

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

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We would like to thank Prof. Su for handling the review process as well as the four anonymous reviewers and S. Zhou for their valuable comments and suggestions on the manuscript. We carefully addressed all comments offered by the four reviewers and the short comment posted by S. Zhou. In order to address some concerns raised by more than one reviewer, we start with a general response, followed by detailed responses to each reviewer. The original manuscript is referred to as V1 and the revised manuscript as V2. A formatted version of the authors' response was uploaded as supplement file.

GENERAL RESPONSE #1: Research objectives

Referee #2 raised an important question: “Which is the target of this manuscript, climate C3273

tology in water balance or water balance changes?” As indicated by remote-sensing data, lakes on the central TP experienced a continuous expansion since at least the 1970s, while lakes on the southern and western TP indicated a continuous shrinkage or were relatively stable during the last decades. Lake-level changes are generally caused by a shift in the water balance. This means that most of the lakes indicated a non-equilibrium state (i.e. imbalance between input and output) already before our study period (2001–2010). Due to the time lag of lakes in responding to climatic changes and the short time period of this modeling study, we cannot prove if the shift towards a positive water balance in the Nam Co and Tangra Yumco basins or the negative shift in the water balance of the Paiku Co basin over the last decades was caused by changes in precipitation, glacier runoff, evapotranspiration, etc.

The overall objective of this study was to identify differences in water-balance components of closed lake basins on the south-central TP during the 2001–2010 period. Therefore, Nam Co and Tangra Yumco with increasing water levels (i.e. positive water balance) and Mapam Yumco and Paiku Co with stable or slightly decreasing water levels (i.e. stable or slightly negative water balance, respectively) were selected. Distributed hydrological modeling was conducted to provide i) information on spatiotemporal patterns of water-balance components (e.g., altitudinal variations, seasonal dynamics) and ii) estimates of water-balance components in order to quantify their contributions to the water balance during the 2001–2010 period (V2, P3, L19–26). From this we cannot draw definitive conclusions about the hydrological changes that have led to the imbalanced water-budget in Nam Co, Tangra Yumco and Paiku Co; however, based on our modeling results and findings of other studies we discuss potential causes for long-term lake level changes in the manuscript (V2, Sect. 5.1.2).

GENERAL RESPONSE #2: Title of the manuscript

As a response to these clarified research objectives and to the comment of Referee #3 that four lakes are not representative for the entire TP, we decided to change the manuscript's title to “Differences in the water-balance components of four lakes in the

south-central Tibetan Plateau”.

GENERAL RESPONSE #3: Structure and content of the manuscript

According to the suggestion of Referee #1, we deleted the contents of Section 4.1 “Seasonal and inter-annual variations of hydro-climatological components in the Nam Co basin” to make the paper concise. A new Sect. 4.1 deals with model evaluation. Section 4.1.1 contains the comparison of simulated and measured lake-level of the Nam Co (as suggested by Referee #1 and S. Zhou), while in Sect. 4.1.2 simulated snow cover dynamics are compared with MODIS data for all four basins (as suggested by Referee #2).

The title of Sect. 4.2 “Regional comparative analysis of multiple lake basins” was renamed to “Comparative analysis of the four selected lake basins”. The title of Sect. 4.2.1 “Spatiotemporal patterns of hydrological components” was maintained, but the content is described in more detail. The title of Sect. 4.2.2 “Regional differences in the water balance” was changed to “Contribution of the individual hydrological components to the water balance”.

Section 5.1 “Comparison with other studies” in the discussion chapter was divided into two sub sections: “Estimation of the water-balance components” (Sect. 5.1.1) and “Factors controlling the water balance and lake-level variability” (Sect. 5.1.2). Section 5.2 “Limitations and uncertainties” was maintained, while Sect. 5.3 “Factors influencing long-term lake-level changes” was removed, but a large part of its content was integrated in Sect. 5.1.2.

