Reviewer's report

Title: Changes in the pattern of service utilisation and health problems of women, men and various age groups following a destructive disaster: a matched cohort study with a pre-disaster assessment

Version: 1 Date: 26 May 2008

Reviewer: Ebru Salcioglu

Reviewer's report:

Re: The issue of labelling study groups:

The additional information provided to justify the comparison between study groups is satisfactory. Although the new labeling does not completely solve the problem, it is acceptable.

It is worth noting that the arguments of various trauma researchers brought to my attention by the authors are actually very much consistent with my criticism about labeling the study groups as ‘victims’ and ‘controls’. Given the difficulties associated with defining trauma exposure in a disaster setting and the importance of subjective appraisal of threat to safety in the ensuing psychiatric problems, the mental health effects of this fireworks disaster may not be limited only to those who were living within an a priori defined area of the city. In response to the authors’ ‘firm disbelief in vicarious traumatisation’ they refer to in their cover letter I would like to invite them to read a very interesting nationwide epidemiological study (Schlenger et al, JAMA, 2002). In that study the prevalence rates of probable PTSD in relation to 9/11 attacks in cities not directly exposed to the attacks were 2.7% in Washington DC, 3.6% in other metropolitan areas, and 4.0% in the rest of the country. Direct exposure to the attacks and the amount of time spent viewing TV coverage on the day of the attacks and few days after were strong risk factors of PTSD severity. These findings show once again that without collecting detailed information on exposure characteristics it is not scientifically safe to denote participants ‘victims’ and 'controls' based on some researcher defined criteria.

Re: The necessity of discussion of theoretical implications of study findings in light of evidence in the literature:

The amendment in the introduction significantly improved the manuscript. I cannot say the same thing for the discussion. The authors concluded that the association between gender / age and post-disaster service utilization and morbidity reported by previous studies is attributable to the design of these studies. This is a very limited conclusion. As I indicated before, a critical look at the evidence in the literature shows that demographic characteristics by themselves have a very small effect on post-trauma mental health and their effect could not be thoroughly understood without controlling their interaction with
trauma exposure and subjective threat appraisal characteristics.

Re: Justification of lumping disorders
The new section on ICPC in the methods section is now sufficiently clear.

Re: Formulation of hypotheses
Satisfactory.

Re: Title change
Satisfactory

On a minor issue:

‘Of the few studies that confirm our findings, four had a “pre-disaster” design similar to our study’: Citations need to be added to this sentence.

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests.