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Responses to Referee Comments by W.R Berghuijs

Referee comments in Italics

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:

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).

We appreciate the reviewer's thoughtful remarks and positive impression of our work. According to the suggestions of the reviewer, we recognize that this work's limitations

must be more explicitly stated within the text to ensure that we do not overstate the accomplishments of this analysis. Also, we will thoroughly go through the manuscript and further improved the grammars and wording. We hope our responses to the comments and the changes made to the text will be satisfactory.

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 cannot 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.

Thanks for the comments by the reviewer. We recognize that the application of this study has its limitation. The assumption that there is no evapotranspiration loss in snowmelt is a compromise between obtaining a concise expression and the lack of understanding on the role of snow on annual water balance at present.

Under the conditions described in Page 947 Lines 16-22, or in some small catchments, after snowfall is melt, the snow water can flow away quickly though channels without evaporation loss. The framework proposed here seems applicable. In fact, it may be more suitable to introduce $k \cdot (1 - r_s) \cdot P$ as "effective available water" for evapotranspiration, where k is a loss parameter need to be further investigated. In the future work, the isotope hydrological method may provide a tool to quantify the

parameter for different catchments.

Apart from the above assumption, the accurate estimation of snow ratio is also important for this framework. However, direct snow observation records are not available for the case study watersheds in this manuscript and the MOPEX watersheds used by Berguhijs et al. (2014). The mean annual snowfall is estimated by empirical method. The threshold temperature is critical for calculating the snowfall amount. A higher threshold temperature will overestimate the snow ratio that may lead to an unreasonable conclusion under the framework in our study.

According to the reviewer's comment and suggestion, we will add a part of discussion on limitation of this framework and future possible work in the revision, as follows:

4.6 limitation of revised Budyko framework

It should be noted that the assumption of no evapotranspiration loss in snowmelt adopted in Section 3.1 is not universally applicable. In small catchments, after snowfall is melt and the concrete frozen ground inhibits snowmelt infiltration, the snow water can flow away quickly though channels without evaporation loss. However, if the location of accumulated snow is far away from channels, or the snowfall amount is large, it will take longer for melt water to run off than the frozen soil thaws. In these cases, a part of snow infiltrates into the ground and later is available for evaporation (Dripps, 2012; Jasechko et al., 2014). In fact, it may be more suitable to introduce $k \cdot (1 - r_s) \cdot P$

as "effective available water" for evapotranspiration, where k is a loss parameter requiring further investigation. To better understand and parameterize the snowmelt loss by evapotranspiration, the site-specific modeling and isotope-based field observations may provide tools for more detailed modeling in the future.

Apart from limitation of the assumption, the accurate estimation of snow ratio is also important for this framework. However, direct snow observation records are not available for the case study watersheds in this manuscript and the MOPEX watersheds used by Berguhijs et al. (2014). Mean annual snowfall is estimated by the air temperature-based empirical method. The threshold temperature is critical for calculating the snowfall amount. A higher threshold temperature will overestimate the snow ratio that may lead to an unreasonable conclusion under the framework in our study. According to the sensitivity analysis of catchment parameter estimation, it shows that a small variation in snow ratio can lead to a significant change in catchment parameter when snow ratio is large enough to be comparable to runoff index. Thus, the accuracy of snow ratio is important to this framework especially when the snow ratio is large, which limits the applicability of this framework in those catchments.

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?

Thank you for this comment. As the reviewer stated, it is complex and still not very clear what factors affect the catchment parameter *n*. The relations between *n* and precipitation seasonality (sometimes rainfall depth) and root zone storage capacity have been discussed in the literatures (Donohue et al., 2012; Cong et al., 2014). In this paper, we focus on the influence of snowfall on the parameter *n*. All the discussion about topography and vegetation coverage is to indicate that the influence of snowfall still exists even when the topography or the vegetation coverage is same. In addition, the vegetation characteristics reflect the information of the precipitation seasonality and root zone storage capacity. Therefore, we did not consider them in this paper, though it is another value topic in Budyko's framework.

Refs:

Donohue, R. J., Roderick, M. L., & McVicar, T. R. (2012). Roots, storms and soil pores: Incorporating key ecohydrological processes into Budyko's hydrological model. Journal of Hydrology, 436, 35-50.

Cong, Z., Zhang, X., Li, D., Yang, H., & Yang, D. (2015). Understanding hydrological trends by combining the Budyko hypothesis and a stochastic soil moisture model. Hydrological Sciences Journal, 60(1), 145-155.

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 affect your study.

Good catch. Because the observation only records daily precipitation amount, the effect of precipitation timing on snowfall cannot be accessed in this study. And the method to estimate snowfall is also developed base on the same data. Furthermore, the topic here is the impact of mean annual snow ratio on mean annual runoff that is over a much longer time scale compared to precipitation event (daily). Our opinion is that effect of precipitation timing on the study is small. We will mention this point in the revised manuscript.

