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Discussions

Interactive comment on "Mapping (dis)agreement in hydrologic projections" *by* Lieke Melsen et al.

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Dear reviewer,

Thank you very much for your constructive suggestions. Below you find a response to the points made.

*Discussion

We agree that the topics mentioned by the reviewer are currently lacking in the discussion, but definitely deserve a place there. Physical explanation can be given in some instances, for example for the result that snow is one of the causes for disagreement, because the three different hydrologic models use different snow parameterizations (degree-day versus energy-balance), the problem with aridity and the related soil moisture-evaporation feedbacks could perhaps be linked to the off-line application of

the GCMs which the reviewer justly refers to. We will investigate more physical explanations and include a discussion on the offline/online application of GCMs to hydrologic models.

*Was the GCM output downscaled and bias corrected against the forcing used in the 1980-2008 simulations? I understand that it has indeed been bias corrected; however, bias correction is always done against some reference database; I would hope that that reference database is the one being used to force the 1980-2008 simulations. Please clarify.

The GCM output was bias corrected on the Maurer-dataset for the period 1950-1999, so this is a different dataset than where the 1980-2008 simulations have been performed with (which was the Daymet-dataset). We compared the temperature and precipitation differences between Daymet and Maurer for the 605 basins. Generally, both data-sets agree pretty well (similar in the mean), although precipitation differences exist in the west, and there are temperature differences in the central US and in the higher basins.

We expect that the effect of the difference in calibration-dataset and bias-correction-dataset on the conclusion of our study is limited, since we do not compare the Daymet-forced model results with the GCM forced model results, but the two GCM-forced model results (historical versus future).

So the effect of this bias correction only applies to the calibration. There is certainly the possibility that the calibration will be different using the Maurer-dataset rather than the Daymet-dataset. We expect that the effect is limited because we use a parameter-sample, but to test this, we decided that we will force a small number of basins with the Maurer-dataset and investigate the effect on the calibration.

*Using 5 years for spin-up is awfully low. Are you sure that this is appropriate? I would have suggested cycling through the 1980 to 2008 a few times. Although this most likely does not disqualify the results, there should be some argument in the paper for why only 5 years appeared to be enough.

For the most complex hydrologic model included in this study, VIC, when run on an hourly basis, three months was shown to be sufficient (Melsen et al., 2016, https://doi.org/10.5194/hess-20-2207-2016). For HBV, it has been stated that one year warm-up period on a daily basis is sufficient in most cases (Seibert and Vis, 2012; www.hydrol-earth-syst-sci.net/16/3315/2012/). Therefore we assumed five years to be ample. An important critical note here is indeed that the warm-up period can be longer in drier climates, which could perhaps explain the lower model performance in these regions.

*Section 3.1.2: "A larger parameter sample could therefore decrease the number of non-behavioural basins and even allow for a more stringent selection criterion." Maybe, but certainly not necessarily. Assuming that the LHS sample is robust, then your model parameters will already capture the details of the parameter space. I agree that given your relatively small sample size, there might be regions of the parameter space that you are disregarding. However, I suspect that it will be fairly minimal.

One reason for this statement is that other studies have applied HBV in the same region with better model performance (Beck et al., 2016, http://onlinelibrary.wiley.com/doi/10.1002/2015WR018247/epdf). Although, indeed, somewhat lower performance can be found in this study as well in the great-plains region (Figure 2). Another explanation, besides the parameter sample, could be the warm-up period as discussed above.

* Section 4: "Climate models disagree more in a more extreme scenario" - How much of this can be attributed simply to the disconnect between your models and the climate models themselves? (See issue regarding offline/online models above)

Based on our results, we cannot say how much of the disagreement could be decreased in an online application, although that would be very interesting. However, the point that we were trying to raise is that, with more extreme emission scenarios,

the spread in projections among the climate models increases. This is regardless of whether this projection is subsequently applied to an offline hydrologic model. Question remains, indeed, whether in an online application the spread would decrease.

* "Furthermore, land-use and soil parameters have been kept constant for both modelling periods. Although it is very likely that land-use will change in the future as a result of climate change or population growth, there are currently no methods to quantify this change and translate that to parameter values for the employed hydrologic models." -> GCMs actually do currently account for land use change. Now to be fair, these changes are mostly due to offline studies that predict that land use will be in the future, however, this information is out there and could be used in theory within the hydrologic models.

Yes, there is literature available on land-use change projections, but it is not straightforward to translate these changes to the changes in the parameters of our conceptual hydrologic models (for VIC it would be fairly easy because land-use is explicitly parameterized, but SAC and HBV are more schematic). We will, however, include some references to land-use change studies for the interested reader.

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