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Atmospheric Chemistry and Physics
Discussions

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

Anonymous Referee #2

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The authors present an analysis of the formation of Tropical Cyclone Nate (2011) from the perspective of hyperbolic Lagrangian boundaries. In addition, they discuss the role of a so-called shear sheath as a further transport barrier. The authors find that, between 700 and 400 hPa, Nate developed within a "pouch", i.e. a region that is closed with respect to horizontal transport. The Lagrangian boundaries (the "pouch") protected the developing system from a nearby region of very dry air. Entrainment of air from outside into the pouch by so-called lobe dynamics is given special attention in this manuscript. At 850 hPa, however, the "pouch" was not closed and thus an open pathway existed through which environmental air could be transported towards the core.

General comments:

The authors follow up on their previous work and continue to investigate tropical cyclogenesis from the innovative and promising perspective of Lagrangian transport barriers. Overall, this manuscript contains much interesting material and will eventually shed important insight into the development of Nate (2011). However, while the introduction and the conclusions are well written, the presentation of the results, and partly that of the methodology also, requires improvement. Furthermore, there is too much discussion of aspects that are not sufficiently introduced or shown in the manuscript. A revised version should have a stronger focus on the material that is actually presented, or should extend the material to better support some of the discussion/ statements. I have conceptual concerns with this manuscript also, specifically with i) the 2D Lagrangian analysis on pressure levels in the vicinity of a baroclinic zone, ii) the emphasis of hyperbolic manifolds vs. elliptic Lagrangian structures, and iii) usage of ECMWF data vs. higher (temporal)-resolution WRF data (see specific comments below). Overall, major revisions are required before this manuscript can be published in final form.

Specific comments:

* 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 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 non-articulated assumptions made by the authors. Convincing justification for the use of an isobaric framework in the presence of strong baroclinicity is needed.

* 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.

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).

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.

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.

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 theta_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 ration 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 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 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.

Technical corrections/ Editorial recommendations:

pg 1, line 2: of –> or

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

pg 3, line 2; “arm of T.S. Lee”: incomprehensible

pg 3, line 5: should read “for a non-AEW disturbance”

pg 2, line 12; suggest: initiated – > first identified

pg 5: should “vortex strip” read “vorticity strip”?

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

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

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

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.

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

pg 8; Lagrangian flow: unclear

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.

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

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

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

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

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