Author response to second round of referee comments and changes to ‘Testing the assumptions...’

April 2024

Dear Editor,

We would again like to thank all three reviewers for giving up their time to read our manuscript and for the positive evaluation. All three now recommend the manuscript for publication.

We would also like to thank them for their suggestions and comments. We have addressed the remaining referee comments (RC3) point-by-point below in bold face print. We have also revised the manuscript based on those suggestions.

Regarding RC2’s question on the different role of the internal variability parameter between CMIP5 and CMIP6 we have added to the discussion in the revised manuscript.

On behalf of all coauthors,

Mark Williamson

1 RC2: Anonymous

I acknowledge that the manuscript

"Testing the assumptions in emergent constraints; Why does the 'Emergent constraint on equilibrium climate sensitivity from global temperature variability' work for CMIP5 and not CMIP6? by Mark S. Williamson, Peter M. Cox, Chris Huntingford and Femke J. M. M. Nijsse

has been substantially improved as compared to the previous version, and that most of the points raised in my comments have been addressed.

I do think, though, that the reason for such a different role of the internal variability parameter in CMIP5 and CMIP6 landscapes is somehow left untackled, and understand that the authors prefer not to speculate about that and commit to investigate into it in a future work.

I do think that this lack of interpretation hinders the relevance of the work, making it a bit dependent on the assumptions made and constraining the implications of the findings for future work, but I do acknowledge that this is not a justification for preventing the manuscript for being published.

Therefore, I leave this as a comment and urge the authors to provide an explanation for what has been found based on further evidences to be included in a future work, and recommend the manuscript to be accepted as is.

We have added to the discussion the following paragraph:

'The question of why σ_Q is correlated to ECS in CMIP6 and not CMIP5 is also left unanswered. However, one can speculate why this may be the case: As previously mentioned σ_Q is a fitting parameter that is designed to capture the effect of chaotic internal variability as well as sub-annual (fast) feedbacks on global mean temperature variability. Zelinka et al. (2020) showed that the increased range of ECS in the CMIP6 models could be explained by the increased range in cloud feedbacks (see also Bock and Lauer (2024)). As σ_Q is fitted to annual temperature timeseries, some of this fast (sub-annual) cloud feedback effect could be included in σ_Q correlating it to ECS. We leave concrete answers to a future study.'

2 RC3: Anonymous

This manuscript is well-motivated, well-written, and interesting. I appreciate the authors effort in investigating this curious issue and the thoroughness of the analysis. My comments are generally minor suggestions that are meant to clarify or improve the presentation to the reader. I recommend the manuscript for publication.

Thank you for the positive review and the recommendation of publication.

3.1 Lines 7 / 52: Schlund et al find an r^2 value of 0.01 in CMIP6. The language here suggests that there was still a (weaker) constraint, but it seems like the constraint (as used in Cox et al) had disappeared / was non-existent in CMIP6.

To quote section 3.2 of Schlund et al (2020) 'a likely ECS range of 3.03 K ± 0.71 K for CMIP5 ($r^2 = 0.31$) and 3.44 K ± 1.15 K for CMIP6 ($r^2 = 0.08$)' for the CHW18 constraint. These correspond to $r = 0.56$ and $r = 0.28$ respectively comparable to the $r = 0.66$ and $r = 0.31$ found in this manuscript. Further on on section 3.2 they say the constraint is 'highly significant for the CMIP5 ensemble ($p = 0.0010$), but only almost significant for the CMIP6 ensemble ($p = 0.0545$)' also comparable to the $p = 0.007$ (highly significant) and $p = 0.079$ (almost significant) found in this manuscript. So I think the language is fine unless the referee is reporting later findings that we are unaware of?

3.2 Line 17: I think the goal is not just to make climate models look increasingly realistic but to make climate models that are actually more realistic representations of Earths climate

We have replaced 'look' with 'are'.

3.3 Lines 24 - 25: Isnt this the likely range?

Good spot. We have replaced 'high confidence' with 'likely'.

3.4 Line 54: Note that you havent actually explained the physical underpinnings of the emergent constraint yet, so you are asserting that it has reasonable physical principles here (or relying on the reader to be familiar with Cox et al 2018).

This is true. We do justify this assertion later on in the text and in previous manuscripts.

3.5 Line 61: Is it true that temperature observations are relatively un-autocorrelated? I assume you mean in time? This strikes me as untrue. Could this be qualified to make it true? Or, alternatively, you could point to evidence that the observations are sufficiently reliable.

Yes, as the constraint is based on global mean temperature variability, we meant in time. Autocorrelation functions have e-folding times of a few years in the models and observations. While still not particularly precise we have changed to '...relatively un-autocorrelated in time (a few years) giving good estimators of the true values.' as the sentence is supposed to be introductory and adding too many details here makes it overly cumbersome.

