# A personal perspective on controversies in the surgical management of oesophageal cancer

**J McK Manson, WD Beasley**

Abertawe Bro Morgannwg University Health Board, UK

**ABSTRACT**
Significant disagreement and debate persist regarding several aspects of the optimal surgical management of oesophageal cancer. We address some of these issues based on our consecutive series of 165 patients undergoing oesophageal resection (reported in full elsewhere) and the available literature. The areas considered are controversial but we argue in favour of a 'traditional' two-stage open approach (Ivor–Lewis), leaving the pylorus alone, making no attempt to perform a radical lymphadenectomy and fashioning a hand sewn anastomosis.

**CORRESPONDENCE TO**
**James Manson**, E: james.manson@wales.nhs.uk

The aim of this paper is to outline and justify the approach we favour for oesophageal resection in malignant disease. In order to accomplish this aim, we draw on our own experience[1] and review of the relevant literature.

## Surgical approach

Perhaps because the oesophagus is present in the neck, the thorax and the abdomen, multiple surgical approaches (and combination of approaches) to oesophageal resection are described. Inevitably, there is controversy over the optimal approach. We favour a laparotomy and right lateral thoracotomy. This is preferred to other approaches such as a three-stage, transhiatal or minimally invasive oesophagectomy (MIO).

In the three-stage (McKeown) approach, gastro-oesophagectomy is followed by a further cervical incision and an anastomosis fashioned in the neck. We are unable to identify any compelling argument to support this approach. It is claimed that anastomosis is technically easier in the neck than in the chest. In south Wales, the majority of patients with oesophageal cancer are overweight with a barrel chest and a short neck. In our experience, no matter how high in the chest it located, an intrathoracic anastomosis is unarguably technically easier than in the neck.

It is also argued that anastomotic leakage following cervical anastomosis is less dangerous than in the chest. Not all oesophageal surgeons accept this. There is evidence that a cervical anastomosis often comes to lie in the upper part of the thoracic cavity.[2] Since our rate of anastomotic leakage is, fortunately, very low (overall leak rate 1.2%, clinical leak rate 0.6%), this is not an issue in our practice.

A further potential argument in favour of the three-stage approach is a greater proximal resection margin; the advantage has been estimated at 1cm.[2] No patient in our series had an involved proximal margin, which suggests that adequate clearance can be obtained with an intrathoracic approach.

The majority of the available evidence suggests that the rate of benign anastomotic stricture is significantly lower in intrathoracic than in cervical anastomoses.[3–5] (See 'Anastomotic technique' section below.) We can identify no advantages to a three-stage procedure. In our series of 165 patients, access to the neck was needed in only two patients who developed significant complications.

It is claimed that a transhiatal approach to oesophageal resection is associated with lower rates of morbidity in general (and respiratory complications in particular) than a transthoracic procedure[6–8] although the available evidence has always failed to support this contention. Three trials and one meta-analysis have shown that the complication rate, including respiratory complications, was no lower in transhiatal than in transthoracic oesophagectomy and, in some cases, somewhat higher.[9–12] There was no significant difference in long-term survival between the two approaches. Dutch investigators compared transhiatal oesophagectomy with a radical transthoracic approach.[15] These workers found no significant difference in operative

mortality or long-term survival but the transthoracic approach produced more morbidity. However, the incidence of some complications in the transthoracic group (anastomotic leak 16%, vocal cord paralysis 21%, chyle leak 10%) was unusually high and these findings may be open to question.

The available evidence suggests, overall, that the incidence of complications such as chylothorax and recurrent laryngeal nerve injury are greater after transhiatal than after transthoracic resection.[11,14] Our concern with transhiatal oesophagectomy is not oncological but a lack of surgical precion and accuracy.

