Response to reviewers

We would like to sincerely thank the reviewers for their thoughtful and constructive comments, which have been very helpful in improving the quality of the manuscript. We have responded to all the comments, clarifying/explaining/justifying aspects.

Reviewer 1

Overall comments

Comment 1. The conclusion that the model has been validated is based on spectra, when phase space plots rather clearly show a mismatch. At the very least language regarding strength of conclusions needs to be toned down considerably.

Response: We thank the reviewer for this remark. The first virtue of analyzing the model dynamics in the phase space is to check that the equations are numerically robust and that they do not converge to an unrealistic trivial solution (i.e. they can be integrated over a very long period of time and the original dynamics is neither a constant, nor a period-1 cycle). We agree that the question of validation is important but it must be reminded that the pointwise and phase-accurate agreement between the modeled and observed trajectories is not expected, in particular when reconstructing reduced-order dynamics from noisy observational data. The aim of the GPoM tool is not to reproduce the observed trajectory (because it is a particular realisation) but to obtain a low-dimensional approximation of the original dynamics.

It should also be kept in mind that validation is a difficult problem as far as autonomous models are considered (that is, models without any explicit forcing as it is the case here). The full phase space cannot be observed; only a particular trajectory, which is both short and noisy, is accessible. In contrast, the derived model can be integrated over long time periods but is fully deterministic and low-dimensional, resulting in a smooth trajectory. We agree that the term *"validation"* may be misleading when comparing the modeled and observed phase space. However, the phase space must be shown and analyzed to check its overall consistency, keeping all these specificities in mind. And then, other elements of analysis may be considered for proper validation. Spectral comparisons and forecasting capacities were already presented in the first submission. Some additional analyses of the variable distributions have been added in the revised manuscript, to reinforce the validation.

Hence, the combined use of phase-space projections and spectral comparisons aims at assessing structural and statistical consistency between observations and reconstructed dynamics. While local mismatches are indeed visible in phase space, the models reproduce important features of the original phase space, including dominant state-dependent couplings, the order of magnitude of the variables, rather than the superposition of individual trajectories which are not expected. Another important element to keep in mind is that phase portraits presented in Fig. 3 represent the attractors at the convergence (transient has been removed). The real system is high-dimensional whereas the obtained equations are low-dimensional and can surely not catch the whole dynamics (which also includes the forcing by the open ocean). When triggered by specific conditions, the dynamics may be ejected out from the permanent regime. And this does not mean that the equations cannot reproduce the dynamics out of the attractor since these conditions may also be triggered using a proper reinitialization (or a more sophisticated data assimilation). To avoid possible confusions, the language has been moderated as suggested by the referee, and additional text has been provided before starting comparing the original and modeled phase space to better explain why a perfect fit is not expected (see lines 229-251) . We hope this can facilitate the reading.

Spectral comparisons provide a complementary and non-redundant diagnostic by evaluating whether the distribution of variance across temporal scales, characteristic timescales, and persistence properties are consistent between the low-dimensional models and the observations; which is the case here, even in the absence of trajectory-level agreement.
Robustness and predictability diagnostics should also be considered (section 3.3). They indicate that the models remain numerically stable over intra-annual timescales, despite exhibiting limited predictive skill from an engineering perspective (note that limited horizon of predictability is fully expected in atmospheric and oceanic dynamics). To reinforce the validation, we have included in appendix (also made available hereafter) a direct cross-comparison between observations and model variables' amplitude distributions (**Figs. 1–4; Figs B1-B4** within the manuscript) along with their first to fourth statistical moments (**Table B1** within the manuscript), which illustrate a good agreement of the modeled and observational phase space from a statistical perspective. Moreover, and for each site, we further performed multiple simulations initialized from randomly selected points on the observations' phase portrait, with additional perturbations (2% of the standard deviation) applied to the initial conditions. These experiments show that the model consistently reproduces the essential looping structure of the dynamics, qualitatively (**Figs. 5–8; Figs C1-C4** within the manuscript). Note that, to have a statistical validation of these forecasting capacities, the statistics of the error growth all along the observed trajectory were already provided (with three levels of confidence provided 75%, 90% and 95%) for all the models as a Supplementary Material (Figs. SM1-SM12). Although the present models are not intended for operational or management purposes, their forecasting performances provide a useful level of confidence in the robustness of the models for subsequent analyses, including the characterization of dynamical regimes, the assessment of predictability limits, and the minimal physical interpretation of the inferred couplings.

We have revised the manuscript accordingly to clarify these points, to tone down the language, and to avoid the use of *"validation"* in a strict sense when the model-data comparison was qualitative rather than quantitative. The models are now explicitly presented as dynamically consistent and interpretable reconstructions of the shoreline–sandbar system, intended to explore intrinsic dynamics and coupling mechanisms rather than predictive accuracy.

Comment 2. There is a lot of dynamical systems verbiage throughout (granted a lot of is standard). I think this should be balanced out by a discussion of the physical system. One example is "dissipation". What are we talking about here turbulent dissipation in the overlying fluid? Grain on grain friction that leads to dissipation? I am guessing "neither", and I think this needs to be clarified.

Response: We agree that the term *"dissipation"* may be ambiguous if interpreted in a strictly physical sense. In this study, dissipation is used in two distinct but clearly defined contexts. First, in a dynamical-systems sense, dissipation refers to an effective contraction of phase space, corresponding to a loss of dynamical memory that represents the net effect of multiple unresolved processes (e.g., sediment rearrangement, profile relaxation, and the integration of event-scale forcing). This effective dissipation limits the growth of shoreline and sandbar excursions and ensures bounded dynamics. Second, when referring to the physical system, dissipation denotes energy loss through physical processes such as wave breaking and sediment transport. Because the reconstructed GPoM models are low-dimensional and data-driven, these physical processes cannot be disentangled individually and are therefore implicitly embedded within the effective nonlinear terms of the reconstructed dynamics.
We have revised the manuscript accordingly to clarify this distinction. Throughout the text, potentially ambiguous terms such as *"dissipation"* are now explicitly qualified to distinguish between their dynamical and physical meanings.

