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Interactive comment on "Sensitive dependence of trajectories on tracer seeding positions – coherent structures in German Bight surface drift simulations" *by* Ulrich Callies

Anonymous Referee #1

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The manuscript investigates Lagrangian metrics such as FTLE and absolute/relative dispersion using model results in the German Bight, showing the high variability of LCSs and enhanced space variability of trajectories close to ridges. The results are interesting but certainly not novel or unexpected, and the paper lacks in my opinion of clear focus and motivation. The author mentions several motivating applications, such as characterization or guidance for the observing system, but it is unclear how this would be carry out.

I think the paper needs an extensive revision or even better a re-submission, where the motivations and the elements of novelty are clearly indicated and developed. Also, there are several specific points that need further clarification, as detailed in the following.

Main points

1) As mentioned above, there is an extensive literature showing the high sensitivity of particle trajectories to their seeding and the use of LCS to characterize it, so the results presented here are not new. I think the paper needs a novel angle, and a more specific motivation to make the present results new and interesting. I thought that the angle mentioned by the author in the Introduction regarding the characterization of an observing system composed of fixed points is interesting. But it needs more focus and more practical applications. For instance, could the results be used to quantify uncertainty at the stations using as proxies the distance from ridges? Or could they be used to indicate areas of influence of the stations, in terms of LCS patterns of dispersion properties? Investigating this type of questions would be very useful from the application point of view and could lead to new results.

2) Since the results are based on the BSH model outputs, it is very important that the model set up and its validation are adequately described, significantly improving Section 2.2. This is especially relevant since the model based results are envisioned to be used in support of the observing system, possibly also in real time. More specifically, has Lagrangian validation ever been performed using drifters? It is also important to be up front regarding model limitations. For instance, given the 1 km resolution we can expect that coastal submesoscale is only partially resolved at best. Also, if the model is hydrostatic, we cannot expect that near surface divergence processes are correctly described.

3) The description of the used techniques in Section 2.4- 2.5 should be improved, indicating also possible limitations and clarifying definitions. For instance, is the definition of FTLE in eq (3) valid in the case of 2-dimensional flows (as the text at line 120 seems to imply)? Also, what is the diference between eq (4) and (5) for dilation? From the

text (line 144-145), they seem to indicate the same thing, but it is unclear. Indeed the results in Fig.1b,c are quite different.

4) In general, I think that the text commenting the results should be more realistic throughout the paper. For instance the comparison between LCS ridges, and the 2 forms of dilation in Fig.1 (lines 188-195 and lines 312-13) is very positive, while I fail to see a good comparison between the figures. I do not see a “striking similarity” between Fig.1a and 1b, where the main North-South ridge is absent. The author acknowledges the clear difference between Fig.1b and 1c, but I do not understand the point of the comparison, given that the model itself is not well suited for this diagnostics. Also the comments on Fig.4 do not seem very grounded to me. In a case with very little gradients, except for the obvious coastal ones, as in Fig.4a and at some extent 4c, it is impossible to draw any meaningful conclusion.

5) Finally, and very importantly in my opinion, new diagnostics and metrics should be investigated, related to the observing system as mentioned in point 1). How can LCS be used to evaluate the observing system? How do LCSs vary on time? At which scales? Which proxies can we use to quantify these changes?

More specific points

The Introduction (and possibly also the title) should be re-written with more focus toward point 1) above. More in details, many phrases are unclear. Some examples are listed below - below line 20: “deficiencies of the underlying hydrodynamics...”. Is this phrase indicating subgrid uncertainties or what? Deficiencies is certainly not the right word - around line 35. Discussion on local-versus nonlocal is not very precise. Indeed, local relative dispersion has been shown to be much faster at small scales and initial times than non local (Poje et al., 2014). It should also be clarified throughout the text wether the emphasis is on mesoscale or submesoscale dynamics

Section 2.1. It would be useful to mention from the beginning (lines 80-85) the geographical extension of the German Bight (lat/long are now mentioned at line 128 in

Section 2.4), and clarify that the area is depicted in all the figures.

Section 3, on results. The author shows 3 examples of LCS (Fig1, 2) for three different flow realizations and dates, 1 example of particle stats (Fig.3) for an other realization, and finally SST (Fig.4) for a mix of realizations. It would be better to focus on 3 cases only, and compare LCS with particle stats, as well as SST.

Section 4. provides a broad discussion on FTLEs and their applications, but there is no clear connection with the present results. Indeed, most of the information are more suitable for the introduction, and in any case should be trimmed and focused on the paper's goals

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