References


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Reply to the Letter to the Editor

Reply to Pramesh et al.

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Keywords: Feeding jejunostomy; Enteral feeding; Esophagectomy

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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Keywords: Topical negative pressure; Sternal dehiscence; Wound debridement

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 debridements 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.

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Reply to the Letter to the Editor

Reply to Tang

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We appreciate Dr Tang’s interest in our study and are grateful for the opportunity to answer her questions. The basis for treatment selection in our study was not elusive, but based on the decision of a surgeon to try a new management strategy. Innovation can only happen if individuals have the courage to commit themselves to a new way. We were intrigued by the assumption that our diagnosis of sternal osteomyelitis rested largely on a clinical impression and not on microbiological criteria. In the methods section we explicitly described the microbiological culture findings for all patients in both groups [1]. As the extent of infection and type of organisms were comparable in both groups, the presented data can not invalidate the comparison between these modalities. We agree that adequate wound debridement is an important corner stone for eventual successful outcome, and practice radical wound debridement of all avital and infected tissues. Repetitive wound debridement was not necessary in all but one of our patients (in the conventional group). Also, at 5 weeks follow up, after discharge from rehabilitation, none of the patients had developed late fistulas or sinus formation involving sequestrated pockets of infected or necrotic tissues. Dr Tang points out two ways in which vacuum assisted therapy contributes towards a successful outcome of a sternotomy wound infection. However, she does not mention the foremost advantage over conventional therapy, which is the accelerated formation of granulation tissue. We demonstrated that even large defects can be covered within a short period of time and additional mutilating surgery can be avoided. That is the essence of our single centre experience.