Comment 3. The fundamental structure of the model is as an unforced system. Much is made of the theorems of dynamical systems theory for autonomous systems. But the physical sandbar system has forcing and explicitly non-autonomous terms (e.g. a strong storm passing through is likely to shift the sandbar a lot). This requires clear discussion.

Response: We agree that the physical shoreline–sandbar system is strongly forced and non-autonomous, with episodic events such as storms playing a major role in sandbar migration. The reconstructed GPoM models are formally autonomous, but they should not be interpreted as representing an unforced physical system. Instead, they constitute effective reduced-order representations of a forced morphodynamic system, in which the influence of external drivers (wave climate, storms, seasonal variability) is implicitly embedded through the observational data used for reconstruction. Of course, all the complexity of the high-dimensional forcing cannot be included in a set of three or four equations, but only a very small part of it. But one part of the forcing will be included in the observed and modeled variables, and we did not try here to separate the part strictly due to the forcing. This separation is an important and very interesting question that should be kept for another study.

From a morphodynamic perspective, the state-of-the-art literature on storm impacts reports a wide range of responses. Storms can induce offshore bar migration and erosion, onshore bar migration and accretion, or erosion of backshore dunes accompanied by sediment return to the nearshore. These contrasting responses highlight the strongly state-dependent nature of storm impacts, which also depend on wave energy thresholds and antecedent beach morphology (Thuan *et al.*, 2016; Almar *et al.*, 2017; Eichentopf, Karunarathna et Alsina, 2019; Harley *et al.*, 2022; Medellín, Franklin et Torres-Freyermuth, 2024) .

In this study, we base our discussion on the storm response taking place at the study sites. Hence, the observations indicate that although extreme events can induce rapid and sometimes large excursions of shoreline and sandbar positions, the dominant system dynamics consistently drive the variables back toward their characteristic range of variability (at least for the four sites analyzed here and over the temporal window considered). This behavior suggests that extreme events act as perturbations superimposed on a persistent underlying dynamical regime, rather than redefining the long-term organization of the system, explaining why the beach system is amenable to a low-dimensional one.

The objective of the modeling approach is therefore not to reproduce the response to individual storms, but to characterize how the shoreline–sandbar system integrates repeated forcing into a low-dimensional intrinsic dynamics. We have revised the manuscript to clarify this distinction and to explicitly discuss the scope and limitations of autonomous reduced-order models for inherently forced coastal systems (see Discussion section). Thank you for this interesting remark.

Comment 4. The sandbars that are modelled are not measured but are inferred. I am a theorist/numericist but I want to note that the comment of a satellite data expert would be very helpful on the uncertainty in the inference.

Response: Indeed, the sandbar positions used in this study are inferred from satellite imagery rather than directly measured *in situ*, and this inference is associated with observational uncertainty. The sandbar detection method employed here relies on satellite-derived wave-breaking patterns and has been specifically developed and validated by coastal remote-sensing experts (Frugier *et al.*, 2025, 2026), including quantitative comparisons with available *in situ* observations. Several co-authors of the present study are specialists in satellite-based coastal monitoring, and this methodology is now routinely used for long-term sandbar tracking.

Reported uncertainties on satellite-derived sandbar crest positions are typically on the order of 20 m RMS, corresponding to approximately two Sentinel-2 pixels (see, e.g., the SBI framework from Frugier et al.). This is consistent with earlier results showing that intensity-based bar crest positions can differ from directly measured bathymetric positions by a time-varying distance of order O(10 m), depending on offshore wave height, water level, and local bathymetry (Van Enckevort et Ruessink, 2001). Importantly, the reconstructed sandbar position should therefore be interpreted as an effective, spatially averaged morphodynamic proxy rather than an instantaneous bathymetric measurement.

For shoreline position, recent assessments based on Sentinel-2 imagery indicate sub-pixel RMS errors typically below 10 m (Bergsma *et al.*, 2024), providing a higher positional accuracy than for sandbar crest detection.

The global modeling approach adopted in this study is designed to capture low-dimensional, large-scale dynamics and is therefore robust to moderate observational noise. Uncertainties in sandbar and shoreline inference primarily affect high-frequency variability, which is filtered during preprocessing and does not control the reconstructed attractor geometry, dominant couplings, or inferred dynamical regimes. We have revised the manuscript accordingly to clarify the nature of the inferred variables, their associated uncertainties, and the implications for the interpretation of the reconstructed dynamics (see Subsection 2.1).

Comment 5. Perhaps a suitable way to conclude is to ask: If I have a site for which climate change leads to an increase in storm incidence and hence sandbar movement, how can the model account for this? By refitting? Is there a way to assert standard causality or for intuition based on science? Indeed what is the role of physics in this modelling exercise?

Response: The present models are inferred from observations over a given historical regime and are therefore best interpreted as effective dynamical representations of that regime. If the forcing statistics shift substantially (e.g., more frequent or more intense storms), the effective parameters and couplings may change. One straightforward approach is therefore regime-dependent reconstruction, e.g. refitting the model over different periods or using moving windows to quantify how the reconstructed feedback structure and dynamical invariants (attractor geometry, Lyapunov exponents, stability metrics) evolve with changes in storm climate (for example). A complementary and more physically explicit route is to extend the framework toward non-autonomous / forced formulations, by including external covariates (storm indices, wave energy metrics, seasonal forcing) as explicit inputs. This would allow testing how specific changes in storm statistics modify the internal shoreline–sandbar oscillator and would enable scenario-based sensitivity experiments. However, the primary goal of this work was to capture the dynamical regime under which sandy beaches evolve, see if we were able to retrieve the characteristic timescales and dynamical relationship between variables. Thus, the intention with these models, in the present study, was never to perform scenario simulations under climatic changes (yet, it may be achievable with this data-driven approach (Sáez *et al.*, 2024)).

Regarding causality, we emphasize that the reconstructed equations do not provide a unique mechanistic attribution at the process level (See Subsection 3.2); rather, they offer an interpretable and testable low-dimensional structure consistent with known morphodynamic feedbacks (buffering by the bar, relaxation and memory). The role of physics in this modeling exercise is therefore to guide variable choice and interpretation, and to use dynamical diagnostics to quantify

stability, predictability limits, and regime shifts in coastal morphodynamics.
We agree that, even if not intended, some of the conclusions may let readers think that THE model was found for sandy beaches. We have revised the discussion accordingly to clarify scope, limitations, and future pathways (see Discussion and Conclusion sections).