Today, most interest revolves around the comparison of traditional open oesophagectomy and MIO. Although MIO has been practised for at least 15 years, no convincing or high quality evidence exists that it is associated with a significant advantage in any measurable endpoint including morbidity, mortality and length of stay.[15–17] The *National Oesophago-Gastric Cancer Audit 2010*, which represents the most significant and relevant data for England and Wales, has reported that MIO has no advantage over open oesophagectomy, the only significant difference being a higher rate of anastomotic leakage in MIO.[18] In addition, it seems clear that MIO is associated with complications very rarely seen in open oesophagectomy, including gastric conduit necrosis.[19] Guidelines published by the National Institute for Health and Clinical Excellence and by professional associations in the UK concluded that there was no convincing evidence that MIO was superior to open oesophagectomy.[20,21]

## The pylorus

What, if anything, should be done with the pylorus represents an ongoing area of controversy in oesophageal resection. As long ago as 1987, Wong *et al* published a randomised controlled trial (RCT) on 72 patients undergoing oesophagectomy, randomised to pyloroplasty or no intervention.[22] Although there were no significant clinical differences between the groups at long or short-term follow-up, the authors concluded that all patients should undergo pyloroplasty.

Four years later, the same group conducted a further larger RCT, which demonstrated a clear advantage in the group who underwent pyloroplasty, both in measured gastric emptying and symptomatically.[23] In two cases, fatal aspiration was blamed on the failure to perform a pyloroplasty and the authors reported five cases where the patient had symptoms of gastric outlet obstruction until death. This has not been our experience as in 165 patients operated without pyloroplasty, there were no cases of gastric outlet obstruction. In a rarely quoted study, a Dutch group showed in a large RCT that the nature of the gastric conduit but not pyloroplasty significantly influenced gastric emptying, concluding a tubular conduit was superior to any other configuration.[24] The suggestion that bile reflux is worse after interference with the pylorus was not substantiated by quantitative measurement in a small RCT.[25]

Hill *et al* showed that the administration of erythromycin (but not cisapride or fashioning a pyloroplasty) significantly improved gastric emptying after oesophagectomy.[26] We construct a tubular conduit and administer erythromycin (oral syrup); none of our patients have required any pyloric intervention, either surgical or endoscopic, or suffered from significant long-term symptoms of delayed gastric emptying. We do not believe that there is any necessity to interfere with the pylorus during oesophageal resection.

## Extent of lymphadenectomy

Perhaps the most controversial aspect of our approach is that we make no attempt to perform a radical lymphadenectomy either in the abdomen or in the chest. The arguments in favour of a radical lymphadenectomy in the surgical management of oesophageal cancer are well rehearsed. They are, essentially, that such an approach improves the staging of an individual patient, leads to a lower rate of locoregional recurrence and, finally, improves long-term survival. All these arguments are flawed.

It is unarguable that in some cases, a more radical lymphadenectomy will lead to improved or more accurate disease staging for an individual patient. However, since there is no evidence that any post-operative adjuvant intervention significantly influences long-term survival ,it is very hard to see how this information would be of benefit to an individual. Locoregional recurrence alone is rare and usually arises in association with systemic recurrence.[27] It has been argued that the nature of recurrence after surgery for oesophageal cancer does not support the concept of radical lymphadenectomy[27] and we agree. Others, predictably, disagree.[28,29] Nevertheless, there is little convincing evidence that radical lymphadenectomy reduces the risk of locoregional recurrence.

As regards long-term survival, there is a need for solid supporting evidence. Those who argue the case for radical lymphadenectomy focus on two subgroups in the Dutch RCT[13] in which there was a trend (not significant) for longer survival after a transthoracic rather than a transhiatal oesophagectomy (type 1 tumours and node positive tumours). This study is quoted in the recent UK guidelines,[21] which (from our perspective inappropriately) advocate radical lymphadenectomy but which are unable to quote any convincing supporting evidence. There is no level 1 or level 2 evidence to support radical lymphadenectomy in oesophageal cancer. In a large comparison from London of patients undergoing transhiatal and transthoracic oesophagectomy (albeit non-randomised), there was no difference in survival on a stage-for-stage analysis.[30] Since radical lymphadenectomy (at least in the mediastinum) is not feasible technically in the transhiatal approach, this does not support the argument that more extensive resection improves survival.

