

Heller Myotomy Versus Heller Myotomy With Dor Fundoplication for Achalasia

To the Editor:

The article by Richards et al¹ is a very interesting and well-conducted randomized, double-blind study concerning the results of laparoscopic treatment of esophageal achalasia. While comparing Heller myotomy plus Dor fundoplication versus Heller myotomy alone, the authors concluded that the former technique is superior in regard to control the incidence of episodes of postoperative gastroesophageal reflux evaluated with 24-hour pH monitoring study. Richards et al analyze one of the major problem concerning the surgical treatment of esophageal achalasia, that is, the impact of gastroesophageal reflux that follows Heller myotomy, while the authors are not concerned about recurrent achalasia, which, in our opinion, is the real problem after treatment.

Nonsurgical treatment of esophageal achalasia essentially performed by gastroenterologists (botulinum toxin injection or balloon dilatation) is followed by good to excellent symptomatic relief in 46% to 93% of patients in the short period,²⁻⁴ alleviating dysphagia but leaving the lower esophageal sphincter incompetent, prone to gastroesophageal reflux to be controlled medically. The question could be: is there the necessity to prevent gastroesophageal reflux or is it sufficient to treat it medically if needed? In a series of 300 patients treated with pneumatic dilatation, Khan et al reported the incidence of 6.6% of cases requiring medical therapy to control the reflux.⁵

Laparoscopic Heller myotomy allows the resolution of the dysphagia with minimal invasion, becoming the treatment of choice among surgeons and even gastroenterologists. The addition of an antireflux procedure to the esophageal myotomy is one of the major aspects of discussion between proponents of the anterior 90° (Dor) and the posterior 270° (Toupet) wrap, while there are some arguments against the routine use of an antireflux procedure.⁶

Rechalasia has been reported in many studies, ranging between 3.4% and 30%.⁷⁻¹⁰ Oelschlager et al compared laparoscopic Heller myotomy with Toupet fundoplication versus laparoscopic Heller myotomy with Dor fundoplication, reporting reachalasia 17.3% incidence after Dor fundoplication versus 3.4% after Toupet operation ($P = 0.001$).⁸ There are several situations that could explain postoperative reachalasia. The length of the myotomy has been debated widely, generally accepting long extended myotomy to be necessary.¹¹ Moreover, the covering of the myotomy site and the exposed mucosa with the Dor fundoplication could lead to adhesion formation between the two surfaces that could favor recurrent achalasia.⁹ While the Toupet fundoplication keeps the myotomy hedges apart, fixating the fundus to each side reducing the risk of fibrosis,⁸ Toupet fundoplication also requires the freeing of the posterior esophageal attachments and the section of some of the short gastric vessels, which could lead to a less competent gastroesophageal junction with consequent postoperative reflux. Additionally, the Toupet procedure is supposed to increase the likelihood of postoperative dysphagia due to angulation of the posterior esophagus.¹² On reviewing several studies of laparoscopic Heller-Dor treatment of achalasia, 9% of patients complained of recurrent dysphagia.⁶

Treatment of recurrent dysphagia has been reported in 5 cases by Duffy et al, describing a relaparoscopic approach.¹² The authors, after having dismantled the previous fundoplication, extended the myotomy only if the possibility of esophageal perforation is eliminated, thus obtaining complete relief of dysphagia in 2 of 5 cases.¹² Other authors^{13,14} recommended pneumatic dilatation in case of incomplete myotomy, with differences in success rates ranging from 80% to 33%. Miller et al advocated remyotomy in cases of inadequate esophagomyotomy and nondilated esophagus, proceeding with resection in selected cases.¹⁵ Esophagectomy carries a 3% to 5% mortality rate in the hands of even the most experienced surgeons, with substantial morbidity attributed to respiratory failure, anastomotic leak, and me-

diastinal bleeding.¹⁶ In 22 cases of recurrence of dysphagia after cardiomyotomy for Chagas' disease, Ponciano et al reported 20% of mild dysphagia after Serra-Dòria procedure (cardioplasty and Roux-en-Y partial gastrectomy), with 10% fistula of the gastroesophageal anastomosis.¹⁷

In a paper recently published, we compared a series of laparoscopic Heller-Dor versus a series of laparoscopic Heller plus His angle reconstruction, which is actually our preferred choice of operation. In the aim of reducing the incidence of this complication, we decided not to perform any kind of fundoplication to avoid either adhesions or scarring between the row mucosal surface of the esophagus and the overflap gastric wall, or the excessive freeing of the posterior side of the esophagus, thus preserving the posterior elements of fixation of the gastroesophageal junction.

