Summary

This paper makes use of a (comparitively) high resolution set of drifter data to test an array of LCS detection methods that have, in the past, predominantly been applied in idealised numerical experiments or on artificial Lagrangian particles released in observed ocean currents. Application of these methods to detecting LCS in real ocean currents is a long-running motivation for their development, and this work is an important step in that direction.

For the most part, the paper is thorough and well-explained. However, I do have some revisions to suggest, particularly focused on the difficulties of interpreting the LCS methods on such a sparse dataset. A little extra care in linking the results of this study to other datasets would go a long way in strengthening the argument that useful/robust LCS signals have been detected from the SPLASH dataset.

Comments

Some of my suggestions are as follows:

Line 82 Do you mean high costs?

Line 243 Do the other methods of divergence/vorticity estimation also break down as the aspect ratio increases? Given that filamentation is bound to occur on any set of particles considered, this aspect ratio condition seems to be very detrimental to the discussion of dilation and LAVD.

To improve this discussion, I think the resulting dilation/LAVD from using different methods should be included, or there should be a more detailed explanation as to why these methods were not employed.

If the lack of confidence in the dilation and LAVD are simply natural consequences of applying these measures to a data set where the exact divergence and vorticity are not known , then that is in itself an interesting result, that has wider implications for the potential use of these two measures in future investigations. However, if that is the case, then I believe it should be a more predominant part of the discussion.

NCOM model comparisons Given the small number of drifter trajectories (compared to the number of Lagrangian particles that would typically be considered in dynamical systems applications), claims that the results indicate the presence of LCS are quite tentative without additional evidence. The NCOM and simulated 'SPLASH-like' data contained in the supplementary material appears to provide this evidence, so why is there so little reference to this in the main text?

I would suggest that there should at least be some demonstration of the background density, surface velocity fields and 'true' fields from the NCOM data sets in the main text of the paper. These would demonstrate the presence of the fronts, as well as the large scale anticyclonic feature which are argued to be the source of the LCS signals in the SPLASH dataset and the reason the frame-dependent.