Reviewer's report

Title: Mental health and resiliency following 44 months of terrorism: A survey of an Israeli national representative sample

Version: 1 Date: 18 May 2006

Reviewer: Sandro Galea

Reviewer's report:

General

This is an interesting and well-written paper. I have a few thoughts that I hope may help make the paper more accessible.

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

Abstract

The Methods line of the abstract is a bit misleading. The actual sample was 501 persons and although this is stated as such, it's quite unclear what the relationship between 828, 702, and 501 is until one reads the full paper. I would suggest rewording the first two lines of the Methods section in the abstract to make clear that out of 828 persons randomly selected, 501 persons were interviewed.

Also in the abstract, in the results/discussion portion, the reference to the fact that "exposure to a terrorist attack was not related to traumatic stress symptoms" is again confusing without having read the whole paper. I think needs to be explained more clearly in another half sentence or else removed from the abstract.

Also in the same section, the first mention of the previous work by this group comes in the last sentence of the Results/Discussion, which is not helpful to the reader who is not intimately familiar with this body of work. I would suggest removing the last sentence on page 2 or else to weave in reference to the previous work throughout the abstract starting in the introduction.

The Introduction is very good

Methods

P 8. What happened with the 30 persons who were part of the pilot? Were they included in the 501? If so was the instrument used with these 30 sufficiently different as to raise problems in terms of comparability of these 30 persons to the other 471?

P 11. Imputing sample means for income, while a commonplace procedure, may be problematic. Might it help to carry out a sensitivity analysis, substituting minimum and maximum values for missing incomes, and also running all analyses with the missing income persons removed from the sample to see if this changes the results in any meaningful way? If the paper can reassure the reader that sensitivity analysis suggests that different ways of dealing with the missing income variable did not substantially influence results, then I think it's perfectly fine to leave this as is, i.e., showing us results with mean-imputed income.

The results are fine

Discussion

The comparison to this group's previous work is helpful and a central part of the value of this particular paper. While reading I kept wishing there was a table given that showed results from the first study side-by-side with results from this study. I would suggest adding such a table. I think this would make the comparisons to the old work much easier for the reader to take in.

Another overall comment about the discussion is a suggestion to consider adding in three subtitles. As I
read it, the discussion has three main parts: 1. overall comments about resilience/distress, 2. societal concerns, 3. risk factors. I think the reader will have a much easier time getting around what's in the discussion if this organization is made explicitly clear upfront and if subheads are introduced to guide the reader.

P 17. I had a difficult time understanding the relation between the reductions in the percentage of respondents who were feeling depressed and who had functional impairment and the increased distress. In some respects the structure of pages 16-18 right now is to present the results separately and to explain them separately. But I could not quite reconcile the two seemingly conflicting results. I actually do not think they are conflicting, but I think the explanation can be made clearer. I would suggest that the reader is told right away that some things are better and some things are worse than they were during the last study. The reason some things are better and others are worse is that there are different determinants of these two universes of factors. These determinants are...and the changes in these determinants is what explains the changing patterns of resilience/distress.

P 21. I thought the limitations section could be improved. Yes, I agree these are limitations but what is the reader to make of these limitations? Do the authors think that these influence results substantially? Why? Why not? What can reassure us that these limitations do not substantially influence the findings shown here?

I enjoyed reading this paper. I hope my comments are helpful.

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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)

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Discretionary Revisions (which the author can choose to ignore)

What next?: Accept after minor essential revisions

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

'I declare that I have no competing interests'