Author's response to reviews

Title: Exploring recruitment, willingness to participate, and retention of low-SES women in stress and depression prevention

Authors:

Judith E.B. van der Waerden (j.vanderwaerden@gvo.unimaas.nl)
Cees Hoefnagels (c.hoefnagels@gvo.unimaas.nl)
Maria J.W. Jansen (Maria.Jansen@ggdzl.nl)
Clemens M.H. Hosman (hosman@psych.ru.nl)

Version: 2 Date: 23 August 2010

Author's response to reviews: see over
Dear Dr Aldcroft

We have the pleasure to submit our revised manuscript entitled “Exploring recruitment, willingness to participate, and retention of low-SES women in stress and depression prevention”. We are grateful for your careful examination of our manuscript and sending us the reviewers’ helpful comments.

The manuscript has been critically revised by a native speaker to improve the style of written English. We have also added an ethics statement in the methods section on page 7 of the manuscript (Approval for conducting this study was provided by the Medical Ethics Committee of the Academic Hospital Maastricht/Maastricht University, the Netherlands, reference number MEC 05-004).

Please find below our point-by-point response to the issues raised by the reviewers. Any changes in the manuscript addressing the reviewers’ comments are underlined.

On behalf of the authors,
Yours truly,
Judith van der Waerden

Judith E.B. van der Waerden, MSc MA
Department of Health Promotion, Maastricht University
P.O. Box 616, 6200 MD Maastricht,
The Netherlands
Tel: +31 (0)43 388 2131
Fax: +31 (0)43 367 1032
E-mail: j.vanderwaerden@gvo.unimaas.nl
**Comments Josephine Etowa**

1. Both the research aim and study design need to be described more clearly. The two research aims do not seem to be closely aligned with the research problem/knowledge gap identified by the authors e.g. in two places under the “Background” section- i.e, last sentence of paragraph 1 and the first sentence of the last paragraph, they clearly identify the research problem as a gap in knowledge about the “factors affecting recruitment, willingness to participate, and retention in depression prevention”. However, the two aims of the study are:

- “The first aim of this study was thus to explore the feasibility of different strategies for recruiting low-SES women from disadvantaged communities for a preventive intervention targeting coping with depressive symptoms and stressors.
- A second aim was to determine whether sociodemographic characteristics and risk status are associated with the successful recruitment and retention of these women.”

The reviewer rightly points out a lack of consistency between research problem and research aims in the manuscript. To remedy this irregularity, first of all the overall research problem has been rephrased to reflect more precisely which aspects of recruitment, willingness to participate and retention we were interested to explore in this manuscript (page 3). Second, we have also partly rephrased the research aims to align them more closely to the stated problem and to elucidate the used research methods (page 4). Since our research sample consisted of low-SES women only, we were interested to know whether certain sociodemographic or risk status factors within this selected population would be associated to their recruitment and retention in depression prevention programs. We have thus rephrased the second study aim in particular, since we felt that its original wording lead to lack of clarity and did not reflect our intentions within the context of this research.

-I’d suggest that the authors change the word “feasibility” bolded in the “first aim” above to “effectiveness” because the strategies are feasible but varies in their degrees of success or effectiveness.

*The reviewer’s suggestion has been adapted and integrated in the manuscript on page 4 as well as the abstract.*

2. If one of the aims of the study is to “determine whether socio-demographic characteristics and risk status are associated with the successful recruitment and retention of these women” as the authors stated in the last paragraph of the “Background” section, then the study sample should include both wealthy and Low SES women/people to be able to assess multiple aspects of the socio-demographic characteristics and risk status. The method section indicated a sample of only “low-SES women aged 20–55 years, with elevated stress or depressive symptoms.”

*The reviewer justly approaches the question about our choice of sample, especially given her interpretation of the second research aim as it was formulated in the original manuscript. If our goal was to compare recruitment and retention of low-SES women compared to high-SES populations we should have indeed included a more diverse sample. However, as explained in response to the first comment, our research aim was to look more specifically which characteristics of low-SES women are associated with their successful recruitment and retention. To answer this particular question, our sample of low-SES women only is suitable.*
By rephrasing parts of the background section and research aims, we feel that our choice of this study sample follows more clearly from the stated aims implying that no additional adjustments to this part of the methods section are needed.

3. Under “methods, paragraph 1, lines 9-10”—The authors indicated that “years of formal education is found to be a valid single estimator of socioeconomic status”. Is this true for all the people who have graduate degrees including PhD but work as taxi drivers and hotel cleaners? This is common experience among immigrant population. See Dr Grace Edward-Galabuze’s (2005) Economic Apartheid.... Maybe this is not the case in the country of study. However, their analysis will be strengthened by providing multiple viewpoints regarding this statement.

Socioeconomic status (SES) is a complex concept for which several indicators are applied in social and medical research. We have chosen to measure SES by the highest level of education attained. Research has indicated that educational level is a good single indicator for SES status, a finding that also applies to the Netherlands (van Berkel-van Schaik & Tax, 1990). However, the reviewer rightly indicates some of the pitfalls when using a single SES indicator, especially in the case of migrant populations. In order to address this issue, we have included a paragraph in the discussion section on this topic in our study limitations (page 14).

4. Overall, the writing style of the manuscript is very good. However, the background section, beginning of the method section and research aims may need to be reworded for clarity and to align the aims more closely with the argument presented by the authors. See details in my comment above.

By addressing comments #1 and #2 we feel that the argumentation and consistency between the sections indicated by the reviewer have been improved, thus contributing to the overall clarity of the manuscript.

Comments Kathleen Ell
1. The ms fails to report outcomes of the participants successfully recruited and participating in the intervention.

We are not sure to which participant outcomes the reviewer refers to. On page 10 of the manuscript we have reported on the characteristics of those participants that have been successfully recruited and subsequently participated in the intervention (“For the 185 participants, attendance ranged from one to eight meetings, with a mean of 5.75 (SD=2.16); 39 women (21.1%) completed all eight meetings. Overall, the only characteristic associated with course completion was age. Women who completed the course were older than those who did not attend all meetings (p<.001). Other demographic variables and risk status were unrelated to course attendance, as was recruitment strategy (p>.01)”).

It might be that the reviewer refers in more particular to the outcomes on participants’ stress and depression levels after completion of the intervention. We feel that reporting these data are not in line with the aims and contents of this manuscript, which focuses especially on the recruitment and retention in a preventive intervention and less so on its expected outcomes. To indicate that intervention efficacy will be taken into account at some point in time, we have added the following sentence at the end of the paragraph “Retention” on page 10:
“Outcomes on stress and depressive symptoms for the participants that were successfully recruited and retained in the intervention will be tested separately in an effect evaluation.”

2. Detailed description of the EWW is needed.

In line with the suggestion of the reviewer about a more detailed description of the Exercise Without Worries (EWW) intervention, we have added such a paragraph in the methods section on introduction on page 5 (EWW is provided by .... more detail elsewhere).