Detailed comments

Comment 6. The Introduction overplays the role of strongly chaotic models; most fluid physicists do not rely on things like the Lorenz model to understand geophysical fluid mechanics.

Response: We thank the reviewer for this comment, but we respectfully disagree. Arguments for chaos in oceanography can be found in many studies and from very different point of view (see for example Sévellec & Fedorov 2014; Waldman et al. 2016; Penduff et al. 2017; Jamet et al. 2019; Cravatte et al. 2021; Charó et al. 2025; Bonel et al. 2025). Fluid physicists may not need to consider if their fluid is chaotic or not, depending on what their practical/theoretical interests are. In particular, they may use primitive equations and generate realistic dynamics, possibly chaotic or even turbulent ones, but without the need to know or to mention what the exact dynamical regime is (and where, since the regime can be spatially different here and there). But our problem here is quite different: we don't have the equations for the dynamics of shoreline-sandbar interactions, and we would like to reconstruct them (or at least obtain a good approximation of them), and directly from the observational time series. Since the dynamics is very likely chaotic (that is, deterministic and very sensitive to the initial conditions), an approach able to account for such an unstable dynamical regime must be fostered. GPoM is well designed for this purpose (and no problem if it is not chaotic as the GPoM tool can also obtain non-chaotic equations if the original regime is not). Hence, it is subjective to pretend we "overplay the role of chaotic models". On the contrary, the role of chaos should not be underestimated there, since it is actually crucial for those who want to reconstruct dynamical equations from observational time series under an unstable dynamical regime.
Indeed, few physicists will use the Lorenz model in geophysical fluid mechanics, but its three variables are adimensional and do not have very direct intuitive meaning, so far. Moreover, the Lorenz system was not obtained from observed geophysical time series. It is actually a subset (three equations), extracted from a seven-dimensional system itself obtained by approximation of a theoretical convection model (Saltzman 1962). With a proper parameterization, its dynamics may better correspond to three specific variables of the Rayleigh-Bénard roll. As it is usually parameterized, we would surely not recommend its use for performing forecasts in geophysical fluid mechanics, but it should not be rejected too lightly because it really has some deep qualitative merit (see e.g. Sévellec 2025). In our present context, the comparison with the Lorenz system does not stand, because we extract the dynamical equations directly from the observed time series of the

system we want to study, that is, to model the shoreline-sandbar interactions.
We hope this clarification helps the reviewer understand our perspective.

Comment 7. In the Introduction the only "field" examples quoted are self-citation. Can the work of others be included, or is this novel only the authors have done it? If it is the latter, say so explicitly.

Response:. We have revised the manuscript to explicitly situate our work within the broader literature on sandbar detection from remote sensing (see Subsubsection 2.1.3). In particular, we now acknowledge earlier studies based on manual extraction from satellite imagery, as well as the limited number of automatic approaches proposed to date.
We also clarify that, aside from the approaches of (Tătui et Constantin, 2020) and Frugier et al. (2015), which rely on satellite imagery, no other study to our knowledge currently addresses the automatic extraction of sandbar positions using publicly available high-resolution satellite data such as Sentinel-2 or Landsat.

Comment 8. The term validation is a bit of a gimmick, since no actual validation in an engineering sense is done. The model does as well as it can (and this would be considered "not very good" in most engineering applications). The real questions are I) What is the accuracy limit of these methods (e.g. would a higher order polynomial fit do better?), II) what would make a better model? In physics based models this is usually thought of as either a data problem, or a parametrization problem. I am not sure what it means here.

Response: The question of the validation has been discussed upper in the response to Comment 1: It has been reminded that the phase space is important but it can only enable a qualitative comparison between model and data since it is not expected to exhibit a point-to-point trajectory; Statistics in terms of forecasting error growth and confidence level were already provided as supplementary material SM1-SM12 in the previous version, these elements have been recalled; Additional analyses based on the variable distributions and their statistics have been performed to reinforce the validation (provided in Appendix).
Following the other comments within comment 8, the accuracy limit of these models is primarily structural, rather than technical. Because the shoreline–sandbar system exhibits chaotic dynamics and is reconstructed from noisy observational data, trajectory-level predictability is intrinsically limited. Increasing polynomial order or model complexity does not improve this limit and typically leads to overfitting and reduced robustness, without enhancing the reconstructed dynamical structure.
In this framework, a "better" model does not mean a more accurate forecasting, but a model that more robustly reproduces the system's global attractor structure, geometry, dominant couplings, and dynamical invariants. Improvements can therefore come from (i) improved data quality or additional observables, and/or (ii) extending the model structure to explicitly include external

forcing terms or regime dependence. This will require further new investigations and will be the topic of another study.
As this comment is closely related to several other remarks, we refer the reviewer to the corresponding revisions in the manuscript, where the considerations raised here have been explicitly incorporated.

Reviewer 2

1. Methodological context

It could be useful to situate the Global Polynomial Modeling approach within the broader set of methods that aim to infer dynamical equations directly from data. In recent years, several equation-discovery approaches have been proposed in the dynamical systems community, including Sparse Identification of Nonlinear Dynamics (SINDy) and related sparse regression frameworks (e.g. Brunton et al., 2016; Rudy et al., 2017). A short mention of these approaches may help place the present work within the wider methodological landscape of data-driven dynamical modeling.

Response

Indeed, GPoM is enrolled within a broader equation-discovery framework environment. Hence, we agree it can be useful to place this study within the wider context of data-driven dynamical modeling in order to provide a global view of the complementary approaches recently or previously developed within the community (note that the Brunton et al. 2016 was already quoted in the previous version of the manuscript, and that a paper by Aguirre & Letellier 2009 giving a historical perspective was already quoted as well). The paragraph dedicated to the introduction of data-driven approaches for inferring dynamical equations was largely rewritten (see Introduction). Several techniques introduced from the early 1990s to present are first introduced. The cases of applications of these techniques to theoretical, then experimental and then environmental observations are then mentioned. The techniques able to extract interpretable equations are then mentioned. And then, cases where these techniques enabled interpretable equations to be obtained from the real world are quoted. We hope this organisation clarify the respective contributions, as expected.

