The Cryosphere Discuss.,
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The Cryosphere Discussions

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

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1.Introduction

Comment: Page 1 Line 29: maybe Pritchard (2019) would be a good reference here? #Reply: Reference seems to be published at a date after the submission of the manuscript and not relevant to the statement suggested for.

Comment: 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. #Reply: We thank the reviewer for pointing out the reference and the authors shall incorporate this in the manuscript.

Comment: 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. #Reply: The authors are trying to

highlight the usage of the freely disseminated global TanDEM-X data. Further, the reference cited by the reviewer used 39 pairs of TanDEM-X SAR data for interferogram generation, which is not the same dataset for the current study.

Comment: 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. #Reply: The suggestion of the reviewer is duly considered and the necessary changes will be made.

2.Study area

Comment: 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. #Reply: The idea was to highlight how each state is performing in terms of mass loss alongside easy comparison with Kääb et al.,2012 as they also performed certain regional studies in a state-wise manner (Table 1, page 497). Therefore, even though the reviewer suggests a different demarcation of region of study, we would like to keep the current state boundaries to highlight the issue, which holds utmost importance in an agrarian economy like India.

3. 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 suggestion which need to be accounted for. 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.

a) Comment: 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. #Reply: The exact date for the TanDEM-X DEM is not possible to state. In fact one DEM tile has multiple acquisitions with significant contribution from each year between 2011 and 2014. For example, in N32E79_DEM, there are 20 acquisitions from the year 2011, 15 acquisitions from the year 2012, 30 acquisitions in 2013 and 26 acquisitions in 2014. The baselines are varying between 95 to 200 and the scenes such that they cover the entire area each year, updating the DEM height information for entire area every year. So each yea, when the acquisition is made for months varying from February to December, the information in the DEM is updated, a mosaic of information provided in finished product. Therefore, it if difficult to provide a specific date. However, to make sure that the authors are not providing wrong information to the readers of this prestigious journal, we made sure to compare the results with reported literature, finding a good correlation of 0.79. Hence, proving that the result presented in this manuscript are not speculative but rigorously evaluated.

b) Comment: 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. #Reply: The TanDEM-X global DEM and version 2 of the SRTM DEM with 90m posting were utilized, which shall be clarified in the manuscript.

c) Comment: 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. #Reply: The radar penetration has been well addressed in the manuscript (section 1.2). However, the authors understand the concern and would like to bring to the notice of the reviewer that penetration of X-band is hardly 40cm considering the wetness (0.5% by vol.) snowpack covered glacier area for different seasons(Manickam et al., 2017; Singh et al., 2017). Himalayan glaciers have snowpack that have moisture throughout February- September [a few centimeter penetration though the snowpack at X-band]. Hence, this X-band bias shall not effect much after inclusion of DEM and penetration bias, which already has been performed in the current study.

d) Comment: 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. #Reply: The RGI inventory shape files for glaciers have been modified according to the period of study i.e. the year 2000. This clarification shall be made as per reviewer suggestions.

e) Comment: 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. #Reply: If >50% of the glacier is not covered in the dataset used, we discard the area and not use void-filling as the results would be significantly biased.

4.Result and discussion

a) Comment: Figure 2: I am sorry, but I cannot see much here: please use another form of presenting 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 #Reply: Gridded data as referred in Brun et al., have been performed over a 111x111 sq. km grid. The trend we show here is on 90m X 90m scale. Hence the disparity. Representing in suggested format might result in under-representing the regions we want to highlight.

b) Comment: 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). #Reply: Off-glacier elevation changes have been considered in the DEM bias corrections. Furthermore, the focus of the study is only the glaciated terrain, hence extracted in figures S2-S8. This representation is widely accepted in previous studies as well (Lin et al., 2017, Vijay and Braun, 2016 and 2018).

c) Comment: 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 Reply: The results in Brun et al, 2017 show basin-wise results which are more pertinent if HMA studies are performed. Cross-check with only one state i.e. Bhutan when performed differs from the result reported by 0.02 m.w.eq. per year.

d) Comment: 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? #Reply: Brun et al, 2017 mentions mass changes for basins which are beyond the region of interest in current study and thus it is not advisable to use for comparison of results. Further, the density assumption is well-established (Huss (2013)) and used by Gardelle et al. (2011),Vijay and Braun (2016).

5.Conclusion

a) Comment: 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. #Reply: The authors thank the reviewer to point this out with reference. Necessary changes will be made in the revised manuscript.

b) Comment: 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. #Reply: This suggestion has been answered in Comment 1 (in Dataset and methodology).

c) Comment: 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. #Reply: The acquisitions are till Jan 2015 (1-2 acquisitions per DEM as opposed to 20-30 acquisitions of previous year) with the complete year of 2014 covered.

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