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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 #1

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The manuscript aims to estimate snow/glacier melt in the Brahmaputra river basin. Considered that different water storage components (snow/glacier vs. soil moisture etc) tend to have different spatiotemporal signatures, the authors apply EOF analysis on the GRACE data to extract these signatures. The underlying hypothesis is that the two dominant EOFs separate snow/glacier mass balance from the other hydrological components. The manuscript also explores the correspondence between energy input (temperature) and the estimated snow/glacier mass balance. The study fits the scope of the journal. The robustness of the analysis mainly depends on the validation of the underlying hypothesis, which needs to be strengthened.

The authors validate their underlying hypothesis by analyzing (a) the correspondence between mode-1 and modeled soil moisture estimates, (b) how a phase difference of

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Discussion paper

certain magnitude between two modes leads to their orthogonality, and (c) the correspondence between mode-2 and the ICESat results. In my opinion, the logic of (b) is questionable and not necessary. The result from (b) is solely determined by the orthogonality of the sinusoidal functions (as one of the modes are fixed as a cosine function in the analysis), and it does not address the physical meanings of these functions. Instead (a) and (c) should be the focus of the validation. For example, the use of soil moisture alone in (a) needs to be justified. The notable mismatch in terms of magnitude and pattern between Fig. 3a and Fig. 5c needs be addressed. Note that the large signal in Fig. 3a likely results from a combination of water demand from irrigation and a decrease in precipitation. How about using detrended time series for the EOF analysis, would it improve the agreement?

Detailed comments:

Line 91. Note that A et al., 2013 does not include a Little Ice Age model. Not accounting for the post-LIA GIA signal will likely affect your results, especially trends.

Lines 94-95. This is an oversimplified treatment of GRACE error. Common sources of error in GRACE application (e.g. measurement error, GIA uncertainty, leakage, etc.) have all been formally treated in the literature, and they should be considered in the study.

Line 100. Is this study focused on the glacier area? References used in the introduction sample both the upper and the entire basin (Lutz et al., 2014 vs. Huss et al., 2017). I think it is better to clarify the study area in the introduction. This could have implication for the snow and glacier mass balance calculation and for the underly hydrological regimes (e.g. mass vs. energy input limitation) that govern meltwater variability.

Line 151. Note that the spread of precipitation estimates (Fig S5) is quite large.

Lines 190-191. This seems to assume that all mass changes occur in the glacier area, but the snow cover (therefore the snow mass change) extends beyond the glacier area.

The rationale of this treatment needs clarification. It is also unclear if this treatment will introduce leakage.

Line 197. Hydrological components such as surface water and groundwater are not considered here. The rationale needs clarification. It is unclear to me why precipitation is included in the comparison given that precipitation affects both snow/glacier mass balance and other water storage components. Precipitation estimates are also known to be uncertain in this area.

Lines 211-215. The logic here is questionable (see my earlier comment on validating the methodology).

Lines 231-232. Showing the seasonality of the second mode in the GRACE series might help with this argument.

Line 249-250. This is a bit confusing. Are you accumulating the GRACE anomalies? These anomalies are state variables, and the difference of the anomalies (between the start and the end of each of these periods) should provide the mass change estimates. Please clarify.

Line 255. This statement seems important but not well developed. What impact, specifically?

Line 259. Using the average temperature from four meteorological stations might cause a representativeness issue. This should be either discussed or addressed in the manuscript. How about temperature from reanalysis, if possible, backed by a comparison with the station data?

Line 304. What does the realistic GS melt refer to? Please clarify.

Lines 307-310. This argument needs some clarification. How are these numbers derived, the 2nd EOF from GRACE? Note that Lutz et al. partitioned runoff while this study calculated mass balance. Are you assuming all of the summer mass changes contribute to meltwater (without evapotranspiration)?

Lines 311-316. Note that this manuscript and the referenced studies (Lutz et al. and Huss et al.) focused on different study domains. Would that cause inconsistency in snowmelt estimates?

Technical comments:

Lines 202-203. Why not use detrended cumulative precipitation?

Line 212. Should specify SI text 3.1 and Figs S9-10 here. Incidentally, I notice there is a discussion about error in the supplementary material. They should be referenced in the main text.

Line 485. Should be (b, d) instead of (column, d).

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