POINT-BY-POINT RESPONSES TO ANONYMOUS REFEREE #1

SPECIFIC COMMENTS:

1. The results are very sensitive to the initial inputs of precipitation. The most uncertainty of this study exists in the meteorological inputs of HAR10. I'm confused about the overestimation of precipitation and underestimation of lake surface temperature. As

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shown by Maussion et al. (2014; 2015), HAR data has been validated and published. Why there exists such overestimation or underestimation?

RESPONSE: No atmospheric or geophysical dataset that we are aware of can be entirely “validated”. Maussion et al. (2014) compared the HAR precipitation products to station observations and TRMM estimates for the entire TP, but it is probable that the accuracy of the precipitation estimates is regionally dependent (as it is the case for global reanalyses). Maussion et al. (2014) found no systematic bias when considering all 31 stations in High Asia (their Fig. 03) but found regional differences (their Supplementary Fig. S9). Furthermore, the stations are all located in the valleys and no assessment can be done for the highest altitudes. Our manuscript is the first attempt to use HAR for hydrological modeling in the south-central TP region and as such provides a new approach to assess its uncertainty. A recent study by Pohl et al. (2015) showed that after applying a precipitation-scaling factor, HAR10 had the highest skill scores in comparison with observations in the Pamir Mountains (we added Pohl et al., 2015 in the reference list). We believe to have thoroughly described and discussed the uncertainties of HAR (V1, P4281, L14-17; P4291, L21-28, and more), but we clarified these points in the revised manuscript (V2, Sect. 5.2). Other factors might play a role, such as uncertainties in the hydrological model and the omission of certain physical processes such as sublimation of blowing or drifting snow (V1, P4292, L5-15). Currently, all these uncertainties are endorsed by the precipitation-scaling factor. With respect to the lake surface temperature (ST) it must be clarified that lake ST is not an output variable of HAR10 (V1, P4276, L13-19). In the WRF model version 3.3.1 (which was used for the generation of the HAR10 data), the lake-surface temperature is initialized by averaging the surrounding land-surface temperatures.

Besides precipitation and lake surface temperature, some validation works with in-situ observation need to be done. Indeed, there are some in-situ measurements from CMA (China Meteorological Administration) or CAS (Chinese Academy of Sciences) over the TP.

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RESPONSE: The comparison of the HAR10 temperature data with in-situ measurements indicated that the HAR10 temperatures in the summer months are closer to ground observations than in winter (Maussion, 2014). However, despite the winter cold bias, the overall seasonality is well reproduced (Maussion, 2014). The cold bias effect on the accuracy of the hydrologic-modeling results is assumed to be low, because hydrological processes governing lake-level changes are more critical during the other three seasons of the year (V2, P5, L20-26). Meteorological data from an automatic weather station installed on the Zhadang glacier in the Nam Co basin was used to validate HAR10 data in high altitude glacierized environments. The results show that air temperature, relative humidity and wind speed from HAR10 are in accordance with observations (Maussion, 2014; Mölg et al., 2014; Huintjes et al., 2015) (V2, P5, L26-30) (we added Huintjes et al., 2015 in the reference list)

2. It's not proper to use a fixed precipitation-scaling factor for the whole study period.

RESPONSE: Unfortunately, we are not sure to understand if the reviewer means different precipitation-scaling factors for single years? We agree with Referee#1's concern about using a fixed precipitation-scaling factor as pointed out in the original manuscript (V1, P4292, L2-4). However, there is no opportunity to derive varying scaling factors for single years for the four studied lake basins due to the lack of observations (V2, P21, L4-6).

Is it possible to tell the scientific community what are the main water vapour sources of increasing precipitation in Nam Co. Is it mainly from the plateau itself or surrounding regions?

RESPONSE: This is a very important question which is still under debate and will probably stay so in the close future. Recent efforts concentrated on quantifying the water vapor input to the Plateau (Feng and Zhou, 2012, Curio et al., 2015), but both studies did not focus on the water transport changes.

Feng, L. and Zhou, T.: Water vapor transport for summer precipitation over the Tibetan

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Plateau: Multidata set analysis, *Journal of Geophysical Research*, 117, D20, 1-16, 2012. Curio, J., Maussion, F., and Scherer, D.: A twelve-year high-resolution climatology of atmospheric water transport on the Tibetan Plateau, *Earth System Dynamics*, 6, 109-124, 2015.

