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Interactive comment on "Relating Climate Sensitivity Indices to projection uncertainty" *by* Benjamin Sanderson

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Overall, I think that the manuscript is well written, the issue has a great scientific relevance, and the arguments here shown provide significant advancement to the discussion on the topic. Thus, I appreciate that the author addresses them critically, emphasizing that their adoption is conditioned to the problem that one needs to focus on. This is in line with previous works having evidenced the limitations of these metrics for the study of the climate response, especially from a modelling perspective.

Many thanks for the positive evaluation and careful reading.

I am a bit skeptical about the effectiveness of the impulse-response model, given that *it is a purely linear context. The addition of the noise+drift, though, is convincing in explaining part of the discrepancy between the simple model and CMIP5 outputs. The arguments about the applicability of the metrics are thus promising also in a "real-world" context (using the notation adopted by the author), although with some caveats. For this reason, I think it is important that the author puts more emphasis on the nature of the impulse-response model, in the framework of linear response theory (LRT) and Hasselmann-type response (see my specific comments), and evidences its limits.*

These points are well taken - and thanks to the reviewer for the additional literary context, which I've endeavoured to include. I've tried to put the two timescale model in appropriate context - the primary defense for this application being that it is already sufficiently complex to show that TCR and ECS do not constrain future warming under strong mitigation, and that non-equilibration is a potential issue for TCR estimation. I believe that these points, which are statements of lack of confidence, are robust to the consideration of a wider set of models with additional response timescales.

I do however agree that the 2-timescale structural assumption is strong - and any constrained distribution (of future warming, EffCS or TCR) need to be considered in the context of these caveats. For this reason, I do not highlight the actual constrained ranges here - and I have added an additional paragraph to the conclusions to explain this.

I think some improvements can be made in terms of how the methodology and results are described. It would be useful to have the "Methods" section in the main part of the manuscript, instead of as an appendix.

I have restructured the document to have the methods in line.

The notation is not always consistent across the methods.

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I’ve worked to reformat the methods extensively following the comments by both reviewers

The MCMC procedure should be explicitly described, not only by mentioning the original reference.

I’ve included an extended description of the algorithm and the reasons for using it.

Specific comments:

- ll. 90-92: I do not have clear how the normalized regression coefficients shown in Figure 1f support this argument. Can the author better clarify it?

I’ve deleted this paragraph - as I think the point is overly subtle.

- ll. 94-96: I do not understand this sentence for a few reasons. Firstly, is the author referring to any specific forcing, when he says that the rate of change in the forcing is approximately constant? In the case of the mitigation scenario (RCP2.6), this is obviously not the case.

Apologies - this paragraph was talking explicitly about RCP8.5, in which total radiative forcing increases broadly linearly throughout the 21st century. I’ve rewritten this section.

Secondly, I do not have clear in mind what the author means by “saturation” of the fast feedback response, and if this refers to the whole period 2000 to 2100 or to the end of the period.

Section now deleted.

- l. 132: the author suggests that the contributions of the two factors are separately addressed in the following, but, in the end, only the overall effect of the bias is taken into account in the following. -

Thanks - corrected. I now come back to the unknown baseline factor in the CMIP detrending exercise at the end of the results section.

ll. 161-162: the author seems to imply that "real-world applications" are prone to the existence of drifts. But this is rather a model issue, as the unforced "real-world" climate system should not have any drift.

Corrected.

- l. 166: the author did not specify anywhere else in the text what is the length of the abrupt 4xCO2 simulation. As a consequence, "end" of the simulation does not seem to have a specific meaning.

Replaced by "years 121-140"

- Appendix B: according to ESD standards, I think that it would be more appropriate if the Methods section are moved in the main text after the Introduction.

Done - methods are now inline in the text

Moreover, a description of the data that have been used is lacking, especially for what concerns the observational-based datasets used for model optimization.

All relevant citations are now included.

- Eqs. B1-B2: the impulse response model here adopted requires using only two timescales. Is it sufficient to describe the response? The FAIR impulse-response

model here mentioned includes a set of four simple feedback equations (cfr. Hasselmann et al. 1993) differing on the magnitude of the feedback parameter (i.e. on the timescale of the response). What happens if one includes more than the two timescales considered in this analysis, given that similar strategies applied to geoengineering scenarios have used, for instance, three exponentials (cfr. Aengenheyster et al. 2018)?

I fully agree that 2 timescales is a structural assumption, and that additional timescales of response would be likely required for longer periods of response. During development, I experimented with different timescales dimensions - 1 timescale can be trivially dismissed as unable to represent the temporal evolution of the models in response to 4xCO2 forcing. Beyond two timescales, only slight improvement is seen in the fitting error - so two timescales was chosen for this study to be (a) consistent with existing literature (i.e. within the framework of FAIR, which is in common usage), (b) lower dimensional so easier to interpret in terms of slow/deep ocean and fast/shallow ocean response and (c) sufficient for demonstrating the main point that drift and noise impact TCR more than ECS.