3.6 Line 89: Should this be designed to model the *impact of* random, fast internally

Similar, but thank you. Good spot. We have changed this to 'fast (sub-annual), chaotic weather forcing on the slower Earth system components.'

3.7 Line 95: The CMIP5 historical experiment ended in 2005. Were these extended with another experiment?

Yes. This is explained in the following paragraph.

3.8 Lines 96 - 97: Im not sure where it would make sense to add this, but some studies indicated that the correlation coefficient was lower when more CMIP5 models were added to the original Cox et al. (2018) analysis (Po-Chedley et al. comment showed a correlation of r = 0.54 and Schlund et al. showed a correlation of r = 0.56). This slightly complicates the story a little bit (it isnt solely the CMIP6 ensemble that showed a weaker constraint). You might be able to allude to this in the paragraph at lines 113 to 116 (and you could note that σ_Q improves the emergent relationship in both CMIP5 and CMIP6 later on in the manuscript).

We have added the following paragraph 'Although there were more CMIP5 models available than the $n = 16$ used in CHW18, the choice of one model per modelling centre was made to avoid biasing the emergent constraint towards similar models. Where multiple models were available from the same centre, the model with the lowest root mean square error to the observational temperature record was chosen. Po-Chedley et al. (2018) and Schlund et al. (2020) repeated the analysis of CHW18 including these additional models and thus had a larger CMIP5 ensemble (larger n). They found the emergent relationship got slightly weaker, although it was still 'highly significant' in the language of Schlund et al. (2020).'

3.9 Eq. 4: I had thought that you might get σ_Q by analyzing variations in the TOA flux. I now realize that would be problematic because TOA balance includes the climate response (λT) and isnt a pure estimate of weather forcing? Regardless, was this formula in Cox et al (2018)? Or a follow-up Williamson et al. paper? If so, could you cite them? I dont have great physical intuition for this equation, so providing more context (in text or via citations) would be helpful.

We have tried getting a proxy on σ_Q along the lines you have suggested without luck so far. This is discussed in the conclusion. Regarding equation 4, a similar formula is given in Williamson et al (2018) (expressed with slightly different although equivalent parameters). We would imagine it appears in multiple contexts regarding guided random walks, Brownian motion, Ornstein-Uhlenbeck processes etc etc. It's a consequence of solving the one-box model with white noise forcing. We have added the citation to Williamson et al (2018) here.

3.10 Line 191: Is this expected because this was the case in Cox et al? If so, you could signal this by noting: Consistent with Cox et al. (2018),

Changed to 'Consistent with CHW18...'.

3.11 Lines 195 - 196: Im a little unclear about what exactly this sentence is saying. Could you just explicitly write out that $Q_{2\times CO2}/\sigma_Q$ is correlated with ECS in CMIP6 (I assume this is what you mean)?

Corrected.

3.12 Figures 2 - 4: Would it make sense to combine these figures into a single figure (with 6 subplots)? They seem sufficiently related that it would be helpful to put them together. This comment could also apply separately to Figures 5 and 6.

This is a good idea and we've done something similar to your suggestion, combining figs 2 and 3 into one figure as well as figs 5 and 6 into one figure.

3.13 Lines 201 - 203: It would be useful to plot the right hand side of Eq. 2 versus ECS and the 1-to-1 line so that the reader can see how accurately this equation predicts ECS in CMIP models (to be clear, I dont expect it to be perfect, but this would be a natural plot to include somewhere).

A similar plot was in the original submission (fig 6) but was removed on the request of RC2 in the first revised submission. We have looked at these plots again, plotting ECS against $\frac{Q_{2\times CO2}}{\sigma_Q}\Psi$. As this relation should hold for all models running any experiment (provided you can remove the forced signal), we used an ensemble composed of all CMIP5 and CMIP6 models running both piControl and historical experiments (a total of 96 data points) and while the proportionality, k, in $ECS = k\frac{Q_{2\times CO2}}{\sigma_Q}\Psi$, holds with a high correlation value ($r = 0.74$, $p < 0.001$), the constant of proportionality is $k \sim 2\sqrt{2}$ rather than the $\sqrt{2}$ predicted by H76 i.e. the theoretical prediction of ECS from H76 (equation 2) is biased low. Using the two-box values and its theoretical prediction for ECS (equation (23) in Williamson et al. (2018)) brings k closer to the

predicted value $k \sim 1.3\sqrt{2}$ with similar high correlation ($r = 0.76$, $p < 0.001$). The two-box model adds a second, longer timescale, however the box model prediction of ECS is still slightly low. The low constant of proportionality could be due to these simple models not having other timescales that the complex models surely have. We have added this discussion to section 4 of the revised manuscript and in the discussion and conclusion as well as figures.

3.14 Line 353: I suggest being more explicit: namely σ_Q is correlated with ECS.

Good suggestion, done.

References

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