

Earth Syst. Dynam. Discuss., author comment AC3
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Reply on RC3

Benjamin M. Sanderson et al.

Author comment on "The potential for structural errors in emergent constraints" by
Benjamin M. Sanderson et al., Earth Syst. Dynam. Discuss.,
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The authors highlight the disagreement present between different estimates of the same quantity arising from multiple single metric emergent constraints and the confusion this may cause. They claim that the cause of this disagreement is over-simplified process representation and a lack of diversity among climate models, and illustrate their argument using a class of simple climate models.

Agreed. Many thanks to the reviewer for the comments and careful review.

Overall I find little to criticise, except that the manuscript reads more like a review than an original research paper.}

This opinion was reflected in all the reviews (and the authors broadly agree). Given this, we requested that the journal reclassify the paper and we now present the study as a review.

The discussion of the different classes of emergent constraints largely reflects what has been written elsewhere (and cited within the manuscript). The main original contribution appears to be the illustrative example, which is informative and welcome. The generality of the conclusions from such an extreme example is perhaps questionable, but many processes within climate models are parametrised equally simply, or more so.

Thanks for the comments. We agree that the simple model is an extreme example of a structural error, which cannot be generalised to parametric limitations in CMIP-class models. However, we do think that the simple example serves to illustrate the potential for how structural errors could project onto emergent constraints, and ultimately to encourage future studies to consider how ensemble-derived relationships could, in fact, arise from simple common parametric assumptions which exist throughout the ensemble. We have endeavored in the revisions to sharpen our language in making the distinction between the toy model and the discussion of CMIP structural errors.

Minor points:

Lines 17-18: From a purely statistical perspective there is no contradiction here. Inferences about the same quantity derived from different lines of evidence are not expected to be the same. As argued by Williamson and Sansom (2019), it is

likely that many of these estimates are over-confident. But even if a more appropriate uncertainty quantification were carried out, the inferences would not and should not be the same. However, I accept that this is confusing for policy makers.

Agreed, apologies for the lazy wording - we've revised as:

"The prevalence of this thinking has led to literature which made confident, yet inconsistent between studies, claims on the probability bounds of key climate variables. "

In general, we agree that there is the potential to use multiple ECs and combine lines of independent evidence. However, we would argue that the majority of EC studies do not present their results as likelihoods which could inform a combined posterior or body of evidence - rather they present their headline result as an absolute probability (see, e.g. Cox 2018, Nijssse 2020, Jiménez-de-la-Cuesta 2019).

Line 19: More likely than what? This is a leading statement, the authors only show that this is one possible explanation for emergent behaviour

Agreed. We've revised the sentence as follows:

"Here, we illustrate that strong ensemble relationships between observables and future climate can arise from common structural assumptions with few degrees of freedom. Such cases have the potential to produce strong, yet overconfident constraints when processes are represented in a common, oversimplified fashion throughout the ensemble, where we might have low confidence in the behaviour of the process in a future climate. "

Lines 66-67: A constraint disappearing in later generations of climate models is not necessarily proof that the constraint was spurious. Convergence among models may make the spread among the observable and/or the target quantity to small for a "significant" relationship to be detectable, the relationship may even appear to change sign. Although, convergence has its own caveats if it is not due to advances in knowledge but rather common acceptance of a least bad solution.

We agree - the original sentence was sloppily worded. We intended to convey the case (as in Klein and Hall 2015), where the new models are outliers in the original constraint, thus degrading confidence in the relationship when the two ensembles are combined. We've revised the sentence to clarify.

Lines 93-95: The pseudo-Bayesian approaches cited have very limited applicability, and most principled statistical approaches to the analysis of emergent constraints rely on regression analysis which does not imply model weights.

Agreed, thanks. Added the caveat that these studies do not test the underlying implicit assumptions of the regression framework.

Line 98: Some level of subjectivity is unavoidable, the idea of objectivity in science where data are interpreted through any model is a delusion. The resampling approach used in the following paragraph applies avoids making parametric assumptions about probability distributions but implies certain assumptions of its own which aren't clearly stated.

Agreed. The analysis has been removed in the revision, following comments from other reviewers, but we fully agree on the impossibility of truly objective analysis

Lines 138-141: The correlation between the Cox and Sherwood D constraints is relatively weak and unlikely to be statistically "significant".

Agreed - apologies for the error. The relevant section has been deleted.

Lines 215-218: See my previous comment on Lines 66-67.}

Again, the wording was sloppy. Now:

"If an emergent constraint has been found in an MME (providing it has not been demonstrated to be statistically spurious by, for example, additional models which significantly weaken the correlation (Klein and Hall 2015))..."

Line 369: What is meant by a meaningful constraint? The two later model indicates a reduction in uncertainty.

Sorry, reference error. The sentence (now reworded) refers to Figure 1d - which shows that T70 is not strongly correlated with T280 in the 2 layer model ensemble.

Line 369: Figure 2d?

Yes, indeed - see above.

Line 654: Karpechko et al. 2013 (DOI: 10.1175/JAS-D-13-071.1) should be cited here.

Agreed - added.

References

Cox, P. M., Huntingford, C., & Williamson, M. S. (2018). Emergent constraint on equilibrium climate sensitivity from global temperature variability. *Nature*, 553(7688), 319-322.

Nijse, F. J. M. M., Cox, P. M., & Williamson, M. S. (2020). An emergent constraint on Transient Climate Response from simulated historical warming in CMIP6 models. *Earth System Dynamics Discussions*, 2020, 1-14.

Jiménez-de-la-Cuesta, D., & Mauritsen, T. (2019). Emergent constraints on Earth's transient and equilibrium response to doubled CO₂ from post-1970s global warming. *Nature Geoscience*, 12(11), 902-905.