**Earth System
Dynamics**
Discussions

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:

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 is 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.

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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