Author’s response to reviews

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

Authors:

Wenjun Ma (mwj68@tom.com)
Shaoping Nie (Nieshp@163.com)
Haofeng Xu (xhf62@163.com)
Yanjun Xu (gdxj05@21cn.com)
Yurun Zhang (gdncd@126.com)
Xiuling Song (songxl918@163.com)

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Author’s response to reviews:

Dear editor,

The revision version of the manuscript is resubmitted to you. The reviewer’s comments are very helpful to improve my manuscript, and I learned a lot from the process of revision. Thanks very much!

Wenjun Ma

Response to the first reviewer’s comments

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.

According to the formula of sample size for 1:1 matched case-control study, at least 300 pair case-controls are estimated for this study. Balancing response rate and research resources, 368 cases were selected randomly to conduct this matched case-control study.

Although the age-matching of 2 years was a little bit wide, the results in Table 1 show that there is no significant difference for age between controls and cases. This manuscript, we only focus on risk factors of nonfatal drowning. In another manuscript, we plan to report the characteristics of nonfatal drowning.
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.

Yes, this is a limitation, and we discussed it in the new version of the manuscript.

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 supervisor during the near-drowning event (which the paper implies), as different supervisors may have been present at different times.

Although there is an item for supervisor to collect relative data, we only collected data about “who was usual supervisor”, and not collect about supervisors during swimming in this study. So, this is a limitation, which might raise questions mentioned by the reviewer. So, in discussion, we talk about the limitation.

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.

It’s a good suggestion. However, only 30 cases are classified severe near-drowning. So, the sample size is not enough for analysis.

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.

It has been revised according to reviewer’s advice.

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.

This is a matched case-control study. Age and gender were matching variables. So, they are not used in multivariable analysis.
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.

Yes, this is an exploratory study because there is no study involved nonfatal drowning in China. I think we should initiate some further studies to examine these risk factors found in this study.

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.

It’s a good suggestion for the future study. However this study is difficult to finish the analysis of moderation and mediation. For example, age as a matching variable is not suitable for analysis in the multivariable analysis.

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.

This is just a hypothesis because there is no population-based study to examine the issue. So, I make revision according to reviewer’s advice.

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”?

It is revised in the new manuscript.

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

In general, morning means 8:00-11.59, and noon refers to 12:00-13.59, and afternoon means 14:00-18:00.

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.

The paper was cited in the new manuscript.

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.

It is revised.

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.

It’s a good suggestion. However, only 30 cases are classified severe near-drowning. So, the sample size is not enough for analysis.

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.

It is revised

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?

An unintentional finding was that a younger supervisor (less than 20 years of age) significantly increased the risk of nonfatal drowning compared to no supervisor. A possible explanation for this is that most of younger supervisors may be older siblings who have a negative impact on risk behaviors of their younger siblings. Although this study did not collect data about person accompanying as drowning, a recent study from Bangladesh found that in drowning cases in which child was accompanied almost half were children who were 10 years or below.

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.

A native English speaker help me to edit it.

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.
It is revised according to reviewer’s advice.

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.
It is revised.

4. Was there a way to handle the situation where a respondent had multiple near-drowning events in the past year to report?
This study only collected the last near-drowning event if a respondent had more than one near-drowning event in the past year.

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?
It will be reported in another 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?
No.

Response to the second reviewer’s comments
1. In table2, “warning sign around natural bodies of water”, it should have 3 categories. If you combined two categories into one, the variable name may change accordingly, for example to “whether there are natural bodies of water”. Same problem occurred in the variable “Using life vest in water in last 12 months”.
It is revised.

2. Supervisor’s age “>=20” or “>=30” was protective factor? In text and in table, they are not consistent.
It should be >=30. I have revised it in the new manuscript.

3. In table2, if the author gave the frequency numbers in each category it would be better for readers to know more about your sample and evaluate your models’ robustness.
Actually the 95% CI may evaluate model robustness. So, We did not add frequency numbers in new manuscript.

4. How about the first aid or treatments after non-fatal drowning? How many
were treated in emergency department and how many hospitalized? If the author can provide this information, it would be better.
Relative information has added in the new manuscript.

5. In the discussion section, after “which is similar with fatal drowning data in China reported by Yang and Fang”, the author should not make the statement “This indicates that the locations of nonfatal drowning are likely to be different between and within countries due to geographical and economic differences.”
It is revised.

Response to the third reviewer’s comments
Major compulsory revisions.
1. In the part on study subject no information is provided on the number of eligible participants. Only absolute numbers of respondents are provided and no response rates are calculated. This should be added to the manuscript. It should be discussed whether a highly selected sample of cases could have been drawn or not.
It is revised in new manuscript.

2. It seems as if different questionnaires were used for cases and controls respectively. This would mean that interviewers were not blinded and the results could be biased (e.g. more 'hunting of risk factors' by the interviewers among the cases). This issue should be explained in the methods section and potential bias should be discussed.
The interviewers were not blinded in this study. So, it is possible that the results could be biased.

3. The case definition should be more clearly described. It seems as if the WHO definition as reported by van Beeck et al was used. Or were any cutt-off values used, e.g medical treatment? if a broad case definition is used this could affect the results. For example, persons with poor swimming skills may interpret a certain event as drowning, where persons with good swimming skills see a similar event as water fun. Maybe this could also explain unexpected results, such as no supervision as a protective factor. The potential effect of the case definition on the risk and protective factors found, should be discussed.
Because only 30 cases needed medical treatment, it is not enough for analysis in this study if we restrict cases to those who had a drowning event that required treatment, or that resulted in loss of consciousness, or that required CPR
treatment. this is discussed in the new manuscript.

4. How were age and gender dealt with in the analyses? I would like to see
effects of both age and gender in the bivariate and multivariate models, before
concluding that males have increased risks.
This is a matched case-control study. Age and gender were matching variables.
So, they are not used in multivariable analysis.

should be included in the discussion, where the effect of swimming skills is
analysed.
It is cited in the new manuscript.

6. I am not convinced that the conclusion is right that developing and developed
countries have such different risk factors. Too few case-control studies in both
types of situations have been conducted so far. I think the study should conclude
that it confirms that water exposures and poor swimming abilities are major risk
factors for nonfatal drowning.
It is revised in the new manuscript.

Minor essential revisions
1. The authors state that little is known about drowning in developing countries,
but could they summarize the existing knowledge, including from Rahman et al
(injury Prevention 2009;15:75-79)?
It is revised.

2. Can more information on the most common water exposures in the Province
be provided? Is there a coast line? Or an important river or ponds or wells etc?
It is revised.