

Interactive comment on “Global patterns of change in discharge regimes for 2100” by F. C. Sperna Weiland et al.

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"Global patterns of change in discharge regimes for 2100" aims to assess a global evaluation of the effect of projected climate change on hydrological regimes, and provides an estimate of the related uncertainties. I would like this manuscript to be accepted after minor revisions. In my opinion, the work is consistent and well-discussed, a few results are significant and I found very interesting the analysis of uncertainties shown in figures 4, 7, 8.

Starting from this point though, I would like to make some criticisms. In the text (e.g. page 10976, lines 24 and following; page 10993, lines 25 and following) the authors point out the power and reliability of the multi-model ensemble. In my opinion this

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concept is sometimes overrated in hydrological science: there is no absolute evidence that the ensemble mean of GCMs provides more reliable results than a single GCM. Also, the first assumption for the quality of an ensemble mean is the independence of members, which is not true for coupled models that share parameterization or even components. In a recent paper (Materia et al., 2010 JHM) we took into account these themes, although in not such an exhaustive way. Discussing the effect of an ensemble of land surface models forcing a river routing scheme on its discharge, we pointed out that: (1) the ensemble mean tends to reduce minimum and maximum peaks, and resulting curves of seasonal cycle of discharge are flattened compared to observations. This applies in most of the rivers analyzed in this paper as well: exceptions are Parana (probably because the land surface model poorly represents the Pantanal, where the upper river flows), Yellow River (in which human management plays a crucial role), Zambezi. In the assessment of a variable whose seasonality and amplitude of the peaks play a crucial role, this can be a remarkable limitation of the multi-model technique. (2) globally, two of the models taking part in the ensemble perform better than the multi-model analysis. I am aware that our paper does not go as deep in statistic as Sperna Weiland et al. (2012) do, but in my opinion this matter should be further examined in the discussion.

Other smaller issues:

1. I would discuss a little further two limitations of this study, and more generally of land surface models. First of all water management and river regulation have not been included: this is a limit of present river routing schemes, and on a global scale there is not much we can do. Also, changes in land use are neglected here, but they could be a crucial variable in the future, especially in the context of water cycle.
2. page 10975, line 12. "As changes...": I don't understand this sentence, maybe you want to get rid of the "as" at the beginning.
3. page 10979, line 7. The concept of "initialization" is not clear in this context, and the

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whole sentence is a bit vague. Please rephrase being more accurate in the description of the setup.

4. page 10985, line 4. I don't understand the sentence starting with "Although...". Please rephrase.

5. Section 3.5. I don't see any figure or table explaining this section, and also I am a bit confused about the meaning of Runoff Coefficient. Please supply this part with definitions and possibly a figure or a table.

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