Reply to 'Comment on egusphere-2023-216', RC1
(In each case, the reviewer's input is in black, our responses are in bold/blue.)

In this very interesting and stimulating review, the authors provide the readers with an introduction to how algebraic topology can cast insight into the behavior of dynamical systems, after recalling how dynamical system theory is very relevant to geophysical models in general and to climate models in particular (it is quite telling that Henri Poincaré took the example of weather as an example of chaos).

Thank you very much for this insightful and supportive review.

The algebraic topology approach outlined in the review is itself based on a topological approach whose defining concepts (e.g., branched manifolds) were laid out by Birman and Williams and whose application to the natural sciences was pioneered by Gilmore and co-workers. The exposition follows a historical perspective where the identification of branched manifolds through cell complexes and homologies characterizing these complexes is first recalled before presenting the latest developments in the fields (templexes and stripexes) which not only take into account the skeleton on the attractor, but how the flow explores it by inferring the underlying semi-flow. Fundamental nonlinear phenomena such as bistability, appearance of self-sustained oscillations through a Hopf bifurcation are clearly explained.

An interesting feature of the review is how it integrates different aspects very relevant to geophysical and climate applications: noise, variability due to non stationarity, eulerian vs lagrangian description which will make the review very useful for the readers of Nonlinear Processes in Geophysics. How these aspects increase the complexity of characterizing geophysical systems is well illustrated. The paper is quite successful in convincing the readers that the approaches advocated have very promising perspectives to tackle the challenges presented by of geophysical problems.

I found the perspective section very rich and interesting.

Thus, I strongly recommend this manuscript for publication in NPG, given that the following minor remarks are taken into account by the authors.

Your strong recommendation is much appreciated, and the authors will do their very best to take into account your remarks and thoughtful suggestions.

(1) In the introduction, it would be nice to shortly discuss a geophysical model of interest to make the discussion more illustrative.

(1) Unfortunately, there is no canonical geophysical model on which all the concepts and methods discussed in this review could be illustrated. But we will give separate examples for all the major ones.

(2) Around Figure 1 and line 135, the authors allude to the stretching, squeezing, folding etc. mechanisms that build a chaotic attractor, but in my opinion, they do not provide sufficient information for an uninformed reader to grasp what these mechanisms are nor do they explain how and why the topological approach is an elegant and natural way to capture these mechanisms.

(3) They introduced branched manifolds without describing them very much nor giving a precise definition of them. Recall that branched manifolds are obtained by identifying points along a given segment of the stable manifold, so that it is a kind of projection. How many stable directions must be taken into account will matter very much, in particular. This is important to understand what is the semi-flow that the authors invoke.

Answers to (2) and (3)

In « How topology came to chaos », Gilmore explains that metric and dynamical invariants do not provide a way to distinguish among the different types of chaotic attractors and that a tool of a different nature was needed to create a dictionary of processes and mechanisms underlying a chaotic system.

"Listening more closely to Poincaré, it was clear that this new tool ought to involve the periodic orbits 'in' a chaotic attractor. A chaotic trajectory winds around in phase space arbitrarily close to any unstable periodic orbit, so it ought to be possible to use segments of a chaotic trajectory as good approximations (surrogates) for UPOs. [...] It was clear that UPOs could also serve as the skeleton of the strange attractor."

While Gilmore, Lefranc and co-workers were "mulling over implementing a program based on building tables of linking numbers and/or relative rotation rates between trajectories, a better solution became available. Joan Birman and Robert Williams had shown that the dissipative nature of a flow in phase space allows projecting the points along the direction of the stable manifold by identifying all the points with the same future."

"Suppose we have a dissipative chaotic flow in three dimensions: there are three Lyapunov exponents ($\lambda_1 > 0$ for the unstable direction, $\lambda_2 = 0$ for the flow direction and $\lambda_3 < 0$ for the stable direction). The dissipative nature of the flow requires $\lambda_1 + \lambda_2 + \lambda_3 = 0$. Then it is possible to project points in the phase space down in the direction of the stable manifold. This is done by identifying all the points with the same future:

$$x \simeq y \text{ if } \lim_{t->+\infty} |x(t) - y(t)| = 0$$

where *x(t)* is the future in phase space of the point *x* = *x(0)* under the flow. This Birman-Williams identification effectively projects the flow down to a manifold almost everywhere, except at the points where the flow splits into branches heading towards distinct parts of phase space, or at the points where two branches are squeezed together. These mathematical structures were called branched manifolds."

