

Interactive comment on “Climate Change Impacts on Yangtze River Discharge at the Three Gorges Dam” by Steve J. Birkinshaw et al.

Anonymous Referee #2

Received and published: 7 July 2016

This study explores the consequences of climate change on the discharge of Yangtze River at the Three Gorges Dam, which corresponds to a catchment of 1,007,200 km² (this is close to the combined area of France and Germany). The distributed process-based model Shetran is forced by projections from 35 CMIP5 GCMs downscaled using a delta change approach. The sign of the annual discharge change under future conditions is uncertain (projections vary from -29.8% to 16.0% depending on the GCM). The authors attribute this uncertainty mainly to differences in how the summer monsoon is simulated by the different GCMs. Overall the study is interesting and thorough. The paper is clearly structured and well written. Yet, although the authors use a distributed model over a very large catchment, they principally discuss changes in discharge averaged over the whole catchment. I think that the hydrological processes leading to changes in discharge should be better discussed (in particular changes in

[Printer-friendly version](#)

[Discussion paper](#)



ET and in snow accumulation/melt) and that changes in subbasins could be explored too. This could provide valuable insights into the realism of the simulations and into the sources of uncertainty affecting the projections. This study is an interesting and relevant contribution, but the processes leading to changes in discharge and to the uncertainty in the projections need to be better discussed.

Major comments

1. *Evapotranspiration*. PET is estimated using Thornthwaite equation, which is an empirical, temperature-based formulation. It has been shown that this formulation can lead to an overestimation of PET under climate change, indicating that it reacts too strongly to temperature increase (Sheffield et al., 2012). I understand the argument developed by the authors in Section 4.2 that data is missing to use a more process-based method, but the limitations of the approach should be more clearly stated and the implications should be better explored. This is particularly important since the projected changes in PET seem to significantly influence the range of the projected future discharge. The authors report that this range changes from [-29.8%,+16%] when Thornthwaite equation is used to [-7.6%, +28.7%] when PET is held constant (page 10, lines 9-13). I suggest that: 1) the author examine whether there is any trend in the observations from the 52 PET stations (ideally over a period longer than 10 years) and 2) the authors extract the ET simulated by the climate models, and assess whether there is a increase in ET, as those simulations can be considered as more reliable than Thornthwaite approximations (Milly and Dunne, 2016). Maybe add a plot to Figure 6 showing ET as simulated by the GCMs and as simulated by Shetran, and add boxplot to Figure 8 showing future ET if PET is held constant. This would illustrate the sensitivity of the projections to the formulation of PET.

2. *Snow*. I understand from Figure 2 that winters are relatively dry in the region, hence that the influence of snow might be smaller than what would be expected from the elevation, but changes in snow accumulation and snow melt should be discussed. The authors report that “the modelling suggests that under the present climate 4.2mm of

June discharge is from snowmelt; this reduces to 2.2mm for the median of the CMIP5 simulations” (page 8, lines 12-13), which is a valuable piece of information. But typically, if more precipitation falls as rain and less as snow, this translates into an increase of winter discharge, which is hard to see in Figure 9. Could the authors comment on this? More generally, it suggest that the authors plot the annual cycle of SWE (monthly means) under current and future climate, and discuss to which extent the snow pack influences current and future discharge.

3. *GCM selection.* The authors find that the simulations of several variables by the GCMs are “implausible” (Tables 3 and 4), yet they still decide to include those models in the ensemble. I suggest conducting a second evaluation, in which they exclude the climate models that they do not deem realistic. Does it lead to a significantly lower spread of the ensemble of discharge projections? In Figure 10, implausible models fall close to the regression line, but I do not consider this as a proof of realism, since they could well fall there for the wrong reasons.

4. *Sources of uncertainty.* “So it could be argued that under a future climate the uncertainties in discharge from using Shetran are smaller than the uncertainties in the projected future climate.” (page 10, line 30). This is a quite general statement, slightly speculative. Previous studies have shown that it tends to be true (e.g. Vano et al., 2014), but that in catchments with a complex topography, where snow plays a key role, the hydrological model can be a major source of uncertainty (e.g. Addor et al., 2014). It is hard to really discuss the relative importance of the differences sources of uncertainty when only one emission scenario, one downscaling method and one hydrological model are used. That said, I recognize that modeling such a large catchment under climate change in an area where comparatively little data are available is already a significant achievement, and I congratulate the authors for this. I encourage them to better explain why they decided to only sample the uncertainty stemming from the GCMs, and in particular why they decided to run a distributed process-based model, when several semi-distributed more conceptual models could probably have been run

[Printer-friendly version](#)

[Discussion paper](#)



for a comparable computing cost.

5. *Distributed modeling.* I find it surprising that the authors chose to run a distributed model, but then barely discuss regional differences within the catchment. Given the size of the catchment and its elevation range, there are probably some interesting spatial patterns. For instance, which regions show the largest changes in terms of ET? And how much is the snow line rising as a result of higher temperature?

Minor comments

There is a relatively strong emphasis on floods in the text (e.g. in first sentence of the abstract and of the conclusions) but floods are not simulated nor discussed in a quantitative way, and as the authors recognize, the delta change approach is not adequate for modeling extremes (page 9, line 21). I suggest that the authors rethink the way they discuss floods.

