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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 #1

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The author offers a discussion of a definition of climate according to which climate is a distribution over time of climatic variables where external conditions are held constant as well as of analogous definitions. The discussion takes into account the fact that external conditions vary. Moreover, it aims to apply non-autonomous dynamical systems theory to analyzing the definitions. Finally, it aims to discuss the similarities and differences between the definitions for constant and varying external conditions. As part of the discussion, a purportedly novel definition of climate is offered. On this definition, T3, climate is the distribution over time under a regime of varying external conditions.

The paper's methodology involves making use of an unrealistic climate model and formal results without sufficient supplementation by empirical and physical considerations. In addition, the discussion of the various definitions of climate does not really

keep up with the literature. In particular, the purportedly novel aspect of T3 – the definition which is purportedly novel and which is preferred out of T1, T2 and T3 – is not novel. Moreover, all the definitions discussed are instances of a kind of definition that has been recently criticized at length in the literature and this criticism does not feature in the paper. Much of the discussion involves presenting rather obvious implications of existing results in dynamical systems theory. That said, the paper is mostly well written and clear. The technical discussion is, in my view, accurate (I, however, recommend that a non-philosopher who works on dynamical systems theory confirm my judgment about this). Moreover, some of the implications of non-autonomous dynamical systems theory for the definition of climate are brought out, as are similarities/differences between the definitions of climate. Perhaps the paper can, accordingly, be viewed primarily as a literature review that brings together material for an uninformed audience. Thus, although I do not recommend publication of the paper in its current form, it might be suitable for publication once an adequate treatment of the literature is provided, at least some further consideration of empirical evidence/physical considerations is introduced and a few methodological comments are added. I recommend revision and resubmission.

An additional issue for the present paper is the question whether it adds enough to Werndl 2014 in order to warrant publication. On the one hand, the present paper's question, the definitions discussed and the main arguments appear in Werndl 2014. Even the main technical results are already stated in Werndl 2014's appendices, which provide references if further detail and precision is desired. On the other hand, BJPS is publishing Werndl 2014 and its treatment of the literature is similar to that of the present paper. Publishing an improved version of the present paper will thus allow a better representation of the debate about what climate is. Responding to the comments below should, at the same time, introduce new material into the paper and might shed more light on how it already contributes to the literature. In addition, the readership of the present journal is substantially different from the BJPS readership. I find it hard to judge what the readership of the present journal will find useful. I suggest that the

editors revisit this matter at a later stage in the review process.

1. The purportedly novel aspect of T3 is its relativization of climate to regimes of changing external conditions. This is not a novel aspect. That climate should be relativized to regimes of changing conditions is already made explicit and incorporated into definitions of climate by Lovejoy, Schertzer and others. Thus, for example, Lovejoy notes that different periods may have different climatic distributions and that, for each relevant period, we need to define a climate state relative to which climate change occurs (Lovejoy 2013):

Using the trichotomy weather, macroweather, climate, we can naturally define "climate states" as the averages over macroweather at the scales at which the variability is at its lowest ($\approx$ 30 yrs) thus conveniently justifying the "climate normal" concept (and indeed nuancing it since 30 yrs is an average over different geographical locations and epochs). "Climate change" thus naturally refers to the change in climate normals at longer (climate) time scales.

2. Lovejoy and Schertzer (see references) criticize scale-independent definitions of climate in terms of distributions of variables such as temperature, including the definitions discussed in the present paper. For one thing, in their view, there are empirical and physical reasons for supposing that such definitions are inadequate. For another thing, in their view, (2013a, chapter 10):

A useful definition of climate should involve a physical basis for the distinction/boundary between weather and climate as well as an identification of each regime with specific mechanisms and a corresponding specific type of variability.

These criticisms should thus be discussed in the present paper.

3. The definitions of climate and climate change provided by Lovejoy and Schertzer should be discussed or, if not, an explanation of why they are not being discussed should be offered. It looks like these definitions should be discussed because they

address the kinds of worries raised by the present paper for other definitions and avoid worries not discussed in the present paper. Bryson (1998) also offers a definition of climate that might be of interest to the author.

4. The paper uses an unrealistic climate model in order to illustrate many of its points. If the use is just illustrative, please make this clearer. If it is not, please justify using the model to assist in supporting substantive conclusions, including conclusions about how the climate system/realistic climate models might (in any substantive sense) be.

5. At a number of points in the paper, empirical and physical considerations are not adequately taken into account. Some of these relate to the scale-dependent behavior of climatic variables (see comment 2 above), but others do not. Most notably, the author considers the question whether the memory of initial values of climate variables washes out over time. In doing so, however, no empirical or physical considerations are mentioned, though the literature primarily focuses on such considerations.

6. The various definitions of climate that are discussed in the paper are treated as definitions that are supposed to hold/be useful in all circumstances. If the author thinks this is the spirit in which all the definitions have been put forward, this should be substantiated. If not, this should be made clearer.

7. There is quite a bit of repetition of claims across the discussion of the different definitions of climate. Perhaps some thought can be given to whether this repetition can be minimized. In addition, the details of technical results from the literature are provided. Isn't it often enough to refer to these results in citations?

8. It would be helpful if the author said a bit more about how the present paper goes beyond Werndl 2014.

-The citation Mancho et al. 2013 on p. 692 should be, according to the bibliography, Mancho et al. 2014.

-'vary' should be 'wary' on p. 696.

-Redundant ‘the’ on p. 697.

References

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