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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 #2

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The authors explore the occurrence and coherence of Lagrangian coherent structures (LCS) associated with strong, moist-convective vortices (VHTs) during tropical cyclone (TC) intensification. These LCS are hypothesized to play an important role in the organization of the vortical remnants and in the stirring of moist entropy during the intensification process.

This study is very original and its results potentially of great importance. A very well written introduction (Sec. 1.1) describes the problem at hand and sets the stage for this study. Very unfortunately, the remainder of the manuscript is hard to follow. The presentation, both of the methodology and the results, is unsatisfying. Some potentially important sensitivities and caveats of the methodology are not discussed. The main results are derived from visual inspection of the LCS and the distributions of moist

entropy and vorticity. While such a descriptive approach is perfectly fine for the exploratory study at hand, the LCS, vorticity and moist entropy fields all show a high level of complexity. This reviewer was not able to identify many of the features pointed out by the authors and, most significantly, was not able to follow the authors' interpretation and verify their conclusions. I recommend that the authors emphasize the explorative nature of their study, restrict their attention to the most robust features, and refrain from drawing strong conclusions.

I cannot recommend publication of this manuscript before the presentation has considerably improved and some further concerns have been addressed. More detailed (and hopefully constructive) comments are given below.

(In the following, only the last 3 digits of the page numbers are given)

Major concerns:

1) Insufficient evidence to support conclusions

A highly complex configuration of LCS, vorticity and theta_e emerges during the TC intensification studied in this manuscript. The main conclusions of this study are derived from visual inspection of these complex configurations. This reviewer appreciates the authors' efforts to reduce the complexity by introducing a simplified schematic of vortices, their characteristic LCSs, and associated vortex interaction, and by labeling specific features to aid tracking of these features. Unfortunately, much of the description still remains vague and general. Even with great efforts, it was impossible for this reviewer to follow the authors' arguments and their conclusions. Furthermore, the presentation seems to be biased at times. The authors tend to generalize from specific LCS features associated with some VHTs or theta_e gradients when, on the other hand, it is found that other VHTs/ theta_e gradients are not associated with similar LCSs (e.g., in Sec. 4.5). In Sec. 4.6 the reader is confronted with a description that does not even refer to a figure. How is the reader supposed to follow such a presentation? In Sec. 4.7, we read "The psi-LCSs travel with the vortices and separate them from turbulence" and have to see in Fig. 10d that this is not the case for the vortex region O2 and the turbulent region T11b. In Sec. 4.8 the authors claim that "RL1 remains with V1 until 40 h" and that "V1 . . . becomes part of the . . . eyewall by 45 h after RL1 disappears" but the authors stop tracking RL1 after 30 h. The examples given above are not a complete list of erroneous, biased, or unclear presentation. Based on such "evidence", I strongly recommend that the authors refrain from over-interpreting their results and from making strong conclusions.

2) A conceptual unclarity:

This comment refers to several parts of the manuscript but may be best phrased in association with the schematic of vortices and their manifolds (Fig. 6) and the associated discussion in Sec. 4.4. In general, this reviewer very much appreciates the effort of the authors to provide such a simplified model to interpret the complex configurations found during TC intensification. I struggle, however, to understand how the evolution of vortices and their LCS can be regarded as independent of each other. Such an independent evolution seems to be implied in Sec. 4 (pg 145, line 11 ". . . single LCSs between vortices may appear, disappear, or change stability type."). Figure 6b) shows the manifold structure of two vortices. Why should one of the manifolds dominate to govern the further interaction of the vortices? I am confused also about the authors' statement that vortices are formed by manifolds (e.g. pg 145, line 22). Isn't it rather the flow associated with the vortices (VHTs and their remnants) that form the flow's manifolds?

