Reviewer's report

Title: A 30-year retrospective study of risk factors for recurrence and life-threatening complications in patients hospitalized with chronic suppurative osteomyelitis of the jaw

Version: 1 Date: 20 May 2013

Reviewer: Gavin Barlow

Reviewer's report:

This is a well written manuscript with a high standard of English.

A. Major Compulsory Revisions

1. The Background section is well written, but far too long - please decrease length of this section by about 50% (half) and include only what is absolutely essential background information for readers to understand the rest of the manuscript. Thanks.

2a. In Methods, the exact definition used for CSOJ is a little 'hazy'/unclear from a scientific perspective - please provide the exact definitions you used to include/exclude patients. Had all patients included not responded to antibiotics/simple debridement? Were patients who, for example, had a 2 year history of recurrent symptoms (suggestive of CSOJ), but no health input prior to definitive therapy included or were only patients who had failed previous health care included? This all needs to be a lot clearer for readers.

2b. Likewise, please provide the exact definitions used for "life-threatening complication" and "respiratory distress"

3. Only patients who underwent antibiotic therapy and surgery were included - this needs to be reflected in both the title and in the discussion please as this is a selected group of patients; for example, patients who died prior to surgery were therefore excluded as were those who for some reason were treated conservatively - inevitably some will have been. For example, a better title might be: "Risk factors for complications following surgery and antibiotic therapy in patients hospitalised with CSOJ" or some variation thereof

4. Many potential confounders (e.g. comorbidities other than diabetes and smoking) were excluded. Also minimal physiological data were included (e.g. pulse, BP, etc.). This does not preclude publication, but it should be discussed explicitly in the Discussion section please as it is a major weakness of your research

5. No microbiological data are presented. Please present any data you collected on this (e.g. in a table) or explain why data were not collected. How many, if any, were thought to be TB? How many were due to resistant bacteria? Was there a
difference in microbiology patterns in those who had received pre-admission antibiotics versus those who did not? This would help justify your comment on "bacterial resistance...allowing residual bacteria to remain,..." in the discussion, which might be the case. Another hypothesis (not mentioned) is that antibiotics before surgery increase the likelihood of a negative culture result, which results in either broad-spectrum antibiotics (generally less effective than narrower spectrum agents when treating fully sensitive bacteria) or even the wrong antibiotic therapy - this hypothesis should be added to the discussion.

6. A bit pedantic perhaps, but the presentation of the age results is a little confusing/misleading in both tables 1 and 2. What these data actually show is that the age groups "less than six" and "19 to 65 years" had significantly lower risk (OR) for recurrence/life-threatening complications compared to the reference age group of >65 years. Those aged 6 to 18 years did not have a lower risk compared to those >65 years. The text in the results and discussion is presented the other way round, which does not reflect the presented data; please change to reflect what it actually shows.

7. Likewise, in Table 2, why was a temperature >40 used as the reference - surely it would have been more appropriate to use a 'normal' temperature range (e.g. approx. 36.8 to 37.8) as the reference with comparison to those in lower and higher temperature cohorts; this is important as some studies in infection have shown that a temperature on admission below the 'normal' range (i.e. relative hypothermia) is a risk factor for poor outcome, but the categorization of temperature in your study (please justify this) does not allow this analysis; please justify (well!) or renanalyse. Did no patients have a temperature less than 36? As in point 6, what the data presented actually shows is that patients with temperatures between 36 and 39 had a lower risk compared to the reference temperature of >40 and again this should be reflected in the text.

B. Minor Essential Revisions

1. In methods, the word "preliminary" is often used when referring to multivariate analyses; "independent" is usually used in most studies.

2. How many patients were excluded and why? Please include in Results section.

3. The study was over 30 years, but obviously the risk factors may have changed over this period, particularly in China which has undergone a lot of social change over this time. Was there any difference in risk factors between the first 15 and second 15 years of the study? This data does not need to be presented in detail, but I think it would be nice to include a comment in the discussion on this.

4. Do you have any data on diabetic control? This is obviously important as it is possible that well controlled diabetes is not a risk factor compared to those with a high HBA1C. If you have any data, please present or comment on this potential confounder in the discussion.

5. Table 1 should also include data on "conservation of pathogenic teeth", but i
cannot see this; presumably missed off by accident - could you add please?

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

Nil