Atmos. Chem. Phys. Discuss., 9, C6576–C6593, 2009
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Lagrangian mixing in an axisymmetric hurricane model" *by* B. Rutherford et al.

Anonymous Referee #2

Received and published: 3 November 2009

——————————————————————- General Comments ————————————————————————-

The problem addressed in this paper is an important one, concerning the characterization of mixing in the inner core of a hurricane, and has implications on the intensification of the hurricane through mechanical or thermodynamic mechanisms. It is particularly interesting for the authors show that strong mixing is positively correlated to elevated wind intensity later in time. The method adopted in this paper belongs to the class of methods used to analyze transport in the phase space of nonlinear dynamical systems extended to finite-time and aperiodic cases. As these methods are still relatively new (less than 2 decades) and their application to study the problem of mixing seems novel to this reviewer, there is definite scientific merit in this paper.

In terms of scientific quality, this reviewer finds that the authors may have overlooked a few areas:

(1) the literature review on chaotic mixing and finite-time aperiodic generalizations of hyperbolic manifolds is imbalanced. While Haller and co-authors have made significant contributions to the field, there are other significant contributions that have been neglected.

(2) the explanation and demonstration of their methodology can be improved as the empirical validity of certain approximate equations are not shown, while mixing measures lack error estimates and the statistical significance of correlations are not indicated.

(3) there is some confusion on the notion of "bifurcations" in dynamical systems theory as applied to the finite-time regime and this concept is (wrongly) invoked at three different parts of the paper.

The quality of presentation also leaves some room for improvement:

(4) The results in the tables could be better presented in alternate ways and the figures lack colour bars, have confusing captions or cannot be easily inter-compared.

(5) More care is to be taken in the notation and definitions to avoid confusion.

All the above comments are elaborated under "Major Comments".

Overall, the manuscript seems rather lengthy and not as insightful as it should be at times. This contributes to an impression of falling quality midway through the paper (sections 5 to 7) but the results in section 8 showing positive correlation between strong mixing and high wind intensity at a later time rekindles interest. So, it is suggested that:

- sections 5 and 6 should be condensed to at least half their length as they are mostly descriptive with little insight offered; - Fig 4-7 and Fig 10-13 are not all necessary: displaying only two well-chosen initial times is enough to illustrate the time-dependent nature of the structures; - Tables 3 and 4 do not seem important to the main thrust of

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section 7 and the authors may consider deleting them while keeping the last sentence of section 7; - Fig 18 and 19 may be removed as they seem only there to illustrate what good correlation entails, which is quite obvious and already illustrated by Fig 17.

In summary, the paper has potential to make a significant contribution to the understanding of mixing in hurricanes with possible implications on mechanisms of hurricane intensification, once the above issues are overcome.

—————————————————————- Major Comments ——————————————————————-

(1) Literature review:

The review of existing measures extended to finite-time and aperiodic systems should be included in Section 2. One suggestion is to examine the more common measures below in one or two paragraphs:

- FSLE: see e.g. Koh and Legras (2002) where FSLE was applied to the stratospheric polar vortex and justified by Haller (2001)'s neccesary condition for hyperbolicity: the stratospheric flow used was an open-boundary, aperiodic flow taken from ECMWF reanalysis;

- Direct Lyapunov Exponent (DLE): see e.g. Salman et al. (2008) where DLE defined by Haller (2001) was applied to a model oceanic double-gyre flow, which was closed-boundary and aperiodic (obeying reduced-gravity shallow-water dynamics).

In the brief review, the authors should point out possible common merits with FTLE, and perhaps possible drawbacks contrasted to FTLE, if these other measures were applied to the problem of mixing in hurricanes.

References:

- Haller, G. (2001), "Distinguished material surfaces and coherent structures in three-dimensional fluid flows," Physica D, 149, 248-277.

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- Koh, T. Y. and B. Legras (2002), "Hyperbolic lines and the stratospheric polar vortex", Chaos, 12(2), 382-394.

