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Earth System Dynamics Discussions

Interactive comment on "A Dynamical Systems Characterisation of Atmospheric Jet Regimes" *by* Gabriele Messori et al.

Anonymous Referee #3

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The authors use dynamical systems metrics (local dimension and persistence) to classify jet regimes in the southern hemisphere.

I think in its current state, the manuscript needs some revision to make it clearer to the interdisciplinary readership and to ensure applicability and reproducibility of the methods applied.

For details on the recommended revisions, please see general and specific comments below.

General comments:

Overall, the authors refer too often to external literature for points/facts that could be briefly elaborated in this manuscript itself to make it a well rounded work, without the

readers having to consult over 10 external papers to actually find out what has been done and what the basic assumptions are.

Having said this, the manuscript is currently lacking a thorough assessment/explanation if the techniques that the authors use in their analysis are actually applicable to the data in their case study. I.e. if the data abides to the ideal conditions for which the dynamical systems metrics were formulated. Hence the authors need to mention what the basic mathematical and physical assumptions underlying the methods and also need to show that their data meets these criterions (which is often not the case when using measured data).

Since dynamical system approaches are neither universally nor straightforwardly applicable to real-world applications, I think it is dangerous to just blindly apply methods to data without properly conveying the basic mathematical assumptions underlying the fundamental theory and calling the approach 'versatile'.

Another shortcoming of the manuscript is the subjective qualitative inspection of how the diagnostic outputs fare in phase space, i.e. the methods applied by the authors allow only a purely descriptive and non-explanatory evaluation. Hence the authors should be more careful when assessing their outputs and avoid bold statements about their alleged ability to provide physical, process-based explanations that in the end are not proven.

Additionally, the authors need to be more careful in reading/interpreting their figures, sometimes the statements made do not quite correspond to what the figure is actually showing (e.g. fig 3).

The authors discuss the special case/simulation no 23 in detail, however, I think the simulation no 7 that stands out in Fig 2 as not 'fitting' the order of decreasing EKE and jet group should also be discussed briefly. I.e. where does it sit in Fig 3c/d?

Specific comments:

L4: why ‘our understanding’? do the authors mean ‘their own understanding’? this is confusing. . .does this differ from the ‘general understanding’?

L 7: this reads as if it could be directly applied to the reanalysis data (which is not the case as the authors do some pre-processing to the data). Please make it clear that it is not the ‘raw data’.

L8: I’m not sure if this study can be called ‘proof-of-concept’, as there were limitations in the output. For a concept to be proofen, more than a single case study is needed. Hence, please rephrase. The same applied to L 275.

L 8-9: ‘versatility and ’computationally efficiency’ have not been investigated here and should hence not be a key output mentioned in the abstract and closing sentence of the manuscript (I.e. L 276). Please rephrase.

L 19: ‘to a good degree’. Please be more specific (i.e. how is ‘good’ quantified) to allow the reader to understand the ‘degree’ of approximation. Or make it clear that the ‘limitations are described below’

L 88: To allow reproducibility of the current study, please provide the model equations as used in the final model in the appendix (no derivations needed).

L 89: Please provide a concise summary of the main limitations of the setup that are relevant to the current study, as this is the duty of the authors and

L 90: For reproducibility, please provide the all these details in the current manuscript. The manuscript should be a ‘stand-alone’ piece of work and one can not expect the readers to go to the authors past works to search for all the details. . ..

L 95: Why 27? Please elaborate how this number was chosen and also how the parameter ranges and the different combinations of H and r were chosen. Additionally, why did the authors choose to have not equal numbers of simulations for each flow regime? This would allow a better comparison of the results. . .

L 97-98: Please briefly elaborate on the representativity of using the mean values here.

L 100: Please elaborate on the degree of similarity, i.e. what do the authors consider to be ‘similar’

L 109: ‘we take these as representative’. I.e. the authors consider them as representative. But are they? Please elaborate why the authors think so.

L 110: ‘To better compare. . .’ Please elaborate why the daily data needs such a high-pass filter. I.e. elaborate on the data characteristics that make the use of such a filter necessary. How does it influence the results?

