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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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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 (t_1, t_2)? 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.

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.

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 $t_2 \to t_1$ 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.

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?

I will skip commenting on the remaining parts (from Sec. 4 to the end) of the paper because, as explained above, I have strong enough criticisms about the soundness of the whole approach to bias my view.

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