

Interactive comment on “Decapitation of high-altitude glaciers on the Tibetan Plateau revealed by ice core tritium and mercury records” by S. C. Kang et al.

Anonymous Referee #1

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With their paper “Decapitation of high-altitude glaciers on the Tibetan Plateau revealed by ice core tritium and mercury records” Kang and others provide data from two high elevation ice cores and relate these to presumably negative mass balance at those sites during recent decades. The paper is generally well written, clear and provides interesting results. It certainly deserves publication in the journal. Nevertheless, I am concerned regarding six major issues, a series of smaller points and some deficiencies regarding figures. I recommend that these points are well looked after before final acceptance of the paper in TC. The six major points are the following:

1) Wording and Title: I think that the wording in the title and in subsequent phrases in the manuscript needs a little revision. To my understanding “decapitation” is in this case

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an inappropriate word that relates to killing of living creatures. In most circumstances, in civilization, people would associate a criminal offense with such kind of action. I don't think glaciology should make use of such martial wording. In contrast to this, climate is a physical system that does not act in the sense living creatures can act. Furthermore, a glacier is a dead body of frozen water. Regardless of the fact that it moves under the influence of gravity it is not a living thing that can be decollated. Therefore, and with respect for anything that is actually an animate being on the planet, I strongly recommend replacing the word "decapitation" with for example "loss of accumulation area" or "diminishing accumulation area" or something similar.

2) Overall conclusions and generalization of results from only two sites to the whole region: Since both ice cores have been collected from sites at 5800 m asl you cannot really say anything regarding higher altitude accumulation areas above 5800 m asl. Therefore, maybe there is no complete loss of accumulation area since there might be remaining bits of accumulation areas further up. Therefore, I strongly recommend to be precisely saying that there has been a loss of accumulation area probably up to about 5800 m asl at the two study sites. Anything that's further up on the glaciers or related to other glaciers in the area is - to my understanding - not covered by this study. In consequence, a complete loss of accumulation cannot be concluded from the study. That doesn't invalidate the study. It simply implies that - while "decapitation" shouldn't be used as a word anyway - even the complete loss of accumulation area is not a valid mature conclusion as long as it is solely based on the analysis of the two ice cores.

3) In the section on methodology it is said that the ice cores were taken from slightly above or around the ELA (P421, L13) above the actual snowline. Isn't the area above the ELA part of the accumulation area? How can you obtain an ice core from above the ELA and at the same time reach the conclusion that there isn't any accumulation area on these glaciers since decades? If the latter would be the case the ELA should lie above the summit. Then it would not be possible to find any coring site above the ELA. Somehow this issue needs clarification.

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4) Counting annual layers backwards from the nuclear bomb horizon could imply that there are years without accumulation before 1982, in such that 1982 not necessarily needs to be the last year of positive accumulation. Maybe the last year with accumulation was later and there have been years without accumulation before 1982? Is it possible to constrain the Hg-records to better than +/- 10 years? Please at least discuss this issue.

5) Most of the time a DDF for snow will need to be used at high elevation sites that have almost permanent snow cover. DDFs of 3 to 8 mm/°C for snow seem to be reasonable to my knowledge, but certainly not DDFs above 10 mm/°C. Otherwise, please cite the references that justify a DDF for snow that is higher than 10 mm/°C. I think that the analysis of uncertainty regarding upper and lower limits of the melting according to the degree day modelling is not sufficient. You should provide three records of melting with the lowest, the middle and the highest reasonable DDF combined with the lowest, middle and highest reasonable temperature lapse rate - making up nine calculations at least. This would provide the range of uncertainty with respect to the DDM. However, the uncertainty is much larger because a DDM is only a rough estimate of the melting since it does not fully cover all relevant physical processes. Further, the uncertainty in the precipitation estimate must be stated more clearly. The plus in precipitation at a high altitude site compared to stations further down in the forelands can easily reach 100%! Do not just give ranges but provide the full range of data in a Figure or a Table. The data provided in Figure 8 is not sufficient for this purpose.

6) It may be a possibility that there were some warm years that removed the nuclear signal from the accumulation area while after that other years still had a positive mass balance. I understand that the Hg-record is a further indication that this is not the case. However, the temporal constraint of the Hg-record is not so clearly provided in the text. I would argue that the authors should more cautiously discuss any possible flaws in their chain of arguments so that the reader gets a better understanding of the reasoning behind the conclusion. There is a chance that overall the case is not quite

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as simple as it appears according to the manuscript. I believe it would strengthen the paper quite a bit if you could elaborate on this in more detail.

Smaller points that still need consideration:

P419, L11; P420 L15, P424, L26, P427, L20, P428, L6: replace “glacier decapitation” and similar wording with more appropriate wording – see my comment above

P420, L1: insert “the before “last decade”

P420, L9: “marker horizons” not “maker horizons”

P424, L6: "Fain" instead of "Fain"

P424, L17: "Hylander" instead of "Hyland"

P424, L25-27 and hereafter: The conclusions or rather generalizing statements based on only few measurement sites should be avoided. I would strictly limit the statement to findings refereeing to the investigation sites of this paper since individual glaciers in the same region may heterogeneously respond to climate forcing.

P425, L10: "in the order" instead of "on the order"

P424, L20: replace “tracks” with “matches” or “agrees”

P 425, L19: “Since 1995, the cumulative mass loss reached 5000 mm with an annual mass loss rate of about 300 mm w.e.”: What is the end date of the period in which mass loss piled up to 5000 mm?

P425, L25: replace “at” with “of”

P426, L3: skip the word “to” in “confirm to widespread glacier ...”. The statement anyway is a bit strong since from your study you can only draw conclusions for the two study sites. Maybe better say that mass loss is in “in agreement” or “consistent” with your finding but refrain from drawing an overall conclusion for the whole region.

P426, L20: skip “of glacier area” in “may occur at the higher elevations of glacier area

compared with . . .”

P226, L25: skip “the” in “according to the previous works . . .”

P427, L3: “mass loss” instead of “mass losing”

P427, L15: change to “of glacierS during the last decade ranging from THE Himalayas . . .”

P427, L16: “northwestern TP” instead of “northwestern of the TP”

Figures:

Fig.1: Number the three parts of the figure (e.g. a,b,c) and give proper explanations in the figure capture. Insert a color legend in the uppermost map. You should overlay altitude lines so that the reader can see the topography and general altitude. Please provide a coordinate system and glacier outlines for the glaciers interesting for your study in the two lower pictures. The lower right picture is of bad quality. Choose a better satellite image. Please use consistent naming of the glaciers (e.g. Xiaodongkemadi in the map and Dongkemadi in the figure caption).

Fig. 6: Clearly indicate in the figure caption if these are measured or modelled mass balance data. Change “Mt. Nyainqentanglha” to “western Nyainqentanglha Mts.”

Fig. 8: This figure gives no additional information because the numbers are already given in the text. I suggest removing this figure. The definition of Min and Max is confusing. Min should be more negative, but in your case it is the least negative (most positive) mass balance.

I am really looking forward to seeing the revised version of your paper.

Interactive comment on The Cryosphere Discuss., 9, 417, 2015.

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