For 140 abrupt-4xCO2 response, only some models show an improved fit with an extra allowed timescale (see GISS-H, for example on the below plot), and even then it's a slight improvement. Most models are adequately described with 2, and adding a 3rd results in a degenerate fit.

Other studies have arrived at the same conclusion for summarizing responses on the century timescale (see Proistosescu and Huybers http://doi.org/10.1126/sciadv.1602821, Smith 2018 http://dx.doi.org/10.5194/gmd-11-2273-2018 , Geoffroy 2012 http://doi.org/10.1175/JCLI-D-12-00196.1).

Ultimately, for this study, the aim is to reproduce the basic features of CMIP ensemble diversity in response to different types of forcing with the minimum possible complexity of model - and I felt that this was both possible and easier to explain with the two timescale model. Clearly, the real world could have the capacity to respond to forcing on a range of timescales, but two timescales adequately describe the response to forcing on the century timescale in the CMIP ensemble.

This is particularly relevant, as the impulse-response model can always be expressed as an infinite sum of exponential behaviors, differing in their timescale, but the response of the real system rarely has the shape of a discrete number of exponential behaviors combined with each other (e.g. Ragone et al. 2016: Lembo et al. 2019). Also, the adoption of the fast-slow scale implies a separation of scale, that is here inferred "a posterior" through heuristic arguments. Nevertheless, there is no reason, in principle, to assume that a scale separation exists, and this problem traces back to the very foundations of the theory about climate response and forced-free fluctuations dichotomy (Lorenz 1979). One way to deal with that would be to evaluate the memory term (cfr. Ghil and Lucarini 2019). I understand that this might go beyond the scope of this work, but I wonder if the author might comment on that in the manuscript.

This point is well taken - though to redesign the model as an infinite sum would create a challenge in terms of a low-dimensional parametric definition which could be used in MCMC. However - I recognise that the discrete response assumption is a strong one, and I've added a paragraph in the discussion to outline this caveat in the interpretation of the results.

0.69in0.81in " *These conclusions are derived from the consideration of a relatively simple two-timescale pulse response model which is sufficient to show that constraining certain types of sensitivity metric is insufficient to constrain future projections, and that non-equilibration may confound measurement, however, the constrained distributions for the metrics are subject to the structural assumptions of the model used. The real world may have more than two response timescales \ cite{*

aengenheyster2018point} , *or may be better described as a continuous sum \ cite{ ragone2016new,lembo2019thediato}* . *Further work should identify how such complexity impacts uncertainty in relevant climate metrics.*"

- Eq. B3: according to the convolution properties, this operation is by all means equivalent to the application of the Ruelle Response Theory (RRT) (Ruelle 1998a; Ruelle 1998b) when a hypothetical impulse perturbation is applied, allowing for a particularly simple derivation of the linear Green function (cfr. Hasselmann et al. 1993). This has found several applications in the context of climate prediction (cfr. Ragone et al. 2016; Lucarini et al. 2017; Ghil and Lucarini, 2019 for a review), not only constraining to the temperature response, but also to a wide range of climatic variables (e.g. Helwegen et al. 2019; Lembo et al. 2019). These arguments provide a rigorous mathematical framework to the experimental protocol here described.

Thanks for these. I've included the references when introducing the model.

- Sect. B1.1: I believe that a complete description of the model is here lacking and should be included. Referring to the model settings, in particular, it is not clear to me how the ensemble is generated and how many members are taken into account.

This section has been significantly expanded, and now includes a perfect model demonstration fitting the model to CMIP members.

- Table B1: is it possible to have a range for rn as well? Also, where does the fr parameter enter the mode? This goes back to my minor comment about consistent notation.

This is now clarified in the text. r_1 is varied (r_2 is (1-r_1) due to the initial boundary condition). F_r is now explicitly detailed in Eqn. 5.

- l. 226: I think that it is important to notice here that in the forcing scenario 1pctCO2 the CO2 concentration reaches doubling after 70 years, as I presume that this motivates the choice of the 61-80 and 131-150 20-years averages.

Now noted explicitly, thanks.

- Figure A1: the caption does not contain an explanation of the panel b content. Particularly, the author might want to explain the meaning of the red shading, and the range encompassed by the dotted lines.

Expanded.

- Figure A2: the author does not explain why the choice of a single member from each CMIP model ensemble is reasonable in this context.

I've now noted that the plot is subject to internal variability, but this is a central point which is being made. I am not trying to assess what is the most robust sensitivity metric given a situation where there is noise and potentially drift in the simulations. To have a subset of models with large ensemble averages (and others without) would confuse that assessment.

