Reply to RC2 - egusphere-2023-216

[The reviewer's input is in italic font, while our responses are in regular font.]

It has been a great opportunity for me to read this review article. The two authors have made a great attempt to bring together – to my knowledge for the first time in a kind of systematic manner – the recent developments regarding applications of the two commonly rather disparate fields of dynamical system theory and algebraic topology in the context of climatology. Both authors are well known for their enormous work on both topics over the last decades, so it is not surprising that the resulting manuscript presents great educational material on both topics.

Thank you very much for this supportive and encouraging review.

There is practically only one, very minor general comment that I may raise regarding this impressive work, and I need to say that this is a very subjective one based on my own knowledge of both fields, which is far from complete. Eventually, this paper may benefit (although I am not sure if that would indeed make up for an improvement) if the authors could highlight the specific potentials for further integration of both mathematical "disciplines" for future climate (or climate-related) studies even a bit more prominently. There might be a kind of "grey zone" between a review and perspectives paper. The way the material is presented is maybe not a classical review in the sense of attempting a complete coverage of the addressed field(s), which I feel absolutely comfortable with when reading this work. In such situation, I may envision this manuscript to become even more impactful when outlining somewhat more transparently the authors' perspective on a possible future research agenda, or at least parts of it. Please take this just as a suggestion, not a definite request of mine.

We agree that there is a grey zone between a review and perspectives paper, and we will add a few paragraphs to improve the paper in its addressing future perspectives.

All other comments I may have regarding certain parts of the manuscript are rather specific and/or technical and listed below:

Specific comments/suggestions

- p.3, ll.68-71: The authors essentially mention codimension-1 bifurcations (is "transverse" the same as "transcritical", the term that I am familiar with?); it might be useful to mention (here or later) the existence (and possible relevance for climate problems?) of bifurcations of codimension 2 or higher.

Thank you for catching this typo. In fact, "transcritical" is a particular kind of codimension-1 bifurcation, in which a single solution branch preserves its existence but changes its stability across the bifurcation point: stable on one side of it and unstable on the other. "Transverse" refers to a particular condition for the intersection of two manifolds. Actually, we meant "transcritical" and will correct this typo, inherited from the Ghil et al (1991) paper.

Thank you also for suggesting that we mention the existence and relevance of codimension-2 bifurcations — such as Shilnikov, which is actually mentioned later, in Sec. 3.1, or Bogdanov-Takens. We will add one or two paragraphs on this important matter in the revised text.

- In general, Section 1.1 could benefit from a few more "tutorial" references on dynamical system theory. The choice of references in this initial overview appears partially a bit "Ghil-centric", which is fine for the more specific discussions in Section 2 focusing on contributions of the first author.

Good point, thank you. There is a rich literature on autonomous dynamical systems, in both mathematical and physical texts, although less so in the climate sciences. The main references in the latter field of applications, due to Henk Dijkstra aside from Ghil, have already been mentioned, as has a considerable amount of NDS and RDS literature in Secs. 2 and 4. But we will be happy to help the reader even more in citing additional good references.

- p.4, ll.94-95: "This complementary view of the way that dynamics and topology interact is a main motivation of the present article." I fundamentally agree with the authors' emphasis on this point. There is a lot of algebraic topology tools in classical as well as modern dynamical system theory. One recent field that seems to provide another link between the two topics, which has also found vast applications in climate science in recent years, would be complex networks. I would leave it to the authors' choice whether or not to elaborate a bit (maybe one brief paragraph in the end) on corresponding recent developments and their potentials. The authors mention this very briefly on p.43, ll. 993-995, which emphasis on a rather specific problem, but I think there might be more to that.

We acknowledge the impressive work being pursued in complex networks and their relevance in time series analysis [Zou et al, Physics Reports, Volume 787, 2019, Pages 1-97]. Algebraic topology is not mentioned in this review, but there have been some papers applying persistent homologies to complex networks [Horak et al J. Stat. Mech. (2009) P03034; Petri et al (2013). PloS one, 8(6), e66506; De Silva & Ghrist (2007). Algebraic & Geometric Topology, 7(1), 339-358].

Horak et al. construct simplicial complexes from graphs (networks) to evaluate the robustness of a network and to distinguish different network types. Petri et al. use persistent homologies to detect particular nonlocal structures, akin to weighted holes within the link-weight network fabric. De Silva & Ghrist propose a method using persistent homologies for nonlocalized sensor networks with ad hoc wireless communications.

Despite the rapid development of computational topology and data science, the triple combination between algebraic topology, time series analysis and complex networks seems to be untouched so far. The network approach is used, however, to reconstruct the phase space, which is a prior and certainly necessary step for the analysis of the topological structure of flows from data.

The prospective directions in the field of complex networks enumerated by Zou et al [2019] share many of the challenges that are also faced by the topology of chaos. We are grateful to the reviewer for having directed our attention to this point and will expand upon it in the revision.

- Somewhat related to the previous point, I might also suggest mentioning the framework of persistent homology and its potential applications, maybe even at the end of the introduction section (it appears only be briefly mentioned on p.24, l.559, before quickly focusing on the BraMAH methodology in the following).

