Below, ‘Q’ is the question/comment, ‘R’ is our response, and ‘C’ is the revision in the manuscript.

R0:

The reviewer raised two major concerns. The first one is that a sinusoidal function is unsuitable for glacier and snow mass change. We add a semiannual variation to improve the simulation. We show that the new model is physically reasonable and accurate enough, and the conclusion still holds. The second one is why there is a big difference in amplitude between the EOF_1 results of GLDAS and GRACE. We explain that it is an inevitable result due to the exclusion of unavailable surface water and groundwater in the GLDAS product.

Q1:

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 certain magnitude between two modes leads to their orthogonality, and (c) the corre- spondence 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.

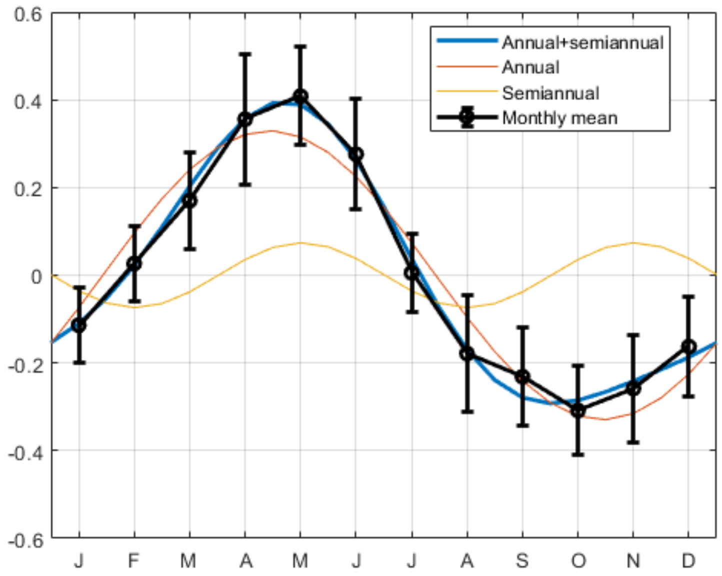
R1:

We feel the hypothesis (b) is very important for two reasons. First, it is the prerequisite for signal separation. Another reviewer suggested us to use the same technique in the west Tibet where the winter westerly is stronger and the summer monsoon is weaker. It cannot be implemented because both hydrological and glacial signals peak in winter, so the orthogonality disappears there. Second, the simulation enlightens the possible leakage between different signals and it is necessary for the uncertainty estimation.

We will update the simulation with an extra semiannual variation and address its physical meaning.

In the previous version, we only use a 1-year-cyle sinusoidal function (the orange curve, refer to Fig. 1) to simulate the temporal pattern of glacier melting. The simulation could be improved by another 0.5-year-cycle sinusoidal function (the yellow), as shown below. Periodic functions with periods shorter than 0.5-year can further improve the fitting to the monthly GRACE PC_2 series (the black curve with error bars), but we can tell that there is little room left for the improvement. A consequent question is how to determine the amplitude and the phase of the semiannual variation, and the answer is the information from GRACE PC_2.

Using GRACE PC2 series to find the monthly changing pattern is self-verifying, but we have no information more accurate than this. Below we will discuss the rationality of the new semiannual variation.



[Fig. 1. Seasonal variation of the PC2]

Here, we express the amplitude of annual and semiannual variations as A1 and A2, respectively. Their corresponding phase is P1 and P2, respectively. In fact, the relationship between As and between Ps can be determined by the varying form of their constituent signal.

As for the glacial mass signal, we have such information: its increase and decrease processes are asymmetric; it is slowly increased in winter, fast increased in spring (due to the spring precipitation), and then drastically decreased in summer. Based on this information, we can conclude that A2 is no more than 0.25*A1, and P2 is slightly larger than P1.