3) Presentation of the methodology:

My main concern with the presentation of the methodology is that no clear physical interpretation of Psi22, one of the main diagnostics of this study, is given. Psi22 appears in the text the first time on pg 144, line 19, without any physical introduction at all. Afterwards it becomes clear that Psi22 isolates hyperbolic separation. Other characteristics of Psi22, however, remain elusive. Some more information on Psi22 and LCS in gen-

eral is intermingled in the text in sections 3 and 4, but this information is hard to extract. To me, the most important questions are a) Does Psi22 measure three-dimensional separation or separation in one plane? b) If in one plane, what is the orientation of this plane? c) What wind field is used for the calculation? The horizontal wind field only as for the FTLEs? d) More general, the role of vertical motion, vertical separation, and how vertical motion is incorporated in the calculation of the diagnostics (Psi22 and FTLEs) remains unclear. e) The role of the "helical coordinate system" remains completely unclear. f) Ridges of Psi22 are not material lines. In what sense can these Psi22-ridges be associated with flow boundaries? g) In what sense do we expect the Psi22-ridges to "organize the flow" from theoretical consideration alone (not because we find some association with the theta_e field later)? h) Is Psi22 equivalent to FTLEs for a flow without shear?

I strongly suggest that the authors dedicate a subsection to address such questions in a focused and physical meaningful way. Preferably, the authors provide an example from a simple flow configuration to clarify the physical role of Psi22 before attempting to interpret the highly complex flow configuration found in an intensifying TC.

4) Neglecting the time dependence of the co-moving coordinate system:

My reading of pgs. 136 and 137 is that the evolution of a perturbation around a trajectory is described in a local coordinate system (t,b,n) that follows this trajectory. The perturbation evolution is described by the two matrices A and B (Eq. 6). The matrices are constructed such that all time dependencies of the coordinate system (t,b,n) and of the wind field are contained in B. In the following it is assumed that these time dependencies are small and thus they are neglected. a) Why does B vanish for a steady wind field? It is clear that u_t vanishes but why should b_t and n_t vanish for a (curved) trajectory in a steady wind field? b) The time dependencies are assumed to be small compared to what? c) The integration time is set to 1 hour. During this time, for a particle rising in the helical updraft of a VHT, the orientation of the coordinate system may change by 2 pi/ 360 degree. In this case, how can it be justified to neglect the time dependence of the coordinate system?

5) Sensitivity of length of LCSs to integration time

On pg 134 the authors note that '... the length of the hyperbolic LCSs is an important factor in VHT interaction, ...'. This length, however, depends rather sensitively on the integration time employed to calculate the LCSs. The authors provide a sensible discussion of their choice of integration time on pg. 142. Still, one could easily argue that 30 min or 2 hours would be reasonable integration times also. I strongly suggest that the authors demonstrate explicitly the sensitivity of LCS-length to integration time for a select time period.

6) A general comment on the presentation:

It is very confusing for the reader when results of this study are presented as already-known facts. The second-last sentence on pg. 131, and pg. 142, lines 23-24 are only two of several examples. A simple “As we will see below, ...”, or variants thereof, often avoids such confusion.

Minor and editorial comments:

Sec. 1: Footnote 3: “previous footnote” → “footnote 1” pg 131, line 1: not clear to what “new model” you refer Beginning of Sec. 1.2: the change to dynamic-system jargon is rather abrupt pg 131, last sentence: Neither the FTLE nor the Psi22 field is later “visualized in a reference frame moving approximately with the speed of the Lagrangian boundary through the time dependent flow ...” This sentence is confusing to me. pg 132, line 6: “generalized, frame-independent” (?) pg 133 ff, paragraph starting at end of page: At this point in the manuscript, this information is not digestible for the reader. Pg 135, line 1: If a reader followed your suggestion, he/she would never learn what your main diagnostic (Psi22) actually is!

Sec. 2: end of pg 135: I suggest to give the formula for the FTLE as used in this manuscript. pg 136,line 4: in what sense can the radial velocity be dominated by

the radial shear of the tangential wind? Line 11: “moving frame of reference” Aren't we moving along the trajectory already? Do you mean that the orientation of the coordinate system may change?

