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Earth System Dynamics Discussions

Interactive comment on "Climate change impact on available water resources obtained using multiple global climate and hydrology models" *by* S. Hagemann et al.

Anonymous Referee #2

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The main objective of this study is to explore the climate and hydrological uncertainties that affect the estimation of future water resources availability under climate change. While uncertainties due to global climate models and future scenarios have been subject of several publications, the impact of uncertainties due to hydrological modeling is usually not taken into account. The study addresses this issue by considering 8 GHMs in combination with 3 GCMs and 2 scenarios. The mean and spread of changes in ET and runoff between control (1971-2000) and future (2071-2100) periods are analyzed at the global scale. One of the main results is that a non negligible part of the spread may be attributed to GHMs, espacially concerning ET.

The paper is well written and addresses the scientifically important issue of the impact of climate change on water resources availability. Globally, the methodology is quite relevant, but as stated by previous reviewers (http://www.earth-syst-dynam-discuss.net/3/1321/2012/esdd-3-1321-2012-discussion.html), I think the paper could be improved with a more comprehensive and critical analysis of the possible causes of uncertainties. I understand that, this study being part of the huge European WATCH project, it is not possible to detail all the aspects of the methodology. All the more so as many specific points (such as bias correction or GHMs comparison) have been addressed in previous papers that are cited by the authors. Nevertheless, the present paper could benefit from reminding previous results and analyzing them in the scope of this study, that are: how the bias correction may affect the spread in ET and runoff? and in which extent can the spread due to GHMs be attributed to the model characteristics?

The questions on bias correction have been widely discussed in the two previous reviews and the authors largely answered them in their first reply. Concerning the GHMs, I also agree with the other reviewers that some details have to be added in the paper, even though a more comprehensive description could be find in Haddeland et al. (2011). Namely, the spread due to GHMs may come from different factors, such as climate forcings, main equations, parameters, calibration strategy. Without necessarily giving too much details, I think the following questions have to be addressed in a future version of the paper:

- which scheme is used to model ET? If the models use different schemes, a large increase in the spread could be expected.
- which data sets are used (such as for the soil characteristics)? Or more specifically, do the model use the same data sets, and if not how these differences could affect the final spread?
- how are the GHMs calibrated? If they use is situ data (such as discharge at gauge stations), a lower spread would be expected over regions where a lot of in situ data are available. Besides, what climate forcing are used for the calibration and what could be the impact of using different forcings (GCMs outputs) on ET and runoff?

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Lastly, the several maps shown in the figures are described in detail in the text, namely the spatial repartition of spread in ET and runoff. While this is a necessary step, the paper could benefit a lot with a more critical analysis of observed patterns instead of simply pointing out such a spread over such region. For example, it might be interesting to distinguish between water- and energy-limited basins, all the more so as the authors mentionned potential differences between GHM and LSM, the latter including energy balance constrains.

All the minor comments I have, such as unclear sentences or too small texts in figures, have been reported by the previous reviewers.

Interactive comment on Earth Syst. Dynam. Discuss., 3, 1321, 2012.