We will follow the reviewer's suggestion in this as well.

- Figure 1: Since it seems not to be further explained, I would find it helpful (for non-experts) if the term "isopleth" could be briefly explained.

The etymology of "isopleth" combines "iso" plus the ancient Greek word plêthos, "a great number". It is generically used to refer to a curve of points sharing the same value of some quantity. In his 1963 paper, Edward N. Lorenz plots the isopleths (isolines) of X as a function of Y and Z of his attractor, to approximate surfaces formed by all points on limiting trajectories. We will add this explanation to our manuscript.

- p.8, ll.194-195: I recommend adding a brief clarification that any bifurcation presents a kind of tipping ("B-tipping" in the language nowadays used by many authors, probably going back to Ditlivsen and co-workers?), while not every tipping behaviour in a complex system originates from an underlying bifurcation.

There are at least two different interpretations of "tipping" and "tipping points" in the literature. One of these, emanating from Gladwell (2000) and Lenton et al (2008), interprets tipping merely as a sudden change, whether due to a well-defined bifurcation or not. In this interpretation, a tipping point is merely a threshold.

The other interpretation sees a tipping point as a generalization to nonautonomous systems of a bifurcation point (Ghil, 2019; Kuehn, C., 2011. A mathematical framework for critical transitions: Bifurcations, fast–slow systems and stochastic dynamics. Physica D, 240(12), 1020–1035). In this case, tipping is necessarily related to a tipping point in phase-parameter space and not every jump or critical transition arises from a such a point.

We will clarify this in the revised version since both points of view have their merits, but confusion should be avoided to the extent possible.

- p.11, ll.272-273: In fact, the orbital cycles emerge from chaotic motion, but have contributions with relatively narrow (yet not exactly fixed) frequency and strongly varying amplitudes. So in reality, one would not assume that 19 kyr and 41 kyr variability components are exactly fixed (and, hence, would not have a simple integer ratio), but may lead to more complex dynamical phase locking-unlocking processes. This is far beyond the scope of the present work, but maybe the specific sentence here could be a bit reshaped to clarify what the authors actually attempt to focus on.

We are not exactly sure whether the reviewer refers to work on the presence of chaos in the planetary system or not (e.g., Varadi, F., M. Ghil, and W. M. Kaula, 1999: Jupiter, Saturn and the edge of chaos, Icarus, 139, 286–294). We will try to clarify this point further, too.

- In Section 2.1, state vectors are denoted in bold face. In Section 2.2, however, vectorial quantities are not written in bold face anymore (e.g. x in Eq. (9), l.307 and following). I strongly suggest revising the appearance of mathematical terms for self-consistency between the different (sub)sections.

Thank you for this correction. We will use boldface vectors throughout.

- p.13, l.320: Please clarify that this refers to chaotic trajectories in the Lagrangian chaos sense, not chaos of the underlying field g(t,x) itself.

Will be clarified, thank you.

- p.13, l.325: Please explain the term "pullback attraction" in a few lay words.

Good idea, thank you. We will use simple language, like "A pullback attractor is a possibly time-dependent object in a system's phase space that exhibits attraction in the sense of convergence at each time t to a set, called a snapshot, to which the system's initial state at time s tends to as s tends to $-\infty$. This is distinct from the forward attractors that can be defined for autonomous systems started at a fixed time t_0."

- p.15, l.375: There is no β in Eq. (16), only σ. Is that one meant here?

Yes, thank you for the correction.

- Section 2.2.3: The symbol ω used here has been previously used for a frequency in Section 2.2.2. Please consider using a different symbol.

Thank you for noticing. We will use two different symbols in Secs. 2.2.2 and 2.2.3 for a frequency and for a realization of the driving noise, respectively.

- p.18, ll.434-437: Could you add a brief note on suitable "mathematical" types of noise distributions?

We will define more precisely the connection between Brownian motion $\mathrm{d}\eta$ and a Wiener process W, including a reference.

- p.20, Fig. 8: The figure caption refers to a color bar that is missing in the figure.

We will explain the color bar in words in the caption.

- p.23, l.517: The term “braids” might be unfamiliar to many readers, so a brief explanation might be helpful. Similar for p.29, l.671 (“templexes”, only introduced in the following subsection) and p.31, l.714 (“knot-holder”).

We can imagine a knot as a thin tangled rope in three-dimensional space whose ends are glued together [Prasolov et al. Knots, links, braids and 3-manifolds: an introduction to the new invariants in low-dimensional topology. No. 154. Amer. Math. Soc., 1997.], while a braid is a collection of strands crossing over or under each other. Both concepts became central around 1987 in the attempt to classify low dimensional (3-D) systems using topological orbit organization.

The knot approach — i.e., extracting the knot content of hyperbolic attractors — is rooted in results from Birman-Williams-Holmes, through a geometrical construction that was named template or knot-holder. The braid approach is based on results due to Thurston on the classification of 2-D diffeomorphisms and the braid content of the diffeomorphism [Natiello, M. A., 2007. The User's Approach to Topological Methods in 3d Dynamical Systems. World Scientific].

