Response to the Reviewer's Comments

Amir E. BozorgMagham Shane D. Ross David Schmale III

September 2, 2015

Dear Editor,

We thank the reviewers for their constructive comments and suggestions. We followed their recommended points and revised our manuscript.

Below is our detailed responses to the reviewers' comments.

Sincerely,

Amir E. BozorgMagham

REFEREE REPORT #1

Thank you very much for your constructive comments and suggestions. We followed your recommended points and revised our manuscript.
Below is our detailed response to your comments:

This manuscript uses a very clever technique, associating small differences in sampling time with initial spatial separation, to extract a local finite time Lyapunov exponent (FTLE) from realistically accomplished local sampling data. The manuscript then details applications of this method to measuring atmospheric dispersal and designing operational experiments to optimally test for wind-born microorganisms. Finally the results are extended with a stochastic model to examine source and destination regions. As the latter is outside of my expertise, I will restrict my review to the FTLE portion of the manuscript. The development of a locally defined FTLE is a very nice innovation with many potential applications, for example in oceanography and limnology, as well as smaller scale experimental or biological flow applications. The mathematical development is exceptionally clear and easy to fallow, well supported by the figures, and should be accessible to a wide audience. The applications are well-motivated and made clear by the provided figures. Literature is well-cited and includes a diversity of interesting references. Overall, I find this to be a very good contribution, and only have minor technical/typo suggestions for improvement.

Minor editing issues:
p 908 line 4 Parentheses around citations are needed.
p 910 line 21 ”equal” not ”equals”
p 911 line 1 ”Figure 3 shows” would be less awkward
p 911 line 25 ”Figure 4b shows”
p 912 line 6 ”upper-left” is incorrect

Thank you very much for your detailed comments. We have incorporated your point in our revised manuscript.

REFEREE REPORT #2

Thank you very much for your constructive comments and suggestions. We followed your recommended points and revised our manuscript.
Below is our detailed response to your comments:

In this paper the authors discuss a new conceptual tool, which they call the local finite time Lyapunov exponent, to characterize flows in real applications and field experiments where samples are collected/released at a fixed location and it becomes imperative to obtain information concerning long distance transport properties from this data.

They main idea is to generalize the well-known concept of FTLE, which involves initial small differences in the initial condition of two (or and ensemble) of tracers, to the case where particles (tracers) are collected/dropped at the same spatial position but different times. This is done by introducing the so-called local FTLE mathematically defined in Eq. (5). The idea, as it stands, would be interesting for obvious reasons, specially in field applications. However, I have strong doubts that the quantity defined in Eq.(5) and the corresponding method, and theorem, are valid.

I explain in detail my main concerns:
1) The main authors claim is that they generalize the FTLE concept. I assume this means that the new exponents in Eq.(5) characterize the maximal exponential growth rates in some time interval (t1, t2)? Unfortunately, the authors offer no proof whatsoever that Eq.(5) yields the maximal FTLE. There is no warranty that perturbing in the direction of the flow will lead to maximal growth. Therefore, there is no proof, as far as I see, that Eq.(5) leads to a set of LEs with the intended meaning. This must be rigorously proven or, at least, strong arguments of plausibility should be provided regarding the meaning of σ^T as a Lyapunov exponent characterizing the maximal expansion rate.

We thank the reviewer for finding this error. Following this important comment, and based on our exploration of the non-autonomous systems, we replace the previously proposed (and as we now see, incorrect) "Theorem" with related "Observations I & II". We agree with the reviewer, believing we have made some helpful observations which fit into the larger emerging Lagrangian transport framework, particularly in geophysical flows; and yet concede that we are not, nor do we now intend, to put this into a rigorous theorem. Instead we leave that for the future or to other authors. We have also included some example analytical vector fields, and the provided numerical experiments on periodic and aperiodic flow fields demonstrate that we seem to be able to approximate the benchmark (true) σ with Eq. (8) [Eq. (5) in the original manuscript]. Our hope is that our observations will have some bearing on practical field applications, and will help foster further connection with time-series based methods, often used in experimental analysis, which commonly assume that the direction of maximum expansion dominates the dynamics of perturbations in arbitrary directions (see Rosenstein et al (1993)).

Regarding perturbing in the direction of the flow, see below.

