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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.

Shuang Yi et al.

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

A colored version with embedded figures is provided in the attachment.

R0:

The reviewer raised six major concerns. The first one is about the undetected western glaciers in the basin. We find there are 17% of glaciers undetected so we estimate the total meltwater by dividing a ratio of 1/0.83. The second is a suggestion of using temperature reanalysis products. We follow this advice and adopt the ERA5 dataset. The third one is why canopy and snowpack in GLDAS is not used. We explain that canopy is three orders of magnitude smaller and snowpack belongs to the second mode. The fourth is how to deal with the glaciers outside the basin. We answer that these glaciers had already been excluded in the basin-scale estimation. The fifth is about uncertainty estimation in GRACE and possible leakage from adjacent regions. We add the content on uncertainty estimation and show that the signals raised by the reviewer are too far to cause a leakage. The sixth is why evaporation was neglected. We discuss that evaporation is supposed to be zero in the maritime glaciers.

Q1:

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 precip- itation. 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 com- munity 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).

R1:

Thanks for the advice. We had written in the previous version that “In our study region, 85% of its meltwater (estimated according to the area proportion) runs into the Brahmaputra and this area accounts for 83% of total glaciers in this basin (9,912 km^2)” and we simply ignored the undetected 17% of glaciers due to their sparse distribution. In this version, we scale our result to consider this part by assuming a similar GS mass change rate.

C1:

[We add an explanation in section 5.3:]

Assuming that the unobserved 17% of glaciers hold the similar rate of GS mass change, our estimate of mass change is scaled by a ratio of $1 \times 0.85/0.83 = 1.02$ to represent the GS mass change of the entire Brahmaputra Basin.

[Fig. 8 of the manuscript is updated and shown as Fig. 1 in this file. (previously the scaling ratio is 0.85, here the ratio is 1.02)]

[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% .

Q2:

2) Climatological data: to prove their claims, the authors employ data from 4 meteo- rological 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.

A2:

Due to the harsh environment, only four stations are available here. We agree that the precipitation condition is not well represented by these four stations due to the manifest spatial and altitudinal variations. In this version, we adopt the ERA5 reanalysis data for temperature variations and the details are shown below.

Q3:

2b) Regarding the gridded datasets used here, the authors briefly mention the limita- tions 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. ?

A3:

APHRODITE ended in 2007, so it is not helpful for this study. The ERA-Interim has been superseded by the ERA5, so we take precipitation data from the ERA5 reanalysis 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.

[Fig. 2. 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.]

C3:

[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.

Q4:

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.

A4:

The reason of excluding canopy component is that the mass changes induced by the canopy component is nearly negligible comparing with hydrological or glacial mass changes in this region. The result of March 2003 is shown as an example below (units are equivalent water height in cm). Note the strength of canopy is three orders of magnitude smaller than the others. The snowpack is almost zero throughout the year in the study region as well. Even if it is not zero, we should not include it in the validation of hydrological signals (mode 1), but in that of glacial signals (mode 2).

[Fig. 3. Components of soil moisture, canopy and snowpack in China and surrounding by using GLDAS.]

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 to get due to its invisibility.

A space-time signal can be separate into five parts as below. 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.

[Fig. 4. Five aspects of a space-time signal]

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

The time series of GRACE and GLDAS had been decomposed into interannual and seasonal time scales and compared in Fig. 5e and Fig. 5f. If you feel interested in the original time series, the original time series are overwhelmed by the strong seasonal variations (the magnitude of GRACE nearly doubles that of GLDAS) so they are difficult for more detailed comparison.

We had already explained the source of the negative trend in the first mode by both precipitation and groundwater pumping: *The negative trend in the first mode is likely due to decreasing precipitation in recent years (Figure S8) and intense groundwater pumping (Shamsudduha et al., 2012).*

C4:

[We have stressed the difference in the magnitude of EOF1 in the manuscript:]

The exclusion of unavailable surface water and groundwater in the GLDAS result also causes a weaker strength of its EOF1 compared to that of GRACE.

Q5:

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?

A5:

We explained that 85% of the total meltwater runs into the Brahmaputra in Section 5.3. We reemphasized this problem in the introduction in this version.

Q6:

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.

A6:

This problem has been responded above. In this version, we change the title to "Monthly glacier and snow mass balance in Southeast Tibet from 2002 to 2017 estimated by satellite gravimetry" so the focus is moved to mass balance.

Q7:

- 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.

A7:

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.

C7:

[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].

