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Discussions

Interactive comment on "Seasonal cycles and trends of water budget components in 18 river basins across Tibetan Plateau: a multiple datasets perspective" *by* Wenbin Liu et al.

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Review Comments (Anonymous Referee #4): General Comments (This review is based on the revised manuscript uploaded by the authors on January 4th 2017.)

The authors present an analysis of the water balance of 18 catchments on the Tibetan Plateau using multiple datasets. The basis of their approach is to consider actual evapotranspiration (ET) as the main unknown in the water balance and estimate this using data on the other components. This work is interesting and potentially a useful contribution to our understanding of the hydrology of the Tibetan Plateau. However, the paper suffers from several major limitations in its current form, as described below.

Thank you very much for your invaluable comments/suggestions. We have revised the manuscript accordingly (please see the point-to-point response below). In the Acknowledgement section [Line 587-588 in the new version]: "We thank the editors and reviewers for their invaluable comments and constructive suggestions".

1. Although uncertainty is briefly addressed in Section 3.4, I do not feel that this issue is considered in enough depth. As the authors acknowledge, there are large uncertainties associated with the datasets used in the study, as well as the method for estimating ET. I feel that these uncertainties should be incorporated into the analysis rather than addressed in general terms afterwards (Section 3.4). Otherwise the significance of the conclusions is unclear, i.e. whether the results are really an artefact of data limitations (see points below). This is my most significant concern and I feel that a fair amount of additional work and revision could be required to address this. (One variant of this approach could be to examine the errors from attempting to close the water balance with published data products, rather than forcing closure by considering ET as the residual term.)

The main objective of this study is to test whether the general hydrological regimes (seasonal cycles and trend of the water balance components) could be inferred from the perspective of multi-source datasets in the data-sparse Tibetan Plateau. We have also showed the potentials through the use of multi-source datasets (e.g., satellite retrievals, LSM simulations) to achieve this goal in this paper. The results obtained are generally in line with earlier studies (e.g., Liu et al., 1999; Dong et al., 2007; Fu et al., 2008; Zhu et al., 2011; Zhang et al., 2013; Cuo et al., 2014), thus the results are not artefact of data limitation.

This study may unavoidably associate with some uncertainty due to the use of multi-source datasets. We totally agree with the reviewer that the uncertainty should be quantified as well in the analysis. We have actually tried to consider the uncertainty in the analysis, for example, we compared the observed CMA precipitation with TRMM and IGSNRR_forcing data during 2000-2011. The water balance-based ETwb was also compared with other six global/regional ET products (including the mean and annual trends) during the period 1982-2006. Moreover, to minimize the uncertainty in the analysis we only analyzed the well-performed ET products together with the observed runoff, precipitation and ETwb of basic water balance components during 1982-2011. However, adequately quantification of uncertainty for each water budget component is difficult. When we focused on the analysis of one variable during the period 1982-2011, few datasets can be used together in the TP to quantify its uncertainty due to the data availability. For example, we have observed CMA precipitation from 1982-2011, but the TRMM precipitation is only available since 2000. We can also calculate ETwb for the period 1982-2011, but Zhang_E is only available from 1983-2006. Moreover, the global datasets for NDVI, LAI, SWE and water storage changes are also limited which, to some extent, restricted our attempts to quantify the uncertainties in the analysis using multi-source datasets.

In response, we have expanded the uncertainty section to discuss the uncertainty related issues as follows [Line 500-527 in the new version], "…In particular, it is well known that land surface models have some difficulties (e.g., parameter tuning in boundary layer schemes) when applying to the TP, even though they have good performances in different regimes (Xia et al., 2012; Bai et al., 2016). For example…There are also considerable uncertainties arising from empirical extending the ET series back prior to the GRACE era. Finally, we obtained the contributions of glacier-melt to discharge in some basins from the literatures and took them as constant numbers…With these caveats, we can interpret the general hydrological regimes and their responses to the changing climate in the TP basins from solely the perspective of multi-source datasets, which are comparable to the existing studies based on the in situ observations and complex hydrological modeling."

Reference: Liu, T.: Hydrological characteristics of Yalungzangbo River, Acta Geogr. Sin., 54 (Suppl.), 157-164, 1999 (in Chinese). Dong, X., Yao, Z., and Chen, C.: Runoff variation and responses to precipitation in the source regions of the Yellow River, Resour. Sci., 29(3), 67-73, 2007 (in Chinese). Fu, L., Chen, Y., Li, W., Xu, C., and He, B.: Influence of climate change on runoff and water resources in the headwaters of the Tarim River, Arid Land Geogr., 31(2), 237-242, 2008 (in Chinese). Zhu, Y., Chen, J., Chen, G.: Runoff variation and its impacting factors in the headwaters of the Yangtze River in recent 32 years, J.Yangtze River Sci. Res. Inst., 28(6), 1-4, 2011 (in Chinese). Zhang, L., Su, F., Yang, D., Hao, Z., and Tong, K.: Discharge regime and simulation for the upstream of major rivers over Tibetan Plateau, J. Geophys. Res. Atmos., 118(15), 8500-8518, 2013. Cuo, L., Zhang, Y.X., Zhu, F.X., and Liang, L.Q.: Characteristics and changes of streamflow on the Tibetan Plateau: A review, J. Hydrol. Reg. stud., 2, 49-68, 2014.