2) Related with the point above is the following. The local FTLE and corresponding "theorem", as defined by Eq.(5), cannot be valid in such a general situation as the authors imply. As it stands, absolutely no requirements seem to exist for mathematical conditions of applicability of this theorem, so we should assume it is of general validity, including any form for v(x, t)?? Well, this cannot be the case because for autonomous systems, where v(x) does not depend explicitly on time, it is known that a perturbation in the trajectory direction gives on average a null FTLE (and of course, never tends to the maximal instability). By the same token, we can also expect that a slow varying v(x, t) will also be problematic for time intervals shorter than the inverse of the typical frequency of variation of v(x, t). In fact, I am afraid that the authors have naively assumed that the perturbation in the direction of the flow will exponentially grow and tend to

align with the direction of maximal growth, however, as far as I see it, this will require some mixing/randomness conditions on v in a general case, which are not totally clear. For instance, one may assume that if v(x, t) is a delta-time-correlated stochastic field this might provide enough randomness to allow the system to scan random disturbances and Eq.(5) could be given a meaning. On the other hand, a smoothly varying field v would be more problematic.

The issue of "perturbing in the direction of the flow" and the resulting null FTLE for autonomous systems is an important concern which escaped our notice before, and for which we thank the reviewer. We now discuss in the revised paper, in light of Observations I & II. We recognize that taking δt to zero will not give the result we expected, and having it too long will violate the linear approximation of the flow map gradient. We are thus led to conclude that for practical applications δt must be in an ad-hoc "appropriate range" to provide a good approximation via Eq. (8) [Eq. (5) in the original manuscript]. This δt range depends on the time-variability of the vector field in question. We make no claim to know what the appropriate range is, a priori.

3) I would expect to see a numerical verification of the new concept in a simplified model of chaotic flow in order to clearly show that, under general enough conditions for v(x, t), the idea works. For instance, by comparing the local FTLE with the true FTLE at x(t), also in the limit $t2 \to t1$ with the true FTLE measured by standard methods, maybe extracting some conclusions on the degree of randomness of v for the method to give reasonable results. Instead the authors go to full scale models and field data, where it is unclear what tests can be used for validation.

Again we thank the reviewer for this constructive comment, and have added four numerical examples from two well-known flow systems with time-variability. We present the results for the double-gyre system and also the aperiodic Rayleigh-Bénard convection model, which help bolster our numerical evidence for Observations I & II.

In this regard, I am very much confused by the comparison with numerical data. I do not understand what is used as benchmark local FTLE in Fig. 5 and 6 for instance. As far as I can see in these plots the real numerical distance $\delta(t)$ is compared with that obtained from Eq.(5). But, Eq.(5) is also used to compute σ^T from the numerical distances $\delta(t)$ so what is exactly proven by these plots? It looks like a simple change of variables. Given the fact that σ^T do not have the meaning of LEs (i.e. characterizing the maximal exponential growth rates) what difference does it make to give the tracers separation as $\delta(t)$ or in terms of σ^T? To be more specific, suppose the true FTLE is very high at some point of the trajectory x(t) for a time horizon T, will this imply anything on the value of the $\sigma^T(x, t)$?? Or is it totally unrelated? Can one compute the LCS from σ^T?

Following this comment and to avoid any confusion, we revised the description of the figures throughout the paper. To be more specific about Fig. 5 and 6 (equivalent to Figs. 9 and 10 in the revised paper, respectively) we must say that Fig. 5a (Fig. 9a in the new revision) shows the "true" FTLE field at a specific moment. Panel (b) of the same figure shows the "true" (black line) and "approximated" (red line) local FTLEs at a specific location. The approximated FTLE (red line) is calculated by Observation I, Eq. (8) [Eq. (5) in the original manuscript]. For this case, we assume that we know the distance between the particles and also the local value of velocity. Fig. 6 (Fig. 10 in the revised paper) shows the "true" (black line) and "approximated distance" (red line) between successively collected particles at the sampling location. The red line (approximated distance) is calculated by Observation II, Eq. (9) [Eq. (6) in the original manuscript]. In this case we assume that we have the information of local FTLE and local velocity. (One should note that, these two figure and the two Observations are independent)
Observations I is a means to calculate the local FTLE value. Therefore it can show

the "temporal" peaks of the local FTLE time-series (for example, please see the new added figures 5 and 6 in the revised paper). Computation of the LCS requires the availability of FTLE over the entire field which is out of scope of Observations I & II.

REFEREE REPORT #3

Thank you very much for your constructive comments and suggestions. We followed your recommended points and revised our manuscript.
Below is our detailed response to your comments:

The authors have proposed a method to calculate a local FTLE using temporal variations of the velocity field at a point. The application to field experiments that the authors discuss are interesting. While the underlying ideas in this paper are interesting, I have some concerns about their formulation.

The definition proposed in equation (5) and the corresponding theorem are not soundly formulated and not properly proved. It is unclear why the two conditions specified are necessary or sufficient. It is merely stated that they are sufficient. The proof that follows makes many assumptions and statements without evidence. For example how large is the Lagrangian time scale compared with σt or compared with the integration time T. How is the Lagrangian time scale defined? The separation between the source points $\delta(x, T, t, \delta t)$ is assumed to be close to the maximum separation between the particles in the past. Is this the maximum separation over the Lagrangian time scale or is it the maximum for any time scale. Does such a maximum even exist? Why is it guaranteed that in an aperiodic flow, the average velocity at the sampling time during the small time interval δt is in fact not zero.