Q8:

- 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.

A8:

The other signals are thousands of kilometers away and their influence on our study region is negligible. As shown below, the location of the North India groundwater depletion is in region E, while here region D and F are studied. The figure is from Yi and Sun (2014).

[Fig. 5. Trend of gravity change in Tibet using GRACE. The figure is excerpted from Yi and Sun (2014)]

Q9:

- 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.

A9:

We considered two widely used filters in the estimates and their difference is included in the trend error estimation. From Fig. S7, you may find the difference between different filters is smaller compared to that between different data solutions.

Q10:

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

A10:

The error estimation has been moved to the manuscript by following your suggestion.

Q11:

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 sum- mer 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, there- fore, 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.

A11:

We have added these descriptions to show the reason of neglecting evaporation. The assumption of neglecting evaporation in maritime glaciers was also made in Ohno et al. (1992).

C11:

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.

Q12:

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

A12:

It has been changed.

Q13:

Line 15: low temporal resolutions of what?

A13:

It has been changed to “the low temporal resolutions in previous observations of GS mass balance”

Q14:

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

A14:

It has been rewritten as:

We find that the “spring-accumulation type” glaciers and snow in the SETP reach their maximum in May.

Q15:

Line 26: I would change regarded to considered.

A15:

It has been changed.

Q16:

Line 27: “The sustainable . . .” rephrase

A16:

It has been rewritten as:

The GS melt is susceptible to climate change, while its sustainable supply is the key to the local freshwater security, flood prevention and control, and hydroelectric development

Q17:

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

A17:

It has been changed.

Q18:

Line 50: Rephrase.

A18:

It has been written as:

The first two geodetic approaches require the average ice density to convert volume changes into mass changes.

Q19:

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

A19:

It has been change.

Q20:

Line 75: Rephrase

A20:

It has been rewritten as:

Therefore, according to the climate stations near NTM, we can observe that there are two peaks in precipitation throughout the year.

Q21:

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

A21:

It has been rewritten as:

The method of this study is based on the fact that the change in GS mass driven by spring precipitation is earlier than the change in hydrological signals

Q22:

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.

A22:

We agree with you here. It is difficult to make a quantitative comparison. Here we only present whether spring precipitation is detectable or not.

Q23:

Line 158: more or less?

A23:

This sentence has been rewritten as:

These results show that spring precipitation can only be partially captured by various measurements/products.

Q24:

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.

A24:

I guess you mean the Pamir Plateau. As you may find in Figure below (units are cm in equivalent water height), the westerly brings strong gravity increase there in winter (ahead of gravity increase in our study region). That region is mostly free from the monsoonal precipitation, so the hydrological and glacial signals are synchronized due their identical driving force. Therefore, our method is not applicable there. In fact, it is impossible to separate them without other data.

The study region in this work is quite particular as the intersection of two distinct climatic systems.

[Fig. 6. Mass anomalies in Tibet from January to March by using GRACE]

Q25:

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?

A25:

I suppose you referred to the trend in the first mode (I could not find contents about a trend in this line) and the mass loss in the north Indian Plains. As explained above, the north Indian Plains are too far away to put an influence here. We discussed in the SI about the possible leakage between signals of water storage and glaciers and concluded that the leakage must be small.

Q26:

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

A26:

We excluded glaciers outside the basin and we had explained it in the result part. We have reemphasized it in the end of the introduction.

Q27:

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

A27:

The method to integrate monthly precipitation is widely used in the comparison of GRACE data with precipitation data, so we did not repeat its exact expression here. For example:
- Crowley, John W., et al. "Annual variations in water storage and precipitation in the Amazon Basin." Journal of Geodesy 82.1 (2008): 9-13.
- Reager, J. T., and J. S. Famiglietti. "Global terrestrial water storage capacity and flood potential using GRACE."Âă*Geophysical Research Letters*Âă36.23 (2009).

Its mathematical expression is

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

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 precipitation accumulation. 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.

[Fig. 7. Different methods for precipitation integration]

Q28:

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 un- certainty. The trend observed here is determined also by the groundwater depletion observed in other studies,

A28:

Here we wrote the negative trend in the first mode is caused by both decreased precipitation and groundwater pumping. I think it is troublesome to quantify this trend, which is influenced by spatial range, time span and accurate modeling of the influence of climatic factors/human activities. Although there have been some studies in groundwater depletion in specific regions, they are not helpful given the larger unknowns. Besides, the 1st mode is not the focus of this study. I am sorry that we cannot provide more accurate evaluation here.

