Reviewer’s report

Title: An analysis of risk factors of non-fatal drowning among children in rural areas of Guangdong Province, China: A Case-Control Study

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Reviewer: David C. Schwebel

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This article addresses a major public health concern, drowning and near-drowning, in a country with a very high drowning rate, China. It offers unique data that contribute to the literature on this important topic. The data were collected from a large and representative sample in Guangdong Province, and an impressive case-control design was utilized. Despite these strengths, I have several significant concerns about the manuscript as it currently reads. They are organized below roughly in the order they appear, and divided into the categories recommended by the journal editors.

Major Compulsory Revisions

1. I am unclear about the sample size, and how the sample was derived. We learn that 7,432 individuals responded to the survey, and 805 had experienced a non-fatal drowning. But only 368 cases were chosen, and then they were matched by sex and age. The age-matching of 2 years was quite wide, I believe, given the sample size. What was the reason for dropping the 437 (805-368) cases? Are there data to offer two matches to each case, which might strengthen the research findings? In short, why was this sample and sample size chosen? Losing over 6500 respondents seems a real shame, unless the data will be reported elsewhere, in a different form.

2. I am greatly concerned about some of the measures. The measure of personality is extremely subjective and not normed. I have serious doubt about its validity. I have similar concerns about the items assessing household economic status and swimming ability. They simply are inexact, subjective measures with no validity to them.

3. Related to #2, how were data about supervisors during swimming derived? If that was from the item on the survey concerning the age of the supervisor, I have concerns – this is not necessarily the literature the supervisor during the near-drowning event (which the paper implies), as different supervisors may have been present at different times.

4. Data were collected from respondents concerning the severity of the near-drowning events, but not reported in the manuscript. It might be meaningful to restrict the cases to those who had a drowning event that required treatment, or that resulted in loss of consciousness, or that required CPR treatment. In other words, separate out serious non-fatal drownings from less serious ones and see if the results are similar.
5. I wonder if the authors considered reporting the non-significant bivariate analysis results in Table 2, along with the significant ones? I think these data would be of interest to readers, and would help readers understand the full picture of the results.

6. I wonder why age was not controlled in the multivariate analysis. Age is so important to injury risk, and especially to supervision issues. I would encourage the authors to include age in the multivariate regression model given the importance of development to the issues being addressed.

7. This manuscript seems quite exploratory. I think hypothesis-driven analyses could and should be conducted, rather than using such exploratory strategies. The authors review an extensive literature that educated this study, and I believe that literature could be used to develop hypotheses about non-fatal drownings that could be tested in a more organized fashion. The current analysis plan seems to be “throw everything in and see what comes out”. A more organized, hypothesis-driven analysis might make more sense.

8. Related to #8, I think there is room to consider moderation and mediation effects. For example, how is supervision moderated by age to create near-drowning risk? And so on. The current results seem rather obvious and do not add greatly to the literature. A set of hypothesis-driven analyses considering moderation or mediation might be more useful to the literature, and are certainly possible given the sample and data available.

Minor Essential Revisions

1. The authors state that it is unclear whether risk for fatal and nonfatal drowning is the same. Why would they not be? It is hard for me to fathom different risks for fatal and nonfatal drownings, but perhaps I am overlooking something.

2. I was not clear if the surveys were completed verbally or in writing. Could this be clarified?

3. The authors say only “significant or near significant” results are presented – but it appears only p < .05 results are presented. Isn’t this “significant” and not “near significant”?

4. How was “afternoon”, “noon”, and “morning” defined? These seem rather non-specific categories.

5. Ruth Brenner and colleagues published a very recent study (spring 2009) on the influence of swimming lessons/swimming ability and drowning risk that should be cited during the discussion of swimming ability and risk of drowning.

6. Citations 36, 37, 38 seem to be a bit mixed up in the text – they are not placed in the correct places, as the discussion does not match the citations.

7. I’m glad the authors mention the limitation of how near-drowning was defined for participants, which is another measurement limitation to the questionnaires. Using the data on the severity of the events might help overcome this limitation to some degree.

8. There appears to be a typo on the 2nd line, left column, of Table 2 – a
parenthetical statement is present that doesn’t belong.

9. I don’t quite understand the last line of Table 2. Does this mean that no supervisor reduced the risk of non-fatal drowning? If so, can the authors explain this surprising finding?

Discretionary Revisions

1. On occasion, English grammar is a bit choppy. A native English speaker might read through the document to improve these minor errors.

2. Under “study site”, the authors might clarify a bit about the geography of Lianping County. Are the natural bodies of water lakes and ponds, or rivers and oceans? Obviously, moving water like rivers and water with currents like oceans are more dangerous than are ponds or lakes.

3. Is the weather in Guangdong province warm enough to permit year-round outdoor swimming? If not, the questions about swimming frequency are a bit misleading.

4. Was there a way to handle the situation where a respondent had multiple near-drowning events in the past year to report?

5. A tremendous amount of data was collected but not presented in the manuscript. Some of it might be quite interesting to report, even if only in descriptive form. Do the authors plan to report it in other publications, or could they put some other tables into this manuscript?

6. Any information on whether lifeguards were present in any of the near-drowning situations? Are lifeguards a useful preventive strategy that could be considered?

In closing, I congratulate the authors on collecting such a large and rich dataset, and on producing this manuscript to disseminate many of their findings. I look forward to seeing further work on this very important public health topic.

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

Quality of written English: Acceptable

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

Declaration of competing interests:

'I declare that I have no competing interests'