It is self-evident that with more radical resections, a larger number of involved nodes can be resected but this does not mean that a more radical lymphadenectomy improves survival. After careful examination of the available data, Jamieson *et al* concluded that there is a 'lack of high-level evidence to support lymphadenectomy'.[31] Our

approach is to perform a limited lymphadenectomy but to make every effort to produce a R0 resection, which is the only surgical parameter clearly shown to be associated with an improvement in long-term survival.[52]

The disease specific five-year survival in our series is 36% (the large majority of patients having T3 and/or node positive disease), which is remarkably similar to results published from the large majority of units in the Western world, whatever surgical strategy is employed. Survival has improved by around 5% or a little more over the last 10–15 years, almost certainly owing to multimodality treatment, although this trend was not supported by our data. Radical lymphadenectomy inevitably increases morbidity. Of five patients in our series who underwent radical lymph node dissection, one developed a bronchopleural fistula. We do not believe it is appropriate to expose our patients to this increased morbidity without any definite evidence of benefit, a view espoused by others.[27]

## Anastomotic technique

The influence of anastomotic technique on outcome is associated with a larger literature than any other aspect of the surgical treatment of oesophageal cancer. A non-randomised comparison of 580 anastomoses and a subsequent large RCT showed that there was no significant difference in leak rate between sutured and stapled anastomoses but that the stapled technique produced a much higher rate of stricture.[33,34] These findings have been supported by a meta-analysis[35] and numerous other studies. Stricture formation is also more common after cervical than after thoracic anastomosis, with strictures requiring dilation in 30–66% of cases.[3–5] Some surgeons staple their intrathoracic anastomoses but hand suture in the neck. This mixing of the risk factors confounds the issue and makes it difficult to draw any meaningful conclusion although some authors attempt to do so.[36,37]

After a hand sewn intrathoracic anastomosis, our leak rates, both clinical (0.6%) and radiological (0.6%), are among the lowest ever reported in a substantial series. As our patients came from a large geographical area, it has to be conceded that we may have missed some patients with late stricture although every effort was made to avoid this. In this series, we were able to identify three patients (1.8%) who required dilation for benign stricture. The majority of the available evidence suggests intrathoracic location and hand sewn technique are both predictors of a low rate of anastomotic stricture.[3–5,33–35]

It seems difficult to justify the use of stapling devices to fashion anastomoses after oesophageal resection. A hand sewn anastomosis is cheap. If done carefully, it can be associated with extremely low rates of leakage and of benign fibrous stricture. The only arguable advantage of a stapled anastomosis is that it is quicker. Although some have claimed this results in a significantly shorter operating time,[38] our hand sewn anastomosis takes 20 minutes, which means the comparative time difference can be no more than 10 minutes at most. In the context of an operation taking several hours, this cannot be of clinical significance.

Our policy was to perform a contrast swallow at 6 days and then introduce oral intake. Analysis of 160 consecutive swallows revealed that this investigation did not alter the management of a single patient and this intervention has been abandoned.

## Conclusion

Most, if not all, the issues addressed above will remain controversial. We have stated our preference and, as objectively and persuasively as we are able, attempted to justify that choice. We are aware that some will remain unconvinced. Such divergence of opinion in areas of surgical practice is not only inevitable, it could be argued that it is both desirable and healthy.

