**Atmospheric
Chemistry
and Physics
Discussions**

Interactive
Comment

# *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

B. Rutherford and M. T. Montgomery

blake5rut@gmail.com

Received and published: 9 April 2012

The authors would like to thank referee 3 for the helpful review. We have carefully considered the comments and will incorporate appropriate changes into the revised manuscript, which we believe has improved the manuscript. We appreciate the additional literature that is now referenced in the revised manuscript. The individual comments are addressed individually.

Major comments: 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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

elsewhere.

We will eliminate the suggestion that the methods were developed in this study.

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.

We recognize that the Eulerian and Lagrangian comparisons could be considered excessive to some readers, however, the use of finite-time Lagrangian methods is not well known in the hurricane community. In particular, forecasters do not have a strong recognition of the co-moving Eulerian frame, which was very recently (Dunkerton et al. 2009) proposed as the reference frame to visualize the rollup of Easterly waves into hurricanes. The primary goal of the PREDICT study was to use the co-moving reference frame in field observations and real-time forecasting to better forecast genesis, and to alert the forecasting community to the utility of the co-moving reference frame. Since the Lagrangian reference frame offers additional benefits over the co-moving frame, and there is very little recognition of the difference in flow boundaries in the hurricane community, we feel that our comparison between the reference frames is justified.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

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 struc- tures.

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.

We have considered the suggested changes to the introduction. We will provide a more

complete introduction of stable and unstable manifolds and material lines. The errors in equations will also be corrected. Thank you for alerting these typos and omissions. We will use the suggested re-structuring of the introduction with the exception of the elimination of the steady flow discussion, for the reasons discussed in the previous comment.

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.

Outside regions of deep convection, relative humidity is considered a good tracer in the tropical circulation where the flow is stably stratified. Under many approximations used in the tropical dynamics, the horizontal velocities of water droplets are very close to the horizontal wind speeds. We now reference Sapsis and Haller, 2009, and discuss these aspects.

We would like to offer two points of clarification regarding our use of the relative humidity (RH) in this study.

In order to help illustrate the penetration of dry air into the pouch, in some cases, we find it useful to advect the RH AS IF IT WERE A TRACER. However, this calculation is used as a diagnostic only. RH is generally not conserved in convective regions and nowhere do we use RH to compute flow boundaries. This potential misunderstanding should be averted in our revised text.

The second point we would like to offer the reviewer and readers about the concern that the current models of hurricanes and tropical circulations are missing the inertia

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

of water droplets and the resulting flow modification thereto. The specific statement that we question is the following: "... this assumption is rather strong and potentially misleading. In hurricanes, droplets are often large and so have inertia which will affect the background flow." Since the momentum of airborne water scales with the mixing ratio, the ratio of momentum of airborne water to the momentum of dry air is on the order of 20 g/kg, or 2 percent in the lower troposphere, and considerably less in the middle and upper-troposphere. For this reason, the inertial effects of airborne water cannot be an O(1) effect in tropical dynamics. The meteorological practice of neglecting the horizontal momentum of airborne water but retaining its energy content is similar to the common approximation made in magneto-hydrodynamics in which the electrons carry the current and the protons and neutrons carry the momentum.

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 recirculation 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. 2 The recirculation regions are not isolated from the outside flow, and the location of LCSs demonstrates this point. We agree with the suggested change in wording.

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.

l. 8-9 We have mentioned Lagrangian coherent structures at this point.

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 tech-

niques. Please modify appropriately. Also the sentence is too long.

P 33274. L. 7 This paper provides both an application of Lagrangian techniques, and an introduction of finite-time Lagrangian techniques for the tropical cyclone community. We will break the sentence to separate the ideas.

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.

p. 33275, l. 12 We have made the suggested correction.

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 sub- tracting 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 meteorol- ogists 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 co- herent 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?