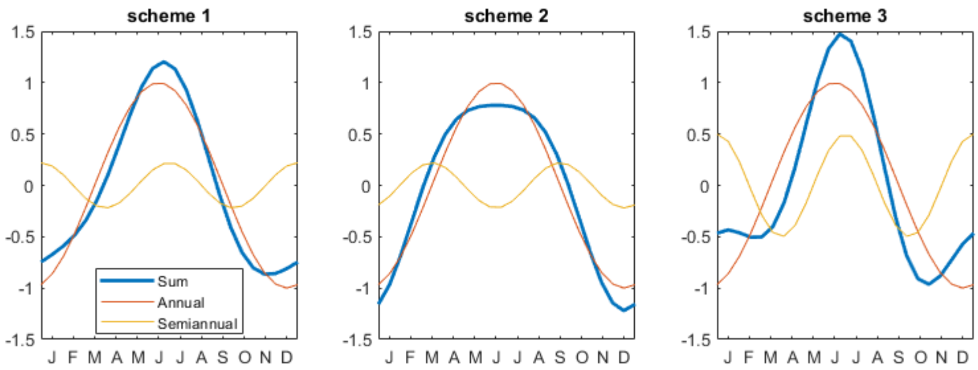
We explain it by showing three schemes of As and Ps below [refer to Fig. 2].

Scheme 1: A2 = 0.22*A1, P2 = P1+half-a-month; this is what we get from GRACE PC_2. The shape of the series matches our understanding of glacier mass change here.

Scheme 2: A2 = 0.22*A1, P2 = P1+three-month; this is the case when P1 and P2 are quite different. This signal is symmetric with a flat peak and a sharp trough (similarly we can construct a signal with a flat trough and a sharp peak).

Scheme 3: A2 = 0.5*A1, P2 = P1+half-a-month; this is the case when A2 is larger. As a result, the signal has double peaks in a year.

The signals by scheme 2 and 3 are unreasonable to explain the glacier and snow mass change here. We then have a good reason to follow the relationship between annual and semiannual variation obtained from GRACE PC_2.



[Fig. 2. Three schemes for simulation of seasonal variations]

With the extra semiannual variation, the leakage error is little changed (relative difference is 3%). If higher frequencies are included for a more elaborate seasonal variation, the influence is expected to be negligible considering their even smaller magnitude. Therefore, we update the simulation with more accurate changes in glacier and snow mass, and the conclusion still holds.

C1:

[We updated the simulation in the supporting materials based on the information show above]

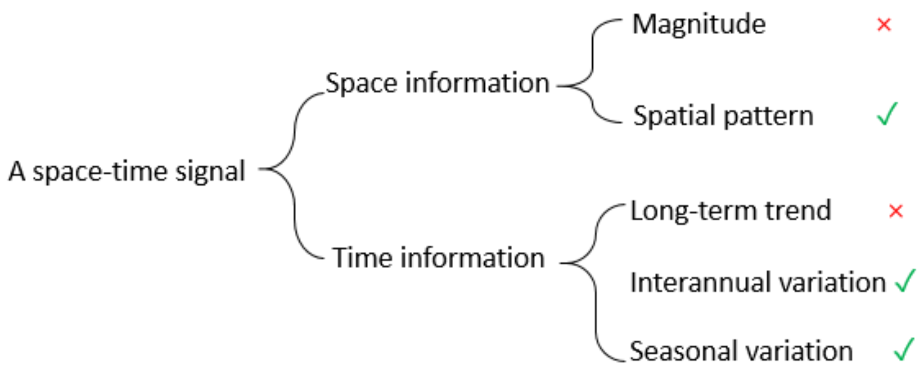
Q2:

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?

A2:

Terrestrial water storage consists of soil moisture, surface water, snow and groundwater. For the explanation of the first mode, we should also include surface water and groundwater. However, the surface water is difficult to estimate due to the changeable boundaries of the braided river, and the groundwater storage is also difficult due to its invisibility.

A space-time signal can be separate into five aspects as below [Fig. 3]. Because the absence of surface water and ground water, we cannot match the magnitude and trend of the signals and that is what we avoided to do. The agreement in the other three aspects can also support the explanation of the first mode, although not completely. That’s the reason we adopted two products for comparison. In this sense, the mismatch in magnitude is inherent, and the detrended EOF analysis is not helpful.



[Fig. 3. Five aspects of a space-time signal]

If we have all the five pieces of information of land water storage change, it means that we can fully quantify it. In this situation, this study would be straightforward—we just need to remove this information from GRACE observations and the residuals would mostly be glacial and snow signals.