A branched manifold can in fact be defined mathematically without reference to a flow, or to the Birman-Williams projection mentioned above.

Definition (from Kinsey page 64). An n-dimensional manifold is a topological space such that every point has a neighborhood topologically equivalent to an n-dimensional open disc with center x and radius r. Such a manifold is said to be Hausdorff iff any two distinct points have disjoint neighborhoods.

The second condition is not satisfied precisely at the junction between branches, i.e., at the locations that describe stretching and squeezing of a flow in phase space. A branched manifold is therefore a manifold that is not required to fulfill the Hausdorff property.

We prefer this more general definition, instead of the one related to the Birman-Williams projection, for several reasons, including the possibility of extending the concept of branched manifold to the structure of instantaneous snapshots of random attractors. This mathematical definition of a branched manifold will also let us extend the procedure to cases in which the hypotheses of the Birman-Williams theorem – in which the dynamical system must be hyperbolic, three-dimensional, and dissipative – are not valid. In most geoscientific applications, for instance, uniform hyperbolicity does not apply.

As the topological structure of a branched manifold is closely related to the stretching and squeezing mechanisms that constitute the fingerprint of a certain chaotic attractor, its properties can be used to distinguish among different attractors. This is how the two-way correspondence between topology and dynamics can be justified. This correspondence remains valid in the case of four-dimensional semi-conservative systems [Charó et al, 2019; Charó et al, JFM, 2021], for which the hypotheses of the Birman-Williams theorem do not hold.

The terms "branched manifold" and "template" have often been used interchangeably. We do not consider them as synonyms, for technical reasons that will be important in the development of the concept of templex. A branched manifold is just a particular type of manifold that can be reconstructed from a set of points in R^n, by approximating subsets of points by cells, which are glued to form a cell complex. The dimension of the cell complex d coincides, by construction, with the local dimension of the branched manifold approximating the point cloud, but there is no restriction in the value of n or of d. Both values are computed directly from the dataset, using successive singular value decompositions. The number of eigenvalues scaling linearly with the number of points grouped in a cell provide the value of d for that cell, and this computation is done on matrices that contain the n coordinates of the points, without performing projections of any kind. These computations construct a cell complex from the point cloud without involving the flow. The information carried by the flow is not contained in the cell complex but will be contained in the digraph of the templex.

We will incorporate these clarifications into the text of our paper.

(4) It is not entirely correct to write that systems whose branched manifolds are topological equivalent are dynamically equivalent. Their orbit content, or the associated symbolic dynamics could differ. But it is true that they cannot be equivalent if the branched manifolds differ.

(4) In page 175 of "How topology came to chaos", we read:

"Branched manifolds are useful constructions for distinguishing among different mechanisms that generate strange attractors. Topological equivalence between branched manifolds is by isotopy. Two things are isotopic if it is possible to mold one into the other without tearing or gluing it. As a result, identifying the branched manifold that describes a strange attractor is a powerful tool for distinguishing one (class of) strange attractors from the other."

It is in this sense that we speak of dynamical equivalence. The metric or dynamical invariants describing the orbit content are not being considered. We will clarify this in the text.

(5) In definition B.1, is R^2_>= or R^2_+ ?

(5) Thank you for noticing the typo in the second line of the definition. Both notations are used but they should not be mixed up. The single $\mathbb{R}^2_{\geq}$ in the definition will be replaced by $\mathbb{R}^2_{+}$.

(6) I find the discussion about pull-back attractors (please define PBA when it first appears!) interesting however I missed the motivation for introducing the concept, which became clearer later in the text. Please explain from the beginning why the concept is interesting and useful. It seems to me that it is particularly relevant for noisy systems, am I right?

(6) Good point. We will pay even more attention to motivation, although several concepts are introduced to a broader audience and the best sequence in which to do this is not always obvious.

(7) Line 375, beta or sigma?

(7) It should be sigma and it will be corrected!

(8) I would distinguish between nonautonomous systems and non-stationary systems. The former can have some sort of regularity (e.g., periodic driving). The latter could experience any type of slow drift. It seems to me that the authors switch from one type of system to the other without prior notice.

