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The Cryosphere Discussions

Interactive comment on "Substantial meltwater contribution to the Brahmaputra revealed by satellite gravimetry" *by* Shuang Yi et al.

Anonymous Referee #2

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This study evaluates the contribution by meltwater (Glacier Melt + Snowmelt) to the total runoff of the Brahmaputra river basin. By employing monthly observations of time-variable gravity from the NASA/DLR GRACE mission, the authors try to partition the total runoff (water transfer in the form of river streamflow from the upstream to the downstream areas of the basin) between by glacier melt, and direct runoff from precipitation. According to the methodology proposed in the study, given the different phases characterizing the annual variation of snowpack, glacier, and terrestrial hydrology, their contribution to temporal changes in terrestrial water storage in the region can be separated by employing an Empirical Orthogonal Function analysis approach. To validate their findings, the authors employ data from independent ground- and satellite-based observations like glacier mass balance estimates from the NASA ICESat mission and

monthly precipitation from the NASA/TRMM project and the HAR reanalysis model. Finally, the authors compare seasonal changes in glacier mass with temperature from 4 atmospheric stations available within the region in order to evaluate the sensitivity of glacier mass balance to changes in temperature. I find that the study fits the scope of the journal and that the approach presented here of interest for the scientific community since findings by previous studies are in large disagreement and characterized by large uncertainties. However, the author's claims are not completely justified by the results presented here. I will present my main observations below:

1) The study focuses on the Brahmaputra river basin. However, the results presented here are relative to the eastern side of the catchment (the authors explicitly refer to the mass balance of the Nyenchen Tonglha Mountains and South-Eastern Himalayas glaciers). The glaciers located on the West are never even mentioned in the article. Even though the total area of these glaciers is significantly smaller than the area of glaciers situated in the east, the effect of their mass change on the final estimates should be considered in the total budget. Note also that the exclusion of these glaciers in the presented evaluation could influence the results of the comparisons with previous studies. The authors should at least explain the reasons for their exclusion from the analysis should be discussed in the article (e.g., given that GRACE coarse resolution, the signal originated from this side of the basin can hardly be separated from signals originated within the surrounding regions).

2) Climatological data: to prove their claims, the authors employ data from 4 meteorological stations available in the region and precipitation estimates from TRMM and HAR.

2a)In the case of the meteorological stations, their distribution is not sufficient to provide an evaluation of atmospheric temperature variability at a regional scale (considering the large variability of local relief in the area). Data from stations should be used with caution in the evaluation of the gridded datasets given their intrinsic bias toward low elevations and underestimation of solid precipitation.

2b) Regarding the gridded datasets used here, the authors briefly mention the limitations of these two data products (Underestimation of solid precipitation in the case of TRMM and the presence of long-term biases in precipitation trend in the case of HAR). I think that adding other datasets to the analysis could help to make the analysis more robust and help to assess the uncertainty associated with these estimates. Why not using outputs from gridded temperature datasets like APHRODITE, ERA-Interim, etc. ?

3) Validation of Mode 1: why is only soil moisture from GLDAS used to validate this mode1. The contribution of the other TWS components in the model (snowpack and canopy) should be included in the comparison. In any case, groundwater would not be considered in the equation. Figures 3 and 5 show a significant difference in amplitude between GRACE and GLDAS. The discussion would probably benefit from a picture showing the monthly time series from GRACE and GLADS. Note also that the negative trend observed in this mode can't be attributed only to decreasing precipitation.

4) GS mass estimation from mode 2: figure 4e shows that the mask used to extract the glacier mass change signal exceeds the basin boundaries. Therefore, the glacier mass change time series presented in the study is representative also glaciers outside the river catchment. Is this approximation considered in the uncertainty evaluation?