1.3 1.In previous studies regarding cyclogenesis, and for the purpose of forecasting, the Lagrangian frame is broadly recognized to be following the primary flow feature,

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

hence subtracting the propagation speed of the feature is considered a Lagrangian reference frame. In this paper, we contrast this 'definition' of the Lagrangian frame with that of an objective frame following particle trajectories. To eliminate confusion caused by the multiple uses of Lagrangian, we now use the term "co-moving frame" to refer to the frame moving at the translation speed of the pouch. We have rewritten this paragraph to make the distinction between frames more clear. We now provide a formula for the computation of velocities in the co-moving frame.

2. We acknowledge that Lagrangian methods are known in meteorological literature, and will provide the additional suggested references. In terms of the hurricane community, however, we feel that based on extensive personal communication and hurricane conference attendance, that there is still little knowledge and/or appreciation of these methods. We have revised the text suitably to convey these points.

3. We now specify these scalar fields are derived from trajectory computations.

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

1.4 We have removed the language that suggests that OW is objective.

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

p. 33281 l. 13 We have removed the words 'time-scale'

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. 14 We have expanded on the turbulence aspect, and have moved this discussion to a later point in the paper.

p. 33281, l. 25 Equation (1) is wrong as well as (xh ). Please correct. You also need to define xi .

p. 33281 l. 25 The equation has been corrected and \xi defined.

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 x0 (see operator dx0 ).

p. 33282 l. 20 We have corrected the Jacobian formulation to show differentiation with respect to the spatial coordinate x.

p. 33284, l. 6 Define *

p. 33284, l. 6 We have defined *

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

p. 33286, l. 6 We have corrected the formulation of the autocorrelation function.

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

p. 33286, l. 8 We have corrected the integral expression and added the integration element dtau.

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. 33286, l. 9-10 We have justified the statements regarding the autocorrelation values, and restricted our use of autocorrelation values to fluid velocities.

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

p. 33287, l. 1 In this paper we adhere to the terminology used by Montgomery et al. 2012 (Bull. Amer. Meteor. Soc.). We use Pre-Karl to refer to Karl before it was declared to tropical storm, and we use ex-Gaston to refer to the Gaston disturbance after it was downgraded from a tropical storm to an invest.

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

p. 33287, l. 23 The feature refers to the tropical storm. We have clarified this point.

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. 15 The Lagrangian reference frame in this case is the frame co-moving with the wave-pouch. We have changed the wording here and elsewhere to 'co-moving' in order to eliminate any potential confusion.

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. 33288, l. 6 For this study we do not make the simplification of a steady flow. The assumption was made as part of the forecasting products used in the PREDICT experiment, with which we compare our results. We will make this distinction more clear.

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

p. 33290, l. 10-11 We agree and will eliminate the wording.

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

p. 33291, l. 5 ITCZ will be defined.

§§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.

Sec. 4.1 and 4.2 The content of these paragraphs will be reduced to eliminate redundancies. The fact that the frames are different is obvious. However, the implications of using one frame versus another to diagnose impermeability of the recirculation region is not as obvious. Therefore, we wish to retain the specifics of why this is the case in the cyclogenesis setting. All results pertaining to the difference in reference frames are now restricted to Section 4.

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 some- times 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 humid- ity. 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?

p. 33294, l. 19-29 We will eliminate the use of the autocorrelation function for OW. The intention in this section was to demonstrate both the time-dependence and properties of different variables as conserved quantities.

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

4.5 The relative humidity analysis in Figure 5 does coincide with the LCSs. We will specify the particular structures that show that this is correct.

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. 8 and 9 We have computed both FTLEs and FSLEs since some literature sug-

gests that FSLEs have advantages for atmospheric flows. However, we find that this is not the case. We will clarify the discussion of Figures 8 and 9 to show how the aggregate FTLE values coincide with the physical processes involved in genesis. We have removed the plots of the time-series of pouch-averaged FSLE values.

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

Figs. 10 and 11 The captions are not identical and are correct. Figure 10 shows the actual RH field as computed by the global model, while Figure 11 shows the RH tracer field.

---

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