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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Detecting and tracking eddies in oceanic flow fields: A vorticity based Euler-Lagrangian method" *by* R. Vortmeyer-Kley et al.

Anonymous Referee #3

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The authors consider the classical problem of detecting and tracking eddies in flow fields (in the title the adjective 'oceanic' is present, but the paper is about kinematic flows). To do so they develop a variant, vorticity-based, of the so-called 'Lagrangian descriptors', and evaluate it in model kinematic flows, as compared with other Lagrangian and Eulerian methodologies. There is some interesting material there, but in my opinion, the paper in its present form does not achieve the quality level required to recommend publication in NPG. In the following I summarize the main points that, in my opinion, would require significant revision:

- There is a huge literature on the problem of eddy detection, coming from very different scientific communities. Thus it is increasingly complicated to do something really

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new and to do justice to the vast literature. I should recognize, however, that the authors do a reasonable summarizing job in their introduction. Unavoidably, there are important recent results missing. From the part of the literature I know, I feel the following two references merit some citation and discussion: Karrasch D, Huhn F, Haller G. 2015 Automated detection of coherent Lagrangian vortices in two-dimensional unsteady flows. Proc.R.Soc.A 471: 20140639. http://dx.doi.org/10.1098/rspa.2014.0639 Haller G., Hadjighasem A, Farazmand M, Huhn F Defining Coherent Vortices Objectively from the Vorticity http://arxiv.org/abs/1506.04061

- There is a number of imprecise or even false statements in the paper. Here is a selection of them: * p. 2, lines 26-28: It is stated that algorithms to find DHT rely on 'Lagrangian descriptors'. Please note that DHTs were defined and computed many years before the introduction of the Lagrangian descriptors. * p. 2, line 31: This sentence makes no sense: 'The unstable manifolds are often called material lines in 2d () and surfaces in 3d flows ()' * In many places the authors use the word 'fixed point' for what are special elliptic or hyperbolic trajectories (moving, and then not fixed at all): abstract, pages 5, 6, 7, 8, 9, 15, 17 ... this is deeply misleading.

- In Mancho et al 2013 it is clearly stated that essentially any fluid property can be integrated along trajectories and provide a suitable 'Lagrangian descriptor'. In this sense the use of the vorticity is just another example of 'Lagrangian descriptor'. I find the name 'Euler-Lagrangian descriptor' and the emphasis given in the discussions to the mixed character rather inadequate.

- I hardly can see any 'manifold' in the plots of M and specially of M_v in Fig. 3. Perhaps tau=0.15 is too small, or the contrast of the figure is not enough.

- At a first sight it looks incorrect to say that M, at variance with M_v, can not distinguish between elliptic and hyperbolic areas, since in any plot of M one can clearly identify them. But after some thinking I recognize that there is a real advantage (perhaps the only one) of M vs M_v, which is the fact that ellipticity and hyperbolicity are

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simply assessed by the maximum or minimum character of M_v, much more easy to automatize that the more complex neighbourhood exploration needed for the case of M. But then I do not understand (and the authors do not give any hint of it) why in Section 4 they say they need a combination of M_v and M, instead of just M_v.

- I think that the most original part of this research is the assessment of the behaviour of the different indicators under different types of noise. Nevertheless, the definitions of noise types in page 11 are all incomplete: for type 1 and 2 one can not reproduce the paper results unless the authors define 'noise strength', given that for white noise this would depend on the particular spatial and temporal discretizations used, which are not completely stated. For type 3, it is only after reading a comment in the Supplemental material that one begins to understand that noise is added to the functions h1 and h2, but again, 'strength' or 'noise level' should be properly defined.

- In the Supplemental material, Sect. S1 there is no indication on how the time-dependence, needed to define T_c, is introduced in the seeded eddy model. Also I find very convoluted (and not well explained) the way the radii of the eddies are sampled. Since at the end the authors restrict to 15-25 km radii, it seems to me that all this complexity is irrelevant and that anything uniform or Gaussian in that range will give the same results.

- Errata: * There is a missing square of the velocity in Eq. (3) * Page 12, line 14: signal to noise ratio small? or large?

In summary, I do not recommend publication of the paper, and recommend extensive revision.

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