## References

1. Beasley WD, Jeffries MT, Gilmour J, Manson JMcK. A single surgeon's series of transthoracic oesophageal resections. *Ann R Coll Surg Eng* 2014; **96**: 151–156
2. Muller JM, Erasmi H, Stelzner M *et al*. Surgical therapy of oesophageal carcinoma. *Br J Surg* 1990; **77**: 845–857.
3. Pierie JP, de Graaf PW, Poen H *et al*. Incidence and management of benign anastomotic stricture after cervical oesophagogastrostomy. *Br J Surg* 1993; **80**: 471–474.
4. Honkoop P, Siersema PD, Tilanus HW *et al*. Benign anastomotic strictures after transhiatal esophagectomy and cervical esophagogastrostomy: risk factors and management. *J Thorac Cardiovasc Surg* 1996; **111**: 1,141–1,146.
5. Williams VA, Watson TJ, Zhovtis S *et al*. Endoscopic and symptomatic assessment of anastomotic strictures following esophagectomy and cervical esophagogastrostomy. *Surg Endosc* 2008; **22**: 1,470–1,476.
6. Goldfaden D, Orringer MB, Appelman HD, Kalish R. Adenocarcinoma of the distal esophagus and gastric cardia. Comparison of results of transhiatal esophagectomy and thoracoabdominal esophagogastrectomy. *J Thorac Cardiovasc Surg* 1986; **91**: 242–247.
7. Boyle MJ, Franceschi D, Livingstone AS. Transhiatal versus transthoracic esophagectomy: complication and survival rates. *Am Surg* 1999; **65**: 1,137–1,141.
8. Omloo JM, Law SY, Launois B *et al*. Short and long-term advantages of transhiatal and transthoracic oesophageal cancer resection. *Eur J Surg Oncol* 2000; **35**: 793–797.
9. Goldminc M, Maddern G, Le Prise E *et al*. Oesophagectomy by a transhiatal approach or thoracotomy: a prospective randomized trial. *Br J Surg* 1993; **80**: 367–370.
10. Jacobi CA, Zieren HU, Müller JM, Pichlmaier H. Surgical therapy of esophageal carcinoma: the influence of surgical approach and esophageal resection on cardiopulmonary function. *Eur J Cardiothorac Surg* 1997; **11**: 32–37.
11. Rindani R, Martin CJ, Cox MR. Transhiatal versus Ivor–Lewis oesophagectomy: is there a difference? *Aust N Z J Surg* 1999; **69**: 187–194.
12. Chu KM, Law SY, Fok M, Wong J. A prospective randomized comparison of transhiatal and transthoracic resection for lower-third esophageal carcinoma. *Am J Surg* 1997; **174**: 320–324.
13. Hulscher JB, van Sandick JW, de Boer AG *et al*. Extended transthoracic resection compared with limited transhiatal resection for adenocarcinoma of the esophagus. *N Engl J Med* 2002; **347**: 1,662–1,669.
14. Bolger C, Walsh TN, Tanner WA *et al*. Chylothorax after oesophagectomy. *Br J Surg* 1991; **78**: 587–588.
15. Gemmill EH, McCulloch P. Systematic review of minimally invasive resection for gastro-oesophageal cancer. *Br J Surg* 2007; **94**: 1,461–1,467.
16. Safranek PM, Cubitt J, Booth MI, Dehn TC. Review of open and minimal access approaches to oesophagectomy for cancer. *Br J Surg* 2010; **97**: 1,845–1,853.
17. Association of Upper Gastrointestinal Surgeons, Association of Laparoscopic Surgeons of Great Britain and Ireland. *A Consensus View and Recommendations on the Development and Practice of Minimally Invasive Oesophagectomy*. London: AUGIS; 2008.
18. *National Oesophago-Gastric Cancer Audit 2010*. Leeds: NHS Information Centre; 2010.
19. Berrisford RG, Wajed SA, Sanders D, Rucklidge MW. Short-term outcomes following total minimally invasive oesophagectomy. *Br J Surg* 2008; **95**: 602–610.

