Reviewer’s report

Title: Injuries in Syria; first population-based estimates and characterization of predominant types

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Reviewer: Marilyn Leff

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General
I am always anxious to see injury surveillance data from areas in which we have none, such as Syria. Good surveillance data can be used wisely to drive prevention programs. While I think the aims of this study were very good, I have several problems with the study that would keep me from publishing it.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)
I had major questions about the validity of the injury estimates. There are 4 major issues that would have to be resolved before I would suggest that the paper be published.

1) The major problem is the estimate of injury by poisoning. I very much appreciate that the authors were straight forward about this problem and noted that as they were developing the questionnaire, they came to realize that “poisoning” really meant food poisoning to many of the respondents. As far as I could tell that type of poisoning that comes from an intentional or unintentional injury was “mixed in” with the gastroenteritis. Since poisoning was the most prevalent form of injury, I don’t think the data reported provide a good estimate of injury in this part of Syria. Did the authors change the question after they realized how respondents defined that question? In fact, if gastroenteritis was taken out the injury rate would be much lower. My guess is that poisoning would no longer be the number cause of injury. Rather this paper says something to me about the need to address food poisoning in this population.

2) The household member data is so different than the respondent data that I question why even report the household member data. The authors point out some possible reasons for this which are quite valid. Was the definition of poisoning confused in this sample as well? To have over a two-thirds drop in the injury rate among adults says to me that these injury rates are not ones in which I would put much confidence and use to develop prevention programs.

3) The authors give no data for us to evaluate whether the household respondents were representative of the adult population in Aleppo. I noticed that about 55% of the respondents were women. How were the respondents chosen? Was an adult member randomly chosen and if not, how similar were those who responded to the survey like the population. Are there census data available to answer that question?

4) Did the authors take into account the complex survey design in the analysis. As the authors point out, this is a multi-stage complex design. There was no mention if this was taken into account in the analysis. If not, acknowledge it and how this might affect the results.
While the data are the critical factor here, I have other suggestions that I think would improve the paper.

1) Since I am not familiar with Syria, I wanted help in putting your findings into context. For instance,
how different or the same is Aleppo to the rest of the country? Is Syria considered a low or middle-income country and what does that mean? How do the rates found compare to other similar or dissimilar countries. The authors point out that not much has been written about the Arab world, but it is always helpful to put the findings in some context. For instance, the authors could chose to compare to other middle or low income countries. Several studies have been done in those parts of the world, e.g., studies in Kenya, Pakistan, Tanzania. I looked at results from the U.S. to find that the right of injury was 69 per 1000 in those 45-64 which was higher than found in this study even when the injury rate included food poisoning. What is the difference? Is it merely in the way the questions are asked or is there something to be learned about prevention in Syria? Just putting the findings in some context would help.

2) The authors sampled by strata based on informal and formal residential districts. They made a point of saying that the characteristics of these neighborhoods may be different. However, there was no information reported on whether injury rates or characteristics were different.

3) I needed more information on how the SES measure was developed in order to understand it. It was interesting that the injury rate seemed to go down for men with increasing SES but increased for women with increasing SES.

4) More information about the sampling and design of the survey would have been helpful. How same or different from other surveys done in low- to middle-income countries? Over what period of time was the survey done?

5) On what basis did the authors chose the time periods for recovery. I questioned why one would mix those who had a day or two recovery time with those who had limited activity for 25 or more days. I think this needs some justification. I also think that the limitations of asking people about injuries in the past year have to be acknowledged. While sometimes it is necessary to do that given the sample size, the results could be underestimated by using that 12 month time frame.

6) Since the question on the injury experienced in the last year, did not ask “unintentional” injuries experienced, why were there no intentional injuries included or reported? Did the authors not record any intentional injuries?

7) Tables:

a) In the tables 1 and 2, I would like to have had the number in the sample for men and women. I think an “n= ” could go under the heading Men, Women and Overall and in table 2 for the children, adult, and overall categories.
b) It would be could to have confidence intervals for the rates. It appears that the rate of injury was not different for men and women in the older age category but was for the younger age. With confidence intervals one could easily see what rates were different between the two columns (e.g., men and women) as well was within a column.
c) Figure 1 - Why would poisonings be different by gender in the household groups but not in the group of adult respondents? There were no difference by adult respondents in the household; perhaps, again indicating difficulties with the all household members data.

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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)
I think that the paper needs some editing. I thought the authors put in to many parentheses. The information would have better been put in sentences. The authors sometimes repeated themselves. I just think having someone with some editing expertise review it would be helpful.
Discretionary Revisions (which the author can choose to ignore)

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

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

Quality of written English: Needs some language corrections before being published

Statistical review: No

Declaration of competing interests:

I declare that I have no competing interests.