- Salman, H., K. Ide and C. K. R. T. Jones (2008), "Using flow geometry for drifter deployment in Lagrangian data assimilation", Tellus A, 60(2), 321-335.

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(2) Methodology:

The authors have not be completely clear in the explanation and demonstration of their computational methodology.

(A) To convincingly show an "exponential decay", the log of "sigmaC(t)-A1" and "G(t)-A1prime", with stated values of A1 and A1prime, should be plotted in Fig. 3 instead. A quasi-linear relation with time should be revealed and discussed at p18558, l15. Such plots are needed to show that the approximations represented by equations (1) and (9) are justified in the first place.

Moreover, from equations (1) and (9), MMR and FMR should not be functions of integration time, since they are fitted over a range of integration time at each initial time t0. Since they do have different values between Tables 1 and 2 and between Tables 3 and 4, one surmizes that MMR and FMR depend on the range of fitting and hence equations (1) and (9) are no longer truly exponential decay. If this line of reasoning is correct, the failure of a truly exponential fit represented by equations (1) and (9) needs to be highlighted and illustrated graphically as suggested above.

(B) Log-log plots to justify the power-law dependence on integration time in equations (12) and (14) should be shown.

Additionally, are the values of RD and FRD computed from the gradient and ordinate-intercept of the power-law fits? If not, how are they computed in practice?

(C) Error estimates are needed for MMR, FMR, RD, FRD in Tables 1 to 4, otherwise, one is unable to judge if the differences in mixing rates between regions are important or not. The power-law and exponential-law fits should come with error estimates, whichever method of fitting is employed (which also should be but is not specified in the manuscript).

(D) Threshold values of statistically significant correlation at 90% or 95% confidence should be computed and the statistically significant correlations should be highlighted in Tables 5 and 6.

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(3) Concept of “bifurcations”

In nonlinear dynamics, "bifurcation" is the term usually used to refer to the doubling of critical points as a dynamical parameter is increased. A classic example is the bifurcation of the attractor in the logistic map below as the governing parameter r is increased: x(n+1)= r*x(n)*[1-x(n)]

In this paper, the authors refer repeatedly to the bifurcation of attracting and repelling manifolds with integration time. (E.g. p.18559, last paragraph has the most elaborate exposition).

Firstly, time is not a dynamical parameter but is a coordinate of the system. Secondly, attracting and repelling manifolds are not the same as critical points in the flow (which should be the analogues of hyperbolic and elliptic stagnation points in periodic flows).

In fact, Fig 8 shows the wrapping round of attracting and repelling manifolds in a homoclinic or heteroclinic tangle as integration time increases. This phenomenon is mathematically proven for time-periodic flows. For aperiodic flows, the same wrapping occurs when integration time increases: the only difference is that the manifolds are not infinitely long and the loci of the manifolds do not repeat with time. If computational resources permit, one test that can be done to reveal this wrapping around of manifolds is to increase by ten-fold the density of FTLE data points in Fig 8 (i.e. 100 times more

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trajectory computations) and make an animation of the plots for integration times from 20 min to 120 min.

For further explanation, the authors may refer to e.g.

J. M. Ottino, (1989), "The Kinematics of Mixing: Stretching, Chaos and Transport", Cambridge University Press, New York.

N. Malhotra and S. Wiggins (1999), "Geometric structures, lobe dynamics, and Lagrangian transport in flows with aperiodic time-dependence, with applications to Rossby wave flow", J. Nonlinear Sci. 8, 401-456.

Therefore, the second paragraph of section 5 must be revised entirely and mentions of bifurcations in the paper should be removed or corrected.

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(4) Tables and figures

All the results in the tables could be better presented alternatively (cf. technical comments).

Bad figures: - Wrong or confusing captions or captions with missing information (not found in the text) in Fig. 1, Fig. 8, Fig. 9, Fig. 17. - No colour bars in Fig. 1 and Figs. 4-13. - The vertical range and aspect ratio should be standardized between Fig. 1, Figs. 4 - 7, Figs. 14 -15 for ease of comparison between Eulerian and Lagrangian structures: in this version of the manuscript, many of the authors' statements on the FTLE distribution with respect to the eye or eyewall cannot be checked clearly. - It is generally hard to connect the details of the figures and tables to the details highlighted in the text (cf. specific comments).

