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

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1.Comment: 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 oth- ers 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

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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 eas- ily 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 in- termediate 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. ##Reply## The consistent concern of uncertainty due to global DEM utility shown by both the reviewers is certainly noted, but it needs to be understood that the global DEM product used is a finished product devoid of much bias as mentioned by DLR with a vertical accuracy up to 2m. Further, to address the issue raised, we have performed the same study over South America as done by Braun et al, 2019 using the TanDEM-X global DEM, and the results are comparable (-0.38 $\pm$0.08 m a-1 for Tierra del Fuego region).Figure attached for reference(Fig. 1). Further, the processing chain for TanDEM-X discussed in references (e.g. Neelmeijer e al., 2017 and Dehecq et al; 2016) are not in the scope of the paper as we utilize finished products, kindly provided by DLR.

2.Comment: 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 ##Reply## The methodology description has been followed using Gardelle et al.,2013, Vijay and Braun,2016 . Description of study area can be improved as per suggestions of the reviewers.

3.Comment: The description of the methods and uncertainties section, as I stated

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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. ##Reply## Uncertainty analysis of DEM, glacier outline and hypsometry have been performed while carrying out the analysis and incorporated in the total mass balance calculations as per Braun et al., 2019 (Page 5,Line 15). The formula is mentioned in the reference, hence a detailed description of the same was thought to be unnecessary.

4.Methods section:

a)Comment: P2 L17 -> What do you define as rugged terrain? In some areas of the Andes is reaching almost 7000 m a.s.l. ##Reply## Rugged terrains of Himalayas are a well-known characteristic (Pandit et al., 2014; Scherler et al., 2011; Tawde et al., 2017), hence reiterated in the manuscript.

b)Comment: 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. ##Reply## Considering the reviewer's suggestions, we found that each DEM tile has a unique set of acquisition dates which encompass ablation as well as accumulation months. For. E.g. Tile N30E79 which falls in Uttarakhand has been generated using 119 scenes both in ascending and descending pass within the time period of Feb 2011- Sept 2014. Hence, for the TanDEM-X DEM product used providing a specific date apart from the end date of acquisition is beyond the scope of this paper.

c)Comment: P4 L3-6 -> how was the radar signal penetration considered? It is not precisely de- scribed. 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 penetra- tion (e.g Dehecq et al., (2016); Neelmeijer et al., (2017); Vijay et al., (2017) (see my suggestion below). ##Reply## The values have been compared to reported

literature as that of Gardelle et al., 2013 (Please refer to Table 1).The references cited by the reviewer have a separate study on altitudinal distribution of radar penetration for a limited number of glaciers. As we present a state-wise mean penetration bias, the altitudinal variation might be moderated but not unaccounted for.

d)Comment: P5 L1-3 -> No detailed information about the hypsometry computation and error assessment. ##Reply## Altitudinal distribution/hypsometric computation is a well-known process performed previously (Bhushan et al., 2018; Braun et al., 2019; Vijay and Braun, 2016) without the description of the computation.

e)Comment: 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). ##Reply## The uncertainty assessment has been performed as per the description in Braun et al., 2019, which has already been mentioned in the manuscript. The details of NMAD was not mentioned as there are many references which explain the same (Dehecq et al., 2016 and Höhle and Höhle, 2009) However, if the reviewer insists, the relevant references for the readers' benefit shall be added.

f)Comment: Uncertainty from the volume to mass conversion. Please use density scenarios (due to multi-seasonality/dates). ##Reply## Volume to mass conversion uncertainty can be calculated for two different scenarios (850$\pm$60 kg/m3 and 900$\pm$60 kg/m3) to account for alpine glaciers and glacier ice following Braun et al., 2019. But even with different scenes Brun et al., 2017 seem to have used the well accepted value of 850$\pm$60 kg/m3 (please refer P669).

g)Comment: 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

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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. ##Reply## A state wise mean penetration bias has been considered for the glaciated terrain (Gardelle et al., 2013) as we estimate mean mass balance (state-wise). However, the cited references, apart from mentioning the altitudinal dependency, do not provide details of utilizing elevation based radar penetration in their calculation. Further, there is no available physical method to estimate and correct the penetration biases from the radar signal sources. It is just an empirical approach adopted in pertinent literature (Gardelle et al., 2013) .

h)Comment: Uncertainty by the hypsometry (please see Berthier et al., 2016; Brun et al., 2017; Vijay et al., 2016; Braun et al., 2019) i)Error from the DEM differencing (please see Berthier et al., 2016; Vijay et al., 2016; Brun et al., 2017; Braun et al., 2019) j)Error from the glacier outlines (please see Berthier et al., 2016; Brun et al., 2017; Braun et al., 2019) ##Reply## Uncertainty and error analysis of hypsometry, DEM and glacier outlines has been performed as per Braun et al., 2019 and shall be clarified in the manuscript.

k)Comment: 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. ##Reply## Intermediate DEM of the TanDEM-X "global" DEM as per the reviewer's suggestion was pursued. For one tile for example, 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. So each year the acquisition is made for months varying from February to December, the information in the DEM is updated, hence a mosaic of information provided. Therefore, it if difficult to provide a realistic date. However, to make sure that the authors are not providing wrong information to the readers of this prestigious

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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.

