Nonlin. Processes Geophys. Discuss., 2, C361–C366, 2015
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Nonlinear Processes in Geophysics Discussions
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Interactive comment on "Local finite time Lyapunov exponent, local sampling and probabilistic source and destination regions" *by* A. E. BozorgMagham et al.

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Received and published: 2 September 2015

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 original comments are in normal font and our response is in bold font):

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

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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.

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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 –> 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 7 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.

Please also note the supplement to this comment:

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http://www.nonlin-processes-geophys-discuss.net/2/C361/2015/npgd-2-C361-2015-supplement.pdf

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