Response to Reviewer #3

We would like to thank the reviewer for his/her thorough analysis of our paper and the suggestions given to improve the manuscript. We have addressed all the concerns of the reviewer and have rewritten the manuscript accordingly. In the following we give detailed answers to the questions raised and indicate the changes in the manuscript made as a response to the reviewers suggestions.

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

We would like to thank the reviewer for pointing out these two recent publications. The first of them we already cited in the first version of our manuscript. We were not aware of the second paper, since it had not appeared yet in a peer-reviewed journal. In a comment to our manuscript, this paper was also pointed out to us and we have now cited it. Additionally, reviewer #1 suggested several other citations, which we have included too.

*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.*

We agree with the reviewer, that the concept of DHT has been developed earlier and have changed the corresponding formulation in the text.

That the unstable manifolds are often called material lines is taken from the literature, where one can find these formulations rather often. But according to the suggestion of the reviewer we have removed this sentence since it does not add to the content of our manuscript.

We thank the reviewer for pointing out the misleading formulation of fixed points in the manuscript. We have rewritten the whole text for the introduction of the Lagrangian descriptor to avoid any confusion. Indeed, when we were writing about fixed points we meant indeed the elliptic and hyperbolic trajectories. We have changed that in the current version of the manuscript.

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.

We completely agree with the reviewer and also reviewer #1 that already Mancho et al. 2013 pointed out, that any fluid property can be used to construct a Lagrangian descriptor. This has been mentioned by us explicitly already in the first version of the manuscript (cf. the sentence "As already pointed out by Mancho et al. (2013) any intrinsic physical ..." in the beginning of the paragraph before formula (4)). Our motivation to introduce the name Euler-Lagranian descriptor was a more practical one. Because we found it difficult to read talking about one and another Lagrangian descriptor, we introduced a distinction by the names Euler-Lagrangian for one of them and Lagrangian for the other. Since this has been found misleading by two reviewers (#1 and #3) because it would look like the definition of a new descriptor, which indeed is not the case as we of course know, we have changed this in the revised version. To emphasize this we have now avoided the name Euler-Lagrangian descriptor in the title and throughout the whole manuscript. Furthermore we have even more than before emphasized that the original idea had been already formulated in Mancho et al. 2013. We now write "We would like to emphasize, that it has been already pointed out by Mancho et al. (2013)…".

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.

We have changed the colorcode to improve the contrast, because a larger tau does not lead to a clearer structure. Unfortunately, the colorcode does not take into account colour-blindness, but we did not find any colorcode with enough color-dimensions that is also valid for color-blindness.

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 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.

As the reviewer pointed out, the possible distinction between elliptic and hyperbolic points (more general distinguished hyperbolic trajectories and distinguished trajectories surrounded by an elliptic region in the sense of Mancho et al. (2013)) is the most important property of M_v to make the detection of eddy cores in flows easier, since one does not need an additional criterion to discern distinguished hyperbolic trajectories and distinguished trajectories surrounded by an elliptic region in the sense of Mancho et al. (2013) as if one would use M.

For the detection of the eddy shape we have used previously a combination of M and M_v because M shows in our test case a clear line of minimum M values that was easier to detect automated than the line in M_v. In general manifolds correspond to singular lines of M and M_v(Mancho et. al. 2013). To construct an eddy shape detection that is more general and only based on M_v, we have improved the shape detection. The improved shape detection relies on the assumption that the eddy boundary is the largest closed contourline of M_v where M_v is an extremum (large gradient of M_v). We have now rewritten the text accordingly and use now only M_v for the detection of the shape.

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.

We have rewritten the definition of the noise to clarify how the noise is applied and what we take as a noise strength.

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.

The eddies in the seeded eddy model live infinite (which is not very realistic). We have chosen this setup because we were only interested in the detection of the different eddy shapes. The way of choosing the eddy radii was due to the fact that we would like to use the same distribution of radii as Abraham (1998) but restrict ourselves to a specific part of the distribution. We agree that one can even use a simpler distribution, because the model is already very artificial and simplified. In the revised version with have changed that completely: We have removed this rather artificial example of the eddy seeded model and replaced it by an example of a velocity field of the western Baltic Sea according to the suggestions of reviewer #2.

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

Thank you very much for pointing to those typos. We have made the corresponding corrections.