Response to Reviewer 2:

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.

Many thanks for the review. Issues with the figures and captions have been revised following comments from the reviewers, and we have taken efforts to clarify both the text and equations.

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.

Thanks for this point. We have made efforts in revision to make the structural and parametric choices in this model more clear. We have also reformulated the presentation of 1b to be more intuitive to the reader. We have also revised the treatment of non-constant forcings following the reviewer's suggestion, and assessed the sensitivity of results to subjective timescale prior assumptions.

(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.

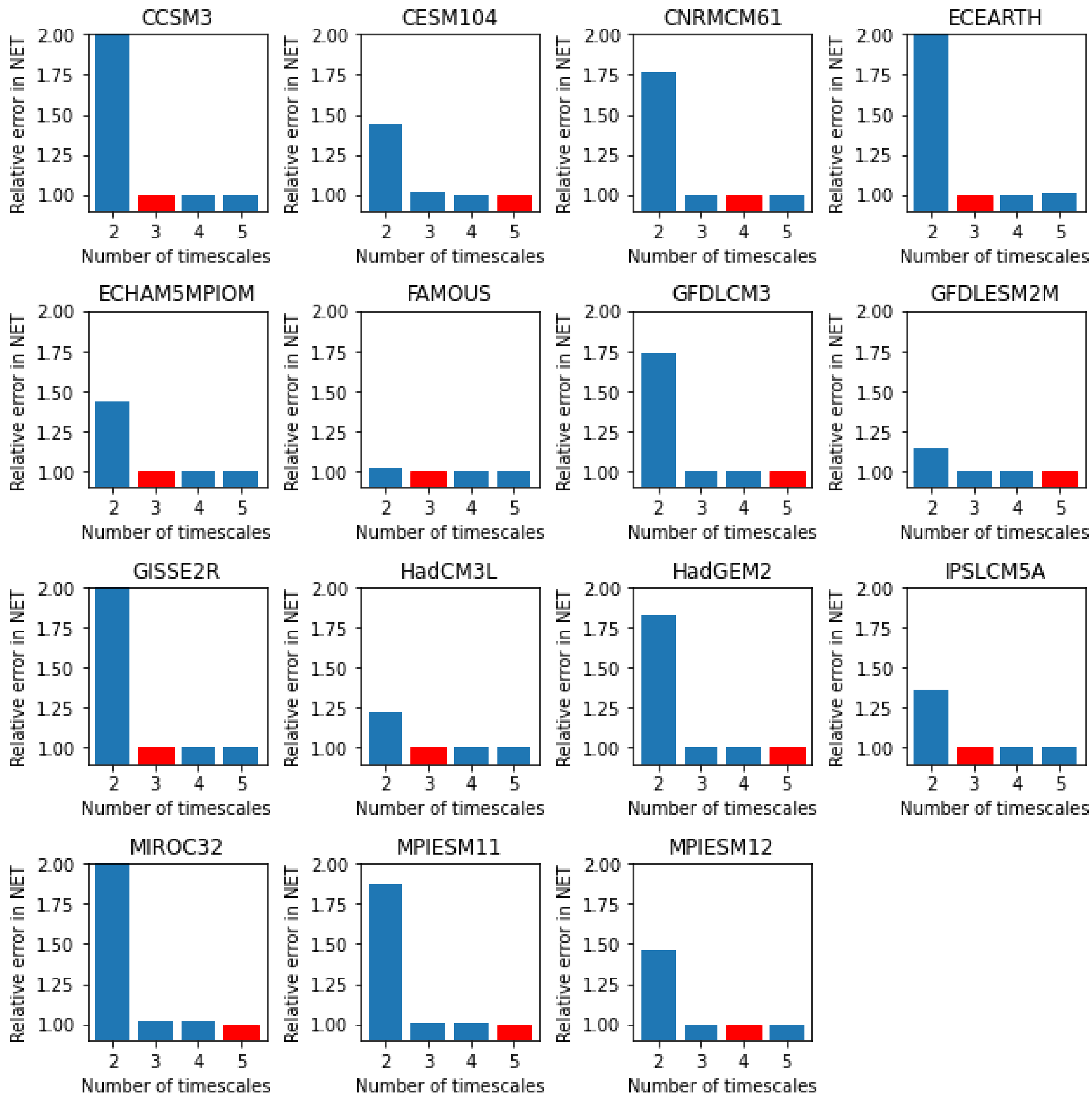
These are very good points - thank you. We make them clear in revision.

