

## ***Interactive comment on “Future hydrological extremes: the uncertainty from multiple global climate and global hydrological models” by I. Giuntoli et al.***

### **Anonymous Referee #1**

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The manuscript covers the interesting topic of hydrological extremes with respect to changes in the 21st century. The impact and uncertainty related to these projections is discussed in the manuscript. Although the topic is of great interest and the writing is good, some issues need to be addressed before the manuscript should be accepted for publication. My biggest concern is the novelty of this work. How is this work related to other studies and related to other papers that also use ISI-MIP data and GCM-GHM combinations (papers are cited in this manuscript) and how does the presented work differ from these other studies.

Major remarks:

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Page 4 Line 24- Page 5 Line 9: why is this analysis so different from Prudhomme et al (2014)? I see the point of the slightly different threshold, but Prudhomme et al also performed an analysis into the uncertainty of the GCM-GHM combinations for the low flows. Moreover, the results are nowhere compared with Prudhomme et al (2014). Are they really so different with this new (short moving window) threshold approach. Please convince me and the reader of the manuscript that this is really new work related to the low flow analysis. Furthermore it is stated on Page 11 Line 6-8 that other studies did study the high flow as well, so I have some problems with the novelty of this work. I see some novel aspect, but they are not clearly pointed out in the manuscript. The author point out 2 studies into future high flow and 2 studies into future low flow, of which some use the ISI-MIP data and are all GCM-GHM combinations. Please convince the reader why this paper is novel compared to the other work, other than that it contains both hydrological extremes in one paper. Moreover, how the finding here differ from the other studies, in particular Prudhomme et al and Dankers et al.

Page 4 Line 25 Why are two different threshold selected for the high and low flows. Why take the top 10% (extreme) for low flows and 5% (exceptional) for the high flows. It is well know that GHMs and GCMs have difficulties reproducing the extremes, which is even truer for the GCM-GHM ensemble mean. When two different thresholds are applied the two cannot be directly compared and the same is true for the patterns and magnitude of the changes. Most likely the GCM-GHM combinations will have a different performance/skill in reproducing and projecting the 10th or 5th quantile. Additionally I found an abstract of the authors at AGU that uses only the 10th and 90th percentile, why did the authors changes this later to 5th? Moreover, what is the impact of the selected threshold level on the uncertainty or SN2 ratio. Would a different threshold level result in different SN2 ratios or are the results stable and not threshold specific?

Page 5 Line 5: JULES was left out of the analysis, how much does this impact your estimates of the uncertainty compared to Prudhomme et al?

Page 6 Line 25-27: Gridcells with no seasonal changes are removed from the analysis.

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In which period (1970-2000 or 2066-2100) should these gridcells show no seasonal change? Moreover, with a changing climate gridcells on the edge of the Sahara or Greenland can become seasonal. This in itself is an interesting finding. To follow up on this an analysis is done for different climate regions. However, these change over time as well and some are more likely to change than others. This was not taken into account and will impact the results.

Specific remarks:

Page 3 Line 6 mentions multi-model ensembles. However, the manuscript continues with single model studies (e.g. Sheffield and Wood 2008). The reader expects a summary of multi-model simulations; maybe move this line to later in the manuscript when multi-model studies are discussed.

Page 4 Line 19-20 I believe the sentence should be ...all GCMs for each GCM averaged over all of the GCM or am I mistaken?

Page 7 Line 11-13: This  $SN2 > 1$  is rather arbitrary. Could there be a more statistically proper way to define this  $SN2$  threshold?

In general: Which dataset was used to computer the Koppen-Geiger climate classifications? I can assume they are different between the GCMs and different datasets. Please specify in the manuscript.

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Interactive comment on Earth Syst. Dynam. Discuss., 6, 1, 2015.

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