Earth System Dynamics Discussions

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Comment on esd-2022-43

Anonymous Referee #1

Referee comment on "Emergent constraints for the climate system as effective parameters of bulk differential equations" by Chris Huntingford et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2022-43-RC1, 2022

Review of Huntingford et al 2023:

On one hand, this paper is clear, well-written, and its PDE examples are simple, relevant, and pleasant to work through. On the other hand, I didn't really learn anything from reading this. For example, I can't imagine anyone understanding Cox et al (2018) without having a deep understanding of the notion that the emergent equations governing temperature changes on various timescales are linked via heat capacity. This left me wondering whether the paper is worth publishing. Ultimately, I think the answer is yes because if someone *didn't* intuitively understand that emergent constraints occur due to links between underlying governing equations, this paper would do a nice job of introducing them to the concept. I doubt the paper will be cited much, but that doesn't mean it shouldn't be published.

Minor comments (Note my convention is P2 L1 = Page 2, Line 1):

- P2 L1: observationalists would disagree that ESMs form the basis of climate research. I tend to say they're a *pillar* of climate research.
- P2 L6: It's not accurate to say that ESMs are typically forced with historical and scenario GHGs. A lot of time is spent on PI control, abrupt4xCO2, 1%CO2, etc. Minor rewording is needed.
- P2 L19: "main possibly simplest answer" is awkward grammar
- P3 L30 – P4 L2: The first sentence here isn't very clear. I think you are saying that Schlund and others found that ECs *based on* CMIP5 were generally worse when applied to CMIP6. As written, it sounds like any EC, including ECs developed from CMIP6 data, would have wider bounds. I also found your wording a bit confusing because wider bounds could come from worse correlations between EC predictor and predictand OR from larger spread in the observations used to constrain. I guess the problem must be the former, but it takes the reader some unnecessary thought to get to that conclusion. Following on this, I think the obvious explanation for larger spread in CMIP6 is that the

ECs from CMIP5 were overtrained: they are capturing noise rather than real EC signal. I'm confused how this possibility isn't even in your proposed reasons at all.

- P5 L15-16: you introduce T* here but don't use it again except P8 L3. I suggest deleting both T* references. In particular, the wording of the first intro to T* was very confusing (and I think, unnecessary).
- P5 L26-28: When you say "running mean", I immediately wonder what the averaging period is. I think it would be better to call this statistic the "annual average". Relatedly, the running mean itself isn't a measure of climate change. The *time derivative* of the running mean is your proxy for climate change. But of course, the annual average isn't special in this regard – the long-term average of the time derivative of the instantaneous T(t) equation would give the same answer because the derivative is a linear operator.
- P6 paragraph starting L8 and P8 paragraph starting L23: I think this discussion can be improved. I think the big point you're trying to make is that while the fact that there *exists* a predictive relationship between the observable and the future quantity of interest allows you to predict ECS, the *slope* of that relationship provides interesting information about the physical equations that underpin that relationship. I think you are further pointing out that even though there may be uncertain terms in the equation governing the current-climate variable and in the equation governing future change, those uncertain terms sometimes cancel out when the quantity you're actually interested in is the *ratio* between predictor and predictand. As it stands, I don't think it is interesting that uncertainty in either of 2 parameters would give rise to the intermodel spread needed to compute an emergent constraint. I also don't think you adequately explained why b_i/H_{0i} would be constant across models.
- This is a minor point, but the seasonal cycle in eq 4 and eq 11 won't be exactly equal to the observed seasonal cycle in a warming planet ($H_0>0$) because the planet will have warmed a bit in the 6 months between winter and summer.
- P8 L17: defining your current-climate metric as d/dt(annual-ave T(x=0)) makes sense, but multiplying it by sqrt(t) seems contrived. If you have such an exact understanding of the underlying equations, you'd probably already know what H_0 was, so a regression would be unnecessary!
- P11 L34-P12 L1: I can't imagine how a real emergent constraint wouldn't have a physical underpinning that can be expressed as an equation. We may not know what that equation is, but if there truly isn't an underlying equation behind an empirical relationship, how could that relationship possibly be real?

I don't think the emergent relationships that distill into an EC are necessarily (or even typically) PDEs. In your examples, the fine scale governing equations are PDEs, but the equations you derive for seasonal cycle and warming tendency are not. Similarly, concepts like "if you don't have much cloud in the current climate, then you don't have much cloud to lose in the future" are fundamentally connecting model state to model change. This doesn't invalidate any of this work, but a reframing of the title and some rewording in the abs