2. It is not clear to me whether trend analysis is fully justified by the data. This is particularly the case for ET, as the authors use an approximate method to extend their ET estimates prior to the period of available GRACE data. As ET is also resolved as the residual in the water balance, any errors in the other components could be compensated for in ET estimates, which could presumably affect trend analysis. It may even be worth focusing on annual and seasonal water balance estimation in this paper and leaving the trend analysis for a more considered treatment in a separate manuscript.

The closure of basin-scale water balance with published data products is actually difficult due to the lack of observation and the uncertainties inherited from multi-source data (remote sensing retrievals, reanalysis outputs and LSM simulations). Nowadays, there is no such a "best" approach to do this due to the data limitation, although some attempts have been published using the empirical and statistical methods. We also proposed a bias-correction based method which regards the basin-wide P minus Q as the biased ET and empirically correct the biased ET with the cumulative distribution of P minus Q and water storage changes (this method has been used in some papers such as Li et al., 2014 and Liu et al., 2016). The general hydrological regimes (e.g. the multiyear means and seasonal cyles of water balance components) concluded in this study were consistent with earlier studies (e.g., Liu et al., 1999; Dong et al., 2007; Fu et al., 2008; Zhu et al., 2011; Zhang et al., 2013; Cuo et al., 2014). These all give us the confidences to believe the rationality of the further trend analysis.

Similar to the response of comment 1, we have expanded the uncertainty section to discuss the uncertainty related issues as follows [Line 500-527 in the new version], "…In particular, it is well known that land surface models have some difficulties (e.g., parameter tuning in boundary layer schemes) when applying to the TP, even though they have good performances in different regimes (Xia et al., 2012; Bai et al., 2016). For example…There are also considerable uncertainties arising from empirical extending the ET series back prior to the GRACE era. Finally, we obtained the contributions of glacier-melt to discharge in some basins from the literatures and took them as constant numbers…With these caveats, we can interpret the general hydrological regimes and their responses to the changing climate in the TP basins from solely the perspective of multi-source datasets, which are comparable to the existing studies based on the in situ observations and complex hydrological modeling."

Reference: Li, X.P., Wang, L., Chen, D.L., Yang, K., and Wang, A.H.: Seasonal evapotranspiration changes (1983-2006) of four large basins on the Tibetan Plateau, J. Geophys. Res., 119 (23), 13079-13095, 2014. Liu, W.B., Wang, L., Zhou, J., Li, Y.Z., Sun, F.B., Fu, G.B., Li, X.P., and Sang, Y-F.: A worldwide evaluation of basin-scale evapotranspiration estimates against the water balance method, J. Hydrol., 538, 82-95, 2016. Liu, T.: Hydrological characteristics of Yalungzangbo River, Acta Geogr. Sin., 54 (Suppl.), 157-164, 1999 (in Chinese). Dong, X., Yao, Z., and Chen, C.: Runoff variation and responses to precipitation in the source regions of the Yellow River, Resour. Sci., 29(3), 67-73, 2007 (in Chinese). Fu, L., Chen, Y., Li, W., Xu, C., and He, B.: Influence of climate change on runoff and water resources in the headwaters of the Tarim River, Arid Land Geogr., 31(2), 237-242, 2008 (in Chinese). Zhu, Y., Chen, J., Chen, G.: Runoff variation and its impacting factors in the headwaters of the Yangtze River in recent 32 years, J.Yangtze River Sci. Res. Inst., 28(6), 1-4, 2011 (in Chinese). Zhang, L., Su, F., Yang, D., Hao, Z., and Tong, K.: Discharge regime and simulation for the upstream of major rivers over Tibetan Plateau, J. Geophys. Res. Atmos., 118(15), 8500-8518, 2013. Cuo, L., Zhang, Y.X., Zhu, F.X., and Liang, L.Q.: Characteristics and changes of streamflow on the Tibetan Plateau: A review, J. Hydrol. Reg. stud., 2, 49-68, 2014.

3. Some of the main conclusions are stated in the abstract and summary as precipitation being the main contributor to runoff and snow water equivalent (SWE) being higher from late autumn to spring. These conclusions seem fairly basic and general. I think it may be possible to draw more substantial and specific conclusions from the analysis presented. This would stem from more focused discussion of results in Section 3. 4.

Similar to the responses of comment #1, the main objective of this study is to test whether the general hydrological regimes (seasonal cycles and trend of the water balance components) could be inferred from the perspective of multi-source datasets in the data-sparse Tibetan Plateau. The results obtained are also generally in line with those concluded from some basin-scale and observe-based studies. Certainly, as we discussed in the paper and responded in comment #1, this study may unavoidably associate with some uncertainty due to the use of multi-source datasets. We thus only draw some general results (e.g., seasonal cycles and trends of water balance components, some results have been obtained from previous basin-specific and observation-based studies but most have not been investigated) confidently considering the considerable uncertainties inherited in different datasets (not more substantial and specific conclusions). The reviewer also mentioned to stem more focused discussion from the results of Section 3.4, please also refer to the responses of comment #1. Thanks.