2. Topological approaches and references

In the section discussing topological analysis methods, the manuscript currently cites Charó et al., (2021). However, the reference that appears most relevant in the context of deterministic dynamical systems and homology-based analysis is Charó et al., (2022), whereas the 2021 paper concerns stochastic differential equations and addresses a different

framework (random attractors).
In the same paragraph, the text can lead to the conclusion that the color tracer mapping method enables analyses that are not accessible through other topological approaches, while the so-called templex theory (Sciamarella & Charó, 2024) has addressed higher-dimensional attractors as well as toroidal chaos (e.g. Mosto et al., 2024).

Response

Indeed, it was a reference mistake to cite Charó et al. (2021). We have corrected the reference accordingly. Thank you for your vigilance.
Concerning the color tracer, our intention was not to suggest that other methods cannot provide analyses accessible through alternative approaches. Moreover, we already acknowledge other efforts and progress recently made in lines XXX: "*Few techniques developed to investigate their structure (Mindlin and Gilmore, 1992; Gilmore and Lefranc, 2002; Mangiarotti et al., 2014). Among them and recently, promising results have been obtained based on homology (Charó et al., 2022; Sciamarella and Charó, 2024), but their application remains difficult in many cases. Developed to characterize foliated structures, color tracer mapping can be an alternative, in particular because it can also be applied to toroidal chaos and to four-dimensional problems (Rosalie and Mangiarotti, 2025; Mangiarotti et al., 2023).*"
We have therefore added some additional elements (highlighted in red, your own reference included) to clarify the rationale behind our choice to use this method: "*Few techniques developed to investigate their structure (Mindlin and Gilmore, 1992; Gilmore and Lefranc, 2002; Mangiarotti et al., 2014). Among them and recently, promising results have been obtained based on homology (Charó et al., 2022; Sciamarella and Charó, 2024, Mosto et al. 2024). However, extracting the topological structure of chaotic attractors remains a hard problem in many cases. Developed to characterize foliated structures, color tracer mapping can be an alternative, in particular because it can also be applied to toroidal chaos and to four-dimensional problems (Rosalie and Mangiarotti, 2025; Mangiarotti et al., 2023). Particularly, for systems exhibiting weakly dissipative chaos as in this study, their attractors' thick structure make it difficult to reduce them to a flat one as a template. Hence, color tracer mapping developed to characterize foliated structures can be an alternative, in particular because it can also be applied to toroidal chaos (weakly dissipative cases included), hyperchaotic attractors and hyperchaotic map (Rosalie and Mangiarotti, 2025; Mangiarotti et al., 2023)*".

3. Relation to simplified morphodynamic models

Since the reconstructed systems are explicit autonomous ODE models, it may be useful to clarify how their dynamical structure relates to the simplified dynamical models historically proposed to describe shoreline–sandbar systems in coastal morphodynamics.

In particular, a brief comparison between the phase-space or topological structure of the reconstructed systems and that of earlier conceptual models (e.g., Dean, 1991; Yates et al., 2009; Davidson et al., 2013) could help clarify what new dynamical insight is obtained from the data-driven reconstruction.

Response

The reviewer raises an important point regarding the relationship between the reconstructed systems and classical shoreline models. Equilibrium-based approaches such as those of Dean (1991), Yates et al. (2009), and Davidson et al. (2013) describe shoreline change as a relaxation process toward a forcing-dependent equilibrium, of the general form $dxs/dt = f(E, xs\_eq(E) - xs)$, where wave energy E enters as an explicit external driver. These models are non-autonomous, one-dimensional in state space, and structurally limited to monotonic decay toward equilibrium. Hence, they cannot, by construction, produce self-sustained oscillations, limit cycles, or sensitivity to initial conditions. Moreover, the sandbar does not appear as an independent dynamical degree of freedom in this framework.

The GPoM-reconstructed systems differ in two fundamental respects. First, they are fully autonomous in their formulation: no external forcing is prescribed, yet the models sustain realistic oscillatory behavior and broadband red spectra. This implies that the influence of wave climate is absorbed into the internal coupling between shoreline and sandbar, which together form a self-regulating dynamical unit. Second, sandbar position enters as an active state variable whose coupling to shoreline dynamics governs both the sign and nonlinearity of the feedback structure. The resulting phase-space geometry with bounded attractors of fractal dimension between 2.3 and 2.85, positive Lyapunov exponents, and site-specific folding structures has no counterpart in equilibrium models and constitutes the primary new dynamical insight provided by the data-driven reconstruction. Following your remark, we have added a short paragraph to the Discussion to make this structural comparison explicit.

4. Lyapunov exponents

Lyapunov exponents are reported as evidence of chaotic dynamics. It would be helpful to indicate the uncertainty associated with these estimates, for instance by providing confidence intervals or by discussing the sensitivity of the calculation to methodological choices (e.g., smoothing, embedding, or time-series length).

Response

Thank you for this comment. The Lyapunov exponents reported in Table 6 are (and were already) provided with uncertainties estimates. These uncertainties were obtained by repeating the

Lyapunov spectrum calculation from multiple initial states sampled along the numerically integrated attractor after transient removal (K = 10), and reporting the mean ± standard deviation across these repeated estimates. We also note that the leading exponent remained positive across the repeated estimates, supporting the robustness of the chaotic interpretation. This was not explicit in the previous version, and we have clarified this point in the revised manuscript within Table 6 description. Moreover, we also added a quick remark on the sensitivity of the Lyapunov exponent's estimation to data processing within the main text as well as discussing, when felt necessary, the Lyapunov and Kaplan-Yorke uncertainties within Torrey Pines and Ensenada paragraphs of Section 3.3. Overall, the conclusion of chaotic dynamics rests not solely on the Lyapunov estimates but on the convergence of multiple lines of evidence.
Finally, for the Duck case of hyperchaos, the uncertainties were indeed not plotted along the single values. We corrected this lack of information by computing the uncertainties within Fig. 9.

