Author's response to reviews

Title: Community physicians' knowledge of secondary prevention after ischemic stroke: a questionnaire survey in Shanxi Province, China

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Author's response to reviews: see over
Re: Revision of Manuscript 7820144351451872, a Research article titled “Community physicians’ knowledge of secondary prevention after ischemic stroke: a questionnaire survey in Shanxi Province, China”

Dear Mr Aldrin Ulep

We very much appreciate the constructive comments from both reviewers and for the opportunity to submit a revised manuscript. As requested, we have revised the manuscript accordingly, and have attached our point-by-point responses to each of the comments below. Your further comments are welcome and we would be happy to address any additional concerns.

We hope that our revised manuscript is now aligned with your requirements and is acceptable for publication in *BMC Medical Education*.

Sincerely yours
Responses to the first reviewer’s comments:

1. The use of English language is suboptimal to the point that it is difficult to grasp the purport of the sentences at times. It is important to have this manuscript professionally edited for grammar and language.

   Thank you for your concern. A native English speaker with a background in medical publication has edited the manuscript.

2. The reader/reviewer does not understand the difference in training/expertise between physicians with junior college degree, bachelor’s degree, and master’s degree in medicine. It is relevant for the reader to understand what the number of years of medical school and the number of years in postgraduate-level (resident/fellow) training.

   We have added a description of the participants’ academic qualifications by including the number of years of medical study completed after secondary school in the first paragraph of Results (page 7).

3. The reader/reviewer is oblivious to the structure of the Stroke systems of care (and healthcare in general) in the Shanxi Province. What does a community physician do with a patient with, for instance, a high ABCD2 score? Does such a patient get referred to a tertiary care center or a Stroke Neurologist for
expedited workup? It is important for the reader to know what Stroke systems of care exist in the Province being surveyed.

If a patient has had a high ABCD2 score, she/he will be transferred or referred to a tertiary hospital equipped with a stroke care center or a neurology department. A sentence specifying that “Standard procedures stipulate that such patients be promptly transferred or referred to a tertiary hospital with a stroke care center” has been added into the Discussion (page 11).

4. In the discussion the authors indicate that the response rate was high because of the involvement of governmental authorities. What role did they play in ensuring a high response rate?

As all the physicians are government employees, responding to the survey was considered to be “mandatory”. We have now noted this on page 13.

5. On pages 3 and 4, lines 65 and 66, authors mention recommendations of the Chinese National Guidelines for prevention of ischemic stroke and transient ischemic attack. They need to provide a reference for this.

Thank you for pointing this out. We have added a reference to the guidelines. (Chin J Neurol 2010;43(2):1–8, which is cited in the text as reference 6).

6. Have the authors considered the possibility that the community physicians might be more acquainted with recommendations of the Chinese guidelines
rather than AHA/ASA guidelines on which the questionnaire was based? Does data exist on the knowledge of Chinese guidelines among community practitioners?

This comment raises an important issue. However, as the AHA/ASA and Chinese guidelines are essentially the same, the questionnaire was aligned with both sets of recommendations. We have cited the Chinese guideline in the revised manuscript (reference 6).

7. Q2 was framed without a context. It would be impossible to answer this question without a context. Did the authors mean to ask about BP control in acute stroke here?

This question was based on the guidelines for control of BP in the event of stroke. We do agree with your comment, consequently, the English language translation of the question has been changed to “In secondary prevention for ischemic stroke or TIA, which of the following antihypertensive recommendations should be used in patients with elevated blood pressure”.

8. The use of p values in the tables is done without a proper statistical framework. Were these analyses planned a priori? What do these mean? Which differences are significant and why?

The $P$-values were obtained by chi-square tests and $P<0.05$ indicates a significant difference between groups with regard to the degree of awareness. This information
has been added as a footnote to the table. The use of chi-square tests is noted in the
description of the statistical analysis, and was preplanned.

Responses to the second reviewer’s comments:

Major compulsory revisions:

1. In the second paragraph of the METHODS, it is not clear whether
questionnaires were sent to all physicians, or a subset of physicians, in the 11
prefectures. Similarly, in the first paragraph of the RESULTS, it is not clear
whether the 1,918 physicians at 832 locations were all the physicians and
locations or a subset of physicians and locations.

