Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2018-80-AC1, 2019

Earth System Dynamics Discussions

Interactive comment on "Minimal dynamical systems model of the northern hemisphere jet stream via embedding of climate data" *by* Davide Faranda et al.

Davide Faranda et al.

davide.faranda@lsce.ipsl.fr

Received and published: 1 March 2019

We thank the reviewer for her/his comments. Our answers are given in bold text, the reviewer's comments are shown as plain text.

General Comments: The average functional form f(x) and the three stochastic subgrid terms are purely heuristic so unlike the case of other reduction techniques and subgrid modeling the connection with the physics of the problem is unclear. How would the results change with different, perhaps more physical, subgrid terms? Without a physical basis for the driving terms it seems unlikely that the simple model will be seen as any more than a curve fitting exercise. The presentation of the article is substandard and

not in a form that would appeal to the audience of ESD. The paper lacks motivation , the mathematics is poorly presented with terms undefined and too many typos and has the feel of a first draft. Perhaps unfortunately, the mathematical nomenclature for what are really very simple concepts (new words for old), would most likely put off an audience of largely data analysts. For this audience the authors should make the article more pedagogical and stand alone.

We thank the reviewer for this comment and we take into serious consideration the criticism that our paper should be better motivated. We disagree with the statement that the embedding methodology we apply amounts to curve-fitting (indeed, the only part of our analysis where this definition may be argued for is for obtaining the return map). This model is motivated by geometrical and temporal evolutions of the jet (in a reanalysis). Virtually any models, including conventional numerical simulations of large-scale atmospheric flows, require some arbitrarily chosen parameters, and our case is no different. We also underline that our coupled map lattice model rests on clear physical hypotheses, such as: 1) the eastward propagation of information within the jet stream, 2) the presence of anticyclones and cyclones (baroclinic activity) i.e. sinuosity of the jet, 3) the presence of geographical constraints, 4) small-scale turbulent disturbances. This again sets it apart from curve-fitting exercises. We indeed view our approach as complementary to other idealised approaches which have attempted to formalise atmospheric waves and sinuosity, such as that of Petoukhov et al. In contrast to the latter, our model does not rely on regular wave decomposition hypotheses that are difficult to verify in practice. We now address this aspect more directly in the introduction to clarify the motivations underlying our analysis. However, we do agree with the Reviewer that our illustration of the physical principles underlying some of our choices were not as clear as they should have been. We now detail how our choices have solid physical underpinnings, issued from both laboratory tank experiments, numerical simulations in the literature and scale arguments applied to fundamental concepts in atmospheric dynam-

ics. Concerning the physical processes specific to sub-grid scales, we highlight that a key advantage of the return plot methodology is that it enables us to ignore detailed microphysics, focusing instead on the largest scale effective dynamics which is observed in the real data. Indeed, such small-scale processes are only indirectly present in the data we use to build our model, through assimilation of observations, but would be largely parametrized in the numerical model underlying the reanalysis dataset. Finally, we would like to stress that our purpose is to develop a minimal model to study the phenomenology of the effective dynamics of the jet flow, emerging from the complex underlying physics. Such a model does not require physical sub-grid terms a priori, but only if they were found to be essential to capture the large-scale phenomenology – which we show is not the case. This approach is fundamentally different from Direct Numerical Simulation (DNS) based studies, which typically start from the detailed microphysics at the cost of not incorporating real large-scale data. The phenomenological properties of our model, such as its bifurcation structure, are largely independent of the selected parameters, except for a few leading terms such as kappa, beta, and epsilon. We thus believe that our approach is a valid complement to the classical DNS-like approaches. Concerning the second part of the Reviewer's comment, we address this in more detail in the responses to the specific comments below. One point we would like to highlight is that we have taken very seriously the Reviewer's encouragement to refocus our article to appeal to ESD readership, especially when explaining the derivation and implementation of the model. To this effect, amongst other changes we have added two appendices providing background on some key concepts leveraged in the paper: Appendix A: Coupled map lattice, and Appendix B: Average return map and noise.

Specific Comments: P2, line8: Perhaps references to Charney and De Vore (1979) and Wiin-Nielsen (1979) would be appropriate.

We have added these references, as suggested.

Section 3: The mathematics is surprisingly poorly presented given that one of the authors is from a Department of Mathematics and Statistics. For example, you need to define n as the time step, i as the longitude and define N=360 when it first appears. You need to check your equations for typos as in equation (3). Also, the equations keep changing until you eventually settle on the system that you eventually address.