C2:

[We have stressed this problem in the manuscript:]

The exclusion of unavailable surface water and groundwater in the GLDAS result also causes a weaker strength of its EOF_1 compared to that of GRACE.

Detailed comments:

Q3:

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.

A3:

Considering the large uncertainty of the Little Ice Age model, we only added it in the uncertainty estimates. The result of uncertainty estimates is moved from the supporting materials to the manuscript in this version.

Q4:

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.

A4:

We have considered different error sources in the supporting materials for the long-term trend estimate (this part is moved to the main text). As you may find, many error sources are negligible in the seasonal variation, so we only consider three error sources: data solution, smoothing techniques and leakage (previously we only considered the former two sources). Therefore, we used 6 combinations of datasets/filters to estimate the monthly uncertainties and the leakage effect on seasonal variation is estimated to be up to 11% (based on simulation results). A more comprehensive uncertainty estimate is given on the long-term trend.

C4:

[We move the uncertainty estimation for the long-term trend from the supporting materials to the main text. We add an extra leakage error of 11% to seasonal variations].

Q5:

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.

A5:

The glacierized zone only exist in the upper basin, so their estimates of absolute melt water are comparable in these studies. We have added a sentence to explain the situation here.

C5:

[We also addressed our study region in the end of the introduction:]
Then we will use the empirical orthogonal function (EOF) analysis to decompose the hydrological and GS signals in our study region, where 85% of the meltwater runs into the Brahmaputra and 83% of glaciers in this basin locate.

Q6:

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

A6:

The spread reflects the standard deviation of values in the same month in different years, so it indicates that the seasonal pattern is not so stable. The long-term monthly average reflects the tendency of precipitation pattern. We have clarified this in the caption.

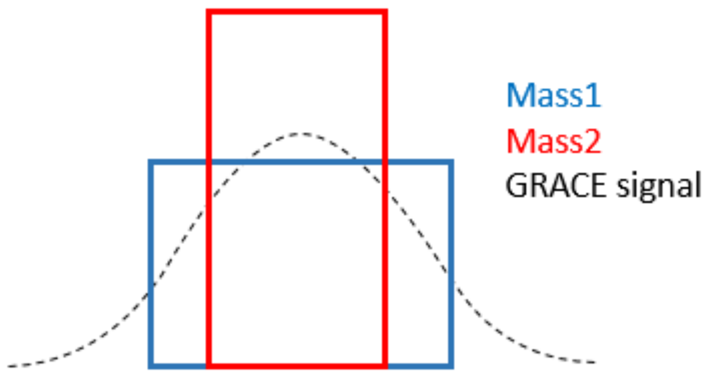
Q7:

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.

A7:

This step will not cause leakage, since the glacial signal has already been extracted by the EOF decomposition. A different mask for mass inversion does not influence the total mass either, as shown below [Fig. 4]. The gravity signals of mass1 and mass2 (mass1 equals mass2 in total mass) fit the GRACE observations equally well and GRACE cannot decide which one is better due to its poor spatial resolution. But we usually only care about the total mass. Since the snow area varies in different years and seasons, it is safe to use the glacierized area.



[Fig. 4. A GRACE signals fitted by two different mass distributions]

We add one explanation in the manuscript:

C7:

By this way we also assume the snow signal comes from the glacierzied area, but it does not influence the total mass estimates.

