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Earth System Dynamics
Discussions

Interactive comment on "Definitions of climate and climate change under varying external conditions" *by* C. Werndl

Anonymous Referee #2

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The paper discusses several definitions of climate and selects a particular one that, in author's view, is the most useful for the climate research.

I start with an observation that the paper is essentially a paraphrase of another one by the author (Werndl, 2014 in the reference list). Hence, this submission does not present an original research contribution.

Nevertheless, I comment briefly on the study, with the main goal of illustrating why I think it does not pass the quality criteria of an ESD publication. My main concerns are that the paper (i) does not provide new technical contribution to the climate studies, (ii) fails to maintain the rigor assumed by the chosen dynamical systems framework, and as a result (iii) does not have a well defined target audience. Accordingly, the paper cannot be recommended for publication in ESD.

The main research problem of this study – definition of climate – is obviously too far away from being resolved, or even allowing a general consensus among the experts. Accordingly, an attempt to provide a technical solution to this problem on the quite noble abstract level of non-autonomous dynamical systems (without even touching any climate data) is equally brave and hopeless. But, indeed, this alone does not prevent a publication on the topic. It happens, however, that the paper is merely a scientific essay aimed at justifying the author's feeling that a particular, not rigorously stated, definition of climate might be better suited for studies of climate change. Although this looks like an honest take on the problem, this conclusion cannot be taken more seriously than any other personal belief of a particular expert. A crucial drawback of the study is that its conclusions are not supported by climate data or comprehensive modeling. Of course, this happens for a good reason – the state of the art in climate studies would not allow such a direct validation.

Now, without data or comprehensive model support the author chooses to seek the climate definition within the framework of deterministic dynamical systems, which naturally assumes a certain level of rigor. However, after a lengthy discussion richly filled with abstract concepts, we are left with an extremely vague proposal of using "certain regime of varying external conditions". This "solution" may fit well a dinner talk on the climate change, but it hardly passes the level of rigor required for the dynamical system theory. Moreover, the proposed solution remains unsupported even on a conceptual, toy-model, level. The presented exercises with the logistic system do not convincingly demonstrate that the definition T3 resolves the problems of climate definition, although it might outperform some of the alternative definitions in particular situations. The choice of a model also calls for a better justification.

Another issue – who is a potential reader of the paper? The text assumes that a reader does not need explanation of "tangent bundle", "contracting invariant subspace", and such, and can readily digest the concepts of Axiom A system, hyperbolic set, and Sinai-Ruelle-Bowen measure from a one-line definition. However, I hardly see that this

paper will attract mathematicians, who probably know well the list of facts mentioned in the text. For the climate researchers, on the other hand, the paper seems to be unnecessarily abstract and unrelated to climate data and models.

Finally, the mathematical part of the paper should be carefully proof-read and revised, as currently it shows annoying typos like an erroneous definition of wandering set, a clumsy explanation of non-reversibility, as well as numerous almost-repeating equations that never go beyond the level of basic definitions and hence may be safely skipped. In addition, I hardly understand statements like "the models used in climate simulations are discrete", which are too general and vague to be meaningfully assessed. The paper summary "The recently developed theory of non-autonomous dynamical systems was employed to mathematically analyze the alternative definitions of climate and to identify. . ." (p. 710) is grossly misleading, since the study does not present any mathematical analysis, which should not be confused with a discussion of existing mathematical results.

It seems that the material in the paper can be reorganized to target either mathematicians who approach the climate problem for the first time (in this case the paper must be seriously shortened), or climate scientists looking for more rigor in their studies (in this case the mathematical jargon should be softened and better data support added). In both the cases, the material in the paper does not pass the research quality criteria for ESD.

Interactive comment on Earth Syst. Dynam. Discuss., 5, 683, 2014.