3. As shown by section 5.2, there are many uncertainties about this study. Therefore, what we get should be a variation range rather than some specific values listed in Table 4.

RESPONSE: The precipitation-scaling factor was found to be the most sensitive parameter with the highest impact on model results. To provide a variation range of several water balance components, the results of the model runs with precipitation-scaling factors ± 0.05 with regard to the reference run were added to Table 4.

4. Some in-situ measurements may help to reduce the uncertainties. To my knowledge, there is a comprehensive station in the Nam Co basin constructed by CAS. Some hydrological and meteorological observations can be achieved to make some validation work for your model outputs.

RESPONSE: Lake-level observations from 2006 to 2010 provided by the ITP/CAS were used for model validation. However, lake level values during the freezing (wintertime) periods are missing, because the lake level gauge was destroyed by lake ice, and therefore, rendered inoperable each winter. Thus, data is only available for the ice-free period (May/June – November/December). The lake-level measurements began again each spring, but, unfortunately, an absolute lake level was not measured. As a result, the lake-level observation data contain an unknown shift between the consecutive years (V2, P6, L23-29; P11, L4-8; Sect. 4.1.1, Fig. 3).

TECHNICAL CORRECTIONS:

1. The paper only focused on four typical closed basins. The title should be specific. It's better to be replaced by 'What are the key drivers of regional differences in the

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water balance of four lakes on the Tibetan Plateau?’

RESPONSE: The title was replaced (see general response #2).

2. The contents of section 4.1 should be deleted to make the paper concise.

RESPONSE: Done. See also general response #3.

3. P4274, L1, ‘Cuo et al., 2014’ should be corrected as ‘Lan et al., 2014’. Similar correction should be done at P4297, L15.

RESPONSE: We are a bit confused, because we checked the first and the last name. We could find other publications from L. Cuo (for example in the reference list from the paper stated above), but we could not find a publication from C. Lan. Thus, we did not change ‘Cuo et al., 2014’ to ‘Lan et al., 2014’.

P4291, L1, ‘Li, B. et al. (2014)’ should be replaced by ‘Li et al. (2014)’.

RESPONSE: The initial of the first name was added by Copernicus Publications Production Office to distinguish between references with the same last name and the same year. See also V1, P4274, L3.

POINT-BY-POINT RESPONSES TO ANONYMOUS REFEREE #2

MAJOR COMMENTS:

1. The title and main point of this manuscript is the regional differences in the water balance among four lakes. Precipitation from HAR is post-processed by a precipitation scaling factor due to the overestimation in HAR. However, fixed factors are adopted for the four lakes and the results were used to analyze the causes of the regional differences in the water balance. This does not make sense. Given the predominant role of precipitation in the hydrological simulation, the scaling factor will have influence not only on the ratio of snow (glacial) melt and precipitation contribution in runoff at single point simulation; but also the spatial distribution among four lakes would be influenced greatly.

