

Interactive comment on “Community Climate Simulations to assess avoided impacts in 1.5 °C and 2 °C futures” by Benjamin M. Sanderson et al.

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Summary

The authors document a new set of experiments specifically designed to address some of the Paris Agreement aims. The experiments are informed by use of an emulator, which allows for the specification of a set of scenarios to achieve 1.5 and 2 degrees for the CAM5 model. The authors document some of the impact-relevant changes in climate. Overall I think this is a well-written, very valuable addition to the literature surrounding Paris. The new set of simulations provides the community with an excellent data set to analyse, which also provides a complimentary method to the HAPPI approach.

I have many comments that should be addressed, but they are only small, so I would

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recommend minor corrections. Three broad comments that the authors should consider are 1) to be a bit more upfront about the limitations of the CAM5 model, by, for instance including a paragraph on how well the model reproduces the past quantities that are looked at in the paper. Referring to AR5 etc, rather than new analysis could do this. 2) the nature of using a coupled ocean means that the climate may not be stable within the century. I think this needs to be clearer in the abstract, but also in discussion of Figure 1. 3) In the abstract, and throughout the text, it should be made clearer that this study is only a broad sweep of impact relevant analysis. For instance, each one of the sections on impacts could (and should) be multiple papers of analysis.

Signed Dann Mitchell

Comments

P1 L6: “impact-relevant long term climate data” – somewhere in the manuscript I think a sentence is needed about what variables are output. Is it everything from AR6? At this stage it is not clear what impact-relevant variables are stored.

P1 L11: “and only 1 in 40. . .” I think ‘only’ can be deleted here.

P1 L19 - P2 L1: “worlds emissions have been closer. . .” is this true for GHGs and aerosol etc? The interplay of both can be very different in different RCPs.

P2 L8-10: This sentence is not clear to me, I think the multi-model mean of CMIP5 RCP2.6 gives $\sim 1.5\text{C}$, which seems in contradiction to your sentence. Of course it depends on pre-industrial definition.

P2 L12: “no individual model” should be “no individual fully coupled model”.

P2 L18-26: I feel James et al, 2017, WIRES should be cited here, as it gives an overview of all these methods.

P2 L21: Can you make a comment on the time scales where the pattern scaling is relevant?

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P2 L27: Acronym is wrong. It should read: “Prognosis and Projected Impacts”. I think Mitchell et al, 2017b should also be added, which was the concept of HAPPI.

P2 L29: Please change ‘will use’ for ‘uses’, as these have already been performed.

P2 L30: “is computationally cheap”, I think “so allows for huge ensembles to be run providing samples of extremes” or something similar should be added, for context.

P2 L34: “have the same SSTs. . .” actually there are tier 2 experiments which sample a much wider range of SST patterns (22 different patterns for 10 different years, which is 220 different patterns). Although I agree that is still doesn’t sample the full range, as the CAM5 setup does.

P4 L5: I think this should be “Figure 1(a) and 1(b)”. Otherwise 1(a) is not referenced. But it makes sense to refer to both panels here anyway.

P4 L11: “1850-1920”, can you provide some justification for choosing this period please.

P5 L13: I think “until 2100” can be removed, otherwise it sounds like it changes there (but actually just the simulations stop there).

P5 L14-15: I do not think the plots look as stable as the authors say, especially not for the 2C experiment. And I wouldn’t see how they would be with the long term ocean time scales. Can the authors expand on this section.

P5 L16: “large scale climate” sounds unclear. I think “global-scale climate” is better.

P5 L20-25: I would note that within uncertainty there is no change over the scenarios.

P7: I think some more text on the reproducibility of the Arctic response is needed, especially in the historical period. There is a large range in CMIP5 projections.

Figure 2: It would be nice to see a bias plot from the model (historical versus CRU, or something similar). This might address the comment above to some extent.

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P8 L12: I think a stronger reference for this point is Gasparrini et al, 2016. You could either add it on, or replace the old reference with it. Also it would be best to put an 'e.g.' in front of these.

P8 L28-end: I didn't follow the use of the GEV distribution here. You have a large sample size, 10 ensemble members of 30 years), so you should be able to calculate a return period from that? Why model it? Perhaps it is to use a covariate and account for the transient response? I think this needs more clarification.

P12 L13-17: It is not clear to me if land use/cover is different in the different scenarios. This will be important here, so should be clarified.

Conclusions/discussions: I think this section should be shorter. I like the discussion part but feel other results are repeated in too much detail.

P13 L17: "simulations to our knowledge" please change to "simulations using a fully coupled model to our knowledge".

P13 L32: "longer than a century" please add "and therefore were not assessed in this study".

P14 L20-27: I feel this section could be a bit more balanced between this study and HAPPI. I.e. there are advantages and disadvantages to both. Also, note that 2 of the HAPPI models have done sister experiments which include some form of ocean coupling. i.e. the NorESM2 models, and the MetUM-GOLM model. In both cases, they use slab oceans.

References

Gasparrini, Antonio, et al. "Mortality risk attributable to high and low ambient temperature: a multicountry observational study." *The Lancet* 386.9991 (2015): 369-375.

James, Rachel, et al. "Characterising half a degree difference: a review of methods for identifying regional climate responses to global warming targets." *Wiley Interdisci-*

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