The standard of English needs to be improved throughout the manuscript. While the meaning is usually (but not always) clear, there are a lot of grammatical errors (far too many to list). I suggest the authors enlist the help of someone with native-level proficiency to carefully revise the text.

We invited our colleague with sufficient English language training (Wee Ho Lim) to participate in this manuscript. We believe that the language structure and grammar is substantial improved in the revised version.

Specific Comments (Line numbers refer to the revised manuscript uploaded by the authors on January 4th 2017.) Table 1 – It is not clear what data sources are behind all of the summary statistics for the catchments listed in this table – in particular for precipitation, temperature, NDVI, LAI and snow cover (imprecise reference). In addition, should the "Second Glacier Inventory of China" appear in Table 2 and/or the reference list? Perhaps it would make sense to introduce the catchment characteristics after the section on data rather than before.

We agree. In the revised version, we have changed Table 1 to Table 2 (and correspondingly the former Table 2 has been changed to Table 1 in the new version). The introductions of catchment characteristics have been moved after the section on data as suggested by the reviewer. The "Second Glacier Inventory of China" and its reference have also added in the end of Table 1 (Table 2 in the old version). Further, some more descriptions have been added in the caption of Table 2 (Table 1 in the old version) to clearly indicate the data sources used for the summary statistics for the catchments listed in this table as follows [Line 824-827 in the new version],

"Table 2: Main features of the 18 TP river basins used in this study. The precipitation and temperature statistics for each basin were calculated from the observed CMA datasets while the NDVI and LAI statistics were extracted from the GIMMS NDVI dataset and GLASS LAI product. The GA% and SC% represented the percentages of multiyear-mean glacier cover and snow cover in each basin which were calculated from the Second Glacier Inventory Dataset of China and the daily TP snow cover dataset (2005-2013)"

Figure 1 – I wonder if the solid shading of catchments is really needed. Good point. We substituted the solid shading of catchments (Figure 1) with catchment boundary

(i.e., Figure R1 in this response).

Section 2.1 – Would it be better to introduce the uncertainties associated with each dataset here? This should include observational uncertainty (e.g. precipitation undercatch (including snowfall) and discharge), as well as all model and remote sensing datasets.

Thanks. We respect the reviewer's viewpoint. However, nowadays these global datasets (e.g., different ET products, SWE products, NDVI and LAI) has rarely been comprehensively assessed in the TP due to the lack of in situ observations. The advantages/disadvantages of different datasets are thus difficult to be fully described. Moreover, detailed validations of these products in the TP are beyond the scope of this study, which need be further investigated in the future works.

We have also tried to add more details associated with some datasets in the uncertainty Section through summarizing the limited studies available as follows [Line 498-527 in the new version]: "…due to their different forcing data; algorithm used as well as varied spatial-temporal resolutions (Xue et al., 2013; Li et al., 2014; Liu et al., 2016a). In particular, it is well known that land surface models have some difficulties (e.g., parameter tuning in boundary layer schemes) when applying to the TP, even though they have good performances in different regimes (Xia et al., 2012; Bai et al., 2016). For example, Xue et al. (2013) indicated that GNoah_E underestimated theãĂŰ ETãĂŮ_wb in the upper Yellow River and Yangtze River basins on the Tibetan Plateau mainly due to its negative-biased precipitation forcing. We thus only usedãĂŰ ETãĂŮ_wb in the trend detection of water budget components in Fig.8, Fig.10 and Fig.11 in this study. The two SWE products also showed large uncertainty with respect to both their seasonal cycles and trends. The VIC_IGSNRR simulated and GlobaSnow-2 SWEs have not been validated in the TP due to the lack of snow water equivalent observations, but in some basins (e.g., Zelingou and Numaitilangan) they showed similar seasonal cycles and annual trends.

Moreover, the interpolation of missing values of runoff with VIC_IGSNRR simulated runoff and the gridded precipitation data (which interpolated from limited gauged precipitation over the plateau) also introduced uncertainties. There are also considerable uncertainties arising from empirical extending the ET series back prior to the GRACE era. Finally, we obtained the contributions of glacier-melt to discharge in some basins from the literatures and took them as constant numbers. It may inherit considerable uncertainty from varied studies using different approaches such as glacier mass-balance observation, isotope observation and hydrological modeling, and the contribution rates would also change under a warming climate. However, reliable quantification of the contribution of glacier-melt to discharge is technically challenging, especially for the data-sparse basins. With these caveats, we can interpret the general hydrological regimes and their responses to the changing climate in the TP basins from solely the perspective of multi-source datasets, which are comparable to the existing studies based on the in situ observations and complex hydrological modeling. ”

Line 178-181 – What additional processing of the GRACE data products was done (regarding the glacier isostatic adjustment correction and destriping)?

The glacier isostatic adjustment correction and destriping have already been done by different data processing centers. We actually did not do any additional processing for the GRACE data in this study. We have deleted this description in this sentence as follows [Line 170-172 in the clean version], “To minimize the errors and uncertainty of extracted ΔS, we averaged these GRACE retrievals (2002-2013) from different processing centers in this study.”