We will replace n with t, and now describe all variables and the full model at the beginning of Section 3. We will further include a brief background review on coupled map lattices (CMLs), to highlight why they are appropriate in our context, in addition to providing a more detailed background in Appendix A.

P4, lines 23: Northern hemisphere blocking occurs in preferred regions so why does the return map not reflect that?

The local topography is represented as small shifts in the return maps. These small differences in the maps induce a sudden large change of the dynamics, corresponding to shifting the jet towards the north or the south and therefore triggering blocking in selected regions. We omit detailed landscape factors such as high mountains in this particular model for studying global phenomenology. However, these may be included via the boundary condition $r^{(i)}$. We now show that the inclusion of $r^{(i)}$ is able to highlight preferred regions where the jet shifts towards northern or southern directions (Figure 1 of this answer). Figure 1 shows the role of the term $r^{(i)}$ in the shift of the jet position towards northern or southern latitudes. The figures show the fraction of shifts towards the north: a value >0.5 indicates that the jet's preferred position is to the north, a value < 0.5 that the jet's preferred position is to the south. a) Model with $r^{(i)}$=0 over oceans and $r^{(i)}$=-0.02 over the mountains (same domains as given in the previous version of the paper). b) Model with $r^{(i)}$=0 for all the latitudes. Red: shift frequency from data. Black: shift frequency from the model: each line corresponds to a realization of the system.We further argue that it is a strength of the model that it reproduces jet-like phenomenology, independently of the choice of the location,

and that geographic effects can be introduced via boundary conditions. This discussion will be added to the new version of the paper.

P4, lines 4-31: Why is necessary to have separate stochastic processes for the effects of (1) convection and gravity waves, (2) effects of topography and (3) effects of baroclinic Rossby waves, rather than combine the three?

Given the CML model, the external perturbations to each local dynamics are categorized as: (0) initial conditions (1) local noise, (2) spatial boundary conditions, and (3) global noise. In this study, we made models for factors (1), (2), and (3). Term (0), the initial conditions, is chosen from a stationary state in the model. These are based mainly on (1) effects of convection and gravity waves, (2) effects of topography, and (3) effects of baroclinic Rossby waves. In brief, we started with modeling phenomenological external perturbations, and then verified the underlying physics which affects factors (1), (2), and (3). To answer the question of the Reviewer, we cannot combine the three terms into one because they act on different spatial scales. More specifically: without (1) the system will be stacked in only one of the three states with no transitions; without (2) the jet position will not have a geographical dependence (see again Figure 1) and would thus not match the patterns observed in the ERA-Interim data; without (3) there will not be persistent blocking. We will add to the paper the new Figure 2, which shows temporal and spatial cluster size distribution for different models and the data, once they have binarized as follows: "1" means a northern shift of the jet with respect to its central position, "0" means a southern shift. The different model runs show the effect of the suppression of noise terms. The figure clearly shows that by suppressing one of the noise ingredients, the spatiotemporal cluster distributions of the data cannot be reproduced. The motivation of our work is precisely to show that these three ingredients are essential to reproduce the features of the jet dynamics.

Also why are these parameterizations purely stochastic when more systematic sub-

grid parameterizations indicate that they should be represented by a combination of deterministic and stochastic terms (e.g., Kisios and Frederiksen 2018 and references therein). In general, the authors should relate their subgrid parameterizations at least in broad terms to physically based parameterizations.

We will add a discussion about the stochastic vs deterministic parametrization based on Kisios and Frederiksen 2018 and references therein. Our goal here is to have only the large scales described by a deterministic term, as we build a global model of the jet dynamics. Indeed, a mixture of deterministic and stochastic terms improve the dynamical description of the jet dynamics, but the addition of other terms will not make our model a minimal model of the jet dynamics.

P4, lines 14-19: The impression that the authors convey here is that the topography is a stochastic term in their model in which case it should be multiplicative noise rather than additive noise. However, according to the above reference, deterministic topography interacting with eddies produces an additive noise contribution as well as contributions from barotropic and baroclinic Rossby waves.

In our model, the topography is given as a boundary condition and therefore is a deterministic term. We will rephrase the model description to say that it is included in the perturbations term, where the perturbations are split into deterministic (topography) and stochastic (turbulence and baroclinic waves) contributions.

P4, line20: baroclinic –> baroclinic and barotropic

Corrected.

P4,line 21: 10ËĘ-3 –> 10ËĘ3

Corrected.

P5, line 5: What exactly is the form of the non-autonomous force? What is the explicit time dependence? You should define your terms for an audience of largely data analysts.

