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

Anonymous Referee #1

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In their TCD manuscript "Glacier elevation and mass changes in Himalayas during 2000–2014" Bandyopadhyay et al. estimated glacier surface elevation changes in the Himalaya mountain range between 2000 and 2014. The authors build their analysis on a digital elevation model acquired in 2000 during the Shuttle Radar Topography Mission and on a more recent TanDEM-X DEM. This study would have had a great impact a couple of years ago, but I have the feeling that in its current form it is lacking novelty in both data processing and relevant glaciological finding. In line with this is the fact that recent and important publications on the topic are not cited. Furthermore, I found the manuscript rather hard to follow (I will not correct for language but the manuscript clearly needs some polishing) and have the feeling that there are several fundamental flaws in the data processing wherefore the manuscript can not be accepted in its

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Discussion paper

current form. I will give some suggestions in the following but I am not sure if the necessary changes can be made within a review. I am really sorry to not be more positive, but I think the authors need to do a proper literature study first in order to place their results into the current research context.

Introduction

This section needs some language polishing, furthermore some references are wrongly placed and some are missing.

Page 1 Line 29: maybe Pritchard (2019) would be a good reference here?

Page 1 Line 31: what about Brun et al. (2017)? This is one of the most important recent studies but is not cited at all.

Page 2 Lines 8-10: what about Lin et al. (2017)? They are using TanDEM-X SAR data for large parts of the study region.

Page 2 Line 20: to my knowledge Braun et al. (2019) did not rely on the TanDEM-X global DEM but process DEMs by themselves. Please double check.

Study area

I am not sure if the authors should use state boarders to separate their study areas. In order to compare results to other studies (e.g. Kääb et al. 2015, Brun er al. 2017) it would be advisable to use their sub-regions or grid the data. Even though the recent study of Maurer et al., 2019 became available after submitting the initial manuscript I very much like their way of presenting results.

Dataset and methodology

Overall I find this section hard to follow, but I presume the authors rely on the global TanDEM-X DEM rather than processing DEMs by themselves? This might be ok, but need to be done in a proper way. In the following I give several fundamental suggestions which need to be accounted for.

1.) Both the SRTM and TanDEM-X global DEM come with a lot of metadata such as error and coverage maps. For example, it is not sufficient to state that the TanDEM-X DEM is from 2014 as this is simply not true. Instead, the authors need to rely on the exact metadata when calculating yearly elevation changes otherwise the results are biased.

2.) There are many versions of the SRTM DEM available some are void filled and some are not. It is not clear which version and at which grid posting the data were used. The latter also applies for the TanDEM-X DEM. I further recommend to read the study of Mukul et al. (2017) to gain a better understanding of errors in the SRTM dataset over India.

3.) Page 3 Lines 21-23: radar penetration depth is a very important point which is widely discussed in the recent literature. It is not clear how the authors correct for this bias. I strongly suggest to read more recent studies dealing with this topic, focusing explicitly on TanDEM-X data (see for example Dehecq, A. et al., 2016, Vijay, S. et al., 2016, Neelmeijer, J. et al., 2017 Abdel Jaber et al., 2018 and Kääb et al., 2018). This point needs much more consideration. Although SRTM-X and TanDEM-X were acquired at the same wavelength, surface properties could still have been different in both years.

Page 4 Lines 1-2: Did the authors update the dataset by themselves? Not clear. How can the time period 2003-2009 be updated with data from 2000? Please clarify.

Page 5 Lines 1-3: how did the authors account for voids? Not clear but important. Please see also McNabb et al., 2019 on this issue.

Results and Discussion

I will not put too much effort into this section as I presume it will be change quite a bit after revision.

Figure 2: I am sorry, but I can not see much here... please use another form of pre-

senting your results, see also my comment on the study area. For inspiration have a look at Brun et al., 2017 or Maurer et al., 2019.

Figure 5: In order to gain a better feeling on the quality of the dataset it would be great to also show off-glacier elevation changes instead of cropping the elevation changes with a glacier mask (the same applies for Figures S2-S8).

Page 11: I very much like the idea to compare the results to in-situ mass balance measurements. However, I have the feeling more effort could be put into this. See also Fischer (2011) on this issue.

Page 11 Line 11: Please compare your results also to the estimates from Brun et al. 2017. Their results are available here: https://doi.pangaea.de/10.1594/PANGAEA.876545

Page 11 Lines 16-18: possibly true but this can be investigated further. Again please see McNabb et al., 2019 concerning the void issue and the TanDEM-X metadata concerning the time issue. Further, penetration bias and density assumption will have an effect and need to be discussed. Maybe this is a little bit beyond the scope of the study but how compare the results of Brun et al. 2017 to these in-situ measurements?

Conclusions

Page 14 Lines 12-15: this is not true. See for example Rankl et al. 2016, Lin et al. 2017 and Neelmeijer et al. 2017.

Page 14 Lines 19-20: I think this can be further quantified by investigating the metadata of the TanDEM-X DEM. As stated above Braun et al. (2019) did not rely on the global TanDEM-X DEM.

Page 14 Line 20: This is not true. If the authors calculate annual elevation changes between 2000 and 2014 but the correct end date is actually 2011 the results are significantly biased.

Additional References:

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