Editorial Comments on the paper:
Multiple scale error growth in a convection-resolving model
By F. Uboldi and A. Trevisan

Dear Authors: After reviewing the referees' comments, your reply to those, and the revised manuscript, I find your paper acceptable for publication in NPG subject to minor revisions. For specific comments, see below. As most readers may look only at the final version of your paper (and not the associated reviews and your responses) I respectfully ask that you incorporate your responses into the manuscript where appropriate. After you submit it, I will inspect your revised manuscript before publication. Thank you for considering NPG for publishing your research results.
Zoltan Toth

Editorial Comments:
Related to Reviewer Comments:
Reviewer 1, Comments #1 about lack of considering model error in your study
Generally I concur with your point that the introduction of model errors would make it more difficult to study the chaotically growing errors that is the subject of your study. Two related, relatively minor points:

a) Please consider adding a quick note also in the abstract, clarifying that this is a "perfect model" type analysis of chaotically growing errors
b) As we understand from a number of studies (including those by Nicolis, Vannitsem, and others) initial value and model related errors evolve in a convoluted manner. For the sake of completeness you may refer to this, to acknowledge another limitation of your approach?

Reviewer 2, Comment #1 about role of model error
I concur that during forecast integrations, model error is not present since the same model is used in the forecast process as that in generating "truth". On the other hand, if the initial condition is chosen to be a state produced by another model (l.205, "free MOLOCH evolution initiated by an external model state"), an initial drift may occur during the forecast integration that is model error related. Can you please consider if that is really the case, and whether that may have any impact on the results?

Reviewer 1, Comment #2 – amplitude vs other features of errors?
I find this a more substantive question. I find your point A somewhat vague. And your response B incomplete. What I mean here is that you do use very special, dynamically constrained BV vectors as initial perturbations – clearly because you believe initial perturbation structures (and not only amplitudes) matter. This is the perspective from where I find your point B incomplete. Perhaps you could consider at what lead time forecasts would be used in data assimilation or forecasting applications (whatever your focus is) and relate your point to that? Clearly, error converge to a dynamically constrained, growing subspace – if your focus is beyond this transitional period, your point B may be valid, but you don't

reflect on this and the reader may be confused. I find your points C and “final comment” convincing. Wonder if you need to strengthen the manuscript by incorporating these points?

I appreciate your effort responding to the other points of the reviewers. Please consider if any additional changes in the manuscript related to the reviwers’ questions may be beneficial for the readers.

Additional Comments from the Editor:

l.186-195 – Nice explanation related to errors associated with different scales / instabilities…

l.244 – “All perturbations are randomly generated, point by point on the 3-D domain, on variables T, U, and V” – you are referring here to the “seed” that you start up the breeding procedure, is that correct? Please clarify

l.316 – “orthogonal projection of the forecast error on the subsets of BVs” – note that Wei and Toth (2003) used the same or very similar technique to compute what they call Perturbation versus Error Correlation Analysis (PECA) scores. In your perfect model experiment, you know and use real forecast errors while they used an estimate of forecast errors (forecast minus verifying analysis):
Wei, M. and Z. Toth, 2003: A New Measure of Ensemble Performance: Perturbation versus Error Correlation Analysis (PECA). Mon. Wea. Rev., 131, 1549-1565.
Your Fig 6a is analogous to their Fig. 4f.

l.426 – “after subtracting the BV-estimated com- ponent, the growth of fast instabilities is reactivated and rapidly brings the error to its previous level in the subsequent free evolution” – while I understand what you say here the word “reactivate” may confuse some. Wonder if a better word / expression could be used here, e.g., “fast perturbation growth fueled by strong instabilities recreates related fine scale errors to bring overall errors to their level before the removal of the fine-scale errors” – or something similar?

l.446 – “In the real case study at hand, it is impossible to compute the entire set of Lyapunov vectors with positive growth rate that would give us all the information on the instabilities of the system.” You point out that it is computationally impossible to compute the full spectrum. Had you been able to determine the full spectrum, you would still need a tool to identify the fastest growing vectors corresponding to particular types of instabilities that dominate once faster growing vectors nonlinearly saturate. You may still need to use breeding or some similar tool to do that? Point is that it is not only due to computational constraints that make you use the breeding algorithm here?

l.453 – "the number of BVs that are active during the convective episodes is not extremely large" – this is vague, can you be more specific?

Fig. 5 – Wonder if an alternative display of the data may be easier to access for the readers? Have you considered switching rescaling ampolitude (X axis now) and lead time (symbols, if I understand it right) in the figure (ie, showing data as a function of lead time, with curves for different amplitudes)?