Equation 2 – I am not sure that this equation is helpful. It may be better just to retain the statement in the preceding paragraph that glacier melt is a component of runoff. At the very least Q would need to be defined differently from Equation 1 (i.e. introducing a term for non-glacial runoff), otherwise the two equations are not consistent (unless glacier melt equals zero). More generally in Section 2.2.1, I am not clear why the glacier melt contribution to catchment runoff needs to be estimated at all for the ET calculations (as observed runoff at the catchment outlets includes glacier melt).

We respect the reviewer’s viewpoint. From water balance perspective, Equation 1 and 2 are consistent to one and another. The difference is the composition of observed runoff (Q). In the non-glacier basins, P=Q+ET+ΔS (Equation 1). All precipitation is converted to Q, ET and water storage change. In the glacier-dominated basins, we consider that the observed Q includes additional the glacier melts which is not be reflected in ΔS.With this conception, we adapted Equation 1 (P=Q+ET+ΔS) into a form that is more feasible for basins. Thus we revised it into Equation 2 (P+M_G=Q+ET+ΔS).

Table 3 – As this is a very approximate estimation of glacier contribution to discharge, quoting percentages to two decimal places seems too precise.

We agree. We have quoted percentages to only one decimal place in the revised version (Table R3). Thanks.

Section 2.2.1 – I am struggling to fully understand all of the bias-correction procedure, particularly the rationale behind the second step. It would be useful if the authors could clarify this please.

We have revised the descriptions of bias-correction procedure to make is more clearer, as follows [Line 268-301 in the new version],

“The terrestrial water storage (ΔS) in Eq.(2) includes the surface, subsurface and ground water changes. It has been demonstrated cannot be neglected in water balance calculation over monthly and annual timescales due to snow cover change and anthropogenic interferences (e.g., reservoir operation, agricultural water withdrawal) (Liu W. et al., 2016a). For the period 2002-2011, we calculated basin-wide ET (ãĂŰETãĂŮ_wb) directly using the GRACE-derived ΔS in Eq.(2). Since GRACE data is absent before 2002, we calculated the monthly ãĂŰETãĂŮ_wb using the following two-step bias-correction procedure (Li X. et al., 2014). We defined P+M_G-Q in Eq. (2) as biased ET (ãĂŰETãĂŮ_biased, available from 1982 to 2011) relative to the “true” ET (ãĂŰETãĂŮ_wb= P+M_G-Q-ΔS, available during the period 2002-2011 when the GRACE data is available). Over the period 2002-2011, we first fitted ãĂŰETãĂŮ_biased and ãĂŰETãĂŮ_wb series separately using different gamma distributions, which has been evidenced as an proper method for modeling the probability distribution of ET (Bouraoui et al., 1999). The monthly ãĂŰETãĂŮ_biased series (2002-2011) can then be bias-corrected through the inverse function (Fˆ(-1)) of the gamma cumulative distribution function (CDF,F) of ãĂŰETãĂŮ_wb by matching the cumulative probabilities between two CDFs as follow (Liu W. et al., 2016a), ãĂŰETãĂŮ_corrrected (m)=Fˆ(-1) (F(ãĂŰETãĂŮ_biased (m)|α_biased,β_biased)|α_wb,β_wb) (3) Here α_biased, β_biased and α_wb , β_wb are shape and scale parameters of gamma distributions for ãĂŰETãĂŮ_biased and ãĂŰETãĂŮ_wb. ãĂŰETãĂŮ_corrected (m) and ãĂŰETãĂŮ_biased (m) represent the monthly corrected and biased ET, respectively. The bias correction procedure can be flexibly applied to the period 1983-2011 by matching the CDF of ãĂŰETãĂŮ_biased (1983-2011) to that of ãĂŰETãĂŮ_corrected (2002-2011). The second step of bias correction is to eliminate the annual bias through the ratio of annual ãĂŰETãĂŮ_biased to annual ãĂŰETãĂŮ_corrected calculated in the first step using the following method, ãĂŰETãĂŮ_final (m)=(ãĂŰETãĂŮ_biased (a))/(ãĂŰETãĂŮ_corrected (a))×ãĂŰETãĂŮ_corrected (m) (4) where ãĂŰETãĂŮ_final (m) is the final monthly ET after bias correction. ãĂŰETãĂŮ_biased (a) and ãĂŰETãĂŮ_corrected (a) represent the annual biased and corrected ET while ãĂŰETãĂŮ_corrected (m) is the monthly corrected ET obtained from the first step. The procedure was then applied to correct the monthly ãĂŰETãĂŮ_biased series and calculated the monthly ãĂŰETãĂŮ_corrected during the period 1982-2001 for all TP basins. We take these results as sufficient representation of the “true” ET (ãĂŰETãĂŮ_wb) for evaluating multiple ET products and trend analysis. ”

In addition, “m” and “a” are used in Equations 3 and 4 but I am not sure that they are defined anywhere.

We have explained “m” and “a” in the revised version as follows [Line 295-297 in the new version], “ãĂŰETãĂŮ_biased (a) and ãĂŰETãĂŮ_corrected (a) represent the annual biased and corrected ET while ãĂŰETãĂŮ_corrected (m) is the monthly corrected ET obtained from the first step.”

The uncertainties arising from extending the ET series back prior to the GRACE data period should also be considered.