5. Dimension of the reconstructed system

The manuscript reconstructs dynamical systems of dimension three or four directly from the time series. It would be useful to clarify how the dimensionality of the reconstructed system was determined. For instance, embedding diagnostics such as false nearest neighbours or related delay-embedding approaches are commonly used to estimate the minimal embedding dimension before fitting dynamical models. A brief discussion of this point would help assess whether the chosen model dimension is sufficient to represent the underlying dynamics.

Response

The reviewer raises a valid methodological point. In the GPoM framework, the embedding dimension is not fixed a priori through classical diagnostics such as false nearest neighbours (FNN) because our objective is to get an interpretable low-dimensional approximation of the observed dynamics. Moreover, to get a robust estimate of the original dimension of the dynamics would require a longer time series. Instead, a more pragmatic approach is used with GPoM, starting from dimension three (because only trivial dynamics can be obtained in dimension 2 as guaranteed by the Poincaré-Bendixson theorem), and progressively increasing the dimension. In practice, it is rare to be able to reconstruct models of dimension higher than 4, in particular when starting from environmental data because they are very noisy (although this was proven possible in several cases, see Saez et al. 2024 or Mangiarotti et al. 2019 in which six-dimensional models were reconstructed for the microatmosphere dynamics or for soil ecohydrological coupling). In the present study, model dimensions m = 3 and m = 4 were explored systematically (Section 2.5), and the retained models were selected based on their ability to reproduce the observed phase-space geometry, spectral structure, and long-term numerical stability as an effective model-selection criterion for dimensionality. This approach is appropriate here because the state space is constructed from two simultaneously observed variables (shoreline and sandbar positions) and their derivatives, rather than from delay embeddings of a single scalar observable. Nevertheless, we agree that a brief clarification of this point is warranted. Hence, we added a sentence in Section 2.5 noting that classical embedding diagnostics were not applied as a pre-step.

Additional remarks

Figure 3: 3D graphics instead of 2D phase portraits might allow for a slightly better visual inspection of the geometric aspects.

Response: Indeed, 3D objects would be nice, but 3D graphics cannot be printed in a 2D journal. Alternative 2D projections of the phase space can be shown as additional information but it should be noted that the usual phase portrait should in principle be fostered in a scientific publication, for the sake of reproducibility. However, it is true that disposing of several views of an attractor will help better figure out the structure of an attractor (and this can be of particular interest for an advanced topologist of chaos). So did we improve our original 2D phase portraits in the main text while placing alternative projections in the appendix.

Section 3.1.2: In connection with the remarks made by Reviewer 1 on the validation of the reconstructed dynamics, it may be worth recalling that different dynamical systems can share similar spectra while differing in geometry, invariant measures, or instability properties.

Response: You are perfectly right. Spectral consistency alone does not uniquely identify the underlying dynamical structure, as distinct systems can produce similar power-law spectra (and similarly for statistical validation based on the distributions). This point deserves clarification.

Figure 6: The bibliography corresponding to the fluence diagrams seems to be missing.

Response: Reference added. Thank you for your vigilance.

Line 545: The PCA performed to analyze the Poincaré section of Duck's attractor would benefit from a brief explanation of how it is carried out in combination with the methodology used.

Response: Specifically, the PCA is applied to the coordinates of the points constituting B0 within the Poincaré section, projected onto the (X1, X3) plane. The covariance matrix of these coordinates is decomposed, and the first principal component (i.e., the direction capturing the largest variance within B0) is extracted. Each point is then projected onto this axis, yielding a scalar value that is linearly mapped to a continuous color scale. This color assignment provides a spatially coherent

label within B0: points that are close together and share a similar position along the dominant axis of the neighborhood receive similar colors, while points at opposite ends of the neighborhood receive contrasting colors. As the dynamics are iterated forward, this labeling is preserved, so that the geometric deformation of B0 (stretching, folding, and squeezing) becomes directly readable from the reorganization of colors in B1, B2, and B3.
A clarification has been added to the manuscript.

Line 548: Could the authors provide justification for the choice of the position of the Poincaré section(s)? What could happen if the Poincaré section was misplaced or had fewer components than necessary?

Response: The placement of Poincaré sections was guided by two principles: transversality and dynamical relevance. Sections defined by X2 = 0 correspond to turning points of the shoreline oscillation, where the flow crosses the section surface transversally and the attractor geometry is most clearly resolved. For Gold Coast, a single section would have been insufficient because the attractor is organized around a genus-3 torus, visiting two spatially separated lobes in phase space. A section placed on one lobe only would capture intra-lobe returns (A→A or B→B) while missing inter-lobe transitions (A→B and B→A), leading to an incomplete and topologically incorrect characterization of the attractor. The two-section construction with a unified coordinate axis was therefore necessary to resolve all four transition types. For Duck, the four-dimensional embedding precludes a complete two-dimensional section, and the color tracer approach was adopted specifically to visualize stretching and folding despite this limitation.
Regarding the consequences of misplacement: if the section is tangent to the flow rather than transversal, intersections become ill-defined and the return map acquires spurious multivaluedness. If the section is insufficient in number of components, as would occur at Gold Coast with a single section, part of the attractor's topology is invisible and transition structures are missed. These considerations motivated our section choices and, in the Duck case.

Line 569: Could the authors clarify why the bifurcation diagram at Ensenada cannot be reconstructed?

Response: Unfortunately, identifying a suitable control parameter for Ensenada proved more difficult than for the other sites during exploration of bifurcation parameters. This is mostly due to the cord-like structure of the attractor which is very sensitive to parameter perturbations. Hence, small variations in parameters during bifurcation search may lead to divergence, or collapse onto simple periodic orbit without passing through bifurcation sequences. Another complementary explanation is the model structure retrieved using GPoM. Indeed, in the case of Ensenada, the sandbar equation contains a strongly nonlinear term $\mathbf{b_4X_3}^2$ with an unusually large coefficient ($b_4 = 3.61 \times 10^2$), which provides a dominant restoring contribution to the sandbar dynamics. As the existence of the attractor depends on a precise balance between this large term and the remaining

coupling structure, this makes the system dynamically fragile under parameter perturbations in a way that is not observed at the other sites.
An explanation has been added to the manuscript to clarify this point.

