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Nonlinear Processes in Geophysics
Discussions

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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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):

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 δt = 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 δt = 0.1h. 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)).

Please also note the supplement to this comment:
http://www.nonlin-processes-geophys-discuss.net/2/C368/2015/npgd-2-C368-2015-supplement.pdf

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Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 903, 2015.