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Interactive comment on "Glacier elevation and mass changes in Himalayas during 2000–2014" *by* Debmita Bandyopadhyay et al.

Anonymous Referee #2

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General comments

In this manuscript, the authors present the glacier elevation and mass changes in the Himalayas covering a period between 2000 and 2014. They derived elevation changes from digital elevation models such as SRTM-C band and TanDEM-X global DEM. The Himalayas, of course, is an area of great interest for many communities due to the relevance and role of glaciers as water supplies. In that sense, additional information and results are always very welcome, and we acknowledge the authors effort.

Overall, the study is presented in a very simple way (e.g. methods) that do not completely support the content in the abstract. I can understand the short description of the methodology due to the authors using two DEMs with global coverage. However, the present manuscript suffers from various conceptual limitations and methodological

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inconsistencies that do not provide assurance of the quality of the results. Therefore, it is my recommendation that this paper is not suitable for publication in its current form. The reasons are the following:

1) One of the key statements or motivation from the authors in this manuscript is that TanDEM-X "Global" DEM has been recently used to calculate glacier elevation and mass changes in South America by Braun et al., (2019). They (Braun et al 2019) did not use the TanDEM-X "Global" DEM. We have to make the difference here. Braun and colleagues (2019) processed hundreds of raw radar images (InSAR) to generate their own TanDEM-X DEM, concentrated in the ablation period. There are also many others studies dealing with TanDEM-X processing that carried out similar procedures (e.g. Necklel et al., 2013; Rankl et al., 2016; Dehecq et al., 2016; Neelmeijer et al., 2017; Vijay et al., 2017; Malz et al., 2018; Abdel Jaber, 2018; Rott et al., 2018). Neelmeijer et al., (2017), provide a clear overview of the processing chain of TanDEM-X in figure 3 or Dehecq et al, (2016) in figure 2. On the other hand, the TanDEM-X Global DEM is an effort from DLR (German Space Agency) to cover the entire globe with low and high-resolution DEM (12 to 90 m) with thousands of intermediate DEMs to generate this large globe DEM mosaic. Unlike of SRTM DEM (February of 2000), the dates for the composition TanDEM-X "GLOBAL" DEM mosaic is unknown or at least not easily found. Which means for the global DEM, we can find different intermediate DEM seasons. This led to one of the major uncertainties: what are the dates of the all intermediate DEM in Himalayas? However, although the TanDEM-X "Global" DEM is a very sophisticated DEM that provides a useful topography for many other fields, for glacier elevation changes calculations may lead to more uncertainties. Hence, I leave this point to the discretion of the editor.

2) I also agree with the reviewer #1 that the present manuscript is missing several previous studies either for the methodology description (e.g. Rankl et al., 2016; Dehecq et al., 2016; Neelmeijer et al., 2017) or for the study area (e.g. King et al., 2017; Brun et al., 2017). The most critical study is from Brun et al., (2017). Brun and colleagues

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(2017) calculated glacier elevation changes very close to the period of the present study (2000-2016). This is a pity, since the authors could compare their results using the same catchment/basin subdivisions and use the opportunity to compare it.

3) The description of the methods and uncertainties section, as I stated before, are not specific enough. I have a lot of doubts about the methodology and the interpretation of results that the authors present in this manuscript, along with the important confusion of the used materials. I will also give you some suggestions, however, substantial work will be needed.

1 Methods section

P2 L17 -> What do you define as rugged terrain? In some areas of the Andes is reaching almost 7000 m a.s.l.

P3 L10-15 -> it is very simplistic to state as 2014, where not further information is provided. As I mentioned above, please try to provide a realistic date to trace the results. I agree with reviewer #1 that the results are biased.

P4 L3-6 -> how was the radar signal penetration considered? It is not precisely described. I am not fully convinced with the values showed by the authors. Other examples dealing with X and C band penetration showed much more radar signal penetration (e.g Dehecq et al., (2016); Neelmeijer et al., (2017); Vijay et al., (2017) (see my suggestion below).

P5 L1-3 -> No detailed information about the hypsometry computation and error assessment.

P5 L10-19 -> There is information is missing in this section (a) no description for the NMAD method (see Höhle and Höhle, 2009). (b) What equation contains the total uncertainty of your study? (e.g. Vijay et al., 2017; Braun et al., 2019).

Despite the methodology is not precise. From what I have seen, the manuscript does not reflect the state of art regarding TanDEM-X assessment (e.g. Neelmeijer et al., 2017; Vijay et al., 2017; Malz et al., 2018; Abdel Jaber, 2018; Rott et al., 2018; Braun et al., 2019). Therefore, I suggest a complete re-analysis /re-organization, including in your analysis:

a. Uncertainty from the volume to mass conversion. Please use density scenarios (due to multi-seasonality/dates).

b. Uncertainty from radar signal penetration. Vijay and Braun (2016) showed that there is a strong altitude dependency of the radar signal penetration bias. They observed a range from 0.84 m (5000 m a.s.l.) to 3.64 m (5800 m a.s.l.). Since the date of season/year of TanDEM-X "Global" DEM is unknown I would use the worst scenario of radar signal penetration. A good example is in Braun et al., (2019), although they calculated glacier mass changes in the ablation period, they applied a radar signal penetration from 0 to 5 m from Equilibrium Line Altitude (ELA), considering negligible below to ELA. Other example is given by Neelmeijer et al., (2017). You should consider similar procedures.

c. Uncertainty by the hypsometry (please see Berthier et al., 2016; Brun et al., 2017; Vijay et al., 2016; Braun et al., 2019)

d. Error from the DEM differencing (please see Berthier et al., 2016; Vijay et al., 2016; Brun et al., 2017; Braun et al., 2019)

e. Error from the glacier outlines (please see Berthier et al., 2016; Brun et al., 2017; Braun et al., 2019)

f. Uncertainty by the dates. This point requires investigation/analysis. It would be good if the authors can get some originals intermediate DEMs of TanDEM-X "GLOBAL" to check some dates.

2 Results and discussion section

From the facts that I mentioned above, this section does not provide reliable results. Furthermore, the figures do not help too much. In the following I give you some suggestions that could improve this section:

-First, I would separate results and discussion, since there are some topics you have to properly discuss. e.g. A section on your error assessment with the proposed accuracy assessment methodology. Comparison with other studies and comparison with your glacier mass balance dataset (glaciological method).

-I also suggest you use the catchment/sub-division used by Brun et al., 2017 or Deheq et al., 2018 in order to have comparable numbers in your results and the discussion.

-In the last few weeks a couple of papers came out with new insight in this region. It would be good to include it (see: Zemp et al., 2019, Wouters et al., 2019; Maurer et al., 2019).

Figures

P6 Figure 5 -> I agree with reviewer #1. For such a big area I am not sure if this is a representative figure. Please also check Menounos et al., (2019) or Kääb et al., (2012) there are some useful figures that you could apply.

P7 Figure 2 -> with a better quality of figure 1 you can remove figure 2.

P9 and P10 Figure 4 -> some figures are not well represented in the main text. Principally in the discussion. For example, figure 4 d, f, and g. These hypsometric plots present patterns that should be discussed.

3 Reference

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