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RESPONSE: We agree that the adaption of HAR10 precipitation for each lake basin investigated in this study consequently raises issues about the reliability of the model results. However, due to the fact that errors in HAR10 precipitation probably vary regionally, there is no precipitation-scaling factor which is valid and applicable for the entire HAR10 domain. HAR10 data has been successfully used in glaciological modeling studies on the TP (Huintjes, 2014; Mölg et al., 2014; Huintjes et al., 2015) and recently in a hydrological modeling study in the Pamir Mountains (Pohl et al., 2015), but with the need to apply a precipitation-scaling factor < 1 in all studies. The values of the precipitation-scaling factor vary enormously among the several studies, from 0.37 in the Pamir Mountains (Pohl et al., 2015) to 0.79 in central TP (Mölg et al., 2014). Due to the scarcity of observation data and the difficulty to obtain meaningful correction factors from comparison with in-situ data, the only viable way to derive a precipitation-scaling factor is the evaluation of modeled hydrological quantities (Pohl et al., 2015). For the Zhadang glacier in the Nam Co basin, Mölg et al. (2014) found a very good agreement between glacier mass-balance model calculations and available in-situ measurements by applying a precipitation-scaling factor of 0.79. This value is relatively close to the precipitation-scaling factors obtained for the Nam Co (0.80), Tangra Yumco (0.75) and Paiku Co (0.85) basins by comparing modeled and satellite-derived lake-volume changes. This gives us confidence that the scaling factors used in our study seemed to be in an acceptable range (V2, Sect. 5.2). The relatively low precipitation-scaling factor of 0.50 obtained for the Mapam Yumco basin seems to be plausible when comparing HAR10 precipitation with weather station data of Burang ($30^{\circ}17'N$, $81^{\circ}15'E$, ~ 30 km to the south, closest station with available data) published in Liao et al. (2013). Huintjes (2014) also found that a reduction of the precipitation by more than 50 % leads to more reliable mass-balance results for the Naimona'nyi glacier (Gurla Mandhata, south western TP) which is located close to the Mapam Yumco basin. As the precipitation-scaling factor can compensate model-structure inadequacies, in particular wind-induced sublimation of suspended snow, the low value of 0.5 in the Mapam Yumco basin and the even lower value of 0.37 in the Pamir Mountains might be an indication that this pro-

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cess plays a major role in regions which are stronger influenced by Westerlies (V2, Sect. 5.2). At this moment, unfortunately, we have no other possibility than to use a scaling factor, in order to account for judged effective precipitation. Having said that, we think the different scaling factors do not affect the overall conclusions drawn from the hydrological modeling. We strongly believe that our model results contribute to a better understanding of the factors controlling the water balance and lake-level variability in the four studied lake basins.

2. Concepts of the climatology and change are kind confused. Which is the target of this manuscript, climatology in water balance or water balance changes? It seems authors refer to the precipitation is the key drivers in the runoff climatology because of the small proportion glacial there. For instance, it says glacial runoff is small due to small glacial coverage in section 4.1. It is self-evident. However, what interested are key drivers of runoff changes response to warming. Unfortunately, not much evidence of relative changes in precipitation, and glacial runoff are demonstrated. In Figure 3a (right), there almost no trend in precipitation in 2001-2010; however, warming is obvious. So, the rising in the lake table in Table 4 should attributes to the glacial runoff change due to warming. Accordingly, the water-balance components in the upper part of Table 4 show relative changes in the water balance components rather than values of ten years climatology. It is kind misleading.

RESPONSE: Thanks for rising this important point. We hope we could make our research objectives more clearly in general response #1 and in the revised manuscript. Furthermore, we clarified the conclusions drawn from the modeling results to avoid the Referee#2's misunderstanding that "[...] precipitation is the key drivers in the runoff climatology because of the small proportion glacial there." We do not think that the 10-year study period can allow us to reach conclusions about runoff trends in the studied basins due to warming. As pointed out in the general response #1, there was already an imbalance between input and output prior to the study period. Thus, we cannot answer the question which factors have led to the shift in the water balance. To our

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knowledge, there is no general agreement about the reasons for lake-level increases on the Plateau (e.g., Li et al., 2014), which might have complex causes (precipitation, evapotranspiration, glacier melt, permafrost degradation, etc.) (see our discussion in V2, Sect. 5.1.2). Moreover, we conducted an ice-free scenario for all basins to better understand the role of glacier runoff during the study period (V2, P15, L18-30). The upper part of Table 4 summarizes mean annual estimates of water-balance components and no relative changes. The table heading was clarified to avoid misleading.

3. As mentioned above, there almost no trends in precipitation in 2001-2010 letting alone the significance test. So, it is not convincing saying precipitation changes are the key drivers of the lake level in such short period. Some previous studies reported a long term lake level changes in recent decades (Yang et al. 2011; Lei et al. 2014). It was demonstrated that the lake level changes are consistent with P-E changes over the Tibetan (Gao et al. 2015).

RESPONSE: Unfortunately, this is also a misunderstanding (see response above). We hope that we could clarify the parts in the revised manuscript that have led to this misunderstanding. Long-term lake-level changes are discussed in V2, Sect. 5.1.2.

