Reviewer's report

Title: Methods For Identifying Surgical Wound Infection After Discharge From Hospital: a systematic review.

Version: 1 Date: 27 March 2006

Reviewer: Mary-Louise McLaws

Reviewer's report:

General

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

1. The QUADAS Items are those commonly used by epidemiologists to assess in critical appraisal to assess the methodologic rigours of a research paper(s). However, the use of QUADAS should be minor (the PDS papers have already been peer reviewed and most have past the important critical appraisal items) and this section over shadows the the actual aims of the review. Replace Table II and replace it with one that answers the important thrust of the paper: (1) what were the actual inhospiatal surveillance and PDS methods used for each hospital that used PDS.(2) were the methods used inhospiatal and post-discharge similar - if not hoat effect does this have on assessing the level of PD SSI rate (the methods will have to be different as there is no hospital that could use routinely a ICN/ICP to make home visits hence there has to always be a proxy method. (3) did the inhospiatal and PDS use a reasonable definition of SSI (it doesnt always have to be CDC as it is not yet considered the univeral definition of SSI, the most important issue in surveillance is consistency first then validity - surgeons still claim CDC is not valid for many procedures including knee and LSCS), was the follow-up period sufficiently long to enable the manifestation of an infection (4) how practical was the the PDS method (it may be valid but far too costly - there has to be a balance) (5) which procedure works in a PDS as it is not suited for all surgical patients (why did the authors only use selected surgical patient - and if they focused on one was this to accomodate the patients participation in PDS?) (6) PDS is not easy and the majority of highly resourced hospitals still do not perform PDS because it is unreliable and expensive, but what was unique about the study that can be built on to move forward towards a reliable, reasonably valid and reasonably inexpensive method of PDS?

The discussion reads like a critical appraisal lambasting all studies rather than bringing something useful to the readers from the five papers. If PDS was simple to perform without methodological errors it would have been achieved by now. This discussion, as is, sends a message that none of these papers have added anything to the literature, which is not correct. Their assertion that an appropriate clinical spectrum of patients should be represented can be challenged on the grounds of economics and prevalence - PDS should focus on those procedures that are associated with higher SSI (SSI are still statistical rare and hence a procedure associated with <5% rate will have unreliable PDS rates), SSI associated with severe morbidity and mortality (so surgeons and surgical ward staff can learn from the PDS rate) and those SSI that will manifest within a reasonable post discharge period (for economical reasons). The cost of performing PDS and performing PDS research is enormous. The discussion can not focus on how to perform the perfect PDS study, unless the national health can fund it, rather it should refocus on assisting the reader choose patient groups, choose the diagnostic method (patient, ICN or surgeon or GP) and the definition of SSI (it doesn't have to be CDC - you cant possible have full validity of a SSI because the definitions some times requires radiology or laboratory tests which are not always performed in the community if GPs
and surgeons treat the incision as infected without pathology) yet if the authors consistently use a definition which has high reliability then this is important.

The assertion that CDC definition should be used is unfounded. The authors need to note that Mitchell et al clarify their definition - "CDC modified in order to simplify self assessment by patients", as did Whitby. PDS studies using a test other than the ICN will have to modify CDC definition as it is complex and patients, GP and surgeons will not be able to apply it without training.

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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)

A correlation of 0.73 is considered substantial not fair.

Sensitivity and specificity properties of a test (ie ICN test) are taken into account when you are deciding whether to use the test or not use it. Once a test is to be used then it is of little help. Using the "patient" or "ICN" as the test, the more important question is what is the probability that they have an infection when they report one, especially when the probability is dependent on the (low) prevalence of SSI in particular surgical procedure. Hence, I would say this is the reasons for the focus was on predictive value rather than sens and specificity, and the authors should focus on what we can learn from the PPP findings (the difficult test result) rather than the PPN (the easier test result).

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Discretionary Revisions (which the author can choose to ignore)

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 limited interest

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

I declare that I have no competing interests.