

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2  
<https://doi.org/10.5194/hess-2021-377-RC2>, 2021  
© Author(s) 2021. This work is distributed under  
the Creative Commons Attribution 4.0 License.



## Comment on hess-2021-377

Anonymous Referee #2

---

Referee comment on "The Spatiotemporal Regime of Glacier Runoff in Oases Indicates the Potential Climatic Risk in Dryland Areas of China" by Xuejing Leng et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-377-RC2>, 2021

---

Review of "The Spatiotemporal Regime of Glacier Runoff in Oases Indicates the Potential Climatic Risk in Dryland Areas of China" by Leng et al. (2021)

The manuscript of Leng et al. derives timeseries of glacier runoff for the dryland areas of China for the period 1961 until 2015 using previously published geodetic mass balance estimates and APHRODITE gridded precipitation and temperature products. Their estimates of glacier runoff are used to indicate the amount of glacier meltwater that comes from the imbalance and balance component of glacier runoff (referred to as meltwater runoff and delayed runoff, respectively, in the manuscript) and to analyze trends. Their analyses are done for 22 basins in northwestern China.

While I find the topic of this paper both an interesting and valuable one for HESS and the dryland areas of China in general, this study is disjointed and left me confused as to the methods and validity of the results. The paper is not well organized, with some topics discussed in far too much detail, often without clearly informing the reader as to a particular method or result. Other topics are either not fully explained, or introduced in different parts of the manuscript, with sometimes conflicting descriptions. The writing and language of this paper requires major improvement if it is to be considered for publication, a careful reading is not sufficient to understand the methods and results – the reader is left to guess how many particular methods were conducted. The sloppy nature of this paper causes confusion with frequent occurrences fragmented explanations and changing or vague terminology and units. Like the Reviewer 1, I cannot recommend this manuscript for publication. However, because I see potential value in the work and great value in this topic, I have included more detailed comments below.

Major comments:

1. Many methods are not well explained: I don't understand how you get precipitation. You use the 0.25° spatial resolution daily precipitation datasets from the Asian Precipitation – Highly Resolved Observational Data Integration Towards Evaluation Of Water Resources (APHRODITE). But then you state that you "Used the Shean estimation to optimize the precipitation gradient per glacier". Shean provides annual mass balance – not precipitation, and does not use any precipitation data, so how specifically are you using his method or data? Equations 2, 3 and 4 only use elevation data and APHRODITE precip and temperature data.. not glacier mass balance data.

a. In the next line you state that you are using a precipitation gradient to correct the original APHRODITE data. You then use this gradient "PG" in equations but then never state how you get the gradients until L253-254: "PG in this paper was obtained by interpolation using the mass balance algorithm and geostatistics method". This is an important point and should be introduced together and fully described, currently this line doesn't tell us how you get the PG. (Also, is PG a widely used abbreviation for precipitation gradient? I haven't seen this used).

b. L214, "The precipitation was corrected by the Shean estimation for high-altitude precipitation gradients...", again, what is this correction, I can't find any precipitation gradient work in Shean et al. (2020). You have annual mass balance from Shean et al. (2020), precipitation data from APHRODITE, and then estimate ablation with a PDD model. How you use these three datasets in conjunction is not clear.

2. L153-156 The PDD values must be stated, what is the range of values? Perhaps show a map of them as a supplemental figure. Further, the uncertainty around these values should be quantified.

3. The terms monthly delayed runoff and meltwater runoff are poorly defined. Is monthly delayed runoff a mix of seasonal snow melt runoff and rainfall over the glacier? In lines 184-186 is Ta meant to be T1? In lines 290-292 you better clarify the terms, which should not be occurring in the results, and still leave the reader confused: "Glacier runoff included delayed runoff that was stored rainfall in the cold seasons and released rainfall in the ablation seasons, while meltwater runoff was caused by glacier mass balance, which was also called excessive meltwater runoff or the imbalanced part of glacial runoff". Do you mean stored snowpack in the cold season? Or both stored seasonal snowpack and stored rainfall in the cold season? Released rainfall in the ablation seasons?

