Reviewer's report

Title: Effects of Screening and Brief Intervention Training on Clinician Alcohol Intervention Behaviours: A pre- post-intervention assessment

Version: 1 Date: 19 July 2005

Reviewer: Ann Roche

Reviewer’s report:

General

1. Is the question posed by the authors new and well defined?

This paper addresses an interesting and important issue in relation to the potency of training in bringing about changes in clinicians’ attitudes and behaviours in regard to screening and intervening for alcohol problems. The paper poses three hypotheses and executes a prospective analysis of clinician change over two time periods. No control or comparison groups are involved.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work?

The study design essentially attempts to assess the impact of training on clinician behaviour. In some respects the study is well described and could be replicated. However in other important respects there are key pieces of information missing. For instance, comparisons are made between the performance of residents and faculty staff, yet no numbers are provided about the size of these two sub samples. The hypotheses did not indicate that the study would involve comparison of these two groups, yet much of the analysis focuses on this.

3. Are the data sound and well controlled?

The data presentation and some of the analyses are very limited. No power calculations are provided making it difficult to assess whether there may be real changes over time even where a simple T test does not indicate such. Some of the samples involves are extremely small. Reporting a 50% increase in a behaviour such as screening or diagnosis an alcohol behaviour with samples as small as 10 or 16 presents problems.

The data presentation does not directly correspond with the hypotheses and it is recommended that the AU’s do this. If comparisons are to be made between different sub groups of the sample this needs to be made clear in the Method and the data presented with greater precision. Moreover, if comparisons are being made between two groups over three points in time then the analyses seem inappropriate (ANOVAS or similar would be expected).

Data was collected on rates of recording by ‘diagnosis’ and CEQ. However no analysis is reported to indicate if these rates change over time (only that the difference between the two methods is statistically different at both time periods – this is the less important question to be addressed).

In other instances it is not clear what analyses where undertaken eg in determining changing in ‘certainty of diagnosis’.

There is little detail on the sample; age and gender (in particular) are known to be important
variables in relation to these types of professional behaviour change. It is assumed that these data are available to the AU’s and could be included in the paper with little difficulty.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
While the overall standard of writing in the paper is good and the general structure of the paper conforms to usual conventions there is a number of areas that are problematic. These include the shift in terminology that occurs in the paper. For instance, the terms ‘recognition’ and ‘confidence’ are used in the hypotheses but these seem to be used interchangeably throughout the paper with the terms ‘diagnosis’ and ‘certainty’. This is not good practice, especially in relation to terms such as ‘confidence’ which can cover a multitude of issues. This level of imprecision in the paper needs to be rectified.

Data presented in the paragraph preceding the Discussion states that ‘residents showed greater increases in brief advice rates (from 4.7% to 7.8%) …’ this does not appear to accord with Figure 1 at all. But as Figure 1 is poorly labeled it is difficult say for certain.

The figures and tables need to be improved.

5. Are the discussion and conclusions well balanced and adequately supported by the data?
The Discussion is not an accurate and well balanced presentation of the findings. As the Results section is not clearly presented it becomes even more difficult to get a handle on the position taken in the Discussion. For instance, the statement is made that “During the project’s intervention phase, clinicians reported providing advice to reduce drinking to 9% of all patients..” While this is true, it is also a non-significant increase over time and after training (and indeed there was a decrease in provision of advice). To further suggest that there is a 50% increase in the percent of problem drinkers receiving advice to reduce drinking needs to be considered in light of the number of cases involved (ie a shift in 8/16 to 49/65). It is very concerning that appropriate cautions and caveats are not included given the type of data involved and the non-significant findings for some key variables.

So while there are some important findings in this study, the data not sufficiently clearly presented and the results are overstated in various places in the Discussion. The paper needs a tight rewrite focusing on the research questions and linking the results directly to these.

A small but important point: The AU’s introduce new terms in the Discussion that would not be familiar to an international audience eg “attendings and residents”.

6. Re title
Is this study really a comparison of different patterns of response to training by different groups eg residents and faculty? If so, the title should clearly reflect this.

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

Discretionary Revisions (which the author can choose to ignore)