The questionnaires were sent to all nontertiary medical institutions (832 locations) in
the 11 prefectures. Each had from one to three physicians who responded to the
questionnaire. In response to your comment, the sentence has been changed to “A
questionnaire with open-ended and multiple-choice questions was mailed to all
physicians practicing in clinics in the 11 prefectures of Shanxi Province, and all of
those who responded provided written, informed consent before completing the
survey.” In the Method section of the revised manuscript

2. The sixth paragraph of the RESULTS, “Knowledge of secondary prevention
strategies in ischemic stroke or TIA” – it seems that presenting these data
according to physician characteristics would be useful. Consider simplifying the
physicians’ responses to “correct” vs “not correct” and including a table that
summarizes percentages of “correct” answers for all questions, across
demographic subgroups including gender, level of education, practice duration,
and location.

We appreciate your suggestion, however, reporting responses as “correct” vs. “not
correct” or “Yes” or “no” without explanation might overlook some important
information that could be useful in the establishment of specific targets for further
education and training of all the practitioners. In addition, the analysis was planned to
compare responses in demographic subgroups rather than across subgroups. Therefore,
we prefer to keep the table in its current form.

3. In the fourth paragraph of the DISCUSSION the authors frame the
single-province scope of this study as a “limitation.” I disagree. Local data is
vital for local decision making. I see the single-province focus as a strength,
because the results can be used to improve public health and health care in that
province.

Your comment is indeed correct. We have revised the discussion of study limitations
and strengths to reflect your view, stating that “The strengths of this study are that all
the 11 prefectures of Shanxi Province were surveyed and that the response rate was
high … ”. Following this, we have added a sentence referring to future conduct of
similar studies that are nationwide in scope.
Minor essential revisions:

4. In the ABSTRACT, the survey is described as “prospective.” However, it seems to me that a more accurate descriptor would be “cross-sectional.”

We agree with your comment and have changed the description of the study design to “cross-sectional”.

5. In the first paragraph of the INTRODUCTION, “This geographical difference may explained …” should be “This geographical difference may be explained …”

Thank you, we have corrected this.

6. Are the 832 locations independent, or are they organized into health care systems? This might have implications for the types of educational interventions that might be employed in the future.

They are organized as a medical care network with primary and secondary levels of care.

7. In the second paragraph of the INTRODUCTION, should the Chinese National Guidelines for Prevention of Ischemic Stroke and Transient Ischemic Attack be cited with a reference for the guidelines?

Thank you for pointing this out. We have added a citation (reference 6) in the text and the bibliography at the end of the manuscript.
8. FIGURE 3 contains very little data; these data could probably be better expressed in a small table. I suggest using a table with three columns, as below. Alternatively, the data in FIGURE 3 could simply be merged into TABLE 2. Thank you for this helpful suggestion, which we have accepted. The data that was presented in Figure 3 is now merged into Table 2.


Discretionary revisions:

10. In the second paragraph of the RESULTS, the authors claim that physicians in the study “under appreciated” the “importance” of certain risk factors – physical inactivity, obesity, smoking, alcohol. This claim depends on how you define “importance” and how you define “under appreciated.” FIGURE 2 clearly shows that these factors were considered less “important” than factors like hypertension, diabetes, prior TIA, etc. But what if physical inactivity, obesity, smoking, an alcohol ARE in fact less “important” than hypertension, diabetes, prior TIA, etc. In that case, it could be argued that the factors labelled
as “under appreciated” were in fact adequately appreciated. And what makes a factor “important?” The strength of its association with recurrent stroke? The potential for interventions to modify the factor? In short, it would be helpful if the authors could explain what they meant by “importance” and by “under appreciated.” How was the concept of “importance” perceived by the physicians who responded to the questionnaire?”

The degree of the importance of a factor for the recurrence of stroke was represented by a scale from 1 to 9, with a larger number indicating higher importance. “Under appreciated” was interpreted as a rating of 6 or less. We explain the rating scale in Methods (last paragraph of page 5) and the rating for “under appreciated” is described in the last paragraph on page 7.

11. TABLE 1 is quite cumbersome, and could perhaps be expressed better as a multi-panel figure, formatted like FIGURE 1. Consider making FIGURE 1 the first panel in a multipanel figure, then repeating the bar chart for the subgroups identified in TABLE 1 – men, women, different levels of education, practice duration, and location. There would be a total of 12 panels in such a figure. I think a multi-panel figure would make it much easier for readers to see differences in the patterns at a glance.

Thank you for this suggestion. Figure 1 has been merged into Table 1.
Do the authors have any information about why physicians are not using stroke scales? This information could be important for designing educational interventions for the future.

Your comment is right on target. We believe that community physicians do not use the stroke scales because of their lack of knowledge in this area. We conducted this study to try to confirm that and to obtain, as you suggest, information about the current status to help design relevant, effective training programs.