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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Lagrangian coherent structures in tropical cyclone intensification" *by* B. Rutherford et al.

Anonymous Referee #1

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This manuscript addresses the interaction of Vortical Hot Towers with the system-scale vortex in the context of tropical cyclone intensification. This is done using a new Lagrangian diagnostics that determines the coherent structures of the flow and their interactions.

The point the authors want to make is quite interesting, from my point of view. However, the authors do not succeed in really demonstrate what they assert. Most of the time, strong statements are made but there is no observational support from the figures to confirm them. In addition, I find the paper hard to follow: we do not know if the statements (Section 3) come from the literature. The section on the conceptual model for vortex interaction does not rely on firm grounds, or on the literature.

From this deficiencies, and the other comments I have (see below), I am inclined to

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reject the paper, as it needs more than a substantial major revision.

Main comments:

1. The abstract states that "Our primary finding are that VHT (i) are coherent Lagrangian vortices that (ii) create a turbulent mixing environment" (line 16, p. 28126). However, I do not clearly see in the paper a proof of (i) or (ii).

2. In many places, there are strong statements, but the authors do not provide any proof using the figures. So one may be quite skeptical and think that the authors just see what they expect to see (and not what really occurs). Examples are

   (a) Paragraph 3.3.1: "Attracting LCS are found... without stretching it". There is no reference to a particular figure. If I look at Figure 4, the first statement is not true at 7km.

   (b) Next sentence: "Repelling LCS guide VHS towards the center of the LCS". Where do you show that?

   (c) Paragraph 4.1: You state that LCS and VHT have a lifespan of 1h. How is computed the lifespan of the LCS and the VHT? By visual inspection on the time evolution? That is in contradiction with the last sentence of the abstract: "These hyperbolic structures [...] outlive the VHTs [...]."

   (d) Paragraph 4.1: why don't you refer to Fig. 7 to show that "merger and disappearance of VHTs alters the saddle-type geometry of the LCS". By the way, what is the stability of an LCS? On which observational fact can the authors say that "The difference in radial velocity between VHTs ... LCSs.

   (e) The sketch of Fig 6c may be potentially right. But can the author prove the statement of line 11, page 28148?

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(f) and so on...

3. Line 14, p. 28134, it is stated that VHT are a type of LCS. Can the author elaborate on that?

4. Equations (5, 16, 17): the idea to compute the new diagnostics for mixing is based a particular basis involving the velocity vector. One can object that this basis will not be Galilean invariant, so that the results may depend on the curvature of the flow (as is the case for the Okubo-Weiss parameter in 2 dimensions). Can the authors discuss that point?

5. The authors should explain more in details what is the physical meaning of matrices A and Ψ and their components. This discussion is lacking and strongly needed for people that do not want to read previous papers of the authors. If this is not done, the reader cannot understand why we need to diagnose such quantities. In addition, at the end of paragraph 3.3.1 the fact that ϕ is related to shear should be put in emphasis.

6. From Eq. 14, $\Psi_{22} > 0$. So why Ψ_{22} is negative in Figure 3?

Minor points:

1. There is no practical definition of the VHT in paragraph 1.1.1. Can we identified VHT from θ_e plots? By just inspecting surface θ_e at the surface?

2. Paragraph 1.1.2: The FTLE was **not** proposed by Haller and coauthors as a tool to measure trajectory separation. It was **first** (at least) by Pierrehumbert (1991, Physics of Fluids) who also introduced the notion of “transport barrier”. The authors should correctly cite the litterature, instead of indirect papers on the subject.

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3. Paragraph 1.1.2: Concerning the application of the FTLE to the atmosphere, many authors by the past have studied it: Pierrehumbert 1991, Pierrehumbert and Yang 1993, Ngan and Sheperd 1999, von Hardenberg et al. 2000, Joseph and Legras 2002, Scott et al. JGR 2003, Cohen and Schultz 2005, D'Ovidio et al. 2009. Other techniques such as Finite Size Lyapunov Exponents are also being used for the same purpose.

4. The fact that "Attracting LCS attract and stretch a tracer blob..." (line 18, p. 28132) is known since a long time ago (see Ottino, ARFM 1990, or his book). These structures applied to unsteady flows are known to be the generalization of the stable and unstable manifold for steady flows.

5. Lapeyre (Chaos, 2002) has shown that the ridges of FTLE materializes the interaction of vortices, as the ridges can join two vortices far apart but that tend to interact at a later time. This study should be acknowledged.

6. The idea of the flow map for Lagrangian trajectories (page 28135) does not come back to Haller and coauthors or Rutherfort and Dangelmayr. This is already stated in Ottino's book ten years before.

7. The fact that FTLE are not well suited for flows such as axisymmetric vortex (or sheared flows) was also discussed by d'Ovidio et al. (2009).

8. Page 28142, line 13. The authors should discuss how their results are dependent on the integration time T.

9. Page 28142, line 19: The authors state that "the existence of LCS are *(is)* dependent on model resolution". This is not true. The LCS is related to tracer blobs (or filaments) that are stretched to the smallest possible scale (what is called the "tracer cascade"). The existence of LCS is independent on model resolution, only, only its exact location.

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10. End of Paragraph 4.1: what is the numerical method to distinguish LCS that last longer that typical VHTs from the other ones?
11. Paragraph 4.4.2. Velasco Fuentes (2005) studied the merging of two vortices and computed the invariant manifolds.

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