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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Climate-induced hydrologic change in the source region of the Yellow River: a new assessment including varying permafrost" *by* Pan Wu et al.

Pan Wu et al.

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1. In dozens of places there are grammatical errors, missing punctuation, or improper (or at least less than idea) word usage. I'd encourage the authors to have an English colleague read the paper carefully or to go through an English editing service. It is not in terrible shape, but it could be improved. Reply: This paper is further modified for reading.

2. Duan et al. (2017) tackle a similar problem to the present study but in a different approach. I'd encourage the authors to explain why their approach is superior, or at least why it might be preferred in some cases. This could occur in the intro or in

the discussion perhaps. In general, the authors should be highlighting that climate change can result in not just differences in the forcing, but also in the system itself (e.g. climate change can alter precipitation regimes which is the hydrologic loading, but it can also alter the hydrologic functioning of a landscape by altering the vegetation and permafrost or seasonally frozen ground distribution)

Duan et al. 2017. Distinguishing streamflow trends caused by changes in climate, forest cover, and permafrost in a large watershed in northeastern China. Hydrol. Process.31

Reply: Yes! We agree, this is a constructive comment. Accordingly, we discussed the modified method used in this study in Sect 5.2: In this modified analysis method, climate change directly impacts (DQc1) are calculated based on existing method and potential climate impacts (DQc2) are analysed by a linear regression analysis between catchment parameters and potential candidate variables. This mothed efficiently considered climate change impacts on catchment properties which provide a more reasonable assessment of human activity impacts. With this flexible method all potential factors can be considered in discharge change analysis by regression analysis, the residual errors of regression analysis possibly indicate impacts of the penitential factors which wasn't considered in regression analysis due to record limitation. Climate changes both impact on system and forcing are highlighted in Sect 1 as follow: Climate changes not only impact on climate forces (precipitation, temperature etc.) but also alter catchment properties (permafrost distribution, vegetation etc.) (Duan et al., 2017).

3. The introduction is generally ok, but I do not like the build up to the objectives at the end. For example, the authors state (P3, L10-13): "these relationships have not been previously examined in the SRYR". So is this just a case study then? If so, perhaps HESS would not be interested in publishing it. I would rather argue that the paper advances the science by further breaking down these offsetting or cumulative discharge disturbances, and specifically examine the role of permafrost degradation

–which has been largely ignored in past studies using these statistical approaches.

Reply: We agree. This sentence is changed into:

Permafrost degradation is mainly caused by climate change, so it should be considered in climate-induced discharge change analysis in the SRYR.

4. P5, L5, the daily frozen depth of the active layer was identified every month. This makes no sense. How do you identify something with daily frequency on a monthly basis? Also, here the authors say active layer, but is there truly an active layer everywhere – in many places there is no permafrost, correct? Anyway, this is all worded confusingly. The "frozen depth of the active layer" could mean the distance from the land surface to the bottom of the frozen zone (which unless there is a horizontal talik would mean all the way to the bottom of the permafrost during the winter) or it could mean the distance from the land surface to the top of the frozen zone, which would be better called the thawed depth. I'd suggest this be reworded Reply: We agree. Thanks for you carefully review. As you said, it is a misleading understand of active layer existed in our manuscript. Permafrost records are not available in these observed records of frozen ground. Because as shown in Figure2, all the MFD observation stations are located in the transition zone or seasonally frozen ground area. So only MFD can obtained from these stations, but the change of annual maximum frozen depth can also be used to indicate adjacent permafrost change and seasonally frozen ground change in this area. We reworded these sentences in Sect. 2.2:

Monthly mean value was obtained from the daily frozen depth of frozen ground and then used to estimate the annual maximum frozen depth (MFD) of the study period. The MFD value obtained from the monthly mean frozen depth was used to indicate the degradation of adjacent permafrost and seasonally frozen ground.

