Reply to the Letter to the Editor

Reply to Metin et al.

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Keywords: Hemoptysis; Respiratory distress; Bronchial carcinoma; Tuberculosis; Bronchectasis; Bronchial arteries

We thank Dr Metin and associates for their interest in our paper and for sharing their experience of massive hemoptysis. They recommended a prompt surgical treatment whatever the patient and whatever the origin of bleeding. We want to congratulate them for their good result (11.5% mortality rate).

However, Dr Metin et al. did not specify the cause of bleeding and one may easily suppose that it is a typical surgical recruitment.

We do not believe that prompt surgery is always possible in poor functional status of the patient.

We think that it is not advisable to perform prompt pulmonary resection for massive hemoptysis occurring in cystic fibrosis patients, as in last course of radiotherapy for unresectable lung cancer, or in case of bilateral bronchectasis?

These cases above mentioned are types of patient usually referred to us. As we wrote management is performed by a multidisciplinary approach. We have highlighted [1] the circumstances in which prompt surgical treatment may be required (bleeding coming from great pulmonary vessels).

One may suppose that at least three patients in Dr Metin’s series might be treated by percutaneous embolization (those treated by segmentectomy). In other words, it seems difficult to treat all patients by prompt surgery in massive hemoptysis.

References


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Letter to the Editor

Enteral feeding access with feeding jejunostomy is advisable after esophagectomy

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We read with interest the article by Page et al. [1] on a randomized comparison between intravenous hydration and naso-jejunal enteral feeding after esophagectomy. We take exception to the conclusions drawn on two counts. Firstly, the number of patients enrolled for the study is too small to detect any difference between intravenous hydration and enteral feeding. The expected differences, the power of the study and the basis of sample-size calculation are not explained. Randomizing 40 patients into two groups would be inadequate regardless of the expected benefit. The power of the study to detect even a 10% difference between the two groups would be less than 25%.

Besides being methodologically and statistically flawed, the study is also conceptually difficult to understand. The rationale of enteral feeding after major surgery is not only for nutritional reasons, and the major benefit of enteral feeding is seen in patients with complications after esophagectomy. Extrapolating the results of a small cohort of patients with no significant postoperative complications on a much larger and heterogeneous group of patients undergoing esophagectomy (often with significant complications including anastomotic leaks) is erroneous. In patients with anastomotic leaks, for example, prolonged avoidance of oral feeding is mandatory. We routinely perform a feeding jejunostomy after esophagectomy and have had less than 2% procedure-related complications in over 800 patients over the last 5 years. Randomized and observational studies have confirmed these low complication rates [2,3]. We have found a feeding jejunostomy to be invaluable in patients with a complicated postoperative course in whom resumption of oral feeds is delayed. The feeding jejunostomy was used in all patients in the immediate postoperative period, for more than 3 weeks in 11%, and for more than 2 months in 6.9% among 523 patients studied by Orringer’s group [2]. They also had a 2.1% complication rate and zero jejunostomy-related mortality. Though a naso-jejunal tube is an alternative to a feeding jejunostomy, its use over a prolonged period would be uncomfortable for the patient. We therefore recommend a routine feeding jejunostomy for all patients undergoing esophagectomy.
References


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Reply to the Letter to the Editor

Reply to Pramesh et al.

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I appreciate Dr Pramesh’s interest in our study. Despite its limitations, which he has pointed out, all of which we acknowledged in the discussion, it was a genuine attempt to look scientifically at the issue of routine enteral feeding in patients having an esophagectomy. Whereas our practice is to avoid this for the sake of simplicity, Dr Pramesh, presumably as a result of his training and personal experience feels jejunostomy feeding is a useful adjunct for all his patients. He obviously has excellent results with this, without jejunostomy complications, so he should not change his practice. Even if we had studied many more patients (several hundred) and still not shown any difference between feeding or no-feeding, I do not think there is any way he would be convinced that he was doing something unnecessary for his patients.

I feel many of the criticisms he has raised have already been dealt with in the paper’s discussion. I must admit I find it hard to accept his contention that “enteral feeding after major surgery is not only for nutritional reasons”.

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Letter to the Editor

Vacuum-assisted suction drainage of sternotomy infection: a new paradigm?

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Doss et al. reported their experience of treating poststernotomy osteomyelitis with vacuum-assisted suction drainage [1]. Along with all other reports on the use of this treatment modality, they described a retrospective cohort for which the basis for treatment selection (conventional vs. vacuum-assist) remained elusive. Despite the explicit title of the manuscript, the diagnosis of sternal osteomyelitis seemed to rest largely on clinical impression rather than made by microbiological criteria. Similarly the eventual success of their treatment regimes were not objectively judged by quantitative wound culture results. These factors can profoundly influence the definition of treatment durations and invalidate any comparison between the modalities.

It is important to define the role of any new or emerging therapy such as vacuum-assisted suction wound drainage. In the context of sternotomy wound infection, it is undoubtedly an invaluable addition to the surgeon’s armamentarium for dealing with this potentially devastating complication. However, it is not a panacea and should be used as part of an overall wound management strategy. The corner-stone of a successful eventual outcome regardless of the choice of wound dressing is adequate wound debridement. Vacuum-assisted therapy facilitates this process in two ways: firstly it encourages the surgeon to perform a more radical initial debridement by providing instant substance and stabilization to the chest-wall defect; secondly it allows for ongoing wound inspection and debridement with minimal trauma to new granulation tissue. The total number of successive wound debridement in each treatment group is therefore important and relevant to the comparison of outcome and is likely to be different between the groups. This crucial information was unfortunately missing from the present study. A reliable surrogate marker of inadequate wound debridement is late fistula or sinus formation involving sequestrated pockets of infected or necrotic soft tissue, bone and/or cartilage. These would occur irrespective of whether vacuum-assisted suction drainage was employed as long as a nidus remained. Without any follow-up data being presented by Doss et al. it remains unclear if this was a significant factor in determining the outcome in their cohort.