

Interactive comment on “Projected impacts of climate change on hydropower potential in China” by Xingcai Liu et al.

Anonymous Referee #2

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General comments

The authors projected Gross Hydropower Potential (GHP) and Developed Hydropower Potential (DHP) of China using the global runoff dataset developed by the ISI-MIP project. The dataset includes global gridded runoff field simulated by 8 global hydrological models for 5 climate models and 2 emission scenarios. They analyzed the spatial and temporal distribution of changes in GHP and DHP in China. Although hydropower is a fundamental source of energy, analyses utilizing macro scale hydrological model have been seldom reported. This report has potential to advance this research field.

As is commonly seen in macro scale hydrological simulations, this study is based on several strong assumptions. I have not been fully convinced by the validity of some of these assumptions. This is partly due to the assumptions themselves, but largely due

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to lack of discussion. Details are noted below.

First, overall discussion on the background mechanism for the results is lacking. The results the authors obtained are well presented, but why and how they were obtained is little described. The Discussion Section should be largely expanded to include the mechanisms. Second, the term (and the model) of DHP should be revisited. What does “developed potential” mean? Which is more close to hydropower generation or technical hydropower potential? If DHP is different from any important indicators in the real world, how should we interpret the results? Without clarification of DHP, it is not clear what was calculated and what for. Third, the quality in runoff field of ISI-MIP should be well discussed. Since the global hydrological models participated in ISI-MIP have not been calibrated except the WaterGAP model, it must be carefully discussed that how the biases in runoff propagate to the results. Fourth, as far as I understood, the authors assigned the national total Installed Hydropower Capacity (IHC) into 447 major reservoirs. Since this might significantly overestimate IHC at individual reservoirs, the validity of this treatment should be validated and discussed. It might be a good idea to start with comparing reported installed hydropower capacity at individual reservoirs with the authors’ estimation.

Specific comments

Page 6 Line 7, “ $DHP = \min(R_m \times h \times g, IHC)$ ”: I found that this equation primarily expresses hydropower generation. Why was this termed “Developed Hydropower Potential”, not hydropower generation? If DHP is not hydropower generation, then what is this correspond to in the reality?

Page 6 Line 8 “ $h=S/A$ ”: Fekete et al. (2010) expressed reservoirs as tetrahedrons in their model. What are the advantage and disadvantage of the authors’ expression (cylinder)?

Page 6 line 15 “no IHC data associated with the GRanD reservoirs” World Register of Dam by International Commission of Large Dams (<http://www.icold-cigb.org/>) includes

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Electric Capacity of individual dams.

Page 6 line 18 “Then the adjusted provincial IHC. . .” As far as I understand, this study deals with storage and discharge for 447 reservoirs in China, while IHC for all the nation. This discrepancy could make $R_m \times h \times g$ substantially smaller than IHC, hence it may have influenced the results. This point should be clarified here.

Page 11 Line 22. I got a general impression that the Discussion Section is superficial. Since the Results Section only introduces the numbers that authors obtained, actually I expected detailed discussion on the background mechanisms of model behaviors and interpretation of the results, but these are seldom provided in the current form of the manuscript. The contents of this section should be substantially added.

Page 12 Line 10 “most regions show poor agreement between models”: In terms of what? Magnitude or signs? What are the results of the WaterGAP model or the only model with calibration?

Page 13 Line 19 “Thus, reservoir regulation could be changed in the future to adapt to climate change”: Too superficial and abstract. How should it be changed based on the findings of this study?

Page 15 Line 5 “Relatively small changes also will occur in late spring and early summer, while large decreases will occur in other months”. Why did these happen in your simulations? Basic mechanisms should be mentioned here. For instance, DHP is a function of monthly discharge (R_m) and water level (h). Which is dominant factor to produce the seasonal variation?

Page 15 Line 10 “DHPs given the current infrastructure will not be able to mitigate the hydrological changes and thus will decrease”: Why and how did the authors conclude this? Would this conclusion be different if the authors modified the reservoir operation rules? Actually, the authors have conducted an elaborate sensitivity test on the parameters of operation. Some of the combination might have worked as “adaptation” to

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climate change.

Figure 1: The figure doesn't have legend. It should be displayed what the height of bars quantitatively indicates.

Figure 5: Specify the base period of these two figures. I'm a bit curious why the plots start from -4% at 2010 (largest change) and gradually “recover” toward 2100 (smallest change) for RCP8.5.

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