- p.41, ll.918-926: The discussion on “wave-like” vs. “particle-like” behaviour (drawing upon a quantum mechanical analogy) reminds me a bit of the traffic jam analogy of atmospheric blocking situations by Nakamura and Huang (Science, 361 (6397), 42-47, 2018). I would wonder if the authors would see some link to this very active field of studies in climate science (persistent atmospheric wave trains, blocking, and extreme events) from their more fundamental “mathematical” (conceptual) perspectives.

Well, the authors are familiar with the very interesting Nakamura and Huang (2018) paper and one of them is working in a separate collaboration on applying extremal length theory (Ahlfors, L., 1973. Conformal Invariants: Topics in Geometric Function Theory, American Mathematical Society) to midlatitude flow diagnostics. But the paper at hand is getting quite long and persistent anomalies have been a “very active field of studies in climate science” for over four decades, so adding yet another approach to Fig. 22 does not seem imperative at this stage. Thank you, though, for finding inspiration in our quantum mechanical analogy.

Technical suggestions:

- p.1, l.3: "in the 1960s"?

Yes, at least in the U.S. spelling (Chicago Manual of Style), which is used throughout this paper, that is correct, and not the more common "1960's." After the first mention, the '60s and '70s are correct, too.

- p.4, l.110: typo "current"
- p.6, l.141: typo "the way"
- p.6, l.142: citation style of Williams (1974) should be adjusted

Right, thanks.

- p.6, l.150: "in the 1990s"? (this would be consistent with l.3 and others. . .)
- p.6, l.160: "in the 1960s and early 1970s"
- p.8, ll.188 and 190: I suggest replacing "in the next subsection" by "in Section 2.1.1"
- p.8, l.211: It might be too much of a request, but citing some original work by Maxwell might be a quite unique thing for a paper in this journal.

Given the early Poincaré references, adding James Clark Maxwell or Pierre Curie would seem quite appropriate. Thank you for sharing our taste for early citations.

- p.9, l.212; p.10, l.220; p.11, ll.258-259: citation style of Ghil and Childress (1987) should be adjusted

Thanks for noticing: this will be done, of course.

- p.11, l.257: replace "in the above figure" by "in Fig. 4"

Thank you, will be done.

- p.11, l.262: "periodicity of glacial cycles"

Not sure what exactly this refers to: "glacial cycles" suggest a unique periodicity, "climatic variability" does not. We beg to differ.

- p.13, l.319: the condition should read "$t_0 \leq t_1 \leq t_2$" (subscript 1 is missing)

Correct, thanks for noticing; will be fixed.

- p.13, Definition B.1 (why are definitions enumerated as B.1, B.2, etc.?): You introduce the set $\mathbb{R}^2_+$, but refer to $\mathbb{R}^2_\geq$ in the definition of the mappings.

The B's will be removed; thanks for noticing their being redundant herein. The difference of notation for the nonnegative real numbers is a typo and will be fixed.

- p.14, l.341: "translation in time"
- p.14, l.344: I would start the sentence with "As an example, analytical computations. . . "
- p.14, l.353: I am not sure if the abbreviation PBA had been defined before; I would suggest to avoid it.
- p.15, Eq. (17): use large brackets
- p.16, Eqs. (22) and (23) (and also a bit of the text in the remainder of Section 2.2.2 mixes up the symbols ϕ and φ. Please keep consistency.
- p.22, l.497: replace "While. . . " with "By contrast. . . " or something similar
- p.27, ll.638-639: Why is the set of ODEs (26a,26b) infinite?
- p.28, l.649: The solution of the Navier-Stokes equations would be a velocity field, not a streamfunction. Better write: ". . . the streamfunction. . . would not correspond to a solution. . . "
- p.32, l.725: "distinct chaotic attractors"
- p.38, l.852: type "known"
- p.40, l.886: In my understanding, Betti numbers are integers, so any change in this property must be necessarily discontinuous. Or do the authors want to emphasize something different here?

Yes, they are integers: the phrase "can be quite sudden" will be changed to "is quite sudden, since they are integers."

- p.42, l.943: "Pacific North America (PNA) pattern"
- p.43, l.964: omit abbreviation "TDA"
- p.43, l.971: ". . . and provided further. . . "
- p.45, l.1038: volume missing in Carlsson & Zomorodian (2007)
- p.46, l.1060: remove "20 pp."
- p.47, ll.1081-1082: this seems to be a duplicate reference
- p.48, l.1123: remove "41 pages"
- p.48, ll.1132-1133: if this is a book chapter, add page numbers; otherwise if this is the full book title, adjust citation style accordingly
- p.49, l.1168: there is something odd with the style of this citation, please check/correct

There is a blank space missing between “Basis.” and “Contribution”; will be fixed.

- p.51, ll.1232 and 1262: please give full (non-abbreviated) journal names
- p.52, l.1295: update reference with volume and page numbers, or provide doi if still only “online first”
- p.52, ll.1204-1306: this seems to be another duplicate reference
- p.53, l.1307: replace page numbers by proper article ID
- p.53, l.1327: volume missing

All the items that are not specifically addressed in our replies will be taken care of as well. Thank you for your extremely careful reading and very constructive comments.