Line 608: The problem of overfitting is mentioned but not discussed in detail.

Response: The discussion on the overfitting problem has been extended.

Typos

The references inserted in the text are, for the most part, not in chronological or alphabetical order when more than one work is cited.

Response: We have ensured that all reference are now in correct order. Thank you for your vigilance.

References

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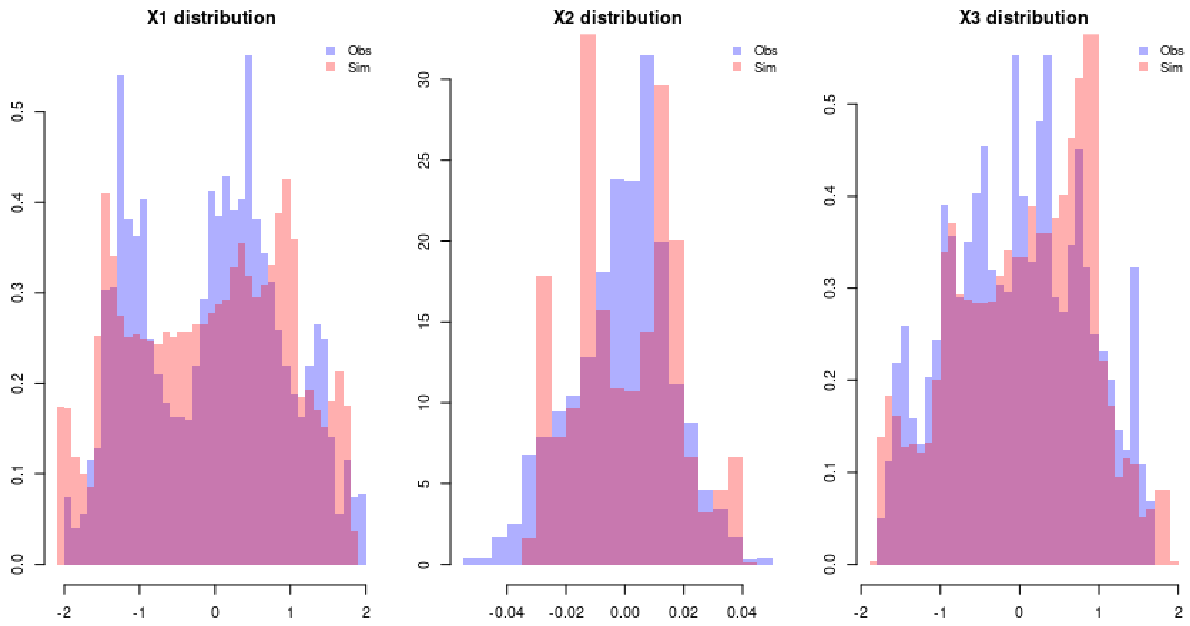

Figure 1. Histograms of the distributions of variables X1, X2, and X3 for the observations and the reconstructed model at Torrey Pines.

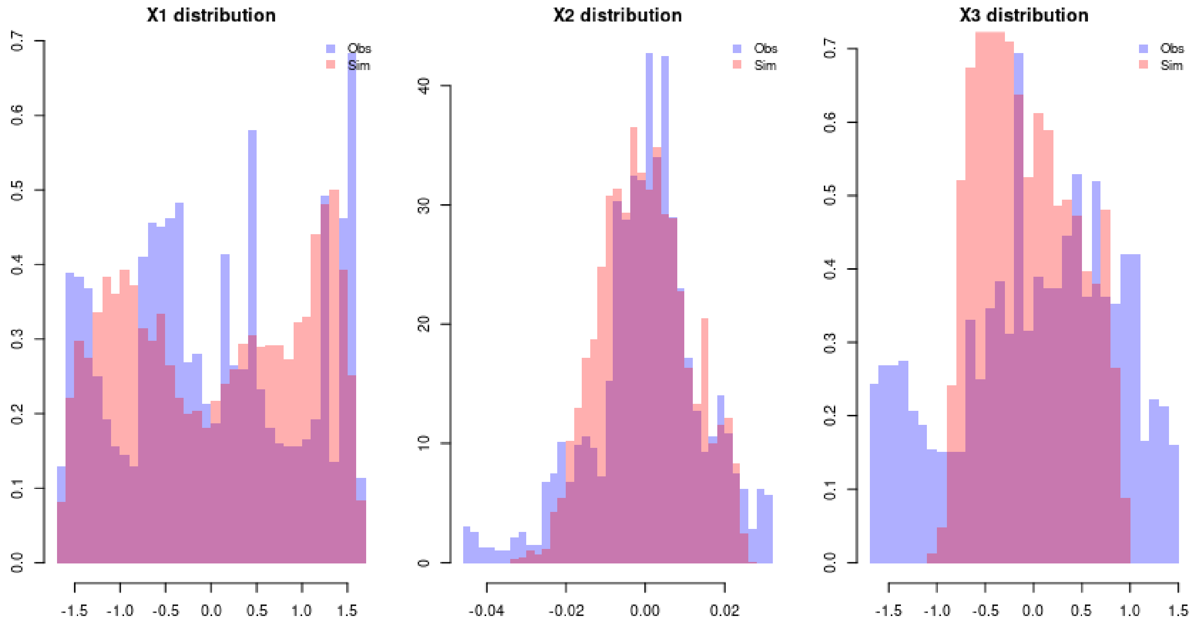

Figure 2. Histograms of the distributions of variables X1, X2, and X3 for the observations and the reconstructed model at Gold Coast.

