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

Anonymous Referee #1

Review

"Minimal dynamical systems model of the northern hemisphere jet stream via embedding climate data"

Authors: D Faranda, Y Sato, G Messori, NR Moloney & P Yiou

Review of esd-2018-80

Recommendation: Major revision.

Summary:

The authors develop a simple stochastic toy model of the latitudinal position of the peak northern hemisphere upper tropospheric jet stream at the different longitudes. Their aim is to study the jet variability due to transitions from wave breaking and block formation. The simple model uses atmospheric data to determine the average functional form $f\grave{I}\check{E}(x)$ of the processes that determine the current location and the effects of small scale subgrid processes such as convection and gravity waves, topographic processes, and baroclinic Rossby wave processes are represented by three stochastic terms. The stochastic subgrid terms are tuned to represent some of the broad statistical properties of observations of the latitudinal representation of the peak northern hemisphere jet.

General Comments:

The average functional form $f\grave{I}\check{E}(x)$ 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.

Specific Comments:

P2, line8: Perhaps references to Charney and De Vore (1979) and Wiin-Nielsen (1979) would be appropriate. 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. P4, lines 2&3: Northern hemisphere blocking occurs in preferred regions so why does the return map not reflect that? 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? Also why are these parameterizations purely stochastic when more systematic subgrid 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. 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. P4, line20: baroclinic –> baroclinic and barotropic P4, line 21: 10ˆ-3 –> 10ˆ3 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. 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.

References: Kitsios, V., and J. Frederiksen, 2018: Subgrid parameterizations of the eddy-eddy, eddy-meanfield, eddy-topographic, meanfield-meanfield and meanfield-topographic interactions in atmospheric models. J. Atmos. Sci. doi:10.1175/JAS-D-18-0255.1

Please also note the supplement to this comment:

https://www.earth-syst-dynam-discuss.net/esd-2018-80/esd-2018-80-RC1-supplement.pdf