

Reply to RC1 (by Christopher Smith)

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We thank Christopher for this review (and yes we chose the collaboration it was not forced upon us ;-)

In response to minor comments:

6: *“omitted important structural uncertainties which should be included”*. Is there a short clear list (suitable for the abstract) of which uncertainties have been excluded in recent studies that claim a narrowing of ECS uncertainty?

Each study is different. But in general the problem is that some relationship is assumed to hold perfectly when in fact it probably will not, or equivalently, that some uncertain but potentially important influence on the result is omitted from consideration. We cannot provide a list per se, but have added some words to the abstract to clarify what is meant here.

*equation 1: it would be nice to highlight the link to the Earth’s TOA energy imbalance  $N$ . An alternative formulation of estimating from the historical record (line 32) uses observational estimates of  $T$ ,  $N$  and  $F$  (with the strong caveat that observing  $F$  is quite difficult, as the authors point out later on in the article (158) particularly in relation to aerosols). This is useful as the authors discuss TOA energy imbalance several times later in the article (126, 153).*

This is a good suggestion and we’ve changed the equation.

47: *“IPCC” – specifically up to and including AR5?*

Yes, now noted.

60: *warming levels were as much to do with the fact that GCMs in CMIP6 were fairly bad as a group at reproducing historical temperatures (e.g. Flynn & Mauritsen, 2020), and in combination with ECS values that were higher than assessed with various lines of non-model-based evidence (S20, and AR6 WG1 Chapter 7), perceived to be less reliable for future projections.*

We have reworded this to clarify that the key driver was not the change in range per se, but the fact that it and the model range had become significantly different from one another (which was due to the combination of a robust narrower constrained range and the “hot models”). As for the poor historical performance, this was also the case for some models in AR5 which suggests that this was not a decisive factor in AR6.

179: *“For a number of reasons this is not satisfactory” – could you suggest some?*

OK we now mention that models and their developers can share common assumptions that may be incorrect or lead to common biases.

196: *no net anthropogenic \*CO<sub>2</sub>\* emissions.*

OK, added

209: *There's no need to cite these papers which are all mine, I offer the following in the spirit of discussion as I've done a lot of work in this area. In an energy balance (or GCM) context there's a second condition in that the historical modelled temperature also has to be correct within uncertainty, in which case aerosol forcing is a good constraint on ECS/TCR (Smith et al. 2018 fig. 7). And in the case of future warming projections in low emissions scenarios, the aerosol forcing is the strongest determinant of future warming (Smith et al. 2019 fig. 4; Watson-Parris & Smith 2022).*

Thanks for suggesting those references, we now cite Smith et al. 2019 who pointed this out first (albeit buried in the text!).

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Reply to RC2

*I was surprised by the statement on lines 120-122 that further narrowing the uncertainty range will be a slow process. The strength of a framework like S20 is that it provides a fairly straightforward mean to assess the impact of new knowledge/refinements on overall uncertainty. Studies that claim to lead to a reduction in ECS uncertainty should be encouraged to use the S20 framework to actually quantify that reduction. That way we could end up with some kind of "living assessment" that might accelerate the uncertainty reduction process. Unless I underestimate the difficulty of re-assessing in the light of new studies?*

We have reworded this to clarify that what we meant is that new evidence and constraints will only slowly change the pdf (not that the process of updating the pdfs will be slow)

*Section 5, and its summary in the abstract, is said to focus on net-zero scenarios. But it seems like it makes a wider argument that ECS is also not relevant to more doom-like scenarios where the possibility of surprises caused by strong warming has increased (lines 211-219). If that is the case, a better section title and summary might be needed.*

Thanks, we changed the section title and changed the wording a bit to clarify that the section is a bit more general.

*Line 31: For the sake of pedagogy, it would be useful to relate ECS to the variables in Eq 1.*

Good point, we have done this.

*Line 43: "were not able to do new research" – to clarify, the IPCC must not do new research.*

Reworded

*Line 54: "combined weight of all evidence" Could point to Figure TS.16b of the IPCC AR6 WGI Technical Summary for a graphical representation of the different contributions. In a way, that suggests that some lines of evidence are redundant, but I suppose that in a Bayesian framework, it is indeed the combined weight that matters?*

No lines are redundant as the reviewer points out, and we have left this as is.

*Lines 78: “they moreover had little effect on the outcome” – that statement needs some reference. Is that published, or documented somewhere in the grey literature?*

It is acknowledged in (Lewis 2022) deep in the text. Now noted for clarity.

*Line 96: “one should average results over the various equivalent alternative” To clarify, is that what was done in S20?*

It is a consequence of the Bayesian approach if one interprets the various alternatives as each having some probability of being correct. S20 roughly applied this principle in a few places, but the statement here in the current text is not meant to refer to S20 or any other past study. No change made.

*Line 162: “Although paleoclimate evidence has great potential”. Is that true? The fundamental issue with paleoclimate studies, as stated by the authors earlier in the paper, is that the forcing is not known. That means that model-proxy differences can seldom be reconciled. This is in essence what the authors argue in the remainder of the paragraph, so I fail to see where the potential lies.*

Perhaps “great” was an overstatement so we have deleted it. However it remains the case that large, past climate changes are the closest analogs to ECS we have, and that we have the potential to constrain past forcings and improve interpretation of past changes through advances in proxy evidence and by applying our best models to this problem. We have added some words to justify the statement based on this argument.

*Line 254: “the historical climate record keeps getting longer” – we cannot really afford to wait much longer, though! I hope we won't get to see a world at 2xCO2 and experience what the ECS is...*

Agreed.

### **Technical comments**

Fixed, thanks.