The model of the local dynamics at location i can be rewritten as: $x_{t+1}^{(i)} = f(x_t^{(i)}) + p_t^{(i)}$. The non-autonomous term $p_t^{(i)}$ includes all driving forces other than $f(x_t^{(i)})$ at position i, and its explicit time dependence cannot be given simply. Non-autonomous dynamical systems theory can be applied to dynamical systems with such "unknown" driving forces. Here, we approximated it as a random variable $p_t^{(i)}$ in [-kappa, kappa], assuming a bounded external force, and analyzed the bifurcation structure of the approximated model. We clearly explain the above mathematical approach in the revised Section 3 in the manuscript.

Section 4: Again the mathematics is poorly presented. I would expect precision and elegance from mathematicians. You will need to explain your terminology for the major audience of ESD. The authors need to carefully check their manuscript for a number of typos.

We will add the mathematical details on the dynamical indicators in two appendices mentioned in the reply to the Reviewer's general feedback.

References: Kitsios, V., and J. Frederiksen, 2018: Subgrid parameterizations of the eddy-eddy, eddy-meanfield, eddy-topographic, mean field-mean field and mean field topographic interactions in atmospheric models. J. Atmos. Sci. doi:10.1175/JAS-D-18-0255.

Interactive comment on Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2018-80, 2018.

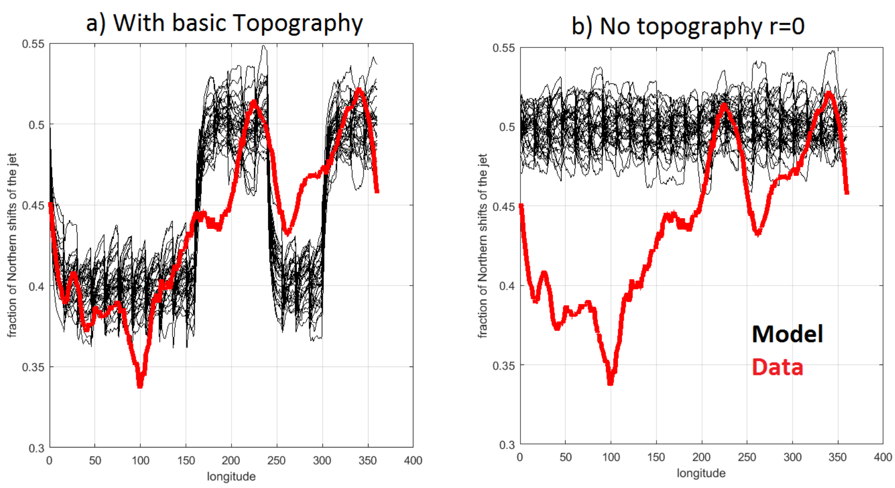

Fig. 1. Role of the term r_i in the shift of the jet position towards northern or southern latitudes. The figures show the fraction of shifts towards the north.

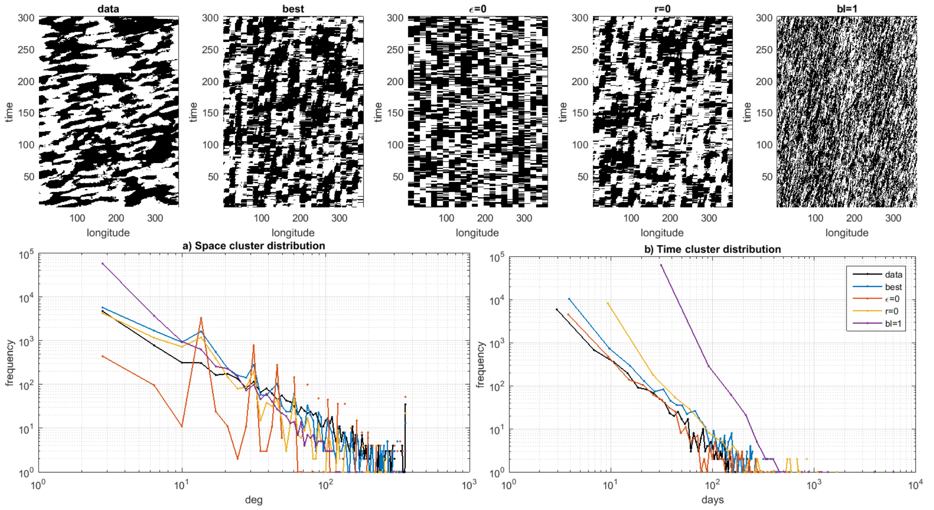

Fig. 2. Upper plots: Temporal and spatial cluster size distribution for the ERA Interim data (top left), and few different model runs. Lower plots: space time cluster distributions