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Interactive comment on "Effects of snow ratio on annual runoff within Budyko framework" *by* D. Zhang et al.

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Overview

This paper investigates how snow fraction (mean snowfall rate/mean precipitation rate) influences mean annual runoff, using a Budyko water balance approach for 282 catchments spread across China. The study is presented as an extension of recent work by Berghuijs et al (2014a), who found a strong role for snow fraction on annual runoff based on data from several hundred catchments located across the contiguous United States. The novel aspects of the presented manuscript are:

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Discussion Paper

1. Investigations of the role of snowiness on annual streamflow are performed for a new region (China). This is a relevant contribution as the physical processes that are causing the role of snow for annual streamflow are yet to be clarified. Hence, data driven studies are needed to test if similar behavior is observed in other regions than explored by Berghuijs et al (2014a).

2. An extension of the Budyko framework is presented that takes into account the role of snowfall. Previously the role of snow has not been included in Budyko type studies, but given the role it has on annual streamflow this extension can be considered relevant.

3. An assessment of future streamflow conditions is given based on climate scenarios and the developed Budyko framework with the snow extension. This approach considers the role of changing snow conditions explicitly, and thus these predictions do consider an important aspect of climate change impacts on annual streamflow that previously have mostly been ignored.

The paper thereby potentially makes a valuable contribution to understanding the role of snow for precipitation partitioning into streamflow and evaporation and develops a generally applicable method to address this issue. The topic covered in the paper therefore seems suitable for publication in this special edition of HESS.

However, before publication in HESS a couple of issues need to be addressed. First several of the assumptions underpinning the developed method need to be clarified and limitations of the method need to be better explained and acknowledged. Additionally, the language used in the manuscript is not always fluent and precise, and needs to be improved. I made some suggestions for improvement, but this list is not complete (please notice I am not a native speaker).

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Major comments

1. In your methodology you assume that melting snow water flows away through channels without evaporation loss (Page 947). Subsequently you use this assumption to derive a set of equations that is central to this study. You give several reasons to clarify why this assumption is acceptable (e.g. frozen ground, etc.). However, data clearly suggests that this assumption cannot apply in many regions of the world, and is unlikely to be representative in many other parts of the world.

   For example, in a classification study of Berghuijs et al (2014b) using several hundred MOPEX catchments, many of the catchments classified as snow dominated (snow fraction $>$ 0.45 + aridity (Ep/P) between 0.75$-$1.75) have a smaller runoff fraction (Q/P) than a snow fraction (Snow/P), implying that your assumption can not be representative in these catchments (even if it is assumed only snow produces runoff, and all rain is evaporated).

   Additionally, the catchments studies also expose a strong response in recharge of soil moisture and groundwater after snowmelt, implying that snow rather infiltrates into the ground and later is (at least partly) available for evaporation (e.g. Buttle, 1989; Dripps, 2012; Jasechko et al., 2014).

   The above mentioned observations limit the general applicability of your develop framework. Therefore you should (1) clarify the limitations of the applicability of the developed framework in regions where this assumption is wrong, and give an indication of the error this potentially introduces, and (2) indicate under what conditions the framework seems applicable, and under what conditions it is not applicable.

2. When you attribute runoff changes to snowfall changes (according to your description in section 3.2) several assumptions underpinning this attribution are not discussed/considered:

You do consider the role of topography and vegetation coverage as potential other secondary controls. Yet, other studies imply that precipitation seasonality and root zone storage capacity are the most important factors (after aridity) for determining annual streamflow (e.g., Milly, 1994; Wolock & McCabe, 1999; Potter et al., 2005; Berghuijs et al., 2014b). Why did you not consider these factors and discuss if it matters that you did not consider for these factors?

One my main concerns is that snow fraction is both a function of precipitation timing and temperature. This study does not explicitly consider precipitation timing, and consequently all differences in snowfall are attributed to temperature effects. Explain to what degree this may effect your study.

Although you use a split sample test to check whether the method appears to predict streamflow well (which it does for the historical time series!), attributing these runoff changes relies on the assumption that a spatial pattern (between catchment comparison) is representative for changing conditions at an individual site. The validity of such space-time symmetry using the Budyko framework has been investigated for specific cases using data (e.g., Sivapalan et al., 2011; Carmona et al., 2014) or model output (e.g., Roderick et al, 2014), but I see little evidence that this space-time symmetry applies (nor is there evidence that it doesn't!) for your analysis. Also given the fact that a new aspect is investigated, the symmetry is an hypothesis rather than something for which is a lot of supporting evidence

3. It is unclear to me why different E_p approximations have been used for the reconstruction of historical conditions compared to the projection of future conditions. I assume that this is due to data availability. However, this change of E_p approximation potentially strongly influences your future projections of streamflow as E_p method can give very different values (Federer et al., 1997; McMahon et al., 2013). Additionally, is there a reason you choose these E_p methods rather than solely net radiation as originally used by Budyko (1974)?

