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Interactive comment on "Emergent constraints on Equilibrium Climate Sensitivity in CMIP5: do they hold for CMIP6?" *by* Manuel Schlund et al.

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Review of "Emergent constraints on Equilibrium climate sensitivity in CMIP5: do they hold for CMIP6?" by Manuel Schlund and co-authors.

In this study a series of mostly process-oriented emergent constraints that were developed on earlier model ensembles are applied to the latest CMIP6 ensemble. This is a very welcome attempt and in a broad sense testing scientific reproducibility. My major concern is with the main conclusions drawn, or perhaps not drawn, from the results. The fact that estimated ECS based on these constraints increases roughly in proportion to the mean ECS increase from CMIP5 to CMIP6 suggests that these constraints are in not actually constraints on ECS, rather, at best they are constraints on the feed-

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Discussion paper

back processes they target. I develop argumentation this below, along with providing some more technical comments. I sign this review such that should the authors have any issues understanding my point they can contact me directly.

Sincerely,

Thorsten Mauritsen

—

Climate sensitivity is inversely proportional to the feedback parameter (lambda in equation 2) which in turn is a sum of a series of processes (sum of lambda_i). My take on the situation is that many of the emergent constraints (except COX) were successful on CMIP5 because inter-model spread in ECS in that ensemble was dominated by spread in low-level cloud feedbacks in the tropics. However, if any other feedback, e.g. water vapour feedback or any other cloud feedback, is biased in the ensemble as a whole then these kinds of process-oriented emergent constraints will necessarily be biased in their estimates of ECS. Probably even collectively since they thrive on the same kind of model spread, and so just because there are many studies that agree doesn't increase our confidence in their quantitative outcome. Likewise, if structural commonalities among models cause an unreasonable low inter model spread in some other feedback process then the emergent constraint is going to be over-confident. All in all, the results suggest that the original studies were overly confident and that changes in feedbacks not constrained by these studies cause them to be biased with a sign that cannot be determined (since CMIP6 probably also contains collectively biased feedback processes). Thus, these process-oriented emergent constraints are perhaps best thought of as constraints on the processes that they target, rather than constraints on ECS, and in extension the original studies have been disproven by the results of this study.

There are alternatives to process-oriented emergent constraints, though, one of them which is included in this study (COX, more about this study and why I think there is a shift below). Emergent constraints that use global temperature change as a predictor of ECS do not suffer from the same problem: even if one feedback is biased in a model ensemble the constraint can in principle still work since both global change and ECS are inversely proportional to the sum of feedbacks. Suggestions of emergent constraints of this kind include Last Glacial Maximum (Hargreaves et al. 2012 doi:10.1029/2012GL053872), Pliocene warming (Hargreaves and Annan, 2016 doi:10.5194/cp-12-1591-2016), and post-1970s warming (Jimenez-de-la-Cuesta and Mauritsen 2019 doi:10.1038/s41561-019-0463-y). All of these ideas have been tested across ensembles including CMIP6/PMIP4 (Tokarska et al. 2020 doi:10.1126/sciadv.aaz9549; Renoult et al. 2020 doi:10.5194/cp-2019-162), finding essentially unchanged results between ensembles. Other studies worth mentioning are Bender et al. (2010, doi:10.1007/s00382-010-0777-3) and Dessler and Forster (2018), although I haven't seen tests of these.

I think all of the above is rather straightforward and fairly easy to understand. I think the authors have everything at hand that they need to draw the conclusion that the process-oriented emergent constraints are not useful for estimating ECS, but rather should be better thought of as ideally constraining part of the cloud feedback. There are several places throughout that needs revising.

Other major points

I think it is not reasonable to provide best estimates of ECS in the abstract and summary based on this study for the following reasons:

1) The above issue.

2) Because the study does not apply the latest observations to the constraints, rather opts for using the original observations. This is a perfectly fine choice given the scope of the paper, but it does mean the constraints are not up to date.

3) Because the study uses an implicit flat prior which in case of weak data automatically

leads to high-biased results.

I would instead suggest the authors cite percentage increases which is anyway all that is relevant here.

Regarding the Cox et al. 2018 constraint, section 3.2, this is built on the Hasselmann (1976) single heat capacity model. In this model there is a linear relationship between Psi and ECS, however, and despite what they claim if you add a deep ocean to the model you obtain a non-linear relationship wherein the relationship is weaker for higher ECS, see Annan et al. (2020 doi:10.5194/esd-11-709-2020), their figure 5 (note flipped axes). If you look at how the CMIP6 models are distributed they are simply situated in the flatter part of the expected curve, and if you fit a straight line to it you will obtain different slopes than for CMIP5.

In this regard, and this applies not only to this study but most of these kinds, I am concerned with the general use of linear regression. The most silly example is SU constraint, where despite getting the wrong sign of the slope in CMIP6, you obtain a constraint on ECS. I think studies must be much more smart about their choice of statistical model, and not just use linear regression when non-linear behaviour is expected or other physical constraints can be applied such as a near-zero intercept, examples in Jimenez-de-la-cuesta-Otero and Mauritsen (2019), Annan et al. (2020) and Renoult et al. (2020). In case of process-oriented emergent constraints one could perhaps think of using Equation (2) in the form ECS $\sim$ a/(b+x) where x is a process-oriented predictor. I am not saying the authors need to change this, but it would be worthwhile acknowledging that using linear regression, heedlessly, can lead to misleading and over-confident results.

I found the discussion of statistical significance somewhat disturbing. The chosen thresholds seem purely subjective, as far as I can tell. I would suggest to delete this whole discussion which seem rather pretentious.

I found Sections 4 and 5 rather long and repetitive. I would suggest revising and sharpening.

Minor things

19, ’of spread in ECS among models’

36, ’concentration over pre-industrial levels’

69, I am not sure Forster et al. 2020 is correctly cited here

85-92, perhaps drop Delta from F, and when using a specific forcing in equation 2 write F_{4x} or something?

90-93. did you account for model energy leakage and drift? Concerning drift, some do account for this, but is not always obvious if there is a best way, nevertheless you must document what you did.

120-123, there are also an intermediate option, e.g. the Cauchy prior used in Annan and Hargreaves (2011, Climatic Change). Regarding the uniform prior, please specify which cut-off you use.

154-155, this is a misinterpretation, the IPCC ’likely’ statements refer to 66-100 percent probability.

158-159, I felt this statement could be made more informative by explaining that it is the covariance of clouds with surface temperature anomalies.

180, ’results to choices made in the analysis’

236-263, these constraints seem to have some legacy with Fasullo and Trenberth (2012, Science), perhaps worth mentioning if the authors agree?

276-277, it is incorrect that Volodin (2008) was the first emergent constraint on ECS, there is Covey et al. 2000 and Knutti et al. 2006 before then.

320, I would suggest deleting ’describing the real world’

354-355, The idea and strength of an emergent constraint is that you use something you can observe to predict ECS. It really shouldn't matter if a process is slightly different in the warmer 2xCO2 world.

357-359, same applies here

360-361, this statement goes further than Zelinka et al. 2020. I would suggest replacing 'dominated by' -> 'to some extent associated with'

402-403, as per my above argumentation, I would be very careful with making this statement.

409, this might also have been shown by Klocke et al. (2011), check.

421-426. the very same paper also shows that the ECS estimated from 4xCO2 runs is higher than twice that in 2xCO2 runs, and that the bias of the same order of magnitude.

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