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Reply on RC1

Hongkai Gao et al.

Author comment on "Diagnosing the impacts of permafrost on catchment hydrology: field measurements and model experiments in a mountainous catchment in western China" by Hongkai Gao et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-264-AC1, 2021

Reviewer#1

This manuscript attempts to understand the impact of permafrost on catchment hydrology in a small mountain catchment in west China. The topic is important. However, I feel there are some major problems of the manuscript need to be addressed. My comments are shown below:

Response: We thank Reviewer#1 for his/her positive comments on the importance of this research topic. Please find our replies to your detailed comments below.

1. The conclusions of exp1-4 is that it is important to consider landscape heterogeneity in models to accurately simulate the hydrological processes in permafrost regions. This finding is not novel because the study area is mountain regions with high elevation gradient, similar conclusion may be found at many previous studies using distributed models. I suggest the author should further analyzed how the landscape heterogeneity influence the permafrost distribution and the impact of permafrost change on runoff, soil moisture and groundwater depth. Another problem is that not all the regions in the study area are covered by permafrost, some are seasonally frozen ground. The vegetation on permafrost and seasonally frozen ground may be different. How about these effects influenced the runoff? I suggest the author further explain them.

Response: We thank Reviewer#1, for pointing out this important issue. Following Reviewer#1's suggestion, we did further distinguish this study catchment into permafrost and seasonally frozen soil regions. We found that permafrost mostly distributes in the higher elevation regions, covered with alpine desert. As Reviewer#1 expected, permafrost and seasonally frozen ground have different vegetation cover. To avoid any confusion, in the revised manuscript, we will make three changes. Firstly, we will change "permafrost" to "frozen soil" throughout the manuscript. Secondly, we will add a map of the study site, showing permafrost and seasonally frozen soil regions. Thirdly, to further investigate the impact of seasonally frozen soil and permafrost on plot scale, we will add one more soil observation in permafrost area, with profile measurements of soil moisture and soil temperature.

2. It is not clear what is the physical meaning of parameter D in exp 8. It seems to represent the water flux from surface to subsurface. If so, it should be calculated in the

model with time and it obviously can not be set as time-invariant. The parameters in the model need to be better physically explained.

Response: Yes, D represents the splitter between water flux from surface (fast response) to subsurface (slow response). I agree that it is an intuitive judgement to take account of a time-variant D while modeling. But as we discussed in the introduction, due to the scaling issue and the complexity of permafrost regions, many intuitive judgements in permafrost hydrology need to be tested and diagnosed, e.g. this parameter D. In this study, we used field measurements and DYNIA method to identify the timing and magnitude of D variation, which we believe is helpful to understand permafrost hydrology from top-down point of view.

3. Figure 7, It seems that the red line is not the best fit for the data points. Why?

Response: The red line is not for fitting the dots. It represents Ks = 80d, which is the bottom envelope of baseflow. The detailed equation derivation can be found in Section 4.2 and (Brutsaerts and Sugita, 2008; Fenicia et al., 2006).

4. The author found that a linear recession can well describe the flow recession processes, and a fixed parameter Ks = 80 d is identified. That may be related to the short study period (4 years) in this study. If a long period (30-40 years) is analyzed, Ks may be changed.

Response: We agree that Ks=80d is not a fixed or time-invariant value. Many previous studies (Ye et al., 2009; StJacques and Sauchyn 2009; Walvoord and Striegl 2007; Rennermalm et al. 2010; Niu et al., 2010) found the increasing trend of baseflow, and decreasing trend of intra-annual streamflow variability (Qmax/Qmin), because of climate change. This means the Ks, in the long term, is likely to change with permafrost degradation. But in this study, we focused on understanding the impacts of existing frozen soil on daily and event scale hydrological processes, of which the climate change effect could be negligible in such a short time scale. We have discussed this issue in Section 6.2. And in the revised manuscript, more relevant discussion will be added.

5. Figure 10 seems important. Can the frozen depth simulation be involved in the model to improve the hydrological simulation?

Response: Yes, Figure 10 is very important in this study. We highly appreciate your very constructive suggestion. Firstly, we would like to clarify that the frozen depth in Figure 10 is measured and not simulated. Secondly, in this study, our initial idea is to diagnose the impacts of permafrost on hydrological processes. Thus involving frozen depth to improve model performance is somehow outside the scope of this study. But anyway, during revision, we will seriously consider this suggestion, to involve the frozen-thawing process to improve hydrological simulation.