(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?

Thanks for this point. In revision, we have tested the model for a range of allowable timescales from 2 to 5 modes, assessing the residual fits as a function of timescales.

In the new scheme, we allow for timescales corresponding to subdecadal (1-10 years), decadal (10-100 years), centennial (100-1000 years), (1000-5000 years) and >5000 years. Incremental solutions allow increasingly long timescales to be considered in the fit.

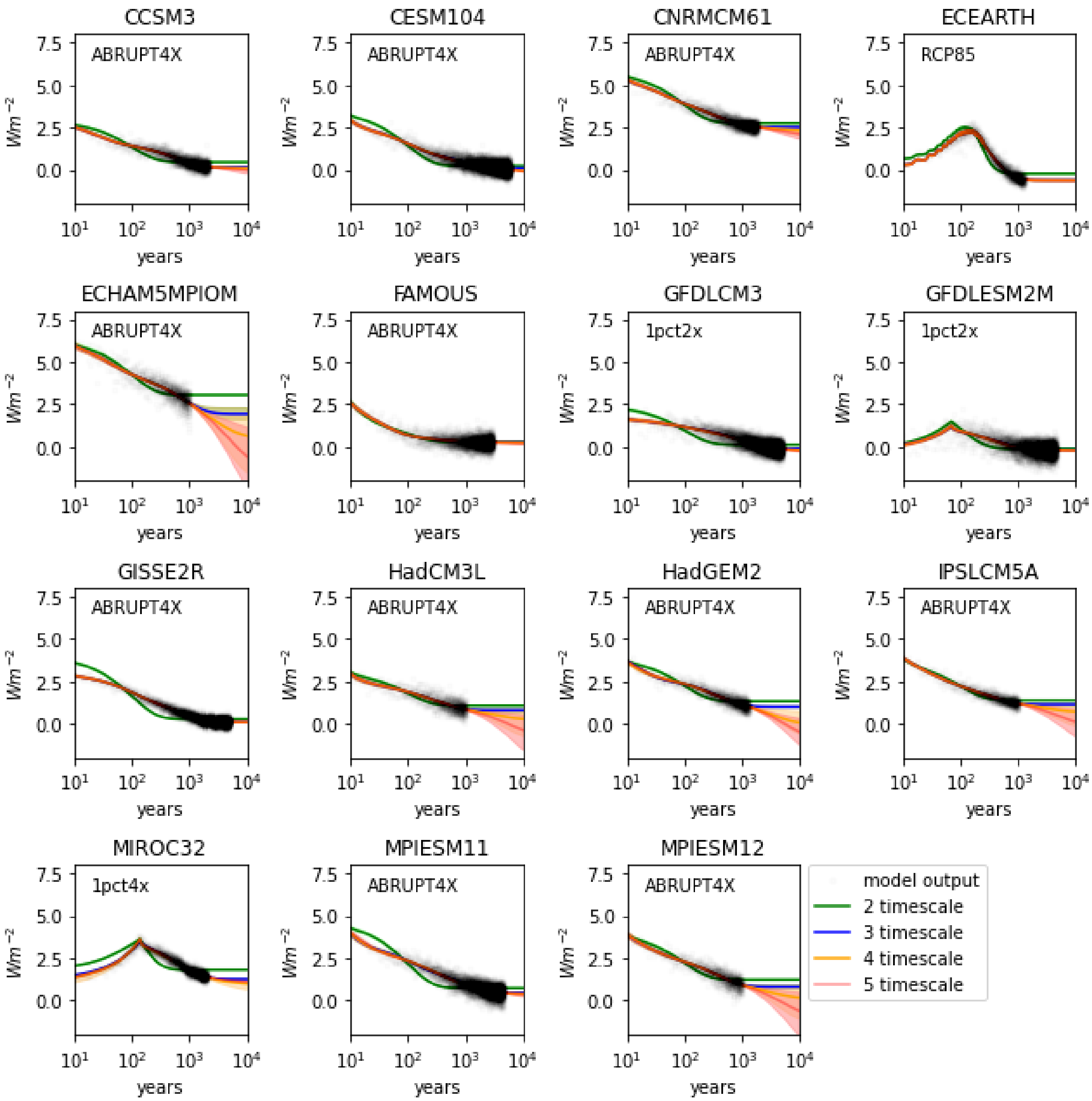
This extension has added some nuances to the conclusions of our original submission. The Figure shown here illustrates in red the number of timescales which results in the lowest overall error in fit of global mean NET TOA radiation. It is evident that some models have improved fits when the longer timescales are allowed (eg GFDLCM3). Models describable with 3 modes (such as CCSM3) show near identical results with 3-5 timescales. Given this, we allow 5 timescales for all models for the rest of the study (which is more time consuming for the MCMC fit).