Page 3, section 2.1: Thiessen polygons were used to account for spatial variations of precipitation and temperature within the catchment. Is it correct the forcing was considered uniform within each polygon (i.e. that no correction was applied to account for elevation changes within each polygon)? For instance, for the polygon located in the north-western corner of catchment, which has an area of about 250km x 250km, was the model, which is run on 10km grid, fed with a uniform forcing based on the measurements of a single station? If this is correct, please discuss the implications for snow modeling.

Page 3, lines Page 4 line 15: If HRUs were used, please explained how they were constructed. If not, please explain why.

Page 5, lines 4-6: “The calibration was for 1996-2000 and the validation period for 2001-2005. The comparison between measured and simulated discharge is made using the Nash Sutcliffe Efficiency (NSE)”. Was any algorithm used for the calibration or was it a manual calibration?

Page 5, lines 21-24: “We analysed changes in precipitation and air temperature between 1981-2010 and 2041-2070 from 21 GCM grid cells over the Yangtze for each of the CMIP5 runs, extracted monthly change factors (ratio for precipitation, absolute for temperature) and modified the observed time series data using the monthly CF from the nearest CMIP5 grid cell.” Maybe clarify whether the “observed time series” are measurements from the 64 precipitation stations and 90 air temperature stations.

Page 6, line 20: “The colouring indicates the quality of the model against observations using the same system as McSweeney et al. (2015)”. Please briefly explain how the different categories were defined. In particular, explain how the colors for the second column of Table 3 (summer monsoon) were obtained.

Page 6, line 24. “It can be seen that many of the models are poor in their simulation of the monsoon”. What is “poor”? Is it with “Significant biases” or “Implausible”? Please be more specific. Page 6, line 26: “all CMIP5 model runs overestimate annual observed precipitation”, indeed the overestimation is quite clear and generalized across the GCMs (Figure 5a). Is it this overestimation reported by other studies focusing on the same region? Can the authors discuss its possible origins?

Page 10, line 25: “There are still uncertainties in using Shetran to predict discharges for precipitation outside the limits of the model calibration and validation period. However, as Shetran is a physically-based model, theoretically this means that the predicted discharges will be representative of future climates.” I disagree with this second sentence. For instance, if PET estimates are biased, the modeled ET will most likely be biased too, and so will the simulated discharge. Also, accounting for land cover is indeed a step towards process-based modeling, but if the land cover is assumed constant under a changing climate although it might well change, this partially defeats the purpose of accounting for land cover. I think the second sentence should be removed.

Section 4.4: “The key to predicting future changes to discharge in the Yangtze basin is correctly predicting how the strength and location of the summer monsoon will change

[Printer-friendly version](#)

[Discussion paper](#)



under a future climate.” The authors should consider adding that another key challenge is to better estimate future ET.

Table 2: Please indicate the parameter ranges used for the calibration.

Tables 3 and 4: Overall, I find that tables of numbers, like Tables 3 and 4 are difficult to interpret. I suggest replacing them by a graphical representation of the same content. Or at least producing a Figure similar to Figure 5 but for temperature.

Figure 1: Please add a color bar showing elevation.

Figure 5: Why are some models represented by a colored line and others by a gray line? I am guessing from the caption of Figure 10 that grey models do not have lateral boundary conditions available. Please amend the caption. Figure 5c: Why did the authors decide to depict the monthly fraction and not the monthly amounts? Without the monthly amounts it is hard to tell how well the GCMs are doing in absolute terms.

Figure 6: mm/month instead of mm?

Figure 7: Would it be possible to replace this Figure by a map, with for instance the color of the grid cells indicating the mean change, and the hatching density indicating the agreement between the different models? Or at least add some kind of information on the location of these grid points, for instance “south-west”, etc.

Figure 8: The second sentence of the caption should probably be “The blue squares show the values for the present climate”, like in the text. But then, which of these values are measured and which are modeled?

Figure 11: I find this comparison really interesting, but the discussion would be easier to follow if Figure 11c was replaced by a map showing the differences between the models. As already stated, I am not convinced by the choice of showing the monthly precipitation fraction instead of the monthly means (Figure 11b.)

References

Addor, N., Rössler, O., Köplin, N., Huss, M., Weingartner, R. and Seibert, J.: Robust changes and sources of uncertainty in the projected hydrological regimes of Swiss catchments, *Water Resour. Res.*, 50, 7541–7562, doi:10.1002/2014WR015549, 2014.

Milly, P. C. D. and Dunne, K. A.: Potential evapotranspiration and continental drying, *Nat. Clim. Chang.*, doi:10.1038/NCLIMATE3046, 2016.

Sheffield, J., Wood, E. F. and Roderick, M. L.: Little change in global drought over the past 60 years, *Nature*, 491(7424), 435–8, doi:10.1038/nature11575, 2012.

Vano, J. A., Udall, B., Cayan, D. R., Overpeck, J. T., Brekke, L. D., Das, T., Hartmann, H. C., Hidalgo, H. G., Hoerling, M., McCabe, G. J., Morino, K., Webb, R. S., Werner, K. and Lettenmaier, D. P.: Understanding uncertainties in future Colorado River streamflow, *Bull. Am. Meteorol. Soc.*, 95(1), 59–78, doi:10.1175/BAMS-D-12-00228.1, 2014.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-231, 2016.

HESSD

[Interactive
comment](#)

[Printer-friendly version](#)

[Discussion paper](#)

