

Earth Syst. Dynam. Discuss., referee comment RC3  
<https://doi.org/10.5194/esd-2020-85-RC3>, 2021  
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## Comment on esd-2020-85

Anonymous Referee #3

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Referee comment on "The potential for structural errors in emergent constraints" by Benjamin M. Sanderson et al., Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2020-85-RC3>, 2021

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### Summary:

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.

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

### 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.

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

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.

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.

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.

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

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

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

Line 369: Figure 2d?

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