Reviewer’s report

Title: Large-Scale Implementation of Alcohol Brief Interventions in New Settings in Scotland: A Qualitative Interview Study of a National Programme

Version: 2
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Reviewer: Nick Heather

Reviewer’s report:

This is an, on the whole, well-written report of a qualitative study of experiences in implementing alcohol brief interventions (ABIs) in settings outside primary healthcare in the Scottish national ABI implementation programme. It was based on semi-structured, audio-recorded interviews with 14 senior implementation leaders in antenatal, accident and emergency and wider settings from 8 of the 11 mainland health boards in Scotland. Although I am not an expert on qualitative research, the method by which data was analysed seems sound and the conclusions derived from these data were, on the whole, justified. The conclusions of the article are clearly stated and will be of interest and useful to those attempting to implement ABIs in non-primary healthcare settings in future. There are, however, some ways in which the article can be improved.

Major compulsory revisions

1) It is stated in the 2nd paragraph of the Introduction that: “Evidence for efficacy in A&E ..., general hospital ..., antenatal ... and other settings including education, pharmacy and criminal justice ... is less convincing.” This is an understatement; in some of these settings it is virtually non-existent. The authors go on: “Notwithstanding this, implementation of ABIs has been recommended in the UK in a wide range of such settings (National Institute for Health and Care Excellence 2010; Scottish Government 2013).”

The issue of the lack of evidence for the effectiveness (not efficacy) is taken up in the penultimate paragraph of the Discussion but I think it needs more attention that this because whether or not an intervention has been shown to be efficacious, or effective, is surely of crucial relevance to a study and discussion of how to implement ABI in novel settings. Do these authors agree with the recommendations of NICE and the Scottish Government in this respect? If not, how do they think this affects efforts to implement ABI in the settings in question? If so, on what grounds? The problem of the lack of evidence for effectiveness of ABI in the relevant settings does not seem to have been mentioned by interviewees since it is not included in the Results section. But do the authors believe that the availability of such evidence could have assisted implementation and, if so, why? If not, why not? And so forth.

The authors may find the following paper useful in this regard: Heather, N (2014). The efficacy-effectiveness distinction in trials of alcohol brief intervention. Addiction Science & Clinical Practice, 9, 13.
2) One of the conclusions of the article is that the interventions to be implemented should be adapted using a pragmatic, collaborative approach with the staff who will be responsible for implementing them. It could well be argued that these adaptations should have occurred before, not during, the attempt to implement. Indeed, development and intensive piloting of interventions should be an early phase in the sequence of research leading to implementation. But even without efficacy or effectiveness trials, piloting and practical adjustments to ABIs should surely have preceded the national roll-out. If this implies a criticism of the Scottish national ABI programme, then so be it.

3) The first conclusion is that having a target for intervention etc. was seen as helpful by interviewees. But in the Results under the heading, The National ABI Target, several drawbacks of having targets are mentioned by interviewees, with supporting quotations. I can see nowhere in the article where the implications of these reservations are taken forward. With these reservations in mind, is there a need to qualify the conclusion reached regarding the helpfulness of targets, either as a topic in the Discussion or as part of the short conclusion in the Abstract. More generally, the authors should make sure that all the implications of material in the Results section are carried forward into the Discussion, at least.

Minor essential revisions

4) In the ABSTRACT/ Results/ (1), the target in question is presumably a target for the numbers of ANIs implemented during a certain period. This may be obvious to the authors but it would be helpful to the reader to have this spelled out in the Abstract and possibly elsewhere. I assume the target is also presumably not a national, local or specific target but embraces all these but, if I am wrong, this should be clarified.

5) The last sentence of the penultimate paragraph in the Introduction says: “National initiatives elsewhere have met with modest implementation success [24, 25]”, referring to national programmes in Sweden and Finland. A more direct and better reference for this point regarding the Swedish programme is: Nilsen, P, Wahlin, S, & Heather, N (2011). Implementing brief interventions in health care: lessons learned from the Swedish Risk Drinking Project. International Journal of Environmental Research & Public Health, 8, 3608-3627. This paper is also directly relevant to the concerns of the present article. Perhaps a similar, more direct report of lessons learned from the Finnish national programme could be found.

6) At the beginning of ‘Data collection’ the reader will be puzzled by the fact that ethical approval was sought from the London School of Hygiene & Tropical Medicine when the research was carried out with personnel from Health Boards in Scotland. I think it is highly unlikely that there are ethical problems with this research, but this matter could be clarified.

7) Results section, 1st para: “Those who were more successful, however, were more likely to report that they had used such strategies from earlier in their implementation efforts.” The meaning of this sentence is unclear. Who are
'those'? Health boards?

Discretionary revisions

8) In the quotation from Interview 4, A&E, is it necessary to translate the word 'wee' as 'colloquial term meaning small'? Surely even the most benighted Sassenach would know the meaning of this word. In any case, it is used previously without translation.

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:

The lead author and one of the other authors are well known to me as colleagues working in the same field of research and I regard them as friends. I am sure, however, that this has not affected the objectivity of my review of this article.