4. To show the difference from Mölg et al. (2014), suggest changing title as "What are the key drivers... Tibetan Plateau, precipitation or glacial melt?" or "What are the key surface drivers...?"

RESPONSE: Title was changed (see general response #2).

5. P4276 L20-29, why not use the consistent MOD11A2 land-surface temperature products for four lakes? The same as other land characteristics (land cover). These differences could lead to differences in four lake simulation. How these results could be used in regional differences.

RESPONSE: We preferably used the lake-surface water temperature (LSWT) observations from the ARC-Lake v2.0 data product as input for the estimation of long-wave radi-

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ation over water surface, because this data set does not contain outliers like MOD11A2 land-surface temperature data and no correction was required as in the case of MODIS. However, unfortunately, there are no ARC-Lake data for Paiku Co and Mapam Yumco and hence the MOD11A2 land-surface temperature data product was used (V1, P4276, L20-28). We compared daily ARC-Lake and MOD11A2 data for the Nam Co and found a very good agreement ($r = 0.99$, deviation between annual means: 0.01°C). Moreover, the model run with MOD11A2 had not led to any significant change that could influence our outcomes. Furthermore, we found that land cover classification data used in this study (they are all based on Landsat data – 30 m resolution) (V2, P6, L12-22) are more accurate than the globally available data sets (MODIS – 500 m resolution, GlobCover – 300 m resolution) in comparison to ground-truth data. Thus, we believe that the Landsat-based land cover classifications are a better choice than a global land cover data set.

6. P4277, MODIS snow cover are used for validation of the snow modeling in the Nam Co basin. Why not do the same validation for other three basins?

RESPONSE: We now use MODIS snow cover data for validation of the snow modeling in all basins (V2, Sect. 4.1.2, Fig. 4).

7. P4278, non-glacial runoff is generated by snowmelt and rainfall. Glacier runoff is generated by snow and ice melt. There is also rainfall over the glacial area. And, snow usually covers over the glacial area. So, the definition sounds not reasonable.

RESPONSE: The sum of snowmelt, ice melt and rain over glaciers is defined as glacier runoff in the model. The definition of glacier runoff was corrected in the revised manuscript.

8. P4285 L9-25, impervious layers might play a role.

RESPONSE: We are not sure to understand if the reviewer means impervious layers caused by the occurrence of permafrost. Unfortunately, we have no information about

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permafrost layers in the studied lake basins and thus the effect of impervious layers on altitudinal variations of runoff cannot be analyzed with the model. We added a discussion sentence about permafrost in Sect. 5.1.2.

POINT-BY-POINT RESPONSES TO ANONYMOUS REFEREE #3

SPECIFIC COMMENTS:

1. Four closed lake catchments in the south-central part of Tibet cannot be representative for the whole Tibetan Plateau. As reported in (Zhisheng et al, 2001; Tao et al., 2004; Yao et al., 2012), temperature, humidity and precipitation differently occur at different parts of the Tibetan Plateau. This makes it likely that different patterns of glacial changes and water level changes occur at different parts of the Tibetan Plateau. Maybe these four lake catchments could belong to the same pattern because of their locations.

RESPONSE: We agree with the referee. As stated in general response #1 and #2, the revised manuscript now focuses on four selected lakes across the south-central TP and not on different regional patterns any more.

2. Analyzing spatiotemporal patterns of water balance components seem to have been forgotten to describe in this manuscript. Four lake catchments are not enough to analyze a spatial statistics problem to determine a correlation of the water-balance components in different parts of the Tibetan Plateau. Furthermore, the water mass change of one or two lake catchments is not representative for a regional pattern.

RESPONSE: We agree with the referee.

3. Quantifying single water-balance components and their contribution to the water balance of a closed lake catchment was estimated by using the water mass balance modelling J2000g. In this paper, the authors applied it to four lake catchments, including the Nam Co catchment (Krause et al., 2010). What factors are improved in methodology?

