Earth System
Dynamics
Discussions

Interactive comment on "A multi-model ensemble method that combines imperfect models through learning" by L. A. van den Berge et al.

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Dear Dr. van den Berge,

The referees' comments on your work have now been received. You will see that the referees have several concerns with the suggested approach. I agree with the referees' concerns and I think that the comments of both referees have to be treated as major comments (i.e: giving rise to major revisions). However, the idea of combining the individual models during the simulation into one super-model is new and there could be applications in the future following this approach. As such the topic addressed by this paper may be worth of studying. Consequently, if you think you can handle major revisions, I am ready to consider a considerably revised manuscript following the referees' comments. For sake of completeness, the Major revisions by the referees'

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are reported at the end of this Editor comment.

-I'd like to add something to what highlighted by the referees:

1) We are talking about a statistical combination of the models which is trained from historical observations. It follows that the resulting multi model is best suited for present climate conditions. The proposed procedure cannot guarantee against possible over-fitting of the training data and as a consequence it is not well indicated in order to sample the climate change related to future scenarios such as the centennial-scale projections which are typically investigated (IPCC AR4). Maybe, this super-model approach could be eventually more indicated for weather and short-term climate predictions. -I cannot agree with the following consideration reported at page 268 (lines 6-12) by the authors: "The problem is not peculiar to the super-modeling approach, but arises with climate models generally, since they are "tuned" on historical data and are used to predict the climate response to a change in greenhouse gas concentrations." I cannot agree with the authors in this consideration since tuning (if any) of climate models is not methodological. It, eventually, indirectly arises while trying to fix models' deficiencies. On the other hand, super-models are, by definition, trained with historical observations. Furthermore, the fact that you obtained "the encouraging result that for the Lorenz 1963 system the super-model was able to accurately predict the change to a doubling of the parameter." do not overcome the basic limitation of the method.

2) At page 251 (lines 12-13) the following text is reported: "Model errors dominate the initial divergence between model and truth, but at later times internal error growth dominates." The opposite behavior is what is actually expected.

If you have any questions, please don't hesitate to contact me. Thank you.

Yours sincerely,

Andrea Alessandri

Referee #1

1) The authors link similar nonlinear model equations as an attempt to improve the prediction of the ensemble of models, instead of simply averaging multiple independent models. A problem with this approach is that combining nonlinear systems does not create a sort of average system; the coupling between systems can cause major changes in the dynamics. Examples of how coupling can change dynamics may be found in Pecora et al, Chaos vol. 7, p. 520-543 (1997).

2) While it is possible to adjust the coupled models to produce reasonable agreement with the true model, a constant problem with modeling nonlinear systems is that beyond the known training data, there may be some bifurcation that does not fit the model. It is always possible to have regions of phase space that are rarely visited, so they may not be seen during training, but they can affect later results. I haven't been able to find a reference, but there was a contest in the 1990's to see how accurately different models could predict the future of a chaotic signal. Some models gave good short term predictions, but none could give long term predictions.

3) In eq. 5, the authors couple their combined model to the true Lorenz signal and use the coupling constant necessary to synchronize the models as a measure of the model accuracy. The coupling constant value may be related to the model error, but it is also possible that the coupling constant value is simple a measure of the stability of the combined system; it may be that the system that is closer to the truth is also less stable in this coupling configuration, so that it requires a larger coupling constant.

4) To summarize, combining models with coupling, as the authors propose, may actually produce a worse model than the individual models. Much knowledge of the actual processes of climate goes into the individual models; coupling these nonlinear models can alter them in unpredictable ways.

5) There are model estimation methods that are similar in spirit to the authors ideas. See, for example, J. C. Quinn et al, Parameter and state estimation of experimental chaotic systems using synchronization, Physical Review E vol. 80, 016201 (2009)

Referee #2

1. The authors mention in the introduction as a motivation for their approach the study by Kirtman et al., (2003) where certain components of two different atmospheric models (momentum flux from one model, heat and fresh water flux from the other model) were coupled to an ocean model. By doing so one will almost certainly violate the physical balances of the systems (momentum and heat fluxes are not independent). The authors would need to address the issue of physical imbalances in a broader context.

2. Approaches that use a learning-from-the past methodology are inherently limited in their learning to a limited sample set of events that happen to have occurred in the past. However, what are the implications for the future? Are there any at all? Is the super-model, in principle, able to simulate a behaviour that is qualitatively distinctively different to the one that was used in the training period? What are the implications to the big question of climate change then, as noted in the introduction of the manuscript?

3. In their study the authors construct the super-model by averaging the individual models. By taking the ensemble mean a lot of very valuable information gets lots. In particular, it is not clear that the ensemble mean itself is an element of the attractor of the system. I would suggest to treat the super model as an ensemble rather than as a deterministic model built from coupling individual models. This then opens the door to analysing model uncertainty in a much wider sense.

4. As the authors point out in the conclusion section, it is not clear a priori which state variables should be connected and which not. This problem is also linked to the question of how to couple systems with very different characteristic time scales. Perhaps the framework of a simplified dynamical system could be used to study these issues.

5. The authors use the paper by Rodwell and Jung (2008) to claim that fast atmospheric processes are the primary cause of systematic model errors. This statement is misleading as the paper relates to a specific example of aerosol and clearly is not

representative for a wider range of typical systematic errors. A recent paper by Palmer & Weisheimer (2010) has some more theoretical discussion on the question of origins of systematic model errors. Reference Palmer, T.N. and A. Weisheimer (2010): Diagnosing the Causes of Bias in Climate Models - Why it is so Hard? Proceedings of the ECMWF Annual Seminar 2009 on Diagnosis of Forecasting and Data Assimilation Systems, Sep 2009, 1-13.

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