However, the longrunmip simulations are insufficiently long (~5000 years) to constrain the exponential decay with those models which exhibit decays on a timescale of >5000 years. Thus, for these models, we now have larger uncertainty bars in our estimates of equilibrium response for some models (e.g. echam4mpiom).

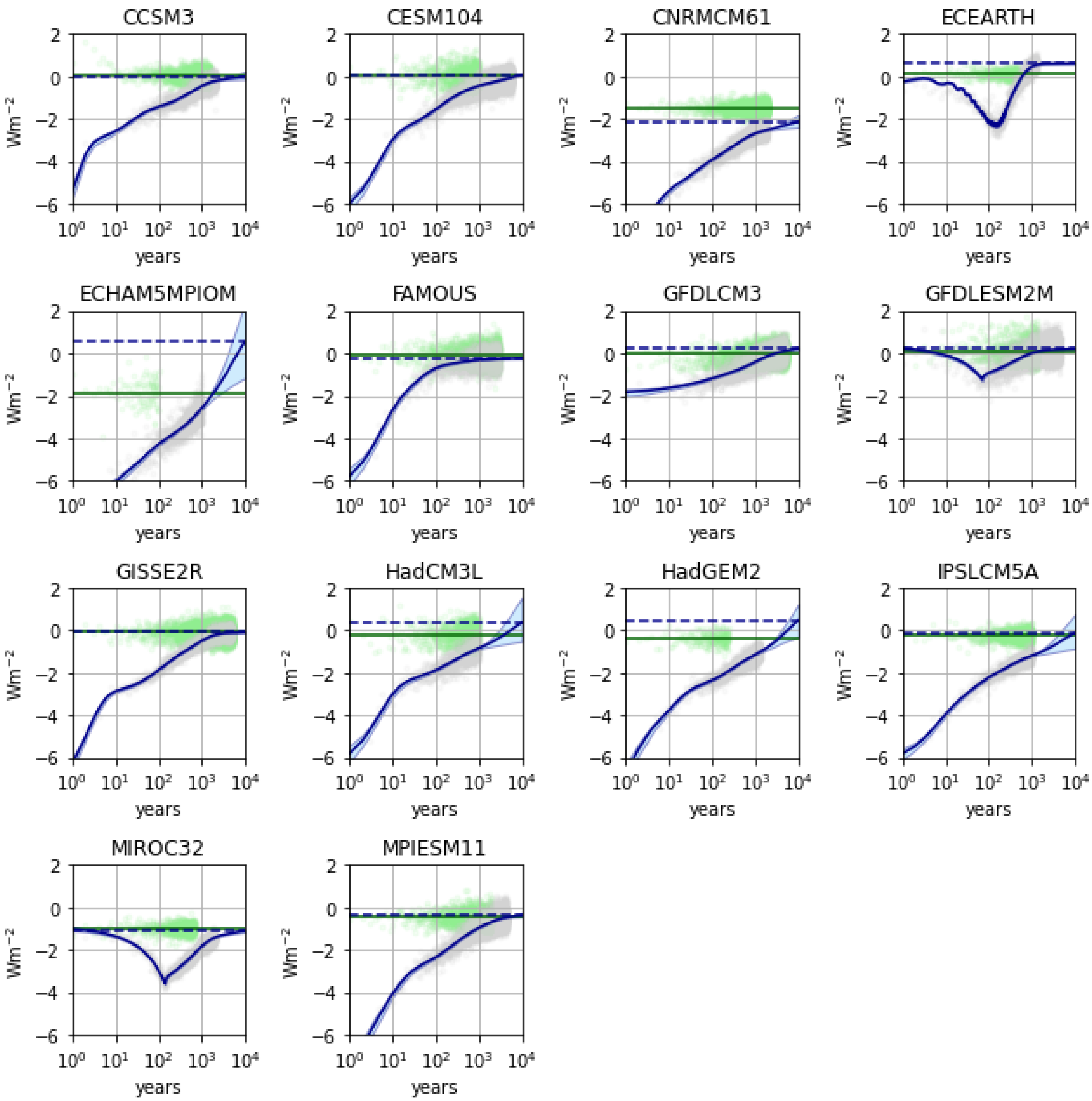
After careful assessment, we think that this uncertainty is a real reflection of the family of plausible evolution on a multi-millennial timescale which is unconstrained by the Longrunmip simulations as they stand. Notably - in the plot below, those models where the >5000yr timescale is well constrained (e.g. GISSE2R), there is a visible tendency towards a stable TOA balance in the LongrunMIP simulation which helps to rule out responses on this longer timescale. For a model where the long timescale is not well constrained (e.g. ECHAM5MPIOM), there is a residual near-linear trend in TOA fluxes over the 5000 year simulation which would be consistent with a wide range of possible exponential decay fits.

We must also consider the possibility for these models that there is no stable state. If energy leaks are a function of the climate state, and the system is not tending towards a state of radiative equilibrium, our evidence that models are converging to a stable temperature is empirical - and longer simulations will be required to investigate these multi-millennial dynamics. Unfortunately, for these models - it means we can say less about their long term trajectory and equilibrium state following the 4xCO2 perturbation, which will now be one of the conclusions of our study.



