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Interactive Comment

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

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Anonymous Referee 1 Received and published: 21 January 2015

Comment-1 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.

Answer We thank the reviewer for the helpful suggestions.
As pointed out by Vetter et al., (2015), numerous studies on climate change impacts have provided useful knowledge, however a synthesis of climate impacts for different regions and a consistent estimation of uncertainties is still missing. Our study goes in this direction: we conduct for the first time a systematic analysis, which enables an interpretation and comparison of results across space and hydrological regime.
We realize we have not sufficiently emphasized aspects regarding the novelty of the paper, which can be summarized in the following points:

- i) we investigate how projections in hydrological extremes are linked to the underlying climate and hydrological processes. For the first time this is done:
  - through a consistent joint estimation of extremes at once
  - at the annual and seasonal scales
  - at the global and regional scales using the Koppen-Geiger analytical framework.

  We thus describe how high and low flows and inherent uncertainty vary at the seasonal and spatial scale, identifying areas where we have more confidence in the climate or in the hydrology (i.e. uncertainty is owed to GCMs or GHMs).
- ii) we assess the uncertainty using a formal statistical approach (2-way ANOVA model - having tested the model assumptions described in Appendix C) partitioning the variance using GCMs and GHMs as factors. Other papers that make use of the ISI-MIP data set (Schewe et al. (2013), and Dankers et al. (2013)) partition GCM/GIM uncertainty using ratios between the variances, while our ANOVA

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approach adds the contribution of the error (or residual) to the partition of the variance. The uncertainty assessment by Prudhomme et al. (2014) omits the partition into GCM/GIM, providing a Signal-to-Noise (standard-deviation/mean and inter-quartile-range/mean) that does not allow for a gridcell by gridcell estimate of the major contribution to uncertainty.

- iii) We make a consistent joint estimation of the contributions to uncertainty for both extremes at once, having identified both extremes with the same method (5-d fixed-window variable threshold method). It should be noted that for high flows Dankers et al. (2013) have looked at a different metric (30 yr flood return levels of runoff of 5-d average peak flows), and for low flows we used an improved version of the Variable Threshold Method used by Prudhomme et al. (2014).

We will amend the manuscript (Introduction and Discussion sections) to reinforce these novelty aspects and to be more exhaustive on the relations and differences with other ISI-MIP based papers.

Major remarks

Comment-2 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

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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.

Answer With respect to low flows, there are three major differences in the method and its implementation. Firstly, the index on which mean changes are computed follows the same concept of variable threshold method employed in the work by Prudhomme et al. (2014), but it has been revisited in order to overcome a limitation of the 30-d moving window for which gridcells were assigned lower threshold values than the theoretical value* (10% for percentile Q10), so a tendency to capture fewer occurrences, i.e. more extreme events (this effect is perhaps attributable to a slow emptying of reservoirs during the recession phase for some GHMs). The use of a 5-d fixed window eliminates this lower-than-theoretical-values effect – although the ensemble results for mean changes do not greatly differ (Prudhomme et al., 2014, Fig 1 with Fig 1-a of our manuscript). Secondly, in Prudhomme et al. (2014) the uncertainty analysis had been carried out grouping mean changes per type of model and computing the Signal-to-noise. This analysis only allows to infer which group (GCM/GHM) brings about highest agreement in the ensemble results; the partition of uncertainty we carry out in this paper moves beyond a signal-to-noise ratio: the quantification of each source (GCM/GHM) to total uncertainty via the sum-of-squares (ANOVA) allows to describe the spatial variability of the contributions gridcell per gridcell. Further, expressing the sum-of-squares of each source via the Koppen-Geiger regions allows for an improved understanding of how the climate and hydrological processes drive uncertainty for both runoff ends. Thirdly, the GHM JULES was excluded from the analysis because its grid-resolution differed from the one of the other GHMs, and also its runs induced considerably larger gridcell masking areas (see response to Comment 4).
With respect to high flows, Dankers et al. (2013) have focused their analysis on annual extreme monthly flood peak (30-yer return level) as their aim was to describe flood

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hazard changes.
The flood hazard patterns (Dankers et al. (2013) Fig. 1) show increases in northern North America, Eastern Asia, northern Latin America, Central/Horn of Africa, North-western China. The comparison of these patterns with our study (i.e. with the changes in the occurrence of high flow days – Fig 1-ab of our manuscript), reveals some similarities: mostly northern North America, and Northern Asia. However, in some regions, e.g. North-eastern Europe, we have opposite patterns. The uncertainty results, that Dankers et al. (2013) expressed with GCM/GHM variance (cf. Fig. 1 lower panels, Dankers et al. (2013)), is in agreement with our findings for the southern hemisphere, mainly driven by GCM uncertainty, whereas there is less agreement for the northern hemisphere (in North America, Central Canada is GCM driven uncertainty, whereas it is GHM driven in our results). It could be argued that the differences in the phenomena analyzed and the methods make the comparison between Dankers et al. (2013) and our work questionable. Thus the findings comparison must consider the differences between the two studies: i) the scope: change in flood hazard vs. change in frequency of high flow days; ii) the method: different metric and uncertainty partition framework (the variance is partitioned into two factors – GCM and GHM – while our framework adds the error as contributor to the variance); iii) the multi-model ensemble: it comprises the same GCMs (5), but uses 3 additional GHMs – JULES, LPJmL, MATSIRO that we did not use as they are not suitable for low flows (LPJmL and MATSIRO showed large areas with very low values hampering the extraction of the low flows index (i.e. hefty cell-masking), to a lesser extent JULES, though the latter was also excluded for grid-resolution issues as mentioned above).
We will better organize these aspects in the manuscript, clearly writing the differences with Prudhomme et al. (2014), and also with the methods and findings from Dankers et al. (2013) in the Introduction and Discussions Sections.

