The manuscript represents a commendable effort to apply Lagrangian chaotic-dynamical methods to address the important question of moist and dry air transport in the development and non-development of tropical cyclones. The results in Section 5 are most interesting. For this reason, the publication of a final improved manuscript would be recommended.

The following comments are mostly (though not only) confined to Section 2, where it is felt that the authors' review of the literature on Lagrangian methods can be lacking in conceptual precision at times. Asterisks denote gross conceptual errors or that important clarification is required.

P33276, l16-22: The second sentence does not quite follow from the first: if the tendency is for the middle and upper troposphere to become progressively drier in the systems that do not develop, why does the most prominent difference between non-developing and developing system lie in the lower troposphere "between the surface and a height of 3 km"?

* P33277, l20-22: There are two senses of "Lagrangian" in this sentence and on the whole, in the manuscript. The first "Lagrangian" refers to following the trajectory of a fluid element; the second "Lagrangian" refers to following the propagation of the Easterly wave. The two meanings must be clearly distinguished. In fact, it is recommended that the latter use of "Lagrangian" should be dropped throughout the manuscript since it is not truly "Lagrangian" as is usually understood in both dynamical systems and meteorological communities. "Wave-relative" might be a better word for the second sense, with an initial definition of this term as referring to the frame following the propagation of the Easterly wave.

* P33279, l3-4: Wrong concept of "Galilean invariant" here. A Galilean transformation only involves two frames moving at constant velocity with respect to each other. Thus, rotations and general time-dependent frame transformations are not Galilean and invariance to such transformations is not Galilean invariance.

P33279, l25-27: Two objections: (1) any method that identifies Lagrangian structures in a flow must be sensitive to the time-dependence of the flow by definition; if a time-dependent and a constant flow field yields the same Lagrangian structure, something must be wrong with the algorithm. (2) Besides the algorithms of Duan and Wiggins (1996) and Ide et al. (2002) mentioned, other algorithms exist and they are not *sensitive* to the persistence of stagnation points, e.g. Koh and Plumb (2000) and Joseph and Legras (2002).

p33281, l17: Did the present authors not published a paper on finite-time Lagrangian diagnostics on a model hurricane development? So "not yet appeared in the tropical meteorology literature" is carrying the claim of novelty a little too far.

P33281, l24: Hyperbolicity is strictly not just a property of the linearized velocity field. In mathematics, a nonlinear flow field can be classified as hyperbolic, elliptic or parabolic as well. Linear analysis in a steady flow field is convenient and helpful but is not necessary in the definition of the concept.

P33282, l17: The notion of convergence is not well-defined, or strictly speaking, does not exist in finite time. The verb "approach" would be more suitable than "converge".

* p33282, l18-21: Any set of trajectories forming a 2D surface in a 3D flow (or a 1D surface in a 2D flow) is not crossed by trajectories. There is nothing unique or distinguished about this feature. Any

such set can form a flow boundary in time-dependent flow. The only catch is whether such a set provides a conceptually and/or practically useful boundary.

P33282, l24: Delete this sentence since complex geometry of stable and unstable manifolds, in particular the formation of lobes, occurs even in time-independent flows.

* p33283, l8-9: The intersection of stable and unstable manifolds do not identify DHT uniquely because as stated clearly on p33282, l25, they also intersect at points other than DHT. So, how is DHT uniquely identified beyond merely labelling the intersections of stable and unstable manifolds? Likewise, p33282, l12-13 should be re-written.

P33284, l8: “larger” or “largest”? Is the flow field considered 2D or 3D in this section? The context suggests 2D and so it must be said explicitly and the extension of the ideas to 3D flow must be discussed if relevant to subsequent diagnostics, e.g. the complication of compressibility in the 2D section of a 3D flow (even if incompressible) must be addressed.

P33285, l5: The authors should elaborate what is meant by “parameter dependent”. Are FTLEs also “parameter dependent” or are they not?

* P33285, l5: The authors should elaborate what is meant by “purely a diagnostic”. Are FTLEs also “purely diagnostic” or are they not? This reviewer has not seen any rigorous proof that shows the ridges of FTLEs or FSLEs in forward and backward time as respectively mathematically equivalent to the stable and unstable manifolds (which are rigorously defined with respect to the mathematically well-defined DHTs). Such rigorous proof would be unavoidable to be convincing that FTLEs or FSLEs are not diagnostic.

* P33286, l9-10: High auto-correlation does not indicated *conservation* of a quantity; high auto-correlation values *for a long time tau* indicates persistence in the sign of a perturbation in that quantity and by implication, a slow decay of the anomalies in that quantity.

p33291, l21 : see the earlier comment on the use of the word “Lagrangian”.

P33293, l2: Streamlines are well-defined at every time-slice of a time-dependent flow and can be computed by keeping the flow from that time-slice fixed. The problem is not that they cannot be computed, but whether it is worthwhile computing them.

* P33296 – p33302: This comment pertains the underlying philosophical interpretation of the results: do the existence of LCSs really explain the transport pattern; or does the transport pattern itself reveal the existence of LCS? (See p33277, l8-10.) To constitute an explanation for the transport pattern, it seems that the existence of LCSs must be related to the meteorology and not simply be “diagnosed” from the kinematic flow pattern. Think about an analogous example: the diagnosis of a hyperbolic stagnation point in a steady flow field does not *explain* the hyperbolicity of the flow and the consequent transport pathways; it is rather more like a succinct and insightful *description* of the flow field. Having said this, LCSs not being an explanation for the flow pattern does not detract from the usefulness of LCSs in understanding the conceptual organization of the flow pattern in an Easterly wave. The results here can be important for the direction of further work on how hurricane development or non-development depends on Lagrangian transport of moist or dry air. Some discussion of this aspect at the end of Section 5 seems pertinent if continued use of Lagrangian kinematic methods is to be encouraged.

p33302, l23: see the earlier comment on the claim on the novelty of this piece of work.

References:

Koh, T. Y. and R. A. Plumb (2000), "Lobe dynamics applied to barotropic Rossby-wave breaking", *Phy. Fluids*, 12(6), pp. 1518-1528.

Joseph B. and B. Legras (2002), ‘‘On the relation between kinematic boundaries, stirring, and barriers for the Antarctic polar vortex,’’ *J. Atmos. Sci.*, 59, 1198 –1212.