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Earth System Dynamics Discussions

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

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**RESPONSE TO REFEREE 2**

First of all, I would like to thank the referee for the criticism, which helps to improve the paper.

POINT 1
*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:*

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 contain a comparison between (i) and (ii).
– The simulations in the ESD discussion paper on the logistic map are entirely new (Werndl, 2014, does not contain them). The logistic map has been repeatedly investigated 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 (e.g., the definitions of climate are only introduced informally with examples, no general mathematical definitions are given). 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.
– Finally: 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 define climate.

POINT 2

*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.*

I am a bit puzzled by this very strong comment, which seems to suggest that discussing definitions of climate is "brave and hopeless". The referee's claim seems to imply that similar work, e.g. Bryson (1997), Lorenz (unpublished), and Lovejoy et al.'s (2013) discussion of definitions of climate, is equally "brave and hopeless" (Lorenz, unpublished, also work with a toy model "without even touching any climate data"). Why is one not allowed to discuss this topic? The referee seems to suggest that the problem does not allow "a general consensus among experts", and, for this reason, it is a hopeless task. But it is common practice in science to discuss controversial topics (where there is no general consensus) and discussing such topics can advance the field. Also, I should mention that my ESD discussion paper is modest in the sense that it just (i) compares definitions of climate as distributions over time under constant and varying external conditions and asks which are most promising, (ii) compares ensemble definitions under constant and varying external conditions and asks which are most promising. A comparison between definitions of climate under constant and varying external conditions is a specific task, where clear and specific arguments can be given and hence does not seem "hopeless". Also, the paper is open to the idea that there can be several fruitful definitions of climate (if several definitions could be found that are promising).



POINT 3

*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.*

I think it is important to distinguish between feelings (which cannot be argued for) and claims which are substantiated by arguments. A part of standard scientific activity is to argue (to substantiate claims by good arguments) and this is what my ESD discussion paper is doing. It is not just presenting certain feelings, but definitions are assessed by providing clear and specific arguments (substantiated by simulations).

POINT 4

*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.*

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). 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). If the referee thinks that this is a crucial drawback that disqualifies the paper from being published, then myriads of other papers (e.g., Lorenz 1964, 1970, Smith 2002) should have never been published. I do not think that the referee's dismissal of such papers is justified because there is a lot that can be learned about the general behaviour of systems from papers such as Lorenz (1964, 1970) and Smith (2002).

POINT 5

*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.*

I agree with the referee that it is a key question for Definition T3 how to define a regime of varying external conditions and that it is good to say more about this. If I am given the chance to revise the manuscript, I would add that Lovejoy's and Schertzer's (unpublished) ideas can be utilized to address the question what a regime of varying external conditions amounts to. 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-independent (as required by Lovejoy and Schertzer). However, I still think that Lovejoy and Schertzers 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). Please see also my reply to Point 3 of the first referee.

POINT 6

*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.*

First of all, it is important to stress while the arguments are illustrated with the simulations, it is the arguments (not just the simulations) that are meant to show that Definition T3 is superior to the other definitions. Second, and more importantly, the referee just claims that something is convincing without explaining why this is not convincing. In order to reply to this point properly, I would have to know why the referee thinks that: "the exercises with the logistic system do not convincingly demonstrate that the definition T3 resolves the problems of climate definition". For instance, if what is meant is that there could be other useful definitions next to Definition T3, then I would agree (see the reply POINT 2 above).

POINT 7

*The choice of a model also calls for a better justification.*

There are several reason why I chose this model were (I would be happy to explain this in more detail):

– Lorenz (1964) investigated this as the simplest possible model "capable of producing a stable climate". I take it that it is interesting to investigate how different definitions of climate fare "for the simplest possible model capable of producing a stable climate".

– The logistic map is one of the most thoroughly investigated toy models in dynamical systems theory and it is better understood than most other systems. Presenting simulations about a well-understood system that many scientists know seems to be a good idea because it will be easier for scientists to understand and interpret these results and to establish connections to other results.

– It should also be stressed that several researchers have used the logistic map as a toy model of the climate (e.g. Lorenz 1964, Frigg et al. 2014).

POINT 8

*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.*

The potential readership consists of:
– Climate scientists who would like to know more about how define climate mathematically with help of dynamical systems theory and the role of constant versus varying external conditions.
– Climate scientists and mathematicians who are interested in conceptual arguments (the benefits and problems of certain definitions). I do not agree that mathematicians will not be interested in this paper: this paper provides a conceptual discussion of the merits and problems of various definitions of climate, and this is not something mathematicians usually engage with.