We respect the reviewer’s viewpoint. However, the uncertainties are actually hard to quantify due to the lack of observed basin-scale ET. We have added more discussion in the Section 3.4 as follows [Line 515-517 in the new version], “. . .There are also considerable uncertainties arising from empirical extending the ET series back prior to the GRACE era. . .”

Line 293-294 – The variable X should be defined.

We have defined X in the revised version as follows [Line 307-308 in the new version], “. . .For example, X (X_1,X_2,. . .,X_n) is a time series data, it will be replaced by. . .”

Line 309-313 – It may be more useful to evaluate the VIC flow results in terms of biases and consistency of anomalies (monthly and annual) relative to observed discharge rather than Nash Sutcliff Efficiency (NSE), as the focus of the study is on water budgets.

In hydrological applications, NSE is commonly used for evaluation of simulated discharges against the observations and we intend to keep it. In the revised version, we supplemented Figure 2 (Figure R2 in this response) with relevant statistical information (e.g., bias. correlation coefficient). Thanks a lot.

From Figure 2 it looks like peak flows are underestimated during “wetter” years. In addition, why does runoff simulated by VIC appear to drop to zero during the low flow season?

The VIC simulated runoff was directly downloaded from the Land Surface Processes and Global Change Research Group in IGSNRR, CAS (Website http://hydro.igsnrr.ac.cn/public/vic_outputs.html). In some basins, the VIC-simulated runoff may drop to zero due to the uncertainties of meteorological forcing data. Similar results were also shown in Zhang et al. (2014).

Reference: Zhang, X., Tang, Q., Pan, M., and Tang, Y.: A long-term land surface hydrologic fluxes and states dataset for China, J. Hydrometeorol., 15, 2067-2084, 2014.

Line 313-317 – What data were used to force the VIC model (i.e. is there any circularity in this comparison of precipitation datasets)? The uncertainties in observation-derived and TRMM-estimated gridded precipitation products should also be acknowledged (i.e. consistency may be encouraging but neither represents "absolute truth").

The VIC model (Zhang et al., 2014) was mainly forced by static parameters (e.g., DEM-derived terrain parameters, satellite-derived NDVI and land cover map) and IGSNRR meteorological forcing data (0.5 degree, 3-hour), which was constructed solely from the CMA station-based meteorological data (756 stations). We have compared the basin-averaged IGSNRR_forcing precipitation (756 stations) and TRMM (2000-2011) against the CMA gridded precipitation (more than 2000 stations) for all 18 TP basins. However, it is difficult to draw all precipitation time series for 18 basins (only the results for the smallest basin as drawn in Figure 2c). We thus simply compared the annual means, correlation coefficient and RMSE in Figure 2d in the manuscript.

In response, we have acknowledged the uncertainties in observation-derived and TRMM-estimated gridded precipitation as follows [Line 332-333 in the new version], "…, although the observation-derived and TRMM-estimated precipitation also has uncertainties"

Reference: Zhang, X., Tang, Q., Pan, M., and Tang, Y.: A long-term land surface hydrologic fluxes and states dataset for China, J. Hydrometeorol., 15, 2067-2084, 2014.

Section 3.1 – In general, it would be useful to see how the different datasets look in a little more detail. For example, what magnitude of storage changes is present according to the GRACE data products? This would have big impacts on the ET calculations, so the authors need to demonstrate the credibility of GRACE data with reference to other studies (and with reference to our understanding of Tibetan Plateau hydrology to date).

Good idea. We have added a Table (Table 4 in the new version and Table R4 in this response) to describe the annual mean magnitude of GRACE-derived storage changes. Moreover, the uncertainties between the GRACE-derived water storage changes from different processing centers for 18 TP basins are also discussed as follows [Line 333-338 in the new version],

"…The magnitudes of GRACE-derived annual mean water storage change (ΔS) in 18 TP basins are relatively less than those for other water balance components such as annual P, Q and ET (Table 4). The uncertainties among GRACE-derived annual mean ΔS from different data processing centers (CSR, GFZ and JPL) are small for 18 basins except for in the basins controlled by Gadatan and Tangnaihai stations. .."

Line 319-331 – Evaluating different ET products with reference to ET calculated as a residual of the water balance depends very heavily on the uncertainties/accuracy of the datasets underpinning the other terms of the water balance. The selection/rejection of ET products for further analysis in this section/paragraph is done without reference to uncertainties–so do we really know that these are the better products?

The basin-scale ET is not measured. In hydrological applications, we often use the water balance-based ETwb (observed precipitation minus observed runoff and GRACE-observed water storage change) as the "true" reference and to assess different ET products against ETwb (Li et al., 2014; Liu et al., 2016) Thus, in this paper, the selection/rejection of ET products for further analysis is done with the reference of ETwb.

Reference: Li, X.P., Wang, L., Chen, D.L., Yang, K., and Wang, A.H.: Seasonal evapotranspiration changes (1983-2006) of four large basins on the Tibetan Plateau, J. Geophys. Res., 119 (23), 13079-13095, 2014. Liu, W.B., Wang, L., Zhou, J., Li, Y.Z., Sun, F.B., Fu, G.B., Li, X.P., and Sang, Y-F.: A worldwide evaluation of basin-scale evapotranspiration estimates against the water balance method, J. Hydrol., 538, 82-95, 2016.

