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## ***Interactive comment on “Self-breeding: a new method to estimate local Lyapunov structures” by J. D. Keller and A. Hense***

### **Anonymous Referee #2**

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The authors try to introduce a new method of breeding in order to obtain a set of vectors that project into the future tangent field. Traditional breeding consists of growing perturbations, using the full nonlinear model, from the remote past in order to have an estimation of the growth directions at present time  $t$ . This procedure informs us on how errors have grown up to time  $t$ , however, this does not tell anything on how errors will grow in the future. With forecasting applications in mind, one would rather like to know what directions will grow faster in the future, say up to a time horizon  $t_{\text{end}} > t$ . Unfortunately, calculating the singular vectors requires to have the adjoint operator of the corresponding linear model, which is not easy to obtain in full scale global models. This is why approximations based on the full nonlinear evolution would be so welcome and that is what the abstract and introduction of this paper claims to achieve here. However, these claims are not supported by the present algorithm or results presented.

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In my opinion the paper is mathematically faulty in many points and, therefore, the results probably do not stand correct. I detail in the following what I see as major problems of this paper:

1) The authors discuss global vs local Lyapunov vectors (LVs). This classification, however intuitive it may seem, is misleading. LVs are mathematically well defined objects and precise mathematical definitions must be provided in order to make sense to what they are meaning by local/global LVs. For instance, the authors say global LVs determine the average growth at the system's attractor scale, while local LV depend on the point in the attractor. Well, there is no such a thing as a LV that does not depend on the position in the attractor. There is no such a thing as a LV that is independent on the point of the trajectory!!

One may "interpret" the authors have in mind backward (forward) LVs as global quantities because they are computed from the remote past (far future) and so they have information of the whole attractor. However, this does not make them independent of the trajectory point. These sets of vectors are always computed at some time  $t$  of the system's trajectory. By the same token, one may also interpret "local LVs" refer to singular (forward or backward) vectors. If this is the case, why not using the existing well accepted terminology. The citations used to refer to both sets of vectors do not help because they are often referring to different objects.

2) Singular vectors are not estimates for the forward LVs, they are eigenvectors of  $M^*M$  operator and tend to the forward LVs in the far future limit. At any short time, they are not even estimates. This is not a minor point, see my comment 5 below.

3) The authors do not seem to have grasped the full implications of the norm choice when constructing BVs. I do not understand the text at the end of section 2.1. They cite Pazo et. al., 2013, but they do not seem to understand that BV depend on the norm and that some norms produce more diversity (higher dimension) of the ensemble than others. BVs collapse into the dominant (backward) LV can be controlled by the use of

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the zero-norm. This avoids using for instance orthogonalization of the BV ensemble, with all its artifacts and unwanted effects (like the reset of the all spatial correlation information contained in the BVs).

4) Orthogonalization as explained in Sec 2.4. Why the whole time cycle summation enters in the formula (6)? I would expect one needs to orthogonalize at the end of the interval. In any case, this step is not well explained.

5) Sec. 4, Local Lyapunov estimates. I have real troubles with this section.

5.a) Here, the authors want to compute the forward LVs so they can compare with their forward BVs. Forward LVs are the eigenvectors of  $M^*M$  and not those of  $MM^*$  (see Legras and Vautard, for instance).

5.b) The authors say they compute 50 random perturbed runs and average  $M_2$ . This I do not understand at all. One does not need perturbed runs to get  $M$  because it is the operator of the linearized evolution equation. It just depends on the system state at time  $t$  and time horizon  $t_{end}$ .

All in all, I am uncertain what set of vectors are the authors comparing to singular forward, backward, or something else.

5.c) The authors then say: “In theory, the global Lyapunov exponents and vectors could be obtained by repeating this procedure for an infinite number of target time steps and averaging over the resulting structures”. This is plain incorrect. The forward LVs and Lyapunov exponents will come from taking the infinite time limit (very long time limit in simulations).

6) I do not fully understand the proposed self-breeding method. They seem to breed a vector for a time cycle, from  $t$  up to time  $t_{bred}$ , then adding the result (rescaled) back to the initial state at  $t$  and repeat for a number of times. However, this does not make much sense to me. After breeding one cycle, one is left with an ensemble of perturbations that can be more or less close to the (forward) tangent space at time

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$t_{\text{bred}}$ . These perturbations are not tangent at time  $t$  and, therefore, they would point out of the attractor when added at the state time  $t$ . They are somehow incompatible with the linear dynamics at time  $t$ .

Note that in the usual (backward) breeding, one resets the amplitude and adds the BV to the present state, not the initial or any other state. This gives an ensemble progressively projected into the tangent space. Just as described in Sec 2.1 of the paper.

7) In my opinion, the orthogonalization of the BVs and comparison with the singular vectors makes little sense when used to validate the algorithm. Given any ensemble of perturbations they tend to collapse into the leading LV direction. Depending on the norm used, this collapse can be complete or partial and so, the ensemble dimension would always be less than the number of members. Orthogonalizing just removes all the information about other unstable directions. Obviously, the orthogonalized set would cover better the subspace spanned by the singular LVs because themselves form a orthogonal set, but this does not mean the ET BVs represent better the dominant instabilities. Actually, the dominant unstable directions are not orthogonal to each other. Therefore, fig. 7b seems trivial.

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