- Figure A3: it appears that the distributions of fast-scale parameters are much more similar to a Gaussian distribution, compared to the slow-scale parameters. I am surprised that the author does not refer to that explicitly and comments on it. Could it be an evidence that the scale separation that is a priori assumed for parameter model optimization is such that the fast-scale system approaches a stochastic process, in the context of the response of the system to the impulse forcing? This would be certainly reasonable, in an "Hawkins and Sutton, 2009 context" (signal-to-noise ratio approach), but the author might want to justify it in a more rigorous way.

I've expanded this discussion a little - though I'm not sure that we can infer any dy-

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namical separation of timescales from the differences in distribution. My interpretation is that the fast timescales are simply more strongly constrained by the observations, whereas there are solutions with a wide range of slow timescale responses.

Minor comments:

- l. 19: in this sentence there is a repetition ("range" and "ranging"). Consider rearrang- ing the sentence;

Thanks, corrected.

- l. 31-32; I found this part of the sentence a bit difficult to read. A suggestion might be to replace it with "a complication has arisen due to the fact that EffCS seems to be better correlated than TCR with 21st Century warming from present day levels under a business-as-usual scenario."

Thanks - corrected as suggested.

- l. 37: replace "have" with "of" .

Thanks, corrected.

- l. 60: remove "to"

Sentence removed

- l. 66: it is not clear whether the author refers here to the Appendix A, Appendix B or both.

Methods are now inline with the paper.

- l. 69: replace "and" with "to" .

Thanks, done

- l. 91: either a sentence breaking is needed here (after the brackets), or "suggest" has to be replaced by "suggesting" .

Sentence removed.

- l. 125: Replace "of CMIP5" with "for CMIP5" .

done

- l. 138: if the "Methods" section is in the appendix, they have to be referred to more appropriately as "Appendix (B)" .

Methods now inline.

- l. 147: replace "the both" with "both" .

Thanks, corrected

- l. 151: replace "Supplemental" with "Supplementary" .

done

- l. 165: replace "an" with "that a" .

done

-Eq. B3: I noticed a potential mismatch in the notation, compared to eq. B1. The author may consider adopting the same notation for the temperature evolution in both equations.

This is now consistent throughout.

- Figure A1: replace "senstivity" with "sensitivity" in the caption.

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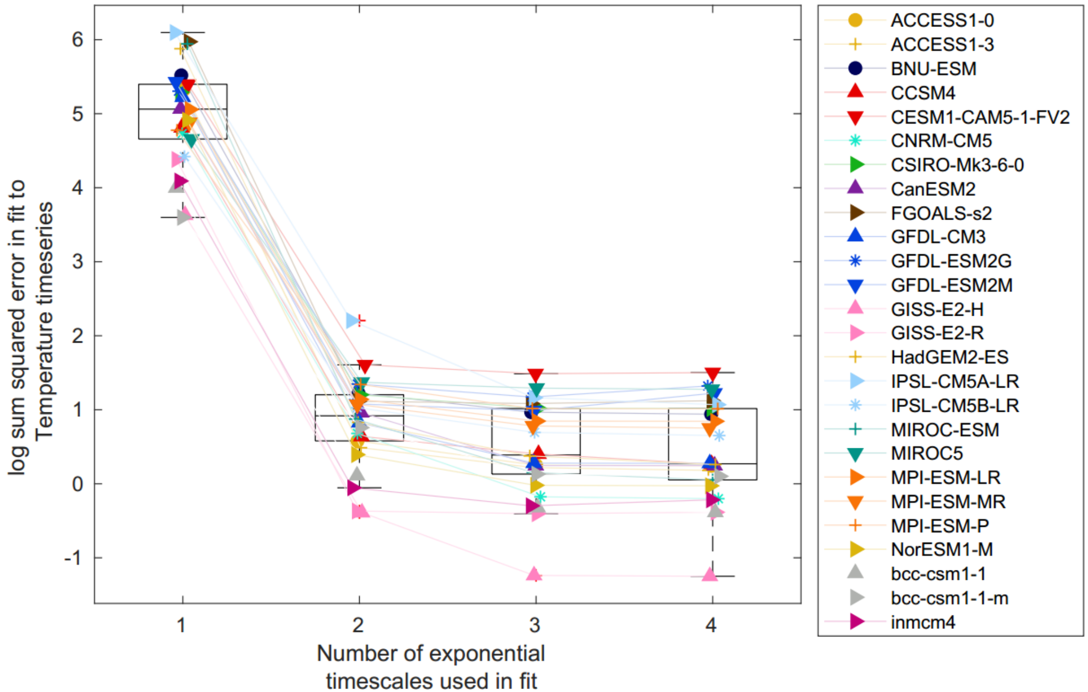

Fig. 1.

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