Figure 3 – This might need more clarity on what time scales are used in the analysis underpinning the figure.

Good point. We have clarified the time scales used in the caption of Figure 3 as follows [Line 912-914 in the new version],

“Figure 3. Comparison of different ET products against the calculated ET through the water balance (ETwb) at the monthly time scale for 18 river basins over the Tibetan Plateau during the period 1983-2006. The boxplot of monthly estimates of different ET products for 18 TP basins are shown in (a) while the correlation coefficients and root-mean -square-errors (RMSEs, mm/month) for each ET product relatively to ETwb are exhibited in (b). ”

Line 333-353 – Could more be made of the discussion of Figure 4? The differences in catchment properties and their relationships to climatic influences are interesting.

Good idea. We have made more discussion in terms of Figure 4 in the revised version as follows [Line 371-376 in the new version],

“. . .The dominant climate systems are overall discrepant for the three TP regions with different water-energy characteristics and sources of water vapor. The westerlies-controlled basins are relatively colder than the Indian monsoon-dominated basins, thus they develop more glaciers (and thus have more snow melt contributions to total river streamflow) and have relatively less vegetation (and thus limit vegetation transpiration). . .”

Figure 5 – It might be easier to interpret this figure if both the primary and secondary vertical axes used the same range (i.e. so precipitation can be compared with ET and runoff). Indeed displaying the data as a bar chart could be preferable (e.g. at its

simplest, separate bars for precipitation, ET, runoff and implied storage change).

In the revised version, we have changed both the primary and secondary vertical axes in Figure 5 (Figure R5 in this response) using the same range as suggested by the reviewer. Thank you very much.

Figure 7 – Some of the VIC SWE estimates look unrealistic for some of the catchments and the range of scales on the secondary vertical axis complicates interpretation. I am not sure whether conclusions on SWE can really be drawn from this dataset. The VIC_SWE may have certain uncertainties due to the limit considerations of snow physics in VIC simulation (Zhang et al., 2014). However, there are only few SWE datasets available for the Tibetan Plateau (e.g., VIC_SWE and GlobSnow-2 SWE, all have considerable uncertainties) which could be used to infer the seasonal cycles of SWE in TP conditions (as we discussed in the uncertainty section). The only available datasets may uncertain for some TP catchments, but sometimes they are useful for other catchments (especially in the data-sparse region). Moreover, the value ranges of VIC_SWE in some basins (e.g., Yangcun and Nuxia) are obviously larger than other basins based on the VIC model simulations, we thus cannot draw the secondary vertical axis in the same scale (the VIC_SWE in some basins with relative smaller SWE values will become nearly a straight line and the seasonal cycles will have to be differentiated), but we have tried to draw the first vertical axis using blue color to make them more interpretable in the revised version (Figure R7 in this response).

Reference: Zhang, X., Tang, Q., Pan, M., and Tang, Y.: A long-term land surface hydrologic fluxes and states dataset for China, J. Hydrometeorol., 15, 2067-2084, 2014.

Section 3.2 – I am not sure if full use is made of the figures and their underpinning analysis in this section generally. I think more focused discussion of the results of the water balance (annual and seasonal) should be possible.

Please refer to the responses of general comment #1 and #3. Thank you very much.

Section 3.3 – Some of the discussion in this section seems speculative, particularly regarding the relationships of calculated trends with climate indicators. This is not a simple subject and I suggest that this section should be worded more carefully and discuss the drivers of trends in less definitive terms.

We agree. In the revised manuscript, we replaced them with more appropriate terms [Line 432-488 in the new version].

Section 3.4 – As discussed above, I do not feel that this is a sufficient treatment of uncertainty (see general comments).

Please also refer to the responses of general comment #1.

In addition, some of the references in the text appear to be misspelt (e.g. line 66 Immerzeel, line 99 Harris). I suggest that all references are carefully checked.

We have double-checked all references and have revised the incorrect ones accordingly. Thank you very much.

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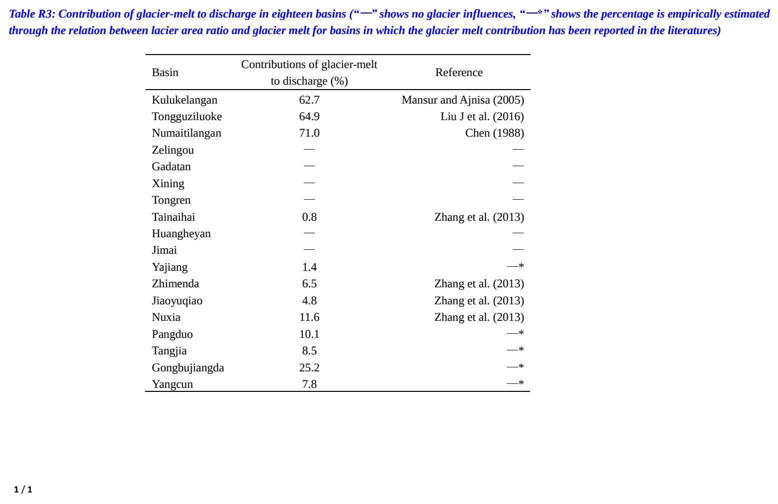