4. Precipitation is prone to under catch in snowy regions, and precipitation approximates often have largest biases in mountain ranges. What role do such potential biases play in your study?

5. Are there any other studies that provide a prediction of runoff changes China for similar future scenarios? If yes, how do they compare and can you better emphasise your novel contribution?

6. Page 945: Line 10-11 How do you calculate this adjustment parameter and where does this parameter comes from?. This needs to be 100% clear as this parameter strongly controls your prediction of future conditions.

7. The simplification from Eq. 10 to Eq. 11 gets inaccurate for catchments with a high snow ratio. Hence, the presented simplified method does not seem applicable to places with a high snowfall rate. Is this already a problem in your analysis for the more snowy catchments and is this a problem when somebody tries to apply the method in a region where most of the precipitation falls as snow?

8. You argue that your quantification of the sensitivity of annual runoff to snow ratio is more robust than by assuming linear correlation between these two variables as by Berghuijs et al. (2014a). However one could also argue (and maybe I am biased as I am the author of the other paper) that using historical variability to approximate future streamflow conditions is much more reliable than relying on the list of assumptions that your method needs. Therefore I am not sure if your claim for being a more robust method than the method of Berghuijs et al is actually valid.

9. You state that the study of Berghuijs et al (2014a) does not provide mechanistic understanding at the catchment scale, which is a motivation for your study. Yet, the only mechanistic explanation you give is by making assumptions about how the system functions. In many ways it seems your study is still an empirical

framework, that doesn't tell us why runoff changes occur under changing snow conditions. Can you better emphasise what we learnt about the mechanistic explanation compared to Berghuijs et al. (2014a)?

10. Considering all the above points, you need to better emphasise the novel contributions and the limitations of your paper.

Technical comments

1. Page 940: Line 2: Replace “winter" by “cold season" or remove the word “winter” because snowfall might also be in autumn and spring, and the precipitation state (snow/rain ratio) of these periods is probably most sensitive to temperature changes.
2. Page 940: Line 4: replace “but tends", by “but also tends"
3. Page 940: Line 20: Replace “winter" by “cold season” or remove the word “winter" because snowfall might also be in autumn and spring, and the precipitation state (snow/rain ratio) of these periods is probably most sensitive to temperature changes.
4. Page 940: Line 21: Unclear what you exactly mean by “Fluctuations in snow amount"; are these snowfall changes, snow storage changes, or both? And what do you exactly mean by “fluctuations” in this case?
5. Page 941: Line 12: Unclear what you exactly mean by “the climate change impact".
6. Page 941: Line 16: What do you mean by “localization of distributed models"?
7. Page 941: Line 17: Can you specify what the “large knowledge gaps" are?
8. Page 941: Line 21: Replace “new’ by “alternative".
9. Page 942: Line 21-22: Unclear what you mean by “all observed data being constrained by water and energy limits"
10. Page 943: Line 3: Unclear what you mean by “is not available at all the above . . .": is the data not available at any of these stations or is it available at some of them?
11. Page 943: Line 19: “by averaging the values of grids covering the analyzed catch-

ments". Does this mean that if 1% of a gridcell covers a catchment it equally contributes to the rainfall rate of this location as a gridcell that for 100% is located in the catchment?
12. Page 943: Line 20 “The interpolated grid temperature was modified by its elevation". How is this exactly done?
13. Page 943: Line 22: How are gridcells “water" and “non-waters" defined. Is a gridcell classified as one of them based on the percentage of landcover? If yes, what is this percentage?
14. Page 944: It is not clear to me where the net radiation values used in Equation 2&3 have been obtained.
15. Page 944: Line 4-8: you forgot the description and unit of T.
16. Page 944: Line 17: replace “(M)is” by “(M) is"
17. Page 947: Line 8: Does “a rough algebraic computation" results in the exact or approximate solution for Equation 9?
19. Page 950: Lines 7-8: It is unclear how you derive that “On the whole, the observed data is consistent with the curve pattern". Do you mean that the points are within the supply and demand limits of the framework?
20.

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