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RESPONSE: In the previous modeling study of the Nam Co basin (Krause et al., 2010) coarse gridded climate data with a resolution of 50 km had to be used as input, due to a lack of climate data. In the present study, the model is driven by the new higher resolved HAR10 data (10 km). Lake-surface temperatures either provided by the ARC-Lake data or derived from MODIS data were used as additional data inputs for the estimation of the long-wave radiation term over the lake surface in the present study. In the earlier study of Krause et al. (2010) a simple catchment distribution with coarse modeling entities was used; whereas, a finer spatial discretization based on the traditional HRU-concept was applied for this modeling approach. Moreover, model adaptations and process implementations have been performed to enhance the representation of processes, with particular importance for the study area (see also the supplementary material of the original manuscript). The long-wave radiation part of the FAO56 calculation was modified according to the recommendations of Yin et al. (2008) (V1, P4279, L24-25). In the standard radiation module of JAMS, there is no distinction between the calculations for the net long-wave radiation for land versus water surfaces. Due to the fact that the exchange of radiant energy between the lake surface and the atmosphere in the form of long-wave (thermal) radiation is significant for large lakes, a new component for the calculation of the net long-wave radiation over lakes that takes into account water-surface temperature was implemented in JAMS (V1, P4279, L25-27). For the estimation of open-water evaporation rates from large lakes, the Penman equation modified by the addition of the lake heat storage was used. As suggested by Valiantzas (2006), the reduced wind function proposed by Linacre (1993) was applied for the estimation of evaporation from large open-water body surfaces (V1, P4279, L27-29 –P4280, L2-3). Within JAMS, the user can choose between a snowmelt module based on a simple day-degree approach (implemented in J2000g and used in Krause et al., 2010) or a more complex calculation method (implemented in J2000) that is principally following the approach developed by Knauf (1980). Due to the high importance of the refreezing process in the snow pack in the study region, the latter module that combines empirical or conceptual approaches with more physically-based

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routines was selected for this study (V1, P4280, L2-4).

Here, the topic focuses on the water mass balance of a closed catchment, so the authors should represent a temporal relation of water-balance components rather than that of climatic parameters during the observed period.

RESPONSE: We are not sure to fully understand this comment. If the referee means Figure 6 in the original manuscript (V1), the focus was on climate forcing in order to illustrate that inter-annual variations in lake evaporation are related to specific combinations of climatic parameters in individual years. However, this figure was removed, because the contents of Sect. 4.1 in the original manuscript (V1) were deleted (see general response #3). In Figure 5 and 7 in the revised manuscript (V2) we show seasonal and inter-annual variations of several water-balance components.

POINT-BY-POINT RESPONSES TO ANONYMOUS REFEREE #4

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 equi-

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librium 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.

RESPONSE: Thanks for raising this important point. Now we present a hypothetical scenario with ice-free conditions for each lake basin in order to clarify the importance of ice-melt runoff for the mean annual water balance (2001-2010) (V2, P15, L18-30). We think that this strengthens the discussion about the role of glaciers.

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.

RESPONSE: Thank you for providing this idea. We considered past lake area changes in the new discussion section (V2, Sect. 5.1.2).

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.

RESPONSE: This is another good point. However, as discussed in the revised manuscript (V2, Sect. 5.1.2) permafrost degradation cannot be considered in the model at this state.

DETAILED COMMENTS

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

RESPONSE: Done.

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

RESPONSE: Done.

- 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.

RESPONSE: Done.

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

RESPONSE: Done.

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

RESPONSE: Evapotranspiration is now included in the model description (V2, P9, L12-14).

- 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.

RESPONSE: Done.

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

RESPONSE: This sentence was completely removed.

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

RESPONSE: Done.

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- 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).

RESPONSE: We tested different thresholds (SWE > 1, 10, 50 mm) (V2, P11, L13-14). The results are presented in V2, Sect. 4.1.2, Fig. 4.

- 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).

RESPONSE: Done.

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

RESPONSE: Done.

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

RESPONSE: Done.

- 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.

RESPONSE: We agree with the referee and considered it in the revised manuscript.

