Reviewer’s report

Title: Medical communication and technology: A video-based process study of the use of decision aids in primary care

Version: 2 Date: 23 October 2006

Reviewer: Elizabeth Murray

Reviewer’s report:

General

Thank you for asking me to review this paper. It is well written, and reports a study that has been well conducted. My main reservation about the paper is that it is difficult to know whether the results have any meaning in the “real world”, and if so, what the meaning is.

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

None.

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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)

Penultimate paragraph of introduction: hypothesis (ii) – should be “affect” not “effect”.

Last paragraph of introduction, line 4. Replace “ethology;” with “ethnology:” (insert “n” and use colon, not semi-colon).

Discussion section, Data limitations, line 1 – should be “mixed-methods” not “mix-method”.

Same section, paragraph 3, penultimate line – should be “actively” (not activity).

Figure 1 – I think this figure has not come out on my copy – the figure I have is meaningless, so I suspect there has been a problem in the conversion.

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Discretionary Revisions (which the author can choose to ignore)

Is the questions posed by the authors new and well defined?
Yes, the authors have identified an area not well addressed in the literature, namely the processes by which clinicians and patients come to a treatment decision, and how these are affected by decision aids. This is an important question.

The introduction is well written, and clearly places the study in terms of what is already known, and why the study was conducted.

Methods.
This study has been meticulously executed, and well reported. It would be possible to replicate the study from the information given. The methods are appropriate, given the constraints of having to fit the work into a larger trial. The various methodological decisions made during the study are well justified.

My main concern about the methods is the very non-naturalistic setting. Study participants had been referred to a GP they did not know, as part of a research study. The authors comment that they are not certain whether the results generalise to the whole of the study population. My concern is whether they have any relevance to normal clinical practice.
Results.
The results could be presented more clearly, and I make two suggestions for how to do this. These are discretionary revisions.

1. Consultation timing.
As far as I could see from Table 1, the excess length of the explicit tool consultations is entirely due to the standard gamble. Once the minutes from the standard gamble phase are removed, the median length of the consultation falls to approximately 27 minutes (interquartile range 24.2 – 33.3) which is a bit less than the time taken in the implicit tool arm. I think this is worth spelling out, although, as per my summary comment, I am not sure of the meaning of this result.

2. Results in Table 3.
The text describing the results in Table 3 would be much easier to read if the Kruskal Wallis results and p-values were in the table.

Discussion.
The authors rightly highlight that these consultations are abnormally long, and hence these decision aids are unlikely to ever be acceptable or feasible in routine British general practice. I am less certain of the authors’ second statement, that “the technological complexity of the decision aid did not seem to affect the balance of technical to socio-emotional language used by GPs”. Although this statement is supported by their data, a more interesting question might be the extent to which the introduction of any decision aid alters this balance. This question cannot be answered by their data, but could be posed in the discussion.

The discussion contains a good critique of the study’s methodological strengths and weaknesses. I think the authors should expand the section on transferability to voice considerable doubts as to the degree to which these results would transfer to more normal clinical situations.

Conclusions.
I enjoyed the authors’ conclusions – but am not certain that they are rooted in the data. If they are, the authors should use the discussion section to explain the link between the results and the conclusions. This might also help clarify the “real world meaning” of the 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: Acceptable

Statistical review: No

Declaration of competing interests:
I have non-financial competing interests in relation to this paper, in that I work with Carl May on another project.