Reviewer's report

Title: Exploring the characteristics linked to the reduction of stigma associated with schizophrenia following an educational program among parents of adolescents

Version: 2
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Reviewer: Heather Stuart

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Public health interest in stigma reduction is growing worldwide. Yet, the bulk of evidence describing the effectiveness of anti-stigma interventions comes from the United States, Canada, and Europe. This study from Japan is a welcome addition to this important field of inquiry. The focus on stigma reduction among parents of adolescents as a possible means of improving early detection and help seeking among parents of children with schizophrenia is novel and an important contribution to the field. The following comments are offered for the authors' consideration:

Major Compulsory Revisions:

1. Individuals who participated in this evaluation were exposed to a 13-minute multi-media slide show focusing on improved clinical knowledge of schizophrenia (eg: characteristics, causes, symptoms, classification, treatment, prognosis, etc.) with the intent of reducing stigma. However, considerable literature (some contained in the discussion section) now shows that mental health literacy (i.e. improved clinical knowledge) does not reduce stigma but may increase it. The theory of change underlying the intervention is not made explicit, nor is it justified with respect to current literature pertaining to literacy and stigma reduction. For example, Table 1 shows that 550 parents (21% of the entire sample, and 52% of the sample analyzed in the table) had increased stigma scores following the program. This is cause for concern but is consistent with population-based meta-analyses showing the increased knowledge is associated with increased stigma. It is also consistent with the study finding that parents with higher levels of knowledge at pretest were more likely to increase their levels of stigma. It is not clear how the intervention provided differs from the “simple knowledge-imparting programs” described in the discussion section as ineffective and why the authors believed it would be effective in reducing stigma.

2. The paper does not provide the overall results for the intervention so it is difficult to tell how, on average, stigma scores changed across the entire sample of 2,645 parents who completed both the pre and posttest. This should be reported in the paper, prior to examining the results in Table 1. Judging from table 1, however, it seems that only 19.2% (n=508) of the sample registered scores that were indicative of decreased stigma. Does this mean the program didn’t work as expected? If the program was not generally effective, does it still
make sense to examine the characteristics of individuals who changed? Some discussion of this would be helpful.

3. It is not clear what Table 3-1 and 3-2 are reporting with respect to the dependent variable. This needs to be specified in the title. The text indicates that table 3 explores the characteristics associated with changes in total stigma scores following the program. If so, it is not clear that logistic regression is the appropriate model (total change scores would have been cordially distributed). This needs to be clarified in both tables. In Table 3-1, all measures are included as independent variables. It is not clear how the dependent variable was calculated and whether it is a variant of one or more of the independent factors. For example, social distance is widely used as a measure of stigma so it is not surprising that it was significant in the regression models. Clearer articulation and rationale for the study measures is needed.

Minor Essential Revisions

1. The sampling frame for the study consisted of 44,000 parents of middle and high school students in Japan. It is not clear if this was a panel of people that were developed by the survey company (such as a marketing panel), or if this was a panel that was representative of the population of parents of middle and high school students. If the latter, then results stand a better change of being representative of the population.

2. The study design is not explicitly stated, but it appears to be a one-group, pretest/post-test design. This should be indicated.

3. Given that a large number of parents (2,690) volunteered to be part of the survey, and given that the video and survey questionnaires were provided to respondents via the web, it is not clear why the study does not include a comparison group and why the intervention was not randomly assigned. This would have provided a much stronger test of the intervention and the web-based platform should have made this feasible to implement. Some discussion of this in the methods section would be helpful.

4. Table 1 is based on the results of 1,058 respondents. Those with scores falling in the range of -2 to +2 were eliminated from the analysis because these scores were not considered to be significantly affected by the educational program. Some explanation as to how this range of scores was identified is needed. Was this based on a confidence interval calculation?

5. Table 2 shows that those with minimal pre-test stigma scores tended to get worse, and those with high stigma scores tended to get better. This may be indicative of regression to the mean (which is endemic in pretest/post-test studies), and which could have been statistically controlled if pretest scores were included in the regression modeling. Table 2 could be made clearer if the column headings indicated which number reflects the n size and which reflects the %.
Level of interest: An article of insufficient interest to warrant publication in a scientific/medical journal

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests: I have no competing interests.