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Earth System Dynamics Discussions

Interactive comment on "On the Future Role of the most Parsimonious Climate Module in Integrated Assessment" *by* Mohammad M. Khabbazan and Hermann Held

Anonymous Referee #1

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The manuscript by Khabbazan and Held assesses the performance of a very simplified climate module currently in use in some IAMs. In particular, the study is motivated by the need to adjust the existing tools to the capability of this module in the light of assessments of below 2°C scenarios. To that end, it is fitted to different CMIP5 RCP2.6 AOGCMs.

The manuscript contains no fundamental flaws although a re-read is in order and the literature list should be checked. One key paper (Foster et al. 2013) is for example missing from the literature list. I presume it's Forster, P. M., T. Andrews, P. Good, J. M. Gregory, L. S. Jackson, and M. Zelinka, 2013: Evaluating adjusted forcing and model

spread for historical and future scenarios in the CMIP5 generation of climate models. J. Geophys. Res. Atmos., 118, 1139– 1150

More fundamentally, however, the scientific advancement presented of this study in my assessment is rather poor and it neglects important recent literature in this context (in fact, the literature list is rather short and at least 3 years old).

On the methodological approach: What's the justification of using the PH99 model (apart from it 'being there')? The authors argue that it's computational efficiency, but how exactly are they convinced that their treatment of non-CO2 GHGs is appropriate. For strong mitigation pathways, these 'minor' differences may become very important, last but not least to determine net-zero global GHG forcing etc. I'd think they would need validate their fit using other strong mitigation scenarios with different non-GHG trajectories (if no others are available then from the GeoMIP experiment) rather than RCP4.5. In particular, it appears that non-CO2 gases obscure our assessments of ECS (see e.g. Myre et al. 2016)

As it stands, I'm not convinced that the simplified model is capable of including non-GHG forcing in a sufficient fashion for the question at hand (i.e. staying below 2°C or 1.5°C).

On the application: I didn't fully the motivation for step 1. What was the reasoning for the authors to assume that their PH99 model would work with AOGCM diagnostics from Forsters et al. directly? Obviously, the derived feedback response time parameter 1/alpha of 34.5 years in the multi-model mean is quite unphysical. It seems that the PH99 model is not equipped to be used in that context.

In a next step, the authors find that with two free parameters they are capable of achieving better fits. That's not particularly surprising, but a physical interpretation of these differences is virtually absent? ECS is substantial decreased by almost 1°C. Can this be understood? The authors continue with fitting derived and fitted ECS and TCR, but I would rather like to see a physical interpretation or an extension of the PH99 model that would correct for this. The authors should also consider their approach in the light of alternative simplified approaches out there i.e. based on a response function approach as in Ragone et al. (2016).

The authors then want to apply an effective correction for their dubious model in the first place. Their results here appear to be prone to outliers. Compare e.g. the low ECS outlier in Fig. 5. When removing it, I guess even a linear fit would deliver decent results and I'm not sure I can deduce any robust trends from these graphs. . .

On the application of this. It seems that the model that is used in these IAMs has many flaws. The question then becomes why it is used at all? And not abandoned for a carbon budget approach that would be even more computational effective and can be determined with more complex models also for these low emissions scenarios (i.e. Rogelj 2016). That becomes in particular relevant since the mitigation challenge ahead is to define pathways that hold warming 'well below 2°C'. "Below 2°C" was interpreted as a 66% chance of non-exceedance (IPCC 2014). What's the added value of using a PH99 model in this context? Would they select an ECS at the 66% quantile and then use this as a basis for the IAM derivations? And if so, why not use carbon budgets directly?

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