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

Anonymous Referee #3

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General comments:

This study aims to estimate glacier and snow mass balance in the Upper Brahmaputra River basin in China using GRACE data with the empirical orthogonal function (EOF) method and then the results of glacier and snow mass changes were compared with estimates from ICESat. The authors also demonstrated that spring precipitation-driven glacier/snow mass changes can be detected by GRACE. This topic would be of interest to readers of this journal. However, the method used in this study lacks novelty and the conclusions offer no new insight into this topic. Also, the authors need to pay attention to a lot of language issues in the manuscript.

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Discussion paper

Specific comments:

1.The Abstract and much of the manuscript are not associated with the title's emphasis of meltwater contribution. It seems that the manuscript is much focused on glacier mass balance. Only summer meltwater contribution was shown in the abstract (line 21) and it is not expressed as a percent which prevents readers from direct comparison with other studies.

2.The introduction could be improved (lines 38-42) with some more up-to-date literatures. Many studies estimate contributions of seasonal meltwater using modeling approaches. The hydrologic model was not only calibrated by streamflow but also by other relevant water component products. Furthermore, hydrologic modeling could provide meltwater time series with much higher temporal resolution than GRACE data. The reason why this study used remote sensing data to calculate meltwater contributions needs to be articulated.

3.Why did the authors use TRMM and HAR to analyze precipitation? In many studies on the evaluations of remote sensing precipitation products, TRMM did not perform well on the Tibetan Plateau. Figure S5 shows that both TRMM and HAR cannot capture spring precipitation well. In addition, the precipitation in spring is from March to May, not January to March (line 144).

4.There are large uncertainties in glacier mass changes derived from ICESat. For example, glaciers in 2009 should have been melted substantially, but the results showed a positive balance (Figure 6). Why did you compare the glacier mass balance derived from ICESat that involves large uncertainty with that estimated from GRACE?

5.Line 176: I don't think it's the reason of "the first mode is much stronger than the second one". The first mode is stronger because it explains the larger portion of

the total variance.

6.Lines 175-177: I cannot see that these two modes are comparable on both seasonal and secular temporal scales. Please give more explanations.

7.Lines 182-184: I think it is too sloppy to conclude that the first mode represents hydrologic signals and the second mode represents glaciers. More solid evidence should be provided.

8.Line 195: Why did the authors choose soil moisture and precipitation data sets to validate mode 1? Maybe air temperature is also strongly correlated to mode 1.

9.Line 203: I am puzzled by the weights (0.4, 0.6, 0.8, and 1). Why don't these values add up to one? Some citations should also be provided although the weights are determined empirically.

10.Line 217: "Atmosphere contribution has already been removed from GRACE observations. . .." This should be more clearly explained.

Technical corrections:

11.Figure 1: This figure is not clear enough to read.

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