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

Thanks. We added the following paragraph to this point: 'CESM1-CAM5 is a single climate model, and like any model is subject to biases in both its present day simulations and in future projections. As such, the ensemble spread in this study does not repre-

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sent true uncertainty in future projections, rather a single estimate of climate evolution. However, multi-model assessments in the past have indicated that CESM1-CAM5 is one of the better performing models in the CMIP5 archive. Sanderson (2015) found this model to be the best performing in a selection of mean state metrics, and Flato et al, Figure 9.37 shows that the model has one of the better simulations of extreme temperature and precipitation metrics in the CMIP5 archive.'

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.

Added this to the abstract to make clear that the emulator was only tested in the 21st century

'These scenarios . . . are realized (for the 21st century) in the coupled model and are freely available to the community.'

And added the following to the temperature evolution results:

'Note that although these results suggest that the predicted emulation of stable temperatures in the emulator is validated for the evolution of global temperatures in the coupled system in the 21st century, there may be ocean dynamical processes at longer timescales in the coupled model which are not represented in the emulator's thermodynamic ocean. As such, we have not tested the stability of global temperatures at multi-century timescales with these emissions pathways."

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.

Changed abstract as follows: "Here we describe the design of the simulations and a brief overview of their impact-relevant climate response. "

Added this to the end of the introduction:

"We aim in this study to provide a short overview of differences in impact-relevant climate variables, with the hope that further studies will focus in more detail on specific processes, regions or societal impacts."

Responses to 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.

The list is far too long to print in the paper. We've added a link to an online table: http://www.cesm.ucar.edu/projects/community-projects/LENS/data-sets.html

P1 L11: "and only 1 in 40. . ." I think 'only' can be deleted here.

done

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.

Expanded as follows: "Since then, the world's greenhouse gas emissions have been closer to the highest emissions pathway (RCP8.5) than any other, even accounting for a recent slowdown in emissions growth (Van Vuuren, 2011) (aerosol evolution differs relatively much less between RCPs, which are so far broadly in line with observations (Kilmont 2013)."

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

Agreed. Changed as follows:

"Although some individual models exhibited less than 1.5C warming in this scenario (median warming was 1,6C), CESM warming in this scenario is closer to 2 degrees (Meehl 2013)"

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

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

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

Edited to say:

Pattern scaling techniques, which assume that patterns of temperature and precipitation change can be scaled by global mean temperatures can produce quite skillful reproductions of mean climate shifts within 21st century projections (Tebaldi, 2014).

P2 L27: Acronym is wrong. It should read: “Prognosis and Projected Impacts”. *Updated*

I think Mitchell et al, 2017b should also be added, which was the concept of HAPPI.

Added.

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

Done.

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.

Done.

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.

Changed as follows: “Because simulations in HAPPI will have one of a finite set of predefined SST patterns, the estimate of significance of the difference in climate states

will not completely sample ocean-driven variability."

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.

It was referenced in the previous section, but we've now included it here as well.

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

The large ensemble initial conditions (which we used for these simulations) did not diverge before 1920 (i.e. there's only one ensemble member until then, after which they branch). As such, using 1850-1880 would have been rather noisy - hence we used the longer time period. We've added a sentence to this effect: "(where pre-industrial is taken as the 1850-1920 mean - averaging over the period before the model initial conditions were branched, and using a 70 year rather than 30 or 50 year mean because there is only one ensemble member for this period). "

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

done

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.

We've added the caveat section in response to your major point above, and we've re-worded the paragraph to remove the word "stabilized":

The 1.5degNE scenario reaches 1.5C above pre-industrial levels and then maintains this temperature until 2100. The 1.5degOS scenario reaches a peak temperature in 2050 of 1.7° above pre-industrial before cooling to 1.5C by 2100, and the 2.0degNE scenario reaches slightly over 2.1C by 2100.

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

done

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

We already say “ The inherent uncertainty in the sea level response at a given emissions level is greater than the difference between the scenarios considered here. “

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.

Added: “And while it should be noted that there is large diversity in the rate of loss of Arctic sea ice in CMIP5 models (Mahlstein 2012), the CESM sea-ice loss per degree of global warming to date is less than that which has been observed (Rosenblum 2017). “

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.

There isn’t space to evaluate CESM’s historical performance here and I would argue that (1) it’s already been comprehensively done and (2) the mean state temperature bias plot tells you relatively little (if anything) about the temperature response (otherwise we would be using it to constrain the response).

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.

Added as suggested

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.

The clarification is well explained in the source reference Tebaldi and Wehner (2016). At the grid-point level, the sample size is insufficient to provide a smooth empirical distribution for this level of extreme event, so the extreme value analysis is necessary, even with a 10 member ensemble.

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.

We already detail this in the introduction: " In each of the low emission scenarios in this paper, only well-mixed greenhouse gas concentration are changed between scenarios, all other forcings (land use, aerosol emissions, and ozone) follow RCP8.5 throughout the 21st century as in (Kay 2012)."

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

We have removed 3 paragraphs which repeat information from the results section.

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

P13 L32: "longer than a century" please add "and therefore were not assessed in this study". *This paragraph is now removed*

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.

Agreed. Re-written as follows: "A key factor determining whether or not there are significant differences between 1.5C and 2C of warming is the magnitude of the climate system response to a difference of half a degree of warming relative to internal climate variability. This question can be informed by ensembles of coupled climate

model simulations including estimates of internal variability arising from the ocean and atmosphere, and yet. These experiments should hopefully provide context (for a single model) for the primary multi-model effort being pursued by the community to address the difference between 1.5C and 2C of warming (HAPPI, Mitchell 2017) which relies on AMIP simulations with prescribed SSTs."

Please also note the supplement to this comment:
https://www.earth-syst-dynam-discuss.net/esd-2017-42/esd-2017-42-AC1-supplement.pdf

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