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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.

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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.

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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 warm-

ing 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 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.

— Response: We thank the reviewer for their thoughtful comments on our paper. Their suggestions with regards to ECS were especially important for improving the paper. The revised paper now contains an additional emergent constraint on ECS.

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.

— Response: We have included further comparison with the Jimenez-de-la-Cuesta and Mauritsen study (henceforth JM19). We have identified several differences: a) JM19 had slightly lower values for TCR compared to the IPCC values. We have now computed TCR values directly (instead of using AR5 values) for CMIP5 to ease comparison with JM19 and CMIP6. Our calculations are very close to the standard IPCC values; b) JM19 compared different periods to those used in our draft paper; c) a minor programming error was found in the analysis software of JM19; d) JM19 used a different statistical method that assumes error solely in the independent variable. This reduces regression dilution. However, for an observation that is less than the average model warming, this method produces lower values (by about 0.1K). Despite these differences, we get a very similar emergent constraint on TCR if we use a similar method (JM19 had a best estimate of 1.67K, and we get 1,66K using similar methods for CMIP5). We have now included the ODR statistical method in a revised figure 3. We also provide details of our attempts to reproduce JM19 in our revised results section, and have added a table of CMIP5 model values to the appendix.

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.

— Response: Our aim in including Figure 4 was to demonstrate that a good constraint on TCR does not imply a good constraint on ECS. However, we accept the reviewer's criticism that our discussion of this point was not well justified by the analysis that we presented. In the revised manuscript, we now attempt to fit the non-linear function between ECS and warming trend, as proposed by JM19. Diego Jimenez de la Cuesta kindly provided us with the python code he used in JM19, which we were able to compare to our own code. We conclude the following: (a) there was a minor coding error in the JM19 code (errors switched between x and y variables), and also some arbitrary adjustments made to compensate for said error; (b) when these are corrected, and we use the warming to 2005 (as JM19), we find no useful constraint on ECS; (c) however,

when we use the warming out to 2019, the corrected code produces a constraint on ECS which is similar to that reported by JM19 (95% ranges of ECS: JM19=1.72-4.12 K; our study=1.76-4.52 K). We have also investigated whether the functional form proposed by JM19 is supported by the relationship between ECS and the warming trend across the models:

ECS = DT/(s' - e'DT).

In order to turn this theoretical relationship into an emergent constraint on ECS, JM19 used the ocean heat uptake parameter e' and the radiative forcing parameter s' as fitting parameters. As these values are highly variable between models, the residual of the DT-ECS emergent relationship should be at least partially explained by model differences in ocean heat uptake and forcing. - if this function is indeed theoretically sound. Yet, DT/(s'-e'DT) correlated better with ECS if the fitted s' and e' are used, instead of model-specific s' and/or e', suggesting that the theory used by JM19 is not fully consistent with the results from the CMIP5 and CMIP6 models. We discuss these issues concerning the relationship between ECS and the warming trend in a revised Section 3.

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.

— Response: In fact, there is only one step of time-averaging (the wording smoothing and running mean referred to the same step), and the years 1965-1970 were not used at all. We have made changes in the text and figure captions to make this clearer (e.g. not using a central year but instead writing the period out explicitly in the x-label of Fig 2).

Detailed comments 5, Please report what range is given.

— Response: this would make the abstract less readable, as it already contains quite a few numbers. The full range is clear from Table 1, and is now also reiterated in the introduction.

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).

— Response: we have removed this rule-of-thumb and added a discussion about the reduced ratio.

31, 'climate trends, variability or other observables

— Response: added

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

— Response: We removed two of the citations, and added two more recent papers by others.

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

— Response: we've added the reference to Gregory and Bengtsson.

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

— Response. We've added a reference to Tanaka & O'Neill, NCC, (2018).

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

relevant?

— Response. Added.

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

— Response: done

58-59, a 1 percent per year increase is also exponential. Table 1, please add number of simulations and the temperature change.

— Response: We've added exponential before 1% to make this point clearer. We'll add more information to Table 1.

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

— Response: Figure 3 shows a sensitivity to parameter choice, of which one is the running mean. We've extended this to 20 years, and conclude not much extra information is obtained if more years are used.

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

— Response: we have clarified this by adjusting the figure captions. We now state the difference between period A and B more clearly.

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).

— Response: With the addition of more models and an extra year, the difference between ending in 2014 and 2019 is more pronounced. There is a trade-off between using a shorter period, that was also dominated by the 'global warming hiatus', and biases from a discrepancy between real and modelled forcings. We have now investigated the sensitivity to forcings by comparing the various SSPs. A difference of only 1% was found between DT for SSP126 and SSP585.

113, what is "post-aerosol"? Figure 1, it would seem that more than 13 lines are plotted.

— Response: post-aerosol changed into post-1970. In Figure 1, up to ten lines per model were shown. To emphasize this, the sentence stating this has been brought forward in the caption.

131, this type of information belongs in Methods.

— Response: moved.

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

— Response: we will do this.

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?

— Response: The sentence referred to 3c. However, with more data analysed, we have adjusted the paragraph. The uncertainty is now minimum between 1970 and 1976. The second part of the confusing sentence has been deleted, as uncertainty is not that big anymore with the larger set of models.

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

— Response: We removed the sentence, and replaced it with a discussion of the nonlinear relationship.

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

— Response: Likewise deleted.

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

— Response: we have added an illustration, replaced the pseudo code with equations and simplified the text.

Geoffroy, O., Saint-Martin, D., Bellon, G., Voldoire, A., Olivié, D. J. L., & Tytéca, S. (2013). Transient climate response in a two-layer energy-balance model. Part II: Representation of the efficacy of deep-ocean heat uptake and validation for CMIP5 AOGCMs. Journal of Climate, 26(6), 1859–1876. https://doi.org/10.1175/JCLI-D-12-00196.1

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