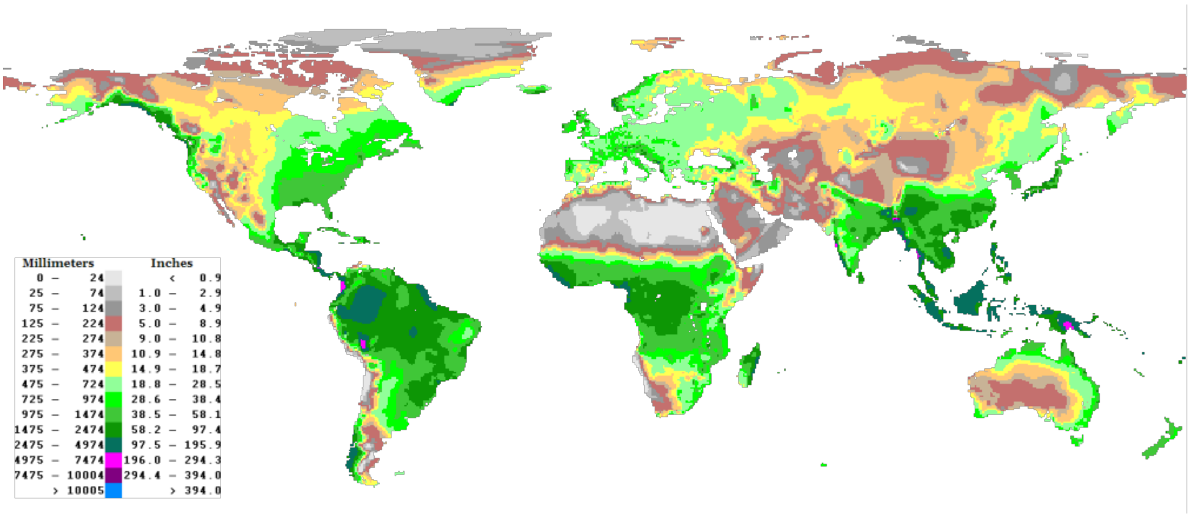
Q8:

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.

A8:

As explained above, surface water and groundwater are excluded due to their unavailability. The precipitation in the study region is one of the strongest in the world (see the map of global precipitation shown in Fig. 5, the dark green region), and the consequent result is that the water storage change, driving by the tremendous precipitation, plays the dominant role in the gravity change here. We could not give a full estimation of water storage change, so we did it in an indirect way: we chose a main component of TWS (soil moisture accounts for over 50% of TWS change) and its major driving force (i.e., precipitation).

The precipitation here can be separated into the widespread monsoon-caused summer one and the regional spring one. The former is so strong that it has a very high signal-noise-ratio. We feel this result is quite reliable since different precipitation products give high agreement. This is the driving force of the mode 1, so we think it is reasonable to validate the mode 1 by it. We agree that precipitation products cannot well capture the regional spring one here, which is also a conclusion given in the manuscript, but this regional uncertainty does not influence the reliability of the overwhelming monsoon-caused precipitation. Even if the precipitation product can well reflect the spring one, which greatly influence snow/glacier mass balance, the spring precipitation should be mostly in the second mode.



[Fig. 5. Map of global precipitation. (Image source: https://www.eldoradoweather.com/climate/world-maps/world-annual-precip-map.html)]

Q9:

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

A9:

Here we discussed the orthogonality in hydrological and glacial signals. This problem has been responded above.

Q10:

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

A10:

Here we wrote that although the snowfall begins in September (as MODIS data indicates the snow area increases since then), the total mass does not increase much because of small snow heights. The seasonal variation from GRACE PC_2 is given in Fig. 1 in this file, we can only see the total mass increases moderately in the winter and then rapidly in the spring.

Q11:

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.

A11:

Here, “monthly anomalies” mean the states relative to the long-term mean, while “monthly change” means the difference between two successive months. It has been rewritten for clarity.

C11:

We calculate annual mass increase and decrease by differencing monthly mass anomalies between November and May and between June and October, respectively.

Q12:

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

A12:

