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

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RESPONSE TO REFEREE 1

First of all, let me thank the referee for the very helpful comments, which will greatly improve the paper.

POINT 1:

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 (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.

It might well be that the relativization of climate to regimes of changing external conditions is not a novel aspect, and I am happy to change the manuscript accordingly (which is not a problem as there are several other novel points/criticisms made in the paper). What I regard as important is that Definition T3 is hypothetical in the sense that it is not just about the actual evolution of the climate system but how the climate system would evolve given a certain regime of varying external conditions. To provide an example: if a certain regime of varying external conditions is only present (in actuality) for 15 years, the climate might still be the distribution that arises under the regime over a time period of thirty years. It might be that also Lovejoy and Schertzer's (unpublished) definition is hypothetical, but then it would be good to stress this more (much of what they write concerns the actual evolution of the climate variables and thus seems to be ambiguous about whether climate is hypothetical in this sense).

POINT 2:

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 definitions 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.

I entirely agree that these criticisms should be discussed. I agree with Lovejoy and Schertzer's criticism that there should be a physical basis for the distinction between weather and climate. This worry can be addressed in the framework of Definition T3 as follows (and I would outline this in the revised manuscript). For Definition T3 a key question is how to define a regime of varying external conditions. Here is where Lovejoy's and Schertzer's (unpublished) ideas can be utilized. The idea of regimes of varying external conditions can be made more precise by defining a regime in terms of the scales of minimum temperature variability. This would then be 10-30 years in the industrial period and closer to 100 years in the pre-industrial period. Such a version of Definition T3 would not be scale-dependent (as required by Lovejoy and Schertzer). However, I still think that Lovejoy and Schertzer's ideas are only one (albeit very useful) way to make the idea of regimes of external conditions more precise (there might be others too, which is fine).

POINT 3:
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.

Thank you to the referee for bringing the papers of Lovejoy and Schertzer to my attention. These are very interesting papers, which certainly need to be discussed. I would do this in Subsection 3.3 because their work can be utilized to make the idea of regimes of varying external conditions more precise. I would outline their definition and then make three points. First, as outlined above, their ideas can be used to make the idea of a regime of varying external conditions more precise (see the reply to Point 2 above). Second, I would stress the point that climate needs to be a hypothetical notion and that Lovejoy and Schertzer's definition can be interpreted in this sense (see the reply to Point 1 above).

Finally, I would comment on a difference between the definition of climate change based on Definition T3 (when regimes are defined by appealing to Lovejoy's and Schertzer's ideas) and Lovejoy and Schertzer's definition of climate change. According to Definition T3, climate change is a change in two consecutive distributions (taken over periods tc, where tc is defined by the minimum temperature variability). For Lovejoy and Schertzer climate change amounts to a change in normals at longer time scales, i.e. taken over time periods which are longer than tc. When a normal over a time period of length 2tc is considered and compared to the climate state (the normal over a time period of length tc), this amounts to the same as climate change for Definition T3 (i.e. comparing two consecutive distributions of length tc). However, if for Lovejoy and Schertzer's account other time periods (longer than tc) are considered, climate change is different from climate change as defined by Definition T3. It seems to me that if in Lovejoy and Schertzer's definition the time period is not 2tc, then there is the danger that one compares macroweather to an incomplete next macroweather or to several macroweathers lumped together, which might be undesirable.

POINT 4
Bryson (1998) also offers a deïňĄnition of climate that might be of interest to the author.

Thank you to the referee for making me aware of this paper, which certainly deserves to be discussed in the revised version of the manuscript. For Bryson (1997, 451) “Climate (climatic status) is the thermodynamic/hydrodynamic status of the global boundary conditions that determine the concurrent array of weather patterns”. This definition is different from all the other definitions discussed in the paper and is most similar to Definition T3. Accordingly, I propose to introduce Bryson’s definition at the end of Subsection 3.3 and to briefly compare his definition to Definition T3 by discussing the following three points. First of all, Bryson’s definition is similar to Definition T3 in that both definitions are hypothetical and not just about the actual evolution of the climate variables: Definition T3 is about the distributions that arise under a certain regime of varying external conditions and Bryson’s definition is about the weather patterns that arise under a certain thermodynamic/hydrodynamic status of the global boundary conditions. Second, for Definition T3 an important question is what a regime of varying external conditions amounts to. Similarly, Bryson’s needs to say more about what weather patterns are. Do these weather patterns amount to what Lovejoy and Schertzer call “macroweather” or statistics of the weather over longer or shorter time scales? Third, the main difference is that Bryson’s definition of climate identifies the climate with boundary conditions that determine weather patterns and not with distributions of the climate variables. This aspect of Bryson’s definition goes against how one intuitively thinks about climate, where climate is some kind of distribution of the temperature, the surface pressure, etc.

