation with esophagogastroduodenoscopy, endoscopy, and manometry is essential for diagnosis. Although manometric measurements of esophageal length below the fifth percentile of normal should raise suspicion of esophageal shortening, they lack sufficient specificity to determine whether a lengthening procedure is required.

The presence of an esophageal stricture, either alone or in combination with a hiatal hernia of 5 cm or more, is a strong predictor of the presence of esophageal shortening. In these patients, the need for a transthoracic approach should be emphasized. Patients with a large hiatal hernia and long segments of Barrett’s esophagus should also be managed with thoracotomy to allow complete mobilization of the esophagus and an evaluation of the esophageal length to achieve the goal of a tension-free antireflux repair in patients with advanced disease.

Presented at the 106th Scientific Session of the Western Surgical Association, Indianapolis, Ind, November 17, 1998.

Corresponding author: Jeffrey A. Hagen, MD, Department of Surgery, University of Southern California School of Medicine, 1441 Eastlake Ave, Suite 7418, Los Angeles, CA 90033-4612 (e-mail: hagen@hsc.usc.edu).

REFERENCES


Richard C. Thirlby, MD, Seattle, Wash: This presentation of a manuscript typifies the strength of previous work from the USC group. This study looks at surgery for GERD as a complex disease entity that requires flexibility in the evaluation of surgical treatment. Most operations that we perform are anatomic. We relieve obstructions, close holes, or remove diseased organs.

Surgery for GERD is unique in that it requires repair of altered physiology. Unfortunately, too many operations for GERD are being performed by surgeons with little or no formal training in the physiologic management of GERD, with only one surgical approach to the whole patient population: the laparoscopic Nissen. Perhaps in no other current area of general surgery could our specialty be subject to the valid criticism of the adage, “If all you have is a hammer, everything looks like a nail.” The USC group exemplifies the ideal. Each patient is studied carefully and surgical treatment is individualized. For that reason, they are to be congratulated.

However, at this point, my agreement with the work presented today ceases. I must confess, I reviewed the abstract with Luke Hill and Don Low at the Mason Clinic last week. Since I am a politically correct kind of guy, I cannot repeat their assessment of this study. I can paraphrase their thoughts, however. In a nutshell, we disagree with most of the conclusions presented today. First and foremost, we believe the term short esophagus is misleading. Sliding hiatal hernias no more shorten the esophagus than sliding groin hernias shorten the colon. The stomach migrates into the chest, and the esophagus contracts. Reduction of the hernia will restore esophageal length in all cases. The combined experience at the Mason Clinic by Luke Hill and Don Low now exceeds 3500 operations for GERD, with no cases of short esophagus requiring Collis gastroplasty. In all cases, the stomach was reduced and the esophagus anchored in the abdomen. Good-to-excellent results are achieved in 80% to 90% of patients at 15 to 20-year follow-up. Clearly, the contention that a 5 cm hiatal hernia implies a short esophagus is simply not true.

What about strictures? In patients with GERD and esophageal strictures, primary Hill repairs result in good-to-excellent results in about 90% of cases, with patients requiring only 1 or 2 perioperative dilatations. Patients requiring multiple dilatations during the years prior to presentation to the surgeon do poorly, yet only about half report good results in long-term follow-up. All strictures therefore are not alike. Most patients with GERD and strictures can be handled with standard operative procedures combined with perioperative dilatation. On the other hand, the patient with strictures who has been dilated every month for years will not do well with any simple operation, whether it be the Hill, the Nissen, or the Collis gastroplasty.

My first question for the authors relates to the management of patients with strictures. What is your recommendation for the treatment of patients with strictures who have been dilated innumerable times? Impaired motility in this segment will likely result in poor results after any procedure. We believe that many of these patients are best served with up-front resections.

What are your criteria for resection and reconstruction? Similarly, what are your recommendations for open abdominal procedures? Getting back to my original suggestion that not all patients are best treated with a laparoscopic Nissen procedure, we believe that some patients with large hernias are best treated with open, abdominal procedures facilitating anchoring of the abdomen.

And what about the Collis gastroplasty? I find it puzzling that Dr DeMeester’s group would, on the one hand, operate on patients and publish papers devoted to the elimination of the Barrett’s esophagus and then, on the other hand, tell us today to surgically create an iatrogenic Barrett’s esophagus. The Collis gastroplasty creates an aperistaltic neoesophagus lined with acid-secreting parietal cell mucosa. Over one third of these patients will have esophagitis at follow-up, and nearly one half will have an aperistaltic neoesophagus. In both theory and practice, this is a bad operation.

With regard to the other conclusions presented today, I have a few problems. The abstract states that on multivariate analysis, the presence of a hiatal hernia greater than 5 cm remains strongly associated with transthoracic approach. I thought they chose the transthoracic approach when there was a long hiatal hernia. In other words, the conclusions were predetermined. With respect to the conclusion that when a stricture is present, a lengthening procedure is likely to be required, I have an alternate conclusion. Thirty-seven of 65 patients operated on via the transthoracic approach required a Collis gastroplasty, whereas none of 171 patients undergoing the abdominal approach required a Collis gastroplasty. I believe the most accurate conclusion one can reach from these data is that the transthoracic approach limits the ability to mobilize and adequately anchor the esophagus in the abdomen and therefore, increases the chance of needing to perform the Collis procedure. In other words, I believe the correct take-home message from the data presented today is, “Don’t operate through the chest if the patient has a large hiatal hernia.”