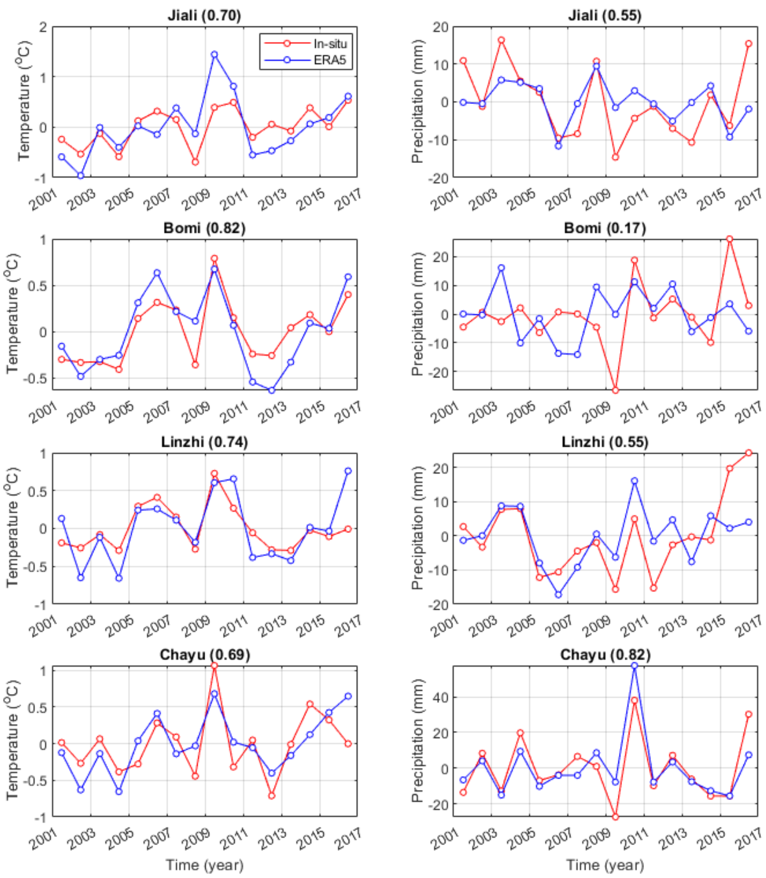
The impact is on the amount of streamflow. It has been rewritten to “Without the buffering effect of the seasonal variation, there will be a tremendous reduction in the streamflow in summer and autumn, …”

Q13:

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?

A13:

Thanks for your advice. In this reversion we adopt temperature data from the ERA5 dataset by ECMWF (https://www.ecmwf.int/en/forecasts/datasets/reanalysis-datasets/era5). The dataset is compared with the station observations. The correlation in temperature ranges from 0.69 to 0.82, indicating the reanalysis temperature data is reliable here. However, the precipitation data from ERA5 and In-situ do not agree well. The conclusion is little changed after this data update. The comparison is given in Fig. 6.



[Fig. 6. Comparison of annual temperature (left) and precipitation (right) records from ERA5 and in-situ observations. The correlation coefficient is given in the title. The mean value of the whole period is removed so only anomalies are shown here.]

C13:

[The data part is changed]

Only four in-situ temperature records may not represent the overall condition of the glacierzied zone, so we adopt the gridded temperature product from the ERA5 reanalysis data processed by the European Centre for Medium-Range Weather Forecasts (ECMWF). The data is available at https://www.ecmwf.int/en/forecasts/datasets/reanalysis-datasets/era5. The reliability of this gridded data is tested by comparing with station observations and the correlation index ranges from 0.69 to 0.82 in the interannual variation, indicating a good consistency.

Q14:

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

A14:

We find a more proper word is "real". "real GS melt" means the amount of melt really happens. As we cannot separate mass ablation/accumulation in GRACE observations, so the mass ablation is a bit offset by the mass accumulation.

Q15:

Lines 307-310. This argument needs some clarification. How are these numbers de- rived, 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)?

A15:

The result of GS is based on the 2nd EOF. Here we ignored evaporation. The assumption of neglecting evaporation in maritime glaciers was also made in Ohno et al. (1992). We have added these sentences to explain the reason:

C15:

GS mass loss may be caused by processes of flow, melting, and evaporation, while the last process does not contribute to river flow. Evaporation is important for continental-type glaciers where the climate is usually cold and dry. E.g., it accounts for 12% of the glacier ablation in Tianshan (Ohno et al., 1992). However, the importance of evaporation is greatly reduced in our maritime glaciers due to the extremely humid air and rapid melting. Therefore, we suppose that the mass loss is completely turned into meltwater and can be compared with analogous outputs from models.

[We also add a warning on the potential bias:]

Although extrapolated mass changes for the undetected 17% of glaciers and the neglected summer evaporation may reduce our estimates of summer meltwater, they definitely cannot explain the difference of more than 100%.

Ohno, H., Ohata, T. and Higuchi, K.: The influence of humidity on the ablation of continental-type glaciers. Annals of Glaciology, 16, pp.107-114, 1992.