(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).

Thanks for this. We agree with the reviewer and have now re-done the analysis as suggested. 1pct2x and 1pct4x are fitted using standard impulse response model assumptions with a linearly increasing forcing. For RCP8.5, we use the published forcing time series from Meinshausen 2011 (noting that this is an approximation, but the trade-offs between different forcer uncertainties are not the focus here). Performance in producing the transient evolution of the LRMIP simulations is generally good (see attached plot). The fitted impulse response model distributions are then used to simulate ABRUPT4X responses for the Gregory plots.



(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.

We have improved the explanation of this derivation, and simplified the equations for the paper . T0 is simply the best estimate of the pre-industrial temperature. We now have R0 as the pre-industrial NET TOA balance, and $R4x_{extrap}$ as the equilibrium imbalance in the 4xCO2 experiment (following the reviewer's suggestion in point 4).

(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?

Some level of subjectivity is unavoidable in MCMC fitting, but our choices are informed by similar studies (e.g. Proistosescu, C., & Huybers, P. J. (2017), Rugenstein and Armour 2021, Caldeira and Myhrvold (2013),Smith 2018) - with a preference towards broad priors. We bin the allowable response timescales into regimes (subdecadal, decadal etc.) to allow for characterization of the response timescales of the different models. We've now explained this in the text

(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.