An antireflux mechanism is performed suturing the left side of the gastric fundus to the right side of the esophageal myotomy, stressing the Hill's angle flap valve. In our experience, we do not record recurrent achalasia.¹⁸

The primary goal of treatment of achalasia should be relief of symptoms and improved esophageal emptying. All the current treatment options for achalasia are limited to reducing the pressure gradient across the lower esophageal sphincter, thus facilitating gravity-aided esophageal emptying, while no treatment can restore the muscular activity to the denervated esophagus in achalasia because esophageal aperistalsis and impaired lower esophageal sphincter relaxation are rarely, if ever, reversed by any mode of therapy.⁵

Rechalasia is, in our opinion, the worse long-term complication after Heller myotomy. On the other hand, gastroesophageal reflux that is present at 24-hour pH studies after Heller myotomy, as reported by Richards et al,¹ is not the cause of severe esophagitis or stricture¹⁹ and is usually controlled with medical therapy.⁵

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Reply:

I thank Dr. d'Angelo and his colleagues at the University of Rome “La Sapienza” for their kind comments and questions regarding the Vanderbilt randomized trial of Heller myotomy versus Heller myotomy with Dor fundoplication for achalasia. Several controversies are raised in the letter:

- Does the extended long myotomy proposed by Oelschlager et al¹ improve long-term outcome with a lower dysphagia or recurrent achalasia rate?
- Does the Toupet fundoplication prevent recurrent achalasia by holding the myotomy open while the Dor sets up adhesions?
- Why worry about esophageal acid exposure after Heller myotomy?

Acid exposure after Heller myotomy for achalasia can be very difficult to diagnose because of the inability of the patients with achalasia to differentiate reflux from stasis, noted by many authors including our group.^{2–4} We and others have noted an across-the-board improvement in reflux symptoms after Heller myotomy for achalasia, which makes it difficult for the clinician to determine whether the underlying cause of recurrent or continued GERD symptoms is related to acid reflux disease or to inadequate clearance, which leads most authors to recommend 24-hour pH studies to objectively assess the condition. Most authors believe strongly that long-term acid exposure in patients with achalasia is detrimental and should be avoided with the partial fundoplication.⁵

The study of extended versus standard myotomy also separated the patients into Dor and Toupet fundoplications and was not a randomized study.¹ While the results of their study certainly bear further investigation, it must be recognized that their study has serious limitations that

limit the conclusions that can be drawn. Follow-up for the Toupet extended myotomy was shorter than the follow-up for the standard myotomy-Dor fundoplication, patients were not randomized, and the continual improvement of operative techniques may have improved the results in the last group of patients operated on (the Toupet-extended myotomy group). Patti et al⁶ noted an improvement in outcome after Heller myotomy for achalasia during the last 6 years of practice compared with the previous 6 years that they attributed to the learning curve and more patients referred for surgery without prior treatment or at an earlier state of disease. Similarly, the improvement seen by Oelschlager et al¹ could be partially due to the same effect because the Heller-Dor operations were performed between 1994 and 1998 while the Toupet-Heller operations were performed between 1998 and 2001.

Our objective randomized clinical trial did measure improvement in dysphagia scores and lower esophageal sphincter pressures and found no difference between the two groups either at randomization or postoperatively.⁷ A limitation of our study is the lack of long-term follow-up, which was not the endpoint of the study but will be as we are asking these patients to return for follow-up studies to evaluate long-term outcomes.

While the Rome group thinks that Toupet will be a better operation for long-term achalasia relief, it is important to note that Hunter et al⁸ in their discussion of their paper detailing their experience with Toupet-Heller for achalasia noted that the posterior Toupet fundoplication may obstruct the large sigmoid esophagus and that Dor fundoplication is a better choice for some patients.

Further randomized trials to evaluate the recurrent achalasia rate with the Washington extended myotomy versus standard myotomy are needed to really evaluate the need for the longer myotomy. Perhaps long-term follow-up of our randomized patient population will give some answers to recurrent achalasia rate in patients undergoing Heller with Dor fundoplication. A clinical trial to show a difference between esophageal acid exposure between Heller-Toupet versus Heller-Dor would take hundreds of patients and likely not to be

clinically significant because of the low risk of pathologic reflux encountered after Heller-Dor.