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 a hypothesis rather than something for which is a lot of supporting evidence.

Thanks for your comments. We agree that the space-time symmetry is still a hypothesis to be tested. The study based on it should be checked carefully as the reviewer commented. However, we think runoff change attribution analysis in this study is not based on that hypothesis. As stated in Section 3.2 Page 949 Lines 8-11, the catchment parameters n' of pre- and post-period are estimated by observations and the difference between them is attributed by the change in land cover. In the attribution analysis, we do not assume whether catchment parameters n' are the same in the two periods. In essential, the attribution equation (13) is a perturbation method (or, the first-order Taylor expansion), which is not based on the assumption that both between-catchment variability and between-year variability follow the same Budyko curve (space-time symmetry).

3.   It is unclear to me why different Ep 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 Ep approximation potentially strongly influences your future projections of streamflow as Ep method can give very different values (Federer et al., 1997; McMahon et al., 2013). Additionally, is there a reason you choose these Ep methods rather than solely net radiation as originally used by Budyko (1974)?

Thanks for your comments. We estimate Ep by using different methods due to the data availability as the reviewer mentioned. Outputs of most GCMs do not meet the data requirements for calculating Ep by the Penman-FAO equation. The monthly mean temperature is available for every GCM. Meanwhile, the monthly temperature is credible and can be used to calculate Ep by empirical equations, such as the Hamon's equation.

We agree with the reviewer that Ep values vary with estimation methods. Therefore, we conduct parameter calibration for each catchment to minimize the difference between two Ep estimation methods. We will explain how to do it in detail when replying to Comment #6.

We do not think there is much more difference between using Ep and net radiation. In Section 3.1, we employ the concept of "effective energy available for evapotranspiration" to account for the effect of snow on actual evaporation. It is more straight-forward and well-understood to use Ep to reflect evaporation capacity rather than net radiation.

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?

Good point. As the reviewer mentioned, the accurate areal precipitation estimation is difficult to measure in mountainous catchments. The effect of potential biases on some

flood events is significant. As for the mean annual water balance, as considered in this study, the effect may be less significant. What's more, how to evaluate the role of potential biases is out of scope of our study. What we did is collecting as many meteorological stations with precipitation records as we can to obtain more accurate estimation of areal precipitation.

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?

Good points. There are some related studied on runoff changes as mentioned in page953: Lines 27-28. The mountainous catchments show significant increasing runoff, partly caused by increasing snowfall, which is consistent with the analysis in our work. More other studies will be included to enrich the discussion according to the reviewer's suggestion. To our knowledge, almost all other studies are based on distributed hydrological models coupled with GCMs outputs whose shortcomings are specified in Introduction section.

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.

Yes. We agree with the reviewer that the accurate estimation of the parameter is important to prediction of future available energy for evapotranspiration, Ep.

As stated in Page 945: Lines10-11, we calculated mean annual Ep (2000-2010) by averaging daily values obtained by the Penman-FAO equation for each catchment. Meanwhile, we calculated mean annual Ep (2000-2010) by averaging monthly values obtained by the Hamon's equation for each catchment. The adjustment parameter is calibrated by minimizing the difference between the two mean annual Eps. The calibration was conducted for each catchment. Therefore, each catchment has its own adjustment parameter which is used to predict 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?

This simplification indeed causes inaccurate values. However, we think it may not result in a big difference. Eq. (10) can be reorganized as:

$$\frac{P-Q}{(1-r_s)\cdot P}=[1+(\frac{E_p}{(1-r_s)\cdot P}-\frac{0.14r_s}{1-r_s})^{-n'}]^{-1/n'}$$

$$\frac{P-Q}{P}=[(1-r_s)^{-n'}+(\frac{E_p}{P}-0.14r_s)^{-n'}]^{-1/n'}$$

When most of the precipitation falls as snow, assuming $r_s=0.9$**, then** $0.14r_s=0.126$**.**

Given that $E_p/P\sim 1.5$**, the relative error (RE) resulting from the simplification is**

$$RE=\frac{0.14r_s}{E_p/P-0.14r_s}=9.17\%$$ **. The RE increases with increasing** r_s**. A large** r_s **of 0.9,**

which we think is seldom seen in real catchment, only leads to RE less than 10%. So we think this simplification is acceptable.

Thanks for pointing this out. This comment helps us look into the simplification more carefully.

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.

Agreed. The wording in discussion paper is inappropriate. We will rephrase the relevant sentences and change Page 951 Lines14-16 it to:

"What's more, quantifying the sensitivity of annual runoff to snow ratio using a new approach based on the Budyko hypothesis may provide more insight into this phenomenon"

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)?

Thanks for this comment. The work by *Berghuijs et al. 2014* did inspire us a lot to put forward this study. We will be more positive when citing the relevant work in the revision.