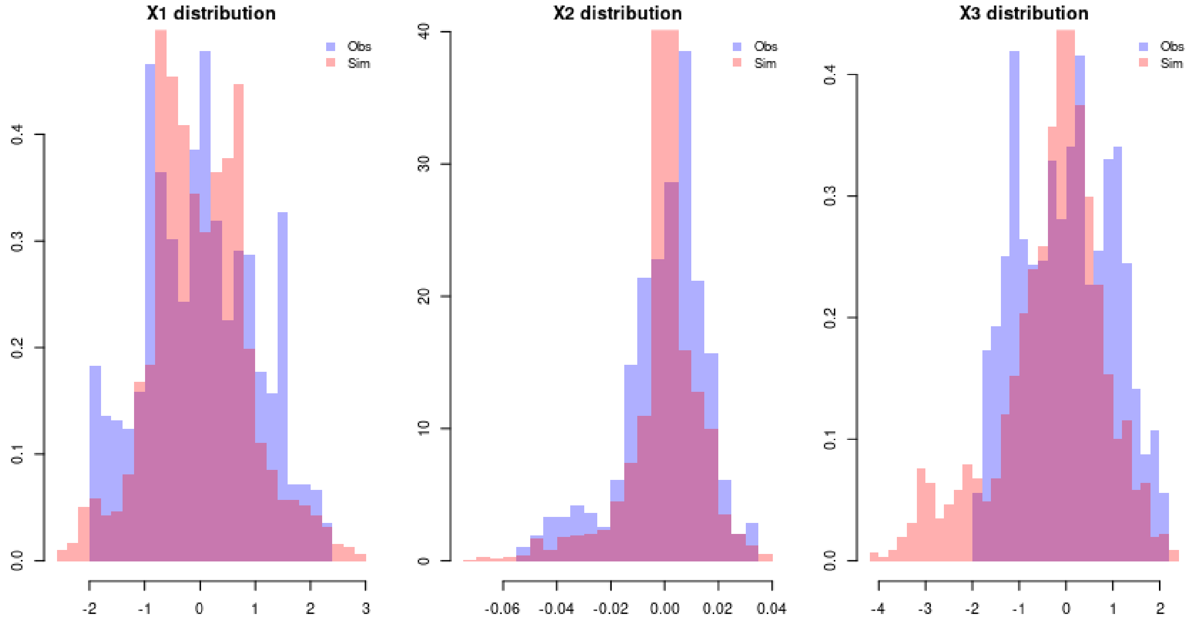

Figure 3. Histograms of the distributions of variables X1, X2, and X3 for the observations and the reconstructed model at Ensenada.

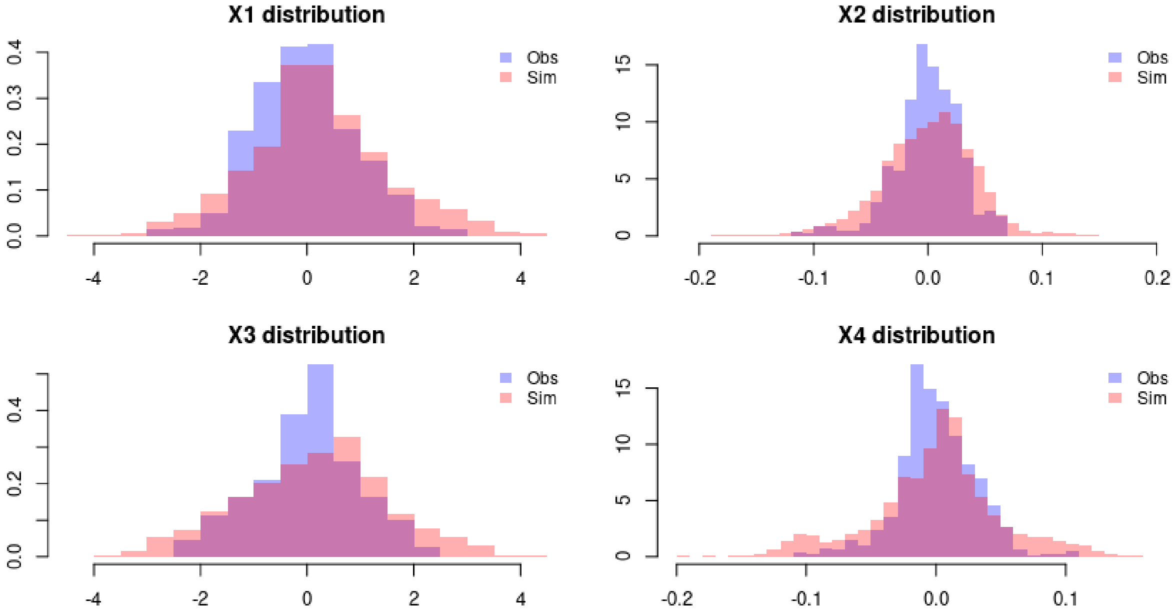

Figure 4. Histograms of the distributions of variables X1, X2, X3 and X4 for the observations and the reconstructed model at Duck.

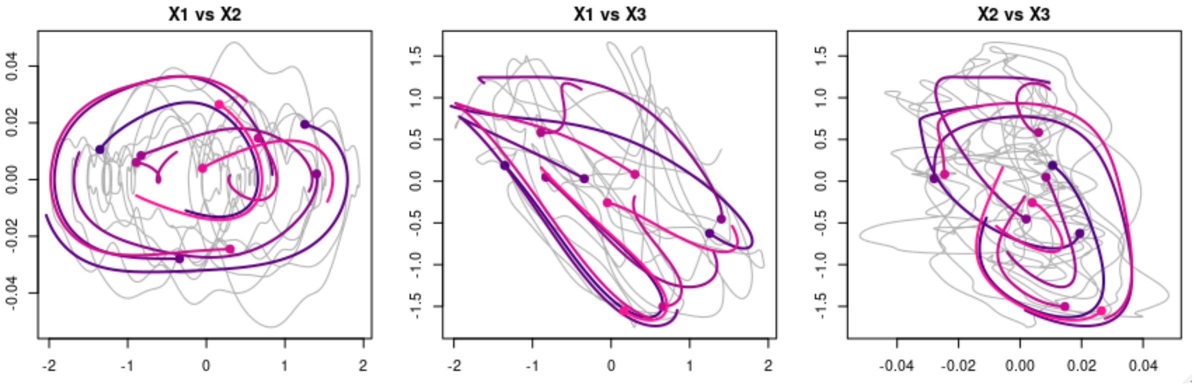

Figure 5 — Torrey Pines. Ten simulations were integrated over 200 days from initial conditions randomly selected on the observations' phase portrait and subsequently perturbed by 2% of the phase portrait standard deviation (dots).

