

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
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Comment on hess-2021-274

Anonymous Referee #2

Referee comment on "Climatic and anthropogenic drivers of a drying Himalayan river" by Gopal Penny et al., Hydrol. Earth Syst. Sci. Discuss.,
<https://doi.org/10.5194/hess-2021-274-RC2>, 2021

The Penny et al. manuscript presents a variety of observations to investigate why the water balance for a Himalayan river has changed. They highlight the limits to a simple water balance calculation and instead include information on land-use, NDVI and water extent as markers for differences in vegetation or crop productivity (i.e., evapotranspiration) and groundwater contributions to surface water, respectively. They focus on the changes between a 15-year (1984-1999) and a 13-year (2000-2013) period, and approach the work as a 'method of multiple hypotheses', testing various hypotheses related to potential drivers of the change in streamflow. I found the manuscript relatively well-structured and I think that it can be a good addition to the literature and a good resource for water managers in the Jhelum river region. However, I do think the manuscript needs some work before it is ready for final publication.

From a clarity perspective, I found that some words were not well defined ("secondary data", "drivers", "changing climate") and that the reader would benefit from describing these better. From a structural perspective, I found that the methods and results were not always matching up. How was the baseflow index calculated? How is a storm event characterized? Why is a basin-average correct value for precipitation preferred over a locally correct value? How were statistical analyses performed?

What was also clearly missing in the manuscript were uncertainty estimations on the data used to test the various hypotheses. The authors indicate an uncertainty in their initial water balance estimation of 15% (which by the way is more than increase in ET and almost as much as the decrease in streamflow of 117 mm?). An estimation of the uncertainty related to individual components of the water balance would be helpful here – or is it 15% of each component? Uncertainty estimations were given for **none** of the following analyses... (precipitation estimations from remotely sensed observations, evapotranspiration estimates using crop factors, NDVI estimations when only few (≤ 5) images were available, and inferring catchment storage from surface water extent). I expect there to be considerable uncertainty in each of these estimations, and expect that to be quantified and discussed in the text of the updated manuscript.

Detailed comments:

Title: What do the authors mean with 'secondary data'? – being specific would be helpful here to guide the potential reader to reading the article.

L28 Instead of pointing out that there are many watersheds where the hydrological drivers are unknown, which is not surprising given the amount of watersheds globally, highlighting prior studies that did identify and quantify drivers would be more helpful.

L34 "Their associated drivers" which drivers are meant here?

L76 "finding"s please add the s

L78 remove "by"

L124-129 consider removing this text since a standard manuscript format is followed, or adapt such that it contains information that is specific to this manuscript.

Fig 1 The panels 1d-g are square, but they are not square where they are indicated on map 1b. Did the coordinate system change? Then please indicate the new coordinates along insets d-g. If not, please make sure that the indication and maps match. Also, for me it would have been helpful to have the line of the river drawn in panels d-g, and the river and catchment boundary are not shown in the legend (I assume light blue and black lines).

Fig 2 snow density is needed to calculate water content from area and depth, and should be included in this scheme as well as in the calculation.

L173-174 define "changing climate" and justify how a dataset that is shorter than 30 years can be representative of a change in climate.

L178 'a reduction in storm size' is not captured in the hypotheses yet – greater storm frequency does not always mean in a reduction in storm size. Also, there was no explanation on how 'storms' were characterized (precipitation magnitudes separated by an

interval of how many hours at least?). Lastly, in many parts of the world a 'storm' can just be a period with a lot of wind, and does not necessarily imply rainfall. Please consider changing it to 'rainfall event' or 'precipitation event'.

L181 The authors hypothesize here about greater bare soil evaporation, but included no information on soil moisture (only groundwater), neither did they explain why this information is not considered.

L185-187 What does it mean for the water balance estimation that a Thiessen polygon results in better matching observations representing the elevation gradient in precipitation? What does this mean for the degree of reality with which the authors represent real-world processes? From the text here, it sounds like the water balance calculation might be quite wrong, and a precipitation distribution that the authors know is not correct has been selected to fit the uncertain water balance calculation. Please add more explanation/justification to show that the Thiessen polygons are still the best representation of precipitation, although they might not reflect the real-world precipitation patterns. A map showing where the two are different and how different they are could be a good start.

L227 how are season defined, and do these match up with crop life cycle? If not, how does that influence the analysis presented?

L265 I am not convinced that hypothesis 8 or 9 are sufficiently supported with the data as presented now. For instance, quantifying by how much the snowmelt date and peak streamflow date have shifted would give better insight in their relation than just only stating "both were earlier, so they appear to be connected". Hypothesis 9 is not based on independent observations (as shown in Fig 2) but on an inference that because evapotranspiration estimates were higher less water will have recharged the groundwater so that now groundwater contributions are lower. Lastly, the use of 'baseflow' analysis in Figure 2 applicable to hypothesis 9 is confusing, because 'baseflow' analysis is a common term used for the analysis of water level data. In this case, there is some analysis of streamflow data (for which there are no methods - confusing?) but the bulk seems to be based on the extent of surface water, which to me is a less common of the term 'baseflow analysis'. Again, an estimation of the uncertainties that are involved are necessary here.

L292 remove "by"

L310-317 This section is not clear to me. A drawing would be helpful.

L355 what is the threshold for a 'storm' if storms smaller than 1mm are included? Would the findings also hold if only storms > 5mm are included? (a common threshold)

L360 32 mm < 15% of 311 mm... please make clear to the reader that the change is smaller than the uncertainty.

L378 please quantify "a substantial level of uncertainty", and the implications that has for this analysis.

Fig 6 I spent five minutes looking at this figure, but still don't fully understand what is shown. Moving the legend from panel a outside of the plot region would certainly help, but then still, I am not sure what the individual panels show and how the panels work together.

L428 how was the baseflow index calculated? What is the associated uncertainty?

Fig 8 how was the significance of these analyses tested?

Fig 9 to me it's not clear what the different subsurface layers mean. Soil? Groundwater? Saprolite?

L475 Doesn't the conclusion that NDVI was leading for ET follow from a model in which ET is calculated from NDVI? And, why did NDVI increase? The authors describe that warming temperatures were a reason for NDVI to increase, but said that temperature did not influence (evapo)transpiration directly? Precipitation amounts were much lower, and an increase in storms < 1mm don't bring much moisture to the soil either, that will be evaporated directly from the plant leaves. How can the climatic conditions be more favorable, while moisture and temperature apparently don't play a direct role?

S1 pleas add 1:1 lines in the mass-mass plots

S2 what does the overestimation of precipitation say about the accuracy of your input data? What do the authors reckon is the importance of the spatial distribution of rain vs. the basin-average precipitation estimation. Spatial distribution might be very important, which is indicated by the small change in Q for various gauges. Commenting on this would be appreciated.

S6 what was assumed when zero or only few (defined as less than 5 in the text) images were available?