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Interactive comment

Interactive comment on "The genesis of Hurricane Nate and its interaction with a nearby environment of very dry air" *by* Blake Rutherford et al.

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This is the author's responses to the review by Anonymous Referee #2 for "The genesis of Hurricane Nate and its interaction with a nearby environment of very dry air". The authors would like to thank the referee for comments, and we feel that we have been able to address all of the areas of concern in the revised version of this paper. These comments have been used to improve the quality of this paper, and our responses to each of the individual comments is given below. The referee comments are given in italic while the author's responses are given in standard font.

Validity of 2D analysis: The manuscript provides little motivation why analysis of Lagrangian boundaries of the quasi-horizontal flow should provide insight into the development of Nate (2011). Certainly, the authors make some assumptions that they had

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articulated in earlier work. These assumptions, however, should be stated also in this manuscript. Importantly, the 2D assumption seems to be in contrast with a statement made by the authors about the role of convection in lobe transport (pg 13, line 28). Clarification is required. Nate develops near the boundary between two air masses. The authors emphasize the large moisture gradient across this boundary. Arguably, the very dry air to the North of Nate is of midlatitude origin and presumably this air mass is also considerably colder than the tropical air mass in which Nate develops. In short, I expect a strong baroclinic zone to the North of Nate and the large-scale, 2D flow follows isentropic rather than isobaric surfaces. I question that the analysis on isobaric surfaces is indeed Lagrangian, in the sense that the authors follow air parcels transported by the large-scale (adiabatic) flow, which is most likely one of the nonarticulated assumptions made by the authors. Convincing justification for the use of an isobaric framework in the presence of strong baroclinicity is needed.

We agree with the reviewer that both baroclinic contributions and vertical motions from convection bring into question the validity of the assumption of 2D velocities. Convection acts to increase the vertical vorticity while decreasing the area and leaving the circulation within any Lagrangian loop, including lobes, unchanged. Thus, the use of 2D velocities in a flow with convection still captures the advective changes to the circulation. The use of isobaric coordinates rather than isentropic coordinates does not change the validity of horizontal velocities used to advect material curves, but only contributes a non-advective flux of vorticity proportional to the pressure vertical velocity (Haynes and McIntyre, 1987). Along the unstable manifold segment that is aligned with the frontal boundary, the tilting flux is approximately 20$\%$ of the magnitude of total vorticity flux which includes the advective flux and the increase in circulation due to contraction of the vortex. The precise computation of these fluxes will be discussed in greater detail in a future paper. In the revised manuscript, we have included isentropic manifolds at the 315 K level, and notice that despite some vertical motions along the frontal boundary, there is no topological change in the manifolds, and only small changes in the fine structure.

The authors focus on the objective identification of hyperbolic Lagrangian structures. Similar methods can be applied to identify elliptical Lagrangian structures, which play an important role in separating a vortex from its environment also. The authors appreciate this role by their qualitative discussion of the 'shear sheet', e.g. on page 6. For a wave's critical layer (e.g. Dunkerton et al. 2009) there is conceptual understanding why the flow boundaries that arise from the environmental flow, and are thus the relevant boundaries, in which the embryo tropical cyclone develops, are hyperbolic structures. Such conceptual background misses for non-AEWs disturbances like pre-Nate, or at least the authors do not provide such background. Therefore, an objective identification of elliptical boundaries will considerably strengthen this manuscript. In addition, the identification of elliptic boundaries would help to introduce the concept of a limit cycle, which is referred to later in the manuscript, and help to define the core of the disturbance, which is undefined in the current version of the manuscript.

We thank the referee for suggesting a further discussion of elliptic Lagrangian boundaries. Much of our discussion in this paper is on the hyperbolic boundaries that are present when there is no distinguished reference frame or hyperbolic structures in that reference frame resulting from the wave flow. The elliptic boundaries are present in all cases of cyclogenesis whether a parent wave is present or not, and in the case of AEW flows, the elliptic structures are located close to closed streamlines in the wave-relative frame interior to the hyperbolic structures. In mature cyclones, elliptic boundaries do play a role in protecting the vortex from its environment because hyperbolic structures do not persist until the point of axisymmetrization. However, elliptic structures protect a developing vortex core from air that has passed through the outer pouch boundary. We have added a description of objective elliptic boundaries, their mathematical definitions, and the concept of a limit cycle to Section 2. These elliptic boundaries are now shown along with the manifolds in Figure 2. The location of the regions of high shear and of solid-body rotation at the core support our previous discussion, and help to show how the shear sheath interior to the outer pouch and external to the core protects the core from air that has penetrated the outer pouch.

pg 1, line 15; vorticity generation by tilting: This aspect is hardly touched on in this manuscript. I recommend omitting reference to this process in the abstract (and in the conclusions).