Q29:

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

A29:

We explained in section 2.2 that glacier density is 850 +- 60 kg/m3 from Huss (2013)

Q30:

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

A30:

This problem has been responded above.

Q31:

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

A31:

It is from this study. It has been clarified.

Q32:

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?

A32:

Another frequently investigated variable is precipitation. However, we could not find a significant correlation between mass gain and spring precipitation (shown below), so this result was not given in the manuscript. The possible cause may be that our data cannot reflect the complex distribution of precipitation well.

[Fig. 8. Correlation between spring precipitation and GS mass change in the southeast Tibet]

Please also note the supplement to this comment:

https://www.the-cryosphere-discuss.net/tc-2019-211/tc-2019-211-AC2-supplement.pdf

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Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-211, 2019.

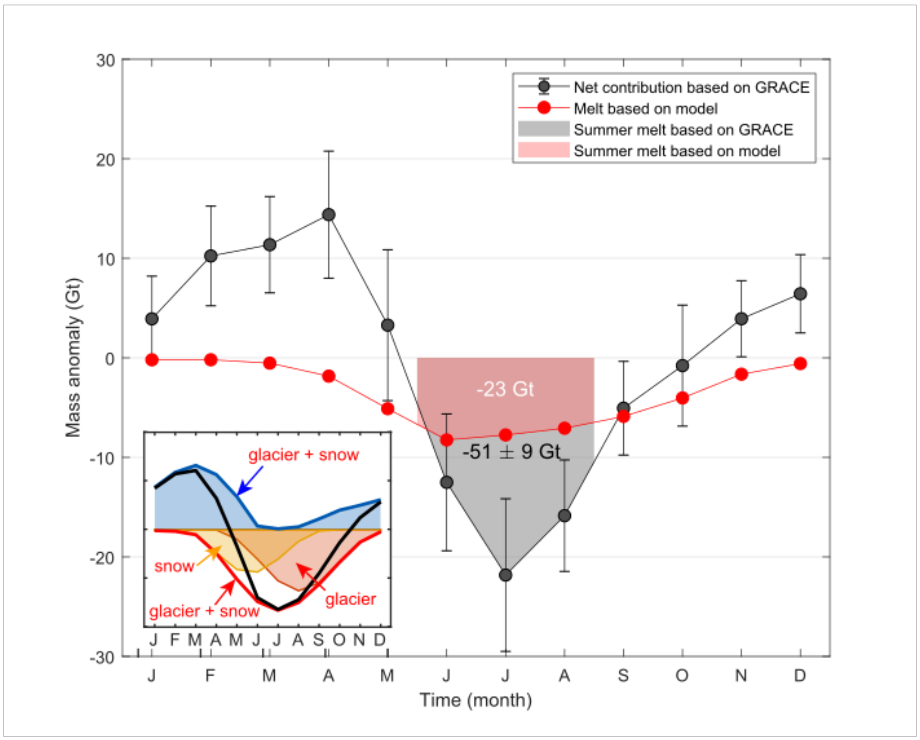


Fig. 1. Updated version of Fig. 8 in the manuscript

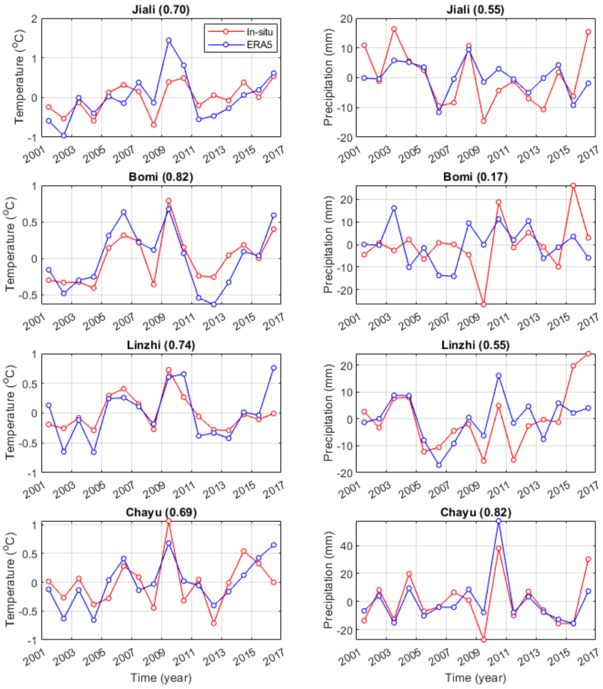


Fig. 2. Comparison of annual temperature (left) and precipitation (right) records from ERA5 and in-situ observations.