Comment-3 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

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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?

Answer There are justifiable physical process based reasons for selecting different threshold levels for low and high flow extremes. The two phenomena (high and low flows) are quite different: low flows are generally characterized by a slower onset, and a longer duration; while and high flows by a sudden onset, and a shorter duration. They are not necessarily symmetric with respect to the median flow. In order to capture the intrinsic differences between the two phenomena we tested Q10 and Q15, and Q90 and Q95. Finally – for low flows: seeking a sufficiently low quantile without compromising the analysis (quantiles lower than 10% become intractable for the large presence of zero pools in some time series for which larger areas of the globe would be screened out) we chose the same as Prudhomme et al. (2014); for high flows we chose the 95th percentile. We had written the abstract for AGU2013 before carrying out the analysis with different quantiles and making the final choice, we then presented results at the fall meeting with the Lowflows Q10 Highflows Q95 setup as in the paper. As the reviewer points out the skill varies changing the quantile, this is especially true for low flows, for which the lower the quantile, the lower the skill (e.g. Gudmundsson et al. (2011) found that the performance of a similar set of global models – WaterMIP decreased systematically from high Q95 to low Q5 runoff percentile over Europe).

Comment-4 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?

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Answer JULES inclusion would have increased the uncertainty for: i) its peculiar setup (inclusion of varying CO2 and vegetation dynamics processes, absent in the other models) which make it an outlier compared to the other GHMs, and ii) its runs have a coarser resolution, so in the regridding process (to match the other GHMs in the Ensemble) from 1.8*1.25 to 0.5*0.5 resolution may influence uncertainty (gridcell fractionation of the same information into smaller sub-gridcells). The first point is quantified in Table S2 of Prudhomme et al 2014, in which the uncertainty (expressed by S2N ratio) is provided on e.g. column 4 for both the entire ensemble ("All GCMs GIMs") and for the ensemble minus JULES ("6GIMs"): the S2N is higher (stronger agreement between the models, i.e. decreased uncertainty) when JULES is not included: S2N@6GIMs 3.24 > S2N@AllGIMs 1.67. To assess the second point - i.e. the degree of impact of coarse to finer resolution on uncertainty - one would need to carry out the analysis using the same model setup and forcings at both resolutions; however, JULES runs at 0.5 are not available. This aspect is currently being investigated by the scientific community (Bierkens et al., 2015) seeking to describe the effects of resolution on model uncertainty. In essence, the inclusion of Jules would have driven the uncertainty towards the GHM source. As an illustration, the uncertainty maps of Fig. 1, Fig 3, Fig 4 would have been less yellow (GCM-driven uncertainty) and more green (GHM-driven uncertainty). During our exploratory analysis we have included Jules in the ANOVA, results of the uncertainty partition with and without JULES are shown in Fig. 1 and Fig. 2 of this document respectively – though test assumptions on the adequacy (i.e. residuals normality and constancy of variance) of the ANOVA model including JULES were less met. It should be noted that, for low flows, JULES inclusion (as seen in Fig. 1 and Fig. 2) has an effect on the masking which is homogenized across all GHMs and for which large areas of the world are screened out as its runoff time series presented large pools of zero values (similarly to LPJmL and MATSIRO).

Comment-5 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.
Answer Gridcells removal is based on the control period (1971-2005). This will be specified in the Data and Methods section and in the Appendix B where we describe the gridcell masking procedure.
With regards to the gridcells potentially becoming seasonal with a changing climate, we feel the frontier no-change/change is difficult to follow at this spatial scale of analysis (i.e. global). As an example, in the literature Alessandri et al. (2014) have looked at the expansion and retreat of specific climate boundaries (Mediterranean climate in Europe and western USA) using CMIP5 climate projections. This aspect is very interesting but we think it lies beyond the scope of our study and it would require focusing on limited areas alone.

Specific remarks

Comment 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.
Answer As described by the authors (see Schewe et al. (2013), caption of Fig. 1, and Dankers et al. (2013), caption of Fig. 1), the GCM variance of the change is computed across all GCMs for each individual GHM, and then averaged over all of the GHMs.

