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Geoscientific Model Development Discussions

Interactive comment on "Half a degree Additional warming, Projections, Prognosis and Impacts (HAPPI): Background and Experimental Design" *by* Daniel Mitchell et al.

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Summary

We thank the reviewers for their nice assessment of our paper. All reviewers suggested minor corrections, and we have responded to all of them, making most suggested changes. A running theme was in expanding the discussion on various choices of model forcing that we have chosen. This has been done, with a specific focus on how fixed SST experiments may alter extreme events compared with coupled-ocean experiments.

Reviewer 1

1 - How the SSTs anomalies are created is currently ambiguous. The text implies the anomalies are the difference between 2006-2015 and 2091-2100 in RCP2.6 (or 4.5). However, Figure 3 would suggest that RCP8.5 is used as the recent period anomaly baseline. The authors should make this clear. We use the RCP8.5 scenario for the 2006-2015 baseline, as this is the closest to observations. This has now been made clearer in the paper. L128-130. 2 - The selection of 2006-2015 as the base period is convenient, as the authors note because it represents a 'stable' recent climate. However, it might also prove trouble- some in exactly recreating a 1.5 degree and 2 degree warming because it's likely that the hiatus years bracketed by this period are likely cooler than the climatological at- tractor (Meehl et al 2016, Trenberth Fasullo, 2013). However, the RCP average for 2006-2015 will represent the climatological attractor. As such, one would expect that the 1.5 and 2 degree SST reconstructions will be biased cold when compared to the CMIP5 multi-model average from which they were derived. The fact that the CMIP5 average temperature for RCP2.6 for 2091-2100 is actually 1.55K above pre-industrial might actually compensate for this bias, but the authors should probably at least dis- cuss the implications of using the recent hiatus period as the baseline. We have now added in this short discussion, but we note that it is the increase of a 0.5K warming in low emissions scenarios that is important, rather than the absolute numbers. L134-139. 3 - The decision to use a linear regression to produce sea ice distributions is well de- fended (or at least a case is made for why it would be inappropriate to use the RCP model sea ice distributions directly), but whether the regression will actually work or not is conditional on exactly why the current generation of models is apparently biased in its Antarctic sea ice distribution. If the discrepancy between recent historical Antarctic sea ice behavior is due to non-temperature mitigated variability (as suggested by Turner et al, 2015), then such a regression-based approach might produce an under-estimate of the likely sea reduction in the antarctic at the end of the century. Perhaps the proto- col could include a second tier sensitivity study to assess how the conclusions might be impacted if the authors instead took a more model-centric view of future sea-ice change. We feel that every choice of forcing

in our setup could be tried and tested through sensitivity analyses, and some of this will be performed by individual modelling centres. However, for this particular case we feel that it is not worth dedicating space in the experimental design, as the same could be said for many other equally important components of our experimental design. Instead we concentrate on the fixed SSTs as the reviewer suggests below. 4 - Framing the experiment as a single 10 year period with anomalies added carries a risk that the significance of the difference in the two climate states is going to be overestimated. Because every simulation in the ensemble will undergo the same SST variability, the difference in the resulting climate arising from the anomaly pattern dif- ferences will be put in the context of atmospheric noise only. This is, of course, an underestimate of the natural variability which would be present if ocean states were also sampled - and as such, there is a concern that studies based on this ensemble might over-state the differences in climate risks between the two temperature levels. It is clear enough why the AMIP-based approach conveys some advantages in its ability to address the Paris Agreement's request, but unless the space of SST variability is sampled - there is a danger that studies based on these ensembles will tend to conclude that differences between the 2 degree and 1.5 degree climate states are more significant than they would actually be in reality. This concern could be addressed using another second tier experiment. Whereas the tier-2 experiments in the current version focus on uncertainty in the anomaly pattern - I would see some stronger logic in using that computing time to sample the impact of SST variability. A simple experiment could be to replace the 2006-2015 baseline with RCP SSTs from individual models, then adding the same anomaly as is used in the original experiments. As such, one could create an ensemble of constructed 2091- 2100 periods with different SST variability - sampling the range of possible modes of varaibility which could potentially define that decade. We agree with the reviewer on this point, and have added an additional experiment set under Tier 2 to reflect this. This experiment uses another model (MetUM-GOML2) to run coupled ocean experiments and fixed SST experiments in a fashion that allows for a direct comparison. As this is a new experimental design, we have dedicated a large section to

explaining it. L194-250. 5 - the choice to only change the CO2 forcing between the 1.5 degree and 2 degree simulations may cause the difference between the two simulations to be less than 0.5K. The decision to not change land use or aerosols between the two simulations is un- derstandable - and the net radiative forcing difference between RCP2.6 and RCP4.5 for these agents is unlikely to be significant. However, this is not true of non-CO2 greenhouse gases (CH4, N2O CFCs) which do differ between RCP4.5 and RCP2.6 and will likely have a significant impact on the net radiative forcing. Furthermore, be- cause these are well mixed gases - it would be easy enough to produce consistent concentrations in a similar manner to the CO2 calculation. Yes, this was originally missed from the paper. All well-mixed GHGs are scaled as the reviewers suggests. L169-173.

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