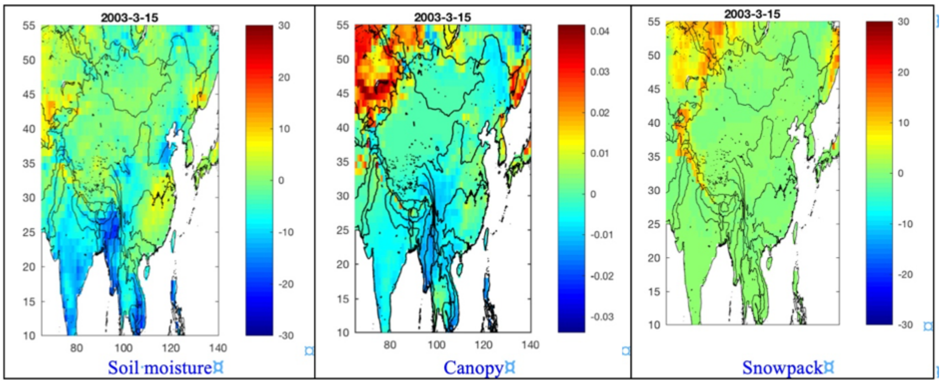


Fig. 3. Components of soil moisture, canopy and snowpack in China and surrounding by using GLDAS

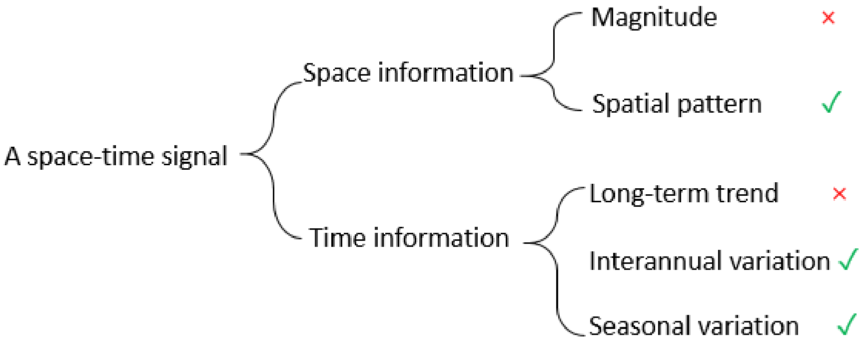


Fig. 4. Five aspects of a space-time signal

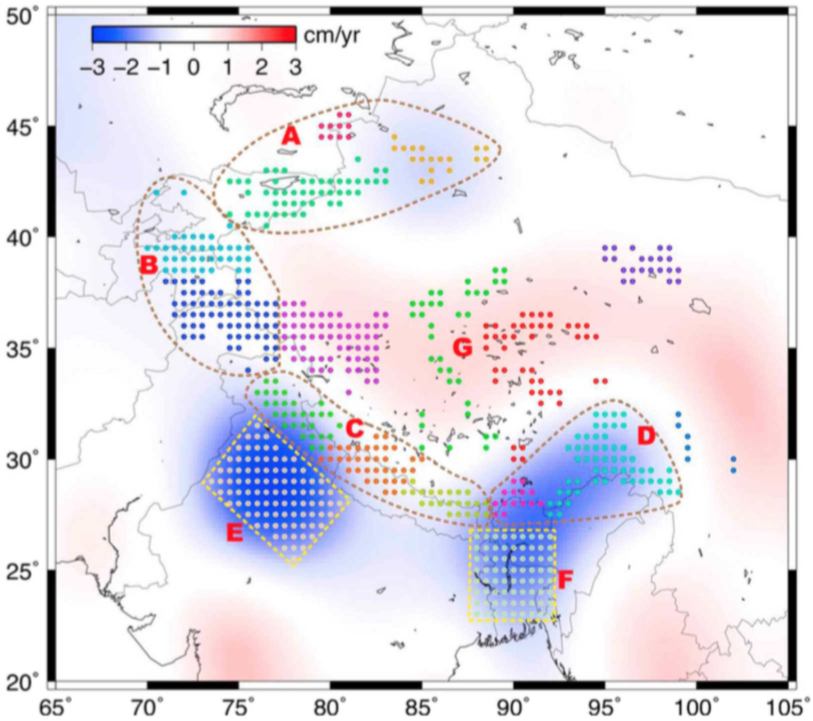
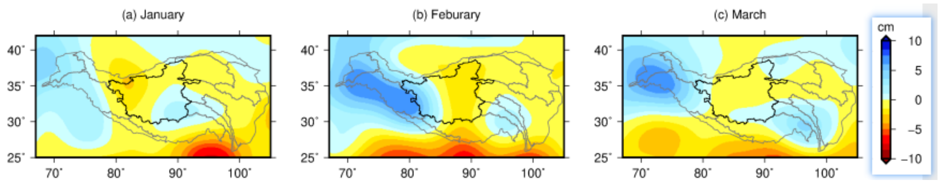


