Reviewer's report

Title: Laparoscopic versus conventional appendectomy - a meta-analysis of randomized controlled trials

Version: 1 Date: 24 August 2010

Reviewer: Stefan Sauerland

Reviewer's report:

This article presents the results of a meta-analysis comparing laparoscopic versus open appendectomy. Compared to previous meta-analyses on this topic, the present manuscript addresses some additional outcomes and includes a few more recent trials. Although the results do not contain major new findings, the articles provides a new look at the data from another perspective. In terms of technical and statistical science the analyses look very well, but the authors should be more careful when pooling so many so diverse trials. The results are very difficult to interpret, and the discussion of the results is very complex.

1. The authors have chosen to include the trial by Olmi et al., although randomisation in this study was based on admission codes. Using admission code numbers, birth dates, or dates of admission as a random allocation scheme is not correct. Such pseudo-randomized trials should be excluded from the meta-analysis, also because the number of correctly randomized trials is so large.

2. Another problem of this meta-analysis is the presence of heterogeneity in most outcomes. The authors correctly explain that random-effects modelling is to be preferred in the presence of heterogeneity, but no statistical model is able to compensate for heterogeneity of more than ca. 90% (I²). Therefore, it is important whether any reasons can be identified which underlay this variation in results. For example, patient characteristics such as age (children vs. adults) or gender (women vs. men) might be important effect-modifying variables.

3. When comparing the primary trials included in the present meta-analysis and my Cochrane review, some discrepancies can be found. Most of the differences can be attributed to the fact that literature searches for the present meta-analysis were restricted to English-language articles. Do the authors believe that the exclusion of non-English literature had any influence on the results?

4. I enclose the article by Bruwer et al., which the authors were unable to obtain. Please include this trial if eligible.

5. From a Western perspective, it would be interesting to know whether any additional trials can be located in the Chinese literature. I know of a trial authored by Yin et al. from Taiwan, and I enclose the PDF file of this trial. Are the any more Chinese trials available from one of the Chinese databases? Ideally, the authors would advantage of the nationality and search CNKI (China National Knowledge Infrastructure) in addition to Medline, Embase, and Cochrane.
6. A tendency towards less pain after LA was found, but this difference was more pronounced in the older than in the more recent trials. However, it is implausible that pain after OA or LA has changed over the years. What appears more likely, is the possible influence of trial quality. If the more recent trials included a high proportion of blinded trials, this would explain the results much better.

7. The flow chart (Figure 1) contains an error. The literature search resulted in 583 hits, but the number of excluded hits (n= 527) and the number of potentially eligible articles (n= 58) total up to 585 hits.

8. Please avoid spurious precision when reporting means and percentages. It is not useful to report length of surgery with two decimals places, as operation time will never be measured with a precision of split seconds.

9. Some of the statistical methods are not concordant with current standards
a) As measure of heterogeneity, most meta-analysts nowadays prefer I2 over the Q test. Furthermore, as I2 results are shown in the forest plots, this measure should be explained in the methods section.

b) For dichotomous outcomes, the odds ratio was used, although the Peto odds ratio was found to give more robust results in case of rare events.

c) For the studies with medians and ranges, were standard deviations calculated as 1/4 or 1/6 of the range? This is not clear from the description in the manuscript.

10. The name “Helmy” is misspelled in Table 2.

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

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.