Hydrol. Earth Syst. Sci. Discuss., 12, C1863–C1867, 2015
www.hydrol-earth-syst-sci-discuss.net/12/C1863/2015/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "What are the key drivers of regional differences in the water balance on the Tibetan Plateau?" *by* S. Biskop et al.

Anonymous Referee #4

Received and published: 30 May 2015

General comments

The manuscript by Biskop and coauthors presents a hydrological model application to lake basins on the central Tibetan Plateau. The authors force the hydrological model with distributed inputs provided by a 10 km x 10 km resolution atmospheric data set. The full water balance of closed lake basins is modeled in order to identify the key drivers of lake level changes. Since the reasons for lake-level changes are not fully understood and have been controversially discussed in recent literature this is a timely study that can provide a robust quantitative assessment about the importance of individual water balance components.

The study is robustly designed and the manuscript reads well. However, I have noted

down a number of points the authors need to address in order to clarify the discussion about key reasons and key drivers for current lake level changes.

Main comments

1. Better highlight the transient character of current hydrological system due to net glacier mass loss

The authors identify high correlations of annual lake volume changes with land runoff and precipitation. No correlations were found between annual glacier melt amounts and lake-volume changes in the four basins. The modeled relative contribution of glacier runoff to total water inflow was between 15 and 30%, from which the authors conclude that glacier runoff plays a minor role, compared to precipitation and snowmelt runoff, for the water balance on the TP (P. 4295, lines 1-5). They also conclude from those results that 'the positive water balance in the Nam Co and Tangra Yumco basins was caused by higher precipitation totals' (P. 4295, lines 7-9). However, I do not fully agree with this assessment. Figure 5 shows that glaciers in the Nam Co basin are not in a balance with the current climate since glacier runoff exceeds precipitation in the corresponding elevation bands of the basin by far. This means that current glacier runoff is largely due to net glacier mass loss, which is however not a sustainable source of runoff which will disappear once the glaciers reach a new equilibrium with the climate (after strong glacial retreat, identified e.g. by Yao et al. 2007). The authors should therefore make very clear if the water balance in the Nam Co and Tangra Yumco basins would still be positive without glacier runoff originating from net glacier mass loss. If not, this would substantially affect the conclusions that can be drawn from this study since glacier runoff would indeed be potentially the main driver of net lake level increases.

2. Provide a better documentation of past lake area changes

As the authors state correctly the time lag in the response of the area of a closed lake to climate fluctuations depends on the geomorphological characteristics of the lake: 'the higher the rate of change of a given lake's area with volume, the faster the lake can

adjust its area' (P. 4294, lines 16-17). The authors should therefore try to document past lake area changes in the four basins in order to understand if the differences in lake level changes are not simply due to differences in the lake response times.

3. Permafrost degradation

Permafrost degradation induced by rising temperature has been identified as another potential driver of lake level changes (e.g. Li et al., 2014). The authors should consider this at least in their discussion.

Detailed comments

- P. 4272, line 25: observational data 'are' missing

- P. 4276, line 12: Please also state which temporal resolution is used.

- P. 4277, line 22 ('are close to the changes rates estimated by. . . '): Please provide the numbers in the text as well and not only (subjective) qualitative measures.

- P. 4278, line 11 ('The J2000g model is a simplified version. . .'): Please be more specific in which respect the model is simplified.

- P. 4278, from line 23 ('the net water budget . . . was estimated by'): Evapotranspiration should also appear here.

- P. 4279, from line 10: which are the 'new model modules' which were required for representing the 'specific characteristics of closed lake basins on the TP'? Please be more specific.

- P. 4279, line 21 ('were regionalized using only IDW'): you can remove the 'only'.

- P. 4280, lines 17-23 ('In the absence of. . .'): This seems rather obvious. Consider to shorten this paragraph.

- P. 4282, line 15: 1 mm SWE seems a very low threshold for detection by MODIS. For comparison with MODIS I suggest testing higher thresholds. Gascoin et al. (2015)

estimate the values of best detection thresholds to as high as 40 mm SWE. This could also explain the significant overestimation of snow cover by the model (P. 4283, line 24).

- P. 4283, line 22: Please report already in the 'Methods' section that sublimation is a process which is taken into account by the model explicitly (and not only through the precipitation correction factor).

- P. 4285, line 10 ('spatially distributed pattern'): I suggest replacing 'distributed' by 'heterogeneous'.

- P. 4288, lines 6-8 ('The relation between. . .'): This is a repetition and also rather obvious. You can remove this sentence.

- P. 4288, line 16 (and elsewhere, e.g. line 22 same page): The authors write contribution of glacier melt was 'only' 14%. However, in comparison to the percent glacier area to total glacier area this is a lot. I would therefore hesitate to say 'only'. See my major comment 1 above.

- P. 4289, line 26 ('Our results are within this range (Table 4)'): Please report the values also in the text.

- P. 4290, lines 20- 29: For this comparison it would be useful to know how the SEB/MB models for Zhadang glacier were evaluated.

- P. 4293, lines 6-7 ('errors in the satellite-derived water volume data'): Is it possible to quantify the mean error in order to provide an uncertainty range?

- P. 4293, lines 12-13 ('Therefore, model outputs might also be influenced. . .'): This is obvious. Remove or change this sentence.

- P. 4293, lines 19-28: it should be stated somewhere which results the non-consideration of lake groundwater interactions could affect. Likely this would only affect the seasonal variability of the modelled lake level and not the multi-annual changes. To

which time scale apply the ranges of relative contribution of exfiltration/infiltration reported here?

References

- Gascoin, S., O. Hagolle, M. Huc, L. Jarlan, J-F. Dejoux, C. Szczypta, R. Marti, and R. Sánchez. (2015). A snow cover climatology for the Pyrenees from MODIS snow products. Hydrology and Earth System Sciences, 19(5), 2337-2351.

- Li, Y., Liao, J., Guo, H., Liu, Z., & Shen, G. (2014). Patterns and Potential Drivers of Dramatic Changes in Tibetan Lakes, 1972–2010. PloS one, 9(11), e111890.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 4271, 2015.