Table R3: Contribution of glacier-melt to discharge in eighteen basins ("—" shows no glacier influences, "—" shows the percentage is empirically estimated through the relation between lacier area ratio and glacier melt for basins in which the glacier melt contribution has been reported in the literatures)*

Basin	Contributions of glacier-melt to discharge (%)	Reference
Kulukelangan	62.7	Mansur and Ajnisa (2005)
Tongguziluoke	64.9	Liu J et al. (2016)
Numaitilangan	71.0	Chen (1988)
Zelingou	—	—
Gadatan	—	—
Xining	—	—
Tongren	—	—
Tainaihai	0.8	Zhang et al. (2013)
Huangheyan	—	—
Jimai	—	—
Yajiang	1.4	—*
Zhimenda	6.5	Zhang et al. (2013)
Jiaoyuqiao	4.8	Zhang et al. (2013)
Nuxia	11.6	Zhang et al. (2013)
Pangduo	10.1	—*
Tangjia	8.5	—*
Gongbujiangda	25.2	—*
Yangcun	7.8	—*

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Fig. 1.

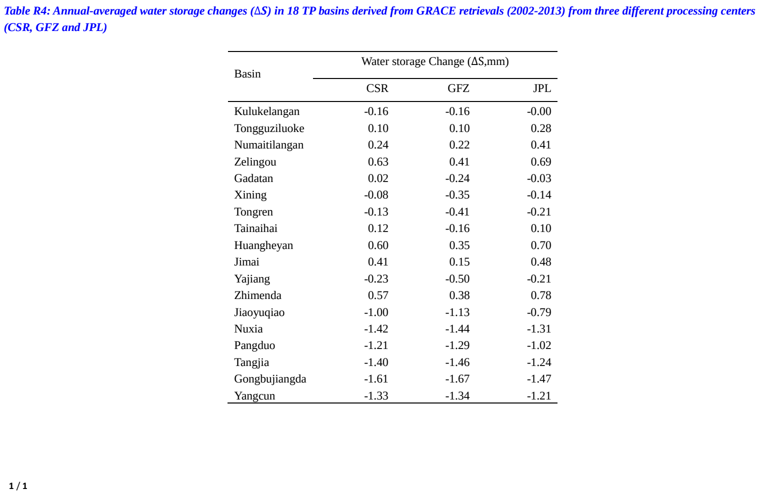

Table R4: Annual-averaged water storage changes (ΔS) in 18 TP basins derived from GRACE retrievals (2002-2013) from three different processing centers (CSR, GFZ and JPL)

Basin	Water storage Change (ΔS,mm)		
	CSR	GFZ	JPL
Kulukelangan	-0.16	-0.16	-0.00
Tongguziluoke	0.10	0.10	0.28
Numaitilangan	0.24	0.22	0.41
Zelingou	0.63	0.41	0.69
Gadatan	0.02	-0.24	-0.03
Xining	-0.08	-0.35	-0.14
Tongren	-0.13	-0.41	-0.21
Tainaihai	0.12	-0.16	0.10
Huangheyan	0.60	0.35	0.70
Jimai	0.41	0.15	0.48
Yajiang	-0.23	-0.50	-0.21
Zhimenda	0.57	0.38	0.78
Jiaoyuqiao	-1.00	-1.13	-0.79
Nuxia	-1.42	-1.44	-1.31
Pangduo	-1.21	-1.29	-1.02
Tangjia	-1.40	-1.46	-1.24
Gongbujiangda	-1.61	-1.67	-1.47
Yangcun	-1.33	-1.34	-1.21

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Fig. 2.

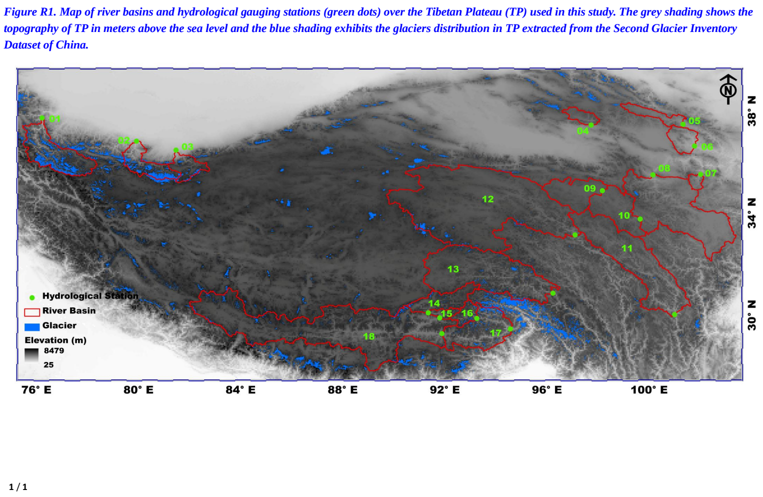

Figure R1. Map of river basins and hydrological gauging stations (green dots) over the Tibetan Plateau (TP) used in this study. The grey shading shows the topography of TP in meters above the sea level and the blue shading exhibits the glaciers distribution in TP extracted from the Second Glacier Inventory Dataset of China.

