

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.

Anonymous Referee #1

Received and published: 24 February 2018

General comments

Wu et al. conduct an analysis of long term climate and discharge data in the source region of the Yellow River to attempt to disentangle the different factors contributing to discharge changes. Their motivation for doing so, in addition to the obvious societal importance of the Yellow River, is that past studies seemingly attribute too high a portion of the discharge sensitivity (or at least discharge changes) to anthropogenic activity despite the relatively low population density. In essence, they modify the classic Budyko approach to demonstrate that changes to the hydrologic framework due to permafrost thaw (as measured by changes in the maximum frozen depth, MFD) produced

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by climate change are more likely to be dominant drivers in the discharge changes, than anthropogenic activity per se (although one might argue that the global-scale anthropogenic activity actually produced the climate change). These complicating effects result in offsetting discharge forcings and thereby increase uncertainty. Generally, the paper is understandable, reasonable, and it moves the field forward. I think parts of it could be communicated better and these modifications should be made before it is seriously considered for publication.

Major comments

1. In dozens of places there are grammatical errors, missing punctuation, or improper (or at least less than ideal) 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.

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

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

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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.

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

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?

6. The transition from Eq. 7 to Equations 8-10 was hard for me to follow. This should be reworded

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.

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.

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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.

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!

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

P4, L3, Why is the SRYR unique? This is not explained

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.*

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?

The figures are generally well done and interesting

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2017-744>, 2018.

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