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Nonlinear Processes in Geophysics Discussions
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Interactive comment on "Network-based study of Lagrangian transport and mixing" *by* Kathrin Padberg-Gehle and Christiane Schneide

Anonymous Referee #1

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The authors of the above manuscript present a methodology for the characterization of flow systems from a Network Theory perspective. They introduce an unweigthed, undirected network where nodes are represented by Lagrangian particles and links are established when the distance among pairs of particle trajectories results to be smaller of a given threshold ε during the considered time interval. Then, local measures such as degree, average neighbors degree and clustering coefficient are computed. A spectral partitioning algorithm is also used to find network clusters using the graph Laplacian associated to the adjacency matrix of the network. Such methodology is applied to an ideal flow system, the Bickley jet, and to a realistic one describing the Antarctic stratospheric circulation.

The general topic of the paper is interesting; indeed Network Theory tools are becoming increasingly popular in many research fields, including Geophysical Flows. The approach here proposed demonstrates its numerical efficiency and broad applicability also to “dirty” datasets and introduce measures that could be useful for further applications to flow systems. However, the method of construction of the network, as it is presented in the paper, does not address the problem of its robustness. This point is extremely relevant because it would affect the reliability of future possible, applications to realistic cases. Moreover, the authors do not discuss similarities and differences of their study with other, relevant, network approaches to dynamical systems that showed up in the last decade. Finally, the authors do not provide solid theoretical foundations for the measures introduced in the paper giving only visual interpretations, mainly steered by the previous knowledge of the systems studied.

Overall, I think this manuscript needs a extended revision to address all the below points before reaching the standard for publication in Nonlinear Processes in Geophysics.

MAJOR POINTS:

– The authors do not provide a proper study of the robustness of their networks to the choice of the crucial parameter ε. They only use 4 different values and plot the results that, in addition, seem to be really sensitive to such parameter. A proper robustness study of ε is necessary, exploring a larger set of values and understanding the reasons behind the sensitivity of the network to this threshold. About this issue, I warmly suggest the reading of the paper: “Ambiguities in recurrence-based complex network representations of time series” by R. Donner et al. in which the authors deal properly with such thresholding ambiguities.

– The methodology here proposed is really similar to the one presented by A. Hadjighasem et al. in “Spectral-clustering approach to Lagrangian vortex detection” and in some sense is an unweigthed version of their approach. The authors should explain more in detail the differences with the approach of Hadjighasem stating which are the advantages and the disadvantages of their method. An explicit numerical comparison of the two ways of building the network would significantly improve the quality of the manuscript.

– In general, the authors do not introduce correctly their work in the field of Network Theory application to Geophysical Systems. Climate Networks are not even cited (e.g. the seminal paper by J.F. Donges et al. "The backbone of the climate network") and Recurrence Networks are cited superficially without exploring the connections of the method here proposed with the Dynamical Systems Theory and the "recurrence" concept. Moreover, the authors do not confront their methodology with other network approaches previously proposed for the study of Lagrangian transport in flow systems (e.g. E. Ser-Giacomi et al. "Flow networks: A characterization of geophysical fluid transport" and M. Lindner et al. "Spatio-temporal organization of dynamics in a two-dimensional periodically driven vortex flow: a Lagrangian flow network perspective") where the local measure that they used were studied for the first time. In summary, the paper presents not only a lack of citations but also a weak framing of its scientific contents with respect to other previous works. The introduction and the Discussion and conclusions sections require also to be revised in the light of the above.

– The local measures proposed in the paper i.e. degree and clustering coefficient show an interesting detection power for mixing and no-mixing regions. However, this point is not well developed from a theoretical point of view. Which are, for instance, the quantitative relations among the degree centrality in this kind of networks and the classical Lagrangian diagnostics? Why the clustering coefficient seems to detect region of weak mixing? Maybe it could be related to some retention propriety of the flow? What is the meaning of a spectral analysis performed on a adjacency matrix that has been built thresholding Lagrangian trajectories distances at different times? Addressing these questions would certainly increase the impact of this paper, making possible connections with more theoretical approaches to Dynamical Systems.

SPECIFIC COMMENTS:

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Discussion paper

– Abstract (line 5): not clear, “Lagrangian trajectory data” is a vague definition. Please, specify that the author refer to low-resolution or incomplete data.

– Abstract (line 10): average neighbors degree and clustering coefficient are missing from the list of local network measures, please complete.

– Introduction (line 26): the author should compare here their approach with the work by A. Hadjighasem et al. here.

– Introduction (line 31-32): node degree and clustering coefficient have been already studied in the context of Lagrangian transport (E. Ser-Giacomi et al. “Flow networks: A characterization of geophysical fluid transport” and M. Lindner et al. “Spatio-temporal organization of dynamics in a two-dimensional periodically driven vortex flow: a Lagrangian flow network perspective” and V. Rodriguez-Mendez “Clustering coefficient and periodic orbits in flow networks”). The authors should discuss differences and similarities of their way to compute these metrics comparing with the ones defined previously in the literature.

– Network of Lagrangian flow trajectories (line 9): please reformulate, the expression “network of these Lagrangian trajectories” is confusing.

– Local network measures (line 17): “immediate importance” is not a proper definition for the degree centrality.

– Local network measures (line 26-27): again, the authors should refer to the papers that introduced these kind of network measures previously and not only add self-citations.

– Local clustering coefficient (line 2): what the authors mean for “certain subgraph”?

– Spectral graph partitioning (line 10-13): connected components and communities are not the same thing, please reformulate.

– Bickley jet (line 16-17): as wrote in major points: a study of robustness is needed.

See: “Ambiguities in recurrence-based complex network representations of time series” by R. Donner et al.

– Bickley jet (line 23-24): again, the strong sensitivity of the clustering coefficient to the choice of the threshold demonstrates that a deeper study is needed to address the robustness issue. Note that the clustering pattern is almost inverted just switching from ε=0.1 to ε=0.2.

– Bickley jet (line 29): It is interesting that the clustering seems to be more robust in the low-resolution case, the authors could discuss this point and try to explain the reason.

– Stratospheric polar vortex (line 11): Here the authors use just one value of the threshold ε. It is not sufficient. I think that, if they would perform a systematic robustness analysis for the idealized flow, here it would not needed to repeat it. But, at least, they should show the results for a few values of ε, possibly selected accordingly to the indications from the robustness analysis for the Bickley jet.

– Stratospheric polar vortex (line 12-14): The authors, for consistency, should show the results of the calculation of the clustering coefficient also for this more realistic case.

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