Reviewer's report

Title: A Systematic Review and Meta-Analysis of the effects of Clinical Pathways on length of stay, hospital costs and patient outcomes

Version: 1 Date: 20 July 2008

Reviewer: Patriek Mistiaen

Reviewer's report:

General: a well written manuscript on an important topic

MAJOR COMPULSORY REVISIONS:

1. definition of clinical pathways

As the authors already state, the term ‘critical pathway’, and all other related names and terms, is not a well outlined concept, and the ingredients can vary greatly, even despite a similar name. A critical pathway is mostly a multi-component intervention with also many providers involved. This makes (primary) intervention studies a real challenge and this is even more true for systematic reviews that aim to synthesize such intervention studies. The authors have done a nice job and know the inherent limitations of their study.

However, the authors fail to give an exact definition of what they meant by clinical pathway; in the manuscript they refer a few times to table 1 for the definition, but there is nothing of a definition in there; the only thing I can see in table 1 is that a clinical pathway must have 3 components (multidisciplinary, evidence-based, algorithm or protocol) but the authors do not define these components, nor do they give data about the extent that the included studies fulfil those components and differ from each other.

2. sensitivity of search strategies

The authors state that they have used a highly sensitive search strategy (p.13). However I have doubts about this. On page 4 the authors state that clinical pathways are also referred to as ‘integrated care pathways’, ‘critical pathways’, ‘care plans’, ‘care paths’, ‘care maps’ and ‘care protocols’. If the authors know about these other terms, then I would expect them to use those terms also in the search strategy, but they haven’t done so. Furthermore they used terms about ‘hospital’ (steps 11-17) without giving a rationale for that and there is chance that they excluded relevant articles by these steps. Finally they applied in the third step a filter for randomized trial, what is defendable but the Cochrane collaboration offers a variety of proven search filters for RCT’s with varying sensitivity/specificity balances: why haven’t the authors used a proven highly sensitive filter?

3. transparency of inclusion process

The authors state that they searched different databases, but they don’t give the
results of each database or data about the overlap. Also no results are presented about the additional search strategies as the ‘footnote chasing’.

The sifting and inclusion process is not well described: how many reviewers were involved, did they work independently? What is the percentage agreement? Did they look only to title, or to title and abstract? What were the exclusion reasons? The authors state also that they would exclude articles with a high risk of bias, but were there articles excluded for this reason?

I advise the authors to give more insight in the inclusion process and also to add all excluded references with the reason for exclusion as additional file to the manuscript.

4. heterogeneity
The authors are well aware of both the clinical and statistical heterogeneity issue, as proven by many text paragraphs in the manuscript.
However, I miss information about the clinical heterogeneity: Are the patient categories comparable enough? The question raised particularly by the text from the authors themselves that their findings do not apply to stroke patients? But do they apply for all other non stroke patient groups?
Also there was heterogeneity in the readmission outcome; some trialists used readmissions within 3 months, other within 6 months or 31 days or 6 weeks: how did the authors account for these differences?
Furthermore, the authors use ‘in hospital complications’ but do not define what this is. And to what extent are the different complications comparable? Is it allowed to sum medication errors with patient falls?
With regard to statistical heterogeneity the authors state that they wouldn’t attempt meta-analyses if the I2-coefficient is above 60%, what is OK, but the meta-analysis presented in figure 5 about LOS in invasive care pathways shows an I2 coefficient of 66.8%, so…?

5. clinical pathways as complex interventions
The authors know that critical pathways are complex interventions with many ingredients and many providers. To what extent were the included studies comparable with regard to the ingredients and their dose and frequency and with regard to the kind and number of providers?
Also important in this regard is the description of the control conditions: what were the elements of these, and to what extent they were comparable across studies and how much contrast did each provide in the single studies? It can be very well the case that effects are masked by insufficient contrast or by incomparable control conditions.
I miss comprehensive information on those aspects and some discussion about this.

MINOR ESSENTIAL REVISIONS
- post hoc/ a priori sensitivity and subgroup analyses: As far as I know, the
authors have not published a review protocol earlier. So the question, the doubt, remains if those analyses are protocol driven or data driven.

- the authors make a distinction between invasive and non-invasive clinical pathways, but is not clear how these were defined. And they refer to theories on health economics with a statement that invasive procedures can be standardized more easily; maybe a reference can be added.

- randomisation in included studies: were either patients from one hospital randomized or were multiple centres used and were the centres randomized? And how were the control groups protected for influence of the intervention?

- table 1 presents rates of quality outcome measures in a way like ‘237/20 (8.4%)’: but I guess that this must be ‘20/237 (8.4%)’

- table 1, reference Usui 2004: apparently no quality outcome measures were reported: why is that study included?

- figure 1: please add number of hits in each database and number of references for each exclusion reason in each inclusion step

DISCRETIONARY REVISIONS:

- p.6: the authors present the statistical formulas for the effect size computations; this is OK but rather technical and I think these can be omitted in the text and or either referred to a publication or put in an additional file. The same applies for the formulas about the logarithmic transformation on page 8.

- table 3: better as an additional file than in the text and a reference to EPOC-group

**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:**

'I declare that I have no competing interests'