Reviewer’s report

Title: Single-Versus Two-Layer Intestinal Anastomosis: A Meta-Analysis of Randomized Controlled Trials

Version: 1 Date: 27 July 2005

Reviewer: Roger Harbord

Reviewer’s report:

General

This is a systematic review and meta-analysis of RCTs comparing single- and two-layer intestinal anastomosis. The topic appears to be one of practical importance to those in the field. The literature search appears sound and the study is well reported, generally adhering to the QUOROM guidelines for reporting meta-analyses that are supported by BMC Surgery. However I have several concerns about the analysis and the conclusions drawn.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

(1) The conclusion that there is no difference between the two methods is too strong. The confidence interval is wide and includes clinically important effects, so the conclusion should be that there is no evidence of a difference but that there is insufficient evidence to rule out a modest but potentially important difference.

(2) The summary estimate and confidence interval for the odds ratio reported appear to be from the fixed-effect analysis, not the random-effects analysis as stated. Figure 2 also appears to show the fixed-effect analysis. The random-effects confidence interval is considerably wider (RR= 0.91, 95% CI 0.49 to 1.69 by my calculations).

(3) As noted by the authors in the Discussion, suture techniques varied between the trials. I am surprised that they do not consider the possible impact of this in more detail. The study with by far the highest risk of leakage in one arm of the trial (Goligher, single layer, 45%) also has a suture technique in that arm that was not used elsewhere. This study is also responsible for most of the between-study heterogeneity and is the only one that when considered on its own gives good evidence for a risk ratio greater than one. It is therefore an “outlier” in several respects. As this trial has the largest weight in the meta-analysis due to its high event rates, if it were excluded as a sensitivity analysis the results would change substantially. Although this is a post-hoc finding (suture technique was not included in the covariates listed as possible sources of heterogeneity in the Methods) it is surely worthy of discussion. I am not a surgeon and have little idea of the plausibility of this as an explanation for the high leakage rate, or whether the suture technique in question (“vertical mattress and Lembert”) is still in common use in intestinal anastomosis. In any case, a sensitivity analysis excluding this study appears highly desirable.

(4) It is not valid to use standard meta-regression techniques to look at the relationship between risk in one trial arm and the risk ratio (Stephen J. Sharp, Simon G. Thompson, and Douglas G. Altman. The relation between treatment benefit and underlying risk in meta-analysis. BMJ 313 (7059):735-738, 1996.). Also the justification for calling the two-layer group the control group and looking at risk only in this group is not stated. Valid methods are available but are somewhat more
complex, so I would suggest an informal graphical exploration using a L'Abbé plot of risk in one arm vs the risk in the other arm (without a fitted regression line).

(5) The search strategy should be documented as described in the Instructions for Authors <http://www.biomedcentral.com/bmcsurg/ifora/>: "For authors of systematic reviews, a supplementary file, linked from the Methods section, should reproduce all details concerning the search strategy. For an example of how a search strategy should be presented, see the Cochrane Reviewers' Handbook."

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

(6) Table 1. The reporting of numbers as single / two-layer is confusing as the usual convention in meta-analysis is (no. in group experiencing event) / (total in group). This is also easier to interpret especially if also given as a percentage, e.g. in the form 31 / 69 (45%).

(7) Figure 2: replace "single-layer favor" by "Favors single-layer" (and similarly for two-layer).

(8) Figure 2: the note in the legend that "the pooled risk ratio for leaks in single-layer group is 1.00 (95% CI = 0.71 to 1.42)" can be deleted as it is both strictly incorrect (a risk ratio _compares_ groups) and unnecessary (it is obvious from the figure and repeated in the main text).

(9) The risk ratio and confidence interval for the Burch study in Figure 2 are not consistent with the numbers reported in Table 1. One or the other requires correcting.

(10) The meta-analysis routines in Stata are user-written add-ons not part of the base package, so the authors should state the particular routines used and reference their sources and authors.

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Discretionary Revisions (which the author can choose to ignore)

(11) "The use of summary scores from quality scales is problematic - it is preferable to examine the influence of key components of methodological quality individually" (Peter Jüni, Douglas G. Altman, and Matthias Egger. Systematic reviews in health care: Assessing the quality of controlled clinical trials. BMJ 323 (7303):42-46, 2001.). In this case the Jadad score appears particularly inappropriate since, as stated by the authors, blinded outcome assessment is virtually impossible in surgical trials. I would therefore suggest that the assessment of quality should separately consider one or more of the other components suggested by Jüni et al: the generation and concealment of the allocation sequence and handling of patient attrition.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

I declare that I have no competing interests