There are a quite number of places where the figures and tables do not show or do not show clearly what is said in the text.

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(5) Notation and definitions

The authors should pay more attention to notation and definitions of quantities. The phrasing of quite a number of statements should be corrected to mean the correct numerical procedure or physics implied. (cf. specific and technical comments).

——————————————————————- Specific Comments ——————————————————————————-

p18548, l5: "The fluid is not incompressible, and the domain is un-bounded, which present a challenge to many current mixing techniques."

Incompressibility is assumed in the theoretical underpinnings of a number of techniques. But how does the unbounded domain affect current mixing techniques? Is it at the theoretical level or merely at the level of implementation?

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p18548, l11: "Local properties of flow structures are generally not valuable for characterizing the entire flow because the time-dependent nature averages out any local effects."

Local properties of flow structures are, of course, not valuable for characterizing the *entire* flow, as they are *local* by definition.

Local properties are valuable for characterizing the *local* flow as long as the frame of reference moves with the flow structures (e.g. the hurricane) and the flow structures continue to exist in a coherent manner (i.e. no vortex splitting or merging). So the local effects average out not because of the time-dependent nature of the flow per se, but because of the injudicious choice of the spatio-temporal frame of reference.

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p18548, l18: "The local methods study particular features such as hyperbolic trajectories and their stable and unstable manifolds, and track the effects of these features."

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The word "local" has a different sense here than in l11 (see last comment). Here "local methods" is referenced to "local methods in a Lagrangian frame", as hyperbolic trajectories are Lagrangian features of the flow; earlier in l11, "local effects" seems to mean "local effects in an Eulerian spatio-temporal frame".

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p18548, l23: "Global measures are difficult to employ because the domain has many distinct mixing regions that do not completely interact with all of the other regions."

It is always understood that a measure is "global" in the sense of "global in a specific chaotic sea / mixing region" and not in the sense of "everywhere". When there are several mixing regimes in a flow, there will be a distinct value for the measure of mixing in each regime.

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p18550, l15-17: Finite-sized Lyapunov exponent (FTSE) has been in use by a number of authors before Green et al. (2006). Acknowledgement should be given to the work of e.g.

V. Artale, G. Boffetta, A. Celani, M. Cencini, and A. Vulpiani (1997), "Dispersion of passive tracers in closed basins: Beyond the diffusion coefficient", Phys. Fluids 9, 3162-3171.

Koh, T. Y. and B. Legras (2002), "Hyperbolic lines and the stratospheric polar vortex", Chaos, 12(2), 382-394.

d'Ovidio F, Fernandez V, Hernandez-Garcia E, et al. (2004), Mixing structures in the Mediterranean Sea from finite-size Lyapunov exponents, Geophy. Res. Lett, 31(17), L17203.

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p18550, l15-17: References are needed for distinguished hyperbolic trajectories, e.g.

G. Haller (2001), "Distinguished material surfaces and coherent structures in three-dimensional fluid flows", Physica D 149, 248-277.

K. Ide, D. Small and S. Wiggins (2002), "Distinguished hyperbolic trajectories in time-dependent fluid flows: analytical and computational approach for velocity fields defined as data sets", Nonlinear Processes in Geophysics, 9(3/4), 237-263.

Salman, H., K. Ide and C. K. R. T. Jones (2008), "Using flow geometry for drifter deployment in Lagrangian data assimilation", Tellus A, 60(2), 321-335.

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p18550, l25-26: A reference is needed for that other "global mixing rate defined through the distribution of FTLE values".

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p18551, equation (1): should be formulated in terms of mixing ratio and not concentration, since compressibility is integral to the hurricane mixing problem. It is the mixing ratio that is homogenized, not the concentration.

The context of taking the absolute value of (t-t0) when t < t0 (backward time integration) should be explained explicitly.

