

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

Benjamin M. Sanderson et al.

bsander@ucar.edu

Received and published: 29 June 2017

Thanks to the reviewer for the thoughtful comments. Attached are responses to individual points:

First, I would recommend using the more recent Kopp et al. semi-empirical equations in place of the Rahmstorf et al. (2007) C1 version.

Done - using the 10th and 90th percentiles of Kopp's distributions of the 'a' and 'c' parameters to define the uncertainty range.

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

C1

discussing the how differences between model and observations during the 1976- 2005 historical period might impact the results.

Thanks for the suggestion, but for a number of reasons we would like to keep the figure as a direct comparison to observations. However, the figure produced in the way you suggest is attached.

The results are very similar as in the original Figure 3, except for a mean offset that comes from the fact that you look for a record in 10 ensemble members rather than 1 observational record. As such, any given record will very likely be higher because it is based on 10x more samples, making that record harder to break in the future. One could pick a record from one arbitrarily individual simulation, but we feel that would make the figure less relevant as a communication tool. Using all ensemble members, all the time series in Fig 3 would shift down but basically keep their shape and relative position. This would imply about 30As such, and for consistency with Lehner (2016), we would prefer to keep the plot as-is.

We added the following caveat: “It is notable that some records in Figure 3 are exceeded before 2005 because the historical evolution of the CESM ensemble differs from the real-world historical evolution and there could potentially be some model regional model biases. However, the behavior of CESM in the period 2006-2016 is in within the range of model record exceedance (both globally and in each of the regions considered), giving confidence that regional biases are not strongly influencing this metric. Note that if the records were taken from the historical simulations of CESM itself for consistency, almost all historical records would be a higher temperature because the effective sample period in a 10 member ensemble is 300 years for the period 1976-2005, which causes a 30 percent reduction in end of 21st century record exceedance (see discussion with reviewer M.Sarofim for further details).”

Other than that, most of my comments below are generally minor sentence edits.

Line by line comments:

C2

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)

Agreed. Done.

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”. *Rephrased as suggested*

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. *Agreed, removed the earlier sentence.*

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

Done.

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 nonGHG forcings and low-GHG-concentrations. In particular, sulfur emissions might be C2 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

C3

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).

Added the following paragraph to address these points:

It should be noted that the assumptions of RCP8.5 trajectories for non greenhouse gas forcers is implemented for practical reasons to make the present study tractable. However, a fully self consistent 1.5 degree scenario from an Integrated Assessment Model (IAM) would likely have slight differences. Sulfur emissions are lower in RCP2.6 than in RCP8.5, and maybe lower still in a 1.5 degree scenario (although there large differences in sulfur emissions between individual IAMs with the same policy constraints). However, any given global temperature target could be achieved with different combinations of aerosol forcing and greenhouse gas forcing, but with regional differences in temperature and precipitation (Xu 2015, Pendegrass 2015), and changes in land use necessary for large scale biofuel production would change surface albedo (Caiazzo 2014). An IAM could also have additional degrees of freedom, with the capacity to reduce N₂O and CFC emissions below the RCP2.6 minimum levels.

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. *Added the following: "CESM also has a higher climate sensitivity (4.0K, Gettelman 2012) than the CMIP5 mean (3.6K, Webb 2013), and so emissions need to be reduced faster than average for this model in order to meet any given temperature target.*

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

C4

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). *Done as suggested. Figure 1 has been recomputed with the Kopp semi-empirical model.*

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.

As in our response to your major point, our issue here is which historical simulation to use - using all ensemble members to define records would create a meaningless result (because the records would be over a much longer time period). Using just one model would introduce an arbitrary bias. We have added a caveat paragraph to explain the potential for model bias - but we feel that the plot has most value in contrast to actual real world records, with a marked increase between the model's exceedance of real-world records in the late 20th century and the mid 21st century suggesting that the model biases do not dominate this metric.