5. Figure 3 and P5, L20-25: How were these change points detected? Was any sensitivity analysis conducted on moving these change points incrementally forward or backward in time? Reply: These change points are used in a comparison anal-

ysis between this study and previous studies. The two change points were carefully chosen in considering change points used in previous studies instead of using theory-based method to detect them. The two change points (1990, 2002) was first defined by Tang et al. (2013) according to the change of zero-flow frequency. And according to the runoff change pattern as shown in Fig. 2, by using this two change points three periods can be divided: pre-change period (1961-1990), low-flow period (1991-2002), recovery period (2003-2013). The baseline period is same as the climate pre-change period defined by IPCC (IPCC, 2007). The first change points (1990) were also adopted by Zheng et al. (2009) and Meng et al., (2016) to analyse streamflow change in the SRYR. A theory-based detect method was used by Zhao et al. (2009) and three different change points are defined: 1985,1989 and 1989 for JM, MQ and TNH, stations, respectively.

The change points adopted by Zhao et al. (2009) are different from Zheng et al. (2009) and this study, however, by using existing method we obtained similar results of catchment properties change impacts on streamflow change as indicated in Table 3 and P 11 L 29-32. 6. The transition from Eq. 7 to Equations 8-10 was hard for me to follow. This should be reworded Reply: The transition is further explained and reworded in P 7 L 21-24 as following: The sensitivity coefficients , and in Eq. 7 are defined as partial derivative to respective variables. To eliminate discretization errors, the mean value of the sensitivity coefficients in the pre-change and post-change periods: , and are defined and used in following study(Jiang et al., 2015): 7. P8, L14-17, a key message of this paper is that climate change can influence the catchment specific parameter. This is likely true, but it should be explained rather than just citing a few other studies. If this is so important, the authors should provide some examples of why this might be from a physical perspective. Reply: P8. L14-17 revised as following: However, this is not true because catchment relative infiltration capacity and soil water storage are also related to climate factors: precipitation intensity and potential evapotranspiration, respectively. (Yang et al., 2007;), the catchment specific parameter can also be influenced by climate type (Williams et al., 2012) and climate change (Jiang et al. 2015).

8. Related to the above, the paper would be much improved by the authors tying in the physical hydrologic environment to the statistical analysis results. For example, why would rainfall-runoff processes be altered by changes in permafrost, particularly in the source region of the Yellow River. What soil is there? What slope? Based on this why might the runoff ratios change? Without tying the results to the physical setting, the entire results section comes across as a bit of an arm waving exercise Reply: Accept, according to this comment, Table 2 is added in the modified manuscript. Indeed! This article lack of physical explanations due to these analyses are based on statistical method. Potential explanations are added in Sect. 5.2 However, potential physical explanations can be found in previous studies. It has been found that the permafrost degradation could enlarge baseflow in cold regions (Walvoord and Striegl, 2007; Jacques and Sauchyn, 2009; Bense et al., 2012; Evans et al., 2015; Duan et al.,2017). Decrease in MFD because of global warming was considered as a major factor for the increase in baseflow in the Qilian mountain, China (Qin et al. 2016). Additionally, the melt ice within permafrost and increasing hydrologic connectivity fallowing permafrost thaw-induced land-cover change will increase the runoff discharge (Connon et al. 2014; Duan et al. 2017). According to these research, Further deeper investigations are required to link the rainfall-runoff and base flow behaviors with the physical mechanism of frozen soils and performed in the SRYR.

Minor comments I'd invite the authors to refer to Wang et al. 2018, which has some overlap with the present study (especially geographically) along themes related to changing MFD. They may be able to feed some data from this prior paper into their statistical approach.

Wang et al. 2018. Historical and future changes of frozen ground in the upper Yellow River Basin. Global and Planetary Change, 172, 199-2011

P1, L29 'are facing serious water shortages' – use of the present tense here might warrant a more recent citation as Yang et al. (2004) is now 14 years old. Reply: Modified as follow: Due to the dry climate and heavy water demands, people in the

Yellow River basin are facing serious water shortages in 1990s.

