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Interactive comment on "An emergent constraint on Transient Climate Response from simulated historical warming in CMIP6 models" *by* Femke J. M. M. Nijsse et al.

Anonymous Referee #1

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Review of "An emergent constraint on transient climate response from simulated historical warming in CMIP6 models" by F. J. M. M. Nijsse and co-authors.

In this paper the authors apply a recently proposed emergent constraint on transient climate response (TCR) on a new set of climate models (CMIP6). The emergent constraint uses warming since the 1970's which is a period that has aerosol forcing which doesn't change too much, and so even if there is uncertainty in the absolute magnitude shouldn't affect the warming rate too much. A best estimate TCR of 1.82 K is obtained, which is about 10 percent higher than that found in other studies. These other studies are Jimenez-de-la-Cuesta and Mauritsen (2019), Tokarska et al. (submitted), and

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implicitly Winton et al. (2020, JAMES, see their Fig. 14).

It is obviously useful to test a method on new model ensembles, as there have been several cases of emergent constraints found in one ensemble that does not work in another. However, the authors have made a series of choices that are different from the original study which hinders a direct comparison. Again, it is useful that choices regarding the statistics are explored, but it is not currently possible to see whether the shift is related to these new methods or something more fundamental. Other problems were that the authors have not used too many models and some of the writing was less insightful. I suggest the authors undertake major revisions.

Major issues

1) When a study obtains different quantitative estimates compared to previous studies (see above), then I expect to be able to understand why. It is not sufficient to say that this is within the error-bounds because the input data is in principle the same.

2) I found the discussion of ECS somewhat problematic; several detailed comments are provided below. The culmination, however, is at the beginning of section 4, when Figure 4 is discussed, plotting ECS against TCR. From this plot it is claimed that, contrary to earlier studies the post-1970s warming does not constrain ECS. However, the plot uses the posterior TCR to make this claim, not observed warming, and furthermore the authors do not provide a statistical analysis to support the claim. It is furthermore claimed that a straight line is superior to any other more physically based model, which is clearly not right. A physical constraint is that ECS -> 0 as TCR -> 0, and this linear fit is far from crossing the origin. Any curve looks linear if you zoom in far enough.

3) A perhaps somewhat less important point is that the authors first apply smoothing, then average over periods and ensembles, which is effectively the same thing. I mention this because it bothered me that the authors would add an unnecessary layer of complexity, and also because it was unclear what is done with the running-mean smoothing when you approach the end of the time-series in year 2018. For the early

period, nominally 1970-1980, it simply means there is some weighing of years outside the interval, out to 1965-1985 for an 11-year filter. But for the late period, which years are then included? All in all, though, there is no reason to do the smoothing at all, averaging over periods as well as ensemble members is a filter.

Detailed comments

5, Please report what range is given.

23, It is well-known that the TCR/ECS ratio is not a constant, but decreases with ECS (Hansen et al. 1985, Science). We now understand that the ratio is dependent on the feedback, heat uptake coefficient and pattern effects (e.g. Armour 2017).

31, 'climate trends, variability or other observables'

32-34, This sentence left me with an impression that the debate over the value of ECS revolves only around Cox et al. (2018a). Please remove or rewrite.

35, I suggest adding more relevant references, e.g. Gregory and Forster (2008), Otto et al. (2013) and Bengtsson and Schwartz (2013) etc.

40-41, What is this claim based on? Please explain and/or provide references.

42, The questions are also science-relevant, why deprive them to being only policy-relevant?

53, Here, and in several other places, the authors refer to the emergent constraint as theirs ("our constraint"). I suggest rewriting.

58-59, a 1 percent per year increase is also exponential.

Table 1, please add number of simulations and the temperature change.

94-95, Why omit so many years of data, 1980-2008 is not used but contains information as well.

97-99, I didn't understand this, see also major point above.

106, if using 2014 does not significantly change the results, then I suggest to stick with 2014 which would allow including many more models and alleviates concerns that stitching together two experiments could lead to biases (e.g. from missing volcanoes in scenarios).

113, what is "post-aerosol"?

Figure 1, it would seem that more than 13 lines are plotted.

131, this type of information belongs in Methods.

133, I would like to see CMIP5 models tabulated as well.

153, I was confused over this sentence, do the authors mean to refer to 3a instead, and the case where end and start year are so close that there is no signal?

175, I am not sure Rugenstein et al. (2019) said this.

176, likewise, I don't think Jimenez-de-la-Cuesta and Mauritsen (2019) said this.

Appendix A, I struggled to understand this. Would it be possible to provide an illustration of how the method works?

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