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

C5

could do several things: 1) note the source of observed temperature data, *We already do - we are using the BEST data as in Lehner (2016): "Observations are from BEST, as in Lehner (2016). "*

2) discuss how well the model reproduces the observations over the 1980-2005 period (and/or if any bias-correction is being used here), *We have added the following caveat: "It is notable that some records in Figure 3 are exceeded before 2005 because the historical evolution of the CESM ensemble differs from the real-world historical evolution and there could potentially be some model regional model biases. " But we also note that the IPCC found CESM to be one of the better models in representing historical temperature extremes, as follows: IPCC-AR5, Figure 9.37 shows that the model has one of the better simulations of extreme temperature and precipitation metrics in the CMIP5 archive. "*

3) discuss the model-observation comparison over the 2006-2016 period. *Thanks for the suggestion - this period is, we think, an assurance that the biases in CESM are not dominating global or regional aggregate numbers of records exceeded: "However, the behaviour of CESM in the period 2006-2016 is in within the range of model record exceedance (both globally and in each of the regions considered), giving confidence that regional biases are not strongly influencing this metric."*

Page 8, Line 26-27: Related, is this "noteworthy" statement regarding the 2006-2016 period? Please clarify, and see above suggestion. *We have deleted this sentence.* 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., xpct of the land area experiences a warming of extremes more than 50

Done as suggested: "(in 2.0degNE, 9% of the land area experiences a warming of extremes more than 50% faster than the mean, while less than 1% experiences a warming 50% slower than the mean)."

C6

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). *Agreed, done: "... the greatest signal to noise is seen at lower latitudes (although the absolute magnitude of warming is smaller)."*

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

Figure 2: "subplot" should be singular. *Fixed* Figure 4: Is the historical period 1976-2005? Please specify. Also, it is based on observed (like Figure 3) or on modeled historical (like I think that most other figures do) *Fixed: "Maps showing the expected number of times that the modeled historical 1 in 20 year 3-day warm event in the period 1976-2005 would be exceeded during the period 2071-2100"*

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?

Expanded as follows:

"Changes in annual mean precipitation at the (a) Global, (b) Land-only and (c) high Northern latitude land. Values are relative to the 1921-1960 average. Grey lines show members of the historical CESM ensemble, while black line shows the historical mean. Thin colored lines show individual ensemble members for future scenarios, thick bold lines show the ensemble mean." Figure C2: Please include a legend for the colors as in Figure 1. *Done* 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

C7

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)

Thanks for the suggestion. Our co-author Alex Jahn is writing a dedicated paper on this topic, so we defer these questions of path dependency to that study.

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

Please also note the supplement to this comment:

<https://www.earth-syst-dynam-discuss.net/esd-2017-42/esd-2017-42-AC2-supplement.pdf>

Interactive comment on Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2017-42>, 2017.

C8

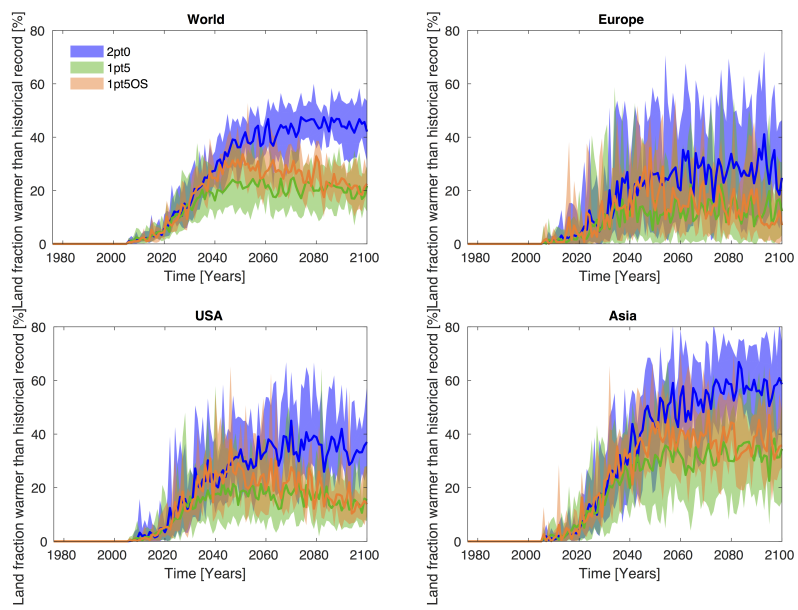


Fig. 1.