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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

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Reading the discussion of this paper, I found a fundamental error that has apparently gone unnoticed so far. In lines 312–320, the author argues that:

"In this study, analysed structures were remarkably consistent for fields of FTLE, FDLD, dilation rate or measures of dispersion. Differences between the FTLE and FDLD fields discussed by Huntley et al. (2015) could not be seen on the spatial scale considered. Fields of path-averaged finite-time Lagrangian divergence FDLD corroborate the role of backward FTLE ridges as lines of convergence (see Fig. 1). This relationship agrees with the results of many oceanographic studies. Olascoaga et al. (2013, their

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Discussion paper

Fig. 1), for instance, provide an example of how a chlorophyll a plume in the Gulf of Mexico coincides with an attracting LCS. Lehahn et al. (2007) found satellite observations of chlorophyll filaments in the northeast Atlantic to well agree even with simulated geostrophic transports, contracting at and stretching along material lines. Referring to Lapeyre and Klein (2006), Lehahn et al. argue that an ageostrophic secondary circulation injecting nutrients from deeper layers may trigger further chlorophyll production."

The author implies that FTLE detects confluence caused by convergence (along-path sampling of the negative divergence of a horizontal velocity). In reality, the attraction identified by FTLE is often due to confluence with negligible divergence, or even confluence in the presence of positive divergence. Indeed, the author himself mentions examples of divergence-free velocity fields. The Olascoaga et al. paper uses altimeter velocity, and the geostrophic velocity in Lehahn et al. will similarly have negligible divergence. These examples are therefore counterexamples to the author's conclusion. An example of confluence in the presence of positive Eulerian divergence can be found here (Fig 4): https://www.nature.com/articles/s41467-020-16281-x

If the author wishes to suggest that geostrophic (or more generally divergence-free) strain may induce ageostrophic circulation, then that is fine, and it has a precedent, e.g. https://www.nature.com/articles/s41467-019-10883-w.

However, to diagnose whether the attraction detected through FTLE is due to divergence-free confluence or convergence, additional computations are required. One can quantify the along-path change in area, as described in appendix A of this supporting information: https://static-content.springer.com/esm/art%3A10.1038%2Fs41598-018-23121-y/MediaObjects/41598_2018_23121_MOESM1_ESM.pdf (The article can be found at https://www.nature.com/articles/s41598-018-23121-y note this is an additional example of divergence-free confluence.)

By computing the Jacobian determinant (fractional change of area) of the flow maps used to compute LCS, we can diagnose when along-path convergence is negligible, in

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which case attraction is due to divergence-free confluence. However, if the change-of-area is substantial, the relative contributions of convergence and confluence still need to be quantified. A Helmholtz decomposition of the velocity may be helpful, yet going this route poses additional challenges.

Relating confluence with convergence without proper diagnosis is not uncommon, and therefore requires clarification as we work to understand the influence of submesoscales on dispersion. Please consider accommodating my comments in your paper.

Disclaimer: I did not read the full paper.

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