5.Results and discussion section:

a)Comment: 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). ##Reply## Authors thank the reviewer for the suggestion but would like to clarify that there is no proposed error assessment methodology. In fact, the errors estimated have been performed as suggested in Braun et al., 2019.

b)Comment: 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. ##Reply## The authors thank the reviewer for suggesting a catchment/sub-division study for the current study area, however, the region of study shall partially cover such regions of interest for a comparative study with that of Brun et al., 2017. In Dehecq et al., 2018, only the velocity measurements are provided. Further, for a comparative analysis, only value for Bhutan is available which can be incorporated in the revised manuscript.

c)Comment: 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). ##Reply## We thank the reviewer for brining to our notice these recent literature and shall duly consider the information that can be extracted from them.

6.Figures: a)Comment: 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. ##Re-

ply## For representative purpose, Figure 2 suffices. Figure 5 is just a magnification for some benchmark glaciers on which a lot of study has previously been done and so the readers will be able to relate better.

b)Comment: P7 Figure 2 -> with a better quality of figure 1 you can remove figure 2. ##Reply## This figure is not comparable to Figure 1 such that it can be replaced. Figure 1 shows the study area extent whereas Figure 2 shows the trend of ice-thickness change over the entire region of interest.

c)Comment: 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 ##Reply## The authors shall elaborate the discussion on Fig 4d, f,g if required. However, the major findings from the hypsometric plots have been sufficiently mentioned in manuscript (please refer P7,L 20-P8,L30)

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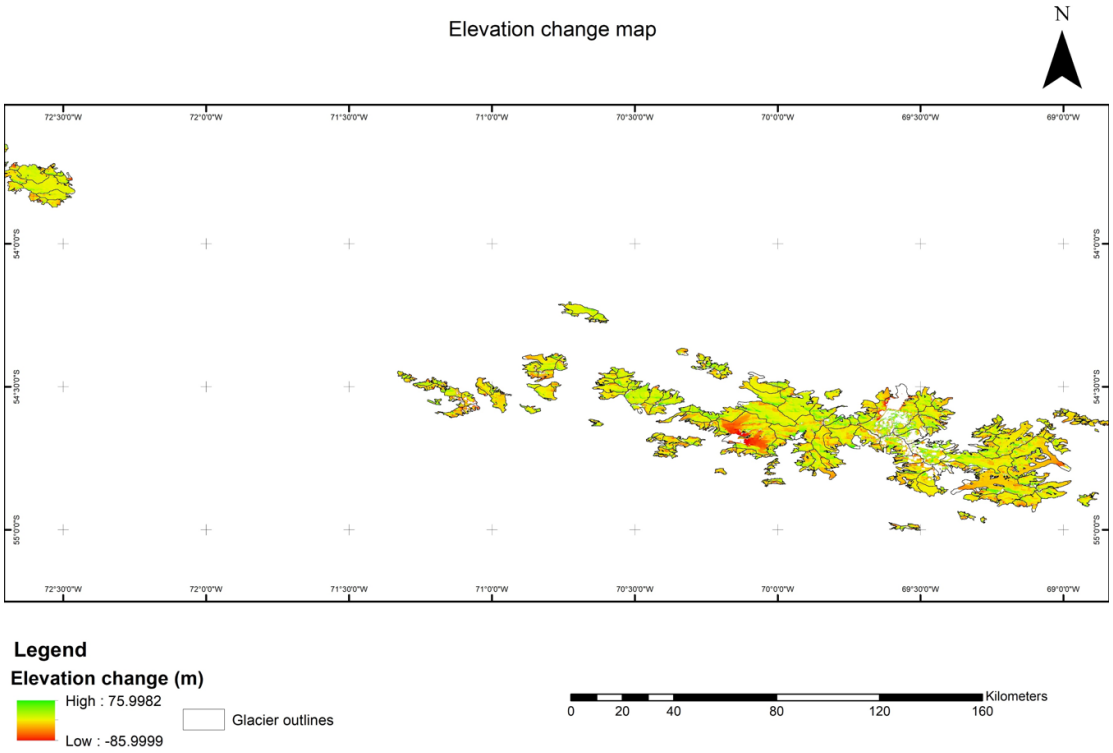

Fig. 1.

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