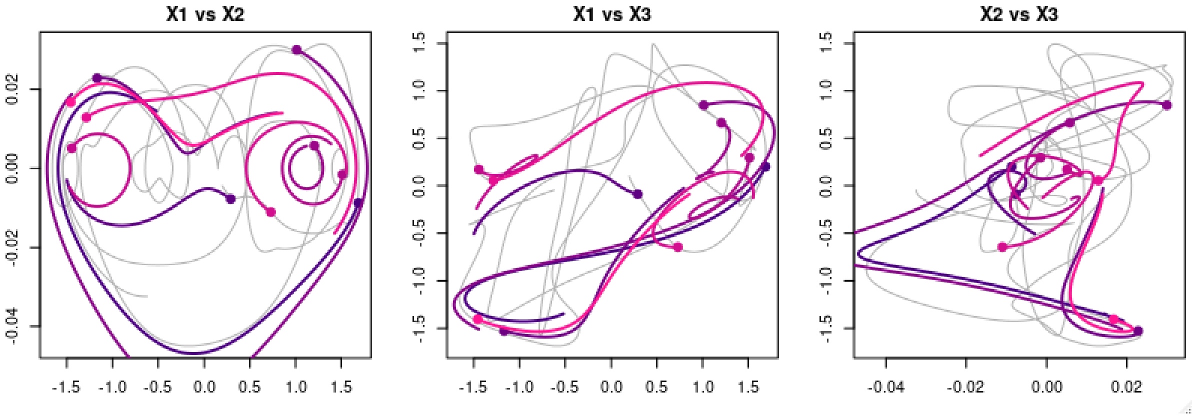

Figure 6 — Gold Coast. Ten simulations were integrated over 200 days from initial conditions randomly selected on the observations' phase portrait and subsequently perturbed by 2% of the phase portrait standard deviation (dots).

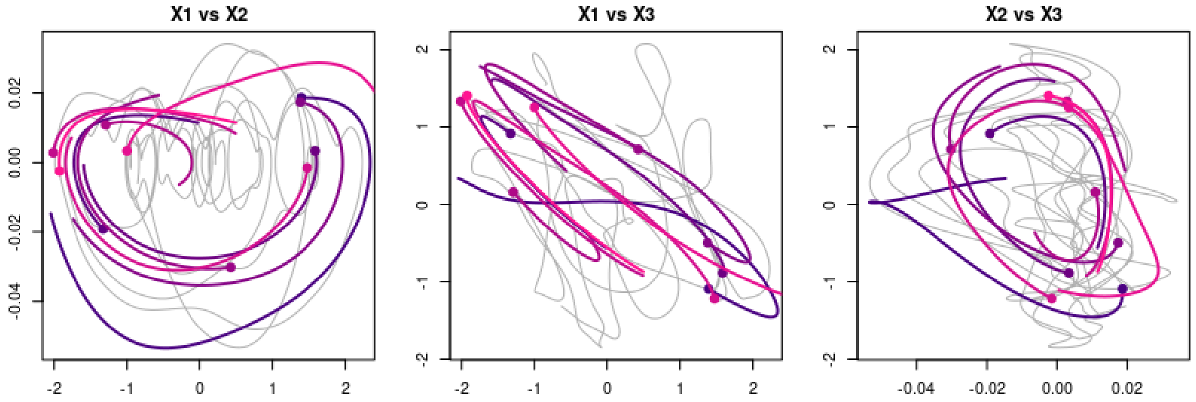

Figure 7 — Ensenada. Ten simulations were integrated over 200 days from initial conditions randomly selected on the observations' phase portrait and subsequently perturbed by 2% of the phase portrait standard deviation (dots).

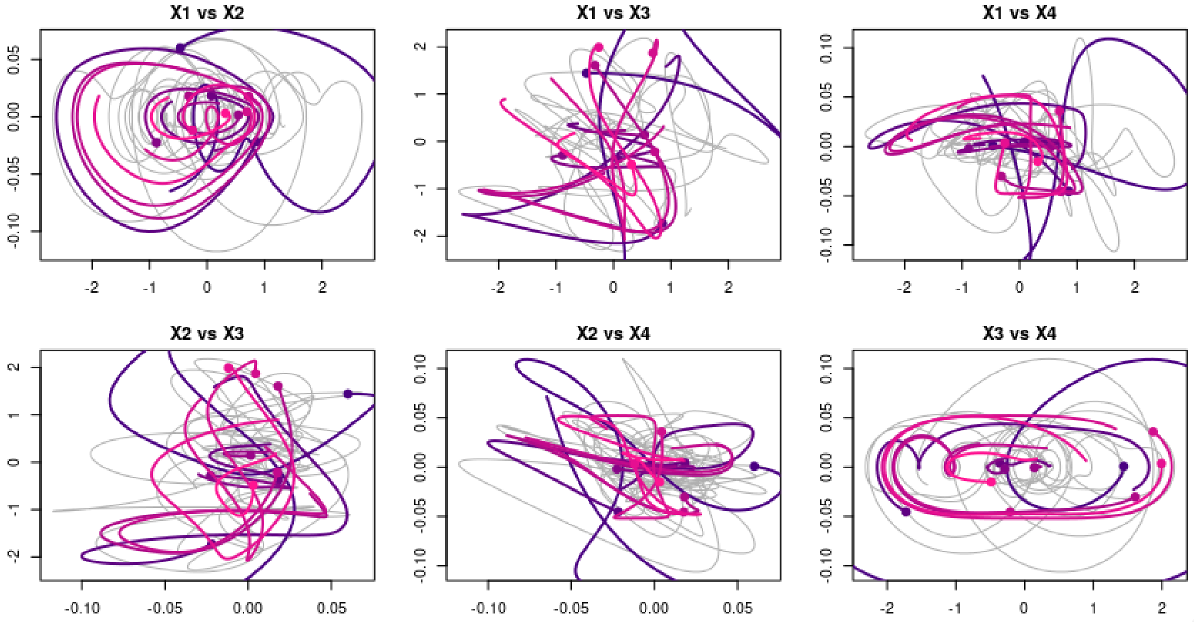

Figure 8 — Duck. Ten simulations were integrated over 200 days from initial conditions randomly selected on the observations' phase portrait and subsequently perturbed by 2% of the phase portrait standard deviation (dots).