20. National Institute for Health and Clinical Excellence. *Minimally Invasive Oesophagectomy*. London: NICE; 2011.
21. Allum WH, Blazeby JM, Griffin SM *et al*. Guidelines for the management of oesophageal and gastric cancer. *Gut* 2011; **60**: 1,449–1,472.
22. Cheung HC, Siu KF, Wong J. Is pyloroplasty necessary in esophageal replacement by stomach? A prospective, randomized controlled trial. *Surgery* 1987; **102**: 19–24.
23. Fok M, Cheng SW, Wong J. Pyloroplasty versus no drainage in gastric replacement of the esophagus. *Am J Surg* 1991; **162**: 447–452.
24. Bemelman WA, Taat CW, Slors JF *et al*. Delayed postoperative emptying after esophageal resection is dependent on the size of the gastric substitute. *J Am Coll Surg* 1995; **180**: 461–464.
25. Chattopadhyay TK, Shad SK, Kumar A. Intragastric bile acid and symptoms in patients with an intrathoracic stomach after oesophagectomy. *Br J Surg* 1993; **80**: 371–373.
26. Hill AD, Walsh TN, Hamilton D *et al*. Erythromycin improves emptying of the denervated stomach after oesophagectomy. *Br J Surg* 1993; **80**: 879–881.
27. van Lanschot JJ, Tilanus HW, Voormolen MH, van Deelen RA. Recurrence pattern of oesophageal carcinoma after limited resection does not support wide local excision with extensive lymph node dissection. *Br J Surg* 1994; **81**: 1,320–1,323.
28. Peyre CG, Hagen JA, DeMeester SR *et al*. The number of lymph nodes removed predicts survival in esophageal cancer: an international study on the impact of extent of surgical resection. *Ann Surg* 2008; **248**: 549–556.
29. Martin DJ, Church NG, Kennedy CW, Falk GL. Does systematic 2-field lymphadenectomy for esophageal malignancy offer a survival advantage? Results from 178 consecutive patients. *Dis Esophagus* 2008; **21**: 612–618.
30. Davies A, Sandhu H, Pillai A *et al*. 2-Stage oesophagectomy confers no survival advantage over transhiatal resection: analysis of 550 consecutive cases in a single unit. *Gut* 2012; **61**: A253–A254.
31. Jamieson GG, Lamb PJ, Thompson SK. The role of lymphadenectomy in esophageal cancer. *Ann Surg* 2009; **250**: 206–209.
32. Siewert JR, Stein HJ, Feith M *et al*. Histologic tumor type is an independent prognostic parameter in esophageal cancer: lessons from more than 1,000 consecutive resections at a single center in the Western world. *Ann Surg* 2001; **234**: 360–367.
33. Law S, Fok M, Chu KM, Wong J. Comparison of hand-sewn and stapled esophagogastric anastomosis after esophageal resection for cancer. *Ann Surg* 1997; **226**: 169–173.
34. Fok M, Ah-Chong AK, Cheng SW, Wong J. Comparison of a single layer continuous hand-sewn method and circular stapling in 580 oesophageal anastomoses. *Br J Surg* 1991; **78**: 342–345.
35. Beitler AL, Urschel JD. Comparison of stapled and hand-sewn esophagogastric anastomoses. *Am J Surg* 1998; **175**: 337–340.
36. Lam TC, Fok M, Cheng SW, Wong J. Anastomotic complications after esophagectomy for cancer. A comparison of neck and chest anastomoses. *J Thorac Cardiovasc Surg* 1992; **104**: 395–400.
37. Raz DJ, Tedesco P, Herbella FA *et al*. Side-to-side stapled intra-thoracic esophagogastric anastomosis reduces the incidence of leaks and stenosis. *Dis Esophagus* 2008; **21**: 69–72.
38. Craig SR, Walker WS, Cameron EW, Wightman AJ. A prospective randomized study comparing stapled with handsewn oesophagogastric anastomoses. *J R Coll Surg Edinb* 1996; **41**: 17–19.