

Earth Syst. Dynam. Discuss., referee comment RC3
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Comment on esd-2021-87

Anonymous Referee #3

Referee comment on "ESD Ideas: A Global Warming Scaling Law" by Mikhail Verbitsky and Michael Mann, Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2021-87-RC3>, 2022

The authors present an approach to analyze the time evolution of global warming based on century-old dimensionality arguments that are commonly used in the field of fluid dynamics. They include more recent insights into the origins of incomplete similarity to hypothesize a characteristic scaling of $T \sim t^2$ for an intermediate pre-hothouse climate regime, before the positive feedbacks start to dominate.

First of all, it could be clarified why other dimensional physical parameters that play an important role in the climate system, such as the active ocean layer depth/mass that responds nonlinearly to global warming, albedo etc. do not feature. Alternatively, explain how they are absorbed into the limited set of parameters shown here. It is possible that they can all be reduced to the proposed time scales, but the way it's presented feels like an extreme simplification.

Secondly, there's a significant difference with fluid dynamics that is not highlighted in this study. In contrast to fluid dynamics, in climate there is no multitude of experiments that provide the required observations time series to determine the scaling parameters. Of course, this does not mean that no information can be distilled from the limited historical climate record - clearly, as many studies have shown, hypotheses can be presented and analyses based on (ensemble) climate methods can confirm or refute these hypotheses, using the historical climate record. However, the proposed scaling behaviour is only feebly, if at all supported by the historical data. I realise that this is due to the nature of the problem - we have to make do with the observations we have. As a result, however, I find the evidence for the theory too tenuous to publish as-is.

A straightforward way to fix this, is that the authors test their hypothesis through another means. Indeed, the simplicity of the assumptions implies that they can use fairly simple general circulation models, or even simpler zero-dimensional energy balance models, to generate temperature time series for the different scenarios. This will allow them to check whether the scaling behaviour holds. Unless this can be provided, I do not recommend publication of this manuscript, as interesting as the underlying ideas are.

