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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A Lagrangian analysis of a developing and non-developing disturbance observed during the PREDICT experiment" *by* B. Rutherford and M. T. Montgomery

Anonymous Referee #3

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The aim of this manuscript is to evaluate the boundaries and exchanges between the regions of recirculation of Ex-Gaston and Pre-Karl with their environment so as to understand why the first one did not develop into a tropical cyclone and the second one did. Assuming a layerwise, two-dimensional flow, the authors diagnose the boundaries using mainly finite-time Lyapunov exponents. The authors demonstrate that Lagrangian diagnostics are better able to describe the flow dynamics whereas Eulerian diagnostics fail. They also explain the import of dry air into Ex-Gaston which could be responsible for its non-development into a tropical cyclone.

The theme of the paper is interesting and timely. However, the presentation of this

paper is unfortunately disappointing. A number of scientific terms and concepts are misused, several equations are either wrong or incomplete, large parts of the manuscript are long-winded and without a clear focus. I therefore cannot recommend this manuscript for publication unless the authors revise it according to the suggestions made below and address all issues mentioned here-in.

Major issues

Both the abstract (l. 7-9) and conclusion (p. 33302, l. 23) suggest that in this paper, new Lagrangian diagnostics are developed. This is not true: a set of diagnostics are employed to identify LCSs but these diagnostics were developed elsewhere.

It is of no surprise that Eulerian methods are not able to identify coherent structures in an objective way. This fact has been well documented in many articles via the use of flows far simpler than the one examined here (see e.g. Haller, JFM, 2005). There might be some interest in showing the discrepancy between the Eulerian point of view and the Lagrangian point of view. However, what is the point of calculating coherent structures in all 3 frames: the Eulerian, what you refer to as "the translated Lagrangian frame" which really is just an Eulerian reference frame moving at a constant speed and the Lagrangian frame? The relevant discussion appearing in a large part of section 4 is also superfluous. The continuous repetition of the benefits of using a Lagrangian frame of reference versus an Eulerian one becomes tiring and should be largely constrained in §2 where the Lagrangian methods are described. The sections containing the results of the paper should solely be devoted on the fluid exchanges between the recirculation region of Karl and Gaston and the external flow. A concise summary of the Lagrangian aspect of this work needs to replace §1.3, 1.4 and 1.5.

The description of the methods employed in this paper (§2) is confusing because of the number of mistakes in the equations (see specific comments below) as well as the incomplete presentation of important notions such as stable and unstable manifolds and material lines (which are only briefly mentioned in §1). The focus on this part needs to be on the methods employed to identify Lagrangian Coherent Structures in **unsteady** flows. The following re-structuring is recommended:

1. State clearly the main focus: define Lagrangian coherent structures and relevant quantities and why these are preferred to other Eulerian quantities.
2. Eliminate discussion relating to steady flows (e.g. §2.1.1) that is not pertinent to the (unsteady) geophysical flows explored in this paper. A good description may be found in Haller and Yuan, Physica D, 2000.
3. Describe clearly how Lagrangian coherent structures can be calculated from finite-time Lyapunov exponents and/or finite-size Lyapunov exponents and how one can deduce hyperbolic points from these exponents. Are there situations where these exponents may not provide a complete picture of the underlying LCSs?
4. Explain why you need to consider both finite-time and finite-size Lyapunov exponents
5. Define the Okubo-Weiss criterion using a mathematical expression and clearly explain how this varies in different frames of reference. Describe how the Okubo-Weiss criterion may be misleading in identifying dynamically different flow structures.
6. Provide a justification of the use of statistical methods such as autocorrelation functions. Are the previous methods that you already described insufficient? If so, the authors should clearly state why this is so.
7. Correct mistakes in the equations (see specific comments below) and define all terms introduced in the equations.

In this paper, relative humidity is treated as a passive tracer, i.e., a tracer that has no feedback on the flow. The Lagrangian methods described here are indeed most suitable for such tracers. However, this assumption is rather strong and potentially misleading: in hurricanes, droplets are often large and so have inertia which will affect the background flow. In this case, the Lagrangian Coherent derived by treating relative humidity as a passive tracer, may not represent the correct boundaries of the flow. Please discuss possible discrepancies that arise as a result of assuming that relative humidity is a passive tracer. A relevant article is Sapsis and Haller, JAS, 2009.

