Author's response to reviews

Title: A Blended Knowledge Translation Initiative to Improve Colorectal Cancer Staging

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A blended knowledge translation initiative to improve colorectal cancer staging
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Reviewer’s Report #1  John Gabbay

1. Is the Hiss methodology reliable and valid for producing an accurate list of opinion leaders?

We recognize that opinion leader identification is a controversial area. The Cochrane Review (2001) suggests that OLs can have a positive effect on changing clinical practice but that further research is required to determine if opinion leaders (OLs) can be consistently identified\(^1\). Ryan (2002) suggests that the mixed outcomes of OL studies results from the diverse OL identification processes and advocates for a return to the ‘classic’ studies in this area\(^2\).

Our group has addressed the issue of whether the Hiss methodology is appropriate for identifying specialist OLs\(^3\). In our study, we described that we could identify specialist OLs for general surgery and pathology but that some of the Hiss criteria may not be applicable to all types of specialists (in our case, pathologists). In addition, in our work, we identified domain experts, specialists who were considered influential but who do not meet all the Hiss criteria. However we do not know what level of influence, if any, domain experts hold.

The Hiss methodology has been used to identify specialist OLs in three studies that we quoted in the paper. We have included an additional three other papers (Gifford et al. 1999, Guadagnoli et al. 2000, Lomas et al. 1991) that have also successfully identified specialist opinion leaders using the Hiss methodology\(^4-6\). In total 4/6 of these papers utilized the influence of OLs as part of a multi-modal plan that has improved patient care suggesting that Hiss OL do hold influence and hence, the Hiss method of identification is both reliable and valid (Methods).

We have also commented in the text (Results) our lack of identification of OLs at each hospital site has been previously described amongst Australian surgeons\(^7\).

2. If the results differ between the academic detailing/ toolkit/ lecture intervention arm and the lecture intervention it will be difficult to determine if it’s the academic detailing or the toolkit that makes the difference.

We plan on completing qualitative studies once the results have been tabulated to address this issue (Discussion).
3. a) The reviewer comments that the three arms of the study may not remain uncontaminated and is concerned that the general awareness of the issue may interfere with results interpretation.

b) When results are available, it will not be easy to determine if the difference between the control and intervention arms are due to the intervention or to the nature of the hospitals (not identifying OLs).

As now noted in the Methods section a unique strength of our study is that data on LN assessment is available at baseline and at the end of the study from all hospitals regardless of whether or not they participated in our trial. Consequently we will be able to determine the degree to which behavior of physicians from hospitals participating in our trial is different from the behavior of physician from hospitals which did not participate.

Further suppose that LN assessment is approximately equivalent at the start of the trial for all hospitals in the province and that by then end of the trial that the number of LN’s assessed increases for both of the randomized trial arms but more so for the patients assessed by doctors at hospitals assigned to the arm which has formal CME plus the influence of a local opinion leader and toolkit. Then the evidence would suggest that the intervention was effective and that there was a Hawthorne effect. In this scenario contamination across trial arms may have reduced the size of the intervention effect not enough to have negated the effect of the intervention. Of course results may be otherwise in which case it might not be possible to distinguish Hawthorne from contamination effects. It is true that we will not be able to quantitatively distinguish the effects of academic detailing from the toolkit. As with any randomized trial we are limited to comparing the assigned interventions.

We will also be collecting data from 2002 – 2005 (intervention 2004) to see if there was a general increase in awareness and change in behaviour. There were a number of meetings and symposia on LN assessment in Ontario in early 2000-2001 so awareness may have been raised; however, we are not convinced that an increase in awareness alone will result in behaviour change. We have also added a sentence clarifying this issue in the Discussion

4. The reviewer comments that physicians were not “explicitly” made aware they were taking part in a trial.

The phrase “explicitly” has been removed from the sentence (p.10 in Methods)
Reviewer’s Report #2 Richard Holubkov

1. The reviewer requests more justification (references) for the various assumptions made in the power calculation.
   Requested changes to the sample size and power calculations have been made (see Methods p. 6)

2. The reviewer requests clarification with regards to sample size statements and “detecting an increase” from 26% to 52% of the number of patients having 12 LN assessed.
   Requested changes to the sample size and power calculations have been made (Methods p.6)

3. Standardization of the descriptions randomization and control arms has been completed throughout the text and in the Figures. Thank you for pointing this out. We clarified that no hospitals refused participation (Results).

4. A section has been added in the discussion that clarifies that the study is ongoing as well as a timeline.
References