My intention was to write a paper which can be read with only basic knowledge about dynamical systems theory (as is the case for most climate scientists – the paper is not primarily aimed at mathematicians). For this reason, the paper also assumes only basic knowledge about dynamical systems theory apart from (as the referee rightly remarks) the discussion of Axiom A systems. I am happy to remove the paragraphs about Axiom A systems. I just added them because a previous referee asked me to add a few paragraphs about these systems. About the referee's concern that the paper is too abstract: please see my reply to POINT 4 above.



POINT 9

*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 mentioned above (POINT 8), I included the material on Axiom A systems, wandering sets, and non-reversibility because a previous referee asked for it (to have concrete mathematical examples). I can also remove this material if the referees prefer this. In any case, these are issues that can be resolved. I can provide a more concise explanation of non-reversibility and a clear and correct definition of a wandering set.

POINT 10

. . . *as well as numerous almost-repeating equations that never go beyond the level of basic definitions and hence may be safely skipped.*

As explained in POINT 8 above, the potential readership consists of climate scientists who would like to know more about how define climate mathematically and climate scientists and mathematicians who are interested in conceptual arguments. From experience I can say that many climate scientists need the "basic definitions" to understand how to mathematically define climate and hence I think it is better not to skip them.

POINT 11

*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.*

All that is meant here is that in simulations time is discrete and hence one deals with discrete-time systems. (So what is meant here is quite specific and this is not something which is too general to be meaningfully assessed). I am happy to rephrase this to avoid any potential confusion.

POINT 12

*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.*

This seems to be just an issue about words. I meant by "analyse" nothing more than that the mathematical framework of non-autonomous dynamical systems theory is used to examine the properties of certain definitions of climate. I did not mean to suggest with "analyse" that novel mathematical results are presented. I am happy to rephrase this to avoid any potential confusion.

POINT 13

*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.*

As explained above (POINT 8), the potential readership consists of: climate scientists



who would like to know more about how define climate mathematically and climate scientists and mathematicians who are interested in conceptual arguments. As stated above (reply to POINT 8), I am happy to soften the jargon to make the paper more accessible to those with little background in dynamical systems theory. Also, as explained above (reply to POINT 4), it is not possible to do simulations with the true climate model, and hence a simple model is studied (a logistic map-type equation) to obtain general insights into the behaviour of dynamical systems (and there is an entire research tradition that engages in this kind of research going back to Lorenz etc).

About the research criteria for ESD: It is stated on the ESD web page under "Aims and Scope" that: "The overall behaviour of the Earth system is strongly shaped by the interactions among its various component systems, such as the atmosphere, cryosphere, hydrosphere, oceans, pedosphere, lithosphere, and the inner Earth, but also by life and human activity. ESD solicits contributions that investigate these various interactions and the underlying mechanisms, ways how these can be *conceptualized*, modelled, and quantified, predictions of the overall system behavior to global changes, and the impacts for its habitability, humanity, and future Earth system management by human decision making [emphasis added".
So the aims and scope of the journal include also conceptual discussions of important concepts in climate science ("how these can be conceptualized"). I take it that "climate" is an important concept. Hence a conceptual discussion of definitions of climate for constant and varying external conditions and a discussion of how a mathematical framework (non-autonomous dynamical systems theory) can be used to mathematically define climate falls within the scope of ESD.

REFERENCES

Bryson, R.A. The Paradigm of Climatology: An Essay. Bulletin of the American Meteorological Society. Vol. 78, No. 3, March 1997.

Frigg, R., Bradley, R. Du, H. and Smith, L.: Laplace's demon and the adventures of his apprentices. Philosophy of Science 81, 31-59, 2014.

Lorenz, E.: The problem of deducing the climate from the governing equations. Tellus, 16 (1), 763, 1-11, 1964.

Lorenz, E.: Climatic change as a mathematical problem. Journal of Applied Meteorology, 9 (3), 325-329, 1970.

Lorenz, E. Climate is what you expect. Prepared for publication by NCAR, 1-33, Unpublished.

Lovejoy, S., Schertzer, D. and Varon, D. Do GCMs predicte the climate ... or macroweather? Earth Systems Dynamics, 4, 439–454, 2013.

Lovejoy, S. and Schertzer, D. climate Is not what you expect. Unpublished.

Smith, L. What might we learn from climate forecasts. Proceedings of the National Academy of Sciences of the United State of America 99 (1), 2487-2492, 2002.

Smith, L. Chaos: a very short introduction. Oxford: Oxford University Press, 2007.



Werndl, C.: On defining climate and climate change. Forthcoming: The British Journal for the Philosophy of Science, forthcoming 2014.

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