4. Many other terms are undefined, e.g.: L347 What is "glacier runoff recharge"?

5. Some references are inappropriate.

a. E.g. in your submission you do not cite the information stated in L597-598 about California, then in your response to Reviewer 1 you state: "For example, due to increased temperature and reduced snowmelt or precipitation, California, in the United States, experienced a severe drought from 2011 to 2015 where hydroelectric power decreased by two-thirds due to declining runoff, including glacier runoff (Gonzalez et al., 2018; Rasul & Molden, 2019)." --- Rasul and Molden (2019) merely reference the Gonzalez work, and do not offer any data on this so is not suitable to be referenced here. The Gonzalez work can be found here:

[https://nca2018.globalchange.gov/downloads/NCA4\\_Ch25\\_Southwest\\_Full.pdf](https://nca2018.globalchange.gov/downloads/NCA4_Ch25_Southwest_Full.pdf), and does not ever mention glaciers.

b. An additional example is in L341 Barnett et al. (2005) is a review paper and did not “simulate glacial runoff” as you claim.

6. Using RGI glacier outlines for 1961-2018 is not appropriate for a 100m resolution study. At least an error analysis on the effect of not incorporating glacier area change should be added.

a. In response to Reviewer 1 you state that the RGI polygons were “All glacier extents were obtained started in the 1990s and finished in 2014 which is consistent with our research time.” This is close to the case, but as pointed out by Shean et al. (2020)

b. source image timestamps used for RGI polygon digitization (~1998–2014) and the DEM timestamps. This means that the polygons were digitize ANY time between those dates, and contain information about the date.

c. So using a single polygon of 1998-2014 origin is either not appropriate, or requires an uncertainty analysis (which should be included regardless).

d. Also, as detailed by Guo et al. (2017) the first Chinese Glacier inventory was finished in 2002 and covered CGI-1 was compiled based on topographic maps and aerial photographs acquired during the 1950s–80s – so would be a potentially suitable starting outline for you study, then updating to the CGI-2 dating to 2006-2010, compiled by Guo et al. (2017): <https://www.cambridge.org/core/journals/journal-of-glaciology/article/second-chinese-glacier-inventory-data-methods-and-results/386DAB512F4869D3335E2DE24B0F43EB>

Specific comments:

Any use of numbers should spell out the number if below 10, e.g. 7 regions --> seven regions.

Please use significant digits e.g. L107-108

L15 Is this total annual glacier runoff? Specify time in the sentence or units.

L29 Add “Glaciers and ice sheets are the...”

L35 This is not a feedback, it is a one-way relationship, glaciers melt, producing runoff, increasing sea level. These citations do not fit your point.

L51-57 The problem is not clearly stated here. You state that “Continuous yearly mass balance data for long time series could not be calculated effectively due to the time consumption and high energy consumption of field observations (Brun et al., 2017; Shean et al., 2020).” This is poorly worded and incorrect. Long time series cannot be calculated because the data don’t exist, which in turn is because field observations are logistically

and financially difficult.

L62 Two problems here, one, this is an incomplete sentence, and further, why is the resolution so low? Perhaps because the data are not sufficient to use at finer resolutions? "...while the energy balance model could be applied in 62 large regions but with low resolution (such as 0.25 degrees (Sakai et al., 2015))".

L99 erased the range?

L102 7 glaciers? Or 7 glacier regions?

Figure 1 Font is too small in axes and legend (commonly in many figures)

L122-123 Doesn't make sense. You are implying that you did the work of Shean et al. (2020).

L126-148 Way too long of a description of these studies, if you want to show the comparison in lines 136-145, use a table, this is hard to read.

L267 Blocks represent modules (add the s)

L289-290 "and we overcame the difficulty of large-scale geodetic mass balance assessment" What? Brun et al. (2017) and Shean et al. (2020) did this.

L300-301 "The creeks of the Kriya Rivers basin were the most unique, with 93.67% of the components coming from delayed runoff; therefore, more attention should be paid to glacier disasters in this basin", wouldn't the opposite be true? Delayed runoff is not directly from glacier wastage (stored seasonal precip), so is more sustainable than ice wastage.

Figure 3 Units on runoff?? The legend just says "5.6". How useful are raw runoff numbers versus percent contributions of total river discharge?

L323-338 Replace most this paragraph with a table and reference that table with a few lines.

L324 Glaciers should be lowercase.