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Nonlinear Processes in Geophysics Discussions

Interactive comment on "Correcting for Model Changes in Statistical Post-Processing – An approach based on Response Theory" *by* Jonathan Demaeyer and Stéphane Vannitsem

Jonathan Demaeyer and Stéphane Vannitsem

jodemaey@meteo.be

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We thank the referee for his nice and constructive review and address its comments below:

1 General comments

We are glad that the referee recognize the potential of this approach. We note that indeed it would be better to already have a realistic application. This article was limited

to the proof-of-concept part. The application to real cases is currently considered, but it might require some time before it is done. As such, we hope that the present publication will provide some guidelines and attract interest to work on this. We address now the specific comments:

1. **since this approach supposes the changes in the model are small, we can wonder if it will really be useful for operations, where small changes in the model may not really impair post-processing. Furthermore, it should be interesting to see whether this approach improves over the use of dynamic filtering of residual biases (via Kalman filtering, classically). Do you have any hint about this alternative?**

   In the present model considered, we note that small changes can really impair the postprocessing. See now Fig. 7(a) and 8(a). Furthermore, experiments showed that in some cases, linear response theory can be valid outside of its predicted range of validity (see Gottwald et al. (2016) and Wormell and Gottwald (2018)). Dynamic filtering could be corrected as well, it is a good point. For instance by adjusting the past predictions in the filtering window to the new model via the response theory presented here.

2. **the stated conditions for using this correcting seem very strong: a tangent model must be available, the model change has to be provided as an analytic function. These two conditions may not be observed or the models themselves may not be available. Would it be possible to follow the same procedure in a data-based approach? In other words, could it be possible to deduce the necessary correction if one has only the two sets of forecasts on a common period without access to the models themselves?**

   A tangent model is often available together with the forecasting model, at least to evaluate the sensivity to initial condition. It would cost less to develop than a reforecasting system, and several out-of-the-box automatic differentiation packages also exist to do it. At this stage of the research, we do not see how to apply the method in a purely data-driven context. This is indeed worth exploring in the future.

3. **the extend of post-processing methods that may be corrected in this way is not very clear. You state that The only requirement is that the outcome of the minimization of the cost functions uses averages of the systems being considered. Does this mean that post-processing methods that do not use cost functions (such as random forest) are not eligible to this approach?**

   We do not have expertise on methods such as random forest, but in any case it is an interesting question that could be addressed in a subsequent work. For instance, the present method could maybe be used in conjunction with random forest, using directly the trajectories of the tangent model instead of the reforecasts trajectories. The caveat here is then to deal with the problem of fat tails. Indeed, some trajectories (the ones forming the fat tails) will show extreme deviations from the others. So new methods should be designed to take care of this issue.

2 Specific comments

1. **Do you have a reference to support this claim that post-processing is useful only up to a lead time of 3 days?**

   We believe that figures 7 (a) and 8 (a) are sufficient to support that claim. The claim concerns only the highly truncated system at hand, and not the current state-of-the-art models.

2. **Could you develop about what kind of sophistication you are thinking about?**

   We are mainly thinking about other MOS schemes, but as said above, other kind of postprocessing could be considered (Random Forest, Machine Learning, ...). We are more specific in the text now, and write:

   > "More sophisticated approaches can be evaluated in the future (other MOS schemes, ensemble MOS, . . .)."

3. **For the article to be self-contained, may you add a more comprehensive definition (with equations) for this system, maybe in the appendix?**

   The equations of the model have been added in an appendix.

4. **Is there a simple explanation why your system has only one blocking regime instead of two?**

   To our knowledge, it is not easy to explain intuitively such regime shift in nonlinear systems. A full understanding of the phenomenon should include a bifurcation analysis, and the analysis should then be translated to a physical description. In short, we do not have such a simple explanation for now.

5. **These pictures are not very clear. In panel (b) only model 0 seems to have two attractors but from the text (page9, line 12 and elsewhere) I understand that reality also should have two attractors. Please can you clarify either the text or the pictures?**

   The referee is right, the global attractor of both reality and model 0 contains two parts. It is now mentioned in the figure's caption :

"The attractors of the reality and model 0 are qualitatively similar, with two different parts which are indicated by ellipses."

6. **Later on, you say that the distribution of perturbation has been approximated with a gaussian distribution. In the conclusion, you propose to use the CLV method to get better corrections at farther lead times. I was wondering if we could improve the correction at long lead times by using a different distribution (with fatter tails) to approximate the perturbation distribution?**

   This is a possibility, but to our knowledge, estimating moments of fat tails distributions is quite difficult. This can be considered in a future work, together with the development of the approach in a more realistic environment.

7. **Based on the results, it seems this approach may require a very short period where both models are available. The length of the comon period (a few months?) seems to correspond to what may be available operationnaly in national weather services. This is a very good point, to check on real data, maybe in a future study.**

   There is no need of an overlapping period, except for verification of the method. In the future we indeed plan to use this approach in a realistic context.

3 Technical corrections

All the technical corrections have been addressed. We thank again the reviewer for his help.

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