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p18552, l1-2: In the absence of tracer sources/sinks, non-zero A1 must depend on the initial variance as the tracer transport equation is linear in tracer mixing ratio (A0+A1). So the "degree of homogenization" should NOT be A1 but the normalized measure A1/(A0+A1).

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p18552, l7: The exponential divergence is not aligned to the LCS but need not be orthogonal (i.e. at right angles) to the LCS.

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p18553, equation (7): epsilon is not defined. Is it the initial magnitude ||x0||?

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p18554, l1: Do the authors mean P(sigma,t,t0) here and in equation (8)?

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p18554, l8-13: "For short integration times, advection dominates diffusion, and the mixing rate from FTLE values gives a good measure of mixing. Since the exponential decay form describes the aymptotic behavior of G(t) for large |t - t0 | this measure is not useful for very short integration times. The integration time must be chosen long enough that LCSs become resolved, but short enough that excessive filamentation of the structures does not occur."

The above reasonings seem rather jumbled: one presumes "short" is significantly longer than "very short" and yet long enough for LCSs to be resolved.

When is filamentation considered "excessive"? When diffusion becomes dominant?

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p18556, l6-7: It is unclear the L2-norm of which quantity is normalized to unity, at what time (initial vs. final) the normalization is done and in what units "unity" is measured in. Is the normalization carried out only for FRD or for both FRD and RD?

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p18556, l23: At what instant of time does Fig. 1 show? At t=380min or 480min or intermediate value?

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p18557, l9: what does a "hyperbolic process" refer to really? Such vague terminologies should be avoided.

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p18557, l10-14: this paragraph should be moved to the results section, since placed at this juncture, the statements appear merely as unsubstantiated assertions.

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p18557, l18: "initial conditions do not become eventually homogenized": do the authors actually mean "the locations of seeded particles" rather than the "initial conditions" in this context?

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p18558. l3-4: The quoted number and initial spacing of trajctories seem to be inconsistent with the model resolution specified on p18556, l10-11 and the schematic in Fig. 2.

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p18558. l18: "log of the time-series G(t)": shouldn't it be "sigmaC(t)-A1" rather than "G(t)" as seen from equation (1)?

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p18559, l5-7: There are several lines of high FTLE and both forward and backward time. Which one "defines" the eye-eyewall boundary? It is not clear that the line of FTLE (whichever one it is, forward or backward) can be used to "define" the eye-eyewall boundary: the boundary is otherwise defined already before the association with FTLE can be elucidated. However, the FTLE ridges do seem to be associated with a "transition region" inclusive of the eye-eyewall boundary as well as the inner part of the eyewall itself.

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p18559, l7-9: The statement is not right: FTLEs (and any other finite-time generaliza-

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tion of Lyapunov exponents) do not distinguish well between high shear and hyperbolic stretching because they are computed over finite time and the computation methodology (i.e. finding eigenvalues of the symmetric matrix dphi/dx) does not make this distinction.

Moreover, FTLEs are not "one-dimensional representation of the stretching" as they are eigenvalues of a 2x2 Lagrangian-flow strain tensor. The orientation of the unstable/stable manifold evolves with time over two-dimensional space.

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p18559, l14: why does Fig. 8(a) look very different from Fig. 4(a)? Are the captions correct? Is the same colour bar used?

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p18559, l14 & l15: these two statements are in direct contradiction: does the local coherent structures become more or less resolved in Fig 8 as integration time increases?

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p18560, l17-18: At no point in the manuscript did the authors explicitly point to or draw out the LCS and so the reader does not see where or what the LCS is exactly. So how does one decide that the trajectories are moving "transverse to the LCS"?

Fig 9: Which time slices are being shown? Why are there 3 values of t0 for 2 panels (a) and (b) only? One guess is that the trajectories are integrated in forward time from 400min to 460min, quite apart from the backward integrations of 20min duration to determine the LCS at time 420min and 460min. Is this guess correct? The figure is very hard to understand and it takes repeated reading of Section 6 before the above guess could be made.

