Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-626-RC1, 2020

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Annual variation characteristics of Eurasian hydrologic elements and their linkage with climate and environment changes during 1951–2015" *by* Jia Qin et al.

Anonymous Referee #1

Received and published: 8 January 2020

The authors have performed substantial analyses on a number of datasets to try to say something about variations over the last several decades in hydroclimate, as represented by the low, mean, and high discharges measured in a number of Eurasian river basins. Overall I applaud their efforts, but I found the exposition a bit difficult to follow and not always convincing. I've reviewed hundreds of papers over the years, and this was, for me, one of the most difficult I've had to review – not based on content, but based on the presentation. There is good science in here, but it's hidden. I recommend major revision before this paper is accepted.

1. The English is poor, enough to reduce significantly the effectiveness of the discus-

Printer-friendly version

Discussion paper

sion. Substantial editing would be needed – not just in terms of correcting individual words but also in terms of the phrasing of arguments. I had a very difficult time getting through some of the longer paragraphs in the results and discussion sections.

2. Much of the data used here is not described well, and appropriate caveats or qualifications are missing. By not discussing the weaknesses of the datasets, the authors are essentially implying that they are all accurate enough for the analyses performed. This may or may not be true. a. What is the rain gauge density underlying the precipitation product? The gauge availability in high latitudes would probably be insufficient for particularly accurate data. Caveats are needed. b. The SWE data are said to come from the Terrestrial Water Budget Data Archive. Some description is necessary. Is this a model product? SWE is notoriously difficult to measure from space, and in situ measurements are presumably not comprehensive. Model products (including reanalyses) are going to be error prone as well, so it's hard to believe that available data will be highly accurate. Again, qualification is needed. c. Data for surface energy budget terms and for soil moisture and temperature are taken from daily reanalysis. I see from the website quoted that these are Interim data rather than ERA5 data; this should be spelled out. Also, appropriate caveats regarding the accuracy of these data, particularly in areas that aren't well measured, are needed.

3. The concept of "trend" is used too loosely in this paper. Typically a trend refers to somewhat consistent changes over multiple decades. Despite what's stated on lines 109-111, I don't see any obvious trend for MD and HD in high latitudes (certainly not a statistically significant trend), and in low latitudes, a trend is seen for MD only if the final ten years are included – before that, there's no trend at all (i.e., for all we know, we could be looking at decadal-scale variability). (Also, it's very strange that the middle latitude MD and HD time series is described as a "slight decline" when this trend seems so much stronger than that for the high latitude MD and HD time series.) Line 123 says that the speed of ice-period shortening during 1996-2012 was intensified compared to that happening earlier, but from Figure 2, except for the Yana, it's not obvious that

Printer-friendly version

Discussion paper

there was a statistically significant trend before 1996. By eye, it's not clear that there's any significant trend in Figure 7, despite the statement on line 144 – plenty of decadal variability, though. Again, talking in terms of trend rather than decadal variability is perhaps reading too much into the data. In any case, significance testing for trends is needed throughout. (I did see some p values listed here and there.)

4. The discussion on p. 7 (lines 190-218) was especially difficult to wade through, and I wasn't especially convinced by the arguments – for me, they came off as convoluted. I read through this text several times and am still not clear on the arguments. The discussion regarding permafrost, for example, comes off as unnecessary speculation. Perhaps breaking the paragraph down into more digestible bites would be useful. Here the concept of trend seems especially unclear, with discussion, for example, of an increasing LH trend up to 2000 and a decreasing trend thereafter (lines 216-217). It makes sense to talk about this in terms of decadal variability, not trends. (Also, in Figure 10, why are there breaks in the data in 1994 and 2009? Aren't the reanalysis data complete?)

Minor points

– A map is needed to show where the river basins are located and which ones are included in the high/middle/low latitude categories. This will help the reader understand how representative these basins are for Eurasia in general.

– People will be confused by the term "Arctic-few discharge" in the abstract and on line 290.

– Line 73: Is the CERA-20C dataset really a climatology dataset, or do the values vary from year to year? I assume the latter. Are these reanalysis data?

– Line 78: It probably should be stated explicitly here that sublimation impacts on the snow water balance are neglected. This could be a questionable assumption, as estimates in the literature seem to suggest that sublimation accounts for ~10-20% of the snow water balance.

– Line 93: What is SPSS software?

– Line 105 and throughout the text: “altitudes” to “latitudes”?

– I don’t understand the slope ratio calculation on lines 165-166. Why not just look at Rˆ2 to determine how river ice to winter LD covary? This would make a lot more sense. The same comment applies to the calculation described on line 178.

– Line 197: Rs to Hs?

– Lines 204-205: I don’t understand this sentence; there must be a typo.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-626, 2019.