Author's response to reviews

Title: Type A personality, hostility, time urgency and unintentional injuries among Chinese undergraduates: A matched case-control study

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Author's response to reviews: see over
Dear Editors,

We are very glad to receive your reply. We thank all editors and reviewers for your detailed and excellent suggestions. We appreciate the thoroughness of your editing, and we are grateful for the invaluable learning experience that the editorial process is providing us.

We endeavor to revise the manuscript according to the reviewers’ suggestions, our revisions are highlighted yellow or in revised format in the revised manuscript file; and will be presented in the original sequence of editors’ remarks in the following paragraphs.

To Reviewer 1: Julie Mytton

1. The authors refer back to their previous publication which they describe as a “cross-sectional study”. The absence of information on the methodology of the previous study requires the reader to seek the previous publication to know whether the sample reported in this manuscript is appropriate. I recommend that the authors include a short description of the previous study to include information that the methodology was a questionnaire survey, year conducted, sampling framework, eligible sample, & response rate.

Response to the reviewer:

Sorry for omitting the methodology of the previous study. We were attempting to refer to our previous paper without reproducing the entire document, thus we did not include this important detail. According to your advice, we have added the information to the revised manuscript as follows: we conducted a self-administered questionnaire survey among college students in 2009. The sampling framework was all classes in these three universities, and the participants were selected by a multi-stage random sampling method. The questionnaire solicited socio-demographic information, experience of injuries during the preceding 12 months, and the scale of type A behaviour pattern. Among the selected 2350 students, 97.3% (n=2287) provided a valid reply.

Thank you for your excellent suggestion.

2. The injury outcome in this manuscript is not clearly stated – it is assumed to be a dichotomous variable of 'no injury' vs 'any injury' that meets one of the three criteria.

Response to the reviewer:
Yes. The injury outcome is a dichotomous variable of 'no injury' vs 'any injury' that meets one of the three criteria. And in the survey, it was measured by the item “have you been injured during the preceding 12 months? ① Yes ② no”. We have also added this statement to the second paragraph of methods section.

3. The terms ‘competitive hostility’ and ‘total hostility’ and ‘hostility’ appear to be used interchangeably – are they the same thing? If so, please use a single term. If not, clarification is required. The opening paragraph of the results section refers to ‘the hostility and competitive component’ which suggests that they are different.

Response to the reviewer:

Yes, they are the same thing. We revised them and now only use the term “the hostility component”. Sorry for the confusion.

4. It is acknowledged that you can only explore the variables included in your original questionnaire, but other potential confounders, e.g. drug use, caffeine use, diagnosis of ADHD or conduct disorder etc, would influence the likelihood of injury and may be associated with personality. If you did not collect information on such confounders the potential impact of unmeasured confounders should be considered in the discussion section of the manuscript.

Response to the reviewer:

Yes. Because we want to focus on type A personality, we did not originally consider these confounding factors, therefore, did not include them in the survey. In the limitations paragraph of the discussion section, we mentioned some other potential confounding factors that we did not include in the survey such as risky use of telephones, insufficient sleep, among others. But we have not taken the factors you mentioned into consideration. These factors have been added to the revised manuscript, also recognizing this point in the limitation paragraph of the discussion section as follows:

We also did not include other potential confounding factors such as driving while not wearing helmets [40-42], risky use of cellular phones while driving [3, 43], insufficient sleep [44, 45], associated training [46], drug use, caffeine use, and diagnosis of conduct disorder [47] which would influence the likelihood of injury, especially road traffic accidents, and may be associated
with personality. However, some of these factors such as driving, drug use, and caffeine use are not very common among these undergraduate students; and should therefore not influence the main results of this study.

5. The methods section states that groups A and mA were combined into one group “Type A” and groups M, mB and B were combined into one group “not Type A”. However the results section appears to report the data by all 5 groups of TABP. Please revise the manuscript to ensure consistency between methods and the results presented.

Response to the reviewer:

Yes, in the tables we report the data mainly by 5 groups of TABP. However, in part 3 (Type A behaviour pattern and non-fatal unintentional injuries among undergraduates) of the results section, the first paragraph showed the results of the comparison between “type A” and “not type A”, and the results were shown as odds ratios and 95% confidence intervals as follows: (First, we found that, compared with the non-type A individuals, the crude OR for unintentional injury among type A subjects was 1.85 (95% CI: 1.21-2.82), P=0.004. After controlling for the number of siblings, family income status, drinking habits, and sports, we got similar results; the adjusted OR was 1.86 (95% CI: 1.20-2.88), P=0.004. When we adjusted further for all variables in table 2, the adjusted OR was 2.04 (95% CI: 1.29-3.24). ) But it was not shown in the tables.

