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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Implications of model error for numerical climate prediction" *by* O. Martínez-Alvarado

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To Hannah Arnold

Thank you very much for reviewing this contribution. In the following I give a response to your comments indicating whether changes to a final version of the paper would be suitable or not. (Your comments are in *italics*).

Specific Comments to Author

It would be helpful to indicate how one L63 model time unit relates to time in the real atmosphere (eg by comparing error doubling times, as in Lorenz, 1996)

The error-doubling times in the Lorenz '63 system range between around 0.15 t.u.

and 8 t.u. (Smith et al. 1999). Choosing a typical error-doubling time scale of 2 t.u. and following the arguments in Lorenz (1996) we could identify 1.t.u. with 1 terrestrial day. However, given the extreme dissimilarity between systems would make me wary of attempting such a comparison and therefore I would prefer to avoid it and regard the toy system as completely detached from any comparison with the Earth's climate system.

P139 L11-14 it is unclear what data you are using to generate your PDFs for this figure. Do you use the cumulative data from many five-t.u. forecasts? Or do you use instantaneous data i.e. plot the pdf of forecasts at a lead time of 1 t.u. In which case, is a sample size of 16 forecasts (80/5) sufficient to estimate this pdf?

Thank you very much for pointing this out. The answer to your question is the second option: The pdf for a given lead time is constructed from the states corresponding to forecasts at that lead time. This is related to the Clarification Note I added to the interactive discussion on 29 April 2014. The long-term integrations were actually 5000 t.u. long, from which 1000 5-t.u. forecast cycles can be performed. Of course, this would be corrected if a revised version of the article is allowed.

Section 4.1: I found this section very interesting - it is an innovative way of using weather forecasts to reveal some of the characteristics of the climate attractor. However, I am a little unsure of how you have produced figure 5, since you have moved from considering deterministic forecasts to ensemble forecasts. Do you collect together all one-day ensemble forecasts and calculate the IQR and median of this mega-ensemble (really looking at the climatology of the forecasts as a function of lead time). If this is the case, I would be interested to see the difference between the stochastically perturbed ensemble forecasts and the deterministic control run using this diagnostic. As you go on to discuss later, the ensemble tends to cluster around the control forecast in figure 6 instead of encompassing the analysis (which we would hope for an ensemble forecast which includes a representation of model error). However, stochastic parametrisation schemes have been shown to reduce model biases, so arguably a stochastic climate

model would have a different attractor to its deterministic counterpart. Can you detect if there is a noticeable difference using your method in figure 5?

I mention later on in the paper (as the second potential caveat of the analysis, P141 L27) that only control members have been used for the analysis. I realise now that this has to be made clear before that point. So the answer to the questions is that the analysis is really made on a set of deterministic forecasts even though they come from operational EPSs. However, I agree that it would be very interesting to perform the analysis that you suggest to find the difference between the stochastically perturbed and the control forecasts. I consider that this is out of the scope of the present paper but I am planning to do a similar analysis in the near future. I am also very interested on the question of the differences between the attractors of a deterministic model and its stochastically perturbed relatives.

Technical Corrections

P134 L27-29: I found this sentence confusing to read. Perhaps replace with "...shares its dimensionality with the prototype system, whereas numerical weather ..."

Correction accepted

P136 L16. Should read "Figure 1"

Correction accepted

P137 L15. Should read "Lorenz '63"

Correction accepted

P150 Fig 2a caption: I found this caption very confusing to read - could it be restructured so that each model is addressed in turn, mentioning both the orbit and observations?

Certainly this can be done in a revised version if this is allowed.

References

Smith, L.A, Ziehmann, C. and Fraedrich K. (1999) Uncertainty dynamics and predictability in chaotic systems. Q. J. R. Meteorol. Soc, 125, 2855–2886.

Interactive comment on Nonlin. Processes Geophys. Discuss., 1, 131, 2014.