POINT 5

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.

The use is just illustrative (I will make this clearer in the revised version of the manuscript). There is a tradition in dynamical systems theory and climate science that studies very simple models to gain insights into the general behaviour of systems (this approach is regarded as useful when working with true models is out of reach and is pursued, e.g., by Lorenz 1964, 1970 and Smith 2002). My ESD discussion paper is in this tradition: It is not possible to do simulations with the true climate model, and hence a simple model is studied (simulations with a logistic map-type equation).

POINT 6

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 behaviour 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.

About the scale-dependent behaviour of climatic variables, see my reply to Points 2 and 3 above. About whether memory of initial values washes out over time: I can see that my discussion here was too brief. First (empirical consideration): I would add that studies of climate models raise doubts that the memory of initial values washes out over the time periods of interest (Lorenz, 1976; McGuffie and Henderson-Sellars, 2005; Schneider and Dickinson, 2000). Second (physical consideration), I would add that the study of certain physical processes provides evidence that the memory of initial values does not wash out over the time periods of interest. Most importantly, the initial values of the ocean does not seem to was out over the time periods of interest, i.e., the temperature of the ocean exerts a long term influence which is much longer than the standard prediction lead times of interest (e.g. Weaver et al. 2000).

POINT 7:
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.

I do not think that there is necessarily a unique definition of climate (i.e. a definition that is supposed to hold in all circumstances). I think that, in principle, there can be several definitions of climate that might be useful for different purposes (some climate scientists think that there is one unique definition of climate; others like Lorenz (unpublished) are happy to work with several different definitions if they prove to be useful). Still, I think that all these definitions have to fulfil certain requirements for them to be useful. For instance, a requirement is that the definition provides the means to conceptualise the future as well as the past and the present climate (only then can we define climate change as well).

POINT 8:
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?*

In the revised version of the manuscript I would aim to get rid of the repetition as much as possible (there is certainly scope for this) and to refer to results with citations (if these results are not needed for the discussion that follows).

POINT 9:
It would be helpful if the author said a bit more about how the present paper goes beyond Werndl 2014.
and
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.

About the difference between this this paper (the ESD discussion paper) and my other paper (Werndl 2014). I deliberately chose to write two different papers because the content as well as the target audience differ. If I am invited to revise the manuscript, I would add a few paragraphs explaining the differences. More specifically:
– The ESD discussion paper presents a comparison between (i) definitions of climate under constant external conditions (and autonomous dynamical systems theory) and (ii) definitions of climate under varying external conditions (and non-autonomous dynamical systems theory). Thus the Sections 3.4 "Constant Versus Varying External Conditions: Differences, Similarities and Assessment" and 4.3 "Constant Versus Varying External Conditions: Differences, Similarities and Assessment" are the crucial sections of the ESD paper. This is also reflected in the title of the ESD discussion

paper “Definitions of Climate and Climate Change Under Varying External Conditions”. For instance, it is pointed out that for constant external conditions there are results about the independence of distributions over time from the initial conditions whereas for varying external conditions no such results are known. This is in contrast to Werndl (2014), which does not present a comparison between (i) and (ii).
– The simulations in the ESD discussion paper on the logistic map are entirely new (they are not in Werndl, 2014). The logistic map has been repeatedly used as a toy model of the climate system and so should be of interest (e.g., Lorenz 1964, Smith 2007).
– Werndl (2014) is first and foremost a conceptual paper with as little technical detail as possible. For instance, the definitions of climate are only introduced informally with examples, no general mathematical definitions are given at all (the appendix contains some technical discussion, but this is extremely brief and will only be understood by those who know the mathematical framework well). In contrast to this, the ESD discussion paper is a mathematical treatment of climate: general mathematical definitions are given and (known) results of dynamical systems theory are applied to analyse them.
– Also, the target audiences are different. Werndl (2014) is aimed at philosophers while the ESD discussion paper is aimed at climate scientists who would like to know more about the difference between definitions of climate under constant and varying external conditions and about how to mathematically characterise climate.
– Finally, as the referee also highlights, in response to this referee report I would make several substantial changes that would be altogether absent from Werndl (2014).

POINT 10: *The citation Mancho et al. 2013 on p. 692 should be, according to the bibliography, Mancho et al. 2014.*
I will change this accordingly.

‘vary’ should be ‘wary’ on p. 696.
I will change this accordingly.

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