L 115: ‘applying a recently developed approach. . .’ But the reverences given are from 2016 and 2017. This is not quite ‘recently’ as we are already in 2020. . . Please rephrase.

L 117-140: As this journal is for an interdisciplinary audience the authors should elaborate in more detail what is being done. Currently with the given description the work is not reproducible. Particularly, the reasoning of why certain theorems (and their modifications) are being used (e.g. why are they appropriate to the system under investigation and what are the basic data assumptions) are missing.

L128: what is the ‘Süveges estimator’? Elaborate on the basic information/assumptions etc

L 131: ‘In our case, we deem daily data sufficient. . .’ Why?

L 135: Please elaborate in what sense/how ‘the two are partly related’.

L 136: ‘This approach has been successfully tested. . .’ Which approach? The entire section? Please specify what has been tested before. Also elaborate how this manuscript now is different to the numerous citations of the authors previous works. What is scientifically new?

L 144: For this manuscript to be a self-contained work, please briefly elaborate the main characteristics of this classification

L 142-154. I think the interdisciplinary/non expert reader might find it difficult to see the points described in the figures mentioned. Some additional elaboration, guiding the reader to important points of the figure will be helpful.

L 156: Please briefly elaborate the differences in the 'temporal variability', that lead to the suggestion of 'well-separated' dynamical systems characteristics.

L 166: 'clear dynamical separation…' Visually they do not look so 'clearly' separated… Please elaborate why the authors think there is a 'clear' separation in 3d

L 170-171: 'display lower d than the eddy-driven jet regime, yet a higher theta…'. This is true for the centroids of the merged jet regime, however not for the centroids of the mixed cases, where there is quite an overlap of the space occupied by d and theta!

L197 and Fig 5: Please add to the manuscript the criteria used to determine which points are plotted in red (i.e. how was the period of day 2895-to 4076 selected?).

L 201: it is not the 'dynamical systems metrics' that 'identifies this anomalous transition', but rather the authors by analysing the output! Please rephrase.

L 215-216: Again, as mentioned above, there is no 'clear separation of regimes' in Fig 3! please rephrase. Additionally, currently it reads as if the existence of a clear separation dictates the choice of the 10th and 90th percentile… This is not a proper elaboration, please rephrase.

L 254: To me these are not 'intuitively related to the concept of predictability'. Please elaborate more on why the authors think this is the case.

L 255: why would be a low d be 'more predictable'? Please add explanation for readers to understand

L 265: how do the authors define ''successfully'? As 'success' is a relative term, I suggest rephrasing to avoid ambiguity.

L 269: Please also discuss the role of the filter used on the data, and if the results would have been the same if no filter would have been used.

L 273: Please add sentence to why the authors think/possible reasons the method does not find a single eddy-driven jet in the reanalysis data.

Figure 2a) For ease of interpretation/completeness please add the symbol x with meaning to the legend

Figure 3 (and for all your future figures in publications) Please do not use read and green colours together in a figure. Such a colour choice is not 'colour-blind safe'! Please prepare figure in line with the 'manuscript preparation' guidelines of ESD https://www.earth-system-dynamics.net/for_authors/manuscript_preparation.html (see point 7 under 'Figure composition'. 'please keep colour blindness in mind and avoid the parallel usage of green and red. For a list of colour scales that are illegible to a significant number of readers, please visit ColorBrewer 2.0.' Additionally, for consistency, please use the same legend labelling as in Figure 2 and 4, particularly as the abbreviations used have not been introduced before.

Figure 4: Please add legend that explains the different colours (i.e. colour scale) used in the plot

Figure 5: For completeness, please add units to wind. I presume that it should read 'the red dots in (a, b)' not (a,c)?

Figure 7: Pressure level please use linear y-axis scale or elaborate why a non-linear axis type was chosen.

Figure 8: Please adjust the colour scale and limits to match the actual absolute maximum value. In 8a &b, the darkest colour shade does not seem to appear on the map. i.e the colour scales should only show the maximum value.

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