

Reviewer's report

Title: Organizational interventions to implement improvements in patient care: a structured review of the research evidence

Version: 1 **Date:** 8 November 2005

Reviewer: Robbie Foy

Reviewer's report:

General comments

The authors undertook a structured review of systematic reviews of organizational interventions to improve the quality of health care. After identifying and appraising 36 reviews published between 1995 and 2003, they found that:

- The revision of professional roles had variable effects on process measures of care but no consistent benefits on patient outcomes;
- The introduction or use of multidisciplinary teams and integrated care systems generally improved processes of care and (more variably) patient outcomes;
- Knowledge management systems work more consistently over a range of settings;
- The evidence was insufficient to reach a clear conclusion on quality management approaches (e.g. CQI)

This paper represents a useful addition to our knowledge about the effectiveness of organisational interventions – even if this is mainly to indicate what we don't know and to help shape the research agenda. Overall, the paper was clearly and concisely written – which is a good achievement considering the difficulties of reviewing evidence in this area.

The search strategy and criteria for including studies are not described in sufficient detail to enable a faithful reproduction. The authors state that 'Organizational strategies were defined as planned re-arrangements of one or more aspects of the organization of patient care' and excluded 'reallocation of services from hospital to primary care settings'. In the absence of a widely accepted definition of organizational interventions, their definition suffices but the possibility of a range of interpretations may undermine the reliability of this category. Their later taxonomy of interventions (box 1) does assist interpretation of the results.

However, the authors do explicitly state that this work represents a structured rather than systematic review. Nevertheless, the search for evidence was actually reasonably comprehensive, only explicitly excluding studies not written in English.

This paper focuses only on evidence cited in reviews as coming from robust study designs, thereby reducing bias likely to over-estimate the effects of interventions. It is not stated how decisions were made about which studies to include in this review, e.g. independent screening by at least two reviewers. Data from reviewed papers were extracted independently. Similarly, study quality was assessed independently using a '9-item' scale. The criteria for judging the quality of reviews are based on Sibbald et al (2004 – although the reference is actually to Supplement 1 of volume 9) which actually describes an 8-item scale.

A narrative analysis was largely used – which seems appropriate given the heterogeneity of study types, interventions and contexts. The authors' conclusions were compatible with their findings.

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

None

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

1. The authors could state whether they used any system (e.g. independent screening of papers) to identify papers for inclusion.

2. Can the authors clarify whether they added an item to the scale devised by Sibbald et al?

3. The abstract is slightly misleading since it specifically refers to 'High Quality Reviews' – implying that results in abstract are based upon HQRs when they actually seemed to be based on findings from all reviews discussed in the results section. This could probably be clarified.

4. I wasn't certain what 'percentage studies with improvements' (PSI) really added to the interpretation of the tables given that the numerator and denominator of a 'vote counting' method (for all of its limitations) at least provide an immediate idea of the number of studies considered.

5. I was also uncertain as to whether the 'adjusted odds ratio' (AOR) implied that the authors themselves had re-calculated the odds ratios themselves from data provided in the papers.

6. Table 4 (continued). Does the 'A' in brackets for the results of Walton review mean anything?

There were some minor errors that will require further copy-editing, e.g.

Line 26 (abstract objectives): should read: 'an insight'

Line 42 (abstract) should read: 'performance was mostly...'

Line 49 (abstract) should read: 'none of the strategies'

Line 64 (introduction) should read: 'contextual factors'

Line 65 (introduction) should read: 'barriers to...'

Line 66 (introduction) could read: 'drivers for' rather than 'incentives for'?

Discretionary Revisions (which the author can choose to ignore)

One might speculate further in the interpretation of the results – with subsequent implications for future reviews and research in this area:

- For the revision of professional roles, were no consistent effects found for patient because of lack of power, the insensitivity to change of these outcomes or because the interventions were truly ineffective? The absence of economic evaluations is critical here – given that professional roles are often revised to make service delivery more cost efficient
- The results were similar for multidisciplinary teams (MDTs) and integrated care systems, again largely in chronic disease management. Perhaps this is not surprising given that there is likely to be some overlap between MDTs and integrated care systems in their operationalisation, i.e. the latter aim to clearly define roles of different MDT members and may foster closer MDT working. Is it worth making a comment on the problems of the lack of a widely accepted taxonomy for organizational interventions?
- Since the authors looked for high quality reviews, is there any indication that these tend (for

example) to show smaller effect sizes or fewer positive results?

What next?: Accept after minor essential revisions

Level of interest: An article whose findings are important to those with closely related research interests

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

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

Declaration of competing interests:

I am currently initiating a systematic review on the effects of changing organizational structures with one of the co-authors of this paper.