In this study we aim to provide more insight into quantifying the relationship between annual runoff and snow ratio using a new analytical approach based on the Budyko hypothesis. The concept of "effective" water/energy available for evapotranspiration was proposed to account for the impact of snow on mean annual actual evapotranspiration. Our study made some progress in mechanistic understanding more or less, although gaps still persist.

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

Agreed. Discussions on the limitation will be added in the revision, as in reply to Comment 1.

Technical comments

1. Page 940: Line 2: Replace "winter" by "cold season" or remove the word "win- ter" 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.

Yes, "cold season" would be better here.

2. Page 940: Line 4: replace "but tends", by "but also tends"

Agreed. The change will be made.

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.

Agreed. The change will be made.

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?

Sorry for the confusion. We have hanged "Fluctuations in snow amount" to "Decrease in snowfall amount"

5. Page 941: Line 12: Unclear what you exactly mean by "the climate change impact".

We have changed this statement as "hydrological response to snow variation induced by climate change"

6. Page 941: Line 16: What do you mean by "localization of distributed models"?

It means site-specific nature of distributed models. A specific distributed model may perform well in humid area, while poor in arid area for its model framework, runoff generation regime, *etc*. So, the detailed distributed model may have its best performance under some specific conditions.

7. Page 941: Line 17: Can you specify what the "large knowledge gaps" are?

Change "Page 941: Lines 15-18" to:

However, large numbers of parameters and localization of distributed models limit us to clarify the dominant factors affecting the connection between snow ratio and runoff. Furthermore, the distributed model may perform well over short time scales, and large knowledge gaps still remain at multi-annual time scale that impede the pursuit of better understanding the effect of snow ratio on mean annual runoff.

8. *Page 941: Line 21: Replace "new' by "alternative".*

Agreed. The change will be made.

9. *Page 942: Line 21-22: Unclear what you mean by "all observed data being con-strained by water and energy limits"*

It means that mean annual actual ET is smaller than potential ET and streamflow is smaller than precipitation for all catchments.

Considering: All observed points are within the supply and demand limits of the framework.

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?*

The record of precipitation type is available before 1979, but there is no record of precipitation type for all stations since 1980. Therefore, Ding et al., 2014 developed an empirical scheme to discriminate the precipitation type. This method was employed in the study to determinates the state of precipitation and calculate the mean annual snowfall.

11. *Page 943: Line 19: "by averaging the values of grids covering the analyzed catchments". 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?*

Yes. Because the Angular Distance-Weighted interpolation used in this study (sorry for mistake in Page 943, Line 18) will produce smooth and even grid data (temperature, precipitation, *etc*.) and the grid resolution of 10×10 km is also sufficient enough for most catchments, we think error introduced by this method is acceptable.

12. *Page 943: Line 20 "The interpolated grid temperature was modified by its elevation". How is this exactly done?*

The gradient for change of temperature with elevation was estimated by fitting the relationship between observed temperature of stations which are used to calculate the targeting grid value and its elevation. Then the temperature of targeting grid was modified by grid-averaged elevation according to the gradient.

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?*

Yes. If more than 50% of the gridcell is water, then this gridcell is defined as water. Otherwise, the gridcell is thought as non-waters.

14. Page 944: It is not clear to me where the net radiation values used in Equation 2&3 have been obtained.

Thanks for pointing out. The radiation data is recorded in 118 of 743 meteorological stations. We estimated solar radiation using the Angstrom equation (Allen et al., 1998). The parameters in that equation were calibrated using the observed data for each month at the 118 stations with solar radiation observations, and their values for each grid were obtained from the nearest station. We will add this statement to "data sources" section in the revision.

[Ref] Allen, R.G., Pereira, L.S., Raes, D., Smith, M., 1998. Crop evapotranspiration: guidelines for computing crop water requirements. FAO Irrigation and Drainage Papers, 56. Food and Agriculture Organization, Rome.

15. Page 944: Line 4-8: you forgot the description and unit of T.

Thanks for pointing out. We will add this described in the revision.

16. Page 944: Line 17: replace "(M)is" by "(M) is"

Thanks for pointing out. A space is needed here.

17. Page 947: Line 8: Does "a rough algebraic computation" results in the exact or approximate solution for Equation 9?

According to Eq.(8) and relevant parameters (Page 946 :Lines 24-25, Page 947: Lines 1-7),

$$R_m / L = \rho_w W(h_f + C_i \overline{\Delta T}) \approx 1 \cdot (r_s \cdot P) \cdot (335 + 2.1 \times 10) / 2500 = 0.14 r_s \cdot P$$

Thus, Eq.(9) is an approximate solution.

18. 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?

The scatter of all 282 points follows the pattern of non-parameter Budyko curve which is similar to the curve derived with $n \approx 1.9$. But a more significant spread can be seen here compared to the analysis by Berghuijs et al., 2014. This sentence may make readers confused. We will delete it in the revision. Thanks reviewer's comment.

We thank Referee W.R Berghuijs for the insightful and detailed comments.
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Mar-15, 2015