Sec. 3: Pg 141, line 19: For comparison, can you give an approximate number of VHTs during rapid intensification? Pg 141, line 24: 'to the to the' pg 142, line 1: Are you sure that the sigma-levels in your version of MM5 vary with time? Pg 143, line 10, heating gradient and radial influx: Can you clarify this statement? Pg 143, line 11: It is hard to identify an eye in this figure. Pg 143, last paragraph: This overview of the role of LCSs is vague and not particular helpful at this point in the manuscript. E.g. what means “LCS ... contribute to the fluid dynamics by ... convergence in the boundary layer ...”? I suggest deleting this paragraph. Pg 144, line 3, “fixed-time Lagrangian scalar fields”: Please clarify. Pg 144, Sec. 3.3.1, vertical separation: This seems conceptually important and should be discussed more focused and more clearly (see also 'Major concern' comment above). Pg 145, first sentence: I understand that shear adds to particle separation but why does shear exclude hyperbolic stability?

Sec. 4: Pg 145, Sec. 4.1: For a reader without good knowledge of LCS, this subsection is not helpful. Again, in this subsection it is not clear what is already known about LCS and what is a result of the current study. Please revise carefully, potentially move subsection to a later part of the manuscript or clarify using examples. Pg 145, first sentence of Sec. 4.2: I assume the coherence of LCS follows from the coherence of the VHTs. Your readers, as I did, may become somewhat frustrated by your presentation at this point: Would we expect such coherence in the LCS from theoretical considerations? Has the coherence been documented before? Is it merely an observation in your idealized case? Pg 146, paragraph starting at line 6: The message of this paragraph is unclear to me. The paragraph will benefit from a better introduction of the 'planar projection'. And again: it is unclear whether 'typically' refers to your study or whether 'tangles of FTLEs' have been associated with convective, rotating structures in previous work. Pg 146, Sec. 4.3: For the reader, at this point in the manuscript, the discussion of the azimuthal averages is unintelligible. Why should Psi22 mix inwards? Why is Psi22 associated with the eyewall? What do we learn from a comparison of the azimuthal averages anyways? Pg 147, line 12 and Fig. 6a): What are the properties of the assumed flow in which the vortex is embedded? Uniform flow? Pg 148, line 1-4: Again, is this know from previous studies? Or is this a concept of vortex interaction that you have developed based on the current study? Pg 148, line 20: Why would the LCSs vanish while the vortices persist? Pg 148, last (complete) sentence: unclear Pg 149, line 14: Usually, a saddle point is identified at the intersection of the stable and unstable manifolds, or at the intersection of FTLE-ridges. Here, you identify the saddle point by the intersection of the attracting and repelling Psi-LCS. Please clarify. Pg 149, line 16 ff, "Over the next . . .": The description in the remainder of this paragraph is absolutely insufficient to shed light on the complex configuration of vorticity and LCSs. E.g. ". . . vorticity pools and LCS travel together . . ." is vague, at best. ". . . vortices A and B come very close, yet remain separated by the attracting LCS." This is at odds with the "unstable interaction" described in Fig. 6d and rather reminiscent of "stable interaction" which, however, is supposed to occur along a repelling LCS. Furthermore, I do not see any LCS that separates vortices B and C in Fig. 7d. And: in what sense are vortices B and C "not connected"? Do you imply that vortices are connected by a separatrix? (line 20). At least linguistically, this would be very confusing. Fig. 7: For the sake of clarity and simplicity, I suggest omitting the theta_e field to illustrate vortex interaction. Sec. 4.5: I find it irritating that the authors first present their conclusions (first paragraph) before they attempt to present supporting evidence in the second paragraph of this subsection. The description in this second paragraph is insufficient to support the authors claim that "LCS reveal preferred locations for the convergence of theta_e . . .". Furthermore, it is not straightforward to see how convergence is related to theta_e gradients. Pg 150, line 15: Why do these LCS have so much longer time scales/ lifetimes than the LCSs described in Fig. 7? Pg 151, line 1: Sec. 4.2 does not describe vortex merger (neither does Fig. 6). Sec. 4.7: Is it actually possible to track the turbulent T-regions? (as suggested by the same labeling at different times) Pg 151, line 21: What

are these "primary structures" that you refer to?

Abstract and conclusions, of course, then need revision to reflect the revised results and conclusions of the improved version of this manuscript.

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