(8) The definition of nonautonomous system that we are using is unambiguous and stated as the explicit appearance of time in the mathematical expression of the governing equations of the system under consideration. Stationarity is a property of solutions, not of the system of equations. An autonomous system can have stationary, periodic or chaotic solutions. A nonautonomous system cannot, as far as we can tell, have stationary solutions, unless the forcing tends to zero.

(9) There is a strong link between topological chaos as studied by Thiffeault and Gouillart and what the authors call "topology of chaos". In a fluid, you have a flow taking particles from points in space to other points, and the same occurs in phase space. Chaos is due to mixing processes in the two situations.

(9) There is a strong link between the two situations, but the keywords refer to different motivations and objectives. Working with the topology of real fluid flow trajectories in physical space implies working in no more than three dimensions, for example. On the other hand, investigating fluid flows in phase space and in physical space is not equivalent. Physical space for fluid flows is most often a plane projection of a higher-dimensional state space.

We will clarify this in the text.

(10) It is not entirely true that periodic orbits approximate actual trajectories in the topological approach. Rather, it estimates the neighboring UPO which the flow is evolving around. That is the opposite perspective (l. 508).

(10) In the methodology that approximates trajectories with knots, trajectories must be closed into a knot that approximates the neighboring UPO around which the flow is evolving.

We will modify the sentence.

(11) l. 660 I do not quite understand what the problem with the standard approach of considering time as a state variable for non-autonomous systems, I feel that the authors should elaborate.

(11) This discussion is central to the Charó et al. [2019] paper. When a dynamical system is governed by a set of equations where the time explicitly occurs, some processes involved in the dynamics are not explicitly described and the state space is not completely determined. In fact, one of the fundamental hypotheses in writing a dynamical system as a set of ODEs is that time is the only independent variable, while

all state variables are time dependent. Making time play a double role – that of the only independent variable and that of a state variable – is misleading.

Working in a space whose dimension is increased by one due to introducing the extra ODE $\dot{t} = 1$ leads to certain difficulties in using the tools borrowed from nonlinear dynamical systems theory—for instance, the state space is no longer bounded. When time t is added as an extra coordinate to the phase space, we get an 'extended phase space'. In this extended phase space, a periodic orbit is no longer a closed curve, simply because when the system returns to the same state, it does not return to the same point. The very definition of phase space in which a point univocally represents a state of the system is no longer valid in the extended phase space. Many of the properties that are valid in a well-defined phase space are altered in an extended phase space, and topology is one of them.

In the case of the driven double gyre discussed by Charó et al. [2019], the starting point is a nonautonomous system of two ODEs. The extended phase space (with a third ODE written as $\dot{t} = 1$) is three-dimensional. But the paper shows that a fourth dimension is needed to rewrite the system as an autonomous set of ODEs without using the trick $\dot{t} = 1$. The phase space of the autonomized driven double gyre has four ODEs: two additional variables are required, *u* and *v*. Such a transformation gets rid of the explicit time dependence with a legitimate procedure that does not run into the previously explained inconsistency. In this four-dimensional phase space, and for certain initial conditions, the topological structure that is obtained is a Klein bottle. A Klein bottle cannot be immersed in three dimensions without self-intersections: the role of the fourth dimension that is required to rewrite the system in an autonomous form is, therefore, highly relevant here.

Thus, to use topological tools self-consistently, one must be prepared to work in a well-defined phase space, and with as many dimensions as required.

(12) The authors should mention other approaches to characterize dynamical chaos in arbitrary dimensions. In particular, the method proposed by Lefranc (Phys. Rev. E 035202, 2006) is based on a triangulation of periodic points that is very similar to cell complexes and characterizes how facets of the triangulation are transformed between themselves, which is a description of the semi-flow. It only considers the problem of estimating the entropy of the flow, but this is also a challenge in geophysics. Similarly, it would be interesting to mention applications of the Conley index by Mischaikow and collaborators.

(12) We will add these references to the text and explain how constructing a cell complex and computing its homology groups differs from the methodologies considered in them.

(13) At several places, there is a discussion of using UPO or not utilizing them, but it could be useful to mention that UPO can be very useful to characterize a chaotic

system because the information about them can be obtained in a finite time, which can be useful in non-stationary systems and because a single UPO can bring much information (see Amon and Lefranc, Phys. Rev. Lett. 094101, 2004).

This reference will be added and discussed.

(14) I am convinced that the authors can easily address these minor issues.

(14) Thank you again for having raised these very useful points.