Comment Page 7 Line 11-13: This SN2 >1 is rather arbitrary. Could there be a more statistically proper way to define this SN2 threshold?
Answer SN2>1 was chosen to facilitate the visualization of the model agreement on the maps. In our case, we wanted to remain consistent with Prudhomme et al. (2014) to facilitate comparisons.

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Comment 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.
Answer The dataset we used for the Koppen-Geiger climate regions is based on present-day following the global data classification from Kottek et al. (2006) (a link is provided at the end of Table 1 in the manuscript - http://koeppen-geiger.vu-wien.ac.at/pdf/kottek_et_al_2006_A4.pdf). Therefore, there is no difference between models: all gridded model outputs have been assigned a Koppen-Geiger climate region from the Kottek et al. (2006) dataset. We will clarify this in the manuscript, in particular in the Data and Methods section (Page 8, Line 7). We are aware that region boundaries can change with time (as investigated by e.g. Wanders et al. (2014)); however, in our study we were seeking changes in projections with a present baseline as reference.

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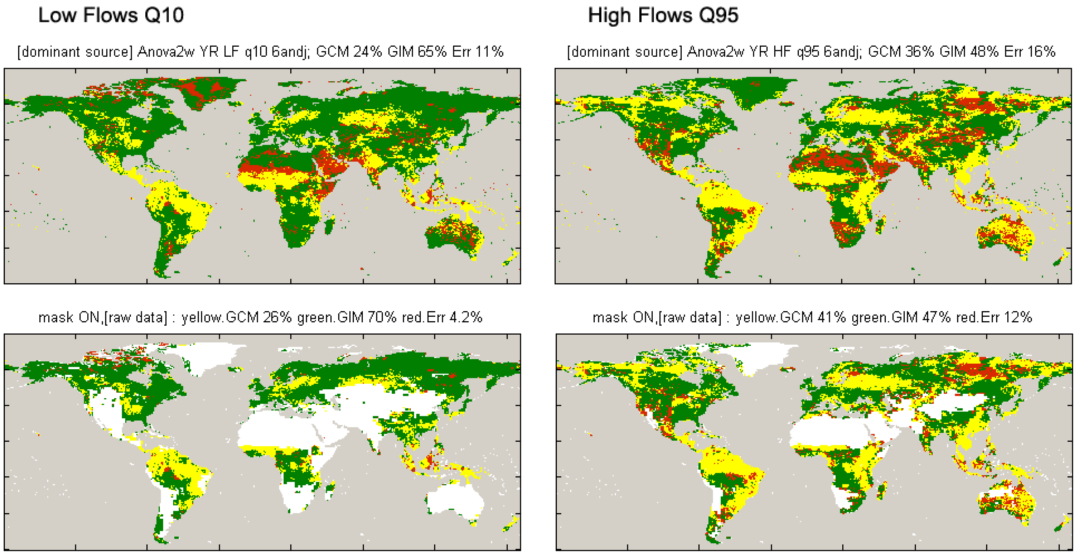

Fig. 1. Dominant source of uncertainty - Including JULES GHM (7GHMs*5GCMs)

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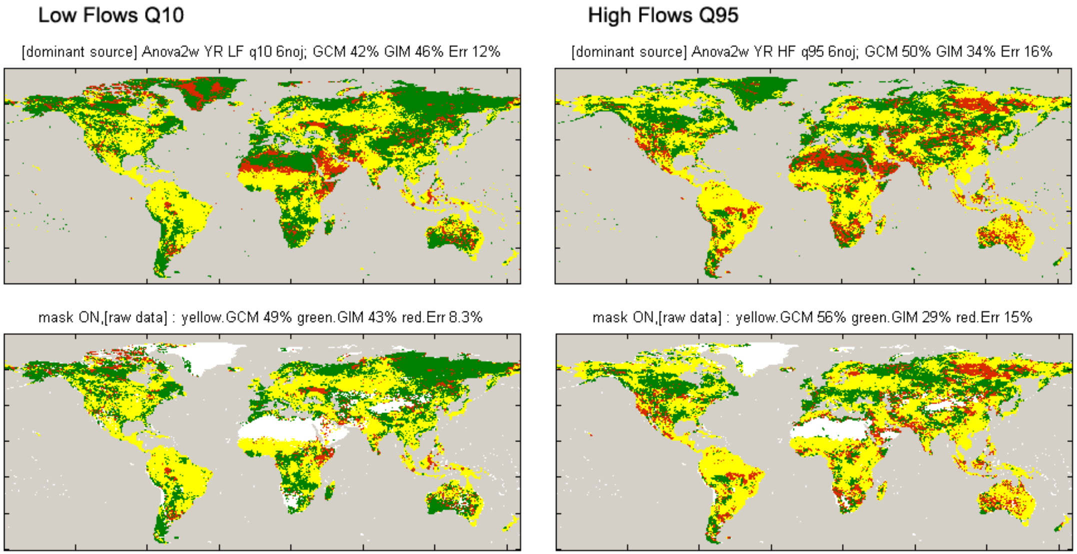

Fig. 2. Dominant source of uncertainty - Excluding JULES GHM (6GHMs*5GCMs)

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