

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.

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

Reply: Thanks for your comments and suggestions. We agree with the opinion. We seriously revised the manuscript and have asked for an editor of a professional translation services company to check and revise the English of the manuscript. All the corresponding revisions will be marked in the manuscript.

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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.

Reply: We fully agree with you and revised the questions one by one in the manuscript (in red). a. The rain gauges density in the study area and the accuracy of precipitation product has been added in the manuscript (Line 111-116). b. The SWE calculations and the data evaluation were added in the line 85-90. c. The description of ERA-Interim data and some caveats were added in the line 92-97.

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 annual 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

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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 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.)

Reply: Thanks for the comments and suggestions. We have revised the corresponding inappropriate sentences or words throughout the text. The significance testing also was added. The specific revisions were in red in manuscript (lines 131-134; lines 146-147; lines 167-173; lines 227-231)

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?)

Reply: we have rewritten this part, and split it into two paragraphs. The analysis was focus on decadal variability, and the unreasonable statements about “trend” were eliminated in the text. The data in 1994 and 2009 in Figure 10 was not complete and abnormal. We have added some explanations in the section “Materials and methods” (lines 95-97).

Minor points – A map is needed to show where the river basins are located and which

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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.

Reply: Thanks for the suggestion. Table 1 listed the locations of the controlled hydrological stations of the eleven major river basins in Eurasia. They are categorized in different latitudinal zones, and the longitudes of the river basins (controlled hydrological stations) in different latitudinal zones were listed from west to east in Table 1. Considering Table 1 and the familiarity of these rivers, after seriously discussion, we decided not to add an additional location map in the manuscript.

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

Reply: Thanks. We agree with you and have revised it in the abstract and the conclusion.

– 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?

Reply: CERA-20C is the ECMWF 10-member ensemble of coupled climate reanalyses of the 20th century. We have done the corresponding revisions in lines 75-77.

– 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.

Reply: thanks for the suggestion, and we have revised the text accordingly.

– Line 93: What is SPSS software?

Reply: the full name of SPSS is Statistical Product and Service Solutions, and we have done the revision in the text (line 107).

– Line 105 and throughout the text: “altitudes” to “latitudes”? Reply: they have been

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revised in the text.

– I don't understand the slope ratio calculation on lines 165-166. Why not just look at R^2 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.

Reply: Thanks for the suggestions. It is true that people always use the R^2 to assess the impact of one factor to another, while in this paper, we believe that the contribution calculations according to slope ratio of river-ice and LD, as well as slope ratio of snowmelt water and HD, could more specifically describe the meaning.

– Line 197: Rs to Hs? Reply: it has been revised in the text.

– Lines 204-205: I don't understand this sentence; there must be a typo. Reply: have been revised the sentence (lines 225-226)

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