We have removed reference to the tilting mechanism in the abstract, and 'tilting' does not appear in the conclusions.

pg 5, line 1; and elsewhere; 'Eulerian boundary': There are references in the manuscript to Eulerian streamline patterns that are not illustrated in this manuscript. In addition, there are references to the role of tropical cyclone Lee that are not illustrated either. For the reader, it is rather hard to follow (and appreciate) these descriptions. I suggest using one or two additional figures to illustrate such points; or to keep such references to a minimum.

We have added an additional figure (2a) showing TS Lee and the regions of high PV from Lee that contribute to Nate. The Eulerian streamlines are not important in any part of our analysis, and there is no distinguished reference frame in which to view the Eulerian streamlines. so we have not included them. The Lagrangian boundaries from Lee make it clear that the Lagrangian boundaries, and not the Eulerian boundaries, are those relevant for the transport of vorticity from the Lee flow to the Nate development region.

pg 7, The subsection 'Manifold computations' requires considerable improvements: The authors use phrases like 'some situations' and 'additional options' but it remains unclear if or when other options are used or what methods are applied in other situations. Most importantly, it remains unclear from this description for how long the underlying trajectories have been calculated. It is well known that finite-time Lagrangian coherent structures are sensitive to the integration time. A more explicit discussion of this integration time and a discussion of the sensitivity of the results to integration time are needed.

This section has been improved by removing vague statements and by explicitly stating

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the length of integration time used in both in obtaining the initial curve segments and in advecting the curves. We now also discuss the sensitivity of Lagrangian boundaries to integration time and explain that a 2-3 day integration is required to achieve a closed pouch, while longer integrations increase the amount of filamentation.

pg 10, 'Relation of Lagrangian ...': Unfortunately, the presentation of the results deteriorates rather significantly from here on. E.g., the authors note that PV and O3 is shown and then continue with a discussion of θ_e, the GOES imagery is presented without units, convergence is presumably confused with confluence (pg. 13, line 1), results from WRF at 600 mb (which should read hPa) are compared to results from ECMWF data at 700 hPa, vorticity is confused with mixing ratio in Fig. 5, it is unclear what the difference is between individual panels in Figs. 4-6, : : : The subsection 'Backward trajectories' is very dense and it seems as some important information is not given to the reader. I cannot identify in the figures several features described by the authors. This is of particular importance with respect to the vertical similarity of manifolds and the limit cycle.

The results from page 10 on have been improved significantly. In particular, the above concerns have all been addressed in the revised manuscript. PV and O3 are now discussed where they are first mentioned, and the discussion of θ_e now follows after that discussion. We have converted the GOES data to temperature (K), and corrected the description and units in both the text and the figure caption. We have changed 'convergent' to 'confluent' to describe the flow along the unstable manifold.

We have now computed the manifolds for the WRF simulation at the $\eta = 0.7$ level to make a proper comparison with the ECMWF 700 hPa analysis, and the analysis at $\eta = 0.7$ has also been included for the SST sensitivity and non-divergent experiments.

The text reference to Figure 5 has been corrected to state that the mixing ratio is shown. The different panels in Figures 4 and 6 now show time-evolving manifolds overlaid on vertical vorticity. The labels on the figures and reference in the text have been improved in the new manuscript. The time labels are now easier to read so that the individual panels are easier to distinguish.

The backward trajectory section has been expanded to make it easier to follow, and each of the panels is now discussed individually and in greater detail. We have also provided further details on how the trajectories were integrated. Key features have been labeled on the trajectory plots.

The comparison between the ECMWF and the WRF data is confusing. Importantly, it is not clear how much the results based on the ECMWF data can be trusted. Furthermore, the comparison of the results using the full wind field and the non-divergent flow only is poorly motivated.