My final question relates to perhaps the most notable deficiency in the presentation: the lack of good outcomes data. What is the duration of follow-up? The bottom line should always be, “how are the patients doing?” Can the authors tell us more about the patients who had the iatrogenic Barrett’s or Collis procedure? Is there any endoscopic or manometric follow-up in these patients? Have there been any problems with esophageal ulcers, poor motility, etc?

In closing, Luke Hill and Don Low asked me to repeat to all of you their standing offer: If any of you have a patient with an apparent short esophagus that you anticipate being unable to reduce, send the patient to Seattle, and they will fix it for free of charge.

Ronald A. Hinder, MD, Jacksonville, Fla: I also believe that the short esophagus is extremely rare, and speak for many surgeons with a large experience in this disease. I would estimate that a truly short esophagus exists in less than 1% of patients who are operated on for GERD. I also wish to ask the authors why they subject 20% or 30% of their patients to a thoracotomy when a laparoscopy is so simple. A very small proportion of patients will fail due to a short esophagus, and in those patients, more extensive surgery can take place as a redo. Why should we subject 12% of patients to an unnecessary thoracotomy?

Philip E. Donahue, MD, Chicago, Ill: Ten percent of the patients with either operation fail, laparoscopic or the transthoracic. I wonder if you could say a word about patterns of failure. Was reflux a problem, or are you talking about other problems such as mechanical slippage of the wrap into the chest?

Jay L. Grosfeld, MD, Indianapolis, Ind: Were any of these patients reoperative candidates, and would you ever perform a Collis gastroplasty on someone who has had previous antireflux surgery?

Dr Peters: I also thank Dr Hinder and the other discussants, particularly Dr Thirlby for his kind comments. I am grateful for the fact that Dr Hill and perhaps the remainder of the Mason Clinic group are not here as well.

The discussants certainly highlight the controversy surrounding the short esophagus, which is why we felt the need and the importance of putting together our paper. As you heard, the very existence of the short esophagus is controversial, let alone how to identify it once it exists. We believe firmly that it does exist. Dr Gastal’s lead slide pointed out that the major reasons for failure of antireflux procedures were reherniation and/or slippage of the wrap. Both are a likely consequence of the short
esophagus. I note that Dr Hinder has a paper tomorrow with similar findings.

Dr Thirlby questions the problem of stricture management. Patients with strictures are indeed a very difficult population, particularly those, as you pointed out, who require repeated dilatations. Primary resection is a consideration in these patients, although the need is relatively rare. Generally, an attempt at an antireflux repair of some sort is warranted unless the esophagus is completely failed. We’ve performed 7 or 8 primary resections in some 10 years of practice in our group for reflux disease. This is relatively new, but it does exist.

You asked whether there is an indication for an open transabdominal approach. I don’t think so these days, with the exception of patients who may have extensive previous abdominal surgery or perhaps, with Dr Hinder’s paper in mind tomorrow, the population of redo patients. Most abdominal procedures can now be performed laparoscopically, and I don’t think there is a great advantage to reducing the hernia or lengthening the esophagus by performing the procedure open abdominally rather than laparoscopically.

Is a Collis gastroplasty an iatrogenic Barrett’s esophagus? That’s not entirely correct. Remember, Barrett’s esophagus is colonic tissue, not gastric tissue by the way we define it today. Its major risk is the development of adenocarcinoma, and it is non-acid—secreting tissue. I think that is stretching the discussion a bit unrealistically.

All of these were primary procedures. At last year’s Western Surgical, we presented our series of primary Collis gastroplasties with an 85% to 90% success rate, and I refer you to that paper for the results of our primary gastroplasty group.

Have we used circular reasoning with regard to how we selected our patients? No. As you may have noticed in the presentation, variables such as criteria, a large and/or a fixed hiatal hernia which we used as criteria for procedure selection, were removed from analysis. The study variables that were significant, that is, Barrett’s esophagus greater than 3 cm or the presence or absence of a stricture, were not criteria for procedure selection.

Does the transthoracic approach limit mobilization? Of course not. It is clear that you can mobilize the esophagus more transthoracically than you can transabdominally without injury to the vagus. This fact is self-evident to those performing both transabdominal and transthoracic procedures.

Mean follow-up time was 36 months, and was slightly longer for the open group than for the laparoscopic group.

Dr Hinder brought up the point that shortening of the esophagus and esophageal mobilization should be rare, less than 1% of the patients. This gets back to the controversy of whether the short esophagus even exists. Again, we feel that it does. I point out, Dr Hinder, that Lee Swanstrom has found a 16% incidence of esophageal shortening defined by the need for a Collis gastroplasty in his laparoscopic group. It seems others are also seeing this.

I don’t think that we should take the approach of doing laparoscopic procedures on everyone and falling back on a second operation on those who fail. The long-term results of second antireflux procedures are clearly not as good.

Dr Donahue points out that 10% of these patients fail, no matter what we do. It is true that there is an incidence of failure with anti-reflux surgery. It is around 10% if you do it well. Most of this tailored concept comes out of the fact that, in the early days, the incidence of failure in patients with severe disease, ie, those with strictures and large hernias, was in the range of 50%. We think we are doing better than that now.

Dr Grosfeld, none of these were reoperative procedures, but we do do Collis procedures for the reoperative population.