An interesting modification of the Dor fundoplication was suggested as a better alternative to the technique used by our group. At least one advantage of the standard Dor placed over the myotomy is the security of the fundic serosa over the mucosa, especially in cases where the mucosa was breached. I have no experience in the modification suggested by Professor Ramacciato and will await further reports of their experience with this procedure. Until further data are collected, we still believe that our randomized clinical trial gives the best evidence that Heller-Dor fundoplication gives the same relief of dysphagia with a much lower rate and severity of pathologic esophageal acid exposure than does Heller myotomy alone and is our procedure of choice for patients with achalasia.

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Does Resident Post Graduate Year Influence the Outcomes of Inguinal Hernia Repair?

To the Editor:

A well-planned research study usually produces many valid questions; accordingly, the work of Dr. Wilkiemeyer et al¹ is to be commended as it documents two significant problems in the performance of inguinal herniorrhaphy by surgical residents: 1) post graduate year (PGY) 1 and PGY2 produced a disturbingly high recurrence rate; and 2) the rate took place even when assisted by board-certified general surgeons, raising questions about both present surgical resident training and attending supervision. In this study, PGY1 and PGY2 were responsible for a 3-year recurrence rate of 7% and 5.2%, respectively. Extrapolated to our national number of yearly herniorrhaphies, which is about 800,000, that performance would yield approximately 56,000 and 41,600 yearly recurrences, respectively, an unacceptably high failure rate.

Inguinal herniorrhaphy is an ideal operation to teach because the regional anatomy has been well described and the repair techniques are well outlined and reproducible. However, it requires a solid knowledge of the necessary steps to first dissect the structures and then complete the repair.

The reality is that the initiate resident comes to the OR with virtually no surgical manual skills and during the first months of training and during those critical first procedures he or she has to learn the basic gestures of incising, dissecting, hemostatic clamping, and suturing tissues. Throughout those months, the resident has to concentrate on learning his handicraft and has virtually no opportunity to master the required repair steps; hence, the high recurrence rate and the lengthy operations. The failure rate declines as their advanced training validates this thesis. This probably happened when the resident's manual skills become automatic and the resident was able to concentrate on the repair itself.

Intensive, supervisor-rated practicing in an animal or dummy setting, the basic surgical handicraft motions of incising, dissecting, clamping, tying, and suturing may permit PGY1 to approach a patient in need of a herniorrhaphy without having to learn those basic skills during the procedure, allowing them to concentrate on the mechanics of the repair. This approach may accelerate resident training and reduce many tense and frustrating OR occurrences.

The fact that direct attendance by board-certified general surgeons did not prevent the initial substandard performance opens another set of questions about the benefit of such supervision if the trainee's manual skills are yet to be developed.

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Reply:

We agree fully with Dr. Deysine's interpretation of our findings. Our data confirm that surgical residencies, at least as they existed during the operative phase of our study (1999–2001), do result in progressive skill acquisition. As residency education undergoes its current metamorphosis, surgical educators should consider the implementation of structured basic skills training in a setting outside of the human operating room. This type of training should also be provided to surgeons learning new skills after completion of their residency program. In addition, "masters classes," akin to those available to musicians, should be incorporated into the programs for maintenance of certification. These interactions with "masters" (highly experienced surgeons) could serve to teach certified surgeons details and nuances of particular procedures.

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Hepatic Resection-Related Hypophosphatemia Is of Renal Origin as Manifested by Isolated Hyperphosphaturia

To the Editor:

The article by Salem and Tray¹ relating early posthepatectomy hypophosphatemia to transient postoperative hyperphosphaturia reports on a very interesting and stimulating study, which strongly suggests the need for additional research on the topic.

We think that an important point to address in greater detail is the relationship between the occurrence of these early posthepatectomy events (hypophosphatemia and hyperphosphaturia) and the subsequent manifestation of complications. This was described by George and Shiu several years ago,² later independently, to varying degrees, by other authors including some of us,³⁻⁵ and questioned by others.⁶ More recently, we have become used to seeing this association reconfirmed in clinical practice, in a volume of 40 to 50 hepatectomies per year. Impaired renal handling of phosphate in acute phase response has been addressed repeatedly in the last 2 decades, and Salem and Tray have demonstrated elegantly that it is an important event after hepatectomy.¹

Hypophosphatemia, explained in full or in part by this event, is known to impair the immune response and other functions but receives inconsistent consideration in the management of many postoperative patients, and the relationship with unfavorable outcomes still remains in several cases speculative or unknown. Furthermore, it is not yet totally clear to what extent phosphorus replenishment may be protective. A key point would be to understand more concretely the underlying mechanism(s) binding all of this together (ie, a common cause for a more severe early hyperphosphaturia and hypophosphatemia and the greater likelihood of subsequent complications), the exceptions (some patients exhibit severe hypophosphatemia within totally uneventful clinical courses), and,

obviously, the practical implications for patient care.