The non-divergent wind field is used to demonstrate that convergent flow is not necessary for a pouch boundary to be closed, and is unnecessary for the formation of lobes. However, the size of the lobes that intrude is far greater for convergent flow. We have made the comparison between the ECMWF data and the WRF data more clear, and now state that the ECMWF data show large non-advective fluxes, but has approximately the same topology as the WRF data. While the resolution of the ECMWF data may lead to errors in the fine-scale filaments, the manifolds still closely follow the gradients of tracers such as O_3, indicating that this data is sufficient to capture the larger-scale transport including the lobe transport prior to extreme filamentation.

The conclusions refer to several aspects that have not been discussed sufficiently in the manuscript. The arguments given in enumerations 1) and 2) are plausible but have not been shown in this manuscript. The 'core' referred to in enumeration 5) has never been defined. Finally, the Eulerian streamlines noted on pg 19 have not been shown in this manuscript. The revised conclusions should focus much more on results and insight that is shown and developed in the manuscript at hand.

With the improvements in the revised manuscript, all of the ideas discussed in the Conclusions have now been discussed in the main manuscript in sufficient detail. Arguments 1) and 2) in the enumeration are now shown more clearly in the manuscript and supported by the addition of the Figure 2a showing the attracting line coming from the Lee flow dividing R_1 and R_2. The inclusion of elliptic structures demonstrates core formation, and the core is defined earlier in the manuscript at the end of Section 2.

Technical corrections/ Editorial recommendations: pg 1, line 2: of > or

We have made the correction.

pg 2, line 33; kinematic structures as consequence of invertibility of vorticity: I cannot follow this argument. Please clarify.

We have clarified the statement to indicate that non-AEW disturbances provide no distinguished frame of reference.

pg 3, line 2; 'arm of T.S. Lee': incomprehensible

We have replaced 'arm' with the 'curved vorticity filaments emanating from' Lee.

pg 3, line 5: should read 'for a non-AEW disturbance'.

We have made the correction.

pg 3, line 12; suggest: initiated > first identified

We have made the suggested correction.

pg 5: should 'vortex strip' read 'vorticity strip'?

We have made the suggested correction.

pg 5, line 20: according to the references it should read 'Rutherford and Dunkerton, 2017.'

We have corrected the reference.

pg 6, line 4: This sentence seems to lack something, maybe 'vorticity' after 'system'?

We have changed the wording to ‘system circulation’ .

pg 6, line 5; ‘isobaric vorticity substance’ is non-standard terminology, possibly in analogy to the misnomer of ‘isentropic potential vorticity’? Please clarify.

We have changed the wording to ‘isobaric absolute vorticity’.

pg 6, line 14ff: This is an important paragraph, as it introduces the role of elliptic Lagrangian structures. As is, however, it is unclear how this paragraph links to the rest of the discussion at this point in the manuscript. I recommend including a similar discussion in the introduction.

We have added a definition of elliptic structures earlier in the paper which includes the Lagrangian vorticity.

pg 7; LCS: This and several other acronyms are not defined. The concept of a LCS (Lagrangian coherent structure) is not introduced either.

We have added the meaning of the acronym LCS and an explanation of what an LCS is. We have also clearly defined all other acronyms used in the paper.

pg 8; Lagrangian flow: unclear

We have changed ‘Lagrangian flow’ to ‘Lagrangian manifolds’.

pg 9: It would be very helpful to mark R1 and R2 in the figure. In general, I find the idea to follow circulation areas and their merging in a Lagrangian sense quite interesting. With the current presentation, however, the discussion does not provide much insight to the reader.

The new Figure 2a showing the potential vorticity from Lee also shows the regions R_1 and R_2 and the curve that separates them, so that it is clear what regions the circulation values refer to.

pg 9, line 15ff: I cannot follow the role of Lee described in this paragraph.

The revised manuscript includes a new Figure 2a demonstrating the regions of high potential vorticity that originated from Lee, and this paragraph has been edited to point the reader to the key features in the figure.

pg 9, line 29, 30; comment on Lagrangian conservation of vorticity. Why should vorticity be conserved materially?

We have changed the wording so that this sentence could not be interpreted to mean that models other than the ECMWF model conserve vorticity.

pg 10, lobe transport of vorticity: It would be quite helpful for the reader to actually show figures including vorticity.

A figure showing the potential vorticity from Lee has been added in Figure 2a, and Figures 4 and 6 now show vorticity from the WRF simulations.

pg 14, line 8-9. Is the difference between 0.48 and 0.44 significant?

We have changed this section to reflect the analysis on the $\eta = 0.7$ level. The values of area reduction have been changed to reflect the new results, and the new text indicates that the difference between the two simulations is small.

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