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Comment on egusphere-2022-167

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Referee comment on "Potential for bias in effective climate sensitivity from state-dependent energetic imbalance" by Benjamin M. Sanderson and Maria Rugenstein, EGUsphere, https://doi.org/10.5194/egusphere-2022-167-RC2, 2022

General Comments:

In the manuscript under discussion, the assumption that global climate models evolve to some top-of-atmosphere radiative balance is put to the test. For this, millenia-long runs from LongrunMIP are used, alongside a linear response model with responses on three time scales. Based on the found energetic imbalances in some models and equilibrium temperature estimates, the biases in the latter are related to the former, concluding that energy leaks might influence common equilibrium climate sensitivity estimates much.

I find this an interesting and important exercise, with conclusions that could have big consequences for long-term projections with some global climate models. However, I am not fully convinced by the used methodology. Further, I think the text could be clearer at certain points. Finally, the presentation of the figures feels a bit sloppy with especially colors and line styles not matching with the captions. These issues should be resolved before I would recommend publication.

Specific Comments:

(1) Central in the manuscript is the linear response type model in equations (1a)-(1b). I do not think that these equations are explained well enough nor that made choices are acknowledged and defended well enough. I also have some problems with their use for non-constant forcings.

(1a) First of all, the form of (1a)-(1b) is now defended as consistent with some simple (linear) climate models. However, it also fits with linear response theory as the response of a non-linear model "in the linear response regime". In [Proistosescu and Huybers

(2017)], they already frame it in this way, and e.g. in my recent paper [Bastiaansen et al (2021)] this link is made even more explicitly. I think it would be good to clarify these things, which also would further communicate the validity of (1a)-(1b). Further, nowhere is it mentioned that equations (1a)-(1b) only hold for constant forcings, and that the parameters would be different for other forcing levels. These important 'terms and conditions' for the use of (1a)-(1b) should be added.

(1b) It is now assumed that all climate models have a response on three distinct time scales. This choice for the number of time scales should be stated explicitly and a better justification needs to be given. Why should all models have the same number of response time scales? Why should there be precisely three time scales? For me, this now seemingly arbitrarily made choice is one of the weakest points of the paper and could render all your conclusions moot: what if a system actually has more than three time scales and all the remaining observed radiative imbalance disappears if you were to take all these time scales into account? So, did you check if results remain similar when a different number of time scales are used?

(1c) For a few models in LongrunMIP, the abrupt4xCO2 experiment was not long enough, and the results for a different forcing scenario were added at the end of the abrupt4xCO2 simulation in an attempt to construct a long enough simulation. However, the used linear response model in equations (1a)-(1b) is only valid for constant forcings, but the used runs have non-constant forcings (1pct2x, 1pct4x and RCP8.5). To me, that means the equations simply cannot be used. In particular, the timing of forcing in these experiments is of uttermost importance to properly assess the response over time, and splicing runs together like this therefore makes no sense to me. An alternative would be to derive a linear response model for the used forcing scenarios, and use that to fit the parameters from which the abrupt response could be inferred (taking some liberty with the 'ensemble-average' assumptions underlying linear response theory).

(2) In (1a) and (1b) the parameters T0 and R4x are playing similar roles. However, they are not determined in the same way, as T0 is derived from the control experiment instead of fitted with the abrupt4xCO2 experiment. The reason for doing this should be explained.

(3) To obtain the model parameters from the data, one way or another a nonlinear fitting procedure needs to be used. Those can be sensitive to the choices for metaparameters -- in this case, the choices for the priors (i.e. the mentioned distributions in Table 1). Did the authors check to make sure the presented results do not depend too much on these priors? Additionally, the choices for the prios should also be explained better; now, it just seems to be some made up numbers, but there certainly is some sort of rationale behind them?

(4) Part of the goal of the paper seems to be to estimate the 'equilibrium' imbalance for abrupt4xCO2 experiments. Why would we want to use equations (1a) and (1b) for that? If one is only interested in that long-term imbalance, why would you not fit a decaying exponential to the last part of the transient of the imbalance instead? In any way, such kinds of choices should be addressed more explicitly in the text, including the rationale of

making these choices.

(5) Figures and captions are not in line with each other. For instance, in Figure 1, the caption talks about a yellow horizontal line but in the figures it seems to be a green horizontal line, regression lines are said to be solid lines but they appear to be dotted lines and vertical lines are said to be dotted but they appear to be solid. There are also blue lines, not all of which seem to be explained in the caption. The authors should verify that the captions match with the figures and explain all lines.

(6) For me, one of the questions remaining after having read the text is what we should consider an equilibrium of the climate system. Would that just be the long-term response of the system, or do we actually want the system to have achieved radiative balance in some way? Most of the equilibrium climate sensitivity methods, including EffCS in the text, are basing their estimation technique on the requirement that there is radiative balance in equilibrium. However, the equations (1a)-(1b) explicitly do not require this. So for instance, the text on page 6, lines 43-44 stating that "if we do not know what the radiative imbalance will be when temperatures stabilise in an ABRUPT4X simulation, we in turn cannot predict the climate sensitivity with precision", hinges on what we interpret as equilibrium; in fact, you could argue that the method used in this paper is an example of a climate sensitivity prediction that does not require prior knowledge on the radiative imbalance in equilibrium, making this statement in the discussion incorrect with regard to the rest of the text. But above all, I think all these points relate to what we define as equilibrium: Originally we would say that it refers to a state in which there is radiative balance. Then when we found consistent imbalance even in the control simulation, we redefined equilibrium to mean having an imbalance similar to the control simulation. And now this paper seems to shake up even that definition in some models. In short, I think the paper would benefit from a discussion of the definition of an equilibrium climate state, relating it to the radiative imbalance and incorporating the implications of the current paper.

(7) For me, the discussion is missing some sort of recommendation or directive to follow-up on the found energy balance issue. I know that on page 6, lines 45-51 there is some text on this, but I feel like it could be a bit more concrete. Should we conclude from this paper that estimates based on inferred equilibrium radiative (im)balance are inapt for some (or all) global climate models? And should we therefore not use such estimation methods anymore? Should we conclude from the paper that global climate models have energy leaks? And should we therefore not trust these on long time scales? Should we make sure that our global climate models have no energy leaks, and e.g. move towards climate models that are discretized in a way that prevents energy leaks or prevent energy leaks by going to finer resolutions? I don't expect the authors to answer all these questions, but at least posing some of these might give the paper some more direction and might make the implications of their findings more clear.

Technical Corrections:

(1) The top-of-atmosphere radiative balance in Figure 1 and Figure 3 have different signs.

That should be made consistent.

(2) In section 2.1, it is said that Table 1 contains parameter ranges and constraints. The caption does, however, say these are the prior ranges. Could the authors confirm that the values in Table 1 indeed correspond to the prior values and that the parameter ranges are not constrained in the fitting procedure? If so, also the text in section 2.1 should be changed to reflect that.

(3) Table 1 misses the parameters R_n.

(4) Figure 4: the whiskers in the sensitivity and zeta plots are missing.

(5) In the tables at the end of the paper, I was not able to find the fitted values for Sn, Rn and especially tau_n.

(6) The tables at the end of the paper, the 95% values seem to just state the 5% values again for some of the parameters.

Literature

Bastiaansen, R., Dijkstra, H. A., & Heydt, A. S. V. D. (2021). Projections of the Transient State-Dependency of Climate Feedbacks. Geophysical Research Letters, 48(20), e2021GL094670.
Proistosescu, C., & Huybers, P. J. (2017). Slow climate mode reconciles historical and model-based estimates of climate sensitivity. Science advances, 3(7), e1602821.