In addition, according to your suggestion in point 11, in the opening paragraph of the results section, here we also compared the proportion of type A students in the cases to that of the paired controls, and then compared the scores of TABP scale, TU score, and CH score between cases and controls as follows: Among the 253 cases, 29.6% were type A students; while the proportion was 18.6% in the control group, the McNemar test showed that there was a statistically significant difference between cases and controls, P=0.005. The total score of the TABP scale was also higher in the case group (24.11±6.28), than that of control group (22.79±5.77), t=2.59, P=0.01. In addition, the mean CH score of cases (12.97±3.58) was also higher than that of control students (12.15±3.23), t=2.86, P=0.005; while the mean TH score of cases (11.14±3.72) was not significantly higher that that of control students (10.64±3.55), t=1.57, P=0.12.

6. It is unclear why the analysis of the relationship between TABP and injury is split into an
analysis that includes some confounders, and a second analysis that includes all confounders. What was the rationale for partially and then fully adjusting the model? If both steps are required, this should be explained in the methods. If only the fully adjusted model is relevant this should be the only model reported in the text and the tables.

**Response to the reviewer:**

In the methods section, we wrote: we fit a series of models adjusting for each covariate to assess potential confounding or mediating effects, and a full model adjusting for all covariates. We adopted this analysis strategy from other studies such as [17]. Actually, we fitted several models to see whether the relationship between TABP and injury was always present, and the results showed it remained consistent, thus we think that the conclusion was relatively robust.

In the methods section, we added the reference study and rewrote as follows: we fit a series of models adjusting for each covariate to assess potential confounding or mediating effects, and a full model adjusting for all covariates as in other studies [17].

7. Penultimate paragraph in results section states “there seems to be a dose response relationship between the time urgency component and injuries”. This appears to be inappropriate since the data in table 5 shows quintiles with confidence intervals with considerable overlap, suggesting that any apparent increase in ORs may be due to chance.

**Response to the reviewer:**

We acknowledge that the dose-response relationship is not statistically significant. Our statement seems unclear. We have revised the sentence as follows: The dose response relationship between the time urgency component and injuries is also not statistically significant.

8. The final paragraph of the results section describes analysing the association between hostility and injuries. Do you mean that you compared CH with injury adjusted for time urgency? No data or figures appear to be presented to support your statement that the hostility remains independently associated, with a dose response relationship.

**Response to the reviewer:**

Originally, we think it was used to confirm the results, hence the detailed data were not shown. We
added the ORs and 95% confidence intervals in the final paragraph in the revised manuscript, that is, (Compared with the first group, ORs and 95%CI for the other four groups were 0.82(0.47-1.43), 1.42(0.71-2.87), 1.50(0.83-2.72), 2.64(1.21-5.75) respectively, \( P=0.022 \)).

9. The fifth paragraph of the discussion section contains the statement that “psychologists, especially in China, should find a better, more accurate scale for assessing type A personality or hostility” – If the measure does not accurately reflect the variable does this not invalidate your study?

Response to the reviewer:

We analyzed the reliability of the scale and the subscale, and we think that the subscale of hostility is acceptable; however, considering that the above statement “psychologists, especially in China, should find a better, more accurate scale for assessing type A personality or hostility” would tend to make some readers doubt our conclusion, we thought it would be better to delete them. The contents pertaining to this point were deleted from the revised manuscript.

10. The last paragraph of the discussion and the conclusions appear to have over-interpreted the findings of your study. The results do not specifically support a recommendation that services should be provided for those learning to drive to prevent RTAs since the outcome of interest in your study was any injury not just road traffic injuries. It is inappropriate to recommend Tai Chi unless there is evidence that use of Tai Chi in persons with TABP reduces injuries. It is not appropriate to generalise findings from a college population to school children or their teachers or supervisors, or recommend combative sports. Recommendations for research, for practice and for policy should be directly supported by the findings of your study.

Response to the reviewer:

Yes, as you suggested, recommendations for research, for practice, and for policy, should be directly supported by the findings of a study. This suggestion has been used as a valuable guide in the revised manuscript.

