Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC2
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Review of MCE v1.2 by Junichi Tsutsui

Christopher Smith (Referee)

Referee comment on "Minimal CMIP Emulator (MCE v1.2): a new simplified method for probabilistic climate projections" by Junichi Tsutsui, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-79-RC2, 2021

This paper uses a Bayesian method to produce constrained projections of future warming in a three time-constant impulse response climate model (the Minimal CMIP Emulator, MCE v1.2). I find the level of detail given to be sufficient and the model description technically sound. The carbon cycle dynamics are impressive. The model is run in configurations that both emulate CMIP6 model behavior (blue lines and in section 3) and a "constrained" ensemble set (orange) that is constrained to ranges of key climate variables that are discussed in Nicholls et al. (2021).

My only major comment is not on the model itself but in the example application. The low projections of the constrained ECS and warming – assuming the ranges of Nicholls et al. (2021) which used the Sherwood et al. (2020) ECS assessment – are possibly too low. Sherwood et al. (2020) gives an ECS very likely (interpreted as 90% probability) range of 2.3 - 4.7 K, whereas the constrained distribution (I'm reading off figure 8 d) has a 90% range of less than 3.5 K. The implications of this are seen clearly in Figure 12: compare AR6 projections in Chapter 4 of that report (for SSP1-2.6, which is a "well-below 2C" rather than a "1.5C consistent" scenario) and assessment of present-day temperature in Chapter 2. The "constrained" range seems over-constrained (compare projections for SSP1-2.6 in AR6). Can you really have this much confidence in the future warming? I would just like some additional explanation as to why the model seems to provide such low constrained estimates of future warming (and whether alternative constraints would show something different). Such a discussion is important, if the aim for this model is to be used by the climate change mitigation community.

Specific minor comments:

Line 36: more specifically, the Working Group 1 contribution

Line 38: insert “the” before “climate assessment”

Line 50: reduced from > smaller than

Line 51: RWF: state that RWF is TCR / ECS

Line 54-55: on aerosol forcing in CMIP5 and CMIP6 – the aerosol forcing is part of the story but not all; as a shameless plug see Smith and Forster (2021): https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2021GL094948

Line 62: Nicholls et al. (submitted) is now published in Earth’s Future (and line 279).

Line 109: “about” 0.2 : would it not be asymptotically exactly 0.2, as this is the partition fraction of the infinite time constant box. I’d just suggest removing “about”.

Line 125-126: the text describes 17th and 83rd percentiles but the caption and legend in figure 3 say 5 and 95.

Line 138: “such as volcanic eruptions and geoengineering mitigation” - these results are also mentioned in Leach et al. (2021: https://gmd.copernicus.org/articles/14/3007/2021/gmd-14-3007-2021.pdf) and Cummins

et al.
(2020:
https://journals.ametsoc.org/view/journals/clim/33/18/jcliD190589.xml) that advanced the benefits of three-box IRMs - might be worth mentioning.

Line 228: AR6, WG1, Chapter 7 supplementary material updates this.

Lines 229-231: that's a shame, but OK. No need to change the model, but it's not too difficult to include these elements. The non-CO2 components could be borrowed from FaIR (Leach et al. 2021), and uncertainties for aerosol forcing in CMIP6 models can be obtained from 11 models that participated in RFMIP and AerChemMIP historical aerosol forcing experiments – tuning the parameters similarly to what has been done in this paper for the carbon cycle with the C4MIP models in Arora et al. (2020) (see Smith et al. 2021, https://doi.org/10.1029/2020JD033622).

Line 330: 90% and 66% *of* members

Line 352: "...and the 'Constrained' trend appears underestimated." Some variation in the forcing time histories, particularly for aerosols, could help here. See Smith et al. 2021: https://doi.org/10.1029/2020JD033622

Line 403: "or later" is incorrect: the SSP forcing label refers to 2100.

Fig 12a, legend: 17-83%?

Fig 12: I can't determine what the black error bars are showing – if it is GMST from observations, why they are different for blue and orange distributions, and if they are from the ensembles, why they don't exactly overlap the uncertainty plumes (in which case they

would be superfluous anyway).

Lines 446-450: can you clarify whether the CO2 forcing expression is only valid in the 1x to 4x range?

Line 460: overwrapping > overlapping

Lines 479-480: Actually, there is no correlation between the strength of aerosol cooling and CO2 warming in CMIP6. See Smith et al. (2020: https://acp.copernicus.org/articles/20/9591/2020/acp-20-9591-2020.html, figure 9, and I also made this Twitter thread showing the difference compared to the Meehl paper step-by-step: https://twitter.com/chrisroadmap/status/1297798254789263360)

Line 491: "cited with" probably not the correct word choice here. "Driven by"?

Line 517: "human-induced warming is similar..." also found to be true in AR6 (sorry, I know you submitted this paper before AR6 became publicly available, and I was heavily involved the report so I see AR6 statements everywhere).

Lines 529-533: Just a note to say I agree, and this actually was considered in the emulator runs for AR6. A long (2250 year) pre-industrial time series of volcanic forcing was used before the anthropogenic forcing starting in 1750 was assessed.