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

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RESPONSE: Done.

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

RESPONSE: Simulated glacier-mass balance estimates were validated against in-situ measurements.

- 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?

RESPONSE: Unfortunately, there is no information on errors in the satellite-derived water-volume data that could be used 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.

RESPONSE: We removed this sentence.

- P. 4293, lines 19-28: it should be stated somewhere which results the nonconsideration 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.

RESPONSE: It would basically have a damping effect on the seasonal lake level because of an additional storage which is not considered by the model (V2, P23, L5-8). That means that the amplitudes can be overestimated. In terms of multi-annual changes we think that the effect can be neglected.

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

RESPONSE: Annual lake-water budgets.

RESPONSE TO SHORT COMMENT POSTED BY S. ZHOU

I mainly looked at the results of Lake Nam Co. I appreciate their work and agree with

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them that the key driver is the precipitation. However, I found large differences between their simulated results and the in-situ observations of most components of the lake water balance. Of the four lakes, Nam Co is the only lake with detailed longterm in-situ observations of lake water balance (Zhou et al., 2013). I wonder that, while they cited the paper by Zhou et al. (2013), they did not refer to it. So I would suggest the authors to compare their results with the observed data by Zhou et al. (2013). In addition, possible water seepage of the lakes could be taken into consideration. I would like to provide some important information: In-situ observations show that the average annual lake level increase of Lake Nam Co was only several centimeters during the period of 2005-2010 (unpublished yet), much lower than the data they adopted (22 cm per year, Table 2). Also, the precipitation was around 340 mm at Nam Co Station during the period from July through December 2006, which is much higher than their data of 270 mm for the whole year (P4283, Line 13).

RESPONSE: We thank S. Zhou for the kind words about our work. As mentioned previously, consistent lake-level measurements from Nam Co are only available for the ice-free period from the years 2006-2010. For the year 2005 we have only 30 values between the end of August and the beginning of November. Due to the lag of continuous lake-level measurements in the studied basins, we used satellite-derived lake-volume changes to calibrate the overall system response (i.e. lake-volume change). Thus, a possible inaccurate mean annual lake-level change obtained from the model could be related to errors in the satellite-derived lake-volume change. Due to an unknown shift between the consecutive years, we are not able to compare modeled multi-annual lake-level changes with in-situ measurements. So, we are wondering how an average annual lake-level change during the 2005-2010 period (unpublished yet) could be estimated without continuous time series. Moreover, it should be note that the modeled mean annual lake-level increase relates to the period 2001-2010, while S. Zhou refers only to the 2005-2010 period. It is difficult to compare mean annual lake-level changes from different time periods. Because of the unknown shift between consecutive years, we compared simulated and measured lake levels only for single years (V2,

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Sect. 4.1.1). Due to the large data gaps in 2005, we did not include the year 2005 in this comparison. The comparison revealed that the simulated mean monthly lake levels agree well with the measurements ($r = 0.81$). However, the modeled lake level indicates a non-systematic pattern compared to the measurements which might be related to errors in HAR10 precipitation (V2, Sect. 4.1.1; V2, P21; L7-9). Specifically, the model underestimates significantly the observed lake level in 2006 (V2, Fig. 3). This is likely due to a precipitation underestimation, which fits with the finding of S. Zhou that the precipitation measured at the Nam Co station was higher than HAR10 precipitation in this specific year. A possible HAR10 precipitation underestimation in 2006 would be increased due to the application of a fixed precipitation-scaling factor of 0.8. As already discussed above, there is no opportunity to derive varying scaling factors for single years. However, we believe that uncertainties which appear to be related to the precipitation-scaling factor, should not affect the overall conclusions drawn from the model results. Possible water seepage of the lakes is addressed in V2, Sect. 5.1.2.

Minor comments: P4284, Lines 11-13: The main reason should be the lower air temperature caused by more precipitation during the melt season (Zhou et al., 2010).

RESPONSE: The contents of Section 4.1 were deleted as suggested by Referee #1 (see also general response #3).

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/12/C3273/2015/hessd-12-C3273-2015-supplement.pdf>

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