Specific points

l. 2 *... an improved understanding of recirculating flow regions on sub-synoptic scales...*
But your focus in this paper is also on the fluid exchanges between re-circulation regions and the external flow. The recirculating regions are not necessarily isolated so that new, dry air may invade this re-circulation regions. I thus suggest you modify this phrase into: *an improved understanding of the fluid exchanges between re-circulation regions and the external flow...*. In this case, *This recirculation problem* becomes: *This problem*

l. 8-9 *...that relax the steady flow approximation*
You have already mentioned that you locate flow boundaries in unsteady flows so this bit of the sentence is superfluous. I suggest that at this point you mention that what you identify are *Lagrangian Coherent Structures*.

p. 33274, l. 7 *This paper provides an introduction of new Lagrangian techniques....*
This paper provides an application and not an introduction of new Lagrangian techniques. Please modify appropriately. Also the sentence is too long.

p. 33275, l. 12 *The center of the ...* You have been talking about the region in the previous sentence. Replace 'the' with 'this'. You also do not need to define this region at this point but it is good that you mention that it is responsible for cyclogenesis.

1.3 Lagrangian aspects of genesis This whole paragraph is both badly written and not precise.

1. Surely the Lagrangian frame of reference cannot be obtained by merely subtracting the wave propagation speed. On that note, it is not obvious what you are subtracting this speed from. This needs to be specified.
2. Lagrangian methods are well known and widely employed among meteorologists for at least two decades (see e.g. the recent AGU conference entitled "Lagrangian models in the atmosphere"). Finite-time Lyapunov exponents and the so-called effective diffusivity have been employed to identify coherent structures such as jets and vortices in the atmosphere (e.g. Pierrehumbert and Yang, Journal of Atmospheric Sciences, 1993; Haynes and Shuckburgh, 2000; Joseph and Legras, Journal of Atmospheric Sciences, 2001; Koh and Legras, Chaos, 2002) or in the ocean (e.g. Nencioli et al., Geophysical Review Letters, 2011). Please remove all comments that imply the isolation of the meteorological community from the dynamical systems community: it is simply not true.
3. *The maximal ridges of scalar fields*
   Which scalar fields?

1.4 The objective pouch identification problem The last paragraph in this part is confusing. The Okubo-Weiss criterion is an Eulerian criterion that is Galilean invariant but not objective (Haller, Journal of Fluid Mechanics, 2005). You suggest (p. 33279, l. 1-5) that a quantity is objective if it is Galilean invariant. This is simply not true.

p. 33281, l. 13 I do not see why you need to include 'time-scale' when you already have 'finite-time'.

p. 33281, l. 14 *When the boundaries are difficult to see due to turbulence*
Please expand on this issue as it appears to be important. You may need to do so once the method is described.

p. 33281, l. 25 Equation (1) is wrong as well as $\nabla(x_h)$. Please correct. You also need to define ξ.

p. 33283, l. 20 The Jacobian is calculated by differentiating with respect to which variable? Your notation in equation (3) suggests that differentiation takes place with respect to x_0 (see operator d_{x_0}).

p. 33284, l. 6 Define $*$

p. 33286, l. 6 Define U, i, σ_i. The bar denotes an operation and cannot be the mean particle position. Please correct. Why does $R_i(\tau)$ not depend on t? What is the relation between the autocorrelation function and the position of the particle?

p. 33286, l. 8 What are the integral limits in expression (7)? You also forgot $d\tau$.

p. 33286, l. 9-10 *High autocorrelation values indicate that a quantity is conserved.* Please justify why this is so. To do so you need to relate the autocorrelation function with the quantity concerned.

p. 33287, l. 1 What is the difference between Gaston and Ex-Gaston, Karl and Pre-Karl?

p. 33287, l. 23 What does the 'feature' refer to?

p. 33288, l. 15 What exactly do you mean by Lagrangian reference frame? How can you obtain this frame by computing the pouch translation speed?

p. 33288, l. 16 Since the computation of finite-time Lyapunov exponents does not require a steady flow, why do you need to assume that this is the case?

p. 33290, l. 10-11 A quantity can be Galilean invariant but how can a frame be Galilean invariant?

p. 33291, l. 5 You need to define ITCZ.

§§4.1 and 4.2 The content of these paragraphs is obvious. Please eliminate and any discussion concerning Eulerian frames should be concise and embedded within the remaining part of this section.

p. 33294, l. 19-29 The autocorrelation function whose expression was stated inaccurately is, by construction, a quantity measured along particle trajectories. The correlation time which is the integral of the velocity correlation function can sometimes be related to the magnitude of turbulent diffusion (see e.g. the book by Vallis, CUP). I do not understand the meaning of correlation times associated to the OW parameter (which is by construction Eulerian) or the relative humidity. You need to clearly explain all this here as well as in the relevant section where you define autocorrelation functions and OW parameter. At the moment this paragraph makes no sense as well as the use of autocorrelation functions.

What is the difference between Figures 10 and 11?

§4.5 Please discuss reasons for which the relative humidity analysis data shown in Fig. 5 cannot be explained by the location of LCSs.

Figs. 8 & 9 You state these figures but do not provide any comments about them. Why did you calculate FSLEs as well as FTLEs? What is to deduce from these figures?

Figs. 10 & 11 Captions are identical. Please correct appropriately.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 33273, 2011.