GRACE Processing:

- As briefly mentioned before, the main limitation with using GRACE in the in this region is that considering the coarse resolution of gravity observations, the GS signal from the Western side of the upper basin can't be resolved or separated from the signal relative to the upper basin of the Ganges river on the South and Tibetan Plateau on the North. The authors focus their analysis on the eastern side of the basin not providing, in this way, a complete evaluation of the glaciers and snow contribution to the total runoff. This limitation in the presented analysis should, at least, be discussed in the paper or in the discussion section. A possible solution could be to consider the effect on river

runoff only on the NTM glaciers which is what the authors actually do. This limitation should be discussed in the article.

- Considering the standard of 6 gravity field solution seem to be a simplistic approach to evaluate the uncertainty affecting the TWS anomaly measurements. Error terms like the GRACE Measurement Error and Leakage from hydrological and glaciological signals originated from regions surrounding the region of interest should be considered.

- In the case of the leakage error, the authors discuss only the effect of the signal leaking between the two main EOF modes (see supplementary material). At the same time, more attention should be paid to the impact of signal leaking from other regions that, in this area, is not negligible. See Anthropogenic water depletion over the Indian Plains and TWS changes of other ice-covered regions of Western and Central Asia. This effect is non-negligible not only when talking about long-term variations but also when analyzing the seasonal changes in TWS and can significantly affect the results of the presented analysis.

- Considering that the authors use GRACE to quantify the average annual contribution by meltwater to the total river runoff, the effect of the application of different smoothing strategies should be quantified or at least discussed.

- I would add the error estimation section, available in the supplementary material, to the main text.

Average summer contribution by GS to the total river runoff: In the final section of the article, the authors use the glacier mass loss measured by GRACE during the summer months to estimate the average contribution by meltwater to river discharge. This approach does not consider the effect of evaporation and other hydrological processes that should be accounted for in this evaluation. The entire evaluation should be, therefore, reviewed. Also, the comparison with Lutz et al. should be considered with caution since, as discussed above, the glacier areas considered in the two studies are different.

Minor Comments:

Line 13: using retreating instead of reducing would probably work better.

Line 15: low temporal resolutions of what?

Line 16: We find that the “spring-accumulation” . . . Rephrase

Line 26: I would change regarded to considered.

Line 27: “The sustainable . . .” rephrase

Line 39: change calibrated by streamflow to calibrated by employing streamflow data.

Line 50: Rephrase.

Line 57: Observations at a monthly temporal resolution. . .

Line 75: Rephrase

Line 136: “The method of this study. . ..”: rephrase.

Line 138: Using rain gauges to compare winter summer precipitation could be a risky approach considering the intrinsic underestimation of solid their intrinsic bias toward low elevation.

Line 156: more or less ???

Line 160: The authors discuss the difference in moths of seasonal changes between the northern and the southern side of the basin as a proof of the orthogonality between the signal associated with glacier and terrestrial hydrology temporal changes. In order to prove this claim, the author could perform the same analysis on the regions located on the western side of the Tibetan Plateau where an even more massive presence of glaciers and a minor exposition to monsoonal precipitation should show the same variation pattern.

Line 175: What is the effect of TWS mass loss in the Indian Plains region on this

negative trend? Can this mass loss be attributed just to glacier mass balance?

Line 190: the glacier mask presented in figure 4e covers glaciers outside the river catchments. Is this considered in the uncertainty evaluation?

Line 200: The methodology used to cumulate monthly precipitation data should be clarified.

Line 220: The same as line 200 – this is an assumption that the authors should prove with further evidence and provide them with a proper evaluation of the relative uncertainty. The trend observed here is determined also by the groundwater depletion observed in other studies,

Line 225: what is the value used as “glacier density” here?

Line 249-246: As mentioned above, the numbers from Lutz et al. can’t be really compared with the number presented here.

Line 254: From where does the -6.5 Gt come from? Please clarify.

Section 5.2 A high correlation between summer mass loss and atmospheric temperature is expected but what is the effect of other climatic variables on the interannual variation of glacier mass balance?

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