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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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The submitted paper details a proposal for HAPPI - a model inter-comparison project designed to respond to the request in the 2015 Paris Agreement for the IPCC to assess climatological differences between futures under which the world underwent a 1.5 or 2 degree warming above pre-industrial values. The paper outlines an experimental protocol in which AMIP experiments are conducted to represent the recent past, and futures corresponding to the two levels of warming. The AMIP experiment for the recent past is a 10 year experiment under a standard AMIP protocol, whereas the future experiments use anomaly fields derived from multi-model averages from the CMIP5 RCP experiments. The 1.5 degree future is achieved using the CMIP5 multi-model

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Discussion paper

average anomaly sea surface temperature fields from the RCP2.6 experiment (2091-2000 vs 2006-2015), which is added to the historical 2006-2015 SSTs. The 2 degree experiment uses a linear combination of RCP2.6 and RCP4.5 SSTs, which is predicted to produce a warming of .5K greater than the 1.5 degree experiment. Non-CO2 forcings and land use are kept constant in the two experiments using the RCP2.6 values for both. Sea ice concentrations will be predicted through a linear regression approach, relating historical SSTs to Sea Ice concentration.

The paper makes a strong case for why this experimental design will provide useful data for the IPCC Special Report on 1.5 degrees. The existing CMIP5 ensemble experiments do not provide a clean answer to the question posed for the report (that of how a 1.5 and 2 degree climate are likely to differ), and the experimental design proposed for HAPPI will certainly provide a well sampled estimate of the climatology associated with these two warming levels. My conclusion is that the paper, and concept are broadly sound - and that the paper and subsequent ensemble will be an asset for the IPCC in their preparation of the special report, in conjunction with other data. I have some minor comments on the technical details of the implementation but see no major flaws in the article.

Minor Comments:

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.

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 troublesome 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 attractor (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 discuss the implications of using the recent hiatus period as the baseline.

3 - The decision to use a linear regression to produce sea ice distributions is well defended (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 protocol 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.

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 differences 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 con-

clude 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.

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 understandable - 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, because these are well mixed gases - it would be easy enough to produce consistent concentrations in a similar manner to the CO2 calculation.

I do not consider any of these issues to be fatal flaws, but look forward to the authors' thoughts. Furthermore, I look forward to seeing the data and results arising the experiment - which should be an asset to the international climate community,

Ben Sanderson

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