

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

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We would like to thank the referee for their comprehensive review and comments which we address below. The referee makes some excellent suggestions for improvements to the paper and we will incorporate these suggestions.

Major comment 1. “Evapotranspiration. . . .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

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constant. This would illustrate the sensitivity of the projections to the formulation of PET”

We will look at 1) Trends in the measured evapotranspiration data. 2) Trends in ET extracted from the climate models. Adding a plot to Figure 6 and a boxplot to Figure 8 are excellent suggestions which we will incorporate into the updated paper.

Major comment 2. “Snow. . . . 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.”

We will consider snow in more detail in the updated paper. As the referee suggests we plan to add an extra figure to show the annual cycles of SWE under current and future climates and discuss the effects of this on discharge.

Major comment 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.”

We will consider another evaluation of the effect of the GCMs on discharge in which we exclude the climate models that are not considered to be realistic. We agree that in Figure 10 if implausible GCMs fall close to the regression line it is not considered as a proof of realism.

Major comment 4 “Sources of uncertainty. . . . 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”

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In the updated paper we will modify the text to explain our choice of using a single model with uncertainty only stemming from the GCMs.

Major comment 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?”

The regional differences are considered in Figure 7 and the corresponding text, where the numbers on the x axis refer to locations within the catchment. We will update the paper to discuss this spatial variation in more detail. As discussed in Major comment 2 an additional Figure showing spatial variations in snow will be added.

Minor comments 1. “ 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 adequate for modeling extremes (page 9, line 21). I suggest that the authors rethink the way they discuss floods”

We will change the focus in the text to remove the emphasis away from flooding.

Minor comments 2. “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.”

This is correct. We will discuss the significance in terms of snow modelling

Minor comments 3. “Page 4 line 15: If HRUs were used, please explained how they

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were constructed. If not, please explain why.”

We are not sure if we fully understand this comment. Shetran is a spatial distributed model and each 10km by 10km grid square has its own individual land use and soil/aquifer type.

Minor comments 4. “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?”

The text will be modified to make it clear that it was a manual calibration

Minor comments 5. “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”

We will clarify that the measurements are from the 64 precipitation stations and 90 air temperature stations

Minor comments 6. “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”

We are planning to reconsider the colours in the 3rd, 4th and 6th columns in Table 3. We agree that currently they are not well defined.

Minor comments 7. “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

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“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?”

We will be more specific about the explaining the term “poor” in the revised paper. We will also try to find more details on the origins of the overestimation of annual observed precipitation.

Minor comments 8. “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”

We will remove the second sentence.

Minor comments 9. “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 under a future climate.” The authors should consider adding that another key challenge is to better estimate future ET”

In the revised paper we will add that the better estimation of future ET is another key challenge.

Minor comments 10. “Table 2: Please indicate the parameter ranges used for the calibration”

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In the revised paper we will make this change.

Minor comments 11. “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”

We plan to add a figure similar to Figure 5 but for temperature.

Minor comments 12. “Figure 1: Please add a color bar showing elevation”

We will add a colour bar showing elevation.

Minor comments 13. “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 term”

If all the GCMs were coloured it is impossible to distinguish which line is associated with which model. So only the best and the worst models are coloured. We will amend the caption to make this clear.

We originally showed the monthly amounts not the monthly fractions but the monthly response was overwhelmed by the annual totals (which are already shown in Figure 5a). For example in a GCM model where the annual totals were twice the measured total all that could be seen were the monthly totals were larger than the measured totals.

Minor comments 14. “Figure 6: mm/month instead of mm?”

We will change the axis title

Minor comments 15. “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

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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”

We agree that currently it is not clear where the numbers on the x axis refer to. We will look at ways of changing the Figure so it is easier to interpret.

Minor comments 16.” 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?”

We will change the caption to make this correct and clear

Minor comments 17. “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).”

We agree that it would be clearer by showing the data in Figure 11c on a map. We will think of the best way of doing this.

Steve Birkinshaw (on behalf of the co-authors)

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