We have now formulated $R4x_{extrap}$ (the equilibrium TOA balance state following the abrupt4x perturbation) to be a parameter in the fitted equation as suggested. We agree that this is a more accurate way of presenting our approach (it makes no difference to the actual derived fits, which were effectively always derived using $R4x_{extrap}$).

45 $$T(t) = \sum_{n=1}^{3} S_n(1 - e^{-(t/\tau_n)}) + T_0$$

$$R(t) = \sum_{n=1}^{3} R_n(-e^{-(t/\tau_n)}) + R^{4x}_{extrap}$$

(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.

Thanks, we have extensively revised the figures in preparing this response. We will endeavor to avoid labeling errors in the revision.

(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.

This is a good point. Nomenclature has been an issue since the outset of this study. It is an unavoidable conclusion of the study that some models do not conserve energy, and that those energy leaks have the potential to be functions of climate forcing. As such, models with such leaks will not reach true energetic equilibrium as defined by $dN/dt=0$. This still allows for the model to reach an asymptotic stable state, which includes the energy leak - but it does not allow for the derivation of effective climate sensitivity which requires prior knowledge of the asymptotic equilibrium TOA balance. The method suggested here presents an alternative approach for deriving climate sensitivity, but it is clearly less than ideal - requiring simulations of 5000 years of simulation (or greater, as we learn in revision) to produce a robust estimate of the equilibrium state.

(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.

Thanks for this point. We copy below our expanded discussion on future recommendations for the community following this study:

This directly impacts our ability to accurately measure $EffCS$ from short simulations, and draws into question whether $EffCS$ should be used as a factor at all in assessing the fidelity of climate models (Hausfather, 2020). Effective climate sensitivity has known limitations that it describes effective feedbacks at a certain representative timescale following a change in forcing (Rugenstein 2021), but our results here highlight another issue that EffCS can only be used if we can be confident in the asymptotic energetic balance of the model. Such confidence can arise either from a ground-up demonstration of structural energy conservation in the model (Hobbs 2016), or by running sufficiently long simulations to be empirically confident both in the pre-industrial energetic balance and in the asymptotic multi-millennial tendencies of the model following a change in climate forcing. However, such experiments are currently difficult to achieve for CMIP class models, the 5000 year simulations conducted in Rugenstein (2020) were significantly longer than any experiments conducted previously - and we find in the present study that they remain too short to have confidence in the asymptotic state.

Given this, this study has multiple recommendations. Firstly, a greater emphasis in climate model design and quality checking needs to be placed on structural closure of the energy budget in the climate system. Models which can demonstrate that energy is conserved in the model equations can allow confidence that the system as a whole will converge to a state of true radiative equilibrium following a perturbation, which would allow a robust calculation of $EffCS$. For models which cannot demonstrate this, longer simulations are required to be confident in the asymptotic state - but these simulations may be prohibitively time and resource consuming. Such limits could potentially be alleviated through the use of lower resolution configurations (Kuhlbrodt 2018, Shields 2012) (with the risk that such models will exhibit different feedbacks from their high resolution counterparts) or by considering analytical approaches to accelerate convergence of complex systems (Xia 2012) . However, in the short term a more practical approach may be to consider alternative climate metrics which do not require assumptions about the equilibrium state of the system. Transient Climate Response does not require assumptions about radiative flux, but it does not provide direct information on the warming expected under stabilising forcing. A possible alternative is A140 (the warming observed 140 years after a step quadrupling in CO2 concentrations (Sanderson 2020), which requires no assumption on equilibrated state - and is more informative on the warming expected under high mitigation scenarios than $EffCS$ itself (even if it is known without bias due to energetic leaks). In conclusion, the role of Effective Climate Sensitivity as a metric in assessing the response of the climate system should be reconsidered, both due to its lack of relevance to projected warming under mitigation scenarios (Knutti 2017, Frame 2006, Sanderson 2020) but *also due to the fact that its derivation requires assumptions about the asymptotic state of the climate system which cannot be demonstrated in a number of Earth System Models.*

Technical Corrections:

(1) The top-of-atmosphere radiative balance in Figure 1 and Figure 3 have different signs. That should be made consistent.