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p18561, l5: What is meant by the LCS "separating from the sea-surface"? Is it joined to

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the sea-surface anywhere? Where is the LCS? Some graphic e.g. dotted line to show where the LCS is would be helpful.

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p18561, l17: Instantaneous stagnation points do not mark hyperbolic trajectories, especially in a highly time-dependent flow in this case.

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p18562, l14-17: "The secondary updraft at 35 km takes some of the boundary layer trajectories, and moves them upward into the region just outside of the eyewall, before a downdraft takes them inward to the eyewall updraft."

The trajectories exhbiting this motion is not shown but is believable. Yet, how is this motion related to the Lagrangian structures revealed in Fig. 15?

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p18562, l22-23: "with higher values of the MMR except for the low-level eye and low-level eyewall regions."

This is not what Table 1 shows. Please check.

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p18562, l8-11: "While higher velocities are generally associated with higher mixing rates, the presence of hyperbolic structures may allow or inhibit transport, which may precede or trail 10 higher intensities. A lead or lag of mixing rates to velocities is then appropriate to capture the hyperbolic effects."

While true, these sentences do not seem to lead to any finding in Section 7. Instead, Tables 3 and 4 show "average" mixing rates and not leading or lagging mixing rates to elevated velocities. One suggestion is to move them to Section 8, last paragraph.

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p18563, l23-25: "The FMR is fit to an exponential decay function, but the curve of the FTLE distribution function does show a decay for t ?t0 10 min. After 10 min, the FMR can be computed by fitting an exponential decay curve to the remaining data."

The two sentences seem to contradict. Is a "not" missing in the first sentence?

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p18564, l2-10: While it is a good effort to compare with the results of Antonsen Jr. et al. (1996), this paragraph does not reveal any understanding on the difference with Antonsen's results. The difference between FMR and MMR are reported to be smaller for hurricane mixing. In fact, MMR is larger than FMR in Table 2 (apart from low-level eye and sometimes low-level eyewall), opposite to the authors' report of Antonsen's results. How is this deviation related to the open nature of the domain and/or the aperiodic nature of the hurricane flow?

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p18564, l11-22: It is not possible to discern super- or sub-diffusion from the reported mixing rates. The authors will need to display an additional table or figure to support the discussion in this paragraph.

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p18565, l8-9: "Lagrangian structures are an effect of the (u,w)-velocity field from previous times in a forward time integration, or future times in a backward time integration."

There is a potential for the concepts to be confused at this juncture. Are these "Lagrangian structures" taken at time t0, the time from which forward and backward integrations are initialized to compute FTLE? If so, the words "forward" and "backward" seem swapped. Contrast with the sentence on p18566, l13-15, which seems to get it right. Please confirm.

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p18565, l20: Are the autocorrelations computed only within the quasi-steady time window?

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p18565, l26-27: Fig. 16 shows that correlation is positive up to about 20min. This means 20min corresponds to half an oscillation, not one oscillation. (An oscillation is defined as the tangential winds changing from maximum, to minimum and back to maximum again.) 40min corresponds to only one oscillation.

The rest of this paragraph should be rewritten because while "40min" is variously noted, the authors did not put forth any explanation to show that these are not coincidences.

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p18566, l19-24, l26-29: Are the authors using the convention "positive lag" = "lead"; "negative lag"="lag"? If so, maybe it would be preferable to call a lead, a "lead" just to avoid potential confusion. If not, these sentences do not make sense.

Why are the results for negative lags not shown? These results would have converse implications for predictability and p18568, l26-27 ("neither of these explanations can be favored by the present results") may not be sustained without showing these results.

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p18566, l25: Table 5 and 6 do not show the correlations for FRD.

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p18568, l13-14: change to "are an extension of mixing rates of Antonsen Jr. et al. (1996) and Huber et al. (2001) established for closed regions or time-periodic velocity fields"". This is because other diagnostics of mixing (e.g. FSLE, DLE) have already been extended to other open and/or aperiodic flow fields (cf. general comments).