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Reply:

The authors thank Dr. Giovannini for this kind comments and thoughtful insights into the phenomenon of posthepatectomy hypophosphatemia. We, too, think that further investigation is required to evaluate the mechanism of this response and perhaps elucidate the putative factor(s) implicated. In general, the role and activities of phosphatonins post surgery have received little attention; this led to our interest in evaluating FGF 23, and although not implicated, other phosphatonins may be responsible. We have also seen profound hypophosphatemia with an uneventful clinical course and have been unable to relate the severity of hypophosphatemia with any clinical scenario. The study model outlined is potentially useful for further

evaluation of this phenomenon, particularly if transportable to an animal model.

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Osteomyelitis of the Foot and Toe in Adults Is a Surgical Disease

To the Editor:

Henke et al described the epidemiology of adult osteomyelitis and evaluated whether early surgical therapy, compared with antibiotics alone, produces an improved probability of wound healing and limb salvage.¹ They used the Nationwide Inpatient Sample and a single institution review from 1993 to 2000, using ICD-9CM codes for lower extremity foot and digit osteomyelitis.

One of their major conclusions is that preadmission antibiotic therapy, in contrast to aggressive surgical debridement/digit amputation and selected use of arterial bypass, is related to decreased wound healing. However, their results do not support this conclusion. The authors report that there are increased odds of decreased wound healing associated with preadmission antibiotic use (odds ratio, 0.2; 95% confidence interval, 0.05-1.1, $P = 0.07$). This odds ratio actually indicates that there is no association between decreased wound healing and preadmission antibiotic use. The authors' odds ratio contains a 1 in the 95% confidence interval, and whenever an odds ratio contains a 1 in the 95% confidence interval, this means that there is no association between the two variables. In addition, the authors used a P value of 0.07 to test the statistical significance of this odds ratio, instead of a P value of 0.05, which is the preferred standard of statistical significance.

The authors' other results, however, do show that there are increased odds of greater limb loss related to preadmission antibiotic use (odds ratio, 0.34; 95% confidence interval, 0.15-0.77, $P = 0.009$). These mixed results indicate that the increased odds of greater

limb loss associated with preadmission antibiotic use may not be a consequence of decreased wound healing. There may be other factors that influence the association between preadmission antibiotic use and greater limb loss among patients with adult osteomyelitis. More research is needed in this area.

Nevertheless, Salvana et al² recommend an integrated team approach to the management of chronic osteomyelitis. They recommend that a surgical approach be combined with appropriate antibiotics based on the sensitivity of the microbes isolated from the infected site. In this way, the infected focus is eliminated and bone length and integrity are restored. Guelinckx and Sinsel³ recommend a short course of antibiotics as part of a one-stage procedure for the management of chronic osteomyelitis.

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Reply:

We appreciate the letter concerning our article. Dr. Morewitz contends that we have overstated our conclusion regarding the association of preadmission antibiotic therapy with delayed wound healing, as our confidence interval included 1.0 and the *P* value was 0.07. We listed those data in the table specifically because we thought that it represented a trend, but we certainly did not state that it was statistically significant. Indeed, on page 888, this was mentioned as a strong association and not independently associated with that outcome. Also, we think that Dr. Morewitz may have misinter-

preted the table as the odds ratio for preadmission antibiotic therapy was associated with less chance of wound healing (odds ratio = 0.23) as was PVOD and IDDM (odds ratio, <1) whereas bypass was associated with an increased likelihood of wound healing (odds ratio, 5.0).

We agree that decreased wound healing per se may not increase limb loss, and this factor was not specifically evaluated by multivariate analysis. However, we do think the data support that delay in revascularization for patients with combined ischemic disease, and osteomyelitis confers an increased risk of limb loss. More importantly, we agree with Dr. Morewitz that an integrated team management is the best approach to managing this disease, and that certainly includes early surgical consultation for appropriate osteotomy, toe amputation, revascularization, or nonsurgical therapy based on the clinical presentation.

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