Q16:

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?

A16:

Their difference is mainly caused by whether the lower Brahmaputra is included or not. Since the meltwater only comes from the upper basin and both studies cover this region, we think their estimates on meltwater is comparable. We have added such a description in the introduction.

C16:

Although these two studies covered different areas, the glacierized zone in the basin was both well enclosed so the estimates should not be so different.

Technical comments:

Q17:

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

A17:

The mathematical expression of precipitation integration is

$$IntP(j) = [\sum_{i=1}^{N} P(j-i+1) * W(N-i+1)] \,/\, \sum_{i=1}^{N} W(i),$$

where IntP represents the integrated precipitation in the jth month, P is short for precipitation, W is the weight for the integration, and N is the integration window.

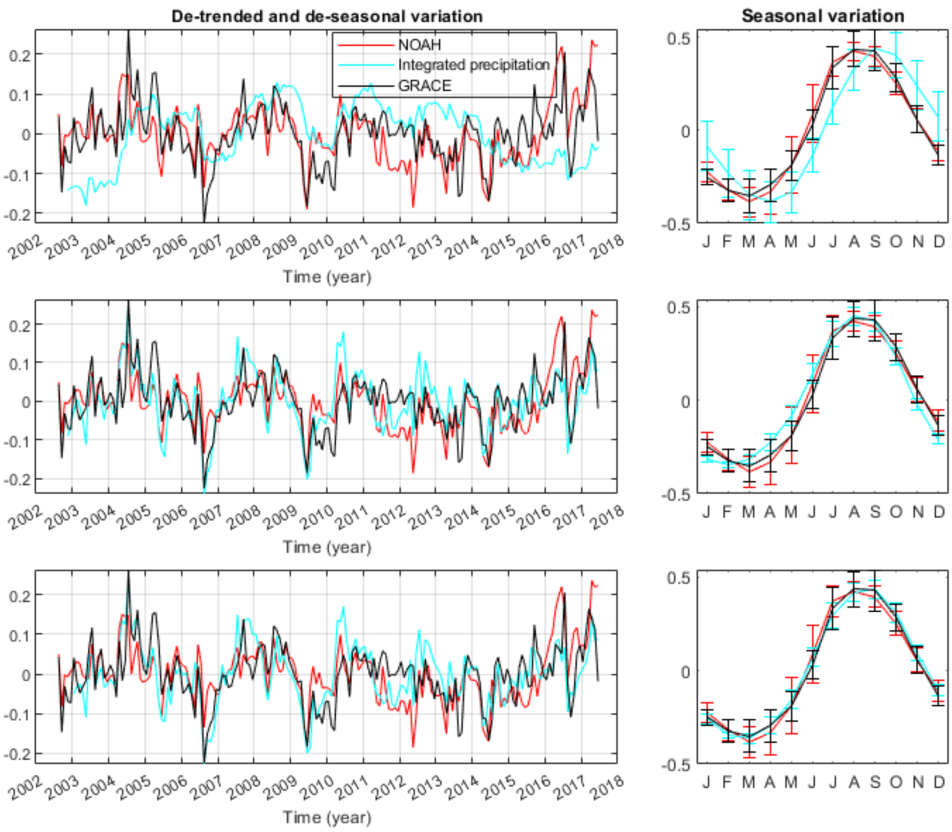
The key parameters are N and W. Below we tried three schemes. (upper), N = j, W(i) = 1; (middle), N = 4, W = [0.4,0.6,0.8,1]; (below), N = 6, W = [1,2,3,4,5,6];

The first scheme is cumulative precipitation (as you suggested). This one does not work well, because it assumes the impact of precipitation in this month is the same as that in months ago, which is not reasonable.

The second scheme is the one used in the previous version.

The third scheme is a new one with a wider integration window. We find it puts little effect on the interannual variation, but improves the agreement in the seasonal variation, so we will change to this scheme in the revision.

The plots are given in Fig. 7.



[Fig. 7. Different methods for precipitation integration]

Q18:

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.

A18:

It has been specified. We also have moved the error estimation to the manuscript.

Q19:

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

A19:

It has been corrected.