——————————————————————-Technical Corrections ———

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A few places in the manuscript: "filimentation" should be spelt as "filamentation"; likewise "filiment" should have been "filament".

p18551, l14, l16 and l26: Naming the region "A" is potentially confusing as the constants in equation (1) are denoted by "A" with subscripts.

Section 2.1 and 2.2: The use of the same symbol sigma for FTLE and for variance is confusing, e.g. see equation (8).

po18552, equation (5): unmatched round bracket; missing prime on the first x0.

p18553, l12: add "respectively" at the end of the sentence.

p18553, l14: "initial-time" – remove hyphen.

p18554, l3: "G(t) should show a similar exponential decay like the tracer variance." This statement sounds like an unsubstantiated assertion. Change the word "should" to "may" to be truer to its meaning.

p18554, l15: "This method is designed..." to be replaced with "The FMR method was originally designed..." to avoid possible confusion.

p18554, l15: Repeated use of mixing rates is confusing. It is surmised that FMR is compared to MMR and to measures of intensity. (i.e. for clarity, use the abbreviations since they are already defined.)

p18555, equation (3): should have brackets enclosing the argument of the exponential function for clarity.

p18558, 117: It is preferable to be consistent in deciding to adopt the abbreviation MMR in the paper, or not

p18558, l19: not "A0" but "A0prime+A1prime" according to equation (9).

p18558, l24-25: "where the initial and final... are... initial and final...": this clause is a

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tautology and anyway, equation (10) has already defined what the mean trajectory is.

p18561, l24: Just to confirm, "highest 80%" is correct rather than "highest 20%"? In other words, between the two composites, are all realizations of the maximum tangential winds are covered?

p18562, l11: Remove the reference to Fig. 16 as it is not related to what is said in the text.

p18564, l1: "short time": add hyphen.

p18564, l1: remove "advective" because FTLE measures advective mixing for all times: it only misses diffusive mixing which operates on longer time-scale.

p18564, l15-17: the hyperbolic manifolds are invariant by definition and other trajectories cannot entrain within the manifolds. This sentence needs to be rephrased.

p18565, 1-3: rephrased as "High particle velocities and velocity gradients at an initial time would indicate high initial separation but may not correlate to high averaged Lagrangian rates which are assigned to the same initial time."

p18566, l11: after "Lagrangian fields", append the words "(i.e. FTLE, FMR, FRD, RD)

p18567, l15-16: Refer to the specific panels (a) or (b) for ease of reference. This is necessary because the captions are so similar between the panels.

p18567, l27: "eywall" should have been "eyewall".

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Tables 1-6: Units should be given for the mixing rates for completeness.

Tables 1-4: These tables do not illustrate well what is mentioned in the text to the extent that text actually does not correspond well to the presented results (cf. specific comment for p18562, l22-23). More obvious ways of presenting the data should be sought to facilitate readers checking the authors' statements... One suggestion is to

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classify the mixing rates into two categories: high vs. low, and present the numbers in big bold vs. normal fonts. Of course, the classification thresholds for each measure for each Table should be stated in the caption and if possible standardized between Tables. The regions could be arranged spatially as boxes like in Fig. 2 to bring out the underlying physical structure of the mixing landscape, and so Tables 1 and 2 will each display 2 sets of 3x2 boxes; Table 3 and 4 will each display 1 set. The 4 measures of mixing (FMR, FRD, MMR, RD) could be arranged in a 2x2 matrix within each box. Thus, for example, all 4 mixing rates in the "BL flow" box will appear in bold font in all tables.

Tables 5 and 6: These results could be better presented as graphs of mixing rates vs. time lag, with the zero-correlation line drawn. In that case, even the results for boundary-layer inflow can be shown.

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Fig 1: Color bars yielding quantitative information are missing. Panels (a) and (b) seems to have been swapped according to the caption.

Fig 2: Box 6 should NOT be named "environment" since it is in a region of convective updraft.

Fig 17: Are the normalizations with respect to the standard deviation of each time series? If so, it needs to be stated explicitly. (However, the red curve seems to have a larger standard deviation than the blue curve and so the above guess may be wrong.))

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