Thanks, fixed following the standard sign convention used in Gregory plots.

(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.

Thanks, text updated to confirm that these are the prior ranges.

(3) Table 1 misses the parameters R_n.

Not used now (see response to main point 2)

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

This was a plot resolution issue, fixed now.

(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.

Thanks - this was not in a table. We include a new table with mean MCMC parameters in revision:

Model	S_1	S_2	S_3	S_4	S_5	R_1	R_2	R_3	R_4	R_5	$R\tau_1$	τ_2	τ_3	τ_4	τ_5	R^{4x}_{extrap}
CCSM3	2.48	1.11	1.49	0.03	0.04	8.49	1.50	1.32	0.16	0.26	0.96	22.99	514.18	2980.28	7585.31	-0.10
CESM104	3.18	1.60	1.16	0.35	0.88	4.16	1.23	0.93	0.29	0.45	4.76	80.11	350.89	3322.39	7609.84	-0.22
CNRMCM61	2.65	2.74	3.36	0.26	0.41	3.43	1.38	1.49	0.30	0.50	3.23	36.04	318.87	3118.36	7608.10	1.96
ECEARTH	0.85	4.04	2.35	0.36	0.70	3.22	1.46	2.32	0.01	0.01	7.58	43.09	269.63	2612.42	7503.20	-0.63
ECHAM5MPIOM	1.66	4.06	4.48	0.94	1.63	3.41	1.92	1.18	1.82	3.02	3.01	38.34	430.45	3180.65	7611.00	-1.54
FAMOUS	3.02	5.58	3.52	1.43	0.93	3.91	1.96	0.38	0.05	0.08	5.18	35.91	323.58	3121.48	7732.14	0.18
GFDLCM3	0.57	2.33	5.68	1.31	0.69	5.05	1.60	1.71	0.90	0.57	5.32	69.16	721.86	2498.57	7620.66	-0.32
GFDLESM2M	2.12	0.05	3.95	0.62	0.11	5.07	0.77	2.24	0.19	0.12	4.28	42.25	406.82	2601.13	7417.61	-0.24
GISSE2R	2.44	0.94	1.40	0.02	0.01	5.37	1.36	1.44	0.07	0.05	2.12	88.29	680.85	2704.30	7642.74	0.07
HadCM3L	2.58	1.59	1.60	0.45	0.91	3.79	0.55	1.08	0.63	1.37	4.91	45.96	245.51	3201.77	7731.69	-0.85
HadGEM2	4.11	1.29	1.83	2.00	3.86	4.54	2.28	1.28	0.79	1.76	1.53	14.19	247.97	3372.98	7536.63	-1.01
IPSLCM5A	2.37	2.59	2.22	0.37	0.75	2.19	1.51	1.10	0.62	1.11	5.39	33.86	238.98	3230.48	7566.52	-0.25
MIROC32	4.15	1.32	3.03	0.07	0.10	4.28	1.74	2.01	0.22	0.28	4.46	17.80	585.56	3117.25	7697.05	1.02
MPIESM11	2.43	1.51	1.68	1.30	0.06	4.14	2.05	1.38	0.95	0.20	3.42	28.18	254.16	1490.62	7421.48	0.31
MPIESM12	2.56	1.56	1.86	0.71	1.52	4.42	1.96	1.50	0.84	1.68	2.85	20.90	272.32	3211.09	7750.07	-1.21

(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.

Thanks, fixed.

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