Fig. 5. Trend of gravity change in Tibet using GRACE


(a) January

(b) Feburary

(c) March

Fig. 6. Mass anomalies in Tibet from January to March by using GRACE

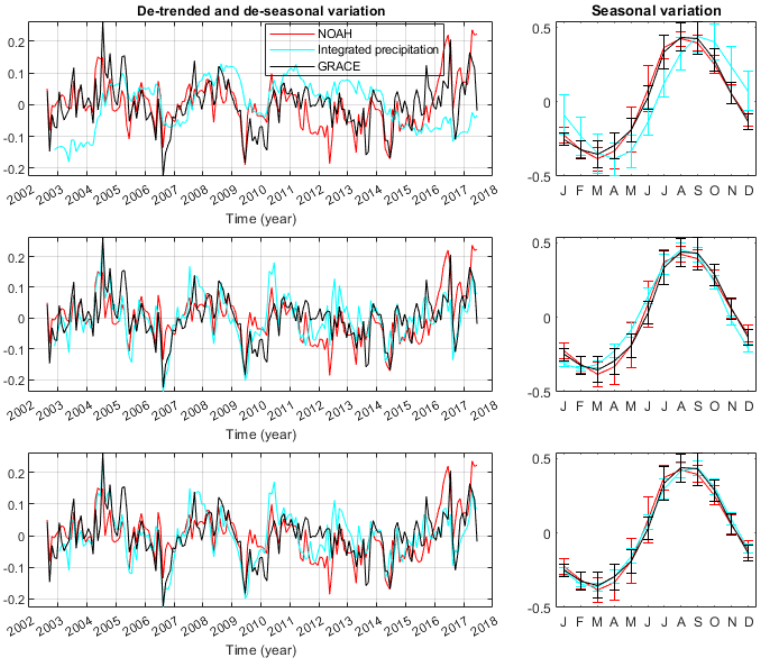


Fig. 7. Different methods for precipitation integration

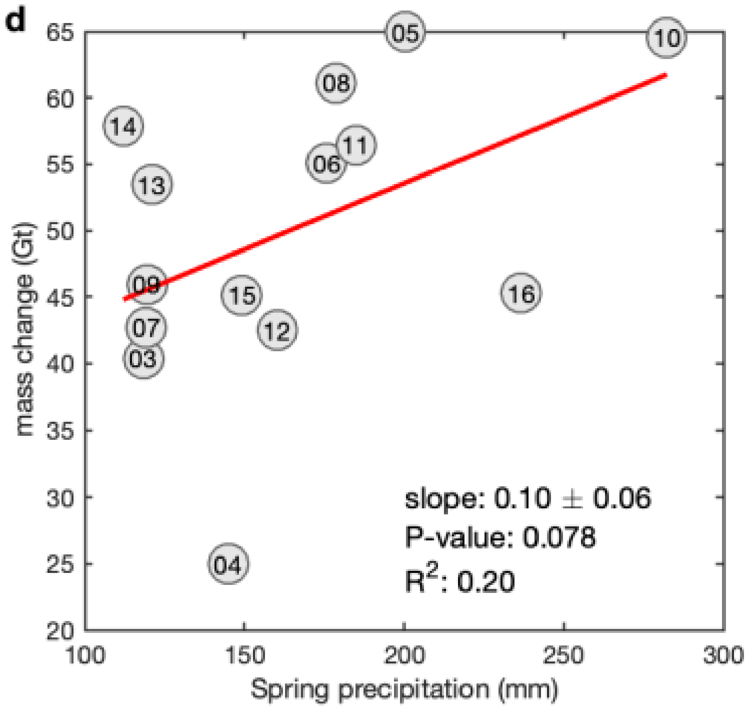


Fig. 8. Correlation between spring precipitation and GS mass change in the southeast Tibet