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Fig. 3.

Figure R2. Comparison of VIC_IGSNRR simulated and observed monthly runoff for Tangnaihai and Panduo stations (a and b) as well as (c) basin-averaged monthly TRMM, CMA gridded and IGSNRR forcing precipitations for the smallest basin (Tongren station) over the period 1982-2011. (d) shows the comparison of TRMM (blue) and IGSNRR forcing (red) precipitations against CMA gridded precipitation for 18 river basins over TP during the period 2000-2011.

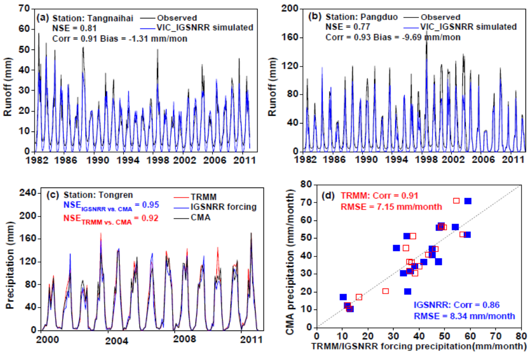

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Fig. 4.

Figure R5. Seasonal cycles (1982-2011) of water budget components in westerlies-dominated (column 1), East Asian monsoon-dominated (columns 2-4) and Indian monsoon-dominated (columns 5-6) TP basins.

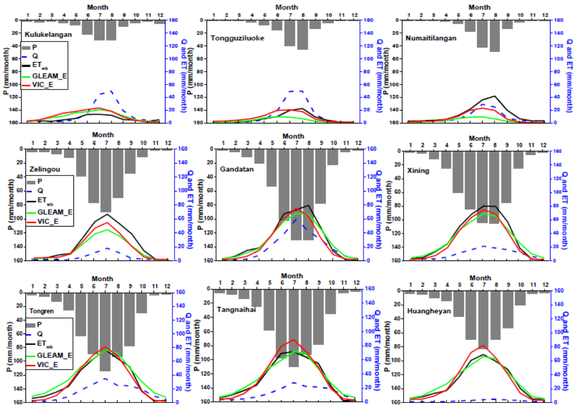

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Fig. 5.

Figure R5: (continued)

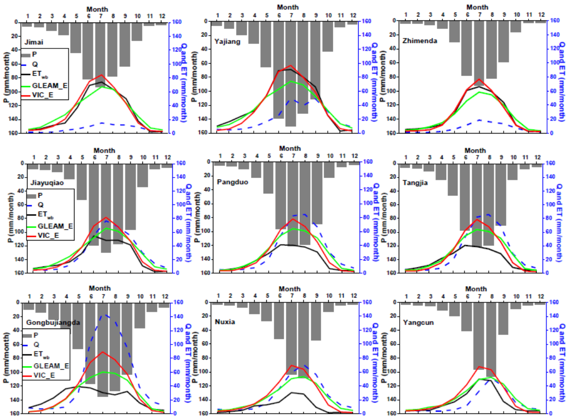

Fig. 6.

Figure R7. Seasonal cycles (1982-2011) of snow cover and snow water equivalent (SWE) in westerlies-dominated (column 1), East Asian monsoon- dominated (columns 2-4) and Indian monsoon-dominated (columns 5-6) TP basins. The snow cover was extracted from cloud free snow composite product during the period 2005-2013. It should also be noted that the GlobSnow data are not available for some basins.

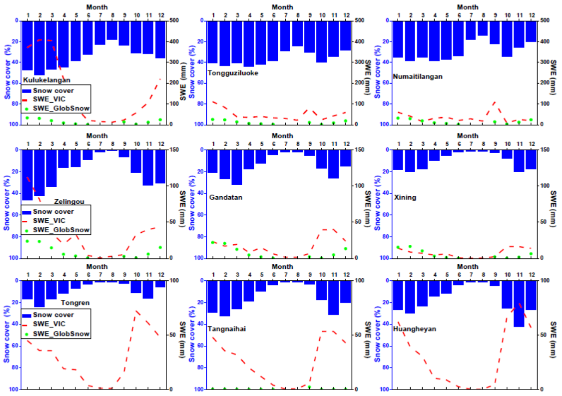

Fig. 7.

Figure 7: (continued)

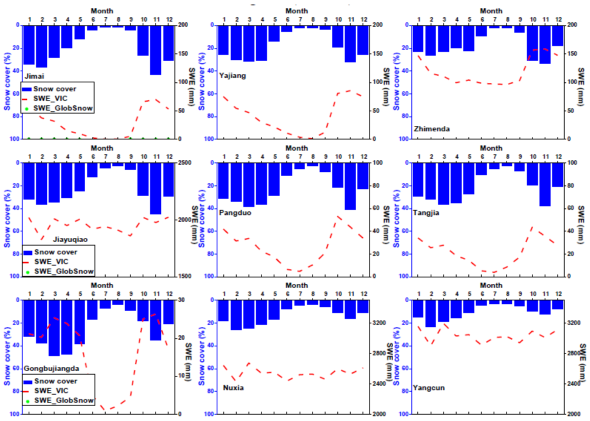

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Fig. 8.