Comments on “Surgical treatment of pyogenic vertebral osteomyelitis with spinal instrumentation” (W. -H. Chen et al.)

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“The treatment of pyogenic vertebral osteomyelitis by the surgeon, the role and responsibilities”

The fore-going review article has a focus upon the surgical treatment of pyogenic spinal infection utilising spinal instrumentation. It is extremely useful and has an excellent reference base. It is good quality and has value for those treating pyogenic infection in that it proposes the use of recent advances in reconstructive spine surgery as a reasonable treatment option in the presence of infection. This may indeed be the case, but what is the range of reasonable options in the treatment of this serious spine pathology? Clinical experience is often the most useful clinical evidence for case management in the absence of quality studies upon which to base a validated treatment algorithm. A review can only be as good as the evidence reviewed and if that evidence is imperfect to begin with then it may be wise to have a balanced moderation so that treatment occurs in context. This is most important for seriously ill patients with significant co-morbidity.

The anterior approaches to the spine popularised by the spinal surgeons of Hong Kong for the treatment of tuberculosis remain reliable in addressing the anterior compressive lesions of the spinal cord. What is clear is that some reconstruction of the spine following this surgery is

necessary if there is loss of structural integrity and a risk of subsequent deformity or instability. A conclusion within the text of the surgical MRC trial was that where adequate facilities are present radical surgery may give the best results, however, there was little clinical difference between the treatment groups [9]. Many of the principals of treatment of non-pyogenic infection spinal infection may have relevance to the treatment of pyogenic infection. However, anecdotal logic is not as reliable as verifiable logic. That is, in the absence of randomised trials comparing anterior instrumentation to other treatments we lack evidence of benefit.

There are no randomised trials; there are no large comparative cohort studies to give either level one or level two evidence on the most effective treatments.

The available options are many. The principal of surgery is clear, first to establish the diagnosis. A second principal is to address any progressive neurological deficit with decompression and a third is to stabilise the spine if necessary. It may be that the spine is stable and no reconstruction is required. Extra focal fixation is preferred by some, others may feel that biological fixation using strut grafts may suffice. Prediction of the success of non operative treatment is related to four independent variables: An age less than 60, Staph Aureus infection, immune status and a decreasing erythrocyte sedimentation rate. In Caragee's series of 111 patients only 14 of the 42 undergoing surgery were considered to require instrumentation [4].

Each case facing a surgeon will have some unique circumstances and treatment will vary according to his personal skill, judgement and experience. The support resources locally in terms of technology for imaging, for pre and post operative care available, and also local health economics will influence decision making.

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A systematic review is a very different process to that of a narrative review. Both have value, one is evidence based, one is experiential. If inadequate evidence exists, then for some topics a narrative review of available publications coupled with clinical experience may stimulate the health care community to evaluate treatment options.

Selection bias can be introduced at many stages in the production of a review. The authors have elected to only utilise what they consider to be the most recent 15 relevant publications to attempt to evaluate best practice. This is not the same as a systematic review and has selection bias which favours newer technology. A single omission of a recent relevant publication may, through no fault of the authors, dramatically alter the conclusions of a review [5].

Levels of evidence are of assistance in according value to a scientific peer reviewed article [6]. Systematic reviews are available to guide us in the best diagnostic imaging of chronic osteomyelitis [1]. Systematic reviews of chronic spine infections remain at level three evidence as a result of the failings of contemporary literature [7]. There are published review articles using level five evidence [8], which is expert opinion only.

Monitoring of infection parameters is extremely useful in clinical practice yet there is much that is not known. The erythrocyte sedimentation rate and its clinical value are well documented in only one publication from 1997 [3]. The C-reactive protein is now commonly used to monitor infection; there are no publications on its predictive value. It is not known at what stage to transfer from intravenous antibiotics to oral. It is not known what the duration of an antibiotic course should be. Many surgeons have strongly held beliefs on these topics in the absence of evidence.

The challenges of the treatment of pyogenic vertebral osteomyelitis are many. Surgery is one small part of that treatment. Prior to surgery, risk assessment and appropriate investigation including adequate tissue sampling [2] with

microbiological advice may avoid the morbidity of surgery. Aggressive surgery may be appropriate for some patients. In similar respects to the treatment of metastatic disease of the spine the patient should be able to survive the magnitude of the surgery and the surgeon should select surgery appropriate to the clinical status of the patient rather than be dazzled by the attractions of shiny new technology.

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