11. It would be helpful if the opening paragraph of the results section could include the fact that the 506 students included in this analysis were drawn from the sample of 2287 students that
completed valid questionnaire responses in your previously published study. The methods section states that groups A and mA were combined into one group “Type A” and groups M, mB and B were combined into one group “not Type A”, therefore this paragraph should present the proportions of cases and controls that were, and were not, Type A behaviour pattern, with their respective scores.

**Response to the reviewer:**

This is a very good suggestion. Now we have added the results between the cases and the paired controls in the opening paragraph of the results section as follows:

In the sample of 2287 students who completed valid questionnaire responses in the previously published study, there were 320 injured students; of these, 253 students were drawn and individually matched with controls. Among the 253 cases, 29.6% were type A students; while the proportion was 18.6% in the control group, the McNemar test showed that there was a statistically significant difference between cases and controls, \( P=0.005 \). The total score of the TABP scale was also higher in the case group (24.11±6.28), than that of the control group (22.79±5.77), \( t=2.59, P=0.01 \). In addition, the mean CH score of cases (12.97±3.58) was also higher than that of control students (12.15±3.23), \( t=2.86, P=0.005 \); while the mean TH score of cases (11.14±3.72) was not significantly higher than that of control students (10.64±3.55), \( t=1.57, P=0.12 \).

Thanks a lot.

12. It is unclear why the abbreviation ‘TH’ has been used for Time Urgency component of Type A Behaviour Pattern – ‘TU’ would appear to be more appropriate.

**Response to the reviewer:**

That’s a habit in China, because some persons used “time hurry” to represent this domain, thus TH is used commonly in China. But in this manuscript we used time urgency, so we changed all TH to TU in order to avoid misunderstanding in the revised manuscript.

**To Reviewer 2: Robert Atkins**

1. Hostility versus Type A personality. In reading the paper I had some concerns that the focus was more on hostility than the Type A personality. As I'm sure that the authors are aware number
of personality researchers have argued that the Type A personality construct is only important as it pertains to hostility and anger (Williams, 2001). At the very least I would suggest that the authors revise the Discussion to acknowledge that point because the findings from the study do seem to support that argument.

**Response to the reviewer:**

Yes, maybe we focused more on hostility than time urgency. One reason is that our study indicated the positive association between hostility and injury but no significant association for time urgency. Another reason is that there seems to be more current studies about hostility than about time urgency. Therefore, we added a sentence in the second paragraph of the discussion section to acknowledge this point as follows: This confirmed previous suspicions that perhaps only a subset of Type A individuals are at higher risk for injuries, or, perhaps the type A personality construct is only important as it pertains to hostility and anger.

2. Active versus sedentary. The authors should revise the manuscript to make it clear that they have not measured sports participation. The authors do not control for the fact that activity is associated with increased likelihood of injury? They did not control for sports – the measure was “do you like sports?” The authors do not provide a measure of how much time the participant engage in sports, the intensity of sports, or the types of sports.

**Response to the reviewer:**

Sorry, in this survey, we only collected the information “do you like sports?”, and we assumed that those students who like sports would likely engage in sports more frequently. Thus we did not collect the information of frequency, intensity of sports, or the types of sports, which is definitely a disadvantage of our research. In the revised manuscript, we mentioned this point as follows:

We assume that students who like sports are likely to engage in sports more frequently; therefore, we did not collect detailed information on frequency, intensity of sports, or the types of sports. There must be some differences between liking sports and the actual frequency of sports, which should be taken into account in future research.

We think this is a good suggestion, and we shall include more detailed information on sports behaviour in any future survey. Thank you very much.
3. Temporal aspects. The authors should also note that they did not control for the possibility that injury may have caused a change in personality trait (e.g., hostility). As this is cross-sectional data it is not possible to conclude that the personality type preceded the injury.

**Response to the reviewer:**

As this is a cross-sectional study, we acknowledge that we cannot conclude that the personality type preceded the injury—although we think that the personality of a person does not change too much with time. Thus, we only conclude that the type A personality and the hostility component were associated with non-fatal unintentional injuries. We added this information in the penultimate paragraph of the discussion section as follows: 

> Additionally, as this is not a prospective study, it is not possible to conclude that the personality type preceded the injury.

Again, thanks a lot.

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Thank you very much for your suggestions.

Sincerely yours,

Shi Hongying