Reviewer's report

Title: Using Intervention Mapping to develop a programme to prevent sexually transmittable infections and HIV among heterosexual migrant men.

Version: 1 Date: 25 October 2006

Reviewer: Daniel Edward Wight

Reviewer's report:

General
1. This is a valuable paper in that it contributes to the slim literature on the careful development of interventions. It has particular potential in qualifying the model of 'Intervention Mapping' which has been little challenged in the literature. Furthermore, it addresses a particularly important sexual health problem in Western Europe, that is the unsafe sexual behaviour of ethnic minority men.
2. However, there are several substantial problems with the paper which, to my mind, mean that it requires major changes before being ready for publication.

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

1. This paper should stand on its own without the need for readers to turn to Bartholemew's original paper/book on Intervention Mapping (IM). However, there are some very confusing aspects of the different steps of the IM model. This might be due to confusion in the original model, in which case the authors should comment explicitly on it's limitations. Alternatively it is due to the authors' poor presentation of the original model, in which case this should be rectified. For instance:
   1a) p.3 'The second step is... selecting objectives... for the programme' but in Figure 1 programme outcomes are decided in Step 1.
   1b) p.3 The terms 'performance objectives' and 'change objectives' are very unhelpful, since their meaning is far from clear: the worst kind of jargon. Why not 'immediate objectives' v. 'long-term objectives' or 'initial change' v. 'subsequent change' or 'pre-requisites for change' v. 'desired change'?
   1c) p.3 Seems very odd that useful theory is selected in Step 3, after the goal-setting process. Is the theory chosen on the basis of how well it fits with the previously decided objectives, or does one's theoretical approach determine one's understanding of what objectives are possible? Most people who value a theoretical approach would assume the latter. This must be clarified.
   1d) p.4 What is meant by 'theoretical methods' (p.4)? Although there is this reference to 'theoretical methods', the description of Step 3 at the bottom of p.4 seems to be entirely atheoretical. Was the intended mechanism of the intervention, or a 'logic model', ever identified? In the Discussion (p.10) it is implied that this was a 'theory led intervention'. But I missed any statement of what the theory was.
2. In the Discussion the difficulties of applying the IM model are mentioned (p.10). The authors should be more explicit about whether the IM model is too complex and demanding, or whether they were simply not given enough time to follow it. This is extremely important since a main contribution of this paper could be a critique of IM.
3. More details need to be provided about how the data were collected. Were group discussions in ethnic-specific groups (p.4)? How were participants selected? Were they original friendship groups or strangers? This is very important in terms of the kind of interaction between them. How long were group discussions? There is confusion about whether the authors started with six ethnic groups or five. How were respondents selected for the 'separate interviews'? How representative were they? On p.5 it is stated that 'more information on certain determinants' was gathered. Which determinants? On p.9 it is stated that the 'intervention was tested among 72 men....'. How were these men selected and how was it tested?
4. Since the data collected were qualitative, the description of findings (e.g. p.6-7) would be greatly improved with some quotations to illustrate the points.
5. Since the paper is all about the development of two interventions, it is rather important that some of the
programme components were not tested. It should be clarified which components were not tested and why they were not tested (p.9). Furthermore, the crucial step of 'designing' the playing cards should be described in detail, with any piloting and subsequent revision clearly documented.

6. In the Discussion (bottom p.10) the authors state that it was very useful for health workers and researchers to collaborate on this intervention. This is very plausible, and an important point, given the difficulties in cross-sector collaboration. However, given importance of this point it would be useful to expand on what was added from each sector in developing the interventions together. If I remember right, Bartholemew et al. discuss this issue, and the challenges involved have been discussed by Wight and Abraham, 2000, 'Health Educ. Research'.

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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)

1. Abstract. 'There is little experience with carefully developed interventions in the HIV/STI prevention field...' Similar statement on p.10. This is greatly overstated. There are now several carefully developed interventions that have been documented (e.g. Obasi et al., 2006, 'AIDS Care'; Flowers et al., 2002, 'Int J. STD AIDS'; Wight and Abraham, 2000, 'Health Education Research' and many more, e.g. cited in 'Effective Sexual Health Interventions' Stephenson, Imrie and Bonell, 2003.

2. p.2 'attention for the sexual health of heterosexual men is relatively new...' This is not accurate. Better to state: 'There has been little specific targetting of heterosexual men to improve their sexual health.'

3. p.2 '...the proficiency of the applicability of...' Do you mean 'how practical'?

4. p.4 '...focus group interviews...' is confusing. Should either be 'group discussions' (or 'focus group discussions' if want to use jargon) or 'interviews'. The latter are generally understood to be individual, and can be structured, unstructured or in-depth.

5. On p.5 there is possible ambiguity about ethnic classification. Does 'non-Dutch' ethnicity mean non Dutch citizens, or non white?

6. Some confusion over 'personal' and 'socio-cultural' factors. Association of condoms with prostitution (p.6) is a socio-cultural factor.

7. Bottom of p.7, social norms are not personal determinants.

8. Top p.8: should give some examples of how the interventions were 'tailored culturally'.

9. p.8 Lack of evidence on sexual health interventions with heterosexual men is overstated. 'limited' might be better than 'little'. If authors went beyond those interventions exclusively targetted at heterosexual men they would find plenty of evidence of effectiveness of different kinds of interventions.

10. p.10 It is not clear why 'self-efficacy' and 'social influence' were initially chosen. Why did this make the interventions more expensive?

11. Figure 1: This should be modified to address the points made above (Major point 1). Should not be reference to PRECEDE model when this is not described. In Step 2 should clarify what the determinants are of. Step 4: why is development of materials and pretesting not a step in itself?

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Discretionary Revisions (which the author can choose to ignore)

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What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field
Quality of written English: Needs some language corrections before being published

Statistical review: No

Declaration of competing interests:

I declare that I have no financial interests in relation to this paper. However, I have published on related themes and so have a professional interest in referring to my work and pursuing my own ideas in this field.