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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

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The paper entitled "Network-based study of Lagrangian transport and mixing" by Kathrin Padberg-Gehle and Chrisiane Schneide presents a method for identifying mixing regions and coherent sets in fluid flows. It constructs an undirected and unweighted network among trajectories of fluid elements: links represent at least one close encounter between the corresponding trajectories. Classical network measures and graph partitioning schemes are then applied. The method is tested numerically in 2D flows with pronounced vortical features: in a conceptual geophysical model and in atmospheric reanalysis data.

The method introduced in this paper is novel (in particular, it is essentially different from other network approaches that have appeared so far in the literature of fluid dynam-

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ics), and it may represent a significant technical advancement in the topic of mixing and coherence in fluid flows. The assumptions underlying the method are plausible, and the results of the numerical tests are convincing about its performance. Its main advantages are its low computational cost and its applicability for sparse or incomplete trajectory data.

The paper is well-written and relatively short, and I find the very concise formulation to be an advantage. There are a few points, however, where giving further details or providing a longer, more general discussion would be useful or even necessary. In certain cases, even further numerical work may be needed.

Based on the above aspects, I definitely recommend to accept the paper, with certain amendments I list below.

Crucial observations:

- An important aspect is the dependence of the constructed network and of the extracted features on the time interval considered. This issue should be discussed. I am thinking of some discussion similar to those in Sections I.b. and V. of Rypina and Pratt (2016). Furthermore, it might also be explained shortly for the example of Bickley jet what would happen if shorter or longer intervals of time were taken. What is more important, however, is a deeper analysis of the case of the stratospheric polar vortex from this point of view. In particular, after one of the two vortices dissolves, the particles formerly composing the dissolving vortex are dispersed within the "background sea", i.e., the larger region around the polar vortex that constitutes a different flow regime. As a result, this "background sea" is "polluted" by light yellow particles in the right plots of Figs. 7 and 8. It should be explained (and maybe numerically confirmed) that this would not happen if the network were constructed from trajectory data only from late October. I would also be happy to see the plots when the network is constructed from trajectory data from 1st to 26th September: maybe the polar vortex is delineated more clearly with this choice, with less green colour seen in the "background" regime. Not directly related to these considerations, I would be interested as well to know if the dissolution itself of one of the two vortices can explicitly be detected by some network measure (along with some appropriate choice of the time interval)?

- The relation of the network measures applied in this paper to the trajectory encounter number of Rypina and Pratt (2016) is described on lines 30-32 of page 2, 24-26 of page 4, and 20-21 of page 11 as "capturing very similar information, "being very related", or that "there is an obvious link". In my understanding, the trajectory encounter number of Rypina and Pratt (2016) is _exactly_ the same as the node degree d_i introduced in Section 3.2 of the present paper. The difference is that the definition here is expressed in a more formal way, and, more importantly, this quantifier is placed here in a much wider context (that of the trajectory-based flow network). I think that this has to be formulated clearly at each occurrence (enumerated above), and references to other network measures (average degree of neighbouring nodes, local clustering coefficient) have to be completely avoided in this relation.

Further specific points:

Page 4, line 4: "By construction": It would be better to write: "By our choice".

In the same line: "so there are": It should be "since there are" or "i.e., there are".

Page 5, line 13: A period is missing after the parentheses.

Page 5, lines 15-16: A reference should be included for the statement.

Page 5, line 25: Maybe a reference can be included for the k-means as well.

At the beginning of Section 4.1: I understand that the Bickley jet is just an arbitrary example, and the main motivation for choosing it was, I guess, the presence of results in the literature that are comparable to those in the current paper. Still, it would be useful to mention the geophysical relevance of the Bickley jet (especially its relation to the other example: the stratospheric polar vortex), and the main properties of the flow with the current parameter setting (like the presence of a meandering central jet and of three regular vortices on each side).

Page 6, line 10, and also page 10, line 10: "we output the particle positions ...": Are only these positions considered for the network construction? If so, it should be explicitly stated (and this raises further, maybe crucial questions about the dependence on the choice of the output). If not (as I suppose), where is advantage made of this special filtering of the trajectories in the paper? (For e.g. the interpretation of the figures, informing the reader about this choice of outputting is not needed.)

Page 6, lines 21 and 28: "Spurious" is not an appropriate word here (it approximately means "false"). "Fuzzy" may be a good substitution.

Page 6, line 24: What does "singularity" mean in the present context? Some more explanation should be given.

Page 8, line 5: "four eigenvalues" should be "three eigenvalues".

Figure 6: It might be mentioned very shortly that the clustering finds a few false yellow points for the sparse case.

Page 11, line 14: After the expression "sparse data", it may be emphasized that this is illustrated by case (ii) in the Bickley jet investigation.

Additional general considerations:

- I believe that the applicability of the proposed method is not restricted to volume-preserving flows. For example, this method might be especially useful in open contracting dynamics. It might be beneficial to mention this in the paper, and to indicate that the numerical examples discussed in this paper originate from volume-preserving flows.

- I think that choosing epsilon such that the graph is not connected may also give insightful information in certain cases, especially in non-volume-preserving dynamics, which could be mentioned. Anyway, it should be more clearly stated in line 16 of page

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3 that the present paper considers choices of epsilon only that give a connected graph.

- Would the authors consider to be a useful possibility to count the total number of encounters between trajectories occurring in the course of time, and to construct a weighted network based on these numbers? If so, it might be mentioned in Section 5, as a natural extension to the presently proposed method.

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