Following this important comment, and based on our exploration of the non-autonomous systems, we replace the previously proposed "Theorem" with "Observations I & II". We argue that δt must be in an ad hoc "appropriate range", which depends on the frequency of variations of the flow field, to provide separation between successively released particles and a good approximation by Eq. (8) and (9) [by Eq. (5) and (6) in the original manuscript]. We remove the vague notion of "Lagrangian time scale" in the statements of Observation I & II. In addition, $\delta(x, T, t, \delta t)$ is often observed to be close to the maximum possible separation. For example please see the new figures 5 and 6 in the revised paper. This observation was formulated in Eq. (9) [eq. (6) in the original manuscript]. We explicitly add the condition of non-zero average velocity for Observation I (Eq. (8)) [eq. (7) in the original manuscript].

The proposed alternative method to calculate the FTLE is for time dependent flows. The method does not work for time independent flows, which should be mentioned by the authors. This raises the question of its validity for periodic flows. Is there a relationship between the time period of the flow, the Lagrangian time scale and δt for which the proposed definition is (in)valid? In the last paragraph of page 909, point (iii) it is stated that, that as δt becomes smaller, errors in equations (5) and (6) decrease. However it is not clear what δ^* converges to when $\delta t = 0$. Point (iv) in the same paragraph is also stated without any proof or reason.

Following this comment, we revised our paper and explicitly state that the proposed Observations I & II work for a time-dependent system. Our numerical results show that both Observations work for a double-gyre periodic system. We also argue that δt must be in a proper range, which depends on the frequency of variations of the flow field, to provide separation between successively released particles and a good approximation by Eq. (8) and (9) [by Eq. (5) and (6) in the original manuscript]. Point (iii) is deleted in the revised paper. Point (iv) is based on our observations from different numerical experiments.

The authors should consider illustrating the definition and proof through the calculation of the FTLE field for simple time dependent flows and a comparison with the standard approach. The statements in the last paragraph of page 909 should also be supported with such simple examples.

To address this constructive comment, we show four numerical examples from two well-known flow systems that we know their true FTLE fields. We present the results of Observations I & II for the double-gyre system and also the aperiodic Rayleigh-Bénard convection model.

The results in figures 4 and 5 are obtained through setting $\delta t = 0.1$h. The temporal resolution of the data set is stated to be 3 hours. Is the average velocity on the interval of 0.1h obtained through numerical interpolation? It is nice to note that the FTLE values in figure 4(b) do not seem to be very sensitive to the choice of δt. However this may be an artifact of the numerical interpolation. This robustness or sensitivity should be demonstrated with a simple analytical example.

For the case of wind velocity field we used third order splines for all necessary interpolations. We agree that the local FTLE time-series is not sensitive to the choice of δt as long as that choice is in a good range. For example, Fig.4b (or equivalently, Fig. 8 in the revised paper) shows this fact. Our numerical observations with periodic and aperiodic systems confirm your point.

On page 912, it is stated that, "(i) by choosing smaller sampling period time, δt, the recovered local FTLE time-series converges to the true one". However the "true" FTLE is not plotted in figure 4(b) for such a comparison.

We showed the "true" FTLE time series in Fig. 5b (or equivalently, Fig 9b in the revised paper). Also we give an argument about the proper choice of δt in the revised paper instead of statement (i) on page 912.

In summary, section 2 on which the paper hinges, is poorly reasoned. The proposed method to calculate the FTLE bears some resemblance to the Eulerian approach suggested in the paper An Eulerian approach to computing the finite time Lyapunov exponent, 2011, by S. Leung in the Journal of Computational Physics. I believe that, that the kernel of the idea on which this paper is based on, is interesting. However the definition, proof and reasoning have to improve significantly. The validity of the method and the claims in the paper should also be demonstrated with simpler analytical examples.

Following this comment we have re-organized our paper substantially. For example we replace the previously proposed "Theorem" with "Observations I & II" and also we add four new numerical examples from well-known analytical fluid systems. We agree with the reviewer, believing we have made some helpful observations which fit into the larger emerging Lagrangian transport framework, particularly in geophysical flows; and yet concede that we are not, nor do we now intend, to put this into a rigorous theorem. Instead we leave that for the future or to other authors. Our hope is that our observations will have some bearing on practical field applications, and will help foster further connection with time-series based methods, often used in experimental analysis, which commonly assume that the direction of maximum expansion dominates the dynamics of perturbations in arbitrary directions (see Rosenstein et al (1993)).