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Interactive Comment

Interactive comment on "Implications of model error for numerical climate prediction" *by* O. Martínez-Alvarado

O. Martinez-Alvarado

O.MartinezAlvarado@reading.ac.uk

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Dear Prof Hsieh,

I have prepared a revised manuscript in which the comments from the reviewers have been considered. I thank again both reviewers for the time invested in the review of my article.

The following changes have been made in response to Dr Arnold's comments:

1. The period-doubling time of the Lorenz '63 system under the settings of the prototype system has been included in the description of the prototype system.

2. The time of integration and the number of available forecasts have been corrected

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from the original description. In response also to Anonymous Referee #2's comments the time of integration is now 10000 t.u. (rather than 5000 t.u.) and the number of available forecasts is 2000 (rather than 1000).

3. The use of the ensemble prediction system's control members only has been made clear at the beginning of the discussion of the use of the TIGGE data. A comment regarding further analysis considering full ensembles has also been added to the conclusions, but this has been left for future research.

4. The suggested changes included in the technical correction have all been made.

The following comments refer to Anonymous Referee #2's comments, indicating also the changes made as a result:

1. The Referee has repeatedly commented that longer simulations are needed even after the inclusion of a clarification note in the interactive discussion (29 April 2014) making clear that the simulations were 5×10^3 t.u. I disagree with the Referee's comments because of the following reasons:

   - The requirement of longer simulations is based on the assumption that the results would change if more states are included in the computation of statistics. However, Fig. 4 in the original version, Fig. 6 in the revised version, shows a PDF comparison between the long-term integration (5×10^3 t.u. equivalent to 5×10^5 points in the original version, 10^4 t.u. equivalent to 10^6 points in the revised version) and the sets of perfect- and imperfect-model forecasts (1000 states in the original version, 2000 states in the revised version). The fact that the three datasets very closely coincide at $t_L = 0$ indicates that the PDF are similar. They are not exactly the same because the datasets are slightly different and of course of different sizes.

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- As hinted in the previous point I have now doubled the time of integration from 5×10^3 t.u. to 10^4 t.u. The joint PDFs generated from these integrations are shown in Fig. 1 of this response. Both integration lengths produce virtually the same results.

2. The Referee has commented that Fig. 1 should show a PDF and in his clarification reply he/she indicates that the reason for this is that the PDF characterises the climatological behaviour of the system. This is correct, but it is not the only option. For the study presented in the article, the argument only required showing that prototype system and the imperfect model evolved in similar but slightly different regions of the phase space. Even though the referee disagrees, this objective was achieved by the original Fig. 1. Nevertheless, I have now included the joint (x,z) PDFs.

3. The Referee has commented that Fig. 2b would change with longer integrations. My original description was misleading in that it seem to say that every initial point was depicted in Fig. 2 whereas the figure only shows a short segment of the whole time series. I have corrected the description in the text to make this clear. Fig. 2b does not change, but I have included a PDF constructed using every initial point. This is now Fig. 3 in the revised version.

4. The Referee has commented that Fig. 3 in the original version would change with longer simulations implying that the conclusions were incorrect. In particular the Referee stated that "the plateau is caused by insufficient statistics". The Referee's comment is incorrect as can be seen by comparing Fig. 4 in the revised version (constructed with the new longer datasets) and Fig. 3 in the original version. Both figures are virtually the same and therefore the original conclusions remain.

5. Regarding Fig. 6 in the original version, Fig. 11 in the revised version, I thank the clarification made by Anonymous Referee #2. From that clarification I conclude

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that his/her argument and mine are essentially the same: The unperturbed model has a given attractor. Perturbing the physics (and initial conditions) produces ensemble forecast with a given spread. Perturbing the physics more or differently can lead to the increase (or decrease) of the spread. However, the attractor of the model will always be different to that of the true system. In the case shown in the article the model appears under-dispersive. Therefore, every member tends "towards the model's attractor and away from the true future state of the system" as stated in the article. I have added a similar comment to this in the discussion of this figure. I must reiterate here that saying "The ensemble fails to reproduce the model error" makes no sense as model error is already present and does not need to be reproduced. That is not the purpose of including stochastic parameterisations in an ensemble prediction system.

6. The Referee has repeatedly commented that the results in the article are not new. Yet he/she has failed to provide relevant references supporting his/her claim. Furthermore, as discussed above, his/her comments regarding changes in the results after increasing the length of integrations are incorrect. This shows that either the results are not that well known or the Referee is aware that there is a set of scientists who know these results well, but neither he/she nor I are members of such a set. In either case, I do not accept these as valid arguments to request the change of my article before being published.

   It is true that several groups are doing work based on comparisons of hindcasts to the reanalyses from which the former are initialised. However, to my knowledge there is no complete theoretical background to support that work. This is of course not the first time this happens: The development of thermodynamics followed the empirical development of steam engines, for example. As in that case, the development of a full theory to support and reinforce the findings of those studies should prove useful. Furthermore, the approach has so far been used to attempt the assessment of model parameterisation leading to model improve-

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ment. This has led to certain degree of success, but we have to accept that model error will never be fully removed. I believe the approach can be useful not only for this purpose but also to understand the development of biases in itself, which should help to interpret the results of climate change projections, especially at the regional level.

7. My response to the Referee's minor comments are the same as those in my original reply (6 May 2014). Nevertheless, I have changed Fig. 2 according to the Referee's suggestions. The 25th and 75th percentiles have not been removed from either Fig. 3a in the original version (Fig. 4a in the revised version) or Fig. 4 in the original version (Fig. 6 in the revised version).

There are several additional changes to the article. These changes are summarised through the new figures:

1. Figure 5 shows a comparison between the results in this paper and theoretical expressions developed by Nicolis et al. (2009) for small model error (through incorrect parameter values) and small initial condition error.

2. Figure 7 shows the change with forecast lead time of the Hellinger distance between the PDFs generated from a perfect model with different levels of initial condition error and that generated through the long-term integration of the prototype system.

3. Figure 8 and Fig. 9 show the apparent relationship between the Hellinger distance for imperfect models and the growth of the distance between prototype system's orbit and model orbits.

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

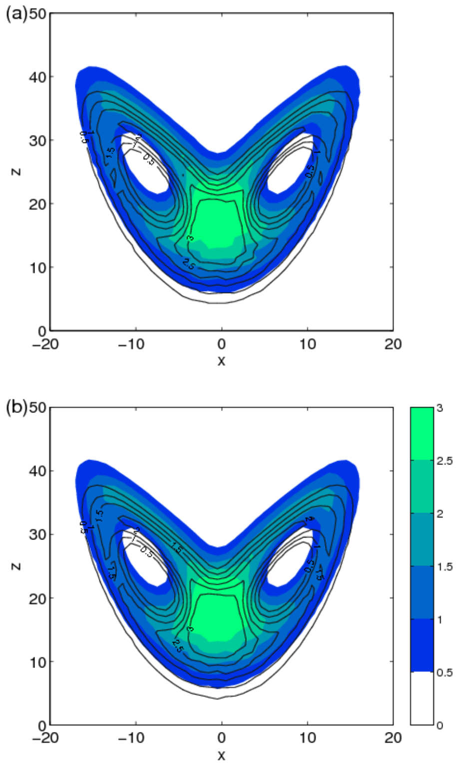

Figure 1: Joint probability density functions ($\times 10^3$) for phase space variables (x, z) resulting from **(a)** 5×10^3 t.u. and **(b)** 10^4 t.u long integration of the prototype system (colour shading) and the imperfect model (line contours). The contour values are the same in both cases.

Fig. 1.

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