P3, L7-8, 'permafrost thawing to surface water discharge' - I don't really like this wording. It seems to imply that permafrost thaw produces streamflow (i.e. the meltwater is a significant contributor to streamflow). If so, that would surely be an incredibly fast thaw rate! Reply: Accept. Modified as follow: Permafrost degradation will increase the depth and length of subsurface flow paths and the lag-times of subsurface water flow from infiltration to surface water discharge.

P3, L30-40, I find this description confusing. The opposite ends of the permafrost spectrum are continuous and isolated, so why would the authors lump those into a single 'continuous' category. Also Figure 1 shows alpine permafrost, but does not indicate if this is continuous or discontinuous. I guess that is what the authors are explaining in that paragraph – the classification system is not standard Reply: Accept! The classification of permafrost was further explained in Sect. 2.1 P4 L1- 7. For this map, there are three permafrost classifications: predominantly continuous permafrost (70-80%), isolated permafrost (40-60%) and alpine permafrost. This classification scheme is different from that of the International Permafrost Association (IPA) (Cheng and Wu, 2007; Ren et al., 2012). In Figure 1b, the predominantly continuous permafrost and the isolated permafrost are further combined into the plateau permafrost in the Tibetan Plateau (Ren et al., 2012). Figure 1 was further modified according to this comment and attached in this reply. P4, L3, Why is the SRYR unique? This is not explained Reply: It is explained in P4 L 6-7. Due to the water resource significance and unique landscape, the SRYR provides an ideal location to observe the hydrological effects of degrading permafrost with climate change. P17, L15, what about aerial geophysical methods in permafrost? See Minsley et al. Minsley et al. 2012, Airborne electromagnetic imaging of discontinuous permafrost. Geophys. Res. Lett. Reply: Yes! Indeed! Airborne electromagnetic method is different from those classical methods. This kind of method can be employed in a larger area catchment. However, long-term dynamic state of permafrost is difficult to be obtained by this kind of method.

P17, L18-24, this paragraph is worded as though it were a key springboard to future work. I found the logic in the section hard to follow. Perhaps the wrong word is used in some sentence, or perhaps my mind is dense after another long day. But they seem to suggest that decreasing MFD is a positive factor and that this is enigmatic – but then they indicate later that Qin et al. 2016 showed this. Are they saying there are no physical explanation for this or that it is unusual? Reply: Qin et al. 2016 did show that positive correlation between decreased MFD and increased baseflow. However, it just emphasises permafrost degradation impacts on groundwater discharge are different from total discharge analysed in this study. Potential physical explanations were added in Section 5.2 as mentioned in reply of comments 8.

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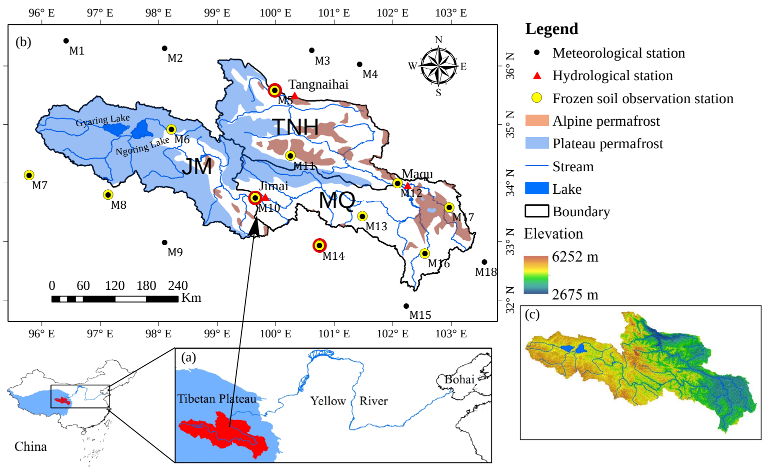

Fig. 1. Figure 1 (a) Location map of the source region of the Yellow River (JM, MQ and TNH) (b) Distribution of permafrost, meteorological and hydrological stations (The red circles indicates stations which h