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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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Sanderson et al. have compiled a very useful set of climate simulations to aid the community in comparing a 1.5 degree future to a 2 degree future. This paper does a good job of laying out the methodology, as well as presenting a limited set of analyses of interesting impacts. The writing is clear, and the figures are informative and well-presented. I also commend the authors on providing links to open-source code for the simple model as well as the data for the simulations.

I do have a number of minor comments for the authors to consider when revising the manuscript for publication. Two in particular might require some recalculations (see line-by-line comments below for more detail). First, I would recommend using the more recent Kopp et al. semi-empirical equations in place of the Rahmstorf et al. (2007)

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version. Second, I would consider comparing future model results against historical records from the model, rather than from observations, in Figure 3 and related text. I can understand not taking the latter recommendation, but in that case, I would include text discussing the how differences between model and observations during the 1976-2005 historical period might impact the results. Other than that, most of my comments below are generally minor sentence edits.

Line by line comments:

Page 1, Line 17: I would also reference Fawcett et al. here as it has a good analysis of the probability of staying below various thresholds for the RCPs (based on simple models, not full GCMs, of course) (DOI: 10.1126/science.aad5761)

Page 2, Line 5: I am unclear how the authors determined that this commitment would lie "on the verge of economic and physical plausibility": as I read Smith et al., they evaluate the cost & capacity of negative emissions technologies, but do not define a plausible upper bound to how fast society could implement these technologies if properly motivated. I would rephrase this sentence to be more judicious, e.g., "likely requiring substantial commitment to negative net carbon emission technologies in the 2nd half of the century".

Page 2, Line 19-20 and 23-24: it seems to me that "finding time periods from other scenarios" is equivalent to "another approach is to 'time-shift' by taking periods...": I would delete one of these descriptions.

Page 4, line 14: delete "in a decade" (it is redundant with "by 2027").

Page 5, line 2-3: I understand why the authors made the justifiable choice of using RCP8.5 non-GHG forcings for all the scenarios – however, it would be useful to have a brief aside that notes that a 1.5 degree scenario that is internally self-consistent might look slightly different than a 1.5 degree scenario that is a hybrid between RCP8.5 non-GHG forcings and low-GHG-concentrations. In particular, sulfur emissions might be even lower in a 1.5 degree scenario (like RCP2.6 has lower sulfur than RCP8.5, though comparing RCPs should also be done with caution because they come from different IAMs so not all differences are necessarily due to policy effects), which would require even tighter GHG emissions reductions, but more relevantly for the paper, might also impact temperature patterns because aerosols have different land/ocean and hemispheric forcings than well-mixed GHGs. Similarly, a 1.5 degree future that relies heavily on bio-fuels would have very different land-albedo forcings. I also note that the choice of setting RCP2.6 as a limit to non-CO2 GHG reductions has an influence as well, possibly contributing to the necessity for the long-term CO2 emission floor to be negative (Table C1, column 7) because the CFCs and maybe N2O have lifetimes long enough that their concentrations would not have stabilized by 2200, requiring ongoing offsetting negative CO2 emissions (I think this study does not include SF6 or PFCs).

i would also potentially be curious regarding where CESM falls in terms of climate sensitivity in the larger CMIP universe, which would also determine how aggressive the GHG mitigation would need to be to stay below given targets.

Page 5, line 3: Fix parentheses – in Kay et al. (2015).

Page 5, line 18-20: I would suggest replacing the Rahmstorf (2007) semi-empirical approach with that from Kopp et al. (2016) (doi: 10.1073/pnas.1517056113): Rahmstorf is a co-author of the latter paper, which claims to reconcile the semi-empirical approach with process-based models, and therefore I would consider this to be an improved update to the Rahmstorf equations. (a correction would still be required for ice sheet melt, so Horton et al. 2014 might still be appropriate there).

Page 8, Line 15-16 (and Figure 3 generally): I note that during the period 1976-2005, models have already exceeded the maximum historical observed temperatures from 1976-2005. This makes me wonder whether the appropriate comparison should be between future model and historical model, rather than future model compared to historical observations. Either that, or there should be a discussion of this potential discrepancy. Reading Lehner et al., I think that paper did do compare to historical models – e.g., Figure 3 has both the observed 1920-2014 and the model 1920-2014, so that one can compare model-future to model-past, e.g., like-to-like. I recognize that there is still the potential for model bias to creep in here (as discussed in Lehner): if the model has more variability than the observed, then it is harder to exceed records, and vice versa, but I think a like-to-like is a cleaner comparison.

In addition, for section 3.3, I don't see a cited source for the observed temperatures – is it BEST as in Lehner? Or ERAi?

I would suggest an additional paragraph here which could do several things: 1) note the source of observed temperature data, 2) discuss how well the model reproduces the observations over the 1980-2005 period (and/or if any bias-correction is being used here), 3) discuss the model-observation comparison over the 2006-2016 period.

Page 8, Line 26-27: Related, is this "noteworthy" statement regarding the 2006-2016 period? Please clarify, and see above suggestion.

pg 8, line 33-34: 1) some regions experience a greater increase in extreme than in mean: is the opposite true as well? 2) following up on that: it would be very interesting to have a quantitative estimate of this effect: e.g., x% of the land area experiences a warming of extremes more than 50% faster than the mean, while y% experiences a warming 50% slower than the mean. Or, averaged across land areas, extremes warm x% faster than the mean. Or something like that.

pg 9, line 9: I might note that the greater signal to noise is seen at lower latitudes even though absolute warming at those latitudes is generally smaller (which has the opposite effect of there being less variability at those latitudes).

pg. 21, line 5: stray period should be deleted.

Figure 2: "subplot" should be singular.

Figure 4: Is the historical period 1976-2005? Please specify. Also, it is based on

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observed (like Figure 3) or on modeled historical (like I think that most other figures do)

Figure 6: legend needs more detail: I assume that black is modeled historical, but it could potentially be observed. Also, what's the time period of smoothing – annual?

Figure C2: Please include a legend for the colors as in Figure 1. Also, I don't think it that this figure extends far enough to demonstrate this, but I'd be curious about whether the 1.5NE and the 1.5OS could be used to investigate path dependence/memory/inertia. For example, one might expect some additional warming of the Arctic Ocean during the years in which the temperature is above 1.5 degrees which might take a number of years to dissipate even after the global surface air temperatures have returned to 1.5 degrees, which might lead to slightly lower sea ice extent in the 1.5OS case than then 1.5NE case for some years after stabilizing back at 1.5 degrees.

(One could go further, and imagine hypothetical tipping points that could be exceeded in the 1.5 OS case which would not be resolved by cooling back down to 1.5 degrees, but I would imagine that this would be somewhat unlikely, and even if such a tipping point existed, this modeling system might not be able to catch it).

(sea level could be a place where there might be some long-term memory of a brief excursion to 1.7 degrees, as in Zickfeld et al., doi: 10.1073/pnas.1612066114: Figure 1(c) seems to show this – it might be interesting to note the divergence between 1.5OS and 1.5NE at 2100 and what date the two scenarios become equal, if they ever do)

Table B1: Would it be possible to include an additional column with the values of each parameter that resulted from the calibration process?

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